Learning in Isolation: the Human and Social Capital Effects of Targeted Schooling Systems

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April 2, 2026

ABSTRACT. Schools shape both what students know and whom they know. We study a system of high quality residential schools in India, that are targeted at disadvantaged castes, using over-subscribed Grade 5 admissions lotteries. Following one cohort to ages 21 and 25, we find that SWS attendance raises completed schooling by 0.38 years (3%), Grade 12 scores by 0.26 s.d., and college enrollment by 19%, closing nearly one-third of the caste gap in attainment. Yet these human-capital gains come with narrower social networks: treated students form 9% smaller and 89% more caste-homogeneous peer ties, which results in smaller job-search networks and a lower probability of finding a job through referrals. Labor-market participation initially rises by 15% but later falls back as unemployment declines and employment levels converge. A simple search model shows how higher ability raises the value of search while homophilous networks slow job-offer arrivals. The results highlight that targeted schooling can equalize learning but not mobility when opportunity flows through segregated social networks.

JEL: I24, I25, J24, J64, O15.
Keywords: school quality; affirmative action; networks and referrals; labor-market search; residential schooling; India; caste; low- and middle-income countries (LMICs).

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We acknowledge the generous support of The Stone Program in Wealth Distribution, Inequality, and Social Policy at Harvard University; the Mittal South Asia Institute at Harvard University; Research promotion policy at BGS College of Engineering & Technology. We thank Ronald Rose (IAS) for supporting the project as Secretary of Telangana Social Welfare Residential Education Institutions Society (TGSWREIS). We thank Emily Breza, Asim Ijaz Khwaja, Eliana La Ferrara, Karthik Muralidharan, Paul Novosad, Gautam Rao, and Abhijeet Singh for their valuable feedback. We thank C Arunjyothi, Ex-ASO of Telangana for excellent research assistance. We thank Dr, Kalanjari Satya of TGSWREIS for administrative support. The findings, interpretations, and conclusions expressed in this paper are the authors' own and do not necessarily represent the views of the Government of Telangana, India. All errors are our own.

1 Introduction

Expanding access to high-quality schooling has been central to development policy for decades. Yet reforms targeting underserved groups seldom consider that schools shape not only what students know, but whom they know. In labor markets where networks play outsized roles, as is common across low- and middle-income countries, human and social capital jointly determine mobility (Carranza and McKenzie, 2024; Breza and Kaur, 2025). A better school can raise skills and aspirations, translating higher quality inputs into more learning and academic achievement (Dobbie and Fryer, 2011; Muralidharan and Sundararaman, 2015; Mbiti, 2016; Kumar, 2023). However, if such schools also isolate students within homogeneous peer groups, the social networks that facilitate job search may narrow (Granovetter, 1973; Montgomery, 1991; Currarini et al., 2009; Munshi and Rosenzweig, 2016). Evidence from educational settings suggests that early peer exposure shapes later social integration and network diversity (Jackson and Rogers, 2007b; Rao, 2024). These two margins—human capital accumulation and social network formation—are typically studied in isolation. Yet they jointly determine how students transition from schooling into work, especially in environments where labor-market opportunities depend on informal connections.

In this paper, we study these dual margins through the lens of Social Welfare Schools (SWSs) in India. This is a large-scale system of public, residential, single-sex schools that are targeted towards students from historically marginalized and disadvantaged communities.[1] These institutions embody a tension in the design of educational systems, between targeting and integration, that recurs in affirmative-action debates worldwide. Targeted schooling systems promise to create protected, high-investment environments for marginalized students. On the other hand, integrative schooling systems seek cross-group exposure in mainstream schools. In India, we see examples of both: a 25% private-school quota under the 2009 Right to Education Act integrates low-income children into private classrooms (Rao, 2024; Romero and Singh, 2022), whereas SWSs and other parallel systems pursue the targeted approach (Jha et al., 2016).[2] The SWS system in the state of

[1] These schools are targeted primarily at members of marginalized caste groups and those from indigenous tribes. "Caste" refers to a hereditary social classification system in South Asia that historically determined occupation, endogamy, and social rank (Munshi, 2019; Oh, 2023; Lowe, 2021). Although caste-based discrimination has been legally abolished since India's 1950 Constitution, caste identities remain salient and continue to influence access to education, networks, and labor-market outcomes. "Scheduled Castes" is a statutory term referring to historically excluded caste groups within India's caste system, and form the vast majority of the students in our sample.

[2] Other examples are the *Navodaya Vidyalayas* (targeted at rural communities), *Eklavya Schools* (targeted at

Telangana, the context of our study, alone enrolls roughly 150,000 students across 268 campuses and commands about 11% of the state's education budget – making it among the largest publicly funded selective-school networks in the developing world.

We exploit oversubscribed lotteries into Grade 5 admissions that allocate entry into SWSs within statutory caste quotas to identify the long-run causal effect of attendance. The empirical strategy follows the large literature on school-choice (Rouse, 1998; Cullen et al., 2006; Dobbie and Fryer, 2011; Abdulkadiroğlu et al., 2011; Deming et al., 2014). We link administrative application lists with detailed alumni rolls, and supplement this data with two detailed follow-up surveys of the 2010-11 lottery entrant cohort, conducted when the modal respondent ages were 21 and 25 respectively. The lotteries provide strong first-stage variation in attendance, with a high degree of compliance and a large F-statistic, yielding credible local average treatment effects on education, networks, and labor market outcomes. Importantly, these comparisons are all within disadvantaged caste categories, as the lotteries are conducted separately within statutory reservation cells. Our estimates thus capture the local average treatment effect for students whose enrollment decisions were induced by the SWS offer, holding caste (and gender) identity constant.[3]

The results reveal a sharp dichotomy between the human and social capital effects of these schools. On the human capital margin, SWS attendance raises completed schooling by 0.38 years (a relative increase of 3 percent), college enrollment by 7 percentage points (19 percent), and Grade 12 exam scores by 0.26 standard deviations – placing the program among the top quartile of global education interventions in terms of test-score impacts (Evans and Yuan, 2022). Importantly, these large effects close 29 percent of the aggregate gap between the more privileged upper caste groups and the marginalized caste groups in educational attainment. In contrast, on the social capital margin, treated students form smaller and more caste-homogeneous networks. At age 21, SWS students report approximately 9 percent smaller "immediate-use" networks (i.e., individuals respondents can rely on for financial or informational support), suggesting a contraction in accessible social capital. Moreover, the own-caste share of their immediate-use networks is larger by 89 percent. By age 25, they report roughly one additional own-caste contact among their ten closest ties and are

tribal communities), and *Kasturba Gandhi Balika Vidyalayas* (targeted at female students).

[3]We locate roughly 68 percent of the original applicant frame in 2022 and re-interview a subsample in 2025, with follow-up rates balanced across treatment arms. Baseline characteristics do not predict tracking differentially by lottery status, indicating limited potential for attrition bias. Main results remain stable in sign and magnitude under Lee (2009) bounds for differential attrition and under inverse-probability weighting using pre-lottery covariates. We also find no treatment effects on pre-lottery covariates.

35 percentage points less likely to have found their current job through referrals, consistent with persistently narrower bridging networks.

These differential effects translate to an interesting set of dynamic patterns in labor market outcomes that mirror this duality. At age 21, SWS students are 9 percentage points more likely to participate in the labor force but no more likely to be employed – consistent with longer search among better educated workers. By 25, participation rates for SWS students fall back below the non-SWS group, unemployment drops and employment levels converge. These effects are systematically larger for female students, who experience an initial increase in labor-force participation of about 20 percentage points relative to non-SWS peers – absorbed almost entirely as unemployment rather than employment – which later reverses by roughly 25–30 percentage points by age 25.[4] This trajectory implies that human capital gains initially raise search intensity, but network homophily slows job-offer arrival, delaying employment without affecting eventual employment levels.

A key mechanism that explains this pattern of results is network size and composition. Residential schooling substitutes family-based ties with peer-based ones (Coleman, 1988; Granovetter, 1973), but in SWSs the peers themselves come almost exclusively from the same caste group. This design, while protective against discrimination and allowing for the channeling of high-quality infrastructure directly to the disadvantaged community, limits exposure to socially diverse contacts that often channel job information in segmented labor markets (Montgomery, 1991; Ioannides and Datcher Loury, 2004; Beaman and Magruder, 2012; Munshi and Rosenzweig, 2016; Srivastava, 2025). In India, where over 70% of entry-level jobs are filled through referrals from peers (Afridi et al., 2020; Chandrasekhar et al., 2020), such network compression can directly affect offer arrival and job matching. Caste is a particularly relevant vector for this mechanism. Historically, caste networks were occupational guilds that organized both information and insurance within endogamous groups (Munshi and Rosenzweig, 2006; Munshi and Rosenzweig, 2016; Munshi, 2019). Despite legal abolition of caste discrimination, occupational mobility remains sharply segmented by caste identity, and referrals often travel along caste lines.

[4] Female labor-force participation in India and South Asia remains among the lowest in the world, despite large gains in female education (World Bank, 2023; Chatterjee et al., 2018; Klasen and Pieters, 2015a). Typical education or skill-training interventions in the region raise women's participation by only 3–7 percentage points (Jensen, 2012; Heath and Mobarak, 2015), making the short-run rise observed here unusually large. The subsequent reversal echoes evidence that women's search frictions, mobility constraints, and social-network bottlenecks limit the conversion of educational gains into sustained employment (Das and Desai, 2003).

To interpret these patterns, we develop a simple search model in which schooling determines a joint bundle of human capital and network diversity. The framework provides a structural lens to reconcile the observed sequence: higher participation, flat early employment, and later convergence, by showing that higher ability raises the value of search while lower diversity slows job-offer arrivals. Using this framework, we show that for SWS students, labor force participation can rise even as employment itself remains flat, because higher ability increases the value of search while lower diversity slows the job offer arrival rate. Over time, discouragement emerges as workers update beliefs about offer rates, reproducing the empirical pattern of early participation gains followed by convergence among SWS and non-SWS students. This model highlights that human capital raises job acceptance but social capital governs arrival, and improving one without the other constrains mobility in labor markets with a dependence on networks for job matching.

Beyond education, labor force participation, and networks, we also measure supplementary outcomes to capture life-course adjustments in family formation, mobility, and household welfare. At the first survey round, SWS attendance modestly delays marriage timing but has no detectable effect on marriage incidence or fertility. Among the unmarried, preferences shift toward more educated partner, mirroring the gains in students' own attainment, while other attributes such as desired age gaps or openness to inter-caste marriage show little change. By the second follow-up, cohort-wide rates of marriage and fertility rise with age, yet these transitions remain statistically similar across treatment arms. SWS graduates are somewhat more likely to have migrated or to reside in urban areas by their mid-twenties, though the magnitudes are small and imprecisely estimated. Across household-asset indicators, including landholding and durable-goods ownership, we detect no significant treatment effects. This suggests that improvements in human capital do not necessarily translate into better realized life outcomes over the horizon we observe.

In order to establish this, we also examine heterogeneity in treatment effects along salient dimensions geography and counterfactual school quality. Students who traveled farther to attend SWS exhibit larger shifts away from family-based networks. Those whose predicted outside option (estimated using a random forest classifier on a set of pre-lottery indicators) was a traditional public school experience the largest improvements in attainment and Grade 12 performance. Learning gains are also stronger in less competitive local school markets, where baseline quality was weaker. These results underscore the gains in human capital driven by higher quality inputs and infrastructure among SWSs.

Our work contributes to three sets of literatures. First, we engage with the literature on the quality of education and long-run outcomes. Selective and well-resourced schools have been shown to raise educational attainment and test scores in both developed and developing country contexts (Dobbie and Fryer, 2011; Muralidharan and Sundararaman, 2015; Mbiti, 2016; Kumar, 2023; Muralidharan, 2024). However, most evidence in this literature is restricted to scholastic achievement and cognitive outcomes. By following students into adulthood, we are among the first to link test-score gains to actual labor-market transitions, showing that schooling reforms can raise learning levels without improving employment when networks mediate job matching.

Second, we engage with the literature on affirmative action and other inclusion and equity-focused policies. Integration-based designs, such as higher-education quotas or electoral reservations, that move under-represented groups into mainstream institutions have been studied widely (Bertrand et al., 2010; Bagde et al., 2016; Cassan, 2019; Khanna, 2020). The SWS schooling system represents the opposite extreme: targeted quality upgrades within segregated schools, where enrollment is restricted to historically marginalized groups. Our results show that such targeting successfully closes human-capital gaps but compresses social capital that is key to bridging, implying that diversity of social networks is an essential complement to investments in quality in such contexts.

Third, we engage with the literature on social networks and job search. A large literature documents that weak or heterogeneous ties accelarate job finding and mobility (Granovetter, 1973; Calvó-Armengol and Jackson, 2004; Calvó-Armengol and Jackson, 2007; Beaman and Magruder, 2012; Chiplunkar et al., 2024; Caria et al., 2023b; Srivastava, 2025). More broadly, Chetty et al. (2022a) and Chetty et al. (2022b) show that U.S. counties with higher cross-class interaction generate substantially greater upward mobility, highlighting the role of bridging social ties in long-run earnings trajectories. However, very few studies trace how early peer environments may shape adult networks and labor market prospects in a causal way. This paper provides direct evidence that formative-period homophily can persist into the labor market, reducing referral-based job matches despite higher skill.

Our findings underscore that expanding access to high-quality schools is not sufficient to ensure equitable mobility when network channels remain segregated. Policies that target disadvantaged groups should therefore pair quality improvements with interventions that foster cross-group exposure and labor-market linkages. Within the SWS system, this could include structured exchange programs with mainstream schools, alumni mentoring and referral initiatives that connect graduates to broader job networks, and partnerships with employers to reduce reliance on caste-based referrals. Analogous exposure-based programs, such as career-counseling pilots and alumni bridge initiatives, have shown that modest interventions can produce substantial gains in job search and placement (Caria et al., 2023a; Alfonsi et al., 2020). Evidence from randomized mentoring and counseling programs in Kenya and Ethiopia, for example, finds that providing students with job-search information or alumni contacts increases employment by 10–20 percent and improves match quality (Abebe et al., 2021; Groh et al., 2016). Embedding similar exposure and linkage programs within targeted schooling systems could help translate educational progress into durable labor-market mobility.

The rest of this paper is organized as follows. In Section 2, we discuss the background for this context and motivating evidence. In Section 3, we present the data and empirical strategy. We discuss the results in Section 4. In Section 5, we present the theoretical framework. In Section 6, we share some extensions of our core results, along with robustness tests. We conclude in Section 7.

2 Background

Public schooling in India is primarily administered by Departments of School Education (DSE) at the respective state governments. Each DSE operates a common set of institutions spanning three stages: *primary* (Grades 1–5), *upper primary* (Grades 1–8), and *secondary* (Grades 6–10 or Grades 1–10). These schools follow a state-prescribed curriculum. Around 54% of all students between Grades 1-10 in India attend public school, but there have been large disparities in access for disadvantaged social groups historically (Mehta, 2022). Against this backdrop, social welfare departments across many states established residential schools to expand access for these groups.

Beginning in the 1980s, these welfare departments allocated funds to construct and operate residential schools. The 1986 National Education Policy provided the policy framework for expansion, with several examples of schooling systems targeted at population sub-groups (e.g. rural, tribal and female students) (Jha et al., 2016). These programs share a common rationale: to equalize opportunity and to shield disadvantaged students from local social and geographic constraints.

Caste-based disparities in schooling remain pronounced. Students from Scheduled Castes

(SC) complete fewer years of education than upper-caste peers and face higher dropout rates (Srinivas, 1957; Desai and Kulkarni, 2008; Hnatkovska et al., 2012). Research attributes these gaps to structural under-provision, lower instructional quality, and discrimination by teachers and peers (Banerjee et al., 2025). Residential schooling—by combining greater resources with environments insulated from local hierarchies—was designed to mitigate these constraints.

Telangana's Social Welfare Schools (hereafter, SWS) extend this national model at the middle and secondary grades, primarily enrolling students from SC and other disadvantaged caste groups. SWS are tuition-free, and follow the same curriculum as other public and private schools in the state. The residential design integrates classroom instruction with housing, meals, and co-curricular programming, creating a full-time learning environment distinct from DSE day schools.

Two features distinguish SWS within the public system. First, they operate at large scale with higher investment. As of 2022, TGSWREIS managed 268 institutions enrolling about 156,000 students statewide, alongside 41,265 schools serving over six million students in the broader state system. Approximately 65% of SWS campuses serve girls. The Society's budget is roughly 11% of DSE spending. Per-pupil expenditure is about INR 80,519 (USD 953) per year in SWS, versus INR 50,169 (USD 591) in traditional public schools.[5] Second, the residential model expands instructional time and provides food, health, and safety services uncommon in day schools. Together, these features raise academic resources while producing more homogeneous peer environments—an equilibrium central to our analysis.

Admissions to SWS are competitive. During our study period, oversubscribed Grade 5 seats were filled via public lotteries. Each school admits either 40 or 80 students depending on whether one or two classrooms are sanctioned. Within schools, seats are reserved by social group: 87% for SC, 6% for Scheduled Tribes (ST), 5% for Backward Classes (BC), and 2% for others. Ballots bearing applicant names are sorted by social group to respect reservation shares, and draws are conducted in public in the presence of parents, students, elected representatives, and senior officials; the process is video-recorded. Selected and wait-listed names are posted the same day, and families have two weeks to enroll. This procedure generates randomized admission offers that form the basis of our identification.

Our study focuses on the erstwhile district of *Mahabubnagar* in southern Telangana.[6] With a

[5]Author calculations from administrative reports; INR–USD figures shown for comparability.

[6]Telangana was bifurcated in 2015 from erstwhile Andhra Pradesh; the former Mahabubnagar district

population near six million, the district is comparable in scale to well-known U.S. lottery contexts such as Charlotte–Mecklenburg (Hastings et al., 2006) and the Harlem Children's Zone (Dobbie and Fryer, 2011). In 2010, twelve SWS campuses operated in the district, including four girls' schools.

3 Data and research design

Data

We assemble our sample from administrative lottery files for twelve SWS campuses in Mahabubnagar, covering the Grade 5 intake in AY 2010–11. These files include a limited set of demographics and each applicant's village of residence. Of 3,641 applicants, 3,298 had valid addresses, which form our target frame. The district's population is roughly six million, and we estimate about 4,500 eligible students in the relevant cohort, implying that close to 80% of eligible students applied. Because the lottery files do not record winner or loser status, we recover assignment from our surveys using student and parent reports, and we cross-reference names against alumni records to identify applicants who ever attended SWS.[7]

Our primary outcomes come from indicators recorded through two waves of surveys conducted in collaboration with the Society for Social Audit and Accountability (Government of Telangana). In the first wave, i.e. *Round 1*, (August–December 2022) we completed 2,011 interviews, which is 68% of the valid frame. In the second wage, i.e. *Round 2*, (August 2025) we re-interviewed a random subsample of 576 Round 1 respondents, creating a two-wave panel. Unless otherwise noted, cross-sectional analyses use the Round 1 sample, and pooled or panel specifications restrict to respondents observed in both waves. We examine balance on pre-treatment and time invariant demographics, and school-choice variables between lottery winners and losers and between tracked and non-tracked applicants, and we address attrition in Section 6.

Lottery applicants entered Grade 5 in AY 2010–11 and completed Grade 10 in AY 2015–16. After two years in higher secondary (Grades 11–12), most were on course to graduate high school in AY 2017–18. Students who continued to college typically finished a three-year degree by AY 2020–21 or a four-year degree by AY 2021–22, which would imply post-college labor-force

has since been divided into five smaller districts.

[7] Matching is on student and parent names and village of residence.

entry around 2021–2022. Figure 1 summarizes educational milestones alongside the timing of both survey rounds.

Round 1 was conducted during the post-pandemic recovery when many respondents were still completing higher education or beginning job search. Two considerations motivated a second follow-up in 2025. First, cohort timing, as by 2025 most respondents had aged into more stable labor-market status, which reduces noise from temporary transitions. Second, macro conditions, since employment and job-matching patterns were still normalizing in late 2022, so Round 1 estimates may reflect short-run conditions. Round 2 re-measures the same outcomes after additional labor-market exposure and adds detail on job search and network composition to inform mechanism tests.

The socio-economic survey comprises six modules that together capture education, labor-market activity, marriage and fertility, household assets and expenditure, and social networks. The education module records years of schooling, college enrollment and choices, standardized exam scores for Grades 10 and 12, and competitive-exam outcomes. The labor-market module measures participation, employment type, job-search channels, and earnings. The marriage and fertility module records marital status and timing, spousal attributes and preferences, number of children, and expectations for those not yet married. The assets and expenditure module tracks durable ownership and household spending. The networks module measures network size and composition. Round 2 repeats key labor and networks outcomes and adds an additional module on job-search activity, which we use in pooled and panel analyses.

Research design

We follow the lottery-based school-effects literature estimating impacts of winning school lotteries (Rouse, 1998; Cullen et al., 2006; Hastings et al., 2006; Dobbie and Fryer, 2011; Abdulkadiroğlu et al., 2011; Deming et al., 2014; Gray-Lobe et al., 2023). We model outcomes as a linear function of years spent at an SWS school:

$$Y_{it} = \delta \cdot \text{SWS years}_i + \gamma \cdot X_i + \phi_t + \varepsilon_{it},$$

where Y_{it} is the outcome of interest; SWS years_i is the continuous number of years at SWS; X_i is a vector of time-invariant covariates (gender, caste, age); ϕ_t are survey-month fixed effects; and ε_{it} is an error term.

The parameter of interest is δ, the causal effect of SWS on outcomes. Because enrollment is not random, OLS may be biased by unobserved household or community characteristics correlated with both school choice and outcomes. We therefore use lottery status as an instrument for SWS attendance.

We assume lotteries were conducted fairly and that winning affects outcomes only through SWS attendance.[8] Under these assumptions, we estimate the local average treatment effect (LATE) of SWS attendance for students induced to attend by a lottery win using standard two-stage least squares.

First and second stages. Formally, the first stage is:

$$S_i = \pi \cdot \text{Lottery}_i + \mu \cdot X_i + \nu_t + \eta_{it},$$

where S_i denotes SWS attendance (either an extensive-margin indicator for ever attending or an intensive-margin measure of years), Lottery_i is an indicator for winning, X_i is the covariate vector, ν_t are survey-month fixed effects, and η_{it} is an error term. The second stage is:

$$Y_i = \kappa \cdot \widehat{S}_i + \theta \cdot X_i + \xi_t + \zeta_{it}.$$

In our main specification, S_i is the extensive-margin indicator for ever attending SWS; an alternative specification using years at SWS is reported in the appendix. The first-stage coefficient π is testable; Table 3 reports results. In Column 1, lottery winners are 59 percentage points more likely to attend SWS than lottery losers. Not all winners enroll, largely due to distance and preferences for residential schooling, and about 8% of lottery losers attend SWS because of minor procedural deviations or misreporting. This non-compliance is limited, and the instrument remains highly relevant. The lottery also raises the intensive margin by roughly two years at SWS. The 59 percentage-point extensive-margin effect aligns with substitution away from traditional public and

[8] Contemporaneous accounts indicate that draws were conducted publicly as large events to ensure transparency.

private schools by 38 and 22 percentage points, respectively. First-stage F-statistics range from 162 to 948; our preferred specification (ever attend as the endogenous variable) lies near the top of this range, comfortably exceeding conventional thresholds for instrument strength (Andrews et al., 2019).

Internal validity checks. We conduct two checks. First, we test balance in time-invariant applicant characteristics and in school-choice variables (e.g., distance to nearest public and private schools; student–teacher ratios at those schools) across lottery winners and losers. Second, we compare baseline demographics for tracked versus non-tracked applicants.

Table 1 summarizes characteristics for the 2,011 applicants we tracked and surveyed. Columns 1 and 2 report means for lottery losers and winners; Column 3 reports mean differences and significance. Across indicators, we find no statistically significant differences by offer status. Nearly three-fourths of parents are illiterate, underscoring that SWS serves an acutely underprivileged population.

A key concern in long-run follow-ups is attrition, especially if tracking varies by treatment (Weiland et al., 2024). Of the original 3,665 valid applicants, we tracked and surveyed about 68%.[9] We test balance between tracked and non-tracked applicants and test for differential attrition by treatment. Table 1 shows no significant differences by tracking status—either in demographics or lottery status.

Analysis of panel variables. To study how effects evolve with age and macro conditions, we pool Rounds 1 and 2 and estimate a two-period panel IV–DiD framework. Let $r \in \{1,2\}$ index survey rounds and define $\text{Post}_r = \mathbf{1}\{r = 2\}$. Because S_i (ever attending SWS) is time-invariant, we focus on $S_i \times \text{Post}_r$ and instrument it with $\text{Lottery}_i \times \text{Post}_r$:

$$Y_{ir} = \alpha_i + \lambda_r + \delta\,(S_i \times \text{Post}_r) + X_i'\gamma + \mu_{m(r)} + \varepsilon_{ir}, \tag{1}$$

$$(S_i \times \text{Post}_r) = \alpha_i^* + \lambda_r^* + \theta\,(\text{Lottery}_i \times \text{Post}_r) + X_i'\rho + \mu_{m(r)}^* + \nu_{ir}. \tag{2}$$

Here α_i are individual fixed effects, λ_r are round fixed effects, and $\mu_{m(r)}$ are round-specific survey-month fixed effects. The coefficient δ captures the change in the SWS effect from Round 1 to Round 2

[9]The main constraint is the absence of unique student identifiers in centralized records.

for compliers (a difference-in-differences LATE). We report both δ and the implied Round 2 effect, $\kappa^{\mathrm{R1}} + \delta$, where κ^{R1} is the Round 1 IV effect from the main cross-sectional specification below. Identification requires standard IV assumptions and that, absent SWS, winners and losers would share common round-to-round trends captured by λ_r and $\mu_{m(r)}$.

Outcomes observed only at endline. Some outcomes—such as job-search mechanisms and network composition during job search—are observed only in the Round 2 survey data. For these, we estimate cross-sectional IV at Round 2 using the original instrument:

$$Y_i^{(2)} = \alpha^{(2)} + \kappa^{\mathrm{R2}} S_i + X_i' \gamma^{(2)} + \mu_{m(2)} + \varepsilon_i^{(2)}, \qquad \text{instrument: Lottery}_i. \tag{3}$$

These endline estimates complement the panel analysis but do not enter the DiD because corresponding measures are unavailable in Round 1.

4 Results

We organize the results into four domains: educational attainment, social networks, labor-market outcomes, and other life-course outcomes. These domains reflect the sequence through which selective residential schooling can shape human capital, social exposure, early labor-market behavior, and household formation. The section begins with findings from the first survey wave and then incorporates evidence from pooled and second-wave specifications to track how effects evolve as the cohort enters early adulthood.

Human capital outcomes

Winning the lottery alters students' schooling environments in ways that are consequential for subsequent outcomes. SWS campuses are residential and single-sex, and lottery-induced enrollment therefore shifts students into settings with same-gender peers and more homogeneous caste composition. As shown in Figure A.1 and Figure A.2, the likelihood of attending a mixed-gender school declines sharply for students who enroll in SWS, and the probability of attending a school where most peers share one's caste increases substantially. These shifts in peer environment and everyday social exposure serve as intermediate channels through which SWS attendance may affect later educational and social outcomes.

Using lottery assignment as an instrument for ever attending SWS, the 2SLS estimates in Table 4 show meaningful gains along the extensive margin of educational attainment. Students induced to attend SWS complete 0.38 additional years of schooling and are 5 percentage points more likely to continue past Grade 10, relative to a high baseline among lottery losers. These improvements narrow the Scheduled Caste–upper-caste gap in years of schooling by roughly 29%, as calculated using IHDS 2011–12 microdata.[10] Students are also 7 percentage points more likely to enroll in higher education—a 19.2% relative increase from the control mean.

Turning to achievement outcomes, we find no detectable difference in Grade 10 pass rates, which exceed 95% even among lottery losers. By contrast, the Grade 12 examination—an important school-leaving credential for college admission—shows a standardized score increase of 0.26 standard deviations and a 5-percentage-point rise in pass rates (Table 5). To benchmark these effect sizes, the 0.26σ gain in Grade 12 performance is comparable to several widely studied interventions: student tracking in Kenya (0.18σ; Duflo et al. (2011)), remedial teaching in China (0.14σ; Lai et al. (2015)), teacher incentives in India (0.17–0.27σ; Muralidharan and Sundararaman (2011)), and school vouchers in India (0.13σ; Muralidharan and Sundararaman (2015)). These comparisons place the SWS impact near the upper range of effect sizes documented for scalable education programs in low- and middle-income countries.

Taken together, the results indicate improvement along both extensive and intensive educational margins: increased years of schooling, higher continuation beyond Grade 10, greater college enrollment, and higher Grade 12 performance. Because SWS schooling ends at Grade 10, the gains observed at Grade 12 likely reflect downstream effects—such as stronger academic preparation, more structured study habits, or higher educational expectations—rather than contemporaneous inputs from SWS itself. Increased higher-education enrollment, combined with stronger Grade 12 performance, also raises the option value of continued search and may influence reservation wages in early labor-market transitions.

[10] Computed using the India Human Development Survey 2011–12 by comparing mean years of schooling among Scheduled Caste and General-category adults; the 0.38-year increase corresponds to approximately 29% of this baseline gap.

Social capital outcomes

We measure personal networks using two survey modules and a roster of closest ties. In the *borrowing* module, respondents list the individuals they would approach for help in a financial emergency; network size is the number of distinct alters named, and responses are recorded without a fixed cap on names. The *advice* module follows the same structure for contacts whom respondents would consult about an important family matter, also without a fixed cap. For each alter in these modules, we record whether the contact is a family member, a same-caste non-family member, or an other-caste non-family member, which allows us to construct respondent-level composition shares. In addition, respondents enumerate their ten closest contacts and classify each person by where the tie was formed (family, school, college, neighborhood, workplace, or other) and by caste group. Together, these measures provide complementary indicators of network size and composition.

SWS students report smaller immediate-use networks across both the borrowing and advice modules. In the borrowing module, the 2SLS estimate indicates roughly a 9 percent decline in the number of alters available for emergency financial support relative to non-SWS students (Table 8). Patterns in the advice module are similar, with a smaller but directionally consistent reduction in the number of people respondents would approach for major family decisions (Table 9). These patterns indicate that students induced into SWS maintain fewer readily mobilizable contacts for emergency support or important family decisions. Composition shifts accompany these size declines. The share of family members falls by about 2–3 percentage points in borrowing networks and 3–4 percentage points in advice networks, while the share of same-caste non-family alters rises—by roughly 7–8 percentage points in borrowing networks and a smaller, less precisely estimated amount in advice networks. These increases are mirrored by declines in the share of other-caste non-family alters. Given that baseline networks are already highly homophilous (roughly three-quarters of named alters are same-caste for non-SWS students), these changes make immediate-use networks smaller and more caste-concentrated.

The roster of respondents' ten closest contacts shows a parallel shift in deeper relational structure. SWS students list a substantially higher share of own-caste alters among their closest ties—an increase of about 9.8 percentage points (Tables 8–9). By contrast, the venues where non-family ties are formed look nearly identical across treatment status: both groups report forming most non-family ties in school or college (Table 10). This indicates that the difference arises not from differential exposure to venues but from whom respondents maintain as close contacts. Taken together, the evidence at roughly ages 22–23 points to networks that are both smaller on immediate-use margins and more caste-homophilous among core ties—a pattern we keep in mind when turning to early labor-market behavior in the next subsection.

By the mid-twenties (the timing of the second survey), closest-ties networks diversify for the cohort as a whole: respondents report more workplace-based ties and fewer family-based ties among their closest contacts. Within this general evolution, however, SWS students continue to exhibit more caste-homogeneous close networks. The pooled IV–DiD estimates imply that, as students age, SWS attendees maintain roughly one additional own-caste contact and one fewer other-caste contact among their ten closest ties. Differences in the origins of these ties—family, school, college, workplace, or other—remain small and imprecisely estimated. Thus, even as social networks naturally broaden with age, SWS students retain a more caste-concentrated set of close relationships, consistent with the patterns documented earlier in early adulthood.

Taken together, the evidence shows that students induced into SWS form networks that are smaller on readily mobilizable margins and more concentrated within own-caste, non-family contacts, and that these differences persist as the cohort transitions from early adulthood into the mid-twenties. These patterns align with established evidence that heterogeneous and bridging ties transmit non-redundant information and connect individuals to broader social and economic environments (Granovetter, 1973; Montgomery, 1991; Ioannides and Datcher Loury, 2004; Calvó-Armengol and Jackson, 2004; Calvó-Armengol and Jackson, 2007). In the Indian context, prior research documents that caste-based homophily restricts the range of social interactions and narrows individuals' opportunity sets (Munshi and Rosenzweig, 2006; Beaman and Magruder, 2012; Munshi and Rosenzweig, 2016). We next turn to early labor-market behavior for the same cohort.

Labor-market outcomes

Lottery-induced attendance at SWS produces marked changes in early labor-market behavior. By age 21, treated students are 9 percentage points more likely to participate in the labor force, a 16% increase relative to the control mean, but are no more likely to be employed (Table 6). The participation gain is absorbed almost entirely as a rise in unemployment: the likelihood of being unemployed increases, with point estimates mirroring the participation effect. Consistent with this pattern of early labor-market entry without rapid job matching, SWS students make more attempts on competitive examinations, including for selective higher-education and government positions (Table 7). These first-wave results establish the central empirical fact that SWS increases labor-force entry but not employment, shifting jobseekers toward a period of more intensive search and credentialing.

This pattern of higher labor-force participation without higher employment is consistent with standard job-search reasoning. When schooling raises human capital, students face higher expected returns to searching, pushing more individuals above the participation threshold even if immediate job arrival does not increase (Pissarides, 2000; Rogerson et al., 2005). In this setting, SWS students appear to enter the labor market with stronger aspirations and higher reservation standards, generating greater early search intensity but not faster placement. The increase in competitive-exam attempts aligns with evidence that Indian jobseekers frequently pursue further credentialing when suitable matches do not arrive quickly (Mangal, 2024). Taken together, the first-wave results describe an early-career phase in which search effort rises but employment does not, a pattern we later formalize through a simple search framework in Section 5.

As the cohort moves into its mid-twenties, the pooled IV–DiD estimates show a marked shift in labor-market behavior. At the cohort level, the likelihood of working rises sharply between surveys, but there is no differential increase for SWS students (Table 13). Instead, the main dynamic margin is labor-force participation: the early 9-percentage-point participation advantage documented in the first wave reverses to an 18-percentage-point deficit by 2025, implying that SWS students become less likely to be in the labor force relative to non-SWS peers. Unemployment also falls more steeply for SWS students than for controls, indicating that many treated students exit active search rather than transition into employment. Consistent with this pattern, the pooled specification shows no additional change in competitive-exam taking beyond the cohort-wide rise between waves. Together, these results indicate that the initial increase in search effort documented at age 21 does not persist; by the mid-twenties, labor-force withdrawal, rather than employment growth, is the dominant differential margin for SWS students.

Job-search channels at endline provide a direct mechanism for the labor patterns. In Round 2, SWS students are about 35 percentage points less likely to report having found a job through referrals (Table 16), while the size of the job-search network is similar and the composition margin indicates relatively more same-caste contacts. This pattern aligns with evidence that referrals and bridging ties facilitate job access and higher-quality vacancies (Granovetter, 1973; Montgomery, 1991; Ioannides and Datcher Loury, 2004; Calvó-Armengol and Jackson, 2004; Calvó-Armengol and Jackson, 2007) and that caste-based homophily can narrow the relevant opportunity set in India (Munshi and Rosenzweig, 2006; Munshi and Rosenzweig, 2016; Beaman and Magruder, 2012). Fewer referral-mediated matches coupled with more homophilous search channels provides a proximate explanation for why employment does not differentially rise for SWS students even as early search effort initially increases. We formalize this interpretation in the next section with a simple search framework (Section 5).

Other life-course outcomes

At roughly ages 22–23 (Round 1), students induced into SWS marry slightly later, but marriage incidence and fertility remain statistically indistinguishable across treatment status (Table 11). The 2SLS estimates show a modest delay in age at first marriage—consistent with standard models in which higher schooling raises the opportunity cost of early marriage and shifts returns toward continued education and search (Becker, 1973; Chiappori et al., 2012). By contrast, coefficients for currently married, ever married, children ever born, and recent fertility are close to zero, reflecting both substantial sampling noise and the young age of the cohort. By the mid-twenties (Round 2), cohort-wide rates of marriage and fertility rise—as expected with age—but the pooled IV–DiD estimates in Table 14 indicate no differential SWS effect on marriage timing, marriage incidence, or fertility. Thus, across both survey waves, residential schooling modestly delays marriage but produces no detectable differences in the rate or level of family formation at these ages.

Among unmarried respondents, SWS attendance shifts some stated preferences about potential partners (Table 12). Students—particularly women—report a stronger preference for a more educated spouse, a pattern consistent with models of positive assortative matching on human capital (Becker, 1973; Chiappori et al., 2012). By contrast, other preference margins such as desired age gap and willingness to marry outside one's caste exhibit coefficients close to zero and are estimated imprecisely. Taken together, these results suggest targeted shifts in the salience of educational characteristics in partner choice, rather than broad changes across all attributes, and function as an early, forward-looking indicator of how increased schooling may shape preferences before marriage decisions are realized.

By the second follow-up, conducted when respondents are in their mid-twenties, cohort-wide family formation rises: marriage increases by roughly 3 percentage points, age at first marriage falls by about 0.4 years, and fertility rises by around 12 percentage points (Table 14). However, none of these transitions differ significantly between SWS and non-SWS students, complementing the Round 1 results. SWS students appear somewhat more likely to have migrated or to reside in urban areas by their mid-twenties, though these differences are small and imprecisely estimated in the current specification. Across indicators of household assets—including landholding and durable-goods ownership—we detect no significant treatment effects. Taken together, these findings indicate that improved human capital does not translate into detectable differences in early adult family formation, mobility, or household welfare over the horizon observed in our data.

5 Theoretical framework

We model how school choice determines a bundle of *human capital* and *network (social) capital*, which in turn shapes job search, early labor force participation (LFP), unemployment, and eventual labor market prospects. The framework is intentionally parsimonious and focuses on economic fundamentals; in Section B we note a small set of moments and elasticities that map cleanly to the data.

School choice and capital bundles

Households choose $s \in \{\text{SWS}, \text{Other}\}$ at the start of secondary school. Choice s deterministically (or in expectation) maps into a pair

$$(H_s, D_s) \in \mathbb{R}_+ \times [0, 1],$$

where H denotes human capital and D denotes the *diversity or bridging* component of network capital.[11] The key qualitative feature is

$$\Delta H \equiv H_{\text{SWS}} - H_{\text{Other}} > 0 \quad \text{and} \quad \Delta D \equiv D_{\text{SWS}} - D_{\text{Other}} < 0, \tag{4}$$

i.e., SWS raises human capital but reduces exposure to diverse or bridging ties.

Job search technology and wages

Time is continuous with discount rate $\rho > 0$. An unemployed worker who *participates* in the labor market receives job offers according to a Poisson process with intensity

$$\lambda(D), \qquad \lambda'(D) > 0,\ \lambda''(D) \leq 0,$$

reflecting that more diverse or bridging networks improve arrival rates through referrals, broader information sets, or vacancy exposure (Granovetter, 1973).

Conditional on an offer, the wage w is drawn from $F_H(\cdot)$, a distribution indexed by H with first-order stochastic dominance in H. We write the acceptance probability at reservation wage $\bar{w}$ as

$$p(H;\bar{w}) \equiv 1 - F_H(\bar{w}), \qquad \frac{\partial p}{\partial H}(\cdot) > 0.$$

This modeling choice allows for wages to move with higher human capital.

While unemployed, the worker incurs a flow search cost $\kappa \geq 0$ and has an outside option B (home production, further education, or family care). Upon accepting a job, she earns flow utility w indefinitely.[12]

Search surplus and participation. Given a reservation wage $\bar{w}$ (which is endogenous but independent of D in the baseline), the *search surplus* from participating in the labor market with

[11] For tractability, we absorb the size of the network into D; results below go through if D is replaced by (S, D) with similar monotonicity.

[12] In this version, we omit separations for transparency and tractability.

state (H,D) is

$$S(H,D;\bar{w}) = \frac{\lambda(D)}{\rho+\lambda(D)} \cdot \Gamma(H;\bar{w}) - \frac{\kappa}{\rho}, \qquad \Gamma(H;\bar{w}) \equiv \int_{\bar{w}}^{\infty} (w-\bar{w})\, dF_H(w), \tag{5}$$

i.e., arrival-adjusted expected match surplus net of search costs.[13]

The worker *participates* (i.e. $LFP = 1$) iff $S(H,D;\bar{w}) \geq B$; otherwise she stays out (i.e. $LFP = 0$).

Short-horizon job finding and unemployment. For a participant with state (H,D) and reservation $\bar{w}$, the job-finding hazard is

$$h(H,D;\bar{w}) = \lambda(D) \cdot p(H;\bar{w}).$$

Hence (i) the expected unemployment duration is $\mathbb{E}[T_U] = 1/h$, and (ii) the probability of being employed within horizon T is

$$J_T(H,D;\bar{w}) = 1 - \exp\big(-h(H,D;\bar{w})\,T\big). \tag{6}$$

Early-life pattern under the SWS bundle

We now characterize the qualitative predictions implied by the bundle in (4).

Result 1: Participation rises under SWS.

We define the semi-elasticities

$$\eta_D \equiv \frac{\partial \ln \lambda(D)}{\partial D}\Big|_{D_{\text{Out}}} > 0, \qquad \eta_H \equiv \frac{\partial \ln \Gamma(H;\bar{w})}{\partial H}\Big|_{H_{\text{Out}}} > 0.$$

A sufficient condition for SWS to *raise* participation at entry is

$$\eta_H \cdot \Delta H > \eta_D \cdot |\Delta D|. \tag{7}$$

The intuition for this result is straightforward. SWS improve the quality of offers (via Γ) enough to offset slower job-arrival rates, so the option value of search rises above the outside option B for

[13] This is a reduced-form representation of the McCall problem with Poisson arrivals. Any monotone re-parameterization that preserves $\partial S/\partial D > 0$ and $\partial S/\partial H > 0$ will yield identical comparative statics.

more agents.

Result 2: Unemployment rises and employment need not.

For participants, the expected unemployment duration is

$$\mathbb{E}[T_U] = \frac{1}{\lambda(D)\, p(H;\bar{w})}.$$

Under the SWS bundle, the log change is

$$\Delta \ln \mathbb{E}[T_U] = -\Delta \ln \lambda(D) - \Delta \ln p(H;\bar{w}).$$

If the (absolute) fall in arrival rates dominates any rise in acceptance probability—i.e.,

$$|\Delta \ln \lambda(D)| \;>\; \Delta \ln p(H;\bar{w}), \tag{8}$$

then unemployment duration *rises*. Moreover, for any finite early horizon T, the employment gain

$$\Delta J_T \approx e^{-hT}\, T \cdot \Big[\underbrace{\lambda(D)\, \partial_H p}_{\text{via } H} \cdot \Delta H + \underbrace{p(H;\bar{w})\, \partial_D \lambda}_{\text{via } D} \cdot \Delta D\Big]$$

is dominated by the networks channel (D) when T is small and $|\Delta D|$ is sizeable. As a result, J_T may change little (or even fall) despite higher LFP.[14]

Result 3: Discouragement and exit from search.

Let workers periodically re-optimize. Suppose search costs rise with duration, $\kappa(t)$ with $\kappa'(t) > 0$, or beliefs about λ are updated using the offer history. Then the *continuation surplus*

$$S_t(H, D;\bar{w}) = \frac{\lambda_t(D)}{\rho + \lambda_t(D)}\, \Gamma(H;\bar{w}) - \frac{\kappa(t)}{\rho}$$

falls over unemployment spells when λ_t is revised downward after no offers (or when $\kappa(t)$ rises). We define the discouragement or search fatigue time

$$T^\star(H, D) = \inf\{t \geq 0 :\; S_t(H, D;\bar{w}) < B\}.$$

[14] As $T \to \infty$, $J_T \to 1$ in this no-separation baseline. Our empirical focus is on early-career horizons, since we only survey respondents 1–5 years after their potential labor market entry.

Lower D (hence lower true λ and more pessimistic posteriors under no offers) *reduces* $T^{\star}$: workers give up earlier.

Thus, the SWS bundle jointly predicts: higher entry LFP (*Result 1*), longer unemployment spells and muted early employment (*Result 2*), followed by higher exit from search (*Result 3*).

Comparative statics

Near the participation margin and holding $\bar{w}$ fixed,

$$\frac{\partial \ln \mathrm{LFP}}{\partial \ln H} \propto \eta_H, \qquad \frac{\partial \ln \mathrm{LFP}}{\partial \ln D} \propto \eta_D,$$

while for participants

$$\frac{\partial \ln \mathbb{E}[T_U]}{\partial \ln D} = -1 + O\left(\frac{\partial \ln p}{\partial \ln D}\right), \qquad \frac{\partial \ln \mathbb{E}[T_U]}{\partial \ln H} = -\frac{\partial \ln p}{\partial \ln H} < 0.$$

Early-horizon employment satisfies $\frac{\partial J_T}{\partial D} > 0$ and $\frac{\partial J_T}{\partial H} > 0$, but the D channel dominates when T is small (cf. *Result 2*).

In our setting, we expect differential effects by gender. Let $B = B_g$ and $\kappa = \kappa_g$ differ by gender g (for instance because of home production, safety, or social norms more broadly). Then the same SWS bundle can induce larger LFP rises for females, since they have lower B_g or κ_g, yet yield similar (or smaller) early employment if their networks are more homophilous (lower D) or face tighter referral bottlenecks (smaller $\partial_D \lambda$).

We anchor this framework with three simple elasticities from moments in our data, and provide the overarching intuition in Figure **??**.

- **Participation with respect to human capital.** Let H be proxied by scores in the Grade 12 examinations, defined in standardized units. Define

$$\varepsilon_{\mathrm{LFP},H} \equiv \frac{\partial \ln \Pr(\mathrm{LFP}=1)}{\partial \ln H} \approx \frac{\widehat{\beta}^{\mathrm{IV}}_{\mathrm{LFP}}}{\widehat{\beta}^{\mathrm{IV}}_{H}} = \frac{0.10}{0.26} = 0.36.$$

  This elasticity summarizes the "entry" margin emphasized in Figure **??**: higher human capital raises the value of search and the probability of participation.

- **Referral-based matching with respect to network diversity.** Let D denote network diversity, measured as the *other-caste share* among the ten closest ties. Define

$$\varepsilon_{\text{referral},D} \equiv \frac{\partial \ln \Pr(\text{found via referral})}{\partial \ln D} \approx \frac{\widehat{\beta}^{\text{IV}}_{\text{referral}}}{\widehat{\beta}^{\text{IV}}_{D}} = \frac{-0.35}{-1.05} = 0.33.$$

  More diverse ties predict more referral-based matches (Granovetter, 1973; Calvó-Armengol and Jackson, 2004; Ioannides and Datcher Loury, 2004).

- **Exit-from-search (discouragement or fatigue) with respect to arrival proxies.** Define the semi-elasticity

$$\eta_{\text{exit},\lambda} \equiv \frac{\partial \ln \Pr(\text{LFP} = 1)}{\partial \ln \lambda} \approx \frac{\widehat{\beta}^{\text{IV–DiD}}_{\text{LFP}}}{\widehat{\beta}^{\text{IV}}_{D}} = \frac{-0.28}{-1.05} = 0.27.$$

  Because lower job arrival is associated with lower LFP over time, we expect $\eta_{\text{exit},\lambda} > 0$ in absolute value but realized as a *decline* in participation when λ falls, thus capturing the discouragement channel.

The mapping $s \mapsto (H, D)$ reflects standard human-capital production (see Ben-Porath, 1967) alongside network formation with homophily (Granovetter, 1973; Currarini et al., 2009). Arrival rates increasing in D capture referral/information channels stressed by (Calvó-Armengol and Jackson, 2004; Calvó-Armengol and Jackson, 2007) and related empirical work on networks and jobs (Ioannides and Datcher Loury, 2004; Montgomery, 1991). The initial pattern—higher LFP, longer unemployment, muted short-run employment, and eventual discouragement—emerges when the SWS-induced loss in $\lambda(D)$ dominates the short-horizon effect of higher H on acceptance probability $p(H; \bar{w})$, while still raising the option value of search enough to cross the participation margin.

The three elasticities imply that search responds meaningfully—but not explosively—to the bundle of human and social capital created by SWS. First, $\varepsilon_{\text{LFP},H} \approx 0.36$ indicates that a 10% rise in human capital raises the probability of being in the labor force by about 3.6%. Because H is measured in standardized Grade 12 marks, this captures the "entry" margin: better credentials shift up the option value of search and push more people over the participation threshold, consistent with the model's comparative statics near the margin. In short, human-capital gains translate into sizable entry, even if job finding takes longer.

Second, $\varepsilon_{\text{referral},D} \approx 0.33$ indicates that a 10% increase in network diversity (other-caste share among the ten closest ties) increases referral-based matches by roughly 3.3%. This is exactly what classic network theory would predict: bridging/heterogeneous ties widen information sets and referral pathways (Granovetter, 1973; Ioannides and Datcher Loury, 2004), and our reduced-form ratio maps those mechanisms into the data. The fact that both the numerator and denominator are negative implies that when diversity falls, referrals fall proportionally, helping explain muted short-run employment despite higher participation.

Finally, the semi-elasticity $\eta_{\text{exit},\lambda} \approx 0.27$ links arrival rates to participation dynamics: a 10% decline in the job-arrival rate reduces LFP by about 2.7% over time. This quantifies discouragement: when heterogeneous leads slow, workers exit search earlier, lowering observed participation in later waves even as their skills improved—a pattern our framework highlights and the text motivates through declining $\lambda(D)$. Put together, the magnitudes are consistent with the literature, wherein diverse networks speed matching, human capital raises acceptance, and lower arrival rates generate longer unemployment and eventual search exit.

6 Extensions

We study heterogeneous treatment effects along four dimensions that theory and context suggest are first order: (i) gender; (ii) students' likely outside option (traditional public vs. private school), constructed with a random-forest classifier; (iii) the extent of relocation (and thus dislocation from family) required to attend an SWS; and (iv) the competitiveness of the local schooling market. Throughout, estimation follows our main IV strategy from the first survey wave (lottery status instrumenting SWS attendance) unless otherwise noted.

By gender. Tables 18 and 19 show that educational attainment and Grade 12 achievement gains are present for both women and men, with similar magnitudes.[15] In contrast, the labor-market extensive margin differs starkly by gender. Table 20 shows that the increase in labor-force participation observed in the first survey wave is concentrated among women and is absorbed as unemployment rather than employment; men show little change at the extensive margin. This

[15] Point estimates are slightly larger for women, but gender differences are not statistically significant in pooled tests.

pattern is consistent with models in which additional schooling raises women's reservation wages, aspirations, and the perceived returns to search, while social norms and narrower networks slow job arrival—producing higher participation without higher employment at short horizons. It also aligns with evidence on the U-shaped relationship between women's education and labor-force participation in India (Chatterjee et al., 2018; Klasen and Pieters, 2015b), and with studies showing that safety and job-search frictions disproportionately bind for women (Jensen, 2012). In our setting, gender-segregated schooling plausibly boosts human capital and self-efficacy for women while simultaneously limiting exposure to bridging ties, a combination highlighted in network-based search models (Granovetter, 1973; Montgomery, 1991; Ioannides and Datcher Loury, 2004; Calvó-Armengol and Jackson, 2004; Calvó-Armengol and Jackson, 2007) and in the Indian evidence on caste-based homophily (Munshi and Rosenzweig, 2006; Munshi and Rosenzweig, 2016).

By likely outside option (public vs. private). A central margin in this setting is the quality of students' *counterfactual outside option*. Admission to an SWS replaces the school a student would otherwise attend, and treatment effects may therefore depend on whether that alternative is a traditional public school or a private school. We study this heterogeneity using two complementary approaches: (i) a model-assisted subgroup LATE that combines IV moments with covariate-based predictions of untreated school choice, and (ii) a prediction-based sample split using a random-forest classifier.

Subgroup LATE using IV reweighting. Let $A_i(0) \in \{\text{public}, \text{private}\}$ denote the school type student i would attend absent admission to an SWS, D_i denote SWS attendance, and Z_i denote lottery assignment. The parameter of interest is the treatment effect for compliers by counterfactual school type,

$$\beta_h = \mathbb{E}[Y_i(1) - Y_i(0) \mid \text{complier}, A_i(0) = h], \quad h \in \{\text{public}, \text{private}\}.$$

Because $A_i(0)$ is observed only for untreated students, we first estimate, on lottery losers, the probability that a student's untreated outside option is private,

$$s_i \equiv \Pr(A_i(0) = \text{private} \mid X_i, Z_i = 0),$$

using time-invariant pre-lottery covariates (parental education, demographics, landholding, and local measures of school supply and quality).

We then combine these predicted probabilities with lottery-based IV moments. For subgroup $h \in \{\text{public}, \text{private}\}$, define weights $w_i^{\text{private}} = s_i$ and $w_i^{\text{public}} = 1 - s_i$, and estimate

$$\hat{\beta}_h = \frac{\frac{1}{n}\sum_i w_i^h \left(\frac{Z_i Y_i}{\hat{p}_Z} - \frac{(1-Z_i)Y_i}{1-\hat{p}_Z}\right)}{\frac{1}{n}\sum_i w_i^h \left(\frac{Z_i D_i}{\hat{p}_Z} - \frac{(1-Z_i)D_i}{1-\hat{p}_Z}\right)},$$

where $\hat{p}_Z$ is the sample lottery-win rate. This estimator is closely related to the weighting representation for complier moments in Abadie (2003): the lottery identifies the complier margin, while the weights $(s_i, 1 - s_i)$ allocate that margin across latent untreated outside options. Identification requires that the relationship between covariates and untreated school choice among lottery losers extends to the complier population.

The resulting estimates show that the *public-school outside option* drives the main extensive-margin effects of SWS attendance (Tables A.11–A.13). Students whose untreated option is likely another public school experience substantially larger gains in years of schooling, continuation beyond Grade 10, higher education, and labor-force participation, alongside increases in unemployment and more caste-homogeneous non-family networks. By contrast, Grade 12 marks increase for both public- and private-margin students. These patterns are consistent with the framework in Section 5: when the untreated alternative is a traditional public school, SWS represents a larger improvement in instructional quality (raising human capital), while also shifting peer exposure toward more homogeneous networks (reducing job-arrival rates).

The κ-weighting estimates are accompanied by two diagnostic exercises designed to assess whether the model-assisted subgroup-LATE is being driven by unstable subgroup assignment rather than meaningful heterogeneity. First, a trimmed-support specification in Table A.14 excludes observations with extreme predicted private-school probabilities, hence directly testing whether the results are sensitive to limited overlap or a small set of observations that the model assigns almost deterministically to one outside-option type. Second, a set of indicators reported in Table A.15 compares model-implied complier subgroup shares to the corresponding Abadie (2003) complier shares recovered from control-state reweighting, and also reports subgroup-specific effective sample sizes and weighted first stages. Together, these diagnostics show that the model-based subgroup split lines up reasonably closely with the design-based complier composition, that both subgroup-specific first stages remain positive, and that the main public-versus-private contrast is not mechanically driven by extreme predictions.

Prediction-based heterogeneity using a random forest classifier. As a complementary approach, we construct a discrete proxy for the untreated outside option using a random-forest classifier trained on lottery losers Breiman, 2001. Using the same set of pre-lottery covariates, we predict each student's counterfactual school type, reserving 20% of lottery losers for validation (Table A.4). Because private-school attendance is relatively rare, the model achieves high specificity but lower precision for the private class.

We then split the sample based on out-of-sample predicted outside options and estimate heterogeneous IV effects, following Banerjee et al. (2024) and the prediction-guided heterogeneity literature (Mullainathan and Spiess, 2017; Athey and Wager, 2019). Tables A.5–A.10 show a similar qualitative pattern: treatment effects are generally larger for students whose predicted outside option is a traditional public school. In particular, gains in educational attainment and the shift toward more caste-homogeneous networks are more pronounced in the public-outside subgroup.

For the random-forest approach, the main concern is that subgroup heterogeneity could reflect misclassification or class imbalance rather than genuine differences in the untreated outside option. We therefore report both prediction-performance diagnostics and out-of-sample heterogeneous IV estimates. The classifier is trained only on lottery losers, with a holdout validation sample used to assess predictive performance; the reported metrics, including the ROC-based statistics and class-specific error rates, indicate that the model has useful discriminatory power even though the private-school class is relatively small (Table A.4).

We also compare the resulting RF-based subgroup estimates to the κ-weighted results, which provides an external consistency check because the two procedures rely on different identifying objects: the RF split uses predicted subgroup membership, whereas the reweighting approach combines lottery loser-based subgroup prediction with lottery-IV reweighting of the complier margin.

Across both approaches, a consistent pattern emerges: SWS primarily dominates the public outside option along broad educational and early labor-market margins, while improving academic performance for both public- and private-margin students. The convergence of results across two distinct approaches—one based on reweighting and the other on prediction-based sample splitting—supports the interpretation that outside options are a first-order determinant of treatment-effect heterogeneity in this setting.

By relocation distance. Because SWS are residential, most students relocate outside their home villages to attend. We geocode each SWS and compute Euclidean distance from the home village at the time of the lottery to the nearest SWS, splitting at the median. Table A.16 summarizes heterogeneous effects on network outcomes. For students farther from an SWS, network substitution is stronger: immediate-use networks (as defined through the borrowing or advice modules) have a smaller family share and a larger same-caste, non-family share. The "closest ten" roster also tilts more toward own-caste non-relatives. This is consistent with meeting-technology models in which where and with whom one spends formative time shapes subsequent link formation Jackson and Rogers, 2007a; Currarini et al., 2009: relocation weakens daily interaction with extended family and neighborhood acquaintances while intensifying ties to school peers, who in SWS are demographically similar. In labor markets where referrals matter, such re-composition can reduce exposure to heterogeneous opportunities even if the number of job-search contacts is unchanged Granovetter, 1973; Calvó-Armengol and Jackson, 2007.

By local school-market competitiveness. A natural extension of this analysis is to look at learning effects differentially by the quality of schooling available to students in terms of their outside option. We proxy the competitiveness or quality of the home education market by the density of schools in the village catchment. Table A.19 shows that learning gains are significantly larger in less competitive markets (fewer proximate schools). This pattern is consonant with models in which school choice delivers the largest human-capital improvements when baseline options are weak, and with evidence that school quality and management vary widely across Indian localities (Muralidharan and Sundararaman, 2015; Muralidharan and Singh, 2021). It is also in line with classic results on competition and school quality (Hoxby, 2000) and the role of private options in low-resource settings (Andrabi et al., 2017). In contrast, network-composition effects do not attenuate in more competitive markets, suggesting that the peer-exposure mechanism (more homogeneous cohorts in SWS) operates similarly across market structures and does not move directly with learning levels.

Across all four partitions, the same qualitative insights recur: (i) sustained gains in educational attainment and Grade 12 achievement (human-capital improvements), and (ii) smaller, more caste-homogeneous non-family networks (reduced network diversity/bridging). Gender differences and relocation amplify the network channel; the outside-option and market-competition splits amplify the human-capital channel. These deeper inquiries help reconcile the first-wave labor outcomes (higher participation without higher employment) with the pooled IV–difference-in-differences dynamics using both survey waves (lower participation by 2025 with flat employment): increases in human capital initially raise the perceived returns to search and exam-taking, while more homophilous networks constrain job-lead arrival and referral-based matching, producing longer early unemployment spells followed by exit from search as cohorts age.

Robustness

Our results are robust to a range of specifications. Our main treatment effects survive a different choice of instrument (using the intensive margin of SWS attendance, i.e., the number of years students spend at an SWS). These results are reported in Tables A.1–A.3. We also find that our results are robust to including observations where students' self-reported indicators on school choice disagree with the alumni records used for validation.

A natural question our results raise is how much of the effects might be driven solely by aggregate differences between public and private schools. To shed light on this, we focus on the set of lottery losers and use students' (pre-lottery) distance to the nearest private school as an instrument for their school choice. Within this set of lottery losers, we find no effects on educational attainment, grades, labor-market, or network outcomes (Table A.17). This finding is consistent with aggregate market-level competition inducing convergence in school performance in this region across school types (Andrabi et al., 2020). Our treatment effects on SWS students appear to be driven by systematic differences in the bundled nature of SWS—better infrastructure and teaching, as well as the residential model.

Given the network results we find, another question is whether these networks are shared by students and spill over onto other students (Jackson, 2014). To investigate this, we construct leave-out shares of SWS students among lottery applicants at the village level. This allows us to compare lottery losers in villages where more of their peers won the lottery to lottery losers in villages where fewer peers won the lottery. We do not find evidence of spillovers in network effects across our key social-network outcomes (Table A.18). This underscores that the network effects we observe arise from links formed while spending time at school, suggesting these are deep links and that peripheral exposure has limited observable value.

Attrition robustness: Lee bounds and IPW. Because Round 1 tracking was incomplete (cf. Section 3), we quantify the most adverse selection patterns using Lee (2009) trimming bounds. Appendix Tables A.15–A.17 report bounds computed by trimming the outcome distribution of the higher follow-up group by the observed winner–loser follow-up gap, separately by outcome family (education, labor, networks). Across specifications, the Lee lower and upper bounds remain the same sign as the IV point estimates and comfortably include the main effects, indicating that our conclusions are robust to worst-case monotone selection (Lee, 2009). As a complementary check, we implement inverse-probability weighting (IPW), constructing survey-response weights from pre-lottery predictors (gender, caste category, age at lottery, parental education, landholding, and local school-market variables). The IPW IV estimates are very similar to the unweighted IV results in levels and significance, reinforcing that differential tracking is unlikely to overturn our main findings (see Appendix Tables A.15–A.17).

7 Conclusion

This paper evaluates a large-scale affirmative-action schooling program that transforms both what students learn and whom they learn with. Using an admissions lottery into high-quality residential schools in Telangana, India, we show that targeted, high-quality residential schooling delivers substantial gains in human capital for historically marginalized students from underprivileged caste groups – higher test scores, more schooling, and greater college enrollment. Yet these gains coexist with a persistent contraction in network diversity. Students who learned more also became more socially homophilous by caste, were less likely to find jobs through their networks, and – especially among women – ultimately withdrew from the labor force despite higher early participation.

These findings underscore a central trade-off in the design of inclusive education systems: targeting improves learning, but network integration expands opportunity. Residential, caste-homogeneous schooling successfully protects and invests in disadvantaged students but also removes them from the mixed social environments where economic mobility is forged. In societies where labor markets operate through referrals and informal networks, the absence of heterogeneous ties constrains the very channels through which improved human capital can translate into better jobs.

Our theoretical framework formalizes this duality. Human capital raises the value of search and job-acceptance probabilities, while network diversity determines the arrival rate of offers. When targeted schooling improves the former but lowers the latter, early labor-force participation may rise without sustained employment gains. Over time, slower job-offer arrivals and discouragement lead to exit from search, a dynamic consistent with our longitudinal evidence.

A broader implication of our work is that in stratified and segregated societies, schools are often the only institutionalized sites of integration. Outside the classroom, residential patterns, marriage norms, and occupational segregation sharply limit cross-group interaction. When educational policy confines marginalized students to homogeneous peer environments, it inadvertently curtails one of the few opportunities for network bridging that such societies naturally provide. Targeted schools thus equalize test scores but can entrench social isolation, weakening the connective tissue of mobility.

A central challenge for educational policy in context like ours is not whether to target or to integrate, but how to balance protection with exposure. High-quality residential systems should be complemented by mechanisms that reintroduce diversity – structured exchanges with mainstream schools, alumni bridge programs, employer linkages, and mentorship networks that connect graduates to heterogeneous peers and firms. Such design choices would allow targeted schooling to preserve its strengths in resource delivery and safety while restoring its integrative function.

Ultimately, our results highlight that the returns to education depend as much on who students learn beside as on what they learn within. When schools serve as both places of learning and integration, they can equalize not only achievement but also access. Failing to preserve that dual role risks reproducing inequality even as average learning improves.

Tables and figures

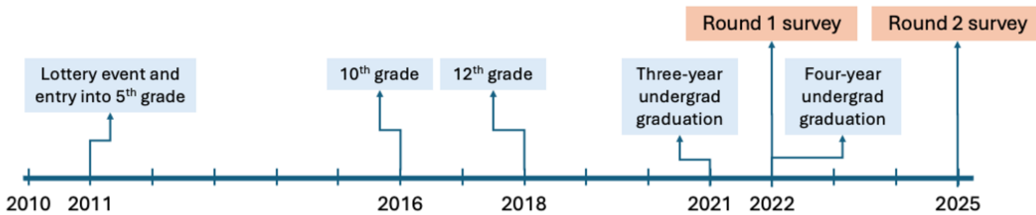

Figure 1: Timeline of educational milestones and survey rounds

Notes: This figure plots the study timeline for the 2010–11 entry cohort. It marks key schooling milestones (Grade 5 entry into the application pool, Grade 10 completion, and Grade 12 completion) and the timing of two adult follow-ups (Round 1 at approximately age 21 and Round 2 at approximately age 25). The figure is intended to orient the reader to the sequence of education and survey events used in the analysis; no estimates are presented here.

Table 1: Baseline balance by lottery status

	(1)	(2)	(3)
	Means		Difference
Variable	Control	Treatment	
Neither parent literate	0.744	0.718	-0.008
	(0.437)	(0.450)	(0.022)
Land owned	4.235	3.703	0.036
	(2.485)	(2.413)	(0.119)
Distance to nearest SWS	10.776	10.190	-0.548
	(7.137)	(7.117)	(0.354)
Distance to nearest public school	0.481	0.446	-0.001
	(0.477)	(0.467)	(0.023)
Distance to nearest private school	3.567	3.517	0.230
	(3.294)	(3.281)	(0.158)
Student-teacher ratio in nearest school	23.808	23.037	-0.319
	(10.927)	(10.373)	(0.544)
Prop. from social group in public school	27.960	26.279	-1.989
	(22.781)	(21.601)	(1.343)
Prop. from social group in private school	12.804	12.155	-0.898
	(10.614)	(10.084)	(0.677)
Observations	1,084	924	2,008

This table compares baseline characteristics of lottery losers (Column 1) and winners (Column 2). Column 3 reports regression-adjusted differences (winner minus loser) from linear regressions controlling for baseline demographics. Baseline covariates are measured prior to random assignment using administrative applicant records and application forms. The unit of observation is the applicant, and the sample includes all tracked respondents. Robust standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Non-response analysis

	(1)	(2)	(3)
	Means		Difference
Variable	Tracked	Not tracked	
Attended SWS	0.187	0.204	0.015
	(0.390)	(0.403)	(0.014)
Gender	0.460	0.381	-0.027
	(0.499)	(0.486)	(0.033)
Scheduled caste	0.569	0.503	-0.042
	(0.495)	(0.500)	(0.030)
Scheduled tribe	0.116	0.112	0.002
	(0.320)	(0.316)	(0.016)
Other castes	0.315	0.385	0.040
	(0.465)	(0.487)	(0.028)
Observations	2,007	1,632	3,639

This table compares baseline characteristics of applicants who were successfully tracked and surveyed (Column 1) and those who were not (Column 2). Column 3 reports the mean difference (tracked minus non-tracked) with robust standard errors in parentheses. Baseline covariates are measured prior to random assignment using administrative applicant records and application forms. The unit of observation is the applicant, and the sample includes all original lottery participants. No controls or weights are used. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: First stage

	Social welfare schools		Other public schools	Private schools
	Ever	Years		
Won lottery	0.594***	1.996***	-0.381***	-0.215***
	(0.019)	(0.103)	(0.023)	(0.017)
Lottery losers mean	0.082	0.461	0.626	0.292
First-stage F-stat	947.7	374.1	279.1	161.5
Adj. R^2	0.48	0.26	0.18	0.14
Observations	2007	2007	2007	2007

This table reports first-stage estimates from 2SLS regressions using random lottery assignment as an instrument for Social Welfare School (SWS) attendance. Column 1 reports effects on an indicator for ever attending an SWS. Column 2 uses years spent at an SWS as the outcome. Columns 3 and 4 report effects on attendance at any other public or private school, respectively. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Effects of SWS attendance on educational attainment

	Years of ed	Continuing in school	Higher ed
	(1)	(2)	(3)
SWS	0.384**	0.051*	0.071*
	(0.192)	(0.029)	(0.041)
Lottery losers mean	12.610	0.807	0.364
Adj. R^2	0.03	0.03	0.01
Observations	2007	2007	2007

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on educational attainment, using random lottery assignment as an instrument for attendance. Column 1 reports years of completed schooling. Column 2 reports an indicator for continuing beyond Grade 10. Column 3 reports an indicator for ever enrolling in college. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Grade 12 completion and scores

	Passed XIIth	XIIth grade marks
	(1)	(2)
SWS	0.050*	0.263***
	(0.030)	(0.079)
Lottery losers mean	0.80	-0.00
Adj. R^2	0.03	0.04
Observations	2007	2003

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on Grade 12 outcomes, using random lottery assignment as an instrument for attendance. Column 1 reports an indicator for passing the Grade 12 examination. Column 2 reports standardized Grade 12 marks. Respondents who did not sit the Grade 12 exam are coded as not passing (0) in Column 1 and assigned a standardized score of 0 in Column 2; estimates should therefore be interpreted on the full sample. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Effects of SWS attendance on early labor-market participation and employment

	In labor force	Working	Unemployed
	(1)	(2)	(3)
SWS	0.095**	0.003	0.092**
	(0.038)	(0.029)	(0.040)
Lottery losers mean	0.59	0.14	0.45
Adj. R^2	0.17	0.06	0.11
Observations	2007	2007	2007

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on early labor-market outcomes, using random lottery assignment as an instrument for attendance. Column 1 reports an indicator for labor-force participation, defined as working or actively seeking work. Column 2 reports an indicator for currently working for pay. Column 3 reports an indicator for being unemployed, defined as not working but actively seeking work. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: SWS attendance and competitive examinations

	Any exam	Number of attempts	Types of exams
SWS	0.037	0.081*	0.056
	(0.035)	(0.045)	(0.039)
Lottery losers mean	0.214	0.240	0.219
Adj. R^2	0.02	0.02	0.02
Observations	2007	2006	2007

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on competitive examination-taking, using random lottery assignment as an instrument for attendance. Column 1 reports an indicator for any attempt at a competitive examination. Column 2 reports the total number of attempts. Column 3 reports the number of distinct examination types attempted. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Network size and composition (borrowing)

	Network size	Family	Caste (outside family)	Others
SWS	-0.084	-0.023*	0.075***	-0.052**
	(0.123)	(0.012)	(0.026)	(0.024)
Lottery losers mean	2.175	0.049	0.761	0.190
Adj. R^2	0.01	0.03	0.02	0.01
Observations	1826	1825	1825	1825

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on the size and composition of borrowing networks, using random lottery assignment as an instrument for attendance. Column 1 reports the number of named contacts respondents could borrow from. Columns 2–4 report the composition of borrowing contacts by category: family members, own-caste non-family contacts, and other-caste non-family contacts, measured as counts or shares as defined in the table. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9: Network size and composition (advice)

	Network size	Family	Caste (outside family)	Others
SWS	-0.215**	-0.034***	0.040	-0.006
	(0.100)	(0.012)	(0.024)	(0.022)
Lottery losers mean	2.302	0.057	0.779	0.164
Adj. R^2	0.02	0.00	0.03	0.02
Observations	1826	1821	1821	1821

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on the size and composition of advice networks, using random lottery assignment as an instrument for attendance. Column 1 reports the number of individuals respondents could seek advice from. Columns 2–4 report the composition of advice contacts by category: family members, own-caste non-family contacts, and other-caste non-family contacts, measured as counts or shares as defined in the table. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Origins of closest social ties

	School	Other ed	Work	Family	Other
SWS	-0.081	-0.006	0.108	-0.090	0.069
	(0.122)	(0.117)	(0.087)	(0.196)	(0.126)
Lottery losers mean	1.194	0.988	0.406	6.507	0.905
Adj. R^2	0.01	0.00	0.00	0.02	-0.00
Observations	1826	1826	1826	1826	1826

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on the origins of respondents' ten closest social ties, using random lottery assignment as an instrument for attendance. Each column reports the number or share, as defined in the table, of a respondent's ten closest contacts formed in school, other educational settings, the workplace, the family, or other settings. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11: Marriage and fertility

	Married	Age at marriage	Any children
SWS	-0.005	0.284	-0.008
	(0.032)	(0.214)	(0.027)
Lottery losers mean	0.365	24.931	0.117
Adj. R^2	0.36	0.28	0.15
Observations	2007	1825	2007

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on marriage and fertility outcomes, using random lottery assignment as an instrument for attendance. Column 1 reports an indicator for being married at the time of survey. Column 2 reports age at first marriage. Column 3 reports an indicator for having any children; coding for respondents not yet married follows the pre-specified outcome definition. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 12: Spousal expectations

	Years of education	Age
SWS	1.103***	0.095
	(0.313)	(0.177)
Lottery losers mean	13.673	24.227
Adj. R^2	0.16	0.52
Observations	1352	1352

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on expectations about future spouses, using random lottery assignment as an instrument for attendance. Column 1 reports expected spouse's years of education. Column 2 reports expected spouse's age. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 13: Labor-market outcomes (pooled IV–DiD estimates)

	Working	In labor force	Unemployed	Log monthly earnings	Log annual earnings	Took any exam
Round 2	0.352***	0.028	-0.164***	3.122***	4.282***	0.348***
	(0.041)	(0.050)	(0.049)	(0.389)	(0.468)	(0.054)
SWS × Round 2	-0.063	-0.275***	-0.221**	-0.750	-1.158	-0.026
	(0.071)	(0.087)	(0.089)	(0.697)	(0.839)	(0.099)
Lottery losers mean (round 2)	0.494	0.658	0.323	4.778	6.290	0.617
Adj. R^2						
Observations	1020	1020	1020	1020	1020	1020

This table reports difference-in-differences 2SLS estimates of the effect of attending a Social Welfare School (SWS) on labor-market outcomes between the first (Round 1) and second (Round 2) adult follow-ups. The coefficient on *SWS × Round 2* captures the change in the treatment effect between Round 1 and Round 2. Outcomes include labor-force participation, employment, and unemployment. All specifications include baseline controls for age, gender, and caste, as well as survey-month fixed effects. The sample consists of respondents observed in both rounds ($N = 576$). Robust standard errors are reported in parentheses. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 14: Marriage and fertility (pooled IV–DiD estimates)

	Married	Age at marriage	Has children
Round 2	0.033	-0.416***	0.121***
	(0.036)	(0.128)	(0.025)
SWS × Round 2	0.074	0.257	-0.060
	(0.059)	(0.249)	(0.046)
Lottery losers mean (round 2)	0.349	24.667	0.227
Adj. R^2			
Observations	1020	1020	1020

This table reports difference-in-differences 2SLS estimates of the effect of attending a Social Welfare School (SWS) on marriage and fertility outcomes between the first (Round 1) and second (Round 2) adult follow-ups. The coefficient on *SWS × Round 2* captures the change in the treatment effect between Round 1 and Round 2. Outcomes include an indicator for being married, age at first marriage, and an indicator for having any children. All specifications include baseline controls for age, gender, and caste, as well as survey-month fixed effects. The sample consists of respondents observed in both rounds ($N = 576$). Robust standard errors are reported in parentheses. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 15: Closest-ties composition (pooled IV–DiD estimates)

	Closest 10: other ed	Closest 10: from work	Closest 10: family	Closest 10: other	Closest 10: own caste	Closest 10: other caste
Round 2	0.152	0.608***	-2.447***	-0.241*	-1.717***	1.717***
	(0.163)	(0.149)	(0.298)	(0.131)	(0.290)	(0.290)
SWS × Round 2	-0.061	-0.278	0.712	-0.407*	1.048**	-1.048**
	(0.305)	(0.249)	(0.547)	(0.230)	(0.506)	(0.506)
Lottery losers mean (round 2)	1.194	1.004	4.119	0.313	5.216	4.784
Adj. R^2						
Observations	983	983	983	983	982	982

This table reports difference-in-differences 2SLS estimates of how attending a Social Welfare School (SWS) changes the composition of respondents' ten closest social ties between the first (Round 1) and second (Round 2) adult follow-ups. The coefficient on *SWS × Round 2* captures the change in the treatment effect between Round 1 and Round 2. Outcomes are counts or shares, as reported in the table, of closest ties by kinship and caste category. All specifications include baseline controls for age, gender, and caste, as well as survey-month fixed effects. The sample consists of respondents observed in both rounds ($N = 576$). Robust standard errors are reported in parentheses. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 16: Job search methods and referrals (Round 2 only, IV)

	Found job through referrals	Job search network: size	Job search network: same caste
SWS	-0.353*	-0.252	0.481
	(0.191)	(0.731)	(0.292)
Lottery losers mean	0.216	2.289	0.831
Adj. R^2	0.16	0.04	0.02
Observations	509	506	507

This table reports cross-sectional 2SLS estimates of the effect of attending a Social Welfare School (SWS) on job-search methods and referral-based job finding in Round 2. Outcomes include indicators for job-search channels and for obtaining a job through referrals, as reported in the table. Random lottery assignment is used as an instrument for SWS attendance. All specifications include baseline controls for age, gender, and caste, as well as Round 2 survey-month fixed effects. The sample consists of respondents surveyed in Round 2 ($N = 576$). Robust standard errors are reported in parentheses. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 17: Other labor-market outcomes (Round 2 only, IV)

	Most recent unemployment duration	Self employed	Any education in the last 3 years
SWS	-5.007	-0.155**	0.175
	(10.660)	(0.078)	(0.142)
Lottery losers mean	30.067	0.112	0.405
Adj. R^2	0.06	0.51	0.10
Observations	509	509	509

This table reports cross-sectional 2SLS estimates of the effect of attending a Social Welfare School (SWS) on additional labor-market outcomes measured in Round 2. Outcomes include self-employment, unemployment duration (in months), and participation in recent training or education programs. Random lottery assignment is used as an instrument for SWS attendance. All specifications include baseline controls for age, gender, and caste, as well as Round 2 survey-month fixed effects. The sample consists of respondents surveyed in Round 2 ($N = 576$). Robust standard errors are reported in parentheses. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 18: Education by gender

	Years of ed		Continuing in school		Higher ed	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male
SWS	0.297	0.482*	0.050	0.055	0.092	0.055
	(0.301)	(0.249)	(0.044)	(0.040)	(0.067)	(0.052)
Heterogeneity p-value		0.957		0.712		0.279
Lottery losers mean	12.86	12.47	0.83	0.79	0.41	0.34
Adj. R^2	0.03	0.01	0.05	0.01	0.01	-0.01
Observations	909	1098	909	1098	909	1098

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on educational attainment, using random lottery assignment as an instrument for attendance. Columns 1–2 report years of completed schooling; Columns 3–4 report an indicator for continuing beyond Grade 10; Columns 5–6 report an indicator for ever enrolling in college. Odd-numbered columns report estimates for females and even-numbered columns report estimates for males; the pooled interaction test is reported in the heterogeneity row. All specifications include baseline controls for age and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 19: Grade 12 outcomes by gender

	Passed XIIth		XIIth grade marks	
	(1) Female	(2) Male	(3) Female	(4) Male
SWS	0.055	0.053	0.267**	0.266***
	(0.046)	(0.040)	(0.130)	(0.101)
Heterogeneity p-value		0.754		0.770
Lottery losers mean	0.82	0.79	0.09	-0.05
Adj. R^2	0.05	0.01	0.03	0.02
Observations	909	1098	908	1095

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on Grade 12 outcomes, using random lottery assignment as an instrument for attendance. Columns 1–2 report an indicator for passing the Grade 12 examination; Columns 3–4 report standardized Grade 12 marks. Respondents who did not sit the Grade 12 exam are coded as not passing (0) in Columns 1–2 and assigned a standardized score of 0 in Columns 3–4. Odd-numbered columns report estimates for females and even-numbered columns report estimates for males; the pooled interaction test is reported in the heterogeneity row. All specifications include baseline controls for age and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 20: Labor-market outcomes by gender

	In labor force		Working		Unemployed	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male
SWS	0.203***	0.028	-0.015	0.035	0.219***	-0.007
	(0.066)	(0.047)	(0.037)	(0.043)	(0.067)	(0.052)
Heterogeneity p-value		0.049		0.046		0.001
Lottery losers mean	0.50	0.63	0.06	0.18	0.45	0.45
Adj. R^2	0.14	0.19	-0.00	0.04	0.10	0.09
Observations	909	1098	909	1098	909	1098

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on early labor-market outcomes, using random lottery assignment as an instrument for attendance. Columns 1–2 report labor-force participation; Columns 3–4 report current employment; Columns 5–6 report unemployment, defined as not working but actively seeking work. Odd-numbered columns report estimates for females and even-numbered columns report estimates for males; the pooled interaction test is reported in the heterogeneity row. All specifications include baseline controls for age and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 21: Marriage and fertility by gender

	Married		Age at marriage		Any children	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male
SWS	-0.065	0.006	0.709*	-0.015	-0.072	0.006
	(0.061)	(0.032)	(0.385)	(0.246)	(0.059)	(0.023)
Heterogeneity p-value		0.928		0.012		0.489
Lottery losers mean	0.59	0.24	22.58	26.08	0.25	0.05
Adj. R^2	0.23	0.43	0.08	0.01	0.15	0.04
Observations	909	1098	824	1001	909	1098

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on marriage and fertility outcomes, using random lottery assignment as an instrument for attendance. Columns 1–2 report an indicator for being married at the time of survey; Columns 3–4 report age at first marriage; Columns 5–6 report an indicator for having any children. Odd-numbered columns report estimates for females and even-numbered columns report estimates for males; the pooled interaction test is reported in the heterogeneity row. All specifications include baseline controls for age and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 22: Spousal expectations by gender

	Years of education		Age	
	Female	Male	Female	Male
SWS	0.877*	1.130***	0.193	-0.031
	(0.522)	(0.396)	(0.264)	(0.227)
Heterogeneity p-value		0.901		0.258
Lottery losers mean	15.871	13.034	27.252	23.347
Adj. R^2	-0.01	0.01	0.04	-0.01
Observations	465	887	465	887

This table reports 2SLS estimates of the effect of attending a Social Welfare School (SWS) on expectations about future spouses, using random lottery assignment as an instrument for attendance. Columns 1–2 report expected spouse's years of education; Columns 3–4 report expected spouse's age. Odd-numbered columns report estimates for females and even-numbered columns report estimates for males; the pooled interaction test is reported in the heterogeneity row. All specifications include baseline controls for age and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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Appendix

A Supplementary exhibits

Figures

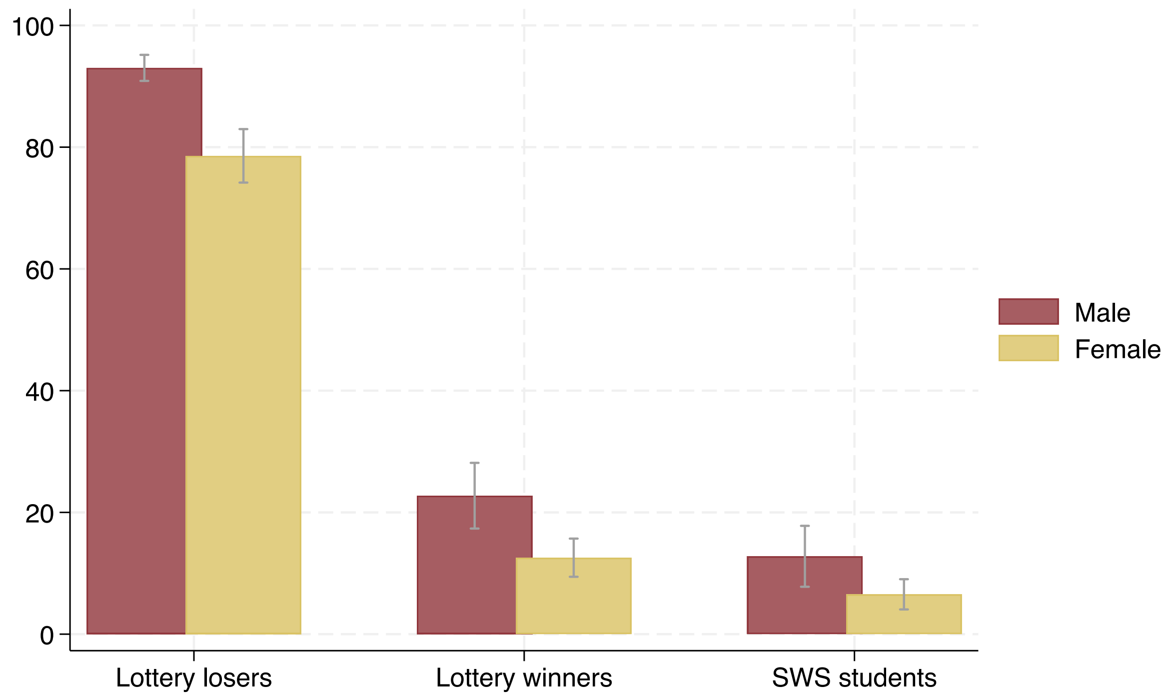

Figure A.1: Probability of attending a mixed-gender school, by gender

Notes: This figure displays the probability that applicants attend a mixed-gender (coeducational) school, separately for females and males. The sample consists of applicants from the 2010–11 entry cohort. The figure is descriptive and intended to document realized school type by gender; no causal estimates are presented.

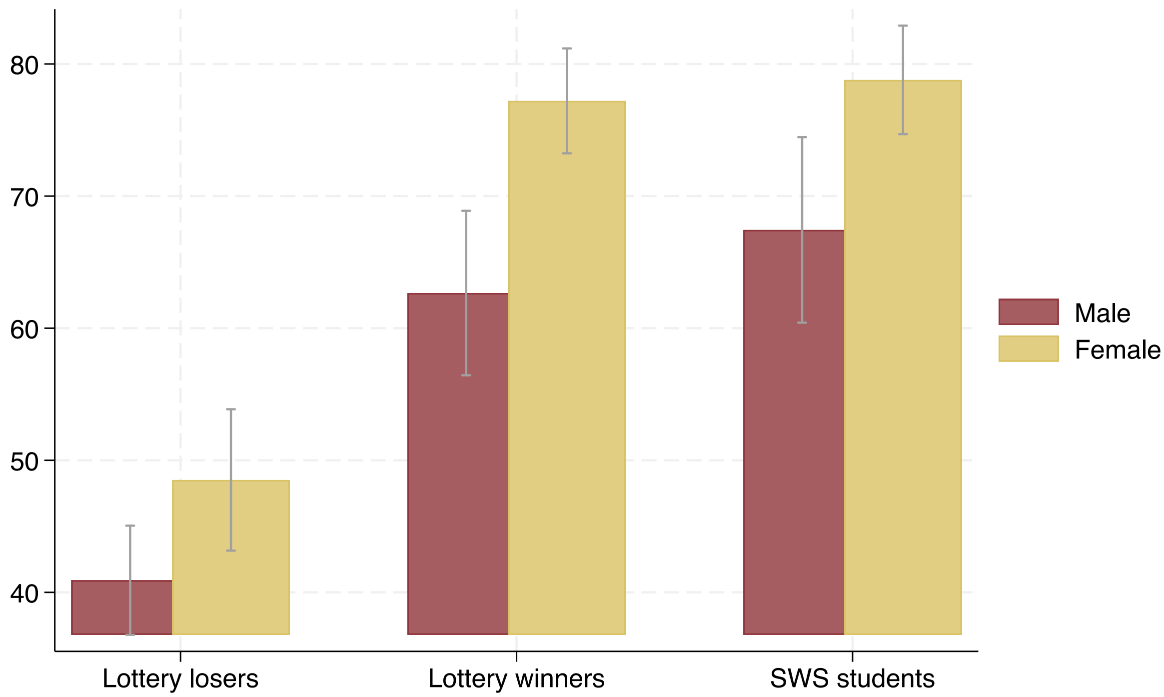

Figure A.2: Probability of attending an own-caste-majority school, by gender

Notes: This figure displays the probability that applicants attend a school in which the majority of enrolled students are of the respondent's own caste group, separately for females and males. The sample consists of applicants from the 2010–11 entry cohort. The figure is descriptive and intended to document realized school composition by gender; no causal estimates are presented.

Table A.1: Effects of years at SWS on educational attainment

	Years of ed	Continuing in school	Higher ed
	(1)	(2)	(3)
Years in SWS	0.114**	0.015*	0.021*
	(0.057)	(0.009)	(0.012)
Lottery losers mean	12.610	0.807	0.364
Adj. R^2	0.04	0.03	0.02
Observations	2007	2007	2007

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for years spent at a Social Welfare School (SWS). Column 1 reports years of completed schooling. Column 2 reports an indicator for continuing beyond Grade 10. Column 3 reports an indicator for any college enrollment. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Effects of years at SWS on Grade 12 completion and scores

	Passed XIIth	XIIth grade marks
	(1)	(2)
Years in SWS	0.015*	0.079***
	(0.009)	(0.024)
Lottery losers mean	0.80	-0.00
Adj. R^2	0.03	0.03
Observations	2007	2003

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for years spent at a Social Welfare School (SWS). Column 1 reports an indicator for passing the Grade 12 examination. Column 2 reports standardized Grade 12 marks. Respondents who did not sit the Grade 12 exam are coded as not passing (0) in Column 1 and assigned a standardized score of 0 in Column 2; estimates are therefore interpreted on the full sample. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Effects of years at SWS on early labor-market outcomes

	In labor force	Working	Unemployed
	(1)	(2)	(3)
Years in SWS	0.028**	0.001	0.027**
	(0.011)	(0.009)	(0.012)
Lottery losers mean	0.59	0.14	0.45
Adj. R^2	0.16	0.06	0.08
Observations	2007	2007	2007

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for years spent at a Social Welfare School (SWS). Column 1 reports labor-force participation, defined as working or actively seeking work. Column 2 reports current employment, defined as working for pay. Column 3 reports an indicator for being unemployed, defined as not working but actively seeking work. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$.

Table A.4: Random-forest classification performance

Parameter	Value
True negative rate (TNR)	0.973
True positive rate (TPR)	0.313
Positive predictive value (PPV)	0.667
Negative predicted value (NPV)	0.891
Mathews correlation coefficient (MCC)	0.399
F1 score	0.426
Diagnostic odds ratio (DOR)	16.363

Notes: This table reports performance metrics from the random-forest model used to predict each applicant's outside option. The positive class corresponds to private-school outside options, and the negative class corresponds to public-school outside options. Reported statistics include accuracy, precision, recall, and the area under the ROC curve, based on cross-validated predictions.

Table A.5: Effects of SWS attendance on educational attainment, by predicted outside option (ML split)

	Years of ed		Continuing in school		Higher ed	
	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	0.536***	1.682	0.062**	0.261	0.096**	0.511
	(0.205)	(1.475)	(0.031)	(0.224)	(0.042)	(0.441)
Lottery losers mean	12.437	13.246	0.790	0.871	0.337	0.466
HTE p-val		0		0		0
Adj. R^2	0.04	0.00	0.03	0.03	0.02	-0.03
Observations	1738	269	1738	269	1738	269

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is split by predicted outside option using a random-forest classifier that distinguishes likely public- and private-school counterfactuals. Column 1 reports years of completed schooling. Column 2 reports an indicator for continuing beyond Grade 10. Column 3 reports an indicator for any college enrollment. The final column reports the p-value for equality of treatment effects across ML groups. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Effects of SWS attendance on Grade 12 outcomes, by predicted outside option (ML split)

	Passed XIIth		XIIth grade marks	
	Pub	Pvt	Pub	Pvt
SWS	0.063*	0.261	0.294***	1.488
	(0.032)	(0.224)	(0.083)	(0.975)
Lottery losers mean	0.780	0.871	-0.051	0.181
HTE p-val		0		0
Adj. R^2	0.03	0.03	0.04	-0.07
Observations	1738	269	1736	267

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is split by predicted outside option using the random-forest classifier. Column 1 reports an indicator for passing the Grade 12 examination. Column 2 reports standardized Grade 12 marks. Respondents who did not sit the Grade 12 exam are coded as not passing (0) in Column 1 and assigned a standardized score of 0 in Column 2; estimates are therefore interpreted on the full sample. The final column reports the p-value for equality of treatment effects across ML groups. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Effects of SWS attendance on early labor-market outcomes, by predicted outside option (ML split)

	In labor force		Working		Unemployed	
	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	0.083**	0.005	-0.006	-0.008	0.088**	0.013
	(0.040)	(0.371)	(0.031)	(0.243)	(0.043)	(0.395)
Lottery losers mean	0.584	0.591	0.142	0.121	0.442	0.470
HTE p-val		.905		.014		.313
Adj. R^2	0.18	0.17	0.06	0.03	0.11	0.08
Observations	1738	269	1738	269	1738	269

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is split by predicted outside option using the random-forest classifier. Column 1 reports labor-force participation, defined as working or actively seeking work. Column 2 reports current employment, defined as working for pay. Column 3 reports an indicator for being unemployed, defined as not working but actively seeking work. The final column reports the p-value for equality of treatment effects across ML groups. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Effects of SWS attendance on advice networks, by predicted outside option (ML split)

	Network size		Family		Caste (outside family)		Others	
	Pub	Pvt	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	-0.241**	0.542	-0.037***	-0.059	0.051**	-0.148	-0.014	0.206
	(0.106)	(0.901)	(0.013)	(0.096)	(0.025)	(0.260)	(0.023)	(0.256)
Lottery losers mean	2.320	2.245	0.059	0.050	0.777	0.783	0.164	0.167
HTE p-val		.338		.522		.527		.023
Adj. R^2	0.01	-0.03	-0.00	-0.05	0.02	-0.01	0.02	-0.07
Observations	1581	245	1576	245	1576	245	1576	245

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is split by predicted outside option using the random-forest classifier. Column 1 reports the total number of individuals respondents could seek advice from. Column 2 reports family advice contacts. Column 3 reports own-caste non-family advice contacts. Column 4 reports other-caste non-family advice contacts. The final column reports the p-value for equality of treatment effects across ML groups. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$.

Table A.9: Effects of SWS attendance on borrowing networks, by predicted outside option (ML split)

	Network size		Family		Caste (outside family)		Others	
	Pub	Pvt	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	-0.049	0.653	-0.026**	-0.019	0.090***	-0.576	-0.064***	0.595
	(0.128)	(1.146)	(0.013)	(0.096)	(0.027)	(0.374)	(0.025)	(0.373)
Lottery losers mean	2.134	2.312	0.050	0.043	0.762	0.758	0.188	0.199
HTE p-val		.127		.961		.464		.448
Adj. R^2	0.00	-0.01	0.03	-0.01	0.01	-0.10	0.01	-0.16
Observations	1581	245	1580	245	1580	245	1580	245

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is split by predicted outside option using the random-forest classifier. Column 1 reports the total number of individuals respondents could borrow from. Column 2 reports family borrowing contacts. Column 3 reports own-caste non-family borrowing contacts. Column 4 reports other-caste non-family borrowing contacts. The final column reports the p-value for equality of treatment effects across ML groups. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$.

Table A.10: Aggregate network measures, by predicted outside option (ML split)

	Network size		Family		Caste (outside family)	
	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	-0.030**	-0.050	0.071***	-0.317	-0.041*	0.367
	(0.012)	(0.086)	(0.023)	(0.251)	(0.021)	(0.254)
Lottery losers mean	0.056	0.049	0.761	0.762	0.183	0.189
HTE p-val		.574		.989		.485
Adj. R^2	0.01	-0.03	0.02	-0.04	0.02	-0.15
Observations	1576	245	1576	245	1576	245

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is split by predicted outside option using the random-forest classifier. Columns report composite measures of overall network size and diversity constructed from the borrowing and advice modules. The final column reports the p-value for equality of treatment effects across ML groups. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: Education and labor-force participation, by likely outside option (Kappa weights)

	Years of ed		Continuing in school		Higher ed		In labor force		Working		Unemployed	
	Pub	Pvt	Pub	Pvt	Pub	Pvt	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	1.706***	-2.384**	0.156***	-0.092	0.150***	0.009	0.288***	0.074	0.005	-0.016	0.282***	0.090
	(0.350)	(1.131)	(0.032)	(0.079)	(0.033)	(0.057)	(0.034)	(0.069)	(0.023)	(0.035)	(0.035)	(0.064)
HTE p-val	0.004		0.008		0.007		0.004		0.438		0.003	
Observations	2008		2008		2008		2008		2008		2008	

Notes: This table reports model-assisted subgroup-LATE estimates by likely untreated outside option, using random lottery assignment as an instrument for SWS attendance. Subgroup probabilities are estimated on lottery losers only using pre-lottery covariates, and the resulting predicted probability of a private-school outside option is used to form soft subgroup weights. Columns labeled "Pub" use weights $1 - \hat{s}_i$ and columns labeled "Pvt" use weights $\hat{s}_i$, where $\hat{s}_i = \Pr(A_i(0) = \text{private} \mid X_i, Z_i = 0)$. Outcomes include years of education, continuation beyond Grade 10, higher education, labor-force participation, current employment, and unemployment. The row "HTE p-val" reports the bootstrap p-value for equality of the public- and private-margin estimates. Standard errors are reported in parentheses and are obtained by bootstrap re-estimation of the outside-option model in each draw. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates should be interpreted as model-assisted heterogeneous local average treatment effects rather than fully design-identified principal-stratum effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: Network composition, by likely outside option (Kappa weights)

	Family		Caste (outside family)		Others	
	Pub	Pvt	Pub	Pvt	Pub	Pvt
SWS	-0.014	-0.039***	0.139***	-0.107	-0.034*	-0.114***
	(0.009)	(0.015)	(0.029)	(0.070)	(0.018)	(0.031)
HTE p-val	0.035		0.005		0.004	
Observations	1822		1822		1822	

Notes: This table reports model-assisted subgroup-LATE estimates by likely untreated outside option, using random lottery assignment as an instrument for SWS attendance. Subgroup probabilities are estimated on lottery losers only using pre-lottery covariates, and the resulting predicted probability of a private-school outside option is used to form soft subgroup weights. Columns labeled "Pub" use weights $1 - \hat{s}_i$ and columns labeled "Pvt" use weights $\hat{s}_i$, where $\hat{s}_i = \Pr(A_i(0) = \text{private} \mid X_i, Z_i = 0)$. Outcomes report the composition of the network defined by each respondent's 10 closest ties, by family ties, same-caste non-family ties, and other ties. The row "HTE p-val" reports the bootstrap p-value for equality of the public- and private-margin estimates. Standard errors are reported in parentheses and are obtained by bootstrap re-estimation of the outside-option model in each draw. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates should be interpreted as model-assisted heterogeneous local average treatment effects rather than fully design-identified principal-stratum effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.13: Network size and composition, by likely outside option (Kappa weights)

	Network size		Family		Caste (outside family)		Others	
	Pub	Pvt	Pub	Pvt	Pub	Pvt	Pub	Pvt
Panel A. Social networks for borrowing								
SWS	0.073	-0.788***	-0.005	-0.032**	0.147***	-0.092	-0.050**	-0.138***
	(0.108)	(0.243)	(0.011)	(0.015)	(0.031)	(0.070)	(0.021)	(0.036)
HTE p-val	0.001		0.022		0.006		0.005	
Observations	1827		1826		1826		1826	
Panel B. Social networks for advice								
SWS	0.062	-0.837***	-0.024**	-0.046***	0.130***	-0.128*	-0.015	-0.085***
	(0.102)	(0.227)	(0.010)	(0.016)	(0.030)	(0.075)	(0.019)	(0.030)
HTE p-val	0.001		0.080		0.005		0.006	
Observations	1827		1822		1822		1822	

Notes: This table reports model-assisted subgroup-LATE estimates by likely untreated outside option, using random lottery assignment as an instrument for SWS attendance. Subgroup probabilities are estimated on lottery losers only using pre-lottery covariates, and the resulting predicted probability of a private-school outside option is used to form soft subgroup weights. Columns labeled “Pub” use weights $1-\hat{s}_i$ and columns labeled “Pvt” use weights $\hat{s}_i$, where $\hat{s}_i = \Pr(A_i(0) = \text{private} \mid X_i, Z_i = 0)$. Panel A reports effects on borrowing networks, and Panel B reports effects on advice networks. “Network size” is the total number of reported contacts in the corresponding module, while “Family,” “Caste (outside family),” and “Others” report the share of contacts in each category. The row “HTE p-val” reports the bootstrap p-value for equality of the public- and private-margin estimates. Standard errors are reported in parentheses and are obtained by bootstrap re-estimation of the outside-option model in each draw. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates should be interpreted as model-assisted heterogeneous local average treatment effects rather than fully design-identified principal-stratum effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.14: Robustness of Kappa-reweighting sample selection

	Baseline support		Trimmed support	
	Pub	Pvt	Pub	Pvt
Years of ed	1.706***	-2.384**	1.615***	-2.108**
	(0.350)	(1.131)	(0.336)	(0.960)
XIIth grade marks	0.355***	0.370***	0.343***	0.369***
	(0.070)	(0.094)	(0.072)	(0.095)
In labor force	0.288***	0.074	0.270***	0.084
	(0.034)	(0.069)	(0.036)	(0.062)
Unemployed	0.282***	0.090	0.261***	0.095
	(0.035)	(0.064)	(0.037)	(0.059)
Caste (total)	0.139***	-0.107	0.140***	-0.094
	(0.029)	(0.070)	(0.029)	(0.061)
Other ties (total)	-0.034*	-0.114***	-0.043**	-0.113***
	(0.018)	(0.031)	(0.020)	(0.030)
Trim rule	None		$0.10 \le \hat{s}_i \le 0.90$	

Notes: This table reports robustness checks for the model-assisted subgroup-LATE estimator by likely untreated outside option. Baseline columns report subgroup-specific weighted Wald estimates using the full sample, while trimmed-support columns restrict the sample to observations with predicted private-school probability in the interval $[0.10, 0.90]$. This trimming exercise tests whether the main heterogeneity results are driven by observations with extreme predicted subgroup membership. Subgroup probabilities are estimated on lottery losers only using pre-lottery covariates, and random lottery assignment is used as an instrument for SWS attendance. Columns labeled "Pub" use weights $1 - \hat{s}_i$ and columns labeled "Pvt" use weights $\hat{s}_i$, where $\hat{s}_i = \Pr(A_i(0) = \text{private} \mid X_i, Z_i = 0)$. Standard errors are reported in parentheses and are obtained by bootstrap re-estimation of the outside-option model in each draw. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates should be interpreted as model-assisted heterogeneous local average treatment effects rather than fully design-identified principal-stratum effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.15: Kappa-reweighting diagnostics

	Baseline support		Trimmed support	
	Public	Private	Public	Private
Predicted complier share	0.680	0.320	0.678	0.322
Abadie complier share	0.617	0.371	0.613	0.375
Effective sample size	1924.3	1605.9	1870.2	1583.8
Weighted first stage	0.482	0.168	0.481	0.174

Notes: This table reports diagnostic statistics for the model-assisted subgroup-LATE estimator by likely untreated outside option. "Predicted complier share" gives the average soft subgroup weight among untreated compliers, while "Abadie complier share" gives the corresponding design-based subgroup share recovered using Abadie-style control-state weights. "Effective sample size" reports the Kish-style effective sample size implied by the subgroup weights, and "Weighted first stage" reports the subgroup-specific first stage for SWS attendance. Baseline columns use the full sample, while trimmed-support columns restrict the sample to observations with predicted private-school probability in the interval $[0.10, 0.90]$. Predicted subgroup probabilities are estimated on lottery losers only using pre-lottery covariates, and random lottery assignment is used to identify the complier margin. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort.

Table A.16: Effects of SWS attendance on networks, by distance to nearest SWS

	SWS > 8 km				SWS ≤ 8 km			
	Network size	Family	Caste	Other	Network size	Family	Caste	Other
SWS	-0.079	-0.094**	0.158*	-0.021	-0.000***	0.004	0.127	-0.045
	(0.147)	(0.043)	(0.086)	(0.023)	(0.000)	(0.076)	(0.116)	(0.041)
Lottery losers mean	2.175	0.157	1.453	0.050	2.175	0.157	1.453	0.050
Adj. R^2	0.01	0.10	0.37	0.01	1.00	0.19	0.31	0.03
Observations	863	862	862	862	785	785	785	785

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is divided by distance to the nearest SWS, split at the median distance of 8 km. Column 1 reports the total number of individuals respondents could borrow from. Column 2 reports family borrowing contacts. Column 3 reports own-caste non-family borrowing contacts. Column 4 reports other-caste non-family borrowing contacts. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.17: Private-school distance as an instrument among lottery losers

	Education			Social networks	
	Years of ed	Continuing in school	Higher ed	Network size	Caste
Private school	-7.164	-0.824	-0.569	-0.155	0.252
	(10.362)	(1.412)	(1.409)	(1.994)	(1.085)
Lottery losers mean	12.609	0.807	0.364	2.175	1.453
Adj. R^2	-2.02	-1.00	-0.39	0.01	0.31
Observations	962	963	963	807	807

Notes: This table reports 2SLS estimates using distance to the nearest private school as an instrument for private-school attendance among lottery losers. Column 1 reports educational outcomes; subsequent columns report corresponding estimates for other outcomes listed in the table. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, restricted to those who lost the SWS lottery. Estimates are local average treatment effects (LATE) for applicants whose school-choice decisions were induced by variation in private-school proximity. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.18: Spillover effects via village peers' lottery wins

	Education			Social networks	
	(1)	(2)	(3)	(4)	(5)
	Years of ed	Continuing in school	Higher ed	Network size	Caste
SWS attendees in village	-0.547	-0.036	-0.157**	-0.223	0.214
	(0.386)	(0.063)	(0.068)	(0.270)	(0.154)
Lottery losers mean	12.609	0.807	0.364	2.175	1.453
Adj. R^2	0.03	0.04	0.02	0.01	0.28
Observations	1223	1224	1224	1065	1065

Notes: This table reports OLS estimates examining spillover effects of peers' SWS lottery wins within villages. The key explanatory variable is the leave-out share of village peers who won the SWS lottery. Columns report network-size and network-composition outcomes listed in the table. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.19: Effects of SWS attendance on educational attainment, by local school-market competition

	< 19 schools			≥ 19 schools		
	(1) Years of ed	(2) Grade 10 marks	(3) Grade 12 marks	(4) Years of ed	(5) Grade 10 marks	(6) Grade 12 marks
SWS	0.424	0.079	91.096**	0.376	0.161	51.873*
	(0.313)	(0.181)	(36.378)	(0.242)	(0.156)	(29.308)
Lottery losers mean	12.609	7.410	585.187	12.609	7.410	585.187
Adj. R^2	0.04	0.04	0.02	0.04	0.04	0.03
Observations	901	844	733	1103	1045	920

Notes: This table reports 2SLS estimates using random lottery assignment as an instrument for SWS attendance. The sample is divided by the number of schools within a 5 km radius, split at the median of 19. Column 1 reports years of completed schooling. Column 2 reports an indicator for continuing beyond Grade 10. Column 3 reports an indicator for any college enrollment. The final column reports the p-value for equality of treatment effects across high- and low-competition areas. All specifications include baseline controls for age, gender, and social group. Robust standard errors are reported in parentheses. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. Estimates are local average treatment effects (LATE) for compliers. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.20: Round 2 sample composition and attrition balance

	(1)	(2)	(3)
	Means		Difference
Variable	Surveyed	Not surveyed	
Female	0.457	0.443	-0.018
	(0.498)	(0.497)	(0.027)
Age	22.085	22.109	0.014
	(1.249)	(1.168)	(0.056)
School: Any public school	0.802	0.841	0.036
	(0.399)	(0.366)	(0.025)
School: SWS school	0.368	0.422	0.054*
	(0.482)	(0.494)	(0.029)
School: Any private school	0.186	0.157	-0.026
	(0.389)	(0.364)	(0.025)
District: Jogulamba Gadwal	0.302	0.002	-0.307***
	(0.459)	(0.044)	(0.040)
District: Mahabubnagar	0.072	0.129	0.056***
	(0.259)	(0.336)	(0.020)
District: Nagarkurnool	0.265	0.398	0.129***
	(0.442)	(0.490)	(0.034)
District: Wanaparthy	0.234	0.251	0.013
	(0.424)	(0.434)	(0.040)
District: Rangareddy	0.067	0.100	0.035**
	(0.250)	(0.300)	(0.017)
District: Other	0.059	0.120	0.074***
	(0.236)	(0.325)	(0.018)
Caste: SC	0.617	0.641	0.017
	(0.486)	(0.480)	(0.034)
Caste: ST	0.099	0.124	0.022
	(0.299)	(0.329)	(0.020)
Caste: BC	0.255	0.225	-0.035
	(0.436)	(0.418)	(0.033)
Observations	1,523	510	2,033

Notes: This table compares baseline characteristics of respondents successfully tracked and surveyed in Round 2 with those not observed in Round 2. Column 1 reports mean values for respondents observed in Round 2. Column 2 reports mean values for respondents not observed in Round 2. Column 3 reports the mean difference between the two groups, with robust standard errors in parentheses. Baseline characteristics are measured prior to random assignment using administrative application records. No controls or weights are used. The unit of observation is the original lottery applicant, and the sample consists of applicants from the 2010–11 entry cohort. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.21: Lee bounds for main outcomes (pooled sample)

Outcome	Category	OLS	Exact Lee Bounds	Generalized Lee Bounds
Years of education	All	0.007	[-0.294, 0.161]	[-0.166, 0.104]
Continue beyond grade 10	All	-0.001	[-0.017, -0.004]	[-0.012, -0.001]
Any higher education	All	0.015	[0.009, 0.009]	[0.015, 0.015]
Passed Grade 12	All	-0.006	[-0.021, -0.009]	[-0.017, -0.006]
Grade 12 marks (sd)	All	0.048	[-0.009, 0.120]	[0.023, 0.098]
In the labor force	All	-0.024	[-0.026, -0.026]	[-0.024, -0.024]
Currently working	All	-0.020	[-0.016, -0.014]	[-0.020, -0.014]
Unemployed	All	-0.004	[-0.010, -0.010]	[-0.004, -0.004]
Married	All	-0.030	[-0.037, -0.025]	[-0.030, -0.022]
Ate at marriage age	All	0.052	[-0.125, 0.297]	[-0.113, 0.240]
Any children	All	-0.020	[-0.021, 0.005]	[-0.020, -0.003]
Total network size	All	0.030	[-0.041, 0.354]	[0.010, 0.298]
Share of network that is family	All	-0.030	[-0.050, 0.141]	[-0.030, 0.119]
Share of network that is same caste	All	0.102	[-0.008, 0.297]	[0.041, 0.249]
Share of network that is other caste	All	-0.037	[-0.057, 0.231]	[-0.037, 0.161]

Notes: This table reports bounds on treatment effects estimated following Lee2009 and Semenova2020. The "OLS" column reports reduced-form intent-to-treat (ITT) estimates using tracked observations only. "Exact Lee bounds" trim outcome distributions symmetrically within gender–caste strata to equalize selection rates between treatment and control groups. "Generalized Lee bounds" implement the forest-based estimator of Semenova2020, which models selection probabilities $\Pr(S{=}1 \mid X, Z)$ and performs local quantile trimming in X. Reported bounds are shown as [lower, upper]. When selection rates are identical across arms, the bounds collapse to the OLS ITT estimate. The sample includes verified lottery applicants with non-missing information on SWS attendance from the 2010–11 entry cohort. Robust standard errors are reported in parentheses. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$.

Table A.22: Lee bounds for main outcomes (men)

Outcome	Category	OLS	Exact Lee Bounds	Generalized Lee Bounds
Years of education	Men	0.134	[-0.016, 0.381]	[0.184, 0.205]
Continue beyond grade 10	Men	0.003	[-0.002, 0.002]	[0.000, 0.003]
Any higher education	Men	0.029	[0.027, 0.027]	[0.029, 0.029]
Passed Grade 12	Men	0.002	[-0.003, 0.001]	[-0.001, 0.002]
Grade 12 marks (sd)	Men	0.106	[0.059, 0.147]	[0.100, 0.110]
In the labor force	Men	0.015	[0.016, 0.016]	[0.015, 0.015]
Currently working	Men	0.002	[0.003, 0.007]	[0.002, 0.017]
Unemployed	Men	0.013	[0.014, 0.014]	[0.013, 0.013]
Married	Men	-0.069	[-0.068, -0.068]	[-0.069, -0.069]
Ate at marriage age	Men	0.329	[0.181, 0.498]	[0.319, 0.337]
Any children	Men	-0.055	[-0.054, -0.054]	[-0.055, -0.055]
Total network size	Men	-0.007	[-0.130, 0.140]	[-0.007, 0.001]
Share of network that is family	Men	-0.024	[-0.082, 0.034]	[-0.029, -0.024]
Share of network that is same caste	Men	0.125	[0.007, 0.269]	[0.130, 0.135]
Share of network that is other caste	Men	-0.083	[-0.125, 0.004]	[-0.083, -0.048]

Notes: This table reports bounds on treatment effects estimated as described in Table A.21, restricting the sample to male respondents. "Exact Lee bounds" and "Generalized Lee bounds" are computed within the male subsample following Lee2009 and Semenova2020. Reported bounds are shown as [lower, upper]. When selection rates are identical across arms, the bounds collapse to the OLS ITT estimate. The sample includes verified male lottery applicants with non-missing SWS attendance data from the 2010–11 entry cohort. Robust standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.23: Lee bounds for main outcomes (women)

Outcome	Category	OLS	Exact Lee Bounds	Generalized Lee Bounds
Years of education	Women	-0.114	[-0.499, -0.000]	[-0.451, 0.028]
Continue beyond grade 10	Women	-0.007	[-0.028, -0.009]	[-0.024, -0.007]
Any higher education	Women	-0.001	[-0.005, -0.005]	[-0.001, -0.001]
Passed Grade 12	Women	-0.014	[-0.035, -0.016]	[-0.030, -0.014]
Grade 12 marks (sd)	Women	-0.005	[-0.060, 0.100]	[-0.043, 0.093]
In the labor force	Women	-0.055	[-0.058, -0.058]	[-0.055, -0.055]
Currently working	Women	-0.032	[-0.030, -0.030]	[-0.032, -0.032]
Unemployed	Women	-0.023	[-0.028, -0.028]	[-0.023, -0.023]
Married	Women	-0.013	[-0.013, 0.007]	[-0.013, 0.006]
Ate at marriage age	Women	-0.032	[-0.351, 0.149]	[-0.477, 0.242]
Any children	Women	0.003	[0.003, 0.049]	[0.003, 0.037]
Total network size	Women	0.077	[0.024, 0.512]	[0.038, 0.551]
Share of network that is family	Women	-0.032	[-0.026, 0.219]	[-0.032, 0.212]
Share of network that is same caste	Women	0.073	[-0.019, 0.317]	[-0.038, 0.304]
Share of network that is other caste	Women	0.019	[-0.006, 0.398]	[0.019, 0.345]

Notes: This table reports bounds on treatment effects estimated as described in Table A.21, restricting the sample to female respondents. "Exact Lee bounds" and "Generalized Lee bounds" are computed within the female subsample following Lee2009 and Semenova2020. Reported bounds are shown as [lower, upper]. When selection rates are identical across arms, the bounds collapse to the OLS ITT estimate. The sample includes verified female lottery applicants with non-missing SWS attendance data from the 2010–11 entry cohort. Robust standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Proofs

Proof of Result 1. In the baseline, the search surplus is

$$S(H, D; \bar{w}) = \frac{\lambda(D)}{\rho + \lambda(D)} \Gamma(H; \bar{w}) - \frac{\kappa}{\rho},$$

with $\lambda'(D) > 0$ and $\partial_H \Gamma(H; \bar{w}) > 0$. Consider a small move from the non-SWS bundle to the SWS bundle:

$$\Delta S \approx \frac{\lambda}{\rho + \lambda} \partial_H \Gamma \cdot \Delta H + \Gamma \cdot g'(\lambda)\lambda'(D) \cdot \Delta D, \quad g(\lambda) \equiv \tfrac{\lambda}{\rho+\lambda}, \; g'(\lambda) = \tfrac{\rho}{(\rho+\lambda)^2}.$$

Write semi-elasticities at the outside (participation) margin as

$$\eta_H \equiv \frac{\partial \ln \Gamma}{\partial H}\Big|_{H_{\text{Out}}} > 0, \qquad \eta_D \equiv \frac{\partial \ln \lambda}{\partial D}\Big|_{D_{\text{Out}}} > 0,$$

so that $\partial_H \Gamma = \eta_H \Gamma$ and $\lambda'(D) = \eta_D \lambda$. Then

$$\Delta S = \frac{\lambda \Gamma}{\rho + \lambda} \Big[\eta_H \Delta H + \frac{\rho}{\rho + \lambda} \eta_D \, \Delta D \Big].$$

Because $\Delta D < 0$ under SWS and $\frac{\rho}{\rho+\lambda} \in (0, 1)$, we have the lower bound

$$\Delta S \geq \frac{\lambda \Gamma}{\rho + \lambda} \Big[\eta_H \Delta H - \eta_D |\Delta D| \Big].$$

Hence, a sufficient condition for $\Delta S > 0$ (and thus a rise in participation at entry) is

$$\eta_H \Delta H > \eta_D |\Delta D|,$$

which is condition (7). ◻

Proof of Result 2. For participants, the job-finding hazard is $h(H, D; \bar{w}) = \lambda(D)\, p(H; \bar{w})$, so

$$\mathbb{E}[T_U] = \frac{1}{h} = \frac{1}{\lambda(D)\, p(H; \bar{w})}.$$

Taking log changes between SWS and non-SWS bundles gives

$$\Delta \ln \mathbb{E}[T_U] = -\Delta \ln \lambda(D) - \Delta \ln p(H; \bar{w}).$$

Under SWS, $\Delta D < 0$ and $\lambda'(D) > 0$ imply $\Delta \ln \lambda(D) < 0$, while $\Delta H > 0$ and $p_H > 0$ imply $\Delta \ln p(H; \bar{w}) > 0$. Thus, if

$$|\Delta \ln \lambda(D)| > \Delta \ln p(H; \bar{w}),$$

namely condition (8), then $\Delta \ln \mathbb{E}[T_U] > 0$ and expected unemployment duration rises.

For any finite horizon $T > 0$, employment by time T is $J_T = 1 - \exp\{-hT\}$. A first-order expansion yields

$$\Delta J_T \approx T e^{-hT} \Big[p(H; \bar{w})\, \partial_D \lambda(D)\, \Delta D + \lambda(D)\, \partial_H p(H; \bar{w})\, \Delta H \Big].$$

When T is small and $|\Delta D|$ is sizable, the networks channel (through λ) can dominate the human-capital channel (through p), so ΔJ_T can be small or negative despite higher participation. □

Proof of Result 3. Let workers periodically re-optimize and allow either (i) search costs $\kappa(t)$ to rise with duration, with $\kappa'(t) > 0$, or (ii) beliefs about λ to be updated from the no-offer history so that the posterior arrival rate $\lambda_t(D)$ is weakly decreasing in t after zero arrivals. The continuation surplus at time t is

$$S_t(H, D; \bar{w}) = \frac{\lambda_t(D)}{\rho + \lambda_t(D)}\, \Gamma(H; \bar{w}) - \frac{\kappa(t)}{\rho}.$$

Because D raises arrival rates, $\partial_D \lambda_t(D) \geq 0$ for all t, and because either $\lambda_t(D)$ falls with t after no arrivals or $\kappa(t)$ rises, S_t is weakly decreasing in t along unemployment spells. Define the discouragement (search-exit) time

$$T^\star(H, D) = \inf\{t \geq 0 : \; S_t(H, D; \bar{w}) < B\}.$$

For $D_1 > D_2$, $S_t(H, D_1; \bar{w}) \geq S_t(H, D_2; \bar{w})$ pointwise, hence $T^\star(H, D_1) \geq T^\star(H, D_2)$. Lower D therefore induces earlier exit from search. □

Comparative statics. **(A) Near the participation margin, holding $\bar{w}$ fixed.** Let $\Pi \equiv \Pr(S(H, D; \bar{w}) \geq$

B) denote labor-force participation. Then

$$\frac{\partial \ln \Pi}{\partial \ln H} \propto \frac{\lambda}{\rho + \lambda} \Gamma \eta_H > 0, \qquad \frac{\partial \ln \Pi}{\partial \ln D} \propto \Gamma g'(\lambda) \lambda \eta_D > 0.$$

(B) For participants, unemployment duration. With $E[T_U] = 1/\{\lambda(D)p(H; \bar{w})\}$,

$$\frac{\partial \ln E[T_U]}{\partial \ln D} = -1 + \mathcal{O}\left(\frac{\partial \ln p}{\partial \ln D}\right), \qquad \frac{\partial \ln E[T_U]}{\partial \ln H} = -\frac{\partial \ln p}{\partial \ln H} < 0.$$

(C) Early-horizon employment. Since $J_T = 1 - e^{-hT}$ with $h = \lambda(D)p(H; \bar{w})$,

$$\frac{\partial J_T}{\partial D} = Te^{-hT} p \, \partial_D \lambda > 0, \qquad \frac{\partial J_T}{\partial H} = Te^{-hT} \lambda \, \partial_H p > 0.$$

$\square$