College Major Choice, Payoffs, and Gender Gaps*

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Abstract

This paper studies how college major choices shape earnings and fertility outcomes. Using administrative data that link students' preferences, random assignment to majors, and post-college outcomes, we estimate the causal pecuniary and non-pecuniary returns to different fields of study. We document substantial heterogeneity in these returns across majors and show that such variation helps explain gender gaps in labor market outcomes: women place greater weight on balancing career and family in their major choices, and these preference differences account for about 30% of the gender earnings gap among college graduates. Last, we use our causal estimates to evaluate the effects of counterfactual assignment rules that target representation gaps in settings with centralized assignment systems. We find that gender quotas in high-return fields can significantly reduce representation and earnings gaps with minimal impacts on efficiency and aggregate fertility.

Keywords: preferences, returns to majors, gender gaps, centralized assignment
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1 Introduction

A growing body of evidence documents that gender differences in major choice contribute to the gender earnings gap among college graduates (e.g., Sloane et al., 2021; Zafar, 2009), but less is known about the factors that contribute to these differences in choices (Patnaik et al., 2021). A plausible factor affecting educational choices is the balance of career and family, which has played a central role in women's advancements in the labor market over the last century (Goldin, 2021). Although both men and women may consider family-related outcomes when selecting a field of study (Wiswall and Zafar, 2021), causal evidence on (i) how majors affect both earnings and fertility and (ii) how differentially valuing these outcomes translates into aggregate gender gaps remains scarce. Moreover, evidence on the impacts of potential policies proposed to address gender gaps is limited.

In this paper, we examine the causal impact of fields of study on economic payoffs and fertility and investigate how they contribute to gender disparities in the labor market—a task typically hindered by data limitations and methodological challenges. Two key obstacles have slowed progress. First, the lack of comprehensive data makes it difficult to integrate student preferences, graduation outcomes, earnings, and fertility. Second, the frontier empirical methods often used to address self-selection into fields of study (Kirkeboen et al., 2016) are data-intensive, which may limit the scope and precision of gender-specific analyses. To address these challenges, we draw on comprehensive Chilean administrative data that record both students' rank-ordered major preferences and their later life outcomes. We then implement a less data-intensive approach that leverages the applicants' rank-ordered lists to measure preferences for fields of study and to construct selection-corrected estimates of treatment effects (Abdulkadiroglu et al., 2020), an approach that we validate using the experimental variation embedded within centralized admission systems in the assignment of students to majors (Abdulkadiroglu et al., 2017; Abdulkadiroğlu et al., 2022).

We structure our analysis in three steps. First, we estimate gender-specific causal returns to fields of study in terms of economic payoffs and fertility, providing some of the first evidence showing how major choices influence fertility outcomes. Second, we assess the extent to which men and women differentially prioritize earnings and fertility impacts when ranking fields of study, an exercise we interpret as assessing gender differences in the balance of career and family. Finally, we take students' preferences as given and use our estimated payoffs to study the effects of supply-side policies that could address underrepresentation, while accounting for potential equity and efficiency impacts on both earnings and fertility outcomes.

To flexibly estimate causal returns to fields of study, we build on the school choice literature (Abdulkadiroglu et al., 2020), and adapt a model that leverages students' reported preferences to construct control functions that account for non-random selection into fields of study. Importantly, our framework captures average returns separately for men and women and empirically assesses the importance of preference-specific match effects. The model distinguishes between vertical returns, which represent the general benefits of completing a particular field, and horizontal returns, which capture the idiosyncratic benefits that vary according to individual characteristics, preferences, and choices. Making this distinction clarifies the assumptions needed to identify each type of return using observational comparisons and a conditional independence assumption that leverages the suite of information available in settings with centralized admissions. A key feature of our single-offer, centralized admissions setting is that post-offer sorting—where students sort after receiving offers—is significantly costlier and thus less common. This reduces the concerns present in decentralized settings, where extensive post-admission sorting can undermine identification strategies that rely on self-revelation principles (Dale and Krueger, 2002; Hoxby, 2009).

We document substantial heterogeneity across fields in both pecuniary and non-pecuniary treatment effects of college thirteen years after high school graduation. All fields of study, except for Humanities, exhibit positive economic payoffs ranging from 17 to 80 log points, with Medicine, Business, and Engineering having the largest returns. This dispersion in returns mirrors the patterns in Altonji et al. (2012) in that heterogeneity in returns rivals the college premium. However, we find modest gender differences in average economic payoffs within majors, with more pronounced gender differences in terms of fertility effects. In line with existing evidence (e.g., Currie and Moretti, 2003), we find that college graduation reduces the likelihood of having a child and overall parity for men and women in their early thirties.[1] The reduction is smallest among graduates in Teaching and Health, and largest among those in Law and Humanities; a pattern consistent with descriptive evidence from Europe and the United States (e.g., Hoem et al., 2006; Michelmore and Musick, 2014) and with Goldin and Katz (2008) observation that "It is, perhaps, not surprising that women who pursue different career paths and have earned degrees in different fields have different numbers of children. It appears that women in careers with the greatest predictability and the smallest financial penalty for time out have the most children." Indeed, we find suggestive evidence that fertility impacts are driven by how various majors steer individuals into careers that either promote or restrict work-family balance (Goldin, 2021)—a central consideration in the most recent theories of fertility (Doepke et al., 2023; Dahl and Loken, 2024; Olivetti et al., 2024).[2]

[1] Although our study period does not allow us to observe long-run impacts on completed fertility, we document a delay in childbearing—the timing component of the total fertility rate (Bongaarts and Feeney, 1998)—which demographic studies associate with lower completed fertility rates (Roustaei et al., 2019).

[2] We employ data from a Chilean household survey alongside complementary measures of gender norms and child penalties by field of study—estimated in administrative data following Bertrand et al. (2015) and Kleven et al. (2019)—and document positive correlations between fertility returns and factors that facilitate balancing career and family, such as job security, progressive gender norms, and modest

With measures of preference intensity derived from rank-ordered choice data, we further show that match effects in economic payoffs are empirically important, indicating that students tend to sort in ways consistent with Roy (1951)-like selection. As in Kirkeboen et al. (2016), but through a different empirical lens, we find that men and women with stronger preferences for a given field of study tend to experience higher payoffs if they earn a degree in that field. However, gender differences in match effects are small, suggesting that selection on gains plays a limited role in explaining gender earnings gaps. We find no evidence that match effects or selection on gains influence fertility outcomes for either men or women.

To validate our empirical approach, we proceed to benchmark our estimates against an alternative estimation strategy that exploits the randomness in the assignment of students embedded in centralized admission systems (Abdulkadiroğlu et al., 2022; Abdulkadiroglu et al., 2017). The evidence indicates that our model reliably characterizes selection into majors, in that predictions from our preferred model closely mirror the treatment effects obtained using random variation in students' assignments to fields of study. In other words, our approach generates estimates of pecuniary and non-pecuniary payoffs that are *forecast unbiased* (Angrist et al., 2017), while also allowing us to examine gender differences and quantify the importance of match effects.

To understand how gender-specific considerations regarding career and family balance influence educational and labor market trajectories long before workforce entry, we examine whether gender differences manifest in the implicit weights students assign to pecuniary and non-pecuniary payoffs during college application. Our analysis which draws on multivariate relationships between estimated mean utilities and our estimated fertility and earnings treatment effects reveals that women prioritize the balance between pecuniary and non-pecuniary payoffs, whereas men prioritize pecuniary returns. A simple counterfactual exercise in which both genders put the same weight on fertility payoffs suggests that equalizing preferences would reduce the representation gap across fields and, as a consequence, would narrow the gender earnings gap by roughly 30 percent.

Finally, recognizing that shifting students' preferences might require changing long-standing norms (e.g., Alesina et al., 2013; Müller, 2023; Carlana and Corno, 2024), we explore short-term interventions that policymakers can implement to address the representation gap across fields of study, taking preferences as given. Leveraging the centralized assignment structure, we assess how rule modifications can reshape the allocation of high-return majors—particularly Engineering, where underrepresentation is most pronounced—and affect subsequent gender earnings gaps. We consider two policy levers: i) expanding Engineering seats uniformly and ii) reserving a share of these seats for

child penalties. Consistent with the fact that the income-fertility relationship has flattened over time (Doepke et al., 2023), our analysis also reveals no association between wages and fertility returns across fields of study.

women.[3] Our counterfactual results show that broad expansions of Engineering opportunities can improve overall access, but they inadvertently widen the gender earnings gap due to underlying differences in college major demand. By contrast, our estimates suggest that targeted seat reservations for women considerably reduce the representation gap and modestly improve earnings equity, all with minimal costs in terms of aggregate efficiency and fertility, relative to the baseline allocation. The minimal aggregate fertility impacts are consistent with growing evidence that public policies tend to have a modest effect on fertility outcomes (Dahl and Loken, 2024).

Our work contributes to three connected literatures. The first relates to a large body of work focusing on estimating the returns to college and college selectivity (Card, 2001; Dale and Krueger, 2002; Mountjoy and Hickman, 2021; Zimmerman, 2014; Chetty et al., 2023; Mountjoy, 2024), and to a growing body of work that pivots focus to the returns to fields of study (Altonji et al., 2012; Hastings et al., 2013; Kirkeboen et al., 2016; Dahl et al., 2020; Andrews et al., 2024; Lovenheim and Smith, 2023; Kirkebøen et al., 2021). One reason for the more recent interest in returns to fields of study is that dispersion in field returns is as large as the dispersion in returns to college (Altonji et al., 2012), an stylized fact that alludes to the potentially important role of major choice in explaining the gender earnings gap among college graduates (Sloane et al., 2021; Aguirre et al., 2020). By estimating field- and gender-specific pecuniary and non-pecuniary payoffs, our work contributes to the literature in two important ways. First, we provide new evidence on how college and college majors affect fertility—a dimension for which existing evidence is scant.[4] Second, we propose a less data-intensive approach that yields gender-specific treatment effect estimates statistically comparable to those based on local random assignment (Abdulkadiroğlu et al., 2022), while also allowing for the estimation of match effects.

Another body of work in higher education has documented vast gender differences in major choice (Bordon et al., 2020; Neilson et al., 2021; Wiswall and Zafar, 2018; Ahimbisibwe et al., 2024; Wiswall and Zafar, 2021; Zafar, 2009). An important question in this literature is what drives gender differences in preferences (Patnaik et al., 2021). Wiswall and Zafar (2021) finds that college students' expectations about how their education will impact their future career and family significantly influence their choice of major and degree completion, with notable differences observed between genders. Our contribution to this strand of the literature is twofold. First, we document and quantify the role of field-of-study representation in explaining the gender earnings gap. Second, we assess the extent to which educational choices diverge by gender in response to both pecuniary and

[3] The latter affirmative-action-like policy receives broad support in Chile (Bursztyn et al., 2023).

[4] Kirkebøen et al. (2025) provide evidence showing that access to elite college programs causes women (but not men) to delay first births and reduce completed fertility in Norway. Most prior research finds that attending college tends to delay childbearing and reduce fertility among women (see Bharati et al. (2023) for a recent review).

non-pecuniary payoffs across fields; and examine how gender differences in family-related considerations contribute to the observed earnings disparities.

Last, our paper contributes to a growing body of evidence studying the potential effects of affirmative action in systems without centralized assignment systems (Arcidiacono, 2005; Bertrand et al., 2010; Arcidiacono et al., 2016; Bleemer, 2023) and with (Otero et al., 2023; Carlana et al., 2024). Most existing work on affirmative action in higher education focuses on race or socioeconomic status. In contrast, we focus on the potential implications of gender-based affirmative action policies to address representation gaps in high-earning fields of study. Our findings also offer novel insights into the broader literature linking education and delayed child-bearing (Currie and Moretti, 2003; Baudin et al., 2015; Goldin, 2021) indicating that which students receive which seats has minimal impacts on national fertility.

The rest of this paper is organized as follows. Section 2 outlines the institutional setting and describes the data we use; Section 3 presents the conceptual framework; Section 4 documents the causal impacts of major choice on pecuniary and non-pecuniary payoffs and validates these estimates. It also presents our findings on the different weights that male and female students assign to these returns; Section 5 assesses various counterfactual assignment policies and their impact on the gender earnings gap; and last, in Section 6, we conclude.

2 Institutional Background, Data, and Descriptive Statistics

This section presents background information on Chile and its centralized college admission system. We discuss the data generated by the college admission system and its linkages with other administrative records that facilitate our analysis. We also discuss descriptive evidence regarding gender gaps and college major sorting.

College Admissions in Chile: Higher education admissions are based on high-school GPA and students' test scores from nationwide entrance exams. High school graduates can register for the national admission test (Prueba Selección Universitaria, PSU), an SAT-type exam with four sub-tests: Mathematics, Language, and a choice between Science or History. After receiving their scores, students can apply to major-institution combinations (e.g., civil engineering at the University of Chile) by submitting a rank-ordered list of up to ten preferences. Each major-institution (a program) also ranks applicants by setting weights on the subject test scores and high school GPA. Given students' rankings, program-specific weights, and capacities, the system offers each student a seat in at most one program, ensuring that no student-program pair would prefer to be matched over their actual assignment. This is done using an assignment mechanism built on Gale and Shapley (1962)'s student-proposing deferred acceptance algorithm. This process creates an admission cutoff for each program, corresponding to the score of the lowest program-weighted score among admitted students.

Data: Our analysis combines administrative data on students' college applications, graduation, fertility, and labor market outcomes. Data on college applications and admissions come from the agency responsible for administering the national college entrance exam (Departamento de Evaluación, Medición y Registro Educacional, DEMRE). For exam-takers, we observe gender, high school identifier and high school location, GPA, test scores, and rank-ordered preferences for major-institution pairs. To focus on the returns of college graduates, we merge these data with graduation records from the Higher Education Information Service (Servicio de Información de la Educación Superior, SIES).

We classify students into fields of study based on the major from which they graduate. Degree programs are classified primarily based on the OECD Handbook for Internationally Comparative Education Statistics (OECD, 2004). There are eight broad categories: "*Agriculture*", "*Science*", "*Social Sciences, Business and Law*", "*Teaching*", "*Humanities and Arts*", "*Engineering, Manufacturing and Construction*", "*Health and Welfare*", and "*Services*". To ease the exposition, we reclassify "*Social Sciences, Business and Law*" into three separate fields: "*Social Sciences*", "*Business*", and "*Law*". We also separate "*Medicine*" from "*Health and Welfare*" and exclude "*Agriculture*" and "*Services*" as they represent small (<5% of graduates) and non-homogeneous college programs in Chile. We end up with nine fields of study (see Appendix A.1 for details).

Fertility data come from the Civil Registration System (Registro Civil). We observe the date of birth of all the children of every student who applied to college. Labor market data come from the Chilean pension system (Superintendencia de Pensiones, SP), which manages social contributions for all formal public and private employment.[5] We complement the SP data with administrative records from the Unemployment Insurance Administrator Agency (Administradora de Fondos de Cesantía, AFC). These data, limited to formal private sector employment, allow us to replicate our main results among a smaller set of individuals but with a higher wage censoring limit.

For our analysis, we focus on students who applied and were accepted into a college major through the centralized admission system between 2004 and 2007. Focusing on accepted applicants ensures that all students we consider have an offer, which is an

[5] While this dataset is comprehensive and of high quality, it has two limitations. First, it does not include the hours in the workers' contract. However, part-time employment is not that common among college graduate workers in Chile. According to the household survey CASEN 2017, only 8.6% of prime-age college graduates report having worked part-time. Second, wages are censored at the Social Security contribution limit. We address this issue by imputing wages above the limit as in Dustmann et al. (2009). See Appendix A.2 for details.

important detail that is highlighted in the empirical framework. Our chosen period allows us to study labor market and fertility outcomes at least 13 years after college application.[6] For earnings, this provides a sufficiently long window to evaluate divergences in the trajectories of individuals, while for fertility, it allows us to assess delays that can meaningfully translate into fertility reductions (Bongaarts and Feeney, 1998; Roustaei et al., 2019).

Table 1 presents descriptive statistics for individuals in our main analysis sample. The table shows substantial variation in the number of students, their average admission test scores, and proxies for socio-economic status—public secondary school enrollment, public health insurance enrollment, and parental education—across genders and fields of study. Accepted students tend to score above average on the college entrance exam, underscoring the competitive nature of college admissions in Chile. Medicine and Law programs are the most competitive, although important elite programs are contained in Business, Medicine, Engineering, and Law (Zimmerman, 2014). As for gender differences in admissions, engineering has the highest number of male graduates, whereas Teaching has the highest number of female graduates. Medicine is the most selective field, while Teaching is the least selective. Compared to Medicine graduates, Teaching graduates come from a more disadvantaged background (e.g., less than 20% of their fathers have a college degree versus 50% of the fathers of graduates from Medicine). Overall, our primary sample is positively selected on incoming achievement due to the competitive nature of the centralized assignment system, but there is still substantial earnings inequality among graduates in the system.

2.1 Gender Earning Gaps in Chile and the Role of Major Choice:

The gender wage gap among college graduates is sizable in Chile, and college major choice seems to play a prominent role. Panels (a) and (b) of Figure 1 show the graduation and log wages of the graduated individuals in our main estimation sample by gender and field of study. A salient fact from this figure is the "STEM gap" as men graduate disproportionately more from Engineering and Science. In contrast, females graduate disproportionately more from Health and Teaching, lower-paying fields compared to Engineering and Science.

To quantify the relative importance of differences in representation, we decompose the gender wage gap among college graduates into two components: differences in representation across majors and differences in returns within majors. For this purpose, let μ_j^g stand for the average earnings of individuals of gender $g \in \{F, M\}$ who graduate from major j, and let s_j^g stand for the share of individuals who graduate from field j among

[6] Access to more recent data would allow us to extend the analysis to at least 17 years after college application. While our current access is limited, our data agreement permits extending the analysis in the event of a revision.

those of gender g. Using this notation, we can write the average gender earnings gap among college graduates as:

$$\mathbb{E}[\text{Earnings} \mid \text{Female}] - \mathbb{E}[\text{Earnings} \mid \text{Male}] = \sum_j s_j^F \mu_j^F - \sum_j s_j^M \mu_j^M .$$

This expression can be decomposed as:

$$\sum_j s_j^F \mu_j^F - \sum_j s_j^M \mu_j^M = \underbrace{\sum_j (s_j^F - s_j^M)\mu_j^F}_{\Delta\text{Representation}} \quad + \quad \underbrace{\sum_j s_j^M (\mu_j^F - \mu_j^M)}_{\Delta\text{Returns}} . \tag{1}$$

The first term of Equation (1) captures differences in sorting patterns holding payoffs constant; thus, it represents the share of the gap explained by the fact that men and women graduate from different fields of study. The second term holds the sorting patterns constant to capture within-major payoff differences, which may be driven by labor market factors differentially affecting women, such as discrimination or child penalties. Figure 2 presents the results from this exercise that decomposes the gender wage gap among college graduates—which corresponds to 19.2% ($4,758 USD per year in favor of men)—into these two components. We see that Engineering and Science are the main contributors to men's higher wages, and this is primarily due to the low representation of females in these fields.

We hypothesize that gender differences in how men and women prioritize the impact of majors on pecuniary and non-pecuniary payoffs (e.g., the balance of career and family) are one potential culprit governing the gender wage gap. To descriptively explore the family dimension of majors, Panel (c) of Figure 1 shows fertility across fields of study, measured as the likelihood of having children 13 years after college application. The fields of study with the highest fertility are Teaching and Health, and those with the lowest are Humanities, Medicine, and Law. These overall low fertility rates across fields are consistent with Chile's national context, where the 2022 fertility rate was approximately 1.5 children per woman of reproductive age, among the lowest in Latin America and globally (World-Bank, 2025).[7]

3 Conceptual Framework

To understand how college major choice contributes to the gender gap among college graduates, we begin by outlining a framework for estimating the causal effects of college majors on both earnings and fertility. Given our focus on major choice and the potential

[7] Drawing on data from the World Bank World Development Indicators database, Appendix Figure A.4 shows that Chile's recent fertility trends align with patterns observed in most high-income economies (Doepke et al., 2023).

impact of reallocative policies, we adopt a framework that accounts for both vertical and horizontal dimensions of major effects. We begin with a largely non-parametric discussion of major effects, following the approach of Mountjoy and Hickman (2021), which transparently analyzes how student sorting affects observational comparisons and delineates how different sorting margins interact with centralized admission systems. We connect this discussion to an empirical approach, which draws insights from settings with rank-ordered choice data (Abdulkadiroglu et al., 2020), and discuss its resemblance to the fallback conditioning discussed in Kirkeboen et al. (2016).

3.1 Environment and Assumptions

We index the population of accepted college applicants by $i \in \mathcal{I}$ and majors by $j \in \mathcal{J}$. The potential outcome of student i completing major j is:

$$Y_{ij} = Y_{i0} + \mu_{ij} \tag{2}$$

where Y_{i0} is their potential outcome if they do not graduate from college, or a summary measure of their ability, and μ_{ij} is the idiosyncratic return of major j. For expositional purposes, we denote ability as $a_i = Y_{i0}$.

In a setting with centralized matches, applicants submit a rank-ordered list of majors, $R_i \in \mathcal{R}$, and report an entrance exam score, $S_i \in \mathcal{S}$. A student type is defined as $\theta_i = (R_i, S_i) \in \Theta$. A centralized system allocates students to majors, producing an offer $Z_i \in \mathcal{J}$. Offers are assumed to be unrelated to potential outcomes conditional on type: $\{Y_{ij}\}_{j \in \mathcal{J}} \perp\!\!\!\perp Z_i \mid \theta$.[8]

After receiving an offer, students decide whether to comply or not. Denote $F_i \in \mathcal{J}$ as student i's completed field or major, which depends on whether or not a student complies with their assignment (offer) Z_i. Let D_i correspond to an indicator for compliance with the assignment. We can then write:

$$F_i = D_i Z_i + (1 - D_i) F_i',$$

where F_i' is a major student i completes in case they do not comply with their offer.

Previous work argues that information in college application and admissions portfolios reveals information about student ability (Dale and Krueger, 2002). In settings with centralized assignments, where access to majors is strongly governed by entrance exam scores, the intuition behind the self-revelation principle also applies. We assume that the composition of the rank-ordered list, in addition to entrance exam scores, adequately summarizes the student's ability.

[8] Abdulkadiroglu et al. (2017) demonstrate that centralized assignment systems produce stratified experiments when we further consider priorities and capacity constraints.

Assumption 1. *Student types proxy for student ability,*

$$a_i = \sum_{\theta \in \Theta} \phi_\theta \mathbb{1}[\theta_i = \theta] + u_i,$$

with $E[u_i|F_i, \theta_i] = E[u_i|\theta_i]$.

Assumption 1 states that any residual variation in ability, not captured by student type θ, is not systematically related to their choice of major.

In decentralized settings, students can reveal their types, but post-offer sorting looms large. Post-offer sorting opens up the scope for additional sources of bias that plague the identification of treatment effects (Hoxby, 2009). In a setting with centralized assignments and single offers, however, this kind of sorting is less of a concern, as a multitude of lotteries govern final assignment to majors. We summarize this in the following assumption.

Assumption 2. *Compliance,* D_i *is as good as random, conditional on type* θ*:*

$$\mu_{ij} \perp\!\!\!\perp D_i \mid \theta \quad \textit{and} \quad u_i \perp\!\!\!\perp D_i \mid \theta.$$

Assumption 2 states that, conditional on type, any non-compliance is unrelated to major returns and student ability. This bears similarity to risk-controlled value-added model assumptions that also assume post-offer sorting is unrelated to potential outcomes in settings with centralized assignment (Angrist et al., 2024).

3.2 Sources of Treatment Effect Heterogeneity

With access to rank-ordered choice data and entrance exam scores, we can now assess how Assumptions 1 and 2 affect type-specific observational comparisons. With observed outcomes Y_i, and fixing $\theta_i = \theta$, we can compare the mean outcomes of major j graduates to those of non-graduates ($j = 0$):

$$E[Y_i|F_i = j, \theta] - E[Y_i|F_i = 0, \theta] = \underbrace{E[\mu_{ij}|F_i = j, \theta]}_{ATT_j(\theta)} + \underbrace{E[a_i|F_i = j, \theta] - E[a_i|F_i = 0, \theta]}_{\text{Selection bias} = 0, \text{ by Assumption 1}}. \quad (3)$$

Equation (3) shows that observational comparisons within types θ recover θ-specific average treatment effects on the treated, as Assumption 1 takes care of selection bias. While empirically interesting and important to some extent, this quantity masks different sources of treatment effect heterogeneity that may be important to policymakers. To see this concretely, we can write the $ATT_j(\theta)$ as:

$$ATT_j(\theta) = \underbrace{E[\mu_{ij} \mid \theta\,]}_{ATE_j(\theta)} + \underbrace{(E[\mu_{ij} \mid F_i = j, \theta] - E[\mu_{ij} \mid \theta])}_{\text{Post Offer Sorting} = 0, \text{ by Assumption 2}}.$$

The first component measures the average treatment effect for all students of type θ. The second component isolates any additional impact arising from selection after offers are made: it captures the extent to which the treatment effect for type θ students who accept an offer differs from the average effect for type θ students. If those who comply systematically differ in their gains, the post-offer term will be nonzero. Institutional features of centralized assignment—formalized in Assumption 2— largely prevent this form of sorting on gains.[9]

Assumption 1 and Assumption 2 address selection bias and post-offer sorting effects that raised concern in prior work. Our empirical focus is now on distinguishing between the vertical and horizontal components of payoffs. We can write the type θ-specific conditional average treatment effect as:

$$ATE_j(\theta) = \underbrace{E[\mu_{ij} \mid \mathcal{I}]}_{ATE_j(\mathcal{I})} + \underbrace{(E[\mu_{ij} \mid \theta] - E[\mu_{ij} \mid \mathcal{I}])}_{M_{ij}}, \tag{4}$$

where $ATE_j(\mathcal{I})$ represents the payoff to the typical student in the pool of accepted applicants—the vertical component—and M_{ij} captures the horizontal component, i.e., if θ types who actually enroll experience systematically different gains than the typical admitted student, then M_{ij} will be nonzero. This sorting on gains is intimately linked to the formation of the rank-ordered list, an element of θ, which is itself governed by inherent preferences. In this respect, the type of sorting considered by our approach is analogous to that considered by Kirkeboen et al. (2016), where preference heterogeneity is associated with potential outcomes.

3.3 Empirical Approach

We now specify a choice model that helps translate complex student types, $\theta = (R_i, S_i)$, into a more manageable, lower-dimensional characterization of ability and preferences. Through a combination of distributional assumptions on unobserved preference heterogeneity in the choice model and by embedding elements of preferences into potential outcomes, this approach also enables us to recover both the vertical component, $ATE_j(\mathcal{I})$, and the horizontal term, M_{ij}. We begin by detailing the structure of the choice model.

[9]This type of sorting has raised concerns in studies estimating the returns to college selectivity (Dale and Krueger, 2002). As Hoxby (2009) points out, "the [Dale and Krueger] strategy generates estimates that rely entirely on the small share of students who make what is a very odd choice. These are students who know that they could choose a much more-selective college and who have already expressed interest in a much more-selective college (they applied), yet, they choose differently than 9 out of 10 students. Almost certainly, these odd students are characterized by omitted variables that affect both their college decision and their later life outcomes." That observation underscores the empirical relevance of this kind of sorting in decentralized markets. In settings with centralized assignment, however, this kind of concern is much attenuated.

Major Choice: We model demand for majors, abstracting away from demand for particular programs or institutions relative to a left-out field of study.[10] For student i, the indirect utility from choosing major j is specified as:

$$u_{ij} = \delta_{c(X_i)j} - \kappa_{c(X_i)} d_{ij} + \eta_{ij}, \tag{5}$$

where $\delta_{c(X_i)j}$ is the mean utility for applicants in a cell $c(X_i)$ defined by their observed covariates (region, school type, PSU score group range, and gender). A given d_{ij} in the vector of distances $d_i = (d_{i1}, \cdots, d_{iJ})$ measures the distance between the student's high school and the nearest program corresponding to field j, and η_{ij} is an unobserved preference shock.

Equation (5) imposes several assumptions that enable us to summarize demand in a parsimonious manner. First, we assume that students form rational expectations about mean potential outcomes in each major. Second, we parameterize preference heterogeneity as covariate-specific and aggregate it into distinct cells $c(X_i)$. Third, and related to financial aid and net price, students in Chile face different financial aid schedules governed by rigid discontinuities in the PSU distribution. We account for this in our choice of cell strata, which include different non-overlapping PSU score groups. This allows us to assume that students' net price is constant within a cell. These three assumptions allow us to summarize demand with cell-specific mean utilities and a distance shifter. Last, we assume that η_{ij} has an Extreme Value Type 1 distribution conditional on $\delta_{c(X_i)j}$ and d_{ij}.

Identification of Payoffs: Our empirical approach links the choice model with potential outcomes via a few parametric restrictions. These restrictions are summarized in the following Assumption.

Assumption 3. *Let η_{ij} correspond to the unobserved preference heterogeneity governing choices in Equation (5). We assume student ability is summarized by:*

$$a_i = \gamma'(X_i, d_i) + \textstyle\sum_l \psi_l(\eta_{ir} - \bar{\eta}) + u_i$$

and treatment effects are parameterized as:

$$\mu_{ij} = \alpha_j \ + \ \beta_j G_i \ + \ \psi_j^*(\eta_{ij} - \bar{\eta}).$$

[10] Collapsing demand estimation is common in the literature. Abdulkadiroglu et al. (2020) estimate demand for schools as opposed to programs, similar to us estimating demand for majors instead of programs. Laverde (2022) uses similar aggregations in the context of pre-K in Boston.

These parametric assumptions imply the following J restrictions:

$$E[Y_{ij}|X_i, G_i, d_i, F_i = j] = \alpha_j + \beta_j G_i + \gamma'(X_i, d_i) + \sum\nolimits_l \psi_l(\eta_{ir} - \bar{\eta}) + \psi_j^*(\eta_{ij} - \bar{\eta}), \quad j = 1, \cdots, J. \tag{6}$$

Equation (6) in Assumption 3 adds three interpretable layers of structure to the potential-outcome model. First, student ability is fully captured by observed characteristics X_i (which include entrance-exam scores) together with the vectors summarizing preference heterogeneity and distance. Two students who appear identical on paper may therefore experience different outcomes if their underlying preferences differ. The coefficient ψ_l translates this selection on levels: a positive ψ_l means applicants who especially favor field l tend to perform better everywhere, no matter which major they finish. Second, the vertical dimension of major quality is allowed to vary by gender through the parameters α_j and β_j. Third, the horizontal dimension of major quality captures the preference-specific match quality and is linear in the unobserved preference heterogeneity. The ψ_j^* captures the match effects, commonly referred to as *selection on gains* in these types of models (Abdulkadiroglu et al., 2020; Einav et al., 2022; Otero et al., 2023; Bruhn et al., 2023). In the case that $\psi_j^* > 0$, students with a higher preference for major j and who complete major j experience an additional earnings gain, evidence of positive Roy (1951)-like selection.

The final step is to arrive at an empirical specification for the observed outcomes. In the school choice setting, Abdulkadiroglu et al. (2020) demonstrate that:

$$\begin{aligned} E[Y_i|R_i, X_i, G_i, d_i, F_i = j] = \alpha_j + \beta_j G_i + \gamma'(X_i, d_i) + \sum_l \psi_l \lambda_l(\theta(R_i, X_i), G_i, d_i) \\ + \psi_j^* \lambda_j(\theta(R_i, X_i), G_i, d_i) \end{aligned} \tag{7}$$

where $\lambda_j(\theta(R_i, X_i), G_i, d_i)$ are control functions derived from rank-ordered choice data contained in R_i and an extreme value type 1 (EVT1) assumption on the unobserved preference heterogeneity. In practice, the embedded random assignment of students to majors in the centralized admissions process induces variation in completed majors conditional on their preferences and observables.

Equation (7) highlights how Assumption 3 connects to Assumption 1 and Assumption 2, allowing us to summarize the conditional average treatment effect of a given major j as:

$$\tau_j(\theta(R_i, X_i), G_i, d_i) = \underbrace{\alpha_j + \beta_j G_i}_{ATE(G_i)} + \underbrace{\psi_j^* \lambda_j(\theta(R_i, X_i), G_i, d_i)}_{M_{ij}(G_i)} \tag{8}$$

Equation (8) illustrates how treatment effects vary with respect to applicant type—via the composition of their rank-ordered list, R_i, the characteristics embedded in X_i (including test scores), gender, G_i, and distance, d_i. This approach bears similarity to fallback conditioning as treatment effects vary according to the composition of a student's rank-ordered list, R_i. For example, the return to a given field includes a match effect governed by the submitted rank-ordered list, including most-preferred field of study and any fall-back options the student ranks. This means that treatment effects for major j differ for individuals with different fallbacks, and even among individuals with similar fallbacks, additional differences in the composition of the rank-ordered list produce additional differences in treatment effects. Therefore, our assumptions governing selection into majors play a dual role. They allow us to estimate causal effects by accounting for preference heterogeneity flexibly while simultaneously allowing us to overcome power limitations from other data-demanding approaches that emphasize the importance of fallback conditioning (Kirkeboen et al., 2016; Dahl et al., 2020). Accounting for this heterogeneity in returns is crucial for any reallocative policy.

Estimating the Choice Model: We leverage rich information contained in applicants' rank-ordered lists to estimate the parameters of the choice model that then allow us to construct the control function $\lambda_j(\theta(R_i, X_i), G_i, d_i)$. We rely on the fact that truthful reporting is a weakly dominant strategy in the Chilean centralized assignment system—which uses a variant of a student-proposing deferred acceptance algorithm (Gale and Shapley, 1962). A student reveals field j as their most preferred if they list it at the top of their list, the next-stated field is their second preferred, and so on. Preferences are summarized by a vector $R_i = (R_{1i}, R_{2i}, \cdots, R_{j(i)i})$ where R_{1i} is the top-listed field and the length of the list, $j(i)$, is allowed to vary across students.[11] Thus, under truthful reporting, students' top-ranked field satisfies:

$$R_{1i} = \arg\max_{s \in \mathcal{J}} U_{is}$$

while the remaining options k satisfy:

$$R_{ki} = \arg\max\nolimits_{s \in \mathcal{J} \setminus (R_{1i}, \cdots, R_{k-1i})} U_{is}, \quad k > 1.$$

The EVT1 assumption allows us to express the likelihood of observing the major-specific rank-ordered list as follows:

$$\mathcal{L}(R_i | X_i) = \prod_{k=1}^{j(i)} \frac{\exp(\delta_{c(X_i)k} + \kappa_{c(X_i)} d_{ik})}{\sum_{r \in R_i \setminus R_{1i}, \cdots, R_{k-1}} \exp(\delta_{c(X_i)r} + \kappa_{c(X_i)} d_{ir})}.$$

We estimate preference models separately for the 36 covariate cells defined above (i.e., 2 school types, 3 macro-regions, 3 financial aid-relevant PSU ranges, and 2 genders) via maximum likelihood to obtain a list of field-specific mean utilities.

[11]For example, some students rank a single field across their list, while others rank multiple fields.

One potential concern with our estimation approach is potential deviations away from truthful reporting. As Fack et al. (2019) points out, some students may skip the impossible, or constrained list lengths may introduce other empirical challenges (Haeringer and Klijn, 2009). Estimating preferences under an *ex-post stability* assumption is an increasingly popular approach that circumvents these issues. We choose not to use this approach in our main analysis because it requires implicit extrapolations that impose strong restrictions on preference heterogeneity across the entrance exam score distribution. Instead, we adopt a strategy that uses all of the information contained in each individual's rank-ordered list and validate it by comparing our estimates to those obtained from an alternative approach that leverages local random assignment to majors (Angrist et al., 2017).

4 Payoffs to Fields of Study, Preferences, and Gender Gaps

This section estimates pecuniary and non-pecuniary impacts of various fields of study, providing gender-specific estimates of mean payoffs and match effects. To validate our empirical strategy, we conduct robustness checks using a bias test in the spirit of Deming (2014) and Angrist et al. (2017). Then, we investigate whether men and women assign different weights to pecuniary versus non-pecuniary outcomes and analyze the extent to which these discrepancies account for existing gender gaps.

4.1 Gender Differences in Payoffs and Match Effects

Using the sample of students admitted through the centralized admission system, we estimate gender-specific pecuniary and non-pecuniary average payoffs and match effects that characterize the horizontal dimension. For a given outcome, we estimate the empirical analog of Equation (7) that incorporates the estimated control functions $\hat{\lambda}_{ij}$:

$$\begin{aligned} Y_i = & \sum_j \alpha_j F_{ij} + \sum_j \alpha_j^G F_{ij} G_i + \sum_j \psi_j \hat{\lambda}_{ij} + \sum_j \psi_j^G \hat{\lambda}_{ij} G_i \\ & + \sum_j \psi_j^* \hat{\lambda}_{ij} F_{ij} + \sum_j \psi_j^{*G} \hat{\lambda}_{ij} F_{ij} G_i + \beta \mathbf{X}_i + \mu_a + \phi_{c(X_i)} + \varepsilon_i, \end{aligned} \tag{9}$$

where the outcomes of interest Y_i are log earnings and a binary indicator for having at least one child thirteen years post-college application. The variable $F_{ij} \equiv \mathbb{1}(F_i = j)$ is an indicator that equals 1 if individual i completed field j, and 0 otherwise; and G_i is another indicator variable that equals 1 if an individual i is female. $\hat{\lambda}_{ij}$ is the control function proxying for individual's i preferences for field of study j and $\mathbf{X}_i$ are quadratic polynomials of the college admission scores in mathematics and language and a distance vector. We include cohort (i.e., year of application) μ_a and cell $\phi_{c(X_i)}$ fixed effects, where a cell is defined by 2 school types (public and private), 3 macro-regions (north, center, and south), 3 financial aid-relevant PSU ranges (<450, $\in$ (450,550), >550), and 2 genders. To ease interpretation, we standardize $\hat{\lambda}_{ij}$ within each cell. We stack across both outcomes—earnings and fertility—and estimate the model jointly.

For inference, we use a parametric bootstrapping procedure to account for estimation errors in the estimated control functions $\hat{\lambda}_{ij}$. Concretely, for each cell c, we have an estimate of the asymptotic distribution of $(\delta_{cj},\ \kappa_c)$, which we can draw from in each bootstrap iteration b to construct different control functions. Thus, we estimate model (9) using ordinary least squares and the bootstrapped control function $\hat{\lambda}^b_{ik}$ one hundred times. For each of the model's parameters, we report the mean estimate across bootstrap iterations and the mean of the standard errors.

Earnings impacts: Table 2 reports how graduation from different majors affects log earnings among accepted college applicants, i.e., relative to not graduating. In Chile, completing a college degree appears highly beneficial: on average, the return is 39.5 log points for women and 40.5 for men, both relative to the gender-specific average earnings among non-graduates. When the share of graduates from each major is considered, these weighted averages are 43 log points for women and 51 for men, underscoring the role of major choice in overall gender gaps. Payoffs vary substantially across majors. Except for Humanities, all fields of study generate positive payoffs ranging from 17 to 80 log points. Medicine, Business, and Engineering stand out with the highest returns—between 55 and 80 log points. The noise-adjusted standard deviation in mean returns for women is 22 log points and 27 for men.[12] This dispersion in economic payoffs mirrors similar patterns reported by Altonji et al. (2012) in that the heterogeneity in returns rivals the college premium.

Appendix B presents additional results on economic payoffs. In Appendix Table B.1, we report fallback-specific returns, leveraging the model's structure to estimate returns to each field conditional on students' next-preferred alternative. As in Kirkeboen et al. (2016), we find substantial heterogeneity in these fallback-specific returns. In Appendix Table B.2, we report the results on total earnings (instead of log earnings) to account for the extensive margin; the results are qualitatively similar. Moreover, to assess the potential impact of top-coding on our results, Appendix Table B.3 reports estimates based on the unemployment insurance records (AFC), which are limited to private sector earnings. The results also remain qualitatively similar.[13]

[12]We calculate the noise-adjusted variance using the formula: $\frac{1}{J}\sum_{j=1}^{J}\left(\left(\hat{\alpha}_j-\mu_\alpha\right)^2-\left(SE\left(\hat{\alpha}_j\right)\right)^2\right)$, where μ_α is the overall mean across field returns $\hat{\alpha}_j$ and $SE\left(\hat{\alpha}_j\right)$ is the estimation error.

[13]One exception is the field of Medicine, for which the estimated payoff becomes smaller. One explanation for this divergence lies in the substantial role of the public sector as an employer for doctors.

Table 2 also reveals strong evidence of selection on gains in our setting. Students with larger estimated preferences for a given major—as captured by control functions summarizing their preferences—experience larger payoffs from that major, compared to others with lower estimated preference intensity. For instance, a one standard deviation increase in preference intensity for Law is associated with a 6 percent increase in earnings for females and an 8 percent increase in earnings for males. Similar to previous evidence presented in Kirkeboen et al. (2016), Figure 3 plots the distribution of the realized match effects ($\hat{\psi}_j \times \hat{\lambda}_{ij}$) for male and female students. Consistent with our match-payoff estimates, the figure shows meaningful selection on gains across fields of study: the composite term $\hat{\psi}_j \times \hat{\lambda}_{ij}$ has a mean (standard deviation) of 0.06 (0.08) for men and 0.04 (0.06) for women.

Finally, when we compare gender differences in both monetary payoffs and match effects, the gaps are modest. Men exhibit statistically significantly higher returns in Science and Engineering. Conversely, estimated returns are slightly higher for women in Social Science, Teaching, Humanities, Health, and Business, although only the difference in Teaching is statistically significant. While gender differences in average returns capture one dimension of economic payoffs across fields, match effects can either attenuate or amplify these gaps. In practice, however, we observe negligible gender differences in selection on gains, leaving within-major wage disparities by gender modest overall.

Fertility impacts: Table 3 reports how college completion and fields of study affect the likelihood of having at least one child in the early thirties. In line with previous evidence (e.g., Currie and Moretti, 2003), we find that college delays fertility for both men and women. For women, completing the typical major reduces fertility in their early thirties by an average of 6.6 percentage points, whereas for men, the reduction is 13 percentage points.[14] Appendix Table B.4 reports analogous results for parity, measuring fertility by the number of children. The dispersion in fertility impacts across fields of study is notable. For women, the noise-adjusted standard deviation of the fertility impact is 0.05, suggesting that the variation across majors is of similar size than the average impact. For men, the dispersion is smaller, with a noise-adjusted standard deviation of 0.03, yet it still reflects meaningful variability across different fields of study. We do not find compelling evidence that selection on gains or match effects meaningfully influences fertility outcomes. In other words, differences in preference intensity for various fields do not systematically explain the observed fertility treatment effects across majors.

Although our evidence indicates that one's chosen field of study significantly affects fertility, the underlying causal pathways warrant further elaboration. To fix ideas, con-

Since many doctors practice across both public and private institutions, estimates derived solely from AFC data may only capture a partial representation of their total annual earnings.

[14]This more pronounced decline among men is consistent with evidence that men tend to have children later in life (Wang et al., 2023).

sider a hypothetical scenario involving two individuals who share identical preferences and are observationally equivalent across all relevant dimensions. Due to stochastic variation in the centralized assignment process, these individuals enroll in distinct fields of study. One pursues a program associated with professional roles offering enhanced job security and diminished penalties for career interruptions (e.g., Teaching). In contrast, the other enters a major characterized by reduced job security and a more unpredictable career advancement trajectory (e.g., Law). Importantly, even if their overall economic returns are comparable—as Table 2 shows is the case for Teaching and Law—the individual in the more stable work environment may find it easier to reconcile career and family, leading to an earlier transition to parenthood and potentially a larger family size. In contrast, individuals facing greater uncertainty and higher penalties for work interruptions may encounter more delays in childbearing.

As Goldin and Katz (2011) notes, the cost of offering family-friendly amenities can differ significantly across industries and occupations, and fields of study not only provide different skill sets; they also steer graduates toward jobs that may vary systematically in their degree of child-friendliness (Adda et al., 2017). Indeed, a growing body of research shows that women place relatively high value on non-wage job attributes (Mas and Pallais, 2017; Corradini et al., 2023; Morchio and Moser, 2024) and that reconciling career and family is central to fertility decisions (Goldin, 2021; Dahl and Loken, 2024; Olivetti et al., 2024). This issue is particularly salient in Chile, where "work/family balance" is cited as the top challenge confronting employed women (Gallup, 2017).

To empirically examine whether fertility payoffs are associated with the extent to which different fields of study facilitate balancing career and family, we use data from Chile's largest household survey. Specifically, we link our estimated heterogeneous fertility impacts to a range of job attributes encountered by graduates from different majors. Using the administrative data, we also construct proxies for gender norms and child penalties by field of study. Building on Bertrand et al. (2015), we measure major-specific gender norms using the earnings distributions of couples of parents to capture the breadwinner norm that "a man should earn more than his wife." Likewise, following Kleven et al. (2019), we estimate child penalties by field of study, focusing on individuals who had their first child after completing their degrees. See Appendix C for details on the construction of these measures.

Appendix Figure B.1 presents the point estimates and confidence intervals obtained from separate regressions of fertility payoffs on jobs and field-specific attributes. We find no significant association between the average wages of graduates in each field of study and our estimated fertility payoffs. However, we observe a positive correlation between fertility impacts and factors that facilitate balancing career and family. Majors with greater job security, proxied by the percentage of graduates who hold a job in the public sector, are associated with higher fertility. Fertility payoffs are also positively correlated with the progressiveness of gender norms in each field of study—measured by the size of the cliffs at the point where the mother starts to earn more than the father.[15] The percentage of graduates who are married or cohabitating, a key feature of the "child production function" (Albanesi et al., 2023), is also correlated with fertility. Conversely, fertility impacts are more negative in fields where unemployment is high and where the career costs of children are higher, as proxied by our estimated child penalties. A regression of fertility payoffs on a career-and-family compatibility index (calculated as the average of the standardized variables mentioned above, adjusted such that higher values indicate greater compatibility) yields a coefficient of 0.080 with a standard error of 0.019. Including logged wages as a control variable in this regression—in the spirit of Oreopoulos and Salvanes (2011)—leaves the coefficient unchanged and only slightly increases the standard error to 0.020. This result aligns with the fact that the income-fertility relationship has flattened over time and with modern fertility models that shift the focus from opportunity costs to the influence that policies, social norms, and workplace flexibility have in shaping fertility decisions (Doepke et al., 2023; Dahl and Loken, 2024).

Our results are also in line with Goldin and Katz (2008) observation that "It is, perhaps, not surprising that women who pursue different career paths and have earned degrees in different fields have different numbers of children. It appears that women in careers with the greatest predictability and the smallest financial penalty for time out have the most children." Of course, there are other ways a college major can influence fertility, such as the gender composition of peers in a student's major (Angrist, 2002). We do not take a stand on which is most prominent and instead document the heterogeneous impacts of fields of study on fertility.

Validation Exercise. Our empirical strategy—which allows us to distinguish between vertical and horizontal returns—relies on accounting for preference heterogeneity captured by the partial ordering observed in rank-ordered lists. Alternative approaches also leverage capacity constraints, identifying numerous experiments embedded within centralized assignment mechanisms (Abdulkadiroglu et al., 2017; Abdulkadiroğlu et al., 2022). The richness of our data and the institutional context enable us to validate our approach by directly comparing our model's predicted payoffs with those obtained from an alternative method that leverages quasi-experimental variation.

The key components of the approach, which leverages random assignment, are the applicant types, θ, a vector of degree cutoffs, τ, and a bandwidth, δ. Abdulkadiroğlu et al. (2022) show that the multitude of quasi-experiments embedded in centralized assignment produce major-specific propensity scores that account for both preferences and

[15]This finding may be interpreted as highlighting the importance of a more equitable distribution of child care responsibilities between mothers and fathers (Doepke and Kindermann, 2019).

capacity constraints, which we denote by $p_{ij}(\theta_i, \tau, \delta)$. These propensity scores are defined locally in settings such as ours with non-random tiebreakers, so they depend on flexible polynomials of the program-specific running variables, $g(\theta_i, \tau, r_i)$, where r_i is the notation for program-specific running variables. Last, because not everyone applies to every program, let a_{im} denote the application to a program m indicator with a_i summarizing the vector of application indicators, and let $p_i = (p_{i1}, \cdots, p_{iJ})$ denote individual i's vector of propensity scores. For expositional purposes, let $f_s(a_i, p_i, g_s(\theta_i, \tau, r_i, \delta)) = \sum_m \gamma_{mas} a_{im} + \sum_j \gamma_{jps} p_{ij}(\theta_i, \tau, r_i) + g_s(\theta_i, \tau, r_i, \delta)$ for $s = 1, 2$ summarize the functions that nest the application indicators, the local propensity scores, and running variables. Conditional on these functions, or equivalently, conditional on a_{im}, p_{ij}, and g_s, the offers to different majors—denoted by Z_{ij}—are *locally random* (Abdulkadiroğlu et al., 2022) and provide the benchmark variation for our validation exercise.

To connect the quasi-experimental approach to the validation exercise, first note that the restrictions implied by Assumption 1, Assumption 2, and Assumption 3 imply $|\mathcal{J}|$ restrictions. Aggregating the random assignment of institution-major pairs discussed above to the major level implies that we have $|\mathcal{J}|$ quasi-experiments shuffling students across majors. Following Angrist et al. (2017), we can assess the average predictive validity of our estimated model based on Assumptions 1–3, and also assess the average predictive validity for different majors via an overidentificaton test. Intuitively, this procedure allows us to benchmark our estimates against estimates derived explicitly using the local random assignment for identification.

For the validation exercises, let Y_i correspond to an observed outcome of individual i and let $\hat{Y}_i$ be the predicted outcome implied by our preferred empirical approach reported in Table 2 and Table 3. The validation exercise amounts to estimating the following model via two-stage least squares:

$$\begin{aligned} \hat{Y}_i &= \sum_j \pi_j Z_{ij} + f_1(a_i, p_i, g_1(\theta_i, \tau, r_i, \delta)) + e_{1i} \\ Y_i &= \psi \hat{Y}_i + f_2(a_i, p_i, g_2(\theta, \tau, r_i, \delta)) + e_{2i}. \end{aligned} \tag{10}$$

If the return predictions implied by our preferred empirical approach are commensurate to the returns implied by the random variation, then $\hat{\psi} = 1$, and our empirical approach is forecast unbiased. The overidentified nature of the validation exercise further allows for a joint test of the predictive validity of our approach for each field of study.

We conduct the validation exercise for both fertility and earnings models separately. Table 4 reports test results for three different models of interest. Column 1 reports test results for an uncontrolled model; Column 2 augments the model with quadratic polynomials in college admission scores, cell fixed effects, and cohort fixed effects; Column 3 adds the control functions. Column 1 is a useful starting point to demonstrate the prevalence of bias in models that do not adjust for observable differences that correlate with major choice. The second model contains observables available in a host of papers and serves as a useful reference point to gauge the potential bias present in those papers (Altonji et al., 2012, 2016; Andrews et al., 2024). Column 3 is our preferred model corresponding to the specification used to obtain the estimates in Table 2 and Table 3.

Panel (a) shows that uncontrolled earnings models exhibit substantial bias, with a forecast coefficient of 0.72. Models that include cell and cohort fixed effects and polynomials of PSU scores, analogous to SAT scores in other papers, show an improvement with a forecast coefficient of 0.86. The forecast coefficient in Column 2 is marginally significantly different from 1. Our preferred model produces a forecast coefficient of 0.98, underscoring the importance of accounting for preference heterogeneity to improve the average predictive validity of earning impacts. This evidence bodes well for other approaches and settings that use preference data to account for selection bias (Mountjoy and Hickman, 2021; Dale and Krueger, 2002).

Panel (b) reports broadly similar findings for fertility impacts. Uncontrolled models have a substantial forecast bias, and adding rich controls attenuates the forecast bias, but to a lesser extent than in earnings models. Adding control functions that characterize selection into majors produces a forecast coefficient of 0.95 that we cannot reject equals one. The overarching conclusion from the validation exercise is that our preferred model adequately characterizes selection into majors and produces treatment effects that line up with quasi-experimental approaches leveraging locally random assignment to majors.

4.2 Preferences for Pecuniary and Non-Pecuniary Payoffs

While the relationship between college majors and earnings is well-grounded in canonical human capital theory—which views education as an investment yielding pecuniary returns through enhanced labor market productivity (Mincer, 1958; Becker, 1964)—the link between college major choices and fertility is more nuanced. Wiswall and Zafar (2021) show that students' ex-ante beliefs about both outcomes affect their choices. Consistent with their evidence, a broader literature documents heterogeneity in fertility preferences (Doepke and Kindermann, 2019), the role of career concerns in fertility timing (Doepke et al., 2023), and tradeoffs between occupational paths and family formation (Adda et al., 2017), reinforcing the intricate relationship we posit between major choice, earnings, and fertility.

We complement this body of work by shifting the lens from expectations to realized (ex-post) payoffs. Whereas Wiswall and Zafar (2021) elicit subjective forecasts when decisions are still pending, we ask whether the objective, causal returns that graduates eventually experience are mirrored in aggregate choice patterns. In particular, we relate the cell-specific mean utilities of each field of study to the cell-specific payoffs of each field. Building on our choice model, we estimate:

$$\hat{\delta}_{jc} = \rho^E \bar{Y}^E_{jc} + \rho^F \bar{Y}^F_{jc} + \beta X_j + e_{jc}, \tag{11}$$

where the parameters of interest are ρ^E and ρ^F, which gauge the relative importance of earnings and fertility payoffs in shaping major choice decisions. To measure $\bar{Y}^E_{jc}$ and $\bar{Y}^F_{jc}$ we consider the causal impact of graduation from major j on earnings and fertility, as captured by our main model (9) estimated at the cell level c. The vector of controls X_j includes the average cost of the programs within each field of study, as well as field-specific attributes related to career and family.

This specification assesses whether students have preferences over earnings and fertility timing—factors that strongly influence the career-family trade-off. This trade-off emerges from the different sets of job opportunities available to students upon graduation, which vary by field of study. Students are assumed to anticipate these opportunities, with heterogeneous preferences generating gender differences in sorting across college majors. In this regard, our approach aligns closely with Adda et al. (2017), who examine the interplay between fertility decisions, labor supply, and occupational choice. However, our framework focuses on an earlier stage—the choice of college major—which subsequently shapes the occupational decisions available to graduates.

Table 5 reports estimates of Equation (11). Panel (a) shows that cell-and-major-specific earnings and fertility payoffs independently predict major choice when we pool across genders. Specifically, Column (1) indicates that a one standard deviation increase in earnings returns leads to a 0.25 standard deviation rise in mean utility. Likewise, Column (2) reveals that a one standard deviation increase in fertility impacts leads to a 0.14 standard deviation increase in mean utility. Column (3) further shows that both pecuniary and non-pecuniary returns remain predictive of choices even when controlling for each other. Consistent with our interpretation that the majors' impact on fertility is mediated by access to careers with varying degrees of work-family balance, column (4) shows that the relative importance of fertility returns declines when controlling for job- and field-specific attributes related to career and family.[16]

Panel (b) examines gender differences in preferences, revealing significant heterogeneity. The demand for fields of study is predicted by earnings payoffs for both men and women; however, monetary considerations are less predictive for women, while family considerations are more salient, suggesting that women place greater emphasis on balancing career and family (Goldin, 2021). This pattern is consistent across both univariate and multivariate preference models. For example, Column (3) indicates that, for women, a one standard deviation increase in causal impacts on fertility, holding earnings con-

[16]The controls used as proxies for career and family compatibility are the unemployment rate, the percentage of graduates in the public sector, the percentage of graduates married or cohabiting, and our estimates of child penalties and breadwinner gender norms by field of study.

stant, is associated with a 0.26 standard deviation increase in mean utility. Conversely, when fertility is held constant, a one standard deviation increase in earnings impacts is associated with only a 0.16 standard deviation increase in mean utility. In contrast, for men, earning impacts (holding fertility impacts constant) are associated with a 0.35 standard deviation increase in mean utility, and their demand is not predicted by family considerations. As shown by column (4), controlling for field-specific attributes related to career and family also decreases the relative importance of fertility in this case. In general, the inclusion of field-specific attributes affects estimated preferences of women and to a much lesser extent that of men.

To what extent does the differing weight placed on family considerations by men and women contribute to gender disparities? To explore this question, we perform a simple counterfactual exercise that assesses how preferences for family considerations relative to earnings affect the gender earnings gap. Specifically, we leverage Equation (11) to construct the predicted mean utilities for men and women when both groups place the same weight on fertility ($\hat{\rho}^F$). We consider two scenarios: one in which both genders adopt the men's family consideration preferences and another in which both adopt the women's preferences. Using the predicted mean utilities, we determine the counterfactual shares of individuals of gender g who would apply to each field j to then estimate the implied gender earnings gaps in each scenario.

Appendix Figure 4 reports the estimates from this exercise. We find that equalizing family considerations between genders—while holding other preferences constant—can reduce the earnings gap by 28-32 percent. Assigning male preferences to everyone has a slightly greater effect, largely because it minimizes the influence of family considerations on choice behavior. As shown by Appendix Figure B.2—which breaks down the changes observed in Appendix Figure 4 by fields of study—we also find that equalizing preferences for family considerations significantly reduces the contribution of Engineering programs to the overall gender earnings gap while diminishing the offsetting impact of Teaching and Health programs.

Although correlational, these results underscore that gender differences in the importance of family considerations can shape pre-labor market human capital investments, thereby contributing to the gender earnings gap. While policies aimed at shifting preferences may offer a long-term avenue for addressing these disparities, changes in preferences are likely to occur gradually, as they often reflect deeply rooted social norms (e.g., Alesina et al., 2013). Therefore, in the next section, we focus on more immediate policy interventions that may reduce gender gaps while taking existing preferences as given.

5 Counterfactual Analysis

In this section, we turn to assessing the potential impacts of policies that aim to address differences in representation in high-earning majors. We use our gender-specific payoffs by field of study and the structure of college admissions to evaluate the impact of different policies on female representation, the gender earnings gap, efficiency, and overall fertility. As a proof of concept of what can be done in systems with centralized assignment, we assess two policy scenarios: expanding capacity in all Engineering programs by 10%, 30%, or 50%, and reserving 10%, 30%, or 50% of Engineering seats for female students, while keeping the capacities of all programs fixed.[17] Capacity expansions are gender-neutral policies, with their distributional impacts shaped by preferences within the applicant pool. Quotas, in contrast, explicitly target specific groups and cause displacement effects for untargeted groups.[18] Both policies carry equity and efficiency implications, which we explore in this section.

5.1 Conceptual Framework

We begin with the population of college applicants $\mathbb{I} \supseteq \mathcal{I}$ and simulate student assignments under alternative capacity and quota policies. The inputs of the counterfactual analysis are a vector of student types $\mathbf{\Theta}$,[19] programs' capacities $\mathbf{q}$, priorities ω, and a matching algorithm, i.e., a function that takes as inputs the previous elements and outputs a vector of student assignments ψ. Changes in the policies we consider correspond to a change in $\mathbf{q}$ for gender-neutral seat expansions or to a change in ω for gender quotas, both of which produce a change in the allocation of students ψ.[20]

Potential outcomes Y_{ij}—among the pool of assigned students $i \in \mathcal{I}$—are defined in Equation (2). When we turn to assignment counterfactuals, students may be shuffled across majors but may also be unassigned. To address this additional state not included in our baseline estimates, we make a slight modification to our notation: j continues to denote fields of study, but $j = 0$ now corresponds to not being assigned.

Equipped with the baseline causal model for assigned students, and an auxiliary causal model for college returns among unassigned students,[21] we can define potential outcomes

[17] We focus on Engineering since female representation in that field is a key driver of the overall gender pay gap (see Figure 2).

[18] In the most recent admissions processes, exclusive seats were reserved for women in STEM-related fields, aiming to reduce the gender gap in Chile. The country ranks fourth lowest among OECD nations in the proportion of women graduating in STEM (MINEDUC, 2023; La Tercera, 2023). Bursztyn et al. (2023) find that 74% of Chilean citizens support affirmative action policies.

[19] As presented in the conceptual framework, the vector of students' types $\mathbf{\Theta}$ are summarized by a vector of rank order lists $\mathbf{R}$ and program-specific scores $\mathbf{s}$.

[20] To not over-complicate notation, we suppress the implicit dependence of ψ on $\mathbf{\Theta}, \mathbf{q}$, and ω.

[21] We estimate an auxiliary model among the unassigned students to obtain their potential outcomes in the unassigned state. We elaborate on the details in the next section.

in terms of students' assignments and graduation probabilities. We define individual i's potential outcome from being assigned to j as:

$$\tilde{Y}_{ij} = \begin{cases} p_{ij}Y_{ij} + (1 - p_{ij})Y_{i0} & \text{if } j > 0 \\ p_{i0}Y_{i01} + (1 - p_{i0})Y_{i00} & \text{if } j = 0 \end{cases} \tag{12}$$

where p_{ij} corresponds to the graduation probability of student i in assignment j if assigned to j and p_{i0} denotes the graduation probability in the outside option if unassigned.[22] Let Y_{ij} denote the outcome when student i, assigned to program j, graduates; let Y_{i0} denote the outcome when they are assigned but do not graduate; and let Y_{i00} and Y_{i01} denote, respectively, non-graduation and graduation outcomes for unassigned students. By defining the outcome $\tilde{Y}_{ij}$ in this way, we explicitly capture how potential outcomes and graduation probabilities shift under different counterfactual scenarios, rather than relying on optimistic assumptions about success in any given major.

Different policies generate changes in assignments to majors that produce changes in expected earnings via various channels. For a given allocation ψ, student i's assignment is $j(\psi_i)$. Thus, we define the conditional average change in expected earnings that student i experiences after a change from ψ to ψ' as:

$$\begin{aligned} \Xi_i(\psi, \psi') \equiv E\Big[&\tilde{Y}_{ij(\psi'_i)} \mid \theta(R_i, X_i),\, G_i,\, d_i,\, Z_{ij(\psi'_i)} = 1,\, p_i\Big] \\ &- E\Big[\tilde{Y}_{ij(\psi_i)} \mid \theta(R_i, X_i),\, G_i,\, d_i,\, Z_{ij(\psi_i)} = 1,\, p_i\Big], \end{aligned}$$

where θ_i, G_i, and d_i are type, gender, and distance vectors, $Z_{ij} = \mathbf{1}\{j(\psi_i) = j\}$ indicates whether student i is assigned to major j or left unassigned ($j = 0$), with $\sum_j Z_{ij} = 1$ due to the single-offer system, and $p_i = (p_{i1}, \cdots, p_{iJ})$ corresponds to the vector of graduation probabilities. For students that are assigned in both ψ and ψ', the previous expression can be conveniently rewritten as:

$$\begin{aligned} \Xi_i(\psi, \psi') = \underbrace{(p_{ij(\psi'_i)} - p_{ij(\psi_i)})}_{\Delta\text{Graduation Prob.}} &\times \tau_{j(\psi'_i)}(\theta(R_i, X_i), G_i, d_i) \\ + \quad p_{ij(\psi_i)} &\underbrace{[\tau_{j(\psi'_i)}(\theta(R_i, X_i), G_i, d_i) - \tau_{j(\psi_i)}(\theta(R_i, X_i), G_i, d_i)]}_{\Delta\text{Returns}}, \end{aligned} \tag{13}$$

where τ_j represents the conditional average treatment effect—defined in equation (8)—experienced by student i when she is assigned to major j. Equation (13) highlights two key forces that may work in opposite directions. The first term reflects the net expected change in earnings induced by a change in students' graduation probability as they transition from $j(\psi_i)$ to $j(\psi'_i)$. The second term captures the potential change in the economic returns if the student graduates from $j(\psi'_i)$ instead of $j(\psi_i)$. If a student

[22]Note that we the p_{ij} defined in this section are different from the p_{ij} in the validation exercise.

is reassigned to a field with higher earnings potential, this second term will be positive, reflecting an increase in expected earnings. However, if the reassignment results in a lower probability of graduation—due, for example, to mismatch effects (Arcidiacono and Lovenheim, 2016; Arcidiacono et al., 2016)—the first term will be negative.[23]

With a definition of expected earnings across assignments and changes in expected earnings, we now define four counterfactual statistics of interest: the change in the representation gap in Engineering, the expected change in the gender earnings gap, the expected change in total earnings, and the expected change in total fertility.

Representation gap in Engineering: Let N_F and N_M correspond to the total number of female and male applicants, and let j^E correspond to Engineering. The representation gap—in Engineering—under allocation ψ is defined as:

$$\Delta S(\psi) = \frac{\sum_{G_i=F} \mathbb{1}\{j(\psi_i) = j^E\}}{N_F} - \frac{\sum_{G_i=M} \mathbb{1}\{j(\psi_i) = j^E\}}{N_M} \tag{14}$$

where $\Delta S(\psi)$ measures the difference in the within-gender shares of Engineering assignments between women and men under allocation ψ. Across counterfactual scenarios, our object of interest is the percentage change in the representation gap in Engineering, i.e., $100 \times (\Delta S(\psi') - \Delta S(\psi))/\Delta S(\psi)$.

Earnings gap: Let $\tilde{Y}_i(\psi) = \sum_j Z_{ij}\tilde{Y}_{ij}$ represent the expected earnings of student i under allocation ψ. The gender earnings gap under allocation ψ is defined as:

$$\Delta(\psi) = \frac{\sum_{G_i=F} \tilde{Y}_i(\psi)}{N_F} - \frac{\sum_{G_i=M} \tilde{Y}_i(\psi)}{N_M}$$

Thus, when the allocation changes from ψ to ψ', the corresponding percentage change in the gender earnings gap is given by $100 \times (\Delta(\psi') - \Delta(\psi))/\Delta(\psi)$.

Total Earnings: To summarize the efficiency implications of each counterfactual assignment ψ', we consider the percentage change in aggregate expected earnings relative to the baseline assignment ψ, defined as:

$$\mathcal{E}(\psi, \psi') = 100 \times \left[\frac{\sum_i \Xi_i(\psi, \psi')}{\sum_i E[\tilde{Y}_i(\psi) \mid \theta(R_i, X_i), G_i, d_i, Z_{j(\psi_i)} = 1, p_i]}\right]. \tag{15}$$

[23] The empirical evidence on mismatch effects is mixed. In the University of California system, Arcidiacono et al. (2016) find that less prepared minority students at top-ranked campuses would have higher graduation rates had they attended lower-ranked campuses, while Bleemer (2023) finds that minority students experienced worse outcomes as they transitioned into lower tier universities following the ban of California's Proposition 209. Also, in the US, Black et al. (2023) find that introducing the Texas Top Ten Percent rule improved enrollment and graduation of highly ranked students at more disadvantaged high schools. In Latin America, Otero et al. (2023) do not find strong evidence of mismatch effects in Brazil, while Carlana et al. (2024) find evidence of mismatch effects for marginal students but not for the average students in Chile.

Equation (15) summarizes the economic distortion induced by the change in the allocation of students. A positive value of $\mathcal{E}(\psi, \psi')$ indicates that ψ' leads to higher aggregate expected earnings than ψ, while a negative value implies the opposite.

Total Fertility: Finally, to account for the impacts on fertility, we consider the analog of Equation (15) but replace the outcome of interest $\tilde{Y}_i(\psi)$ with the likelihood of having at least one child 13 years after college application. In other words, we summarize the fertility distortion induced by the change in the allocation of students as the percentage change in expected fertility relative to the baseline allocation.

5.2 Empirical Methods

To construct our statistics of interest, we need estimates of potential outcomes and graduation probabilities for all students and assignments. The potential outcomes model discussed in Section 3 serves as the starting point to define Y_{ij} among assigned students, and we use an auxiliary causal model to estimate Y_{i01} and Y_{i00}. To estimate graduation probabilities p_{ij}—which are instrumental in understanding potential mismatch effects—we follow Heller et al. (2022) and train and estimate a gradient boosting model (GBM).

We use estimates of Equation (9) to construct expected potential outcomes $\hat{Y}_{ij}(\psi)$ for each individual that is assigned to a field j in allocation ψ. Our causal model centers on potential outcomes for students who are assigned to a field of study (i.e., $j > 0$). However, for this exercise, we also need to construct outcomes for unassigned students—those who do not receive a college offer ($j = 0$). To that end, we estimate our causal model using the same rich vector of attributes that are part of our estimating Equation (9) but in a sample of applicants who are not assigned to any program. In particular, we estimate a selection-corrected causal model that utilizes the rank-ordered choice data employed in our main analysis, with the distinction that we do not estimate returns to majors but instead focus on an omnibus return to college among the unassigned. To summarize, the estimates of the baseline causal model provide us with potential outcomes for individuals assigned to a field $j > 0$, and the auxiliary causal model provides us with the potential outcomes for unassigned individuals ($j = 0$). It is worth noting that our approach implicitly rules out peer effects and behavioral responses as treatment effects are invariant to the composition of students across counterfactual policies—an assumption aligned with prior studies (Otero et al., 2023; Larroucau and Rios, 2022).

To estimate the graduation probabilities p_{ij}, we train GBMs tailored to each major. The models incorporate various features, including demographic characteristics, entrance exam scores, preferences revealed through rank-ordered lists, information about the selectivity of institutions that accept applicants, and all other relevant attributes that are part of Equation (9). Our inclusion of preference and admission attributes in the prediction model parallels Arcidiacono et al. (2016), who incorporate controls akin to those in Dale and Krueger (2002) in their structural modeling of graduation outcomes across university-program pairs. To improve model performance, we fine-tune the parameters for each major separately, optimizing them to minimize the average Brier score over cross-validation samples.[24] Importantly, we find that out-of-sample estimates of $\hat{p}_{ij}$ for the most relevant fields of study are forecast unbiased for both men and women. Appendix D offers additional details and summary statistics for the GBM and tests the robustness of our counterfactual analysis by replacing the GBM with a causal graduation model analogous to our baseline outcome model.

5.3 Results

Figure 5 summarizes the impacts of several counterfactual policies targeting Engineering admissions. The first three gray bars in Panel (a) show that expanding Engineering seats without gender-specific intervention worsens the representation gap. Because men demand Engineering seats more strongly, they secure most of the newly available places, further widening the gender imbalance: the representation gap grows by 7-20 percent relative to its 16 percentage point baseline. In contrast, the next three maroon bars illustrate that gender quotas of 10%, 30%, and 50% mechanically lower the representation gap. However, even these quotas do not fully eliminate the disparity, reflecting persistent differences in application rates between men and women.[25] Turning to the total gender earnings gap in Panel (b), we see that gender-neutral Engineering seat expansions not only worsen the representation gap but also increase the overall gender pay gap. In contrast, quota policies reduce the gender earnings gap between 3 and 12 percent, depending on the quota size.

The differences in policy effects stem from variations in where students come from, where they go, and how their graduation probabilities change under each rule. Under the seats expansion policy, although some students enter Engineering from Science and Business, the majority of men and women are drawn from the outside option (i.e., previously unassigned), thereby boosting overall enrollment (see Appendix Table D.1). Importantly, seat expansions affect nearly twice the proportion of men as of women. The gender quota policy, by contrast, affects men and women in relatively similar proportions but triggers more extensive reshuffling: a large share of women *enter from* the outside option—yet roughly 30% are diverted away from Science and Business—while about 50% of men are *displaced into* the outside option and 40% into Science or Business (see Appendix Table D.2). Regarding the changes in the graduation probabilities among assigned students,

[24] By minimizing the cross-validation average Brier score, we place emphasis on providing reliable and well-calibrated predictions of graduation probabilities across different majors.

[25] When introducing quotas, we modify the matching algorithm to consider reserved seats by processing open seats first and then applying the quotas. See Dur et al. (2018) for the significance of precedence in allocating reserved seats.

Appendix Figure D.3 indicates that, on average, seat-expansion policies increase graduation probabilities for both genders, whereas quota policies raise women's graduation rates but lower those of men.

What are the implications for total earnings and fertility? One may be concerned that reallocative policies could generate efficiency losses or further reduce fertility, a particularly salient issue in Chile, where the fertility rate has fallen to 1.5, well below the replacement level. We measure these possibilities by aggregating the outcomes of interest implied by our counterfactuals, considering all applicants in the centralized admission system. Panel (c) of Figure 5 reports small impacts on efficiency as captured by aggregate earnings ($< 1\%$ change relative to the baseline). Due to the seat expansion, more students gain college access, leading to a small increase in aggregate earnings, while gender quotas lead to small declines in aggregate earnings. Finally, Panel (d) of Figure 5 reports impacts on overall fertility, measured as the probability of having at least one child 13 years after college application. In this case, we find that both policies would reduce aggregate fertility, but quotas would lead to very small declines ($< 0.5\%$).

All in all, the results illustrate that gender quotas in Engineering have the potential to reduce gender earnings gaps while having a minimal impact on total earnings and fertility.[26] Taking our estimates at face value suggests that even aggressive 50% female quotas in Engineering would have small impacts on efficiency and shave less than half a percentage-point off the share of applicants who have become parents after 13 years. This fertility effect is an order of magnitude smaller than Chile's existing shortfall from replacement levels. In other words, our estimates suggest that policies that close representation gaps and narrow gender earning gaps can be pursued without meaningful economic and demographic sacrifice in the short term. The latter finding contributes to the growing evidence that policies tend to have a modest impact on fertility outcomes (Dahl and Loken, 2024).

6 Conclusion

This paper examines how college majors affect earnings and fertility outcomes, and how these causal effects contribute to gender differences in representation across college majors. We focus on the balance of career and family—embodied by the relationship between majors, earnings, and family considerations—allowing us to elucidate important factors that generate rifts in the educational trajectories of men and women.

[26] On net, changes in total earnings and fertility are modest as few applicants are (directly or indirectly) affected in each policy scenario (see Appendix Figure D.4). Appendix Figure D.5 further shows that concerns about the quality-fit trade-off discussed by Arcidiacono and Lovenheim (2016) are minimal as Engineering programs tend to produce no-worse outcomes in expected earnings across the ability distribution of students reshuffled in the counterfactual scenarios.

Using administrative data that link higher education rank-ordered choices to long-run earnings and fertility, we first document large, heterogeneous causal returns across majors. Earnings premia span nearly 60 log-points from the least- to the most-remunerative field. Fertility effects also differ markedly by major—programs that funnel graduates toward stable, family-friendly occupations (for example, Teaching and Health) are associated with earlier child-bearing, whereas Law and Humanities degrees postpone parenthood. Yet, within a given major, women and men experience similar economic payoffs.

We then show that the way students weigh these ex-post payoffs explains a significant share of the gender sorting we observe. Women, on average, attach substantially more value to the family-formation consequences of a major, while men place relatively greater weight on monetary returns. When we equalize those weights, the gender earnings gap among graduates shrinks by roughly one-third, primarily because more women flow into high-return Engineering and Science majors.

Finally, taking existing preferences and our causal estimates as given, we evaluate common supply-side proposals. We find that expanding Engineering capacity without reservation largely benefits men and widens the wage gap, but reserving 30-50 percent of seats for women narrows the gap by up to 12 percent, with virtually no loss in aggregate earnings and less than a half-percentage point reduction in fertility. These results underscore two policy lessons: (i) admissions rules can move equity outcomes quickly when preferences are slow to change, and (ii) alleviating the career-family trade-off through workplace flexibility or more egalitarian norms may offer a longer-run path to convergence that does not rely on quotas (Goldin, 2021).

While we have documented that the balance between career and family influences major choices and subsequently contributes to policy-relevant disparities, a comprehensive understanding of the factors driving gender differences in preferences remains elusive. Understanding how such preference-shifting forces take root, and whether similar dynamics arise in decentralized admissions systems—where students make major choice decisions at older ages—remain promising avenues for future work.

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_ **and** _ , "Human capital investments and expectations about career and family," *Journal of Political Economy*, 2021, *129* (5), 1361–1424.

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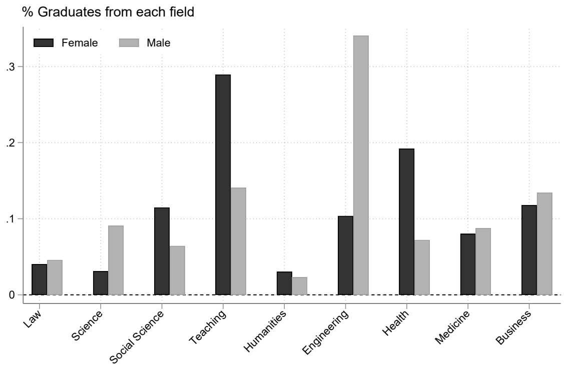
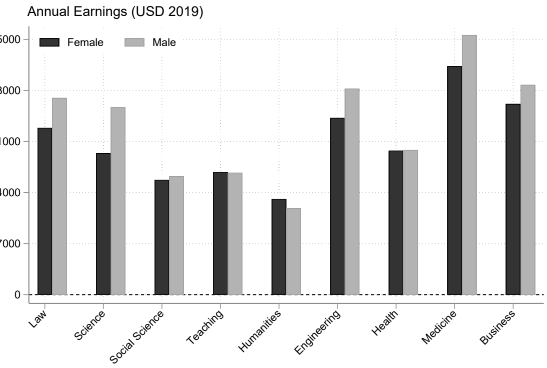
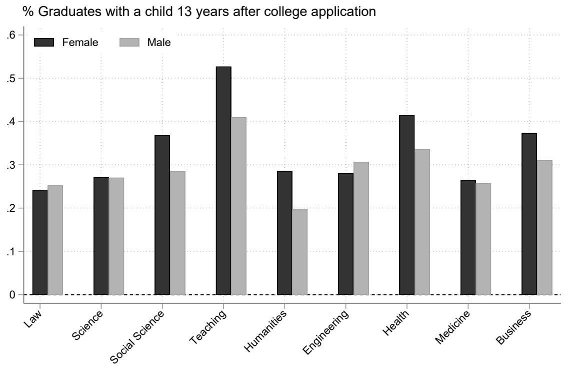
Figure 1: Graduation, Earnings, and Fertility



(a) Graduation



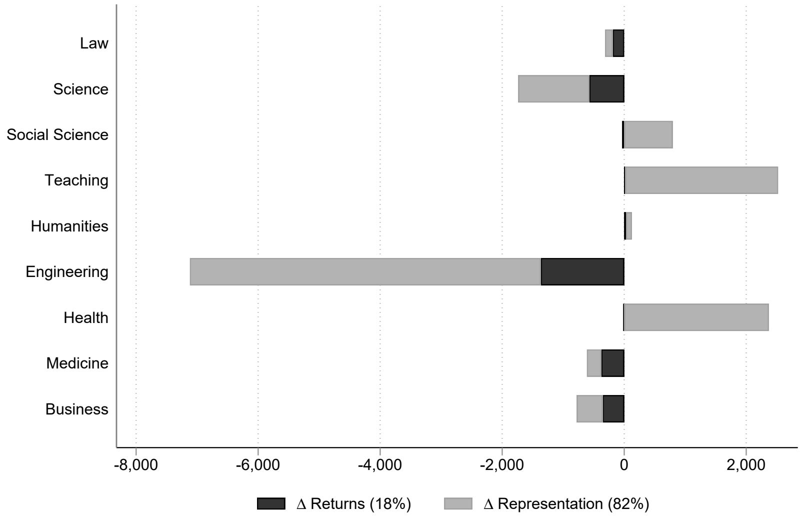
(b) Earnings



(c) Fertility

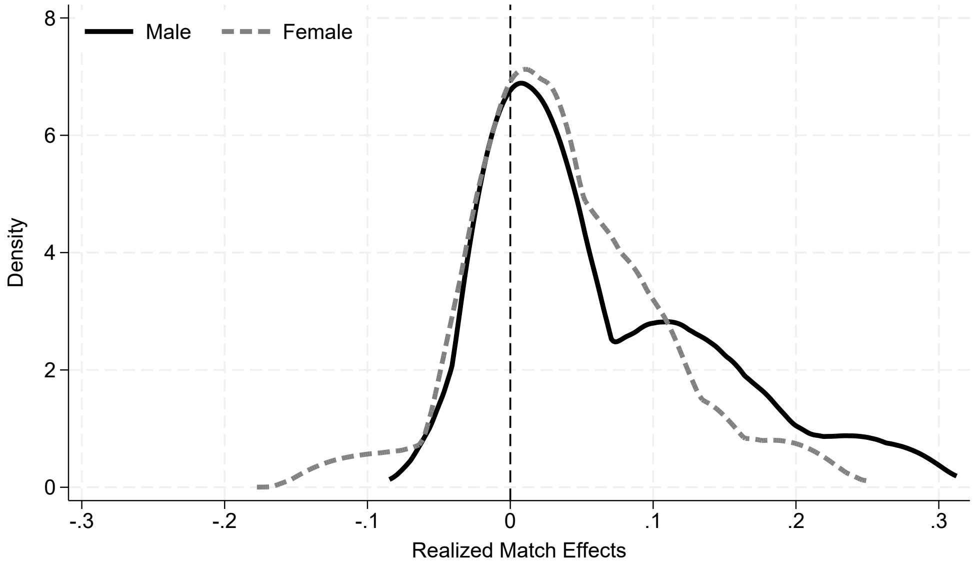
Notes: Panel (a) shows the share of male and female graduates in each field of study. Panel (b) shows the annual earnings (as of 2019) of male and female graduates in each field of study. Both panels consider students who were employed in 2019. Panel (c) shows the share of students with at least one child thirteen years after their college application. All panels consider students who applied through the centralized admission system between 2004 and 2007 and graduated between 2007 and 2019.

Figure 2: Earnings Gap: Sorting versus Returns



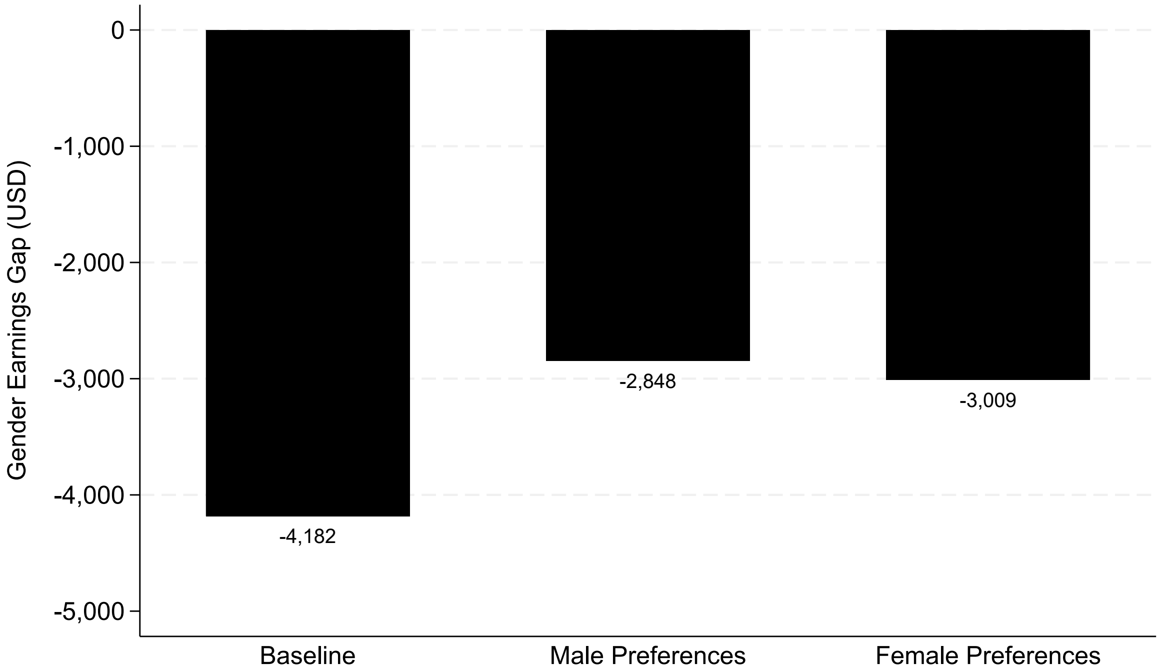
Notes: This figure shows the result from the exercise described by Equation (1) that decomposes the gender earnings gap into differences in sorting between fields of study and differences in returns within fields of study. We consider the annual earnings in 2019 of students who applied through the centralized admission system between 2004 and 2007, graduated between 2007 and 2019, and were employed in 2019.

Figure 3: Distribution of Realized Match Effect Estimates



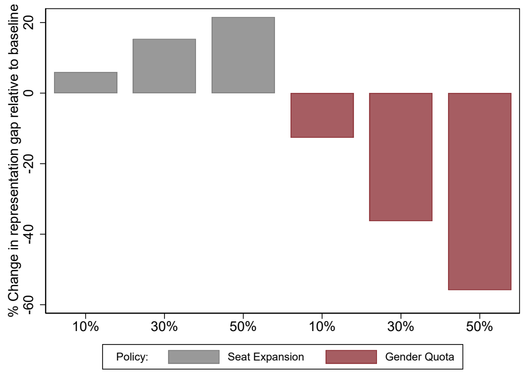
Notes: This figure shows the density of realized match effect estimates (i.e., $\hat{\psi}_j \times \lambda_{ij}$) for the population of accepted male and female students. For each student, we consider the $\hat{\psi}_j$—reported in Table 2—corresponding to their completed field and interact that with the student's control function estimate, λ_{ij}, for the same field. The sample includes 102,597 observations of students who applied and were accepted into a program through the centralized admission system between 2004 and 2007 and were employed in 2019.

Figure 4: Baseline and Counterfactual Earnings Gap

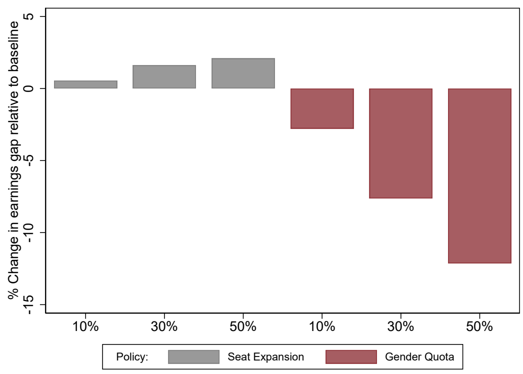


Notes: This figure reports the gender earnings gap in US dollars in three scenarios. We use the annual earnings 2019 of students who applied and were accepted through the centralized admission system between 2004 and 2007 and were employed in 2019. The first bar reports the baseline and observed gender earnings gap among this sample of students. The second bar, referred to as Male Preferences, assigns all students the preferences for family considerations of men. Similarly, the third bar, referred to as Female Preferences, assigns all students the preferences for family considerations of women. For each counterfactual bar, we use estimates reported in Table 5, in the main body of the paper, to construct counterfactual enrollment shares.

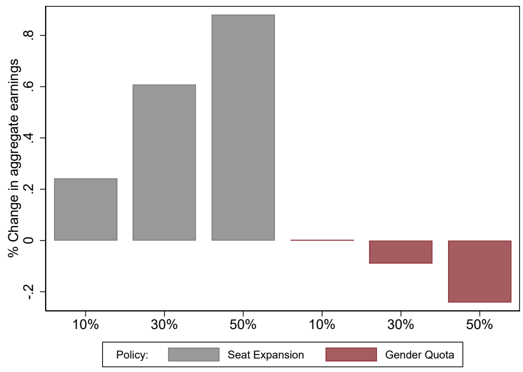
Figure 5: Counterfactual Policies



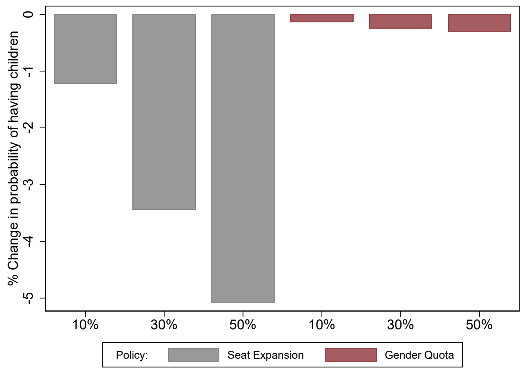
(a) Representation Gap in Engineering



(b) Total Earnings Gap



(c) Total Earnings



(d) Total Fertility

Notes: These figures report the changes (relative to baseline) implied by different counterfactual scenarios. We consider 10, 30, and 50% seat expansions in Engineering programs (gray bars) and a 10, 30, and 50% quota for women in Engineering (red bars). Panel (a) shows the impacts on the Engineering representation gap. Panel (b) reports the impacts on the total gender earnings gap. Panels (c) and (d) show the total impacts on earnings and fertility. The sample includes the 87,599 students who applied through the centralized admission system in 2007.

Table 1: Descriptive Statistics

	Non-Grad (1)	Law (2)	Science (3)	Social Science (4)	Teaching (5)	Humanities (6)	Engineering (7)	Health (8)	Medicine (9)	Business (10)
Panel A: Female										
PSU Score	0.03	1.00	0.57	0.41	-0.02	0.69	0.76	0.57	1.53	0.40
Father is College Grad	0.20	0.46	0.29	0.34	0.17	0.47	0.38	0.26	0.52	0.28
Mother is College Grad	0.15	0.41	0.24	0.29	0.13	0.39	0.30	0.22	0.46	0.23
Has Public Health Insurance	0.59	0.37	0.52	0.47	0.62	0.36	0.45	0.53	0.32	0.50
Attended Public K-12 School	0.29	0.14	0.24	0.21	0.33	0.12	0.15	0.25	0.10	0.20
Observations	10,477	1,697	1,307	4,833	12,184	1,280	4,365	8,085	3,374	4,961
Panel B: Male										
PSU Score	0.24	1.18	0.63	0.59	0.18	0.64	0.84	0.65	1.72	0.65
Father is College Grad	0.22	0.50	0.29	0.34	0.17	0.35	0.37	0.23	0.50	0.36
Mother is College Grad	0.17	0.44	0.23	0.30	0.14	0.29	0.28	0.19	0.44	0.27
Has Public Health Insurance	0.59	0.32	0.51	0.47	0.65	0.46	0.45	0.58	0.33	0.45
Attended Public K-12 School	0.29	0.16	0.26	0.25	0.35	0.22	0.20	0.33	0.18	0.20
Observations	15,494	1,584	3,146	2,223	4,861	797	11,772	2,493	3,030	4,634

Notes: This table presents the average test scores (PSU Scores) and available background characteristics of all students in our main analysis sample, by gender and field of study. Student characteristics come from the forms that students must complete before taking the college admission exam. Our main analysis sample includes all students who applied and were accepted into a program through the centralized admission system between 2004 and 2007 and who were employed in 2019.

Table 2: Gender Differences in Pecuniary Returns: Log Earnings in 2019

	Law (1)	Science (2)	Social Science (3)	Teaching (4)	Humanities (5)	Engineering (6)	Health (7)	Medicine (8)	Business (9)
					Panel A: Female				
Returns	0.364	0.333	0.214	0.415	-0.056	0.554	0.347	0.779	0.606
	(0.069)	(0.053)	(0.026)	(0.010)	(0.054)	(0.027)	(0.022)	(0.036)	(0.018)
Selection on Gains	0.062	0.029	0.038	-0.004	0.081	0.011	0.059	-0.035	0.017
	(0.023)	(0.026)	(0.014)	(0.008)	(0.022)	(0.015)	(0.011)	(0.014)	(0.012)
					Panel B: Male				
Returns	0.437	0.536	0.171	0.321	-0.184	0.623	0.338	0.801	0.598
	(0.081)	(0.028)	(0.043)	(0.018)	(0.070)	(0.015)	(0.036)	(0.042)	(0.022)
Selection on Gains	0.088	0.039	0.068	0.058	0.095	0.013	0.076	0.003	0.050
	(0.027)	(0.016)	(0.019)	(0.010)	(0.025)	(0.012)	(0.017)	(0.019)	(0.010)
Est. Females = Est. Males									
Δ Returns (p-val)	0.491	0.001	0.376	0.000	0.145	0.020	0.818	0.689	0.751
Δ Match Effects (p-val)	0.458	0.730	0.193	0.000	0.674	0.926	0.401	0.100	0.028

Notes: This table presents the estimates obtained from our main regression model, presented in Equation (9), and estimated jointly with fertility outcomes. We display the averages—of coefficients, robust standard errors, and p-values—across 100 regressions, each of which uses a different set of control functions obtained after drawing from the asymptotic distribution of the demand model estimates. The sample includes 102,597 observations of students who applied and were accepted into a program through the centralized admission system between 2004 and 2007 and were employed in 2019. Panel (a) focuses on parameter estimates for Female students, and Panel (b) focuses on Male students. In each panel, we report return estimates and selection on gains parameters, also referred to as match effects in the text. Underneath each estimate in parentheses is the bootstrapped standard error. The final two rows of the table report p-values from two additional statistical tests. The Δ Returns p-values correspond to the null hypothesis that the Female and Male returns are equivalent for a given column. The Δ Match Effects p-values correspond to the null hypothesis that the Female and Male match effects are equivalent for a given column. In addition to main effects for field of study and control functions and gender interactions, all specifications include quadratic polynomials of the college admission scores in mathematics and language, a distance vector, cohort fixed effects and cell fixed effects, where a cell is defined by school type, macro-region, financial-aid relevant score ranges, and gender. For ease of interpretation, $\hat{\lambda}_{ij}$ is standardized within each cell. The R^2 of this regression is 0.19.

Table 3: Gender Differences in Non-Pecuniary Returns: Any child 13 years after college application

	Law (1)	Science (2)	Social Science (3)	Teaching (4)	Humanities (5)	Engineering (6)	Health (7)	Medicine (8)	Business (9)
					Panel A: Female				
Returns	-0.156	-0.116	-0.066	0.045	-0.094	-0.071	-0.010	-0.073	-0.054
	(0.025)	(0.023)	(0.012)	(0.008)	(0.020)	(0.015)	(0.011)	(0.018)	(0.010)
Selection on Gains	0.019	-0.006	0.006	-0.011	0.008	-0.013	-0.001	-0.002	0.022
	(0.009)	(0.011)	(0.007)	(0.005)	(0.009)	(0.008)	(0.006)	(0.007)	(0.007)
					Panel B: Male				
Returns	-0.142	-0.166	-0.148	-0.070	-0.210	-0.109	-0.107	-0.113	-0.126
	(0.026)	(0.012)	(0.017)	(0.012)	(0.021)	(0.008)	(0.016)	(0.017)	(0.011)
Selection on Gains	0.009	0.013	0.011	-0.006	0.016	0.001	0.005	0.015	0.003
	(0.009)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)	(0.005)
Est. Females = Est. Males									
Δ Returns (p-val)	0.686	0.052	0.000	0.000	0.000	0.018	0.000	0.102	0.000
Δ Match Effects (p-val)	0.410	0.151	0.629	0.576	0.528	0.176	0.520	0.109	0.021

Notes: This table presents the estimates obtained from our main regression model, presented in Equation (9), and estimated jointly with fertility outcomes. We display the averages—of coefficients, robust standard errors, and p-values—across 100 regressions, each of which uses a different set of control functions obtained after drawing from the asymptotic distribution of the demand model estimates. The sample includes 137,339 observations of students who applied and were accepted into a program through the centralized admission system between 2004 and 2007. Panel (a) focuses on parameter estimates for Female students and Panel (b) focuses on Male students. In each panel, we report return estimates and selection on gains parameters, also referred to as match effects in the text. Underneath each estimate in parentheses is the bootstrapped standard error. The final two rows of the table report p-values from two additional statistical tests. The Δ Returns p-values correspond to the null hypothesis that the Female and Male returns are equivalent for a given column. The Δ Match Effects p-values correspond to the null hypothesis that the Female and Male match effects are equivalent for a given column. In addition to main effects for field of study and control functions and gender interactions, all specifications include quadratic polynomials of the college admission scores in mathematics and language, a distance vector, cohort fixed effects and cell fixed effects, where a cell is defined by school type, macro-region, financial-aid relevant score ranges, and gender. For ease of interpretation, $\hat{\lambda}_{ij}$ is standardized within each cell. The R^2 of this regression is 0.06

Table 4: Testing for Bias

	Panel (a): Earnings		
	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Forecast Coefficient ϕ	0.717	0.856	0.984
	(0.066)	(0.078)	(0.091)
Covariates in the model:			
Controls	No	Yes	Yes
Control function	No	No	Yes
Tests:			
First-stage F- Statistic:	251	213	171
p-values			
Forecast bias ($\phi = 1$)	0.000	0.065	0.861
Overidentification	0.068	0.212	0.087
Observations	44,684	44,684	44,684
	Panel (b): Fertility		
	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Forecast Coefficient ϕ	0.555	0.768	0.946
	(0.109)	(0.144)	(0.176)
Covariates in the model:			
Controls	No	Yes	Yes
Control function	No	No	Yes
Tests:			
First-stage F-statistic	471	321	239
p-values			
Forecast Bias ($\phi = 1$)	0.000	0.107	0.758
Overidentification	0.047	0.110	0.116
Observations	59,859	59,859	59,859

Notes: This table reports the results of tests for bias following Equation (10). Column 1 reports test results for an uncontrolled model; Column 2 augments the model with quadratic polynomials in PSU scores, cell fixed effects, and cohort fixed effects; Column 3 adds the control functions. The forecast bias test checks whether the forecast coefficient equals 1, and the overidentification test checks the IV model's overidentifying restrictions. Panel (a) considers logged earnings in 2019 as the outcome, and Panel (b) considers fertility (any child 13 years after college application) as the outcome.

Table 5: Preferences for Pecuniary and Non-Pecuniary Returns

	Mean Utility			
	(1)	(2)	(3)	(4)
	Panel (a): Pooled Estimates			
Earnings	0.252		0.254	0.256
	(0.051)		(0.051)	(0.046)
Fertility		0.142	0.146	0.096
		(0.055)	(0.054)	(0.047)
	Panel (b): Estimates by Gender			
Earnings	0.354		0.353	0.286
	(0.073)		(0.073)	(0.066)
× Female	-0.213		-0.190	-0.069
	(0.108)		(0.104)	(0.094)
Fertility		0.038	0.023	0.008
		(0.075)	(0.074)	(0.063)
× Female		0.206	0.234	0.161
		(0.108)	(0.106)	(0.086)
R-squared:				
Panel (a)	0.27	0.24	0.29	0.48
Panel (b)	0.28	0.25	0.32	0.49
Career-Family Controls	No	No	No	Yes
Observations	324	324	324	324

Notes: This table reports estimates from bivariate and multivariate regressions of mean utilities on pecuniary and non-pecuniary returns. We consider causal estimates of earnings and fertility by field of study for each cell c. Cells are defined by macro-region, test score ranges, high school type, and gender. All specifications include cell fixed effects and control for the average cost of each field of study. Career-Family controls include the unemployment rate, the percentage of graduates in the public sector, the percentage of graduates married or cohabiting, and our estimates of child penalties and breadwinner gender norms by field of study. Standard errors are robust and reported in parentheses.

Online Appendix for:
College Major Choice, Payoffs, and Gender Gaps

Christopher Campos Pablo Muñoz Alonso Bucarey Dante Contreras

Table of Contents

A Data Appendix

This section discusses additional details regarding the data used in our analysis. We begin by providing details about our categorization of majors that are crucial in our analysis. Then, since earnings records are top-coded, we discuss our imputation procedure. We also report additional descriptive evidence related to trends in gender representation, preferences, and standardized test scores mentioned in the paper.

A.1 Major Classification

We classify students into fields of study based on the major from which they graduate. Degree programs are classified by field of study mostly based on the OECD Handbook for Internationally Comparative Education Statistics (OECD, 2004). There are eight broad categories: "*Agriculture*", "*Science*", "*Social Sciences, Business and Law*", "*Teaching*", "*Humanities and Arts*", "*Engineering, Manufacturing and Construction*", "*Health and Welfare*", and "*Services*".

We reclassify the category "*Social Sciences, Business and Law*" into three separate fields of study "*Social Sciences*", "*Business*", and "*Law*". The field of "*Social Sciences*" includes the degrees of anthropology, library science, political sciences, social communication, geography, journalism, psychology, sociology, and social work. The field of "*Business*" includes the degrees: commercial engineering, accounting, commerce engineering, business administration, marketing engineering, logistics engineering, foreign trade engineering, management control engineering, human resources engineering, finance engineering, public management, advertising, and public relations; while the field of "*Law*" includes law degrees.

We also separate "*Medicine*" from "*Health and Welfare*". In particular, we include all degrees covered by the "Medical Law" in Chile (Ley N 19,664) into "*Medicine*". These degrees are in medicine, dentistry, pharmaceutical chemistry, and biochemistry. Finally, we drop "*Agriculture*" and "*Services*" as they represent very few graduates (less than 5%) from non-homogeneous college programs in Chile.

A.2 Data Imputations

To impute wages above the social security contribution limit, we proceed as in Dustmann et al. (2009) and Card et al. (2013). First, we fit a series of Tobit models to log wages separately by gender, including the type of high school, test-score range, and region controls.

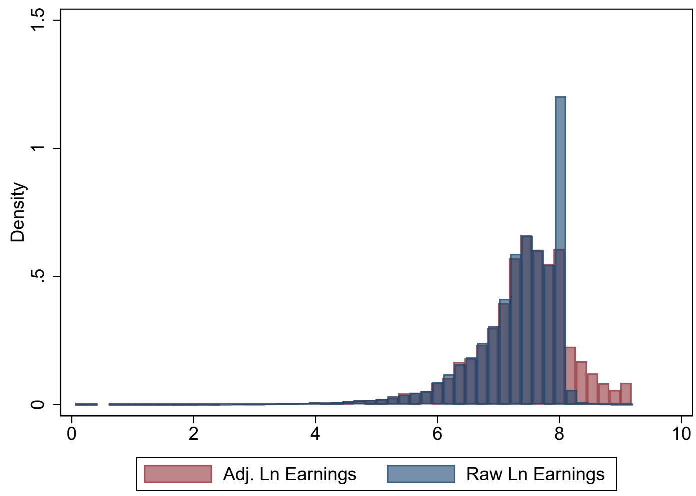
Then, we impute an uncensored value for each censored observation using the estimated parameters of these models and random drawings ϵ from a truncated distribution.

Following Gartner et al. (2005):

$$\epsilon_i = \Phi^{-1}\left(u \times \left[1 - \Phi\left(\frac{c - X_i'\hat{\beta}}{\hat{\sigma}}\right)\right] + \Phi\left(\frac{c - X_i'\hat{\beta}}{\hat{\sigma}}\right)\right),$$

where $u \sim$ U[0,1], c is the social security contribution limit, $X'\hat{\beta}$ is the Tobit prediction, and $\hat{\sigma}$ is the standard deviation of the Tobit error. Figure A.1 below presents both the distribution of the original log earnings and the imputation-adjusted log earnings used in our analysis.

Figure A.1: Log Earnings Distribution



Notes: This figure presents the histogram of the original log earnings and the imputation-adjusted log earnings. We adjust earnings at the contribution limit following Dustmann et al. (2009) and Card et al. (2013). For details, see section A.2.

A.3 Descriptive Evidence

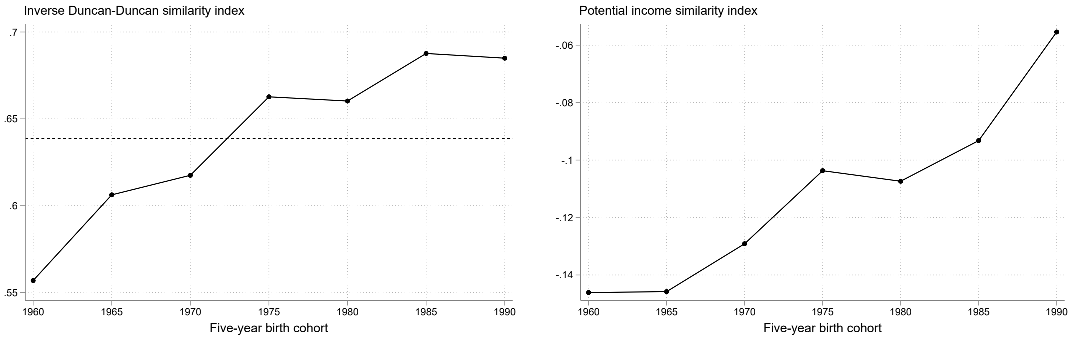
We present descriptive evidence that motivates and complements our work.

Historical Trends: Figure A.2 documents trends in gender representation across majors and its potential contribution to the overall gender gap. In the spirit of Sloane et al. (2021), and leveraging the largest household survey in Chile, the figure shows the major "similarity index" (as a re- normalization of the inverse Duncan-Duncan index) and a "potential wage index" (that assigns to everyone within a major the average hourly wages of prime-age male workers in that major). This figure shows that majors in Chile have become less segregated over time. See section 2 for details.

Fields of Study: To characterize preferences for fields of study, Figure A.3 reports fallback major prevalence. Overall, we find sensible patterns. For example, Social Science is the most common fallback option among students who rank Law programs as their most-preferred field, while Science is the most common fallback major for individuals who rank Engineering as their most-preferred field.

Fertility: Figure A.4 uses data from the World Bank World Development Indicators database and replicates the Figure presented in Doepke et al. (2023), including Chile. The figure shows total fertility rates since the seventies and highlights that Chile's fertility rates today are remarkably similar to those of high-income countries.

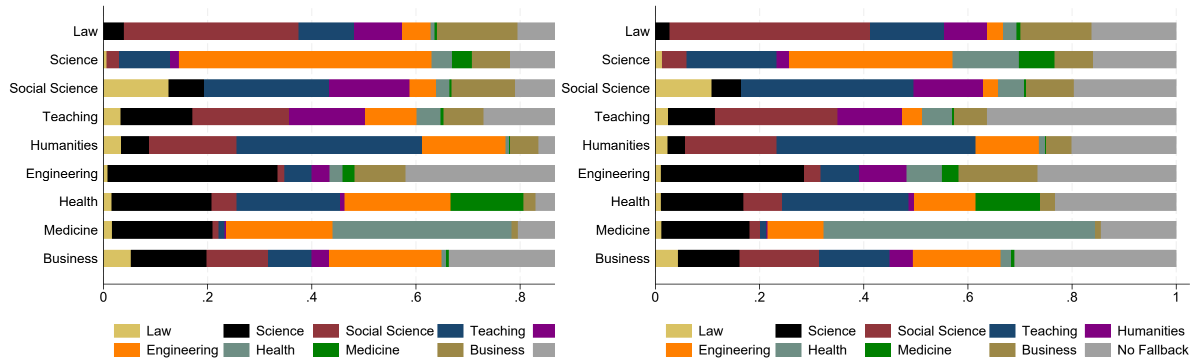
Figure A.2: Similarity and Potential Wage Indexes by Field of Study Across Cohorts



(a) Gender Similarity Index (b) Potential Wage Index

Notes: This figure presents trends in the similarity index and wage potential by field of study across cohorts, as in Sloane et al. (2021). Data comes from the largest household survey in Chile and is restricted to those with at least a bachelor's degree. Panel (a) plots the renormalized, inverse Duncan-Duncan index for different cohorts of Chilean college graduates. Panel (b) plots the potential wage index for different cohorts of Chilean college graduates.

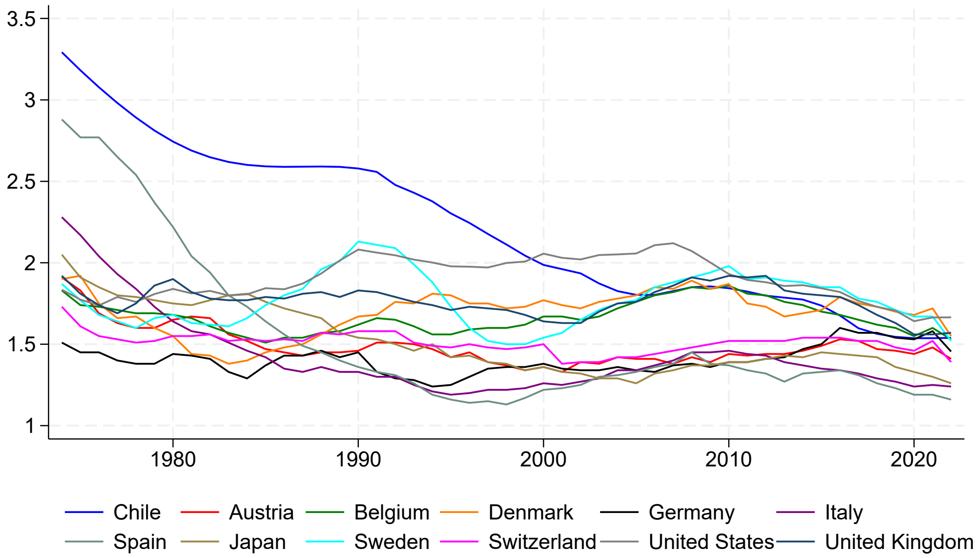
Figure A.3: Applicants' Fallbacks



(a) Male (b) Female

Notes: This figure shows students' fallback fields of study based on their ranked order lists. Among students ranking a given field as their first choice (y-axis), we compute the share of students ranking each of the remaining fields of study as a second option (i.e., fallback). Panels (a) and (b) show the relevance of each fallback for males and females, respectively. We consider all students who applied and were accepted through the centralized admission system between 2004 and 2007 and were employed in 2019.

Figure A.4: Total Fertility Rates Over Time



Notes: This figure shows the total fertility rates for the high-income countries considered in Doepke et al. (2023) and Chile (in blue). The data comes from the World Bank World Development Indicators database.

B Supplementary Results

This section provides additional results mentioned in the paper. First, we provide evidence that our approach is consistent with that of Kirkeboen et al. (2016). Second, we show results on the dynamics of the gender earnings gap through the lens of a decomposition between differences in returns and differences in match effects. Finally, we discuss a set of additional results that all point to qualitatively similar conclusions as the main results in the paper.

B.1 Relative Payoffs and Comparative Advantage:

Our findings complement and extend the evidence provided by Kirkeboen et al. (2016) on the importance of next-best alternatives and comparative advantage in the choice of college major.

Our approach resembles the fallback conditioning presented in Kirkeboen et al. (2016), as treatment effects vary according to the composition of a student's rank-ordered list, i.e., the payoff to a given field includes a match effect governed by the submitted rank-ordered list, including the fallback option and any subsequent fields a student ranks. This means that treatment effects for major j differ for individuals with different fallbacks; and even among individuals with similar fallbacks, additional differences in the composition of the rank-ordered list produces additional differences in treatment effects. Table B.1

shows the fallback-specific returns estimated by our model. Results are consistent with those of Kirkeboen et al. (2016) in that payoffs are highly heterogeneous depending on each student's fallback. For instance, the payoff to Science go from positive for those with next best alternatives in Social Science, Teaching or Humanities to negative for those with next best alternatives in Business or Medicine.

B.2 Additional Results

Private Sector Earnings Data: Appendix Table B.3 reports estimates using the private sector data discussed in Section 2. The overarching results are qualitatively similar to our main estimates. In this case, however, the return to Medicine is relatively smaller. This stems from the fact that many medical doctors are employed in the public sector in Chile, so estimates using private sector data will invariably miss a significant portion of their labor market. For this reason, we use the pension system records as our primary data.

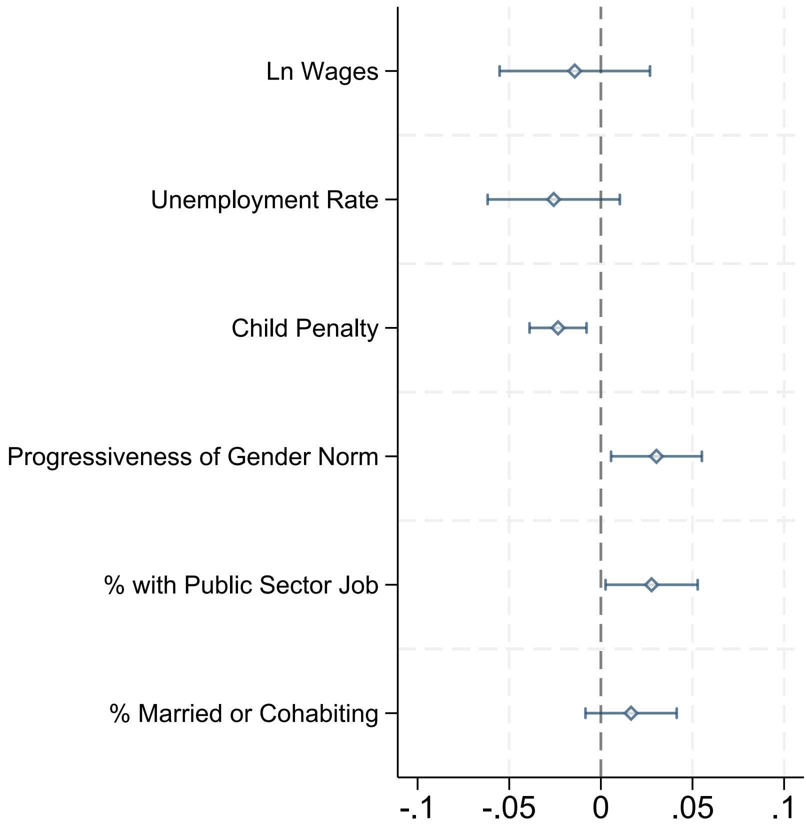
Total Earnings: Appendix Table B.2 reports estimates using the pension data but considering total yearly earnings instead of log earnings as the dependent variable. This allows us to include individuals not employed in the Chilean formal labor market in 2019. We find similar results when considering this outcome variable encompassing the intensive and extensive margins of the labor market. We find differences in returns across fields of study and gender. In line with our main results, men exhibit larger earnings in Science and Engineering, and women exhibit higher earnings in Teaching and Humanities, fields with relatively lower returns.

Alternative Non-Pecuniary Returns: The fertility results in the paper are on the extensive margin of having a child by a certain time after graduation. Appendix Table B.4 reports the estimates obtained when the outcome is the total number of children. This captures both extensive and intensive margin effects. Reassuringly, we find similar results when accounting for the intensive margin of fertility.

Fertility Correlations: We conjecture that majors impact fertility by allowing students to access jobs with different attributes and career opportunities. Our interpretation is consistent with the "new theories" underscoring that factors affecting the compatibility of women's careers and families are key drivers of fertility (Doepke et al., 2023). We present evidence supporting our hypothesis in Figure B.1, which reports the point estimates and confidence intervals obtained from separate regressions of fertility returns by field of study on job and field-specific attributes. For additional details, see Appendix C.

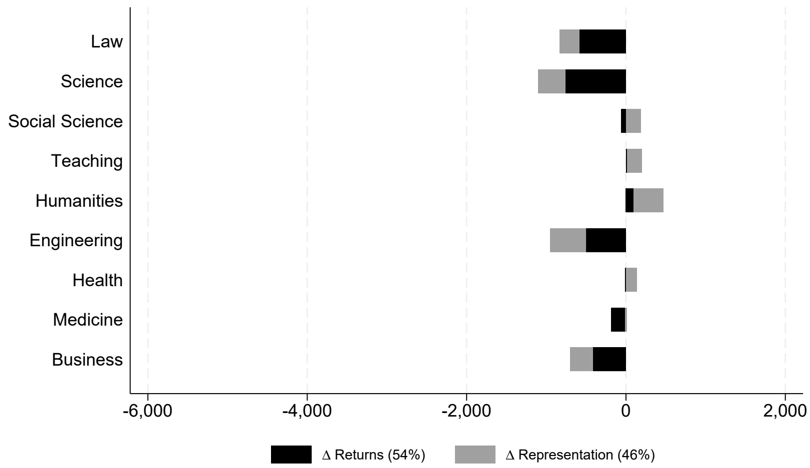
Counterfactual Preferences: Figure B.2 offers additional evidence on the impact of equalizing family considerations between genders. Based upon the decomposition presented in Equation (1), the Figure B.2 shows that equalizing preferences for family considerations significantly reduces the contribution of Engineering programs to the overall gender earnings gap while also diminishing the offsetting impact of Teaching and Health programs.

Figure B.1: Fertility Correlations

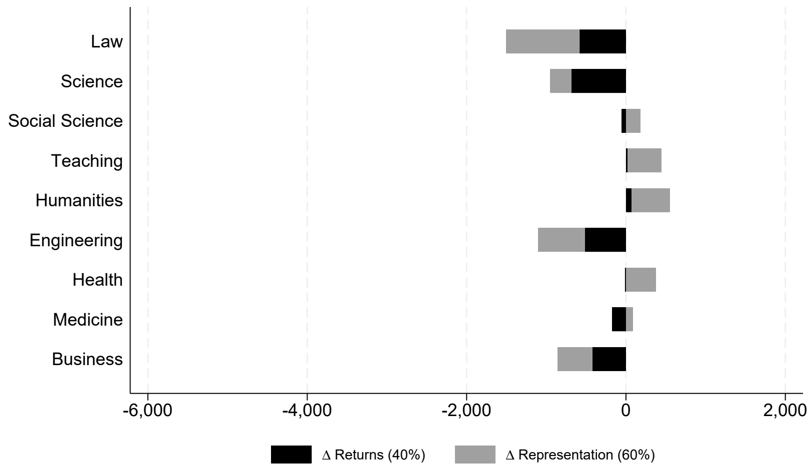


Notes: This figure reports the point estimates and confidence intervals obtained from separate regressions of fertility returns by field of study on job and field-specific attributes. All independent variables are standardized to have a mean of zero and a standard deviation of one. Confidence intervals are constructed using robust standard errors. For details on the construction of Child Penalties and Progressiveness of Gender Norm, see Appendix C.

Figure B.2: Baseline and Counterfactual Earnings Gap: Sorting versus Returns



(a) Baseline



(b) Counterfactual

Notes: These figures show the result from the exercise described by Equation (1) that decomposes the gender earnings gap into differences in sorting between fields of study and differences in returns within fields of study. We consider the annual earnings in 2019 of students who applied through the centralized admission system between 2004 and 2007, graduated between 2007 and 2019, and were employed in 2019. Panel (a) plots the results from the decomposition when we assign male preferences for family considerations to all students. Panel (b) plots the results from the decomposition when we assign female preferences for family considerations to all students. The predictions use estimates reported in Table 5, discussed in the main text.

Table B.1: Estimates of Relative Payoffs

Completed field (j):	Next best alternative (k):								
	Law	Science	Social Science	Teaching	Humanities	Engineering	Health	Medicine	Business
Law	.	0.15	0.35	0.21	0.60	0.04	0.20	-0.12	-0.03
Science	-0.01	.	0.24	0.10	0.47	-0.06	0.07	-0.23	-0.12
Social Science	-0.22	-0.12	.	-0.10	0.27	-0.28	-0.09	-0.44	-0.34
Teaching	-0.11	-0.03	0.17	.	0.39	-0.17	0.02	-0.31	-0.23
Humanities	-0.41	-0.38	-0.14	-0.28	.	-0.48	-0.27	.	-0.53
Engineering	0.07	0.11	0.36	0.22	0.58	.	0.20	-0.16	-0.03
Health	-0.06	0.03	0.21	0.07	0.41	-0.11	.	-0.27	-0.19
Medicine	0.21	0.27	0.46	0.34	0.86	0.15	0.32	.	0.09
Business	0.14	0.21	0.42	0.28	0.64	0.09	0.27	-0.03	.

Notes: In the spirit of Kirkeboen et al. (2016), this table presents the matrix of the payoffs to field j as compared to k (for those who prefer j and have k as next-preferred field) implied by our model. The rows represent completed fields and the columns represent next-ranked fields.

Table B.2: Gender Differences in Returns and Match Effects: Total Earnings

	Law (1)	Science (2)	Social Science (3)	Teaching (4)	Humanities (5)	Engineering (6)	Health (7)	Medicine (8)	Business (9)
					Panel A: Female				
Returns	7,079	4,261	3,212	7,245	-2,402	10,040	7,442	17,444	12,056
	(1,185)	(961)	(336)	(148)	(558)	(660)	(286)	(956)	(483)
Selection on Gains	1,418	197	539	-196	1,245	486	1,008	40	1,002
	(417)	(480)	(188)	(115)	(235)	(362)	(172)	(415)	(234)
					Panel B: Male				
Returns	6,042	9,170	2,507	6,451	-2,739	14,226	7,013	21,592	14,093
	(1,368)	(698)	(538)	(258)	(671)	(374)	(604)	(1,200)	(542)
Selection on Gains	2,450	1,128	1,289	625	1,548	744	1,221	38	1,396
	(482)	(413)	(253)	(155)	(253)	(325)	(264)	(478)	(284)
Est. Females = Est. Males									
Δ Returns (p-val)	0.566	0.000	0.262	0.005	0.696	0.000	0.511	0.007	0.005
Δ Match Effects (p-val)	0.105	0.141	0.017	0.000	0.379	0.596	0.499	0.997	0.285

Notes: This table presents the estimates obtained from our main regression model, presented in Equation (9). We consider the sum of earnings in 2019 (in USD) instead of the log of earnings to account for the extensive margin. We restrict the sample to students who applied and were accepted through the centralized admission system between 2004 and 2005 and were employed in 2019. The sample includes 137,339 observations. The final two rows of the table report p-values from two additional statistical tests. The Δ Returns p-values correspond to the null hypothesis that the Female and Male returns are equivalent for a given column. The Δ Match Effects p-values correspond to the null hypothesis that the Female and Male match effects are equivalent for a given column. All specifications include quadratic polynomials of the college admission scores in mathematics and language, and control for level-effects as defined in the main text, cohort fixed effects and cell fixed effects, where a cell is defined by school type, macro-region, financial-aid relevant score ranges, and gender. For ease of interpretation, $\hat{\lambda}_{ij}$ is standardized within each cell. The R^2 of this regression is 0.18.

Table B.3: Gender Differences in Returns and Match Effects: Private Sector Data

	Law (1)	Science (2)	Social Science (3)	Teaching (4)	Humanities (5)	Engineering (6)	Health (7)	Medicine (8)	Business (9)
					Panel A: Female				
Returns	0.391	0.376	0.222	0.284	-0.023	0.517	0.246	0.429	0.525
	(0.071)	(0.045)	(0.020)	(0.011)	(0.047)	(0.022)	(0.023)	(0.054)	(0.018)
Selection on Gains	0.058	0.009	0.039	0.021	0.069	0.016	0.065	-0.013	0.019
	(0.023)	(0.022)	(0.011)	(0.008)	(0.019)	(0.013)	(0.013)	(0.023)	(0.009)
					Panel B: Male				
Returns	0.434	0.531	0.192	0.193	-0.124	0.614	0.278	0.428	0.580
	(0.092)	(0.023)	(0.036)	(0.021)	(0.065)	(0.013)	(0.049)	(0.069)	(0.019)
Selection on Gains	0.105	0.036	0.060	0.066	0.087	0.004	0.066	0.032	0.052
	(0.030)	(0.014)	(0.016)	(0.011)	(0.023)	(0.011)	(0.019)	(0.027)	(0.010)
Est. Females = Est. Males									
Δ Returns (p-val)	0.710	0.002	0.459	0.000	0.203	0.000	0.551	0.983	0.025
Δ Match Effects (p-val)	0.224	0.307	0.289	0.001	0.532	0.489	0.942	0.203	0.014

Notes: This table presents the estimates obtained from our main regression model, presented in Equation (9). We consider the earnings of workers in the private sector. The sample includes 78,336 observations of students who applied and were accepted through the centralized admission system between 2004 and 2007 and were employed in the private sector in 2019. The final two rows of the table report p-values from two additional statistical tests. The Δ Returns p-values correspond to the null hypothesis that the Female and Male returns are equivalent for a given column. The Δ Match Effects p-values correspond to the null hypothesis that the Female and Male match effects are equivalent for a given column. All specifications include quadratic polynomials of the college admission scores in mathematics and language, and control for level-effects as defined in the main text, cohort fixed effects and cell fixed effects, where a cell is defined by school type, macro-region, financial-aid relevant score ranges, and gender. For ease of interpretation, $\hat{\lambda}_{ij}$ is standardized within each cell. The R^2 of this regression is 0.25.

Table B.4: Fertility: Number of children 13 years after college application

	Law (1)	Science (2)	Social Science (3)	Teaching (4)	Humanities (5)	Engineering (6)	Health (7)	Medicine (8)	Business (9)
					Panel A: Female				
Returns	-0.184	-0.239	-0.072	0.119	-0.151	-0.063	0.087	-0.006	-0.005
	(0.050)	(0.040)	(0.022)	(0.015)	(0.040)	(0.028)	(0.019)	(0.033)	(0.023)
Selection on Gains	0.060	0.001	-0.006	-0.020	0.022	-0.016	-0.010	0.018	0.012
	(0.018)	(0.020)	(0.012)	(0.011)	(0.016)	(0.016)	(0.011)	(0.015)	(0.011)
					Panel B: Male				
Returns	-0.241	-0.345	-0.277	-0.136	-0.436	-0.126	-0.142	-0.144	-0.122
	(0.049)	(0.023)	(0.030)	(0.022)	(0.036)	(0.015)	(0.031)	(0.037)	(0.021)
Selection on Gains	0.058	0.043	0.012	-0.003	0.047	-0.008	0.020	0.046	-0.018
	(0.018)	(0.014)	(0.014)	(0.012)	(0.014)	(0.013)	(0.013)	(0.015)	(0.011)
Est. Females = Est. Males									
Δ Returns (p-val)	0.415	0.018	0.000	0.000	0.000	0.040	0.000	0.004	0.000
Δ Match Effects (p-val)	0.929	0.075	0.338	0.294	0.239	0.687	0.091	0.177	0.059

Notes: This table presents the estimates obtained from our main regression model, presented in Equation (9). The sample includes 137,339 observations of students who applied and were accepted through the centralized admission system between 2004 and 2007. The final two rows of the table report p-values from two additional statistical tests. The Δ Returns p-values correspond to the null hypothesis that the Female and Male returns are equivalent for a given column. The Δ Match Effects p-values correspond to the null hypothesis that the Female and Male match effects are equivalent for a given column. All specifications include quadratic polynomials of the college admission scores in mathematics and language, and control for level-effects as defined in the main text, cohort fixed effects and cell fixed effects, where a cell is defined by school type, macro-region, financial-aid relevant score ranges, and gender. For ease of interpretation, $\hat{\lambda}_{ij}$ is standardized within each cell. The R^2 of this regression is 0.05.

C Child Penalties and Gender Norms

This section details how we leverage the administrative data to estimate the child penalties and gender norms measures discussed in Section 4 of the main body of the paper.

C.1 Child Penalties

Following Kleven et al. (2019b), we estimate child penalties with the model:

$$\tilde{Y}_{it} = \sum_{k \neq -2} \beta_j D_{it}^k + \mu_a + \gamma_t + \nu_{it}, \tag{16}$$

where $D_{it}^k \equiv \mathbb{1}[t = c + k]$ is a binary variable that indicates the time relative to the period in which the first child was born c. μ_a are age-fixed effects that account for the life cycle effects on the outcome variable, and γ_t are time-fixed effects that control for temporal trends at the 6-month frequency. We omit the time binary variable corresponding to $t = -2$ since, in our case, it represents the period of child conception. Our primary outcome variable is labor market earnings in period t relative to the period of child conception t^*, i.e., $\tilde{Y}_{it} = Y_{it}/Y_{it^*}$.

We estimate the impacts of children on women and men separately and define the child penalty at event time t as $\hat{\beta}_t^m - \hat{\beta}_t^w$, which measures the percentage by which women fall behind men due to children. Figure C.1 shows the effects of parenthood on earnings across fields of study. To use a symmetric window, and since we have earnings starting only in 2015, we focus on college graduates whose first child was born in 2016 or 2017. This figure also reports the estimated child penalty at time $t = 4$. We have a more limited window of coverage compared to existing studies (Kleven et al., 2019a) and can estimate child penalties only up to 4 years after the birth of a child, so our estimates are relatively short-run. Nonetheless, most evidence shows a somewhat immediate drop in mothers' earnings, which changes minimally over time.

Table C.1 complements previous evidence and reports the estimated child penalty before child conception, at time $t = -4$, and after it, at time $t = 4$; and Appendix Figure C.2 reports child penalty estimates for each field of study. There is a vast amount of heterogeneity, with Science and Social Science exhibiting the largest penalties (36% and 32% percent, respectively) and Business and Medicine exhibiting the lowest child penalties. The average penalty across majors is a 16% reduction in earnings for women relative to men after the birth of their first child, with a noise-adjusted standard deviation of 10%. Although the heterogeneity we document alludes to an empirical interaction between major choice, gender, earnings, and fertility, the evidence reported in Figure C.2 is not causal.[27]

[27] An econometric concern is that we observe child penalties for a selected sample of parents who choose to have children earlier. Melentyeva and Riedel (2023) finds that age heterogeneity matters but

Another explanation is that some majors lead to careers in industries or occupations with more flexible work arrangements, which could help mitigate or amplify the child penalty. Additionally, as suggested by the evidence in Table 5, individuals who prioritize work-life balance and anticipate having children may choose majors perceived as more family-friendly. However, certain industries—often aligned with specific majors—may enforce stronger gender norms or biases. In traditionally male-dominated fields like Engineering or Science, the child penalty might be more or less pronounced depending on implicit or explicit biases against women taking maternity leave or reducing work hours.

C.2 Gender Norms

We follow the pioneering work by Bertrand et al. (2015) and construct a measure of major-specific gender norms associated with the distribution of relative labor earnings within households. We focus on graduates who had their first child after they obtained their college degree and on their partners (i.e., a couple). For each couple of parents, we use their labor market earnings from the third and fourth quarter before childbirth and focus on couples where both members earn positive income and are between 20 and 40 years old. We define $Relative\ Earnings_i$ as $\frac{Woman\ Earnings_i}{Woman\ Earnings_i + Man\ Earnings_i}$, where i indexes the couple, and $Woman\ Earnings_i$ and $Male\ Earnings_i$ are the total labor income of the will-be mothers and fathers, respectively.

Our proxy for the progressivity of gender norms is measured by the size of the cliff of $Relative\ Earnings_i$ at 0.5. To gauge the magnitude of the drop at the point where the female starts to earn more than the male, we calculate the fraction of couples in each of twenty 0.05 relative income bins and project it on a discontinuity indicator while controlling for quadratic polynomials at each side of the 0.5 cutoff. Table C.2 presents the OLS estimates associated with the discontinuity indicators obtained from separate regressions for each of our nine fields of study. Panel (a) presents the results when we focus on female graduates (and their partners), and Panel (b) repeats the exercise but focuses on male graduates (and their partners).[28]

Figure C.3 depicts, for each of the nine fields of study, the frequency distribution of relative earnings grouped in 20 bins, along with a lowess (locally weighted scatterplot smoothing) estimate of the distribution on each side of 0.5, the point where the woman starts to earn more than the man. In most fields of study, the distribution of relative earnings exhibits a sharp drop at 0.5, with the largest drops in Law, Science, Engineering,

that child penalties are largest for younger mothers as they are more likely to disconnect from the labor market in years with steep career growth.

[28] Overall, we find similar variation in gender norms across fields of study, independently of whether we focus on male or female graduates (and their partners). Two exceptions are Humanities and Medicine, which show smaller and larger drops (at the point where the female starts to earn more than the male), respectively, when the graduate is male.

Medicine, and Business and the smallest in Social Science, Teaching, Humanities, and Health.

What drives the variation in the estimated cliffs at 0.5? Besides gender norms, a few other features could contribute to the variation in the estimated cliffs. First, majors offering students a menu of jobs with more flexible and less non-linear pay schedules allow for choices that generate a smoother within-household earnings distribution (Goldin, 2024). From that perspective, the estimated cliffs are not a product of norms but a consequence of differences in major-specific access to jobs. To partly gauge the extent of that, we corroborate our norm estimates in publicly available survey data, where we can further probe the sensitivity of the estimates to differences in labor hours. As shown by Figure C.4, we find broadly similar norm estimates among the subset of couples with full-time jobs whose hours worked per week are 40 or more.[29] Second, some majors have closer ties to the public sector, and variation in public sector access could contribute to variation in the cliffs. We do not find evidence of this as Health, Medicine, and Teaching all provide strong access to the public sector in Chile. Third, some scholars have noted that cliff estimates may be sensitive to bunching at 0.5, casting doubt on its interpretation as variation in gender norms, especially when using survey data (Binder and Lam, 2022; Hederos and Stenberg, 2022; Zinovyeva and Tverdostup, 2021). In our *administrative* records, however, few couples (7%) have equal earnings. Figure C.5 further investigates the potential impact of bunching on interpreting differences in cliffs as variations in gender norms. Panel (a) presents the cliffs when pooling data across fields of study, while Panel (b) shows the cliffs after removing cases where parents have equal earnings. Reassuringly, the cliffs are similar across panels.

We, therefore, argue that the variation in the estimated cliffs can be interpreted as variation in breadwinner norms across fields of study.

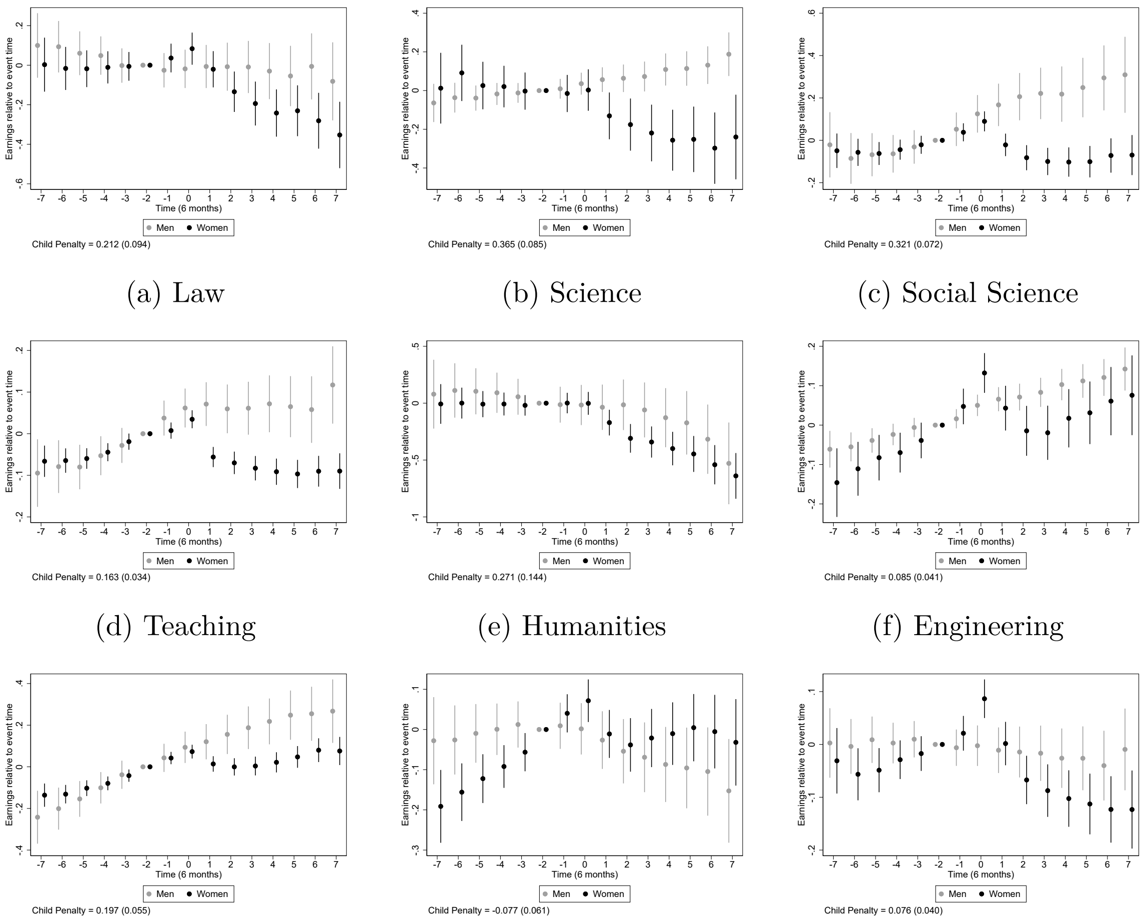
[29] The patterns in the estimated cliffs using survey data remain broadly the same. The CASEN survey groups Health and Medicine into a single field of study, so we cannot estimate the cliffs separately for Health and Medicine.

Table C.1: Child Penalties

	Law (1)	Science (2)	Social Science (3)	Teaching (4)	Humanities (5)	Engineering (6)	Health (7)	Medicine (8)	Business (9)
Female - Male Gap Before ($t = -4$)	-0.060	0.038	0.020	0.008	-0.099	-0.046	0.021	-0.093	-0.032
	(0.064)	(0.058)	(0.049)	(0.023)	(0.098)	(0.028)	(0.038)	(0.042)	(0.027)
Female - Male Gap After ($t = 4$)	-0.212	-0.365	-0.321	-0.163	-0.271	-0.085	-0.197	0.077	-0.076
	(0.094)	(0.085)	(0.072)	(0.034)	(0.144)	(0.041)	(0.055)	(0.061)	(0.040)
Number of observations	24,155	24,436	60,385	131,242	19,859	95,012	71,754	27,935	80,003

Notes: This table presents estimates obtained from estimating equation 16. Specifically, it shows the estimated child penalty (and its standard error) before child conception, at time $t = -4$, and after it, at time $t = 4$.

Figure C.1: Child Penalty by Field of Study



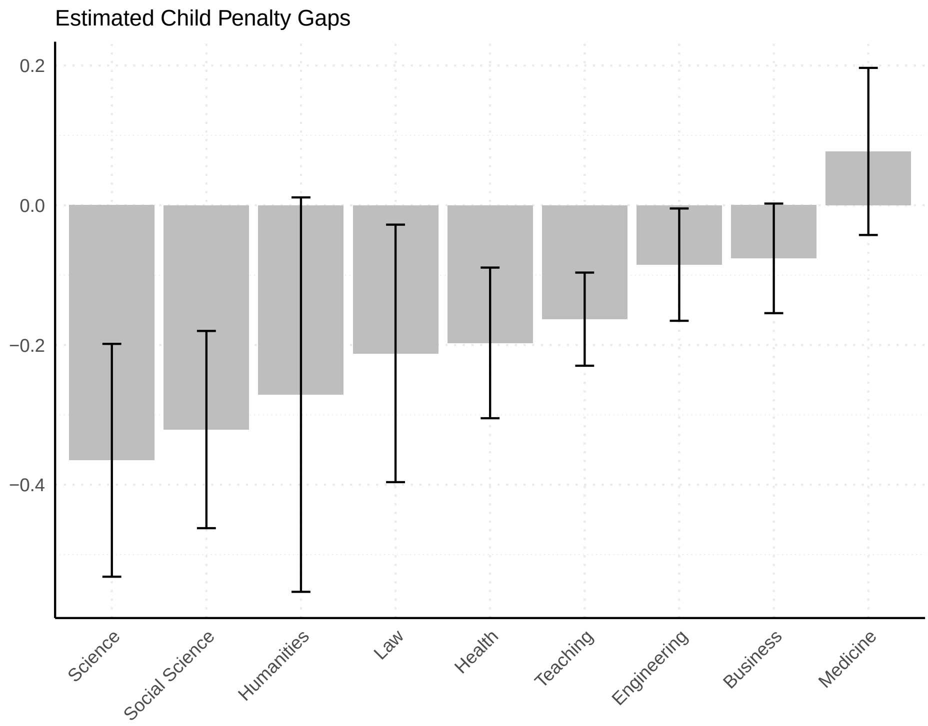
(a) Law (b) Science (c) Social Science

(d) Teaching (e) Humanities (f) Engineering

(g) Health (h) Medicine (i) Business

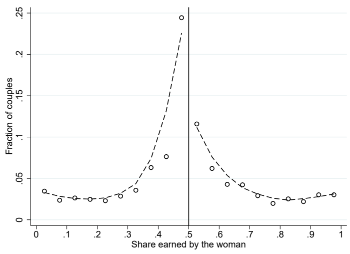
Notes: These figures show the percentage effects of parenthood on earnings across event time for men and women. Each panel corresponds to one of the nine fields of study considered in our analysis. We consider college graduates who had their first child between 2016 and 2017 and after their graduation. The figure also displays the average child penalty at the end of our time window (4 years after childbirth). Earnings are not conditional on employment status, and the effects include extensive and intensive margins.

Figure C.2: Child Penalty Gaps Across Fields of Study

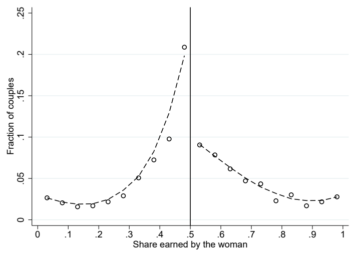


Notes: This figure shows the estimated child penalty gap four years after birth (and the corresponding confidence interval). We consider college graduates who had their first child between 2016 and 2017 and after graduation. Earnings are not conditional on employment status, and the effects include extensive and intensive margins. Estimation follows Kleven et al. (2019b) and for additional details, see Appendix C.

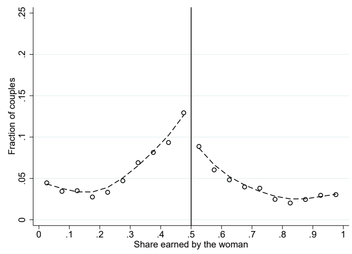
Figure C.3: Gender Norms by Field of Study



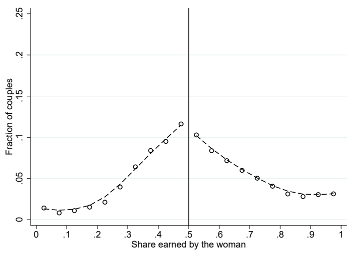
(a) Law



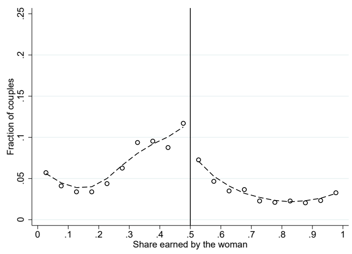
(b) Science



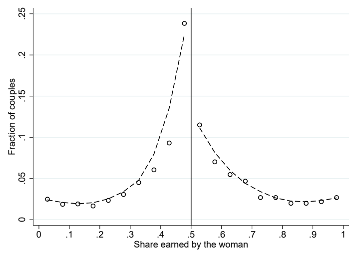
(c) Social Science



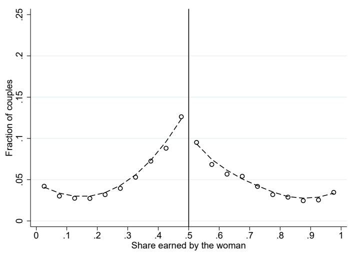
(d) Teaching



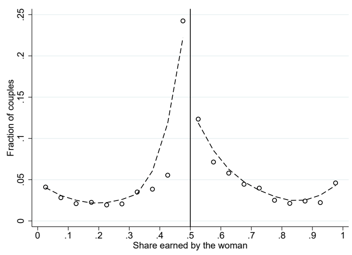
(e) Humanities



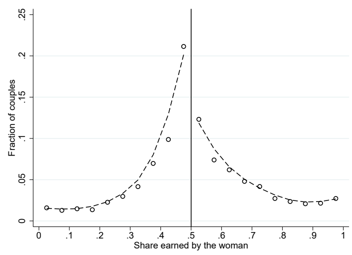
(f) Engineering



(g) Health



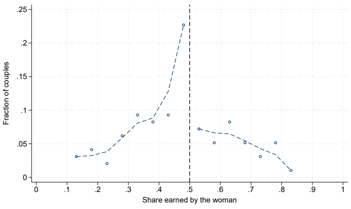
(h) Medicine



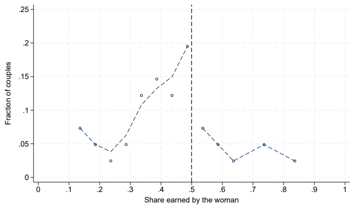
(i) Business

Notes: These figures report the relative income distribution for females who graduated from different fields of study. The sample includes couples of parents who both earn positive income. We use the observation from the third and fourth quarters before childbirth. Each dot is the fraction of couples in a 0.05 relative income bin. The vertical line indicates the relative income share=0.5. The dashed line is the lowess smoother applied to the distribution, allowing for a break at 0.5.

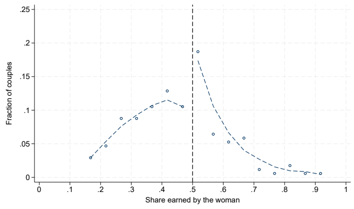
Figure C.4: Gender Norms by Field of Study: Full-time workers (CASEN survey)



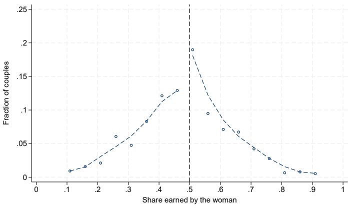
(a) Law



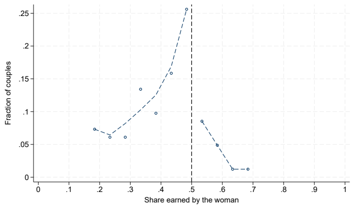
(b) Science



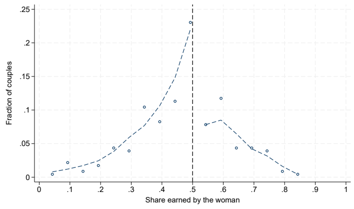
(c) Social Science



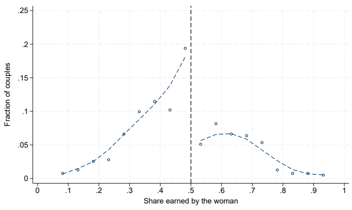
(d) Teaching



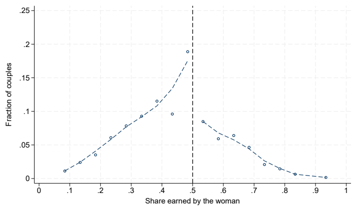
(e) Humanities



(f) Engineering



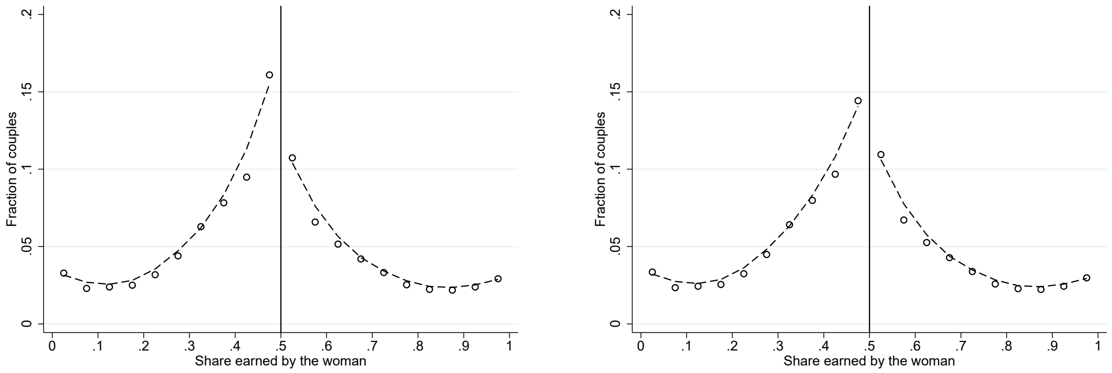
(g) Health and Medicine



(h) Business

Notes: These figures report the distribution of relative income separately for females who graduated from different fields of study. The sample includes couples in the household survey CASEN, where both earn positive income and have full-time jobs (work more than 40 hours a week). Each dot is the fraction of couples in a 0.05 relative income bin. The vertical line indicates the relative income share=0.5. The dashed line is the lowess smoother applied to the distribution, allowing for a break at 0.5. Since the survey records do not separate Medicine from Health, both fields are embedded into the Health figure.

Figure C.5: Gender Norms: Removing bunching at 0.5 (pooled)



(a) All (b) No Bunching

Notes: These figures report the distribution of relative income. The sample includes couples of parents who both earn positive income. Panels (a) consider all parents, while Panel (b) removes cases in which parents report the same income. We use the observation from the third and fourth quarters before childbirth. Each dot is the fraction of couples in a 0.05 relative income bin. The vertical line indicates the relative income share=0.5. The dashed line is the lowess smoother applied to the distribution, allowing for a break at 0.5.

Table C.2: Gender Norms

	Law	Science	Social Science	Teaching	Humanities	Engineering	Health	Medicine	Business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel (a): Female graduates (and their partners)								
Break	-0.104	-0.095	-0.048	-0.023	-0.054	-0.099	-0.036	-0.080	-0.080
	(0.034)	(0.019)	(0.007)	(0.006)	(0.014)	(0.028)	(0.005)	(0.039)	(0.021)
	Panel (b): Male graduates (and their partners)								
Break	-0.109	-0.046	-0.033	-0.019	-0.012	-0.078	-0.008	-0.143	-0.111
	(0.019)	(0.013)	(0.011)	(0.011)	(0.012)	(0.011)	(0.010)	(0.026)	(0.019)

Notes: This table reports the estimated discontinuity in the distribution of relative earnings of individuals from different fields of study. The dependent variable is the fraction of couples. We calculate the fraction of couples in each of the twenty 0.05 relative income bins and project it on a discontinuity indicator while controlling for a quadratic polynomial at each side of the 0.5 cutoff. This table presents the OLS estimates of the discontinuity indicators obtained from separate regressions for each field of study. Panel (a) considers mothers who graduated from a field of study and their couples, and Panel (b) considers fathers who graduated from a field of study and their couples. We use the observations from the third and fourth quarters before childbirth and focus on couples where both members earn positive income.

D Counterfactual Details

This section reports additional details related to the counterfactual policies we analyze in Section 5. We discuss our implementation of the deferred acceptance algorithm, offer details on the machine learning model used to predict graduation probabilities, describe the bootstrap procedure used in our counterfactual analysis, and the conclusion provides additional details underpinning the main counterfactual results in the main paper.

D.1 Deferred Acceptance Algorithm

We use the following algorithm to produce an outcome of the student-proposing Deferred Acceptance algorithm.

Step 1) Each student proposes to her first choice. Each program tentatively assigns seats to its proposers one at a time, following their priority order based on the weighted score. Students are rejected if no seats are available at the time of consideration.

In general, in

Step k) Each student who was rejected in the previous step proposes to her next best choice. Each program considers the students it has tentatively assigned together with its new proposers and tentatively assigns its seats to these students one at a time following the program's priority order based on the weighted score. The student is rejected if no seats are available when she is considered.

The algorithm terminates either when there are no new proposals or when all rejected students have exhausted their rank order list of preferences. When introducing quotas, we apply a precedence order by first processing the open seats and then the gender-specific seats. Following Dur et al. (2018), this order of precedence maximizes the impact of quotas.

D.2 Prediction Accuracy and Summary Statistics

We use statistical learning methods to predict graduation between the time of being accepted and up to thirteen years after application. In particular, we train a Gradient Boosting Model (GBM) to generate predictions. Predictors include the income quintile of the student at the time when they applied to college, type of high school and health insurance, PSU scores (Math, Language, History, and Science), parent's educational attainment, indicators for applying to different universities and majors, indicators for acceptance at different universities, preferences, and the control functions from the choice model.

We train a separate GBM for each field to generate an out-of-sample predicted graduation probability for each field for each student. We make out-of-sample predictions for all students in the admissions process in 2007. We use applicants from the years 2008 and 2010 for fitting and out-of-sample tuning. Chilean students can apply to college for multiple years, and as a consequence, applicants in the year 2007 (our validation year) might appear in our training sample for the years 2008-2010. To avoid this, we restrict our sample so that students appear only in the last year they applied to college. To improve model performance, we fine-tune the parameters of each GBM (i.e., number of boosting rounds, maximum tree depth, and learning rate) for each field separately using 5-fold cross-validation and a grid search on the training sample. Specifically, we identify the combination of parameters that minimizes the Brier score averaged across the 5 cross-validation iterations.

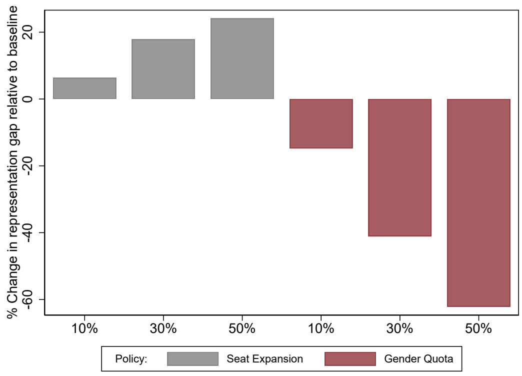
Appendix Figure D.2 reports forecast bias assessments. The forecast coefficients are statistically equal to one for fields of study that are heavily impacted in the counterfactual analysis. The model tends to underpredict graduation probabilities in Teaching, Humanities, and Health. These three fields account for less than seven percent of the roughly four percent of women affected in the counterfactuals and around eight percent of the five percent of affected men. Importantly, the graduation probabilities are forecast unbiased for Engineering, Business and Science, the three most important fields for comparison across counterfactual scenarios.

D.3 Causal Graduation Probabilities

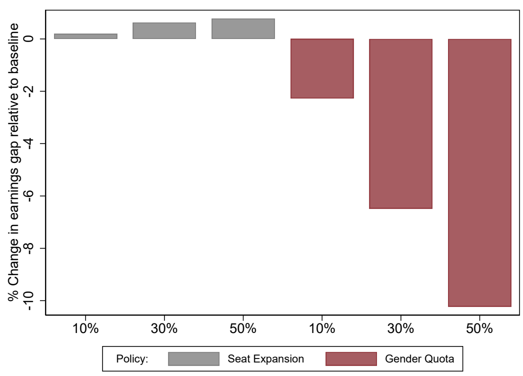
We use machine learning methods to predict graduation probabilities in the main body of the paper, but an alternative approach is to estimate a causal model of graduation. In this section, we assess the sensitivity of our main counterfactual results to the use of graduation probabilities estimated from a causal model. Specifically, we estimate graduation probabilities using Equation (9) in the main body of the paper (i.e., using graduation as an outcome and assignment as the treatment).

Appendix Figure D.1 reports estimates analogous to Figure 5. Reassuringly, the results are qualitatively and quantitatively similar when using graduation probabilities estimated in the causal model.

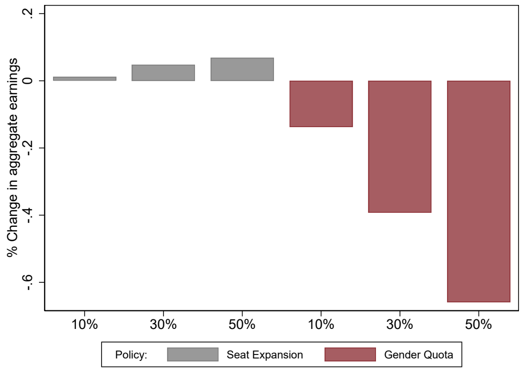
Figure D.1: Counterfactual Policies



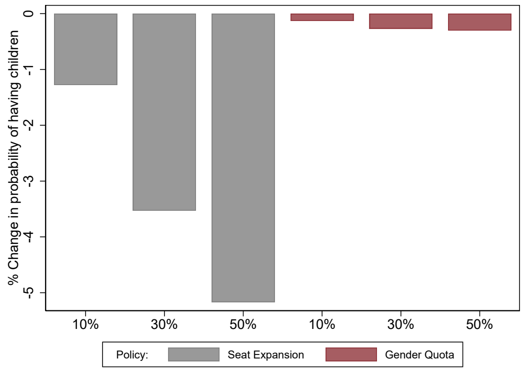
(a) Representation Gap in Engineering



(b) Total Earnings Gap



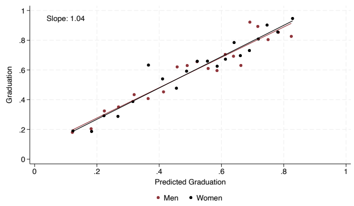
(c) Total Earnings



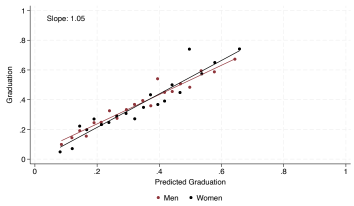
(d) Total Fertility

Notes: These figures report the changes (relative to baseline) implied by different counterfactual scenarios using the predicted graduation obtained from our causal model. We consider 10, 30, and 50% seat expansions in Engineering programs (gray bars) and a 10, 30, and 50% quota for women in Engineering (red bars). Panel (a) shows the impacts on the Engineering representation gap. Panel (b) reports the impacts on the total gender earnings gap. Panels (c) and (d) show the total impacts on earnings and fertility. The sample includes the 87,599 students who applied through the centralized admission system in 2007.

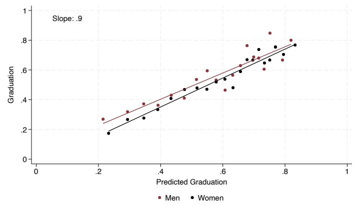
Figure D.2: Predictive Accuracy



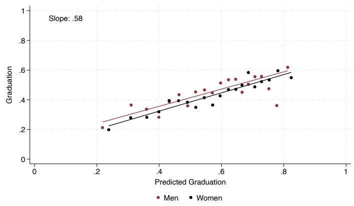
(a) Law



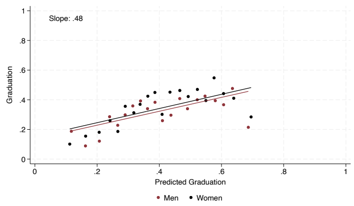
(b) Science



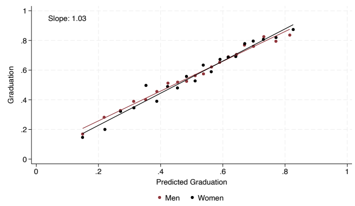
(c) Social Science



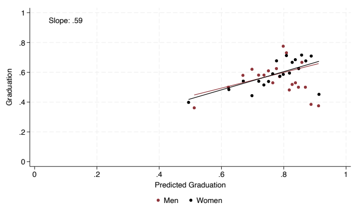
(d) Teaching



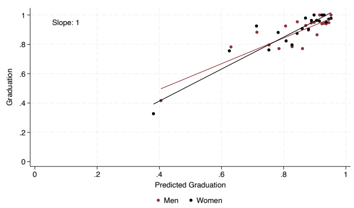
(e) Humanities



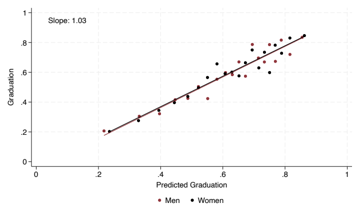
(f) Engineering



(g) Health



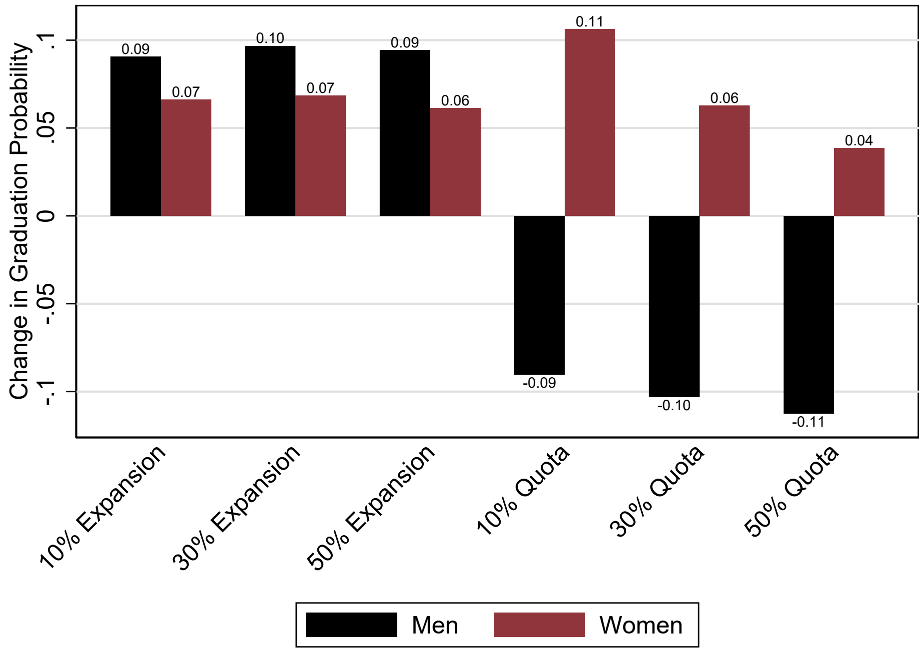
(h) Medicine



(i) Business

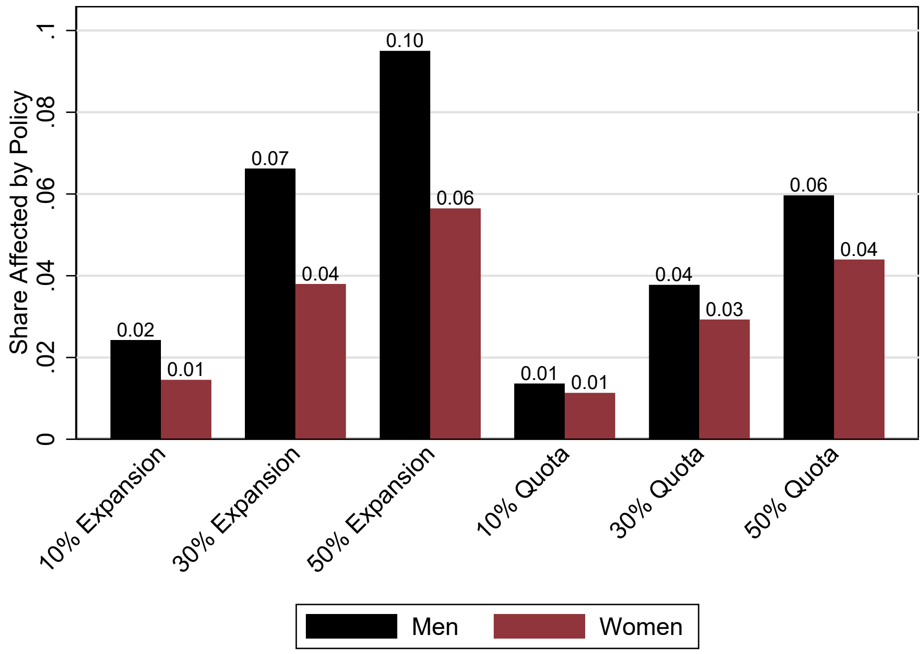
Notes: These figures report binscatter plots of graduation on out-of-sample predicted graduation rates separately for each field of study among accepted applicants in 2007.

Figure D.3: Change in Graduation Probabilities



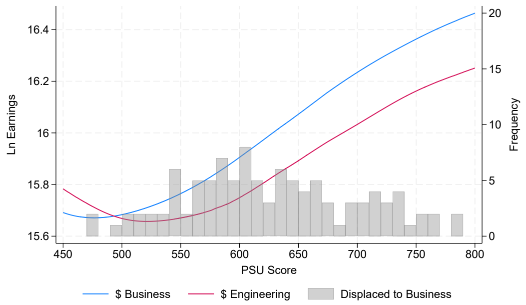
Notes: This figure shows the change in graduation probabilities—as predicted by our Gradient Boosting Model—for all male and female applicants who are assigned in each counterfactual scenario.

Figure D.4: Share Affected by Each Policy

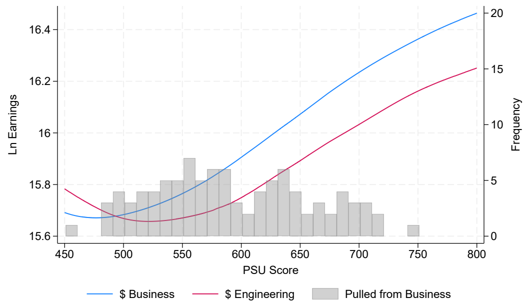


Notes: This figure shows the percentage of male and female applicants directly or indirectly affected in each counterfactual scenario.

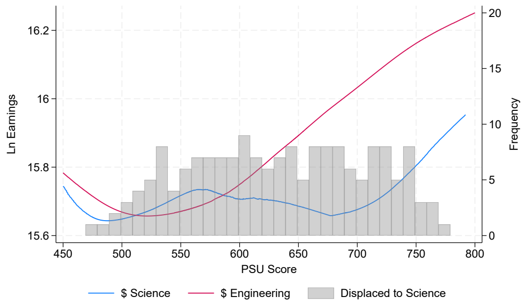
Figure D.5: Displaced Students



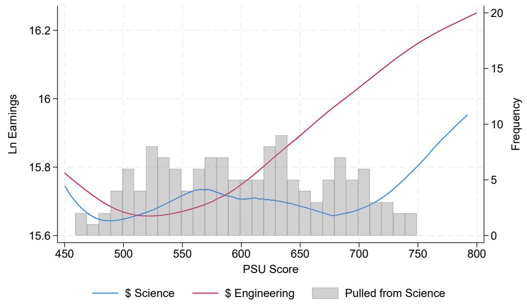
(a) Pushed to Business from Engineering



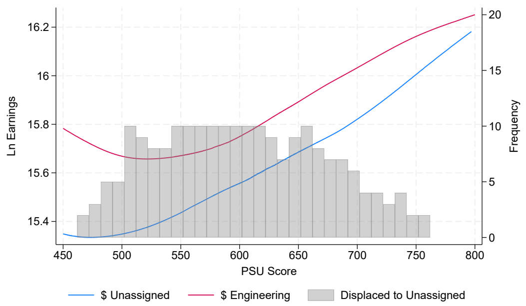
(b) Pulled from Business into Engineering



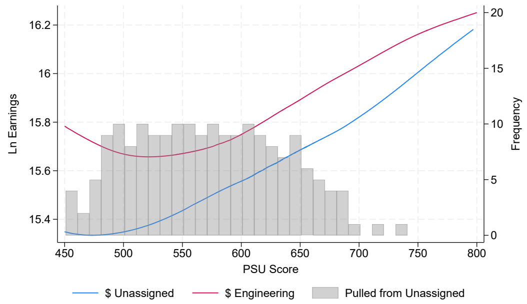
(c) Pushed to Science from Engineering



(d) Pulled from Science into Engineering



(e) Pushed to Unassigned from Engineering



(f) Pulled from Unassigned into Engineering

Notes: These figures present log earnings as a function of test scores for graduates from different fields of study. Gray bars show the test score distribution of students reshuffled across fields under the counterfactual quota policy of 50% in Engineering. We focus on students who were either “displaced to” or “pulled from” the three main fallback options for Engineering: Business, Science, and Unassigned.

Table D.1: Origin of Affected Students: 50% Engineering Seat Expansion

Fields	Women		Men	
	Percent among women moved into Engineering	Percent among all women	Percent among men moved into Engineering	Percent among all men
	(1)	(2)	(3)	(4)
	Panel (a): Effect in Engineering			
Law	0.17	0.01	0.09	0.01
Science	12.16	0.45	16.24	1.24
Social Science	1.51	0.06	0.84	0.06
Teaching	5.20	0.19	2.61	0.20
Humanities	5.54	0.21	2.84	0.22
Health	1.68	0.06	0.60	0.05
Medicine	0.00	0.00	0.00	0.00
Business	12.25	0.46	6.79	0.52
Unassigned	55.45	2.07	64.63	4.94
All affected Engineering	100.00	3.73	100.00	7.64
	Panel (b): Effect in Other Fields			
All affected other fields	-	1.92	-	1.86

Notes: This table presents a breakdown of changes in assignment and returns for students whose field of study was affected by a 50% seat expansion in Engineering relative to baseline. Panel (a) presents the impact on Engineering. Columns (1) and (2) present the impact for women whose counterfactual assignment is Engineering. Column (1) shows the percentage of those affected (by baseline field of study) out of all women whose counterfactual field is Engineering. Column (2) shows the percentage of all women. Columns (3) and (4) present the analogous figures for men. Panel (b) summarizes the impact on women and men who experienced a reallocation in the counterfactual scenario but were neither pushed into nor displaced from Engineering.

Table D.2: Origin and Destination of Affected Students: 50% Engineering Quota

	Women		Men	
Fields	Percent among women moved into Engineering	Percent among all women	Percent among men displaced from Engineering	Percent among all men
	(1)	(2)	(3)	(4)
	Panel (a): Effect in Engineering			
Law	0.24	0.01	0.27	0.01
Science	14.98	0.58	26.29	1.37
Social Science	1.20	0.05	1.22	0.06
Teaching	4.78	0.19	3.33	0.17
Humanities	5.10	0.20	3.26	0.17
Health	1.75	0.07	2.72	0.14
Medicine	0.08	0.00	0.07	0.00
Business	13.78	0.54	10.94	0.57
Unassigned	57.45	2.24	49.18	2.57
All affected Engineering	100.00	3.90	100.00	5.23
	Panel (b): Effect in Other Fields			
All affected other fields	-	0.49	-	0.74

Notes: This table presents a breakdown of changes in assignment and returns for students whose field of study was affected by a 50% female quota in Engineering relative to baseline. Panel (a) presents the impact on Engineering. Columns (1) and (2) present the impact for women whose counterfactual assignment is Engineering. Column (1) shows the percentage of those affected (by baseline field of study) out of all women whose counterfactual field is Engineering. Column (2) shows the percentage of all women. Columns (3) and (4) present the analogous for men displaced from Engineering into other fields of study (each row). Panel (b) summarizes the impact on women and men who experienced a reallocation in the counterfactual scenario but were neither pushed into nor displaced from Engineering.

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