The Impact of Crime on Public Transportation Demand: Evidence from Six Latin American Capitals*

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Abstract

Public transportation systems are central to reducing traffic congestion and urban pollution, yet in high-crime cities, safety concerns may undermine their use. This paper studies how crime affects the demand for public transportation, using three pre-registered experiments with over 5,000 participants across six Latin American capital cities. First, we find that users place a high value on safety in public transportation: reducing crime is valued at over 50% of the fare. Second, crime reduces the likelihood of choosing public transport over private alternatives, especially among women. For some users, even eliminating fares does not offset the deterrent effect of higher crime. As a result, crime lowers the responsiveness of public transport demand to fare changes, limiting the effectiveness of subsidies in increasing ridership in high-crime settings. Finally, we find that in a budget allocation task, safety concerns do not reduce support for environmental goals, suggesting room for coordinated interventions. Overall, our findings show that crime creates a negative externality on congestion and pollution by limiting shifts to cleaner modes of transport. They underscore the importance of integrating public safety into transport and environmental policy, and highlight the potential for aligning safety and sustainability objectives.

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1 Introduction

Urban public transportation systems are central to efforts to reduce traffic congestion and urban pollution. In many countries, governments invest heavily in expanding and subsidizing public transport to encourage a shift away from private vehicles. However, in high-crime areas, safety concerns may undermine these investments by making public transport less attractive. When users perceive buses or trains as unsafe, they may opt for private alternatives to avoid exposure to crime. These decisions can generate negative externalities, including more congestion and emissions. While safety and environmental policy are typically treated as distinct agendas, their goals may in fact be complementary. Policymakers in many cities appear to recognize this, at least implicitly: recent years have seen a rise in safety-oriented transit interventions such as all-women train cars, increased police presence in stations, or improved lighting at stops. These responses suggest an underlying intuition that improving safety is not only a criminal justice objective, but also a condition for sustaining public transport use. Yet, despite these policy efforts, there is little systematic evidence on how much crime affects demand for public transport and the effectiveness of green transport subsidies.

While a number of case studies and travel surveys suggest that perceptions of crime influence transportation choices (Ceccato et al., 2022; Delbosc & Currie, 2012), most of the existing evidence is correlational and it neither directly quantifies the effect of crime nor distinguishes between possible mechanisms. In theory, crime can affect public transport demand through two distinct channels. On the extensive margin, individuals with strong safety concerns may exclude public transport from their choice set entirely, regardless of price or convenience. On the intensive margin, those who still consider public transport may require stronger incentives —such as lower fares or better service— to offset the disutility associated with crime. Both margins imply that crime may dampen the effects of fare subsidies, reducing the responsiveness of demand to price. Disentangling these mechanisms is important for effective policy design. For example, if crime mainly affects the extensive margin, then fare subsidies will do little to increase ridership among those deterred by safety concerns —implying that investments in security may be more effective. If instead the intensive margin dominates, fare reductions or service improvements may still be effective tools, even in high-crime areas. Observational data makes it difficult to separate these effects, since safety perceptions and fare exposure are endogenous to transport use. An experimental approach can overcome these challenges by exogenously varying perceived crime while holding other attributes constant, enabling a clean identification of the causal effect of crime.

To address these challenges, we conduct three pre-registered[1] online experiments with 5,160 participants across six Latin American capital cities: Bogotá, Buenos Aires, Mexico City (CDMX), Guatemala City (Guatemala), Lima, and Santiago de Chile. These cities combine high levels of crime with extensive, subsidized public transport systems, offering a relevant context to study the interaction between safety and environmental policy. In Experiment 1, participants choose between two public buses that differ in fare and crime rates, allowing us to estimate how much users are willing to pay to reduce crime exposure during their commute. In

[1] AEA RCT Registry (AEARCTR-0013745): https://www.socialscienceregistry.org/trials/13745

Experiment 2, we examine substitution between public and private modes —specifically, a public bus versus a private taxi— when crime levels in public transport vary. This design enables us to estimate both the direct effect of crime on public transport demand and its impact on price elasticity. In Experiment 3, we study how crime perceptions shape support for transport policies by randomly assigning participants to read real news articles that frame public transport as safe, unsafe, or neutral. After the priming, participants allocate a hypothetical budget across various policy alternatives, including crime reduction, fare subsidies, service improvements, and emissions abatement. By experimentally varying crime while holding other attributes constant, we identify the causal effects of safety on transport choices and policy preferences, offering clean estimates of willingness to pay, substitution patterns, and the interaction between crime and subsidy effectiveness.

We find that commuters place substantial weight on crime when making transport decisions, and are willing to pay a large premium for safer public transport. Using mouse-tracking, we reveal that participants consistently seek out information about crime and price before considering other trip attributes such as emissions or duration, suggesting that safety concerns are central to how users evaluate transport options. These behavioral patterns are consistent across cities and demographic groups, and have a direct correlate on how users value safety in public transport. In public-public transport choices, participants are willing to pay a premium equivalent to 51% of the current fare to reduce crime by 20%, and require a 61% discount to tolerate a 20% increase[2]. In public-private choices —where the alternative is a private taxi— a 50% improvement in safety in public transportation increases the fare at which participants are indifferent between the two options by 27% ($p-$value < 0.001). These valuations underscore that safety is a key driver of modal decisions.

Building on these valuations, we next examine how crime affects transport choice and the responsiveness of demand to price. When choosing between a private taxi and a public bus, participants are 29% more likely to choose public transport when crime levels are lower ($p-$value < 0.001). This effect, equivalent to a 15 percentage point shift in mode choice, underscores the extent to which crime levels depress public transport demand and push users toward more polluting private alternatives. We also find that crime moderates price sensitivity: higher crime rates reduce the price elasticity of demand by 0.13 ($p-$value < 0.001). This effect of crime is most notable at the extensive margin: the share of commuters who cannot be incentivized to use public transit through pricing doubles when crime rates are higher ($p-$value < 0.001). In other words, individuals become less responsive to price incentives in high-crime settings, limiting the effectiveness of fare subsidies. This pattern holds across respondents with varying levels of transit use and crime exposure, but is particularly pronounced among women, whose price elasticity declines by 0.16 compared to 0.11 among men. These differences align with prior evidence that women are more sensitive to safety concerns in public transport (Ouali et al., 2020). Taken together, these results suggest that crime not only lowers public transport demand directly, but can also undermines the power of pricing policies to shift behavior. By weakening the responsiveness of commuters to price incentives, crime

[2] In the study countries, the regular ticket fare costs, on average, USD 0.7 at the average official exchange rate of September 2024, when the survey took place. Specifically, the prices of an average bus fare in Port of Spain (Trinidad and Tobago), Kingston (Jamaica), Buenos Aires, Santiago, Lima, Bogotá, Guatemala and CDMX are approximately USD 0.45, USD 1.01, USD 0.3, USD 0.77, USD 0.93, USD 0.72, USD 0.65 and USD 0.25, respectively.

creates a second-best setting in which environmental subsidies might be less effective, highlighting the need for safety and environmental policies to be implemented as complements rather than substitutes.

The idea that crime reduction and environmental policies can be complementary may seem counterintuitive. Prior research suggests that immediate concerns, such as safety, can crowd out attention and support for longer-term goals like environmental protection (Dechezleprêtre et al., 2025; Weber, 2010). In particular, individuals with a zero-sum view of policy priorities might reduce their support for pro-environmental measures when crime is more salient. To test this idea, we randomly assign participants to read a real news article about public transport in their city that either does not mention safety, frames it as safe, or frames it as unsafe. We measure perceived victimization risk before and after the priming, and then ask participants to allocate a fixed transport budget across four policy areas: crime reduction, fare subsidies, service frequency, and emission abatement. Using the randomized priming as an instrument for crime perception, we find that higher perceived crime increases support for safety investments but does not reduce support for emission reduction. Even under a binding resource constraint, participants do not perceive crime and environmental goals as competing. These results suggest that the public might be open to policy frames that present crime reduction and climate action as mutually reinforcing objectives, rather than substitutes.

Related Literature. — This paper contributes to the broad literature on travel mode choice initiated by McFadden (1974) and Ben-Akiva and Lerman (1985). While many studies have examined how pricing can boost transit ridership and reduce congestion (Almagro et al., 2024; Anderson, 2014; Parry & Small, 2009), often relying on estimates of price elasticity (Davis, 2021), the role of crime in shaping transit demand and pricing effectiveness has received little attention. Yet safety concerns are likely to be central to mode choice decisions, especially in high-crime settings such as Latin America. This is supported by case studies and local surveys: Börjesson (2012) and Holmgren (2007) highlight the importance of safety in stated preference studies, while Delbosc and Currie (2012) show that perceived safety correlates positively with ridership in Melbourne, and Ingvardson and Nielsen (2022) find that this relationship is particularly strong for women in Copenhagen. However, these studies rely on correlations and cannot isolate causal mechanisms. Our experimental design provides exogenous variation in crime levels and perceptions, allowing us to identify the effects of crime on both demand and price elasticity, across a large sample from six different countries.

Choice and survey experiments have proven particularly valuable in eliciting valuation of public goods and policy preferences (see Haaland et al. (2023) or Stantcheva (2023) for recent reviews). In the context of crime, Domínguez and Scartascini (2024) use a similar methodology to estimate willingness to pay for safety improvements in the Americas. Participatory budgeting approaches, which similarly ask individuals to allocate public resources, have also been used to reveal policy preferences (Ardanaz et al., 2023; Banerjee et al., 2010; Olken, 2010). While some have raised concerns about the external validity of experimentally revealed preferences, there is growing evidence that such measures predict real-world behavior (Dechezleprêtre et al., 2025; Funk, 2016; Hainmueller et al., 2015). Our work contributes to this literature by using experimental variation to uncover how transit users respond to changes in crime and price, revealing how safety policies can complement environmental policies.

More broadly, this paper contributes to a growing literature on policy complementarities and the institutional conditions under which policies are effective. Recent work shows that strengthening state capacity and combining complementary interventions can substantially increase policy effectiveness at relatively low cost (Ganimian et al., 2024; Mbiti et al., 2019; Muralidharan et al., 2016, 2021). Ignoring these complementarities can lead to policy failures or unintended consequences. In the context of transportation, Bento et al. (2014) show that environmental policies can backfire when interactions across externalities are overlooked. Our findings align with this perspective: when crime is high, public transport becomes less attractive and commuters become less responsive to fare reductions. By documenting how crime weakens the effectiveness of environmental policies, which is the main predictor of public support for these policies (Dechezleprêtre et al., 2025), our findings underscore the importance of coordinated interventions that address both safety and climate objectives.

The rest of the paper is structured as follows: Section 2 offers a simple conceptual framework of the externalities crime has over public transport. Section 3 details the experimental design, providing a comprehensive overview of the sample and the methodologies employed across the three experiments. Section 4 details the empirical analysis. Section 5 reports the results of each experiment and their implications. Finally, Section 6 offers a discussion of the broader significance of our results for urban transport and sustainability policies.

2 Conceptual Framework

We develop a simple framework to illustrate two channels through which crime impacts public transport demand. We model the commuter's decision as a discrete choice problem, where agents select a transport mode from a set of alternatives. Crime influences this decision through two channels. On the extensive margin, each commuter has an individual safety threshold: only transport modes with crime levels below this personal threshold enter their consideration set. On the intensive margin, commuters then compare the relative safety of the transport options within their consideration set, factoring crime levels into their final choice.

Let commuter i choose between modes $j \in \mathcal{J}$ to travel from a given origin o to a given destination d. We focus on the set I_{od} of agents that commute from o to d and have to decide which transport mode to use. Thus, agent i chooses the transport mode j that maximizes her utility. Let p_j, s_j, and x_j denote the price, safety level, and 'other' characteristics of mode j, respectively. The commuter solves the following problem:

$$\max_{j \in \mathcal{J}} u_{ij}$$

Let the utility of not completing a trip be normalized to 0. We assume that each agent i needs a minimum safety level $\bar{s}_i$ to even consider a transport mode among the alternatives and that the mode utility function follows a random utility model (Ben-Akiva & Lerman, 1985; McFadden, 1974). That is, there is a deterministic component that depends on observable mode characteristics and an additively separable component ε_{ij} that depends on unobserved characteristics that vary at the individual level and have an extreme value type 1 distribution. Formally:

$$u_{ij} = \begin{cases} g(p_j, s_j, x_j) + \varepsilon_{ij} & \text{if } s_j > \bar{s}_i \\ 0 & \text{if } s_j \leq \bar{s}_i \end{cases} \tag{1}$$

Thus, the agent only considers those modes j above the safety threshold $\bar{s}_i$. Define the *consideration set* $\mathcal{C}_i \subseteq \mathcal{J}$ as the set of modes considered by agent i. That is:

$$\mathcal{C}_i = \{j \in \mathcal{J} : s_j > \bar{s}_i\}$$

Then the *conditional* choice probability of mode j among the consideration set will be given by

$$P_{ij|\mathcal{C}_i} = \frac{\exp(g(p_j, s_j, x_j))}{\sum_{k \in \mathcal{C}_i} \exp(g(p_k, s_k, x_k))} \tag{2}$$

We allow for heterogeneity in the safety threshold among commuters, with $F(\bar{s})$ being the distribution of safety thresholds in the population. For simplicity, consider the case where $\mathcal{J} = \{private, public\}$, where *private* correspond to private modes (e.g., taxi or private car) and *public* corresponds to public modes (e.g., bus or train). Given that ε_{ij} are random draws across the population, we can map individual probabilities to the aggregate demand for mode P_j. Importantly, if $s_{private} > s_{public}$, the share of the population that chooses *public* modes for a given safety level in public modes s_{public} is:

$$P_{public} = F(s_{public}) \cdot P_{public|\mathcal{C}_i} \tag{3}$$

Equation (3) highlights two channels through which crime affects public transportation demand. First, on the extensive margin, higher crime can lead some commuters to exclude public transport from their choice set entirely, thereby reducing overall demand. Second, on the intensive margin, among those who still consider public transport, higher crime lowers its relative attractiveness compared to private alternatives. Both mechanisms reduce the effectiveness of price subsidies: on the extensive margin, price changes are irrelevant to individuals who do not consider public transport; on the intensive margin, those who do consider it will require lower prices to offset the disutility from crime.[3] Therefore, considering both margins, we expect demand for public transport to increase as safety improves. However, the impact on price elasticity is, in this context, ambiguous. On the one hand, greater safety expands the number of commuters who consider public transport as part of their choice set. Therefore, changes in prices will affect a greater number of people, scaling the effect. On the other

[3] Suppose $g(p, s, x)$ is decreasing in p and increasing in s. To keep the choice probability $P_{ij|\mathcal{C}i} = \frac{\exp(g(p_j,s_j,x_j))}{\sum k \in \mathcal{C}_i \exp(g(p_k,s_k,x_k))}$ constant when s_j decreases, we must hold $g(p_j, s_j, x_j)$ constant. Taking the total derivative: $0 = \partial g/\partial p \cdot dp_j + \partial g/\partial s \cdot ds_j$. Since $\partial g/\partial p < 0$, $\partial g/\partial s > 0$, and $ds_j < 0$, it follows that $dp_j < 0$. Hence, p_j must also decrease.

hand, as public transport becomes more attractive due to reduced crime, its elasticity of substitution decreases. Ultimately, whether demand becomes more or less elastic with increased safety remains an empirical question.

3 Data and Experimental Design

In 2024, we partnered with the firm Offerwise to collect data across six Latin American capital cities: Bogotá, Buenos Aires, Mexico City (CDMX), Guatemala City (Guatemala), Lima, and Santiago de Chile[4]. Offerwise provides a sample of respondents, who were compensated for completing the online experiment[5]. Table A1 compares key observable characteristics between participants in our experiment and those from a representative survey sample.[6] We acknowledge that our sample differs somewhat from nationally representative surveys—most notably, participants tend to be slightly younger and more educated—so caution is warranted when extrapolating our findings to broader populations. Table A2 presents descriptive statistics for the overall subject pool. The sample consists of 52.6% males, with an average age of 36.2 years. On average, participants live with 2.9 other people, 96% have at least some secondary school education, and 52% have some university education.

Participants complete three experiments in the same order. The objective of Experiment 1 is to measure willingness to pay (WTP) for crime reduction in public transport. Participants are presented with the characteristics of two bus options (hereinafter, Bus A and Bus B): trip duration, emissions (in grams of CO2), price, and crime rates. We use a mouse-tracking design to reveal information relevance (Brocas et al., 2014). The attributes are initially hidden behind clickable tags, which participants have to click to reveal the information (see Figure B1). After revealing all attributes, participants select their preferred bus option. We record the order in which participants clicked each box as a non-choice measure of the importance they place on each attribute. Since the order in which attributes are displayed on the screen is randomized, deviations from a uniform distribution of look-ups indicate differences in the perceived value of the information.

In Experiment 1, participants are randomly assigned to one of four treatment groups, each exposed to a different crime rate for Bus B (while the crime rate for Bus A is fixed at the average rate for buses in their city). Specifically, the crime rate for Bus B is described as being 'X% (higher/lower) than the average crime rate in bus lines of your city,' where $X \in \{-30\%, -10\%, +10\%, +30\%\}$. Additionally, the price of Bus B also varies by treatment group. While all participants face the same price for Bus A (the standard bus fare in their city), those in the first two groups (-30% and -10%) see a price 50% higher than Bus A, whereas those in the latter two groups (+10% and +30%) see a price 50% lower. If a respondent chooses Bus A (or Bus B), they are asked to repeat the choice with the price of Bus B adjusted to 10% lower (or higher). This process

[4] The IRB and preregistration also include the intention of obtaining samples from Jamaica and Trinidad and Tobago, but logistic issues prevented data collection in these countries.

[5] Only participants between the age of 18 and 65 were eligible to participate.

[6] The national surveys are representative at the country level, but we restrict our comparison to respondents from cities included in our experiment, applying the nationally defined expansion factors. To compute the representative means, we used data from the last quarter of 2023 from the following national household surveys: the Encuesta Permanente de Hogares (EPH) in Argentina, the Encuesta de Caracterización Socioeconómica Nacional (CASEN) in Chile, the Gran Encuesta Integrada de Hogares (GEIH) in Colombia, the Encuesta Nacional de Empleo e Ingresos (ENEI) in Guatemala, the Encuesta Nacional de Ingresos y Gastos de los Hogares (ENIGH) in Mexico, and the Encuesta Nacional de Hogares (ENAHO) in Peru.

continues until the respondent reverses their initial choice or completes five iterations. The price that makes the respondent indifferent between the two options is calculated as the midpoint between the price at which they switched and the price in the previous iteration. The WTP for safety is then estimated as the difference between this indifference price and the price of Bus A (Gabor & Granger, 1964). This design also allows us to estimate price elasticity of demand for different levels of crime[7].

Experiment 2 follows a similar structure but involves a choice between a bus and a private taxi instead of two buses (see Figure B2). Again, respondents are randomized into four treatment groups. The crime rate for the private taxi is fixed at the average crime rate for taxis in their city, while the crime rate for the bus is described similarly to Bus B in Experiment 1, with $X \in \{-30\%, -20\%, +20\%, +30\%\}$. In this experiment, the initial prices of the alternatives do not vary between groups[8]. If a respondent chooses the bus (or private taxi), they are asked to choose again with the bus price adjusted to 20% higher (or lower). The process is repeated for up to five iterations, with the indifference price calculated in the same manner as in Experiment 1.

In Experiment 3 (see Figure B3), participants are randomized into three groups. The first group (hereinafter, *Dangerous* group) is exposed to a real newspaper headline referring the high levels of crime on the public transport system of their city; the second group (hereinafter, *Safe* group) sees a headline priming that public transport in their city is safe; the third group (hereinafter, *Control* group) sees a headline about public transport but unrelated to its safety. Participants are then asked to allocate a hypothetical budget of 120,000 USD across four public transportation policies: increasing bus frequency, cutting fares, reducing crime, and reducing CO2 emissions. To incentivize truthful responses, participants are informed that the study results will be shared with officials from their city's Department of Transportation, potentially influencing real policy decisions. Participatory budgets have been successfully used before to elicit policy preferences (Ardanaz et al., 2023; Olken, 2010). We also elicit participants' perceived probability of encountering crime in public transport, both at the beginning of the session (before Experiment 1) and after the news priming in Experiment 3.

4 Empirical Analysis

4.1 Valuation of Crime Reduction in Public Transport

The first goal of Experiment 1 and Experiment 2 is to estimate the willingness to pay for crime reductions in public transport. To this end, we estimate the following model:

$$y_i^* = \alpha + \beta_1 1\{Crime = +10\%\}_i + \beta_2 1\{Crime = -10\%\}_i + \beta_3 1\{Crime = -30\%\}_i + \Phi_c + \mathbf{X}\Delta + \epsilon_i \quad (4)$$

[7] There is a possibility that participants take prices as an additional signal of crime levels, believing that lower prices might incentivize more criminals into using public transportation. However, this might reduce the informativeness of our crime signals, making our estimates a lower bound.

[8] The private taxi price presented corresponds to a 20-minute Uber trip from the city hall on a weekday at 8 PM. The initial bus price is the standard bus fare.

Where $1\{Crime = X\%\}_i$ is an indicator function denoting whether respondent i was exposed to a X% crime variation treatment group. Φ_c correspond to city fixed effects, and in all specifications the vector of control variables ($\mathbf{X}$) are: age, gender and level of education. Note that for Experiment 2, we estimate the same model depicted in Equation (4) but we replace the treatment groups -10% and +10% for -20% and +20%. This specification follows closely that of Domínguez and Scartascini (2024).

We model y_i^* as a latent variable indicating the indifference price between the two transport mode alternatives following Berlinski and Busso (2016). Keep in mind that we do not observe the *real* indifference price, but instead an upper and lower bound, which are the price displayed in the last and the second to last iteration[9]. Each parameter corresponds to the difference in the estimated indifference price between groups, which is our measure of willingness to pay for a crime reduction from +30% (the omitted group variable) to the corresponding crime level denoted by the indicator variable.

The second goal of these experiments is to analyze the effect of crime abatement on environmental outcomes. In particular, we are interested in studying first, if safety improvements can induce substitution from private to public mode choices and second, if lower crime rates increases the effectiveness of subsidies. Therefore, in Experiment 2, we consider the following specification:

$$1\{ChooseBus_i\} = \alpha + \beta_1 1\{Crime = +20\%\}_i + \beta_2 1\{Crime = -20\%\}_i + \beta_3 1\{Crime = -30\%\}_i + \Phi_c + \mathbf{X}\Delta + \epsilon_i \tag{5}$$

Where $ChooseBus_i$ is an indicator function equal to 1 if respondent i chose the public transport over the private taxi on the first decision of the experiment. In this case, the parameters are interpreted as how more or less likely the average respondent will choose the bus over the private taxi, at current prices, for each treatment group (relative to the +30% group).

Second, we estimate how crime affects the price elasticity of demand. Specifically, we study the share of respondents who choose the treated alternative (Bus B in Experiment 1, and the bus in Experiment 2) for each given price and crime combination[10]. We use a linear probability model where we interact the price levels with the crime levels to test whether crime affects price elasticity, and include the same battery of controls as in the previous models. Thus, the coefficient of this interaction should be interpreted as how much the probability (share) of choosing the *treated* choice varies if the price increases by 100%.

4.2 Crime Perception and Crowding Out

Experiment 3 addresses whether crime perception crowds out support for environmental and other policies in the context of the public transportation. Previous research has found that more immediate concerns, like safety,

[9] For those respondents who never switched their decision we consider as the willingness to pay the last price displayed.

[10] Note that, since the experiment ends when participants switch choices, we don't observe a choice for each price, so we assume choice consistency. This is, if a participant chooses Bus B at some price, we can safely assume they would still choose it at a lower price.

affect environmental perceptions and support for green policies (González & Sánchez, 2022; Weber, 2010). In regions with high crime rates such as Latin America, concerns about safety may overshadow considerations for environmental impact or efficiency. To test this hypothesis, we explore whether crime perceptions (elicited before any news priming) are linked to differential support for public transport policies: increasing bus frequency, reducing ticket prices, reducing crime, and reducing CO2 emissions.

To check whether this relation is causal, we vary crime perceptions exogenously by exposing participants to different newspaper headlines. To test whether participants actually change their perceptions after the information provision, we elicit participants' perceived probability of being victim of a crime in public transport both before Experiment 1 and after being exposed to the newspaper headline, in line with the literature on belief updating (Andre et al., 2023; Cullen & Perez-Truglia, 2022). We estimate this first stage model:

$$\Delta CrimePerception_i = \alpha + \sum_j \beta_j 1\{Article_i = T_j\} + \Phi_c + \mathbf{X}\Delta + \epsilon_i \tag{6}$$

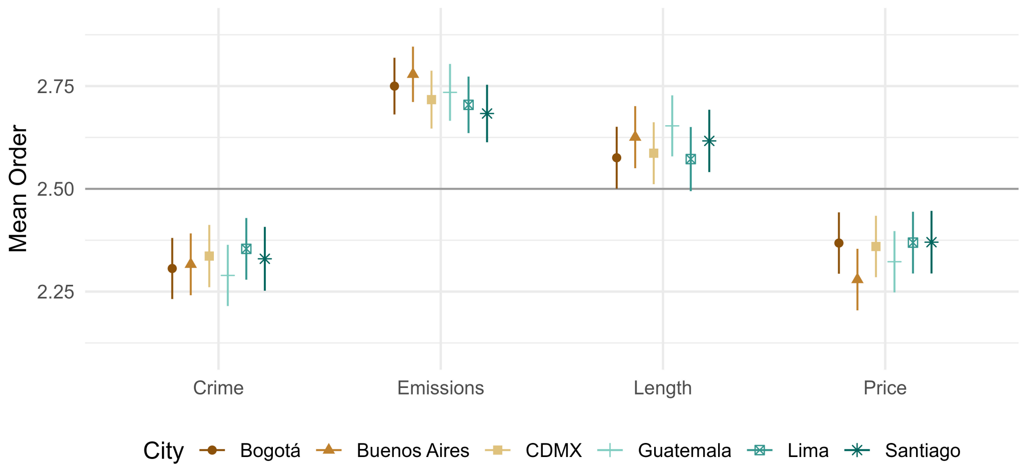
Here, $\Delta CrimePerception_i$ corresponds to the difference between the reported probability of being victim of a crime reported before and after the experimental intervention. $1\{Article_i = T_j\}$ indicates which news $T_j \in \{Dangerous, Safe\}$ participant i read. We control for the same set of variables as in Equation (4) and use city fixed effects.

After the experimental intervention, subjects were asked to allocate a budget of 120,000 USD among 4 policies[11]. To test whether crime perceptions affect policy support, we estimate the following 2SLS model:

$$y_i^k = \alpha + \beta_1 \Delta Crime\widehat{Perception}_i + \Phi_c + \mathbf{X}\Delta + \epsilon_i \tag{7}$$

where y_i^k is the share of the budget that was allocated to policy k. Recall that in this experiment we consider four policies: increasing bus frequency, reducing ticket fares, reducing crime and reducing CO2 emissions. For the sake of simplicity, we normalized the dependent variable to be the share allocated to policy k over the total money allocated. $\Delta Crime\widehat{Perception}_i$ is the fitted value obtained from estimating Equation (6). In this case, β_1 is interpreted as how many more percentage points of the budget are allocated to policy k if crime perception exogenously increases by 1 percentage point.

Figure 1. Mouse-tracking results



Notes: The figure shows the average look-up order for each attribute, where 1 means being clicked first, 4 last. Error bars show the 95% CI. The mean order if the look-ups were random (2.5) is shadowed.

5 Results

5.1 Valuation of Safety in Public Transport

5.1.1 Non-Choice Data

We present the results following the strategy defined in Section 3. Figure 1 presents the average order of clicks for each attribute (i.e., 1 if first, 4 if last). If each attribute had been looked up in random order, the mean order of each attribute would be 2.5. However, it varies by attribute: price and safety are looked up significantly earlier than emissions and trip length (p-value < 0.001), but we do not find a significant difference between the order in which price and safety attributes are looked up (p-value $= 0.151$). Participants seek information about price and safety earlier than pollution and trip length, indicating these attributes are more relevant for decision making. The relevance of crime and price is strikingly robust across countries.

Figure A1 presents further heterogeneity analyses. We find no gender differences in the order of look-ups, which might be a byproduct of women not expecting safety levels to vary across buses. The mean look-up order of the crime attribute is lower among respondents who have been victims of a crime on public transportation. This is not surprising since we expect respondents in this group to be more aware of crime risks than their counterparts. Moreover, we find a positive correlation between the age of the respondent and the look-up order of the emissions attribute, consistent with surveys that suggest that young people tend to care more about the environment (Gallup, 2018). Finally, according to our relative relevance measure, participants with higher education tend to prioritize less the information on crime and price and more the length of the trip when

[11] This is a slight variation to what's included in the pre-registration plan and in the IRB file. Originally, participants had to state how much they agreed with the budget being allocated to each policy on a scale from 1 to 10. We modified the decisions to be zero-sum to make the trade-offs between policies more salient.

deciding between transport modes. Overall, these results robustly show that, when choosing a transport mode, obtaining information about crime is as important as information about price, and these are more important than information about trip duration or emissions.

5.1.2 Choice Data

Experiment 1 estimates the willingness to pay for changes on crime in public transportation. To standardize the results across different cities, we normalize prices in terms of current bus fare units. For instance, if the price of Bus B is 20% higher than the current bus fare, we normalize this price as 1.2. We conduct a randomization balance check, which is presented in columns (2)-(4) of Table A2. The table shows that the only significant difference between treatment groups is whether the participant has ever been a victim of a crime on public transport. Consequently, we include this as a control variable in our analysis.

We begin by examining the mean indifference prices between groups. Table A4 shows that, when the alternative is an average bus in their city (Bus B), participants are willing to pay 37.8% of the current bus fare to ride a bus with a +30% crime rate (relative to the average crime rate on bus lines in their city), 39.7% for a bus with a +10% crime rate, 149.4% for one with a -10% crime rate, and 152.5% for one with a -30% crime rate. Notably, the effect is not linear. While participants are willing to pay 1.9% of the current bus fare to lower the crime rate from +30% to +10% (and similarly from -10% to -30%), they are willing to pay 110.1% to lower it from +10% to -10%. Figure A3 shows the similarity of results between variations of the same sign (i.e., between +10% and +30%), which is also apparent throughout the entire distribution of responses (CDF of indifference prices).

A potential explanation for this non-linearity is that participants may not understand probabilities, leading them to misinterpret changes in crime rates. To address this, we include a sanity check at the end of the survey, asking participants to report the probability of getting heads in a random coin toss—a standard test in experimental research (Stöckl & Gleissner, 2018). Figure A2 shows that most respondents answered correctly, though there is some variation in the responses. As a robustness check, Table A6 reproduces the analysis but only considering participants who answered the coin toss probability question correctly.[12] The magnitudes of the point estimates are similar in both cases, and the statistical power remains strong. Most importantly, even when considering only respondents who answered the coin toss question correctly, we still find no significant differences between crime treatment groups of the same sign.

This suggests that the observed phenomenon, known as scope insensitivity, is driven by behavioral attenuation (Enke et al., 2024), rather than by a fundamental misunderstanding of probabilities. Scope insensitivity is a phenomenon commonly observed in contingent valuation studies (Diamond & Hausman, 1994), even in valuation by experts (Toma & Bell, 2024). In our experiment, participants may interpret crime changes simply as "reductions" or "increases", rendering decisions insensitive to variations in magnitudes away from the zero

[12]Subjects answered this question using a continuous slider, which introduces some motor noise in the responses. We therefore allow for a 5% error margin.

boundary. Given the lack of significant differences between different magnitudes of positive and negative crime changes, for illustration purposes we decided to aggregate our treatment groups into ±20% instead of ±10% and ±30%. However, the coefficient should be interpreted as the average willingness to pay to reduce crime for +10% or +30% to -10% or -30% (relative to the average crime rate). We present the aggregated results in the text and include the disaggregated results in the Appendix (see Table A5).

Table 1. Reduced-form estimates of Experiment 1

	(1)	(2)	(3)	(4)	(5)	(6)
1(-20% Crime)	1.12***	1.12***	1.12***			
	(0.009)	(0.009)	(0.009)			
Crime % (Continuous)				-0.023***	-0.023***	-0.023***
				(0.0002)	(0.0002)	(0.0002)
Observations	5,161	5,161	5,161	5,161	5,161	5,161
Controls	No	No	Yes	No	No	Yes
City FE	No	Yes	Yes	No	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 1. $1(Crime = -20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -10% or -30%. *Crime % (Continuous)* is a continuous variable corresponding to the level of crime rate that the participant was exposed to. The vector of control variables considered are age, level of education, gender and whether the participant reported having being a victim of a crime in public transport. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Table 1 reports the regression estimates from Equation (4). In our preferred specification, which includes controls and city fixed effects, participants are willing to pay 112% of the bus fare to reduce the crime rate from +20% to -20% (p-value<0.001). Thus, users are willing to pay a premium of more than a bus fare to ride safer public transport. Although the effect is non-linear, for completeness, columns (4)-(6) of Table 1 present the estimates from Equation (4) using crime rates as a continuous variable. Consistent with the results in the discrete specification, we find that participants are willing to pay 2.3% of the current bus fare for a 1% reduction in the crime rate.

Table A7 shows the results by city. We register that the city with the highest willingness to pay for safety is Mexico City, while Lima and Santiago have the lowest —though all estimates exceed 100% of the current fare and are of a similar order of magnitude. Furthermore, Table A8 presents the heterogeneity of treatment effect by subsamples. In particular, we study whether treatment effects vary by reported crime perceptions, by gender, and by look-up order of the safety attribute. First, we find our results to be robust across different crime perceptions. However, females are willing to pay 6% (p-value<0.001) more than males to reduce crime. Furthermore, those who clicked safety first or second in the attributes look-up display a willingness to pay 5.7% (p=0.002) higher, confirming our mouse-tracking measure indeed captures attribute valuation.

5.2 Effect of Crime on Public Transport Demand

The main goal of Experiment 2 is to explore how crime affects the substitution between private and public transport. As detailed in Section 3, Experiment 2 closely follows the design of Experiment 1, but participants now choose between a bus and a private taxi (i.e., Uber). The initial prices of the two options do not vary by treatment group, which eliminates any anchoring effects and allows us to examine both the extensive and intensive margins of decision-making. Following the discrete choice model we present in Section 2, the extensive margin captures how crime rates in public transport influence the likelihood of choosing it over a private taxi (i.e., a discrete choice), while the intensive margin measures the price difference required to make respondents indifferent between the two alternatives, given different crime rates.

Table 2. Reduced-form estimates of Experiment 2

	Chose Bus			WTP		
	(1)	(2)	(3)	(4)	(5)	(6)
1(-25% Crime)	0.149***	0.150***	0.150***	0.273***	0.274***	0.273***
	(0.014)	(0.013)	(0.013)	(0.021)	(0.021)	(0.021)
Observations	5,161	5,161	5,161	5,161	5,161	5,161
Controls	No	No	Yes	No	No	Yes
City FE	No	Yes	Yes	No	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 2. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -20% or -30%. Columns (1)-(3) correspond to the extensive margin results and columns (4)-(6) to the intensive margin. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

The simple model presented in Section 2 differentiated several effects of crime on public transport demand. First, a higher crime level would increase the share of commuters who do not even consider public modes as a viable alternative. Results from Experiment 2 are consistent with this prediction: the fraction of people that would not choose the bus at any price increases from 13.9% to 25.2% when the bus alternative presents a crime level higher than the city average (p-value<0.001). Second, higher crime rates will decrease the appeal of public transportation, leading to a higher share of commuters choosing private options. Columns (1)-(3) in Table 2 show this effect of crime at the extensive margin[13]. Our preferred specification (column (3)) indicates that participants are 15 percentage points (or 29%, p-value<0.001) more likely to initially choose the public option when crime rates are 25% lower than average. This finding suggests that high crime rates in public transport lead people to substitute with private transportation, which would result in higher emissions.

Finally, the model predicts that when crime in public transport decreases, commuters preferences would shift towards public options, increasing the fare that would make them indifferent between modes. Columns

[13]As in Experiment 1, we aggregate the results for crime rates of +20% and +30%, and -20% and -30%, to ±25% because we find no significant differences between magnitudes of the same sign.

(4)-(6) present the results for this intensive margin. Our estimates reveal that the bus price making participants indifferent between the two options is 27.3% higher when crime rates are 25% lower than average, compared to when they are 25% higher. Overall, participants are willing to pay up to 27% more for a bus with less crime. These results are robust to only considering the correct respondents of a coin toss question[14] (see Table A10) and are consistent across cities, as shown in Table A11. Moreover, Buenos Aires and Santiago display the least sensitivity to crime rates among the cities studied, both at the intensive and extensive margins[15]. Table A12 shows that treatment effects are robust across other subsamples.

The interpretation of the results is straightforward. Safety is a valuable attribute when deciding between transport modes and therefore agents have a positive willingness to pay to use modes with higher safety levels (all else constant). Therefore, keeping prices fixed, when public transport safety decreases, some commuters will stop considering this option, while others who were at the margin will change to other transport modes. To prevent this marginal commuters from changing to private options, they would need to be compensated with lower fares. Overall, these results provide evidence of two mechanisms through which crime impacts public transport demand, generating the aforementioned externality on congestion and pollution.

Experiment 2 rules out any concerns about anchoring effects caused by different starting prices in Experiment 1. Even when the starting price is the same for all treatment groups, we find significant differences between the reported indifference prices for public transport across groups. The pattern mirrors that of Experiment 1: we estimate significant differences between treatment groups of opposite signs but find no differences between groups of the same sign, indicating, again, that the effect is non-linear. Furthermore, Figure A4 illustrates the distributions of indifference prices in both experiments: the mode indifference price is on the extremes of the distribution. This suggests that participants tend to deviate as much as possible from the starting price, which served as the only plausible anchor (0.5 and 1.5 in Experiment 1, and 1 in Experiment 2).

5.3 Effect of Crime on Price Elasticity

Crime might have an indirect effect on public transport demand by affecting price elasticity. If this is the case, the effectiveness of subsidies to promote ridership will depend on crime levels. Following Domínguez and Scartascini (2024), we estimate a linear probability model using an indicator of whether the treated alternative was chosen. In Experiment 1, the coefficients reflect the effects of price and crime on the probability of choosing the bus with a different crime rate, when the outside option is an average bus. For Experiment 2, this refers to the probability of choosing the treated bus when the outside option is an average private taxi.

Table 3 presents the estimates of price elasticity for both experiments. In Experiment 1, we find an

[14] As standard in the experimental literature, when dealing with probabilities is important to test whether participants understand what a probability is. Therefore, as a sanity check, we asked to all of the participants which is the probability that a coin lands on heads if it is tossed randomly.

[15] Also, Table A4 reports the estimates of the indifference prices for each crime treatment group. Interestingly, on average, respondents in all groups are willing to pay a higher bus fare than the current one. A plausible explanation is that, in the context of the cities considered in the experiment, the bus fare is so low (or the private taxi price so high) that commuters are already willing to pay more to avoid taking private transport.

average elasticity of -0.8 across treatments. However, safety in public transport significantly affects its elasticity. Specifically, when the crime rate is 20% higher than average, elasticity decreases by 0.13 in absolute terms (p-value < 0.001), indicating that higher crime rates reduces demand elasticity. In other words, price cuts in contexts of high insecurity are less effective in boosting ridership: elasticity ranges from -0.87 in the -20% crime treatments to -0.73 in the +20% treatments. These estimates, although larger in magnitude than the typical rule of thumb, remain within the range of previous research (Holmgren, 2007). The larger magnitude may be attributed to the experimental nature of the decision-making process. Furthermore, baseline prices across study cities are low, making small increases in absolute terms seem large in percentage terms. Table A13 details the heterogeneity of these results by city, while Table A14 explores heterogeneity by gender, crime perception, frequency of transport use, and car ownership. Importantly, crime rates have a higher impact on price elasticity for women (0.16) than for men (0.11), and a higher crime perception amplifies the effect of crime.

Table 3. Elasticity main results

	Experiment 1 (1)	Experiment 2 (2)
Price	-0.735***	-0.258***
	(0.007)	(0.004)
1(-20% Crime)	1.06***	
	(0.011)	
Price × 1(-20% Crime)	-0.135***	
	(0.010)	
1(-25% Crime)		0.126***
		(0.007)
Price × 1(-25% Crime)		0.009
		(0.006)
Observations	56,771	56,771
Controls	Yes	Yes
City FE	Yes	Yes

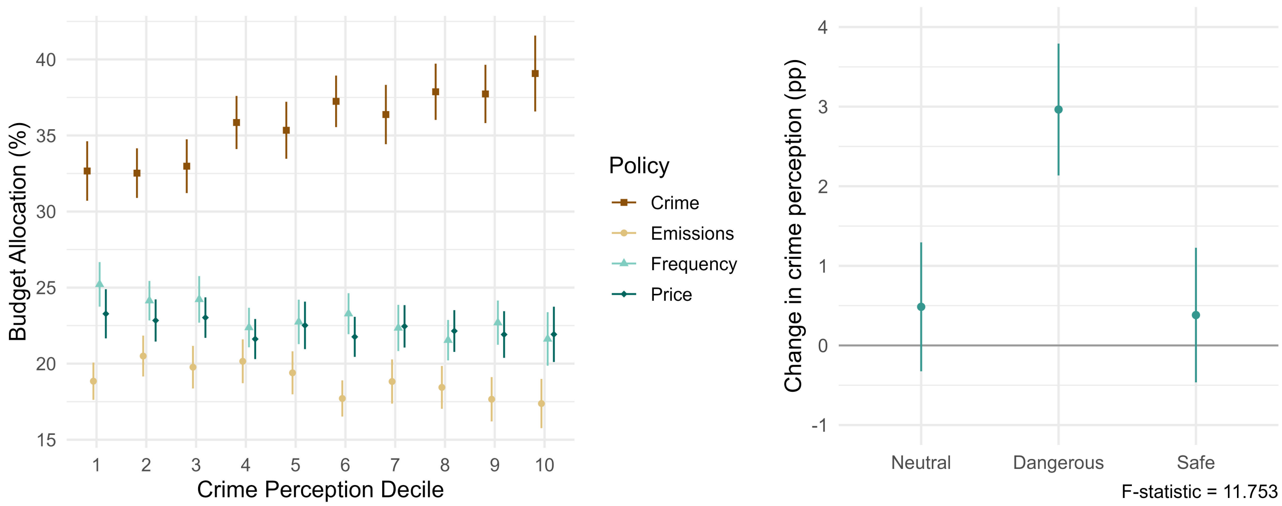
Notes: This table presents the results of the elasticity exercise. The dependent variable is an indicator variable of whether the respondent chose the 'treated' alternative (Bus B in Experiment 1 and the bus in Experiment 2). *Price* is a variable corresponding to the price of the treated alternative in current bus fare (of its city) units. $1(Crime = -20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -20%. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -25%. Columns (1) corresponds to the results of Experiment 1 and column (2) to Experiment 2. All specifications include control variables and city fixed effects. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Column (2) of Table 3 reports the elasticity estimates for Experiment 2. When the outside option is a private taxi, demand for public transport becomes less elastic than in Experiment 1 (averaging -0.25) and, as Table A13 shows, this result holds when we consider each city separately. Consistent with the ambiguous theoretical predictions, safety does not significantly affect price elasticity in this case. This result might stem from strong personal preferences for either mode of transport or from the substantial price differential between them, as Figure A4 shows, more than half of the respondents consistently stick to one alternative regardless of the price. However, we must note that 13.9% of participants cannot be incentivized through price to take the bus when crime is lower, but this fraction jumps to more than 25% when crime rates are higher. This is suggestive of crime affecting the price elasticity of those at the tail of the distribution rather than across its entirety.

5.4 Effect of Crime Perception on Policy Preferences

The goal of Experiment 3 is to determine whether crime perception crowds out support from other public transport policies. We conduct two analyses to this end. First, we examine the correlation between crime perception (i.e., the perceived probability of being a victim of a crime while using public transport) and transport policy preferences. Second, we introduce exogenous variation in crime perception by priming participants with a real newspaper headline. As described in Section 3, participants were randomized to read one of three headlines (*Dangerous*, *Safe* and *Control*) about the public transport system in their city. After priming, we again elicit the perceived crime probability and use the change in perceptions induced by the priming as an instrumental variable for our analysis.

Figure 2. Experiment 3



(a) Crime perception and policy support

(b) Effect of priming on crime perception

Notes: Panel (a) shows the mean budget allocation for each policy, binned by deciles of crime perception in each city. Error bars show the 95% CI. Panel (b) shows the average effect on crime perception (in percentage points) of the news priming. Error bars show the 95% CI.

Panel (a) of Figure 2 illustrates the correlation between crime perception and policy support. Individuals who perceive a higher likelihood of being a victim of a crime in public transport allocate more of the proposed budget to crime reduction and less to improving frequency, with no significant effect on the allocation to fare subsidies or emission reduction.

To test whether this link is causal, we use news priming to induce exogenous variation in crime perception. First, we assess the effectiveness of our instrument in modifying respondents' crime perceptions. Panel (b) in Figure 2 shows that while the *Dangerous* treatment effectively changes participants' perceptions as expected, the *Safe* and *Neutral* treatments do not. The failure of the *Safe* treatment to improve safety perception might be due to the fact that crime is still mentioned even when news focus on safety. Moreover, the regression-equivalent of the figure reports an F-statistic of 11.75, meeting the standard rule of thumb for testing the relevance of instruments in the literature.

Table 4. Reduced-form estimates of Experiment 3

	Crime (1)	Emissions (2)	Frequency (3)	Price (4)
Change in CP	0.579^{**}	-0.257	-0.499^{**}	0.177
	(0.294)	(0.200)	(0.217)	(0.206)
Observations	4,850	4,850	4,850	4,850
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes

Notes: This table presents the results of the estimation of Equation (7) for Experiment 3. *Change in CP* is the fitted change in the perceived probability of being a victim of a crime in a public transport trip as specified in Equation (6). The dependent variable is the share of the total budget allocated to each policy. All specifications include control variables and city fixed effects. The vector of control variables considered are age, gender, and level of education. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

The change in crime perception impacts public transportation policy preferences. We report the estimates of Equation (7) in Table 4. Specifically, starting from a budget of USD 120,000, a 1pp increase in crime perception raises the allocation to crime reduction policies by 0.6pp (USD 720) and reduces the allocation to frequency improvement by 0.5pp. It is worth noting that we did not require participants to use the entire available budget to test their attention and engagement. A strikingly high 97.3% of the sample allocates the entire budget, suggesting they feel engaged with the allocation. As a robustness check, we re-estimated our key regressions considering only those participants who used the whole budget. Table A15 shows that these results are consistent with those from the full sample.

This set of findings demonstrates that higher crime perception leads citizens to prioritize crime reduction policies at the expense of efficiency improvements, but there is no crowding out from environmental policies. We find two aspects of these results particularly valuable. First, our results shed light on the complementarities between the types of transport policies considered. In particular, participants do not perceive a trade-off between environmental and crime abatement policies even in a stringent zero-sum setting. Secondly, the point estimates of columns (1)-(4) of Table 4 correspond to the semi-elasticity of budget allocation with respect to safety perception. Thus, column (1) suggests that a 1% increase in crime perception would increase the budget allocation to safety measure by 0.58%, highlighting how important safety is among the participant's preferences.

6 Conclusion

This study contributes to the debate on the policy levers to increase public transport ridership and reduce greenhouse emissions by focusing on the critical role of crime and crime perceptions. Through three pre-registered experiments in six Latin American capital cities, our work highlights three takeaways for policy makers.

First, crime is a determinant factor in transport modal choice. We quantify users valuation of crime reductions in public transport, estimating that users are willing to pay a premium of 51% of current bus fares to ride safer transport. This valuation offers a tangible measure of the value that users place on safety, which can be incorporated into cost-benefit analyses of public transport policies. Our mouse-tracking results offer more evidence about the importance of crime in transport mode choice: participants consider crime as relevant as price and more relevant than other trip attributes when choosing among transport options. Overall, our results show participants place a substantial value on safety in their public transport systems.

Second, we provide evidence that crime affects public transport demand through two channels: by changing the appeal of public transport, and by changing the price elasticity of demand. Participants are 29% more likely to choose public over private transportation at current prices when the public option is 25% safer than average. Thus, reducing crime in public transport can boost the substitution from private to public transport and reduce emissions. In addition to this direct effect on demand, crime also affects how users react to fare changes. Higher crime rates make demand for public transport more inelastic, especially among women. Intuitively, if fear of crime is high enough, commuters will be reluctant to use public transport no matter the price. This indirect effect of crime can limit the effectiveness of current and proposed subsidies to boost public transport ridership. Taken together, these direct and indirect effects imply that higher crime rates can limit the efforts to reduce urban congestion and pollution. As long as these effects are not taken into account by policymakers, crime will continue to limit the effect of environmental policies.

While crime reduction and environmental policies are often seen as unrelated—or even opposed due to their political positioning—both can be leveraged toward joint objectives. We test whether individuals perceive a trade-off between these policies, where heightened crime perceptions might reduce support for environmental initiatives. By experimentally inducing an exogenous change in crime perceptions, we find no evidence that such perceptions crowd out support for green policies. Even in a strict zero-sum setting, participants do not view crime reduction and emissions abatement as conflicting goals.

The evidence presented in this paper underscores the critical role of both actual and perceived crime in shaping public transport demand and policy preferences. Our findings suggest that the success of initiatives to increase public transport usage, an essential component of urban sustainability policies, may depend on addressing safety concerns alongside fare subsidies and service improvements. In Latin American cities, where crime is a persistent challenge, the implications are particularly striking: without substantial investments in safety, policies aimed at reducing private vehicle use and its associated congestion and pollution may prove ineffective. Future research should further explore the complex interplay between crime, safety perceptions, and transport demand to inform more effective urban and environmental policies.

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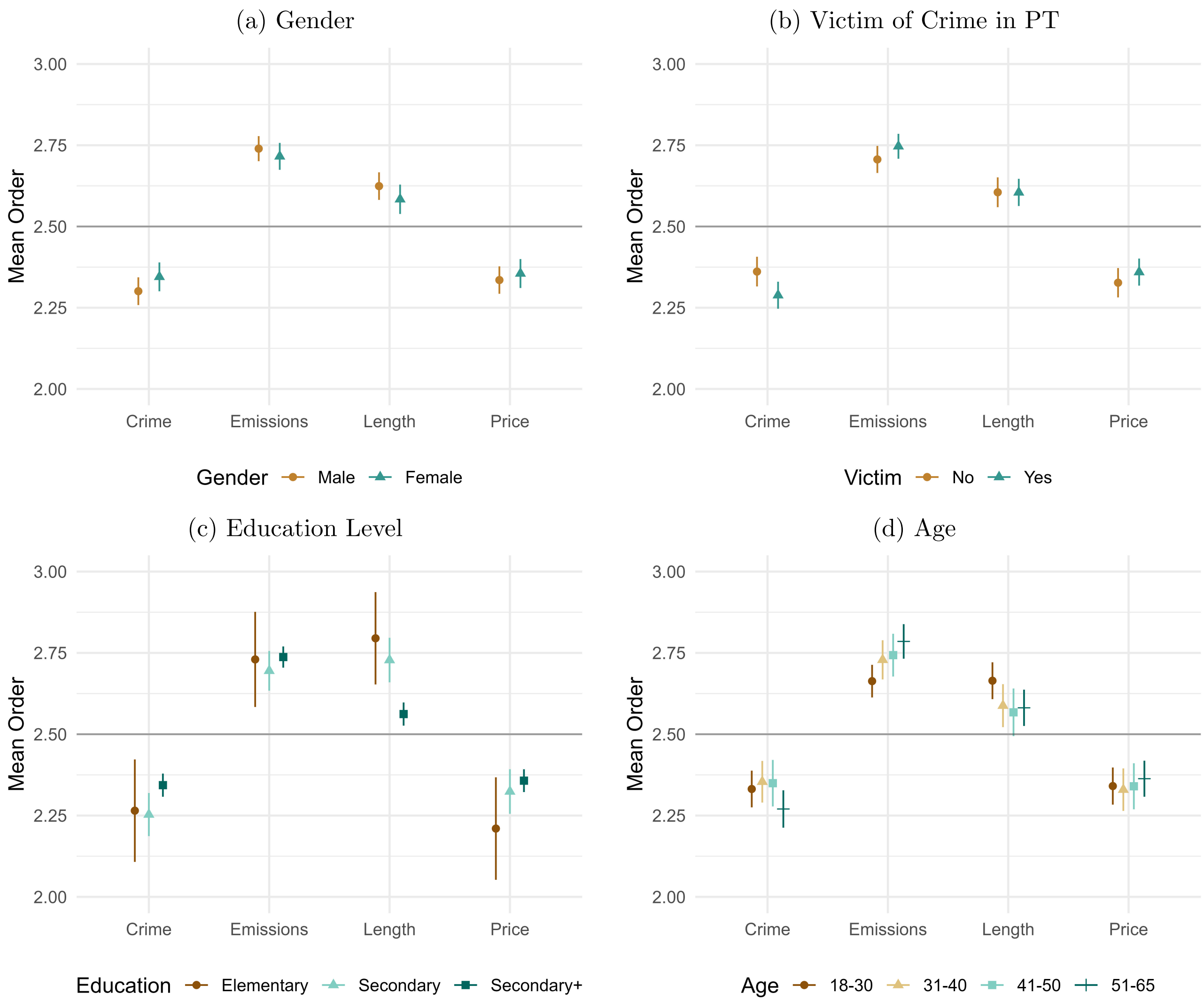
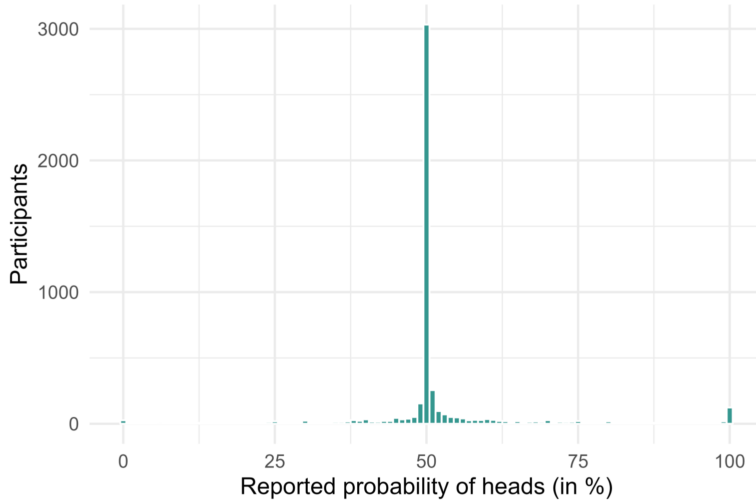
Appendix Tables and Figures

Figure A1. Heterogeneity in mouse-tracking results



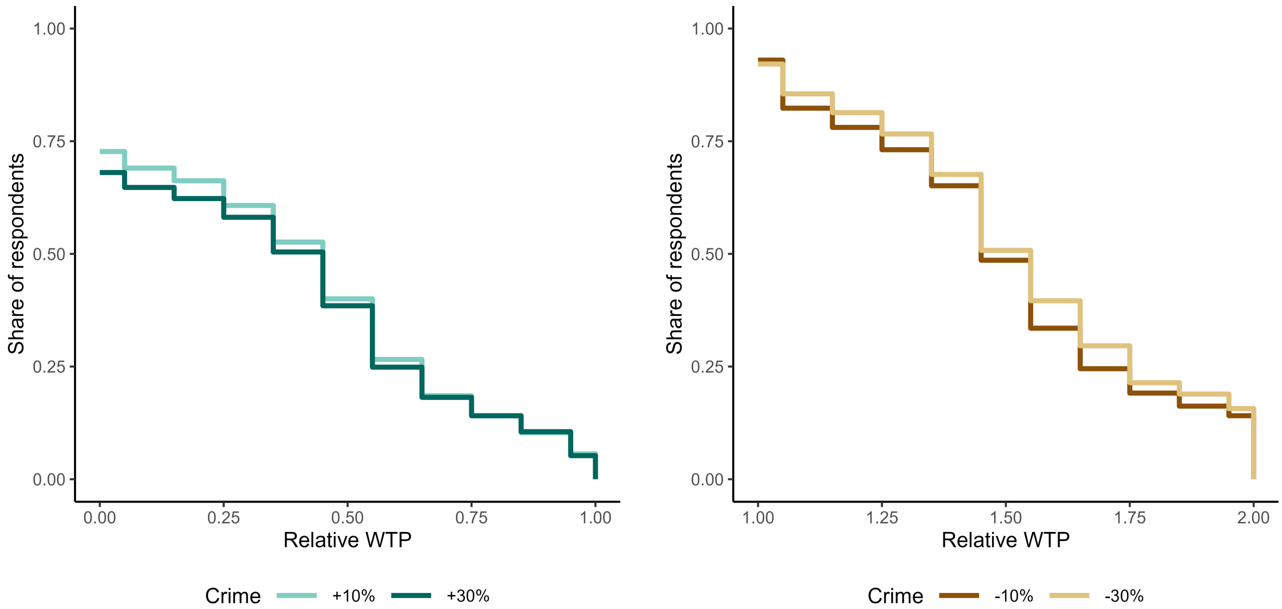
Notes: This figure shows the mean and standard errors of the order of look-up of each attribute. The mean order is depicted by a point and the 95% CI interval by the bars around it. Panel (a) groups the estimates by gender; Panel (b) differentiates the results between respondents who were victims of a crime in the public transport in the past; ; Panel (c) differentiates the results by education level; Panel (d) differentiates the results by age range. The education levels considered are *Elementary* (maximum education level is either complete or incomplete elementary schooling), *Secondary* (maximum education level is either complete or incomplete secondary schooling) and *More than Secondary* (maximum education level is more than complete secondary schooling).

Figure A2. Histogram of reported probabilities of a coin landing on heads



Notes: This figure shows the distribution of the reported probability of a random coin toss landing heads by our sample of respondents.

Figure A3. Distribution of relative WTP for safety in public transport, Expt. 1., by treatment

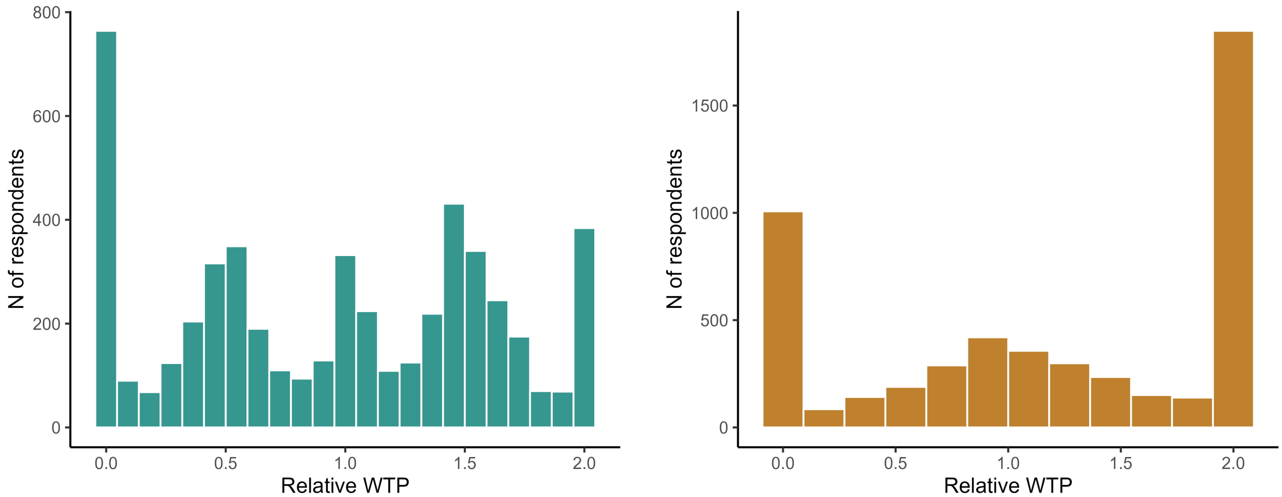


(a) Positive variation in crime

(b) Negative variation in crime

Notes: This figure shows elasticity with respect to price for each treatment group. Each line corresponds to the share of participants who would choose the Bus B in Experiment 1 at each price, in current bus fare units (of their city). Panel (a) corresponds to the results of the +10% and +30% treatment groups and Panel (b) to the -10% and -30% treatment groups.

Figure A4. Distribution of relative WTP for safety in public transport



(a) Experiment 1

(b) Experiment 2

Notes: This figure shows the indifference price distribution reported in each experiment. Panel (a) correspond to the results of Experiment 1 and Panel (b) correspond to the results of Experiment 2.

Table A1. Comparison of Sample and Population

	Bogotá		Buenos Aires		CDMX		Guatemala		Lima		Santiago	
	Experiment	National Survey	Experiment	National Survey	Experiment	National Survey	Experiment	National Survey	Experiment	National Survey	Experiment	National Survey
Age	37.472	38.715	37.991	38.876	35.510	38.050	30.882	36.573	37.711	39.032	37.855	39.111
Male (%)	0.604	0.481	0.495	0.497	0.556	0.461	0.487	0.447	0.607	0.481	0.404	0.505
Parent (%)	0.634	0.712	0.589	0.799	0.603	0.864	0.568	0.872	0.611	0.853	0.619	0.734
HH Size	3.764	3.210	3.496	3.799	4.136	4.446	4.540	4.854	4.523	4.250	4.132	3.560
Years of Education	14.473	11.609	13.328	11.904	14.649	9.587	13.317	6.674	14.909	11.534	14.331	12.604

Notes: This Table presents a comparing the mean of a set of variables between our sample and the values computed using surveys of the same cities drawn from a nationally representative survey. For each survey we considered only the responses of the cities that match the ones included in our survey and respondents between the age of 18 and 65, which is the criteria that we used to filter candidates in our experiment. To compute the representative means we used the data of the last quarter of 2023 of the following surveys: EPHC (Argentina), CASEN (Chile), GEIH (Colombia), ENEI (Guatemala), ENIGH (Mexico) and ENAHO (Peru).

Table A2. Descriptive statistics and balance test

	Summary Stats	Balance Table							
		Experiment 1			Experiment 2			Experiment 3	
		+10%	-10%	-30%	+20%	-20%	-30%	Dangerous	Safe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male (=1)	0.526 (0.499)	0.008 (0.020)	-0.006 (0.020)	-0.001 (0.020)	0.015 (0.020)	0.038. (0.020)	0.033. (0.020)	0.018 (0.017)	-0.008 (0.017)
Residents in HH	3.905 (1.892)	-0.108 (0.076)	-0.068 (0.075)	-0.014 (0.075)	-0.019 (0.076)	0.011 (0.074)	0.041 (0.076)	0.094 (0.066)	0.009 (0.062)
Secondary Schooling	0.218 (0.413)	0.0010 (0.016)	0.014 (0.016)	0.005 (0.016)	0.005 (0.016)	0.004 (0.016)	0.009 (0.016)	0.009 (0.014)	0.018 (0.014)
University Schooling	0.744 (0.437)	0.004 (0.017)	-0.018 (0.017)	-0.010 (0.017)	0.002 (0.017)	-0.006 (0.017)	-0.017 (0.017)	-0.007 (0.015)	-0.008 (0.015)
Age	36.222 (13.417)	0.683 (0.525)	0.609 (0.526)	0.918. (0.522)	0.398 (0.532)	0.256 (0.525)	0.676 (0.533)	-0.377 (0.460)	-0.509 (0.452)
Ideology (Right)	5.794 (2.168)	-0.041 (0.086)	0.031 (0.085)	0.088 (0.085)	-0.035 (0.087)	-0.141. (0.086)	0.164. (0.086)	-0.052 (0.074)	0.037 (0.073)
Trust in Police	4.583 (3.04)	0.053 (0.120)	-0.008 (0.119)	0.006 (0.120)	-0.003 (0.122)	0.052 (0.119)	0.201. (0.121)	-0.201. (0.104)	0.070 (0.103)
Owns Car (=1)	0.433 (0.496)	0.025 (0.019)	-0.005 (0.019)	0.010 (0.019)	0.011 (0.020)	0.010 (0.019)	-0.023 (0.019)	0.007 (0.017)	-0.003 (0.017)
Freq. PT	4.084 (2.046)	0.026 (0.081)	0.112 (0.080)	0.115 (0.080)	-0.005 (0.081)	-0.100 (0.081)	-0.076 (0.081)	0.051 (0.070)	0.033 (0.069)
Victim (=1)	0.542 (0.498)	0.038. (0.020)	0.052** (0.019)	0.051** (0.020)	-0.010 (0.020)	-0.033. (0.019)	-0.004 (0.020)	0.023 (0.017)	0.011 (0.017)

Notes: This table presents summary statistics of a given set of covariates and the results of the randomization balance test. *Male(=1)* corresponds to an indicator variable equal to 1 if the observation is male. *Residents in HH* corresponds to the number of people living in the same house as the respondent. *Secondary* and *University Schooling* are two indicator variables equal to 1 if the participant has some Secondary or University Schooling, respectively. *Age* corresponds to the age of the respondent. Ideology is a variable that can take values from 0 to 10, the closer to 0 (10) the closer the respondent identifies to a left- (right-) line of thought, politically. *Trust in Police* is a variable that can take values from 0 to 10, the highest the value the more trust the participant has in the police. *Owns Car (=1)* is an indicator variable equal to 1 if the respondent owns a car. *Freq. PT* corresponds to the number of days that the respondent used the public transport in the last 7 days. *Victim (=1)* is an indicator variable equal to 1 if the respondent has ever been victim of a crime in the public transportation. Column (2) presents the mean of each variable with the standard deviation in parenthesis.
Columns (2) to (8) report the coefficients and standard errors for a regression of each variable on each treatment group in the corresponding experiment including city fixed effects (the results from each row come from an independent regression). Columns (2)-(4) correspond to the treatment groups of Experiment 1. Columns (5)-(7) correspond to the treatment groups of Experiment 2. Columns (8)-(9) correspond to the treatment groups of Experiment 3. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1.

Table A3. Summary statistics of mouse-tracking

	Crime	Emissions	Length	Price
Mean Order	2.32	2.73	2.61	2.34
95% CI	(2.29, 2.35)	(2.7, 2.76)	(2.57, 2.64)	(2.31, 2.38)

Notes: This table presents the mean and 95% CI of the order in which each attribute was clicked in Experiment 1. The variable goes from 1 (if it was clicked first) to 4 (if it was clicked last).

Table A4. Mean willingness to pay for transport by crime rate, as fraction of current bus fare

	Experiment 1	Experiment 2
+30%	0.38	1.04
	(0.36, 0.4)	(1, 1.09)
+10% (+20%)	0.4	1.04
	(0.38, 0.42)	(0.99, 1.08)
-10% (-20%)	1.49	1.32
	(1.48, 1.51)	(1.28, 1.36)
-30%	1.53	1.3
	(1.51, 1.54)	(1.26, 1.34)

Notes: This table presents the mean and 95% CI of the indifference price between 'treated' and 'non-treated' alternative by treatment group in each experiment, in current bus fare terms. The 'treated' alternative is Bus B in Experiment 1 and the bus in Experiment 2. Each row corresponds to a different treatment group. Note that second and third row correspond to a 10% variation in the crime rate for Experiment 1 and 20% for Experiment 2.

Table A5. Reduced-form estimates of Experiment 1, disaggregated

	(1)	(2)	(3)	(4)	(5)	(6)
1(+10% Crime)	0.019	0.018	0.018			
	(0.013)	(0.013)	(0.013)			
1(-10% Crime)	1.12***	1.12***	1.12***			
	(0.013)	(0.013)	(0.013)			
1(-30% Crime)	1.15***	1.15***	1.15***			
	(0.013)	(0.013)	(0.013)			
Crime % (Continuous)				-0.023***	-0.023***	-0.023***
				(0.0002)	(0.0002)	(0.0002)
Observations	5,161	5,161	5,161	5,161	5,161	5,161
Controls	No	No	Yes	No	No	Yes
City FE	No	Yes	Yes	No	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 1. $1(Crime = +10\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group +10%. $1(Crime = -10\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -10%. $1(Crime = -30\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -30%. *Crime % (Continuous)* is a continuous variable corresponding to the level of crime rate that the participant was exposed to. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

Table A6. Reduced-form estimates of Experiment 1, only correct respondents to coin question

	(1)	(2)	(3)	(4)	(5)	(6)
1(+10% Crime)	0.028*	0.026*	0.026*			
	(0.015)	(0.015)	(0.015)			
1(-10% Crime)	1.15***	1.15***	1.15***			
	(0.015)	(0.015)	(0.015)			
1(-30% Crime)	1.18***	1.18***	1.18***			
	(0.015)	(0.015)	(0.015)			
Crime % (Continuous)				-0.023***	-0.023***	-0.023***
				(0.0003)	(0.0003)	(0.0003)
Observations	3,789	3,789	3,789	3,789	3,789	3,789
Controls	No	No	Yes	No	No	Yes
City FE	No	Yes	Yes	No	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 1. $1(Crime = +10\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group +10%. $1(Crime = -10\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -10%. $1(Crime = -30\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -30%. *Crime % (Continuous)* is a continuous variable corresponding to the level of crime rate that the participant was exposed to. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Table A7. Reduced-form estimates of Experiment 1 by city

	Bogotá (1)	Buenos Aires (2)	CDMX (3)	Guatemala (4)	Lima (5)	Santiago (6)
1(-20% Crime)	1.11***	1.14***	1.27***	1.09***	1.05***	1.06***
	(0.021)	(0.022)	(0.021)	(0.023)	(0.023)	(0.022)
Observations	872	863	859	871	856	840
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 1 by city. $1(Crime = +20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +10% or +30%. $1(Crime = -20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -10% or -30%. The vector of control variables considered are age, level of education and gender. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Table A8. Heterogeneity of reduced-form estimates of Experiment 1

	All (1)	Crime Perception Q4 (2)	Female (3)	MT Safety (4)
1(-20% Crime)	1.12***	1.15***	1.15***	1.15***
	(0.009)	(0.017)	(0.013)	(0.012)
Observations	5,161	1,523	2,446	2,982
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes

Notes: This table presents the heterogeneity in the estimation of Equation (4) for Experiment 1 by subsamples. $1(Crime = +20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +10% or +30%. $1(Crime = -20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -10% or -30%. *Crime Perception Q4* is an indicator variable equal to 1 if the participant's reported perceived probability of being victim of a crime in a trip by bus in their city is within the fourth quartile of the reported probability of their city. *MT Safety* is an indicator variable equal to 1 if the participant clicked the crime attribute first or second in Experiment 1. All specifications include control variables and city fixed effects. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

Table A9. Reduced-form estimates of Experiment 2, disaggregated

	Chose Bus			WTP		
	(1)	(2)	(3)	(4)	(5)	(6)
1(+20% Crime)	-0.009	-0.004	-0.004	-0.007	0.001	0.002
	(0.020)	(0.019)	(0.019)	(0.032)	(0.031)	(0.031)
1(-20% Crime)	0.155***	0.159***	0.159***	0.279***	0.286***	0.285***
	(0.019)	(0.019)	(0.019)	(0.031)	(0.030)	(0.030)
1(-30% Crime)	0.135***	0.138***	0.138***	0.260***	0.264***	0.265***
	(0.019)	(0.019)	(0.019)	(0.030)	(0.030)	(0.030)
Observations	5,161	5,161	5,161	5,161	5,161	5,161
Controls	No	No	Yes	No	No	Yes
City FE	No	Yes	Yes	No	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 2. $1(Crime = +20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group +20%. $1(Crime = -20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -20%. $1(Crime = -30\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment group -30%. Columns (1)-(3) correspond to the extensive margin results and columns (4)-(6) to the extensive margin. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Table A10. Reduced-form estimates of Experiment 2, only correct respondents to the coin toss question

	Chose Bus			WTP		
	(1)	(2)	(3)	(4)	(5)	(6)
1(-25% Crime)	0.168***	0.169***	0.171***	0.302***	0.302***	0.304***
	(0.015)	(0.015)	(0.015)	(0.025)	(0.025)	(0.025)
Observations	3,789	3,789	3,789	3,789	3,789	3,789
Controls	No	No	Yes	No	No	Yes
City FE	No	Yes	Yes	No	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 2 considering only participants who answered correctly what is the probability of a random coin toss landing heads (with 5% margin of error). $1(Crime = +25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +20% or +30%. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -20% or -30%. Columns (1)-(3) correspond to the extensive margin results and columns (4)-(6) to the extensive margin. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

Table A11. Reduced-form estimates of Experiment 2 by city

	Chose Bus						WTP					
	Bogotá (1)	Buenos Aires (2)	CDMX (3)	Guatemala (4)	Lima (5)	Santiago (6)	Bogotá (7)	Buenos Aires (8)	CDMX (9)	Guatemala (10)	Lima (11)	Santiago (12)
1(-25% Crime)	0.205***	0.172***	0.356***	0.290***	0.351***	0.292***	0.143***	0.086***	0.181***	0.177***	0.185***	0.142***
	(0.052)	(0.050)	(0.051)	(0.053)	(0.054)	(0.051)	(0.034)	(0.032)	(0.032)	(0.034)	(0.034)	(0.032)
Observations	872	863	859	871	856	840	872	863	859	871	856	840
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 2 by city. $1(Crime = +25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +20% or +30%. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -20% or -30%. Columns (1)-(3) correspond to the extensive margin results and columns (4)-(6) to the extensive margin. All specifications include control variables: age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

Table A12. Heterogeneity of reduced-form estimates of Experiment 2

	Chose Bus				WTP			
	All	Crime Perception Q4	Female	MT Safety	All	Crime Perception Q4	Female	MT Safety
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(-25% Crime)	0.149***	0.140***	0.155***	0.133***	0.272***	0.245***	0.277***	0.250***
	(0.013)	(0.025)	(0.019)	(0.018)	(0.021)	(0.040)	(0.031)	(0.028)
Observations	5,161	1,523	2,446	2,982	5,161	1,523	2,446	2,982
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of the estimation of Equation (4) for Experiment 2 by city. $1(Crime = +25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +20% or +30%. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -20% or -30%. *Crime Perception Q4* is an indicator variable equal to 1 if the participant's reported perceived probability of being victim of a crime in a trip by bus in their city is within the fourth quartile of the reported probability of their city. *MT Safety* is an indicator variable equal to 1 if the participant clicked the crime attribute first or second in Experiment 1. Columns (1)-(3) correspond to the results of the extensive margin and columns (4)-(6) to the ones of the extensive margin. All specifications include control variables and city fixed effects. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Table A13. Elasticity results by city

	Experiment 1						Experiment 2					
	Bogotá (1)	Buenos Aires (2)	CDMX (3)	Guatemala (4)	Lima (5)	Santiago (6)	Bogotá (7)	Buenos Aires (8)	CDMX (9)	Guatemala (10)	Lima (11)	Santiago (12)
Price	-0.718***	-0.755***	-0.740***	-0.702***	-0.722***	-0.777***	-0.282***	-0.262***	-0.243***	-0.256***	-0.243***	-0.259***
	(0.018)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.010)
1(-20% Crime)	1.14***	1.09***	1.12***	1.00***	0.921***	1.11***						
	(0.027)	(0.028)	(0.027)	(0.029)	(0.029)	(0.028)						
Price × 1(-20% Crime)	-0.206***	-0.125***	-0.072***	-0.139***	-0.095***	-0.178***						
	(0.024)	(0.024)	(0.024)	(0.025)	(0.025)	(0.024)						
1(-25% Crime)							0.088***	0.037**	0.168***	0.154***	0.187***	0.135***
							(0.017)	(0.015)	(0.015)	(0.017)	(0.017)	(0.015)
Price × 1(-25% Crime)							0.012	0.049***	0.009	-0.013	-0.013	0.010
							(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Observations	9,592	9,493	9,449	9,581	9,416	9,240	9,592	9,493	9,449	9,581	9,416	9,240
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of the elasticity exercise by city. The dependent variable is an indicator variable of whether the respondent chose the 'treated' alternative (Bus B in Experiment 1 and the bus in Experiment 2). *Price* is a variable corresponding to the relative price of the treated alternative in current bus fare (of its city). $1(Crime = +25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +20% or +30%. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -20% or -30%. Columns (1)-(6) corresponds to the results of Experiment 1 and columns (7)-(12) to the ones of Experiment 2. All specifications include control variables and city fixed effects. The vector of control variables considered are age, level of education and gender. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p < 0.01 , **p < 0.05, *p < 0.1..

Table A14. Elasticity main results, heterogeneity by sumbsamples

	Experiment 1					Experiment 2				
	All	Crime P. Q4	Female	Frequent PT	Car Owner	All	Crime P. Q4	Female	Frequent PT	Car Owner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Price	-0.735***	-0.707***	-0.705***	-0.768***	-0.672***	-0.258***	-0.248***	-0.241***	-0.269***	-0.221***
	(0.007)	(0.014)	(0.011)	(0.012)	(0.011)	(0.004)	(0.008)	(0.006)	(0.007)	(0.007)
1(-20% Crime)	1.06***	1.08***	1.10***	1.03***	1.08***					
	(0.011)	(0.021)	(0.017)	(0.019)	(0.018)					
Price × 1(-20% Crime)	-0.135***	-0.151***	-0.159***	-0.128***	-0.138***					
	(0.010)	(0.019)	(0.015)	(0.016)	(0.016)					
1(-25% Crime)						0.126***	0.128***	0.148***	0.091***	0.161***
						(0.007)	(0.012)	(0.010)	(0.010)	(0.011)
Price × 1(-25% Crime)						0.009	-0.007	-0.010	-0.005	-0.012
						(0.006)	(0.011)	(0.009)	(0.009)	(0.009)
Observations	56,771	16,753	26,906	20,999	24,574	56,771	16,753	26,906	20,999	24,574
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of the elasticity exercise by subsamples. The dependent variable is an indicator variable of whether the respondent chose the 'treated' alternative (Bus B in Experiment 1 and the bus in Experiment 2). *Price* is a variable corresponding to the price of the treated alternative in current bus fare (of its city) units. $1(Crime = +20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +10% or +30% in Experiment 1. $1(Crime = -20\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -10% or -30% in Experiment 1. $1(Crime = +25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups +20% or +30% in Experiment 2. $1(Crime = -25\%)$ is an indicator variable equal to 1 if the participant was assigned to the crime treatment groups -20% or -30% in Experiment 2. Columns (1)-(6) corresponds to the results of Experiment 1 and columns (7)-(12) to the ones of Experiment 2. Columns (1) and (6) consider the whole sample. Columns (2) and (7) consider respondents whose perceived probability of being victim of a crime in a bus trip is in the 4th quartile of their city. Columns (3) and (8) consider only respondents who identify themselves as females. Columns (4) and (9) consider the respondents whose reported days that they used the public transport in their city last week is above the median answer of their city. Column (5) and (10) report the results for participants who own a car. All specifications include control variables and city fixed effects. The vector of control variables considered are age, level of education and gender (except for the female subsample). Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

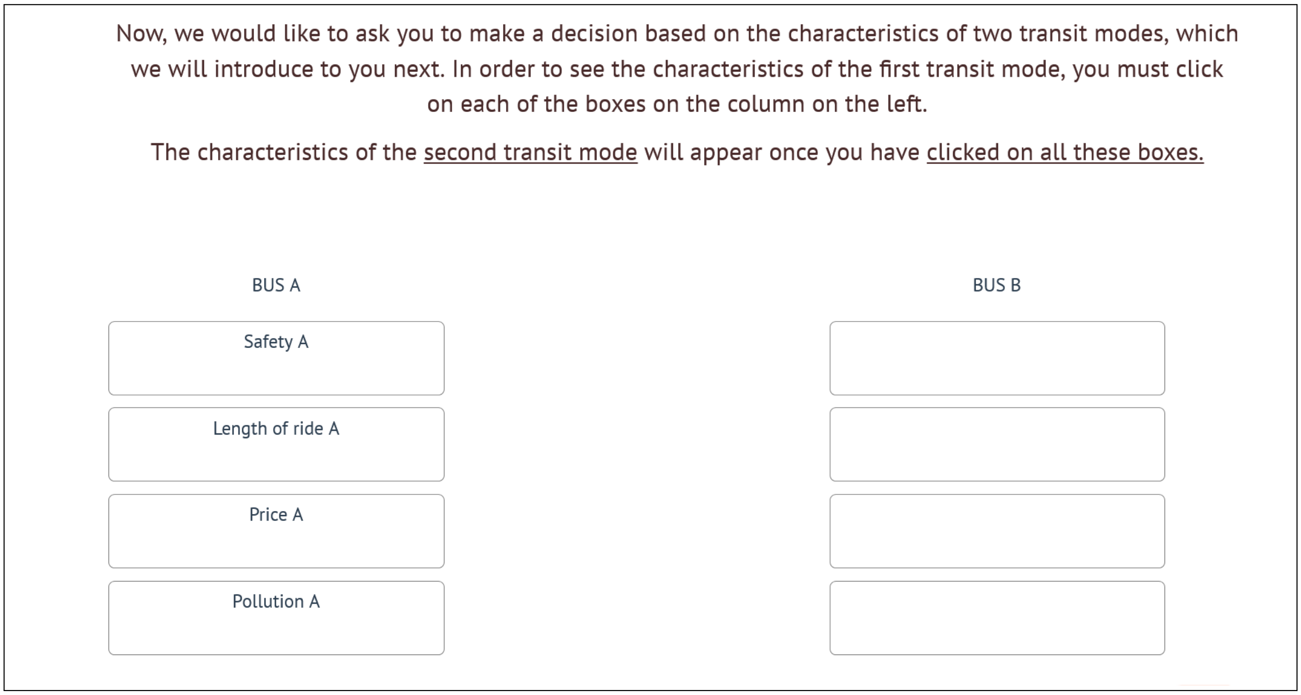
Table A15. 2SLS estimates of Experiment 3, participants that allocated the totality of the budget

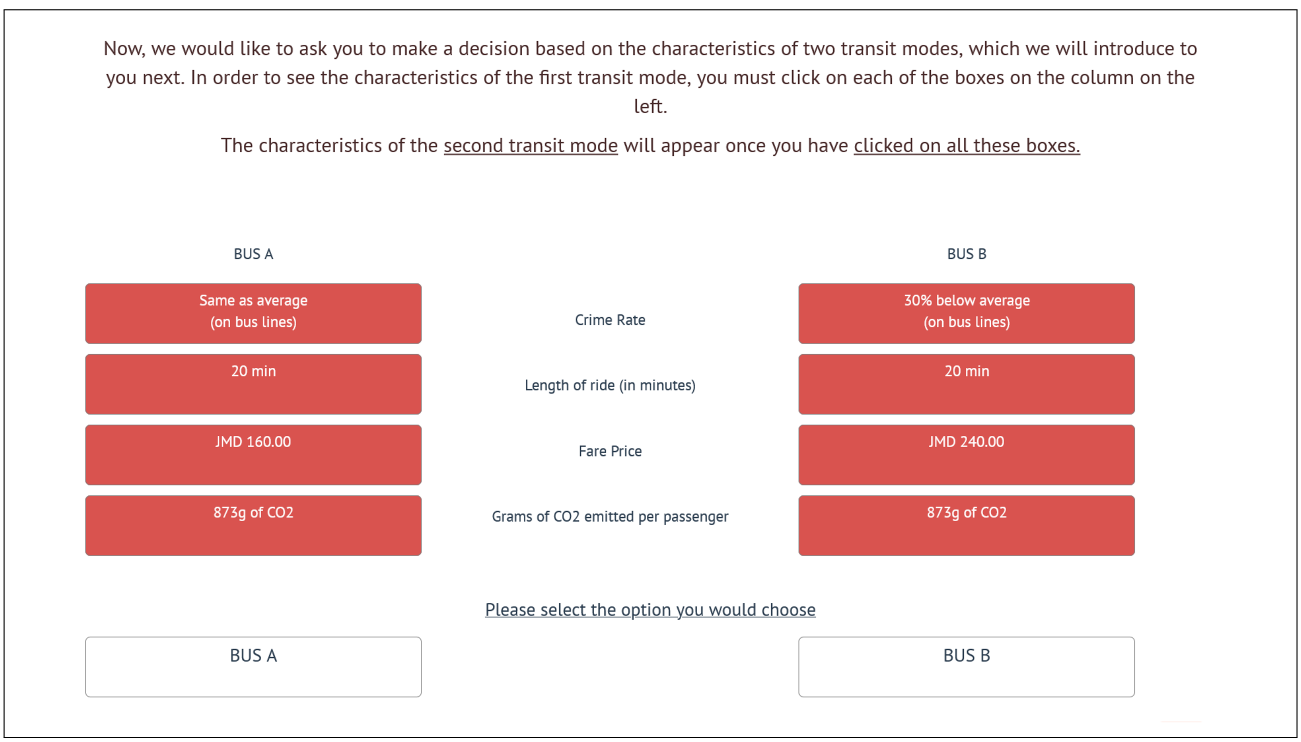
	Crime (1)	Emissions (2)	Frequency (3)	Price (4)
Change in CP	0.630**	-0.209	-0.566**	0.145
	(0.295)	(0.196)	(0.227)	(0.207)
Observations	4,718	4,718	4,718	4,718
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes

Notes: This table presents the results of the estimation of Equation (7) for Experiment 3 using the sample of participants who allocated the totality of the available budget. *Change in CP* is the fitted change in the perceived probability of being a victim of a crime in a public transport trip as depicted in Equation (6). The dependent variable is the share over the total budget allocated that was allocated to the policy detailed in each column. All specifications include control variables and city fixed effects. The vector of control variables considered are age, gender, and level of education. Heteroskedasticity-robust standard errors are considered. Significance levels: ***p $<$ 0.01 , **p $<$ 0.05, *p $<$ 0.1..

B. Online Experiment

Figure B1. Experiment 1





What decision would you make now that the price of Bus B is **higher**?

BUS A		BUS B
Same as average (on bus lines)	Crime Rate	30% below average (on bus lines)
20 min	Length of ride (in minutes)	20 min
JMD 160.00	Fare Price	~~JMD 240.00~~ JMD 256.00
873g of CO_2	Grams of CO_2 emitted per passenger	873g of CO_2

Please select the option you would choose

BUS A	BUS B

Figure B2. Experiment 2

Now, instead of choosing between two buses, you will have to decide between an Uber Private Taxi and a public bus in your city.

UBER		BUS
20 min	Length of ride (in minutes)	33 min
Pollution 1530g of CO2	Grams of CO2 emitted per passenger	Pollution 873g of CO2
Same as average (in private taxis)	Crime Rate	20% below average (on bus lines)
JMD 5,000.00	Fare Price	JMD 160.00

Please select the option you would choose

UBER

BUS

Figure B3. Experiment 3

Robos en manada en colectivos: la nueva modalidad que sufren los pasajeros en la Ciudad

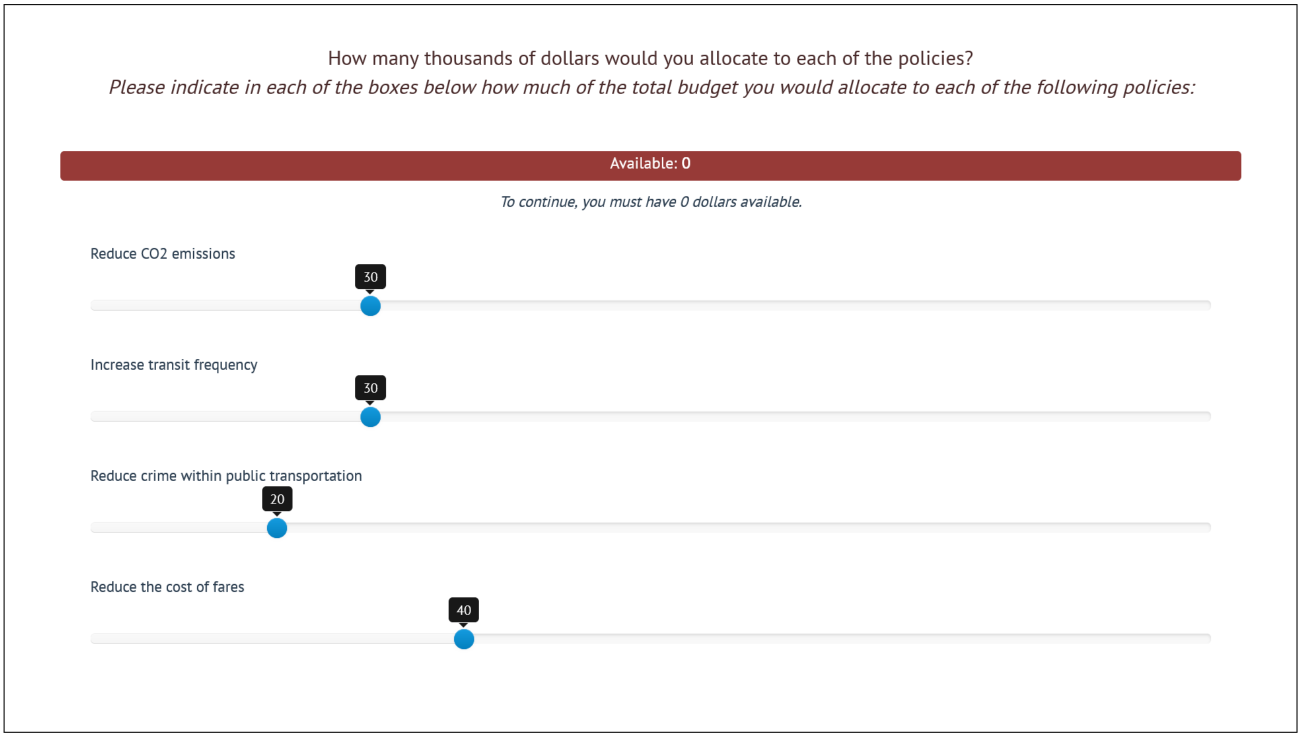
Son grupos que suben a los micros, fingen no conocerse y antes de bajar arrebatan celulares y mochilas. Palermo y Recoleta, las zonas más afectadas.

Imagine that Buenos Aires' government is debating how to spend a **US$120,000** budget to improve public transit.
Please keep in mind that the money spent on one area cannot be spent on another area.

You will be asked about how you think these funds should be used.
The results of this study will be presented to the agency in charge of public transit in your city. Your answer may **influence** the government's choices in your city, so it is in your best interest to be honest and careful in your answers.

From 0% to 100%, in which 0% means *impossible* and 100% means *most certainly*.

During your average commuting, how likely would you say you are to be a victim of crime while using public transit in your city?


How many thousands of dollars would you allocate to each of the policies?

Please indicate in each of the boxes below how much of the total budget you would allocate to each of the following policies:

Available: 0

To continue, you must have 0 dollars available.

Reduce CO2 emissions

30

Increase transit frequency

30

Reduce crime within public transportation

20

Reduce the cost of fares

40