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# GIFTED CHILDREN PROGRAMS' SHORT AND LONG-TERM IMPACT: HIGHER EDUCATION, EARNINGS, AND THE KNOWLEDGE ECONOMY

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**ABSTRACT**

This paper examines the short- and long-term consequences of participation in high school Gifted Children Programs (GCPs). Using administrative data that follow individuals from primary school to adulthood, we find that while these programs have negligible effects on high school academic achievement, they substantially influence university trajectories. Specifically, GCP participation shifts the choice of field of study, increases the incidence of double majors, and increases the probability of attaining advanced degrees. Interestingly, we find no effect on earnings or employment—either overall or within knowledge-based sectors—indicating that the high baseline productivity gifted children possess is not further enhanced by program participation. However, we find that GCP participation positively affects spouse “quality” and significantly increases the likelihood of working with fellow GCP peers, suggesting that the program has important long-run effects on the social connections of gifted students. We discuss potential mechanisms within the context of psychological theories of giftedness.

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# 1 Introduction

Human capital, particularly at the top of the ability distribution, is a fundamental driver of economic growth. This is especially salient in innovation-driven countries, where technological progress has increased the marginal value of high skills. In Israel, high-tech sectors have underpinned GDP growth in recent decades and account for nearly a third of total exports.[1] Gifted students constitute a vital segment of the workforce in these sectors and receive special attention within the Israeli education system, as is common in other countries.[2] Despite substantial resources devoted to gifted programs, causal evidence on their long-run impacts—including occupational choice, individual productivity, and integration into the knowledge economy—remains strikingly limited.

This paper provides new longitudinal evidence on the short and long-term effects of Gifted Children's Education Programs (GCP) by exploiting a long-standing national program in Israel. By leveraging unique administrative data linking secondary school records to university registries and tax data, we follow gifted students from high school into the labor market. The program selects the most talented students into specialized gifted children classes, typically beginning in 10th grade (or 7th grade in some schools). The GCP aims to cultivate the next generation of leaders and innovators in science, culture, and society by offering an accelerated curriculum, access to university-level coursework, and high-quality instruction tailored to their specific needs.[3]

We follow 22 cohorts of GCP participants who graduated from high school between 1992 and 2013. The data include rich information on students' pre-treatment backgrounds, including test scores in multiple domains measured at different ages, academic choices that may proxy for motivation, and detailed family characteristics. They also include detailed outcomes, such as high school matriculation scores, higher education choices, labor-market outcomes, and personal outcomes. To ensure the greatest data density, our main analysis focuses on high school graduates from 2006 to 2010, while earlier and later cohorts allow us to assess the persistence and external validity of the results.

We examine the effects of participating in GCPs by exploiting quasi-exogenous variation in access to these programs across different localities in Israel. During our study period, secondary education GCPs were only available in several localities. This enables us to establish a counterfactual by comparing the outcomes of gifted children

[1] Data from the Israeli Democratic Institute, (Report on the Future of the Israeli Growth Engine, retrieved on 27-02-2023).

[2] According to our administrative data, about 42 percent of gifted students work in knowledge sectors, compared with fewer than 18 percent of other students.

[3] According to the Section of Gifted and Talented in the Israeli Education Department (available here, retrieved on 17-09-2024).

in localities with GCP access to those in localities without such access. We implement two complementary identification strategies. The first is a locality-level Difference-in-Differences (DiD) analysis that compares students predicted to be gifted in GCP-accessible localities with their counterparts in non-GCP localities, while controlling for differences among non-gifted students. Falsification tests confirm that pre-treatment characteristics are balanced across these groups.

The second strategy matches each GCP participant with an equally gifted student from a locality without access. This approach has two main advantages: broader coverage and greater statistical power. It relies on a conditional independence assumption, which we argue is credible in our setting because our data include rich information on academic ability, motivation, and family background, allowing us to compare very similar students. In addition, because "untreated" status in our sample reflects geographic unavailability rather than self-selection, this approach reduces concerns about endogenous participation. We assess this strategy in several ways. Balancing tests show that matched students are similar, including on variables not used in the matching itself. A placebo test using high-ability students in regular classes in GCP localities but different high schools yields null effects. Finally, this strategy produces estimates very similar to those from our first approach, which relies only on locality-level variation in GCP access. This suggests that any remaining bias in the matching estimates is likely to be very small in practice.

Our analysis reveals that GCP participation does not significantly affect academic achievement in high school, measured by matriculation scores. In post-secondary education, participation causes gifted students to begin and complete their degrees earlier; however, extensive margin attainment remains unaffected, as nearly all gifted students eventually earn a degree. The program's impact is instead concentrated on the intensive margin of educational choice. GCP participants are significantly more likely to graduate with double majors and attain advanced degrees (PhDs). Furthermore, while the program does not increase the overall likelihood of STEM enrollment, it shifts students away from engineering and toward "pure" STEM fields such as computer science (CS), Mathematics, and the Physical Sciences.

Crucially, we find no significant effects on career outcomes, including earnings and employment rates, even within knowledge-producing sectors. Within these sectors, we observe a slight shift from tech manufacturing to tech services and academic employment, mirroring the shifts in field-of-study choice. This "zero earnings effect" suggests that gifted students possess high baseline productivity that is not further augmented by the GCP curriculum. Instead, their choices appear driven by non-pecuniary considerations. This interpretation is supported by the high incidence of advanced degrees and double majors, which, in our sample, are associated with zero or negative wage returns after controlling for ability.

Finally, we document profound impacts on personal and social outcomes. While GCP participation does not affect the likelihood of marriage or having children, it significantly improves spouse “quality”, measured by spouses’ test scores, a result driven partly by participants marrying their high school classmates. Participants are also more likely to work with fellow GCP peers, suggesting that the program shapes the social and professional networks of gifted students. Together, these results suggest that a key benefit of gifted education lies in exposing students to similarly high-achieving peers and fostering elite social networks and high-skill assortative matching. Combined with its enriched academic environment, GCP appears to support “multipotentiality” and self-exploration, rather than being primarily oriented towards labor market returns. Next, we examine how the effects vary by gender, socio-economic status (SES), giftedness level (based on pre-program academic ability), and participation length. We find suggestive evidence that the effects on academic choices and partner ”quality” are concentrated among male participants. This is consistent with the idea that gifted programs may narrow a non-cognitive skills gap between boys and girls and thereby produce gender-differentiated impacts on long-term outcomes (Card et al., 2024). We find no systematic heterogeneity across the other dimensions.

Finally, to ensure the validity of our findings, we conduct extensive robustness checks to address concerns about reliance on specific functional forms and model specifications. Our results are consistent across a range of alternative model specifications. Additionally, we use different samples and employ three distinct sets of test scores—administered at different ages and available for different cohorts—as proxies for ability. These batteries cover multiple domains and age windows, offering a comprehensive view of the abilities assessed. The consistency of results across samples, time periods, and ability measures strengthens the internal and external reliability of our findings.

The evidence we present in this paper contributes to the nascent literature on the causal effects of GCPs. Card and Giuliano (2014) apply a fuzzy regression discontinuity (RD) design to estimate a GCP’s impact on primary school test scores, finding no significant improvements.[4] In the same setting, Card et al. (2024) further find that high-IQ boys from disadvantaged backgrounds show differential gains in college attendance, secondary school course taking, and grades following GCP participation. Bui et al. (2014) also study the effects of GCPs on test scores in the U.S. Southwest, using both a fuzzy RD design around the admission cutoff and a lottery among oversubscribed middle schools offering the program; they likewise find no significant effect

[4] However, it has been demonstrated that participation in these classes yields significant and positive effects on the achievement of high-achieving non-gifted students (Card and Giuliano, 2016).

on student performance. More recently, Chin and Kocks (2025) use RD and lottery designs to show that gifted programs in kindergarten in New York City did not raise test scores through eighth grade, but did increase applications to and enrollment in elite middle schools. While these studies focus on gifted classes initiated at the kindergarten, primary-school, and middle-school levels, our research extends this literature to high school, where we likewise find no evidence of gains in standardized achievement at this level.

In a recent study, Cohodes (2020) examined the short and long-term effects of participation in dedicated classes for high achievers in grades 4 to 6 in Boston public schools. While she also did not find statistically significant effects on test scores, her results provide evidence that participation enhances college enrollment, particularly for minority students. In our context, where baseline higher education enrollment rates are already relatively high among the gifted population, we did not observe a similar increase in enrollment. However, we document significant changes in the intensive margin of higher education outcomes in our setting, particularly regarding the chosen field of study, double majors, and the pursuit of advanced degrees. This discrepancy likely arises from the different populations under study. Whereas Cohodes (2020) examines a broader "high-achiever" track at younger ages, our study focuses on the extreme right tail of the ability distribution during the transition to adulthood.

Other research has examined gifted enrichment programs. Redding and Grissom (2021) find that such programs in public primary schools are associated with modest achievement gains. Booij et al. (2016) examine an individualized pull-out program in a specific Dutch school. Using a fuzzy RD design, they find that participants achieve higher grades, express stronger beliefs about their abilities, and choose university fields of study that provide higher financial returns. In a separate paper, the authors analyze similar programs across multiple schools (Booij et al., 2017) and demonstrate that the effects are more pronounced for students farther from the admission cutoff.

This paper presents several significant contributions to the existing literature. First, our study is based on a long-standing national program that has been active for over three decades, enabling a comprehensive assessment of long-term outcomes. Specifically, we examine critical university choices, including fields of study and attainment of advanced degrees, outcomes that are particularly relevant to gifted children but are often overlooked in studies of younger cohorts. Additionally, we follow GCP participants beyond degree completion to investigate employment trajectories and family-formation patterns. The paper also quantifies the integration of gifted students into the knowledge economy, measuring their presence in high-tech sectors and academic institutions.

We relate our findings to theoretical frameworks in the psychological literature regarding gifted children, specifically studies on affective development. The "big fish small pond" hypothesis, which suggests that students may feel less competent in more competitive environments, may be key in understanding our finding that GCP does not affect high school test scores (see Marsh et al., 2008). Of particular relevance are also studies on the effect of "labeling" and excessive parental or peer expectations (e.g., Robinson et al., 2002). Economic and psychological literature together coin the term 'the gifted paradox', in which high ability enables self-exploration, yet external pressures often lead students to choose prestigious yet traditional professions. This paradox is related to "multipotentiality," a trait common among GCP participants (Leung et al., 1994). Our findings, that GCP shifts adolescents toward double majors and advanced degrees despite a lack of incremental wage returns, are likely related to this paradox, suggesting that the program provides an environment that better supports their intellectual exploration and "multi-talent" tendencies.

The paper is structured as follows. Section 2 provides an overview of gifted education programs in Israel and elsewhere, and Section 3 presents the dataset. Section 4 presents the methodology we use to identify the effects of GCPs. In Section 5 we present the main results and in Section 6 we explore key heterogeneities and the mechanisms driving the effects. Finally, Section 7 concludes and offers further discussion.

# 2 Context and Background

## 2.1 Gifted Children Programs

In most innovation-led countries, fostering gifted students' talent is regarded as essential in the knowledge economy and crucial for securing new generations of scientists, creators, and innovators. Yet, how to deliver gifted education is at the center of a long-standing and still hotly debated topic in education policy discourse. Since the 1960s, various nations have introduced specific practices for talented students (Vrignaud et al., 2005; Mönks et al., 2005; Boettger and Reid, 2015), ranging from early primary school enrollment and grade skipping to intensive enrichment and specialized tracking. Notably, despite the prevalence of these programs, there is little causal evidence on the relative effectiveness of gifted education programs across demographic groups and long-run outcomes.

Selection procedures also vary significantly across jurisdictions. Early GCPs relied almost exclusively on intelligence assessment (e.g., IQ scores), a method that remains prevalent today despite criticism regarding potential ethnic or socio-economic bias. In response, some researchers and practitioners have suggested broadening eligibility criteria to include a combination of cognitive and non-cognitive measures.

## 2.2 Gifted Children's Education in Israel

By the late 1980s, Israel had developed a centralized program for highly gifted students spanning grades 3–12.[5] This curriculum incorporated elements of enrichment, extension, and acceleration. The institutional framework was solidified in 1994, when the Ministry's Department for Gifted Education assumed extensive responsibilities, including standardized testing, teacher training, and oversight of specialized regional centers.

Three types of GCPs are currently offered: (1) "Pull-out" programs, consisting of weekly enrichment days for primary school students (grades 3–6); (2) Specialized Gifted Classes, where students are taught in separate, autonomous cohorts within regular secondary school, and (3) Extracurricular afternoon enrichment.[6]

This paper focuses on the upper secondary "Specialized Classes" (Type 2) because they represent a high-intensity intervention with a meaningful sample size. Furthermore, these separate-class models resemble many GCPs in Europe and the U.S., enhancing the external validity of our results.[7] Admission to these programs is strictly based on psychometric tests undertaken the year preceding the program.[8]

Our analysis focuses on participants in the eleven foundational programs. In the primary analysis, we restrict our attention to the 2006–2010 cohorts, for whom we observe both pre-treatment test scores and labor-market outcomes at prime ages. Approximately a third of the students in this sample also participated in the GCP during middle school, allowing us to exploit variation in exposure duration to estimate dosage effects. We validate the robustness and persistence of our results using the extended cohorts (graduating 1992–2013), tracking some individuals up to age 46.

While GCP availability was concentrated in 10 major or central municipalities (e.g., Tel Aviv, Jerusalem, Haifa), these municipalities exhibit significant socio-economic heterogeneity. Critically for our identification strategy, several localities without GCP programs possess comparable population concentrations, geographic locations, and socio-economic conditions. We leverage this geographic "unavailability" to address selection concerns and ensure our control group provides a valid counterfactual.

[5] The material presented in this section draws details from https://giftedphoenix.wordpress.com/2012/11/15/gifted-education-in-israel-part-one (retrieved on 06-09-2021).

[6] One exception is a residential school for the gifted that serves children from all over the country and is located in Jerusalem.

[7] Note that we focus on middle and high schools since dedicated classes for gifted students were very rare in primary schools in Israel during our research years.

[8] Further details on the selection process can be found in official reports by the Section of Gifted and Talented in the Israeli Education Department (available here, retrieved on 17-09-2024) and by the Israeli Parliament Research Center (available here, retrieved on 17-09-2024).

## 2.3 Israel's High School and Higher Education Systems

Secondary education in Israel is tracked. Upon entering 10th grade, students enroll in either an academic or non-academic track. The academic track leads to a matriculation certificate (Bagrut), awarded upon passing national exams in core and elective subjects. Courses are categorized by difficulty (1 to 5 credit units), with 5-unit subjects being broadly comparable to Advanced Placement (AP) courses in the U.S. system.

The Bagrut is a high-stakes credential. While only about 52% of high school seniors attained it in the 1999–2000 cohort, the rate among gifted children exceeds 90%. A typical study program for gifted children includes several subjects at the highest (5-unit) level, significantly exceeding the minimum requirement of a single advanced subject. The higher education sector includes ten universities and 50 academic colleges. Universities, which generally maintain higher admission thresholds and greater research intensity than colleges, require both the Bagrut and the University Psychometric Entrance Test (UPET) for admission. Because university admissions are highly competitive and field-specific, the Bagrut score serves as a critical determinant of a student's initial academic trajectory.

# 3 Data

We employ a comprehensive administrative database from Israel's Central Bureau of Statistics (CBS), accessed via their restricted research facility. The core strength of our data lies in the longitudinal linkage of multiple sources: the population registry, Ministry of Education records (primary through secondary), the Council for Higher Education (post-secondary), and the Israel Tax Authority (earnings and employment). The baseline sample comprises all individuals born in Israel between 1970 and 1995.[9]

## 3.1 Analysis Samples and Ability Measures

A primary empirical challenge is that we do not observe the official scores from the gifted screening exam. Furthermore, because these exams were primarily administered in municipalities with established GCPs, students from other localities, who are essential for constructing a valid counterfactual, lack systematic screening data. To address this, we rely on pre-treatment ability measures available for both treated and comparison students.

Our data include two standardized assessments of cognitive ability. The first is the Meitzav (GEMS) exams, taken in 5th and 8th grade in the four core subjects (Science,

[9] For more details on the database and its sources, see Appendix A.

Math, English, and Hebrew).[10] The second is the University Psychometric Entrance Test (UPET), which assesses quantitative, verbal, and English domains.[11]

The Meitzav scores offer the distinct advantage of being administered before GCP entry, thereby ensuring they are not contaminated by program participation. However, because these exams were introduced in 2002, we observe 8th-grade scores only for high school graduates from 2006 onward. Consequently, our primary analysis sample consists of 2006–2010 graduates. By 2020 (the final year of our labor market panel), these individuals are aged 28–32, a window in which most have completed undergraduate studies and established a stable labor market trajectory. This sample includes all students who participated in the 8th-grade Meitzav, representing approximately half of the students in each cohort, as determined by the Ministry's sampling design.

We supplement this with two additional samples to assess dosage and long-run persistence. The first is the sample of the 2009–2013 Graduates: Observed with 5th-grade (early) Meitzav scores, tracking outcomes through ages 25–29. The second sample is the 1992–2005 Graduates: Tracked through prime working ages (33–46). While we only observe UPET scores for this group, we provide evidence that these scores are not endogenously affected by prior GCP participation in our setting.

Our main sample includes 792 GCP participants. While the sample size is constrained by our focus on the foundational GCP schools (11 institutions) and the Meitzav sampling rate, our supplementary analyses extend this to roughly 6,000 GCP participants, providing sufficient power to detect nuanced labor market effects. [12]

## 3.2 Definitions of the Main Outcomes

**High school achievement.** We calculate a composite matriculation GPA based on mandatory national exam scores.

**Higher education.** We construct indicators for undergraduate enrollment and completion. To capture shifts in the intensive margin of education, we define dummy variables for field-of-study groups using CBS classifications (see Appendix Table A1 for full details). Our primary focus is on STEM degrees with high market returns, such

[10] The Meitzav or GEMS (Growth and Effectiveness Measures for Schools) tests are administered by the Division of Evaluation and Measurement of the Ministry of Education. It follows a representative 1-in-2 sample strategy, with every elementary and middle school in Israel participating in Meitzav once every two years. The Meitzav test scores are standardized for comparability across different years. Virtually all students, with the exception of those in special education classes, are tested, resulting in a testing rate exceeding 90 percent.

[11] The UPET is required for university applicants in Israel and is administered by The National Institute for Test and Evaluation (NITE). According to NITE, the UPET is a tool for predicting academic success at higher education institutions in Israel.

[12] According to a report by the Israeli Parliament Research Center, during the study period only about 20% of gifted children of the relevant ages participated in gifted programs in middle or high school (available here, retrieved on 17-09-2024).

as CS and Engineering. We also identify advanced-degree attainment (MA/PhD) and double-major enrollment as key outcomes reflecting intellectual breadth and "multipotentiality."

**Employment.** We define indicators for participation (non-zero months worked) and self-employment. Crucially, we categorize employment within the "Knowledge Economy" using three-digit sector codes. This includes high-tech manufacturing industries—pharmaceuticals, computer machinery, electronic components, and aerospace—as well as high-tech services, including telecommunications, computer services, and R&D. The academic sector is defined as employment at colleges and universities.[13]

For our earnings analysis, we use age-conditional earnings rank as the primary outcome to account for lifecycle wage growth, while also reporting results for log annual earnings and nominal values.[14]

**Personal outcomes.** We identify marriage and fertility. Leveraging the dyadic nature of the administrative data, we observe outcomes for marital partners, allowing us to measure spouse "quality" via their GCP status and standardized test scores—a direct proxy for assortative matching within the gifted population.

# 4 Methodology: Estimating GCP Effects

We implement two complementary empirical strategies to identify the causal impact of Gifted Children Programs (GCP), leveraging quasi-exogenous variation in access across Israeli localities.

## 4.1 Locality-Level Analysis: ITT and LATE

Our first approach estimates the effects of GCP availability at the locality level. We compare the outcomes of "likely gifted" children—defined as those with a very high predicted likelihood of GCP participation—in localities with and without programs, and assess these differences relative to those observed among non-gifted children. This identifies an Intention-to-Treat (ITT) effect and sidesteps individual-level selection into participation. We estimate the following Difference-in-Differences (DiD) specification:

$$Y_i = \alpha_{l(i)} + \beta \times G_i + \delta \times G_i \times LocGCP_{l(i)} + X_i'\gamma + \varepsilon_i \tag{1}$$

[13] We further validate the reliability of the labor market data by comparing their means to the respective statistics based on labor survey data available for a sub-sample of individuals in our sample. However, we do not use this data in our analysis because the sample is small.

[14] We exclude observations that deviate by six or more standard deviations from the mean to account for earnings outliers. Only a few observations are dropped; this procedure does not affect the results.

where $i$ indexes individuals and $l(i)$ denotes individual $i$'s locality. $\alpha_{l(i)}$ represents locality fixed-effects, $LocGCP_{l(i)}$ is an indicator for a locality with access to a high school GCP, and $X_i$ is a vector of individual-level control variables that capture ability, motivation, and background (detailed in subsection 4.3). The coefficient $\delta$ captures the differential outcome gap between gifted and non-gifted students in treatment versus control localities.

We further implement an Instrumental Variables (IV) strategy to identify the Local Average Treatment Effect (LATE) for compliers, using the interaction $G_i \times LocGCP_{l(i)}$ as an instrument for actual participation $GCP_i$. The first and second stages are defined as:

$$GCP_i = \theta_{l(i)} + \pi\, G_i + \lambda\, (G_i \times LocGCP_{l(i)}) + X_i'\phi + u_i \tag{2}$$

$$Y_i = \zeta_{l(i)} + \rho\, G_i + \delta^{IV}\, \widehat{GCP}_i + X_i'\eta + v_i \tag{3}$$

We estimate this model using Two-Stage Least Squares (2SLS) and cluster standard errors at the locality level. The identification requires instrument relevance, which is supported by the strong first stage reported below. Second, it requires monotonicity, which is plausible by design given that the instrument is based on local GCP availability. Third, it requires instrument validity, meaning that the instrument is conditionally independent of potential outcomes and satisfies the exclusion restriction. This is the key identifying assumption in our setting. Specifically, conditional on the vector $X_i$ of individual-level control variables and locality fixed effects, differences in potential outcomes between students in localities with and without access to a GCP must be the same for gifted and non-gifted students. This assumption is analogous to a conditional parallel trends assumption in a difference-in-differences setting, except that the comparison is across gifted and non-gifted students rather than over time. It would be violated if localities that offer a GCP differ systematically from other localities in unobserved ways that affect gifted students differently. We assess the plausibility of this assumption using the falsification tests reported below.

## 4.2 Student-Level Analysis: Matching

To maximize statistical power and coverage, we complement the locality analysis with a student-level strategy estimating the Average Treatment Effect on the Treated (ATT). We match each GCP participant to an observationally similar gifted student from a locality where the program was unavailable.

We estimate the following controlled regression on the matched sample:

$$Y_i = \alpha + X_i'\beta + \tau GCP_i + \varepsilon_i \quad (4)$$

Where $\tau$ captures the effect of GCP participation. Standard errors are clustered at the school level.[15] The validity of this approach rests on the Conditional Independence Assumption (CIA). While matching on observables is a strong assumption, it is highly credible here for two reasons. First, by including rich covariates, we control for granular measures of intelligence, prior academic motivation (high school track), and socio-economic background. Second, because of exogenous non-participation, the untreated status in our control group is driven by geographic unavailability rather than an individual choice to opt out, significantly reducing selection concerns. We also include cohort fixed effects.

## 4.3 Identifying Gifted Students Using Observables

To implement both strategies, we must identify gifted children in localities without programs. We use three sets of variables to predict GCP participation: intelligence (standardized 8th-grade Meitzav scores), academic motivation (choice of high school track and advanced-credit subjects before 10th grade), and family background (parental education, birth order, and siblings).

We estimate $Pr(GCP_i = 1|X_i)$ with a Logit specification, using the sample of GCP participants and students from other localities (the matching comparison group pool). The model includes quadratic terms for test scores. As proxies for motivation, we include indicators of enrollment in "5-unit" (advanced) Math, English, and specific scientific electives that are overrepresented among GCP students.[16]

**Gifted indicator for the locality-level analysis.** For the locality analysis, we define $G_i$ using a propensity score cutoff of $c = 0.1$. This threshold balances precision and recall: it captures 40% of all GCP participants, while 35% of those above the cutoff are actual participants. We demonstrate that our results are robust to alternative cutoff choices (Appendix Figure A1).

**Matching procedure.** We employ nearest-neighbor matching without replacement within a caliper of 0.1 standard deviations. We enforce exact matching on gender, school religiosity, and matriculation track (academic vs. technical). Combining match-

[15] We also validate that the standard errors are similar to clustered bootstrapped standard errors (see Appendix Table A2).

[16] Estimated coefficients are reported in Appendix Table A4.

ing with regression adjustment further reduces sensitivity to residual imbalance and functional-form misspecification.[17] [18]

## 4.4 Validating the Identification Strategy

**Locality-level falsification.** We estimate our DiD model (equation 1) using parental and student characteristics as placebo outcomes. Table 1 confirms no significant differences in 18 variables (parental income, age at birth, etc.), with only one estimate reaching significance—consistent with random chance.[19]

**Individual-level balance.** Figure 1 presents the propensity score distribution before and after the matching. We matched the majority of GCP participants (698 of 792, nearly 90 percent); unmatched observations are concentrated at the top of the propensity-score distribution.[20] After matching, the propensity-score distributions for treated and matched comparison students are closely aligned and statistically indistinguishable.

Figure 2 presents the distributions of the 8th-grade scores before and after the matching. As expected, there are substantial differences between GCP participants and the sample of students from other localities before matching, since most students in these localities are not gifted. However, the matching eliminates these differences in test scores, as all the distributions become very similar. Kolmogorov–Smirnov tests reject equality of the distributions for all four subjects before matching. The matching eliminates these differences, and after the matching, the distributions are statistically indistinguishable in all subjects.

Table 2 presents a detailed summary of descriptive statistics for our matched sample, including a test of mean differences in the background characteristics of GCP participants and the matched comparison students. Columns 1 and 2 show the averages of the comparison and treatment (GCP) groups. Column 3 shows the estimated

[17] Rosenbaum and Rubin (1983) proposed an approach that circumvents the curse of dimensionality when using selection on observables to identify causal effects. They prove that if treatment assignment can be ignored given x, then it can be ignored given any balancing score that is a function of x, particularly the propensity score. See Abadie and Cattaneo (2018) for an updated survey of econometric methods for program evaluation and a useful comparison of matching/propensity score models with other methods.

[18] See Abadie and Imbens (2002) for discussion of regression adjustment after matching. A regression fit on the full sample (including gifted and non-gifted students) would rely on stronger functional-form assumptions and on extrapolation to units for whom treatment is not feasible. Because only gifted children are eligible to participate in GCP, our parameter of interest is the average treatment effect on the treated (ATT) for treated gifted students, rather than an average effect for the full population.

[19] We also find no significant differences in these characteristics when examining gifted and non-gifted students separately across localities with and without GCP access.

[20] Unmatched students tend to have stronger pre-treatment characteristics; for example, their 8th-grade test scores exceed those of matched GCP participants by roughly 0.15–0.30 SD, making close matches harder to find.

difference. Both groups are strongly balanced on all characteristics. Panel A shows the balance on the number of siblings, family order, and the share born in Israel. Panel B shows the balance of parental characteristics, including income, education, birthplace, and age at birth. Interestingly, the groups are balanced on variables not included in the matching specification, such as parental earnings, parental country of birth, and parental age at birth. Table 2 further shows balance on variables not used in the matching itself (e.g., parental earnings), suggesting that unobservables are also likely balanced.

**Individual-level placebo exercise.** We conduct a placebo exercise to assess whether talented students who are similar on observables, but live in localities with and without access to a GCP, differ systematically in ways that predict future outcomes even when they do not participate in the program. To do so, we define a placebo "treated" group consisting of talented students in localities with a GCP who attend regular classes in other high schools without a GCP, and therefore do not participate in the program. We select these students so that they are observationally similar to the actual treated group, and then apply the same matching procedure as in the main analysis to construct comparison students from localities without access to a GCP. Table 3 reports estimated differences in the main outcomes between this placebo group and their matched comparison students. The estimates are close to zero, which is consistent with the view that locality type, by itself, does not generate outcome differences among observationally similar nonparticipants. This finding supports the identifying assumption behind the main analysis.[21]

# 5 Main Results: GCP Effects

## 5.1 Locality-Level Estimates

Table 4 reports estimates of equation 1 for the main outcomes. The specification compares differences in outcomes between localities with and without access to a GCP for students classified as likely gifted ($G_i > 0.1$), relative to the corresponding differences for other students. Column (2) reports estimates without controls. Column (3) adds controls for students' test scores. Column (4) further adds proxies for academic choices and family background. The results are generally stable across specifications. Column (5) reports instrumental-variables estimates, using GCP participation as the endogenous treatment variable.

[21] We also implement this placebo using a random sample of students in these schools, without restricting the sample to high-ability students, and again find similarly small and statistically insignificant differences.

**First stage.** Before turning to the main outcomes of the locality-level analysis, we first show that our model strongly predicts GCP participation. The first row of Panel A in Table 4 shows that the gap in the probability of GCP participation between localities with and without a GCP is 30 percentage points larger above the threshold for likely gifted students than below it. This estimate is highly statistically significant, with an $F$-statistic of approximately 27, supporting the instrument's relevance.

**Main Estimates.** The rest of Table 4 presents estimates of the effects of GCP availability on the main short- and long-term outcomes. Panel A shows that GCP availability does not affect gifted students' matriculation achievement, as measured by GPA. The IV estimate is positive but small—less than a 1 percent increase relative to the baseline mean—and statistically insignificant. While this null result may be due to our limited power to detect small effects, we show below that it is also highly robust to other methods with greater statistical power. We also discuss possible explanations in the following section.

In Panel B, we extend the analysis to higher-education outcomes. GCP availability does not significantly affect BA enrollment. The estimate is positive but imprecise, and baseline enrollment rates are already very high, suggesting that nearly all gifted students pursue a BA. By contrast, GCP availability has a significant effect on advanced-degree enrollment. The estimated LATE on the probability of pursuing an MA degree is very large, amounting to an increase of about 20 percentage points. The estimated effect on PhD enrollment is also large, although it is imprecisely estimated in this specification.

In Panel C, we also examine field-of-study choices, focusing on high-return STEM degrees. We find a positive effect on studying Math and CS fields, and a negative but only marginally significant effect on Engineering. However, we find a large positive effect on the decision to double major, with an LATE of about 19 percentage points.

In Panel D, we examine the main labor-market outcomes of interest and find no effect of GCP availability on either students' earnings rank or their probability of working at knowledge-economy firms. While somewhat surprising, this result is robust across methods and samples. One possible explanation is that GCP participants do not primarily seek to maximize financial returns, which is consistent with our higher-education findings, including an increased likelihood of pursuing advanced degrees. We discuss this and additional labor market outcomes in the following sections.

Finally, Panel E shows that while GCP availability does not affect marriage likelihood, it does increase the partner's ability, as measured by the partner's UPET score. As we discuss below, this effect is mainly driven by within-class matches with other gifted students.

**Robustness.** Appendix Table A3 shows that the results are very similar when we use alternative thresholds to define likely gifted children. As expected, the first stage—the effect of the instrument on the probability of attending a GCP—becomes larger at higher thresholds. Consistent with this, the magnitude of the ITT effects on outcomes that do respond, such as the likelihood of choosing a double major, pursuing an advanced degree, and spouse "quality," also generally increases with the cutoff. In contrast, for outcomes with no detectable effect, such as labor-market outcomes and matriculation GPA, the estimates remain close to zero across thresholds. Differences across columns may reflect both changes in the strength of the first stage and heterogeneity in treatment effects across values of $G$.

To further mitigate concerns that our results might be driven by locality-level properties, policies, or resource allocations that disproportionately affect high-ability students' outcomes, we also conduct several additional robustness checks that are not reported here because of space constraints. First, we examine whether the main results change when we exclude the lower tail of the ability distribution by removing students with $P_i(X_i)$ in the bottom decile or quintile, thereby comparing differences among likely gifted students to differences among regular students rather than to those with the lowest predicted ability. The results are not sensitive to these restrictions, suggesting that potential disproportionate differences along the ability distribution between localities are unlikely to bias our results. Second, we verify that the results are also robust when we restrict the sample to large localities only, which are more comparable to the localities with a GCP. Third, we show that the results remain similar when giftedness is defined solely on the basis of pre-treatment test scores, without using the propensity score model. However, these estimates are noisier than the baseline estimates, consistent with the propensity score model being better at distinguishing gifted from non-gifted children.

## 5.2 ATT Estimates based on a Matching Approach

In this subsection, we report results for the main outcomes based on our second identification strategy, the matching algorithm. These results offer greater power and broader generalizability. Furthermore, to support their validity, we subject them to a battery of robustness tests and validation exercises, which we present in subsection 5.3.

Column (1) of Table 5 reports estimates from our main matching specification.[22] The pattern closely mirrors the results from the previous section. We find no meaningful effects on high school achievement or labor-market outcomes: the estimated effect

[22] Note that we report only the estimates based on Equation 4, including all control variables throughout the paper. We also validate that the results are similar if we exclude them. This robustness is not surprising as we show that the groups are balanced in all important measures.

on matriculation GPA is small and statistically insignificant, and the same is true for earnings rank and employment in the knowledge economy. These consistent null estimates are important because they reinforce the findings from the previous strategy and are estimated with greater precision.

In contrast, and consistent with the locality-level estimates, GCP participation has sizable effects on higher education choices. It increases the probability of enrolling in a BA by 3.4 percentage points, an MA by 10.1 percentage points, and a PhD by 3.9 percentage points. Because some individuals in the sample may enroll in these degrees at older ages, these estimates may partly reflect differences in timing. Reassuringly, when we examine outcomes at older ages using other samples below, the BA effect disappears, whereas the effects on advanced degrees remain large and statistically significant, indicating persistent long-run impacts on graduate education. We discuss the persistence of our effects in greater detail in the following sections.

GCP participation also raises the likelihood of studying Math or CS by 4.5 percentage points and of pursuing a double major by 10.2 percentage points, while reducing the probability of studying engineering by 6.4 percentage points. Finally, it improves spouse "quality", as measured by spouse UPET score, by about 32 points.[23]

Note that in this analysis, about 10% of the most gifted students are excluded because the matching algorithm cannot find suitable comparisons. By contrast, the previous strategy identifies local effects in the upper part of the giftedness distribution, effectively excluding students near the lower end of that group. These differences in sample composition may explain some variation in magnitudes, but the overall pattern of results is very similar, which strengthens the credibility of the matching approach and suggests that any remaining bias is likely to be small in practice.

## 5.3 Robustness of the Individual-Level Estimates

**Matching specification.** We first show that the matching results are robust to specification changes. Columns (2)-(3) of Table 5 show that the results are similar when using other calipers (0.2SD and 0.05SD). We also validate that the results are identical when matching with replacement (column (4)). Furthermore, we validate that the results are robust to changing the propensity score model. We use a gradient-boosting algorithm (Chen and Guestrin, 2016), instead of a logit model, which allows a more flexible fit. We find similar results, as shown in column (5).

[23] Overall, our main effects on academic choice and spouse "quality" are large and accurately measured and remain statistically significant even under a conservative Bonferroni correction for multiple hypothesis testing. For example, the conventional $p$-value for the increase in MA degree enrollment is about 0.00004, so the Bonferroni-adjusted $p$-value for 11 tests is still significant ($p = 0.00044$).

**Potential concerns about the identification assumption.** We also discuss three main potential concerns regarding this strategy, which relies on the Conditional Independence Assumption (CIA) and the quasi-exogenous variation in GCP access across cities in Israel. The first concern is an endogenous relocation: families may relocate based on access to the GCP in a given locality. To explore this, we examined whether families with GCP participants exhibited a higher mobility rate before 10th grade, when high-school GCPs begin, compared to families with gifted children who did not participate in a GCP. Our findings indicate no such differential mobility rate. The rate of families relocating between the 6th and 10th grades is 19.5% in the comparison group and 18.8% in the treatment group. The difference is negative at -0.7 percentage points and insignificant (with a standard error of 2.1), thus mitigating concerns related to increased relocation among families of GCP participants.

The second potential concern is systematic locality differences: gifted children in cities with a GCP, which are typically larger, may differ systematically in their potential outcomes from gifted children in other cities. For example, educational and economic preferences may differ between individuals in larger and smaller towns.[24] However, restricting the comparison group to students from larger cities yields qualitatively similar results for the main outcomes (Column (6) of Table 5).

Another concern is omitted variable bias: missing an important variable from the matching specification, one that is both a factor in GCP selection and an outcome driver, is a concern. Given that our dataset includes detailed information on academic ability, motivation, and family background, we think it is unlikely such a variable could significantly bias our results. This is further supported by the observed balance between the two groups of gifted children after matching, as well as by the placebo exercise results for non-gifted students discussed earlier.

**Alternative measures of academic ability and motivation.** Our main specification relies on particular proxies for academic ability and motivation. In this section, we show that the results are robust to using alternative measures. This matters because it reduces concerns that some of the variables we condition on may themselves be affected by GCP participation.[25]

[24] Because Israel is a small country with substantial geographic mobility, access to universities, job opportunities, and marriage markets is not tightly tied to local area of residence.

[25] Some control variables in the main specification may be determined after entry into the GCP, including elective subjects in high school and 8th-grade Meitzav scores (for students who enter a GCP in 7th grade). This is only problematic if GCP participation affects them. In this context, such variables can still serve as useful proxies for otherwise unobserved ability or motivation. This logic is similar to that appear in other recent papers, such as Kleven et al. (2021). To address this concern, we re-estimate the results excluding these variables and replacing them with strictly pre-treatment measures. The estimates remain similar, suggesting that our results are not driven by conditioning on potentially endogenous controls.

First, in the main specification, we include indicators for elective matriculation subjects as proxies for academic motivation. We re-estimate the matching specification excluding these variables. Column (2) of Appendix Table A5 shows that the main findings remain qualitatively similar when these indicators are omitted.[26] We do find a very small positive effect on matriculation GPA. Although this estimate is statistically significant, it is close to the main estimate and still implies no meaningful effect on high-school GPA. The higher-education results are also similar to the main estimates, including the strong positive effects on the likelihood of pursuing double majors and advanced degrees, as well as the shifts in field of study. The same broad conclusion also holds for labor-market and personal outcomes: we find no effect on earnings or marriage, but a positive effect on partner "quality." In this specification, we also find a marginally significant increase in employment in the knowledge sector.

Second, in the main specification, we use $8^{\text{th}}$-grade Meitzav test scores as proxies for academic ability. A potential concern is that, because some students begin the program in grade 7, these scores could, in principle, already reflect some treatment exposure. As a robustness check, we therefore use $5^{\text{th}}$-grade Meitzav test scores, which are available for high school graduates from 2009 onward. We focus on graduates from 2009 to 2013 to assess whether the main results are robust to using earlier scores in the matching specification. Students in this sample are younger, aged 25–29 in 2020. Column (3) of Appendix Table A5 shows that the results remain very similar to the main findings. The null effect on partner "quality" may reflect lower marriage rates at these younger ages. Similarly, the null effect on PhD enrollment may reflect the fact that many individuals in this sample are still too young to have completed such degrees.

An alternative measure of ability is the UPET score. Although this test is typically taken at relatively late ages, often during or after GCP participation, we provide evidence that UPET scores are likely not affected by the program. This also allows us to use them as an alternative measure of ability in the matching specification. We use the UPET scores below to examine whether the results persist at older ages, as these are the only available ability measures for the older cohorts.

# 6 Understanding the Mechanisms of the Effects

In this section, we present additional findings regarding the effects of GCP participation on short- and long-term outcomes to offer a more comprehensive understanding of the underlying mechanisms. We focus on the matching methodology, which provides

[26] The specification still includes indicators for completing five credits in math and English. These variables are unlikely to be affected by GCP participation and are very common among high-ability students.

greater statistical power. Also, as demonstrated in the previous section, this approach yields results consistent with the locality-level analysis and passes relevant falsification tests.

## 6.1 High School Achievement

While the previous sections established a null average effect of GCP on high-school achievement, we now examine whether this masks heterogeneity across the score distribution. Figure 3 compares the matriculation GPA distributions of GCP participants and matched comparison students. The distributions are statistically indistinguishable. Additionally, Appendix Table A7 shows the estimates for the average GCP effects on test scores in all compulsory subjects. Estimated effects are small for all subjects, and most are insignificant.

The pattern of stagnant matriculation performance is especially intriguing given the abundance of educational inputs GCP participants enjoy.[27] Table 6 compares the class-level characteristics of gifted students in GCPs with those in the comparison group. Notably, GCP classes tend to be smaller, averaging nearly seven fewer students. GCPs also exhibit a higher proportion of male students than regular classes, with a difference of 9 p.p. Furthermore, GCP students are exposed to peers with higher socio-economic backgrounds, as indicated by their parents' years of schooling, and to peers with better academic achievement, as evidenced by their higher UPET and Meitzav scores.

In addition, institutional information indicates that GCP teachers are typically more highly qualified and have undergone additional training. GCPs also often provide access to advanced curricula and opportunities for university-level courses. Given these features, the absence of positive effects on high-school exit performance is puzzling and suggests that these systemic advantages do not translate into standardized exam premiums. We propose several potential explanations for this null result.

First, GCP studies and topics not directly relevant to the matriculation standard curriculum are prioritized over learning within the standard scope. Additionally, gifted students' matriculation scores are typically very high even without GCP participation, allowing them sufficient academic mobility. Thus, it is plausible that GCP participants accrue other educational benefits unobserved in standardized testing.

Alternatively, the null results could reflect the psychological effects of the change in within-class ordinal ranking. Placement in self-contained programs introduces gifted students to peers of equal or superior academic caliber, potentially harming those who

[27] Although the impact of smaller class sizes on outcomes remains uncertain, with mixed evidence in the literature (see, e.g., Angrist et al., 2019), one might anticipate that the presence of more capable peers, high-quality teachers, and advanced curriculum would have an effect.

feel overshadowed.[28] They may also find that the "top student" status they enjoyed in regular classrooms is no longer guaranteed.

Therefore, the student placed among higher-ability peers typically experiences a temporary decline in self-concept relative to peers who remain with less able classmates. This phenomenon is known as the Big-Fish–Little-Pond Effect (BFLPE) (Marsh and Parker, 1984; Herrmann et al., 2016).[29] Although the BFLPE is not specific to gifted programs, it is a widely discussed mechanism in gifted education (Plucker et al., 2004; Dai and Rinn, 2008)). In our context, GCP participants moved from the top of the middle-school rank order to a class of academic equals. As a result, this ordinal decline may lower their academic self-concept and increase anxiety (Marsh and Parker, 1984; Zeidner and Schleyer, 1999; Marsh and Craven, 2002; Preckel et al., 2008).

Lastly, the proportion of male students is 9 p.p. higher in GCPs. Since a higher presence of females in a class has been linked to positive effects on achievement (Lavy and Schlosser, 2011), the lower representation of females in GCPs might contribute to the lack of score gains.

## 6.2 Higher Education Choices

GCP participation significantly alters higher-education trajectories, particularly regarding degree depth and timing. First, GCP participants have a significantly higher enrollment rate in university courses during high school.[30] Consequently, GCP participants complete undergraduate degrees at younger ages. The probability of attaining a BA degree by age 25 is 23.3% for GCP participants versus 14.5% for the comparison group—a 9.0 percentage-point gap (SE = 2.1 p.p.), significant at the 1% level. This gap becomes much smaller when we consider indicators for enrolling in or completing an

[28] Theoretically, this could also be beneficial because a peer group of equal academic caliber gives personal validation to one's identity and mutually reinforces each other's talents and interests. But, the literature on GCPs emphasizes the negative impact.

[29] Recent work in the economics of education has documented this mechanism in other contexts. Elsner and Isphording (2017) show that students' ordinal rank significantly affects educational outcomes later in life, such as finishing high school, attending college, and completing a 4-year college degree. Exploring potential channels, these authors find that higher rank is associated with higher expectations about future careers, greater perceived intelligence, and more teacher support. Murphy and Weinhardt (2020) show that ordinal academic rank during primary school impacts secondary school achievement independent of underlying ability. In addition, they found significant effects on test scores, confidence, and subject choice during secondary school, even though they had a new set of peers and teachers unaware of their previous ranking in primary school. Relatedly, Fabregas (2023) finds that marginal admission to higher-achieving middle schools yields adverse effects on GPA, on-time graduation, aspirations, conscientiousness, and self confidence, consistent with unfavorable peer comparisons influencing educational trajectories.

[30] One potential concern in interpreting this result causally is the possibility that our comparison group might live farther from universities. Our findings, however, are robust to restricting the comparison group to students from the same localities, suggesting that proximity to universities does not drive the result.

undergraduate degree at any age, because nearly all gifted students eventually obtain a degree (as shown in the previous section).

These results suggest that while the likelihood of pursuing any undergraduate degree is unaffected, GCP participants may choose different undergraduate degree programs. Therefore, we analyze their university field of study decisions.[31]

Table 7 shows more information on the estimated GCP effects on university fields of study. Panel A focuses on STEM fields, including Math, CS, Engineering, Physical Sciences, and Biological Sciences. While there is no effect on the likelihood of achieving any STEM degree, there is a significant shift from Engineering programs to CS and physics programs. The estimated effects are very large. The likelihood of studying engineering declines by 6.4 p.p., amounting to a fall of 27 percent. The likelihood of studying CS or Math increases by 4.5 p.p., implying a 28 percent rise. And the likelihood of studying physical sciences increases by 3.0 p.p., implying a 49 percent increase.

When we examine non-STEM fields, we find a significant increase in the likelihood of choosing any non-STEM major: 5.8 percentage points relative to a baseline rate of 29%. Looking separately across non-STEM fields, we find large increases in the likelihood of studying the humanities—by 5.5 percentage points from a baseline of 4.0%—and the social sciences—by 5.3 percentage points from a baseline of 12.3%. By contrast, we find no significant changes in the likelihood of majoring in other non-STEM fields, such as law, business, medical sciences, arts, or education.

Program participation also substantially impacts the likelihood of attaining a double major. As shown in Panel C, the estimated effect is statistically significant and substantial, amounting to 10.2 p.p. relative to a baseline of 15.5%. This implies a 66% relative increase in the likelihood of pursuing a double major. The results in this panel also show that the increase is driven mainly by double majors outside STEM. By contrast, the effect on double STEM majors is statistically insignificant, although still non-negligible in relative terms, at about 30%.

Graduating with a double major may be related to the multipotentiality of gifted children. Multipotentiality is defined as "the ability to select and develop any number of career options because of a wide variety of interests, aptitudes, and abilities" (Kerr and Erb, 1991, p.1). It is commonly characterized as a feature of highly gifted individuals who possess both the capacity and the inclination to pursue various activities and goals, especially related to career choice (Sajjadi et al., 2001; Sampson Jr and Chason, 2008). Such tendencies are likely supported in an environment where giftedness status is formally recognized.

[31] We also examined the likelihood of obtaining a degree from an elite university and found a marginally significant increase of 4.7 percentage points (SE = 2.4 p.p.). Following the Israeli CBS, we classify Tel Aviv University, the Hebrew University of Jerusalem, and the Technion as elite universities.

The evidence shows that GCPs significantly affect adolescents' university choices. Our results suggest that the "gifted" label shapes expectations toward a future-oriented stance where students feel pressure to "maximize" rather than "waste" their potential. To realize their full potential, they may feel compelled to pursue multiple paths and keep their options open, making them less inclined toward early specialization. This mechanism, alongside multipotentiality, explains the shift toward double majors and advanced degrees and the reduced propensity for engineering, a field that typically entails a narrower vocational trajectory.

### 6.3 Labor Market Outcomes

An important question regarding gifted children's education is whether participation in a GCP significantly affects their career outcomes. The previous subsections provided the first evidence regarding this issue by studying the long-term effects of GCP participation on early career outcomes (ages 28-32), and showed no average gains in terms of earnings or employment in knowledge-producing sectors. Here, we further explore the full earnings distribution. Figure 4 compares the earnings distributions of GCP participants and matched comparison students. Both groups of gifted students earn more than non-gifted students, but there are no differences in earnings between GCP participants and the matched comparison students.

In addition, Panel A of Table 8 shows estimates for the average effects on earnings, their natural log, and their rank (conditional on age). We do not find any evidence that GCP participation affects these outcomes. Consistent with the impression from Figure 4, we also find that the likelihood of becoming a top 10% earner does not change significantly. Additionally, Panel B of Table 8 shows that the GCP has no significant effect on the likelihood of being employed or self-employed.[32]

GCP participants are modestly more likely to work in academic professions (especially positions requiring a PhD) and modestly less likely to work in tech manufacturing firms (Panel C of Table 8). For example, about 40% of gifted children were employed in 2020 in high-tech services or manufacturing, and the academic sector.[33] Although these estimates are imprecisely measured, they align with the observed shifts away from engineering degrees and toward double majors and advanced degrees. This alignment with academic paths rather than financial gain suggests that GCP participants prioritize intellectual exploration over pure wage-maximization.

[32] In the main analysis, we focus on labor market outcomes in 2020, which is the latest year we observe. However, we also validate that the results are similar if we analyze labor-market outcomes during 2018–2020 (Appendix Table A8).

[33] We also examine changes in the likelihood of being employed in other common sectors among gifted students, such as the public, education, and health sectors. We do not find evidence for significant changes in any of these outcomes.

## 6.4 Personal Outcomes

Panel A of Table 9 shows the results for various personal and social outcomes. We find no evidence for effects on marriage, fertility or the likelihood of living abroad. [34] Panel B shows that GCP participants are substantially more likely to marry another GCP participant, driven by within-class matches, which may partly explain why their partners have higher "quality", measured by their UPET scores. The effect on marriages with the same GCP participants is notable in light of the recent work by Kirkebøen et al. (2021), showing that colleges in Norway matter considerably for whom one marries by inducing matches within programs. Our findings on the importance of GCPs for the marital matches of gifted children suggest that such matching effects are also important at earlier education settings.

This finding highlights a crucial aspect of the GCP's impact on gifted students' lives: exposure to other high-achieving peers. To explore this further, we extend our examination beyond marital considerations and explore professional connections. Specifically, we analyze outcomes at the firm location (plant) level. For each individual we measure the total number of workers in the firm and the total number of workers who are graduates of high school GCPs (excluding the individual).[35] The results, presented in Panel C of Table 9, affirm that GCP participants tend to work alongside peers who have also participated in similar programs, providing further evidence that the GCP significantly impacts the social connections of gifted students.[36]

## 6.5 Persistence of the Results at Older Ages

We extend the analysis to high school graduates from 1992 to 2005 to examine whether the effects persist at older ages. These individuals were 33–46 years old in 2020, compared with 28–32 in our main sample. For these cohorts, the only available measure of ability is the UPET score. We therefore focus on the first test attempt of each student, on students who took the test relatively early and use only their quantitative UPET score in the matching specification. As we explain below, this helps reduce concerns that the score may itself have been affected by GCP participation.

A key challenge is that many students take the UPET only after high school, since

[34] We further verify that our estimates are robust to alternative assumptions about the (missing) earnings of gifted students living abroad. For example, we impute the 10th (90th) percentile earnings of gifted students in our sample for those living abroad, and the estimated effects on the 2020 earnings rank remain similar and insignificant.

[35] We exclude individuals working at plants with 500 or more employees because such observations likely reflect large multi-site organizations rather than discrete plants; results are robust to including these observations.

[36] Although the mean number of GCP coworkers is relatively large, this may be due to a concentration of many high-ability workers in specific firms. In an additional calculation, we find that the share of participants who work with at least one GCP coworker is 55%.

higher education in Israel often begins at ages 22–23. However, many GCP participants take the test while still in high school and often before the midpoint of the program. This creates a useful opportunity to use the UPET as an alternative measure of ability. In addition, the UPET is designed to measure cognitive skills, making it less likely to be affected by participation in the program.

To assess this concern directly, we examine whether UPET scores vary systematically with the age at which the test is taken. Appendix Table A6 shows that the differences between students who took the test earlier and later are generally small. This is especially true for GCP participants and for the quantitative score in particular (column (4)), where all differences are statistically insignificant.[37]

This evidence supports treating the quantitative UPET score as a predetermined variable and using it in the matching specification as a robustness check.[38] We therefore focus on the first test attempt of each student, on students who took the test relatively early, and use only their quantitative UPET score as the primary matching variable.

Table 10 shows that the main findings persist at older ages. We continue to find a significant increase in the likelihood of completing a double major, as well as an improvement in partner "quality," with no meaningful effects on labor-market outcomes such as earnings rank or employment in knowledge-producing sectors. Because these cohorts are older in 2020 than those in our main sample, we can also detect a sizable increase in PhD attainment: 4.6 percentage points, or about 42%.

Finally, Figure 5 compares the earnings distributions of the two gifted groups. As in the main sample, there are no significant earnings differences between GCP participants and their matched comparison students. At the same time, the figure shows a large earnings gap between both gifted groups and non-gifted students. Overall, the evidence suggests that gifted students perform well in the labor market regardless of whether they participated in a GCP, and this is not limited to early career outcomes.

[37] The finding that UPET scores are not correlated with age among talented students is not especially surprising, given that the structure and content of the test resemble the SAT and ACT, which are used for similar purposes in the United States. Existing research also documents a strong correlation between these tests, IQ, and other measures of cognitive ability (Koenig et al., 2008; Beaujean et al., 2006; Frey and Detterman, 2004).

[38] We further assess the validity of this approach by examining whether each ability measure helps eliminate baseline differences in the alternative measure. In Appendix Figure A2, we show the distributions of UPET scores across all domains before and after matching in our main specification, which uses Meitzav scores as the ability measure. Before matching, GCP participants have clearly higher UPET scores than comparison students. After matching, most of these differences disappear, and the remaining gap in quantitative scores is only marginally significant. We also verify the reverse exercise: matching on quantitative UPET scores substantially reduces baseline differences in 8th-grade Meitzav scores, especially in math, where the post-matching distributions become statistically indistinguishable. Finally, we confirm that, for the overlapping cohorts, the main results remain very similar when UPET scores are used instead of 8th-grade Meitzav scores.

### 6.6 Heterogeneity of the GCP Effects

In this section, we examine the heterogeneity of the effects. To estimate how the effects vary along dimension $z$, we estimate the following model:

$$Y_i = \alpha^h + X_i'\beta^h + \gamma^h \times z_i + \tau^h \times GCP_i + \delta^h \times GCP_i \times z_i + \varepsilon_i \tag{5}$$

The coefficient of interest is $\delta^h$, which captures how the treatment effect varies with $z_i$. The coefficient $\tau^h$ captures the treatment effect when $z_i = 0$. Thus, the treatment effect for an individual with characteristic $z_i$ is given by $\tau^h + \delta^h z_i$. Below, we report estimates of $\tau^h$ (Baseline effect) and $\delta^h$ (Interaction) for the main outcomes.

**Gender.** We begin by investigating potential gender differences in the impact of GCP participation. Columns (1) and (2) of Table 11 present the estimated disparities in the effects of GCP on boys and girls for our main outcomes. We find that our main effects on higher education choices and spouse "quality" are much stronger among male students. Specifically, we find large and significant effects of GCP participation on MA degrees for boys (compared to their gifted non-participant matches), with much smaller effects for girls. Similarly, the effects on the field of study are stronger for boys.

Finally, the effects on spouse "quality" are large for boys: GCP participation increases partner UPET score by about 60 points, while we find no corresponding effect for girls. By contrast, the estimated effect on marrying a GCP participant is similar for both genders, at about 6–7 percentage points. This pattern suggests that gifted female students tend to marry very high-ability partners even when those partners are not GCP participants, whereas for males the increase in matching with GCP peers translates into a clear improvement in partner ability. This is consistent with evidence that household formation and partner sorting may operate differently by gender, with gender norms related to relative status within the household playing a role in shaping matching patterns (Bertrand et al., 2015).

These significant gender differences are consistent with recent findings showing that gifted programs may be more effective for boys, by narrowing the preexisting non-cognitive skills gap between girls and boys (Card et al., 2024).[39]

**Socio-Economic Status (SES).** Another potentially important source of heterogeneity in GCP treatment effects is the participants' SES. To explore this, we estimate equation 5, defining $z$ as an indicator for higher SES backgrounds, proxied by the father's education being at least 15 years (an approximate threshold for a college degree). Columns (3) and (4) of Table 11 show the estimate. The results indicate that GCP

[39] Relatedly, Autor et al. (2016) show that boys benefit more from higher quality schools.

effects do not vary significantly with students' SES background, with no significant estimated interaction. These results are similar when using parental income as the proxy for SES. This lack of SES-based heterogeneity stands in contrast to many other educational interventions, whose impacts often differ by students' family background.

**Giftedness Level.** We also examine a model where we allowed for GCP impact heterogeneity by the giftedness level. To test whether treatment effects vary with pre-treatment ability, we split the sample at one standard deviation above the mean on the 8th-grade Meitzav math test score and estimate the heterogeneous-effect specification. The results are shown in columns (5) and (6) of Table 11. Almost all estimated interactions are null, indicating that the effects of GCP participation are generally consistent across the distribution of the ability level.

**Length of Participation in the GCP.** About a third of the GCP participants in the primary sample had been part of the program since middle school, while the remainder began the GCP in high school. This setup enables us to address a potentially interesting aspect of heterogeneity in GCP treatment effects: whether extended participation (from 7th to 12th grades) affects student outcomes differently than shorter participation (from 10th to 12th grades). Columns (7) and (8) of Table 11 show that participation length does not materially affect the main outcomes.

# 7 Conclusion

Gifted children receive special attention in many education systems driven by the policy-level assumption that nurturing high-ability students is essential for the long-term production of innovators and scientists. Yet, most empirical work on gifted programs has focused on short-run outcomes, standardized test scores, and educational attainment, leaving the broader question of long-run life-course outcomes underexplored. We address this question using Israel's unique setting, which combines a large-scale long-standing GCP with rich administrative data that allow us to track participants from adolescence through their peak earning years (up to age 46).

We report several novel findings. First, we find no meaningful effect of the GCP on high-school achievement. This result is surprising given the intensity of educational inputs that GCP participants receive relative to those of students in regular classes. We propose two possible explanations for this null effect. First, psychological mechanisms: moving from a setting where students feel relatively more able (a "Big-Fish-Little-Pond" environment) to one with uniformly high-ability peers can increase anxiety and reduce academic self-concept, potentially neutralizing the benefits of an enriched curriculum. Second, curricular divergence: the GCP curriculum covers advanced topics beyond the standard matriculation curriculum; hence, participants may gain educational benefits that are not reflected in standardized test performance.

Second, we find substantial effects on higher-education outcomes: a large increase in double-majoring and a higher likelihood of pursuing advanced degrees. These patterns are consistent with the "multipotentiality" of gifted youth and the internal and social pressure to "maximize" rather than "waste" one's potential. We argue that GCP participation reinforces a future-oriented academic identity that favors breadth and intellectual prestige over early vocational specialization or immediate financial returns. For that reason, the absence of an earnings effect is consistent with our other findings: participants select paths that emphasize academic rigor rather than wage-maximization. The null effect on entry into knowledge-producing sectors is less obvious, but it likely suggests that the baseline propensity for these sectors is established early; thus, the program shifts the level of academic attainment within those sectors (e.g., from BA to PhD) rather than the probability of entry into them. Finally, we find that participation in a GCP profoundly matters for marital matches and professional networks. The persistent concentration of GCP alumni in the same social and professional enclaves suggests that exposure to similarly gifted peers is an important benefit of GCP participation.

Importantly, the benefits and gains accruing in gifted children's programs should be weighed against broader social costs. Previous studies (Lavy et al., 2012; Balestra et al., 2021) suggest that non-gifted students benefit from having high-achieving peers. Thus, the systematic removal of gifted students may impose negative externalities on the overall student population. Given the high fiscal costs of these programs and the absence of measurable labor-market premiums or increased entry into knowledge-producing sectors, our findings raise important questions about the overall social benefits of gifted education as currently designed.

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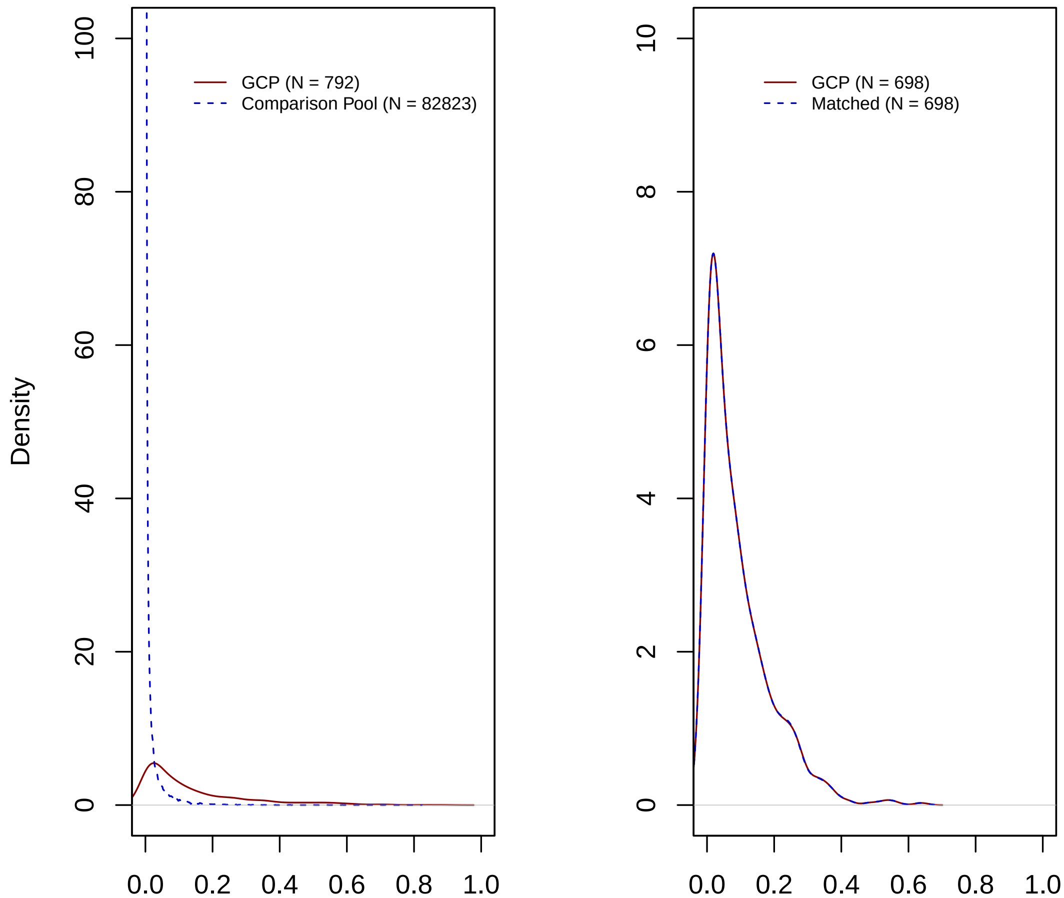
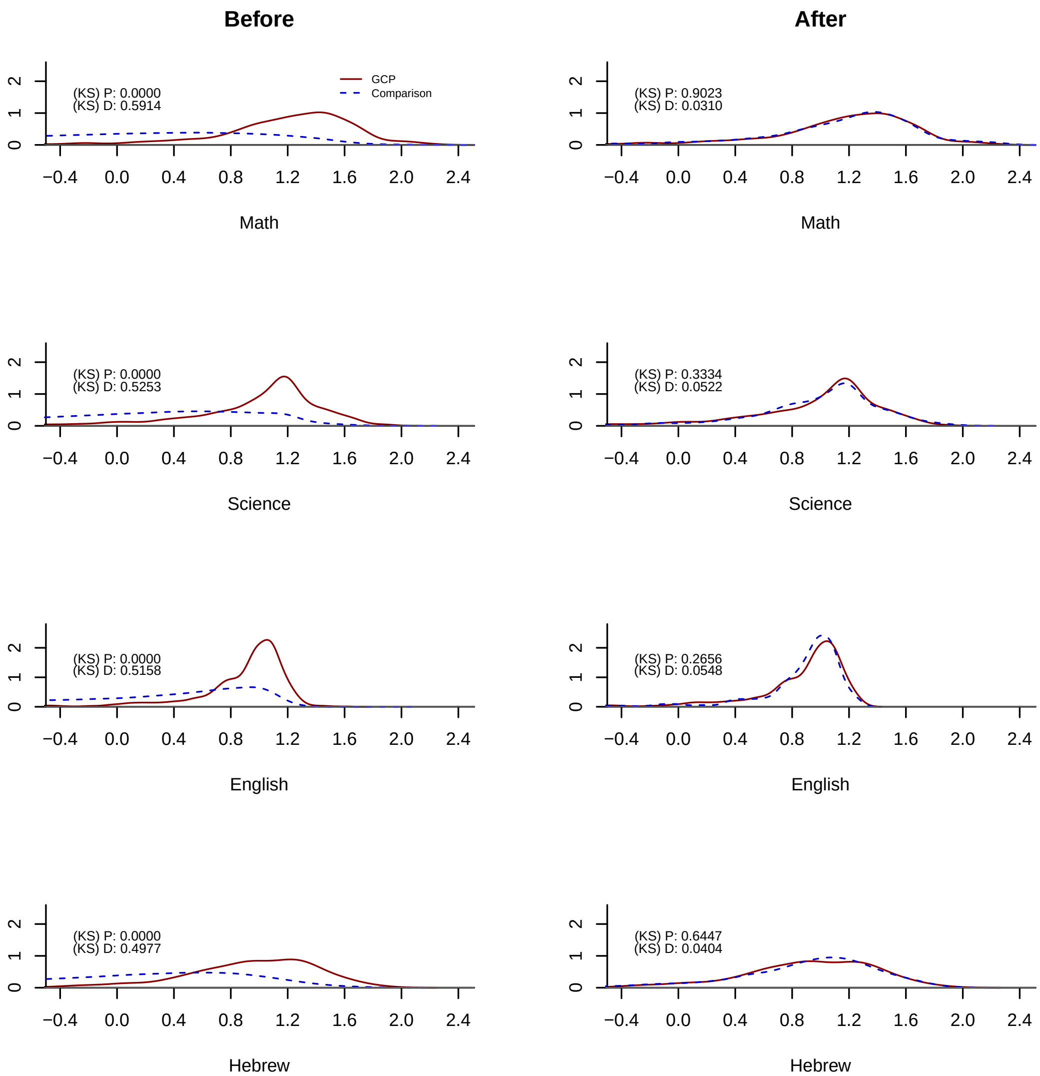
Figure 1: Estimated Propensity Score Distributions for GCP Participation, Before and After Matching



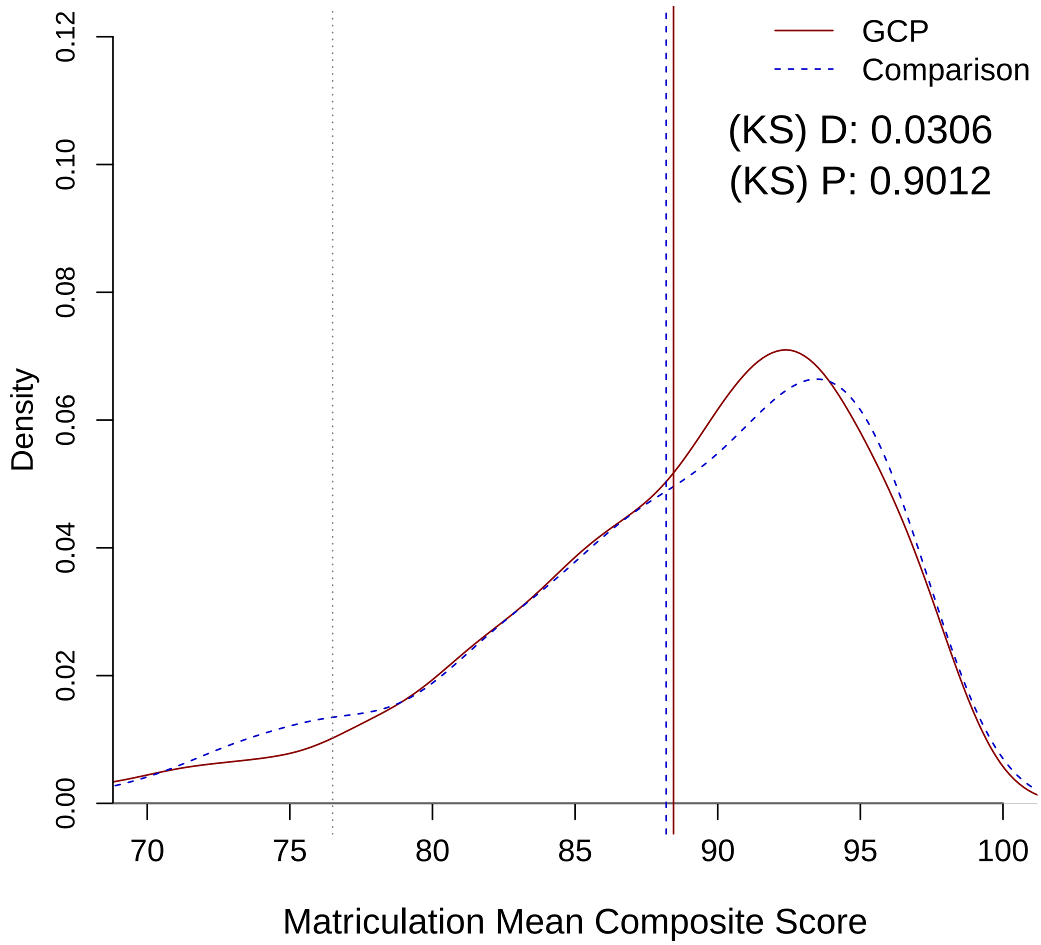

*Notes:* This figure plots the propensity score distributions for GCP participation. The solid red line represents the sample of GCP students, while the blue dashed line represents the comparison group, which includes non-GCP students from other cities. The left panel shows the distributions before matching, and the right panel shows the distributions after matching. The sample includes students who participated in the Meitzav middle school test during their 8th grade, constituting about half of the students in cohorts of high-school graduates from 2006 to 2010. GCP stands for Gifted Children's Program.

Figure 2: Pre-Treatment Middle-School Test Score Distributions, Before and After Matching



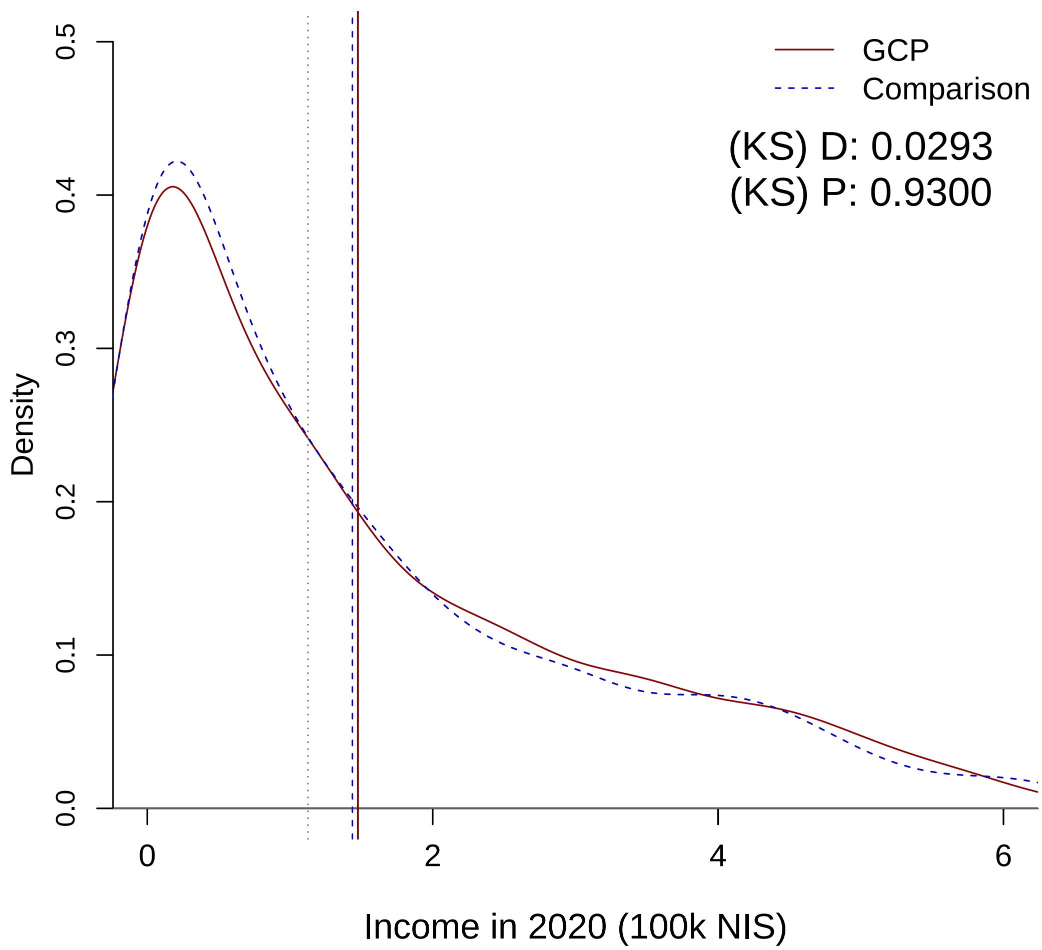

*Notes:* This figure plots the pre-treatment 8th-grade Meitzav test score distributions. The solid red lines represent the sample of GCP students, and the blue dashed lines represent the comparison group (which includes non-GCP students from other cities). The figures on the left show the distributions before the matching, and those on the right show the distributions after the matching. The figures also show the Kolmogorov–Smirnov test for the equality of the probability distributions. The sample includes students who participated in the 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. GCP stands for Gifted Children's Program.

Figure 3: Matriculation GPA Distributions, GCP Participants and Comparison Group



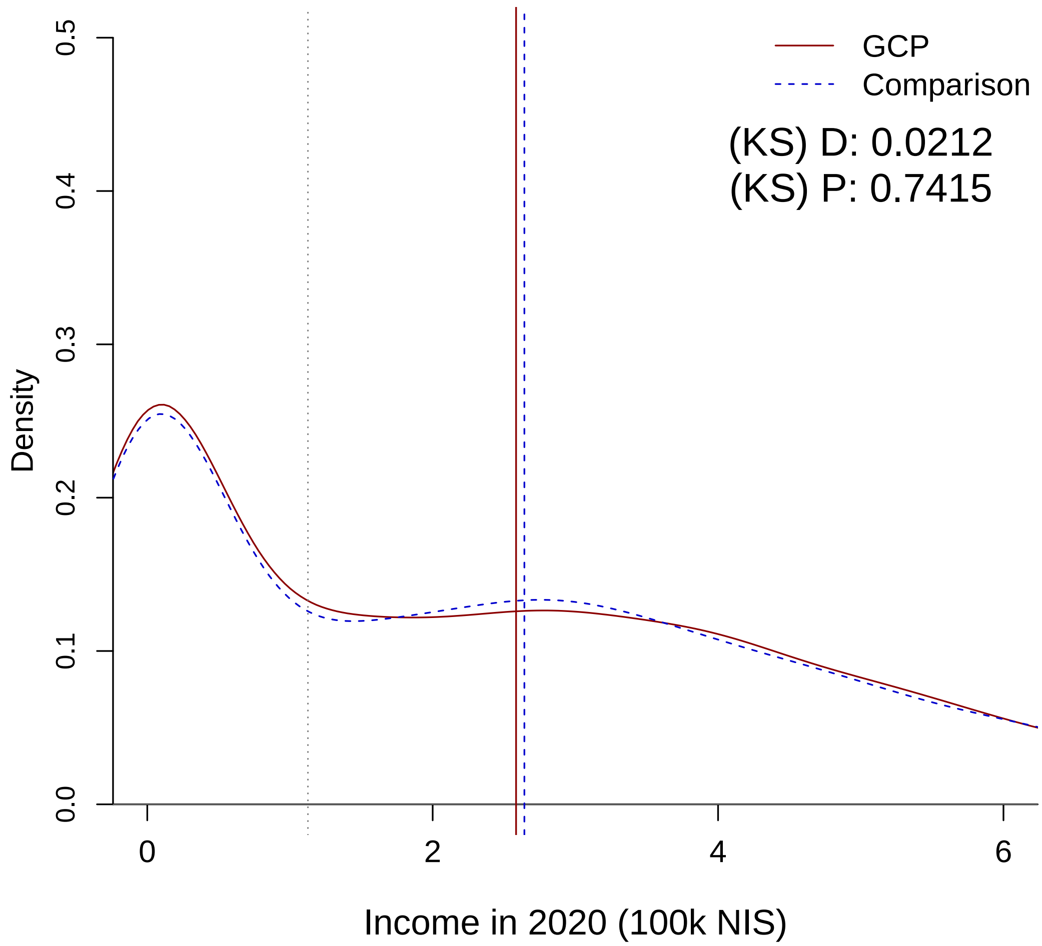

*Notes:* This figure plots the matriculation GPA distributions of GCP participants and the matched comparison group. The solid red line represents the sample of GCP students, and the blue dashed line represents the matched comparison group. The figure reports the Kolmogorov–Smirnov test for the equality of the probability distributions. The vertical lines represent the averages. The dotted grey line represents the average among students in the comparison group's pool, including all students from cities with no GCPs. The figure is based on the sample of students who participated in the 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. GCP stands for Gifted Children's Program.

Figure 4: Annual Earnings Distributions, GCP Participants and Comparison Group




*Notes:* This figure plots the distribution of the annual earnings in 2020 of GCP participants and the matched comparison group. The solid red line represents the sample of GCP students, and the blue dashed line represents the matched comparison group. The figure reports the Kolmogorov–Smirnov test for the equality of the probability distributions. The vertical lines represent the averages. The dotted grey line represents the average among students in the comparison group's pool, including all students from cities with no GCPs. The figure is based on the sample of students who participated in the 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. GCP stands for Gifted Children's Program.

Figure 5: Annual Earnings Distributions, GCP Participants and Comparison Group, 1992-2005 Graduates




*Notes:* This figure plots the distribution of the annual earnings in 2020 of GCP participants and the matched comparison group. The solid red line represents the sample of GCP students, and the blue dashed line represents the comparison group (which includes non-GCP students from other cities). The figure also shows the Kolmogorov–Smirnov test for the equality of the probability distributions. The dotted black line represents the comparison group pool, which includes non-gifted students. The sample includes students who participated in the UPET (until the age of 17) from the cohorts of high-school graduates in 1992-2005.

Table 1: Balancing Tests for the Locality-Level Analysis of GCP Availability

| | Baseline Mean (1) | Diff-in-Diff (2) |
|---|---|---|
| **A. Parental Characteristics** | | |
| Father Education | 16.00 | 0.16 |
| | | (0.16) |
| Mother Education | 15.72 | 0.07 |
| | | (0.14) |
| Father Born in Israel (%) | 57.66 | 0.43 |
| | | (2.14) |
| Mother Born in Israel (%) | 61.30 | 0.67 |
| | | (2.76) |
| Father Age at Birth | 32.33 | 0.22 |
| | | (0.23) |
| Mother Age at Birth | 29.48 | 0.16 |
| | | (0.28) |
| Father Earnings (100K NIS) | 2.54 | -0.02 |
| | | (0.21) |
| Mother Earnings (100K NIS) | 1.26 | 0.01 |
| | | (0.06) |
| **B. Background** | | |
| Number of Siblings | 1.67 | 0.10 |
| | | (0.07) |
| Family Order | 1.74 | 0.02 |
| | | (0.06) |
| % Born in Israel | 79.90 | 1.59 |
| | | (3.08) |
| **C. Matriculation** | | |
| Total Credits | 32.80 | 1.51** |
| | | (0.68) |
| % Math, 5 Credits | 96.65 | 1.34 |
| | | (1.27) |
| % English, 5 Credits | 98.86 | 0.21 |
| | | (3.06) |
| % Physics, 5 Credits | 61.44 | 4.73 |
| | | (4.23) |
| % Computer Science, 5 Credits | 72.92 | -0.51 |
| | | (3.86) |
| % Chemistry, 5 Credits | 36.56 | 1.37 |
| | | (5.89) |
| % Biology, 5 Credits | 10.48 | 4.14 |
| | | (2.53) |
| Students | 131,065 | 131,065 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table presents falsification tests for the locality-level analysis of the effects of GCP availability. Column (1) reports the baseline mean for likely gifted students ($G_i = 1$) in localities without access to a GCP. Column (2) reports the estimate of the difference-in-differences coefficient ($\delta$ from equation 1). Standard errors are clustered at the locality level and reported in parentheses.

Table 2: Balancing Tests for the Matched Sample

| | Mean | | Difference |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| **A. Background** | | | |
| Number of Siblings | 1.72 | 1.69 | -0.03 |
| | | | (0.07) |
| Family Order | 1.79 | 1.82 | 0.04 |
| | | | (0.05) |
| % Born in Israel | 81.38 | 82.81 | 1.43 |
| | | | (2.05) |
| **B. Parental Characteristics** | | | |
| Father Earnings (100K NIS) | 2.34 | 2.11 | -0.23 |
| | | | (0.15) |
| Mother Earnings (100K NIS) | 1.13 | 1.11 | -0.01 |
| | | | (0.06) |
| Father Education | 15.21 | 15.14 | -0.07 |
| | | | (0.16) |
| Mother Education | 15.20 | 15.04 | -0.16 |
| | | | (0.15) |
| Father Born in Israel (%) | 59.03 | 60.03 | 1.00 |
| | | | (2.63) |
| Mother Born in Israel (%) | 61.32 | 64.76 | 3.44 |
| | | | (2.58) |
| Father Age at Birth | 32.29 | 32.54 | 0.26 |
| | | | (0.28) |
| Mother Age at Birth | 29.46 | 29.74 | 0.29 |
| | | | (0.28) |
| **C. Matriculation** | | | |
| Total Credits | 29.37 | 29.90 | 0.54 |
| | | | (0.33) |
| % Math, 5 Credits | 74.21 | 73.93 | -0.29 |
| | | | (2.35) |
| % English, 5 Credits | 90.83 | 91.26 | 0.43 |
| | | | (1.53) |
| % Physics, 5 Credits | 47.99 | 46.42 | -1.58 |
| | | | (2.67) |
| % Computer Science, 5 Credits | 44.84 | 48.14 | 3.30 |
| | | | (2.67) |
| % Chemistry, 5 Credits | 30.80 | 28.08 | -2.72 |
| | | | (2.44) |
| % Biology, 5 Credits | 15.47 | 16.33 | 0.86 |
| | | | (1.96) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows the balance between GCP participants and the matched comparison group. The sample includes only students who participated in the 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006–2010. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the unconditional difference and its standard error. Standard errors are clustered at the school level. In panel B, annual earnings refer to total earnings in 2006, measured in 100K NIS. GCP stands for Gifted Children's Program.

Table 3: Falsification Test - Matching Approach - "Impacts" of Regular Classes

| | Mean | | Estimated "Effect" |
|---|---|---|---|
| | Comparison | Placebo | |
| | (1) | (2) | (3) |
| **A. Matriculation** | | | |
| GPA | 81.08 | 81.11 | 0.02 |
| | | | (0.38) |
| **B. Degree Enrollment** | | | |
| % BA | 76.83 | 77.40 | 0.57 |
| | | | (2.09) |
| % MA | 17.67 | 20.81 | 3.14 |
| | | | (2.19) |
| % PhD | 1.00 | 1.82 | 0.82 |
| | | | (0.70) |
| **C. Field of Study** | | | |
| % Math/CS | 6.00 | 5.42 | -0.58 |
| | | | (1.32) |
| % Engineering | 10.83 | 9.62 | -1.21 |
| | | | (1.68) |
| % Double Major | 12.17 | 11.83 | -0.34 |
| | | | (1.85) |
| **D. Labor Market** | | | |
| Earnings Rank | 54.82 | 55.35 | 0.53 |
| | | | (1.40) |
| % Knowledge | 20.83 | 20.06 | -0.77 |
| | | | (2.26) |
| **E. Personal** | | | |
| % Married | 42.50 | 42.23 | -0.27 |
| | | | (2.77) |
| Spouse UPET Score | 571.75 | 561.24 | -10.50 |
| | | | (11.39) |
| Students | 600 | 600 | 1,200 |
| Schools | 194 | 110 | 304 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows the results of a placebo exercise, estimating the effects of studying in regular classes (in high schools with no GCP, located in localities with a GCP), using the same matching algorithm we use throughout our analysis. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. The "treatment" group is defined as the matched comparison units in the model selecting comparison units from the same localities (in other high schools). Columns (1) and (2) show the means among the matched comparison group (students in other localities) and the treatment group (high-ability students from regular classes in localities with a GCP). Column (3) shows the conditional difference in the outcome ($\tau$ from equation (4)) and its standard error (clustered at the school level).

Table 4: Effects of GCP Availability, Locality-Level Analysis

| | Baseline Mean | Diff-In-Diff | | | IV |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **A. High School** | | | | | |
| GCP Participation | 0.00 | 31.35*** | 31.33*** | 31.25*** | — |
| | | (5.99) | (5.99) | (5.95) | |
| Matriculation GPA | 91.77 | 1.16** | 0.67 | 0.23 | 0.75 |
| | | (0.56) | (0.66) | (0.52) | (1.72) |
| **B. Degree Enrollment** | | | | | |
| % BA | 96.51 | 3.93* | 2.58 | 1.35 | 4.33 |
| | | (2.03) | (1.83) | (1.17) | (3.97) |
| % MA | 35.21 | 7.63*** | 7.19*** | 6.18** | 19.77** |
| | | (2.47) | (2.39) | (2.56) | (8.23) |
| % PhD | 5.27 | 2.35* | 2.31* | 2.19* | 7.01 |
| | | (1.25) | (1.24) | (1.31) | (4.41) |
| **C. Field of Study** | | | | | |
| % Math/CS | 23.24 | 5.23** | 5.17** | 5.01** | 16.05* |
| | | (2.37) | (2.31) | (2.39) | (9.69) |
| % Engineering | 27.94 | -1.81 | -1.99 | -3.02* | -9.66 |
| | | (1.84) | (1.87) | (1.80) | (5.90) |
| % Double Major | 21.03 | 6.66*** | 6.35*** | 5.78*** | 18.50*** |
| | | (1.50) | (1.63) | (1.63) | (6.04) |
| **D. Labor Market** | | | | | |
| Earnings Rank | 61.41 | 0.01 | -0.12 | -0.43 | -1.39 |
| | | (1.43) | (1.43) | (1.48) | (4.66) |
| % Knowledge | 48.61 | 1.86 | 1.53 | 0.99 | 3.18 |
| | | (2.35) | (2.28) | (2.39) | (7.77) |
| **E. Spouse** | | | | | |
| % Married | 42.34 | 0.78 | 1.00 | 0.24 | 0.76 |
| | | (2.51) | (2.49) | (2.58) | (8.29) |
| UPET Score | 605.77 | 12.77** | 13.13** | 10.43* | 36.55* |
| | | (5.66) | (5.58) | (6.04) | (18.70) |
| Test Score Controls | | | + | + | + |
| Other Controls | | | | + | + |
| Students | | | 131,065 | | |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table presents the estimated effects of high-school GCP availability at the locality level on gifted students' outcomes, which can also be interpreted as the intention-to-treat effects (columns (2)–(4)) and as the local average treatment effects (column (5)). Column (1) displays the baseline mean for likely gifted students ($G_i = 1$) in localities without access to a GCP. Columns (2)–(4) show the estimated effects from the diff-in-diff analysis ($\delta$ from Equation 1) while column (5) shows the effects from the IV analysis ($\delta^{IV}$ from Equation 3). In column (2), the estimation is conducted without any control variables. In column (3), only test scores are included as controls. In columns (4) and (5), all control variables are incorporated. Standard errors are clustered at the locality level and reported in parentheses.

Table 5: The Impact of GCP Participation, Matched Samples

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **A. Matriculation** | | | | | | |
| GPA | 0.26 | 0.24 | 0.29 | 0.27 | 0.30 | 0.42 |
| | (0.29) | (0.29) | (0.30) | (0.30) | (0.31) | (0.31) |
| **B. Degree Enrollment** | | | | | | |
| % BA | 3.42** | 3.47*** | 3.54** | 3.30** | 4.53*** | 4.23*** |
| | (1.34) | (1.29) | (1.39) | (1.35) | (1.42) | (1.42) |
| % MA | 10.10*** | 9.96*** | 9.71*** | 10.19*** | 12.44*** | 8.88*** |
| | (2.46) | (2.43) | (2.50) | (2.48) | (2.44) | (2.58) |
| % PhD | 3.90*** | 4.09*** | 4.13*** | 3.47*** | 5.10*** | 3.89*** |
| | (1.20) | (1.19) | (1.22) | (1.21) | (1.16) | (1.25) |
| **C. Field of Study** | | | | | | |
| % Math/CS | 4.45** | 4.71** | 4.16** | 4.02** | 4.96** | 5.44*** |
| | (1.94) | (1.93) | (1.96) | (1.95) | (2.07) | (1.97) |
| % Engineering | -6.43*** | -6.67*** | -6.47*** | -6.23*** | -7.45*** | -7.38*** |
| | (2.12) | (2.10) | (2.14) | (2.15) | (2.18) | (2.22) |
| % Double Major | 10.21*** | 9.92*** | 10.34*** | 10.72*** | 9.53*** | 5.62** |
| | (2.14) | (2.12) | (2.18) | (2.13) | (2.19) | (2.25) |
| **D. Labor Market** | | | | | | |
| Earnings Rank | 0.62 | 0.58 | 0.70 | 0.81 | -1.63 | -2.95* |
| | (1.59) | (1.57) | (1.62) | (1.61) | (1.61) | (1.65) |
| % Knowledge | 1.41 | 0.95 | 1.02 | 1.09 | -2.30 | 0.01 |
| | (2.51) | (2.48) | (2.57) | (2.53) | (2.59) | (2.60) |
| **E. Personal** | | | | | | |
| % Married | 1.62 | 1.57 | 2.01 | 1.61 | 1.36 | -4.56* |
| | (2.54) | (2.51) | (2.60) | (2.57) | (2.53) | (2.67) |
| UPET Score | 31.86*** | 33.05*** | 27.49*** | 30.00*** | 20.75** | 29.03*** |
| | (9.38) | (9.20) | (9.68) | (9.54) | (9.95) | (9.84) |
| Replacement | | | | + | | |
| Caliper | 0.1 | 0.2 | 0.05 | 0.1 | 0.05 | 0.1 |
| PS Estimation | Logit | Logit | Logit | Logit | XGB | Logit |
| Comparison localities | Other | Other | Other | Other | Other | Large |
| Students | 1,396 | 1,438 | 1,338 | 1,373 | 1,442 | 1,274 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on outcomes, using different matching specifications: with/without replacement, changing the caliper, the method for estimating the propensity score, and the comparison group's pool. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006–2010. Each column shows the conditional difference ($\tau$ from equation (4)) in the outcome mentioned on the left and its standard error (clustered at the school level).

Table 6: The Impact of GCP Participation on Peers' Characteristics

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| Number of Students | 33.99 | 27.67 | -6.32*** (0.27) |
| % Males | 52.81 | 62.09 | 9.28*** (0.65) |
| Father Education | 14.20 | 15.58 | 1.37*** (0.06) |
| Mother Education | 14.30 | 15.32 | 1.02*** (0.05) |
| Total Matriculation Credits | 25.93 | 30.34 | 4.42*** (0.15) |
| % 5 Credits in Math | 36.83 | 76.30 | 39.47*** (1.04) |
| % 5 Credits in English | 70.03 | 91.73 | 21.70*** (0.82) |
| % 5 Credits in Physics | 24.99 | 50.08 | 25.09*** (0.95) |
| % Matriculation | 85.92 | 93.18 | 7.26*** (0.56) |
| UPET Score | 586.58 | 658.45 | 71.88*** (2.15) |
| Meitzav Math Score | 0.64 | 1.14 | 0.50*** (0.02) |
| Meitzav Science Score | 0.53 | 0.96 | 0.43*** (0.01) |
| Meitzav English Score | 0.54 | 0.84 | 0.30*** (0.01) |
| Meitzav Hebrew Score | 0.48 | 0.89 | 0.40*** (0.01) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on high school class-level outcomes. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Columns (1) and (2) show the means in class-level outcomes among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level). GCP stands for Gifted Children's Program.

Table 7: The Impact of GCP on Undergraduate Degree Fields of Study

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| **A. STEM Fields** | | | |
| % Any STEM | 46.42 | 45.52 | -0.90 |
| | | | (2.38) |
| % Math, Computer Science, Statistics | 15.76 | 20.20 | 4.45** |
| | | | (1.94) |
| % Engineering | 23.93 | 17.49 | -6.43*** |
| | | | (2.12) |
| % Physical Sciences | 6.16 | 9.16 | 3.00** |
| | | | (1.40) |
| % Biological Sciences | 4.58 | 4.41 | -0.18 |
| | | | (1.06) |
| **B. Non-STEM Fields** | | | |
| % Any Non-STEM | 29.23 | 35.02 | 5.80** |
| | | | (2.34) |
| % Humanities | 4.01 | 9.51 | 5.50*** |
| | | | (1.36) |
| % Social Sciences | 12.32 | 17.70 | 5.38*** |
| | | | (1.88) |
| **C. Double Majors** | | | |
| % Any Double Major | 15.47 | 25.68 | 10.21*** |
| | | | (2.14) |
| % Double STEM | 6.45 | 8.48 | 2.03 |
| | | | (1.36) |
| % STEM & Non-STEM | 2.29 | 3.90 | 1.61* |
| | | | (0.92) |
| % Double Non-STEM | 6.73 | 13.30 | 6.57*** |
| | | | (1.61) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on fields of study in undergraduate degrees. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level). GCP stands for Gifted Children's Program.

Table 8: The Impact of GCP on Labor Market Outcomes

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| **A. Annual Earnings** | | | |
| Earnings (100K NIS) | 1.44 | 1.45 | 0.01 |
| | | | (0.08) |
| Log Earnings | 11.62 | 11.65 | 0.02 |
| | | | (0.08) |
| Earnings Rank | 57.02 | 57.64 | 0.62 |
| | | | (1.59) |
| % Top 10% Earners | 23.07 | 23.74 | 0.67 |
| | | | (2.17) |
| **B. Employment** | | | |
| % Salaried Employment | 79.23 | 78.20 | -1.03 |
| | | | (2.24) |
| % Self Employment | 5.87 | 4.30 | -1.57 |
| | | | (1.18) |
| **C. Employment in Knowledge Producing Sectors** | | | |
| % Any Knowledge Sector | 40.26 | 41.67 | 1.41 |
| | | | (2.51) |
| % Tech Services | 29.23 | 31.52 | 2.30 |
| | | | (2.38) |
| % Tech Manufacturing | 4.44 | 2.50 | -1.94** |
| | | | (0.91) |
| % Academic | 6.59 | 7.65 | 1.06 |
| | | | (1.38) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on labor-market outcomes in 2020. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level). GCP stands for Gifted Children's Program.

Table 9: The Impact of GCP on Personal, Spouse, and Coworkers Outcomes

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| **A. Personal Outcomes** | | | |
| % Lives Outside Israel | 4.73 | 6.07 | 1.34 |
| | | | (1.21) |
| % Married | 38.11 | 39.73 | 1.62 |
| | | | (2.54) |
| % Divorced | 1.15 | 1.47 | 0.33 |
| | | | (0.61) |
| % Has Kids | 19.63 | 20.28 | 0.65 |
| | | | (1.99) |
| **B. Spouse Outcomes** | | | |
| UPET Total Score | 589.92 | 621.78 | 31.86*** |
| | | | (9.38) |
| % GCP Participant | 1.63 | 8.24 | 6.61*** |
| | | | (1.88) |
| **C. Coworkers Outcomes at Plant** | | | |
| Total | 136.73 | 131.52 | -5.21 |
| | | | (12.15) |
| GCP Participants | 1.83 | 2.81 | 0.99*** |
| | | | (0.35) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on personal, spouse, and coworker outcomes. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level). GCP stands for Gifted Children's Program, and UPET stands for University Psychometric Entrance Test.

Table 10: Impact of GCP on Main Outcomes, based on a Sample of 1992–2005 Graduates

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| **A. Degree Enrollment** | | | |
| % BA | 97.52 | 98.04 | 0.52 |
| | | | (0.45) |
| % MA | 56.48 | 57.71 | 1.24 |
| | | | (1.52) |
| % PhD | 11.00 | 15.60 | 4.60*** |
| | | | (1.04) |
| **B. Field of Study** | | | |
| % Math/CS | 21.14 | 22.14 | 1.00 |
| | | | (1.22) |
| % Engineering | 27.38 | 22.92 | -4.47*** |
| | | | (1.30) |
| % Double Major | 26.90 | 30.72 | 3.82*** |
| | | | (1.40) |
| **C. Labor Market** | | | |
| Earnings Rank | 68.21 | 67.74 | -0.48 |
| | | | (1.02) |
| % Knowledge | 40.19 | 39.54 | -0.65 |
| | | | (1.48) |
| **D. Personal** | | | |
| % Married | 67.19 | 67.06 | -0.13 |
| | | | (1.45) |
| UPET Score | 601.51 | 619.08 | 17.58*** |
| | | | (3.64) |
| Students | 2,100 | 2,100 | 4,200 |
| Schools | 156 | 11 | 167 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on main outcomes. The sample includes only students who took the UPET early (until the age of 17) from the cohorts of high-school graduates between 1992–2005. The matching specification includes only the quantitative UPET scores as the ability measure. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level). GCP stands for Gifted Children's Program, and UPET stands for University Psychometric Entrance Test.

Table 11: Heterogeneity in the Impact of GCP

| | Gender | | SES | | Ability | | Length | |
|---|---|---|---|---|---|---|---|---|
| | Baseline effect | Interaction | Base. effect | Inter. | Base. effect | Inter. | Base. effect | Inter. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **A. Matriculation** | | | | | | | | |
| GPA | 0.11 | 0.39 | -0.33 | 0.98 | 0.28 | -0.03 | 0.34 | -0.23 |
| | (0.38) | (0.57) | (0.52) | (0.62) | (0.55) | (0.57) | (0.33) | (0.43) |
| **B. Degree Enrollment** | | | | | | | | |
| % BA | 4.47*** | -2.71 | 2.93 | 0.80 | 5.89** | -3.54 | 3.54** | -0.34 |
| | (1.71) | (2.70) | (2.53) | (2.96) | (2.74) | (2.77) | (1.57) | (1.76) |
| % MA | 14.20*** | -10.58** | 10.59*** | -0.82 | 12.76*** | -3.82 | 10.13*** | -0.09 |
| | (3.15) | (5.06) | (3.65) | (4.89) | (3.94) | (4.60) | (2.86) | (4.07) |
| % PhD | 5.14*** | -3.20 | 3.24* | 1.08 | 2.29 | 2.31 | 3.95*** | -0.14 |
| | (1.65) | (2.47) | (1.77) | (2.33) | (1.70) | (2.05) | (1.45) | (2.27) |
| **C. Fields of Study** | | | | | | | | |
| % Math/CS | 8.61*** | -10.73*** | 0.94 | 5.76 | 5.50** | -1.51 | 3.80* | 1.88 |
| | (2.81) | (3.69) | (2.84) | (3.84) | (2.76) | (3.46) | (2.21) | (3.17) |
| % Engineering | -7.60*** | 3.01 | -3.48 | -4.85 | -3.51 | -4.19 | -4.95** | -4.30 |
| | (2.83) | (4.29) | (3.38) | (4.35) | (3.19) | (3.74) | (2.41) | (3.14) |
| % Double Maj. | 11.34*** | -2.92 | 9.17*** | 1.71 | 9.35*** | 1.23 | 8.10*** | 6.09 |
| | (2.79) | (4.40) | (3.24) | (4.32) | (3.32) | (4.00) | (2.43) | (3.79) |
| **D. Labor Market** | | | | | | | | |
| Earn. Rank | 1.13 | -1.32 | 0.86 | -0.40 | -0.89 | 2.16 | -0.94 | 4.49* |
| | (2.18) | (3.17) | (2.48) | (3.22) | (2.47) | (2.94) | (1.79) | (2.57) |
| % Knowledge | 1.55 | -0.36 | 4.43 | -4.97 | 0.50 | 1.30 | 0.10 | 3.77 |
| | (3.35) | (5.12) | (3.84) | (5.08) | (3.89) | (4.57) | (2.84) | (4.13) |
| **E. Personal** | | | | | | | | |
| % Married | 4.24 | -6.75 | -0.11 | 2.84 | -2.31 | 5.63 | 1.99 | -1.07 |
| | (3.17) | (5.36) | (4.11) | (5.22) | (4.08) | (4.60) | (2.88) | (3.94) |
| Spouse UPET | 57.73*** | -60.31*** | 28.42* | 5.36 | 5.02 | 37.44** | 27.07** | 13.42 |
| | (12.22) | (20.02) | (16.14) | (20.18) | (14.55) | (16.29) | (10.68) | (13.12) |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the heterogeneity of the impact of GCP participation on outcomes. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006–2010. Columns (1), (3), (5), and (7) show the estimated effect on the baseline group ($\tau^h$ from equation (5)). Columns (2), (4), (6), and (8) show the estimated difference between the effects on different groups of participants ($\delta^h$), according to the characteristics mentioned at the top row: indicators for females, high-SES, high-ability, and for long GCP participation (since middle school). Standard errors are clustered at the school level. GCP stands for Gifted Children's Program, and UPET stands for University Psychometric Entrance Test.

# Online Appendices

# Appendix A Appendix

## A.1 Data Sources

We use several panel datasets from Israel's Central Bureau of Statistics (CBS). CBS allows restricted access to this data in their protected research lab. The underlying data sources include the following. The population registry data consists of a fictitious individual national I.D. number that appears in all the datasets described below and enables the matching and merging of the files at the personal level. It also contains marital status, number of children, and birth year. In addition, administrative records of the Ministry of Education on Israeli high schools' universe during the 1992-2016 school years provide the following student's family-background variables: parental schooling, number of siblings, country of birth, ethnicity, student's detailed study program by subject and level, a variety of high school achievement measures, and test scores in all national matriculation exams in 10th-12th-grades. Another source is the Higher Council of Education records of post-secondary completed degrees (B.A., MA, and Ph.D.), the institution of study (colleges and universities), majors (one or two), and completion date. Finally, we also observe Israel Tax Authority (ITA) information on income and earnings of employees and self-employed individuals for 2000–2020, and three-digit code of industry of employment. CBS matched and merged these files using the individual-level national I.D. number. The matching is perfect without the loss of observations.

## A.2 Identifying GCP classes

We begin by constructing a treatment indicator to represent participation in any of the first eleven GCPs in Israel, as detailed in the main section of our study. These eleven GCPs were identified using the high school identifiers and class numbers found in our dataset. Out of these, eight GCPs were established by 1990, the earliest year included in our dataset, two GCPs were introduced in 1995, and one started in 1999. During this time, nine of the schools provided only one gifted class per cohort, while the remaining two schools exclusively offered gifted classes.

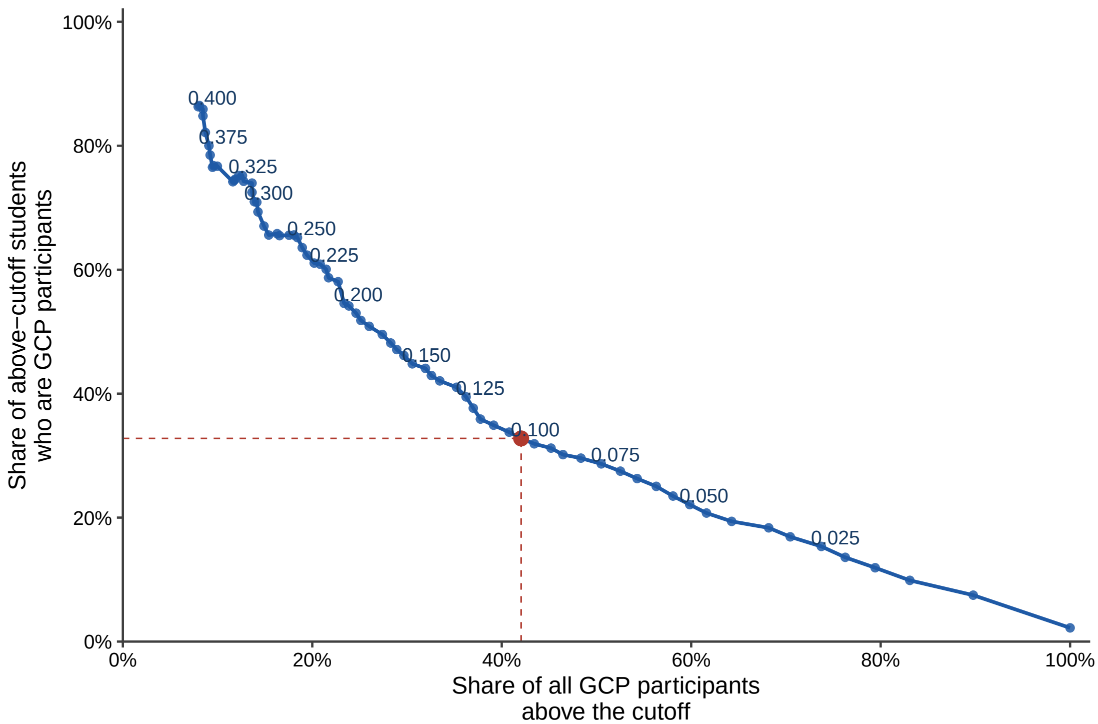
Beginning in the 2000s, the Israeli Ministry of Education began documenting class types, with specific markers for gifted classes. We utilize this information to pinpoint new GCPs introduced during the timeframe of our study. However, our analysis deliberately excludes these newer GCPs to maintain a consistent treatment group, focusing solely on students from the earliest established gifted classes. Nonetheless, we leverage this data to remove any locality-cohort combinations that have access to a GCP from our comparison group, ensuring our control group does not include localities with available gifted education options.

## A.3 Missing Values Imputation

For some variables used in our matching specification, including parental years of education, our data had a small share of missing values. To handle this, we imputed the median value of this outcome in our sample and added an indicator variable for imputed observations. Then, we include in our matching specifications the variable itself, with the imputed data and the indicator for imputed observations.

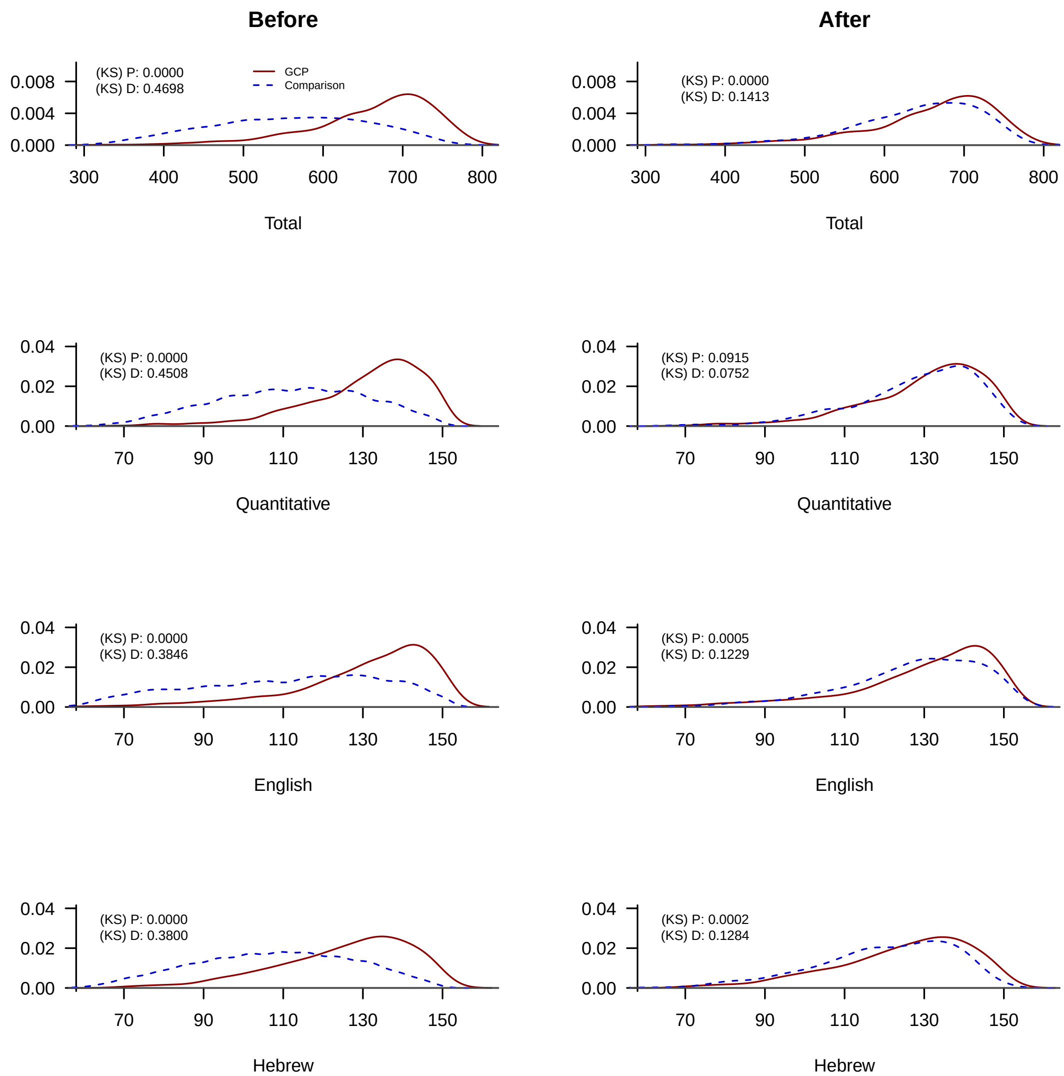
## A.4 Other Figures and Tables

Appendix Figure A1: Tradeoffs Between Share of Students and Share of Participants in the choice of Threshold $G_i$




*Notes:* This figure plots, for a range of cutoff thresholds for $G_i$, the precision of the classifier on the y-axis—defined as the share of students above the cutoff who are GCP participants—against its recall on the x-axis—defined as the share of all GCP participants who lie above the cutoff.

Appendix Figure A2: UPET Score Distributions, Before and After Matching




*Notes:* This figure plots the UPET scores distribution. The solid red lines represent the sample of GCP students, and the blue dashed lines represent the matched comparison group (which includes non-GCP students from other cities). The figures on the left show the distributions before the matching, and those on the right show the distributions after the matching. The figures also show the Kolmogorov–Smirnov test for the equality of the probability distributions. The sample includes students who participated in the 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Note that the UPET scores are not included in the matching specification.

Appendix Table A1: Definitions of field-of-study groups used in the paper

| Paper group | STEM in paper | CBS codes | Main included fields |
|---|---|---|---|
| Math / CS | Yes | 900–999 | Mathematics, statistics, computer science, information systems, data science, bioinformatics, operations research. |
| Physical sciences | Yes | 1000–1099, 1700, 1710 | Chemistry, physics, mechanics, nanoscience, geology, geophysics, oceanography, hydrology, climate and earth sciences. |
| Biological sciences | Yes | 1100–1299 | Biology, microbiology, genetics, biochemistry, neuroscience, ecology, agriculture, food science, veterinary medicine. |
| Engineering | Yes | 1300–1399 | Civil, mechanical, electrical/computer, chemical, industrial, aeronautical, materials, biomedical, and energy engineering; architecture and urban planning. |
| Humanities | No | 1–199, 1401–1402, 1500–1501, 1550, 1800, 2000, 2500 | Bible/Jewish studies, philosophy, history, archaeology, Hebrew and foreign languages, literature, linguistics, translation, library science. |
| Social sciences | No | 400–499 | Economics, sociology/anthropology, political science, international relations, psychology, social work, geography, communications, criminology. |
| Business & management | No | 500–599 | Business administration, accounting, labor studies, public administration, marketing, finance, banking, logistics, hotel/tourism management. |
| Law | No | 600–601 | Law. |
| Education | No | 200–299 | Education, curriculum/didactics, educational administration, educational counseling, special education, math/science teaching tracks. |
| Arts | No | 300–399 | Art, art history, music, theater, film/television, dance, design, creative arts therapy. |
| Medicine | No | 700–799 | General medicine, dentistry, and post-MD medical studies. |
| Medical / health professions | No | 800–899 | Pharmacy, public health, nursing, paramedicine, rehabilitation, occupational therapy, physiotherapy, nutrition, imaging, optometry. |

*Notes:* The table reports the field-of-study coding used to construct the higher-education outcomes in the paper. Definitions follow CBS classifications. The paper's STEM aggregate is defined as the union of Math/CS, Physical Sciences, Biological Sciences, and Engineering. The listed fields are illustrative rather than exhaustive; the CBS codes column provides the exact inclusion rule.

Appendix Table A2: Standard Errors Calculation Robustness, Cluster-Bootstrapped

| | Estimate | Standard Error | |
|---|---|---|---|
| | | Main | Bootstrap |
| | (1) | (2) | (3) |
| **A. Matriculation** | | | |
| GPA | 0.26 | 0.29 | 0.44 |
| **B. Degree Enrollment** | | | |
| % BA | 3.42 | 1.34 | 1.45 |
| % MA | 10.10 | 2.46 | 3.16 |
| % PhD | 3.90 | 1.20 | 1.37 |
| **C. Field of Study** | | | |
| % CS/Math | 4.45 | 1.94 | 2.82 |
| % Engineering | -6.43 | 2.12 | 2.72 |
| % Double Major | 10.21 | 2.14 | 2.78 |
| **D. Labor-market** | | | |
| Earnings Rank | 0.62 | 1.59 | 2.75 |
| % Knowledge | 1.41 | 2.51 | 2.71 |
| **E. Personal** | | | |
| % Married | 1.62 | 2.54 | 2.01 |
| Spouse UPET Score | 31.86 | 9.38 | 10.62 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* This table presents a validation test for the standard error calculation. Column (1) shows the estimated effect and Columns (2) and (3) compare the standard errors with the cluster-bootstrapped standard errors at the school level. The sample used for calculating the standard errors is our main sample, which includes about half of the students in cohorts of high-school graduates in 2006-2010 (those who participated in 8th-grade Meitzav tests).

Appendix Table A3: Locality-Level Analysis, Varying Giftedness Thresholds

| Threshold | 0.05 | 0.075 | 0.1 | 0.125 | 0.15 |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **A. High School** | | | | | |
| GCP | 21.11*** | 27.51*** | 31.25*** | 37.72*** | 42.73*** |
| | (4.55) | (5.26) | (5.95) | (7.03) | (7.35) |
| Matriculation GPA | 0.28 | 0.36 | 0.23 | 0.28 | 0.23 |
| | (0.37) | (0.49) | (0.52) | (0.55) | (0.56) |
| **B. Degree Enrollment** | | | | | |
| % BA | 2.51** | 1.93* | 1.35 | 1.21 | 0.59 |
| | (1.25) | (1.09) | (1.17) | (1.39) | (1.71) |
| % MA | 2.48 | 4.22** | 6.18** | 5.98** | 6.06** |
| | (1.73) | (2.07) | (2.56) | (2.78) | (2.94) |
| % PhD | 1.58* | 0.81 | 2.19* | 2.59* | 2.19* |
| | (0.82) | (1.00) | (1.31) | (1.44) | (1.29) |
| **C. Fields of Study** | | | | | |
| % CS/Math | 3.84*** | 4.74*** | 5.01** | 5.15* | 5.67* |
| | (1.25) | (1.74) | (2.39) | (3.07) | (3.35) |
| % Engineering | -3.72*** | -3.51** | -3.02* | -2.01 | -3.28 |
| | (1.36) | (1.51) | (1.80) | (1.81) | (2.45) |
| % Double Major | 1.28 | 2.79* | 5.78*** | 5.41*** | 6.80*** |
| | (1.44) | (1.53) | (1.63) | (1.96) | (2.44) |
| **D. Labor Market** | | | | | |
| Earnings Rank | 1.16 | 1.03 | -0.43 | -0.01 | -0.22 |
| | (1.12) | (1.11) | (1.48) | (1.68) | (2.05) |
| % Knowledge | 1.54 | 1.91 | 0.99 | 1.87 | 1.12 |
| | (1.37) | (1.87) | (2.39) | (2.85) | (3.52) |
| **E. Spouse** | | | | | |
| % Married | -0.31 | -0.55 | 0.24 | 0.41 | -0.63 |
| | (1.56) | (2.15) | (2.58) | (3.55) | (3.95) |
| UPET Score | 6.65 | 6.40 | 10.43* | 16.33** | 26.24*** |
| | (4.56) | (4.97) | (6.04) | (7.93) | (7.82) |
| Students | | | 131,065 | | |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table displays the estimated effects of high-school GCP availability at the locality level on gifted students' outcomes, which can also be interpreted as the intention-to-treat effects. Columns (1)–(5) exhibit the estimated effects ($\delta$ from Equation 1) and their standard errors (clustered at the locality level). The threshold for defining an indicator for gifted students ($G_i$) varies between the columns (0.05, 0.075, 0.1, 0.125, 0.15).

Appendix Table A4: Coefficients from PSM Regression

| Predictor | Estimate |
|---|---|
| Hebrew Score | 0.51*** (0.13) |
| Hebrew sq | -0.00*** (0.00) |
| Math Score | 0.58*** (0.14) |
| Math sq | -0.00*** (0.00) |
| English Score | 0.52*** (0.19) |
| English sq | -0.00*** (0.00) |
| Science Score | 0.61*** (0.15) |
| Science sq | -0.00*** (0.00) |
| Share Both Parents Born in Israel | -0.03 (0.12) |
| Share Oldest Siblings | 0.08 (0.12) |
| Share With Number of Siblings Above Median | -0.36** (0.14) |
| Share With Total Matriculation Credits Above Median | 0.57*** (0.14) |
| Share With 5 CS Credits | 1.27*** (0.14) |
| Share With 5 Physics Credits | 0.07 (0.14) |
| Share With 5 Chemistry Credits | 0.77*** (0.14) |
| Share With 5 Literature Credits | 1.62*** (0.20) |
| Share With 5 Geography Credits | -1.67* (0.87) |
| Share With 5 Electronic Systems Credits | 1.20*** (0.32) |
| Share With 5 Systems Credits | -0.10 (0.30) |
| Share With 5 Arabic Credits | 1.02*** (0.19) |
| Share With 5 Math Credits | 0.64** (0.25) |
| Share With 5 Eng Credits | -0.04 (0.46) |
| Share With 4 Math Credits | -0.20 (0.25) |
| Share With 4 Eng Credits | -0.22 (0.47) |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* This table reports the coefficients from the PSM regression. The estimates are presented with standard errors in parentheses. The sample includes GCP participants and students from other localities with information on at least three 8th grade scores. The sample size is 83,615. Coefficients for fixed effects are not shown.

Appendix Table A5: Robustness of the Matching Results, Alternative Academic Proxies

| | (1) | (2) | (3) |
|---|---|---|---|
| **A. Matriculation** | | | |
| GPA | 0.26 | 0.70*** | 0.66** |
| | (0.29) | (0.27) | (0.27) |
| **B. Degree Enrollment** | | | |
| % BA | 3.42** | 2.72** | 2.28 |
| | (1.34) | (1.19) | (1.57) |
| % MA | 10.10*** | 10.77*** | 8.26*** |
| | (2.46) | (2.33) | (1.93) |
| % PhD | 3.90*** | 3.44*** | 0.59 |
| | (1.20) | (1.21) | (0.75) |
| **C. Field of Study** | | | |
| % CS/Math | 4.45** | 9.21*** | 5.54*** |
| | (1.94) | (1.84) | (1.83) |
| % Engineering | -6.43*** | -8.42*** | -3.40** |
| | (2.12) | (2.07) | (1.69) |
| % Double Major | 10.21*** | 6.15*** | 3.89** |
| | (2.14) | (2.09) | (1.79) |
| **D. Labor-market** | | | |
| Earnings Rank | 0.62 | 1.43 | 0.81 |
| | (1.59) | (1.47) | (1.34) |
| % Knowledge-Economy | 1.41 | 4.53* | 1.71 |
| | (2.51) | (2.42) | (2.36) |
| **E. Spouse** | | | |
| % Married | 1.62 | 1.18 | -0.78 |
| | (2.54) | (2.39) | (1.79) |
| UPET Score | 31.86*** | 27.36*** | -17.27 |
| | (9.38) | (8.42) | (22.82) |
| Cohorts | 06-10 | 06-10 | 09-13 |
| Ability Measure | 8th-grade | 8th-grade | 5th-grade |
| Matriculation Program | + | | + |
| Students | 1,396 | 1,556 | 1,516 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on outcomes, using different sets of variables in the matching specification as proxies for academic ability and motivation. Column (1) shows our main sample and specification. Column (2) shows the results excluding the indicators for elective matriculation subjects (including only the mandatory math and English subjects). Column (3) shows the results using 5th-grade Meitzav test scores as the ability measure. The sample includes only students who participated in the relevant tests from the cohorts mentioned in the table. Each column shows the conditional difference ($\tau$ from equation (4)) in the outcome mentioned on the left and its standard error (clustered at the school level). GCP stands for Gifted Children's Program, and UPET stands for University Psychometric Entrance Test.

Appendix Table A6: UPET Scores by the Age of Taking the Test

| | Total | | Quantitative | |
|---|---|---|---|---|
| Coefficient | (1) | (2) | (3) | (4) |
| Intercept | 547.79*** | 660.83*** | 113.44*** | 131.79*** |
| | (0.83) | (2.87) | (0.16) | (0.54) |
| I(Age $<$ 17) | -1.91 | 29.97 | 1.42 | 8.01 |
| | (11.01) | (33.00) | (2.06) | (6.21) |
| I(Age = 18) | 5.47*** | 8.25 | 0.20 | 0.93 |
| | (1.44) | (5.46) | (0.27) | (1.03) |
| I(19 $\leq$ Age $\leq$ 21) | 1.65 | 11.26** | -3.91*** | -1.36 |
| | (1.04) | (5.36) | (0.20) | (1.01) |
| I(Age $>$ 21) | 23.00*** | 26.18*** | -0.62*** | 0.81 |
| | (0.99) | (5.43) | (0.19) | (1.02) |
| Sample | All | GCP | All | GCP |
| Students | 82,292 | 1,431 | 82,292 | 1,431 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* This table presents the results of the regressions to predict students' UPET scores. The outcome variable in columns (1) and (2) is the total UPET score, and in columns (3) and (4) is the quantitative UPET score of each individual. The explanatory variables are indicator variables for ages at the test (presented on the leftmost column). The baseline sample includes students who participated in the UPET from cohorts of high-school graduates in 2006-2010. The sample in columns (2) and (4) is restricted to GCP participants. Standard errors are shown in parentheses.

Appendix Table A7: The Impact of GCP on Matriculation Test Scores

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| GPA | 88.19 | 88.46 | 0.26 |
| | | | (0.29) |
| GPA, Mandatory Only | 87.06 | 87.29 | 0.23 |
| | | | (0.30) |
| Math | 86.85 | 84.33 | -2.51*** |
| | | | (0.77) |
| Hebrew | 85.80 | 85.80 | -0.00 |
| | | | (0.44) |
| English | 90.08 | 91.05 | 0.97** |
| | | | (0.40) |
| Bible | 85.82 | 85.21 | -0.61 |
| | | | (0.58) |
| History | 83.26 | 84.47 | 1.21* |
| | | | (0.63) |
| Literature | 78.99 | 80.21 | 1.22 |
| | | | (0.84) |
| Citizenship | 83.57 | 82.99 | -0.57 |
| | | | (0.60) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on matriculation test scores. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Test scores are measured on a 0-100 scale. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level).

Appendix Table A8: The Impact of GCP on Labor-market Outcomes in 2018–2020

| | Mean | | Estimated Effect |
|---|---|---|---|
| | Comparison | GCP | |
| | (1) | (2) | (3) |
| **A. Annual Earnings** | | | |
| Earnings (100K NIS) | 1.23 | 1.22 | -0.00 |
| | | | (0.07) |
| Log Earnings | 11.39 | 11.40 | 0.02 |
| | | | (0.07) |
| Earnings Rank | 53.71 | 54.70 | 1.00 |
| | | | (1.63) |
| % Top 10% Earners | 18.05 | 18.79 | 0.74 |
| | | | (2.02) |
| **B. Employment** | | | |
| % Salaried Employment | 86.96 | 86.27 | -0.69 |
| | | | (1.86) |
| % Self Employment | 8.74 | 9.07 | 0.33 |
| | | | (1.50) |
| **C. Employment in Knowledge Producing Sectors** | | | |
| % Knowledge Economy | 48.57 | 51.92 | 3.35 |
| | | | (2.53) |
| % Tech Services | 34.67 | 37.93 | 3.26 |
| | | | (2.47) |
| % Tech Manufacturing | 7.45 | 4.08 | -3.37*** |
| | | | (1.19) |
| % Academic | 11.03 | 12.65 | 1.62 |
| | | | (1.71) |
| Students | 698 | 698 | 1,396 |
| Schools | 158 | 11 | 169 |

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

*Notes:* The table shows estimates for the impact of GCP participation on labor-market outcomes during 2018–2020. The sample includes only students who participated in 8th-grade Meitzav tests, about half of the students in cohorts of high-school graduates in 2006-2010. Columns (1) and (2) show the means among the matched comparison group and the treatment group (GCP participants). Column (3) shows the conditional difference ($\tau$ from equation (4)) in the outcome and its standard error (clustered at the school level).