Aggregate and Individual Effects of Information in a Coordination (Traffic) Game*

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ABSTRACT

Using an existing coordination (traffic) experiment, we investigate information's effect on traffic congestion when subjects already have a history of past play. In contrast to previous studies, our interventions neither alter aggregate nor individual payoffs. A second study isolates individual-subject response to information using a fixed distribution of past subjects. We find information alters subject play: subjects switch roads more often and receive higher payoffs conditional on switching roads. Because switching reduces payoffs unconditionally, information does not generally improve payoffs overall. Only subjects that receive information upon starting the game appear to increase their payoffs due to the information treatment.

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I. INTRODUCTION

Priced managed lanes (MLs) provide an option for the traveler to either meet a certain criterion (carpooling being a common one) to travel toll-free or pay a toll to use the lanes. These lanes can be broadly differentiated based on operational characteristics or toll collection techniques. These lanes are gaining popularity as a congestion management measure providing a fast and reliable travel option.

In a standard rational framework, we expect commuters to fill MLs and general-purpose lanes (GPLs) and earn equal profits across both options. That is, the gains in time from using an ML should equal its toll cost. This analysis would implicitly assume that travelers are making an optimization decision between MLs and the adjacent toll-free GPLs and updating their decision as the underlying parameters of that decision change. However, recent research shows that many travelers exhibit all-or-nothing behavior while traveling on a freeway with MLs. Using revealed preference data from the Katy Freeway MLs (Houston) and the North Tarrant Express Lanes (Dallas), Burris and Brady (2018) observed that nearly 85 percent of travelers drive either on the GPLs or the MLs. The variables that make up the rational decision-making framework of this choice (e.g., price, congestion level, time savings) vary greatly day-to-day. Moreover, among the 15 percent of travelers who vary their lane choice, many made choices in opposition to what would be expected based on their travel time savings and toll rate (Burris and Brady, 2018).

A possible intervention in this environment is the supply of additional information. Such informational disclosures could take the form of highway signs or mobile phone alerts. However, in a game theoretic setting such as traffic coordination, response to information is not transparent. Laboratory experiments may provide some guidance. This paper presents experiments to bridge the gap between these known empirical findings in traffic and the experimental representations of traffic games.

In the work on which much of our design is based, Selten et al. (2004, 2007) model traffic as a group of 18 subjects choosing one of two roads. Subject travel time (and ultimately payoffs) depends on picking the less crowded road relative to capacity. In their control, all subjects only learn their own travel time and can infer their payoff only on their chosen road. In their treatment, subjects learn information on both roads. Results indicate that the added information of the treatment *decreases* route changes and *increases* payoffs toward equilibrium levels.

These experimental findings appear at odds with the current intuition in the managed-lanes, empirical environment. The field results suggest that a large portion of drivers are prone to constant behavior and need informational interventions to produce change, increasing payoffs. In contrast, the experiments suggest that subjects change routes too often in the laboratory and increase payoffs by staying in place.

What factors might produce such different results? We see three main differences between the respective environments. First, in the experimental environment, subjects enter without any experience in the traffic game; informational interventions in the field will occur on drivers who have been driving the same routes many times previously and thus are more prone to habitual behavior. Second, informational interventions will only reach part of travelers; at most, they will affect a small portion of the population. Finally, while the experimental environment is relatively large, with 18 players in a game, the number of players is still small enough that any individual may credibly believe their decisions could alter aggregate behavior. In the field, the game involves such a large number of players that this belief is no longer realistic.

This paper presents an experiment, based on Selten et al.'s (2004, 2007) design, that looks to account for these differences and see how they might affect results. In our first study, Study 1, we repeat the group experiment, only allowing treated subjects to become informed after 50 periods of playing the game without information. In many cases, only a portion of subjects is treated. In a second study, Study 2, we examine the effects of providing information to subjects playing against past distributions of the first study. The design choice puts subjects in a decision (rather than strategic) environment where their choices cannot alter the strategies of other players, similar to real-world freeway travel.

The changes in design choice alter our results relative to Selten et al. (2004, 2007). Information treatment has little effect on total earnings or equilibrium convergence in Study 1. With the focus on individual subjects, Study 2 allows us better to examine the causes of the failure of this information. Subjects were randomly selected to be treated with information after period 50, like in our Study 1 experiment. Across two distinct study populations, we observe the same two general findings. First, information treatment does not increase the earnings of treated individuals. Second, information treatment increases how often subjects switch roads. Another treatment examines what happens when information is given to subjects starting in the first period (i.e., "fully" rather than "partially" treated), similar to the original Selten et al. (2004, 2007) study. Only among the fully treated do we observe a positive effect on earnings of information treatment (and only in the first 50 periods), suggesting that the benefits of these information interventions are most significant when subjects have no prior history of play.

Further analysis reveals that any information treatment increases a subject's propensity to switch roads regardless of whether the newly-chosen road had greater, equal, or lesser payoffs than the subject's previously-chosen road in the last round. However, this effect is most pronounced when the newly-chosen road's payoffs exceed the previously-chosen road in the previous period. Indeed, an analysis of possible game-theoretic strategies employed by subjects reveals that the partial information treatment makes subjects exhibit a greater propensity to be a direct responder, that is, respond to the past period's payoffs as if they are the current payoffs. Curiously, no such relationship is found in the full information treatment. Such strategy, as well as switching in general, are generally not profitable in the experiment. While both information treatments appear to make switches, on average, more profitable (relative to the control), the effect on payoffs is offset by the greater frequency in which subjects switch. Thus, we generally do not observe an effect on aggregate payoffs. Taken together, these results support some findings from both the previous experimental paradigm (i.e., switching is not profitable) and conventional wisdom in the empirical analysis of traffic (i.e., information prods individuals toward action).

It is unclear whether the benefits of direct response and switching are positive in actual field traffic settings. In such environments, information treatment may increase individual payoffs. For example, using google maps or a traffic report on the radio to avoid congestion on your initial route. As long as the new route is not flooded with rerouted drivers there should be a positive payoff. To provide general guidance to practitioners in this area, in our penultimate section, we examine what types of subjects are most likely to benefit from informational intervention. Our regression results positively correlate with the number of correct answers on the cognitive reflection task (CRT). A standard deviation increase on the test is correlated with a gain (or loss) in per-period earnings of about 0.25, roughly a 3% impact on earnings. The deeper implications of cognitive reflection are consistent with these results.

A. Literature Review

Outside of Selten et al., several previous studies explore route choice behavior under both exogenous and endogenous uncertainty using laboratory experiments. Overall, results tend to show that information helps travelers navigate congestion better. Lu et al. (2011) study the impact of real-time information regarding exogenous uncertainty, ex-post information about alternative (non-chosen) routes, and the combination of both types of information on route choice in a three-route network subject to congestion. Their findings suggest that the most efficient information structure is to provide real-time information rather than foregone-payoff information. Liu et al. (2020) evaluate the impact of different proportions of users having complete network information about route choice behavior in a three-route Braess network. There are two types of players in their setting: players that observe the complete route choice distribution for all of the previous periods and players who only possess information about their chosen routes. They obtain results similar to Rapoport et al. (2009), finding that traffic flow in different informational conditions tends to converge to equilibrium regardless of the proportion of players with complete route-choice information. Regarding feedback information, Bogers and van Zuylen (2005) showed that respondents who were provided with foregone payoffs; that is, feedback for both chosen and non-chosen alternatives, spent less time on the road, though these benefits decreased over time as more experience was accumulated. Noussair and Qiao (2021) conducted a binary route choice laboratory experiment to study the influence of past information penetration levels on congestion and the valuation of information. They find that in the long run, partial and full dissemination of information to participants achieves less congestion on aggregate than no information about the number of entrants on the two possible routes.

Market entry games are another class of games found in the experimental literature that is closely related to our set-up. Often in these games, subjects have the choice to either enter a market or stay out (Rapoport et al., 2002; Erev and Rapoport, 1998). A pattern, referenced as “magic” by Kahneman (1988) and Erev and Rapoport (1998), observed in such games is that the aggregate behavior of players exhibits regularities and converges to equilibrium outcomes. The payoff for entering the market is a decreasing function of the number of entrants, and the payoff for staying out is a constant. Other variations of the market entry game have been studied, such as choosing between two routes – road and metro, where travel cost is increasing in the number of commuters who choose the road while decreasing in the number of commuters who choose the metro (Dechenaux et al., 2013). One may argue that the route choice game is similar to a market entry game with two markets instead of one. However, subjects cannot choose to stay out of both markets.

Our approach to match subjects against distributions of play from past sessions has only been used one other time to the best of our knowledge – Ferraro and Vossler (2010) used this method in a repeated public goods game that features a dominant strategy to never contribute. Since subjects are playing against past distributions, no other party can benefit from a subject's decision to contribute to the public good. The authors find that many subjects still choose to contribute positive amounts and interpret this result as due to subject confusion.

II. EXPERIMENTAL DESIGN AND PROCEDURES

A. Study 1: An Interactive Traffic Coordination Game in Groups

The experiment consisted of subjects making decisions in the traffic coordination game of Selten et al. (2004, 2007). Subjects represented a single vehicle and chose to travel from point A to point B either on a "main road" (M) or a "side road" (S). In each period, eighteen vehicles represented eighteen individual subject decisions on the roads; travel time increased on the main or side road depending on how many vehicles chose that respective option. Formally, the travel times t_M and t_S were determined by the number of vehicles on each road, n_M and n_s, respectively, where t_M=6+2n_M and t_S=12+3n_S. Payoffs were determined by $40-t$ with $t= t_M$ if M was chosen and $t= t_S$ if S was chosen.

All pure-strategy Nash equilibria of this game feature 12 drivers on the main road and 6 on the side road. This would result in all drivers taking 30 minutes on their road choice, resulting in a payoff of 10 for all players. We know this outcome is a Nash equilibrium because should a driver on the main road deviate to the side road, he would earn 40-[12+3(6)]=7, reducing earnings; should a driver on the side road deviate to the main road he would earn 40-[6+2(13)]=8, also reducing earnings.[1] There also exist asymmetric mixed-strategy equilibria in which some players play pure strategies while others mix their entry decision between the main and side roads. In such equilibria, $n_M<12$ enter the main road with probability one, $n_S<6$ enter the side road with probability one, and the remaining $18-n_m-n_s$ enter the main road with probability $p_m=(11.6-n_m)/(17- n_m- n_s)$. Note that for $n_m=n_s=0$, which describes a situation where no player chooses one of the two roads deterministically, equilibrium entry in the main road (p_m) is that of the symmetric mixed strategy equilibrium where all players mix their entry decision between the two roads with the same probabilities.

In Appendix B, we explore how expected road changes fluctuate with asymmetric mixed strategy equilibria. Further, note that p_m increases in n_s which is intuitively expected given that higher entry in the side road means incentives rise for entering the main road. Also, p_m decreases in n_m, meaning incentives for entering the main road decrease as more people deterministically sort into the main road. In any one of these asymmetric mixed Nash equilibria, the expected number of entrants in the main road is $n_m + (11.6-n_m)/(17- n_m- n_s)(18- n_m- n_s)$, and the expected number of entrants in the side road is $n_s + [1-(11.6-n_m)/(17- n_m- n_s)](18- n_m- n_s)$ which together add up to 18 for any $0\leq n_m<12$ and $0\leq n_S<6$.

[1] The only symmetric Nash equilibrium entails mixing between entering the main road and side road. The symmetric mixed-strategy equilibrium exists where all drivers choose the main (side) road just over 68% (just under 32%) of the time, but there is little evidence of these types of strategies being employed in either Selten et al.'s (2004, 2007) data and our own. We discuss further possibilities of equilibria in Mathematical Appendix B.

A distinct Pareto-optimum outcome also exists, n_M=11 and n_S=7, which results in payoffs of 12 on the main road and 7 on the side road. The total payoff for all players is 180 in the Nash Equilibrium and 181 in the Pareto optimum. The Pareto optimum is not an equilibrium; a subject on the side road could deviate to the main road and increase her earnings by 3.

Following Selten et al. (2004, 2007), full details on the determinants of traffic time (e.g., the functions for t_S and t_M) were not provided to subjects. Instead, subjects were given details about (i) their road choice in the previous period, (ii) the travel time endured on that road choice, and (iii) the respective payoff. As in the previous experiment, subjects were also informed that their travel time increases with the number of commuters on that road, and if the same number of subjects pick the main and side road, the main road is faster (i.e., the capacity of the main road is larger). In general, subjects were not informed about travel time nor the potential payoff on the road choice not taken.

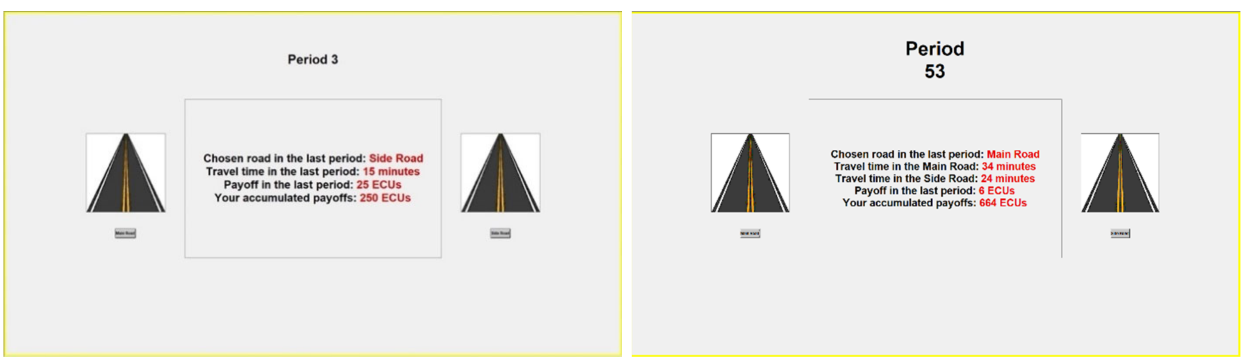

Figure 1: Interface for Study 1. Subjects played simultaneously in groups of 18 in a group session. Subjects only learned the payoffs of the road they chose (a, left) unless it was part of a treatment (b, right) to be given past payoffs on the road not chosen. The information treatment only occurred after period 50. Subjects were not told the explicit formula that maps subject choices to payoffs.

Subjects made their traffic decision for 100 periods. In all treatments of Study 1, subjects received the same level of information for the first 50 periods. Beginning in Period 51, depending on treatment, some subjects were selected to be informed about the payoffs on the road they did not choose in the past period in addition to the payoffs on the road they did (see Figure 1).[2]

It is important to recognize that this design, similar to the original Selten et al. (2004, 2007) experiments, does not give subjects common knowledge of the payoff function. Common knowledge of payoff structure is a standard assumption in game theory. Thus, any evidence of non-equilibrium behavior cannot be viewed as a rejection of the underlying theory. Instead, we are examining how well these types of equilibrium models apply to situations where game theoretic assumptions are relaxed. Our motivation is about the general applicability of game theoretical models to empirical traffic patterns. In empirical traffic situations, drivers can only infer their underlying payoff functions and often only learn the actual payoff of the road taken. Yet equilibrium predictions are often applied to empirical traffic patterns nonetheless and thus are relevant for our purposes.

Similarly, we also depart from Selten et al.'s design by informing only a limited number of subjects. We cannot claim that in such cases there is common knowledge of the payoff functions, as some subjects are better informed than others. Thus, we cannot reject any equilibrium theory in its own domain with our results. We can examine whether applying equilibrium concepts to stylized traffic-based coordination games where these assumptions are relaxed is still useful.

Treatments were administered at the session level. Four different treatments varied whether each subject would receive payoff information on the choice not taken in the second half (Periods 51-100) of the experiment.

[2] No mention of this information treatment was made to any subject prior to it occurring. To avoid issues with asymmetric information, no treated or untreated subject was informed the details of how many subjects were information treated. Experimental instructions are available as supplementary materials.

Treatment 0 (Group-Control): All subjects were equally uninformed about the travel time and respective payments of the road not taken.

Treatment 1 (Group-All): The first 50 periods follow the same protocol as the control. In the last 50 periods, all participants were provided information about the road that they did not choose in the preceding period.

Treatment 2 (Group-Frequent-4): The first 50 periods follow the same protocol as outlined above. In the last 50 periods, the four most frequent "choosers" (those that changed road choice the most in the first 50 periods) within a session were provided information about the road that they did not choose in the preceding period.

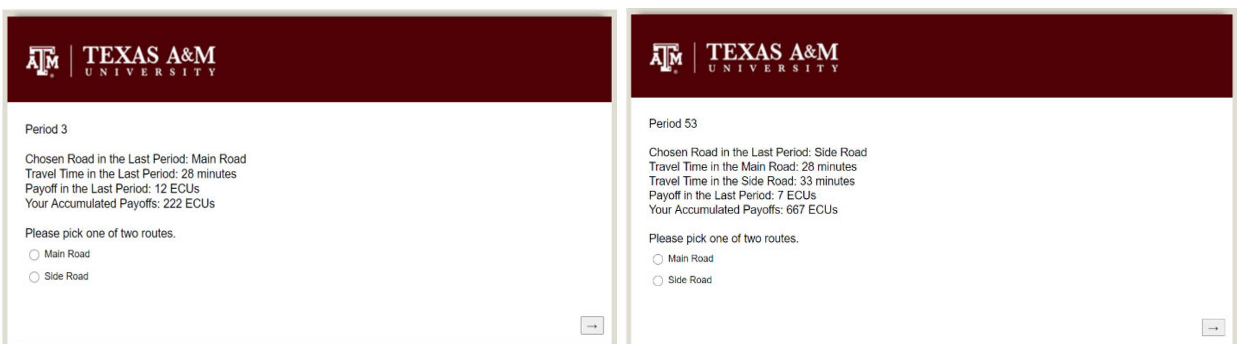

Figure 2: Interface for Study 2. Subjects made individual decisions against past distributions of a session in Study 1. Subjects only learned the payoffs of the road they chose (a, left) unless it was part of a treatment (b, right) to be given past payoffs on the road not chosen. The information treatment could occur after period 1 under "full information treatment" or after period 50 under "partial information treatment." Subjects were not told the explicit formula that maps subject choices to payoffs.

Treatment 3 (Group-Infrequent-4): The first 50 periods follow the same protocol as outlined above. In the last 50 periods, only the four participants who changed roads the least in the first 50 periods (infrequent choosers) within a session were provided information about the road they did not choose in the preceding period.

B. Study 2: Individual Decisions Against Distributions of Past Play

Study 2 featured the same 100 periods of choices as study 1. However, the underlying game structure was different: individual subjects played against the past decisions of a group session (see Figure 2).[3] Subjects were fully informed through experimental instructions that they were not playing against active subjects but rather past distributions. Information was provided in one of three treatments.

Treatment 0 (Individual-No Information, Control): Subjects in this group did not receive any information on the past play of other subjects.

Treatment 1 (Individual-Partial Information): This group of subjects received payoff information on the road not chosen, but only after 50 periods of play without any information.

Treatment 2 (Individual-Full Information): The final group of subjects received payoff information on the road not chosen for all 100 periods of the experiment.

C. Experimental Procedures

1. Study 1

Study 1 consisted of ten sessions comprised of 18 subjects each (total 180 subjects) which were run in-person at the Texas A&M Economic Research Laboratory in October and November 2019. Two sessions each of the *Control* and *All* Treatments were run; three sessions of the *Frequent-4* and three sessions of the *Infrequent-4* were conducted. The payoffs for each round were in experimental currency units (ECUs), with one ECU being \$0.015. Subjects began the experiment with an initial endowment of 200 ECUs. Each session lasted around 70 minutes, and average earnings were \$16.21 (not including the participation payment).

[3] All subjects played against a distribution of past play from Study 1, either session 1 or 2. For each round, a distribution of 17 past subject responses was drawn without replacement from the same round in the past session. The subject's choice was added to these 17 decisions to determine values of n_M and n_S which ultimately determined the individual subject's payoffs.

2. Study 2

Unlike Study 1, Study 2 was administered to two distinct populations, the student population that made up the entirety of the Study 1 population and a population of commuters from the DFW area.

Table 1A: Breakdown of experimental treatments in Study 1.

Treatment	Travel Time Information Provided in Periods 1-50	Travel Time Information Provided in Periods 51-100	Total Sessions/ Subjects Run
Control	Chosen Road Only	Chosen Road Only	2/36
All	Chosen Road Only	Both Roads	2/36
Frequent-4	Chosen Road Only	Both Roads (4 Most Frequent Switchers); Chosen Road Only (All others)	3/54
Infrequent-4	Chosen Road Only	Both Roads (4 Least Frequent Switchers); Chosen Road Only (All others)	3/54

For the student population, data from the Study 1, Session 1, which was under the *Control* treatment, was used to develop a distribution of past play (see footnote 2). Originally designed to be administered in the laboratory using specific software, five sessions of Study 2 were run on 91 subjects in May 2020 via Qualtrics using newly-developed online protocols. Guidelines were developed to conduct the survey session via Zoom to ensure the previous laboratory settings were replicated. The students attended a supervised Zoom session to participate in the experiment and to answer the survey questionnaire. Online sessions lasted around 41 minutes, and average earnings were $15.69 (not including the participation payments). Of the 91 student subjects, 42 were randomly chosen to be informed (treatment group) and 49 were randomly chosen to be uniformed (control group) comprising five sessions. An additional 93 fully treated subjects were run in July and August 2022 (see Table 1B). Of these 93 subjects, 41 played against the Study 1 session 1 distribution and 51 played against the Study 1, Session 2 distribution.

Whether in-person or online, all student subjects were recruited from the econdollars.tamu.edu database of undergraduate subjects based on the ORSEE code (Greiner, 2015). Surveys were identical for laboratory and online sessions and written in Qualtrics.

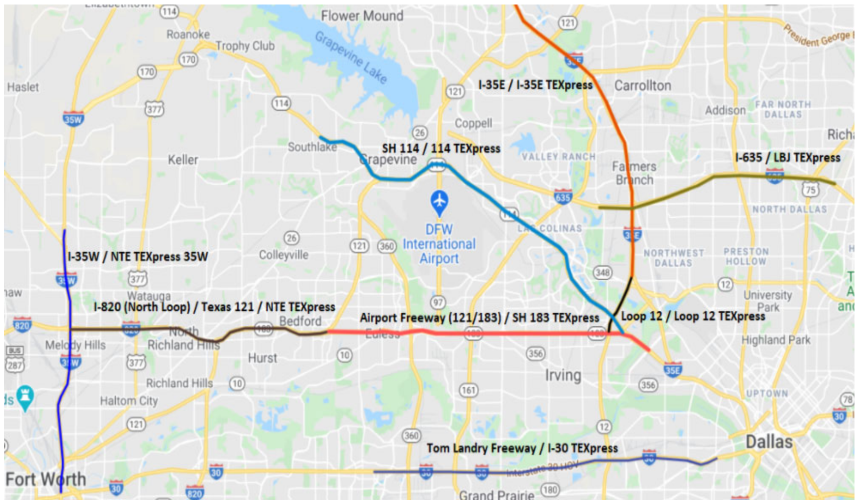

Figure 3: Study area in Dallas

Study 2 was also repeated with a commuter population. To examine the robustness of our results across a different population, one more familiar with MLs, the experiment was administered via online experiments on regular commuters in the Dallas metropolitan area. With this specific population, we chose to use data from Study 1, Session 2, which was also under the *Control* treatment as the past distribution of play for which student subjects played against.

Participants were recruited through a partnership with Cintra. Cintra is a large private-sector transportation infrastructure company that operates multiple express lanes in Dallas. Recruitment emails were sent via the official communication channels of Cintra during November 2020. Around 6000 travelers were contacted during the initial recruitment process that enquired about their use of MLs and willingness to attend subsequent stages of the study. There was a response rate of nearly 10 percent. Self-reported travel patterns on the following highways (see Figure 3) were a primary way to select study subjects: (i) I-635 / LBJ TEXpress, (ii) I-35W / NTE TEXpress 35W, (iii) I-820 (North Loop) / Texas 121 / NTE TEXpress, (iv) SH 114 / 114

TEXpress, (v) Airport Freeway (121/183) / SH 183 TEXpress, (vi) Tom Landry Freeway / I-30 TEXpress, I-35E / I-35E TEXpress, (vii) Loop 12 / Loop 12 TEXpress.

Among the 585 respondents, 280 were unwilling to attend the whole study. Further elimination of unfinished responses and removal of respondents who did not travel on the above highways resulted in 273 respondents. From the 273 interested respondents, 133 final participants were chosen to obtain a distribution of subjects based on their travel patterns, age, and gender. These 133 participants attended a supervised online session to do the traffic experiment and answer a post-experiment travel survey. During the months of November and December 2020, 22 online sessions were conducted to collect data from these participants.

Table 1B: Breakdown of experimental treatments in Study 2.

Treatment	Travel Time Information Provided in Periods 1-50	Travel Time Information Provided in Periods 51-100	Individual Subjects
Control	Chosen Road Only	Chosen Road Only	49 students 67 DFW drivers
Partial Information	Chosen Road Only	Both Roads	42 students 66 DFW drivers
Full Information	Both Roads	Both Roads	93 students

On average, it took 13 minutes and 30 minutes to complete the traffic experiment and travel survey part, respectively. The Dallas driver population began the experiment with an initial endowment of 200 ECUs; the conversion rate was 1 ECU = \$0.04. The average payment for the traffic experiment was \$42 (minimum \$36, maximum \$47), and they were paid \$50 for completing the travel survey part.

At the end of both studies, subjects completed a survey that investigated their real-world use of MLs and GPLs, their lane choice decision-making process, and trip details such as origin, destination, mode, number of passengers, etc. The survey also collected information on the student’s socio-demographic characteristics and six psychological traits. The psychological traits included conscientiousness (Goldberg et al., 1999), need for cognitive closure (Roets and Van Hiel, 2011), cognitive reflection (Toplak, West and Stanovich, 2014), maximization (Schwartz et al. 2002), risk choice (Eckel and Grossman, 2008) and general mental ability (Arthur 2017).

D. Statistical Power

Our study is primarily based on the design of Selten et al. (2004, 2007), who compare two treatments of 6 sessions each. Each session consists of a group of 18 subjects. They measure three key outcome variables at the session level and compare them using a rank sum non-parametric test. The authors perform analyses using a 1-tailed analysis at a critical p-value threshold of 0.10. Of the three values they measure, number of players on side road, route changes and payoffs, the latter two are statistically significant at their chosen levels (see Table 2). Using effect sizes calculated from their data and power of 0.8, it would take 26, 24, and 12 sessions to detect similar effects in our data (Faul et al., 2009), respectively.

Because it is most efficient to do so, we will focus on differences in payoffs (where comparing 6 sessions from each treatment should be sufficient to reach our power threshold). However, our design contains 4 sessions of solely treated or untreated observations and 6 of both treated and untreated. To conform to the structure of the rank sum test, we will treat these as 4+6x2=16 unpaired observations. This leads to implied power of 0.651, 0.680, and 0.891, respectively, for the three outcome variables. Since we are treating 6 paired observations as 12 unpaired, we will check the robustness of this assumption using the novel statistical test of Derrick et al. (2020) for combining paired and unpaired observations. Should the test effects differ substantially, we will reconsider our statistical power calculations.

In study 2, subjects are treated at the individual level. With 317 subjects across our two populations, if the effect sizes at the group level of Selten et al.'s experiments are comparable to the individual level, we should have the appropriate power to detect individual-level differences. That is, under the previous experiment's effect sizes, the probability of rejecting the null hypothesis is near 1.

Table 2: Per-period averages of players choosing side road and route changes from Tables 1 and 2 of Selten (2007) and per-period average payoffs from Tables 3 and 4 of Selten (2004).

Treatment	Session	Players on Side Road	Route Changes (Standard Deviation)	Payoffs per Period
No Information	I-01	6.02 (1.81)	5.08 (2.30)	9.10 (4.26)
	I-02	5.91 (1.69)	3.87 (1.87)	9.18 (3.94)
	I-03	6.01 (1.85)	5.16 (1.93)	9.06 (4.34)
	I-04	5.85 (1.75)	5.19 (1.93)	9.10 (4.08)
	I-05	6.10 (1.81)	5.28 (2.39)	9.13 (4.27)
	I-06	6.03 (1.79)	4.35 (2.08)	9.12 (4.26)
	Overall	5.99 (1.78)	4.82 (2.08)	9.11 (4.19)
All Information	II-01	5.98 (1.64)	3.99 (2.00)	9.25 (3.80)
	II-02	6.05 (1.58)	3.68 (2.04)	9.32 (3.77)
	II-03	5.99 (1.53)	3.67 (2.09)	9.35 (3.60)
	II-04	6.10 (1.93)	5.19 (2.32)	8.99 (4.58)
	II-05	6.06 (1.63)	4.67 (2.48)	9.28 (3.85)
	II-06	6.17 (1.69)	4.44 (2.04)	9.26 (4.03)
	Overall	6.05 (1.67)	4.27 (2.16)	9.24 (3.94)
Rank Sum Comparison		z=-1.203 p=0.114[1]	z=1.363 p=0.086[1]	z=-1.925 p=0.027[1]
Effect Size		-0.882	0.930	-1.328

[1] Rank sum test p-values are calculated using 1-tailed values as in the original source.

III. HYPOTHESES

Table 2 provides session-level comparisons of three key outcome variables from Selten et al.'s original experiment. Recall these experiments treat subjects with information for all periods or not at all. The effect of information is positive: subjects earn more by making fewer road changes. The effect on aggregate equilibrium behavior (i.e., getting exactly 6 subjects on the side road) is unclear.

A. Study 1

Study 1 deviates from the Selten paradigm in two main ways: subjects are treated with information after 50 periods of play. In some sessions, Frequent-4 and Infrequent-4, only four of 18 subjects are treated with information. These changes were desinged to bridge the Selten et al.'s study with empirical traffic observations where information is often supplied after drivers have a past history with routes and only given to some drivers. Recall empirical observations of drivers in the field suggests they alter routes too infrequently and do not equilibrate (Burris and Brady, 2018); the implication is that information would increase activity and better equilibrate overall traffic.

Hypothesis 1 (Null): Information-treated and untreated subjects do not act differently in terms of road changes or earnings.

Hypothesis 1A (Selten): Information-treated subjects make fewer road changes and earn greater earnings than information untreated.

Hypothesis 1B (Empirical Traffic Patterns): Information-treated subjects make more road changes and earn greater earnings than information untreated.

Another potential comparison can be made between the studies that differ in the number of informed subjects. Information leaked to a few may be sufficient to lead to better equilibration or may not be as effective as informing all.

Hypothesis 2 (Null): Aggregate statistics for sessions, proximity to equilibrium, road changes, and payoffs do not differ by the number of subjects in the session treated with information.

Hypothesis 2A: Increasing the number of information-treated subjects increases proximity to equilibrium and payoffs.

B. Study 2

Study 2 allows us to examine the effects of information treatment without concern about altering the existing equilibrium structure of the game. This makes the comparison of information-treated to untreated subjects much clearer. We can reexamine the first set of hypotheses in this study.

Hypothesis 3: Information-treated and untreated subjects do not act differently in terms of road changes or earnings.

Hypothesis 3A (Selten): Information-treated subjects make fewer road changes and earn greater earnings than information untreated.

Hypothesis 3B (Empirical Traffic Patterns): Information-treated subjects make more road changes and earn greater earnings than information untreated.

Having knowledge of Study 1 results, we can also propose a hybrid hypothesis that unifies that study with Selten et al.'s experiments.

Hypothesis 3C (Hybrid): Subjects treated for all 100 periods of information (fully treated) earn more than the partially treated or control treatments.

IV. RESULTS

A. Study 1

Table 3 provides statistics on side road entrance for all ten laboratory sessions under the four treatments. Observing the standard deviations, it is clear that equilibrium convergence does not occur under any of the four treatments. While on average subjects equilibrate, fluctuations around equilibrium persist until the end under all informational interventions.

In each session, the median number of players on the side road is 6. The mean number of players on the side road is between 5.82 and 6.2 during the first half for all sessions. This is evidence that even with minimal information about the alternative road, or opportunity cost, subjects (on average) equilibrate well. In the *Control* treatment, one of the sessions gets closer to the theoretical equilibrium while fluctuations decrease for both sessions over time. In the *All* treatment, where we provide all subjects in the session with travel time information about the non-chosen road in the latter half, both sessions diverge from reaching the theoretical equilibrium; this may be due to a restart effect (e.g., Andreoni 1988), whereupon receiving additional information, subjects adopt new coordination strategies. Concerning informational interventions, *Frequent-4* and *Infrequent-4*, the evidence is inconclusive as to whether providing information to a few select subjects facilitates equilibrium convergence with information having little or no significant impact. Ordered Cuzick trend tests fail to reject null Hypothesis 2. The extent of subjects treated with information (i.e., 1=no info; 2=four have info; 3=all have info) has no effect on payoffs, number of side road travelers, or overall switch rates (three separate tests have p-values ranging from 0.322-0.741, two-tailed).

Deviations from (pure-strategy) equilibrium can be measured by the standard deviation of the number of participants choosing the side road per period (Table 3). This standard deviation is between 1.88 and 2.42 in the first half for all sessions. In all but Session 6 (*Frequent-4*), fluctuations decrease in the latter half of the experiment.

Table 4 shows the mean and standard deviation of the number of road changes. In most sessions, the number of road changes decreases over time with exceptions in Session 2 (Control) and Session 4 (All). Subjects appeared to settle more generally on pure strategies as the experiment progressed.

Table 3: Number of subjects choosing side-road per period. The equilibrium value is 6

Treatment	Session	Number of Subjects Choosing Side Road (Standard Deviation)	
		Periods 1-50	Periods 51-100
Control	1	5.82 (2.45)	5.98 (1.49)
	2	6.02 (2.20)	6.12 (1.99)
All	3	6.08 (2.11)	5.86 (1.96)
	4	6.04 (1.96)	6.24 (1.84)
Frequent-4	5	5.96 (2.18)	5.88 (1.99)
	6	6.10 (2.06)	5.82 (2.35)
	7	6.14 (2.22)	5.94 (1.96)
Infrequent-4	8	6.12 (1.96)	5.80 (1.90)
	9	6.02 (1.94)	5.84 (1.49)
	10	6.20 (1.90)	5.74 (1.90)

Table 5 provides per-period subject payoffs for each session for the first 50 and last 50 periods. It is clear from the table that session-level idiosyncrasies are quite pronounced, payoffs range from 8.30-9.08 ECUs in the untreated first 50 rounds, and the effect of particular treatments is not so clear as total payoffs range from 8.42-9.39 ECUs in the last 50 periods. The highest and lowest values come from the same session, Session 1.

Table 6 provides a comparison of the second half of our experiment for treated and untreated subjects. Values for Players on Side Road and Route Changes are standardized to per 18 subjects for easy comparisons with Tables 2-5. A particularly interesting comparison is with Table 2, Selten et al.'s data. Similar to those findings, we see a modest but statistically insignificant increase in informed subjects choosing the side road. The estimated effects in our two studies are not statistically different. Our hypotheses concern the other two measures. Unlike Selten et al.'s experiment, we do not find statistically meaningful treatment effects for information on road switching or overall payoffs. That is, we cannot reject null Hypothesis 1 in favor of an alternate hypothesis. Further, the sign of both measures goes in the opposite direction of Selten et al., meaning our estimated treatment effects are significantly different from theirs ($p<0.05$).

Table 4: Number of subjects changing their road choice from the previous road per period. In the symmetric mixed-strategy equilibrium the number is 7.92.

Treatment	Session	Number of subjects changing their road choice per period (Standard Deviation)	
		Periods 1-50	Periods 51-100
Control	1	6.20 (2.73)	3.64 (1.85)
	2	6.50 (2.60)	6.70 (2.32)
All	3	5.44 (2.33)	5.16 (2.20)
	4	5.86 (2.31)	6.50 (2.61)
Frequent-4	5	5.70 (2.60)	5.46 (2.01)
	6	6.08 (2.37)	5.80 (2.16)
	7	5.94 (2.35)	5.12 (2.15)
Infrequent-4	8	5.94 (2.49)	5.60 (2.58)
	9	4.92 (3.11)	3.42 (1.67)
	10	6.02 (2.63)	4.54 (2.00)

Table 5: Average and standard deviation of per-period payoff for each subject. The theoretical equilibrium value is 10.

Treatment	Session	Per Period Payoff for each Subject (Standard Deviation)	
		Periods 1-50	Periods 51-100
Control	1	8.30 (5.65)	9.39 (3.49)
	2	8.69 (5.15)	8.96 (4.74)
All	3	8.82 (4.89)	8.90 (4.51)
	4	8.97 (4.53)	9.15 (4.34)
Frequent-4	5	8.70 (4.96)	8.88 (4.53)
	6	8.87 (4.76)	8.42 (5.60)
	7	8.70 (5.27)	8.93 (4.47)
Infrequent-4	8	9.00 (4.51)	8.94 (4.36)
	9	8.98 (4.54)	9.34 (3.41)
	10	9.08 (4.51)	8.91 (4.50)

Our initial analysis used the rank-sum test treating informed and uninformed groups in the same session as unpaired. In reality, sessions 1-4 are unpaired, but sessions 5-10 involve linked comparisons of untreated and treated subjects. As a robustness check, we apply the technique of Derrick et al. (2020) to test paired and unpaired observations jointly. These updated results show little difference between what is found in Table 5 (the corresponding p-values fall between 0.49-0.70).

Table 6: Comparison of information treated and untreated subjects, Periods 51-100. Data are scaled to per 18 subjects for values that allow easy comparison with Selten et al. (see Table 2).

Treatment	Session	Players on Side Road per 18 subjects	Route Changes per 18 subjects	Payoffs per Period
			(Standard Deviation)	
No Information	1	5.98 (1.49)	3.64 (1.85)	9.39 (3.49)
	2	6.12 (1.99)	6.70 (2.32)	8.96 (4.74)
	5 (14 subjects)	5.66 (2.28)	4.83 (2.24)	8.93 (4.52)
	6 (14 subjects)	5.53 (2.66)	5.35 (2.43)	8.34 (5.52)
	7 (14 subjects)	5.01 (2.07)	4.37 (2.28)	9.04 (4.39)
	8 (14 subjects)	5.84 (2.43)	6.35 (3.19)	8.77 (4.35)
	9 (14 subjects)	5.86 (1.86)	3.99 (2.00)	9.23 (3.39)
	10 (14 subjects)	6.38 (2.08)	4.83 (2.09)	8.97 (4.59)
	Overall	5.80 (2.13)	5.01 (2.33)	8.96 (4.37)
All Information	3	5.86 (1.96)	5.16 (2.20)	8.90 (4.51)
	4	6.24 (1.84)	6.50 (2.61)	9.15 (4.34)
	5 (4 subjects)	6.66 (5.08)	7.65 (4.57)	8.71 (4.60)
	6 (4 subjects)	6.84 (4.09)	7.38 (3.61)	8.72 (5.86)
	7 (4 subjects)	9.18 (4.89)	7.74 (4.64)	8.53 (4.70)
	8 (4 subjects)	5.67 (3.38)	2.97 (3.47)	9.55 (4.34)
	9 (4 subjects)	5.76 (2.23)	1.44 (2.48)	9.70 (3.48)
	10 (4 subjects)	3.51 (3.78)	3.51 (4.10)	8.68 (4.17)
	Overall	6.22 (3.61)	5.29 (3.57)	8.99 (4.50)
Rank Sum Comparison		z=-0.945 p=0.345	z=-0.420 p=0.674	z=0.420 p=0.674

We see a clear interpretation of our results compared to Selten et al.'s. While much of our experimental design was similar to theirs, our design featured one key difference: we let all subjects play without information for the first 50 periods. Such a start to the experiment may cause subjects to get stuck playing a strategy even after variables change. As this tendency has precedent in the literature (e.g., Romero 2015), we find little evidence of a treatment effect, and our results significantly differ from Selten's; we conclude that this key difference—something we are concerned occurs in empirical field traffic scenarios—is likely behind our main findings in the group sessions of Study 1.

V. STUDY 2: THE EFFECT OF INFORMATION AT THE INDIVIDUAL LEVEL

Our initial study, when compared to Selten et al., suggests that providing information on payoffs for actions not taken has a differential effect depending on when the information is provided. At the aggregate level, it appears that providing the information at the beginning of the experiment reduces switching and increases payoffs for all subjects (Selten's study). There are no comparable effects when information is provided halfway through an experiment (our Study 1). Surprisingly, this conclusion does not seem to depend on whether the information is given to all or some subjects.

To investigate these differences, we observe the individual behavioral response to information when the information is provided 1) at the beginning of a session for 100 periods, 2) halfway through a session for 50 periods, or 3) not at all. We randomize whether subjects receive each of these information treatments and have them play against past subjects from Sessions 1 and 2 of our experiment. Importantly, these are decisions (i.e., not games); no choice made by any subject in Study 2 affects another subject in Study 2. The information provided is private; none of the past players in Sessions 1 and 2 received this information. We utilize 3 regression frameworks to study these effects:

(1) $$y_{ij} = \alpha + \beta_1 \text{info}_{ij} + \beta_2 \text{2ndhalf}_i + \varepsilon_{ij}$$

(2) $$y_{ij} = \alpha + \beta_1 \text{info}_{ij} + \beta_2 \text{2ndhalf}_i + \beta_3 \text{info}_{ij} \text{ x } \text{2ndhalf}_i + \varepsilon_{ij}$$

(3) $$y_{ij} = \alpha + \beta_1 \text{info}_{ij} + \beta_2 \text{2ndhalf}_i + \beta_3 \text{info}_{ij} \text{ x } \text{2ndhalf}_i + \beta_4 \text{fullinfo}_j \text{ x } \text{2ndhalf}_i + \varepsilon_{ij}$$

Here y_{ij} represents the relevant dependent variable (e.g., profit per period, switch rate per period, side road choice per period) for subject *j* in half *i*. The other variables are indicators, 2ndhalf_i takes on a value of 1 (0 otherwise) if half *i* is in the second half of the experiment; info_{ij} takes on a value of 1 if subject *j* is treated with information (0 otherwise) in half *i*; fullinfo_j takes on a value of 1 if subject *j* is assigned to the full information (i.e., all 100 periods of information) treatment. The error term ε_{ij} represents an idiosyncratic error clustered at the subject level.

Regression (1) shows the overall effect of information treatment on subjects through the term β_1. Regression (2) shows the effect of information on the first half of the experiment, β_1, and the difference in this effect in the second half, β_2, treating the effect as equal for fully and partially treated subjects. Regression (3) also shows the effect of information on the first half of the experiment, β_3, but separately identifies the change in the second half on the partially treated (who have just received information in Period 51), β_4, from the fully treated, $\beta_3+\beta_4$, (who have been receiving information since Period 1).[4]

Table 7 provides the results of the three regression structures of profit per period on information treatment over each half of the experiment. The overall coefficient of information, β_1, is positive but not significantly different from 0 (Regression (1)). Subjects provided with information in the first half of the experiment outperform those who did not receive that information by 0.221 ECUs/period in the first half (Regression (2), $p<0.10$), but there is no overall benefit to information in the second half as treated subjects underperform by 0.032 ECUs/period (0.221+-0.252, $p\approx0.732$). Regression (3) shows that subjects treated with information after period 50 underperform the no information baseline by 0.174 ECUs/period in the second half of the experiment (0.221+-0.394, $p\approx0.109$), and subjects that have already been treated with information for 50 periods outperform the baseline by 0.133 ECUs/period (0.221+-0.394+0.307, $p\approx0.228$). While neither of these results is conventionally significant, the difference between them is. The fully treated group outperforms the partially treated group by 0.307 ECUs/period in the second

[4] Our appendix provides all regression results with subjects from the Dallas population removed. Recall, this subject population only went through no information and partial information, never encountering the full information treatment.

half of the experiment ($p<0.01$). Appendix table A2 suggests we should treat these last results carefully; if we remove the Dallas subject population (who are not included in the full information treatment group), we find essentially no differences between treatments in the second half of the experiment. However, the positive effect of information over the first half of the experiment is largely unchanged.

Table 7: Regressions of profit per period (in ECUs) on information treatment

variables	(1) Profit/ period	(2) Profit/ period	(3) Profit/ period
information treated (β_1)	0.087	0.221*	0.221*
	(0.079)	(0.122)	(0.122)
2nd half (β_2)	0.155**	0.270***	0.270***
	(0.070)	(0.084)	(0.084)
information treated x 2nd half (β_3)		-0.252*	-0.394**
		(0.146)	(0.162)
full treatment x 2nd half (β_4)			0.307***
			(0.117)
constant	8.420***	8.381***	8.381***
	(0.050)	(0.053)	(0.053)
observations	634	634	634
subject clusters	317	317	317
R^2	0.014	0.019	0.029

$^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$

Table 8 provides the results of the three regression structures of the rate of switching roads per period on information treatment over each half. The effects are more consistent across the regression structures, indicating a more uniform effect of receiving information on subjects. In general, a subject treated with information is 14 percentage points more likely to switch from their past period's road choice than a subject who wasn't receiving information ($p<0.01$, all three regressions). All subjects appear to reduce their switch rate by roughly 3 percentage points over the second half of the experiment regardless of treatment ($p<0.01$, all three regressions). Results from only student subjects are largely similar: information-treated subjects increase switch rates by 8-9 percentage points, but there is no 2nd half effect (see Table A3).

Table 8: Regressions of switch rate per period on information treatment

variables	(1) Switch-rate/ period	(2) Switch-rate/ period	(3) Switch-rate/ period
information treated (β_1)	0.136***	0.143***	0.143***
	(0.018)	(0.019)	(0.019)
2nd half (β_2)	-0.033***	-0.027*	-0.027*
	(0.010)	(0.016)	(0.016)
information treated x 2nd half (β_3)		-0.013	-0.022
		(0.023)	(0.030)
full treatment x 2nd half (β_4)			0.019
			(0.026)
constant	0.282***	0.280***	0.280***
	(0.011)	(0.011)	(0.011)
observations	634	634	634
subject clusters	317	317	317
R^2	0.115	0.116	0.116

***$p<0.01$, **$p<0.05$, *$p<0.1$

Table 9 provides the results of the three regression structures of side road choice per period on information treatment over each half of the experiment. We see a more general effect again; a subject treated with information is roughly 10 percentage points more likely to choose the side road over the main road compared to a subject not treated with information. In the second half, all subjects appear to reduce their choice of side road by five percentage points, but this effect is mitigated by the fully treated subjects who don’t appear to change their propensity to choose the side road. Interestingly, these effects do not appear significant in the student data only, which suggests the overall side road effect may only be found in the Dallas driver population (Table A4), and the fully treated effect may be simply a result of comparing different populations rather than something due to full treatment of information.

Of our three main results, only one appears robust. Providing additional information on the road-not-chosen increases the rate at which subjects will switch to that road. It is less clear whether information treatment affects the choice of side road or profit. Information provided initially rather than after several periods of experience appears to have a greater effect on profit. This result might explain some of the differences in the effects of information treatment between the original Selten et al. experiments and our Study 1 results.

Table 9: Regressions of the rate of side road choice on information treatment

variables	(1) Side road rate	(2) Side road rate	(3) Side road rate
information treated (β_1)	0.107***	0.092***	0.092***
	(0.018)	(0.020)	(0.020)
2nd half (β_2)	-0.038***	-0.051***	-0.051***
	(0.010)	(0.018)	(0.018)
information treated x 2nd half (β_3)		0.028	0.007
		(0.027)	(0.035)
full treatment x 2nd half (β_4)			0.047*
			(0.028)
constant	0.311***	0.316***	0.316***
	(0.014)	(0.015)	(0.015)
observations	634	634	634
subject clusters	317	317	317
R^2	0.057	0.058	0.061

***p<0.01, **p<0.05, *p<0.1

It is difficult to pick a winner in regard to our hypotheses. Hypothesis 3A is easiest to reject as subjects clearly are increasing their switching of roads in response to information. Null Hypothesis 3 is mainly correct on earnings but incorrect on switch rates. Hypothesis 3B is correct on switch rates but not on earnings. Because it is informed by the results of Study 1, Hypothesis 3C is most accurate as earnings are differentially higher for fully treated subjects, at least over the first half of the experiment.

Because increased rates of switching do not necessarily translate to higher payoffs (e.g., in a mixed-strategy equilibrium it makes no difference), one can understand how information treatment can always increase rates of switching but only sometimes have an effect on payoffs. We note that per-period rates of switching are negatively correlated with per-period average earnings for subjects in the experiment (-0.127 overall; -0.162 not treated; -0.140 partially treated; -0.163 fully treated; $p<0.05$ for all values). The correlations are not remarkably different for treated and untreated subjects. Thus, if one still wonders why the information treatment increases switch rates without a corresponding increase in earnings, it may be as simple as overall increases in switching generally do not increase earnings; if anything, they may reduce them. In the next section, we go further into the classification of road switching and the game theoretic concepts that underlie them.

A. Game Theoretic Classifications

As shown in the previous section, there is a tenuous relationship between information prompting a subject to action and that action generating higher payoffs. This principle is inherent in many coordination games. For instance, knowing that Road A had a higher payoff than Road B in the last period does not automatically mean that Road A will have a higher payoff next period. For one, if all subjects are aware of this information, they may all respond by choosing Road A. Alternatively, if they believe most other subjects will follow the preceding reasoning, it would make sense to choose Road B. Selten (2004, 2007) classified these two types of responses as "direct" and "indirect," respectively. Direct responders choose the road with the higher payoff last period; indirect responders do the reverse. While it is natural and instinctive to stick with a strategy that proved recently fruitful, our results suggest that subjects that leaned towards a direct response tend to earn relatively less compared to indirect responders. We formalize direct and indirect response in Table 10.

There is a strong theoretical basis for classifying these responses into two types. Under a many steps of thinking model, such as level-k (Nagel 1995; Crawford 2013), one could set the base level of thinking as a direct response or level-1 thinking. A level-1 responder reasons that it is only natural to stick with a road if it yielded above-equilibrium payoffs and switch roads if it yielded below-equilibrium payoffs. Then, after iterating through this step and assuming other players are level-1 responders, a level-2 responder decides to stick with the same road choice if it yielded below-equilibrium payoffs and switch roads if the last chosen road yielded above-equilibrium payoffs. Iterating through this thinking again, a level-3 responder would respond directly to payoffs by choosing the faster road from the last period. Following a convention of making level-1-thinkers direct responders, all odd levels in level-k would play the strategy consistent with direct response, and all even levels would play the strategy consistent with indirect response.

Table 10: Classification of direct and indirect response.

Event	Direct Responder Action	Indirect Responder Action
Road payoff <10	Switch roads next period	Maintain road next period
Road payoff >10	Maintain road next period	Switch road next period
Road payoff=10	No prediction	No prediction

Note: Unless treated with information, subjects only knew the payoff on their road each period, though it was known that the other payoff was negatively correlated. In equilibrium, the payoff on both roads is 10.

Tables 11a and 11b show the mean payoffs per round for a strategy of direct and indirect response in each of the ten Study 1 coordination-game sessions. In the first 50 periods, the strategy of indirect response outperforms direct response in all 10 sessions by an average 2.28 ECUs. That differential is reduced to 1.19 ECUs in the second 50 periods and, interestingly, does not exist in Session 1, where direct and indirect responses have identical mean earnings as strategies. This does not mean payoffs are equivalent, however. Appendix tables A5a and A5b show mean earnings if subjects always made the payoff-maximizing road choice vs. payoff-minimizing road choice, respectively. Generally, the payoff differential for picking the high-payoff road ex-post is 7.5 ECUs. The ability to have a better than a random sense of which road will be better next round would be profitable to subjects.

Table 11a: Average and standard deviation of personal per period payoff for direct responders.

Treatment	**Session**	**Periods 1-50**	**Periods 51-100**
Control	1	8.64 (6.40)	10.06 (4.07)
	2	8.94 (5.88)	9.24 (5.45)
All	3	8.26 (5.14)	9.52 (5.24)
	4	9.68 (5.04)	9.58 (4.59)
Frequent-4	5	8.62 (5.40)	9.26 (5.00)
	6	8.60 (5.05)	9.64 (6.45)
	7	8.88 (5.60)	10.08 (5.14)
Infrequent-4	8	8.94 (4.78)	9.30 (5.29)
	9	9.14 (4.82)	9.38 (3.66)
	10	9.30 (4.97)	8.88 (5.12)

Note: The theoretical equilibrium value is 10.

Table 11b: Average and standard deviation of personal per period payoff for indirect responders.

Treatment	**Session**	**Periods 1-50**	**Periods 51-100**
Control	1	11.84 (5.57)	10.06 (3.50)
	2	11.34 (5.12)	10.94 (4.59)
All	3	11.66 (5.05)	10.72 (4.69)
	4	10.48 (4.89)	10.38 (4.87)
Frequent-4	5	11.42 (5.33)	11.26 (4.83)
	6	11.20 (5.12)	10.34 (5.50)
	7	10.98 (5.54)	10.48 (5.00)
Infrequent-4	8	11.04 (4.97)	10.80 (4.24)
	9	11.04 (4.90)	10.58 (3.85)
	10	10.80 (4.59)	11.28 (4.36)

Note: The theoretical equilibrium value is 10.

Since subjects do not perfectly play direct or indirect responses, we use an index for their propensity to respond directly or indirectly. Following a logistic specification, we define λ as the propensity to respond. As values become more positive, they indicate a greater tendency to choose the road with higher payoffs in the previous period (direct response). As values become more negative, they show the reverse propensity (indirect response). A value of 0 suggests a subject's decisions cannot be predicted using the last payoffs on either road (unidentified). For each subject, λ_i is calculated as a maximum likelihood estimator to the problem

(4) $\lambda_i = \prod_{t=1}^{T} p_{it} y_{it} + (1 - p_{it})(1 - y_{it})$ where $p_{it} = 1/\left(\exp\left(-\lambda_i (M_{it-1} - S_{it-1})\right) + 1\right)$

where y_{it} takes a value of 1 if the main road was chosen or 0 if the side road was chosen by subject i in period t. Here λ_i represents the logit coefficient that determines a subject's propensity to be a direct or indirect responder. M_{it-1} and S_{it-1} denote the payoffs on the main and side road in the last period, respectively. Values of λ_i are determined by subject decisions in periods 2-50, 51-100, and all periods. We bounded this value from above and below at 1 and -1 to classify perfect direct and indirect responses, respectively. Otherwise, the resulting numbers would be undefined at positive and negative infinity. (Note that no subject other than perfect responders have λ values above or below 1 and -1.)

Table 12 applies regression structures (1)-(3) to individual-subject λ values for each half of the experiment. The overall effect of information is slight; subjects increase their propensity to direct respond by 0.03 percentage points ($p<0.10$). The effect appears stronger over the second half of the experiment, an increase of 0.04 percentage points (equation (2): 0.017+0.024=0.041, $p<0.05$). Interestingly, this effect is not significant on fully treated subjects over the first half of the experiment. Specification (3) shows us that the effect is entirely driven by the partially treated group who increase their propensity to respond by almost 0.09 percentage points upon receiving information in the second half of the experiment (0.017+0.068=0.086, $p<0.01$), subjects in the fully treated group do not differ from the baseline in response mode over the second half of the experiment (0.017+0.068+-0.097=-0.01, $p\approx 0.652$). An examination of Table A6 shows that these differences between the information treatments are not due to different subject populations; if anything, the effects look more robust when we remove the Dallas population from the analysis.

Overall, providing information at the beginning of the experiment does not alter the response mode in a clear direction. However, providing that information halfway through the experiment unambiguously shifts subjects towards a direct response mode.

Table 12: Regressions of response mode (λ) classification on information treatment

variables	(1) Response mode (λ)	(2) Response mode (λ)	(3) Response mode (λ)
information treated (β_1)	0.030*	0.017	0.017
	(0.017)	(0.020)	(0.020)
2nd half (β_2)	0.012	0.001	0.001
	(0.009)	(0.007)	(0.007)
information treated x 2nd half (β_3)		0.024	0.068**
		(0.020)	(0.032)
full treatment x 2nd half (β_4)			-0.097***
			(0.033)
constant	0.011***	0.014***	0.014***
	(0.004)	(0.003)	(0.003)
observations	634	634	634
subject clusters	317	317	317
R^2	0.012	0.013	0.042

***p<0.01, **p<0.05, *p<0.1

The timing of the delivery of information affects whether subjects tend toward direct response mode. Of course, the direct response involves two distinct tendencies: the propensity to change roads when one's payoff is lower and the tendency to stay on one's road when one's payoff is higher. We refer to the first tendency as the propensity to "chase" payoffs. To break down how information treatment affects strategy, we look at subjects' rates of chasing payoffs in these experiments over each half of the experiment by treatment group. Table 13a shows regression results of the rates of switching roads when the payoff on the road not taken was higher in the previous period. Information treatment has a clear overall effect: subjects switch to the higher paying road 10 percentage points more often than compared to the baseline (regression (1), $p<0.01$). There is a difference in effect between the partially treated and fully treated in the second half of the experiment; the partially treated are 16 percentage points more likely to switch relative to the baseline (regression (3): 0.082+0.078, p<0.01) while the fully treated are only 9 percentage points more (regression (3): 0.082+0.078+-0.71, p<0.05), the difference between the two is marginally significant (p<0.10). If anything, these effects are more robust when looking only at the student population (see Table A7a).

Table 13a: Regressions of the rate of switching to the previous period's higher payoff road (when feasible) on information treatment.

variables	(1) positive chase rate	(2) positive chase rate	(3) positive chase rate
information treated (β_1)	0.106***	0.082***	0.082***
	(0.024)	(0.027)	(0.027)
2nd half (β_2)	-0.001	-0.022	-0.022
	(0.013)	(0.019)	(0.019)
information treated x 2nd half (β_3)		0.045	0.078*
		(0.030)	(0.042)
full treatment x 2nd half (β_4)			-0.071*
			(0.038)
constant	0.335***	0.342***	0.342***
	(0.014)	(0.014)	(0.014)
observations	634	634	634
subject clusters	317	317	317
R^2	0.048	0.050	0.057

***p<0.01, **p<0.05, *p<0.1

Interestingly, the information treatment did not only increase subjects' propensity to switch roads to chase higher payoffs. It also appears to have increased subjects' propensity to switch roads when payoffs in the other road last period were lower or equal. Tables 13b and c reveal that treated subjects increased their net difference of switching roads to the lower payoff road by 17 percentage points and increase their net difference of switching roads to an equal payoff road by 15 percentage points (both p<0.01, regression equation (1)). For chasing lower payoffs, the two information treatments produce differing effects. Over the second half of the experiment, subjects that have been fully treated with information since Period 1 increase their rates of switching to lower payoff roads by 21 percentage points (Table 13b, equation 3: 0.227+-0.182+0.169=0.21, $p<0.01$). In contrast, partially treated subjects do not show a significant increase (Table 13b, equation 3: 0.227+-0.182=0.4, $p\approx 0.156$). The 17-percentage point difference between the two effects is also significant ($p<0.05$). Table 13c shows regression results of subjects' propensity to switch roads when the payoffs between roads are equal in the past period. There is a clear effect of information treatment, a 15-percentage point increase in switching overall (regression structure (1), $p<0.01$). While there may be slight differences between the fully treated and partially treated groups, they are not of the same magnitude as the previous cases. Taken together, we seem to be following the same story of confirmatory evidence. Information treatment increases road switching overall, but only full treatment increases switching to roads that were lower paying in the past period. Thus, partial treatment increases direct response.

Table 13b: Rate of switching to the previous period's lower payoff road (when feasible) by information treatment.

variables	(1) negative chase rate	(2) negative chase rate	(3) negative chase rate
information treated (β_1)	0.173***	0.227***	0.227***
	(0.025)	(0.028)	(0.028)
2nd half (β_2)	-0.064***	-0.017	-0.017
	(0.015)	(0.020)	(0.020)
information treated x 2nd half (β_3)		-0.104***	-0.182***
		(0.032)	(0.040)
full treatment x 2nd half (β_4)			0.169***
			(0.038)
constant	0.186***	0.170***	0.170***
	(0.013)	(0.013)	(0.013)
observations	634	634	634
subject clusters	317	317	317
R^2	0.102	0.111	0.146

***p<0.01, **p<0.05, *p<0.1

Table 13c: Rate of switching when previous period road payoffs are equal by information treatment.

variables	(1) neutral chase rate	(2) neutral chase rate	(3) neutral chase rate
information treated (β_1)	0.146***	0.176***	0.176***
	(0.024)	(0.032)	(0.032)
2nd half (β_2)	-0.079***	-0.054**	-0.054**
	(0.016)	(0.023)	(0.023)
information treated x 2nd half (β_3)		-0.057	-0.076*
		(0.037)	(0.045)
full treatment x 2nd half (β_4)			0.042
			(0.033)
constant	0.258***	0.249***	0.249***
	(0.016)	(0.016)	(0.016)
observations	634	634	634
subject clusters	317	317	317
R^2	0.076	0.078	0.080

***p<0.01, **p<0.05, *p<0.1

Our final analysis will examine the profitability of switching roads in general. For each subject, we examine the difference in average payoffs for periods where a subject remained on the same road and those where a subject changed roads. In general, road switching is not profitable. For an untreated subject, the payoffs for a period where they switched roads is 1.28 points (1.22 in the second half) less profitable than a period where they stayed on the same road (regression (1), p<0.01). For treated subjects overall, this value diminishes by 0.8 points (regression (1), p<0.01) but is still positive (0.475, 0.414 (second half), p<0.01), meaning information-treated subjects are still doing worse in periods where they switch roads. There is no significant difference in this measure in terms of the fully treated or partially treated in the second half of the experiment, even when excluding the Dallas subjects (see Table A8).

To summarize our results, randomly treating subjects with payoff information on the road choice not taken in the previous period varies depending on whether this intervention occurs at the start of the experiment (i.e., fully treated) or halfway through the experiment (i.e., partially treated). Much like in the comparison between our Study 1 game and Selten et al.'s, the strongest results appear to occur when subjects are a blank slate rather than already having a history of playing the game. Specifically, we find information treatment (1) increases payoff performance but only for fully treated subjects in the first half of the experiment, (2) unconditionally increases the rate of switching roads, (3) increases direct response rates only for partially treated subjects (4) increases the effectiveness of switches (i.e., higher payoffs when switching), but not enough to make switching profitable. Because switching is generally not profitable, increasing both the propensity of switching and the effectiveness of switching produces opposite effects on total payoffs, resulting in a more ambiguous payoff change for treated subjects, especially the partially treated.

Table 14: Regressions of average payoff difference for subjects on periods they stayed in the same road vs. periods they switched roads on information treatment.

variables	(1) payoff difference	(2) payoff difference	(3) payoff difference
information treated (β_1)	-0.804***	-0.767***	-0.767***
	(0.184)	(0.260)	(0.260)
2nd half (β_2)	-0.061	-0.025	-0.025
	(0.176)	(0.284)	(0.284)
information treated x 2nd half (β_3)		-0.074	0.090
		(0.358)	(0.382)
full treatment x 2nd half (β_4)			-0.356
			(0.244)
constant	1.280***	1.268***	1.268***
	(0.175)	(0.198)	(0.198)
observations	593	593	593
subject clusters	297	297	297
R^2	0.033	0.033	0.035

***p<0.01, **p<0.05, *p<0.1

VI. DISCUSSION: ARE THERE SUBJECTS WHO BENEFIT FROM INFORMATION?

An initial motivation for this experiment concerned the fact that there may be several types of individuals that approach the problem of route selection differently. Indeed, many of the surveys administered to subjects described in Section IIA were motivated by the idea that these instruments may be able to separate subjects into two types – those that chose routes based on available data and those that generally acted more passively, ignoring available information. This investigation is the topic of another paper (Ashraf, Burris, Brown and Vitaku, 2021).

Nonetheless, along these same lines, we may ask a similar question. Given that the treatment of information had somewhat ambiguous benefits to subjects on average, are there certain subjects that one could select ex-ante who are more prone to use the information in ways that would lead to payoff increases? One would hope these subjects are able to use the informative value of the treatment to make better road changes (as information-treated subjects do on average) without increasing the frequency of non-profitable road changes (as information-treated subjects do on average). To this end, we restructure regression equation (1) to consider a model to identify patterns in subject personality that suggest they are more likely to benefit rather than be hurt by our information treatment.

(5) $$\text{Profit}_{ij} = \beta X_j + \beta_1 \text{info}_{ij} \text{ x } Xj + \beta_2 \text{2ndhalf}_i \text{ x } X_j + \varepsilon_{ij}$$

where Profit_{ij} is a continuous variable representing subject *j*'s average profit in half *i*. The matrix X_j contains the personal characteristics of subject *j*, specifically general mental ability, conscientiousness, need for cognitive closure, cognitive reflection, satisficing score, risk tolerance, gender, ethnicity, and income level.[5] The other variables are indicators; 2ndhalf_i takes on a value

[5] Income level is a categorical variable from survey responses coded 1-9 with values 1=less than $10,000, 2=$10,000-$15,000, 3=$15,000-24,999, 4=$25,000-$34,999, 5=$35,000-$49,999, 6=$50,000-$74,999, 7=75,000-$99,999, 8=$100,000-$199,999, 9=$200,000 or more. Survey questions are available as supplementary materials.

of 1 (0 otherwise) if half i is in the second half of the experiment; $info_{ij}$ takes on a value of 1 if subject *j* is treated with information (0 otherwise) in half *i*. The error term ε_{ij} represents an idiosyncratic error clustered at the subject level.

Appendix Table 10 presents the results of two regressions. Regression (2) differs from (1) in that it also features the stand-alone inclusion of treatment without interaction with explanatory variables in X_j. While our evidence of an overall treatment effect on earnings is mixed (see Table 7), we provide both specifications for robustness. An F-test rejects the null hypothesis that the treatment interaction variables are jointly zero in both regressions ($p \approx 0.007$).

One variable emerges as the clear winner in explaining the effects of treatment on payoffs. For every additional cognitive reflection task question answered correctly, a subject earns 0.129 more per period ($p<0.01$, both regressions). The standard deviation for the subject population on this test is roughly 2 questions correct, so a full standard deviation shift on this variable is associated with a 0.25-point gain or loss per period.

The sign of this relation makes sense. The cognitive reflection task is about a subject's ability to refrain from doing the obvious, impulsive answer and pick the more deliberative and ultimately correct answer. A direct application to this experiment would be that the obvious choice is to switch roads to the other road when the other road is revealed to have had a higher payoff last period. We know such approach is not profitable. The deliberative alternative would likely involve less switching, perhaps only when the other road generated lower payoffs last period (the indirect response). Indeed, similar regressions of the dependent variable of each half's λ (recall equation (4) and Table 12) show each half's calculated λ values, the number of CRT questions correct is negatively correlated with overall λ values, and the interaction of CRT questions with treatment is also negatively correlated (results available in supplemental materials).

There are other factors we cannot explain. The interaction of income level and treatment is associated with a negative effect on payoffs, roughly 0.07 per period. We also ex-ante had several explanations on how measures like general mental ability, conscientiousness, need for cognitive closure, and risk aversion might explain behavior. For the most part, we do not observe explanatory power of any of these on treatment effects. Though, the need for cognitive closure is associated with lower payoffs overall.

VII. CONCLUSIONS

Both field (Burris and Ashraf, 2019; Burris and Brady, 2018) and experimental analysis (Selten et al. 2004, 2007) of coordination behavior in traffic studies reveal patterns of behavior that are not consistent with agents responding to changes in parameters in ways that will lead to systems equilibrating. Both suggest that informational interventions may increase payoffs for drivers and system equilibration. However, they differ on whether this payoff increase will occur by making drivers change routes more or less often.

We build upon Selten et al.'s design to examine whether these informational interventions might lead to (i) better equilibration at the aggregate level and (ii) better differential payoffs for subjects that receive information when subjects already have a history of past play without this specific information. Specifically, we examine whether revealing payoff information on roads subjects did not take in the previous period will lead to better performance on aggregate or individually.

Our results on both items are decidedly negative. Providing information does not have a pronounced effect on group behavior in situations where some or all group members receive that information. As a follow-up study, we focus on individual decisions where everything else is constant, and opponents' behavior is known to be fixed. When subjects already have a history of past play, we find that providing this information does not increase subjects' payoff relative to not having received that information. Only when subjects receive information at the start of their information process—and only for the first half of the experiment—do we observe a differential effect on payoffs from information treatment.

Informational treatment still affects subjects, albeit in different ways. Our analysis carefully characterizes how the additional information tends to prod subjects to switch roads more often. The effect is not specific to instances where the newly switched-to road has higher past-period payoffs, though that is the most common. In general, subjects switch roads at times that are less profitable, and being prodded to switch roads more often appears to depress payoffs. However, subjects treated with information lower their payoffs less from switching roads than untreated subjects. Thus, the overall effect on payoffs is mixed.

With regard to game-theoretic strategies employed, the results are quite interesting. Partial treatment of information (i.e., after 50 periods of play) unambiguously increases subjects' direct response rate. There is no similar strategy alteration for subjects that receive information from the beginning of the experiment (i.e., full treatment). As the direct response is generally less profitable than indirect response, we can explain the slight payoff differences between the two treatments with this characterization.

Of course, the speed of equilibration may vary from our laboratory experiments to different field environments. This variance may alter the payoffs from switching routes; in some cases, switching and direct response may be profitable on average. Our analysis suggests these environments—perhaps where new traffic patterns just starting to emerge and not yet fully equilibrated—are likely the best environments to implement such an informational intervention. Nonetheless, we note that individuals who score higher on the CRT tend to benefit more from additional information. Given the nature of cognitive reflection, these results make sense. Direct and indirect response modes intuitively fit with impulsive and deliberative thinking, respectively. We note this relation across two different subject populations. Future studies will need to confirm their validity in more field environments. Another avenue of promising research would be the content of information in messages.[6] There may be some promise in selectively targeting these groups with information. As a general result, we note that providing travelers with more information on their choices – even post-trip – may encourage them to consider alternative options more often.

[6] The framing of the content of information may also generate better responses. We do note promising preliminary work suggests prospective messages based on future time savings appear more likely to bring about change than retrospective ones based on past time savings. Further, reporting travel speeds rather than travel times appears to generate a greater subject response (Burris et al., forthcoming).

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APPENDIX A: ADDITIONAL TABLES AND FIGURES

Table A1a: Summary table of choice variables by population (over all periods)

Variable	Both populations (All)		Students		DFW residents	
	N	mean (standard deviation)	N	mean (standard deviation)	N	mean (standard deviation)
profit per period	317	8.538 (0.643)	184	8.643 (0.615)	133	8.392 (0.656)
switch-rate per period	317	0.329 (0.172)	184	0.377 (0.157)	133	0.263 (0.170)
side-road per period	317	0.342 (0.198)	184	0.392 (0.159)	133	0.273 (0.225)
response rate (λ)	317	0.018 (0.104)	184	0.024 (0.132)	133	0.011 (0.044)
switch-rate (higher payoffs)	317	0.384 (0.215)	184	0.434 (0.212)	133	0.315 (0.199)
switch-rate (lower payoffs)	317	0.237 (0.228)	184	0.297 (0.239)	133	0.155 (0.183)
switch-rate (equal payoffs)	317	0.284 (0.221)	184	0.324 (0.215)	133	0.228 (0.220)
no-switch payoff differential	308	0.835 (1.756)	184	0.450 (1.379)	124	1.406 (2.078)

Table A1b: Summary table of choice variables by treatment (over all periods)

Variable	Both populations (All)		No information		Partial Information		Full Information	
	N	mean (sd)	N	mean (sd)	N	mean (sd)	N	mean (sd)
profit per period	317	8.538 (0.643)	116	8.578 (0.574)	108	8.362 (0.605)	93	8.692 (0.723)
switch-rate per period	317	0.329 (0.172)	116	0.266 (0.180)	108	0.329 (0.152)	93	0.409 (0.150)
side-road per period	317	0.342 (0.198)	116	0.282 (0.207)	108	0.349 (0.213)	93	0.409 (0.140)
response rate (λ)	317	0.018 (0.104)	116	0.009 (0.043)	108	0.026 (0.050)	93	0.021 (0.179)
switch-rate (higher)	317	0.384 (0.215)	116	0.327 (0.209)	108	0.415 (0.197)	93	0.420 (0.228)
switch-rate (lower)	317	0.237 (0.228)	116	0.168 (0.203)	108	0.185 (0.177)	93	0.386 (0.242)
switch-rate (equal)	317	0.284 (0.221)	116	0.220 (0.221)	108	0.276 (0.208)	93	0.371 (0.211)
no-switch differential	308	0.835 (1.756)	111	1.239 (2.176)	104	0.808 (1.620)	93	0.383 (1.134)

Table A2: Regression of profit per period (in ECUs) on information treatment (student subjects only)

variables	(1) Average profit/	(2) Average profit	(3) Average profit
information treated (β_1)	0.126	0.302^{**}	0.302^{**}
	(0.094)	(0.141)	(0.142)
2nd half (β_2)	0.353^{***}	0.607^{***}	0.607^{***}
	(0.090)	(0.136)	(0.136)
information treated x 2nd half (β_3)		-0.401^{**}	-0.347^{*}
		(0.179)	(0.186)
full treatment x 2nd half (β_4)			-0.079
			(0.107)
constant	8.388^{***}	8.299^{***}	8.299^{***}
	(0.075)	(0.089)	(0.089)
observations	368	368	368
subject clusters	184	184	184
R^2	0.054	0.066	0.066

$^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$

Table A3: Regression of switch rate per period on information treatment (student subjects only)

variables	(1) Switch-rate/ period	(2) Switch-rate/ period	(3) Switch-rate/ period
information treated (β_1)	0.079^{***}	0.092^{***}	0.092^{***}
	(0.026)	(0.023)	(0.023)
2nd half (β_2)	-0.020	-0.001	-0.001
	(0.012)	(0.026)	(0.026)
information treated x 2nd half (β_3)		-0.029	-0.031
		(0.029)	(0.036)
full treatment x 2nd half (β_4)			0.003
			(0.027)
constant	0.338^{***}	0.332^{***}	0.332^{***}
	(0.019)	(0.018)	(0.018)
observations	368	368	368
subject clusters	184	184	184
R^2	0.046	0.047	0.047

$^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$

Table A4: Regression of side road per period on information treatment (student subjects only)

variables	(1) Average profit	(2) Average profit	(3) Average profit
information treated (β_1)	0.031	0.022	0.022
	(0.025)	(0.026)	(0.026)
2nd half (β_2)	-0.018	-0.031	-0.031
	(0.012)	(0.027)	(0.027)
information treated x 2nd half (β_3)		0.021	-0.007
		(0.032)	(0.041)
full treatment x 2nd half (β_4)			0.041
			(0.028)
constant	0.381^{***}	0.386^{***}	0.386^{***}
	(0.021)	(0.022)	(0.022)
observations	368	368	368
subject clusters	184	184	184
R^2	0.008	0.009	0.013

$^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$

Table A5a: Average and standard deviation of the maximum per period payoff.

Treatment	Session	Periods 1-50	Periods 51-100
Control	1	14.84 (3.78)	12.86 (2.40)
	2	14.24 (3.32)	13.74 (2.97)
All	3	14.36 (2.93)	13.82 (3.23)
	4	13.98 (2.87)	13.68 (2.614)
Frequent-4	5	14.22 (3.59)	13.86 (3.40)
	6	13.90 (3.41)	14.64 (3.49)
	7	13.98 (3.66)	13.78 (3.34)
Infrequent-4	8	14.04 (2.81)	13.60 (3.33)
	9	13.84 (2.90)	13.18 (2.34)
	10	13.30 (3.23)	13.68 (3.22)

Note: The theoretical equilibrium value is 10.

Table A5b: Average and standard deviation of the minimum per period payoff.

Treatment	Session	Periods 1-50	Periods 51-100
Control	1	5.34 (4.24)	7.16 (2.57)
	2	5.74 (3.87)	6.14 (3.63)
All	3	5.56 (3.12)	6.32 (3.32)
	4	5.98 (2.99)	6.08 (2.87)
Frequent-4	5	5.82 (3.56)	6.26 (3.23)
	6	6.00 (3.46)	5.54 (4.24)
	7	5.88 (4.20)	6.28 (3.21)
Infrequent-4	8	5.84 (2.77)	6.60 (3.33)
	9	6.14 (3.23)	6.98 (2.02)
	10	6.50 (3.63)	6.58 (3.45)

Note: The theoretical equilibrium value is 10.

Table A6: Regression of response mode (λ) classification on information treatment (student subjects only)

variables	(1) Response mode (λ)	(2) Response mode (λ)	(3) Response mode (λ)
information treated (β_1)	0.023	0.020	0.020
	(0.021)	(0.020)	(0.020)
2nd half (β_2)	0.039***	0.034**	0.034**
	(0.015)	(0.014)	(0.014)
information treated x 2nd half (β_3)		0.008	0.160***
		(0.027)	(0.059)
full treatment x 2nd half (β_4)			-0.221***
			(0.058)
constant	0.010	0.012***	0.012***
	(0.007)	(0.004)	(0.004)
observations	368	368	368
subject clusters	184	184	184
R^2	0.014	0.015	0107

***p<0.01, **p<0.05, *p<0.1

Table A7a: Regression of rate of switching to the previous period's higher payoff road (when feasible) on information treatment (student subjects only).

variables	(1) positive chase rate	(2) positive chase rate	(3) positive chase rate
information treated (β_1)	0.028	0.023	0.023
	(0.033)	(0.032)	(0.032)
2nd half (β_2)	0.038**	0.032	0.032
	(0.017)	(0.030)	(0.030)
information treated x 2nd half (β_3)		0.010	0.135**
		(0.036)	(0.053)
full treatment x 2nd half (β_4)			-0.182***
			(0.046)
constant	0.399***	0.401***	0.401***
	(0.023)	(0.022)	(0.022)
observations	368	368	368
subject clusters	184	184	184
R^2	0.011	0.011	0.056

***p<0.01, **p<0.05, *p<0.1

Table A7b: Regression of switching rate to the previous period's lower payoff road (when feasible) by information treatment (student subjects only).

variables	(1) negative chase rate	(2) negative chase rate	(3) negative chase rate
information treated (β_1)	0.127***	0.167***	0.167***
	(0.034)	(0.033)	(0.033)
2nd half (β_2)	-0.069***	-0.012	-0.012
	(0.019)	(0.035)	(0.035)
information treated x 2nd half (β_3)		-0.090**	-0.251***
		(0.043)	(0.053)
full treatment x 2nd half (β_4)			0.233***
			(0.041)
constant	0.251***	0.231***	0.231***
	(0.023)	(0.022)	(0.022)
observations	368	368	368
subject clusters	184	184	184
R^2	0.057	0.064	0.124

***p<0.01, **p<0.05, *p<0.1

Table A7c: Regression of rate of switching when previous period road payoffs are equal by information treatment (student subjects only).

variables	(1) neutral chase rate	(2) neutral chase rate	(3) neutral chase rate
information treated (β_1)	0.105***	0.150***	0.150***
	(0.034)	(0.038)	(0.038)
2nd half (β_2)	-0.067***	-0.003	-0.003
	(0.019)	(0.033)	(0.033)
information treated x 2nd half (β_3)		-0.101**	-0.139**
		(0.043)	(0.054)
full treatment x 2nd half (β_4)			0.055
			(0.035)
constant	0.298***	0.275***	0.275***
	(0.026)	(0.026)	(0.026)
observations	368	368	368
subject clusters	184	184	184
R^2	0.048	0.057	0.061

***p<0.01, **p<0.05, *p<0.1

Table A8: Regressions of average payoff difference (in ECUs) for subjects on periods they stayed on the same road vs. periods they switched road on information treatment (student subjects only)

variables	(1) payoff difference	(2) payoff difference	(3) payoff difference
information treated (β_1)	-0.328	-0.617	-0.617
	(0.240)	(0.395)	(0.396)
2nd half (β_2)	-0.610***	-1.059***	-1.059***
	(0.200)	(0.406)	(0.406)
information treated x 2nd half (β_3)		0.694	0.541
		(0.458)	(0.463)
full treatment x 2nd half (β_4)			0.227
			(0.222)
constant	0.972***	1.118***	1.118***
	(0.284)	(0.357)	(0.357)
observations	357	357	357
subject clusters	179	179	179
R^2	0.031	0.036	0.037

***p<0.01, **p<0.05, *p<0.1

Table A9a: Summary table of demographic/survey variables by population (over all periods)

Variable	Both populations (All)		Students		DFW residents	
	N	mean (standard deviation)	N	mean (standard deviation)	N	mean (standard deviation)
General Mental Ability	317	37.691 (7.850)	184	39.359 (7.172)	133	35.383 (8.182)
Conscientious-ness	317	72.864 (11.695)	184	68.897 (11.157)	133	78.353 (10.128)
Need for Cog Closure	317	57.199 (10.748)	184	58.304 (10.442)	133	55.669 (11.014)
Cognitive Reflection	317	2.817 (2.085)	184	3.130 (2.089)	133	2.383 (2.007)
Maximization	317	55.300 (10.891)	184	59.103 (9.779)	133	50.038 (10.165)
Risk Tolerance	316	2.835 (1.471)	184	2.783 (1.436)	132	2.909 (1.521)
Gender (Male=1)	317	0.483 (0.500)	184	0.408 (0.493)	133	0.586 (0.494)
Ethnicity (White=1)	317	0.558 (0.497)	184	0.370 (0.484)	133	0.820 (0.386)
Income Level (1-9)	285	6.312 2.404	162	5.321 (2.594)	123	7.618 (1.245)

Table A9b: Summary table of demographic/survey variables by treatment (over all periods)

Variable	All treatments		No information		Partial Information		Full Information	
	N	mean (sd)	N	mean (sd)	N	mean (sd)	N	mean (sd)
Gn Mental Ability	317	37.691 (7.850)	116	36.310 (8.265)	108	37.185 (7.727)	93	40.000 (6.981)
Conscienti ousness	317	72.864 (11.695)	116	75.983 (10.887)	108	74.370 (11.642)	93	67.226 (10.815)
Need for Cog Close	317	57.199 (10.748)	116	57.491 (11.131)	108	55.648 (9.611)	93	58.634 (11.377)
Cognitive Reflection	317	2.817 (2.085)	116	2.431 (1.988)	108	2.917 (2.175)	93	3.183 (2.037)
Maxim-ization	317	55.300 (10.891)	116	53.500 (10.748)	108	53.824 (11.072)	93	59.258 (9.898)
Risk Tolerance	316	2.835 (1.471)	116	2.767 (1.506)	107	2.822 (1.413)	93	2.935 (1.502)
Gender (Male=1)	317	0.483 (0.500)	116	0.491 (0.502)	108	0.528 (0.502)	93	0.419 (0.496)
Ethnicity (White=1)	317	0.558 (0.497)	116	0.612 (0.489)	108	0.676 (0.470)	93	0.355 (0.481)
Income Level	285	6.312 (2.404)	103	6.689 (2.187)	99	6.949 (2.002)	83	5.084 (2.660)

Table A10: Regression of per-period profit (in ECUs) on survey/demographic variables interacted with treatment

	(1)	(2)
	Per-period Profit	Per-period Profit
Information Treated	-	0.459 (0.774)
Treated x		
General Mental Ability	0.011 (0.011)	0.009 (0.012)
Conscientiousness	-0.003 (0.007)	-0.006 (0.008)
Need for Cognitive Closure	-0.005 (0.007)	-0.006 (0.007)
Cognitive Reflection	0.129*** (0.043)	0.128*** (0.043)
Maximization	0.004 (0.007)	0.002 (0.008)
Risk Tolerance	-0.056 (0.053)	-0.058 (0.053)
Gender (Male=1)	0.100 (0.171)	0.092 (0.171)
Ethnicity (White=1)	-0.053 (0.184)	-0.060 (0.185)
Income Level	-0.067* (0.035)	-0.069* (0.035)
Dallas population	0.035 (0.246)	0.026 (0.245)
Second Half x		
General Mental Ability	-0.004 (0.010)	-0.004 (0.010)
Conscientiousness	-0.001 (0.007)	-0.001 (0.007)
Need for Cognitive Closure	0.005 (0.007)	0.005 (0.007)
Cognitive Reflection	-0.053 (0.040)	-0.054 (0.040)
Maximization	0.005 (0.007)	0.005 (0.007)
Risk Tolerance	0.058 (0.048)	0.057 (0.048)
Gender (Male=1)	-0.178 (0.150)	-0.178 (0.150)
Ethnicity (White=1)	0.258 (0.164)	0.257 (0.164)
Income Level	-0.005 (0.037)	-0.004 (0.037)
Dallas population	-0.390* (0.215)	-0.393* (0.215)
General Mental Ability	-0.009 (0.007)	-0.008 (0.007)
Conscientiousness	-0.001 (0.005)	0.001 (0.005)
Need for Cognitive Closure	-0.013** (0.005)	-0.012** (0.005)
Cognitive Reflection	-0.007 (0.030)	-0.005 (0.030)
Maximization	-0.001 (0.005)	-0.000 (0.006)
Risk Tolerance	-0.017 (0.039)	-0.016 (0.039)
Gender (Male=1)	0.032 (0.124)	0.035 (0.124)
Ethnicity (White=1)	-0.004 (0.138)	-0.002 (0.139)
Income Level	0.047 (0.031)	0.048 (0.031)
Dallas population	-0.161 (0.157)	-0.154 (0.156)
constant	9.399*** (0.412)	9.228*** (0.563)
observations	568	568
Subject clusters	284	284
R^2	0.138	0.139

APPENDIX B: MATHEMATICAL APPENDIX

The payoff for the chosen route is decreasing in the number of entrants. Let n_M and n_S denote the number of players that deterministically choose the main and side road in equilibrium, respectively. In the pure-strategy equilibrium, $n_M=12$ and $n_S=6$. In this game, there also exist mixed-strategy equilibria in which some players may play pure strategies while others mix their entry decision between the main and side roads with the same probabilities. In such equilibria, $n_M<12$ enter the main road with probability one, $n_S<6$ enter the side road with probability one, and the remaining $18\text{-}n_m\text{-}n_s$ mix their entry decision between the two roads. Let p_M denote the probability with which the $18\text{-}n_m\text{-}n_s$ players enter the main road. Then, the payoff for entering the main road is $\pi_M=10+2[12\text{-}1\text{-}\ n_M\text{-}\ p_M(18\text{-}\ n_m\text{-}n_s\text{-}1)]$ and the payoff for entering the side road is $\pi_S=10+3[6\text{-}1\text{-}\ n_S\text{-}(1\text{-}p_M)(18\text{-}\ n_m\text{-}n_s\text{-}1)]$.

In equilibrium, the payoffs corresponding to the two entry decisions must be equal. Setting $\pi_M=\ \pi_S$ and expressing p_M as a function of the number of players who deterministically enter the main and side roads, we obtain the following expression:

$$p_M=(11.6\text{-}n_M)/(17\text{-}\ n_m\text{-}n_s)$$

Setting $n_m=n_S=0$,which describes a situation where no player chooses one of the two roads deterministically, equilibrium entry in the main road ($p_m=11.6/17$) is that of the symmetric mixed strategy equilibrium where all players mix their entry decision between the two roads with the same probabilities. Note that p_m increases in n_s which is intuitively expected given that higher entry in the side road means incentives rise for entering the main road. Also, p_m decreases in n_m, meaning incentives for entering the main road decrease as more people deterministically choose the main road. In equilibrium, expected entry on the main road is between 11.7 and 12.57. It's minimized at $\{n_M=11, n_S=0\}$ and maximized at $\{n_M=0, n_S=5\}$ obtained from the following minimization/maximization problem:

$$min/max \quad n_M+(18-n_M-n_S)(11.6-n_M)/(17-n_M-n_S)$$

$$s.t.\ 11\geq n_M\geq 0,\ 5\geq n_S\geq 0$$

Assuming sorting and equilibration in the last period, we can also compute values for n_M and n_S under which the number of road changes is maximized or minimized. The pure-strategy equilibrium only prescribes the aggregate profile of entries in the two roads, but assuming sorting where subjects stick with the same road if playing deterministic strategies, the number of road changes for any period is minimized at 0 in the pure-strategy equilibrium in which $\{n_M=12, n_S=6\}$. To pin down the equilibrium profile under which the number of road changes is maximized, we need to maximize the following function:

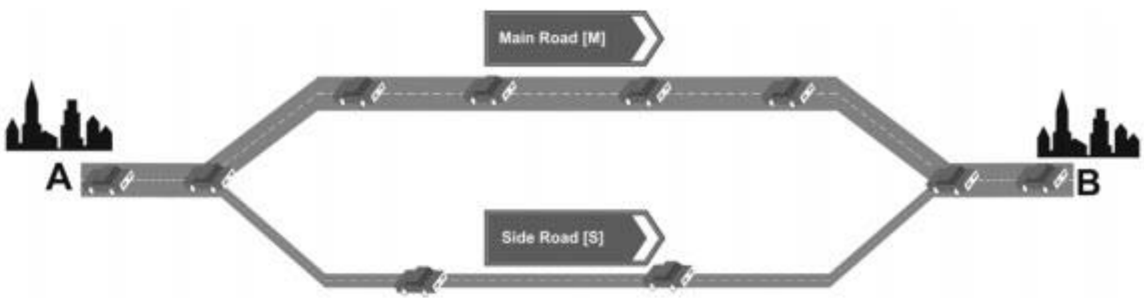
$$max \quad (12-n_M)(1-p_M)+(6-n_S)p_M$$

$$s.t.\ 11\geq n_M\geq 0,\ 5\geq n_S\geq 0$$

The number of road changes is maximized under the symmetric-mixed strategy equilibrium profile $\{n_M=0, n_S=0\}$ with 7.91 expected road changes. Assuming $\{n_M=6, n_S=3\}$, in expectation, there would be 3.9 road changes. As n_M and n_S get close to 11 and 5, respectively, the number of expected road changes decreases; i.e., at $\{n_M=11, n_S=5\}$, there is only 1 expected road change.

Experimental Instructions – Group

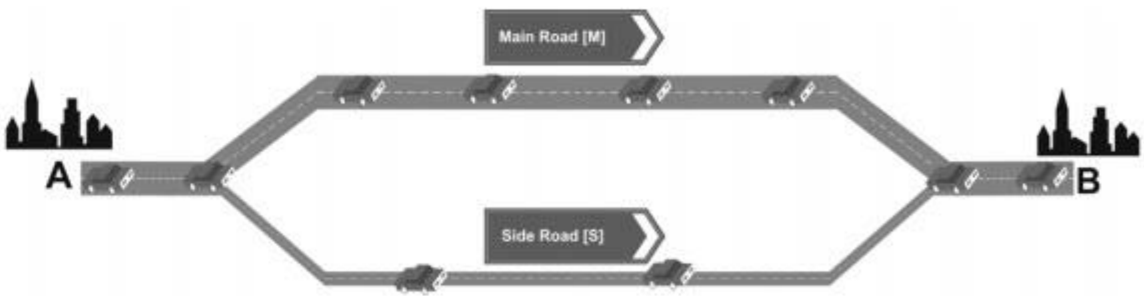
- Altogether 18 persons are participating in this experiment. The game situation is the same for every participant. The experiment consists of 200 periods.
- In each period you are travelling from a starting point A to an arrival point B. You can either choose a main road or a side road to get from A to B (see drawing below).



- For the travel time from A to B, the following holds: On both routes, the travel time increases with increasing traffic and decreases with decreasing traffic. If traffic is the same on the main and on the side road, the travel time is shorter on the main road than on the side road.
- You can make a new route choice in every period.
- Your payoffs per period: After each period you will receive a period payoff P which depends on the travel time T. Hereby holds: $P = 40 - T$, i.e. the shorter the travel time needed, the higher the payoff.
- Your information per period:
  - The travel time on the route that you chose in the preceding period
  - Your route chosen in the preceding period
  - Your period payoffs in the preceding period in Experimental Currency Units (ECUs)
  - Your cumulative payoffs before the route choice in ECUs
  - Number of the current period
- Each participant receives a seed capital of 200 ECUs. The exchange rate is $0.015 (1.5 cents) per ECU.
- Independent of your success in the game, you will receive a $10 lump-sum payment for participation.

Experimental Instructions – Solitary

- In each period you are to travel from a starting point A to an arrival point B. You can either choose the main road or the side road to get from A to B (see drawing below).



- On both roads, the travel time from A to B increases with increasing traffic and decreases with decreasing traffic. If traffic is the same on the main and on the side road, the travel time is shorter on the main road than on the side road.
- You can make a new road choice in every period.
- Your payoffs per period: After each period you will receive a period payoff P which depends on the travel time T. The payoff is: $P = 40 - T$, i.e. the shorter the travel time needed, the higher the payoff.
- During the months of October and November of last year (2019), this game was played in 10 sessions of groups of 18 with TAMU students making decisions each period in the Economic Research Laboratory.
- Today you are making decisions by yourself. But traffic will be determined based on the actions of 17 Virtual Subjects whose actions mirror 17 other TAMU students in an experimental session from October 23. Your road choice together with the 17 other subjects' choices will determine the traffic flow for both roads.
- Of course, your decisions will have no effect on the other players or modify their choices. The road choice of all of the 17 Virtual Subjects has already been determined for all of the 100

periods. But they are decisions, made by real people, who were also trying to make money in the experiment. They were human decisions, and are not random.

- Your information per period:
  - The travel time on the road that you chose in the preceding period
  - Your road chosen in the preceding period
  - Your period payoffs in the preceding period in Experimental Currency Units (ECUs)
  - Your cumulative payoffs before the road choice in ECUs
  - Number of the current period
- Each participant receives a seed capital of 200 ECUs. The exchange rate is $0.015 (1.5 cents) per ECU.
- Independent of your success in the game, you will receive a $10 lump-sum payment for participation.

Survey – Students

Start of Block: managed lane use items

Q1 Dear Student,

Thank you for agreeing to participate in this transportation study. In this part of the study, you will be asked to fill in the following survey to the best of your ability. After this, you will be asked to participate in a computer lab study that will also ask you transportation-based questions. For doing both parts of the study, you will receive payment between $45 and $55. Payment will vary based on your choices in the lab study.

Q3

In the past 6 months have you traveled on a road that included free lanes and toll lanes (often called express lanes) ? Texas examples include the I-10 Katy Freeway Managed Lanes (as shown in figure above) and the METRO HOT Lanes (US 290, I-45 North, Gulf Freeway) in Houston and the TEXpress Lanes on I-30 (Tom Landry Freeway), I-35E, I-635 (LBJ Freeway), I-820/SH 121/SH 183 (North Tarrant Express) in Dallas/Fort Worth and the MoPac (Loop 1) Express Lane in Austin.

○ Yes (1)

○ No (2)

Skip To: Q21 If Q3 = No

Page Break

Q4 **These questions relate to the most recent time you drove on a freeway with tolled (express) lanes.**

Q5 **What was the purpose of your most recent trip on the freeway with both tolled and free lanes? (Choose one of the following answers)**

○ Commuting to or from my place of work (going to or from work) (1)

○ Recreational/Social/Shopping/Entertainment/Personal Errands (2)

○ Work related (other than to or from home to work) (3)

○ To attend class at school or an educational institute (4)

○ Other (5) ____

Q6 **On that trip, did you travel in the non-toll lanes or the tolled lanes?**

○ Non-toll lanes (Regular Lanes) (1)

○ Tolled lanes (Express Lanes) (2)

Q7 **For the trip above, did you think about which type of lane (regular or tolled lanes) to use?**

○ Yes, I thought about which lane to use (1)

○ No, I just used the lane I usually use (2)

Display This Question:

If Q7 = Yes, I thought about which lane to use

Q8 **Which of the options below best describes your decision?**

- ◯ I spent a few seconds making the decision while driving (2)
- ◯ I decided which type of lane to choose before I got into the car (3)
- ◯ I decided to use the toll lane when I unexpectedly encountered heavy traffic on the non-toll lane (4)
- ◯ Other (5) ____

Display This Question:

If Q7 = No, I just used the lane I usually use

Q9 **Do you ever think about which type of lane (regular or tolled lanes) to use for any of your trip?**

- ◯ Yes (1)
- ◯ No, I just use the lane I always use (2)

Display This Question:

If Q9 = Yes

Q10 **How do you decide which lane (regular or tolled lanes) to travel in?**

○ I sometimes spend a few seconds making the decision on my way (2)

○ I sometimes decide which type of lane to choose before I get into the car (3)

○ I sometimes decide to use the toll lane when I unexpectedly encounter heavy traffic on the non-toll lane (4)

○ Other (5) ____

Display This Question:

If Q9 = No, I just use the lane I always use

Q11 **Why don't you choose between the regular and tolled (express) lanes? In other words, why do you always use the same lane?**

____

Page Break

Q12 **On what day of the week was your recent trip?**
(Choose one of the following answers)

- ○ Sunday (1)
- ○ Monday (2)
- ○ Tuesday (3)
- ○ Wednesday (4)
- ○ Thursday (5)
- ○ Friday (6)
- ○ Saturday (7)

Q13 **What time of day did that trip start? (for example, when did you leave <u>your house or driveway</u>?)**
(Choose one of the following answers)

- ☐ 12:00 AM (26)
- ☐ 12:30 AM (27)
- ☐ 1:00 AM (28)
- ☐ 1:30 AM (29)
- ☐ 2:00 AM (30)
- ☐ 2:30 AM (31)
- ☐ 3:00 AM (32)
- ☐ 3:30 AM (33)

- 4:00 AM (34)
- 4:30 AM (35)
- 5:00 AM (36)
- 5:30 AM (37)
- 6:00 AM (38)
- 6:30 AM (39)
- 7:00 AM (40)
- 7:30 AM (41)
- 8:00 AM (42)
- 8:30 AM (43)
- 9:00 AM (44)
- 9:30 AM (45)
- 10:00AM (46)
- 10:30 AM (47)
- 11:00 AM (48)
- 11:30 AM (49)
- 12:00 PM (50)

- ☐ 12:30 PM (51)
- ☐ 1:00 PM (52)
- ☐ 1:30 PM (53)
- ☐ 2:00 PM (54)
- ☐ 2:30 PM (55)
- ☐ 3:00 PM (56)
- ☐ 3:30 PM (57)
- ☐ 4:00 PM (58)
- ☐ 4:30 PM (59)
- ☐ 5:00 PM (60)
- ☐ 5:30 PM (61)
- ☐ 6:00 PM (62)
- ☐ 6:30 PM (63)
- ☐ 7:00 PM (64)
- ☐ 7:30 PM (65)
- ☐ 8:00 PM (66)
- ☐ 8:30 PM (67)

☐ 9:00 PM (68)

☐ 9:30 PM (69)

☐ 10:00 PM (70)

☐ 10:30 PM (71)

☐ 11:00 PM (72)

☐ 11:30 PM (73)

Q14 **What kind of vehicle did you use for that trip? (Choose one of the following answers)**

○ Motorcycle (1)

○ Passenger car, SUV, or pick-up truck (2)

○ Bus (3)

Skip To: Q20 If Q14 = Motorcycle

Skip To: Q19 If Q14 = Bus

Display This Question:

If Q14 = Passenger car, SUV, or pick-up truck

Q15 **How many people including you, were in the Passenger Car/SUV/Pick-up Truck? (Choose one of the following answers)**

◯ 1 (1)

◯ 2 (2)

◯ 3 (3)

◯ 4 (4)

◯ 5 or more (5)

Skip To: Q20 If Q15 = 1

Skip To: Q16 If Q15 != 1

Display This Question:

If Q15 != 1

Q16 **Were you the driver or a passenger on that trip? (Choose one of the following answers)**

◯ Driver (1)

◯ Passenger (2)

Skip To: Q18 If Q16 = Passenger

Skip To: Q17 If Q16 = Driver

Display This Question:

If Q16 = Driver

*

Q17 **How much extra time did it take to pick up and drop off the passenger(s)?**

◯ In minutes (1) ____

Q18 **Who did you travel with on that trip?(Check any that apply)**

- ◯ Neighbor (1)
- ◯ Child (2)
- ◯ Co-worker/person in the same, or a nearby, office building (3)
- ◯ Adult family member (4)
- ◯ Other (6) ____

Skip To: Q20 If Q18 , Neighbor Is Displayed

Display This Question:

If Q14 = Bus

Q19 **How much did you pay to ride the bus each way?(Choose one)**

- ◯ per trip (1) ____
- ◯ per day (2) ____
- ◯ per week (3) ____
- ◯ per month (4) ____

Q20 **What was your travel time on that trip (from the time you got in your vehicle to when your arrived at your destination)?**

○ In minutes (1) ____

Q25 **What is the ZIP Code of that trip's origin?**

○ Write the 5 digit zip code (1)

____

Q26 **What is the ZIP Code of that trip's destination?**

○ Write the 5 digit zip code (1)

____

Display This Question:

If Q3 = No

Q21 **Have you ever used tolled (express) lanes?**

○ Yes (1)

○ No (2)

Display This Question:

If Q21 = No

Q22 **What are the primary reasons you do NOT use the toll (express) lanes?(Check all that apply)**

- [ ] Access to/from to the toll lanes is not convenient for my trips (1)
- [ ] I have the flexibility to travel at less congested times (2)
- [ ] I do not feel safe traveling on the toll lanes (3)
- [ ] The toll is too expensive for me (4)
- [ ] The toll lanes do not offer me enough time savings (5)
- [ ] It is too complicated/confusing to use the toll lanes (6)
- [ ] I avoid tolls whenever possible (7)
- [ ] I don't want to have a toll transponder in my vehicle (8)
- [ ] I don't have a credit card needed to setup a toll transponder account (9)
- [ ] I don't like that the toll changes based on the time of day (10)
- [ ] I don't have anyone to carpool with (11)
- [ ] Other (12) ____

Skip To: Q113 If Q22 , Access to/from to the toll lanes is not convenient for my trips Is Displayed

Display This Question:

If Q21 = Yes

Q23 **What are the main reasons you do use the toll (express) lanes? (Check all that apply)**

- ☐ Access to/from to the toll lanes is convenient for my trips (1)
- ☐ Being able to use the lanes for free/reduced toll as a carpool (2)
- ☐ Travel times on the toll lanes are consistent and predictable (3)
- ☐ The toll lanes save me time (4)
- ☐ During the peak hours the toll lanes will not be congested (5)
- ☐ The toll lanes are safer than the general purpose lanes (6)
- ☐ The toll lanes are less stressful than the general purpose lanes (7)
- ☐ Trucks and large vehicles are not allowed on the toll lanes (8)
- ☐ Someone else pays my tolls (9)
- ☐ Other (10) ____

Skip To: Q113 If Q23 , Access to/from to the toll lanes is convenient for my trips Is Displayed

Q24 **On average, how much was the toll for your trip on the toll lanes?**

- Less than $2.00 (1)
- $2.00 - $5.00 (2)
- $5.01 - $10.00 (3)
- $10.01 - $15.00 (4)
- $15.01 - $20.00 (5)
- More than $20.00 (6)

Page Break

Q113 ***IMPORTANT***

Please note that once you click the next button at the bottom right of the present page, **<u>you cannot return to the previous assessments</u>**.

End of Block: managed lane use items

Start of Block: cognitive reflection task

Q76 **INSTRUCTIONS** Below are seven (7) problems that vary in difficulty. You have 10 minutes to complete them. Try to answer as many as you can.

Q77 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

JS *

Q78 1. A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?

____

JS *

Q79 2. If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

____

Q80 3. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

Q81 4. If John can drink one barrel of water in 6 days, and Mary can drink one barrel of water in 12 days, how long would it take them to drink one barrel of water together?

Q82 5. Jerry received both the 15th highest and the 15th lowest mark in the class. How many students are in the class?

Q83 6. A man buys a pig for $60, sells it for $70, buys it back for $80, and sells it finally for $90. How much has he made?

Q84 7. Simon decided to invest $8,000 in the stock market one day early in 2008. Six months after he invested, on July 17, the stocks he had purchased were down 50%. Fortunately for Simon, from July 17 to

October 17, the stocks he had purchased went up 75%. At this point, Simon has:

- ○ broken even in the stock market. (1)
- ○ is ahead of where he began. (2)
- ○ has lost money. (3)

End of Block: cognitive reflection task

Start of Block: conscientiousness (IPIP - 20-item version)

Q71 **INSTRUCTIONS**

Listed below are phrases describing people's behaviors. Please use the scale provided below to identify how accurately each statement describes you. Describe yourself as you generally are now, not as you wish to be in the future.

Describe yourself as you honestly see yourself in relation to other people you know of the same sex and roughly the same age as you. Please read each statement carefully, and then rate the extent to which it accurately describes you.

	Very inaccurate (1) (1)	Inaccurate (2) (2)	Neither inaccurate nor accurate (3) (3)	Accurate (4) (4)	Very accurate (5) (5)
Am always prepared. (1)	◯	◯	◯	◯	◯
Leave my belongings around. (21)	◯	◯	◯	◯	◯
Pay attention to details. (22)	◯	◯	◯	◯	◯
Make a mess of things. (23)	◯	◯	◯	◯	◯
Get chores done right away. (24)	◯	◯	◯	◯	◯
Often forget to put things back in their proper place. (25)	◯	◯	◯	◯	◯
Like order. (26)	◯	◯	◯	◯	◯
Shirk my duties. (27)	◯	◯	◯	◯	◯
Follow a schedule. (28)	◯	◯	◯	◯	◯
Neglect my duties. (29)	◯	◯	◯	◯	◯
Am exacting in my work. (30)	◯	◯	◯	◯	◯
Waste my time. (31)	◯	◯	◯	◯	◯
Do things according to a plan. (32)	◯	◯	◯	◯	◯
Do things in a half-way manner. (33)	◯	◯	◯	◯	◯

Continue until everything is perfect. (34)	○	○	○	○	○
Find it difficult to get down to work. (35)	○	○	○	○	○
Make plans and stick to them. (36)	○	○	○	○	○
Leave a mess in my room. (37)	○	○	○	○	○
Love order and regularity. (38)	○	○	○	○	○
Like to tidy up. (39)	○	○	○	○	○

Q72 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

End of Block: conscientiousness (IPIP - 20-item version)

Start of Block: need for cognitive closure

Q73 **INSTRUCTIONS** A number of statements are listed below. Please use the scale provided to indicate the extent to which you agree with them.

	Completely disagree (1) (1)	(2) (2)	(3) (3)	(4) (4)	(5) (5)	Completely agree (6) (6)
I don’t like situations that are uncertain. (1)	○	○	○	○	○	○
I dislike questions which could be answered in many different ways. (2)	○	○	○	○	○	○
I find that a well ordered life with regular hours suits my temperament. (3)	○	○	○	○	○	○
I feel uncomfortable when I don’t understand the reason why an event occurred in my life. (4)	○	○	○	○	○	○
I feel irritated when one person disagrees with what everyone else in a group believes. (5)	○	○	○	○	○	○
I don’t like to go into a situation without knowing what I can expect from it. (6)	○	○	○	○	○	○
When I have made a decision, I feel relieved. (7)	○	○	○	○	○	○
When I am confronted with a problem, I’m dying to reach a solution very quickly. (8)	○	○	○	○	○	○

I would quickly become impatient and irritated if I would not find a solution to a problem immediately. (9)	○	○	○	○	○	○
I don't like to be with people who are capable of unexpected actions. (10)	○	○	○	○	○	○
I dislike it when a person's statement could mean many different things. (11)	○	○	○	○	○	○
I find that establishing a consistent routine enables me to enjoy life more. (12)	○	○	○	○	○	○
I enjoy having a clear and structured mode of life. (13)	○	○	○	○	○	○
I do not usually consult many different opinions before forming my own view. (14)	○	○	○	○	○	○
I dislike unpredictable situations. (15)	○	○	○	○	○	○

Q74 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

End of Block: need for cognitive closure

Start of Block: maximization

Q75 **INSTRUCTIONS** A number of statements are listed below. Please use the scale provided to indicate the extent to which you agree with them.

	Completely disagree (1) (1)	(2) (2)	(3) (3)	(4) (4)	(5) (5)	(6) (6)	Completely agree (7) (7)
Whenever I'm faced with a choice, I try to imagine what all the other possibilities are, even ones that aren't present at the moment. (1)	○	○	○	○	○	○	○
No matter how satisfied I am with my job, it's only right for me to be on the lookout for better opportunities. (2)	○	○	○	○	○	○	○
When I am in the car listening to the radio, I often check other stations to see if something better is playing, even if I am relatively satisfied with what I'm listening to. (3)	○	○	○	○	○	○	○
When I watch TV, I channel surf, often scanning through the available options even while attempting to watch one program. (4)	○	○	○	○	○	○	○

I treat relationships like clothing: I expect to try a lot on before finding the perfect fit. (5)							
I often find it difficult to shop for a gift for a friend. (6)							
Renting videos is really difficult. I'm always struggling to pick the best one. (7)							
When shopping, I have a hard time finding clothing that I really love. (8)							
I'm a big fan of lists that attempt to rank things (the best movies, the best singers, the best athletes, the best novels, etc.). (9)							
I find that writing is very difficult, even if it's just writing a letter to a friend, because it's so hard to word things just right. I often do several drafts of even simple things. (10)							

No matter what I do, I have the highest standards for myself. (11)	○	○	○	○	○	○	○
I never settle for second best. (12)	○	○	○	○	○	○	○
I often fantasize about living in ways that are quite different from my actual life. (13)	○	○	○	○	○	○	○

Q114 ***IMPORTANT***

Please note that once you click the next button at the bottom right of the present page, **<u>you cannot return to the previous assessments</u>**.

End of Block: maximization

Start of Block: GMA form b instructions

Q111 **PLEASE READ THE INSTRUCTIONS BELOW BEFORE MOVING ON TO THE NEXT PAGE.**

This is a 10-minute timed test and because it is timed, it requires uninterrupted, undisturbed time to complete it. And so if you do not have 10 minutes of uninterrupted, undisturbed time, then please log out and come back when you have time to complete the test. Once you start this test, you will be unable to pause it.

When you click on the right arrow below, you will be presented with a set of problems. There are a total of 60 items, but the test will probably be too long for you to finish. However, complete as many items as you can in the allotted time. Work quickly and accurately. Do not spend too much time on any one item. Your score will be the number of items that you answer correctly.

You may also want to have scratch paper and a pen or pencil ready before you start since some of the problems you will encounter may require some "figuring out". Please do not use a calculator, a dictionary, or any other aid.

Click the right arrow at the bottom of this page to begin the test. The timer will begin once you enter the next page.

End of Block: GMA form b instructions

Start of Block: GMA form b

Q90 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

The 60 questions used in the survey cannot be shared. Hence, not included here.

End of Block: gma form b

Start of Block: delay discounting

Q119 Out of the 5 gambles below, which one do you prefer most?

- 50% chance of receiving $10 and 50% chance of receiving $10 (1)
- 50% chance of receiving $18 and 50% chance of receiving $6 (2)
- 50% chance of receiving $26 and 50% chance of receiving $2 (3)
- 50% chance of receiving $34 and 50% chance of receiving -$2 (4)
- 50% chance of receiving $42 and 50% chance of receiving -$6 (5)

End of Block: delay discounting

Start of Block: demographics

Q27 **How do you identify yourself?**

- ○ Male (1)
- ○ Female (2)
- ○ Other (Please specify) (3) ____

Q28 **Which of the following age categories represents your age?**

- ○ 18-24 (1)
- ○ 25-34 (2)
- ○ 35-44 (3)
- ○ 45-54 (4)
- ○ 55-64 (5)
- ○ 65 and over (6)
- ○ Refused (7)

Q70 **How would you describe yourself?**

- ○ White (1)
- ○ Hispanic or Latino (2)
- ○ Black or African American (3)
- ○ Asian (4)
- ○ American Indian or Alaskan Native (7)
- ○ Native Hawaiian or Other Pacific Islander (8)
- ○ Other (5) ____

Q54 **You are an:**

- ○ Undergraduate student (1)
- ○ Masters student (2)
- ○ Doctoral student (3)
- ○ Post-doc (4)
- ○ Other (6) ____

Q32 **What is your annual HOUSEHOLD income?**

- ○ Less than $ 10,000 (1)
- ○ $ 10,000 - $ 14,999 (2)
- ○ $ 15,000 - $ 24,999 (3)
- ○ $25,000 to $34,999 (4)
- ○ $35,000 to $49,999 (5)
- ○ $50,000 to $74,999 (6)
- ○ $75,000 to $99,999 (7)
- ○ $100,000 to $199,999 (8)
- ○ $200,000 or more (9)
- ○ Refused (10)
- ○ It's easier to tell my hourly wage rate (11) ____

Q118 **What kind of device did you use to complete this assessment?**

○ Desktop computer (1)

○ Laptop computer (2)

○ Notebook computer (3)

○ Tablet (4)

○ Phablet (5)

○ Smartphone (6)

End of Block: demographics

Start of Block: free response BCS transport

Q85 **Let us know if you have any comments on the transportation issues in Bryan/College Station**

____

____

____

____

____

End of Block: free response BCS transport

Start of Block: contact info for payment

Q87 **Thank you for your time.**

APPENDIX B

Survey – Dallas

The survey was similar to the one administered to students. There were minor changes in the Managed Lane Use block and Socio Demographic questions. A new section consisting of certain statements based on ML use was included. Those specific parts are included in this appendix.

Start of Block: be4ml - first page

Dear Traveler,

Two weeks ago you took a short survey and indicated your willingness to participate in our transportation study. Thank you! The survey attached to this e-mail is the first phase of the study. For completing this survey you will be paid $50. The survey should take about 40 minutes. We will send you information on the next phase of the study soon after you complete this survey.

This research study has been reviewed by the Human Subjects' Protection Program and/or the Institutional Review Board at Texas A&M University. For research-related problems or questions regarding your rights as a research participant, you can contact these offices at (979)458-4067 or irb@tamu.edu.

If you desire additional information about this study or have technical problems completing the online survey, please contact the research team at (979) 845-9875 or DriverSurvey@TTIMAIL.TAMU.EDU.

By clicking **NEXT** you acknowledge that you have read this information and consent to completing the survey.

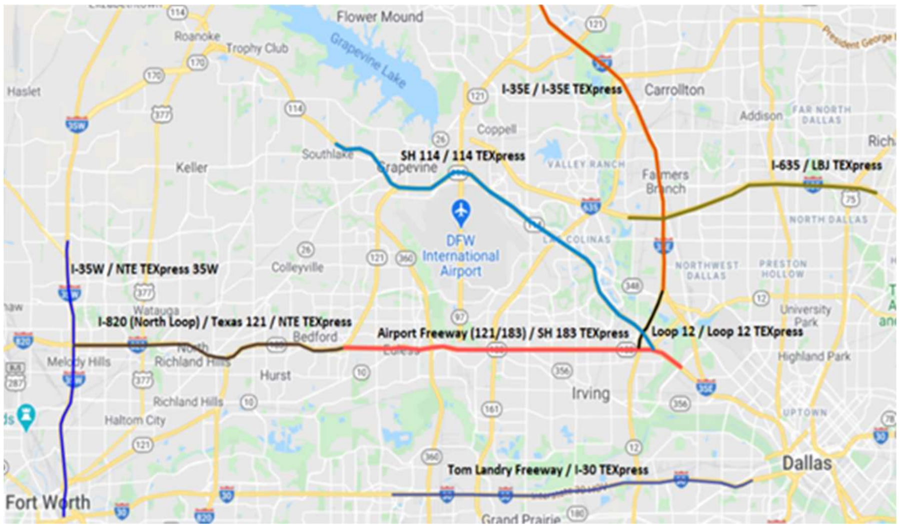
End of Block: be4ml - first page

Start of Block: managed lane use items_dallas_rv101720

Q118 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Q1 **As part of the survey 2 weeks ago you indicated that you have traveled on** I-635/ LBJ TEXpress or I-35W/ NTE TEXpress 35W or I-820 (North Loop)/ Texas 121/ NTE TEXpress or SH 114/ SH 114 TEXpress or Airport Freeway(121/183)/ SH 183 TEXpress or Tom Landry Freeway/ I-30 TEXpress or I-35E/ I-35E TEXpress or Loop 12/ Loop 12 TEXpress. **These questions relate to the most recent time you drove on any of those roads.**



Q3 **Which road did you travel on most recently:**

◯ I-635/ LBJ TEXpress (3)

◯ I-35W/ NTE TEXpress 35W (2)

◯ I-820 (North Loop)/ Texas 121/ NTE TEXpress (4)

◯ SH 114/ SH 114 TEXpress (8)

◯ Airport Freeway (121/ 183)/ SH 183 TEXpress (10)

◯ Tom Landry Freeway/ I-30 TEXpress (5)

◯ I-35E/ I-35E TEXpress (7)

◯ Loop 12/ Loop 12 TEXpress (6)

Page Break

Q119 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Q4 **What was the purpose of your most recent trip on that road?**
(Choose one of the following answers)

- ○ Commuting to or from my place of work (going to or from work) (1)
- ○ Recreational/Social/Shopping/Entertainment/Personal Errands (2)
- ○ Work related (other than to or from home to work) (3)
- ○ To attend class at school or an educational institute (4)
- ○ Other (5) ____

Q5 **On that trip, did you travel in the non-toll lanes or the tolled lanes (**LBJ TEXpress, NTE TEXpress 35W, I-820 NTE TEXpress, SH 114 TEXpress, SH 183 TEXpress, I-30 TEXpress, I-35E TEXpress, Loop 12 TEXpress**) ?**

- ○ Non-toll lanes (Regular Lanes) (1)
- ○ Tolled lanes (Express Lanes also known as TEXpress Lanes) (2)

Q6 **For the trip above, did you spend time thinking about which type of lane (regular or tolled lanes) to use?**

○ Yes, I thought about which lane to use (1)

○ No, I just used the lane I usually use (2)

Display This Question:

If For the trip above, did you spend time thinking about which type of lane (regular or tolled lanes... = Yes, I thought about which lane to use

Q7 **Which of the options below best describes when you made your decision whether or not to use the tolled lanes?**

○ I made the decision while driving, but before seeing the price sign (2)

○ I decided which type of lane to use before I got into my vehicle (3)

○ I decided which lane to use after seeing the price or just before the entrance (4)

○ Other (5) ____

Display This Question:

If For the trip above, did you spend time thinking about which type of lane (regular or tolled lanes... = No, I just used the lane I usually use

Q8 **Do you ever consider which type of lane (regular or tolled lanes) to use for any of your trips?**

○ Yes (1)

○ No, I just use the lane I always use (2)

Display This Question:

If Do you ever consider which type of lane (regular or tolled lanes) to use for any of your trips? = Yes

Q9 **How do you decide which lane (regular or tolled lanes) to travel in?**

- ○ I usually spend a few seconds making the decision on my way (2)
- ○ I usually decide which type of lane to choose before I get into my vehicle (3)
- ○ I usually decide which type of lane to use once I see the toll price (4)
- ○ Other (5) ____

Display This Question:

If Do you ever consider which type of lane (regular or tolled lanes) to use for any of your trips? = No, I just use the lane I always use

Q10 **Why don't you choose between the regular and tolled (**North Tarrant Express or I-35W Express or 183 Express or I-30 Express or Loop 12 Express or I-35E Express or 114 Express or DFW Connector Express**) lanes? In other words, why do you always use the same lane type?**

____

____

____

____

____

Page Break

Display This Question:

If On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Non-toll lanes (Regular Lanes)

Q22 **Have you ever used tolled (**LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXpress or SH 114 TEXpress or SH 183 TEXpress or I-30 TEXpress or I-35E TEXpress or Loop 12 TEXpress**) lanes?**

○ Yes (1)

○ No (2)

Display This Question:

If On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Tolled lanes (Express Lanes also known as TEXpress Lanes)

Q23 **Have you ever used the non-tolled lanes on** I-635 or I-35W or I-820 (North Loop)/ Texas 121 or SH 114 or Airport Freeway(121/183) or Tom Landry Freeway or I-35E or Loop 12**?**

○ Yes (2)

○ No (3)

Page Break

Q122 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Display This Question:

If On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Tolled lanes (Express Lanes also known as TEXpress Lanes)

And Have you ever used the non-tolled lanes on I-635 or I-35W or I-820 (North Loop)/ Texas 121 or SH... = Yes

Or If

On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Non-toll lanes (Regular Lanes)

And Have you ever used tolled (LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXpress or SH 114 TEXpr... = Yes

Or If

On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Non-toll lanes (Regular Lanes)

And Have you ever used tolled (LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXpress or SH 114 TEXpr... = No

Q24 **What are the primary reasons you do NOT use the toll (**LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXpress or SH 114 TEXpress or SH 183 TEXpress or I-30 TEXpress or I-35E TEXpress

or Loop 12 TEXpress) **lanes?**
(Check all that apply)

- ☐ Access to/from to the toll lanes is not convenient for my trips (1)
- ☐ I have the flexibility to travel at less congested times (2)
- ☐ I do not feel safe traveling on the toll lanes (3)
- ☐ The toll is too expensive for me (4)
- ☐ The toll lanes do not offer me enough time savings (5)
- ☐ It is too complicated/confusing to use the toll lanes (6)
- ☐ I avoid tolls whenever possible (7)
- ☐ I don't want to have a toll transponder in my vehicle (8)
- ☐ I don't have a credit card needed to setup a toll transponder account (9)
- ☐ I don't like that the toll changes based on the time of day (10)
- ☐ I don't have anyone to carpool with (11)
- ☐ Other (12) ____

Page Break

Q123 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Display This Question:

If On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Tolled lanes (Express Lanes also known as TEXpress Lanes)

And Have you ever used the non-tolled lanes on I-635 or I-35W or I-820 (North Loop)/ Texas 121 or SH... = Yes

Or If

On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Non-toll lanes (Regular Lanes)

And Have you ever used tolled (LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXpress or SH 114 TEXpr... = Yes

Or If

On that trip, did you travel in the non-toll lanes or the tolled lanes (LBJ TEXpress, NTE TEXpres... = Tolled lanes (Express Lanes also known as TEXpress Lanes)

And Have you ever used the non-tolled lanes on I-635 or I-35W or I-820 (North Loop)/ Texas 121 or SH... = No

Q25 **What are the main reasons you do use the toll (**LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXpress or SH 114 TEXpress or SH 183 TEXpress or I-30 TEXpress or I-35E TEXpress or Loop

12 TEXpress**) lanes?**
(Check all that apply)

- ☐ Access to/from to the toll lanes is convenient for my trips (1)
- ☐ Being able to use the lanes for free/reduced toll as a carpool (2)
- ☐ Travel times on the toll lanes are consistent and predictable (3)
- ☐ The toll lanes save me time (4)
- ☐ During the peak hours the toll lanes will not be congested (5)
- ☐ The toll lanes are safer than the general purpose lanes (6)
- ☐ The toll lanes are less stressful than the general purpose lanes (7)
- ☐ Trucks and large vehicles are not allowed on the toll lanes (8)
- ☐ Someone else pays my tolls (9)
- ☐ Other (10) ____

Page Break

Q124 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Display This Question:

If What are the main reasons you do use the toll (LBJ TEXpress or NTE TEXpress 35W or I-820 NTE TEXp... , Access to/from to the toll lanes is convenient for my trips Is Displayed

Q26 **On average, how much was the toll for your trip on the toll lanes?**

- ○ Less than $2.00 (1)
- ○ $2.00 - $5.00 (2)
- ○ $5.01 - $10.00 (3)
- ○ $10.01 - $15.00 (4)
- ○ $15.01 - $20.00 (5)
- ○ More than $20.00 (6)

Q161 **INSTRUCTIONS** A number of statements are listed below. Please use the scale provided to indicate the extent to which you agree with them.

	Strongly disagree (1) (1)	Disagree (2) (2)	Neither agree nor disagree (3) (3)	Agree (4) (4)	Strongly agree (5) (5)
Unless there is no traffic on the freeway, I choose the managed lane since traffic could become congested at any time. (1)	○	○	○	○	○
When buying fuel for my car, I use the most convenient gas station and do not pay much attention to price. (7)	○	○	○	○	○
I cannot understand why someone would pay to use the managed lane when the general purpose lanes are available for free, especially when it may or may not save time. (8)	○	○	○	○	○
I only choose to use the managed lane if the general purpose lanes seem crowded. (9)	○	○	○	○	○
The coordination involved with carpooling is more hassle than it is worth. (10)	○	○	○	○	○
I do not like relying on others for rides. (11)	○	○	○	○	○

Start of Block: Demographics

Q129 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Q27 **How do you identify yourself?**

○ Male (1)

○ Female (2)

○ Other (Please specify) (3) ____

Q28 **Which of the following age categories represents your age?**

- ○ 18-24 (1)
- ○ 25-34 (2)
- ○ 35-44 (3)
- ○ 45-54 (4)
- ○ 55-64 (5)
- ○ 65 and over (6)
- ○ Refused (7)

Q70 **How would you describe yourself?**

- ○ White (1)
- ○ Hispanic or Latino (2)
- ○ Black or African American (3)
- ○ Asian (4)
- ○ American Indian or Alaskan Native (7)
- ○ Native Hawaiian or Other Pacific Islander (8)
- ○ Other (5) ____

Q54 **What is your highest level of education?**

- ○ Less than high school (1)
- ○ High school graduate (3) 0
- ○ Some college or vocational school (2) 1
- ○ College graduate (4) 2
- ○ Postgraduate degree (7) 3
- ○ Refused (6)

Q113 **What category best describes your occupational or work status?**

- ○ Professional/managerial (1)
- ○ Technical (2)
- ○ Sales (3)
- ○ Administrative/clerical (4)
- ○ Manufacturing (5)
- ○ Stay-at-home homemaker/parent (6)
- ○ Student (7)
- ○ Self-employed (8)
- ○ Retired (9)
- ○ Other (10) ____

Q32 **What is your annual HOUSEHOLD income?**

- Less than $ 10,000 (1)
- $ 10,000 - $ 14,999 (2)
- $ 15,000 - $ 24,999 (3)
- $25,000 to $34,999 (4)
- $35,000 to $49,999 (5)
- $50,000 to $74,999 (6)
- $75,000 to $99,999 (7)
- $100,000 to $199,999 (8)
- $200,000 or more (9)
- Refused (10)
- It's easier to tell my hourly wage rate (11) ________________

Q115 **What kind of device did you use to complete this assessment?**

- ○ Desktop computer (1)
- ○ Laptop computer (2)
- ○ Notebook computer (3)
- ○ Tablet (4)
- ○ Phablet (5)
- ○ Smartphone (6)

End of Block: demographics

Start of Block: free response DALLAS transport

Q130 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Q85 **Let us know if you have any comments on the transportation issues in the Fort Worth area.**

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End of Block: free response DALLAS transport

Start of Block: contact info for payment

Q131 Timing

First Click (1)
Last Click (2)
Page Submit (3)
Click Count (4)

Q87 **Thank you for your time.**

Please provide your contact details to make sure that your data is correctly linked with the answers you have provided before. This information will be kept private and will be used only for payment purposes.

○ Full name (4) ____

○ Email address (5) ____

Q89 **If you wish to retake the survey, you can do so by pressing the back button. If you want to save your answers and exit the survey, please press the submit button.**

End of Block: contact info for payment