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THE IMPACT OF DIVERSITY ON PERCEPTIONS OF INCOME DISTRIBUTION AND PREFERENCES FOR REDISTRIBUTION

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ABSTRACT

Does diversity affect people's perceptions of income distribution and their preferences for redistribution?I leverage variation from a Colombian financial aid reform boosting the share of low-income students at an elite university. Combining college records and original survey data, I study how diversity affects high-income students' social networks, perceptions, and preferences by exploiting treatment variation across cohorts and majors using difference-in-differences. Exposure to low-income peers caused high-income students to diversify their social networks, have more accurate perceptions of the income distribution, and support progressive redistribution. My preferred interpretation is that diversity raised students' concerns about (the lack of) equal opportunity.

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A data appendix is available at http://www.nber.org/data-appendix/w30386

1 Introduction

What drives people's preferences for redistribution? Researchers have shown that individual preferences can respond to (mis)perceptions about the distribution of income and social mobility, which are formed endogenously through individuals' access to information and social interactions (Alesina et al., 2018; Cruces et al., 2013; Kuziemko et al., 2015). Can diversifying individuals' social networks by exposing them to socioeconomically diverse peers change their (mis)perceptions and support for redistribution? Answering this question is challenging because social networks are endogenous, making it difficult to identify causal effects. Indeed, previous studies have rarely manipulated social interactions outside the lab or field experiment. As a result, we know little about how real-life policies promoting socioeconomic diversity affect individuals' perceptions and preferences.

This paper overcomes these challenges to estimating how exposing high-income individuals to low-income peers affects their perceptions of the income distribution and social mobility, and their support for progressive redistribution. In 2015, the Colombian government implemented a massive financial aid program for low-income high achievers to attend selective universities. The policy generated an unprecedented and unanticipated influx of low-income students into elite institutions, boosting the socioeconomic diversity of their student body (Londoño-Vélez et al., 2020). Unlike affirmative action, which usually trades off diversity for measured ability, Colombia's financial aid policy left the college admissions process virtually unaffected. Low-income students were not given preferential treatment in admissions, allowing me to disentangle between exposing high-income students to socioeconomically diverse versus lower-achieving peers. This unusual setting enables me to estimate how perceptions and preferences are causally influenced by exposure to more low-income peers (of similar measured ability).

I focus on a university that historically catered to high-income students, but quadrupled its share of entering low-income students due to the policy. I collect original survey data six and twelve months after the policy rollout. I survey students who began their studies before and after the policy, and measure their social networks, perceptions, and preferences. Finally, I combine these survey records with the partner university's administrative records on admissions and classroom composition to create a measure of a student's exposure to low-income peers—the main treatment variable.

I leverage four key institutional features for identification. First, the policy raised diversity for cohorts entering college in spring 2015, whereas older cohorts were significantly less affected by the policy. Second, I can compare high-income students across cohorts because their composition remained constant immediately after the policy. Third, there is substantial variation in the share of low-income students across majors, which are declared when people apply to college and, therefore, before applicants know the socioeconomic composition of their peers. Fourth, thanks to the surprise introduction of the policy and other institutional details, there is little room for self-selection into courses with more or fewer low-income peers. A difference-in-differences (DD) design leverages these crucial features to identify the effect of exposing high-income students to low-income peers (hereafter, the treatment). The DD compares high-income students across cohorts (starting college before and after the policy rollout) and majors (with smaller and greater shocks in diversity). Therefore, the identifying variation in exposure to socioeconomic diversity comes from comparing students in the same major across cohorts and students in the same cohort across majors.

To examine an individual's awareness of another person's socioeconomic status (SES), I exploit a unique feature of this setting: Colombia's socioeconomic stratification system (*estratos*). This system explicitly stratifies households from 1 to 6 by affluence based on neighborhood and dwelling characteristics (1 being the poorest).[1] Although strata do not correlate perfectly with SES (Joumard and Londoño-Vélez, 2013), they provide useful information beyond income measures. Many Colombians use stratum to mean social class. Moreover, most Colombians are well aware of their stratum, making this information easy to collect. Furthermore, an individual can infer others' strata (based, for instance, on where they live) better than they can other measures of SES, like household income or wealth. For this reason, the stratum is a popular measure of SES among researchers and policy-makers in Colombia. In addition, its salience makes it ideal for studying perceptions of SES and inequality.

There are four key findings. First, the policy caused high-income students to be exposed to low-income peers and fostered interactions among students of diverse socioeconomic backgrounds. The diversification of social networks is substantial. For instance, high-income students beginning college after the policy rollout are three times as likely to have worked in a group project with a program beneficiary than students from older cohorts and twice as likely to name a low-income student among their five closest friends. By contrast, previous research has shown that changing the group of potential peers does not necessarily change the peers one chooses to associate with or may change them in unexpected ways (Carrel et al., 2013). However, I find that high-income students

[1] Colombia's stratification system was created in 1994 for municipalities to classify their population into groups with similar social and economic characteristics in order to establish cross-class subsidies to help those in the lower strata pay for utilities (water, electricity, gas, sewage, and telephone). Since then, the State has used the stratification system to target other programs (e.g., student loans).

have significantly increased interactions with lower-income students due to the policy.

Second, high-income individuals have a dramatically biased perception of the income distribution. In particular, they underestimate the share of low-income people—both the official poverty rate and the percentage of people in the lower socioeconomic strata—and overestimate the share of people in the higher socioeconomic strata. Interestingly, high-income students are more biased in their perception of the distribution of socioeconomic strata than in their perception of the poverty rate, severely overestimating the share of high-income people in socioeconomic strata similar to theirs. This upward bias is consistent with endogenous reference groups. That is, high-income students interact primarily with similarly affluent peers and have few interactions with low-income peers, partly due to the severe socioeconomic segregation of high schools and colleges. As a result, high-income individuals mistakingly use their high-income peers as a reference when inferring the entire income distribution, causing them to underestimate the poverty rate and the level of inequality.

Third, exposing high-income students to low-income peers slashes the upward bias in their perception of the income distribution and causes more accurate perceptions about the poverty rate and the share of low- and high-strata families. For example, a ten percentage point increase in exposure to low-income peers boosts the perceived share of the population earning below the official poverty line by 2.59 percentage points, fully eliminating the bias in the perceived poverty rate. It also raises the perceived share of low-strata individuals by 1.83 percentage points and lowers the perceived share of high-strata individuals by 2.19 percentage points. However, because high-income students dramatically overestimate the share of people in their own socioeconomic strata, the implied bias reduction is more moderate in percentage terms. Overall, a ten percentage point increase in exposure to socioeconomic diversity raises a summary index of perceived poverty by 0.12 standard deviations (the p-value is 0.009 when comparing students within majors and across cohorts, and 0.084 in the DD specification).

Fourth, exposure to low-income peers strengthens high-income students' support for progressive redistribution, in terms of both taxing the rich and subsidizing the poor. Specifically, a ten percentage point increase in exposure to low-income peers boosts support for taxing the rich by 8 percentage points, which is an 11.4% increase relative to a baseline mean of 70.8%. Diversity has a similar, but more noisily estimated, effect on increasing support for subsidizing the poor. Overall, a ten percentage point increase in socioeconomic diversity strengthens high-income students' preferences for redistribution by 0.12–0.15 standard deviations (the p-value is 0.009 when comparing students within majors and across cohorts, and 0.025 in the DD specification).

In theory, correcting people's bias in their perception of the poverty rate and the level of inequality can affect their preferences for redistribution for several reasons. First, if high-income individuals are self-interested, exposing them to low-income peers makes them realize that they are richer than they initially thought and stand to lose from raising taxes on the rich, making them oppose redistribution. Indeed, this is the prediction from the canonical model of self-interested voters (Meltzer and Richard, 1981). However, my finding that high-income people become *more* (not less) favorable to redistribution do not support this prediction.[2]

Second, people's attitudes toward redistribution might be influenced by their perceived past and future income trajectories (Benabou and Ok, 2001; Hirschman and Rothschild, 1973; Piketty, 1995). On the one hand, if exposure to low-income peers makes people more optimistic about the prospects of upward social mobility for low-income individuals, they might become less inclined to support government intervention. On the other hand, high-income students might become more supportive of redistribution if the treatment negatively changes their expectations about their own income trajectory (Bergolo et al., 2021; Ravallion and Lokshin, 2000). However, I do not find evidence of this: people's perception of upward (downward) social mobility for low- (high-)income individuals does not seem affected by their exposure to low-income peers.

Lastly, people's preferences for redistribution might depend on their views on distributive justice and whether they believe market outcomes are fair. For example, suppose that the treatment made people more pessimistic about fairness and equal opportunity. In that case, they might become more supportive of government intervention—even if they are self-interested and optimistic about the prospects of upward social mobility. Indeed, I provide evidence consistent with the treatment significantly strengthening people's perception of how difficult it is to overcome poverty without government intervention: a ten percentage point increase in exposure to diversity raises concerns about equal opportunity without government intervention by 26.6%.

These findings contribute to the literature on how individuals form preferences for redistribution based on subjective perceptions of their position in the income distribution (Cruces et al., 2013; Hoy and Mager, 2021; Hvidberg et al., 2021; Karadja et al., 2017; Meltzer and Richard, 1981), social justice (Alesina and La Ferrara, 2005; Alesina and Angeletos, 2005; Alesina et al., 2001), social mobility (Alesina et al., 2018; Benabou and Ok, 2001; Piketty, 1995), inequality (Ariely and Norton, 2011; Kuziemko et al., 2015), culture (Luttmer and Singhal, 2011), interpersonal preferences (Luttmer, 2001), and reference

[2]Recent studies also suggest that people do not behave purely based on self-interest. For instance, Cruces et al. (2013) show, using a field experiment, that informing people who underestimated their own position in the income distribution does not affect their stated preferences for redistribution.

points (Charite et al., 2016). Most of these studies use surveys, labs, or field experiments to estimate causal effects on the demand for government redistribution.[3] By contrast, I study the formation of perceptions and preferences using policy-induced variation. This is crucial because how people behave in the lab or the field might differ from how they react to government interventions in practice. Specifically, I evaluate how a real-life, large-scale policy intervention—a financial aid program for low-income students—affects individuals' perceptions and preferences. This natural experiment is useful for studying the causal effects of peer composition on perceptions and preferences. It is also especially policy-relevant, given recent evidence of socioeconomic segregation across colleges (Chetty et al., 2020) and the prospect of fostering diversity and boosting social mobility through need-based financial aid (Hoxby and Avery, 2013).

A related literature in behavioral economics studies the effects of affirmative action—which directly promotes socioeconomic and/or racial diversity, often at the expense of measured ability—on social behaviors, like generosity, discrimination, stereotyping, and prejudice (Boisjoly et al., 2006; Burns et al., 2019; Rao, 2019).[4]

The remainder of the paper is organized as follows. Section 2 introduces the intuition for why exposure to low-income peers might affect individuals' perceptions of the income distribution and their preferences for redistribution. Section 3 provides some institutional background and describes the financial aid program. Section 4 presents the data, while Section 5 presents the methodology used. Section 6 describes social interactions between high- and low-income students and their awareness of each other's SES. Section 7 presents the main results. Finally, Section 8 concludes.

2 Conceptual Framework

This section summarizes the statistical inference problem in individuals' assessment of the income distribution (for a more detailed discussion, see Cruces et al. (2011, 2013) and references therein). Individuals infer the distribution of income based on their access to information about other individuals' level of income and their ability to process this information. In the presence of limited information, individuals observe the income levels of only a subset of the population and apply Bayes' rule to infer the entire distribution from

[3] A notable exception is Perez-Truglia (2020), who studies the effect Norway's policy of posting citizens' tax records online on people's well-being.

[4] In addition to recent works by economists (e.g., Bazzi et al., 2019; Billings et al., 2021; Burns et al., 2019; Carrell et al., 2019; Finseraas et al., 2019; Lowe, 2021; Rao, 2019), the study of contact theory (Allport, 1954) has also been explored by psychologists, sociologists, and political scientists (see Paluck et al., 2019, for a recent review). I bring together the literatures on intergroup contact theory and perceptions about inequality and support for redistribution in the context of a real-world public policy.

the subset they observe (the "reference group"). If individuals are fully rational, they arrive at consistent estimates of the income distribution by accounting for the relative size of the reference group, the selection or non-representativeness of this reference group, and their ability to make probability judgments. If, instead, individuals are naïve—i.e., they fail to apply Bayes' rule properly—they have systematically biased inferences of the income distribution.

Selection into a reference group is likely a function of income: individuals with a "high-income" reference group are more likely to observe high-income individuals and vice versa. Thus, naïve persons with a high-income reference group are more likely to overestimate the share of high-income individuals and have biased estimates of many moments of the income distribution, such as the mean, dispersion, and fraction of individuals under the poverty line (for an illustration, see Figure A.1). More generally, if a reference group is more homogeneous in income than the total population, perceptions about income inequality will be biased downward.

Applying this conceptual framework to my empirical setting, consider a college student who infers the income distribution from their reference group, including friends and classmates. In a segregated higher education system, a high-income student might have a high-income reference group due to exposure only to similarly wealthy classmates and friendship homophily. The conceptual framework described above offers several testable predictions. First, a naïve high-income student with a high-income reference group systematically overestimates the share of high-income individuals and underestimates the share of the population below the poverty line. Moreover, exogenously exposing the naïve high-income student to low-income peers reduces their misperception.

Correcting the bias might impact individuals' stated preferences for redistribution depending on the individuals' self-interest and perceptions of social mobility and distributive justice. First, exposure to low-income peers might make high-income students realize that they are richer than they initially thought. Since progressive redistribution is financed by taxes on relatively high incomes, a self-interested high-income individual may realize that they stand to lose from redistribution and may, therefore, oppose it. This is the prediction of the seminal contribution by Meltzer and Richard (1981), which has mixed empirical support (Cruces et al., 2013; Hoy and Mager, 2021; Karadja et al., 2017).

Second, people's attitudes toward redistribution depend not only on their current income but also on their perceived past and future income trajectories (Hirschman and Rothschild, 1973; Piketty, 1995). In particular, people who are more pessimistic about the prospect of upward social mobility tend to favor more generous redistributive policies and higher levels of government involvement. This is the "POUM hypothesis" of Benabou and

Ok (2001), which has been supported empirically (Alesina et al., 2018; Bergolo et al., 2021; Ravallion and Lokshin, 2000). If exposure to low-income peers changes people's views on social mobility, this might influence their demand for government redistribution. On the one hand, if the treatment makes people more optimistic that poor individuals can easily move up the socioeconomic ladder, this will weaken their support for redistribution. On the other hand, if the treatment negatively affects high-income individuals' expectations about their own income trajectory—for instance, by making them more pessimistic about their future mobility as they learn that the fall can be harder—they might become more supportive of redistribution "just in case."

A third possibility is that people's preferences for redistribution depend on their perceptions about distributive justice. If people perceive that market outcomes are fair or the product of effort, they will support low redistribution. If, instead, they believe them to be the product of luck, they will demand higher redistribution to level the playing field (Alesina and La Ferrara, 2005; Alesina and Angeletos, 2005; Alesina and Giuliano, 2011; Fong, 2001). It is possible that exposure to low-income peers affects high-income people's views on what determines one's position on the social ladder, i.e., whether market outcomes are fair or the product of equal opportunity. In particular, if exposure to low-income peers makes high-income people perceive more unfairness, they may become more supportive of redistribution—even if they are self-interested and optimistic about the prospects of upward social mobility.

3 Background

Our setting is Colombia, an upper middle-income country with a highly segregated higher education system. Postsecondary institutions vary dramatically in terms of quality and tuition fees. Private and high-quality universities are costly, and, given the dearth of financial aid available, sorting across private universities is strongly defined by the tuition rates they charge (Riehl et al., 2019). While heavy government subsidies render public universities more affordable for low-income students, the best-quality institutions have excess demand and reject most applicants. As a result, most low-income students attend low-quality colleges or cannot access higher education (Ferreyra et al., 2017; Melguizo et al., 2016; Sanchez and Velasco, 2014).

In October 2014, the government announced *Ser Pilo Paga* (roughly, "hard work pays off" in Spanish; hereafter referred to as SPP), Colombia's first large-scale merit-based college financial aid program for low-income students. SPP was unusually comprehensive, covering the full tuition fee in any four- or five-year undergraduate program and providing a modest stipend. To be eligible, applicants had to be among the poorest 50% of high school seniors, as measured by Colombia's main proxy-means testing instrument, SISBEN. They also had to score among the top 9% of the national standardized high school exit exam, SABER 11, and be admitted to one of Colombia's 33 government-certified "high-quality" universities. Between 2014 and 2018, roughly 40,000 students benefited from SPP.

Londoño-Vélez et al. (2020) showed that the policy dramatically expanded low-income high achievers' access to higher education because students had been severely constrained in their ability to finance their collegiate studies through credit markets. The enrollment impacts were dramatic for high-quality universities and especially high-quality *private* universities. In particular, low-income students barely eligible for SPP based on their test scores became 46.6 percentage points more likely to enroll in these elite institutions immediately after high school. Relative to the 3.3% of low-income students barely *in*eligible for SPP accessing elite institutions, this represents a more than fifteenfold increase in enrollment. As a result, the policy radically diversified the student body composition of Colombia's elite universities: the share of low-income students entering high-quality private universities increased by nearly 50%.[5]

This unprecedented increase in socioeconomic diversity was exceptionally pronounced at one of the elite universities, hereafter referred to as the partner university. This university had historically catered to high-income students and had very few low-income students attending before SPP. However, by January 2015—three months after the announcement of SPP—roughly one-third of new entrants were beneficiaries of SPP. Figure 1 plots spring-entering students by their socioeconomic stratum (a measure of SES). The share of low-income students—henceforth defined as those from the bottom two strata—almost quadrupled from 7.1% in 2014 to 27.3% in 2015. The diversification of the college campus continued in the following years, with low-income students constituting 33.3% of all new entrants in 2016.[6]

Moreover, the share of SPP recipients varied substantially across majors. For instance, while 71.4 % of the entering Philosophy majors were beneficiaries of SPP, none of the entering Art History majors were beneficiaries of SPP at the partner university (Figure 2). This heterogeneity is likely a reflection of differences in tastes among the SPP

[5] A key factor for why low-income students are able to access top-ranked universities in Colombia is that postsecondary institutions predominantly base their admissions on SABER 11 scores and rarely require qualitative inputs, like letters of recommendation or essays. Moreover, unlike the SAT in the United States, SABER 11 is taken by virtually all high school seniors, regardless of their post-secondary intentions. See Londoño-Vélez et al. (2020).

[6] As other Colombian universities, admissions at the partner university are semi-annual because high schools operate on two different academic calendars. Most high school students begin classes in the spring term, while a minority—mostly from elite private high schools—begin courses in the fall. Consequently, fall cohorts have a higher socioeconomic background (see Figure A.2).

recipients. Applicants select their major when applying to the partner university and receive admission if their SABER 11 test score is above the major-specific threshold, which is unknown to applicants and depends on the available seats (see Barrera-Osorio and Bayona-Rodriguez, 2019). Critically, the share of SPP students in a given major is uncorrelated with the major's admission cutoff (the coefficient from regressing the percentage of SPP recipients on the admission cutoff is -0.25 with a p-value of 0.148).

Two features of SPP are essential for the analysis. First, *SPP did not affect the composition of high-income students entering the partner university immediately before and after the policy*. This is because, first, financial aid was awarded to new enrollees starting in spring 2015; the policy did not affect the composition of older cohorts. Second, admissions remained based solely on SABER 11 scores. Third, despite the increase in applications from SPP-eligible students, the admission rate of *high*-income applicants remained constant (Figures A.3 and A.4). Indeed, high-income students scored well above SPP's eligibility cutoff; on average, in the 98th percentile of national test scores in spring 2014 and spring 2015. Further, the university somewhat expanded the supply of its seats available in spring 2015 due to the increased demand triggered by the policy (Figure A.5).

The second key feature of SPP is that *students cannot self-select into being exposed to more or fewer SPP classmates* (*based, for instance, on their affinity for low-income peers*). This is partly thanks to the surprise introduction of the policy: the government announced SPP *after* students had taken the SABER 11 exam and shortly before the partner university's application deadline (Figure 3). Moreover, the diversification of elite campuses was disclosed *after* the spring 2015 term had begun (La Silla Vacía, January 13, 2015). As a result, high-income students from the spring 2015 cohort did not modify their application and enrollment decisions at the partner university in response to the policy: their number of applications, admission rate, and yield rate all remained constant (Figure A.3).[7]

In addition, high-income students have little ability to self-select into classroom exposure to low-income peers at the partner university. Universities were not allowed to track students by their SES and integrated SPP beneficiaries into the same classrooms as non-beneficiaries. This is important because students' primary interaction was inside the classroom, as there were no dorms available at the partner university during my study period. Moreover, the course curriculum is relatively set within a major at the partner university. As a result, students have significantly less freedom to choose their courses

[7]While students who find that they particularly dislike low-income classmates may theoretically switch to a major with a smaller prevalence of SPP recipients, there was no increase in switching to majors with fewer SPP beneficiaries (Figure A.6). The option of transfering to a non-SPP-receiving university is also unlikely, as there are high long-term costs of attending a university without high-quality certification (Camacho et al., 2017). Moreover, universities cannot self-select into receiving beneficiaries of SPP or not.

than at traditional American universities (especially during their first year). Together, these institutional features generate a unique opportunity to identify the causal effect of diversity.

4 Data

This section describes the administrative and survey data used, which comes from four main sources:

1. **Administrative records from the partner university on undergraduate admissions and enrollment.** The admissions records include detailed student-by-semester level information about applications, admissions, and matriculation for 2010–2016. These records include applicants' sociodemographic characteristics (e.g., sex, date of birth, socioeconomic stratum, parental education), SABER 11 test score, and major, as well as the university's admission score (i.e., a major-by-semester weighted average of the different components of SABER 11) and the major-by-cohort admission cutoff. For students enrolled at any moment between 2000 and 2016, I observe detailed semesterly information about the courses taken, which is used to construct the main treatment variable (the share of SPP classmates) as well as alternative treatment variables (e.g., the share of classmates from strata 1 and 2).

2. **Administrative micro-data from Colombia's Ministry of Education and ICETEX, the institutions in charge of the SPP program.** This includes student-level information about all SPP beneficiaries and is used to build measures of intensity of social interaction between high-income students and their SPP peers.

3. **Administrative data from ICFES, the institution in charge of administering the SABER 11 high school exit exam.** It contains information on all students taking the SABER 11 standardized exam between 2003 and 2016. I use this data to normalize test scores.

4. **Survey data collected by myself specifically for this research project using Qualtrics online survey software.**

The remainder of this section describes the survey design and implementation. I sampled high-income students—henceforth defined as socioeconomic strata 4, 5, and 6—enrolled in any undergraduate program at the partner university. My main sample in this study consists of students from all majors and three cohorts: those who entered in spring 2014, fall 2014, and spring 2015.[8] The questionnaire collected information on the student's perceptions about the income distribution, social justice, and redistributive preferences. Moreover, the survey recorded the student's social and study networks to measure how intensely high-income students interact with low-income peers and their perceptions about their SPP classmates. Appendix B includes the screenshots of the Qualtrics survey.

The link to the online survey was sent to students' institutional email from the partner university's Office of Admissions and Registration. The survey consent form did not mention SPP to avoid experimenter demand effects. Instead, it stated that the purpose of the survey was to "gather information on college students' beliefs and political attitudes." For their participation, respondents were compensated in cash (2015 COP 10,000 or roughly 2015 USD 3.4) or in kind (a burger combo at a popular burger chain near campus). In addition, respondents were allowed to donate their compensation.

I collected two waves of survey data in the first week of instruction in the fall and spring semesters. First, I completed wave 1 in August 2015, after one semester of treatment (Figure 3). Then, I collected wave 2 in early February 2016, after one year of treatment. I pool both waves in my main estimates and include the results for each wave separately in the Online Appendix.

In all, 20.2% of the 4,991 students from the three cohorts receiving the survey link (2,200 in wave 1 and 2,791 in wave 2) answered the survey. There is balance in the response rate across cohorts and I cannot reject the null hypothesis that the coefficients on the three cohort dummies are the same: the p-value on the joint F-statistic is 0.429 without controls and 0.332 with controls.[9] Moreover, the treatment (i.e., the share of SPP classmates) does not influence the likelihood of responding to the survey (Table A.1).

[8] Wave 1 surveyed three cohorts (spring 2014, fall 2014, and spring 2015). In light of the higher competition for college entry after SPP, wave 2 added a module about college preparation and application choices—which are not part of this study—and expanded the sample with two older cohorts (spring and fall 2013) and two younger cohorts (fall 2015 and spring 2016). Although Section 7.3 shows that the results hold using more cohorts, this study focuses on the three initial cohorts. First, they are the most comparable—they entered college immediately before and after SPP was implemented—and provide the highest internal validity. Second, students from these cohorts applied and matriculated with no knowledge of how SPP would diversify their campus. By contrast, younger cohorts arguably knew about SPP and could select their exposure to SPP recipients by choosing different college-major pairs or deferring their admission to a fall semester, which has fewer low-income students (Figure A.2). Finally, younger cohorts have a different composition, including a higher measured ability, because the admission rate dropped after spring 2015 (Figure A.3), raising average quality (Figure A.4).

[9] The likelihood of answering the survey is decreasing in students' socioeconomic stratum—22.0% for stratum 4, 20.3% for stratum 5, and 17.3% for stratum 6, consistent with the survey compensation being less likely to induce the highest-income students to respond. Moreover, the response rate is slightly increasing in students' SABER 11 test scores: a one percentile increase in the test score raises the likelihood of answering the survey by 0.5% (Table A.1). Since the highest-SES students potentially have the most biased perceptions about the income distribution, my estimates arguably represent a lower bound on the effect of diversity.

5 Empirical Strategy

This section describes the empirical approach used to estimate the effect of socioeconomic diversity on perceptions of the income distribution and preferences for redistribution. I exploit the plausibly exogenous exposure of high-income students to low-income peers introduced by the SPP financial aid policy as well as the variation in the treatment intensity across majors using the following DD specification:

$$y_{imkw} = \alpha + \beta \text{ Share of SPP Classmates}_i + \delta_m + \psi_k + \theta_w + \mathbf{X}_i'\Gamma + e_{imkw} \tag{1}$$

where y_{imkw} is outcome y for student i in major m and cohort k surveyed in wave w, Share of SPP Classmates$_i$ is the main treatment variable and represents the student's average share of classmates that receive SPP financial aid (on a scale from 0 to 100), δ_m are major fixed effects, ψ_k are cohort fixed effects, θ_w are survey wave fixed effects, $\mathbf{X}_i$ is a vector of individual controls, and e_{imkw} is a student-specific error term.[10] The β coefficient is thus the average effect on outcome y of a one percentage point increase in the share of classmates that are SPP recipients and is the key parameter of interest. This approach identifies the average effect on high-income students of adding low-income peers (who are of similar ability), which is a relevant estimate for common policies.

This DD approach exploits two sources of variation in the treatment variable. First, it leverages exogenous variation in treatment intensity across majors. Indeed, the share of SPP classmates varies considerably, from more than 50% for the Psychology, Languages, and Philosophy majors to less than 10% for the Business and Art History majors (Figure 2). As explained in Section 3, this cross-major variation in treatment intensity is unrelated to students' measured ability. It is also uncorrelated with students' attitudes toward low-income peers, as students choose their major when applying to college and, therefore, without knowing their future exposure to SPP classmates. Further, while students' perceptions and preferences may vary systematically across majors (if, say, Economics majors are less supportive of government intervention), the inclusion of major fixed effects in Specification (1) controls for such time-invariant characteristics. Second, the approach leverages the policy-induced variation in treatment intensity across cohorts: students who enter college after SPP are more exposed to low-income peers than those before it, as Figure

[10]The treatment variable is computed using the partner university's administrative data on course enrollment. Since students were surveyed in the first week of each term, I define the treatment as the share of SPP classmates in the previous term. For instance, survey wave 1 was collected in the first week of the fall 2015 term and I use information on course enrollment in the spring 2015 term. The vector $\mathbf{X}$ contains sex, age and its quadratic term, a migrant dummy, SABER 11 percentile, dummies for parental education, risk aversion (from a heads or tails game), and socioeconomic stratum.

A.7 shows. While students in different cohorts may vary in unobservable ways that may be correlated with the outcomes of interest, the cohort fixed effects in Specification (1) control for this. Therefore, the identifying variation in exposure to socioeconomic diversity comes from comparing students in the same major across cohorts and students in the same cohort across majors.[11]

To support the validity of this empirical design, Figure A.9 shows that the treatment is *not* correlated with student observable characteristics. Indeed, a common approach to validate the quasi-random nature of a treatment is to examine balance in observable characteristics between treated and control units. Following Chetty et al. (2014), I use the predicted outcomes, $\hat{y}_{imkw} = \mathbf{X}_i'\hat{\Omega}$, to have all the covariates on the same scale. Specifically, I regress the outcome on the baseline covariates $\mathbf{X}_i$ and define $\hat{y}_{imkw}$ as the predicted value. The figure plots the coefficients and associated 95% confidence intervals from regressing the predicted value of the outcome using a given baseline covariate, $\hat{y}_{imkw}$, on the treatment using Specification (1). The first line in each panel shows the result when $\hat{y}_{imkw}$ is based on all the covariates. The following rows separately use a single covariate to predict $\hat{y}_{imkw}$ at a given time. There is balance in observable covariates between students exposed to more versus fewer SPP peers.

6 Social Interactions between High- and Low-Income Peers

This section documents the extent to which the policy fostered socioeconomically diverse interactions. This point is important because one could be concerned that high-income students do not observe low-income students. For instance, they may be unable to infer others' SES, or they may not interact with them (as would be the case, for example, under extreme homophily). Either of these scenarios would bias my estimates toward zero.

Table 1 presents summary statistics combining both survey waves for the three cohorts of high-income students separately. The first row reports the average share of SPP classmates—the main treatment variable. For students entering college in spring 2014 (a year before SPP began), only 2.7% of their classmates in 2015 were SPP beneficiaries. By contrast, this share is 13.1% for students in the spring 2015 cohort. On average, the spring 2015 cohort is over ten percentage points or nearly five times more exposed to SPP beneficiaries.

To measure social networks and examine students' awareness of each other's SES, I asked survey participants to list their five closest friends and study partners (Figures B.1 and B.2) and their listed friends' socioeconomic strata (Figure B.3). There is a large

[11] Figure A.8 illustrates the variation in the treatment variable leveraged in the regression analysis.

variation in how diverse students' social groups are. For instance, the second row of Table 1 shows that students from the fall 2014 cohort, who graduated from elite private high schools and are wealthier (Figure A.2), have fewer low-income peers than spring cohorts. Moreover, only 4.8% of the spring 2014 cohort and 3.5% of the fall 2014 cohort report having a low-income friend, i.e., a friend from stratum 1 or 2. By contrast, students from the spring 2015 cohort are almost twice as likely to have low-income friends than older cohorts, and this difference is statistically significant (the p-value is 0.007). This reflects the dramatic effect of the policy on friendship formation among students with diverse socioeconomic backgrounds.

I next asked students about their perception of SPP classmates and their interactions. Specifically, I asked them to report what share of their classmates were SPP beneficiaries (Figure B.4) and how many times they had worked in a group project with SPP beneficiaries (Figure B.5). The results, reported in the third and fourth rows in Table 1, show that high-income students are well aware of their classmates' SPP status. Sociological work by Alvarez (2019) and Alvarez et al. (2022) finds SPP status (and SES status more generally) is often inferred from the clothing and mobile phone brands students use, the high school they attended, and the way they speak Spanish and English. In addition, while students from the spring 2015 cohort perceive a significantly higher prevalence of SPP classmates than students from older cohorts, all cohorts overestimate the actual share. Lastly, the spring 2015 cohort is three times more likely to have worked with an SPP beneficiary (the p-value is 0.000), consistent with the policy leading to more interactions of high-income students with low-income peers.

The final row shows how the policy diversified social networks by comparing the likelihood that a high-income student named an SPP beneficiary among their five closest friends or study partners. One in four students from the spring 2015 cohort named an SPP beneficiary among their five closest friends or study partners compared to only 4 percent in the older cohorts, on average. If people randomly chose their friends and study partners among their classmates, the spring 2015 cohort would have a 51% chance of having at least one SPP recipient among their five closest friends/study partners compared to 21.6% and 12.6% for the fall and spring 2014 cohorts, respectively. Thus, to the extent that peer group formation is inherently endogenous, Table 1 suggests that the policy substantially influenced social interactions between high- and low-income students and generated more socioeconomically diverse peer groups.

In contemporaneous work, several other researchers have documented how SPP diversified social interactions and promoted integration among high- and low-income students at the elite partner university. In particular, Velasco (2022) uses turnstile data on students' co-movements across campus to identify which students socialize based on how frequently they enter and exit campus buildings together. She finds that the number of turnstile-elicited links between high- and low-income students more than doubled, while the probability of high-income students having any link with a low-income student increased by 42%. In addition, sociological work by Alvarez (2019) and Alvarez et al. (2022) analyzes friendship networks using surveys, in-depth interviews, and ethnographic observations. They also find a high level of SPP-induced social integration at the elite university.[12]

7 Results

In this section, I test whether exposure to socioeconomic diversity affects high-income students' perceptions about the distribution of income and their preferences for redistribution.

7.1 Perceptions of the Income Distribution

The survey asked respondents two questions to measure their perceptions of the income distribution. First, it asked, "*What percentage of Colombians do you think are poor (that is, those earning less than 200 thousand pesos [2014 USD 84.16] per month)?*" on a scale from 0 to 100% (Figure B.6). Next, it asked about respondents' perceived distribution of socioeconomic strata: "*What share of Colombians do you think belong to each socioeconomic stratum?*" on a scale from 0 to 100% (Figure B.7). The latter question complements the former in several ways. First, eliciting students' perceptions of the distribution of socioeconomic strata enables mapping perceptions about the *entire* SES distribution and not only the fraction earning below the poverty line. Second, the stratum is a popular proxy measure of SES in Colombia; people know their own stratum and can infer others'. Third, the stratum provides useful information beyond income measures because many Colombians understand stratum to mean social class. For these reasons, asking people to think about the distribution of socioeconomic strata is helpful for studying perceptions of inequality.

[12] Following the rollout of SPP, several media outlets expressed their concern that traditional students at elite universities bullied or discriminated against SPP beneficiaries. Thus, survey wave 1 included a module about students' attitudes toward SPP recipients (and toward financial aid more generally) to examine these concerns. The results presented in Appendix C do not support out-group prejudice. Instead, high-income students appear to support SPP recipients on their campus, financial aid, and diversity, and the treatment had no impact on these attitudes. Moreover, respondents who completed the first survey could donate part of their compensation "to fund poor, high-achieving students studying at high-quality universities in Colombia." Again, the treatment did not affect the probability of donating.

Figure 4 plots the responses given by the spring 2014 cohort. The gray bars report the actual poverty rate in 2015 (DANE, 2020) and the actual distribution of socioeconomic strata using information from high school graduates in 2014. The black bars plot the mean perceived values of the spring 2014 cohort.[13] In line with the first prediction from the conceptual framework, high-income students barely exposed to SPP classmates have biased perceptions about the SES distribution. In particular, they significantly underestimate the share of low-income individuals—both the official poverty rate and the share of people in strata 1 and 2.

Notably, high-income students have a meaningfully more biased perception of the distribution of socioeconomic strata than of the official poverty rate: they underestimate the latter by 2.51 percentage points and the former by 28.5 percentage points. They also overestimate by 25.5 percentage points the fraction of individuals in their same socioeconomic strata (strata 4, 5, and 6). This severe upward bias in the perceived SES distribution is consistent with endogenous reference groups. High-income students entering college before the SPP policy have very few low-income friends, as Table 1 showed, and socialize mainly with similarly high-income people. Mistakingly, they use their (high-income) peers as a reference when assessing the income distribution and fail to correctly apply Bayes' rule, resulting in a dramatically upwardly biased perception of the SES distribution.

Next, I test whether exposing high-income individuals to low-income peers shifts their reference groups and lessens this bias. Table 2 reports the estimated β coefficient from Specification (1). Each panel uses a different measure of the perceived income distribution as the outcome of interest. For instance, Panel A uses the official poverty incidence as the dependent variable. Column (1) shows that a ten percentage point increase in the share of SPP classmates—which Table 1 showed is roughly the difference in exposure between the spring 2014 and 2015 cohorts—raises the perceived incidence of poverty by 3.09 percentage points (the p-value is 0.007). This coefficient remains significant and relatively stable when progressively adding wave fixed effects in Column (2), major fixed effects in Column (3), baseline controls in Column (4), and cohort fixed effects in Column (5). For instance, Column (4), which leverages the variation in exposure to low-income peers within majors and across cohorts, shows that a ten percentage point increase in exposure to diversity raises the perceived incidence of poverty by 2.59 percentage points (the p-value is 0.008). Column (5), which adds the cohort fixed effects and represents the DD specification, shows that a ten percentage point increase in exposure to diversity raises the perceived incidence of poverty by 2.59 percentage points (the p-value is 0.071), thus entirely eliminating the

[13] Figure A.10 presents kernel distributions of these two measures.

bias plotted in Figure 4.

Panels B and C show that exposure to low-income peers also makes high-income students perceive more low-income people (strata 1 and 2) and fewer high-income people (strata 4, 5, and 6). Column (5) of Panel B suggests that a ten percentage point increase in the share of SPP classmates raises the perceived percentage in strata 1 and 2 by 1.83 percentage points, although the standard errors are large, and I cannot reject the null of no effect. The same column in Panel C shows that a ten percentage point increase in exposure to low-income peers reduces the perceived share in strata 4, 5, and 6 by 2.19 percentage points (the p-value is 0.10). Since high-income students are more biased in their perception of the distribution of socioeconomic strata than in their perception of the poverty rate, the implied bias reduction is smaller: 6.4% for strata 1 and 2 and 8.6% for strata 4–6.

Lastly, Panel D uses an index measure to summarize how exposure to diversity affects high-income students' perception of poverty. This measure is based on the official poverty incidence and the share of the population in strata 1 and 2. Following the procedure described in Anderson (2008), I first standardize using the mean and standard deviation of each outcome of the spring 2014 cohort. Then, I create a weighted average using all of the outcomes in the domain, using the inverse of the covariance matrix of the transformed outcomes in the domain. Column (1) shows that a ten percentage point increase in the share of SPP classmates raises the perceived incidence of poverty by 0.13 standard deviations (the p-value is 0.004). This coefficient is significant and stable when progressively adding wave fixed effects, baseline controls, major fixed effects, and cohort fixed effects. Indeed, Column (5) suggests that a ten percentage point increase in exposure to diversity raises the perceived incidence of poverty by 0.12 standard deviations (the p-value is 0.084). Thus, exposing high-income individuals to low-income peers shifts their reference groups and lessens the upward bias in their perceived income distribution.

7.2 Preferences for Redistribution

Next, I test whether exposure to socioeconomic diversity affected redistributive preferences for high-income students. The survey asked respondents to report, on a scale from 1 (strongly disagree) to 7 (strongly agree), how strongly they agreed or disagreed with the following statements: "*The state should tax the rich*" and "*The state should subsidize the poor*" (Figure B.8). I dichotomize these measures into indicators that take the value one if the person strongly agreed, agreed, or somewhat agreed with each statement and 0 otherwise. On average, 70.8% of students from the spring 2014 cohort support taxing the rich, while 41% support subsidizing the poor.

Table 3 presents the estimated β coefficient from Specification (1) using these two measures of redistributive preferences as the dependent variable in Panels A and B, respectively. Column (1) of Panel A suggests that a ten percentage point increase in the share of SPP classmates boosts support for taxing the rich by 2 percentage points. Columns (2) through (5) show that the inclusion of controls increases the magnitude of the estimated coefficient and improves its precision. For example, Column (5), which reports the DD coefficient, shows that a ten percentage point increase in the share of SPP classmates raises support for taxing the rich by 8 percentage points (the p-value is 0.039). This effect represents an 11.4% increase relative to the baseline mean of 70.4% for the spring 2014 cohort. Similarly, Panel B suggests that a ten percentage point increase in exposure to diversity increases support for subsidizing the poor by 6 percentage points or 14.6%, although the standard errors are large in the DD specification and do not allow rejecting the null of no effect. Lastly, Panel C presents the results when the dependent variable is a summary measure of these two outcomes. Column (5) shows that a ten percentage point increase in the share of SPP classmates raises support for redistribution by 0.15 standard deviations (the p-value is 0.025). The expansion of socioeconomic diversity, therefore, appears to have boosted high-income students' support for progressive redistribution.

Why would exposure to diversity affect high-income people's support for redistribution? Section 2 proposed three channels. First, if exposure to low-income peers made high-income students more aware of their own position in the socioeconomic ladder—and therefore more aware of what they stand to lose from redistribution—the Meltzer and Richard (1981) model of self-interested voters would predict a *fall* in support for progressive redistribution. I do not find evidence supportive of this mechanism. First, Panel A of Table 4 tests whether the treatment affected students' self-reported socioeconomic stratum. Column (5) shows that the DD coefficient is zero and not statistically significant (the p-value is 0.999), suggesting no impact on self-reported SES. I can rule out effects smaller than -0.008 and larger than 0.008. Moreover, Table 3 shows that exposure to low-income classmates *raised* high-income students' support for redistribution.

Second, the policy may have affected people's support for redistribution by changing their perceptions of social mobility. On the one hand, the policy may have made students more optimistic about the prospect of upward social mobility for low-income individuals—especially since the SPP policy dramatically improved postsecondary enrollment for low-income high achievers (Londoño-Vélez et al., 2020)—which would *lower* support for progressive redistribution (Benabou and Ok, 2001; Piketty, 1995). On the other hand, socioeconomic diversity might raise high-income students' perceptions of their own risk of falling in the income distribution, which would *increase* support for redistribution (Ravallion and Lokshin, 2000).

To examine whether the treatment affected people's perceptions of upward or downward social mobility, I asked students: "*Suppose a baby is born in stratum* [*1/2/3/4/5/6*] *in Colombia. Where do you think he or she will end up as an adult?*" (Figure B.9). First, I create an index measure of upward social mobility for low-income individuals: I generate indicators equaling one if an individual born in stratum 1 or 2 ends up in a higher socioeconomic stratum as an adult and use a summary index based on these two variables. Next, I create an index measure of downward social mobility for high-income individuals: I generate indicators equaling one if an individual born in stratum 4, 5, or 6 ends up in a lower socioeconomic stratum as an adult and use a summary index based on these three variables.

Panels B and C of Table 4 report the results of this exercise. Panel B shows that the treatment had a positive but non-significant effect on high-income students' perceived upward mobility for low-income people. The p-value in the DD specification is 0.121 and I can rule out effects smaller than -0.003 and larger than 0.025. Similarly, Panel C shows that the treatment had a positive but non-significant effect on high-income students' perceived downward mobility for high-income people. The p-value in the DD specification is 0.347 and, again, I can rule out effects smaller than -0.008 and larger than 0.023. Therefore, my findings are inconsistent with the hypothesis that changed perceptions of social mobility cause individuals to choose less government intervention in the economy.

Lastly, the policy might have influenced individuals' views on fairness (Alesina and Angeletos, 2005). To test this possibility, I asked respondents how often they believed the economic system provided equal opportunity to overcome poverty on a 7-point scale from "always" to "never" (Figure B.10).[14] Unfortunately, this question was asked only in the first survey wave, which halves the estimation sample. Nevertheless, there is widespread skepticism about fair market outcomes: 46.6% of students from the spring 2014 cohort believe the economic system "never" or "almost never" provides equal opportunity to overcome poverty. (Relatedly, Appendix C shows that there is widespread skepticism about access to quality education being meritocratic.) Panel D of Table 4 presents the results from Specification (1) using this dichotomized measure of fairness concerns. Column (5), which reports the results from the DD specification, suggests that a ten percentage point increase in the share of SPP classmates raises fairness concerns by 12 percentage points (the p-value is 0.012) or 26.6% relative to a mean of 46.6% from the

[14] I focus on this particular outcome in lieu of more commonly used measures, like lack of effort vs. luck determining income (Alesina and Angeletos, 2005), because it is the most interpretable given the policy. For instance, exposure to low-income peers might raise the perception that poverty is due to lack of effort, since hard-working low-income students are now able to attend their selective university thanks to the policy. Or it can make them more likely to report luck as a determining factor, if exposure to SPP recipients make them more aware of their own privilege.

spring 2014 cohort.

In sum, the results from Table 4 support the hypothesis that an increase in exposure to low-income peers caused by a government-sponsored financial aid program makes high-income students more concerned about (the lack of) fairness in market outcomes and, as a result, more supportive of government intervention. Admittedly, however, there may be other reasons why the treatment strengthened people's support for redistribution. For example, it could be that the policy boosts the perception that progressive spending policies, like the SPP financial aid program, are more efficient. If this were the case, then the increased support for government redistribution would reflect a greater perception of efficiency of public spending rather than a concern for fairness. I cannot rule out this possibility because the survey did not ask students how efficient they perceive public spending to be. Notwithstanding this omission, the survey did ask participants about their attitudes toward the SPP program. Appendix C shows that support for this program is widespread: 78% of students from the spring 2014 cohort believe that the state should offer need-based financial aid, and 83% support expanding financial aid. The treatment had no statistically significant impact on these measures.

7.3 Robustness

This section briefly discusses the results from several robustness tests that change the definition of the treatment, address the existence of outlier values in the treatment variable, examine heterogeneity by survey wave, and include additional cohorts in the analysis.

First, the treatment variable in Specification (1) is the share of SPP classmates. However, one could be concerned that some SPP beneficiaries may not be poor, and some older students may be low-income but not benefit from SPP. Therefore, as a robustness test, I substitute the treatment variable with the share of low-income classmates—defined as those from strata 1 and 2. Tables A.2 and A.3 reproduce the results from Tables 2 and 3, respectively, when substituting the share of SPP peers with the percentage of low-income classmates. When comparing students within majors and across cohorts, the coefficient is 0.013 instead of 0.012, and the p-value is 0.021 instead of 0.009. While the DD coefficient slightly falls from 0.012 to 0.011, the standard error increases from 0.007 to 0.008, and I cannot reject the null of no effect on perceived poverty at conventional values. By contrast, the magnitude and the significance are very similar for support for redistribution: the DD coefficient is 0.017 instead of 0.015, and the p-value is 0.042 instead of 0.025.

Second, Figure A.7 showed potential extreme values in the treatment variable. To address the existence of outliers, I reproduce the results for the main index measures when winsorizing the treatment variable at the 1st and 99th percentiles, where the share of SPP classmates is 0% and 25.77%, respectively. Table A.4 shows that the estimated effects are similar when using the winsorized treatment variable. For the index of perceived poverty, the DD coefficient increases from 0.012 to 0.013, and the p-value slightly falls from 0.084 to 0.068. The coefficient is similar using the index measure of support for redistribution, and the p-value slightly increases from 0.025 to 0.035.

Third, the main estimates in Tables 2 and 3 combined both survey waves, collected six and twelve months after the rollout of SPP. However, one may be concerned that exposure to diversity dissipates over time if, for instance, people end up sorting into more homogeneous peer groups. To show how the effects compare after six and twelve months of treatment, I present the results separately by survey wave in Table A.5. The impact on perceived poverty is broadly consistent, although perhaps more precisely estimated for the first survey wave when the treatment gap between younger and older cohorts was the largest. (Indeed, the difference in the treatment between younger and older cohorts shrinks over time because older cohorts become more exposed to SPP classmates.) Nonetheless, the effect on support for redistribution is strong even after twelve months of treatment.

Lastly, the primary analysis focused on the three cohorts surveyed in the first wave: students entering college in spring 2014, fall 2014, and spring 2015. This sample offers the highest internal validity. Students from these cohorts applied to the partner university and selected their major and courses without knowing how SPP would diversify their campus. Moreover, students from these cohorts are the most comparable in age, entering college before and after SPP was implemented. By contrast, the second survey wave expanded the sample to include older and younger cohorts. Table A.6 shows that the main effects tend to hold when adding two older cohorts (spring and fall 2013, for whom less than 2% of classmates are SPP recipients) and one younger cohort (fall 2015, for whom 6.5% of classmates are SPP recipients). Specifically, the DD coefficient on the summary index of perceived poverty is positive but more noisily estimated (the p-value is 0.166). By contrast, the coefficient on the summary index of support for redistribution is quantitatively similar in magnitude and significance (the p-value is 0.049).[15]

[15]Table A.6 excludes the youngest cohorts of spring 2016. First, this cohort knew about SPP and could select their exposure to SPP recipients by choosing different college-major pairs or deferring their admission to a fall semester with fewer low-income students (Figure A.2). Second, the composition of high-income students changed for this cohort because the admission rate dropped after spring 2015 (Figure A.3), raising the measured ability of matriculated students (Figure A.4). Third, the second survey wave was conducted in the first week of instruction in the spring 2016 term, only days after the spring 2016 cohort began college. Therefore, students are not yet "treated" by exposure to low-income peers.

8 Conclusion

This paper investigates whether socioeconomic diversity affects individuals' perception of the income distribution and their preferences for progressive government redistribution. I focus on a context with high inequality and de facto socioeconomic segregation of higher education. In this setting, boosting diversity at an elite university through financial aid greatly impacted students' interactions and social networks. Thanks to the desegregation, high-income students became more likely to interact and socialize with low-income individuals. Moreover, this diversification of social networks reduced high-income students' bias in their perception of the income distribution. It also strengthened their support for progressive redistribution, possibly because it raised their concerns about (the lack of) equal opportunity.

There are two caveats worth discussing. First, exposure to low-income peers can affect people's perception of the income distribution and preferences for redistribution through several mechanisms. For example, the paper discussed that high-income students exposed to more low-income peers might learn that there are more poor people than they initially thought. Alternatively, they might learn that low-income peers are talented and hard-working. Another possibility is that they might realize that low-income peers, if helped, can "make it." While I discuss these mechanisms in the paper, it is difficult to disentangle them.

Second, the policy does not expose high-income students to a completely diverse set of representative low-income individuals in my setting. Instead, they interact with a positively selected sample of low-income individuals with high measured ability, stronger parental education backgrounds, and, arguably, better non-cognitive skills (e.g., grit, motivation, perseverance). These characteristics might induce more sympathy from their high-income peers than if, for instance, they interacted with the average low-income individual of the same age or with a more diverse group of low-income individuals. For example, we may not expect the same results from alternative policies to promote diversity on college campuses, like affirmative action and "top percent" programs, insofar as these policies can entail distinct (usually lower) admissions standards for the targeted group. I leave a study of the effect these other types of interactions might have for future research.

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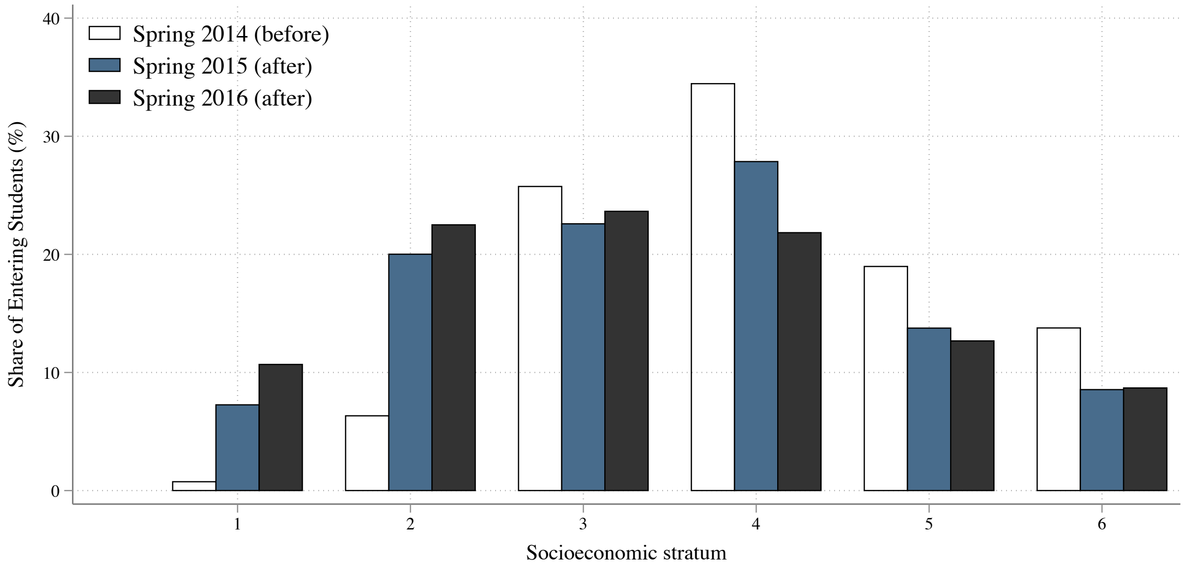
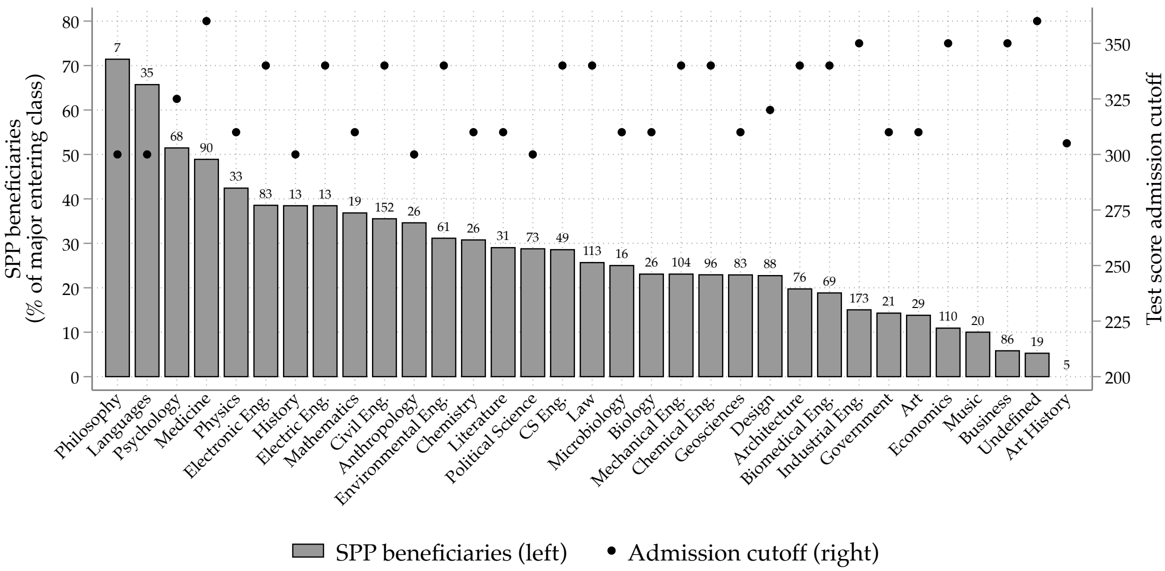
Figure 1: A Dramatic Increase in Socioeconomic Diversity at an Elite University



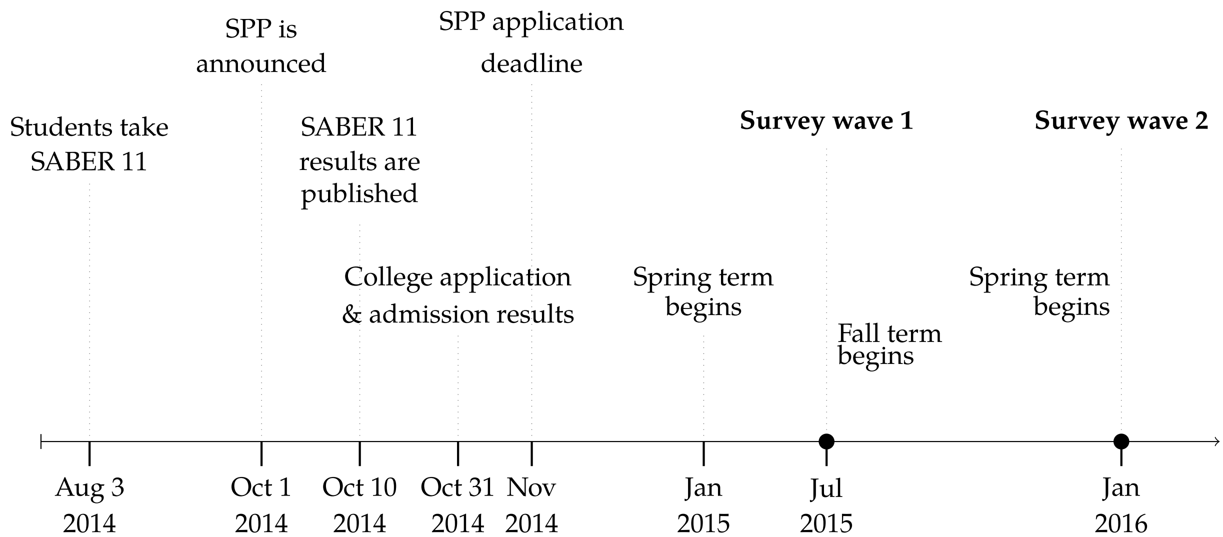
Notes: This figure plots the distribution of entering students at an elite university by their socioeconomic stratum (1 is the poorest, 6 is the wealthiest) in spring 2014 (before SPP financial aid program), spring 2015 (after), and spring 2016 (after). Financial aid dramatically promoted socioeconomic diversity, almost quadrupling the share of low-income students (i.e., strata 1 and 2) from 7.1% in 2014 to 27.3% in 2015 to 33.3% in 2016.
Sources: Author's calculations using college admissions records.

Figure 2: The Share of SPP Beneficiaries Varies across Majors



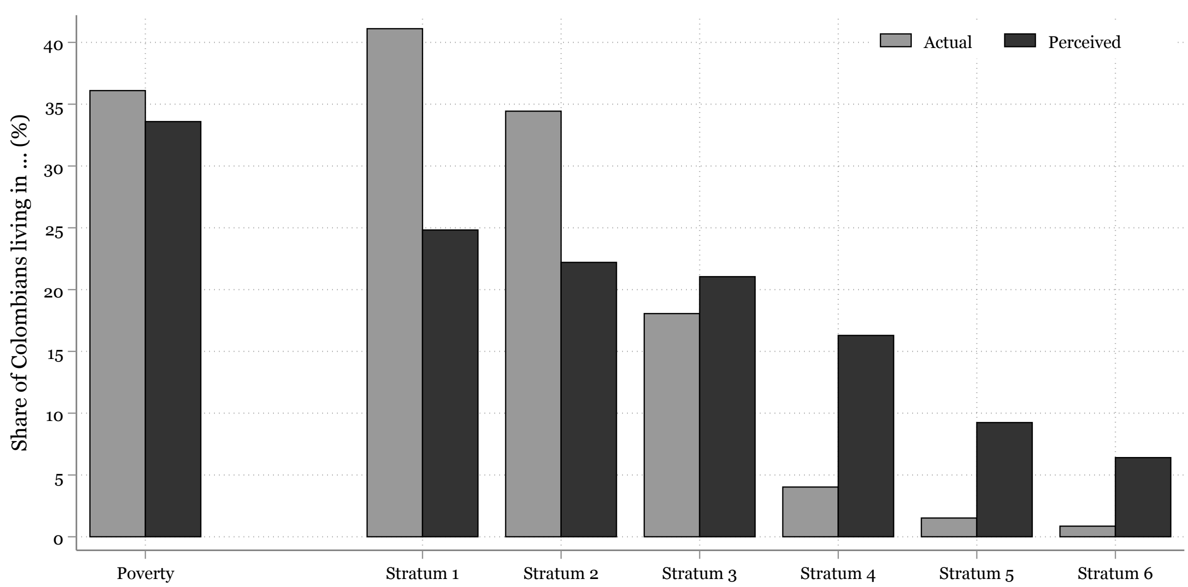
Notes: The left axis in this figure plots the share of entering students in spring 2015 at the partner university who are beneficiaries of the SPP financial aid program by major (in gray bars). This share ranges from 71.4% in Philosophy to 0% in Art History. The numbers above the bars represent the total number of students enrolling for the first time in a given major in spring 2015. Thus, 10.9% of the 110 students entering Economics in spring 2015 were beneficiaries of SPP. The right axis plots the major-specific admission cutoff for spring 2015 (in black round markers). The admission cutoff does not predict the share of SPP recipients in a major ($p = 0.148$).
Sources: Author's calculations using college admissions records.

Figure 3: Timeline of Events



Notes: This figure plots a timeline of events taking place between August 2014 and January 2016 (not drawn to scale). The diversification of elite college campuses took place in mid to late January 2015, with SPP beneficiaries attending private and high-quality universities. I collected the first survey wave six months later. I collected the second survey wave in early February 2016, i.e., after one year of exposure to SPP beneficiaries.

Figure 4: High-Income Individuals Have Biased Perceptions of the Income Distribution



Notes: This figure plots the actual versus perceived distribution of individuals in poverty and in each socioeconomic stratum (1 is the poorest, 6 is the wealthiest). The gray bars report the actual poverty rate (DANE, 2020) and the actual distribution of strata for all SABER 11 test takers (ICFES, 2014). The black bars plot the average response by high-income students (strata 4, 5, and 6) from the spring 2014 cohort.
Sources: Author's calculations using college records and student survey data.

Table 1: Intensity of Interactions with SPP Recipients by Entry Cohort

	Entering Cohort		
	Spring 2014 Cohort	Fall 2014 Cohort	Spring 2015 Cohort
	(1)	(2)	(3)
Share of SPP Classmates (%)	2.712	4.747	13.129
	(3.24)	(3.33)	(5.5)
Reports friends' stratum is 1 or 2	.048	.035	.085
	(.21)	(.18)	(.28)
Perceived Share of SPP Classmates (%)	15.636	17.735	34.526
	(14.98)	(14.05)	(18.61)
No. times worked with SPP recipient	1.229	1.125	3.357
	(2.28)	(2.22)	(3.2)
$\mathbb{1}$(SPP recipient among 5 closest friends)	.022	.059	.243
	(.15)	(.24)	(.43)
$\mathbb{1}$(SPP recipient among 5 study partners)	.033	.052	.275
	(.18)	(.22)	(.45)
N	271	288	342

Note: This table presents means (and standard deviations in parentheses) for high-income students by entering cohort, i.e., the semester in which they first began their studies at the partner university.
Sources: Author's calculations using college records and student survey data.

Table 2: Exposure to Low-Income Peers Reduces Bias in Perceptions of the Income Distribution

	(1)	(2)	(3)	(4)	(5)
Panel A: *Perceived Poverty Incidence*					
Share of SPP classmates (%)	0.309***	0.270**	0.217**	0.259***	0.259*
	(0.111)	(0.116)	(0.103)	(0.095)	(0.142)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	894	894	893	893	893
Panel B: *Perceived Share in Strata 1 and 2*					
Share of SPP classmates (%)	0.161*	0.122	0.157*	0.180**	0.183
	(0.083)	(0.087)	(0.085)	(0.089)	(0.140)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	899	899	898	898	898
Panel C: *Perceived Share in Strata 4, 5, and 6*					
Share of SPP classmates (%)	-0.164**	-0.148*	-0.187***	-0.208***	-0.219*
	(0.080)	(0.082)	(0.069)	(0.074)	(0.132)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	899	899	898	898	898
Panel D: *Summary Index of Perceived Poverty*					
Share of SPP classmates (%)	0.013***	0.011**	0.010**	0.012***	0.012*
	(0.004)	(0.005)	(0.005)	(0.005)	(0.007)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900

Notes: This table presents the β coefficient from Specification (1). The dependent variable is the perceived share of Colombians living in poverty in Panel A, in strata 1 and 2 in Panel B, and in strata 4, 5, and 6 in Panel C. Panel D uses an index measure of perceptions about poverty, i.e., the perceived share of Colombians living in poverty and in strata 1 and 2, following the procedure described in Anderson (2008). The sample includes both survey waves and is composed of high-income students (strata 4, 5, and 6) who first enrolled in the partner university in spring 2014 (before SPP), fall 2014 (before SPP), or spring 2015 (after SPP). Each column represents a separate regression. Controls include age, age squared, sex, SABER 11 test score percentile, socioeconomic stratum, an indicator for having attended high school outside of Bogotá D.C., a measure of risk aversion, and dummies for parental education. Standard errors, in parentheses, are clustered at the major-by-cohort level. $***p < 0.01, **p < 0.05, *p < 0.1$.
Sources: Author's calculations using college records and student survey data.

Table 3: Exposure to Low-Income Peers Boosts Support for Redistribution

	(1)	(2)	(3)	(4)	(5)
Panel A: *State Should Tax the Rich*					
Share of SPP classmates (%)	0.002	0.002	0.003	0.005**	0.008**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900
Panel B: *State Should Subsidize the Poor*					
Share of SPP classmates (%)	0.004	0.004	0.005**	0.006**	0.006
	(0.003)	(0.003)	(0.002)	(0.003)	(0.005)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900
Panel C: *Summary Index of Support for Redistribution*					
Share of SPP classmates (%)	0.006	0.006	0.009**	0.012***	0.015**
	(0.003)	(0.004)	(0.004)	(0.004)	(0.007)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900

Notes: This table presents the β coefficient from Specification (1). The dependent variable is an indicator for supporting the rich and subsidizing the poor in Panels A and B, respectively. Panel C uses an index of support for redistribution based on these two variables, i.e., taxing the rich and subsidizing the poor, following the procedure described in Anderson (2008). The sample includes both survey waves and is composed of high-income students (strata 4, 5, and 6) who first enrolled in the partner university in spring 2014 (before SPP), fall 2014 (before SPP), or spring 2015 (after SPP). Each column represents a separate regression. Controls include age, age squared, sex, SABER 11 test score percentile, socioeconomic stratum, an indicator for having attended high school outside of Bogotá D.C., a measure of risk aversion, and dummies for parental education. Standard errors, in parentheses, are clustered at the major-by-cohort level. $***p < 0.01, **p < 0.05, *p < 0.1$.
Sources: Author's calculations using college admissions records and student survey data.

Table 4: Mechanisms: Perceptions of Own SES, Social Mobility, and Fairness

	(1)	(2)	(3)	(4)	(5)
	Panel A: *Self-reported Socioeconomic Stratum*				
Share of SPP classmates (%)	-0.009	-0.01	-0.008	0.004	0
	(0.008)	(0.008)	(0.006)	(0.003)	(0.004)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900
	Panel B: *Upward Social Mobility for Strata 1 and 2*				
Share of SPP classmates (%)	0.002	0.007*	0.007	0.006	0.011
	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900
	Panel C: *Downward Social Mobility for Strata 4, 5, and 6*				
Share of SPP classmates (%)	0	0	-0.001	-0.002	0.007
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	901	901	900	900	900
	Panel D: *System Does Not Offer Equal Opportunity*				
Share of SPP classmates (%)	0.008***	0.008***	0.008***	0.009***	0.012**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Wave FE		X	X	X	X
Major FE			X	X	X
Controls				X	X
Cohort FE					X
N	454	454	453	453	453

Notes: This table presents the β coefficient from Specification (1). The dependent variable is the self-reported socioeconomic stratum in Panel A. In Panel B (C), it is the index of perception of upward (downward) social mobility among low-income (high-income) people, i.e., that an individual born in strata 1 and 2 (4, 5, and 6) can end up in a higher (lower) stratum as an adult, calculated following the procedure described in Anderson (2008). In Panel D, the dependent variable is an indicator for whether the respondent believes the economic system "never" or "almost never" "*provides equal opportunity to overcome poverty*," which was only asked in the first survey wave. Otherwise, the sample includes both survey waves and is composed of high-income students (strata 4, 5, and 6) who first enrolled in the partner university in spring 2014 (before SPP), fall 2014 (before SPP), or spring 2015 (after SPP). Each column represents a separate regression. Controls include age, age squared, sex, SABER 11 test score percentile, socioeconomic stratum, an indicator for having attended high school outside of Bogotá D.C., a measure of risk aversion, and dummies for parental education. Standard errors, in parentheses, are clustered at the major-by-cohort level. $***p < 0.01, **p < 0.05, *p < 0.1$.
Sources: Author's calculations using college records and student survey data.