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THE DEMAND FOR MOBILITY: EVIDENCE FROM AN EXPERIMENT WITH UBER RIDERS

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ABSTRACT

Optimal transportation policies depend on demand elasticities that interact across modes and vary across the population, but understanding how and why these elasticities vary has been an empirical challenge. Using an experiment with Uber in Egypt, we randomly assign large price discounts for transport services over a 3 month period to examine: (1) the demand for ride-hailing services, (2) the demand for total mobility (km/week), and (3) its contributions to external costs (e.g. congestion). A 50% discount more than quadruples Uber usage and induces an increase of nearly 49% in total mobility. These effects are stronger for women, who are less mobile at baseline and perceive public transit as unsafe. Technology-induced reductions in the price of ride-hailing services could generate substantial benefits to users (4.3% of GDP) that would be accompanied by considerable increases in external costs (1% of GDP), with benefits accruing to the most affluent and costs being borne by the entire population.

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A randomized controlled trials registry entry is available at
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1 Introduction

The introduction and expansion of ride-hailing services represents one of the most dramatic changes in global transportation markets in decades. This is especially true in the developing world, where the high fixed costs of car ownership and low levels of reliability/safety of taxi services limit private transit use. While previous work has found substantial consumer surplus from ride-hailing services (Cohen et al., 2016, Alvarez and Argente, 2020a), it has been challenging to properly account for the associated external costs borne by society (Hall et al., 2018, Tirachini and Gomez-Lobo, 2020). It is well-understood that shifts from mass transit to the same travel in private vehicles involves considerably higher congestion and emissions externalities (FTA, 2010, FHA 2018). Credible estimates of how changes to private travel affect external costs requires exogenous variation in prices *and* comprehensive micro-data that can capture total mobility and substitution behavior on all available transportation choices.

To overcome these challenges, we implement a demand-side experiment on the Uber platform.[1] The study randomizes large, sustained changes to the prices facing Uber riders in Cairo, Egypt and introduces a new method for collecting comprehensive data on participants' mobility patterns using Google Maps' *Timeline* software. We randomly assign 1,373 Uber riders into three groups: (1) participants who face prices that are reduced by 50% for the 3-month study period, (2) participants who face prices that are reduced by 25% for the 3-month study period, and (3) a control group. We use individual-level data collected from Google Maps' Timeline, a mobile app that measures the total daily travel for each participant, to estimate the demand for *total mobility (km/day)*.[2] We combine this with data collected in follow-up phone surveys to examine how impacts on total travel are split across private and public modes of transport, each of which contributes differently to economy-wide transport externalities.[3]

We find evidence of a strong demand response to the price reductions, with those receiving a 25% price reduction more than doubling their Uber utilization and those receiving a 50% reduction more than quadrupling it. We find that these effects also translate into large increases in overall mobility – participants receiving the 50% treatment increase their vehicle kilometers traveled (VKT) by 49%, an increase of 1,211 km over the 12-week period. This increase in total travel understates the increase in private vehicle kilometers traveled (PVKT) due to substitution behavior. Using direct evidence on transport mode-switching, we find that the proportion of trips taken by public trans-

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[1] Individuals volunteered to join the research program, as outlined in section 2.2 below.

[2] Google Maps' Timeline feature is part of the Google Maps app and when activated tracks an individuals movement throughout the day. This allows us to get high quality data on the total amount of kilometers traveled by each participant during the study period.

[3] We focus on kilometers traveled as opposed to the number of trips taken because that is the relevant metric for assessing congestion and emission externalities. We also report impacts on trips, which are similar.

port declines by approximately 10%. Combining the effects on distance traveled with the substitution from public to private vehicles, we estimate that a 50% price reduction in ride-hailing can result in a 60% increase in private vehicle kilometers traveled.

We then examine impacts by subgroup and find that these average effects mask important heterogeneity by gender. Point estimates indicate that the price elasticity of demand for mobility is substantially higher among women (-1.47) relative to men (-0.60). Female participants are less mobile at baseline but have higher baseline Uber utilization, and they respond to the 50% treatment by expanding their Uber usage as well as their overall mobility more than men. We use data on transport mode use and safety perceptions to examine key mechanisms underlying these differences. We find that women feel more unsafe than men on all modes of transit aside from private cars and ride-hailing (where all participants tend to report feeling safe). While men primarily use Uber to increase their overall travel, a substantial portion of Uber use among women involves substitution away from public buses – the least safe travel option reported by female participants in our study. This substitution pattern is particularly strong among the subset of women who reported the public bus as an unsafe mode of travel at baseline. The price treatment on Uber leads to important increases in safety experienced in recent travel for female participants but not for male participants.

Researchers have predicted that costs in ride-hailing markets could fall by 40-80% as connected and autonomous vehicle (CAV) technologies improve (Narayanan et al., 2020). Given our strong reduced-form evidence of substantial latent demand for travel, it's important to consider the implications of these potentially large changes in future prices on both benefits and external costs (e.g. congestion & emission externalities), which are critical for designing optimal transport policies. We begin by estimating the welfare change associated with a technology-induced price reduction in the cost of ride-hailing. We compute the private benefits from price reductions using a measure of compensating variation in income under minimal assumptions. A key advantage of our experimental elasticity estimates is that our intervention shifts the price of Uber services faced by participants without affecting markets for complementary or substitute modes of transportation, which is typically not possible outside an experimental setting. We find that a price reduction results in substantial private benefits, but that these gains are heterogeneous, with the largest benefits accruing to women who find public transit to be unsafe.

While the elasticities that we estimate from the experiment are experimentally identified, they do not take into account potential market-level responses in terms of increased congestion. We construct a simple model that allows us to account for the effects of increased congestion on the equilibrium elasticity of total travel. In the model, agents choose how much to travel on private and public modes of transportation, with private modes contributing much more to congestion than public modes. Based on our partial equilibrium elasticities and a set of additional parameters including the value of time, the shape of the congestion function, and the share of the population that utilizes ride-hailing, we provide an estimate of the equilibrium elasticity for travel and the associated benefits at the population level in Cairo, Egypt.

We then estimate the increase in external costs by estimating the change in kilometers traveled on both private and public modes of transportation in equilibrium, and we combine this with comprehensive World Bank estimates of the cost of transport externalities in Cairo, Egypt (Nakat et al., 2013). In our preferred specification, we find that a 50% reduction in the price of ride-hailing would yield benefits that are equivalent to 4.3% of Cairo's GDP, but also increase external costs by 1.0% of GDP. This increase in benefits would be concentrated among users of ride-hailing services, who have higher incomes relative to Cairo's overall population, while the external costs are borne by the full population, which raises important questions about optimal taxation and redistribution.

A new database identifies more than 45 cities within Brazil, China, India and Mexico alone that have implemented uniform tax instruments to address externalities in the ride-hailing market and to redistribute the surplus (World Resources Institute, 2020). Our elasticity estimates suggest that taxes are likely to have strong effects on ride-hailing behavior in developing country cities like Cairo, but that implementing a *uniform* tax to more equitably address the regressive nature of imbalance between benefits and external costs would have a disproportionate impact on women. Our estimates indicate that a uniform tax would reduce overall female mobility by 46% more than the reduction in male mobility, with the greatest negative impact on women who feel unsafe on public transport. Earlier work has shown how transport accessibility and safety concerns can affect a variety of downstream outcomes for women including education and labor market choices (Kondylis et al., 2020, Kreindler, 2020, Anderson, 2014, Bryan et al., 2014, Desmet and Rossi-Hansberg, 2013). This suggests that policymakers must carefully consider heterogeneity in price elasticities when utilizing price instruments.

We highlight three important caveats to consider when interpreting our results. First, as with any experimental study implemented on a particular sample, we must be careful to consider the extent to which these results will generalize to non-experimental settings. We run two auxiliary experiments to test the importance of key features of our experimental design – the salience and length of the price reductions. We recover consistent elasticities when varying these features in independent experimental samples, providing strong evidence that they do not affect the interpretation of our results. Second, we describe the generalizability of our results using the framework prescribed by List (2020). We examine the transport characteristics of Cairo relative to other developing country megacities, and find a similar combination of high public transit ridership and high levels of harassment on public transit, suggesting that our findings could be important for understanding the mechanisms that might lead to outsized effects of price reductions on private travel for populations in several emerging economies. Third, our experimental design does not allow us to assess the full range of general equilibrium effects of large reductions in the price of ride-hailing services. Making personalized travel more accessible could have wide ranging impacts on outcomes and on time-scales that fall outside the scope of this specific experiment.

This paper contributes to a large empirical literature on the impact of transportation services on commuting patterns and economic activity in cities (Bryan et al., 2019, Campante and Yanagizawa-Drott, 2017, Asher and Novosad, 2018, Hanna et al., 2017, Duranton and Turner, 2011). A primary challenge in this literature is that the provision and prices of transportation services are almost never randomly assigned. As a result, empirical efforts have focused on settings characterized by exogenous shocks in service provision (Gupta et al., 2020, Gorback, 2020, Yang et al., 2020, Tsivanidis, 2018, Gonzalez-Navarro and Turner, 2018, Ahlfeldt et al., 2015, Anderson, 2014), available instruments (Severen, 2018, Baum-Snow et al., 2017, Duranton and Turner, 2011, Baum-Snow, 2007), and structural approaches (Heblich et al., 2020, Allen and Arkolakis, 2019, Redding and Rossi-Hansberg, 2017). Recent studies have demonstrated the benefits of high-frequency price variation in estimating price elasticities for gasoline or private transportation services (Levin et al., 2017, Cohen et al., 2016), though it remains difficult to study sustained changes in the price of transport services (Schaal and Fajgelbaum, 2020, Ahlfeldt et al., 2016). We contribute to this literature by randomizing the price of a transport service for a 3-month period and collecting comprehensive travel data, allowing us to provide a novel experimental estimate of the demand for mobility. We use this and other experimental parameters, along with a simple congestion feedback model to provide a framework for estimating the both the benefits and the external costs associated with changes in the price of travel.

An important feature of our research design is the measurement of overall mobility patterns using a mobile app, which helps to avoid recall/reporting biases. We combine these data with information from follow-up surveys to examine the mechanisms through which price reductions in transport services affect mobility, including substitution across modes, changes in the geography of travel, and learning. There is growing interest in using digital technologies to measure transportation decisions and map physical movements (Kreindler and Miyauchi, 2021, Kreindler, 2020, Martin and Thornton, 2017, Glaeser et al., 2018). Advances in data collection on mobile devices will facilitate direct observation of mobility patterns in future research, though these sources also involve important measurement challenges. We combine data from mobile phones with trip-level data on Uber travel and a trip survey, allowing us to evaluate the robustness of our central findings and perform validation tests that can inform future work on individual mobility patterns.

Our paper also builds on a growing set of economic studies of the impacts of ride-hailing on the travel choices of riders. Studies that consider how ride-hailing may act as a complement/substitute to other transportation modes have relied mainly upon stated preference methods (Leard and Xing, 2020, Young and Farber, 2019) or observational methods using aggregate behavior on outside modes (Hall et al., 2018).[4] Our detailed data on mode use and total distance traveled allow us to track the ways in which ride-hailing can act as *both* a complement and a substitute to other modes of transportation.

The previous literature on ride-hailing has largely focused on the benefits to participants (riders/drivers) (Buchholz et al., 2020, Alvarez and Argente, 2020b, Goldszmidt et al., 2020, Castillo, 2019, Moskatel and Slusky, 2019, Cohen et al., 2016). Our estimation strategy differs from prior work that relies upon exogenous price variation in observational settings, such as that from surge pricing as in a recent analysis of consumer surplus from ride-hailing in the U.S. by Cohen et al. (2016). Relative to these approached our sustained randomization allows us to overcome concerns about the relationship between price variation and local demand/supply conditions during a particular ride request.[5] We also contribute to the literature on ride-hailing, and transportation more generally, by providing a framework that researchers could use to estimate the external costs associated with changes in the price of transport. In our setting, we find these costs are considerable in magnitude and critical for optimal policy.

Finally, we also contribute to a strand of research that demonstrates that reducing the monetary cost of transportation can improve the economic outcomes of mobility-constrained populations (Franklin, 2018, Bryan et al., 2014, Phillips, 2014). We identify key sources of heterogeneity by gender and safety perceptions in Cairo's transport market, linking to the growing literature on the importance of female safety in transportation. There is evidence that perceived safety levels can affect educational attainment and earnings in developing country settings (Kondylis et al., 2020, Jayachandran, 2019, Velásquez, 2019, Borker, 2018). These safety considerations are also relevant in high income countries. For example, Kaufman et al. (2018) find that 54% of women are concerned about being harassed while using public transportation in New York City. Liu and Su (2020) show that the spatial distribution of jobs in the US contributes to the gender-wage gap due to differential preferences by gender about commuting. We find that subsidies for ride-hailing services result in disproportionate effects on women in several outcomes: Uber utilization, total mobility, substitution away from less safe options (buses), and self-reported safety in recent trips. Our results suggest the need for attention to the benefits of safety improvements and the safety of outside options when designing pricing

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[4] Relatedly, Alvarez and Argente (2020a) use experiments to estimate how demand for Uber changes based on riders' payment method, cash or credit.

[5] If surge pricing is commonly engaged during very busy periods with increased congestion, then the elasticity estimates used to compute CS may differ from the elasticities from non-surge trips. Shen (2023) suggests that ridehailing elasticities are higher in the face of congestion, which would imply that Cohen et al. (2016) estimates are upper bounds. Elasticies from surge pricing also differ from the variation we would observe from a market-wide experiment, where a change in price could affect road congestion and the effective prices of travel on outside modes (which we discuss in Section 6.2).

instruments for ride-hailing services, which are becoming widespread.

The paper proceeds as follows: Section 2 describes the setting and experimental design, Section 3 provides details on the data we collect and Section 4 reports the impacts on Uber Utilization. Section 5 reports the impacts on total mobility and presents robustness checks. Section 6 estimates changes in welfare and external costs and discusses policy implications. Section 7 discusses study limitations and Section 8 concludes.

2 Study Setting & Experimental Design

Cairo is a city of approximately 20 million inhabitants and is expected to continue to grow in the coming years. Cairo suffers from high levels of traffic congestion and underinvestment in public transit services (Nakat et al., 2014), and travel is perceived to come with non-trivial accident and harassment risk (Parry and Timilsina, 2015), similar to many other large cities in the developing world (see Appendix Table B4).

The primary modes of travel in Cairo include: private cars and taxis, private and public buses (though no official bus map exists for the city), a metro line that runs through the heart of the city, and other small transport vehicles such as mini-buses (private vans) and auto-rickshaws (locally called tuktuks).[6] Ride-hailing services are also well-established in Cairo. Egypt is one of Uber's larger markets, with over 4 million users (Reuters, 2018), where it launched in 2014. The ride-hailing market also includes another option in "Careem," which provides services that are similar to Uber.[7] At the time of the study, the market was considered competitive, with promotions and subsidies used regularly to attract both riders and drivers to the platform. Promotions usually take the form of coupons for 5-10% off of a set number of upcoming rides.

Cairo's residents spend between 5-7% of their income on transportation-related expenses.[8] Household expenditure on transportation services differs across the income distribution. At the lower end of the income distribution, individuals tend to spend less of their income on transport and rely upon low cost options, while those in the highest quintile spend closer to 7% of their income.[9] This is because there are large price differences between public and private options. A typical bus ticket costs 5 EGP, and a typical metro fare is also 5 EGP, for trips that can be as long as 40km. Ride-hailing services on the other hand can cost 6 EGP per kilometer traveled, as is also true of the costs of taxis.

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[6] Auto-rickshaws are not allowed on the highways, but otherwise there are no restrictions on type of vehicles allowed on the road network. i.e. there are no bus-only lanes/roads.

[7] Uber acquired Careem in 2019, but regulators approved the purchase conditional on Careem continuing to operate as an independent brand with independent management (Saba, 2019).

[8] This estimate comes from Egypt's Household Income, Consumption and Expenditure Survey of 2015 (Economic Research Forum, 2015).

[9] For comparison, this is somewhat lower than the share of income spent on transport in Latin American cities, where households spend between 12-15% of income on transport (Gandelman et al., 2019).

2.1 Experimental Design

We study the demand response to experimental variation in the price of ride-hailing services in Cairo. The experiment applied discounts that reduced the price[10] of Uber mobility services over a period of 12 weeks for two randomly-assigned groups of individuals that opted in: (1) a 50% reduction or (2) a 25% reduction to the price of Uber services. Participants in the control group continued to face standard market prices on the Uber app. The experiment reduced the prices on five of Uber's services, including the most common- UberX which provides a private car on demand based on the individual's requested start location and time. Participants also received a price adjustment on UberXL (similar to UberX but with larger cars), Uber Pool (rides shared with other passengers that are less expensive but may take longer to complete), Uber Scooter (rides on a two-wheeled motorcycle that are significantly cheaper than the car-based services, but potentially less safe/comfortable), and Uber Bus (a newer, high-occupancy service provided along a dynamic path across certain zones of the city).[11] See Appendix K for a discussion of ethical considerations regarding the experimental design.

2.2 Recruitment

To recruit the study sample, Uber's engineering team sent text messages to a random subset of riders who had taken at least one ride in Cairo over the past 4 weeks. The text message informed riders that researchers at the University of Illinois were conducting a study on mobility patterns and participants had a chance to receive discounts on their future Uber rides. Interested individuals were given a link to a registration page that provided more detailed information about the study and the opportunity to enroll.[12] Upon enrollment, participants received a phone call to confirm their understanding of the study and to implement the baseline survey that is outlined in section 3.1 below. Recruitment occurred in batches, with a group of messages sent out every 2-3 weeks, allowing for the surveyors to complete data collection on the existing cohort before sending recruitment messages to a new one.

2.3 Randomization and Enrollment

After successful completion of the baseline survey, participants were randomized into one of the two treatment groups or the control group. The randomization was conducted at

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[10] Any time we reference a "price reduction" in this paper, we refer to changes to the price faced by the consumer due to the researchers providing a discount and not through any changes in the market price of Uber services.

[11] Participants were informed that price reductions would not apply to rides on Uber Select, which is a service that provides on-demand rides in luxury cars and is Uber's most expensive option. This restriction was implemented to safeguard against the potential depletion of funds on services that were not commonly used and less relevant for the study.

[12] The response rate to the text message was about 2%, which is typical of these types of solicitations (Allcott et al., 2020, 2021).

the individual level and was stratified by gender and whether individuals were looking for a job. Each cohort was randomized separately (cohort fixed effects are included in all regressions). After randomization, individuals were sent an email to welcome them into the study and to inform them about their treatment status.[13] The first cohorts were enrolled in July 2019, with the final cohorts enrolled in December 2019.[14] During the study period, all participants were sequestered from other incentives that Uber provides on the basis of recent ridership. Those in the two treatment groups were told that they were provided their respective price reduction for 12 weeks and informed that they could apply it to any service except "Uber Select." Participants were also informed that the discounts could not be transferred to another person.[15] Subsidy treatments were applied directly to a participant's account and were applied to prices displayed to participants whenever they used the app, such that participants in each of the different groups faced different prices directly and in real-time in the context of a trip decision. For those assigned to treatment groups, the Uber App would display the reduced fare and below that, a smaller display of the original fare with a strike-through (an example can be found in Figure A.1).[16]

3 Data Collection & Sample Characteristics

3.1 Baseline Survey

Prior to their enrollment in the study, participants were asked to complete a baseline phone survey to collect individual characteristics such as gender, age, education, marital status and employment information. Appendix Table B1 reports the characteristics of the experimental sample of 1,373 participants at baseline. The sample is composed of 47% women (53% men), approximately half of whom are married. Participants in the control group make an average of 4,655 EGP in monthly income. 78% of the sample is currently working, though 48% of participants are looking for work at baseline. About a quarter of the sample owns a car. We compare our participants to a representative sample of Cairo residents in Appendix Table B2. We find that our sample is younger,

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[13] We do not have data regarding whether the participants had read the enrollment email but the results below will show that individuals respond to the subsidies within the first week (see Figure 1), providing evidence that the emails were seen in a timely fashion. Individuals were also cross-randomized into an information treatment. The entirety of treatment was two additional sentences in the enrollment email. One group was informed about a popular online job board that includes thousands of vacancies, and another was informed about a website that provided data on harassment risk around the city. We control for these additional treatments in our regressions, but their impacts are outside the scope of this paper.

[14] As discussed in Appendix J, we exclude the final cohort which was affected by COVID-19. Including them in our estimates does not qualitatively change any of our results.

[15] It is possible for Uber engineers to identify whether people were utilizing their account to provide discounted rides for other people. There were a negligible number of rides that fit that criteria in our sample.

[16] The 'discount display' (strike-through) was a requirement of the Uber engineering team. While not prominent on the screen, it could possibly affect the behavior of participants.

more educated, and has a higher income than the average Cairene, which is not surprising given that selection depends on utilization of Uber.

We also collect data on overall transport behavior through the survey and Google Maps Timeline (which we detail below). We ask respondents to report the number of trips they took on a variety of transport modes during the day before the survey.[17] This includes trips on the metro, on the bus, on taxis, in private cars as well as ride-hailing services (we group Uber and Careem in this question). Furthermore, in an effort to better understand baseline travel behavior and perceptions of available options, we collected detailed data on a participants' longest trip (in distance for a single direction of travel) taken the day before the survey. We began by collecting information on the mode of travel used for that trip. Figure B1 plots the fraction of trips on the 6 primary modes that participants use for their longest trips on a given day. The 3 primary modes of transit are bus, ride-hailing services, and private car, which together constitute more than 85% of trips. While these three modes are the primary modes used by both genders, men report the greatest reliance on bus services whereas women report the greatest reliance on Uber services for long trips.

Survey enumerators asked participants to report the perceived duration, cost, and level of personal safety for the longest trip they took yesterday. They then asked them to imagine taking the exact same trip using each of the 5 other primary modes available to them: private car, taxi, ride-hail (i.e. Uber or Careem), public buses (including private mini-buses), private bus (Swvl), and metro.[18] Participants were then asked to report their expectations about the duration, cost, level of safety, and likelihood of on-time arrival on each counterfactual mode. Figure B2 plots these counterfactual perceptions on each mode relative to ride-hailing services. Not surprisingly, ride-hailing is considered a more expensive option than all but taxi services. Ride-hailing is also considered to offer a faster trip from origin to destination than bus and taxi's but not substantially different from metro services or transport by private car. Interestingly, ride-hailing services are also considered to be substantially safer than all options aside from private car. An additional survey question asked participants to categorize the purpose of their trip as for work, school, leisure, or other.[19] Results reported in Table B7 indicate that the majority of trips were related to work (47%) or leisure (46%), with school representing 6% of trips in the sample.

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[17] To simplify comparisons across our different measures we adjust all of our variables so that they are reported over a 7-day period. For example, while we only ask about the number of trips taken across modes in the day before our survey, we multiply these estimates by 7 and report it as the number of trips taken on that mode weekly.

[18] We ask about ride-hailing as a whole to capture the overall effects on ride-hailing services, which include substitution from Careem to Uber. A few companies in Cairo (such as *Swvl*) now provide private bus services that people reserve in advance. Mini-buses in Cairo are vehicles that are about the size of a large van and can hold about a dozen passengers. They are usually the cheapest form of transit and follow varied routes usually starting and ending at well known landmarks.

[19] The leisure category includes: personal, family visit, shopping and health.

3.2 Google Timeline Data

To complete enrollment in the study, we asked individuals to adjust the settings on their mobile phones during the baseline survey to allow Google Maps to record their locations as they travel. Google uses this information to generate a "Timeline" of travel. This option is available for all mobile devices that have access to Google services (i.e. Android and iPhone devices), but is turned off by default. Some participants in our sample already had this service turned on at the time of recruitment, but the majority did not.[20] When turned on, Google then uses the location data to generate summary statistics on mobility patterns, including daily reports that provide the distance and time spent traveling on different transport modes (as shown in Figure A.2). Participants who had it off received guided instruction on how to turn on their Google Timeline and a follow-up call (4-7 days later) to confirm functionality and report to us the summary statistics for their travel on each of the past three days, which is then included in their baseline data.[21]

To our knowledge, this is the first case of researchers using Google's Timeline feature to collect data on the mobility behavior (total km traveled) of participants in an experiment. Digital and mobile-based technologies provide distinct advantages over earlier methods that depend exclusively upon respondent recall (Kreindler, 2020, Martin and Thornton, 2017). Google Timeline records the places an individual has been, how long it took to get there and how long they stayed there. Users can access both the summary of their travel and more detailed data which breaks the day into separate trips including information on the exact locations and exact times of their travel. Depending on the city, Google Timeline can differentiate between modes of travel including private car, bus, train, as well as plane, motorcycle and walking. In Cairo, Google's mode algorithm is unable to differentiate between car and bus travel since the two modes use the same routes and travel at similar speeds. We use the Timeline data to measure the total daily travel for each participant in the study – participants read their summary statistics to enumerators over the phone. We utilized this method to avoid participant concerns about potential violations of privacy.

The daily travel measurements on the Timeline app rely upon GPS measurements and a proprietary algorithm that is designed to detect and minimize error for a given set of measurements. While the large user base and importance of accurate trace data for many of Google's products may yield a more robust set of measurements than those collected from other available trace-retrieval applications (and their correction algorithms), little work has been done on the accuracy of the daily travel measurements from the Timeline

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[20] It is possible that part of the treatment effect is coming from making participants more cognizant of their Google Maps app and timeline and that this leads to a differential impact by treatment. But since the app is pre-installed on all Android phones and is one of the most downloaded apps on iPhone, we think any impact would be small relative to the direct treatment effect.

[21] We adjust these data by multiplying the values by $\frac{7}{3}$ so that the values reported in the tables represent a weekly time period. This allows for easier comparisons across measures.

app. Most prior studies that have used GPS data have relied exclusively on the single source, making it difficult to understand the magnitude or implications of measurement error. In Appendix C, we provide an analysis of measurement error in total daily travel using trip logs conducted by our research team prior to the experiment as well as using Uber administrative data and additional survey information for participants during the study. We find that the data from Google Timeline serve as a good measure of total distance traveled.

3.3 Follow-Up Surveys and Uber Administrative Data

Upon completion of the baseline survey (including reporting on their total daily distance traveled from Google Timeline), we randomized individuals into the different treatment groups. We then implemented multiple rounds of follow-up phone surveys with each participant in the sample, with four attempts per participant. Follow-up surveys mirror the baseline survey in collecting data on recent travel, counterfactual expectations about a participant's longest trip using alternate modes, and Google Timeline data over the past three days using the summary feature in the mobile application. Individuals were informed that for each successfully completed survey they will receive 25 EGP in Uber credit on their account. This is distinct from the subsidized prices shown only to participants in treatment.[22]

All participants consented to allow Uber to share trip-level Uber utilization data with the research team, including the 3-month period preceding the study, the study period, and the 3-months following the completion of the study.[23] For each trip, this dataset records the Uber service used (e.g. UberX, Uber Bus, etc.), the time of the trip (rounded to the nearest hour), the start and end locations of the trip (rounded to the 4th digit latitude/longitude), the distance and duration of the trip, the fare (both before and after the application of the price treatment, if appropriate), and any credits applied for payment of a trip (including the 25 EGP credits obtained after the completion of each survey).

4 Impacts on Uber Utilization

We use the following specification to estimate the impact of price treatments on outcomes:

$$Y_{it} = \beta_1 T_{1i} + \beta_2 T_{2i} + \beta_0 Y_{0i_{DPL}} + \delta_C + \gamma_t + \lambda_S + \varepsilon_{it} \tag{1}$$

where Y_i is the outcome of interest (e.g. weekly kilometers on Uber), T_1 and T_2 are indicators for the 25% treatment and 50% treatment respectively, $Y_{0_{DPL}}$ represents the

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[22] These one-time credits have the potential to have differential impacts due to their interaction with reduced prices. On average, 1 km of travel on Uber costs approximately 6 EGP, so those in the 50% treatment could travel an additional 4 km on each credit relative to control. A conservative upper bound estimate of this impact would be 20 km over the study period. By comparison, our impact estimates are equivalent to an increase of over 700 km in distance traveled on Uber in the 50% group relative to control during the study period.

[23] We analyze the post-treatment impacts of the subsidies in Appendix G.

set of baseline controls chosen using the double post-lasso procedure outlined in Belloni et al. (2014), δ_C are randomization cohort fixed effects, γ_t represents fixed effects for each round of follow-up surveys, and λ_S represents randomization strata fixed effects.[24] Standard errors are clustered at the individual level. Our results are robust to adjusting for multiple hypothesis testing using the methods outlined in List et al. (2019, 2021). To maximize power we make these adjustments on a regression where we include a combined treatment indicator. We report these results for all main tables in Appendix D.

For continuous variables, we measure outcomes using the Inverse Hyperbolic Sine (IHS) transformation, which confers three primary advantages: (1) our outcome data follow a log normal distribution, which lends itself to the IHS form; (2) it allows us to interpret the coefficients as percentage changes. To properly translate the coefficients into percentage change, we can calculate "$exp(\beta) - 1$," which for small values of β are approximately equal to β. As described below, several estimates that we report are quite large and the values can differ as a result (Bellemare and Wichman, 2020). We therefore report both the IHS coefficient in the tables and the corresponding changes in the text where appropriate; (3) The IHS transformation dampens the effects of outliers, while retaining realizations in outcomes that have a value of zero.[25]

4.1 Effects on Uber Usage

Table 1 reports estimates of the effects of the price reduction on the utilization of Uber services for transportation in the three experimental groups: control, 25% price reduction treatment, and 50% price reduction treatment. Column 1 reports effects on weekly distance traveled, which are estimated using the IHS transformation. Relative to the mean of 13.6 km per week for the control group, we estimate that the utilization of Uber services increases by 1.01 IHS points (approx. 23.7 km or 175% per week) for participants who receive the 25% price reduction and by 1.70 IHS points (approx. 60.8 km or 447% per week) for participants who receive the 50% price reduction.

Average effects mask important differences between male and female participants. In Column 2, we include an interaction term for male riders. These estimates indicate that female participants are more price elastic than their male counterparts. Weekly distance traveled on Uber in the 25% treatment group increases by 1.11 IHS points among

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[24] In addition to results with baseline controls chosen with the double post-lasso (preferred specifications), we also report our main results while controlling only for the baseline value of the outcome variable in Appendix H. We find no substantial differences in the two specifications, aside from increased precision in our preferred estimates. We list all controls provided to the lasso in Appendix H. We also control for two additional information treatments that were cross-randomized on the sample which are outside the scope of this paper.

[25] A recent paper discusses the potential for the scale of the dependent variable to affect estimated elasticities (Aïhounton and Henningsen, 2020). When we implement the procedure from Aïhounton and Henningsen (2020), we find that kilometers is close to the optimal level of scaling and provides slightly more conservative estimates. Our elasticity estimates are also very similar to the estimates generated using nominal levels instead of the IHS transformation.

female riders and by 0.93 IHS points among male riders. A similar difference is found in the 50% treatment group, where Uber utilization increases by 1.85 IHS points among female riders and by 1.58 IHS points among male riders. While differences by gender are not always statistically significant for the 25% group, we run a pooled specification to maximize power and report the findings in Appendix D. Even after correcting for multiple hypothesis testing concerns we find that the gender differences remain significant at the 10% level.

Columns 3 and 4 report effects on the average number of trips taken in a week.[26] Estimates in column 3 indicate that relative to the mean of 1.5 trips per week for the control group, participants who receive a 25% reduction increase their Uber trips by 1.8 trips per week (to 3.3) and participants who receive a 50% reduction increase trips by 3.7 per week (to 5.2). Estimates in column 4 indicate that the differential effect on trips for female participants in the two treatment groups parallels the findings on distance. In the low treatment group, the number of trips increases by 131% for women, and 100% for men. The 50% price treatment increases trips by 274% for women (from 1.6 to 5.7 trips per week) and by 205% for men (from 1.5 to 4.8 trips per week).

Figure 1 plots treatment effects for each group (upper panel) and average kilometers traveled on Uber across the 12 weeks of the study by gender and treatment group (lower panel). While the initial increase in utilization for the 25% group levels off after the first week, the (larger) initial increase for the 50% group continues to grow across the first 3-5 weeks of the study. These increases appear to level off during the latter half of the study, suggesting that participants maybe be adjusting during the first month of the study period, such that estimates from a 1-trip, 1-day, 1-week, or even 1-month study (commonly used in other work) would underestimate the long-run effect. The magnitude of estimates after the first month (week 4) are quite stable, which suggests that effects may asymptote toward a longer-run effect. Specifically, we do not find any evidence of differences in the magnitude of effects after the first month of treatment.

We plot the results from quantile regressions of the treatment effect in Figure B3. We do not interpret these as quantile treatment effects, as that would require a strong rank-preservation assumption. On the other hand, it provides suggestive evidence that our estimates of average treatment effects are not driven by a small group of "super-users." Panel A presents the estimates on total distance traveled. We find that they are relatively evenly distributed across quantiles. In both the 25% and 50% price treatments, there are a small fraction of riders that do not respond to the treatment, a large increase in the middle of the distribution, and a moderate increase at the top of the distribution. Panel B presents the estimates for trips taken, which illustrate a steady increase over the distribution, with larger increases for women relative to men. In each of the quantile regressions, we utilize bootstrapped standard errors with 1,000 repetitions, clustered at

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[26] Since the number of trips in a week is usually small we analyze this variable using levels instead of IHS.

the individual level.

4.2 Price Elasticity of Demand for Uber

In Panel B of Table 1, we explicitly estimate price elasticities of demand for both distance traveled and trips per week. Demand elasticities for total Uber kilometers average -9.5 for women and -6.8 for men. Elasticities estimated based on the number of trips taken are more similar across genders, with women averaging -5.1 and men averaging -4.4. The confidence intervals for these elasticity estimates generally overlap between genders.

Our estimates are larger than recent private travel elasticities from the United States gasoline market, which are larger than had been found in prior studies with aggregate data and cross-sectional designs Levin et al. (2017). They are also larger than those found in the United States taxi market (Rose and Hensher, 2014) However, they are consistent with recent estimates from ride-hail services in Prague (Buchholz et al., 2020). Our estimates may differ with the earlier literature for a few potential reasons: (1) Prior studies have typically examined the effects of short-run price changes. As far as we are aware, this price treatment was the largest and longest that Uber has provided to riders. We will test the importance of this in the following subsection. (2) Whereas prior studies have typically focused on transport markets with higher-quality substitutes, this study specifically focuses on a transit-constrained city. The large price changes examined in this study may induce significant substitution across different modes of travel, including other ride-hailing services; We assess the importance of substitution across modes in Section 5.2. (3) The experimental elasticities in Table 1 isolate the response to a change in price alone, while studies of market-wide price changes examine responses to changes in monetary costs as well as endogenous increases in time cost related to congestion effects. We examine differences between the effects of monetary price changes in our sample and the equilibrium effects of market-level price reductions in Section 6.

Experiments on the Salience and Length of Treatment

It is possible that our pre-announced price reductions affected the salience of discounted Uber services, leading to increased utilization due to the attention our study brings to travel as opposed to the price effects alone. In order to better disentangle the experimental effect of the price change from the salience and length of announced discounts, we implemented two separate 1-week experiments with additional waves of participants.

In the first auxiliary experiment, we split the sample into 3 treatment groups (50% price reduction, 10% price reduction, control) and held all elements of the experimental protocol constant aside from the length of the intervention.[27] Participants were sent an email telling them that they were enrolled in the study, and that they would get a *1*

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[27] We reduced the treatment in the low group from 25% to 10% as a result of implementation costs. We also note that due to an implementation error in this experiment, the 50% group was provided a one-time price change instead of a week-long price change and so we omit them from the table.

week subsidy based on their treatment group (as opposed to the 3 months in the main experiment).

In the second auxiliary experiment, we split a different sample into 3 treatment groups (50% price reduction, 10% price reduction, control) but instead of informing the participants of their impending discount we simply applied the discount to their accounts automatically for 1 week. These individuals did not know in advance that they would have a price reduction during this time, nor did they know how long the price reduction would continue for. This experiment deviates from the main experiment in two ways: (1) in the length of the subsidy (i.e. 1 week vs 3 months) and (2) in the salience of the subsidy (pre-announced vs unannounced).

Table 2 reports the results of these two experiments alongside estimates of effects from the first week of the main experiment. We assess the importance of salience by comparing impacts on Uber utilization for the 10% treatment group in columns 3 & 4 versus columns 5 & 6. If it were the case that prior knowledge of the discount was leading to strategic overuse of Uber during the 1 week of the discount (e.g. moving up travel they were planning to take in the future to benefit from the discount), we would expect greater increases among participants in the pre-announced experiment relative to those in the unannounced experiment. Instead, we find that the effects on weekly kilometers are nearly the same across the two experiments, while the number of trips is somewhat smaller but not statistically different in the pre-announced experiment. Even without strategic overuse, bringing attention to the subsidy could have led to additional utilization due to salience effects. We do not find any evidence to support this hypothesis.

We evaluate the effect of knowledge of the 3-month experimental treatment by comparing the impacts from the 1-week experiments to the impacts from the first week of our main experiment. The point estimate for weekly kilometers from the 50% price reduction is 0.65 in the main experiment versus 0.77 in the 1-week experiment. These estimates are statistically equivalent. Hence, it does not appear that intervention length has an important impact on the findings reported in our main experiment.

5 Effects on Overall Mobility and Substitution

The estimates reported in the prior section demonstrate that price reductions on Uber services dramatically increase Uber utilization. Furthermore, we are able to use Uber administrative data on the origin and destination locations of trips taken by study participants to show that subsidies increase Uber travel to an expanded set of locations in Cairo (which we explore in Appendix F). However, these estimates alone are not sufficient for determining to what extent the price treatments increase mobility (total kilometers traveled) versus inducing substitution from other modes. To our knowledge, no prior study has measured effects on total mobility or fully accounted for substitution behavior in the context of reductions in the cost of private transport services. This is likely to be especially important in transport markets in developing country cities, where travel is not dominated by a single transit mode (such as private car travel).

5.1 Effects on Overall Mobility

To test for effects on total mobility, we estimate differences in *total distance traveled* by participants during the intervention using data from each participant's Google Maps Timeline (described in section 3.2 above).[28] Table 3 reports estimates for each of the treatment groups. Columns 1 and 2 report effects on total distance traveled during the week before the survey, as reported on a participant's Google Timeline during follow-up surveys.[29] Relative to the mean of 205 km per week for the control group, point estimates suggest that total mobility increases by 0.10 IHS points (approx. 22 km or 10.5% of the control mean) for participants who receive a 25% price reduction, though this effect is not statistically significant. Total mobility increases by approx. 101 km or 49% of the control mean among participants who receive a 50% reduction.[30]

The average male participant in the control group travels nearly twice as much as the average female participant (261 km vs. 145 km per week). Column 2 reports effects on overall mobility for female versus male riders. Among female riders, our estimates suggest a larger (but non-significant) increase of approx. 29 km or 19.7% of the control mean in the low treatment group. In the high treatment group, we estimate an increase of approx. 106 km or 73% of the control mean. Differences by gender are not statistically significant, but suggest much smaller effects for men in both treatment groups.

Price Elasticity of Demand for Mobility

In Panel B of Table 3, we report estimates of the price elasticity of demand for mobility (total travel). We begin by calculating the price-elasticity of demand for mobility with respect to the price of Uber. The estimated elasticities for the full sample are -0.44 for the low subsidy and -0.99 for the high subsidy. The average elasticity for women is -1.32, and for men it is -0.38. These estimates are consistent with other estimates of price elasticity of travel demand. Power calculations conducted prior to the experiment suggested that treatment effects on total travel could be difficult to detect for the 25% group and indeed we cannot rule out an elasticity of 0 in the 25% group. Hence, another possible interpretation of our results is that moderate changes in cost of Uber may not change overall mobility, but large price changes do. Figure B3 includes results from quantile regressions of total distance traveled by treatment and gender in Panel C. We find that

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[28] We describe a battery of test to assess the accuracy of these data in Appendix C.

[29] As mentioned above, we collect the three days of data prior to our follow-up survey from Google Timeline. We multiply this by $\frac{7}{3}$ to simplify comparison across measures.

[30] The estimated impacts for the two treatment groups are statistically different at the 1% level.

the results are relatively evenly distributed across all quantiles, providing evidence that our average treatment effects are not driven by a small subset of users who dramatically increase their overall mobility.[31]

Next, we compute an alternate statistic: the price-elasticity of demand for mobility with respect to the overall price of mobility. This elasticity describes how a change in the price of any transport service will affect total distance traveled. This parameter can be used by researchers and policymakers to make more informed decisions about how price changes will affect congestion and emission externalities in the absence of direct data on overall mobility.

We formalize this notion with a representative consumer's utility function: $U(M, Y)$ where M is mobility and Y is everything else. Transportation is produced based on a production function $M = f(q)$ where q is a vector of quantities of products bought on the market and f is homogenous of degree 1. If we let p be the price vector corresponding to q, this yields a definition of a "cost of mobility" in terms of the following unit cost function:

$$c(p,1) = \{\min_{q>0} p \cdot q \quad \text{s.t} \quad f(q) = 1\} = \min_{q>0} \frac{p \cdot q}{f(q)}$$

Defining the price of Y as p_Y and income as w, the consumer's optimization problem becomes:

$$\max_{M,Y} U(M,Y) \quad \text{s.t} \quad c(p,1)M + p_Y \cdot Y \leq w$$

We can recover an estimate of the change in the aggregate cost of mobility resulting from a change in the price of Uber using:

$$\frac{\partial c(p,1)}{\partial p_1} = \frac{q_1}{M}, \quad \frac{\partial^2 c(p,1)}{\partial p_1^2} = \frac{\frac{q_1}{M}}{\partial p_1}$$

where $\frac{q_1}{M}$ represents the quantity of Uber usage per unit of mobility produced. This yields the following expression for the percent change in the cost of mobility that result from a given percent reduction in the price of Uber services:

$$\Delta\% c = s_1 \Delta\% p_u + \varepsilon_{uu} s_1 (\Delta\% p_u)^2 \tag{2}$$

where s_1 measures the share of mobility completed using Uber services, $\Delta\% p_u$ measures percent changes in the price of Uber, and ε_{uu} measures the (budget) share elasticity of Uber, i.e. the response of a consumer's transport budget allocation to Uber in response to a price change on that mode. Using values from our experiment, we estimate that a 25% or 50% decrease in the price of Uber reduces the aggregate price of 1km of travel for

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[31] The quantile regressions utilize bootstrapped standard errors with 1,000 repetitions, clustered at the individual level.

the average participant in our sample by 6.1% and 13.7%, respectively.

Panel C of Table 3 reports the price-elasticity of demand for mobility relative to the overall price of travel in our sample. We find that elasticities of -1.81 and -3.62 for the 25% and 50% price treatments. By accounting for the optimal allocation of a transport budgets across services, these elasticities allow for a more general analysis of how the changes in the price of a given service will affect total mobility. For example, using this measure we estimate that a 50% reduction in the price of bus service would result in a 31% increase in the total daily travel by the average participant in our sample.[32] This compares to the 49.5% increase in total travel that results from a 50% price reduction on Uber.This difference is driven in large part by the much smaller impact of a 50% reduction in the price of bus services on the aggregate price of travel in our Cairo sample. While public bus travel accounts for an important share (33.5%) of the average participant's total trips, it accounts for a substantially smaller fraction (11.4%) of their transport budget.

5.2 Is Uber a Substitute or a Complement to Other Modes?

Cities around the world are interested in the extent to which travelers use ride-hailing services as a substitute or complement to public transit. Empirical studies have produced mixed results, with some concluding that ride-hailing services increase private vehicle kilometers traveled (PVKT) (Tirachini and Gomez-Lobo, 2020) and others indicating that they increase public transit use (Hall et al., 2018).[33] The literature has thus far been unable to reconcile these results, which is critical for developing optimal transport policies.

Our research design allows us to evaluate how transport mode choice responds to changes in Uber usage at the individual level. Table 4 reports effects on the number of trips taken on each the 5 main modes of transportation on the day before our survey.[34] The bottom panel reports corresponding effects on mode choice probabilities.[35] The estimates reveal evidence of *substitution* away from the primary transit mode used by the Cairo sample: the public bus. The 50% fare reduction reduces the number of weekly bus trips by 1.51 and the probability of taking a bus trip by 10 percentage points. We also observe a smaller shift away from taxis, which are perceived as less safe and more costly than ride-hailing services. We find suggestive evidence of small increases in the number

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[32] This comparison relies on the assumption that $\varepsilon_{uu} = 0.98$ found for Uber services is also comparable for public bus services. Estimates of 0.75 or 0.5 for the price-elasticity of demand for bus services imply respective increases of 28.6% and 26% in total mobility.

[33] Using variation in entry timing and growth of Uber services across metropolitan areas, Hall et al. (2018) suggest that within 2 years of entry, Uber services *increased* public transit use by 5% for the average transit agency in the U.S.

[34] We multiply the number of trips by 7 to simplify comparison with the weekly time period used in the other tables in the paper.

[35] We compare effects on mode choice probabilities for all trips to those for longest trips in Appendix Table C7 and find that they are highly consistent.

of trips taken by metro and private car in the 50% treatment, although these differences are not statistically significant.

Our survey collects data on the total number of ride-hailing trips, including Uber as well as other services such as Careem. By comparing the treatment effects estimated using Uber admin data to treatment effects on total ride-hailing trips from the survey, we can evaluate the magnitude of substitution between Uber and other ride-hailing services in response to the price change. While those in the 50% group take an extra 3.66 trips on Uber (based on our estimate in Table 1), they only take an additional 2.32 trips on any ride-hailing service. Assuming the self-reported trip data are perfectly comparable to the Uber administrative data, this implies a substitution effect of approximately 1.34 weekly trips from Careem to Uber, which is about a third of the increase in Uber utilization. The same substitution behavior occurs in the 25% treatment group, about half as often. These data allow us to go beyond estimating Uber specific elasticities and calculate elasticities for ride-hailing in general.

Our results indicate that price reductions on Uber induce substitution away from bus trips, taxi trips and other ride hailing services. Nonetheless, a reduction in the proportion of travel taken on public bus doesn't necessarily imply a decrease in the total travel taken on public transit. While we do not directly measure changes in the distances traveled for each trip taken by each mode for each individual, results in Appendix Table B5 indicate that the average length of Uber trips increases substantially for those in treatment.[36] In Appendix Table B6, we estimate the total distance travelled separately by public and private modes under the assumption that total distance traveled on a mode is proportional to the rate of utilization of that mode. Under this assumption, we find no evidence of a significant decrease in total distance traveled on public transit, with point estimates consistent with a potential increase. This suggests that ride-hailing could serve as a complement to public transit in certain contexts, with individuals taking fewer but *longer* trips on average.

The findings above illustrate the importance of understanding multi-margin responses to shifts in the price of transport services. As participants become more mobile, they may increase their use of other modes in multi-part journeys or for return trips. Our micro-level findings indicate that price reductions have considerable effects on trip substitution, though these substituation effects may not convert into large reductions in the kilometers traveled using public buses use when accompanied by strong increases in

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[36] Estimates reported in Appendix Table B5 indicate an increase of 0.17 IHS-points in the length of trips on Uber in the 50% treatment group, which corresponds to an 18.5% increase. The results from Table 4 indicate participants in the 50% group take 1.2 additional trips per week (across all modes of transport), a statistically significant 6% increase relative to control. Combining these two estimates produces a calculated increase in total mobility of 26%, which lies within the confidence interval of our estimates of the impact of the 50% price reduction on total mobility using the Google Maps Timeline measure, providing additional evidence of consistency in the estimated effects obtained using the different data sources.

total travel. The implication for metro use, where point estimates suggest an increase in the 50% group that is not statistically significant, is that the 50% price reduction may have induced a net increase through complementarity. This is corroborated by the finding (from Appendix Table F.1, using administrative data on the location of trips) that price reductions increased Uber travel to and from metro stations.

5.3 Safety Concerns Help Explain Heterogeneity by Gender

Our baseline survey reveals important gender disparities in baseline mobility levels and in expectations regarding safety on public transit. In the presence of large fare reductions for ride-hailing services, women may benefit from shifting existing trips away from modes where they feel less safe, which could help explain why we find greater substitution behavior by women relative to men. We explore this below using two different pieces of information: (1) self-reported levels of safety on recent trips and (2) heterogeneity in effects on Uber use and total mobility among safety-conscious riders.

In Table 5, we report the estimated effects of the treatments on the reported *safety* of the longest trip that a participant took on the day prior to the survey. We find significant increases in the perceived safety of recent trips among participants in the high treatment group. However, they appear to be entirely driven by female participants, who report a 0.2 point increase in the safety of yesterday's trip from an average baseline rating of 4 out of 5. We find no impact on perceived safety among men.[37] To assist interpretation, estimates in Columns 3 & 4 standardize the outcome variable. Perceived safety increases by 0.17 standard deviations in the 50% group, which is considered large in other literatures with hard-to-interpret outcomes (e.g. test scores in education as in Evans and Yuan (2020)).

Panel A of Table 6 reports the results of tests for differences in the effects of the price interventions on mobility for individuals who used the bus at baseline. These tests suggest important gender differences that also vary across the two treatment groups. Whereas our estimates suggest that the intervention may have had somewhat *smaller* effects among male bus riders in both groups, we find *substantially larger* effects for female bus riders in the 50% treatment group (Columns 2 & 3). The intervention increases Uber utilization by 2.29 IHS points for this group. Our point estimate becomes even larger when we examine effects for female bus riders who perceive public transit as unsafe (at baseline) (Column 5). For this group, the 50% price reduction increases Uber utilization by 2.93 IHS points.[38]

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[37] Table E2 in the appendix shows that nighttime travel on Uber is similar across both genders, implying that these safety gains are more due to adaptations to the general safety environment as opposed to specifically unsafe times of day.

[38] It is worth noting that while women are much more responsive to the price change, the overall level of Uber utilization for male bus users in control is more than twice as large as female bus users in control. This changes after the price change, with women who feel unsafe on the bus at baseline increasing their level of Uber usage to surpass the the level of usage by men who felt the bus to be unsafe.

In Panel B, we report effects on total mobility for the same groups. These estimates indicate that while female bus riders increase their Uber usage relative to non-bus riders, they do not increase their overall mobility relative to non-bus riders. This result holds for women who perceived the bus as unsafe at baseline. Appendix Table E3 helps explain this by showing how women who took the bus at baseline substitute away from the bus more, while men don't. Taken together, these results indicate that price reductions on Uber lead to important differences in travel by gender and baseline behavior and perceptions. In particular, women substitute away from using the bus and subsequently report feeling more safe on their recent trips. This result is stronger for women who perceived the bus as an unsafe mode of transit at baseline.

5.4 Robustness Tests

We consider three main types of robustness tests: (1) implicit transfer effects, (2) survey response rates, and (3) sensitivity to controls.

One underlying concern in our experimental design is that the price intervention also serves as an implicit income transfer. By making these trips cheaper, the overall budget constraint for participants has changed and it is possible that participants use Uber more because they have more income to spend on travel. Our intervention is different from the pure transfers found in some other programs (e.g. Banerjee et al. (2017)), since participants in treatment still face a non-zero price in every transaction, and so our impacts are unlikely to be driven primarily by these implicit transfers.

Second, Appendix Tables B8 - B10 provide information about survey response rates. Column 1 shows that 94% of the control group responded to at least 1 follow-up survey, with 96% of the low treatment group responding to at least one and 97% of the high treatment group. Columns 2-5 provide information about response rates for each survey. The first two follow-up surveys indicate that control group response rates fall in the 80% range while the latter two suggest much lower response rates. Treatment assignment does lead to a statistically significant increase in response rates. Reassuringly, Appendix Tables B9 & B10 illustrate that there is no differential response based on observable characteristics. In other words, individuals who are responding to the surveys in the treatment groups are observationally equivalent to those who respond to the surveys in the control group. This is true both for whether they respond to any follow-up survey, as well as for their response rates for all follow-up surveys. We also estimate lee bounds for both our "Total Mobility" and "Safety" outcomes in Appendix Tables B11 & B12 (we have no attrition in the Uber admin data by design).

Third, our main results utilize the double-post lasso procedure outlined in Belloni et al. (2014). This procedure allows us to maximize statistical power while remaining agnostic regarding which controls to include in our regressions. In Appendix H we redo our main tables using the ANCOVA specifications that were previously standard in the experimental literature (McKenzie, 2012). Those tables include the results from regressions of the outcome variable on treatment indicators and control for the baseline value of the outcome variable when available (as well as all relevant strata and survey round fixed effects). We find no meaningful differences between both sets of results.

6 Benefits and External Costs

Combining our randomization and careful data collection allows us to credibly estimate several parameters that are essential for designing optimal policy. In this section we combine these estimates with a simple framework that allows us to estimate the benefits and external costs associated with a market-level change in the price of private travel. Some researchers have estimated that technological innovations such as autonomous driving could reduce the cost of private vehicle travel by 40-80% (Narayanan et al., 2020). In section 6.1 we explain how our experiment allows us to credibly estimate the change in welfare due to price changes such as these, and how they might differ across the population. In section 6.2 we develop a simple feedback model of congestion that allows us to translate our partial-equilibrium elasticities into general-equilibrium elasticities.[39] In section 6.3, we then use these equilibrium elasticities to estimate how total benefits and total external costs change in response to a 50% price reduction in the cost of private transport. Finally, in section 6.4 we explore the implications of our results on the impacts and incidence of a uniform tax on ride-hailing.

6.1 Benefits from Price Reductions

Our research design allows us to use experimentally-identified demand elasticities for travel on Uber services in a simple framework that allows us to recover estimates of the benefits in terms of compensating variation in income under minimal assumptions. Similar to above, we consider a representative agent that is choosing transportation technologies to maximize utility in Cairo's transport market using the following value function:

$$V(p) = \max U(q) + y \qquad \text{s.t} \qquad p.q + y \leq w \tag{3}$$

where y is a numeraire good and q are consumption goods. We assume that income effects are negligible.[40] The above utility representation can be thought of as denoted in monetary terms. This formula allows us to approximate (to second order) the benefits using a measure of the compensating variation in income associated with a price change

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[39] Our model allows riders to respond to changes in congestion at the market level, but does not take into account for larger responses in the economy in response to changes in transport prices, such as adjustment to housing and labor markets.

[40] Consumption shares for transportation are just 5.5-6.1% of the budget for households in Cairo (as seen in Appendix Table B3) suggesting that large income effects are unlikely.

for Uber services.[41]

$$\Delta V \approx \sum_{i=1}^{k} q_i p_i \Delta\% p_i + \sum_{i=1}^{k} \sum_{i=1}^{k} \Delta\% p_i \Delta\% p_j \varepsilon_{ij} q_i p_i \tag{4}$$

A key advantage of the current study stems from the fact that our intervention shifts the price of Uber services faced by participants in the experiment without affecting the prices of complementary or substitute modes in each participant's transportation consumption bundle. In the context of random assignment, we can assume that $\Delta\% p_{bus} = 0$, $\Delta\% p_{car} = 0$, $\Delta\% p_{metro} = 0$, etc., and represent the price treatments on Uber as $\Delta\% p_u = 25\%, \Delta\% p_u = 50\%$. The equation above, which would otherwise give rise to a very large number of cross-price elasticity parameters, reduces to the following simple expression:

$$\Delta V = q_u p_u \Delta\% p_u + \Delta\% p_u \Delta\% p_u \varepsilon_{uu} q_u p_u \tag{5}$$

This equation can be re-written to accommodate the experimentally-identified demand elasticities for travel on Uber services from Table 1.[42]

$$\Delta V = q_u p_u \Delta\% p_u (1 + \Delta\% p_u \varepsilon_{uu}) \tag{6}$$

Our experimental estimates indicate that 25% and 50% reductions in the price of ride-hailing services generate benefits of 49 EGP per week and 194 EGP per week for the average rider in each of the two respective treatment groups. However, we find substantial heterogeneity within each of the two treatment groups. Table 7 reports benefits calculations for different subpopulations in the 50% treatment group. Here we find that benefits to to the average car owner (131 EGP per week) are 33% lower and fall below the 95% confidence interval for the average rider [163-244 EGP] in the 50% treatment group. We do not find significant differences in the magnitude of benefits for the average female participant relative to the average male participant in the sample. However, we do find evidence of statistical and economically meaningful differences benefits for the female participants that are most affected by public transit safety in Cairo. Relative to benefits of 181 EGP/week received by the average female participant in the sample, female participants who ride the bus at baseline receive 255 EGP per week in benefits. Female participants that view public transit as unsafe receive 286 EGP per week. We estimate 450 EGP per week in benefits to female riders who report feeling unsafe on public transit *and* who ride the bus at baseline, which is more than twice as large as those accruing to the average respondent in the 50% treatment group. Benefits to male

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[41] Specifically, the envelope theorem gives $\nabla V(p) = q$, which differentiating a second time gives $\nabla V^2(p) = V_p q(p)$, which implies that the welfare effect of a moderate sized price change will be given by: $\nabla V \approx q^T \Delta p + \Delta p^T \nabla_p q(p) \Delta p$. If the first k prices are changed, then WLOG this yields $\Delta V \approx \sum_{i=1}^{k} q_i \Delta p_i + \sum_{i=1}^{k} \sum_{i=1}^{k} \Delta p_i \Delta p_j \frac{\partial q_i}{\partial p_j}$ or Eq. 4, which is expressed in terms of elasticties and percent price changes.

[42] We use the following estimates from Table 1: $q_u = 13.6$, $p_u = 5.07$, $\varepsilon_{uu-25\%} = 7.03$, and $\varepsilon_{uu-50\%} = 8.96$

participants who view public transit as unsafe or ride the bus at baseline are, if anything, smaller than those accruing to the average participant in the sample.

These results contribute three novel findings to the literature on the welfare impacts of reductions in the cost of transportation: (1) Several recent studies have indicated that reducing the monetary cost of transportation can improve the economic outcomes of mobility-constrained populations (Franklin, 2018, Bryan et al., 2014, Phillips, 2014). The estimates above provide a measure of welfare gain based directly on participant demand for total mobility and suggest benefits that are equivalent to 14% of the monthly income of the average participant in our sample. (2) This measure indicates that benefits accrue disproportionately to women in our Cairo sample. The same women report feeling unsafe on the primary mode of public transportation and have lower baseline levels of mobility than their male counterparts. This provides further evidence to support the general finding that safe, low-cost transportation services dramatically improve the welfare of mobility-constrained populations and the body of emerging evidence on the specific importance of mobility for women in developing countries (Kondylis et al., 2020, Jayachandran, 2019, Velásquez, 2019, Borker, 2018).

Third, we show how randomized pricing experiments in ride-hailing markets can produce elasticities that are useful for directly estimating the welfare effects of technological changes in transportation markets. Cohen et al. (2016) report that Uber in the US provides \$1.60 in surplus for every dollar spent on the service in 2015. Their strategy relies on using data from individual searches on the Uber app and a regression discontinuity framework that leverages variation form Uber's surge pricing strategies, which increases prices dynamically based on the demand and supply in a particular area & time. Our design allows us to use experimentally derived ride-hailing elasticities to estimate the private benefits under less restrictive assumptions. Estimates derived from times when there is a "surge" may be higher than estimates from normal operations because of the increased value of ride-hailing services than is typically the case (e.g. because it's raining, or there is a time-sensitive sporting event, etc).[43] It is also different from the variation we would observe from a market-wide experiment, where a change in price could affect road congestion and the effective prices of travel on outside modes (which we discuss in the following section).

6.2 Equilibrium Responses to Market-Level Price Reductions

Pricing experiments provide unique opportunity to isolate the price elasticity of travel demand in the absence of changes in congestion. But understanding how people will respond to *market-level* reductions is important for considering optimal policy design, as govern-

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[43] In contrast to the \$1.60 in surplus estimated in Cohen et al. (2016), applying a similar approach using our elasticity estimate of -1.17 would imply that every dollar spent in Cairo provides an additional \$0.43 in surplus. It is likely that differences in income and other characteristics of populations in U.S. markets versus Cairo also contribute to variation in these estimates.

ments grapple with how to respond to advances in transport technologies and growth in transport demand. In this section, we estimate how market-wide price reductions would compare to elasticities from a pricing experiment due to changes in congestion.

We focus on the equilibrium elasticity of demand for vehicle kilometers traveled (VKT) by considering the combined impact of changes in overall travel and substitution between modes in a model with endogenous congestion. Our simple model is motivated by theoretical and empirical findings indicating that a market-level price reduction would increase congestion, which would in turn exert downward pressure on demand. We use the demand elasticities from the experiment to calibrate the model, and estimates of rider value of time (VOT) to estimate equilibrium outcomes. In the next subsection, we use these equilibrium demand elasticities to study the implications of a market-level 50% price reduction on private benefits to consumers of ride-hailing services and the external costs produced by their travel.

A Simple Continuous Framework for Equilibrium Mobility

In the following framework, an agent maximizes their utility by choosing an optimal level of kilometers traveled on public transit and in private vehicles, subject to a constraint that their spending on these two goods does not exceed their transportation budget.[44] Public and private travel differ in the level of utility they provide to the agent, the cost of each unit (i.e. kilometer), and in their contribution to congestion (i.e. increases in private travel affect congestion more than increases in travel on public transportation.) These are given by the following expressions:

$$\max U(X_{priv}, X_{pub}) = (\omega X_{priv}^{\rho} + (1-\omega) X_{pub}^{\rho})^{1/\rho} \tag{7}$$

$$\text{s.t. } X_{priv} * P_{priv} + X_{pub} * P_{pub} \leq W$$

where X_{priv} represents the total kilometers traveled on private transportation (i.e. ride-hailing, taxi's, personal car), while X_{pub} represents total kilometers on public transportation (i.e. buses or rail). In our estimation, we assume that the price of Uber services approximates the price of all private travel after accounting for the costs of depreciation, gas, taxes, etc. As the price of private travel declines, we expect that individuals will increase their usage. We solve for optimal values by setting the price of public travel as

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[44] We assume that the budget share for transportation remains constant (but will relax this assumption later), and we assume CES utility and that changes in ridership levels do not affect safety perceptions. The model also assumes perfectly elastic supply, which may be a strong assumption depending on how the ride-hailing market evolves. If a technologically-induced price reduction results from the proliferation of autonomous vehicle capabilities, it may be the case that capital frictions become a more important constraint than the supply of drivers, which is discussed in current research about supply-side parameters in ride-hailing markets (e.g. Castillo (2019)). We assume that capital friction will be resolved in equilibrium, as the analysis of supply elasticities or capital frictions are beyond the scope of our experiment. To the extent that the supply response is not perfectly elastic, rider responses may be attenuated due to higher prices.

a numeraire and iterating along a wide range of values for the relative price of private travel. By calculating how the optimal X's change as the price of private travel changes, we can estimate the relevant price elasticity in this equilibrium. We estimate ω and ρ using elasticity estimates from the randomized experiment to calibrate the model in the partial equilibrium state where individual choices do not affect congestion.[45]

Next, we introduce congestion to the optimization problem by including a term that accounts for the effect of changes in travel choices on overall congestion and its associated costs. In particular, we expand equation 7 to the following expression:

$$\begin{aligned} \max U(X_{priv}, X_{pub}) = \psi * (\omega X_{priv}^{\rho} + (1-\omega) X_{pub}^{\rho})^{1/\rho} \\ - \gamma(X_{priv}, X_{pub}) * (X_{priv} + X_{pub}) * VOT \end{aligned} \tag{8}$$

The congestion function, γ, determines the percent change in congestion relative to the base condition with no change in prices. We first consider a linear case:

$$\gamma(\cdot) = (1 + \alpha \cdot \left(\frac{X_{priv} - X_{priv_0}}{X_{priv_0}} \right) + \beta \cdot \left(\frac{X_{pub} - X_{pub_0}}{X_{pub_0}} \right)) \cdot S \tag{9}$$

The first term captures the percent change in private kilometers traveled multiplied by a congestion weight (α) that represents the contribution of a kilometer of private travel to road congestion (we normalize α to one). The second term captures the percent change in public travel multiplied by a congestion weight (β) that captures the contribution of a kilometer using public travel to road congestion. We use a base value of $\beta = 0.2$ from Authority (2017), which implies that congestion impact of the average kilometer of travel using public transit is 20% as large as the effect of an additional kilometer made using private transport.[46] We then multiply this weighted change in congestion by S, which reflects the share of the population that uses ride-hailing services. We treat the linear congestion function as our primary specification given the evidence in favor of this assumption as found in Kreindler (2020), and since small changes are often given a linear approximation. We also consider a non-linear specification.[47]

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[45] We estimate the partial equilibrium elasticity of private vehicle kilometers traveled by combining our data on total distanced traveled and our data on the proportion of travel taken on private modes for each survey observation from a given participant. By multiplying these two measures, we recover a measure of total distance traveled on private modes. We then regress those measures on treatment (in Appendix Table B6) and use those coefficients to estimate the elasticity. The estimated partial equilibrium elasticity is -1.2. This differs from the elasticity of overall mobility (approximately -1), illustrating the importance of directly accounting for mode substitution when analyzing of the impact of price changes on private vehicle travel.

[46] Values of $\beta = 0.2$ normally range from 0.15 to 0.3 in the transportation literature (Authority, 2017) and our results are not very sensitive to using higher or lower estimates from this range.

[47] We consider a quadratic expression as follows: $((\left|\alpha \cdot \frac{X_{priv} - X_{priv_0}}{X_{priv_0}} + \beta \cdot \frac{X_{pub} - X_{pub_0}}{X_{pub_0}}\right| + 1)^2) \cdot S$, multiplied by the sign of the expression between the absolute value bars. Another non-linear expression follows the BPR functions that are widely used in the transport engineering literature (e.g. Geroliminis and Daganzo (2008)). They often take a form such as $1 + \alpha(\Delta X)^{\beta}$, where α and β are empirically estimated from traffic data. We find that at conventional values of these two parameters, using our quadratic expression

The other elements are: (1) the total amount of travel ($X_{priv} + X_{pub}$), since a 10% increase in congestion makes *all* travel 10% longer, and (2) VOT: the value of time, which represents an individual's private cost of additional time per kilometer spent in travel.[48,49] All together, the intuition in the case of a linear congestion function is that if a price decrease leads to a 20% increase in travel by those that use ride-hailing, and 30% of the population use ride-hailing, then *all* travel becomes 6% longer. This additional congestion decreases utility based on the value of time, which we estimate to be either 75% of hourly wage in line with Goldszmidt et al. (2020) or 150% of hourly wage in line with Parry and Timilsina (2015). Finally, we include a scaling parameter ψ, which makes the benefits of the first term of the utility function comparable to the monetary costs of the second term.[50]

Table 8 reports elasticity estimates using the parameter values described above as well as testing sensitivity to the model's three primary assumptions: (1) the shape of the congestion function (linear vs. quadratic), (2) the share of the population that utilize ride-hailing services ($S = 0.2, 0.3, 0.4$), and (3) the value of time ($VOT = 75\%$ or $VOT = 150\%$ of the hourly wage).[51] Relative to a partial equilibrium elasticity of -1.2, we find that the equilibrium elasticity can be dampened to a value of -0.85 or to as much as -0.17 in the case of quadratic congestion and a high value of time. Our preferred specification assumes a linear congestion function (as shown in Kreindler (2020)), a value of time equivalent to 75% hourly wage (as estimated in Goldszmidt et al. (2020)), and assumes that the proportion of the population using ride-hailing grows from a baseline of 20% at the time of the study to 30% with a 50% price reduction (Reuters, 2018).

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provides larger increases in congestion, implying that our estimates are conservative upper bounds of the costs.

[48] A kilometer traveled on public transit takes about 30% longer than on private travel, based on our data collected from participants on their counterfactual expectations of travel time on the longest trip they took yesterday. We can adjust for this by multiplying X_{pub} by 1.3. On the other hand, there is evidence that congestion affects private travel more than public travel, because, for example, a significant portion of the time on a bus is spent stopping, which is not as affected by congestion (Nguyen-Phuoc et al., 2018, Akbar and Duranton, 2017). When we include these considerations we find that equilibrium elasticities increase slightly.

[49] We do not model the impact that increased congestion has on the population that do not utilize ride-hailing services. Since they don't benefit from the price decrease, their travel should decrease in response to the additional congestion, which would soften the feedback loop, implying that our estimates are conservative upper bounds of the dampening effect. Research has also suggested that autonomous vehicles would have a lighter traffic footprint than normal cars which would further soften this dampening effect (Bagloee et al., 2016).

[50] We estimate this parameter by by setting gamma equal to one in the partial equilibrium case and then finding the triple of (ψ, ρ , ω) that allows us to recover the elasticities from our experiment. By setting gamma equal to 1 in the partial equilibrium case, we abstract away from concerns related to the capacity and relative use of different roads, and assume that congestion responds in relation to baseline congestion.

[51] We also consider the implications of the price reduction on increases in the share of income spent on travel (i.e. increasing W in the budget constraint), and find that it further softens the feedback loop, though the overall effect of changes in W on equilibrium outcomes is small. We conservatively assume that expenditure shares remain constant.

Under these assumptions, we recover an equilibrium elasticity of -0.74, implying a 38% dampening relative to the partial equilibrium estimate of -1.2.

6.3 Private Benefits and External Costs in Equilibrium

We are able to use the equilibrium elasticity of private vehicle kilometers traveled to estimate the external costs associated with the change in travel behavior using the following expression:

$$\alpha_{eq} = \alpha_0 * h(\Delta X_{priv}, \Delta X_{pub}) \tag{10}$$

External costs are a function of the baseline cost of congestion in Cairo (α_0) and changes in the quantities of public and private travel. A comprehensive World Bank study of transport externalities in Cairo estimates a total cost that is equivalent to 47 billion EGP ($10.9B PPP), which was 3.6% of Egypt's GDP in 2010 (Nakat et al., 2014, 2013). The report carefully characterizes 10 different dimensions of congestion costs including travel time delay, reliability, excess fuel consumption, excess CO_2 emissions, road safety, and suppressed demand. We scale this estimate by the increase in congestion, varying the increase based on the elasticities reported in Table 8, which capture the range of assumptions described above.[52] In the case with linear congestion, a value of time that is equivalent to 75% of the median wage, and where 30% of the population use ride-hailing, a 50% price reduction results in a 10.7% increase in overall congestion.[53] We then multiply this overall change in congestion by the the baseline cost of congestion for Cairo (α_0) and examine sensitivity to the range of values of time assumed in our model. This generates an estimate of an increase in external costs of $3.3B PP, or 1% of Cairo's GDP. Table 8 reports estimates for the full range of parameter values.

In Table 8, we also calculate and report the change in consumer welfare that comes from a market-level change in the price of ride-hailing. We follow the same strategy as described in section 6.1 above, and calculate the welfare increase using the equilibrium elasticities for each of the different combinations of parameters. We then extend these benefits for the entire share of the population that uses ride-hailing services and transform the full amount into percent of Cairo GDP to make them easily comparable to the estimates of external costs.[54]

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[52] Since this baseline estimate of costs includes the costs of time delays due to congestion and our model of equilibrium elasticities incorporates these adjustments, we construct the costs of congestion separately for ride-hailing users and non-users to avoid double-counting the costs of time delays. The report assumes a value of time of approximately 75% of average hourly income, and so we adjust the estimated costs of the time delays up to 150% in specifications that assume higher values of time.

[53] This comes from the equilibrium elasticities of both private and public travel that we generate in the model. This represents the net effect of an 11.1% increase in congestion from private travel and a 1.8% reduction in public travel. Together this increases congestion by $11.1 - 0.2 * 1.8 = 10.7\%$ (assuming public travel contributes to congestion at 20% the rate of private travel, as above).

[54] When aggregating the welfare benefits we do so for all ride-hailing and taxi trips. If we extend the benefits of the price decrease to all private travel the welfare benefits approximately double.

Across all sets of parameter values we find evidence that a technology-induced price change would provide considerable benefits to consumers but also lead to a substantial increase in external costs.[55,56] While the welfare benefits from a technology-induced price reduction exceeds the external costs, the surplus would be concentrated in the higher-income, higher-educated segment of the population that uses Uber.[57] The external costs, however, would be more evenly distributed across the population, given general effects on road users (including bus riders) and residents affected by pollution exposures. Hence, a technology-induced price reduction may be distributionally regressive.

6.4 Impacts and Incidence of a Uniform Tax on Ride-hailing

Governments around the world have begun using tax instruments to address the effects of ride-hailing services on society (World Resources Institute, 2020). As the demand for private transport increases in a context of technology-induced price reductions, taxation strategies will become even more important. Policy concern about (regressive) external costs may result in taxes on ride-hailing services. Our results point to a potential unintended consequence of standard taxation strategies. Suppose that a government were to place a uniform Pigovian tax on ride-hailing services in response to a technology-induced price reduction. A tax of this sort would reduce the mobility of female riders at a rate that is more than twice that of men (2.45x). If we assume that responses to price increases in the current pricing environment are symmetric to the responses to price reductions, then the implied incidence of a uniform tax would disproportionately reduce the welfare of female riders who find public travel to be unsafe. Importantly, this also suggest that the disparity in tax incidence could be managed through improvements in the safety of public transit services, since safety concerns help explain the differences between the demand elasticities of women and men. The combination of evidence across our treatments and data sources clarifies the need to address transport externalities while carefully considering distributional impacts by gender, and more generally by different subgroups of the population.

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[55] If the price-reduction was implemented through a government subsidy, it would no longer be welfare enhancing. The total monetary cost of a ride-hailing subsidy program would be equivalent to \$14.3 billion PPP, or 4.35% of Cairo's GDP, which does not take into account the increase in external costs. This calculation uses the following values: the equilibrium elasticity is -0.74, the average weekly KM traveled on ride-hailing and taxis at baseline is 59.6, the average cost of a kilometer is 6.2 EGP, the penetration rate is 0.3, the population of Cairo is 15.56 million, and the PPP conversion rate is 4.32.

[56] Note also that a 50% decrease in price would not be profit maximizing for Uber until technological advances change the cost structure of providing these types of rides. Similarly, even with our estimates for highly elastic demand, it is unlikely that Uber can increase profits by decreasing prices. Based on public statements at the time of the study, Uber was already operating at an average loss per ride served.

[57] Appendix Table B2 illustrates that Uber riders are likely to have higher incomes than the average Cairo resident.

7 Study Limitations

We identify five main study limitations: (1) sample size, (2) incomplete data on all travel locations during the study period, (3) measurement of longer-run impacts, (4) equilibrium effects on non-transport markets, and (5) generalizability.

While our study and data collection procedures were designed to ensure sufficient power to detect impacts on mobility, downstream impacts such as labor market outcomes are noisier and likely require larger sample sizes for precision. Future studies could secure and invest the additional funds necessary to provide subsidies to a larger sample.

We are also limited in our ability to fully characterize certain mobility choices. For instance, our overall mobility data cannot help determine whether price reductions lead to travel to new places or to the same places more often. Using trip-level data from Uber, we find that participants in treatment increase their Uber travel to new locations, but this does not guarantee that a participant would not have otherwise traveled to that location using a different mode of transportation. Future research designs might focus more on the geographic effects of price reductions by collecting detailed data on participant location during all times of the study. Of course, this comes at a cost to participant anonymity.

As is true of many studies of transportation behavior, the 3-month study period limits our analysis of impacts on margins that involve longer-run adjustments such as vehicle purchase decisions and residential location decisions.[58] Our experimental design also does not permit a comprehensive examination of the general equilibrium effects from price reductions on ride-hailing services for the full population of Cairo. A broader examination of effects that includes adjacent sectors like housing, education, and the labor market is an important area for additional research.

As with any study of a particular intervention or policy, we are limited in how broadly our results will generalize to other contexts. We do three things to address this. First, we consider the SANS conditions outlined in List (2020) and used in Holz et al. (2023). The SANS conditions help make comparisons across studies easier by describing selection, attrition, naturalness and scaling. In our case, our selection of Uber users provides a sample that is richer and younger than the general population (as shown in Appendix Table B2), but constitute a policy-relevant group for our exercises. Thanks to our survey incentives our attrition from the sample is low (as shown in Appendix Tables B8 & B9) and shows that there is no differential response by observable characteristics by treatment. Our intervention would score high on a naturalness scale, as a natural field experiment (Harrison and List, 2004) inside the Uber app makes it exactly the choice

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[58] We planned to follow up with the participants in our study 6 months after the onset of treatment to examine effects on longer-run outcomes from the 3 month treatment. While our 12-week treatments were effectively complete before the onset of the COVID-19 crisis (see Appendix J), the pandemic resulted in significant disruptions to travel behavior and survey capacity. We paused data collection for longer-term 6-month follow-ups that coincided with COVID-19, which was true for the majority of our sample, limiting what we can say about longer-run impacts on mobility.

setting of interest. Our model in Section 6.2 shows that scaling the intervention would dampen the effects due to increases in congestion, and we treat this as an important policy consideration.

Second, we compare Cairo to several other developing country megacities in Appendix Table B4. This helps us consider how preferences, beliefs and constraints may differ across contexts, such as in Nairobi, Bogota and Mexico City. We find that the combination of high levels of female harassment risk on public transit and high levels of public transit ridership that characterize Cairo are similar in several other large cities in the developing world. Finally, we designed and implemented a set of auxiliary experiments that test the importance of certain features of our experimental design. These experiments provide support for the conclusion that our estimated effects are driven by strong demand for mobility in Cairo, and not unique features of the experimental design. Future research could go further by implementing similar experiments in other contexts.

8 Conclusion

Ride-hailing services will continue to transform the transportation option set in cities around the world. When paired with careful data collection methods, digital platforms provide an opportunity for researchers and policymakers to more rigorously examine complex behavioral responses to shifts in the transportation sector and develop a basis for the design of evidence-based policy instruments. The present study provides evidence that in developing country cities like Cairo, individuals travel substantially more when the cost of ride-hailing services falls and they are not close to satiating their demand for mobility. These findings have important implications for researchers and policymakers, as they imply that improvements in transportation services could substantially increase urban mobility. They reinforce prior results from Duranton and Turner (2011), who find that expanding road capacity leads to a commensurate increase in travel.

Our estimates suggest that technology-induced price changes would yield large benefits to users as well as substantial external costs from increases in private vehicle kilometers. They also provide important evidence that the benefits of cheaper ride-hailing services may be pronounced for groups that face safety/harassment risk on outside options such as public buses. These benefits are concentrated among higher-income individuals that use ride-hailing services, while external costs would be borne by everyone who uses public roads or is affected by associated pollution. Tax instruments could be used to redistribute the gains more equally across society, though a uniform tax could reduce female mobility much more than it would reduce male mobility. Policymakers therefore need to anticipate the potential for substantial increases in utilization while also considering the nuanced distributional implications of price changes on population subgroups.

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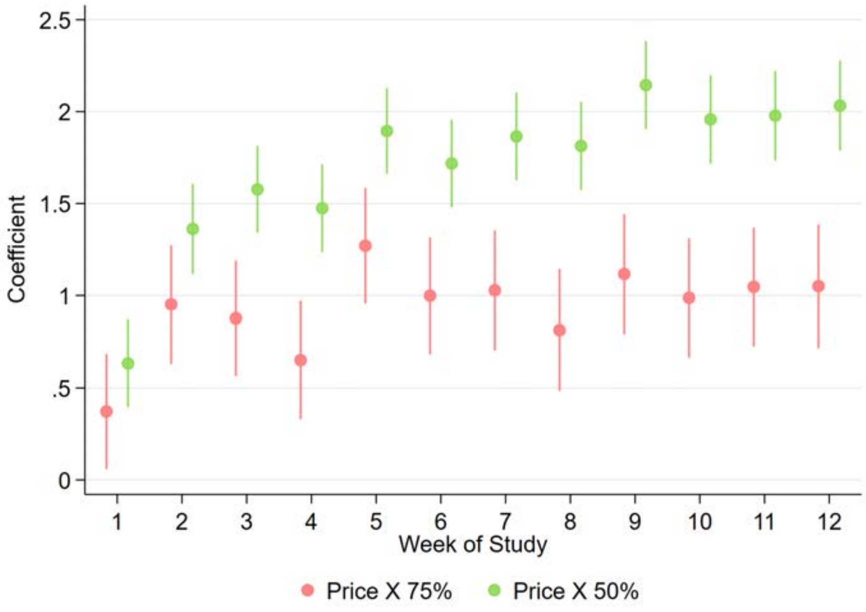
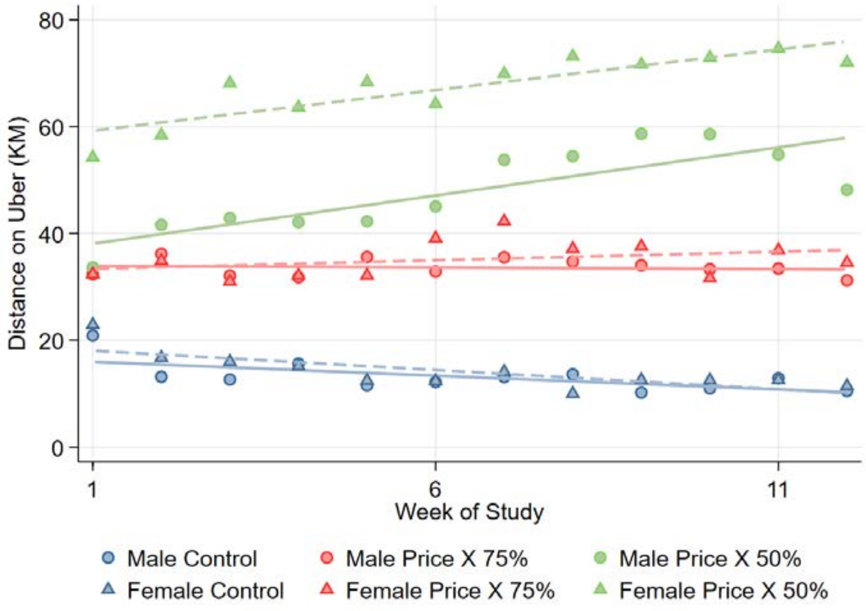
Figures

Figure 1. Uber Usage Across the Study Period

Treatment Effects by Week on Uber (IHS)



Average Weekly Travel: All Groups



Notes: Figure plots average weekly kilometers traveled on Uber. The upper panel split reports weekly treatment effects by treatment group, with effects estimated relative to participants in control and vertical lines representing 95% CI using standard errors clustered at the individual level. The bottom panel plots kilometers traveled on Uber by experiment group, split by gender. The y-axis is reported using nominal kilometers, and the x-axis is the week of the study.

Tables

Table 1. Impacts of Uber Subsidies on Uber Utilization

Panel A: Experimental Impacts

	Weekly KM on Uber (IHS)		Weekly Trips on Uber	
	(1)	(2)	(3)	(4)
Price X 75%	1.01*** (0.08)	1.11*** (0.11)	1.76*** (0.15)	1.96*** (0.21)
Price X 75% * Male		-0.18 (0.15)		-0.35 (0.30)
Price X 50%	1.70*** (0.08)	1.85*** (0.12)	3.66*** (0.20)	4.12*** (0.31)
Price X 50% * Male		-0.27* (0.16)		-0.84** (0.41)
Observations	16440	16440	16440	16440
Control Group Mean Levels	13.6	14.1	1.5	1.6
Control Group Mean Levels (Male)		13.2		1.5

Panel B: Estimated Elasticity

	Weekly KM on Uber (IHS)			Weekly Trips on Uber		
	(1) Overall	(2) Female	(3) Male	(4) Overall	(5) Female	(6) Male
Price X 75%	-7.03 [-5.38 , -8.67]	-8.17 [-5.45 , -10.89]	-6.04 [-4.02 , -8.05]	-4.65 [-3.86 , -5.43]	-4.93 [-3.87 , -5.98]	-4.26 [-3.12 , -5.41]
Price X 50%	-8.96 [-7.23 , -10.67]	-10.74 [-7.83 , -13.65]	-7.63 [-5.58 , -9.67]	-4.85 [-4.33 , -5.37]	-5.20 [-4.46 , -5.94]	-4.49 [-3.80 , -5.19]

Notes: Panel A: Column (1) reports the impacts of the two treatment arms on the inverse hyperbolic sine of weekly kilometers traveled on Uber. Column (2) reports the results from a specification that interacts a dummy variable for men, showcasing the differential impact the treatments have for that subgroup. Columns (3) & (4) report the estimates from a regression on the weekly number of trips taken on Uber (in levels). The bottom rows of Panel A report the control means in levels for each group in Columns (1) & (3), and split the means by gender in columns (2) & (4). Regressions include strata, cohort and follow-up round fixed effects as well as controls chosen using a double-post-lasso procedure. Standard errors clustered at the individual level in parentheses. Significance: *.10; **.05; ***.01. Panel B: Elasticities are calculated using the standard transformation of the coefficients estimated in Panel A. Values in brackets are the 95% confidence intervals of the estimated elasticities.

Table 2. Experiments on the Length and Salience of the Price Reduction

	Long Experiment 1st Week		Preannounced Short Experiment		Unannounced Short Experiment	
	(1) Weekly KM	(2) Trips	(3) Weekly KM	(4) Trips	(5) Weekly KM	(6) Trips
Price X 90%			0.41*	0.38	0.44*	0.51
			(0.19)	(0.24)	(0.18)	(0.32)
Price X 90% * Male			-0.24	-0.21	-0.46	-0.35
			(0.25)	(0.33)	(0.26)	(0.45)
Price X 75%	0.29*	0.86***				
	(0.17)	(0.30)				
Price X 75% * Male	0.01	-0.12				
	(0.24)	(0.42)				
Price X 50%	0.65***	2.11***			0.77***	1.45***
	(0.17)	(0.37)			(0.19)	(0.36)
Price X 50% * Male	-0.07	-0.80*			0.04	0.79
	(0.24)	(0.47)			(0.27)	(0.56)
Observations	1370	1370	1000	1000	1500	1500
Control Mean (Levels)	22.9	2.6	13.4	2.0	20.4	2.2
Control Mean for Men	20.9	2.2	18.7	2.2	21.4	2.1

Notes: Columns (1), (3), & (5) report the impacts of the two treatment arms and their interactions with a male dummy variable, on the inverse hyperbolic sine of weekly kilometers traveled on Uber during the first week of the experiment, the pre-announced experiment and the unannounced experiment respectively.Columns (2), (4), & (6) report the same but with number of trips as the outcome variable. The bottom rows report the control means in levels and split by gender. Regressions include strata, cohort and follow-up round fixed effects as well as controls chosen using a double-post-lasso procedure in columns (1) and (2). Standard errors clustered at the individual level in parentheses. Significance: *.10; **.05; ***.01.

Table 3. Impacts on Total Mobility

Panel A: Experimental Impacts		
	Total KM Past Week (IHS)	
	(1)	(2)
Price X 75%	0.10	0.18
	(0.10)	(0.16)
Price X 75% * Male		-0.13
		(0.21)
Price X 50%	0.40***	0.55***
	(0.09)	(0.14)
Price X 50% * Male		-0.29
		(0.18)
Observations	3476	3476
Control Group Mean Levels	205.2	144.6
Control Group Mean Levels (Male)		261.0

Panel B: Elasticity w.r.t Price of Uber			
	Total KM Past Week (IHS)		
	(1) Overall	(2) Female	(3) Male
Price X 75%	-0.44	-0.84	-0.15
	[-1.33 , 0.46]	[-2.3 , 0.67]	[-1.22 , 0.92]
Price X 50%	-0.99	-1.47	-0.60
	[-1.52 , -0.46]	[-2.40 , -0.55]	[-1.21 , 0.02]

Panel C: Elasticity w.r.t Cost of Mobility			
	(1) Overall	(2) Female	(3) Male
Price X 75%	-1.81	-3.04	-0.75
	[-5.47 , 1.89]	[-8.33 , 2.43]	[-6.10 , 4.60]
Price X 50%	-3.62	-5.40	-2.31
	[-5.56 , -1.68]	[-8.82 , -2.02]	[-4.65 , 0.08]

Notes: Panel A: Column (1) reports the impacts of the two treatment arms on the inverse hyperbolic sine of total kilometers traveled in the three days prior to our follow-up survey as reported by Google Maps' "Timeline" feature. Column (2) reports the results from a specification that interacts treatment with a dummy variable for men. The bottom rows of Panel A report the control means in levels and split by gender in Column (2). Regressions include strata, cohort and follow-up round fixed effects as well as controls chosen using a double-post-lasso procedure. Standard errors clustered at the individual level in parentheses. Significance: *.10; **.05; ***.01. Panel B: Elasticities are calculated using the standard transformation of the coefficients estimated in Panel A. Values in brackets are the 95% confidence intervals of the estimated elasticities. Panel C: Elasticities are calculated using the standard transformation of the coefficients estimated in Panel A and the change in the cost of mobility for each group

Table 4. Impacts on Trips by Mode of Travel

Panel A: Number of Trips	All Modes		Metro		Bus		Taxi		Uber/Careem		Car	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Price X 75%	1.00	1.19	-0.05	-0.06	-0.15	-0.31	-0.09	-0.20	1.11***	1.11**	-0.11	0.54
	(0.68)	(0.89)	(0.21)	(0.29)	(0.52)	(0.71)	(0.13)	(0.20)	(0.35)	(0.52)	(0.52)	(0.61)
Price X 75% * Male		-0.40		0.04		0.35		0.16		0.06		-1.00
		(1.35)		(0.44)		(1.04)		(0.27)		(0.70)		(1.03)
Price X 50%	1.35**	1.50*	0.13	0.20	-1.51***	-1.80***	-0.30**	-0.34*	2.32***	2.42***	0.54	0.67
	(0.62)	(0.79)	(0.21)	(0.29)	(0.47)	(0.67)	(0.11)	(0.18)	(0.36)	(0.54)	(0.51)	(0.59)
Price X 50% * Male		-0.29		-0.12		0.48		0.08		-0.32		-0.21
		(1.22)		(0.42)		(0.95)		(0.23)		(0.72)		(0.99)
Observations	3465	3463	3463	3463	3463	3463	3463	3463	3465	3463	3463	3463
Control Group Mean	18.57	16.94	1.29	1.03	6.72	5.45	0.65	0.79	3.97	4.62	5.96	5.06
Control Group Mean (Male)		20.07		1.53		7.90		0.53		3.38		6.79

Panel B: Proportion of Trips	Metro		Bus		Taxi		Uber/Careem		Car	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Price X 75%	-0.00	-0.02	-0.03	-0.04	-0.01	-0.02*	0.06***	0.06*	-0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)
Price X 75% * Male		0.02		0.02		0.02		-0.00		-0.04
		(0.02)		(0.04)		(0.01)		(0.04)		(0.04)
Price X 50%	0.00	0.00	-0.10***	-0.11***	-0.02**	-0.02*	0.12***	0.12***	-0.01	0.00
	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)
Price X 50% * Male		0.00		0.02		0.01		-0.01		-0.01
		(0.02)		(0.04)		(0.01)		(0.04)		(0.04)
Observations	3133	3133	3133	3133	3133	3133	3133	3133	3133	3133
Control Group Mean	0.06	0.06	0.34	0.29	0.04	0.05	0.24	0.29	0.32	0.31
Control Group Mean (Male)		0.06		0.39		0.03		0.19		0.33

Notes: Panel A shows the coefficients from 5 regressions on the number of trips taken the previous day of our follow-up survey. Even numbered columns report the results from a specification that interacts treatment with a dummy variable for men. The bottom rows report the control means in levels, split by gender in even numbered columns. Panel B shows the coefficients from 5 regressions on a continuous outcome that show the proportion of trips taken the previous day of our follow-up survey. Proportion of observations decline in panel B because we do not use observations where individuals report not taking any trips. Even numbered columns report the results from a specification that interacts treatment with a dummy variable for men. The bottom rows report the control means in levels, split by gender in even numbered columns. Regressions include strata, cohort and follow-up round fixed effects as well as controls chosen using a double-post-lasso procedure. Standard errors clustered at the individual level in parentheses. Significance: *.10; **.05; ***.01.

Table 5. Impacts on Reported Safety on Recent Trips

	Feeling on Longest Trip Yesterday 5=Very Safe, 1=Very Unsafe		Feeling on Longest Trip Yesterday Standardized Variable	
	(1)	(2)	(3)	(4)
Price X 75%	0.06	0.17*	0.05	0.15*
	(0.06)	(0.09)	(0.05)	(0.08)
Price X 75% * Male		-0.22*		-0.19*
		(0.12)		(0.10)
Price X 50%	0.09*	0.20**	0.08*	0.17**
	(0.05)	(0.08)	(0.05)	(0.07)
Price X 50% * Male		-0.19*		-0.16*
		(0.11)		(0.10)
Observations	3182	3182	3182	3182
Control Group Mean	3.98	3.90	-0.04	-0.12
Control Group Mean (Male)		4.06		0.03

Notes: Column (1) reports the impacts of the two treatment arms on the reported level of safety felt during the longest trip taken by the individual during the day prior to the follow-up survey. Column (2) reports the results from a specification that interacts treatment with a dummy variable for men. Column (3) reports the impacts of the two treatment arms on the standardized reported level of safety felt during the longest trip taken by the individual during the day prior to the follow-up survey. Column (2) reports the results from a specification that interacts treatment with a dummy variable for men. The bottom rows report the control means in levels, split by gender in Column (2) & (4). The bottom rows report the control means in levels, split by gender in even numbered columns. Regressions include strata, cohort and follow-up round fixed effects as well as controls chosen using a double-post-lasso procedure. Standard errors clustered at the individual level in parentheses. Significance: *.10; **.05; ***.01.

Table 6. Effect on Baseline Bus Riders

Panel A:Weekly Uber Usage (KM)

	Weekly KM on Uber (IHS)			Weekly KM on Uber (IHS) Perceive Bus as Unsafe		
	(1) Overall	(2) Female	(3) Male	(4) Overall	(5) Female	(6) Male
Price X 75%	1.10***	1.11***	1.08***	1.03***	1.20***	0.81***
	(0.09)	(0.14)	(0.12)	(0.15)	(0.20)	(0.22)
Price X 75% * Bus User	-0.32**	-0.08	-0.47**	-0.39	-0.44	-0.07
	(0.16)	(0.23)	(0.22)	(0.34)	(0.41)	(0.48)
Price X 50%	1.70***	1.69***	1.70***	1.55***	1.67***	1.28***
	(0.10)	(0.14)	(0.13)	(0.14)	(0.19)	(0.21)
Price X 50% * Bus User	0.02	0.60***	-0.36	0.04	1.26***	-0.49
	(0.17)	(0.23)	(0.22)	(0.31)	(0.47)	(0.40)
Observations	16440	7272	9168	6012	3336	2676
Control Group Mean Levels	25.5	25.7	25.4	25.9	27.5	23.5
Control Group Mean Levels (Bus User)	13.4	14.0	13.1	12.6	6.2	15.6

Panel B:Total Mobility (KM)

	Total Mobility (KM) in Past Week (IHS)			Total Mobility (KM) in Past Week (IHS) Perceive Bus as Unsafe		
	(1) Overall	(2) Female	(3) Male	(4) Overall	(5) Female	(6) Male
Price X 75%	0.09	0.20	-0.05	-0.01	-0.03	0.09
	(0.12)	(0.19)	(0.16)	(0.18)	(0.25)	(0.25)
Price X 75% * Bus User	0.09	0.09	0.06	0.84*	0.44	0.70
	(0.22)	(0.35)	(0.28)	(0.36)	(0.72)	(0.44)
Price X 50%	0.37***	0.59***	0.16	0.28	0.47*	-0.13
	(0.11)	(0.16)	(0.16)	(0.16)	(0.20)	(0.27)
Price X 50% * Bus User	0.03	-0.18	0.16	0.62	0.33	0.55
	(0.20)	(0.31)	(0.24)	(0.34)	(0.70)	(0.42)
Observations	3476	1666	1810	1313	780	533
Control Group Mean Levels	218.8	142.3	303.7	223.4	158.3	333.5
Control Group Mean Levels (Bus User)	176.3	151.3	191.7	147.3	122.6	160.2

Notes: Panel A: Columns (1), (2), & (3) report impacts on the inverse hyperbolic sine of weekly kilometers traveled on Uber in a specification that interacts the treatment with a dummy variable that takes the value of 1 if the individual reports at baseline that the longest trip took in the previous day was using a bus and 0 otherwise. Columns (4), (5), & (6) in panel A report the result for a specification that includes only people who perceived the bus as unsafe in the baseline survey. Panel B reproduces the same regressions but with total kilometers traveled as the outcome variable. The bottom rows in each panel report the control means in levels, split by if they were bus users at baseline. Regressions include strata, cohort and follow-up round fixed effects as well as controls chosen using a double-post-lasso procedure. Standard errors clustered at the individual level in parentheses. Significance: *.10; **.05; ***.01.

Table 7. Private Benefits from 50% Price Reduction on Uber

	All	Car Owner	Public Unsafe	Bus Riders	Bus Riders Unsafe
Overall	193.77	131.14	199.73	142.69	132.70
	[163.19 , 224.41]	[91.00 , 171.27]	[153.00 , 246.26]	[103.35 , 182.17]	[53.08 , 212.32]
Men	213.18	125.56	127.35	100.35	80.66
	[154.49 , 261.88]	[75.88 , 175.23]	[83.35 , 171.15]	[63.73 , 136.84]	[24.30 , 137.37]
Women	180.72	158.91	286.46	254.50	450.19
	[142.25 , 219.00]	[84.52 , 233.31]	[199.64 , 373.29]	[156.32 , 352.81]	[80.15 , 820.33]

Notes: Upper panel shows the estimates of welfare change when there is a 50% reduction of Uber Price. Confidence intervals of the estimates at 95% are in square brackets. Bottom panel shows the average income by subcategory.

Table 8. Private Benefits and External Costs from a 50% Price Reduction

Share of Population Using Ridehailing	Functional Form of Congestion	Equilibrium Elasticity of Private Travel	Individual Welfare Change (EGP/Week)	Population Increase in Welfare (% GDP)	Population Increase in External Cost (% GDP)
Value of Time = 75% of Median Wage					
0.2	Linear	-0.85	263	3.0%	0.8%
0.3	Linear	-0.74	253	4.3%	1.0%
0.4	Linear	-0.64	243	5.5%	1.1%
0.2	Quadratic	-0.50	231	2.6%	1.1%
0.3	Quadratic	-0.38	220	3.7%	1.2%
0.4	Quadratic	-0.30	212	4.8%	1.1%
Value of Time = 150% of Median Wage					
0.2	Linear	-0.65	245	2.8%	0.9%
0.3	Linear	-0.54	235	4.0%	1.1%
0.4	Linear	-0.46	227	5.1%	1.2%
0.2	Quadratic	-0.30	212	2.4%	0.9%
0.3	Quadratic	-0.22	205	3.5%	1.0%
0.4	Quadratic	-0.17	201	4.5%	0.9%

Notes: Top panel shows the estimates of welfare change when there is a 50% reduction of Uber Price but no affects from congestion. The second panel estimates the equilibrium elasticticy of private travel using the model in section 6.2 assuming that the value of time is equal to 75% of median wage. The bottom panel recalculates the elasticity assuming a value of time equal to 150% of median wage. These elasticites are then used to calculate the change in welfare the population of ridehailing users, and external costs for all road users.