

From Conflict to Human Capital?

Evidence from Colombia's Peace Process

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Draft: August 2026

Abstract

Can the end of armed conflict reshape the educational pipeline from secondary-school exit to the final stage of university? I combine a municipality-year conflict panel with administrative records for 5.35 million regular, second-semester Saber 11 examinees from 2009 to 2018 and their university-level Saber Pro records through 2024. The analysis covers conflict, Saber 11 participation and achievement, university near-completion, progression time, score value added, financing, migration, and field of study. Historical FARC municipalities experience a large relative decline in conflict after negotiations begin: the pooled conflict-index estimate is -0.42 standard-deviation units relative to historical ELN municipalities, with stable pre-trends. The educational evidence is much weaker. Neither the FARC versus ELN design nor the PDET comparison provides credible evidence of higher university near-completion. The PDET comparison shows more Saber 11 test taking after the agreement but lower mean mathematics and verbal scores, while the FARC versus ELN secondary-school estimates remain near zero. Conditional progression, value added, financing, and program-choice results do not reveal a consistent mechanism. The paper contributes a student-level account of how a verified security improvement propagated, or failed to propagate, through several stages of Colombia's education system. The results show that reduced violence can be consequential without being sufficient to improve later human-capital outcomes when educational capacity remains constrained.

Keywords: peace agreements, armed conflict, education, higher education, Colombia, human capital

JEL codes: D74, I21, I23, O15

1 Introduction

The end of a civil conflict can transform educational investment. Lower violence may reduce the risk of traveling to school, weaken incentives for recruitment into armed groups, and lengthen the horizon over which families value schooling. Peace can also open local labor markets, attract institutions, and make higher education newly feasible. None of these effects, however, follows mechanically from disarmament. A territory emerging from conflict may still lack schools, universities, roads, credit, and effective public institutions. The same reduction in violence can expand participation among students who were previously excluded while lowering average test performance through composition. A peace process may therefore increase educational opportunity without immediately increasing measured learning.

Colombia offers an unusually informative setting in which to study this distinction. Formal negotiations between the national government and the Revolutionary Armed Forces of Colombia, the FARC, began in 2012. The FARC declared an indefinite unilateral ceasefire in December 2014, and the parties signed a final agreement in November 2016. These events separated political negotiation from a credible reduction in hostilities and from the later implementation of territorial policy. They also generated sharp spatial variation. Some municipalities had long histories of FARC presence, while others were exposed to the National Liberation Army, the ELN, which did not participate in the FARC agreement.¹ In 2017, the government additionally designated 170 conflict-affected, poor, rural municipalities for the Programas de Desarrollo con Enfoque Territorial, or PDET, (Colombia, 2017; United Nations, 2017).

This paper asks whether the process of peace changed the path from the end of secondary school to the final stage of university. I build a linked administrative data set that begins with students taking Saber 11, Colombia's secondary school exit examination, during the regular second semester session. I then search for those students in Saber Pro, the national examination taken by university students who have completed at least 75 percent of their degree credits. Because taking Saber Pro is a degree requirement, appearance in the examination is a strong indicator that a student has progressed close to university completion, but it is not a formal graduation record (ICFES, 2026). The outcome equals one only for a university-level Saber Pro appearance within six years. Students who never appear, who appear only in a technical or technological program, or who reach university-level Saber Pro outside the window remain in the denominator and receive zero.

The empirical analysis combines two designs that answer related but distinct questions. The first compares students from PDET municipalities with students from non-PDET municipalities. This design has statistical power and captures the places at the center of the post-conflict policy agenda, but treatment status was assigned after the peace agreement using poverty, rurality, institutional weakness, illicit crops, and conflict exposure. PDET and non-PDET municipalities are consequently quite different in levels. The second design compares municipalities with historical FARC presence with those with historical ELN presence. This comparison follows the logic of Fajardo-Steinhäuser (2024): both groups experienced insurgent conflict, but only FARC territories were directly exposed to the 2014 ceasefire and the subsequent agreement. The FARC and ELN samples are much more similar in pre-period educational achievement, although the ELN control group is small.

Three findings organize the paper. First, the peace process generated a real security dividend in its intended territories. Historical FARC municipalities experience a pooled decline of 0.42 standard-deviation units in a core conflict index relative to historical ELN municipalities after 2012. The corresponding log event-rate estimate is -0.66 . Pre-treatment coefficients are jointly insignificant, and the

¹The Spanish names are *Fuerzas Armadas Revolucionarias de Colombia* and *Ejército de Liberación Nacional*, which generate the acronyms FARC and ELN.

event path turns persistently negative after negotiations begin. This first stage matters because it establishes that the educational null is not produced by a peace process that failed to reduce the forms of violence measured in the data.

Second, the FARC versus ELN educational comparison shows no credible university-completion gain despite that conflict decline. The pooled post-2012 difference in university-level Saber Pro within six years is -0.43 percentage points with a standard error of 0.42 . The years-four-through-six estimate is -0.41 points. Both confidence intervals include zero, and both outcomes have differential pre-trends because the 2009 FARC-ELN gap differs from the 2011 gap. FARC and ELN municipalities also show no differential improvement in Saber 11 mathematics, verbal performance, or residual value added between Saber 11 and Saber Pro. The contrast between a large, well-behaved conflict reduction and fragile education estimates is the paper’s central result: security improved, but human-capital accumulation did not automatically follow.

Third, the PDET comparison does not establish an access effect. The six-year coefficient is -0.49 points in 2012 and 0.88 points in 2018, with both confidence intervals including zero. The pooled post-2012 estimate is 0.45 points, and the years-four-through-six estimate is 0.52 points, but both specifications reject parallel pre-trends. Estimates at four and five years, conditional progression time, credit use, and the semester-based delay proxy provide no coherent evidence that negotiation uncertainty altered university trajectories.

The paper makes three contributions to research on conflict, post-conflict recovery, and higher education. First, while prior Colombian work studies violence, primary or secondary enrollment, secondary achievement, or aggregate economic activity, this paper follows individual recent school leavers from a national secondary exit examination to the final portion of a professional university program. It therefore measures a long educational transition that the existing peace literature does not observe. Second, it places a direct municipal conflict first stage and several downstream educational outcomes in the same design. This comparison reveals a consequential asymmetry: the peace process changed security much more clearly than it changed human-capital production. Third, it accounts explicitly for a change in the higher-education testing system. ICFES established a separate Saber TyT reporting baseline and examination calendar for technical and technological students in 2016, while Saber Pro became the examination for professional university programs (ICFES, 2016).² This university-only definition preserves the same educational outcome across cohorts and keeps nonmatches and non-university records in the denominator.

The central lesson is deliberately narrower than the claim that peace had no educational impact. The evidence shows that peace substantially reduced conflict, yet it does not reveal a robust average improvement in the observed path from secondary exit to university near-completion. Security is an input into human-capital formation, not a substitute for educational capacity, and this paper identifies the empirical gap between those two margins.

2 Related Literature and Conceptual Framework

The first relevant literature studies how conflict destroys human capital. Violence can close schools, displace families and teachers, increase child labor, expose students to trauma, and reduce the expected re-

²In the pre-2016 microdata used here, technical and technological examinees appear in files historically distributed with Saber Pro. From 2016 onward, ICFES reports these students separately through Saber TyT. Counting every appearance in the older Saber Pro files would therefore change the meaning of the outcome across cohorts. I instead count only records identified as university-level in every year and code all other Saber 11 students as zero.

turn to geographically specific investments. Cross-country evidence associates civil war with lower educational expenditure and enrollment, while microeconomic studies document persistent schooling losses for cohorts exposed to conflict in Tajikistan, Guatemala, and Peru (Shemyakina, 2011; Chamarbagwala and Morán, 2011; León, 2012). In Colombia, previous studies associate conflict exposure with lower primary or secondary enrollment, more child labor, and weaker performance on the Saber 11 secondary-school exit examination (Rodríguez and Sánchez, 2012; Gómez Soler, 2016, 2017). Those studies provide background rather than outcomes estimated in this paper: I observe participation in Saber 11, not enrollment in secondary school. Justino (2016) emphasizes that conflict can restrict both families' demand for education and the state's ability to supply it.

The post-conflict question is conceptually different. Removing violence relaxes one constraint, but damaged infrastructure, limited employment and credit opportunities, low state capacity, and weak educational supply may remain. A peace dividend should therefore be separated into a security first stage and subsequent behavioral responses. Colombian evidence already suggests such complementarity. The FARC ceasefire reduced violence and was followed by changes in primary and secondary education outcomes and entrepreneurship, but broad economic activity remained weak where effective state institutions did not expand (Namen et al., 2021; Bernal et al., 2024; Fajardo-Steinhäuser, 2024). The new conflict panel allows this paper to document the security change directly and then ask whether the same spatial contrast changed later educational outcomes.

Uncertainty provides one possible bridge between violence and schooling decisions. Education is costly, partially irreversible, and pays off with delay. When households face uninsured income or security risk, they may postpone enrollment or favor liquid resources over long-horizon human-capital investments (Kazianga, 2012). College decisions are also sequential: enrollment has option value because students learn about aptitude and costs before deciding whether to continue (Stange, 2012). Completion risk can consequently dampen enrollment even when the college premium is high (Athreya and Eberly, 2021). In Colombia, credit and aid can sharply increase enrollment, especially for low-income students, while institutional supply responds to demand (Melguizo et al., 2016; Londoño-Vélez et al., 2020; Caranza and Ferreyra, 2019). These mechanisms motivate the paper's timing, financing, and interruption proxies.

The predictions are deliberately separated. If negotiation uncertainty temporarily delayed investment, early post-2012 cohorts should be less likely to reach Saber Pro at short horizons, with recovery after the ceasefire. If liquidity constraints were central, credit or scholarship financing could change among later university examinees. If interrupted or delayed trajectories were important, elapsed time should rise relative to the semester reported at Saber Pro. None of the latter variables is a direct enrollment history, so the paper treats them as mechanism diagnostics rather than additional causal outcomes.

3 The Colombian Peace Process and Education

3.1 From negotiation to ceasefire and implementation

The FARC emerged in the 1960s and became Colombia's largest guerrilla organization. Its territorial presence was concentrated in rural areas where state authority, road access, legal markets, and public services were often weak. Armed conflict affected education through direct violence, displacement, recruitment, teacher turnover, school closure, and the opportunity cost created by illicit and armed activity. Earlier Colombian evidence associates conflict exposure with lower enrollment, more child labor, and weaker school achievement (Rodríguez and Sánchez, 2012; Gómez Soler, 2016, 2017).

The peace process unfolded in stages. Public negotiations began in 2012, but negotiation itself did not end fighting. The more consequential break in security occurred on December 20, 2014, when the FARC initiated an indefinite unilateral ceasefire. Violence attributable to the FARC subsequently fell sharply. A bilateral ceasefire followed in 2016, and the revised Final Agreement was signed on November 24 and ratified by Congress later that month. The agreement entered into force on December 1, 2016 ([United Nations, 2016, 2017](#)). This sequence matters for educational choices. A family may react little to talks that can fail, but substantially to an observed and persistent reduction in violence.

The agreement was ambitious. It combined disarmament and political incorporation with rural reform, crop substitution, victim reparations, and territorial development. Yet implementation began in municipalities with limited fiscal resources and weak public-service networks, and other armed groups could enter territories vacated by the FARC. The end of one armed relationship therefore did not instantly produce a complete or uniform peace.

3.2 PDET territories and the educational margin

Decree Law 893 of 2017 created the PDET as the territorial vehicle for implementing rural reform in the areas most affected by conflict. It selected 170 municipalities organized into 16 subregions. Selection reflected armed conflict, poverty, institutional weakness, rural conditions, and illicit economies ([Colombia, 2017](#)). These municipalities are central to post-conflict policy because they concentrate the population for whom a peace dividend was most needed. They are not, however, a treatment group created by random or quasi-random assignment.

The distinction between security and policy is central to interpretation. A PDET event study around 2012 or 2014 captures the evolving relative trajectory of territories later selected for post-conflict investment. It may reflect the ceasefire, expectations, local policy, and differential recovery. It should not be described as the causal effect of PDET designation before 2017. By contrast, the FARC versus ELN design more directly isolates exposure to the FARC ceasefire, conditional on the assumption that historical ELN municipalities provide a valid counterfactual.

Higher education is a useful outcome in this setting because it requires a sequence of investments. Students must complete secondary school, take Saber 11, enroll in a professional program, persist through most required credits, and take Saber Pro. A change in near-completion within six years therefore captures more than a short-lived enrollment response. At the same time, the measure can change because of university supply, student demand, migration, financing, or selection. I examine Saber 11 participation and scores alongside university progression to distinguish a change in who reaches the end of secondary school from a change in who later approaches university completion.

4 Data and Measurement

4.1 Linked examination records

The source data are administrative examination records from the Colombian Institute for the Evaluation of Education, ICFES. Saber 11 is the national examination taken near the end of secondary school. The analysis retains regular students who present the test in the second semester of each year. This restriction is intentional: the paper is designed to follow recent high-school graduates, and the second-semester session is the standard examination period for students finishing grade 11 in Colombia's traditional academic calendar. Restricting attention to this session makes the starting cohort more homogeneous

and makes elapsed time to higher education interpretable as a post-secondary trajectory rather than a mixture of recent graduates, adult examinees, and off-cycle test takers.

The main regression sample contains 5,354,444 recent school leavers with valid municipality information from the 2009 to 2018 Saber 11 cohorts. Student identifiers are used to link these records with Saber Pro through 2024. Among them, 886,604 reach university-level Saber Pro within six years. The analysis begins in 2009 because the Saber Pro archive starts in 2012. For the 2009 cohort, only appearances one or two years after Saber 11 are unavailable, and such rapid university progression is empirically rare. Starting in 2008 would additionally miss year three, which is more plausible because Saber Pro eligibility begins after completion of 75 percent of degree credits.

Saber Pro is designed for students near the end of professional university programs. ICFES defines eligible examinees as those who have completed at least 75 percent of their program credits, and the examination is a graduation requirement (ICFES, 2026). I therefore refer to the main outcome as “university-level Saber Pro within six years” or “university near-completion within six years.” Calling it graduation would overstate what the administrative record directly observes. The indicator equals one only when the linked record is classified as university-level and occurs no later than six years after Saber 11. It equals zero for students who never appear in Saber Pro, appear only in a technical or technological program, or do not reach university-level Saber Pro during the window. This coding keeps the entire eligible Saber 11 cohort in the denominator.

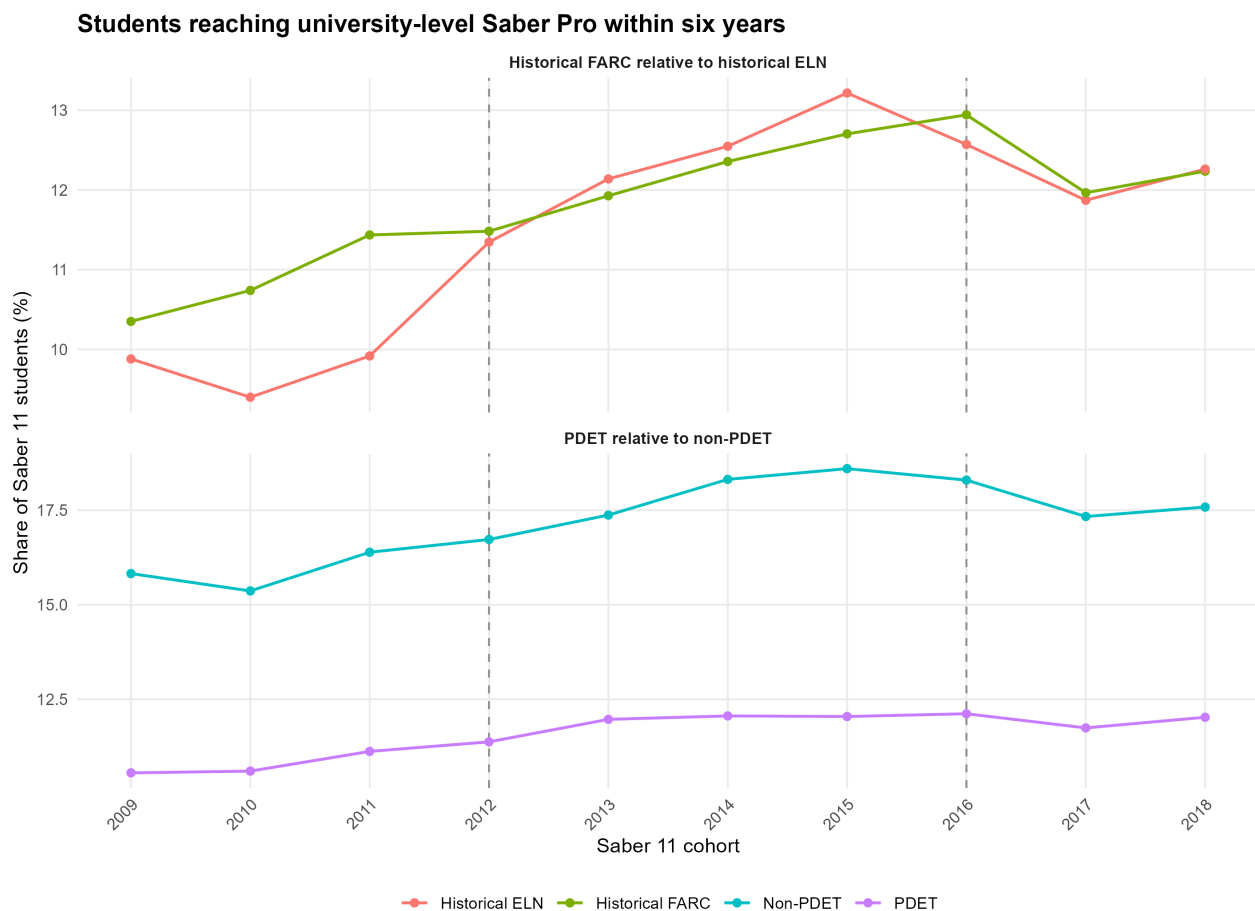
The six-year horizon is preferred for interpretive and measurement reasons. Colombian professional degrees normally require four or more years, but Saber Pro eligibility at 75 percent of credits means some students can appear before formal degree completion. A six-year window allows standard program duration plus moderate delay while maintaining exactly the same maximum follow-up for every cohort through 2018. A shorter four-year window would disproportionately classify ordinary five-year degrees and modest delays as failures. An unrestricted window would mechanically favor older cohorts. As a robustness check, I require the university-level Saber Pro record to occur between years four and six. Only 7,327 observations, 0.14 percent of the full descriptive sample, distinguish the two definitions, so the primary result is not driven by implausibly rapid completion.

4.2 Treatment definitions and baseline comparability

The first treatment indicator equals one if the student’s municipality of residence at Saber 11 is among the 170 PDET municipalities. The comparison group contains the remaining observed municipalities. For the second design, I reconstruct the historical-presence classification in Fajardo-Steinhäuser (2024). A group is active in a municipality-year when the conflict panel records at least one event attributed to that group. Historical presence requires activity in at least eight of the 13 years from 1996 through 2008. The FARC sample contains municipalities that meet this threshold for the FARC but not the ELN; the ELN sample applies the reverse rule. I exclude the largest 2 percent of municipalities by average 1996 to 2008 population, where conflict patterns and educational markets are less comparable. The resulting mutually exclusive comparison contains 216 FARC municipalities and 41 ELN municipalities. Appendix Section C.1 provides the event categories and complete construction.

Table 1 reports pre-period characteristics for 2009 to 2011. PDET municipalities are far poorer and have much lower initial achievement than non-PDET municipalities. Ninety-two percent of PDET students are in household strata 1 or 2, compared with 74 percent outside the PDET.³ Baseline mathematics

³Residential strata run from 1, the lowest, to 6, the highest, and determine utility subsidies. The examination record reports household stratum.



All cohorts have equal follow-up through 2024. All other Saber 11 students are coded zero.

Figure 1: University-level Saber Pro within six years by Saber 11 cohort

Notes: Each point is the share of regular, second semester Saber 11 examinees reaching university-level Saber Pro within six years. Students never observed, observed only in technical or technological programs, or observed outside the six-year window are coded zero. The panels compare PDET with non-PDET municipalities and historical FARC with historical ELN municipalities.

and verbal achievement are approximately 0.3 standard deviations lower. University near-completion within six years is 5.1 percentage points lower. These differences do not invalidate a fixed effects event study if the groups would otherwise follow parallel trends, but they make extrapolation demanding and rule out a simple cross-sectional interpretation.

The FARC and ELN groups are much closer on educational achievement. Their baseline mathematics difference is 0.03 standard deviations and the verbal difference is effectively zero. University near-completion differs by 1.2 percentage points, and conditional time to Saber Pro is almost identical. Low socioeconomic status remains more common in FARC municipalities, with a standardized difference of 0.19. Overall, the conflict comparison sacrifices power for greater baseline overlap in levels, although the event studies below reveal an important 2009 differential relative to the 2011 baseline.

Table 1: Baseline characteristics, 2009 to 2011

Design	Characteristic	Treated	Comparison	Std. difference
PDET	Female student	56.0%	54.8%	0.02
PDET	Household stratum 1 or 2	91.6%	73.7%	0.49
PDET	Saber 11 mathematics, cohort SD	-0.27	0.03	-0.32
PDET	Saber 11 verbal achievement, cohort SD	-0.26	0.03	-0.30
PDET	University-level Saber Pro within six years	10.8%	15.9%	-0.15
PDET	Years to university-level Saber Pro	5.29	5.21	0.11
FARC-ELN	Female student	55.2%	56.5%	-0.03
FARC-ELN	Household stratum 1 or 2	91.8%	86.0%	0.19
FARC-ELN	Saber 11 mathematics, cohort SD	-0.17	-0.14	-0.03
FARC-ELN	Saber 11 verbal achievement, cohort SD	-0.16	-0.16	0.00
FARC-ELN	University-level Saber Pro within six years	10.9%	9.7%	0.04
FARC-ELN	Years to university-level Saber Pro	5.26	5.30	-0.06

Notes: Treated denotes PDET or historical FARC municipalities. The comparison denotes non-PDET or historical ELN municipalities. Years are conditional on reaching university-level Saber Pro within six years. Standardized differences divide the difference in means by the pooled individual-level standard deviation.

4.3 Municipal conflict, timing, and financing measures

I organize ten administrative event files into a balanced municipality-year panel for 2007 to 2018 and merge them with population estimates from the Centro de Estudios sobre Desarrollo Económico, CEDE, at Universidad de los Andes (CEDE, 2022). The core conflict measure uses five event families with complete coverage: subversive actions, attacks on bridges and roads, kidnappings, terrorism, and oil-pipeline bombings. For each family, I convert the annual count to events per 100,000 residents, apply $\log(1 + x)$, standardize it over the municipality-year panel, and give it equal weight in the average index. Thus a one-unit change in the index is interpretable in standard-deviation units of its underlying components, rather than as a count of events. Homicide and extortion are analyzed separately because they capture broader public safety and can reflect criminal activity beyond the FARC conflict. Attacks on the security forces begin only in 2010 and are retained as a supplementary outcome. Cocaine records describe seizures or enforcement activity, not coca cultivation, and are not used as a main conflict outcome. Appendix Section C.1 gives formulas, source coverage, and the historical armed-group classification.

The conflict panel contains 1,122 municipalities and 13,461 municipality-year observations. It includes all 170 PDET municipalities observed in the population data and reproduces the 216 histori-

cal FARC and 41 historical ELN municipalities used in the student analysis. Missing municipality-year events are coded as zero only within the observed coverage period of each administrative source. Conflict outcomes are analyzed at the municipality-year level with municipality and year fixed effects, population-normalized dependent variables, and municipality-clustered standard errors.

The timing and financing analysis starts from the same linked student sample and separates observed variables from proxies. I directly observe elapsed years from Saber 11 to the first linked university-level Saber Pro record, the semester reported at Saber Pro, whether tuition is financed with credit or a scholarship, and whether the examinee worked during the previous week. I do not observe applications, the date of initial higher-education enrollment, or semester-by-semester enrollment histories. Consequently, reaching university-level Saber Pro within four or five years measures rapid progression to near-completion, not first-year enrollment. I also construct excess elapsed time as years since Saber 11 minus one-half of the semester reported at Saber Pro. The intuition is simple: a student observed six years after Saber 11 who reports being in semester 8 has spent four reported academic years in the program and therefore has two years of excess calendar time. That difference may reflect delayed entry, interruptions, transfers, part-time study, program changes, irregular semester reporting, or measurement error. A value of at least one year defines a deliberately labeled delay or interruption proxy; it is not a direct measure of any one of those mechanisms.

Financing, employment, semester, and excess-time outcomes are available only among students observed in university-level Saber Pro within six years. They are therefore conditional composition outcomes. The models include Saber Pro examination-period fixed effects when variable definitions or reporting can vary across exam administrations. Desired-institution questions in Saber 11 are not used as application measures because they disappear after the 2014 redesign and record preferences rather than submitted applications.

4.4 Secondary achievement and value added

The secondary school analysis aggregates Saber 11 records to municipality by year. Outcomes include test takers per thousand residents, schools represented per 100,000 residents, cohort-standardized mathematics and verbal scores, and student composition. ICFES introduced a redesigned Saber 11 examination in the second semester of 2014: critical reading replaced the earlier language and philosophy structure, mathematics was reorganized, and the score scale changed (ICFES, 2014). I link language before the redesign to critical reading afterward and refer to the resulting domain as verbal performance. I standardize every score within its national examination cohort to mean zero and standard deviation one. The coefficients therefore compare changes in students' relative national standing, not raw score points across incompatible scales. The pooled post-agreement specification is restricted to the redesigned era and defines 2017 as the first fully post-agreement cohort.

For linked students who appear in Saber Pro, I also construct quantitative and reading value added. Saber Pro scores are standardized within test year and residualized on the corresponding Saber 11 score and baseline covariates. The municipality level residual measures performance near the end of university relative to what would be predicted from secondary school achievement. This is a selected-completer measure: peace can change who reaches Saber Pro, so it is not an unconditional effect on the original Saber 11 cohort.

Figure 2 is a roadmap to the evidence. Reading from left to right, the paper first asks whether more students reach the secondary exit examination and how they perform, then whether they reach the final portion of university, and finally how the trajectories of observed university examinees differ. The lower

box reports learning comparisons among those selected completers. The arrows organize outcomes over the educational life cycle; they do not claim that the paper estimates causal mediation from one box to the next.

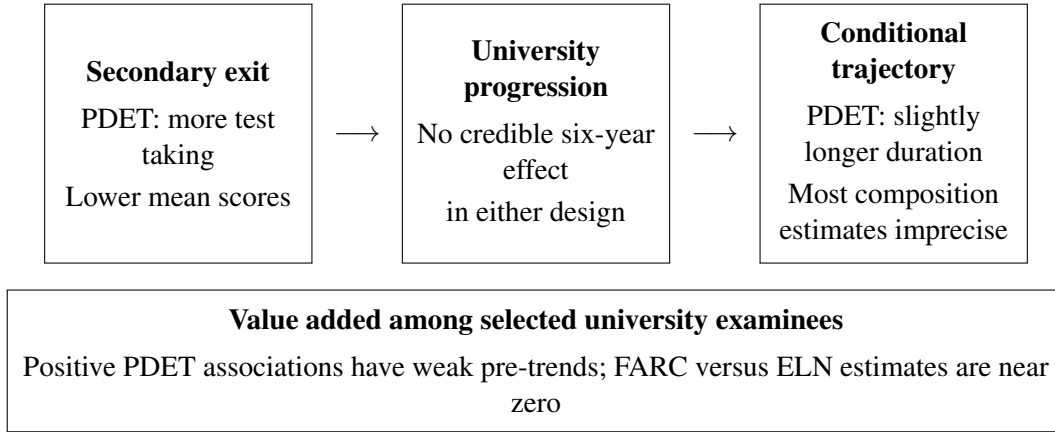


Figure 2: The educational pipeline studied in the paper

Notes: The boxes summarize group-specific findings, not a single causal chain. “PDET” compares prioritized with non-prioritized municipalities; the second design compares historical FARC with historical ELN municipalities. Exact estimates, confidence intervals, and pre-trend diagnostics are reported in Section 5 and the appendix.

5 Empirical Strategy

The main event study is estimated at the student level:

$$Y_{imt} = \alpha_m + \lambda_t + \sum_{k \neq 2011} \beta_k (\mathbb{1}\{t = k\} \times T_m) + \varepsilon_{imt}, \quad (1)$$

where Y_{imt} is an outcome for student i from municipality m in Saber 11 cohort t , α_m are municipality fixed effects, λ_t are cohort fixed effects, and T_m is either PDET status or historical FARC presence. The omitted cohort is 2011, the year before public negotiations. Standard errors are clustered by municipality. For binary outcomes, coefficients are reported in percentage points.

The event coefficients serve two purposes. Pre-2012 estimates provide a direct diagnostic for differential trends. Post-2012 estimates reveal whether adjustment occurred at negotiation, ceasefire, agreement, or implementation. A conventional pooled difference in differences model complements the event study:

$$Y_{imt} = \alpha_m + \lambda_t + \delta (T_m \times Post_t) + \varepsilon_{imt}. \quad (2)$$

I report pooled effects using 2012 and 2015 as alternative post-period starting dates. The first captures the full negotiation period. The second begins after the December 2014 ceasefire and is closer to a de-escalation effect. For the secondary school municipality panel, I estimate analogous models with municipality and year fixed effects and municipality clustered standard errors.

For the conflict first stage, the unit is municipality-year rather than student. I estimate Equation 1 after replacing the individual outcome with the conflict index or a population-normalized event rate. The main FARC versus ELN model compares 2012 to 2018 with 2007 to 2011 and omits 2011 in the

event study. I also report a post-agreement specification beginning in 2017, the first full calendar year after the final agreement. Equal municipality weights are primary, with population-weighted estimates as a robustness check. The conflict index is preferred to any single event category because it reduces outcome selection and averages several administrative measures with common coverage.

The mechanism checks motivated by uncertainty retain 2011 as the reference cohort and estimate three cumulative university timing outcomes: university-level Saber Pro within four, five, and six years. All eligible Saber 11 students remain in each denominator. I also estimate the years-four-through-six outcome that excludes unusually early appearances. These horizons ask whether the shape of progression to near-completion shifted, not when students first enrolled. Elapsed time is the calendar time between Saber 11 and the first university-level Saber Pro record; excess elapsed time subtracts the academic time implied by the semester reported at Saber Pro. Conditional models for those timing measures, credit, scholarship, and work include Saber Pro period fixed effects. The uncertainty hypothesis would receive its strongest support if early cumulative progression fell after 2012 and recovered after credible de-escalation while direct proxies moved in the predicted direction with stable pre-trends. Because applications and enrollment histories are unobserved, even that pattern would remain suggestive rather than definitive.

The identifying assumption differs across designs. In the PDET comparison, it requires that prioritized and non-prioritized municipalities would have experienced similar changes after 2011 absent the peace process and its territorial consequences. The university-level outcome does not satisfy this diagnostic: the 2009 coefficient is negative and statistically different from the 2011 baseline, and the joint pre-trend test rejects. The PDET university estimates are therefore descriptive relative trajectories, not causal effects.

In the FARC versus ELN design, the assumption is that education in historically ELN municipalities approximates the counterfactual trajectory of historically FARC municipalities after common national shocks. This design removes many differences between conflict and non-conflict Colombia and matches the treatment contrast in [Fajardo-Steinhäuser \(2024\)](#). The conflict first stage supports this comparison, but the university outcome again rejects stable pre-trends because of the 2009 coefficient. It can also fail if FARC and ELN territories faced different post-2012 shocks, if armed groups selected systematically different locations, or if spillovers affected ELN areas. With only 41 ELN municipalities, confidence intervals are wider than in the PDET analysis.

No single comparison is decisive, so I compare the two designs rather than rely on either one alone. A result is most credible when it appears in both, follows the timing of de-escalation, has stable pre-trends, and is consistent across related outcomes. When the designs disagree, I do not treat that disagreement as a common causal effect. Instead, I examine whether it reflects the distinctive selection of PDET places, the smaller ELN comparison group, or a mechanism tied to one policy contrast.

6 Results

6.1 Conflict as the first stage

Figure 3 begins with the outcome closest to the treatment itself. Historical FARC municipalities experience a pronounced decline in the standardized core conflict index relative to historical ELN municipalities after negotiations begin. The pooled post-2012 coefficient is -0.422 with a standard error of 0.094 . For the log core-event rate, the corresponding estimate is -0.656 with a standard error of 0.129 . Joint tests do not reject the 2007 to 2010 pre-treatment coefficients, with p values of 0.462 for the index

and 0.491 for the log event rate. The negative differential appears in 2012, becomes larger in 2013, and persists through 2018.

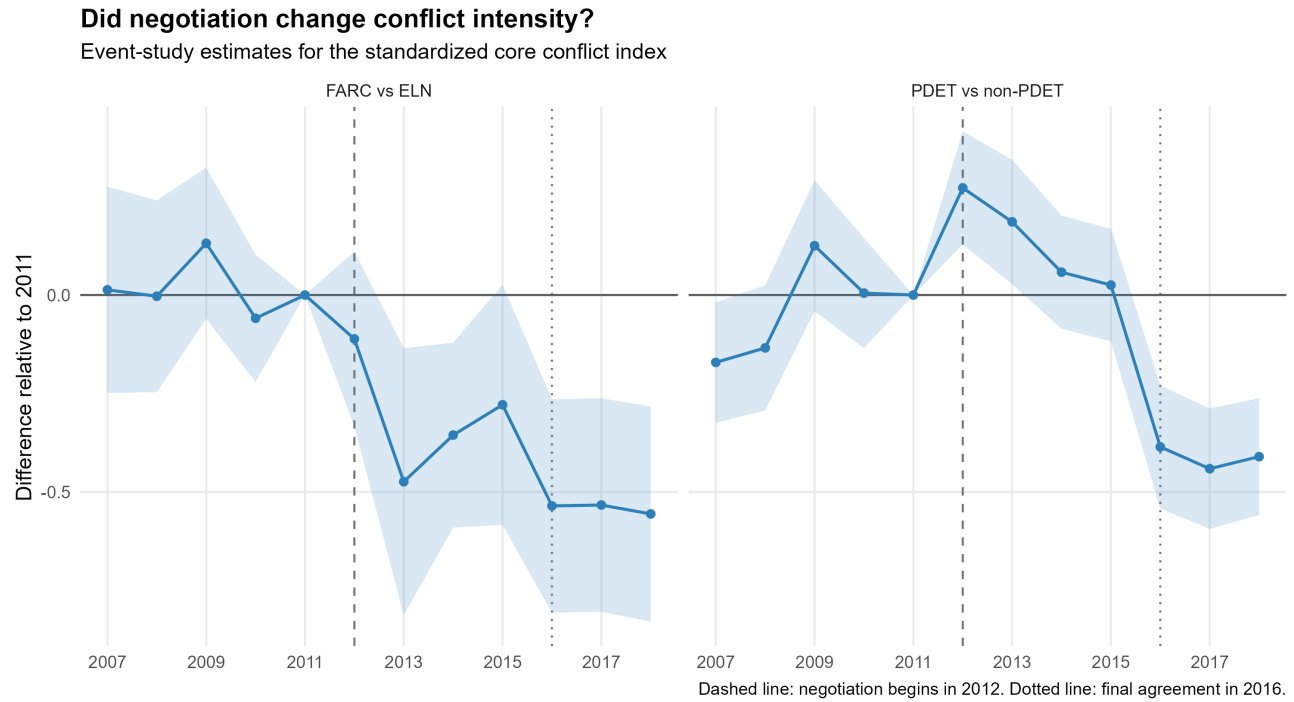


Figure 3: Conflict intensity in historical FARC and ELN municipalities and PDET territories

Notes: The outcome is the standardized core conflict index constructed from subversive actions, attacks on bridges and roads, kidnapping, terrorism, and oil-pipeline bombing. Models include municipality and year fixed effects and omit 2011. The left panel compares historical FARC with historical ELN municipalities. The right panel compares PDET with non-PDET municipalities. Bands are 95 percent confidence intervals from municipality-clustered standard errors. Dashed and dotted lines denote the beginning of negotiations and the final agreement.

The PDET pattern is different. There is little pooled change during the negotiation period, followed by a large relative decline after the agreement. The post-2017 conflict-index coefficient is -0.423 with a standard error of 0.050 , and the log event-rate estimate is -0.524 with a standard error of 0.072 . However, PDET pre-trends are jointly significant for both measures. This is useful descriptive evidence that conflict fell sharply in prioritized territories after 2016, but it is not as credible a causal contrast as FARC versus ELN.

Broader public-safety outcomes do not move uniformly with guerrilla-related violence. In PDET municipalities, reported extortion rises relative to non-PDET municipalities during both the negotiation and post-agreement periods, and homicide rises after the agreement. In FARC municipalities, homicide falls relative to ELN municipalities during negotiations, while extortion is statistically indistinguishable from zero. This divergence cautions against describing demobilization as the disappearance of all insecurity. The core conflict reduction can coexist with criminal substitution, reporting changes, or entry by other armed actors. Appendix Figure 13 reports the pooled estimates across outcomes.

The first stage changes how the education results should be read. The FARC versus ELN education null does not arise because the treatment failed to alter conflict. On the administrative outcomes observed here, conflict fell substantially and persistently in FARC territories, yet the education pipeline did not differentially improve. This pattern closely matches the state-capacity interpretation in [Fajardo-Steinhäuser \(2024\)](#): security is necessary for a peace dividend, but it is not sufficient when complemen-

tary public and market institutions remain weak.

6.2 Progression to the end of university

Figure 4 presents the PDET event study. Relative to 2011, the probability that a PDET-origin student reaches university-level Saber Pro within six years is 0.49 percentage points lower in 2012, with a 95 percent interval from -1.12 to 0.13 . The estimates remain modestly negative through 2015 and turn positive from 2016 onward. The 2018 coefficient is 0.88 points with a standard error of 0.49, so its confidence interval still includes zero. The pooled post-2012 estimate is 0.45 points with a standard error of 0.27.

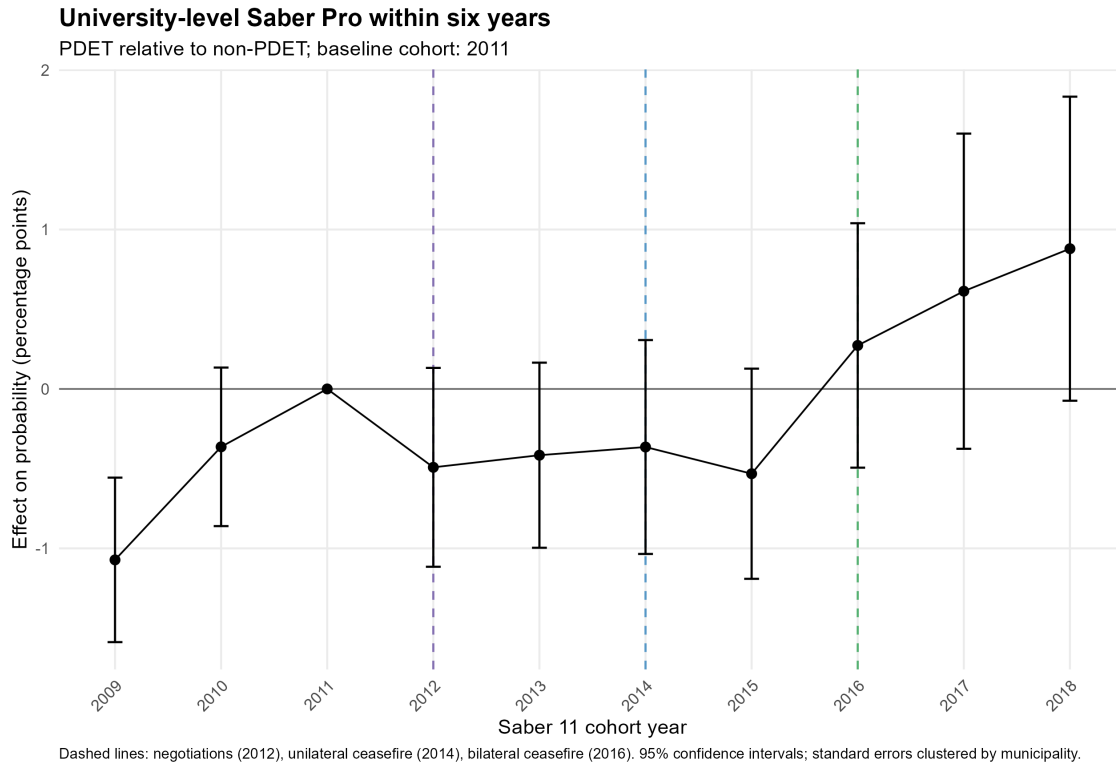


Figure 4: PDET event study: university-level Saber Pro within six years

Notes: Coefficients are interactions between cohort indicators and PDET status in a model with municipality and cohort fixed effects. The omitted cohort is 2011. Bars are 95 percent confidence intervals from municipality clustered standard errors. Estimates are percentage points.

This path does not establish a PDET access dividend. The 2009 coefficient is -1.07 points relative to 2011, and the joint pre-trend test has a p value below 0.001. The later positive point estimates could reflect differential recovery, changing composition, or post-agreement territorial policy, but they cannot be cleanly attributed to negotiations or the ceasefire. The university-level definition is applied consistently in every cohort because the testing system separates professional programs from technical and technological programs beginning in 2016. Thus the plotted series always measures the probability of reaching the final portion of a professional university program, with every nonmatch and non-university record coded zero.

Figure 5 subjects the same outcome to the FARC versus ELN design. The post-period coefficients are generally negative relative to 2011. The pooled post-2012 estimate is -0.43 percentage points with

a standard error of 0.42, and the pooled post-2015 estimate is 0.10 points with a standard error of 0.51. Neither differs statistically from zero. Here too, however, 2009 is lower than 2011 by 1.42 points, and the pre-trend test rejects. The design therefore provides no credible evidence of improved university near-completion, but it cannot identify a precise negative causal effect either.

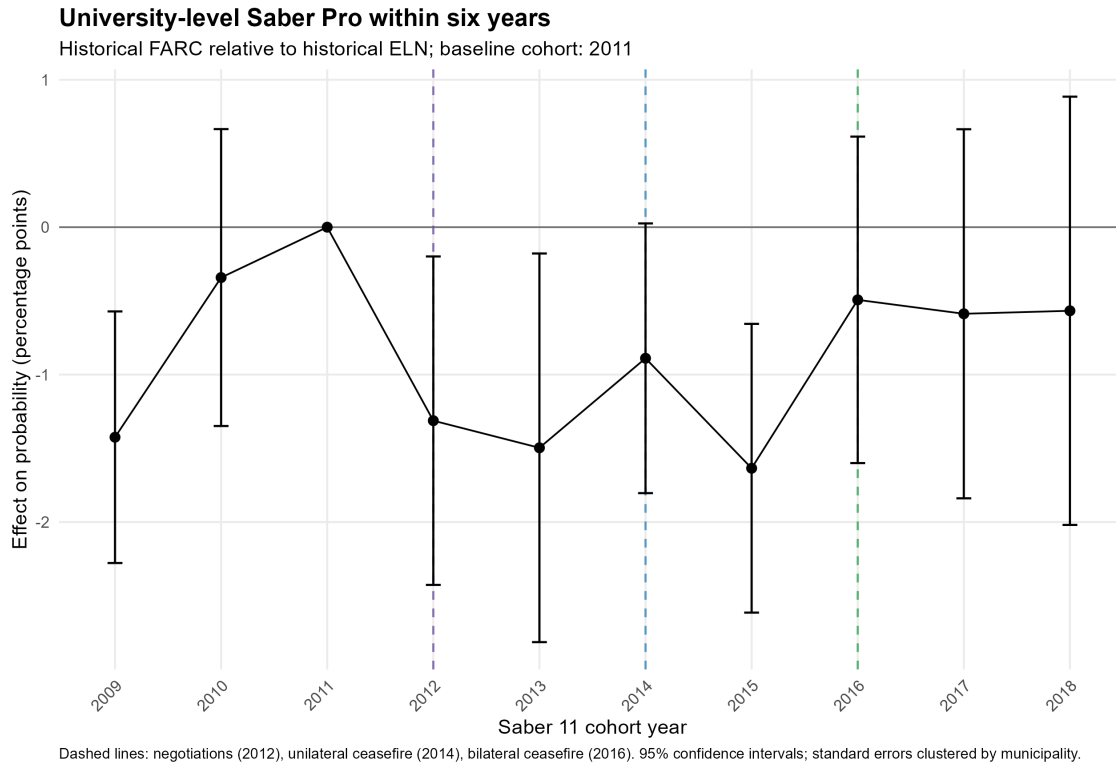


Figure 5: FARC versus ELN event study: university-level Saber Pro within six years

Notes: Coefficients compare historical FARC municipalities with historical ELN municipalities. The omitted cohort is 2011. The model includes municipality and cohort fixed effects. Bars are 95 percent confidence intervals from municipality clustered standard errors. Estimates are percentage points.

The contrast between the conflict and education estimates is the paper's most important inferential fact. The FARC versus ELN conflict index has stable pre-trends and falls sharply after negotiations. The analogous university outcome neither improves nor satisfies the pre-trend diagnostic. This does not prove a structural zero effect of peace on every educational margin. It shows that the available student-level evidence cannot support a positive university-completion effect, even in places where the security treatment clearly occurred.

6.3 Speed of progression among completers

If peace improved the efficiency of the education pipeline, students who reached Saber Pro might do so faster. The data do not support that mechanism. In the PDET design, conditional time to Saber Pro begins to rise in 2014. The estimate is 0.030 years in 2014, 0.055 in 2015, 0.067 in 2016, and 0.075 in 2017. The 2017 increase is about 0.9 months. The pre-trend test has a p value of 0.069, making causal interpretation additionally fragile.

Among students reaching university-level Saber Pro within six years, those from PDET municipalities take slightly longer than other students in later cohorts. This estimate should not be interpreted as the

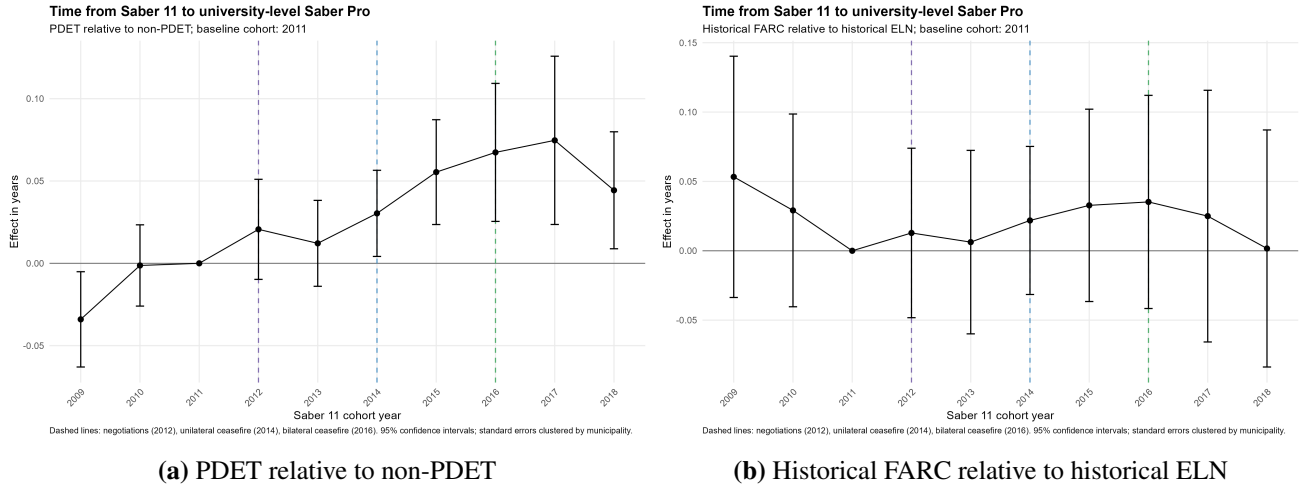


Figure 6: Time from Saber 11 to university-level Saber Pro

Notes: The sample is conditional on appearing in university-level Saber Pro within six years. Coefficients are measured in years relative to the omitted 2011 cohort. The PDET and FARC versus ELN pre-trend p values are 0.069 and 0.479, respectively.

consequence of expanded access because the full-cohort results do not establish higher near-completion. It describes a selected group whose membership can change across cohorts. The increase could reflect delayed entry, interruption, part-time study, transfers, program length, or changing composition, and the available records do not distinguish among them. More generally, conditional outcomes require caution: even without a proven extensive-margin effect, any change in selection into Saber Pro can alter the average trajectory observed among university examinees.

6.4 Timing, financing, and the uncertainty hypothesis

Figure 7 compares cumulative university progression at four, five, and six years. For PDET-origin students, the 2012 coefficients are -0.31 percentage points within four years, -0.54 within five, and -0.49 within six. By 2018 they are -0.14 , 0.72 , and 0.88 points, respectively. The four-year series therefore does not reproduce the later positive movement in the longer horizons.

The credibility of the horizons is not identical. The four-year PDET outcome has an acceptable pre-trend test, but its pooled post-2012 estimate is negative at -0.48 points. The five-year estimate is essentially zero. The six-year and years-four-through-six estimates are positive at 0.45 and 0.52 points, respectively, but both reject parallel pre-trends. In the FARC versus ELN design, the four-year estimate is near zero with acceptable pre-trends, while the longer horizons again reject. This lack of a coherent gradient provides little support for negotiation uncertainty as an explanation.

Direct proxies provide little support for a financing or interruption channel. Table 4 and Appendix Figures 14 and 15 report the pooled estimates. In the PDET models, credit financing changes by 0.35 percentage points with a standard error of 1.16 , while the delay or interruption proxy changes by -0.48 points with a standard error of 0.77 . Excess elapsed time relative to reported semester falls by 0.018 years and is not statistically significant. The observed time from Saber 11 to university-level Saber Pro rises by 0.055 years, about 0.65 months, but its pre-trend test is borderline. The data do not show that the extra elapsed time reflects a clean increase in interruption or delayed entry.

The FARC versus ELN mechanism estimates, reported in the same table and appendix figures, are also mostly null. Credit use changes by 0.95 percentage points with a standard error of 1.92 , the delay

How quickly do students reach Saber Pro?

Event-study estimates relative to the 2011 Saber 11 cohort

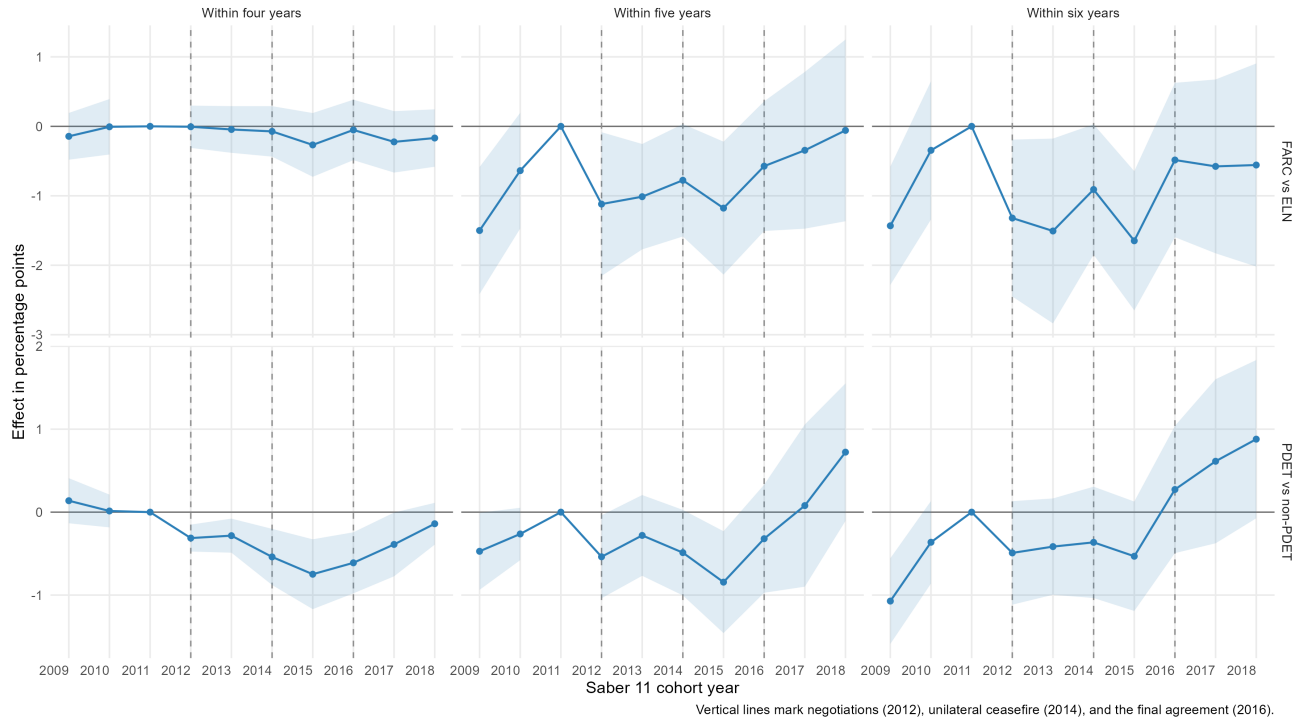


Figure 7: Timing of progression to university-level Saber Pro

Notes: Panels report event-study coefficients for reaching university-level Saber Pro within four, five, and six years of Saber 11. All other eligible Saber 11 students are zero. The omitted cohort is 2011. The top row compares historical FARC with historical ELN municipalities; the bottom row compares PDET with non-PDET municipalities. Estimates are percentage points with 95 percent confidence intervals from municipality-clustered standard errors. These are near-completion horizons, not first-enrollment outcomes.

or interruption proxy changes by 1.15 points with a standard error of 1.51, and total elapsed time is effectively unchanged. Excess elapsed time rises by 0.034 years and is just statistically distinguishable from zero, but that isolated conditional estimate does not identify delayed enrollment or interruption.

These results do not establish negotiation uncertainty as an educational mechanism. The administrative data do not observe applications or entry dates, the cumulative near-completion horizons do not move coherently, and the financing and semester proxies do not change in the predicted pattern. First-enrollment dates, applications, and semester-by-semester histories would be required for a direct test.

6.5 Secondary school participation and achievement

Figure 8 reports pooled secondary school effects for the redesigned examination era. PDET municipalities have 0.69 additional Saber 11 test takers per thousand residents relative to non-PDET municipalities after the agreement. They also have 3.34 additional represented schools per 100,000 residents, although this estimate is only marginally precise. At the same time, mean mathematics falls by 0.055 standard deviations and mean verbal performance falls by 0.031 standard deviations.

This pattern is naturally read as composition, not proof of declining school quality. When more disadvantaged or rural students remain in school long enough to take Saber 11, the mean score of examinees can fall even if no individual learns less. In fact, the PDET post-period is associated with a lower measured low socioeconomic share but a higher rural school share, suggesting multiple compositional forces. The administrative exam data do not observe students who never reach Saber 11, so they cannot separate a change in individual learning from a change in who is tested.

The FARC versus ELN estimates again center close to zero. Test taking rises by 0.42 per thousand residents, but the standard error is 0.31 and the estimate is not statistically significant. Mathematics changes by -0.004 standard deviations and verbal performance by -0.014 , both imprecise. These results reinforce the conclusion that the FARC peace process did not generate a general achievement dividend relative to other conflict-affected municipalities.

6.6 Learning between Saber 11 and Saber Pro

The linked examinations make it possible to ask whether students improved their relative performance between the end of secondary school and the end of university. Figure 9 compares the PDET and FARC versus ELN event studies for residual quantitative and reading value added. In pooled PDET regressions, the post-2012 associations are 0.049 standard deviations for quantitative value added and 0.101 standard deviations for reading. These are selected-completer associations rather than effects for the original Saber 11 cohort.

These PDET estimates are suggestive rather than causal for two reasons. First, the reading event study rejects stable pre-trends and the quantitative pre-trend is borderline. Second, value added is observed only for students who reach university-level Saber Pro, and peace may change precisely who enters that selected group. Positive residual gains can therefore combine institutional learning with changes in selection.

The FARC versus ELN design yields a null. Post-2012 quantitative value added is -0.021 standard deviations with a standard error of 0.020, while reading value added is 0.003 with a standard error of 0.024. Their confidence intervals include zero. The evidence does not support a general improvement in learning caused by the FARC ceasefire.

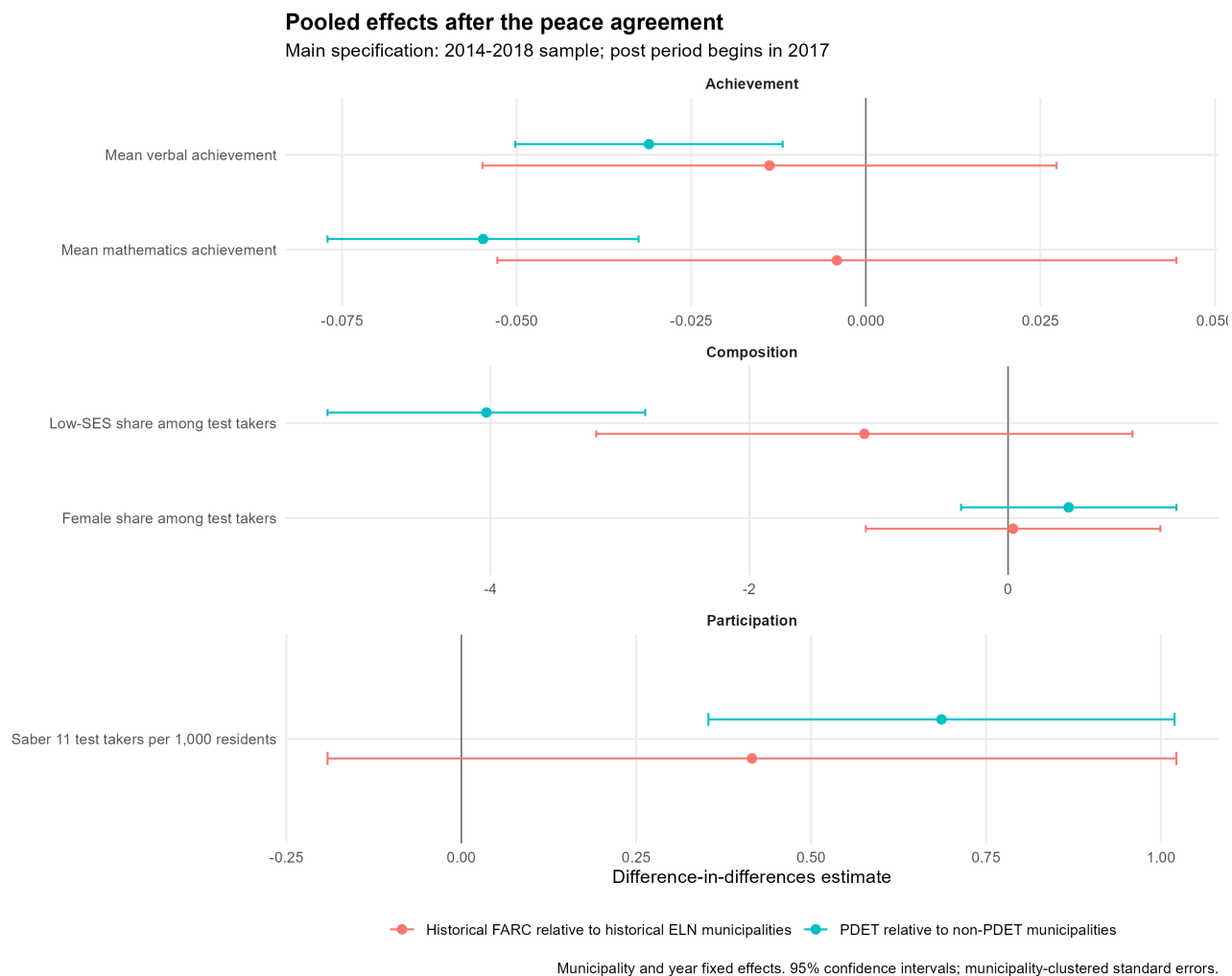


Figure 8: Pooled post-agreement effects on Saber 11 participation and achievement

Notes: Points report treatment by post-agreement coefficients from municipality level models with municipality and year fixed effects. The main specification uses the redesigned Saber 11 era and treats 2017 as the first fully post-agreement cohort. Scores are standardized within cohort. Standard errors are clustered by municipality.

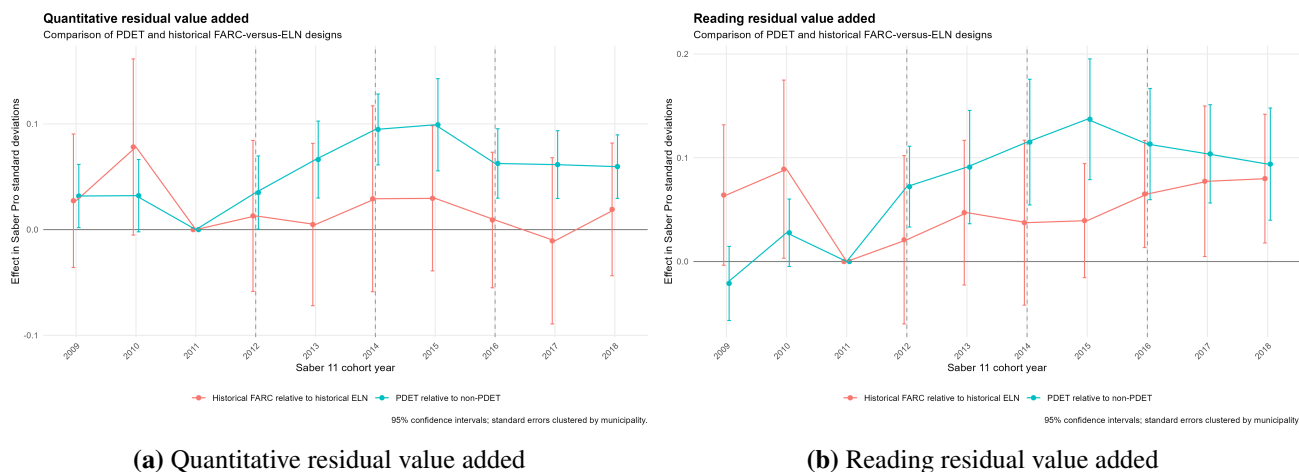


Figure 9: Value added between Saber 11 and Saber Pro

Notes: Saber Pro scores are standardized within test year and residualized on the corresponding Saber 11 score and baseline covariates. Estimates condition on reaching university-level Saber Pro within six years. The PDET pre-trend p values are 0.062 for quantitative and 0.007 for reading value added. The FARC versus ELN pre-trend p values are 0.183 and 0.096.

6.7 University destination, migration, and field of study

The type of university trajectory also matters. Because unconditional university-specific outcomes often have differential PDET pre-trends, I emphasize composition among students who are observed in university-level Saber Pro. These estimates answer whether the destination of completers changed, not whether the original cohort became more likely to complete a particular type of program. Figure 11 and Figure 12 in the appendix report the full event paths.

The migration results do not reveal a robust mechanism. Among PDET completers, the probability of studying outside the home municipality is generally one to two percentage points lower after negotiations, but confidence intervals usually include zero. In the FARC versus ELN comparison, the point estimates are positive, ranging from about 3.7 to 7.6 percentage points between 2012 and 2016, but the intervals widen sharply because the ELN comparison group is small. Peace did not clearly eliminate the need to leave the home municipality, nor is there precise evidence that it increased educational migration.

The most coherent composition result concerns public universities. Among FARC-origin completers, the public-university share is approximately 4.5 to 7.2 percentage points lower than among ELN-origin completers throughout the post-2012 period. The pre-trend test does not reject at conventional levels, with a p value of 0.606. This pattern could reflect expansion through private institutions, geographic differences in public supply, or selection among the students who reach Saber Pro. It should not be read as evidence that public universities contracted because of peace, but it does show that the response did not take the form of an across-the-board expansion of public provision.

There is no robust movement into STEM. PDET estimates become modestly negative after 2015, while FARC versus ELN estimates fluctuate around zero with wide confidence intervals. Education programs show a more notable decline among PDET completers, reaching -2.67 percentage points in 2018 with a standard error of 1.20 and acceptable pre-trends. FARC versus ELN estimates for education are also generally negative after 2014, although less precise. Together, these results do not indicate a shift toward fields most directly associated with local public-service provision or technological upgrading.

Table 2 collects the main educational estimates. The appendix reports the conflict first stage and

timing or financing regressions separately so their samples and scales remain transparent. The table gives priority to pre-trend diagnostics and distinguishes full-cohort university outcomes from selected-completer measures.

Table 2: Selected educational estimates using a consistent university-level outcome

Outcome and comparison	Estimate	Std. error	Pre-trend <i>p</i>
<i>Panel A: PDET relative to non-PDET municipalities</i>			
University-level Saber Pro within six years, pooled post 2012, pp	0.453	0.270	0.000
University-level Saber Pro in years four to six, pooled post 2012, pp	0.519	0.264	0.000
University-level Saber Pro within six years, 2012 event, pp	−0.492	0.318	0.000
University-level Saber Pro within six years, 2018 event, pp	0.880	0.487	0.000
Years from Saber 11 to university-level Saber Pro, pooled post 2012	0.055	0.012	0.069
University quantitative value added, pooled post 2012, SD	0.049	0.015	0.062
University reading value added, pooled post 2012, SD	0.101	0.024	0.007
<i>Panel B: Historical FARC relative to historical ELN municipalities</i>			
University-level Saber Pro within six years, pooled post 2012, pp	−0.432	0.417	0.001
University-level Saber Pro in years four to six, pooled post 2012, pp	−0.409	0.415	0.000
University-level Saber Pro within six years, 2012 event, pp	−1.312	0.568	0.001
University-level Saber Pro within six years, 2018 event, pp	−0.567	0.741	0.001
Years from Saber 11 to university-level Saber Pro, pooled post 2012	−0.007	0.022	0.478
University quantitative value added, pooled post 2012, SD	−0.021	0.020	0.183
University reading value added, pooled post 2012, SD	0.003	0.024	0.096

Notes: Binary outcomes are percentage points. All eligible Saber 11 students remain in the university-outcome denominator; nonmatches, technical or technological records, and university records outside the horizon equal zero. Event coefficients omit 2011. Models include municipality and cohort fixed effects and cluster standard errors by municipality. Pre-trend tests evaluate 2009 and 2010 jointly. Conditional duration and value-added estimates are selected-completer outcomes.

7 Interpretation: Security without a University Dividend

The results are best understood as a sequence from security to educational capacity. In the FARC versus ELN comparison, the first link is strong: core conflict falls substantially after negotiations begin and the pre-period paths are stable. The next links are weak. Secondary participation, scores, university near-completion, and value added do not differentially improve in a credible and internally consistent way. This combination is more informative than either result alone. It shows that a real peace dividend in security need not become a university-completion dividend within the observed horizon.

The PDET estimates do not establish movement at the university access margin. Point estimates become positive in the late cohorts, but their confidence intervals include zero and they sit on top of a rejected pre-trend. Saber 11 participation can still rise while university near-completion remains unchanged: reaching the secondary exit exam and persisting through most of a professional degree require different institutional inputs. PDET status also captures poverty, rurality, coca exposure, state weakness, and later territorial policy, which makes its relative trajectory difficult to assign to peace alone.

Several mechanisms can generate this combination. Lower violence can reduce displacement, recruitment risk, road insecurity, and the direct cost of attending school. Families may also lengthen their planning horizon once a ceasefire appears durable. These mechanisms primarily affect whether a student participates. Learning and speed, by contrast, depend on teachers, schools, university places, transport, finance, connectivity, and institutional quality. Such inputs adjust slowly, especially in municipalities selected precisely because poverty and state weakness are severe.

The findings therefore fit a state-capacity interpretation of the peace dividend. [Fajardo-Steinhäuser \(2024\)](#) finds that violence fell sharply in historical FARC areas without a parallel improvement in broad economic activity, and links the null to weak state entry. This paper reproduces the sharp relative conflict decline using a different administrative event panel and shows the same disconnect for education. Security makes investment possible, but public and private capacity determines whether possibility becomes achievement. The positive but transitory firm entry documented by [Bernal et al. \(2024\)](#) similarly shows that expectations and complementary programs mediate the response to de-escalation.

Uncertainty is theoretically plausible but not empirically established here. Education is a long-horizon investment, and households facing insecure income, travel, or territorial control may wait for information before committing ([Kazianga, 2012](#); [Stange, 2012](#)). Yet the four-, five-, and six-year horizons do not display a common response, credit use and the semester-based delay proxy do not change detectably, and first enrollment is unobserved. The paper therefore does not use uncertainty to explain the main results.

The results need not contradict studies documenting changes in school enrollment, dropout, or secondary achievement after the ceasefire. [Namen et al. \(2021\)](#), [Montaño-Bardales and Palacios \(2025\)](#), and [Avendaño Santiago \(2021\)](#) study earlier margins of the educational pipeline, different populations, or different post-conflict contrasts. This paper asks whether a recent grade-11 examinee reaches at least 75 percent of a professional university degree within six years. Peace may affect school attendance without generating the finance, preparation, geographic mobility, and university supply required for that later outcome.

The null result is informative for two reasons. First, the conflict first stage verifies that the relevant security environment changed sharply in historical FARC areas. Second, the university outcome spans a consequential transition from secondary exit to near degree completion. The absence of a robust response therefore identifies where the peace dividend did not automatically propagate. It does not say that violence is irrelevant to education; it says that removing one constraint was insufficient to shift this demanding downstream margin on average.

Conditional destination and field results do not overturn this conclusion. They describe the selected students who reach university-level Saber Pro and can change when the composition of completers changes. The absence of a robust movement into STEM, public universities, or faster progression is nevertheless difficult to reconcile with a large, coordinated expansion of professional higher-education capacity in former conflict territories.

8 Threats to Identification and Scope

The first limitation is treatment definition. PDET status was established in 2017, after negotiations and the ceasefire. It is a marker for territories prioritized by the peace architecture, not a policy shock that began in 2012. Since designation reflects poverty, rurality, illicit crops, and conflict, differential recovery can arise for reasons other than peace. Municipality and cohort fixed effects remove stable gaps and national shocks, but the university outcome rejects the PDET pre-trend test. The PDET coefficients should not be interpreted causally.

Second, the FARC versus ELN design has only 41 control municipalities. Its confidence intervals are wider, historical presence can be measured with error, and the ELN is not a perfect untreated group. ELN territories experienced national political change and may have been affected by spillovers, changing armed competition, or their own negotiations. The design estimates a relative effect between two types of conflict exposure, not the effect of peace relative to continued war in a sealed laboratory.

Third, municipality is measured at Saber 11. Students may migrate for university, so the outcome captures whether youth originating in a municipality reach Saber Pro, not whether higher education is locally supplied. That origin-based interpretation is valuable for human capital opportunity but does not identify local university expansion. Future work could use institution and campus identifiers to separate local supply, educational migration, and destination quality.

Fourth, appearing in Saber Pro is not identical to graduating. It indicates completion of at least 75 percent of a professional program and usually occurs close to graduation. Some examinees may not graduate. Technical and technological records are deliberately coded zero because their separate Saber TyT series lacks a pre-peace comparison and because including them in early Saber Pro years creates a time-varying outcome definition. The outcome is therefore a conservative professional-university progression measure, not a measure of all tertiary credentials.

Fifth, the linked data condition on taking Saber 11. If peace primarily changes whether students reach the exam, the postsecondary analysis misses youth who remain outside the observed secondary school population. The municipality level Saber 11 participation results partly address this concern and suggest that selection into the starting sample changed. A complete welfare analysis would link population cohorts, enrollment records, dropout, secondary completion, higher education enrollment, and formal graduation.

Sixth, the conflict panel measures recorded events rather than latent territorial control or victimization. Reporting can change with police presence, access, or institutional capacity. The core index reduces dependence on a single category and the FARC versus ELN pre-trends are stable, but administrative zeros need not mean an absence of insecurity. Homicide and extortion are kept separate precisely because the end of FARC operations can change the composition of violence without ending all criminal activity.

Seventh, the uncertainty tests are constrained by the examination architecture. Saber Pro reveals that a student reached the final portion of a program, not when the student applied or first enrolled. Reported semester is a contemporaneous self-report and can be affected by program changes, transfers, part-time study, and inconsistent semester counting. Financing and employment are observed only among eventual university-level Saber Pro examinees. These outcomes can describe the selected completion path, but they cannot replace administrative enrollment, application, ICETEX, or semester-by-semester persistence records.

9 Conclusion

Colombia's peace process generated a substantial security dividend without a corresponding measured university-completion dividend. Historical FARC municipalities experience a large and persistent decline in core conflict relative to historical ELN municipalities, with stable pre-trends. Nevertheless, neither the FARC versus ELN comparison nor the PDET comparison provides credible evidence of greater university-level Saber Pro participation within six years. Saber 11 achievement and conditional university value added also show no general improvement. This is the paper's strongest conclusion: reducing organized armed violence was consequential, but insufficient by itself to transform this downstream human-capital margin.

Consistency of outcome definition is central to that conclusion. The testing system separates technical and technological examinees into Saber TyT from 2016 onward, so the main outcome counts only university-level Saber Pro records in every cohort and codes every nonmatch or non-university appearance as zero. The six-year window includes the full range of plausible professional-program trajectories while giving each Saber 11 cohort the same observation horizon. As a sensitivity check, the years-four-through-six definition excludes unusually early appearances. It differs from the six-year measure for only 7,327 students, about 0.14 percent of the descriptive sample, and yields the same inferential conclusion. The main result is therefore not driven by implausibly rapid progression.

The broader evidence provides little support for gains along other observed educational margins. Students who reach Saber Pro do not progress faster, achievement gains are not robust, and migration, field, financing, and delay estimates do not reveal a coherent mechanism. These findings do not imply that peace had no educational consequences. They delimit the claim: within the available cohorts, the security improvement did not reliably raise the probability that recent secondary-school examinees reached the final stage of a professional university program.

This distinction has direct policy relevance. In post-conflict regions, security policy and educational policy are complements. Lower violence can make long-horizon investment possible, but professional university completion also requires secondary preparation, transport, finance, institutional supply, and credible state presence. Peace removes a barrier. It does not itself provide the educational capacity needed to cross it.

A Additional Descriptive Evidence

Figure 10 displays raw standardized achievement by cohort. The persistent PDET gap makes clear why comparisons in levels are uninformative. FARC and ELN trends overlap much more closely, supporting their use as a conflict-exposed comparison while also showing that the design has limited variation from which to estimate large differential changes.

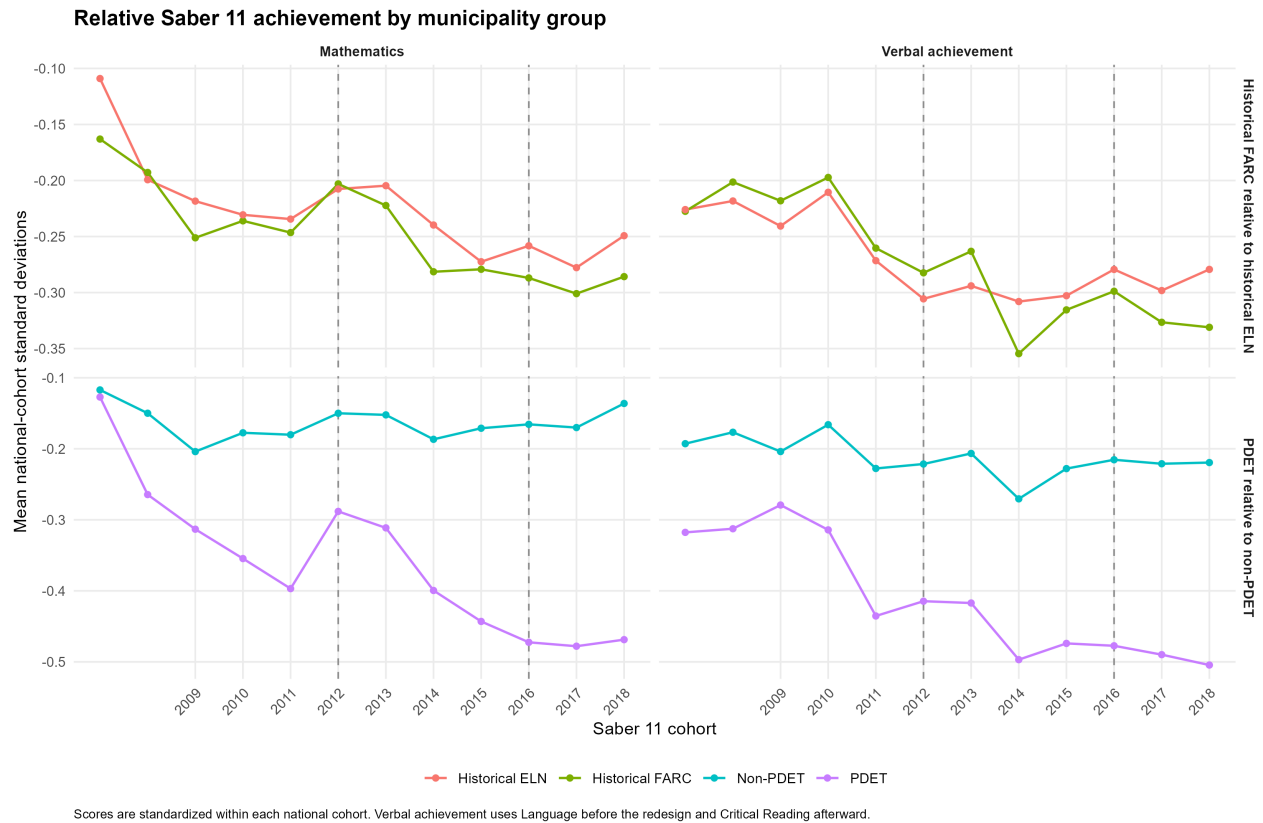


Figure 10: Raw Saber 11 achievement trends

Notes: Mathematics and verbal scores are standardized within test cohort. Lines report group means before regression adjustment.

B Conditional University Trajectories

The panels below report outcomes measured among students observed in university-level Saber Pro. Conditioning on university progression makes these composition results potentially endogenous to the peace process. They are included to characterize the students and institutions behind the main extensive-margin result, not as independent causal estimates for the original Saber 11 cohort.

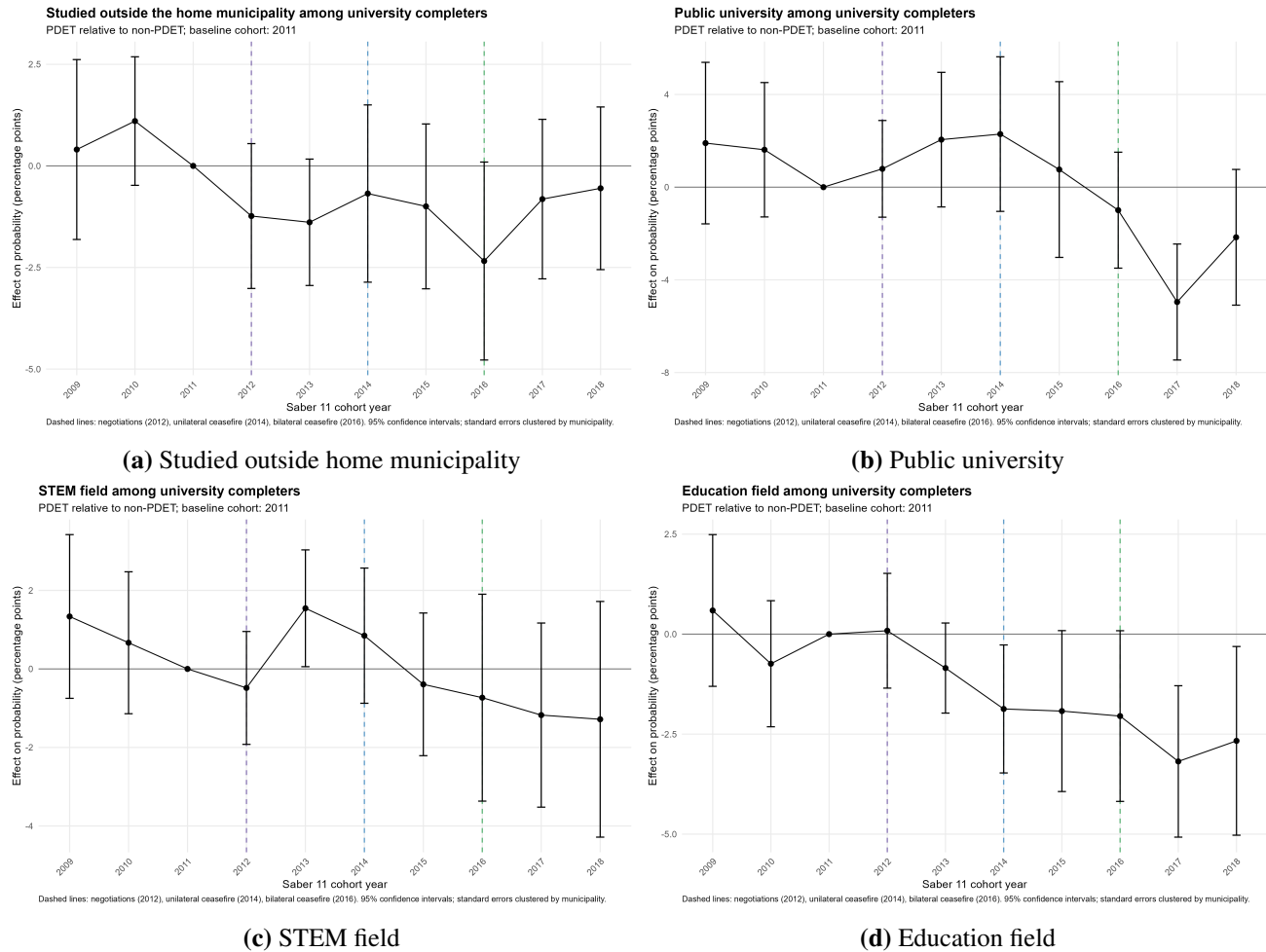
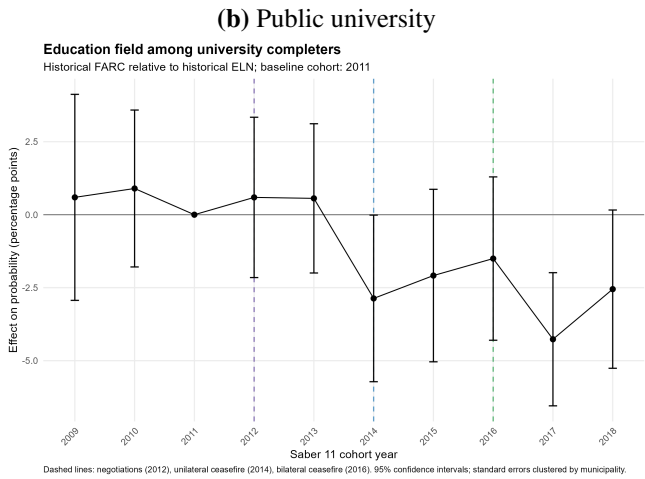
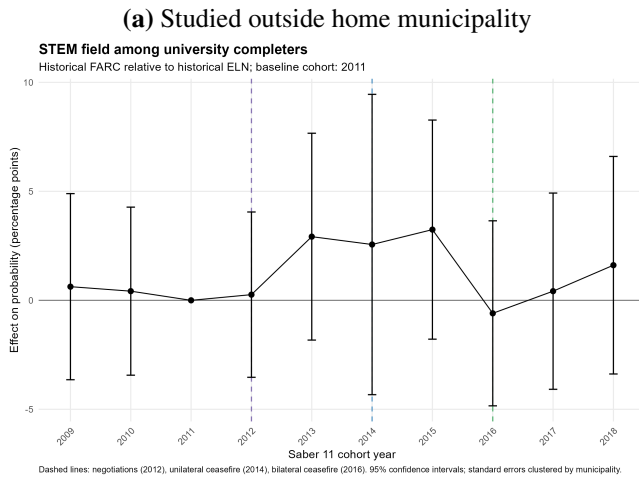
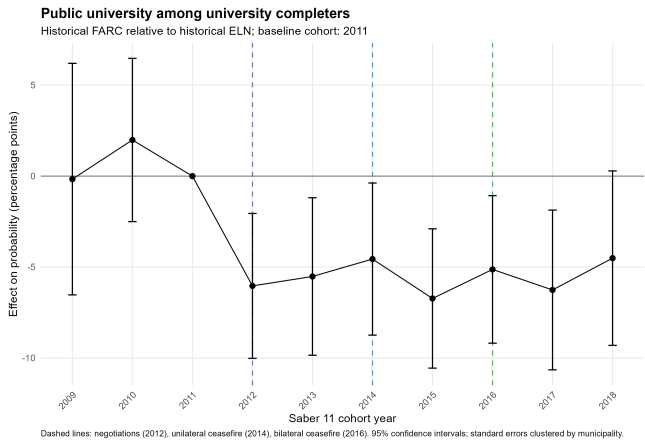
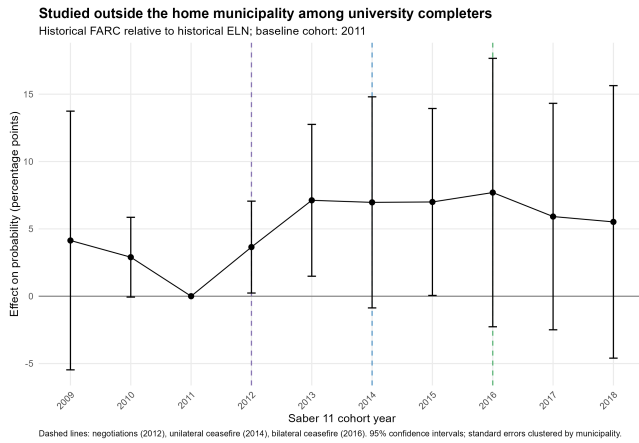


Figure 11: Composition among PDET-origin university completers

Notes: Effects are percentage points relative to the 2011 cohort. Pre-trend p values are 0.297 for migration, 0.507 for public university, 0.407 for STEM, and 0.364 for education.



(c) STEM field

(d) Education field

Figure 12: Composition among historical FARC and ELN university completers

Notes: Effects compare historical FARC with historical ELN municipalities and are percentage points relative to 2011. Pre-trend p values are 0.153 for migration, 0.606 for public university, 0.947 for STEM, and 0.806 for education.

C Conflict Outcomes and Robustness

Figure 13 compares the pooled negotiation and post-agreement estimates for the conflict index, core event rate, homicide, and extortion. The FARC versus ELN decline is present under both policy windows and is robust to replacing the standardized index with the log population-normalized event rate. PDET conflict reductions are concentrated after the agreement, while extortion and homicide illustrate that broader public safety need not follow guerrilla-related violence one for one.

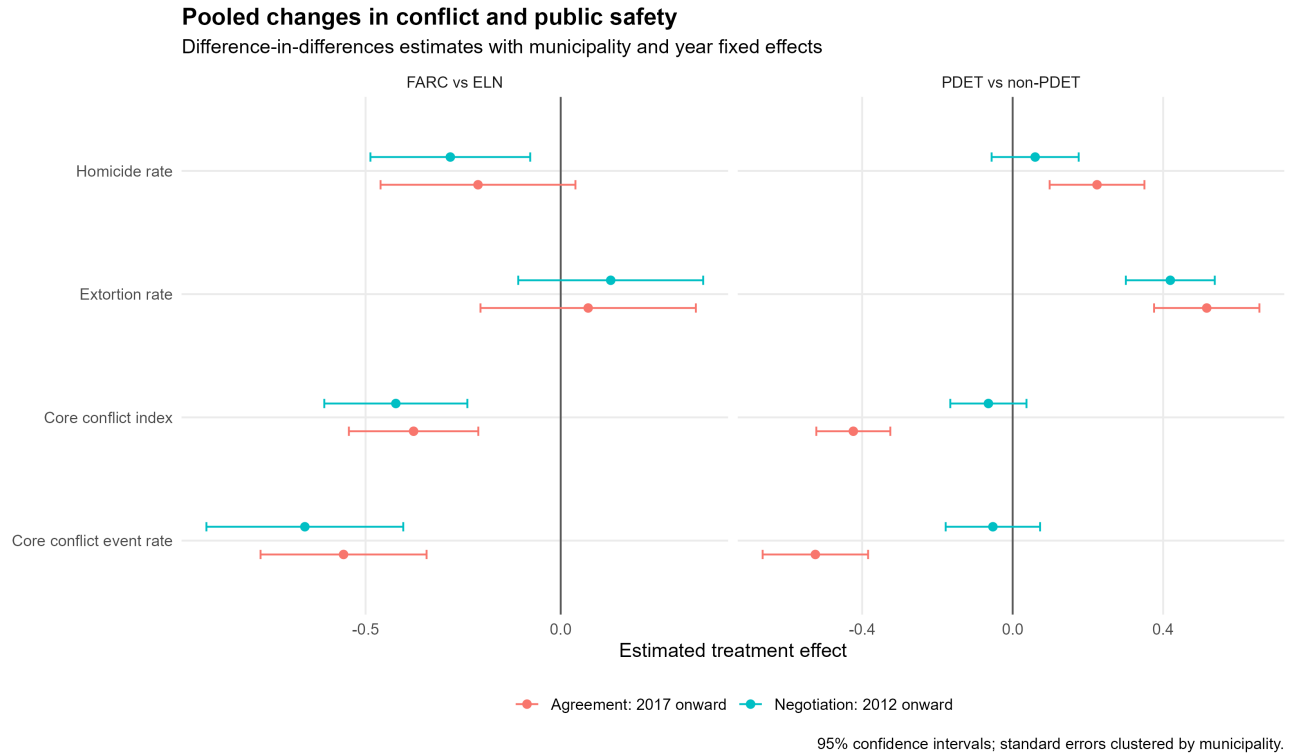


Figure 13: Pooled conflict and public-safety regressions

Notes: Points report treatment-by-policy-period coefficients from municipality-year models with municipality and year fixed effects. Equal municipality weights are used. Horizontal bars are 95 percent confidence intervals from municipality-clustered standard errors. Negotiation models define 2012 to 2018 as post; agreement models define 2017 to 2018 as post.

Table 3: Main conflict regressions

Design	Outcome	Policy period	Estimate (SE)
PDET vs non-PDET	Core conflict index	Negotiation, 2012 onward	-0.064 (0.052)
PDET vs non-PDET	Core conflict event rate	Negotiation, 2012 onward	-0.053 (0.064)
PDET vs non-PDET	Core conflict index	Agreement, 2017 onward	-0.423 (0.050)
PDET vs non-PDET	Core conflict event rate	Agreement, 2017 onward	-0.524 (0.072)
FARC vs ELN	Core conflict index	Negotiation, 2012 onward	-0.422 (0.094)
FARC vs ELN	Core conflict event rate	Negotiation, 2012 onward	-0.656 (0.129)
FARC vs ELN	Core conflict index	Agreement, 2017 onward	-0.377 (0.085)
FARC vs ELN	Core conflict event rate	Agreement, 2017 onward	-0.556 (0.109)

Notes: All models include municipality and year fixed effects. Standard errors in parentheses are clustered by municipality. Dependent variables are defined in Code 10.

Notes: Each entry reports a treatment-by-post coefficient followed by its municipality-clustered standard error in parentheses. All specifications contain municipality and year fixed effects. The core index averages standardized log population-normalized rates for the five event categories with complete 2007 to 2018 coverage.

C.1 Conflict-variable construction

The event files are merged by official municipality code with the CEDE municipal panel (CEDE, 2022). Let C_{jmt} denote the count for event family j in municipality m and year t , and let Pop_{mt} be population. For the five families with complete 2007 to 2018 coverage, I construct

$$r_{jmt} = 100,000 \frac{C_{jmt}}{Pop_{mt}}, \quad z_{jmt} = \frac{\log(1 + r_{jmt}) - \bar{\ell}_j}{s_j}, \quad ConflictIndex_{mt} = \frac{1}{5} \sum_{j=1}^5 z_{jmt},$$

where $\bar{\ell}_j$ and s_j are the municipality-year mean and standard deviation of $\log(1 + r_{jmt})$. Equal weighting prevents a high-count category from mechanically dominating the index. The five families are subversive actions, attacks on bridges and roads, kidnapping, terrorism, and oil-pipeline bombing. Attacks on the security forces are excluded from the index because that file begins in 2010. Homicide, extortion, and land invasion are treated as broader public-safety measures. Cocaine observations are excluded from the main conflict regressions because the source records enforcement quantities and does not measure hectares cultivated, production, or territorial exposure to coca markets. For each source, a zero is assigned only to municipality-years between the first and last observed source years; I do not turn years outside a file’s coverage into zero conflict.

The historical FARC and ELN indicators use a separate attribution panel covering 1996 to 2008. A municipality-year is active for an armed group if at least one attributed event is recorded in any of the available categories, including attacks on civilians, incursions, assaults, terrorist actions, kidnappings, road piracy or checkpoints, harassment, attacks on public institutions or private property, roadblocks, political attacks and homicides, ambushes, clashes, armed contacts, and deaths or injuries attributed to combat. A municipality has historical presence when the group is active in at least eight of the 13 years, a threshold of approximately 60 percent. The comparison retains only mutually exclusive FARC-presence and ELN-presence municipalities and drops the top 2 percent by average 1996 to 2008 population. This procedure follows the territorial comparison in Fajardo-Steinhäuser (2024) and produces 216 FARC and 41 ELN municipalities.

D Timing, Financing, and Measurement Limits

Figure 14 separates the two continuous timing measures and reports effects in months. Total time to Saber Pro rises in PDET territories, while excess time relative to semester does not. This distinction is why the main text does not equate longer progression with enrollment interruption.

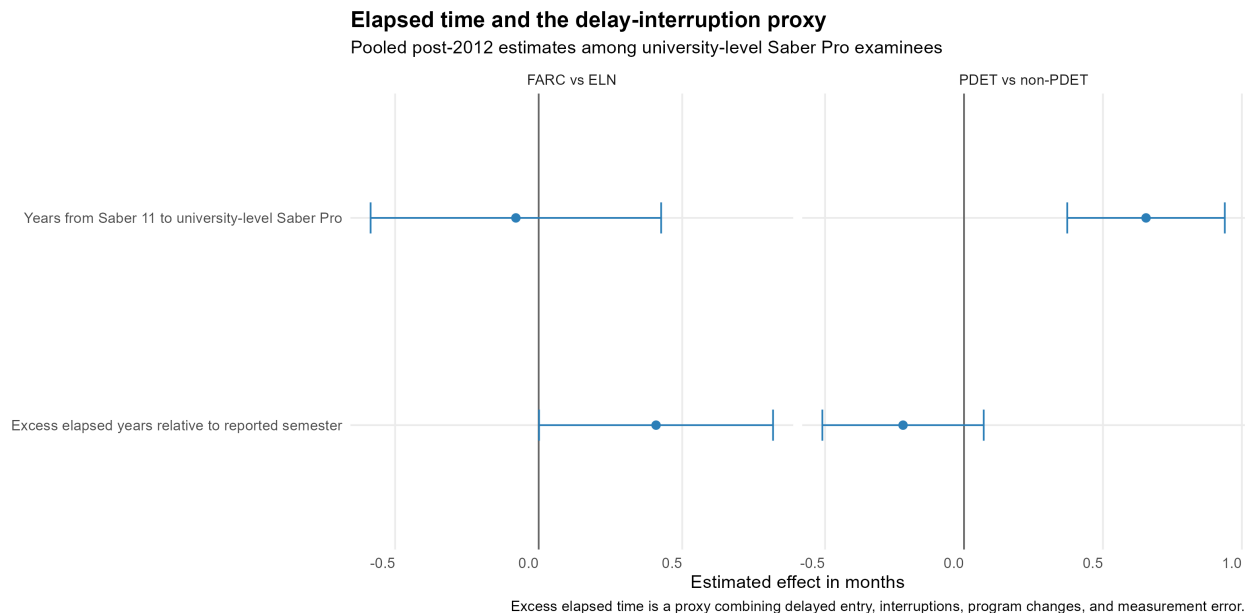


Figure 14: Elapsed time and the delay or interruption proxy

Notes: Pooled post-2012 models condition on reaching university-level Saber Pro within six years. Excess elapsed time subtracts one-half of the semester reported at Saber Pro from years since Saber 11. It is a proxy combining delayed entry, interruptions, program changes, and measurement error. Models include municipality, Saber 11 cohort, and where appropriate Saber Pro period fixed effects.

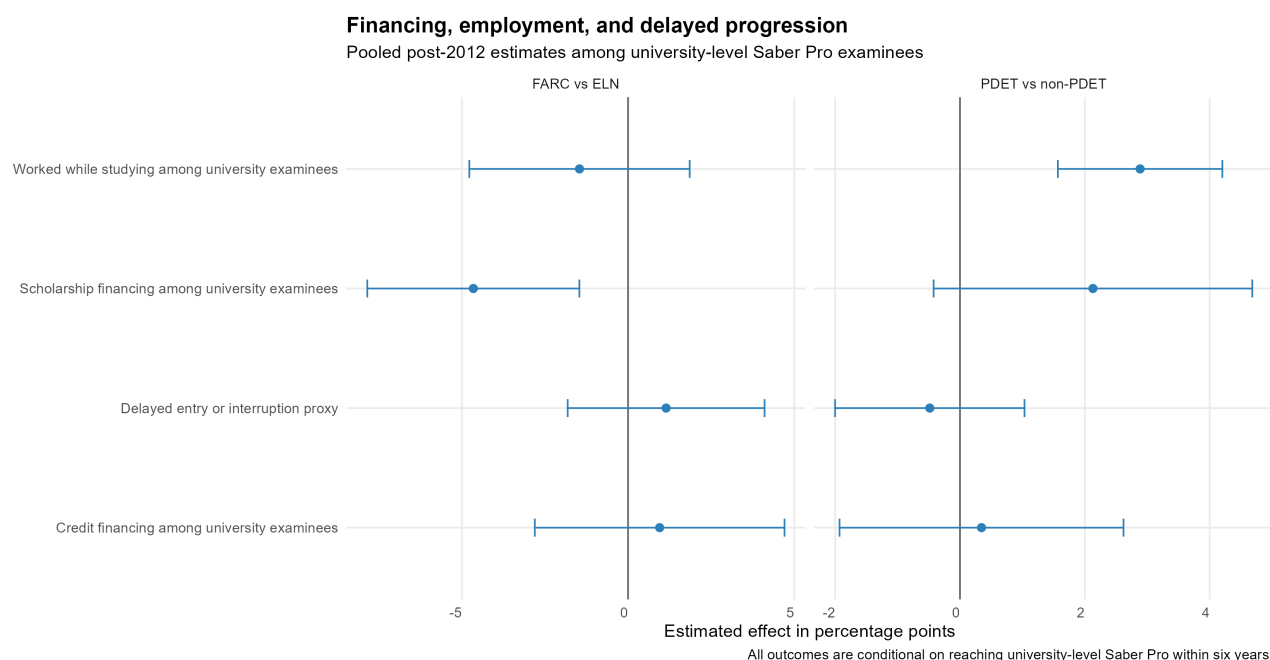


Figure 15: Financing, employment, and delayed progression among university examinees

Notes: Outcomes are measured among students reaching university-level Saber Pro within six years and are expressed in percentage points. Financing variables describe whether tuition was paid with credit or scholarship resources. Employment records whether the examinee worked during the previous week. The delay or interruption indicator equals one when excess elapsed time is at least one year.

Table 4: Pooled timing and financing regressions

Design	Outcome	Estimate	SE	Pre-trend <i>p</i>
FARC vs ELN	Credit financing among university examinees	0.951	1.917	0.539
FARC vs ELN	Delayed entry or interruption proxy	1.145	1.511	0.668
FARC vs ELN	Excess elapsed years relative to reported semester	0.034	0.017	0.718
FARC vs ELN	University-level Saber Pro within four years	-0.073	0.127	0.653
FARC vs ELN	University-level Saber Pro within five years	-0.032	0.309	0.005
FARC vs ELN	University-level Saber Pro within six years	-0.432	0.417	0.001
FARC vs ELN	University-level Saber Pro in years four to six	-0.409	0.415	0.000
PDET vs non-PDET	Credit financing among university examinees	0.345	1.161	0.860
PDET vs non-PDET	Delayed entry or interruption proxy	-0.484	0.774	0.322
PDET vs non-PDET	Excess elapsed years relative to reported semester	-0.018	0.012	0.844
PDET vs non-PDET	University-level Saber Pro within four years	-0.476	0.135	0.469
PDET vs non-PDET	University-level Saber Pro within five years	0.004	0.219	0.086
PDET vs non-PDET	University-level Saber Pro within six years	0.453	0.270	0.000
PDET vs non-PDET	University-level Saber Pro in years four to six	0.519	0.264	0.000

Notes: Binary outcomes are reported in percentage points; excess elapsed time is reported in years. All models include municipality and Saber 11 cohort fixed effects and control for cohort-standardized Saber 11 mathematics and reading scores. Conditional Saber Pro outcomes include examination-period fixed effects. Pre-trend tests jointly evaluate the 2009 and 2010 event coefficients.

Table 5: Observed and unobserved timing margins

Margin	Status in ICFES data	Interpretation
University-level Saber Pro within 4, 5, or 6 years	Directly observed from linked exam dates	Speed to near-completion, not speed to first enrollment
Years from Saber 11 to Saber Pro	Directly observed	Total elapsed progression time among examinees
Current program semester	Self-reported in most Saber Pro years	Program position at the exam; not a complete enrollment history
Excess elapsed time	Constructed proxy	Delayed entry, interruption, transfer, program change, or reporting differences
Credit and scholarship use	Directly reported at Saber Pro	Financing composition among selected near-completers
Work while studying	Directly reported at Saber Pro	Employment in the week before the exam
Applications	Not observed	Requires application records from institutions or a centralized system
Initial enrollment and interruptions	Not observed	Requires SNIES or SPADIES student histories linked to Saber 11

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