Accents as Capital[*]

Leopoldo Fergusson[†] Natalia Garbiras-Díaz[‡] Michael Weintraub[§]

March 20, 2025

First draft. Please do not circulate.

Please read the latest version here.

Abstract

Do accents shape social and economic interactions? We investigate this question using a conjoint experiment embedded in an online survey of 6,000 respondents in Colombia. Respondents evaluate pairs of hypothetical individuals, each accompanied by an audio featuring a high- or low-class accent, along with information on income, education, and other attributes. We document a significant *accent premium*: individuals with high-class accents are consistently preferred in both work-related and social interactions, even after controlling for income, education, and personality traits. This premium is larger among high-SES respondents, revealing in-group bias. Experimental variations excluding income and education confirm that the premium is not merely a proxy for these factors. However, it disappears for high-class *foreign* accents, as natives struggle to recognize class-based cues in non-native speech. Exposure to class-based accents also reduces interpersonal trust and increases concerns about inequality, particularly among low-SES respondents. These findings highlight how accents reinforce social barriers and restrict opportunities for disadvantaged individuals, with implications for policies to reduce inequality and promote social mobility.

[*]We thank Hernán Carvajal and Jorge Luis Ochoa for their excellent research assistance. We also appreciate feedback received from the following audiences: EGAP Express Review Session, EGAP 2023 and 2024 LatAm Regional Meeting, REPAL 2024 Annual Conference, the 2024 LatAm EGAP Learning Days, CEDE Seminar, ITAM-CIDE Seminar, 2024 Congreso de Economia Colombiana, Second Congress of Social Sciences and Governement (Triada) 2024, and the Society for the Study of Economic Inequalities (ECINEQ) Meeting at ColMex. We thank Juana García for her contributions to earlier versions of this project. This research was made possible with support from the TREES Research Grant Fund. The contents are the authors' responsibility and do not necessarily reflect the views of the initiative. TREES is an initiative of the Center for Economic Development Studies of the Universidad de los Andes that promotes and participates in dialogues on inequalities from the Global South through rigorous research, pedagogical innovation, and impact on narratives about inequality. This study is published as part of the "Inequality and Governance in Unstable Democracies: The Mediating Role of Trust" project, implemented by a consortium led by the Institute of Development Studies (IDS). The support of the UK Economic and Social Research Council (ESRC) is gratefully acknowledged. Finally, we appreciate the additional financial support from Harvard Business School.

[†]Facultad de Economía, Universidad de los Andes, Bogotá, Colombia, lfergusson@uniandes.edu.co

[‡]Harvard Business School, Harvard University, ngarbirasdiaz@hbs.edu

[§]Departamento de Ciencia Política y Estudios Globales and Centro de Estudios sobre Seguridad y Drogas, Universidad de los Andes, Bogotá, Colombia, mlw@uniandes.edu.co

1 INTRODUCTION

Accents—the distinctive ways a language is pronounced, including the articulation of specific sounds, stress patterns in words, intonation, and rhythm—are potent signals of a person's background. Accents vary at many levels: national, regional, ethnic, gender, generational, and across social dimensions such as class and education. Therefore, accents encode diverse characteristics that influence how others perceive and treat speakers. Differential treatment based on accents begins early in childhood: while five-year-olds prefer peers of the same race, they prefer those who share their accent to an even greater extent (Kinzler et al., 2009).

Variations in accents by social class can be stark, and sociolinguistics has extensively documented the distinct speech patterns associated with different social classes (Labov, 2006). Labov's influential study on the use of the (r) sound as a marker of higher social status in New York department stores found that its usage was highest in Saks (the most expensive store) and lowest in Kleins (the cheapest) (Labov, 1972). Lower-class shoppers were more likely to pronounce the higher social status (r) sound when they realized they were being observed.

These class-based accent variations can have profound consequences in both everyday encounters and more structured interactions in the workplace. Because first impressions are formed quickly and may leave lasting effects, they constitute a significant source of bias.[1] In many brief encounters, such as job interviews or casual conversations, individuals may not have the opportunity to entirely demonstrate relevant traits and merits like skills or education to counteract negative stereotypes associated with their accent. Evidence confirms that higher-status accents are routinely deemed more hireable than their lower-status counterparts (Grogger, 2011; Spence et al., 2024).

This paper examines whether class-based accents influence interpersonal relations, expected performance in the labor market, and broader societal views. Our starting point is recognizing that class-based accents are daily reminders of gaps between socioeconomic groups and their relative status. Moreover, class-based accents convey that an unequal society is not merely fragmented along income or educational fault lines. Differences in how we speak reflect the social and cultural abyss between classes: we speak differently because we shared a particular social circle with a specific culture during our formative years. If higher-status individuals are preferred over lower-status ones, then class-based accents do not simply reflect class and status differences but also reproduce them.

Exploring these ideas empirically is challenging. It is relatively straightforward to establish that class-based accents matter for how we perceive others, our beliefs about their professional performance, or our broader societal views. Yet uncovering the mechanisms driving these effects is more complicated: people might prefer to interact with those who speak with a high-class accent because they use accent cues to make inferences about an interlocutor's *other* desirable characteristics. In particular, class-based accents convey differences along two crucial correlated forms of capital: human capital (education) and economic capital (income or wealth).

We implement an experiment that *unbundles* the extent to which class-based accents reflect

[1] On snap judgments more broadly, see Ambady and Rosenthal (1993); Todorov et al. (2005).

differences in economic or human capital. We administer an online conjoint survey experiment in Bogotá, Colombia, with over 6,000 respondents. We ask respondents to evaluate pairs of hypothetical individuals with a randomly varied high-class or low-class accent, captured in an audio snippet. By randomly varying the hypothetical profiles' education levels, incomes, and other attributes related to ability and likability, we also isolate the effect of accents from other (potentially correlated) forms of capital.

The Colombian case is particularly appropriate for studying the effect of class-based accents on social and economic outcomes. Like many Latin American countries, Colombia has a longstanding history of significant social and economic disparities (Eslava and Valencia Caicedo, 2023), in which wealthy households frequently opt out of the public service systems (Hirschman, 1970; de la O et al., 2023; Fergusson, 2019), further widening the social and cultural gap between different socioeconomic groups. Because the education system is segregated along income lines (Cárdenas et al., 2021), socioeconomic status is deeply embedded in individuals' identities, creating hard-to-bridge social divisions (Uribe-Mallarino, 2008).[2] Stark contrasts between high and low-class accents are potent indicators of these divisions.

Our experimental findings reveal three key patterns. First, we uncover a substantial high-class accent premium: individuals with high-class accents receive consistently more favorable evaluations across multiple dimensions, even when controlling for income and education. For outcomes related to interpersonal relationships, the marginal effect of having a high-class accent ranges from 5.5 to about 14.5 percentage points. This effect is always positive, unlike the impacts of other forms of capital, which can sometimes produce adverse effects. Individuals with a high-class accent are perceived as 5.5 points more empathetic, 14.5 points more trustworthy, and 13.4 points more likely to be chosen as friends compared to those with a low-class accent. The effects on labor market outcomes are also substantial: a 12-point increase in preference for business partnerships, a 13-point increase in preference as a colleague, an 11-point rise in the likelihood of being seen as a boss, and a 16-point increase in the expectation of career advancement. Importantly, this premium is not merely a signal of economic or human capital; it persists even when these attributes are held constant. Accents, therefore, reinforce status differences, deepening inequality and social stratification and undermining equality of opportunity.

To further understand whether the effects of class-based accents operate indirectly through income and education or exert an independent influence, we implement an additional experimental variation that omits these characteristics, following the causal mechanisms approach outlined by Acharya et al. (2018). This method decomposes the overall Average Treatment Effect (ATE) of class-based accents into two components: the Average Controlled Direct Effect (ACDE), which isolates the direct effect of accents while holding mediators (income and education) constant, and the Eliminated Effect, which captures the combined indirect effects and causal interactions. Our analysis, which fixes mediators at both high and low levels, reveals that the ACDE is consistently positive and statistically significant across all outcomes. In other words, high-class accents have a direct effect *beyond* inferred income or education.

[2]Cross-class interactions in Bogotá, for example, come with significant relational costs, as lower-income students face barriers like discrimination and gaps in cultural and economic resources when interacting with more privileged peers (Álvarez–Rivadulla et al., 2023).

Second, while the high-class premium is widespread, its strength varies by socioeconomic group. People tend to favor those who share their accents, reflecting in-group preference. Among low-SES respondents, a high-class accent increases perceived trustworthiness, friendship and work preference, and career advancement expectations by 13 to 16 percentage points. For high-SES respondents, these effects are about 10 points stronger. The impact also rises from 5 to 8 points for perceived empathy, 12 to 19 points for preferred business partners, and 10 to 17 points for expectations that the profile could be one's boss. This finding highlights an essential dimension of segregation: accent-based differentiation is not solely about hierarchy but also about identity and maintaining group boundaries.

Third, we explore whether accent-based biases influence broader societal attitudes. In a control condition, speaker profiles are presented only in text, omitting audio. Comparing this group to respondents who hear class-based accents, we test whether making cultural and social dimensions of inequality more salient shapes beliefs about society. We assess six outcomes: economic aspirations, expectations of social mobility, beliefs about meritocracy, concerns about inequality, and trust in others and the state. Results show that exposure to class-based accents heightens awareness of social inequality, especially among low-SES respondents. It also decreases trust in strangers relative to close relatives, reinforcing class divisions. These results suggest that accent-based biases not only shape interpersonal judgments but also influence broader social cohesion and awareness of inequality.

Several additional findings help explain these effects. Subjects correctly associate the high-class accent with a higher social class after controlling for other attributes. They also predict that individuals with a high-class accent have socially influential networks and prefer activities and music linked to the upper class, underscoring the role of sociocultural traits beyond income and education. An experimental variation highlights language's role in reinforcing hierarchy: deferential letters are expected to address high- more than low-class profiles. To ensure the high-class accent does not simply capture other desirable traits common among high-SES individuals, we take two approaches. First, we control for personality using the Big Five traits. Second, we test whether *Bogotanos* (citizens of Bogotá) classify and prefer high- and low-class foreign *Santiaguino* accents (from Santiago de Chile). *Bogotanos* fail to classify high-class *Santiaguino* accents, and the high-class premium disappears across borders, confirming that class-based accents function as culturally determined markers rather than as other universal advantages of higher-status individuals.[3] Building on these results, we interpret the effects as primarily driven by accents acting as key signals of social and cultural resources.

Our study contributes to multiple strands of literature. First, we add to the economic literature on barriers to equity and mobility across several dimensions. While research has examined disparities in labor market and social outcomes based on race and gender (Bertrand et al., 2010; Chandra, 2000), and recent contributions have highlighted how socioeconomic background plays a significant role in career progression (Stansbury and Rodriguez, 2024), we highlight a comparatively

[3]Crucially, our experimental design avoided several confounders by construction. To isolate class-based vocal variations from correlated traits like income, education, and personality, actors followed strict guidelines on language, reading speed, filler sounds, pitch, and audio quality, with all accents performed by similar-age actors.

underexplored source of disadvantage: accent-based discrimination. Existing literature has also shown how economic disparities are reinforced by spatial segregation, particularly through neighborhood effects on intergenerational mobility (Corak, 2013; Chetty et al., 2014,0). We extend this literature by demonstrating a novel "segregation by sound," where class-based accents serve as implicit barriers to mobility. Our results align with those in Chetty et al. (2024): mechanisms driving inequality extend beyond tangible economic resources to cultural and linguistic cues. We also add to the economics literature on persistent inequality (Bowles and Gintis, 2002; Durlauf, 1996), highlighting how accents can perpetuate disparities beyond their function as signals of material advantages.

Second, our findings build on classical themes in sociology on the role of class and status in reproducing inequality (e.g. Bourdieu, 1991). This has traditionally been studied in labor markets (Rivera, 2012; Rivera and Tilcsik, 2016; Lang and Lehmann, 2016; Oreopoulos, 2011) and education (Álvarez-Rivadulla, 2024), although sociolinguistics and communications psychology have shown how accents shape attitudes (Kinzler, 2021; Fuertes et al., 2012). We extend this work by demonstrating that preferences for high-class accents extend beyond their roles as markers of income and education. We also broaden the scope of existing work, moving beyond the effects of accents on interpersonal relations and beliefs about labor market performance to study their consequences for societal views more generally.

Third, we contribute to the literature on identity formation, the construction and maintenance of group boundaries, and the economic consequences of identity (Akerlof and Kranton, 2000; Shayo, 2009; Bénabou and Tirole, 2011; Bonomi et al., 2021; Fouka and Tabellini, 2022). We extend this work by showing how subtle linguistic features can both create and reinforce in-group and out-group distinctions, in ways that have the potential to influence economic and social outcomes substantively.

Fourth, our study has implications for research on migration (Borjas, 1994; Card, 2005; Ottaviano and Peri, 2012; Boustan et al., 2020) and cultural assimilation (Avola and Piccitto, 2020; Dovidio et al., 2010). While prior work has explored linguistic assimilation and penalties associated with foreign accents, we provide an account of a novel benefit for some associated with migration: those with low-class accents may experience a class-based premium relative to their experience in their home countries, whereas those with high-class accents may be unable to benefit from a high-class premium, given that local interlocutors in receptor countries are unable to infer their high-class status from their accents.

Finally, we make a methodological contribution. By employing a conjoint design with audio stimuli—the first of which we are aware—we provide a novel way to study discrimination and social biases in a controlled setting. Our experiment provides an approach that unobtrusively measures implicit preferences and biases while simultaneously minimizing concerns about social desirability bias (Stantcheva, 2023). This allows us to understand the extent to which accents simply reflect differences in economic capital, or whether they encode other forms of capital.

Our findings provide robust evidence that class-based accents play a significant role in shaping economic and social outcomes. We show that these linguistic markers do more than signal material

advantage: they actively reinforce class divisions, contributing to persistent disparities in labor markets and social interactions. Our study highlights a previously underappreciated mechanism through which inequality is maintained and reproduced.

2 RESEARCH DESIGN

2.1 Recruitment and Sample

We conducted an online survey with embedded experiments among over 6,000 respondents in Bogotá, Colombia, between March 6 and June 15, 2024. We focus on Bogotá because urban inequality is more pronounced, and the city reflects broader Latin American patterns of social and economic stratification (Banco Mundial, 2021). This stratification is evident in education, where upward mobility remains limited to a few high-achieving, low-income individuals who access elite institutions through targeted programs (Bank, 2018). A key driver of segregation is the *Sistema de Estratificación*, introduced in the early 1990s to cross-subsidize public services but later becoming a rigid social marker that reinforces class clustering (Uribe-Mallarino, 2008).[4]

We used a three-stage matched-quota sampling design to achieve sample distributions that reflect Bogotá's population distribution in terms of gender, age, and income level. This method also ensures the participation of a sufficient number of subjects from hard-to-reach socio-demographic groups, especially high-income respondents.

We create strata by combining sex, age, and income levels and determine the number of subjects in each stratum in Bogotá using data from the 2018 Colombian Population Census. To maintain a low cost per survey, we define three income groups, proxied by the *estratos* described above.We also create two age groups and two sex groups. Thus, our strata consist of men and women, either younger or older than 35 years old, belonging to *estratos* 1 and 2 (low-income group), *estratos* 3 and 4 (middle-income group), or *estratos* 5 and 6 (high-income group). Using these strata, we then contrast the information with the potential reach of the Facebook and Instagram APIs. The Appendix Table A.14 provides details on our sampling strata, while more information on our recruitment strategy can be found in the Appendix C.1.

2.2 Measuring and manipulating class-based accents

We hired male and female actors to read scripts in high- and low-class accents for both a Bogotá and a Santiago de Chile accent, embedding class-based speech in a self-introduction. We avoided a "class-neutral" accent because its existence is debatable and, in pilot tests, it often sounded more professional (implying neutrality could spuriously estimate the effects of ability).

Table 1 shows the scripts we used to describe the personality traits. We followed the Big Five instruments validated in Latin America by Benet-Martínez and John (1998). The audios thus

[4]This system classifies Colombian households into six socioeconomic strata to determine public service taxes and subsidies, with the first three representing lower-income groups and the last two the wealthiest.

convey five critical aspects of the profiles: main personality trait, high- versus low-class accent, Bogotá versus *Santiaguino* accent, and sex.

Table 1: Big Five Personality Descriptions

Big Five Personality Trait	Description (audio script)
	`Audio opening:` *Hello, let me tell you a bit more about myself....*
Openness	*... I am a person with a lot of imagination and varied interests. I have many ideas and am both creative and analytical.*
Agreeableness	*... I am a kind and generous person, and I always try to collaborate and help others.*
Conscientiousness	*... I am persistent with the plans I set for myself and follow them carefully. I am known for being meticulous and efficient.*
Extraversion	*... I am an outgoing and sociable person. I consider myself to be full of energy and very enthusiastic.*
Emotional stability*	*... I am a calm person who knows how to manage stress. I know how to keep calm, even in high-pressure situations.*
	`Audio closing:` *...Below, you will find a table that will provide more information about me.*

Notes: * Emotional stability is the opposite of neuroticism. The original opening in Spanish is "Hola, te cuento un poco más sobre mí" and the closing is "A continuación, encontrarás una tabla que dará más información sobre mí." The personality descriptions are as follows: *Openness:* "Soy una persona con mucha imaginación y con intereses muy variados." *Agreeableness:* "Se me ocurren varias ideas y soy a la vez ingenioso y analítico." *Conscientiousness:* "Soy perseverante con los planes que me propongo y los sigo cuidadosamente. Me caracterizo por ser minucioso y eficiente." *Extraversion:* "Soy una persona extrovertida y sociable. Considero que estoy lleno/a de energía y muy entusiasta." ' *Emotional stability:* "Soy una persona calmada que sabe controlar el estrés. Sé mantener la calma, aún en situaciones de presión."

We implemented several precautions to prevent repetition effects and minimize experimenter-demand biases from actors performing both high- and low-class accents. First, we used Audacity's pitch modifier to slightly alter pitch (by no more than 10%), ensuring the same actor sounded different across versions.[5] Second, we restricted the conjoint task to prevent the same actor and pitch from appearing in both profiles, ensuring that if Person A displayed a big five trait y, Person B had a different trait y'.

Our experiment ensures that audible variations capture class-based distinctions without conveying correlated traits. Such possible traits include, above all, income and education, but also confidence, kindness, or any other cultural or personality characteristic. First, the survey setup eliminates visible signals such as body language, presentation, or ethnic background. Second, we held language constant and instructed actors to avoid accent traits that might imply mispronunciation or different pause patterns, which could signal income or education.[6] Third, we minimized differences in reading speed, as it may signal personality or cultural traits. Fourth, actors avoided

[5] After pretesting for natural-sounding voices, we created three pitch variations per sex-location combination: male Bogotá (low-class +/-5%, high-class +10%), female Bogotá (low-class -10%, high-class original and -10%), male Santiago (low-class +/-5%, high-class -10%), female Santiago (low-class -10% and +5%, high-class original).

[6] Sociolinguists distinguish language variations by geography (dialect), social factors (sociolect), individuals (ide-

filler sounds (e.g., like, well, um), which can indicate education or personality beyond class. Fifth, we made only minor pitch modifications, and the same actor performed both high- and low-class accents to ensure similarity. Sixth, we normalized audio quality, including volume, to control for speech-related biases.[7] Finally, all actors were aged 25 to 39 to account for generational accent shifts.

2.3 Survey design and analysis

Our survey was designed to be administered in approximately 20 minutes and has three core parts.[8] First, a characterization module determines whether respondents grew up in Bogotá, excluding those who did not to ensure familiarity with class-based accents. It also collects information on sex, age, household size, income (from predefined ranges), and *estrato*. These questions appear at the survey's start, allowing any priming effects to dissipate before the experiments. Given their common use in Colombia, we do not expect them to cause pre-treatment effects or raise suspicions about the survey's purpose. Next, we assess generalized trust (belief that people have good intentions), reciprocity (positive and negative), willingness to donate a hypothetical windfall income to good causes (of COP $10,000 or USD $2.5), and fairness (willingness to punish unfair behavior at a personal cost).

The third part of the survey includes the experimental component, including our core conjoint experiment. Conjoint experiments have several desirable properties, including reducing social desirability bias and approximating behavioral benchmarks (Hainmueller et al., 2015). We randomize five versions of the conjoint experiment, shown in Figure 1. We ask respondents to consider two hypothetical individuals (A and B) in all versions. Both profiles are presented in tabular form, as shown in panels (a)-(c) and (e) of Figure 2, following standard conjoint practices (Hainmueller et al., 2014).

In all but one version of the conjoint experiment, the table includes an audio snippet where each person describes their key personality traits. This spoken description appears first, similar to CVs that include qualitative self-descriptions, followed by objective qualifications and accomplishments. Attribute order is fixed *within* subjects to reduce cognitive load but randomized *across* subjects to prevent anchoring or ordering effects.

In each choice task, we ask respondents which person:

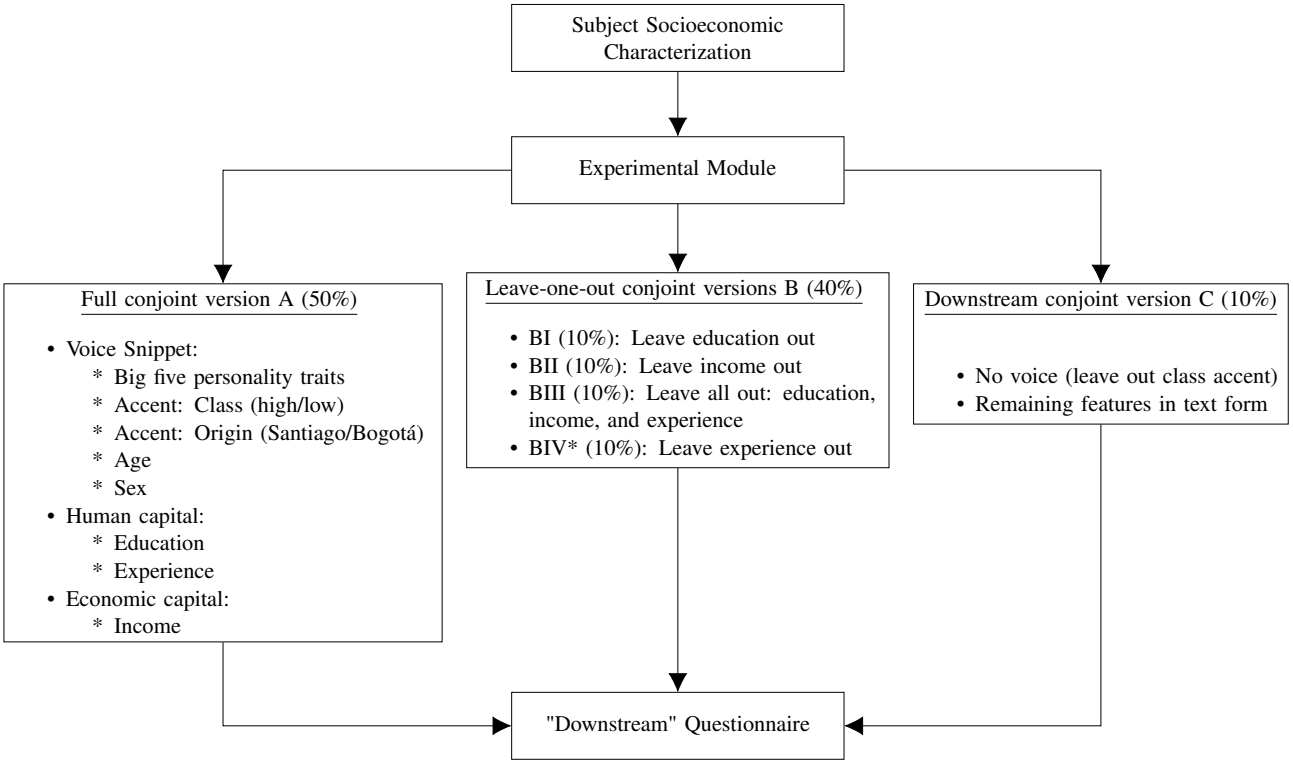
1. Is more empathetic ("able to put himself in other people's shoes");
2. Is more trustworthy;
3. They would prefer to start a business with;
4. They would prefer to establish a friendship with;
5. They would prefer to share their workplace with; and

olect), or ethnicity (ethnolect). We focus on class while abstracting from other variations that may correlate with income and education.

[7] There is a rich sociolinguistic literature examining social perceptions from different features of speech, heightening the importance of these precautions (see, for example, Guyer et al. (2021); Campbell-Kibler (2010)).

[8] The survey instrument is available in the following links: Spanish version and English version.

Figure 1: Experimental Design



Notes: Schematic description of the survey sequence and treatment conditions. All subjects answer the socioeconomic characterization module. 50% of the sample is randomly exposed to version A of the conjoint experiment, displaying all profile features. 40% instead solves one of four sets of leave-out versions BI to BIV where one or more profile traits are omitted. Finally, 10% of the sample is exposed to conjoint tasks with full information (in text form) but no audios of varying class-accents. Answers from this latter group are compared to those of others in the final, "downstream" questionnaire that all subjects respond. *Analysis for the leave-one-out variation BIV, excluding experience, is relegated to the Appendix.

6. Is more likely to be their boss.

In addition to these questions, we show subjects a letter offering a job opportunity to a candidate and ask which of the two hypothetical profiles would most likely receive it. We also randomize two versions of the letter: one using more informal, colloquial language (including *tú*, the informal third person singular typically reserved for friends, family, or younger interlocutors), and one using more deferential language (including *usted*, the formal third person singular, in addition to a much more formal greeting and more careful word choice).

After the conjoint experiment, we assess the "downstream effects" of listening to different class-based accents through the following questions:

1. Trust in various groups and institutions, including strangers, family, friends, foreigners, the Colombian State, and democratic institutions;
2. Trust in strangers, measured by the perceived likelihood of a lost wallet being returned;

Figure 2: Conjoint Experimental Conditions

(a) Conjoint (A)

	Person 1	Person 2
Audios	0:00 / 0:13	0:00 / 0:14
Personal Income	0 - 1'100.000	More than 6'800.000
Education Level	High School	University
Work Experience	Low	High

(b) Leaving out Education (BI)

	Person 1	Person 2
Audios	0:00 / 0:14	0:00 / 0:13
Work Experience	High	Low
Personal Income	1'100.001 - 2'600.000	1'100.001 - 2'600.000

(c) Leaving out Income (BII)

	Person 1	Person 2
Audios	0:00 / 0:14	0:00 / 0:14
Education Level	University	High School
Work Experience	High	Medium

(d) Leaving out Income, Experience and Education: Only audio (III) (BIII)

	Person 1	Person 2
Audios	0:00 / 0:14	0:00 / 0:10

(e) Leaving Out Accent: text-only version (C) (BIII)

	Person 1	Person 2
Sex	Woman	Woman
Quality	Kind and Generous	Thorough and Efficient
Education Level	Postgraduate	Technical
Work Experience	Medium	High
Personal Income	4'200.001 - 6'800.000	0 - 1'100.000
Region	Medellin	Santiago de Chile

***Notes*:** Screenshots of the different randomized versions of the conjoint experiment. Version labels described in Figure 1 are in parentheses. Analysis for the leave-one-out variation that excludes experience, BIV, is relegated to the Appendix.

3. Personal economic expectations (improvement or deterioration over the next year);
4. Beliefs about economic mobility (likelihood of someone from the lowest income quintile reaching the top quintile);
5. Beliefs about meritocracy (whether wealth reflects hard work or other advantages);
6. Ranking of Colombia's most pressing problems, including inequality.

Subjects in the full study completed at least four conjoint tasks. To increase participation, we offered an extra lottery ticket for a tablet to those who completed three additional tasks, up to three times, allowing a maximum of 13 tasks per subject. This strategy was pre-tested in a pilot study in Bogotá and Santiago de Chile, as detailed in Appendix C.1.1.

In what follows, we explain the different survey components and our estimation strategy for each.

The "Full" Conjoint Experiment. The main experiment (Figure 1 version A and Figure 2 panel (a)), contains the audio snippet with the personality descriptions and the table with *all* other attributes. Half of the sample received this version.

In addition to audio snippets, three attributes reflect each profile's socioeconomic status. The first is personal income, the most direct measure of financial resources.[9] The second is educational attainment, categorized according to the Colombian system.[10] The third is work experience, classified as low, average, or high (for the given age level), due to the lack of survey data on this aspect in Colombia.

Since Average Marginal Component Effects (AMCEs) depend on the distribution of attributes, they should be computed using profile distributions that reflect real-world patterns. To enhance external validity, we follow De la Cuesta et al. (2022) by using real-world data to match the relative frequency of each profile in the target population, ensuring more realistic profiles that reduce cognitive load and implausible responses.[11] This design-based estimation of a *population* AMCE (pAMCE) improves external validity by accounting for interactions between attributes, unlike uniform attribute distributions. In addition to these precautions, we also used pilot study data to identify less credible profiles and tested the robustness of our results by excluding them from the analysis.

The pAMCE represents the causal effect of a single profile attribute while averaging over the remaining attributes, accounting for the relative frequency with which each profile occurs in the population. The pAMCE can generally be estimated with the following equation:[12]

$$Y_{ipt} = \tau^{pAMCE}\text{High-class accent}_{ipt} + \mathbf{X}_{ipt}\beta + \varepsilon_{ipt}, \tag{1}$$

where Y_{ipt} is one of our binary outcome variables for respondent i, in profile p, and choice task t. The indicator of a High-class accent is our attribute of interest. The vector $\mathbf{X}_{ipt}$ includes all other attribute levels as indicator variables. ε_{ipt} is the idiosyncratic error term. Our coefficient of interest, τ, captures the effect of the factor `accent` and level High-Class accent compared to the baseline level of Low-Class accent, conditional on control factors. Throughout, we estimate cluster standard errors at the respondent i level, accounting for differential variance across treatment conditions and evaluated profiles.

[9] Based on the Large Integrated Household Survey (GEIH), we divided incomes into quintiles, further splitting the top quintile to distinguish rich from very rich profiles and merging the second and third quintiles due to their socioeconomic similarities.

[10] This includes five levels: elementary school, high school, technical education, undergraduate, and postgraduate, validated against large-scale Colombian surveys such as GEIH and the Quality of Life Survey, ENCV.

[11] Appendix C.2 details this joint distribution. The high-class accent is randomized uniformly, while income and human capital attributes follow a realistic joint distribution.

[12] Given the binary nature of our main attribute of interest, we do not present results for the marginal means proposed in Leeper et al. (2020), as these two quantities are equivalent for the full sample.

Following de la Cuesta et al. (2022), estimating equation (1) yields a *weighted Difference-in-Means* estimator, assigning greater weight to observations that occur more frequently in the target profile distribution.[13]

We estimate the effect of class-based accents conditional on the SES of respondent i with the following specifications:

$$\begin{aligned} Y_{ipt} =& \alpha_1 \text{High-class accent}_{ipt} + \alpha_2 \text{SES=high}_{ipt} + \\ & \alpha_3 \text{High-class accent}_{ipt} \times \text{SES=high}_{ipt} + X_{ipt}\beta + \varsigma_{ipt}, \end{aligned} \tag{2}$$

We added an experiment to test whether subjects associate high-class accents with better labor market prospects and greater deference. Respondents were randomly assigned two job offer letters—one colloquial, one deferential—and asked which profile was more likely to receive the letter. This tests whether high-class accents signal success and whether deferential language affects beliefs, particularly among low-SES respondents. We estimate the following model:

$$\begin{aligned} Y_{ipt} =& \alpha \text{High-class accent}_{ipt} + \theta \text{Deferential}_{ipt} + \\ & \tau^{Deferential} \text{High-class accent}_{ipt} \times \text{Deferential}_{ipt} + X_{ipt}\beta + \varsigma_{ipt}. \end{aligned} \tag{3}$$

In this specification, α captures the accent premium in the labor market, whereas $\tau^{Deferential}$ tests if the deferential letter changes this premium, particularly for low-SES subjects who might be accustomed to using or observing more deferential language towards high-class individuals.

Causal mechanisms: "Leave-One-Out" Conjoint Experiments. The "leave-one-out" variations (versions BI–BIV in Figure 1) help determine whether accents signal economic or human capital or have an independent effect. Each variation is assigned to 10% of the sample. In BI and BII, we omit education and income levels, respectively. In BIII, we remove all human capital and income markers: education, experience, and income. Experience serves mainly as a control, as it has no clear *a priori* relationship with social status, and our pilot showed minimal effects on outcomes. Thus, we do not emphasize the leave-experience-out condition (BIV) and relegate its results to Appendix Figure A.5.

These variations test whether respondents infer missing attributes from available information, as accents in daily interactions often signal background traits like education or income. Given the multidimensional nature of social class and the correlation among its features, accents reflect many forms of capital (Bourdieu, 1986).

We use a structured approach to analyze the causal mechanisms through which class-based

[13] Weights are the product of two components: the randomization distribution weights, $\Pr^R$, conditional on control factors, and the ratio of the target profile distribution, $\Pr^*$, to the randomization distribution. Models are estimated using the `R` package `factorEx` developed by Egami (2022).

accents influence outcomes. This method tests whether accents operate indirectly via income and education or have independent effects through other channels. Following Acharya et al. (2018), we decompose the overall average treatment effect (ATE) into two components: the "average controlled direct effect" (ACDE), which isolates the effect of accents while holding mediators constant, and the "eliminated effect," which captures both the indirect influence of accents on outcomes via their influence on income and education perceptions, and the potential "causal interactions" where the impact of accents depends on these factors.

Table 2 illustrates key quantities for the causal mechanisms analysis, controlling for high income. The Average Total Effect (ATE), shown in the first row, measures the difference in outcomes when the population is exposed to a High-Class accent (t_1) versus a Low-Class accent (t_0), defined as $TE(t_1, t_0) = \mathbb{E}[Y_{ipt}(t_1)] - \mathbb{E}[Y_{ipt}(t_0)] = \tau$. We estimate τ using its sample analog, $\hat{\tau}$, in equation (1). Importantly, this effect is measured in the "inferred" income arm, where respondents did not observe income and may have inferred it from the high-class accent.

Table 2: Quantities of interest for causal mechanisms (Acharya et al., 2018). Illustrating effects at high income

	Treatment (T_i)		
Mediator Arm (D_i)	**High-Class accent** (t_1)	**Low-Class accent** (t_0)	**Difference**
Inferred-Income arm (d_*)	$\mathbb{E}[Y_{ipt}(t_1)]$	$\mathbb{E}[Y_{ipt}(t_0)]$	$TE(t_1, t_0)$
Manipulated-Income arm (d_0)	$\mathbb{E}[Y_{ipt}(t_1, \text{High Income})]$	$\mathbb{E}[Y_{ipt}(t_0, \text{High Income})]$	$ACDE(t_1, t_0, \text{High Income})$
Difference	$ANME(t_1, \text{High Income})$	$ANME(t_0, \text{High Income})$	$\Delta(t_1, t_0, \text{High Income})$

Notes: The Average Total Effect (ATE), or $TE(t_1, t_0)$, is the difference in outcomes when the population is exposed to a High-Class accent (t_1) versus a Low-Class accent (t_0). The Average Controlled Direct Effect (ACDE) measures the effect of accent while holding a mediator variable, m, fixed at "High income." The Average Natural Mediator Effect (ANME) captures the impact of the mediator's induced or "natural" level under treatment t relative to the baseline m. The eliminated effect, defined as the difference between the ATE and ACDE, quantifies how income mediates the influence of a high-class accent, both as part of the causal pathway and as a key contextual factor.

The Average Controlled Direct Effect (ACDE) captures the effect of `accent` while holding a mediator variable constant, defined as $CDE(t_a, t_b, m) = \mathbb{E}[Y_{ipt}(t_a, m)] - \mathbb{E}[Y_{ipt}(t_b, m)]$. The second row of the table estimates this effect by fixing income at a high level. The sample analog follows a variation of equation (1), where we regress the outcome on attributes while estimating separate models for each level of the mediator (in this case, fixing income at "high"). This analysis is restricted to respondents who directly observe income rather than inferring it from accents.

The Average Natural Mediator Effect (ANME) compares rows within a column rather than columns within a row. It captures how the mediator's induced or "natural" level under treatment t compares to a baseline m. For a High-Class accent (t), the ANME measures the effect of inferred income relative to a fixed high-income baseline. If respondents already perceive the hypothetical profile as high-income, their NME is zero since the inferred and baseline values align.

Finally, the lower right corner of Table 2 presents the eliminated effect, defined as the difference between the ATE and ACDE. This difference quantifies how income mediates the impact of a high-class accent, both as part of the causal pathway and as a contextual factor. Our analysis includes three mediator variables—income, education, and experience—estimating equation (1)

for all levels of these mediators. However, we primarily focus on income and education, fixing them and their combinations at the randomized high and low levels.

Downstream effects: "No Audio" Conjoint Experiment. For 10% of the sample, respondents receive no audio snippets and thus no class-based accents. Instead, all information is presented in textual form, omitting the hypothetical subject's accent.[14] As shown in Figure 1, we compare responses from subjects exposed to audio (conditions A, BI, BII, BIII, and BIV) with those who were not (condition C). We assess the impact of receiving audio snippets on several outcomes: interpersonal and institutional trust, economic optimism, perceptions of social mobility, beliefs about meritocracy, and views on inequality as Colombia's main problem.

More formally, for outcome Y_i of individual i, we estimate:

$$Y_i = \alpha + \tau^{downstream} \times \mathbb{1}(\text{Class-based accents})_i + \varepsilon_i, \tag{4}$$

where $\mathbb{1}(\text{Class-based accents})_i$ equals one if respondent i was exposed to tasks involving class-based accents and zero otherwise. Our coefficient of interest, $\tau^{downstream}$, estimates the causal effect on Y_i of making the differences in cultural and social resources more salient through class-based accents.

For robustness, we exclude the leave-one-out variations (BI to BIV) from the downstream effects analysis, as these versions contain less information than A and C. Additionally, we run covariate-adjusted models incorporating a vector Z_i' of pretreatment characteristics, including sociodemographic variables (e.g., age, sex, income level) and prior preferences and attitudes measured prior to administering the conjoint experiments. We estimate fully saturated models (Lin, 2013) and use lasso selection methods to identify covariates that predict treatment effects.

High-class premium across borders. We expect respondents to distinguish class differences more easily in a local accent than in a foreign one. Empirically, this implies that the effect of "class" in accents should be weaker when evaluating speakers with accents from different regions. To test this, we use a difference-in-differences estimator by interacting the speaker's accent with an indicator for whether it matches the respondent's city of origin. We also account for the regional accent as a factor of substantive interest. The following specification estimates the Conditional Average Treatment Effect (CATE):

$$\begin{aligned} Y_{ipt} =& \alpha_1 \text{High-class accent}_{ipt} + \alpha_2 \text{Santiaguino}_{ipt} + \\ & \alpha_3 \text{High-class accent}_{ipt} \times \text{Santiaguino}_{ipt} + X_{ipt}\beta + \varsigma_{ipt}, \end{aligned} \tag{5}$$

[14] As explained earlier, we did not use a "neutral" accent as a control. The only drawback is that the "no audio" condition removes the cognitive process of listening, but since this is unlikely to affect downstream outcomes, we opted to omit audio altogether.

We use a *Santiaguino* accent for the "migrant" or "foreigner" condition for several reasons. First, like Bogotá, Santiago de Chile has high inequality and social segregation, with significant linguistic differences across social classes. Second, to avoid confounding effects from Venezuela's recent mass migration to Colombia, we deliberately exclude a Venezuelan accent, as most recent migrants are low-SES and culturally closer to Colombians. Finally, while Chileans are more culturally distant, their shared history and background provide a useful test of our hypothesis.

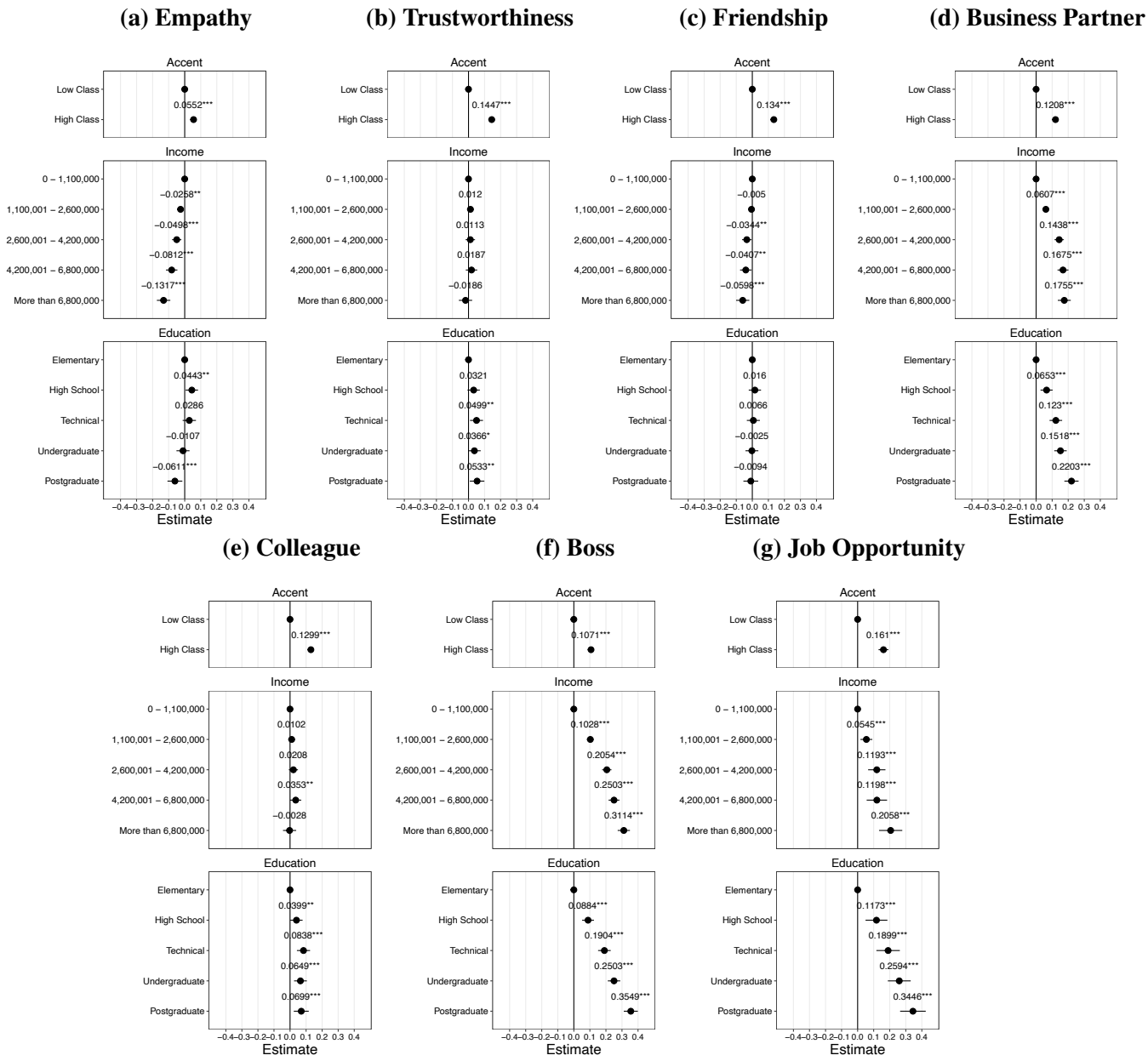
To control for the false discovery rate, we implement multiple hypothesis corrections. After separating analyses into primary and secondary tests, we create indices by "families" of outcomes and report p-values that adjust for false discovery rates within these families. There are three primary outcome families: first, all the outcomes in the conjoint choices for quotidian and labor market interactions; second, the analysis of the deferential letter experiment; and third, the set of downstream outcomes.

3 Results

We now report five main analyses. First, we test the effect of high-class accents on outcomes relevant to everyday interactions and labor market perceptions using version A of the conjoint experiment, which includes full profile descriptions. Second, we present results from the "leave-one-out" exercises and mediation analyses to assess whether accents signal economic or human capital or operate through other channels. Third, we explore the heterogeneous effects of our main results based on profile levels of education and income and on respondents' SES. Fourth, to assess downstream effects, we compare respondents exposed to audio-based class cues with a control group that received only text-based descriptions. Finally, we test discrimination against foreigners with low-class accents, leveraging the random assignment of Bogotá respondents to Bogotá or Santiago de Chile accents.

High-class premium: the "full" conjoint experiment. We first estimate the population AMCE for the full-information conjoint experiment (version A), assessing the effect of high-class accents on social interactions and labor market outcomes. Figure 3 presents point estimates for accents alongside other intertwined forms of capital, such as income and education (Appendix Table A.3 reports estimates and standard errors). The analysis focuses on Bogotá respondents evaluating *Bogotano* accents, excluding *Santiaguino* accents, which are examined separately to assess cross-border effects.

Figure 3: Conjoint tasks with full information (version A), Population Average Marginal Component Effects



Notes*:** Estimates in this figure correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; and Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels 3a to 3f are presented in all tasks to the respondent. However, the dependent variable in panel 3g is shown once per round. All panels include our independent variable of interest *accent*, and controls such as income, education, sex, experience, and the big 5 personality traits. However, for compactness we present only estimates for accent, income, and education. Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Confidence intervals are shown at the 95% level. *** p $<$ 0.01; ** p $<$ 0.05; * p $<$ 0.1.

For quotidian interactions, individuals with high-class accents are perceived as more empathetic (by nearly 6 percentage points, panel a) and more trustworthy (by 14 percentage points, panel b). They are also over 13 percentage points more likely to be preferred as friends (panel c). High-class accents also yield significant advantages in labor market interactions. Profiles with a high-class accent are 12 percentage points more likely to be chosen as business partners (panel d) and 13 percentage points more likely as workplace colleagues (panel e). Respondents also perceive a clear labor market premium: they are 10.7 percentage points more likely to believe the speaker could be their boss (panel f) and 16 percentage points more likely to think they received a job interview offer (panel g).[15]

Turning to the effects of income and education levels, higher income consistently improves perceptions of adeptness for business partnerships, boss perception, and job opportunities. However, income hurts empathy and friendship. These results highlight that financial status plays a positive role in the labor market yet a negative one in social warmth. Education also significantly enhances perceptions, especially for workplace-related outcomes. However, again, education has minimal or even negative effects on empathy and friendship, with postgraduate education slightly lowering perceived empathy. In short, while income and education strongly influence professional evaluations, they appear less relevant to interpersonal relations, highlighting the distinct role of class markers in social and economic interactions.

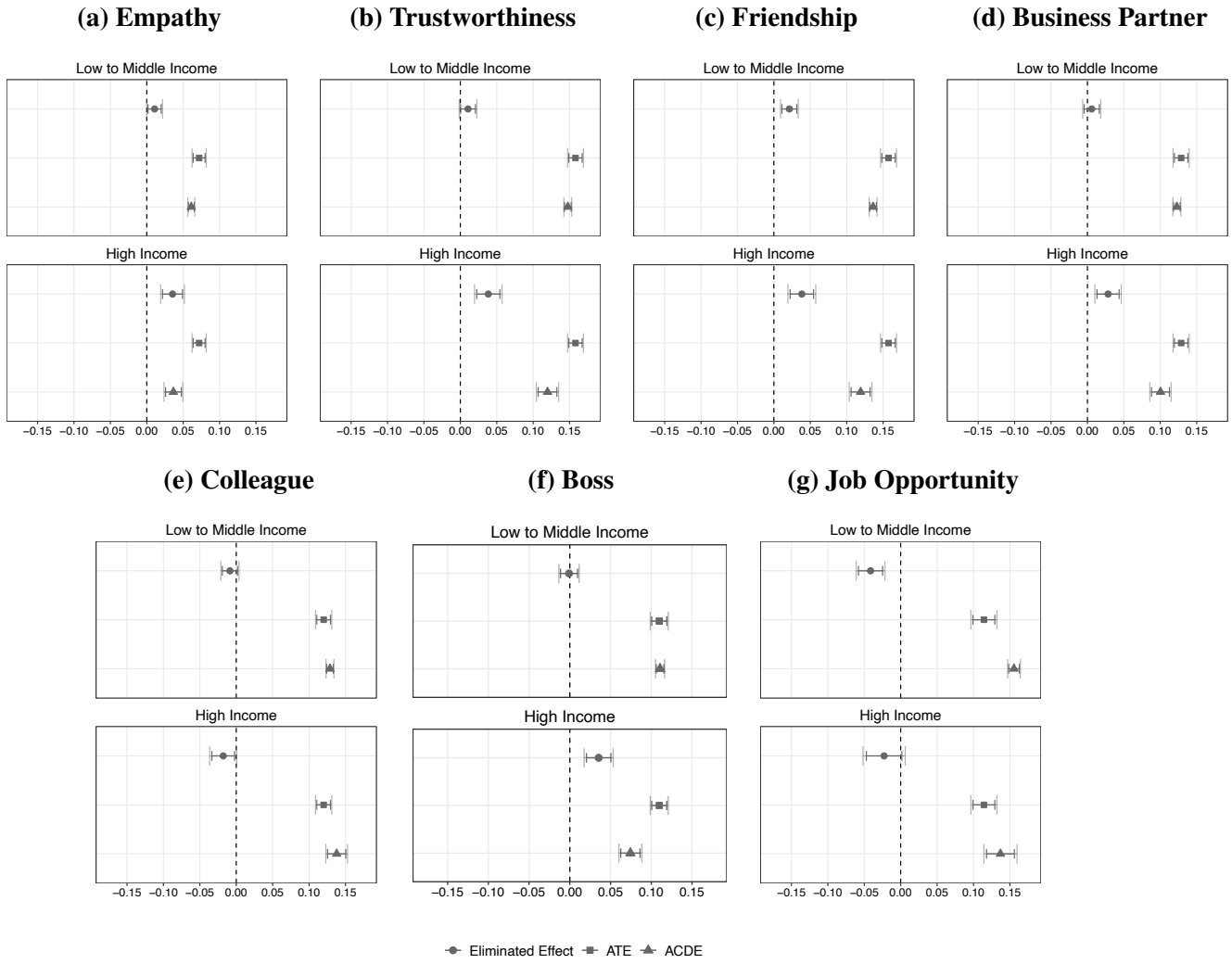
Causal mechanisms for the high-class premium: leave-one-out variations. High-class accents consistently led to more favorable evaluations, with large and highly significant effects across all outcomes. These effects isolate the impact of class-based accents by controlling for income and education, which are also randomized and explicitly provided in the task. To examine causal mechanisms in greater detail, we next present the findings by following the Acharya et al. (2018) structured approach leveraging experimental variations excluding income, education, or both.

Figure 4 decomposes the Average Total Effect (ATE) of accents in the inferred income arm—where income is not observed—into two components: the Average Controlled Direct Effect (ACDE) at a given income level and the remaining difference, or Eliminated Effect. Panels (a) to (g) show results for all conjoint outcomes, with rows distinguishing between low/middle-income and high-income groups.

The key finding is that the ACDE of a high-class accent, while holding income constant, is always positive and statistically significant. For example, in panel (a), measuring perceived empathy, the ACDE at low/middle income is very similar to the ATE, indicating a direct effect of over 5%. At high-income levels, the ACDE remains close to the ATE but is slightly lower, around 4%, suggesting that most of the high-class accent premium for empathy operates directly rather than through inferred income.

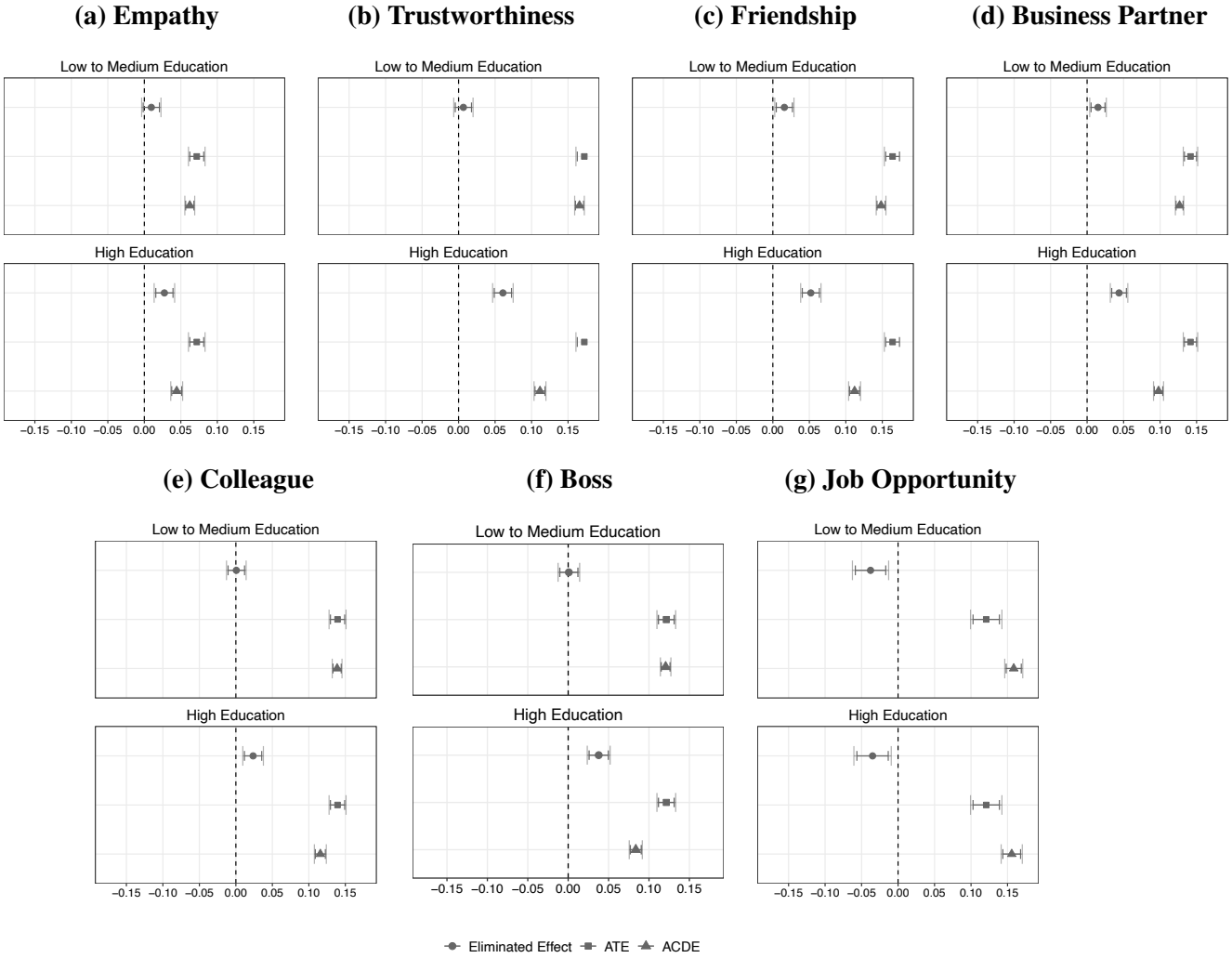
[15] Our results remain robust to multiple hypotheses corrections. Appendix Figure A.7 shows that the results remain consistent when following different correction approaches (Bonferroni, Holm, and Benjamini-Hochberg).

Figure 4: Income as a causal mechanism for class-based accents
Average Total Effect (ATE) =
Average Controlled Direct Effect (ACDE) + Eliminated Effect at each income level



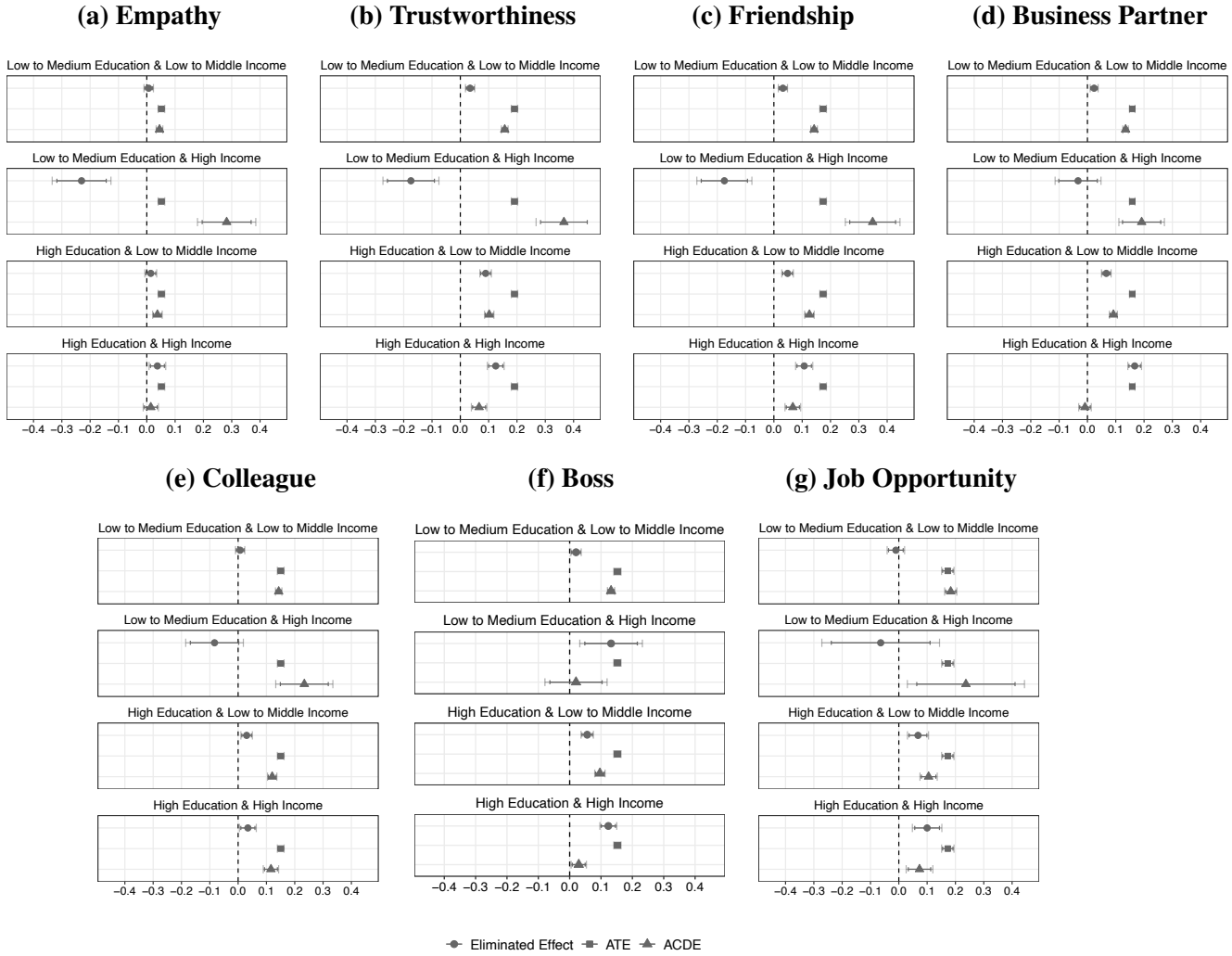
***Notes*:** Estimates for the **Eliminated effect** = **ATE**−**ACDE** are shown in **circles**. Estimates for the **Effect of accent without fixing income (ATE)** are shown in **squares**, while estimates for the **Effect of accent fixing income (ACDE)** are shown in **triangles**. Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels 4a to 4f are presented in all tasks to the respondent. However, the dependent variable in panel 4g is shown once per round. Estimates for the ATE and the ACDE in each panel correspond to the **population AMCE (*pAMCE*)** estimator as described in de la Cuesta et al. (2022), while the eliminated effect is defined as the difference between the ATE and the ACDE. Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Bootstrap 95% (darker line) and 90% (lighter line) confidence intervals are based on 5,000 bootstrap replications. Low and middle income correspond to profiles with income levels from 0 to 4.2 million Colombian pesos, whereas high income corresponds to profiles with 4.2 million Colombian pesos or more.

Figure 5: Education as a causal mechanism for class-based accents
Average Total Effect (ATE) =
Average Controlled Direct Effect (ACDE) + Eliminated Effect at each education level



***Notes*:** Estimates for the **Eliminated effect** = **ATE**−**ACDE** are shown in **circles**. Estimates for the **Effect of accent without fixing income (ATE)** are shown in **squares**, while estimates for the **Effect of accent fixing income (ACDE)** are shown in **triangles**. Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels 5a to 5f are presented in all tasks to the respondent. However, the dependent variable in panel 5g is shown once per round. Estimates for the ATE and the ACDE in each panel correspond to the **population AMCE (*pAMCE*)** estimator as described in de la Cuesta et al. (2022), while the eliminated effect is defined as the difference between the ATE and the ACDE. Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Bootstrap 95% (darker line) and 90% (lighter line) confidence intervals are based on 5,000 bootstrap replications. Low and medium levels of education correspond to profiles with elementary, high school and technical degrees, while the high level includes those with undergraduate and postgraduate degrees.

Figure 6: Education and income as a causal mechanisms for class-based accents
Average Total Effect (ATE) = Average Controlled Direct Effect (ACDE) + Eliminated Effect at each education level



***Notes*:** Estimates for the **Eliminated effect** = **ATE**−**ACDE** are shown in **circles**. Estimates for the **Effect of accent without fixing income (ATE)** are shown in **squares**, while estimates for the **Effect of accent fixing income (ACDE)** are shown in **triangles**. Following the analysis in Table 2 and the research design in Figure 1, estimates in this figure correspond to setting experience to average, while allowing education and income to vary by profile. Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels 5a to 5f are presented in all tasks to the respondent. However, the dependent variable in panel 5g is shown once per round. Estimates for the ATE and the ACDE in each panel correspond to the **population AMCE (*pAMCE*)** estimator as described in de la Cuesta et al. (2022), while the eliminated effect is defined as the difference between the ATE and the ACDE. Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Bootstrap 95% (darker line) and 90% (lighter line) confidence intervals are based on 5,000 bootstrap replications. Low and medium levels of education correspond to profiles with elementary, high school and technical degrees, while high level includes those with undergraduate and postgraduate degrees. Low and middle income correspond to profiles with income levels from 0 to 4.2 million Colombian pesos, and high income corresponds to profiles with 4.2 million Colombian pesos or more.

A second notable result is that the Eliminated Effect is sometimes negative and significant. Recall that this measure aggregates two mechanisms: indirect effects (where accents influence beliefs about income, which then affect outcomes) and causal interactions (where income information alters the effect of accents). For instance, consider the negative Eliminated Effect in panel g, which examines expectations about labor market opportunities. This effect is unlikely to stem from an indirect effect. Since income and education increase the likelihood of a profile getting a job, and respondents associate high-class accents with higher socioeconomic status, an indirect effect would predict a positive Eliminated Effect. Instead, the negative effect suggests a causal interaction–providing explicit income information may shift how respondents interpret accents. Such interaction effects are quite plausible in our contest. For example, a low-income individual with a high-class accent might be perceived as attempting to mask their social background, a trait that could be viewed either positively or negatively.

Similar patterns appear when considering education as a mediator (Figure 5). Here we compare low/middle education (elementary, high school, and technical degrees) to high education (undergraduate and postgraduate). Again, the ACDE is positive and significant for all seven outcomes, confirming that class-based accents have a direct effect beyond their role in signaling income or education. The Eliminated Effect is comparably small, especially when fixing education at low to medium levels. For higher education levels it is positive, raising the possibility that accents at least partly signal education, leading to positive outcomes. However, even in these instances, the ACDE remains positive, significant, and large, demonstrating that accents matter directly for these outcomes, not only because subjects use them to infer outcome-relevant education levels. Finally, like when controlling for income, the Eliminated Effect is negative for perceived chances of receiving a job opportunity (panel g), reinforcing that causal interactions plausibly shape this outcome in complex ways.[16]

In Figure 6, we decompose the ATE of class-based accents into the ACDE and Eliminated Effect, while holding *combinations* of income and education fixed.[17] The results show that the ACDE is consistently positive and significant across all outcomes, again indicating that class-based accents directly shape perceptions, independent of inferred socioeconomic status. This direct effect is particularly strong for trustworthiness, job opportunities, and leadership in the workplace (boss). While the ACDE remains robust across all education and income groups, it is somewhat weaker among high-income, high-education profiles, implying that when other explicit indicators of status are present, the direct impact of accents is slightly reduced but still meaningful. Overall, these findings confirm that class-based accents persistently influence social and economic perceptions, reinforcing socioeconomic disparities even in contexts where education and income gaps are minimized.

These findings have significant policy implications. Even if public interventions reduce economic and educational gaps, direct effects of class-based accents may sustain socioeconomic disparities. Below, we argue that these effects likely stem from differences in cultural and social

[16]The corresponding analysis with experience as a mediator is in Appendix Figure A.5. As in our pilot results, which motivated conceiving experience mainly as a control, the ATE and ACDE are very similar throughout most outcomes, with little evidence of meaningful causal mechanisms involved.

[17]Throughout the table, experience is fixed at the medium or "average for the age" level.

resources, highlighting the need for policies that address disparities in access to these forms of capital.

High-class premium across different profile and subject characteristics. We now examine how the effects of high-class accents vary by the profiles' education and income levels and by respondents' socioeconomic status (SES).

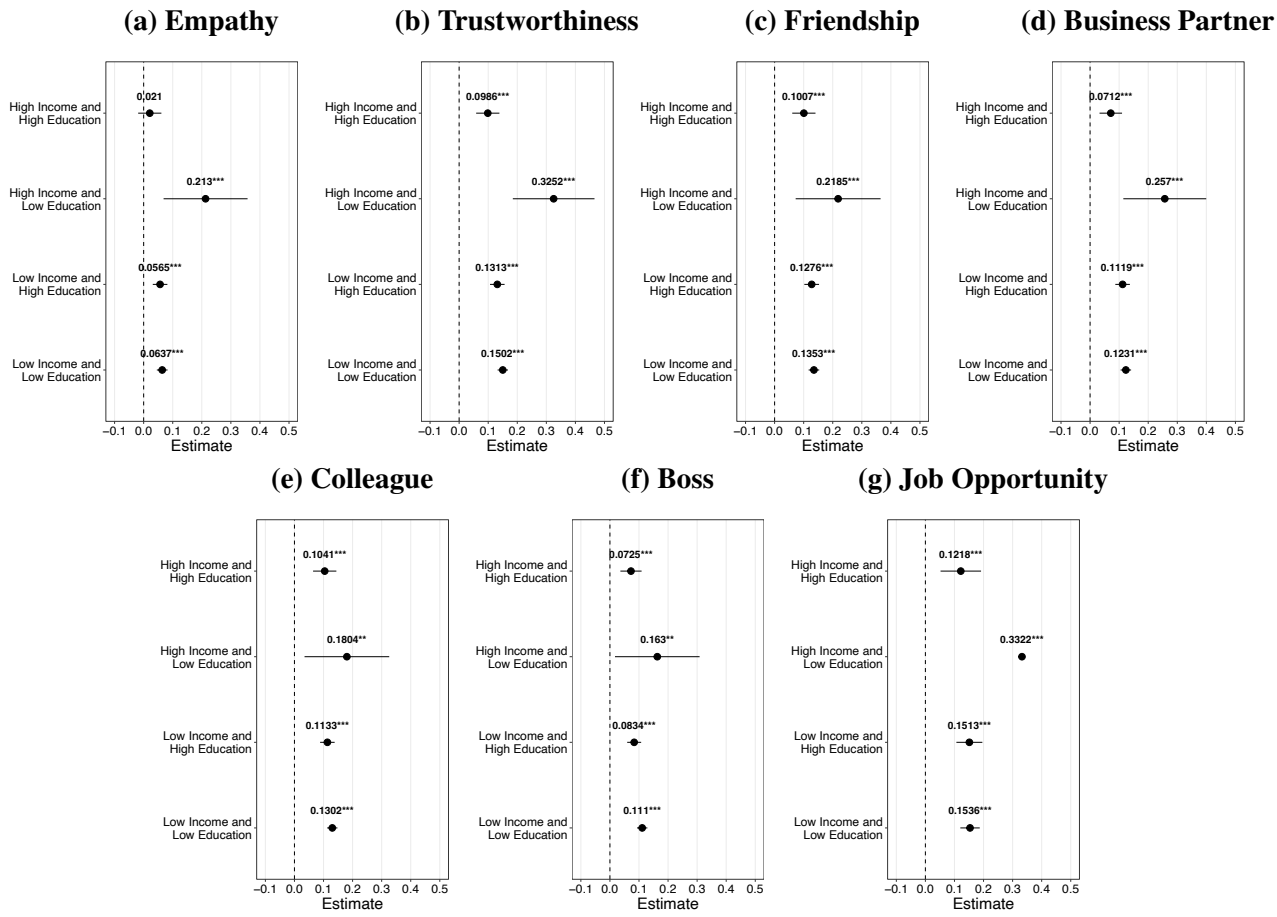
Figure 7 presents pAMCE estimates for the impact of a high-class accent across different income and education levels in the conjoint experiment. Across all outcomes, the strongest positive effects occur when a high-class accent is paired with high income but low education, particularly for the outcomes measuring empathy, trustworthiness, business partnerships, and job opportunities. These results suggest that class-based accents act as status signals, but their impact depends on the presence of other high-status traits.

One possible interpretation is that once income and education are both high, the additional boost that a person experiences from a high-class accent is comparatively less. At the other extreme, a high-class accent still provides an advantage when both income and education are low, but the effect is smaller, likely because the signal lacks credibility. The fact that high income combined with low education yields a greater accent premium than high education with low income suggests that respondents view accents as a stronger substitute for education than for income. This aligns with Colombia's highly segregated education system, where respondents might override the "low education" signal when encountering an accent typically corresponding to well-educated elites.

Next, in Table 3, we ask whether a particular socioeconomic subgroup drives preferences for high-class accents, using principal component analysis (PCA) to measure SES. This measure combines questions regarding income, wealth, and individuals' self-reported *estrato*, taken from their electricity bill. We interact the high-class accent indicator with a dummy for high SES (above the median). The direct high-class accent coefficient, capturing the premium among low SES subjects, is positive and significant across all seven outcomes. Low SES respondents perceive individuals with high-class accents as more capable, trustworthy, and preferable for business and workplace interactions. The effect is particularly strong for trustworthiness (0.15), workplace preference (0.13), and likelihood of receiving the letter (0.1604). In addition, highlighting the role of group identity and boundaries, the interaction between a high-class accent and respondents' SES reveals that high-SES respondents exhibit stronger biases in favor of high-class accents.[18]

[18] We also estimate marginal means to account for differences in preferences across subgroups (see Appendix Figure A.3).

Figure 7: Conjoint tasks with full information (version A)
PAMCE estimates by high and low income and education levels



Note: This figure presents estimates for the **population AMCE (*pAMCE*)** estimator as described in de la Cuesta et al. (2022). Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels 7a to 7f are presented in all tasks to the respondent. However, variable in panel 7g is shown once per round. All panels include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. However, we present only estimates for accent across all possible groups of high and low income-education groups. We define high income as all the profiles with more than $4,200,001, and low income as all the profiles with income lower than $4,200,000. Similarly, we define high education as profiles with undergraduate and postgraduate degrees, and low education as profiles with elementary, high school, and technical degrees. Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Confidence intervals are shown at the 95% level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 3: Conjoint tasks with full information (version A), heterogeneous treatment effects by respondents' SES

	Dependent Variable: (With) which of the two individuals...						
	Seems more capable of steeping into the other's shoes?	Inspires more trustworthiness?	Would you prefer to have a friendship?	Would you prefer to do business?	Would you prefer to share your workplace?	Is more likely to be your boss?	Is more likely to receive the letter?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accent - Baseline: *Low Class*							
High Class	0.0537***	0.147***	0.1392***	0.1177***	0.131***	0.107***	0.1604***
	(0.0093)	(0.009)	(0.009)	(0.0091)	(0.0087)	(0.0089)	(0.0162)
Socioeconomic Status (SES) - Baseline: *Low SES*							
Respondent SES:	0.0064	0.0404***	0.0448***	0.0174**	0.0334***	0.0229***	0.0318**
	(0.0071)	(0.0072)	(0.007)	(0.0069)	(0.0069)	(0.0069)	(0.0124)
Interaction *Accent x Respondent SES*							
Accent: High Class × SES: High	0.033***	0.1041***	0.1062***	0.072***	0.0906***	0.0719***	0.107***
	(0.0093)	(0.0092)	(0.009)	(0.0092)	(0.0088)	(0.009)	(0.0165)
Income - Baseline: *0-1,100,100*							
1,100,001 - 2,600,000	-0.0194*	0.0109	-0.0035	0.0643***	0.0095	0.1115***	0.0601***
	(0.0113)	(0.0113)	(0.0111)	(0.0116)	(0.0114)	(0.0114)	(0.0209)
2,600,001 - 4,200,000	-0.0516***	0.0048	-0.043***	0.1416***	0.0108	0.2098***	0.1114***
	(0.0156)	(0.0157)	(0.0157)	(0.0157)	(0.0153)	(0.0155)	(0.029)
4,200,001 - 6,800,000	-0.0921***	0	-0.0571***	0.1584***	0.0191	0.2474***	0.1213***
	(0.0186)	(0.0187)	(0.0187)	(0.0179)	(0.0184)	(0.0178)	(0.0333)
More than 6,800,000	-0.1403***	-0.039*	-0.0779***	0.1674***	-0.0139	0.3035***	0.188***
	(0.0218)	(0.0216)	(0.0215)	(0.0211)	(0.0213)	(0.0199)	(0.038)
Education - Baseline: *Elementary*							
High School	0.0508**	0.0487**	0.0343*	0.0774***	0.0521***	0.0994***	0.1354***
	(0.0202)	(0.02)	(0.02)	(0.0198)	(0.02)	(0.0195)	(0.036)
Technical	0.036*	0.0619***	0.0221	0.1264***	0.0971***	0.1993***	0.2019***
	(0.0215)	(0.021)	(0.0212)	(0.0204)	(0.0212)	(0.0207)	(0.0385)
Undergraduate	-0.0107	0.0442**	0.006	0.156***	0.0707***	0.2617***	0.2753***
	(0.0213)	(0.0203)	(0.0208)	(0.0203)	(0.0206)	(0.0202)	(0.037)
Postgraduate	-0.0667***	0.0466**	-0.0059	0.2212***	0.0685***	0.3592***	0.3501***
	(0.0239)	(0.0233)	(0.0237)	(0.0227)	(0.0239)	(0.0226)	(0.0411)
Sex - Baseline: *Male*							
Female	0.0638***	0.1014***	0.0931***	0.0716***	0.0864***	0.072***	0.0769***
	(0.0102)	(0.0103)	(0.0101)	(0.0099)	(0.01)	(0.0098)	(0.0179)
Experience - Baseline: *Low*							
Generous	0.1709***	0.1655***	0.1178***	0.0917***	0.1104***	0.0422***	0.0557**
	(0.0145)	(0.0146)	(0.0141)	(0.0144)	(0.0144)	(0.0148)	(0.0265)
Persistent	-0.0303**	0.0734***	-0.0345**	0.1454***	0.0526***	0.1148***	0.1222***
	(0.0151)	(0.0151)	(0.0155)	(0.0151)	(0.0157)	(0.0148)	(0.0269)
Calm	0.0618***	0.0846***	0.0054	0.0886***	0.0826***	0.056***	0.0619**
	(0.0152)	(0.0149)	(0.0148)	(0.0147)	(0.015)	(0.0151)	(0.0263)
Imaginative	0.0409***	0.0731***	0.0334**	0.1171***	0.0728***	0.0733***	0.1165***
	(0.0152)	(0.0146)	(0.0148)	(0.0147)	(0.0152)	(0.0147)	(0.0264)
Big 5 - Baseline: *Extroverted*							
Average	0.0097	0.0343***	0.0059	0.0529***	0.0347***	0.0728***	0.0386*
	(0.0107)	(0.0107)	(0.0106)	(0.0107)	(0.0107)	(0.0107)	(0.02)
High	0.0108	0.0374***	0.0194*	0.0744***	0.0366***	0.1099***	0.0881***
	(0.0108)	(0.0107)	(0.0107)	(0.0108)	(0.0109)	(0.0106)	(0.0197)
Number of Respondents	2,139	2,139	2,139	2,139	2,139	2,139	1,947
Observations	16,287	16,287	16,287	16,287	16,287	16,287	4,689

Notes*:** Estimates in this table correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Variables reported in this table are defined as follows. All variables take the value of one if the profile is selected, zero otherwise. Variables from column one trough six are shown in every task presented to the respondent. However, variable in column 7 is shown only one time per round. All columns include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Downstream effects. We now examine whether making class-based accents more salient through simple exposure to our audios has any downstream effects on beliefs about how society functions. Table 4 presents the differences between respondents assigned to conjoint versions with audio snippets featuring class-based accents and those who completed the tasks using only written information. Since versions A and B provided different levels of information, Panel A compares all respondents who heard an audio message (groups A and B in Figure 1) to those who did not (group C, the control group), while Panel B focuses on the comparison between groups A and C.

Results show that individuals exposed to audio snippets exhibit higher levels of familism, meaning they place greater trust in close relatives than strangers (column 1). However, there is no effect on a common measure of interpersonal trust——the perceived likelihood of a lost wallet being returned (column 2). Similarly, we find no significant impact on institutional trust (column 3), economic optimism (column 4), perceptions of social mobility (column 5), or beliefs about meritocracy (column 6). Notably, respondents who listened to class-based accents are more likely to rank inequality as Colombia's most pressing issue (by 2.8 and 3.2 percentage points in Panels A and B).[19]

The findings on familism and heightened concern for inequality suggest that class-based accents increase awareness of social divisions and their role in persistent inequality. We also examine how these effects vary with respondents' socioeconomic status (SES) in Appendix Table A.7. While interpersonal and institutional trust results are less conclusive, increased concern for inequality is primarily found among respondents with low SES, as expected.

The evidence on downstream effects remains suggestive, as we find no corresponding pessimism on related concepts like perceptions of social mobility and meritocracy. Additionally, while the Benjamini and Hochberg p-values for multiple hypothesis correction are 0.046 and 0.094 in Panels A and B, the p-values for inequality are much higher (0.25 in both panels), indicating a lack of statistical significance at conventional levels.[20]

We do not believe the non-effects are due to a lack of statistical power (see the power analysis preregistered in our pre-analysis plan in Appendix C.3). We thus explore whether including pretreatment covariates as controls can reduce variance and increase the statistical power of our tests. We rely on lasso selector models in Appendix Table A.6. While the effect of reduced trust in the government becomes significant for both sets of comparisons, the outcomes for which we find effects remain unchanged.[21]

Additional results and robustness. We tested whether results varied based on the perceived realism of the profiles. Although profile randomization accounted for the joint distribution of sex, income, and education (see Appendix Table A.1), some profiles may still have seemed unrealistic. To assess this, we included a social-class classification task in the pilot phase, where respondents

[19] Appendix Table A.5 shows results for components of the indices, noting that the impact on familism is primarily driven by an increase in trust towards friends and a decrease in trust towards foreigners. Also, the rise in concern for inequality is matched with a decrease in concern for unemployment.

[20] Appendix Figure A.6 shows p-values for other multiple hypotheses correction methods, with similar findings.

[21] The limited impact of covariates is not surprising, as they are similar between experimental conditions (see Appendix Table A.2).

Table 4: Downstream effects

	Dependent variable:						
	Index of familism	Probability of returning a lost wallet	Index of Institutional trust	Optimism future	Social mobility perceptions in Colombia	Low meritocracy	Inequality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. A + B vs. C							
Class-based accents = 1	0.107***	0.0398	-0.0384	0.0488	-0.0043	0.0013	0.0281*
	(0.0394)	(0.0475)	(0.0421)	(0.0306)	(0.0409)	(0.0159)	(0.0171)
Control Mean	1.624	2.971	2.469	2.381	2.395	0.814	0.246
Control Sd	0.993	1.199	1.068	0.744	1.031	0.39	0.431
Observations	6067	6028	6079	5657	6037	5797	6107
R2	0.0012	0.0001	0.0001	0.0004	0.0000	0.0000	0.0004
Benjamini & Hochberg (1995) p-value	0.0463	0.5627	0.5627	0.2581	0.9362	0.9362	0.2581
Panel B. A vs. C							
Class-based accents = 1	0.1021**	0.0562	-0.051	0.051	0.0037	0.0094	0.0315*
	(0.0413)	(0.0496)	(0.0439)	(0.032)	(0.0427)	(0.0165)	(0.0179)
Control Mean	1.624	2.971	2.469	2.381	2.395	0.814	0.246
Control Sd	0.993	1.199	1.068	0.744	1.031	0.39	0.431
Observations	6067	6028	6079	5657	6037	5797	6107
R2	0.0012	0.0001	0.0001	0.0004	0.0000	0.0000	0.0004
Benjamini & Hochberg (1995) p-value	0.0943	0.3601	0.3601	0.2579	0.9307	0.664	0.2579

***Notes*:** All outcome variables presented in the table are expressed in terms of levels. The index of familism is computed as the difference between mean trust in family and friends and mean trust in foreigners and strangers. The index of institutional trust is computed as mean trust in government and democratic institutions. Questions corresponding to each question in order from left to right: *What is the probability that a lost wallet, containing valuable documents and money, will be returned to its owner?* (1 = close to 0, 5 = quite high), in column 2; *When you think about the future, how do you think your economic situation will be in the next 12 months?* (1 = better), in column 4; *Suppose Colombia is divided into 5 groups, where group 1 is made up of the 100 poorest families and group 5 is made up of the 100 richest families. What do you think the probability is that a person from the 100 poorest families will make it into one of the 100 richest families in the future?* (1 = close to 0, 5 = quite high), in column 5; *What do you think has more to do with a person being rich in Colombia?* (1 = because they had more advantages than others), in column 6; and *In your opinion, what are the main problems affecting Colombia?* (1 = inequality), in column 7. Robust standard errors are presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

indicated whether each profile appeared unrealistic. Using this data, we estimated the likelihood of a profile being perceived as unrealistic based on its attributes.[22] As shown in Appendix Tables A.8 and A.9, the high-class accent premium emerges only among profiles classified as realistic.

Recall that participants first completed four conjoint tasks, with up to three additional invitations to complete three more. Most completed two rounds (seven tasks, see Appendix Figure A.1). Robustness checks confirm that results remain consistent when restricting analysis to first-round tasks, addressing concerns about potential differences between those who completed additional rounds and those who did not (see Appendix Figure A.4).

One concern in our analysis is social desirability bias (SDB), where respondents answer in ways they believe will be viewed favorably. To assess this, our pilot study included a 13-item short form of the Marlowe-Crowne Social Desirability Scale (MCSDS) (Crowne and Marlowe, 1960; Reynolds, 1978,9), where respondents agree or disagree with statements presenting socially approved but improbable behaviors or common but undesirable behaviors.[23] Appendix Table A.12

[22] Profiles were classified as realistic if they fell under the 75th percentile of this predicted probability, corresponding to a 0.38 likelihood. Appendix Figure A.8 displays the distribution of predicted probabilities, highlighting those above the 75th percentile. Since these probabilities remain relatively low, this further supports our design.

[23] Appendix Figure A.9 shows the distribution of scores from zero (least conforming) to thirteen (most conforming).

presents regressions on social class categorization (social class scale, sports, leisure and musical preferences, and perceived social influence of the profile's social network). Panel A includes the (demeaned) MCSDS scale and its interaction with High Class as regressors, while Panel B replaces this with an indicator for high SDB sensitivity (above-median MCSDS scores). Individual SDB levels are orthogonal to treatments by design, confirmed by the MCSDS coefficients, which are consistently precise zeros. Crucially, the interaction terms show no systematic differences in classification choices, lessening concerns that hearing a high-class accent could trigger conformity among socially sensitive subjects. This strengthens our confidence that the experiment is free from this bias, reinforcing the advantage often attributed to conjoint experiments.

Our PAP also outlined heterogeneous effects based on subjects' pro-social views and beliefs, including generalized trust, reciprocity, altruism, and concern for fairness. Appendix Table A.10 shows no robust or systematic differences in our main outcomes across these dimensions.

Statistical- and taste-based discrimination. Our approach parallels classic statistical versus taste-based discrimination frameworks. Our leave-one-out approach can be reframed as testing whether people use accents as signals for other correlated traits, engaging in statistical discrimination. Controlling for these correlated traits—especially income and education—would isolate pure taste-based discrimination based on accents. Much of the economic discrimination literature thus compares estimations with full and partial information to learn about subjects' inference processes (Bertrand and Duflo, 2017). The effect of class-based accents would increase when the subject observes partial information if they use this marker to infer the positively correlated missing dimension (income, education, or both). However, Appendix Table A.4 shows that estimates remain similar across full and partial information conditions, dismissing this possibility and reinforcing our structured approach to causal mechanisms (Acharya et al., 2018).

We recognize that distinguishing between statistical and taste-based discrimination is more complex and depends on the cognitive process.[24] More broadly, we agree with Bertrand and Duflo (2017) that the sharp distinction economists draw between statistical and taste-based discrimination is often blurred by insights from other disciplines. A more productive approach is to design experiments that directly test the theories of interest. In our case, our primary aim is to determine whether class-based accents matter because they signal income or education, or whether they independently influence key outcomes.

4 Discussion

Our results demonstrate that high-class accents confer distinct advantages for both interpersonal relationships and labor market outcomes. These impacts are not due solely to income or

Our sample (N=765) exhibits substantial variation, with an average score of 8.

[24]For example, Bartoš et al. (2016) introduce "attention discrimination," where people pay less attention to certain profile features that trigger prejudice. Bohren et al. (2023) show how inaccurate beliefs lead to *inaccurate* statistical discrimination. In housing markets, Ewens et al. (2014) examine how statistical discriminators process signals differently by race. These studies highlight that comparing full and partial information environments can lead to misleading conclusions.

educational disparities and are present for all SES levels, yet they are stronger among high SES individuals. Furthermore, merely highlighting class-based accents appears to alter societal perceptions, increasing trust disparities between close acquaintances and distant strangers while intensifying concerns about inequality. In short, the high-class accent premium hinders the social mobility of lower-class individuals and undermines social cohesion.

Our finding that class-based accents reflect more than income and education raises the question of what mechanisms might explain their influence. We now consider and progressively discard several possibilities, and then advance our preferred interpretation with a set of complementary findings.

One possibility is that accents reflect wealth, which we do not control for in our experiments. This seems unlikely for several reasons. First, income and wealth are very strongly correlated in our setting. Thus, income differences that are accounted for will largely proxy for wealth. Second, recall that the high-class accent premium is palpable for profiles with high income, precisely where respondents could expect them to possess wealth.

A second possibility is that accents capture ethnic or racial differences. This is also implausible in our context. First, our professional actors used accent variations that conveyed no ethnic or racial backgrounds. Class-based variations cut across ethnic and racial dimensions, with socioeconomic status being the defining distinctive feature between the high- and low-status accents they portrayed. Second, actors could do this precisely because there is an imperfect correlation between those features and socioeconomic status in Bogotá. This is not to deny that, in Colombia at large, Black people are economically disadvantaged. Had the actors portrayed a Black accent (recurring in this case to regional variations), we plausibly would have found discrimination against them. Appendix Figure A.2 shows the correlation between household income and skin color from the 2018 Latin American Public Opinion Project round. While negative, this correlation is small (-0.059) in Bogotá and about half the magnitude for Colombia.

To offer an interpretation, consider grouping all forms of capital into four broad categories: economic, human, social, and cultural Cárdenas et al. (2021). We define *economic capital* as assets that can be directly converted into money (Bourdieu, 1986). *Human capital* refers to knowledge, skills, and health that enhance economic productivity. While *social capital* can be studied at the community level, we define it at the individual level, capturing the value derived from personal networks. Finally, *cultural capital* consists of knowledge, skills, and behaviors that confer social status. These are embedded in norms that grant privilege (e.g., vocabulary, etiquette), objectified in goods that signal prestige, and institutionalized through formal recognition (e.g., academic degrees or certifications).

Having accounted for direct indicators of human and economic capital in our experiment, accents likely matter partly because they convey valuable social and cultural capital.[25] Sociologists

[25] Cárdenas et al. (2021) describe social and cultural resources as *immaterial social assets*. The economic value of social capital depends on relationships and is socially determined, while cultural capital derives its worth from society's attribution of distinction to behaviors and possessions. In contrast, though socially constructed, economic capital remains non-social in an impersonal marketplace where $100 always holds the same purchasing power regardless of social ties. Despite the rise of intangible forms of money, economic capital remains the most material. Human capital

have long argued that cultural and social resources are crucial to explaining the persistence and reproduction of socioeconomic inequality (e.g., Bourdieu 1994; Bennett et al. 2009), with language playing a critical role in signaling such resources (e.g., Bourdieu 1991). In unequal and segregated societies, cultural tastes diverge between high and low social classes, resulting in differential and systematically structured access to opportunities (Chan, 2010; Breen and Goldthorpe, 1999; Lareau and Weininger, 2003; Rivera, 2012; Rivera and Tilcsik, 2016). Cultural capital allows some individuals to acquire a certain level of prestige within society, providing them with more potential pathways to success (Reeves and De Vries, 2019), including converting their cultural capital into social capital (Lewis and Kaufman, 2018).

Accents can serve as precise indicators of these cultural and social resources. A class-based accent signals both cultural affinity and group belonging. While correlated with other forms of capital, it does not directly measure knowledge and cognitive skills associated with human capital or income linked to economic capital.[26] We now marshal a set of complementary findings that reinforce the idea that class-based accents act as signals of cultural and social resources.

Classifying class, tastes and social influence. Panels A and B of Table 5 present results from our pilot study, which validated our class-based accents and provided insights into their effects. Respondents classified each profile's social class as high, middle, or low and predicted their likely preferences for sports, leisure activities, and music. Each category included three options, ranging from those typical of higher classes to those more common among lower classes.[27]

When *Bogotanos* classify *Bogotanos* and *Santiaguinos* classify *Santiaguinos*, the high-class accent increases the ordinal classification for social class, music, leisure activities, and sports activities. This provides evidence that our voice actors successfully portrayed social class with their choice of accents, and culturally proximate subjects could infer social class from the audio snippets, whether directly positioning profiles in a social class scale or via the preferences most commonly associated with a class. These effects control for the remaining profile attributes, with coefficients consistent with expectations (e.g. the more affluent and educated typically rank higher).

Taken together, the results are consistent with the high-class accent serving as a powerful signal of cultural features (columns 2 to 4) and social resources (column 5), which help explain its

is immaterial but also non-social, as its value is relatively independent of social relations in a market setting.

[26]Following Bourdieu's view that cultural capital helps wealthier children acquire educational credentials, most studies measure wealthy through individual or family educational attainment, although this may also reflect human capital effects. An alternative measure, the DiMaggio scale (DiMaggio, 1982), assesses cultural preferences and tastes, such as interest in artistic activities and occupations. However, since some tastes are more expensive and partly shaped by education, the scale correlates with economic and human capital, making it difficult to separate their effects fully.

[27]In Bogotá, the options were as follows. Sports: Horse riding, tennis, and Sundays at the *ciclovía* (a public program that transforms streets into car-free zones on Sundays and public holidays). Leisure activities: Theater, movies, and flying a kite in a park. Music preference: Jazz, salsa, and *vallenato* (a traditional Caribbean Colombian genre blending folk storytelling with distinctive rhythms). For Chile, the categories where the same but included jazz, classic rock, and *cumbia villera*—a subgenre of cumbia that emerged in Argentina in the late 1990s and gained popularity in Chile's working-class neighborhoods. To determine each category, we consulted with several local experts in each country, and, in the case of Colombia, we also verified that the music consumption patterns by socioeconomic status match those in the 2020 Cultural Consumption Survey of the National Statistical Institute, DANE.

Table 5: Social class classification in Bogotá and Santiago de Chile

	Dependent variable:				
	Social Class	Sport	Leisure	Music	Social Influence
	(1)	(2)	(3)	(4)	(5)
Panel A. *Bogotanos* Classifying *Bogotanos*					
Accent - Baseline: *Low Class*					
Accent: High Class	0.201**	0.137	0.148*	0.210**	0.453***
	(0.064)	(0.084)	(0.071)	(0.075)	(0.106)
Observations	338	338	337	338	335
Mean DV	2.148	1.636	2.205	2.186	3.224
SD DV	0.613	0.711	0.666	0.683	0.988
R-squared	0.245	0.104	0.114	0.098	0.141
Number of NA	0	0	1	0	3
Panel B. *Santiaguinos* Classifying *Santiaguinos*					
Accent - Baseline: *Low Class*					
Accent: High Class	0.199**	0.141	0.292**	0.408***	0.528**
	(0.060)	(0.080)	(0.092)	(0.081)	(0.160)
Observations	258	258	258	258	258
Mean DV	1.907	1.403	2.101	1.973	3.043
SD DV	0.659	0.630	0.726	0.730	1.074
R-squared	0.436	0.210	0.289	0.245	0.239
Number of NA	0	0	0	0	0
Panel C. *Bogotanos* Classifying *Santiaguinos*					
Accent - Baseline: *Low Class*					
Accent: High Class	0.047	0.085	0.117	0.011	-0.015
	(0.057)	(0.074)	(0.071)	(0.091)	(0.082)
Observations	342	341	342	342	340
Mean DV	2.023	1.545	2.105	2.070	3.212
SD DV	0.612	0.708	0.707	0.782	0.951
R-squared	0.261	0.187	0.220	0.138	0.185
Number of NA	0	1	0	0	2

***Notes*:** The outcome variable for columns (1) to (5) is a categorical variable that takes the values of "low", "middle", and "high". The table presents results for the "Audio Profile" task type sample. Panel A presents estimates for the *Bogotanos* classifying *Bogotanos* sample. Panel B presents estimates for the *Santiaguinos* classifying *Santiaguinos* sample. Panel C presents estimates for the cross-region estimates, with *Bogotanos* classifying *Santiaguinos*. Robust standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

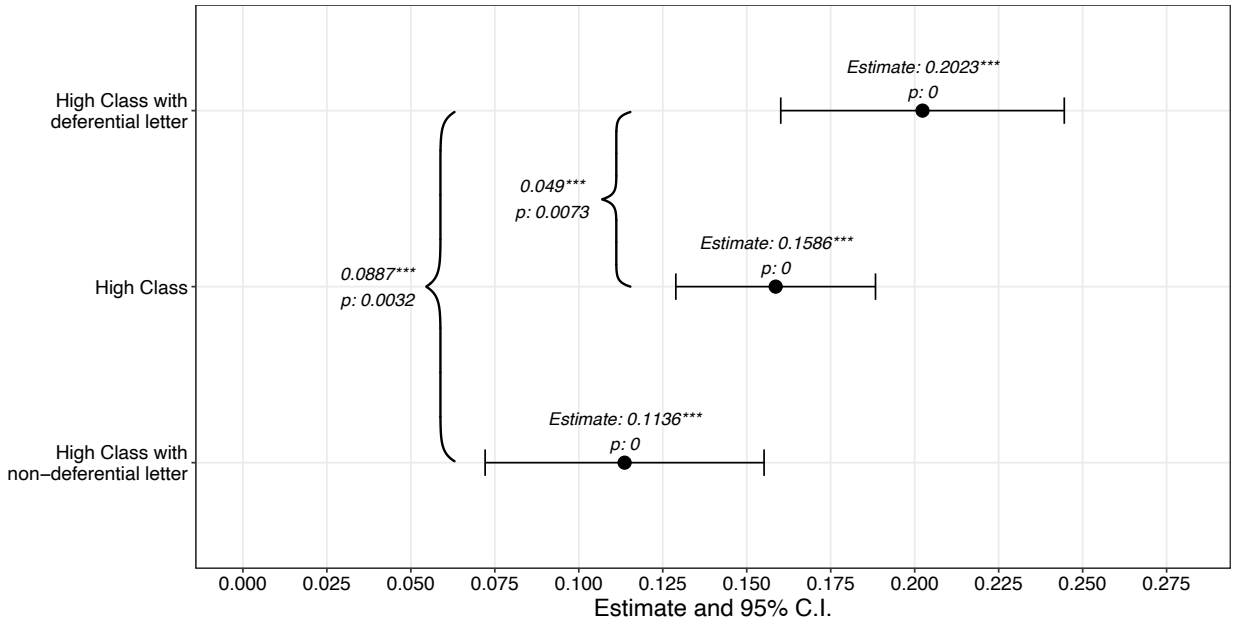
premium in interpersonal and labor market contexts.

The role of language: deferential letter experiment Figure 8 provides results for the deferential letter experiment. A high-class accent increases the likelihood that respondents attribute a letter to a profile by 15.9 percentage points on average. However, class-based accent cues interact with linguistic style to enhance perceptions of labor market success. When deferential language is

used, the high-class premium equals 20 percentage points, compared to 11 percentage points when the letter is informal.[28]

When manipulating the audio snippets, we intentionally avoided language variations since these could transmit other features like speaking ability and education, influencing vocabulary choice. However, the results of the deferential letter experiment help highlight the role of language in establishing social hierarchies. In this sense, they are consistent with our interpretation of the independent effect of class-based accents beyond income and education.

Figure 8: Comparison of point estimates for conjoint tasks with full information (version A) for the deferential letter experiment



Notes*:** This figure reports estimates for the high class premium across our deferential letter treatment. Coefficients correspond to the estimates themselves or a linear combination of estimates in Table **??**. The "high Class" coefficient reported corresponds to the first column in Table **??**. The "High Class with non-deferential letter" coefficient corresponds to the second column coefficient of high-class accent. In curly brackets we present the estimate and p-value of the difference in our highlighted estimates. Estimates in this figure correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Confidence intervals are shown at the 95% level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

High-class premium across borders. In Panel C of Table 5, we examine how subjects classify profiles with high- and low-class *Santiaguino* accents. When *Bogotano* respondents assess *Santiaguino* profiles, the high-class accent does not lead to higher ordinal classifications for social class, sports, leisure, or music (columns 1–4). Similarly, it does not increase the likelihood of being associated with more socially influential acquaintances (column 5). Instead, more objective attributes such as income and education—less dependent on cultural context—drive higher rankings.

[28] Appendix Table A.11 presents these results in regression format (Columns 1 and 2). Contrary to our PAP hypotheses, we find no evidence that low-SES respondents were more likely to believe a speaker with a high-class accent received the deferential letter (Column 3). The table also includes p-values corrected using the Family-Wise Error Rate (FWER) Bonferroni adjustment. Column 2 shows that this correction increases the p-value for the interaction between class accent and the deferential letter from $p < 0.01$ to 0.06.

We also tested whether high-class foreign accents are preferred in personal and labor market outcomes using our standard conjoint analysis. Figure 9 presents marginal means by region and accent, showing that the high-class premium for *Santiaguino* accents—the difference in bracket terms between marginal means—is consistently much smaller than for *Bogotano* accents. These effects never exceed 2.5 percentage points, indicating a negligible impact.

The *Santiaguino* accent itself can have positive or negative effects depending on the outcome. Overall, *Santiaguino* profiles show minimal disadvantages across domains, as their marginal means tend to be slightly below 0.5.[29]

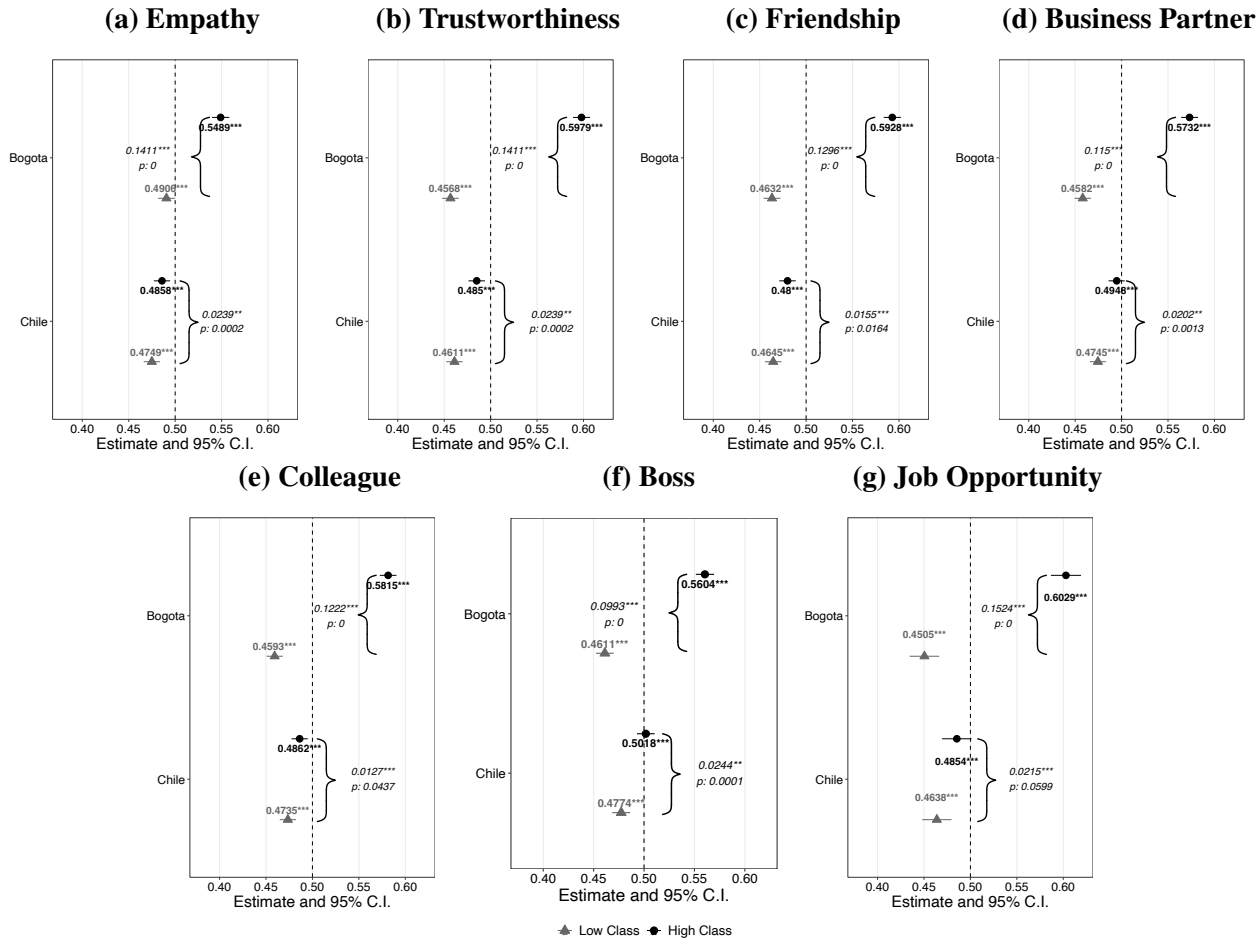
Sociolinguistics acknowledges both universal and culture-specific aspects of speech, though the balance between them remains debated (Schuller and Batliner, 2013). Socioeconomically advantaged individuals may exhibit confidence, assertiveness, or kindness, potentially leading to universally recognizable speech patterns. Thus, a possible critique of our findings is that they might capture these traits rather than class-based distinctions. Our experimental design minimizes this risk by controlling for personality traits and ensuring actors follow strict guidelines on language, speed, filler words, and pauses. Moreover, if such traits were universally linked to high-status individuals, we would expect a high-class premium for *Santiaguino* accents, which we do not observe. This confirms that class-based accents serve as culturally determined status markers rather than universal indicators of desirable traits.[30]

These results have broader implications for migration. As noted in the introduction, migration is often seen as an economically beneficial but culturally challenging path to upward mobility. However, class-based cultural differences complicate this view. Natives struggle to recognize class-based cues among migrants—especially accent-based ones—as readily as they do among fellow natives. This dynamic could lead to more equal perceptions of migrants from different social classes compared to natives, as accent ceases to reliably signal social status in cross-cultural interactions.

[29] However, this is not our main focus. More importantly, these effects likely depend on the foreigner's national identity. For instance, Venezuelan migrants—due to recent large migration waves—may face stigma in Colombia, while migrants from wealthier countries could be viewed more favorably. In contrast, our findings on the reduced class-based premium for foreign accents are likely more generalizable, as they capture broader cultural distance between nations.

[30] An alternative explanation is that migrants exhibit other highly salient characteristics, reducing class-based discrimination against lower-class accents as respondents focus on national origin. While we cannot fully distinguish between this "salience" hypothesis and the cultural distance explanation, our choice of a Santiaguino accent—rather than a Venezuelan accent, which might trigger stronger reactions due to recent migration waves—aims to isolate the latter effect.

Figure 9: Conjoint tasks with full information (version A)
Marginal means



Note: Estimates in this figure correspond to the differences in the **Marginal Means** (***MM***) estimator by region. Estimates for low class accent are shown in light colored triangles, while estimates for high class accent are shown darker colored circles. In curly brackets we present estimates for the differences in MM by region accompanied by their respective p-values. Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels 9a to 9f are presented in all tasks to the respondent. However, the dependent variable in panel 9g is shown once per round. All panels include our factor of interest *accent*, and all control variables such as income, education, sex, experience, and the big 5 personality traits. Confidence intervals are shown at the 95% level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

5 Conclusion

In socioeconomically stratified societies, social class is a crucial determinant of access to opportunities, with accents serving as a powerful marker of class and correlated factors. Using online survey experiments with 6,000 participants in Bogotá, Colombia, we isolate the effect of accents from these correlated factors, such as income and education. The controlled design of our conjoint experiment also removes potentially confounding attributes, such as a speaker's style of dress, body movements, and facial expressions, which could otherwise transmit signals that might contaminate the pure effect of class-based accents.

Our results reveal a strong preference for individuals with high-class accents, who are perceived as more empathetic, trustworthy, and likely to succeed in the labor market. These effects persist even when attributes that signal economic or human capital are held constant. Using the causal mechanisms approach proposed by Acharya et al. (2018), we further whether accents simply reflect the indirect impact of income and education. We find that high-class accents do indeed have direct impacts on our outcomes, over and beyond inferred income or education.

We also find that the strength of the high-class premium depends upon the socioeconomic status of respondents, and that people tend to favor those who share their accents: the high-class accent premium is more pronounced among those who themselves have high SES.

In our downstream effects analyses, we find that individuals who were randomly assigned to perform conjoint tasks that included audios that cue class-based accents, when compared to those reading otherwise similar descriptions of hypothetical individuals, have heightened awarness of social inequality. They also are less trusting of strangers and more trusting of those with familial connections. These results suggest that inequality's cultural and social dimensions affect a set of meaningful outcomes.

Our study underscores the critical role that class-based accents play in shaping access to social and economic opportunities. It also highlights the importance of interventions that could reduce the biases and barriers that class-based accents create and reinforce. One promising avenue may be creating spaces that foment contact among individuals possessing different accents. Another may be encouraging diverse representation of accents in media, advertising, and public communication, to normalize different ways of speaking and reduce negative stereotypes associated with culturally disadvantaged accents. For example, the BBC in the UK began introducing anchors with regional accents beyond Received Pronunciation (RP), also known as "Queen's English," aiming to diversify representation (e.g., Eschner, 2020; Crystal, 2022). Programs that help individuals with lower-class accents build social capital and access opportunities otherwise hindered by accent-related biases offer promising pathways to equity.

While class-based segregation in Colombia, particularly in Bogotá, provides a useful context for examining the effects of accents on individuals' attitudes and behaviors, the dynamics we observe are not confined to highly stratified societies in Latin America. Accents hold significance and influence in ways that transcend region, affecting social and economic dynamics across different cultural settings. Evidence from Germany, for example, shows that accent preferences and

biases are widespread and have substantial consequences for individuals' social interactions and perceptions (e.g., Rakić et al., 2011). In the United Kingdom, accents influence social evaluations from a young age (Kinzler et al., 2009). Public policies that solely address the material disparities underpinning class divisions, yet fail to tackle their social and cultural manifestations, may fall short if class-based accents hinder low-status individuals' opportunities in the labor market and interpersonal relationships more broadly.

REFERENCES

Acharya, A., Blackwell, M., and Sen, M. (2018). Analyzing causal mechanisms in survey experiments. *Political Analysis*, 26(4):357–378.

Akerlof, G. A. and Kranton, R. E. (2000). Economics and identity. *The quarterly journal of economics*, 115(3):715–753.

Álvarez-Rivadulla, M. J. (2024). *Costly Opportunities: Social Mobility in Segregated Societies*. Cambridge University Press.

Ambady, N. and Rosenthal, R. (1993). Half a minute: Predicting teacher evaluations from thin slices of nonverbal behavior and physical attractiveness. *Journal of personality and social psychology*, 64(3):431.

Avola, M. and Piccitto, G. (2020). Ethnic penalty and occupational mobility in the italian labour market. *Ethnicities*, 20(6):1093–1116.

Banco Mundial (2021). Hacia la construcción de una sociedad equitativa en colombia. *Banco Internacional de Reconstrucción y Fomento & Banco Mundial*.

Bank, W. (2018). Taking on inequality: Poverty and shared prosperity 2016.

Bartoš, V., Bauer, M., Chytilová, J., and Matějka, F. (2016). Attention discrimination: Theory and field experiments with monitoring information acquisition. *The American Economic Review*, 106(6):1437–75.

Bénabou, R. and Tirole, J. (2011). Identity, morals, and taboos: Beliefs as assets. *The quarterly journal of economics*, 126(2):805–855.

Benet-Martínez, V. and John, O. P. (1998). Los cinco grandes across cultures and ethnic groups: Multitrait-multimethod analyses of the big five in spanish and english. *Journal of personality and social psychology*, 75(3):729.

Bennett, T., Savage, M., Silva, E. B., Warde, A., Gayo-Cal, M., and Wright, D. (2009). *Culture, class, distinction*. Routledge.

Bertrand, M. and Duflo, E. (2017). Field experiments on discrimination. *Handbook of economic field experiments*, 1:309–393.

Bertrand, M., Goldin, C., and Katz, L. F. (2010). Dynamics of the gender gap for young professionals in the financial and corporate sectors. *American economic journal: applied economics*, 2(3):228–255.

Boas, T. C., Christenson, D. P., and Glick, D. M. (2020). Recruiting large online samples in the united states and india: Facebook, mechanical turk, and qualtrics. *Political Science Research and Methods*, 8(2):232–250.

Bohren, J. A., Haggag, K., Imas, A., and Pope, D. G. (2023). Inaccurate statistical discrimination: An identification problem. *Review of Economics and Statistics*, pages 1–45.

Bonomi, G., Gennaioli, N., and Tabellini, G. (2021). Identity, beliefs, and political conflict. *The Quarterly Journal of Economics*, 136(4):2371–2411.

Borjas, G. J. (1994). The economics of immigration. *Journal of Economic Literature*, 32(4):1667–1717.

Bourdieu, P. (1986). The forms of capital. In *Handbook of theory and research for the sociology of education*, pages 241–258. Greenwood.

Bourdieu, P. (1991). *Language and symbolic power*. Harvard University Press.

Bourdieu, P. (1994). Social space and symbolic space. In *Contemporary Sociological Theory*, pages 354–358.

Boustan, L. P., Abramitzky, R., and Eriksson, K. (2020). The effects of immigration on the economy: Lessons from the 1920s border closure. *Review of Economic Studies*, 87(1):383–419.

Bowles, S. and Gintis, H. (2002). The inheritance of inequality. *Journal of Economic Perspectives*, 16(3):3–30.

Breen, R. and Goldthorpe, J. H. (1999). Class inequality and meritocracy: a critique of saunders and an alternative analysis1. *The British journal of sociology*, 50(1):1–27.

Campbell-Kibler, K. (2010). Sociolinguistics and perception. *Language and Linguistics Compass*, 4(6):377–389.

Card, D. (2005). Is the new immigration really so bad? *Economic Journal*, 115(507):F300–F323.

Cárdenas, J. C., Fergusson, L., and Villegas, M. G. (2021). *La quinta puerta: de cómo la educación en Colombia agudiza las desigualdades en lugar de remediarlas*. Ariel.

Chan, T. W. (2010). *Social status and cultural consumption*. Cambridge University Press.

Chandra, A. (2000). Labor-market dropouts and the racial wage gap: 1940–1990. *American Economic Review*, 90(2):333–338.

Chernozhukov, V., Hansen, C., and Spindler, M. (2016). hdm: High-dimensional metrics. *R Journal*, 8(2):185–199.

Chetty, R., Dobbie, W. S., Goldman, B., Porter, S., and Yang, C. (2024). Changing opportunity: Sociological mechanisms underlying growing class gaps and shrinking race gaps in economic mobility. Technical report, National Bureau of Economic Research.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, 106(4):855–902.

Chetty, R., Hendren, N., Kline, P., and Saez, E. (2014). Where is the land of opportunity? the geography of intergenerational mobility in the united states. *The Quarterly Journal of Economics*, 129(4):1553–1623.

Corak, M. (2013). Income inequality, equality of opportunity, and intergenerational mobility. *Journal of Economic Perspectives*, 27(3):79–102.

Crowne, D. P. and Marlowe, D. (1960). A new scale of social desirability independent of psychopathology. *Journal of consulting psychology*, 24(4):349.

Crystal, D. (2022). Received pronunciation: Old and new. Accessed: 2024-08-27.

De la Cuesta, B., Egami, N., and Imai, K. (2022). Improving the external validity of conjoint analysis: The essential role of profile distribution. *Political Analysis*, 30(1):19–45.

de la Cuesta, B., Egami, N., and Imai, K. (2022). Improving the external validity of conjoint analysis: The essential role of profile distribution. *Political Analysis*, 30(1):19–45.

de la O, A., Rossel, C., and Manzi, P. (2023). Opting out from public services and the social contract in Latin America. *Latin America and Caribbean Inequality Review*, WORKING PAPER 127.

DiMaggio, P. (1982). Cultural capital and school success: The impact of status culture participation on the grades of us high school students. *American sociological review*, pages 189–201.

Dovidio, J. F., Gluszek, A., John, M.-S., Ditlmann, R., and Lagunes, P. (2010). Understanding bias toward latinos: Discrimination, dimensions of difference, and experience of exclusion. *Journal of Social Issues*, 66(1):59–78.

Durlauf, S. N. (1996). A theory of persistent income inequality. *Journal of Economic Growth*, 1(1):75–93.

Egami, N. (2022). R package factorex: Design and analysis for factorial experiments. https://github.com/naoki-egami/factorEx.

Eschner, K. (2020). How a north england voice overturned the bbc tradition. Accessed: 2024-08-27.

Eslava, F. and Valencia Caicedo, F. (2023). Origins of latin american inequality. *Latin America and Caribbean Inequality Review*, WORKING PAPER 95.

Ewens, M., Tomlin, B., and Wang, L. C. (2014). Statistical discrimination or prejudice? a large sample field experiment. *The Review of Economics and Statistics*, 96(1):119–134.

Fergusson, L. (2019). Who wants violence? the political economy of conflict and state building in colombia. *Cuadernos de Economía*, 38(78):671–699.

Fouka, V. and Tabellini, M. (2022). Changing in-group boundaries: The effect of immigration on race relations in the united states. *American Political Science Review*, 116(3):968–984.

Fuertes, J. N., Gottdiener, W. H., Martin, H., Gilbert, T. C., and Giles, H. (2012). A meta-analysis of the effects of speakers' accents on interpersonal evaluations. *European Journal of Social Psychology*, 42(1):120–133.

Grogger, J. (2011). Speech patterns and racial wage inequality. *Journal of Human resources*, 46(1):1–25.

Grow, A., Perrotta, D., Del Fava, E., Cimentada, J., Rampazzo, F., Gil-Clavel, S., Zagheni, E., Flores, R. D., Ventura, I., Weber, I., et al. (2021). How reliable is facebook's advertising data for use in social science research? insights from a cross-national online survey. *Planck Institute for Demographic Research. https://www. demogr. mpg. de/papers/working/wp-2021-006. pdf. Zugegriffen*, 13.

Guyer, J. J., Briñol, P., Vaughan-Johnston, T. I., Fabrigar, L. R., Moreno, L., and Petty, R. E. (2021). Paralinguistic features communicated through voice can affect appraisals of confidence and evaluative judgments. *Journal of Nonverbal Behavior*, 45(4):479–504.

Hainmueller, J., Hangartner, D., and Yamamoto, T. (2015). Validating vignette and conjoint survey experiments against real-world behavior. *Proceedings of the National Academy of Sciences*, 112(8):2395–2400.

Hainmueller, J., Hopkins, D. J., and Yamamoto, T. (2014). Causal inference in conjoint analysis: Understanding multidimensional choices via stated preference experiments. *Political analysis*, 22(1):1–30.

Hirschman, A. O. (1970). *Exit, voice, and loyalty: Responses to decline in firms, organizations, and states*, volume 25. Harvard university press.

Kinzler, K. D. (2021). Accent, language, and social relations. *Current Directions in Psychological Science*, 30(5):431–437.

Kinzler, K. D., Shutts, K., DeJesus, J., and Spelke, E. S. (2009). Accent trumps race in guiding children's social preferences. *Social cognition*, 27(4):623–634.

Kühne, S. and Zindel, Z. (2020). Using facebook and instagram to recruit web survey participants: A step-by-step guide and application. *Survey Methods: Insights from the Field (SMIF)*.

Labov, W. (1972). The social stratification of (r) in new york city department stores. In *Sociolinguistic Patterns*, pages 43–64. University of Pennsylvania Press, Philadephia.

Labov, W. (2006). *The social stratification of English in New York city*. Cambridge University Press.

Lang, K. and Lehmann, M. F. (2016). Racial discrimination in the labor market: Theory and empirics. *Journal of Economic Literature*, 54(4):959–1006.

Lareau, A. and Weininger, E. B. (2003). Cultural capital in educational research: A critical assessment. *Theory and society*, 32:567–606.

Leeper, T. J., Hobolt, S. B., and Tilley, J. (2020). Measuring subgroup preferences in conjoint experiments. *Political Analysis*, 28(2):207–221.

Lewis, K. and Kaufman, J. (2018). The conversion of cultural tastes into social network ties. *American journal of sociology*, 123(6):1684–1742.

Lin, W. (2013). Agnostic notes on regression adjustments to experimental data: Reexamining Freedman's critique. *The Annals of Applied Statistics*, 7(1):295 – 318.

Neundorf, A. and Öztürk, A. (2021). Recruiting research participants through facebook advertisements: A handbook. *OSF Preprints*.

Oreopoulos, P. (2011). Why do skilled immigrants struggle in the labor market? a field experiment with six thousand resumes. *American Economic Journal: Economic Policy*, 3(4):148–171.

Ottaviano, G. I. and Peri, G. (2012). Rethinking the effect of immigration on wages. *Journal of the European Economic Association*, 10(1):152–197.

Rakić, T., Steffens, M. C., and Mummendey, A. (2011). When it matters how you pronounce it: The influence of regional accents on job interview outcome. *British Journal of Psychology*, 102(4):868–883.

Reeves, A. and De Vries, R. (2019). Can cultural consumption increase future earnings? exploring the economic returns to cultural capital. *The British journal of sociology*, 70(1):214–240.

Reynolds, W. M. (1978). Factor analysis of the marlowe-crowne social desirability scale. *Journal of Clinical Psychology*, 34(3):636–638.

Reynolds, W. M. (1982). Development of reliable and valid short forms of the marlowe-crowne social desirability scale. *Journal of Clinical Psychology*, 38(1):119–125.

Rivera, L. A. (2012). Hiring as cultural matching: The case of elite professional service firms. *American sociological review*, 77(6):999–1022.

Rivera, L. A. and Tilcsik, A. (2016). Class advantage, commitment penalty: The gendered effect of social class signals in an elite labor market. *American Sociological Review*, 81(6):1097–1131.

Rosenzweig, L., Bergquist, P., Pham, K. H., Rampazzo, F., and Mildenberger, M. (2020). Survey sampling in the global south using facebook advertisements. *SocArXiv*.

Samuels, D. J. and Zucco, C. (2013). Using facebook as a subject recruitment tool for survey-experimental research. *Available at SSRN 2101458*.

Schuller, B. and Batliner, A. (2013). Paralinguistics in speech and language—state-of-the-art and the challenges ahead. *Computer Speech Language*, 27(1):4–39.

Shayo, M. (2009). A model of social identity with an application to political economy: Nation, class, and redistribution. *American Political science review*, 103(2):147–174.

Spence, J. L., Hornsey, M. J., Stephenson, E. M., and Imuta, K. (2024). Is your accent right for the job? a meta-analysis on accent bias in hiring decisions. *Personality and Social Psychology Bulletin*, 50(3):371–386.

Stansbury, A. and Rodriguez, K. (2024). The class gap in career progression: Evidence from us academia. *Working paper*.

Stantcheva, S. (2022). How to run surveys: A guide to creating your own identifying variation and revealing the invisible. *Annual Review of Economics*.

Stantcheva, S. (2023). How to run surveys: A guide to creating your own identifying variation and revealing the invisible. *Annual Review of Economics*, 15(1):205–234.

Todorov, A., Mandisodza, A. N., Goren, A., and Hall, C. C. (2005). Inferences of competence from faces predict election outcomes. *Science*, 308(5728):1623–1626.

Uribe-Mallarino, C. (2008). Estratificación social en bogotá: de la política pública a la dinámica de la segregación social. *Universitas humanistica*, (65):139–172.

Zhang, B., Mildenberger, M., Howe, P. D., Marlon, J., Rosenthal, S. A., and Leiserowitz, A. (2020). Quota sampling using facebook advertisements. *Political Science Research and Methods*, 8(3):558–564.

Álvarez–Rivadulla, M. J., Camelo, P., Vargas–Serani, M., and Viáfara, D. (2023). The relational costs of crossing class lines. *The British Journal of Sociology*, 74(2):113–130.

Online Appendix

A Additional Figures and Tables

Table A.1: Attribute distribution for experimental profiles

Factor Variable	Level	Joint Distribution (%)	Full Conjoint Version A (%)	Leave-one-out designs				
				Conjoint Version BI (%)	Conjoint Version BII (%)	Conjoint Version BIII (%)	Conjoint Version BIV (%)	Conjoint Version C (%)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accent	High Class	50.000	49.691	50.721	50.244	50.151	50.19	-
	Low Class	50.000	50.309	49.279	49.756	49.849	49.81	-
Education	Elementary	5.095	4.908	-	5.025	5.181	-	5.456
	High School	38.814	38.813	-	36.963	39.003	-	38.851
	Postgraduate	12.427	12.597	-	12.697	12.376	-	12.113
	Technical	16.584	16.718	-	17.350	16.577	-	16.825
	Undergraduate	27.080	26.965	-	27.964	26.863	-	26.755
Experience	Average	33.330	33.240	32.901	-	33.595	-	33.116
	High	33.330	33.580	33.645	-	33.025	-	33.937
	Low	33.330	33.180	33.454	-	33.380	-	32.947
Income	0 - 1,100,000	40.957	41.240	41.524	39.864	-	-	40.856
	1,100,001 - 2,600,000	35.805	35.803	36.023	35.954	-	-	35.781
	2,600,001 - 4,200,000	11.283	11.125	10.528	12.229	-	-	11.360
	4,200,001 - 6,800,000	6.557	6.491	6.650	6.640	-	-	6.623
	More than 6,800,000	5.398	5.341	5.275	5.312	-	-	5.380
Sex	Female	50.000	50.176	50.214	49.309	49.752	49.83	49.053
	Male	50.000	49.824	49.786	50.691	50.248	50.17	50.947
Big 5	Calm	20.000	20.243	19.793	19.836	19.765	19.92	20.225
	Extroverted	20.000	20.010	20.142	20.240	20.271	20.15	19.261
	Generous	20.000	19.965	20.108	19.836	20.465	20.10	20.377
	Imagi -tive	20.000	19.821	19.860	20.580	18.785	19.85	20.233
	Persistent	20.000	19.961	20.097	19.507	20.713	19.98	19.904
Region	Bogota	50.000	50.091	50.992	49.713	50.442	49.99	33.505
	Chile	50.000	49.909	49.008	50.287	49.558	50.01	32.769

***Notes*:** This table reports the proportion of each of the attributes in the experimental profiles that subjects observe (by conjoint experiment type). Column 1 shows the joint distribution we target. Columns 2-7 show the empirical distribution in each conjoint type (with full information in column 2 and the variations leaving information out in columns 3 to 7).

Table A.2: Balance across conjoint versions

	Dependent Variable:	
	Random assignment of accent	
	(1)	(2)
Age	-0.00119**	-0.00066**
	(0.00051)	(0.00032)
Sex: Male	0.00632	0.00502
	(0.01338)	(0.00851)
Region: Bogota	0.00231	0.00476
	(0.01494)	(0.00959)
Household size: 1	0.00555	0.00213
	(0.03269)	(0.02038)
Household size: 2	0.01531	0.00337
	(0.03124)	(0.01950)
Household size: 3	0.01768	0.00935
	(0.03131)	(0.01949)
Household size: 4	-0.01349	-0.01552
	(0.03442)	(0.02172)
Household size: 5	0.03922	0.01080
	(0.03868)	(0.02508)
Household size: ≥ 6	-0.02608	-0.02185
	(0.04387)	(0.02788)
Respondent SES (Electricity Bill): 2	0.01321	-0.00129
	(0.03235)	(0.01946)
Respondent SES (Electricity Bill): 3	0.00486	-0.00569
	(0.03161)	(0.01897)
Respondent SES (Electricity Bill): 4	-0.01338	-0.01570
	(0.03481)	(0.02118)
Respondent SES (Electricity Bill): 5	-0.02469	-0.01440
	(0.04641)	(0.02784)
Respondent SES (Electricity Bill): 6	-0.00311	-0.01253
	(0.06543)	(0.04231)
Conjoint	A Vs. C	A+B's V.s C
Observations	3,577	5,882
R^2	0.00398	0.00204

***Notes*:** This table reports empirical evidence on the randomization of treatment across conjoint versions. In both columns, the dependent variable is an indicator equal to one if the choice tasks were provided with an audio file showcasing class-based accents, zero otherwise. For column 1, we compare the full conjoint (i.e., the dependent variable equals one when subjects observe the full information conjoint experiment or type A) to the experiment with no voice snippets (i.e., the dependent variable equals zero when subjects observe the conjoint experiment type C). In column 2, the dependent variable also equals one when subjects observe partial information conjoint experiment, thus comparing types A and B to C. We regress these dependent variables on subjects' predetermined characteristics: age, sex, region where they live, household size, and respondent's socioeconomic stratum in their electricity bill. Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.3: Conjoint version A estimated pAMCE

	Dependent Variable: (With) which of the two individuals...						
	seems more capable of stepping into the other's shoes?	inspires more confidence	would you prefer to have a friendship?	would you prefer to do business?	would you prefer to share your workplace?	is more likely to be your boss?	is more likely to receive the letter?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accent - Baseline: *Low Class*							
High Class	0.0552***	0.1447***	0.134***	0.1208***	0.1299***	0.1071***	0.161***
	(0.00903)	(0.00877)	(0.00871)	(0.00877)	(0.00855)	(0.00863)	(0.01569)
Income - Baseline: *0 - 1,100,000*							
1,100,001 - 2,600,000	-0.0258**	0.012	-0.005	0.0607***	0.0102	0.1028***	0.0545***
	(0.01022)	(0.01025)	(0.01028)	(0.01046)	(0.01028)	(0.01047)	(0.01883)
2,600,001 - 4,200,000	-0.0498***	0.0113	-0.0344**	0.1438***	0.0208	0.2054***	0.1193***
	(0.01472)	(0.01493)	(0.01501)	(0.01482)	(0.01453)	(0.01479)	(0.02751)
4,200,001 - 6,800,000	-0.0812***	0.0187	-0.0407**	0.1675***	0.0353**	0.2503***	0.1198***
	(0.01819)	(0.01813)	(0.01818)	(0.01737)	(0.01769)	(0.01727)	(0.03243)
More than 6,800,000	-0.1317***	-0.0186	-0.0598***	0.1755***	-0.0028	0.3114***	0.2058***
	(0.02137)	(0.02114)	(0.02104)	(0.02059)	(0.02093)	(0.01945)	(0.03692)
Education - Baseline: *Elementary*							
High School	0.0443**	0.0321	0.016	0.0653***	0.0399**	0.0884***	0.1173***
	(0.01972)	(0.01958)	(0.01952)	(0.0192)	(0.01953)	(0.01875)	(0.03472)
Technical	0.0286	0.0499**	0.0066	0.123***	0.0838***	0.1904***	0.1899***
	(0.02116)	(0.02063)	(0.02076)	(0.02002)	(0.0209)	(0.02004)	(0.03729)
Undergraduate	-0.0107	0.0366*	-0.0025	0.1518***	0.0649***	0.2503***	0.2594***
	(0.02079)	(0.02)	(0.02045)	(0.01983)	(0.02033)	(0.01977)	(0.03639)
Postgraduate	-0.0611***	0.0533**	-0.0094	0.2203***	0.0699***	0.3549***	0.3446***
	(0.02367)	(0.02313)	(0.02352)	(0.02258)	(0.02381)	(0.0223)	(0.04072)
Sex - Baseline: *Male*							
Female	0.0645***	0.112***	0.098***	0.0807***	0.0899***	0.0806***	0.083***
	(0.00914)	(0.00923)	(0.00928)	(0.0088)	(0.00902)	(0.00879)	(0.01603)
Big 5 - Baseline: *Extroverted*							
Generous	0.1852***	0.1764***	0.1309***	0.0942***	0.1192***	0.041***	0.0578**
	(0.01313)	(0.01332)	(0.01295)	(0.01324)	(0.01302)	(0.01337)	(0.02443)
Persistent	-0.0142	0.0681***	-0.0345**	0.1377***	0.0508***	0.1054***	0.1116***
	(0.01379)	(0.01376)	(0.01404)	(0.01396)	(0.01401)	(0.01342)	(0.02421)
Calm	0.08***	0.0974***	0.0243*	0.0983***	0.0889***	0.0648***	0.065***
	(0.01354)	(0.01344)	(0.0134)	(0.01327)	(0.01333)	(0.01351)	(0.02394)
Imaginative	0.0523***	0.0775***	0.0377***	0.1196***	0.0748***	0.0655***	0.1034***
	(0.01349)	(0.01312)	(0.01341)	(0.0132)	(0.01345)	(0.01327)	(0.02411)
Experience - Baseline: *Low*							
Average	0.0157	0.0379***	0.0072	0.0602***	0.0336***	0.0714***	0.0428**
	(0.00954)	(0.00966)	(0.00945)	(0.00954)	(0.00975)	(0.00958)	(0.01778)
High	0.0168*	0.0363***	0.0201**	0.0759***	0.0345***	0.1056***	0.0857***
	(0.00961)	(0.00954)	(0.00956)	(0.00959)	(0.00962)	(0.00951)	(0.01806)
Number of Respondents	2139	2139	2139	2139	2139	2139	1947
Observations	16287	16287	16287	16287	16287	16287	4689

Notes*:** This table reports several measures of our outcome variables. The variables reported in this table are defined as follows. All variables take the value of one if the profile is selected, zero otherwise. Variables from column one trough six are shown in every task presented to the respondent. However, variable in column 7 is shown only one time per round. All columns include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. Estimates in this table correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.4: Statistical discrimination - full conjoint Vs. leave one out designs

	***Dependent variable**: (With) which of the two individual ...*						
	Seems more capable of stepping into the other's shoes?	Inspires more trustworthiness?	Would you prefer to have a friendship?	Would you prefer to do business?	Would you prefer to share your workplace?	Is more likely to be your boss?	Is more likely to receive the letter?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
***Panel A.** Full conjoint*							
Accent: High Class	0.0552***	0.1447***	0.134***	0.1208***	0.1299***	0.1071***	0.161***
	(0.009)	(0.0088)	(0.0087)	(0.0088)	(0.0085)	(0.0086)	(0.0157)
***Panel B.** Conjoint B1 - No education*							
Accent: High Class	0.0548***	0.1441***	0.1338***	0.1204***	0.1297***	0.1069***	0.1592***
	(0.009)	(0.0088)	(0.0087)	(0.0088)	(0.0086)	(0.0086)	(0.0156)
Difference	4e-04	6e-04	2e-04	4e-04	1e-04	1e-04	0.0019
	(0.0128)	(0.0124)	(0.0123)	(0.0124)	(0.0121)	(0.0122)	(0.0222)
***Panel C.** Conjoint B2 - No income*							
Accent: High Class	0.0549***	0.1444***	0.1341***	0.1206***	0.1298***	0.107***	0.1602***
	(0.009)	(0.0088)	(0.0087)	(0.0088)	(0.0086)	(0.0086)	(0.0157)
Difference	3e-04	3e-04	0	2e-04	1e-04	1e-04	8e-04
	(0.0128)	(0.0124)	(0.0123)	(0.0124)	(0.0121)	(0.0122)	(0.0222)
***Panel D.** Conjoint B3 - Leaving all out*							
Accent: High Class	0.0549***	0.1441***	0.1338***	0.1202***	0.1296***	0.1069***	0.16***
	(0.009)	(0.0088)	(0.0087)	(0.0088)	(0.0086)	(0.0086)	(0.0156)
Difference	5e-04	2e-04	6e-04	3e-04	2e-04	0.001	
	(0.0128)	(0.0124)	(0.0123)	(0.0124)	(0.0121)	(0.0122)	(0.0222)

Notes*:** This table reports several measures of our outcome variables. The variables reported in this table are defined as follows. All variables take the value of one if the profile is selected, zero otherwise. Variables from column one trough six are shown in every task presented to the respondent. However, variable in column 7 is shown only one time per round. All columns include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. Estimates in this table for the "High Class Accent" coefficient correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). The difference in the estimate row in panels B,C, and D correspond to the difference of both estimates of the "High Class Accent" estimate in the full conjoint and each of the Leave-one-out designs. Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.5: Downstream effects - Other variables

	Dependent variable: How much fo you trust ...						*Dependent variable:*			
	the following groups of people:				the following institutions:		Main problem in Colombia			
	Family	Friends	Strangers	Foreigners	Government	Democratic Institutions	Insecurity	Unemployment	Inequality	Corruption
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. A + B vs. C										
Class-based accents = 1	0.045	0.093**	0.0149	-0.0785*	-0.0693	8e-04	0.003	-0.0407**	0.0281*	0.0124
	(0.0392)	(0.0424)	(0.0379)	(0.0435)	(0.0479)	(0.0455)	(0.0142)	(0.0187)	(0.0171)	(0.0172)
Control Mean	4.264	3.774	1.941	2.85	2.506	2.422	0.151	0.346	0.246	0.253
Control Sd	0.989	1.077	0.952	1.085	1.211	1.144	0.358	0.476	0.431	0.435
Observations	6043	6052	6037	5942	6038	6040	6107	6107	6107	6107
R2	2e-04	9e-04	0	5e-04	4e-04	0	0	8e-04	4e-04	1e-04
Benjamini & Hochberg (1995) p-value	0.4182	0.1485	0.8689	0.2368	0.296	0.9865	0.9235	0.1485	0.2485	0.6749
Panel B. A vs. C										
Class-based accents = 1	0.0436	0.0754*	0.0174	-0.0927**	-0.0794	-0.0156	8e-04	-0.0464**	0.0315*	0.0179
	(0.0408)	(0.0443)	(0.0397)	(0.0455)	(0.05)	(0.0475)	(0.0148)	(0.0195)	(0.0179)	(0.018)
Control Mean	4.264	3.774	1.941	2.85	2.506	2.422	0.151	0.346	0.246	0.253
Control Sd	0.989	1.077	0.952	1.085	1.211	1.144	0.358	0.476	0.431	0.435
Observations	3681	3688	3673	3620	3677	3671	3717	3717	3717	3717
R2	3e-04	8e-04	1e-04	0.0011	7e-04	0	0	0.0016	8e-04	3e-04
Benjamini & Hochberg (1995) p-value	0.4585	0.2219	0.8242	0.2086	0.2244	0.8242	0.9574	0.1757	0.2219	0.4585

***Notes*:** All outcomes are in levels. The index of amoral familism is computed as the difference between the mean of trust in family and friends, and the mean of trust in foreigners and strangers. Questions corresponding to each question in order from left to right: *How much do you trust the following groups of people or institutions* (1= not at all, 5=very much); *What is the probability that a lost wallet, containing valuable documents and money, will be returned to its owner* (1= close to 0, 5 = quite high); *When you think about the future, how do you think your economic situation will be in the next 12 months* (1= better); *When you think about the future, how do you think your economic situation will be in the next 12 months?* (1= better); *Suppose Colombia is divided into 5 groups, where group 1 is made up of the 100 poorest families and group 5 is made up of the 100 richest families. What do you think is the probability that a person from the 100 poorest families will make it into one of the 100 richest families in the future?* (1 = close to 0, 5= quite high); *What do you think has more to do with a person being rich in Colombia?* (1 = because they had more advantages than others); and *In your opinion, what are the main problems affecting Colombia?* Robust standard errors are presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.6: Downstream effects - Lasso covariates

	Dependent variable:															
	How much do you trust the following groups of people:						How much do you trust the following institutions:						Main problem in Colombia			
	Index of Amoral familism	Family	Friends	Foreigners	Strangers	Probability of returning a lost wallet	Index of institutional trust	Government	Democratic institutions	Optimism future	Social mobility perceptions in Colombia	Low meritocracy	Insecurity	Unemployment	Inequality	Corruption
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A. *A+B vs. C*																
Class-based accents = 1	0.098**	0.044	0.077*	-0.076*	0.016	0.033	-0.047	-0.082*	-0.007	0.045	0.005	0.005	0.003	-0.042**	0.031*	0.012
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.03)	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Control Mean	1.63	4.26	3.79	2.85	1.95	2.98	2.48	2.51	2.43	2.38	2.39	0.81	0.15	0.35	0.24	0.25
Control SD	0.98	0.99	1.07	1.09	0.96	1.20	1.07	1.22	1.15	0.75	1.04	0.39	0.36	0.48	0.43	0.44
Observations	5,884	5,862	5,866	5,757	5,853	5,848	5,898	5,854	5,861	5,485	5,853	5,626	5,923	5,925	5,925	5,925
R^2	0.011	0.019	0.055	0.032	0.008	0.008	0.004	0.006	0.004	0.007	0.030	0.001	0.005	0.001	0.001	0.000
Panel B. A vs. C																
Class-based accents = 1	0.093**	0.044	0.064	-0.084*	0.017	0.050	-0.059	-0.095*	-0.022	0.044	0.008	0.014	0.000	-0.050**	0.035*	0.019
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Control Mean	1.63	4.26	3.79	2.85	1.95	2.98	2.48	2.51	2.43	2.38	2.39	0.81	0.15	0.35	0.24	0.25
Control SD	0.98	0.99	1.07	1.09	0.96	1.20	1.07	1.22	1.15	0.75	1.04	0.39	0.36	0.48	0.43	0.44
Observations	3,578	3,566	3,572	3,503	3,559	3,557	3,585	3,563	3,559	3,340	3,565	3,421	3,602	3,602	3,602	3,602
R^2	0.008	0.021	0.059	0.029	0.000	0.010	0.004	0.005	0.000	0.009	0.030	0.001	0.000	0.002	0.001	0.000

***Notes*:** All outcomes are in levels. The index of amoral familism is computed as the difference between the mean of trust in family and friends, and the mean of trust in foreigners and strangers. Questions corresponding to each question in order from left to right: *How much do you trust the following groups of people or institutions* (1= not at all, 5=very much); *What is the probability that a lost wallet, containing valuable documents and money, will be returned to its owner* (1= close to 0, 5 = quite high); *When you think about the future, how do you think your economic situation will be in the next 12 months* (1= better); *When you think about the future, how do you think your economic situation will be in the next 12 months?* (1= better); *Suppose Colombia is divided into 5 groups, where group 1 is made up of the 100 poorest families and group 5 is made up of the 100 richest families. What do you think is the probability that a person from the 100 poorest families will make it into one of the 100 richest families in the future?* (1 = close to 0, 5= quite high); *What do you think has more to do with a person being rich in Colombia?* (1 = because they had more advantages than others); and *In your opinion, what are the main problems affecting Colombia?*. For our estimates in this table we use the function `rlassoEffect` from package `hdm` as in Chernozhukov et al. (2016). Robust standard errors are presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.7: Downstream effects - Het. effects by SES

	Dependent variable:															
	How much do you trust the following groups of people:						How much do you trust the following institutions:						Main problem in Colombia			
	Index of Amoral familism	Family	Friends	Foreigners	Strangers	Probability of returning a lost wallet	Index of institutional trust	Government	Democratic institutions	Optimism future	Social mobility perceptions in Colombia	Low meritocracy	Insecurity	Unemploy-ment	Inequality	Corruption
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A. *A+B vs. C*																
Class-based accents = 1	0.100**	0.011	0.040	-0.114**	-0.004	-0.011	-0.079	-0.105*	-0.043	0.028	-0.031	0.007	0.001	-0.014	0.006	0.010
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Low SES	-0.053	-0.218**	-0.424***	-0.359***	-0.124	-0.041	-0.042	0.059	-0.141	0.043	0.106	0.004	0.024	0.083**	-0.077**	-0.031
	(0.08)	(0.08)	(0.09)	(0.09)	(0.08)	(0.10)	(0.09)	(0.10)	(0.09)	(0.06)	(0.08)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Class-based accents = 1 × Low SES	0.009	0.122	0.134	0.135	0.050	0.125	0.099	0.087	0.114	0.055	0.073	-0.000	0.006	-0.088**	0.073**	0.009
	(0.08)	(0.09)	(0.10)	(0.09)	(0.08)	(0.10)	(0.09)	(0.11)	(0.10)	(0.07)	(0.09)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Control Mean	1.63	4.26	3.79	2.85	1.95	2.98	2.48	2.51	2.43	2.38	2.39	0.81	0.15	0.35	0.25	0.25
Control SD	0.98	0.99	1.07	1.09	0.96	1.20	1.07	1.21	1.15	0.75	1.04	0.39	0.36	0.48	0.43	0.44
Observations	5,867	5,845	5,852	5,744	5,838	5,831	5,879	5,839	5,842	5,473	5,835	5,607	5,905	5,905	5,905	5,905
R^2	0.002	0.004	0.021	0.012	0.002	0.001	0.001	0.003	0.001	0.004	0.006	0.000	0.002	0.002	0.001	0.001
Panel B. A vs. C																
Class-based accents = 1	0.094*	0.009	0.022	-0.129**	0.002	0.013	-0.099*	-0.129**	-0.061	0.016	-0.029	0.014	0.001	-0.025	0.011	0.017
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Low SES	-0.053	-0.218**	-0.424***	-0.359***	-0.124	-0.041	-0.042	0.059	-0.141	0.043	0.106	0.004	0.024	0.083**	-0.077**	-0.031
	(0.08)	(0.08)	(0.09)	(0.09)	(0.08)	(0.10)	(0.09)	(0.10)	(0.09)	(0.06)	(0.08)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Class-based accents = 1 × Low SES	0.002	0.118	0.133	0.142	0.048	0.115	0.126	0.125	0.129	0.091	0.093	0.001	0.001	-0.078*	0.071*	0.007
	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)	(0.11)	(0.10)	(0.11)	(0.10)	(0.07)	(0.09)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Control Mean	1.63	4.26	3.79	2.85	1.95	2.98	2.48	2.51	2.43	2.38	2.39	0.81	0.15	0.35	0.25	0.25
Control SD	0.98	0.99	1.07	1.09	0.96	1.20	1.07	1.21	1.15	0.75	1.04	0.39	0.36	0.48	0.43	0.44
Observations	3,569	3,557	3,564	3,495	3,549	3,549	3,575	3,554	3,549	3,334	3,556	3,412	3,592	3,592	3,592	3,592
R^2	0.002	0.005	0.022	0.013	0.002	0.001	0.002	0.005	0.001	0.007	0.007	0.000	0.001	0.003	0.002	0.001

***Notes*:** All outcomes are in levels. The index of amoral familism is computed as the difference between the mean of trust in family and friends, and the mean of trust in foreigners and strangers. Questions corresponding to each question in order from left to right: *How much do you trust the following groups of people or institutions* (1= not at all, 5=very much); *What is the probability that a lost wallet, containing valuable documents and money, will be returned to its owner* (1= close to 0, 5 = quite high); *When you think about the future, how do you think your economic situation will be in the next 12 months* (1= better); *When you think about the future, how do you think your economic situation will be in the next 12 months?* (1= better); *Suppose Colombia is divided into 5 groups, where group 1 is made up of the 100 poorest families and group 5 is made up of the 100 richest families. What do you think is the probability that a person from the 100 poorest families will make it into one of the 100 richest families in the future?* (1 = close to 0, 5= quite high); *What do you think has more to do with a person being rich in Colombia?* (1 = because they had more advantages than others); and *In your opinion, what are the main problems affecting Colombia?* Robust standard errors are presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.8: Conjoint version A estimated pAMCE (short version) - Unreal

	***Dependent variable**: (With) which of the two individual . . .*						
	Seems more capable of stepping into the other's shoes?	Inspires more trustworthiness?	Would you prefer to have a friendship?	Would you prefer to do business?	Would you prefer to share your workplace?	Is more likely to be your boss?	Is more likely to receive the letter?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accent - Baseline: Low Class							
High Class	0.0139	0.1141***	0.1028***	0.107***	0.1258***	0.1059***	0.136***
	(0.02344)	(0.02308)	(0.02318)	(0.02273)	(0.0229)	(0.02096)	(0.04148)
Income - Baseline: 2,600,001 - 4,200,000							
4,200,001 - 6,800,000	-0.0083	-0.0321	5e-04	-0.0284	0.0509	-0.0558*	-0.1279*
	(0.03723)	(0.03704)	(0.03543)	(0.03526)	(0.0357)	(0.03276)	(0.06866)
More than 6,800,000	-0.0548	-0.0677*	-0.0227	-0.0259	-0.003	0.0175	-0.0454
	(0.03965)	(0.04034)	(0.03839)	(0.03881)	(0.03867)	(0.03655)	(0.07083)
High School	-0.0308	-0.2942	-0.4165	0.123	-0.7338***	-0.0656	-0.2808***
	(0.36707)	(0.36785)	(0.35891)	(0.36941)	(0.14018)	(0.36181)	(0.03392)
Technical	0.0756	-0.1879	-0.2095	-0.2041	-0.2941	-0.0664	-0.4036***
	(0.23404)	(0.23527)	(0.22231)	(0.23251)	(0.22538)	(0.21924)	(0.03534)
Undergraduate	0.0828	-0.1015	-0.2572*	0.0869	-0.1982	-0.0283	0.136***
	(0.15436)	(0.1563)	(0.13472)	(0.1601)	(0.14131)	(0.14121)	(0.04148)
Postgraduate	0.0625	-0.0955	-0.269**	0.1054	-0.2264	0.0016	-0.3597
	(0.15533)	(0.157)	(0.13434)	(0.15976)	(0.14145)	(0.14144)	(0.30074)

Notes*:** This table reports several measures of our outcome variables. The variables reported in this table are defined as follows. All variables take the value of one if the profile is selected, zero otherwise. Variables from column one trough six are shown in every task presented to the respondent. However, variable in column 7 is shown only one time per round. All columns present results of estimating the specification in equation 1 including our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. Estimates in this table correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Additionally, estimates in this table comprehend all profiles deemed as unrealistic profiles as explained in the pilot and PAP. Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.9: Conjoint version A estimated pAMCE (short version) - Real

	***Dependent variable**: (With) which of the two individual . . .*						
	Seems more capable of stepping into the other's shoes?	Inspires more trustworthiness?	Would you prefer to have a friendship?	Would you prefer to do business?	Would you prefer to share your workplace?	Is more likely to be your boss?	Is more likely to receive the letter?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accent - ***Baseline**: Low Class*							
High Class	0.0603***	0.1495***	0.1384***	0.1234***	0.13***	0.1077***	0.1665***
	(0.00969)	(0.00946)	(0.00943)	(0.00941)	(0.0092)	(0.00934)	(0.01697)
Income - ***Baseline**: 0 - 1,100,000*							
1,100,001 - 2,600,000	-0.0258**	0.0117	-0.0053	0.0601***	0.0095	0.102***	0.055***
	(0.01028)	(0.01031)	(0.01033)	(0.01051)	(0.01034)	(0.01053)	(0.01892)
2,600,001 - 4,200,000	-0.0425**	3e-04	-0.0386**	0.1315***	0.0243	0.1827***	0.084***
	(0.01715)	(0.01744)	(0.01739)	(0.01724)	(0.01689)	(0.01714)	(0.03146)
4,200,001 - 6,800,000	-0.0538	-0.0099	-0.0745	0.166***	0.0199	0.2252***	0.1694*
	(0.05588)	(0.05475)	(0.05456)	(0.05155)	(0.05329)	(0.05105)	(0.0936)
More than 6,800,000	-0.0595	-0.0387	-0.0569	0.0209	-0.0667	-0.0121	0.0265
	(0.09143)	(0.09137)	(0.09145)	(0.09154)	(0.0911)	(0.09035)	(0.22468)
Education - ***Baseline**: Elementary*							
High School	0.0397**	0.0307	0.0145	0.0644***	0.0416**	0.0926***	0.1205***
	(0.02002)	(0.01989)	(0.01977)	(0.01946)	(0.0198)	(0.01891)	(0.03501)
Technical	0.0254	0.049**	0.0053	0.1245***	0.0867***	0.1951***	0.1937***
	(0.02137)	(0.02088)	(0.02093)	(0.0202)	(0.02108)	(0.02015)	(0.03767)
Undergraduate	-0.0084	0.0338	-6e-04	0.1371***	0.063***	0.2337***	0.2604***
	(0.02134)	(0.0206)	(0.02106)	(0.02041)	(0.02096)	(0.02026)	(0.0374)
Postgraduate	-0.0496*	0.0538*	0.0064	0.1975***	0.0818***	0.3249***	0.3098***
	(0.02983)	(0.02929)	(0.02979)	(0.02846)	(0.03024)	(0.02812)	(0.05365)

Notes*:** This table reports several measures of our outcome variables. The variables reported in this table are defined as follows. All variables take the value of one if the profile is selected, zero otherwise. Variables from column one trough six are shown in every task presented to the respondent. However, variable in column 7 is shown only one time per round. All columns present results of estimating the specification in equation 1 including our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. Estimates in this table correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Additionally, estimates in this table comprehend all profiles deemed as realistic profiles as explained in the pilot and PAP Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.10: Conjoint tasks with full information (version A), heterogeneous treatment effects by respondents' characteristics

	Dependent Variable: (With) which of the two individuals...						
	Seems more capable of stepping into the other's shoes?	Inspires more trustworthiness?	Would you prefer to have a friendship?	Would you prefer to do business?	Would you prefer to share your workplace?	Is more likely to be your boss?	Is more likely to receive the letter?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Generalized trust							
Accent: High Class	0.0420 ***	0.1385 ***	0.1257 ***	0.1140 ***	0.1265 ***	0.1085 ***	0.1691 ***
	(0.0114)	(0.0111)	(0.0110)	(0.0108)	(0.0109)	(0.0102)	(0.0193)
Generalized Trust: High Trust	-0.0183 *	-0.0159	-0.0123	-0.0069	-0.0027	-0.0042	0.0126
	(0.0110)	(0.0107)	(0.0111)	(0.0108)	(0.0107)	(0.0103)	(0.0194)
Accent: High Class x Generalized Trust: High Trust	0.0368 **	0.0144	0.0219	0.0140	0.0100	-0.0071	-0.0296
	(0.0182)	(0.0178)	(0.0178)	(0.0179)	(0.0175)	(0.0172)	(0.0312)
Number of Respondents	2,131	2,131	2,131	2,131	2,131	2,131	1,912
Observations	16,239	16,239	16,239	16,239	16,239	16,239	4,674
Panel B. Reciprocity							
Accent: High Class	0.0228	0.0983 ***	0.0904 ***	0.0898 ***	0.0802 ***	0.1361 ***	0.1277 ***
	(0.0312)	(0.0287)	(0.0304)	(0.0301)	(0.0304)	(0.0266)	(0.0487)
Reciprocity: High Reciprocity	-0.0071	-0.0104	-0.0140	0.0043	-0.0159	0.0264	0.0230
	(0.0197)	(0.0197)	(0.0194)	(0.0190)	(0.0199)	(0.0176)	(0.0314)
Accent: High Class x Reciprocity: High Reciprocity	0.0362	0.0502 *	0.0477	0.0330	0.0545 *	-0.0325	0.0340
	(0.0326)	(0.0301)	(0.0317)	(0.0314)	(0.0317)	(0.0280)	(0.0513)
Number of Respondents	2,137	2,137	2,137	2,137	2,137	2,137	1,934
Observations	16,278	16,278	16,278	16,278	16,278	16,278	4,687
Panel C. Altruism							
Accent: High Class	0.0563 ***	0.1446 ***	0.1335 ***	0.1215 ***	0.1297 ***	0.1144 ***	0.1568 ***
	(0.0106)	(0.0103)	(0.0103)	(0.0100)	(0.0101)	(0.0097)	(0.0182)
Altruism: High Altruism	0.0021	0.0185	0.0012	0.0081	0.0081	0.0169	-0.0034
	(0.0134)	(0.0128)	(0.0131)	(0.0131)	(0.0134)	(0.0126)	(0.0226)
Accent: High Class x Altruism: High Altruism	0.0154	-0.0101	-0.0022	-0.0120	-0.0121	-0.0327	0.0039
	(0.0223)	(0.0215)	(0.0213)	(0.0223)	(0.0221)	(0.0209)	(0.0363)
Number of Respondents	1,958	1,958	1,958	1,958	1,958	1,958	1,736
Observations	14,856	14,856	14,856	14,856	14,856	14,856	4,276
Panel D. Fairness							
Accent: High Class	0.0553 ***	0.1528 ***	0.1288 ***	0.1184 ***	0.1298 ***	0.0899 ***	0.1754 ***
	(0.0143)	(0.0140)	(0.0143)	(0.0140)	(0.0135)	(0.0133)	(0.0239)
Fairness: High Fairness	-0.0038	-0.0020	-0.0141	-0.0072	-0.0088	-0.0183	0.0298
	(0.0135)	(0.0132)	(0.0134)	(0.0130)	(0.0130)	(0.0124)	(0.0229)
Accent: High Class x Fairness: High Fairness	0.0026	-0.0034	0.0281	-0.0086	0.0141	0.0345 *	-0.0386
	(0.0225)	(0.0217)	(0.0216)	(0.0216)	(0.0213)	(0.0206)	(0.0373)
Number of Respondents	1,399	1,399	1,399	1,399	1,399	1,399	1,261
Observations	10,689	10,689	10,689	10,689	10,689	10,689	3,078

Notes*:** Estimates in this table correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Variables reported in this table are defined as follows. All variables take the value of one if the profile is selected and zero otherwise. Variables from columns one through six are shown in every task presented to the respondent. However, the variable in column 7 is shown only once per round. All columns include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. High generalized trust equals 1 if respondents rated 4 or 5 on a 1–5 scale for: *"I assume that people always have the best intentions."* High reciprocity equals 1 if respondents rated 4 or 5 on a 1–5 scale for: *"When someone does me a favor, I am willing to return it."* High altruism equals 1 if respondents chose 60, 80, or 100 thousand pesos when asked how much of an unexpected 100,000 pesos they would donate, and 0 if they chose 0, 20, or 40 thousand pesos. High fairness equals 1 if respondents rated 4 or 5 on a 1–5 scale for: *"How willing are you to punish someone who treats others unfairly, even at a cost to you?"* Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.11: Conjoint tasks with full information (version A) for the deferential letter experiment and heterogeneous treatment effects by SES - Adjusted p values

	Dependent variable: Which of the two individuals is more likely to receive the letter?		
	(1)	(2)	(3)
Accent: High Class	0.1586***	0.1136***	0.1168***
	(0.0152)	(0.0212)	(0.0291)
	[0]	[0]	[0.0014]
Letter: Deferential		-0.049***	-0.0527**
		(0.0183)	(0.0248)
		[0.13801]	[0.76968]
Accent: High Class x Letter: Deferential		0.0887***	0.0978**
		(0.0301)	(0.0417)
		[0.06147]	[0.43659]
SES: Low Class			0.0182
			(0.0263)
			[1]
Accent: High Class x SES: Low Class			-0.007
			(0.0425)
			[1]
SES: Low Class x Letter: Deferential			0.0066
			(0.0367)
			[1]
Accent: High Class x Letter: Deferential x SES: Low Class			-0.0183
			(0.0603)
			[1]
Number of Respondents	1947	1947	1947
Observations	4689	4689	4689

Notes*:** Estimates in this table correspond to the **population AMCE** (pAMCE***) as described in de la Cuesta et al. (2022). Variables reported in this table are defined as follows. The variable takes a value of one if the profile is selected and zero otherwise. The dependent variable is shown only once per round. Column 1 presents estimates for our factor variable of interest, *accent*, including all control variables such as income, education, sex, experience, and the big 5 personality traits. We proxy SES via a Principal Component Analysis (PCA) using information on the electricity statement, number of members in the household, and total household income. Results presented in this table correspond to the "Bogotanos classifying Bogotanos" subsample. Standard errors are clustered at the respondent level and presented in parenthesis and corrected p values using the Family-Wise Error Rate (FWER) Bonferrini correction are presented in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.12: Assessing Social Desirability Bias - Bogotá

	Dependent variable:				
	Social Class	Sport	Leisure	Music	Social Influence
	(1)	(2)	(3)	(4)	(5)
Panel A. *Demean MCSDS scale variable*					
Accent					
Accent: High Class	0.201**	0.140	0.147*	0.208**	0.448***
	(0.064)	(0.084)	(0.072)	(0.075)	(0.106)
SDB measures					
MCSDS Scale	0.015	-0.011	0.006	0.002	0.042
	(0.019)	(0.021)	(0.021)	(0.021)	(0.028)
Accent x SDB measures					
High Class × MCSDS Scale	-0.030	-0.003	-0.005	0.014	-0.048
	(0.024)	(0.031)	(0.026)	(0.029)	(0.043)
Constant	2.195***	1.525***	1.978***	2.255***	2.805***
	(0.125)	(0.181)	(0.180)	(0.189)	(0.232)
Task Type	Audio Profile	Audio Profile	Audio Profile	Audio Profile	Audio Profile
Other Attributes	Yes	Yes	Yes	Yes	Yes
Observations	338	338	337	338	335
Mean DV	2.148	1.636	2.205	2.186	3.224
SD DV	0.613	0.711	0.666	0.683	0.988
R-squared	0.249	0.106	0.114	0.100	0.147
Panel B. *Dummy MCSDS scale variable*					
Accent					
Accent: High Class	0.257***	0.136	0.183	0.205*	0.463***
	(0.073)	(0.107)	(0.094)	(0.093)	(0.135)
SDB measures					
MCSDS Scale	0.013	-0.152	-0.012	-0.054	0.048
	(0.120)	(0.114)	(0.116)	(0.117)	(0.178)
Accent x SDB measures					
High Class × MCSDS Scale	-0.145	0.020	-0.089	0.020	-0.032
	(0.144)	(0.168)	(0.148)	(0.149)	(0.223)
Constant	2.185***	1.586***	1.973***	2.270***	2.795***
	(0.124)	(0.186)	(0.187)	(0.195)	(0.251)
Task Type	Audio Profile	Audio Profile	Audio Profile	Audio Profile	Audio Profile
Other Attributes	Yes	Yes	Yes	Yes	Yes
Observations	338	338	337	338	335
Mean DV	2.148	1.636	2.205	2.186	3.224
SD DV	0.613	0.711	0.666	0.683	0.988
R-squared	0.251	0.113	0.117	0.099	0.141

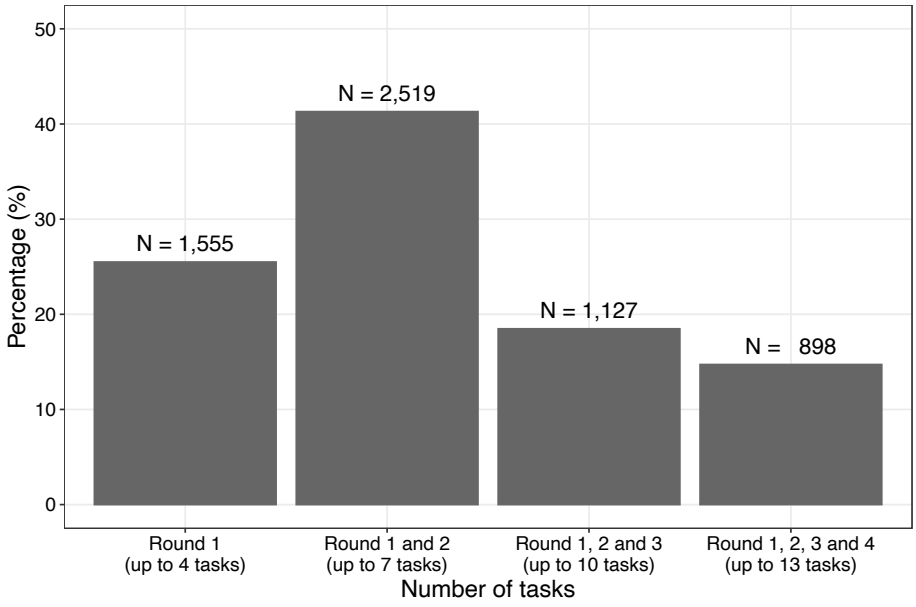
***Notes*:** The outcome variable for columns (1) and (8) is a categorical variable that takes the values of "low", "middle", and "high". The outcome variable for columns (2) and (9) is a categorical variable that takes the values of "going to the *Ciclovía* on Sundays", "play tennis", and "practice horse riding". The outcome variable for columns (3), and (10) is a categorical variable that takes the values of "fly a kite in the park", "go to the movie theater", and "go to the theater or art galleries". The outcome variable for columns (4) and (11) is a categorical variable that takes the values of "*cumbia villera - vallenato*", "classic rock - *salsa*", and "Jazz". The outcomes variables for columns (5), (6), (7), (12), (13), and (14) are categorical variables that take the values of "low", "middle-low", "middle", "middle-high", and "high". Columns (1) trough (7) present within regions estimates for *Bogota* only. Estimates in columns (8) trough (14) present cross region estimations. MCSDS is the 13-item version of the Marlowe-Crowne Social Desirability Scale. The table presents results for the "Audio Profile" task type sample. Robust standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.13: Social class classification of *Bogotanos* classifying *Santiaguinos*

	Dependent variable:				
	Social Class	Sport	Leisure	Music	Social Influence
	(1)	(2)	(3)	(4)	(5)
Accent - Baseline: *Low Class*					
Accent: High Class	0.047	0.085	0.117	0.011	-0.015
	(0.057)	(0.074)	(0.071)	(0.091)	(0.082)
Income - Baseline: *2'600.001 - 4'200.000*					
0 - 1'100.000	-0.506***	-0.455***	-0.560***	-0.287	-0.636***
	(0.100)	(0.120)	(0.114)	(0.161)	(0.181)
1'100.001 - 1'700.000	-0.494***	-0.389***	-0.307*	-0.085	-0.709***
	(0.103)	(0.113)	(0.119)	(0.151)	(0.172)
1'700.001 - 2'600.000	-0.307**	-0.243	-0.196	-0.041	-0.278
	(0.093)	(0.140)	(0.125)	(0.165)	(0.148)
4'200.001 - 6'800.000	0.067	0.180	0.097	0.198	0.174
	(0.092)	(0.144)	(0.100)	(0.166)	(0.147)
More than 6'800.000	0.107	0.068	0.076	0.232	-0.084
	(0.102)	(0.128)	(0.114)	(0.148)	(0.185)
Education - Baseline: *Technical*					
Elementary	0.037	0.031	0.000	0.102	-0.001
	(0.102)	(0.122)	(0.120)	(0.130)	(0.157)
High School	-0.030	-0.241*	-0.062	-0.100	-0.159
	(0.081)	(0.108)	(0.112)	(0.131)	(0.158)
Undergraduate	0.185	0.122	0.150	0.127	0.357*
	(0.105)	(0.123)	(0.113)	(0.131)	(0.158)
Postgraduate	0.329***	0.220	0.390***	0.423***	0.308*
	(0.093)	(0.114)	(0.106)	(0.124)	(0.145)
Sex - Baseline: *Male*					
Woman	-0.050	-0.102	0.032	-0.070	-0.118
	(0.063)	(0.083)	(0.076)	(0.088)	(0.092)
Big 5 - Baseline: *Persistent*					
Extroverted	-0.005	-0.137	0.023	-0.290*	0.148
	(0.102)	(0.114)	(0.115)	(0.131)	(0.144)
Generous	0.001	-0.051	0.131	-0.030	-0.121
	(0.101)	(0.113)	(0.104)	(0.114)	(0.147)
Calmed	-0.157	-0.109	-0.159	-0.249	-0.117
	(0.105)	(0.116)	(0.107)	(0.132)	(0.168)
Imaginative	0.124	0.019	0.188	0.124	0.123
	(0.106)	(0.126)	(0.120)	(0.132)	(0.156)
Experience - Baseline: *Average*					
Low	-0.103	-0.179*	-0.112	-0.014	-0.170
	(0.070)	(0.088)	(0.093)	(0.096)	(0.122)
High	-0.063	-0.036	0.078	-0.056	-0.063
	(0.070)	(0.094)	(0.083)	(0.106)	(0.129)
Observations	342.000	341.000	342.000	342.000	340.000
Mean DV	2.023	1.545	2.105	2.070	3.212
SD DV	0.612	0.708	0.707	0.782	0.951
R-squared	0.261	0.187	0.220	0.138	0.185
Number of NA	0	1	0	0	2

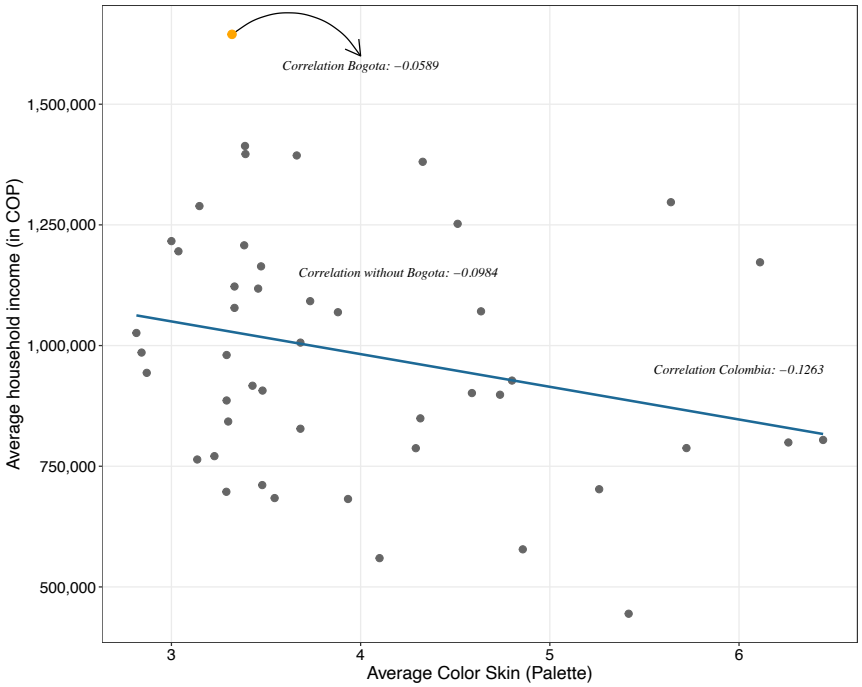
***Notes*:** The outcome variable for columns (1) to (5) is a categorical variable that takes the values of "low", "middle", and "high". The table presents results for the "Audio Profile" task type sample. Panel A presents estimates for the *Bogotanos* classifying *Bogotanos* sample. Panel B presents estimates for the *Santiaguinos* classifying *Santiaguinos* sample. Panel C presents estimates for the cross-region estimates, with *Bogotanos* classifying *Santiaguinos*. Robust standard errors are clustered at the respondent level and presented in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure A.1: Subject Distribution by Number of Completed Rounds



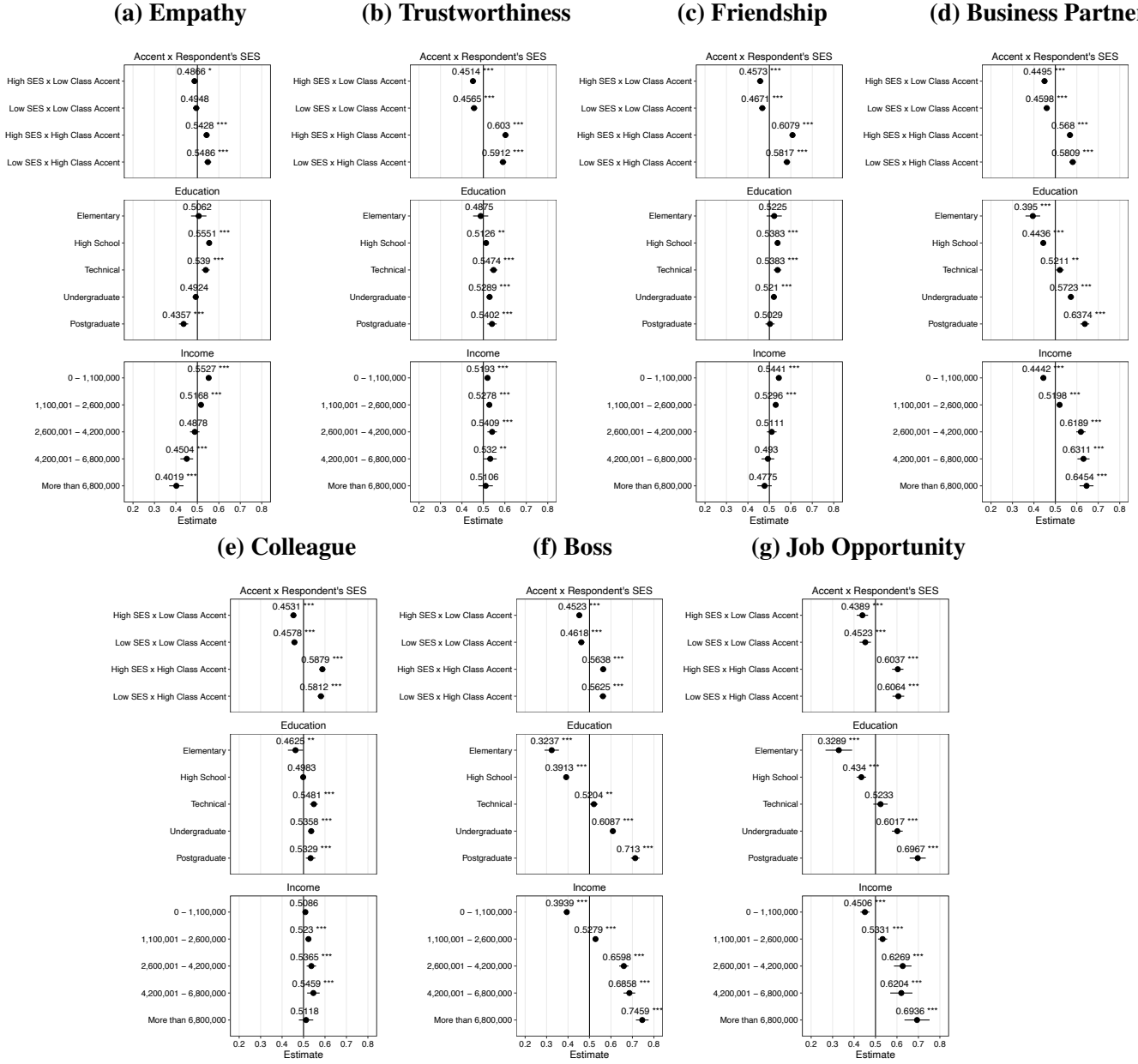
Note: This figure presents the number and percentage of subjects completing a given number of rounds of conjoint tasks, including all conjoint experimental variations. The first round included four choice tasks, while rounds 2 to 4 offered subjects the chance to complete three more tasks in each case for an additional lottery ticket.

Figure A.2: Correlation household income and skin color



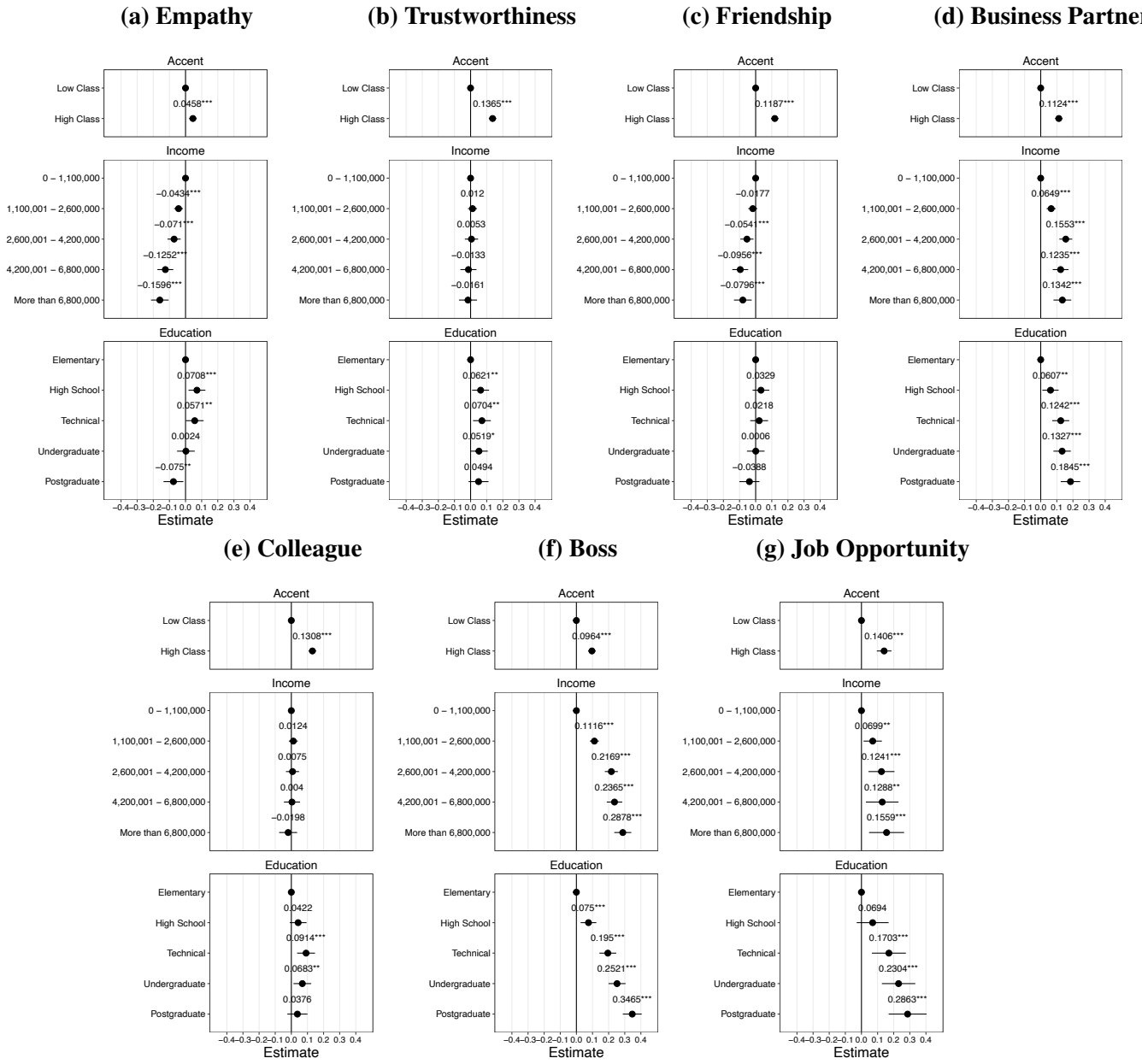
Note: The data for this figure come from the Latin American Public Opinion Project, specifically its 2018 round in Colombia. For income, we use question Q10new, which asks individuals to report their total monthly household income, broken down into intervals. For skin color, which is used as a proxy for ethnic and racial considerations, we rely on a color palette used by the enumerator to report the respondent's perceived skin color (question `colorr`), measured on a scale of 1 to 11, from lightest to darkest.

Figure A.3: Heterogeneous effects by respondent's SES - Marginal means



Notes*:** Estimates in this figure correspond to the differences in the **Marginal Means** (MM***) estimator. Estimates correspond to coefficients in equation 2. Variables reported in this figure are defined as follows. All variables take a value of one if the profile is selected and zero otherwise. All panels include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. Confidence intervals are shown at the 95% level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

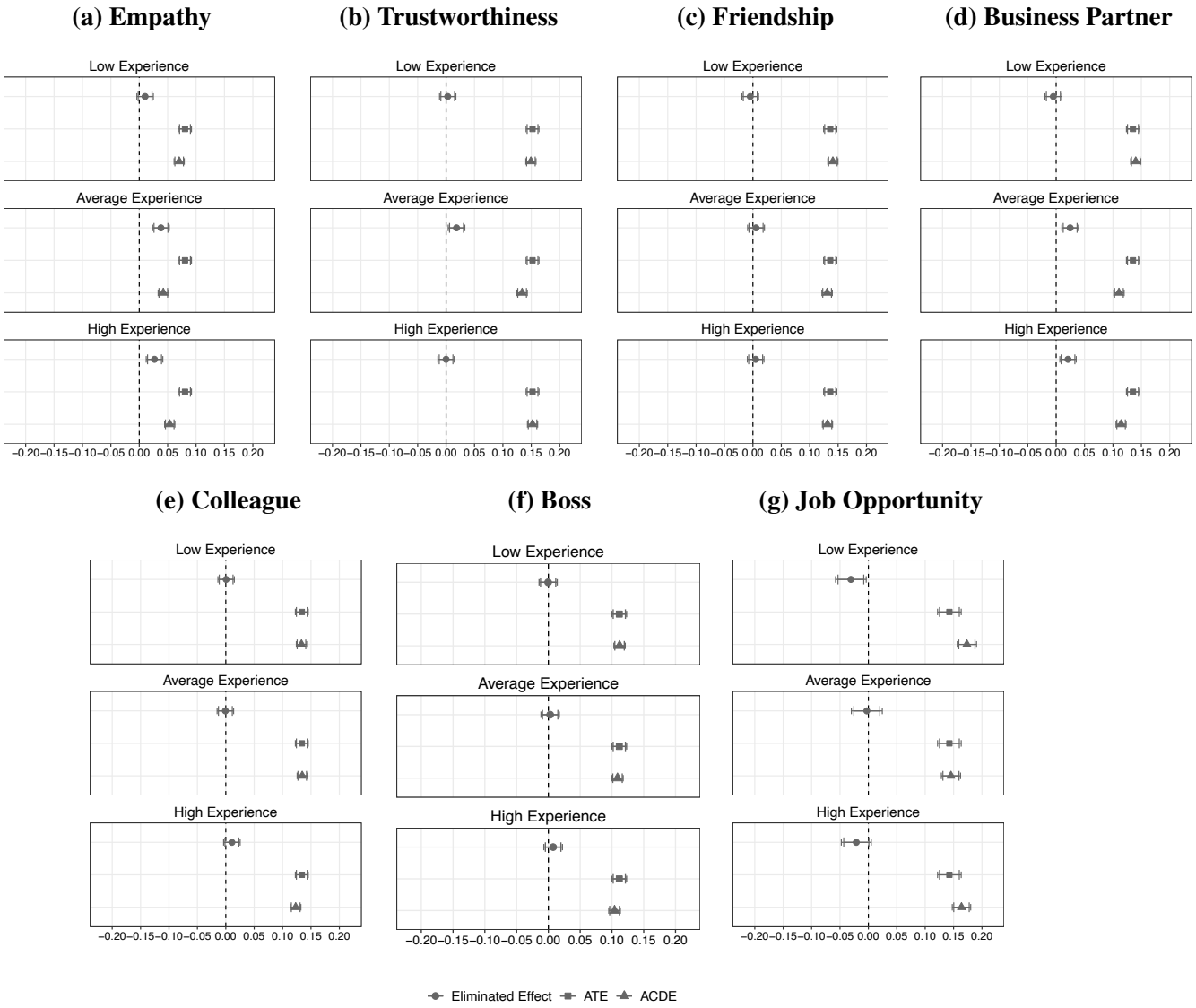
Figure A.4: Conjoint A point estimates for the pAMCE - First round estimates



***Notes*:** Estimates in this figure correspond to the **population AMCE (*pAMCE*)** as described in de la Cuesta et al. (2022). Variables reported in this figure are defined as follows: Empathy, corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness, corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner, corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship, corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague, corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss, corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity, corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels A.4a to A.4f are presented in all tasks to the respondent. However, variable in panel A.4g is shown once per round. All panels include our factor of interest *accent*, and all control factors such as income, education, sex, experience, and the big 5 personality traits. However, we present only estimates for accent, income and education. Results presented in this figure correspond to the "Bogotanos classifying Bogotanos" subsample using only data from the first round choice tasks. Confidence intervals are shown at the 95% level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

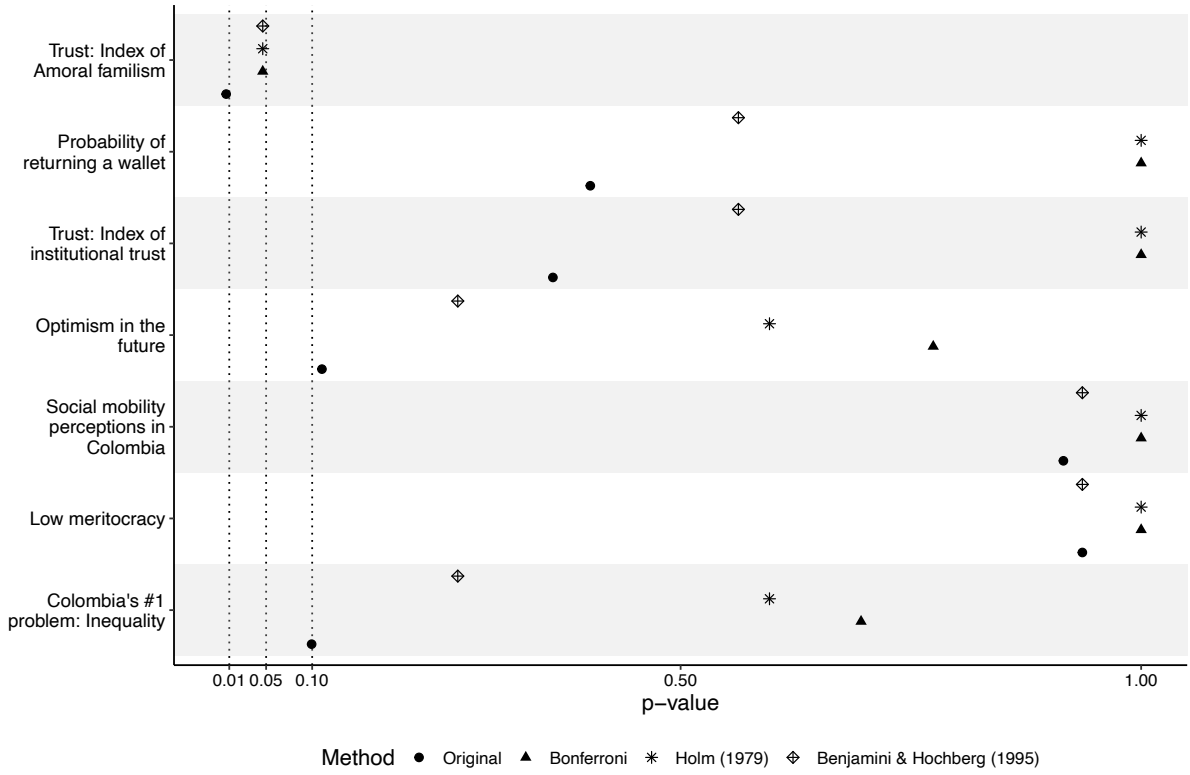
Figure A.5: Experience as a causal mechanism for class-based accents

Average Total Effect (ATE) = Average Controlled Direct Effect (ACDE) + Eliminated Effect at each income level



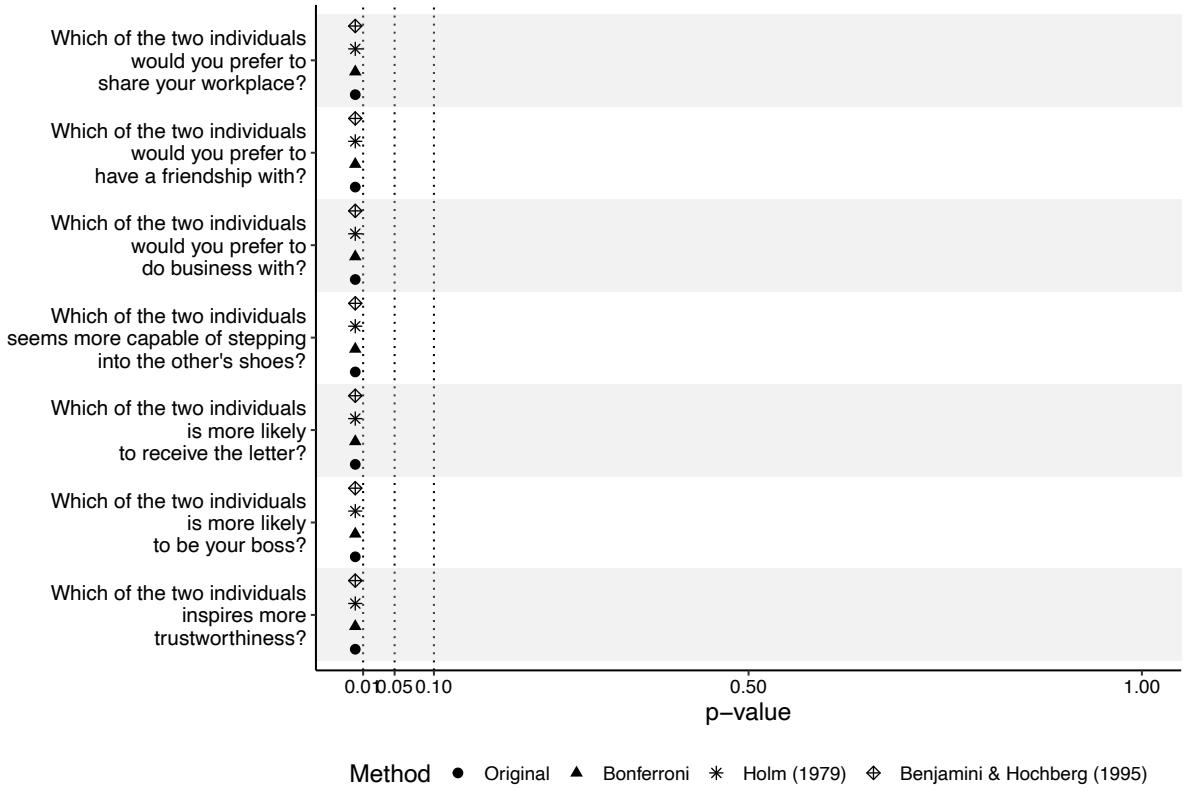
***Notes*:** Estimates for the **Eliminated effect = ATE−ACDE** are shown in **circles**. Estimates for the **Effect of accent without fixing experience (ATE)** are shown in **squares**, while estimates for the **Effect of accent fixing experience (ACDE)** are shown in **triangles**. Variables reported in this figure are defined as follows: Empathy corresponds to the question *Which of the two individuals seems more capable of stepping into the other's shoes?*; Trustworthiness corresponds to the question *Which of the two individuals inspires more trustworthiness?*; Business partner corresponds to the question *Which of the two individuals would you prefer to do business with?*; Friendship corresponds to the question *Which of the two individuals would you prefer to have a friendship with?*; Colleague corresponds to the question *Which of the two individuals would you prefer to share your workplace?*; Boss corresponds to the question *Which of the two individuals is more likely to be your boss?*; Job opportunity corresponds to the question *Which of the two individuals is more likely to receive the letter?*. All variables take a value of one if the profile is selected and zero otherwise. All dependent variables from panels A.5a to A.5f are presented in all tasks to the respondent. However, the dependent variable in panel A.5g is shown once per round. Estimates for the ATE and the ACDE in each panel correspond to the **population AMCE (*pAMCE*)** estimator as described in de la Cuesta et al. (2022), while the eliminated effect is defined as the difference between the ATE and the ACDE. Results presented in this figure correspond to the "*Bogotanos* classifying *Bogotanos*" subsample. Bootstrap 95% (darker line) and 90% (lighter line) confidence intervals are based on 5,000 bootstrap replications.

Figure A.6: Downstream adjusted p-values - Conjoint A + B vs. C



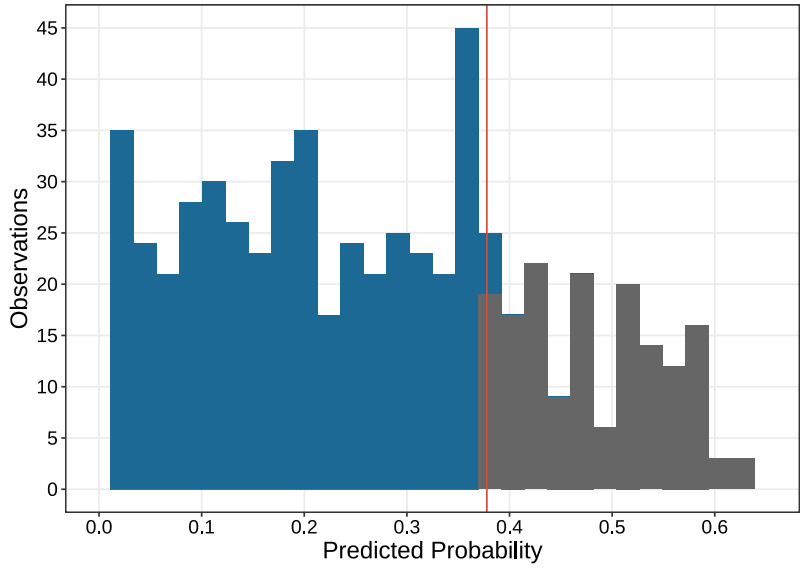
***Notes*:** This figure presents estimates correcting for multiple comparisons in our baseline estimation of downstream effects. The figure presents two types of corrections. First, we address the Family-Wise Error Rate (FWER) with the Bonferroni and Holm corrections. Second, we address the False Discovery Rate (FDR) with the Benjamini & Hochberg correction. Additionally, we present the original p-value from the baseline regressions for comparison.

Figure A.7: Adjusted p-values - Conjoint A estimates



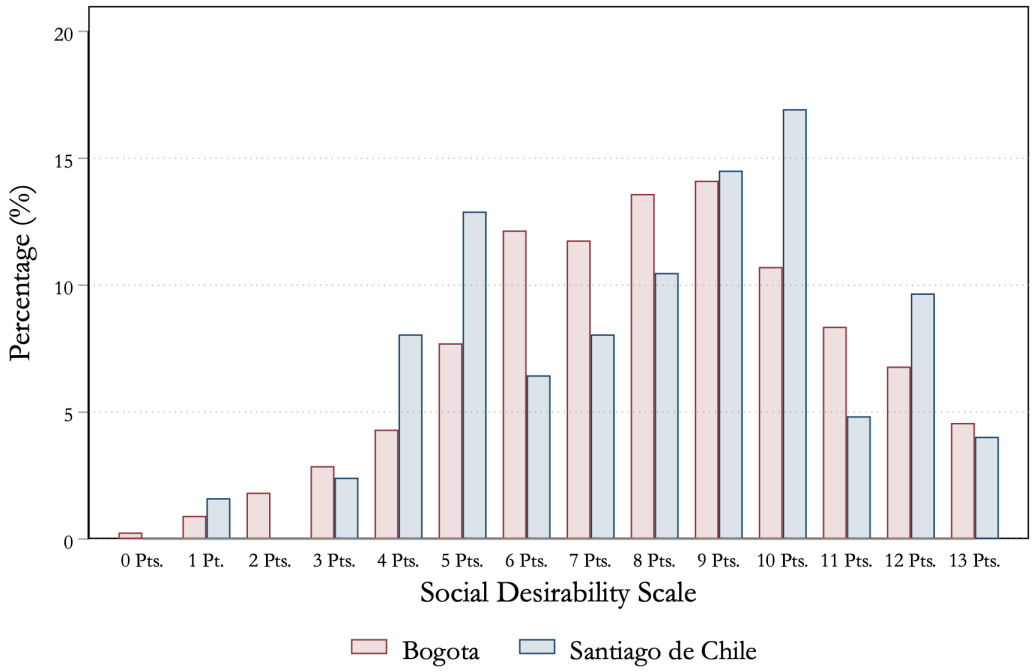
***Notes*:** This figure presents estimates correcting for multiple comparisons in our baseline estimation of class-based accents in the full information conjoint experiment, type A. The figure presents two types of corrections. First, we address the Family-Wise Error Rate (FWER) with the Bonferroni and Holm corrections. Second, we address the False Discovery Rate (FDR) with the Benjamini & Hochberg correction. Additionally, we present the original p-value from the baseline regressions for comparison.

Figure A.8: Distribution of the predicted probability of an "unrealistic" profile



***Notes*:** The figure shows a histogram of a predicted probability. The dependent variable in the model corresponds to a dummy variable equal to one if the respondent classifies the presented profiles as unrealistic, zero otherwise. We use as control variables all attributes of the profile: income, education, experience, sex, big five personality traits, and accent. We estimate the model using the "audio profile" classification task sample but predict it in the complete sample. We use robust standard errors.

Figure A.9: Social Desirability Score, Bogotá



B ETHICAL CONSIDERATIONS

We decided to investigate these questions in the context of an online survey experiment, which provides a useful setting for testing our theory. It offers participants the privacy to answer questions truthfully without the stress of feeling observed, while allowing us to reach a larger sample. The choice of a survey experiment also provides a contained environment to evaluate the theory and learn lessons for designing a field intervention of a broader scope.

Respondents are invited to participate in the study voluntarily, with no direct compensation for the time spent completing the survey. However, we incentivize participation by drawing tablets for those who complete the survey. Participants may leave the survey at any time with no personal consequences.

All data collected from survey responses will initially be stored in Qualtrics. Once data collection is complete, the data will be downloaded and stored on a password-protected computer. Only the research group will have access to the study data, as will a research assistant, who must sign a confidentiality agreement before accessing the data. Six months after the completion of the project, the data will be deleted from Qualtrics. However, pseudonymized data will be retained in perpetuity for replication purposes.

The study will not involve any invasive intervention with participants, who will only be exposed to written and verbal randomized information and asked to answer standard opinion survey questions. In addition, although the audio snippets embedded in the conjoint experiment include accents as markers of cultural capital and region, they are not expected to cause discomfort to respondents as they do not cover sensitive topics and are no different from situations that people might experience in their daily interactions.

However, some participants may feel uncomfortable answering some questions because they may feel stigmatized. To minimize this possibility, we take the following measures.

- We conducted a pilot to ensure that no questions were included that would cause risk, distress, or stigma to respondents.
- In both the pilot and full study surveys, we included an open-ended box at the end of the survey where respondents could express their opinions and share their feelings after all the experiments in the form of comments.
- The informed consent of participants will be recorded to ensure respect for the autonomy and well-being of respondents. It makes it clear to respondents that participation in the survey is voluntary and that they should feel free to refuse to answer certain questions or to withdraw from the session entirely at any time without adverse consequences.
- Since participants will be recruited through Facebook ads, we will take the following steps to ensure that only adults are included in the study: we will only target these ads to individuals

who are 18 years of age or older; the consent form will consist of a declaration that the respondent is not a minor; finally, we will ask respondents about their age in the first section of the survey and exclude those under 18 by redirecting them to the end of the survey.

- All of our advertisements and recruitment tools will clearly state that we will draw a Samsung Galaxy A9 tablet for every 1,000 people who complete the survey.
- It will be made clear to participants that all information collected will be kept strictly confidential, that the data will be encrypted and stored on a password-protected computer, and that only the researchers involved in this project will have access to the (de-identified) information collected. In addition, we will not ask respondents to provide any personal information.
- Participants will be informed that there are no right or wrong answers to the survey questions and that the information will be kept strictly confidential. This motivates participants to answer honestly without fear of being discriminated against, stigmatized, or judged for their ideas and positions.
- At the end of the survey, we will leave an open space for respondents to comment on the questionnaire. Likewise, we will include the emails of the principal investigators, as well as those of the officers of the two institutions involved in the IRBs of the research (i.e., Universidad de los Andes and Harvard University), for any individual who may wish to write further and share any suggestions or concerns with us.

C SURVEY AND EXPERIMENTAL DESIGN DETAILS

C.1 Recruitment and sampling

C.1.1 Pilot background study

We conducted pilots in Bogotá and Santiago de Chile.We programmed our survey using Qualtrics. In Bogotá, our pilot sample, which targeted people 18 or older living in Bogotá, was recruited by Netquest, a well-established company in over 20 countries that maintains an online panel for marketing and other purposes. This allowed us to minimize potential contamination with the sample recruited online in the final survey. Fieldwork was conducted between February 5 and February 20, 2024.To ensure high-quality responses, the company monitored participants through the survey and used Attention Check questions and Real ID checks to ensure participants didn't submit duplicate data. All survey participants received an informed consent form (approved by both the Harvard and Universidad de los Andes IRBs). For analysis, we removed all incomplete surveys and participants under 18 or living outside Bogotá (as they may not accurately classify the social class of the profiles).

In Santiago, we recruited our sample using social media ads to test the recruitment strategy we would use for the complete study in Bogota. Thus, we ran ads inviting people to participate in a drawing for one tablet for every 1,000 who completed our survey (as we did in the case of the full study in Bogota). Given our interest in class, we also targeted the ads to get enough responses from across the income distribution, meeting income quotas by focusing our online recruitment campaign in geographic areas that concentrated subjects with the required income levels (see map in Figure A.10). Fieldwork took place between February 1 and February 16, 2024.

Our pilot survey targeted 1,500 participants in Bogotá and 200 in Santiago de Chile. We included citizens from all backgrounds, regardless of sex, race, ethnicity, and socioeconomic status, among other characteristics. In Bogota, we set quotas by *estrato*, sex and age.[31] In Santiago, we only enforced income quotas.

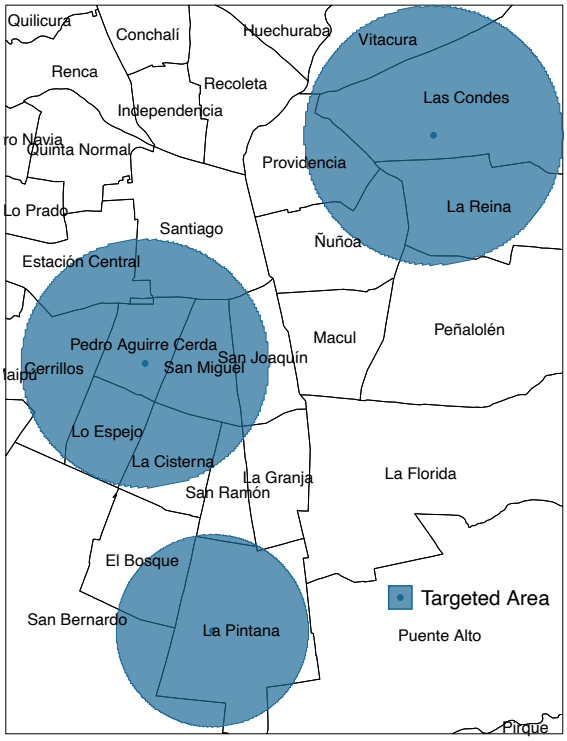
The pilot's resulting effective sample size was 863 respondents in Bogotá and 125 in Santiago de Chile. Our participant pool was diverse, reflecting the demographics of each city. In Bogotá 52.40% are men and 47.60% are women, while in Santiago de Chile, 36.80% are men, 61.60% are women, and 1.60% do not identify with either sex. Moreover, in Bogotá, 48.78% of participants are younger than 35 years old, and 51.22% are older; in Santiago de Chile, 12.80% are younger than 35, and 87.20% are older. Finally, in Bogotá, 28.14% of our sample belong to the low-income group (*estratos* 1 and 2), 60.05% to the middle-income group (*estratos* 3 and 4), 11.68% to the high-income group (*estratos* 5 and 6), and 0.13% do not belong to any *estrato*.[32]

In Bogotá, we created two versions of the pilot questionnaire and randomly assigned respondents to one of them. In one version, subjects were presented with the conjoint task as they would

[31] Age groups: 18-25 ≈180; 26-35 ≈320; 36-45: 250; 46-55 ≈130; and 56-65 ≈120; Sex: male ≈500 and female ≈500. SES/Stratums: 1 and 2≈430; 3≈340; 4: 102/140; and 5 and 6≈90.

[32] Chile does not have a socioeconomic stratification scheme.

Figure A.10: Geographic Location of Census Tract Clusters - Santiago



see it in the full study, and in the second version, they had to perform a classification task, which we used to validate the accents' interpretation, as discussed in the main text.

C.1.2 Full study

We conduct an online survey and recruit a convenience sample using Facebook and Instagram ads, which allows us to reach respondents of all income levels cost-effectively.[33]

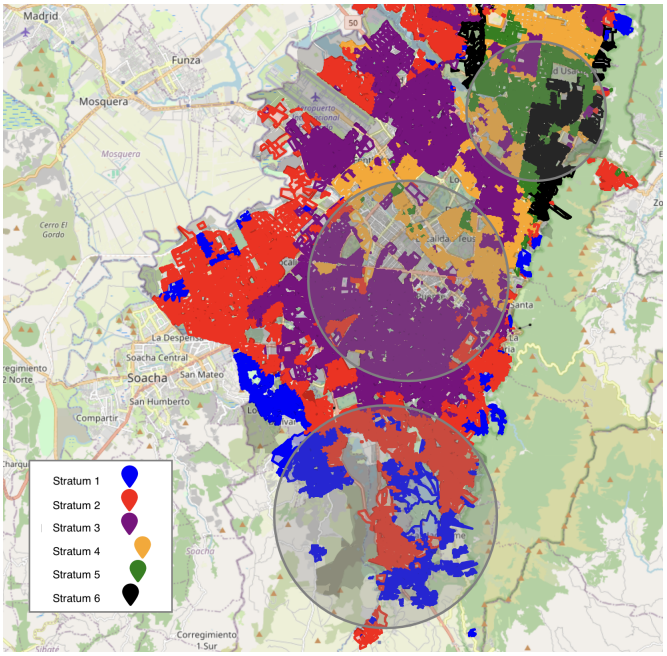
We now focus on the sampling design. After defining the sampling strata, we characterize the census tracts (known as *manzanas* in Colombia) based on their *estrato*. Utilizing Facebook's ad tool, we target ads to specific geographic units within a given radius of a geolocation, identifying three *clusters* of census tracts. Each cluster corresponds to one of the previously discussed income groups, optimizing our ad targeting and recruitment efficiency. Figure A.11 displays the geographic location of each cluster. Using this method, we aim to sample census tracts to obtain a minimum of 5,000 completed surveys. These numbers ensure representativeness and provide a sufficiently large sample to conduct analyses within Bogotá by quota subgroup.

We invite Facebook and Instagram users to participate in the survey.[34] We redirect people

[33] Boas et al. (2020) find that Facebook offers broad geographic coverage and facilitates targeted sampling compared to Mturk or Qualtrics panels. In addition, we rely on questions about respondent characteristics to account for sample selection.

[34] To design the recruitment process, we follow the latest developments and best practices as described in Neundorf and Öztürk (2021) and Kühne and Zindel (2020). Recent projects have used Facebook to achieve low-cost, represen-

Figure A.11: Geographic Location of Census Tract Clusters



Note: The lower cluster depicts the low-income group, the middle cluster shows the middle-income group, and the upper cluster denotes the high-income group.

clicking on the ads to complete our questionnaire on Qualtrics. The integration of Facebook and Instagram with Qualtrics enables us to enforce the quotas mentioned above (Zhang et al., 2020; Rosenzweig et al., 2020), in addition to tracking the daily response rate to manage costs per completed survey per census tract and prevent overspending in census tracts that do not meet expected quotas.

Table A.14 summarizes the quota completions and costs per result (e.g., ad clicks and completed surveys). To address challenges in reaching three of the four low-income quotas, we expanded the low-income sampling cluster to additional areas in Bogotá and Soacha, a nearby municipality closely linked to the capital's economy and society. Figure A.12 illustrates the newly sampled low-income areas. The cost per result remains low across all quotas, staying below USD 15. As expected, middle-income strata have the lowest costs, while low—and high-income respondents—who are harder to reach—incur higher costs, likely due to lower participation interest or greater time constraints.

Our recruitment ads were designed to engage a broad audience and encourage survey completion by inviting participants to share their views on issues affecting Colombians. To appeal to all income groups, we used images depicting diverse individuals. We included call-to-action messages emphasizing that researchers from Harvard Business School and Universidad de los Andes conduct the study. The ads targeted adults over 18 and clearly stated that all survey participants

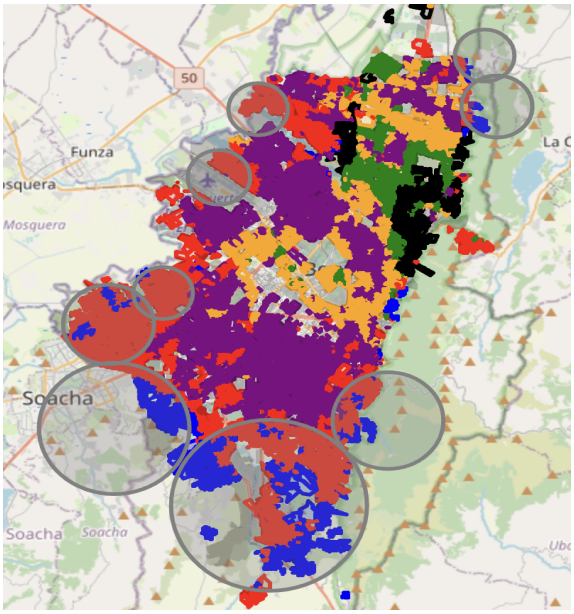
tative surveys (Grow et al., 2021; Stantcheva, 2022) in high-income economies (e.g. Zhang et al. (2020) in the United States) as well as in countries in the Global South (e.g. Rosenzweig et al. (2020) in Mexico and Kenya, and Samuels and Zucco (2013) in Brazil).

Table A.14: Recruitment Campaign Statistics

Quotas	Required Surveys	Completed Surveys	Cost per Result (USD)
Women			
High-income Group - Older than 35 Years Old	106	119	8.21
High-income Group - Younger than 35 Years Old	36	38	10.19
Middle-income Group - Older than 35 Years Old	755	1,202	5.73
Middle-income Group - Younger than 35 Years Old	361	604	5.53
Low-income Group - Older than 35 Years Old	692	552	13.69
Low-income Group - Younger than 35 Years Old	454	448	10.99
Men			
High-income Group - Older than 35 Years Old	81	127	5.46
High-income Group - Younger than 35 Years Old	31	58	4.59
Middle-income Group - Older than 35 Years Old	597	1,350	3.81
Middle-income Group - Younger than 35 Years Old	346	581	5.35
Low-income Group - Older than 35 Years Old	586	705	7.93
Low-income Group - Younger than 35 Years Old	455	439	11.26
Total	4,500	6,223	≈ 7

Note: Data based on authors' calculations and Facebook analytics as of June 17th, 2024, upon completion of the experiment.

Figure A.12: Additional Low-Income Sampling Clusters



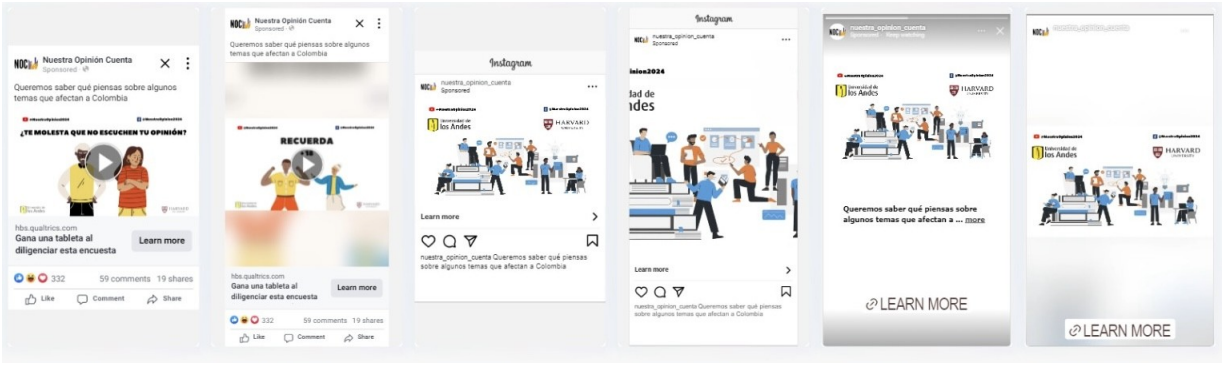
Note: All clusters depict the additional low-income areas sampled to reach low-SES strata.

would be entered into a raffle for a Samsung Galaxy A9 tablet. Figure A.13 shows the main screens of our recruitment ads.

We supplemented our ads with a Facebook fan page and a YouTube channel[35], where we shared a brief project description (*Nuestra Opinión Cuenta* or Our Opinion Counts), study objectives, informed consent details, and a contact email for participant inquiries. We also uploaded a short video introducing our research team, outlining our interests, and summarizing the project's goals. This strategy expanded our reach, strengthened recruitment credibility, and increased transparency. Additionally, we expected it to make participants feel more comfortable engaging in the study.

[35]Our Facebook fan page is available here, and our YouTube channel here.

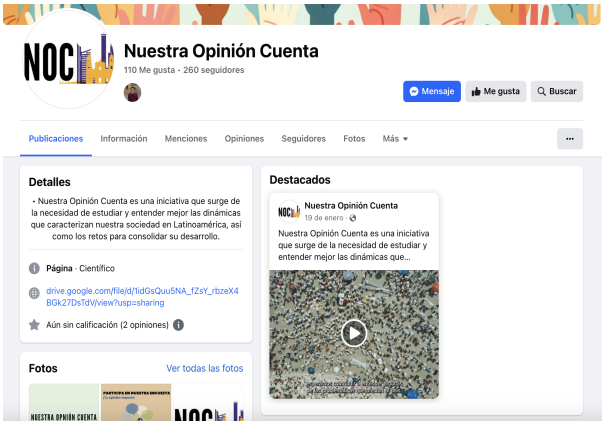
Figure A.13: Recruitment Ads Main Screens



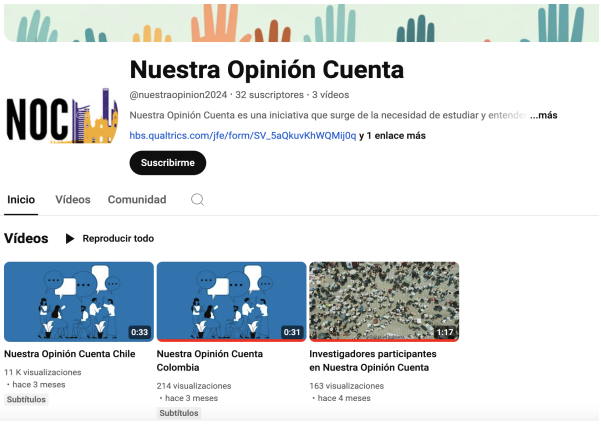
Figures A.14a and A.14b show our Facebook page and YouTube channel.

Figure A.14: Recruitment Pages

(a) Facebook Fan Page



(b) YouTube Channel



C.2 Implementing the population AMCE

To apply the "design approach" in de la Cuesta et al. (2022) and calculate a more externally valid population Average Marginal Component Effect, we constructed profile attributes using data from the 2022 wave of the *Gran Encuesta Integrada de Hogares* (GEIH). This survey, conducted by Colombia's statistical institute DANE, provides a representative sample for Bogotá and includes rich socioeconomic information such as sex, income sources and levels, and education.

We randomized profiles based on the joint distribution of sex, income groups, and educational attainment, excluding individuals still studying. We focused on individuals aged 25 to 45 to align with the range in our audio files. Since no reliable data on work experience existed in this or other surveys, we assigned a uniform distribution of experience levels—high, medium, and low for the profile's age—to ensure interpretability for respondents.

Tables A.15 and A.16 present the resulting joint distributions. The first two columns show the frequency of profiles assigned in the pilot survey, while the next three describe the probabilities used for randomization based on the GEIH sample. In a few sex $\times$ income $\times$ education cells (3 out of 60), there were no cases in the GEIH data. To address potential small-sample concerns, we made minor adjustments, imputing a small frequency in each case.[36]

Finally, no empirical data exists to estimate realistic distributions of Big Five personality traits in Bogotá while also accounting for income and education. Given this limitation and our strong interest in this dimension, we treated the Big Five traits as attributes of interest and randomized them uniformly.

[36]For instance, the GEIH sample contained no males with primary education earning between 4.2 and 6.8 million pesos. Since there were five cases in the preceding income range and one in the subsequent one, we linearly interpolated and assigned three cases (as shown in the 'Frequency (adjusted)' column). We applied a similar adjustment for females with primary education earning between 1.7 and 2.6 million pesos. For women with primary education and incomes above 6.8 million pesos, we assigned the smallest frequency of 1, consistent with other high-salary cases at that education level.

Table A.15: Joint distribution of income and human capital – Male

Profile	Pilot Conjoint Profiles		GEIH Survey Profiles		
	Frequency	Probability (%)	Frequency	Frequency (Adjusted)	Probability (%)
Primary 0 - 1,100	101	4.269	126	126	4.16
Primary 1,100 - 1,700	37	1.564	41	41	1.35
Primary 1,700 - 2,600	6	0.254	10	10	0.33
Primary 2,600 - 4,200	5	0.211	5	5	0.16
Primary 4,200 - 6,800	1	0.0423	0	3	0.1
Primary More than 6,800	0	0	1	1	0.033
Secondary 0 - 1,100	592	25.02	782	782	25.80
Secondary 1,100 - 1,700	289	12.21	390	390	12.87
Secondary 1,700 - 2,600	85	3.593	99	99	3.27
Secondary 2,600 - 4,200	17	0.719	35	35	1.15
Secondary 4,200 - 6,800	11	0.465	9	9	0.30
Secondary More than 6,800	1	0.0423	4	4	0.13
Technical 0 - 1,100	119	5.030	141	141	4.65
Technical 1,100 - 1,700	121	5.114	163	163	5.38
Technical 1,700 - 2,600	75	3.170	91	91	3
Technical 2,600 - 4,200	25	1.057	40	40	1.32
Technical 4,200 - 6,800	9	0.380	12	12	0.4
Technical More than 6,800	3	0.127	4	4	0.13
University 0 - 1,100	75	3.170	104	104	3.43
University 1,100 - 1,700	115	4.861	137	137	4.52
University 1,700 - 2,600	151	6.382	190	190	6.27
University 2,600 - 4,200	151	6.382	179	179	5.91
University 4,200 - 6,800	63	2.663	91	91	3
University More than 6,800	51	2.156	58	58	1.91
Postgraduate 0 - 1,100	2	0.0845	4	4	0.13
Postgraduate 1,100 - 1,700	4	0.169	10	10	0.33
Postgraduate 1,700 - 2,600	37	1.564	37	37	1.22
Postgraduate 2,600 - 4,200	72	3.043	82	82	2.71
Postgraduate 4,200 - 6,800	70	2.959	82	82	2.71
Postgraduate More than 6,800	78	3.297	101	101	3.33
Total	2,366	100	3,028	3,031	100

***Notes*:** Income level in thousands of Colombian Pesos

Table A.16: Joint distribution of income and human capital – Female

Profile	Pilot Conjoint Profiles		GEIH Survey Profiles		
	Frequency	Probability (%)	Frequency	Frequency (Adjusted)	Probability (%)
Primary 0 - 1,100	71	2.922	107	107	3.46
Primary 1,100 - 1,700	17	0.700	15	15	0.49
Primary 1,700 - 2,600	7	0.288	0	8	0.26
Primary 2,600 - 4,200	0	0	1	1	0.03
Primary 4,200 - 6,800	1	0.0412	0	1	0.03
Primary More than 6,800	1	0.0412	0	1	0.03
Secondary 0 - 1,100	610	25.10	783	783	25.32
Secondary 1,100 - 1,700	172	7.078	217	217	7.02
Secondary 1,700 - 2,600	24	0.988	37	37	1.12
Secondary 2,600 - 4,200	4	0.165	7	7	0.23
Secondary 4,200 - 6,800	9	0.370	7	7	0.23
Secondary More than 6,800	0	0	1	1	0.03
Technical 0 - 1,100	192	7.901	284	284	9.18
Technical 1,100 - 1,700	146	6.008	182	182	5.89
Technical 1,700 - 2,600	62	2.551	73	73	2.36
Technical 2,600 - 4,200	11	0.453	15	15	0.49
Technical 4,200 - 6,800	1	0.0412	4	4	0.13
Technical More than 6,800	6	0.247	6	6	0.19
University 0 - 1,100	116	4.774	165	165	5.34
University 1,100 - 1,700	149	6.132	179	179	5.79
University 1,700 - 2,600	188	7.737	235	235	7.60
University 2,600 - 4,200	169	6.955	188	188	6.08
University 4,200 - 6,800	83	3.416	86	86	2.78
University More than 6,800	43	1.770	45	45	1.46
Postgraduate 0 - 1,100	11	0.453	10	10	0.32
Postgraduate 1,100 - 1,700	12	0.494	15	15	0.49
Postgraduate 1,700 - 2,600	46	1.893	67	67	2.17
Postgraduate 2,600 - 4,200	107	4.403	138	138	4.46
Postgraduate 4,200 - 6,800	80	3.292	106	106	3.43
Postgraduate More than 6,800	92	3.786	109	109	3.53
Total	2,430	100	3,082	4,092	100

***Notes*:** Income level in thousands of Colombian Pesos

C.3 Power calculations

We simulate random samples of all possible profiles, weighted by an empirical joint distribution, to calculate the power of our desired estimates of the effects of a high-class accent on outcomes. We present results for two main estimators: the population AMCE or pAMCE and a high-class accent profile's Marginal Means (MM).

We ask subjects to solve four conjoint tasks and then incentivize them to continue with three additional tasks for a maximum of three more times. Thus, tasks per respondent are in the 4 to 13 range. We err on the conservative side for our power calculations and assume that each respondent completes six tasks. Recall that the number of respondents in each conjoint version varies, with type "A" assigned to half of the total sample and each of the remaining type B and C versions assigned to 10% of our respondents.

To estimate power, we create a grid of hypothesized effect sizes, defined as a marginal probability, with one hundred elements ranging from 0 to 0.2. For each hypothesized effect, we run 1,500 simulations. In each group, we create an indicator variable equal to one if the estimated effect is statistically significant at the 95% level and zero otherwise. Finally, power is defined as the proportion of statistically significant simulations. Given our experimental design, the effective sample size for each simulation is defined as $N = \#\text{Profiles} \times \#\text{Tasks} \times \#\text{Respondents}$ (Hainmueller et al., 2014). The number of profiles equals 2, the number of tasks is set to 6, and the number of respondents varies within the simulation. We expect to get nearly 5,000 respondents, so we define a grid of possible respondents ranging from 1,000 to 5,000 with 500 steps.

C.3.1 Full conjoint

In this section, we present estimates for the full conjoint version, labeled version "A," which comprises 50% of the sampled respondents (see Figure 1). Recall that Appendix C.2 describes the empirically realistic joint distributions that we use to simulate different profiles to improve the external validity of our exercise. For our power calculations, we use the joint distributions as presented there.

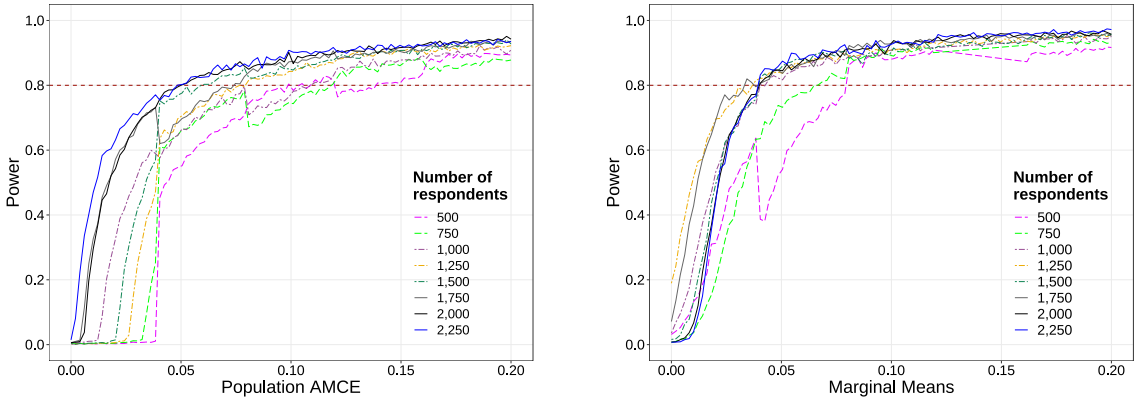
Figure A.15 shows the power calculations using hypothetical experiments in the full conjoint version "A." Panel A.15a shows the calculations for the population AMCE (pAMCE), while panel A.15b shows the marginal means (MMs). In each panel, we plot the estimated power (y-axis) in each of our 1,500 simulations along the grid of target effects (x-axis) by the number of respondents (from 500 to 2,500 in increments of 250, obtained by multiplying the hypothesized total number of subjects by 0.5). The horizontal dashed line represents a power of 80%.

Based on these calculations, we are powered to detect fairly small effects. Specifically, given our expected number of respondents (5,000),[37] we can detect effects of a high-class accent of 0.0485 (2,250 respondents) using the pAMCE estimator. On the other hand, as for the MMs, we can detect effects of a high-class accent of 0.0404 (2,250 respondents).

[37]Notice that, by the time we uploaded this pre-analysis plan, the number of completed surveys, as reported by Qualtrics, was close to $6,000$.

Figure A.15: Power Calculation Simulations

(a) Population AMCE **(b) Marginal Means**



***Notes*:** This Figure shows power calculations for the population AMCE (panel a) and the marginal means (panel b) estimator. For this exercise, we use a sample of 500 to 2250 with steps of 250 respondents, 1,500 simulations, and a grid of desired effects of a hundred elements ranging from 0 to 0.2. Power is the proportion of simulations in which the estimated effect is significant. Each line represents the estimated power for every sample in our full conjoint version A. To estimate the $pAMCE$, we use the function `design_pAMCE` from package `factorEx`. On the other hand, to estimate the MM, we use the function `cj` from package `cregg`. All simulations were done in `R` using `R` version 4.3.1 in a Windows operating system.

Table A.17 presents the Minimum Detectable Effect (MDE) by the number of respondents. Each row represents the number of respondents in the sample. Column 1 presents estimates for the population AMCE and column 2 for the MM. We estimate to detect effects for the pAMCE ranging from 0.0485 (2,225 respondents) to 0.1192 (750 respondents). As for the MM, we estimate to detect effects ranging from 0.0343 (1,250 respondents) to 0.0808 (500 respondents).

Table A.17: Minimum Detectable Effect by estimator

Sample size	MDE estimate	
	pAMCE	Marginal Means
	(1)	(2)
500	0.0990	0.0808
750	0.1192	0.0667
1,000	0.1091	0.0404
1,250	0.0768	0.0384
1,500	0.0606	0.0404
1,750	0.0747	0.0343
2,000	0.0505	0.0404
2,250	0.0485	0.0404

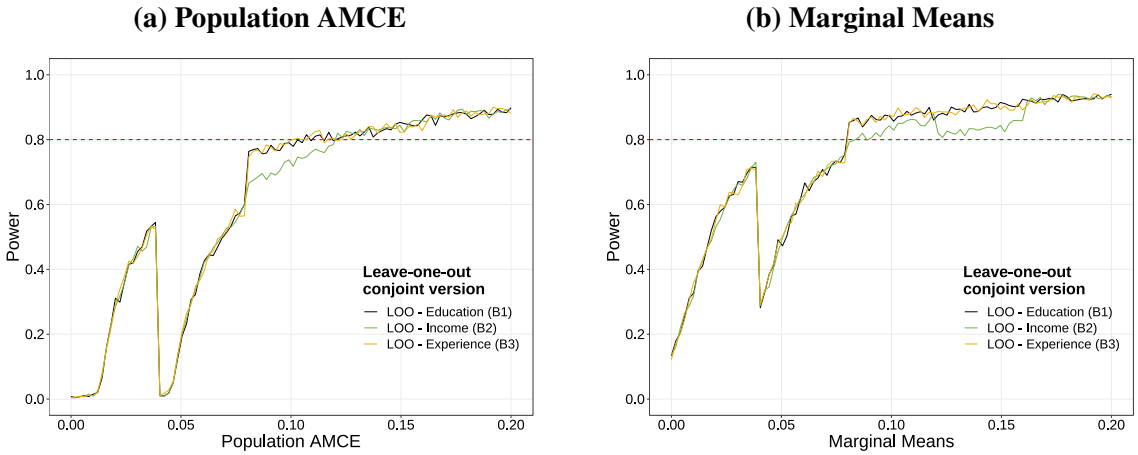
***Notes*:** This Table shows power calculations for the population AMCE (column 1) and the marginal means (column 2) estimator. For this exercise, we use a sample of 500 to 2250 with steps of 250 respondents, 1,500 simulations, and a grid of desired effects of a hundred elements ranging from 0 to 0.2. Power is the proportion of simulations in which the estimated effect is significant. Each row represents the minimum detectable effect for each sample size using the ad-hoc rule of thumb of 80% power. To estimate the $pAMCE$, we use the function `design_pAMCE` from package `factorEx`. On the other hand, to estimate the MM, we use the function `cj` from package `cregg`. All simulations were done in `R` using `R` version 4.3.1 in a Windows operating system.

C.3.2 Leave-one-out conjoints

In this section we present estimates for the "Leave-one-out" (LOO) conjoint versions, labeled as versions "B": LOO education conjoint (B1), the LOO experience conjoint (B2), and the LOO Income conjoint (B3). Under the experimental design, each conjoint version comprises 10% of the sampled respondents (see Figure 1). As in Section C.3.1, we use the joint distribution described in Appendix C.2. However, for conjoint versions B1 and B3, we update our joint distribution by dropping the income and education variables from the estimation process. Thus, to address external validity, we aggregate the probabilities in tables to match the variables in the specification (e.g., for B1, we aggregate over income and sex to get a new joint distribution).

Figure A.16 shows results for LOO conjoint versions B1, B2, and B3. Panel A.16a presents the estimates for the population AMCE, while panel A.16b presents estimates for the MM. Each panel plots the estimated power (y-axis) in each of the 1,500 simulations along the grid of target effects (x-axis) for an effective sample size of 500 respondents (remember that each LOO conjoint version comprises 10% of the sample, and we expect to survey a total of around 5,000 respondents). The black solid line presents estimates for the LOO conjoint version B1. The green solid line presents estimates for the LOO conjoint version B2, and estimates for the LOO conjoint version B3 are shown in yellow.

Figure A.16: Power Calculation Simulations



***Notes*:** This Figure shows power calculations for the population AMCE (panel a) and the marginal means (panel b) estimator. For this exercise, we use a sample of 500 respondents, 1,500 simulations, and a grid of a desired effect of a hundred ranging from 0 to 0.2. Power is defined as the proportion of simulations in which the estimated effect is significant. The black solid line presents the estimates of our Education Leave-one-out conjoint version (B1). The green solid line presents our Income Leave-one-out conjoint version (B2) estimates. The yellow solid line presents estimates of our Experience Leave-one-out conjoint version (B3). To estimate the $pAMCE$, we use the function `design_pAMCE` from package `factorEx`. On the other hand, to estimate the MM, we use the function `cj` from package `cregg`. All simulations were done in `R` using `R` version 4.3.1 in a Windows operating system.

Our estimates show that we have the power to estimate fairly small effects. Panel A.16a shows a MDE of 0.103 for the LOO conjoint B1 version, 0.1212 for the LOO conjoint B2 version, and 0.101 for the LOO conjoint B3 version. Panel A.16b shows an MDE of 0.08081 for the LOO conjoint B1 version, 0.08485 for the LOO conjoint B2 version, and 0.08081 for the LOO conjoint B3 version.