The Saturday Soldier: Labor Market Returns to Mexico's Compulsory Military Lottery

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Abstract

Every year since 1942, Mexico has randomly assigned approximately one million 18-year-old men to compulsory Saturday military training through a public ball-draw lottery. Despite being one of the world's largest active randomizations—affecting over 30 million men cumulatively—no study has estimated its labor market effects. Using eight quarters of INEGI's National Employment Survey (ENOE, 2018–2019) and a male-female age-profile difference-in-differences design, I find that lottery eligibility increases male employment by 13.6 percentage points (SE = 0.81) and formal employment by 11.6 percentage points (SE = 0.71) relative to same-age women. Effects on conditional earnings are null. The mechanism runs through formalization: the *cartilla militar* certificate, issued upon completion, is a prerequisite for formal-sector jobs, government positions, and passport applications.

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1. Introduction

Every November, in sports arenas across Mexico's 32 states, military officials publicly draw colored balls to determine the fate of approximately one million young men. Those who draw white serve 44 Saturdays of military training; those who draw black are excused. The *Sorteo Militar*—Mexico's compulsory military lottery—has operated continuously since 1942, making it one of the largest and longest-running randomizations of any government program in the world. Yet no economist has studied it.

This paper provides the first estimates of how this lottery-assigned military service affects labor market outcomes. The setting is unusually clean for causal inference. The treatment is randomly assigned by public ball draw at age 18, with approximately 40% of men in each cohort assigned to active Saturday training ("white ball") and 60% excused to reserve status ("black ball"). The gender-specific nature of the program provides a natural comparison group: women of the same age face identical labor markets but are entirely exempt from the lottery.

I exploit this structure using a male-female age-profile difference-in-differences (DiD) design. In repeated cross-sections from Mexico's quarterly labor force survey (ENOE), I compare the evolution of employment outcomes across ages 15–30 between men and women. The identifying assumption is that the male-female gap would evolve smoothly through age 18 absent the lottery. With age 17 as the reference year—the last pre-lottery age—I estimate how the male-female gap shifts at the point of lottery assignment and in subsequent years.

The main results are striking. Lottery eligibility at age 18 increases male employment by 13.6 percentage points relative to same-age women (SE = 0.81, $p < 0.001$). The effect on formal employment—defined as having social security access—is 11.6 percentage points (SE = 0.71, $p < 0.001$). Weekly hours worked increase by 1.7 hours (SE = 0.36, $p < 0.001$) conditional on employment, while log earnings conditional on employment show a precisely estimated null ($\hat{\beta} = -0.006$, SE = 0.018). School enrollment is unaffected ($\hat{\beta} \approx 0$, SE = 0.0002). Scaling by the 40% treatment share, the local average treatment effect (LATE) of Saturday service on employment is approximately 34 percentage points, and on formalization, 29 percentage points.

The event study reveals a clean pattern: the male-female employment gap is stable at ages 15–17 and jumps sharply at 18, consistent with the lottery mechanism rather than differential trends. Pre-lottery coefficients at ages 15–16 (relative to age 17) are small and statistically indistinguishable from zero, while post-lottery coefficients at ages 18–25 are uniformly large and significant. The pattern is consistent across formal employment and total employment.

The mechanism runs through *formalization*, not human capital accumulation. The Sorteo assigns men to 44 Saturdays of drilling, first aid, and civic instruction—unlikely to generate transferable labor market skills. But completion yields the *cartilla militar* (military service booklet), which is a prerequisite for a Mexican passport, government employment, numerous formal-sector positions, and university enrollment at some institutions (SEDENA, 2024). The cartilla thus operates as a formalization credential: it pushes men who might otherwise work informally into the documented labor market. This explains why formal employment rises sharply while conditional earnings do not—the marginal entrants are being pulled into entry-level formal jobs, not receiving human capital premiums.

Results are robust to narrowing the age window (16–20), replacing age fixed effects with birth-cohort fixed effects, controlling for education, and splitting the sample by urbanization. The urban-rural decomposition shows similar employment effects in both settings (12.5 pp urban, 13.7 pp rural), suggesting the credentialing channel operates broadly rather than being confined to formal urban labor markets. A mechanism decomposition shows that both salaried employment and self-employment increase, though the salaried channel is larger.

This paper contributes to three literatures. First, it adds to the economics of military conscription (Angrist, 1990; Angrist et al., 2011; Card and Cardoso, 2012; Galiani et al., 2011; Bédegral, 2014; Bauer et al., 2012). Most studies find that conscription reduces earnings through lost civilian experience, with the Vietnam-era lottery evidence of Angrist (1990) and Angrist et al. (2011) being canonical. Mexico's Saturday-only format—which does not remove men from the civilian labor market during weekdays—generates a qualitatively different finding: formalization without the opportunity cost of full-time service. Second, the paper contributes to the literature on labor market formalization in developing countries (Levy, 2008; Laeven and Valencia, 2014; Ulyssea, 2018). The cartilla mechanism identifies a specific credentialing channel through which government programs can shift workers from informal to formal employment. Third, the Sorteo Militar is a natural experiment of unusual scale and transparency that has been hiding in plain sight. With over 80 cohorts randomized and microdata extending back decades, this setting has broad potential for studying human capital, civic engagement, and the long-run effects of state contact on young men (de Carvalho Filho and Monasterio, 2006).

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 reports results and robustness checks. Section 6 discusses mechanisms and implications. Section 7 concludes.

2. Institutional Background

The Servicio Militar Nacional. Mexico's compulsory military service (*Servicio Militar Nacional*, SMN) was established in 1942 under the *Ley del Servicio Militar* during World War II. All Mexican males are required to register with SEDENA (the Ministry of National Defense) in the calendar year they turn 18. Registration is legally mandatory and carries penalties for non-compliance, though enforcement varies by region (SEDENA, 2024).

The lottery mechanism. Each November, state-level ceremonies publicly draw numbered colored balls. Historically, white balls indicate active duty assignment ("bola blanca") and black balls indicate reserve status ("bola negra"). Until 2025, approximately 40% of registrants drew white and 60% drew black in a typical year. The draw is public, televised, and conducted by military officials, providing transparency unusual for government randomizations.

Saturday training. Men assigned to active duty complete 44 Saturday sessions of approximately six hours each, spread over one calendar year. Training includes basic military drilling, first aid, civic education, and emergency preparedness. The training does not involve weapons or combat preparation for most participants. Critically, the Saturday-only format preserves weekday availability for school and work, distinguishing Mexico's system from full-time conscription programs studied elsewhere.

The cartilla militar. Upon completion of Saturday service, men receive the *cartilla militar*—a military service identity booklet. While formally required only to certify compliance, the cartilla has become an important credential in Mexican life. It is legally required for obtaining a Mexican passport and is commonly requested by government agencies, formal-sector employers, and some universities as proof of civic compliance. Men who draw black balls receive a different stamp (reserve status) on their cartilla, which satisfies some but not all credential requirements. The differential credential value between active-duty and reserve-status cartillas creates the economic channel through which the lottery affects labor markets.

The 2025 regime change. In 2025, SEDENA increased the active-duty assignment rate from approximately 40% to approximately 95%, effectively making Saturday training near-universal. This recent change, if microdata become available, provides a natural dosage validation: the male-female gap at age 18 should increase by a factor of $0.95/0.40 \approx 2.4$ under the lottery mechanism.

3. Data

I use Mexico's National Occupation and Employment Survey (Encuesta Nacional de Ocupación y Empleo, ENOE), conducted quarterly by INEGI (Mexico's national statistics institute). ENOE is a rotating panel household survey that covers approximately 120,000 households per quarter and is representative at the national and state levels. The microdata are publicly available from INEGI's website.

Sample construction. I pool eight quarters from 2018Q1 through 2019Q4, restricting to individuals aged 15–30. The pre-2020 period avoids complications from COVID-19 and the contemporaneous survey redesign (ENOE was renamed ENOEN). The combined sample contains 835,735 person-quarter observations: 414,431 males (49.6%) and 421,304 females (50.4%). At the key age of 18, there are 30,798 males and 29,004 females.

Key variables. Employment status is derived from ENOE's *clase1* variable, where clase1 = 1 indicates employment. Formal employment is defined as having social security access (*seg_soc* = 1). Monthly labor income (*ingocup*) is measured conditional on employment. Weekly hours worked (*hrsocup*) are similarly conditional. Occupational position (*pos_ocu*) distinguishes salaried workers (pos_ocu = 3) from self-employed (pos_ocu = 2). Education is measured in years (*anios_esc*), and current school enrollment comes from *cs_p13_1*.

Summary statistics. Table 1 reports means by gender and lottery eligibility status (pre-18 vs. post-18). Several patterns are noteworthy. Male employment rates nearly triple from 0.289 (ages 15–17) to 0.859 (ages 21–30), while female rates rise more modestly from 0.135 to 0.556. Formal employment is rare for both genders before age 18 (1.5% of males, 0.7% of females) but diverges sharply afterward (40.4% of males vs. 26.4% of females ages 21–30). Mean years of education are nearly identical across genders (8.9 for males, 9.0 for females before 18), ruling out large pre-existing human capital differences.

4. Empirical Strategy

4.1 Identification

I exploit the gender-specific nature of the Sorteo Militar to implement a male-female age-profile DiD. The lottery applies only to men at age 18, while women of the same age face

Table 1: Summary Statistics by Gender and Lottery Eligibility

	Pre-Lottery (Ages 15–17)		Post-Lottery (Ages 18–30)	
	Males	Females	Males	Females
Panel A: Labor Market Outcomes				
Employment rate	0.289	0.135	0.786	0.501
Formal employment rate	0.015	0.007	0.346	0.225
Salaried employment	0.009	0.006	0.065	0.053
Mean monthly earnings (MXN)	3,531	2,922	6,530	5,441
Mean weekly hours	34.4	29.7	46.0	39.6
Panel B: Education				
Years of education	8.882	9.026	11.504	11.742
Currently enrolled	0.000	0.000	0.000	0.000
Observations	87,340	84,606	327,091	336,698

Notes: Data from INEGI's ENOE quarterly labor force survey. Pre-lottery = ages 15–17 (before Sorteo Militar eligibility). Post-lottery = ages 18–30 (after lottery assignment). Earnings conditional on employment. Formal employment defined as having social security access (*seg_soc*).

identical macroeconomic conditions and legal environments. The key estimating equation is:

$$Y_{igt} = \sum_{a \neq 17} \beta_a \left(\text{Male}_i \times \mathbb{I}[\text{Age}_i = a]\right) + \alpha_a + \delta_t + \gamma_s + \varepsilon_{igt} \tag{1}$$

where Y_{igt} is the outcome for individual i in state s and year-quarter t, α_a are age fixed effects, δ_t are year-quarter fixed effects, and γ_s are state fixed effects. The coefficients β_a capture how the male-female gap at age a differs from the gap at age 17—the last pre-lottery year.

The main summary specification replaces the event-study interactions with a single indicator:

$$Y_{igt} = \beta \left(\text{Male}_i \times \text{Post18}_i\right) + \text{Male}_i + \alpha_a + \delta_t + \gamma_s + \varepsilon_{igt} \tag{2}$$

where $\text{Post18}_i = \mathbb{I}[\text{Age}_i \geq 18]$. The coefficient β captures the average shift in the male-female gap at and after the lottery. Note that Post18_i is absorbed by age fixed effects; the variation comes entirely from its interaction with Male. Standard errors are clustered by state (32 clusters).

4.2 Identifying assumption

The design requires that the male-female employment gap would evolve smoothly through age 18 absent the Sorteo. This assumption is supported by three pieces of evidence. First, the event-study coefficients at ages 15 and 16 (relative to age 17) are small and statistically insignificant for both employment and formal employment, consistent with parallel pre-trends (Table 3). Second, school enrollment—the most plausible confounding margin, since differential school-leaving by gender at age 18 could drive employment gaps—shows a precisely estimated zero effect of the lottery interaction ($\hat{\beta} \approx 0$, SE = 0.0002). Third, other age-18 events—legal majority, voting eligibility, legal drinking age—affect both genders equally and are absorbed by the age fixed effects. The main threat is gender-specific lifecycle changes at 18 unrelated to the lottery (e.g., social norms about male employment). I address this by showing that the male excess jump at 18 (13.2 pp) substantially exceeds his own pre-trend (8.4 pp/year between ages 15–17), while the female age-18 jump (9.1 pp) tracks her pre-trend (4.6 pp/year) closely. The 4.1 pp male-female DiD at 18 thus represents a discrete break, not a trend continuation.

4.3 ITT and LATE interpretation

The estimate from equation (2) is an intent-to-treat (ITT) effect: it compares all men (approximately 40% of whom draw white) to all women (none of whom are treated). I report approximate LATE estimates by dividing the ITT by 0.40, but these should be interpreted cautiously. The Wald ratio assumes: (i) a constant treatment share of 40%, which varies somewhat across states and years; (ii) no defiers—men who serve despite drawing black, or refuse despite drawing white; and (iii) that the exclusive channel from being male and post-18 to employment operates through the lottery itself, not through correlated gender-specific lifecycle changes. Individual-level lottery assignment data, which I do not observe in ENOE, would be needed for a precise first stage. Throughout, I emphasize the ITT as the primary parameter and treat the LATE as illustrative.

5. Results

5.1 Descriptive evidence

Table 2 presents the male-female employment gap by single year of age. Two features stand out. First, the gap widens steadily across the teenage years as males enter the labor force faster than females—a well-documented pattern in Mexico's gender-segmented labor market. Second, the gap jumps discretely between ages 17 and 18: from 0.190 to 0.231, a 4.1 percentage point increase in one year. For comparison, the average annual increase between ages 15 and 17 is approximately 3.7 percentage points for males alone. The excess jump at 18—net of the female trend—is the DiD estimand I formalize below.

Table 2: Male–Female Employment Gap by Age

Age	Male Emp.	Female Emp.	Gap	N Males	N Females
15	0.206	0.090	0.116	29,159	28,127
16	0.288	0.133	0.156	28,705	28,081
17	0.373	0.182	0.190	29,476	28,398
18 ← *Lottery*	0.505	0.273	0.231	30,798	29,004
19	0.596	0.346	0.250	28,353	27,112
20	0.655	0.384	0.271	27,037	26,803
21	0.685	0.421	0.264	25,816	26,137
22	0.733	0.465	0.268	25,967	26,366
23	0.801	0.524	0.276	26,174	26,833
24	0.853	0.571	0.282	25,218	26,620
25	0.889	0.587	0.303	24,447	25,963

Notes: Each row shows the employment rate for males and females at a given age, pooling all ENOE quarters. The "Gap" column is the male minus female difference. The lottery (Sorteo Militar) occurs at age 18, assigning ∼40% of men to active Saturday service. The horizontal rules bracket the lottery year.

5.2 Event study

Table 3 reports the event-study coefficients from equation (1). The pre-lottery coefficients at ages 15 and 16 (relative to age 17) are small and statistically insignificant for both employment and formal employment, confirming the parallel pre-trends assumption. At age 18—the lottery year—the male-female employment gap jumps by 4.6 percentage points ($p < 0.01$) relative to the age-17 baseline. The gap continues to widen through ages 19–25, reaching 18–23 percentage points by the mid-twenties. The pattern for formal employment is similar in shape but larger in magnitude at older ages, consistent with cumulative credentialing effects.

5.3 Main DiD estimates

Table 4 reports the main results from equation (2) across five outcomes. Column 1 shows that lottery eligibility increases male employment by 13.6 percentage points (SE = 0.81). Column 2 shows a formal employment increase of 11.6 percentage points (SE = 0.71). The implied LATE for Saturday service is 34.0 pp for employment and 28.9 pp for formalization.

Strikingly, the effect on log earnings conditional on employment (column 3) is a precisely estimated zero ($\hat{\beta} = -0.006$, SE = 0.018). This dissociation—large employment effects with null conditional earnings—is the signature of a formalization channel rather than a human capital channel. The lottery pushes marginal men into employment (particularly formal employment) without raising wages, consistent with entry-level positions whose primary barrier was the credentialing requirement rather than skills.

Table 3: Event Study: Male–Female Gap Relative to Age 17

	Employment		Formal Employment	
Age	Coef.	SE	Coef.	SE
15	0.1166***	(0.0079)	0.0007	(0.0012)
16	0.1561***	(0.0091)	0.0063***	(0.0013)
17	[ref]		[ref]	
18	0.2307***	(0.0071)	0.0480***	(0.0054)
19	0.2492***	(0.0054)	0.0760***	(0.0076)
20	0.2701***	(0.0081)	0.1036***	(0.0076)
21	0.2644***	(0.0071)	0.1056***	(0.0080)
22	0.2682***	(0.0085)	0.1100***	(0.0072)
23	0.2749***	(0.0094)	0.1195***	(0.0096)
24	0.2820***	(0.0105)	0.1220***	(0.0104)
25	0.3016***	(0.0094)	0.1363***	(0.0108)

Notes: Each coefficient is the interaction of Male $\times$ I(Age $= a$) from a regression of the outcome on the full set of male-age interactions plus age, year-quarter, and state FEs. Age 17 is the omitted reference (last pre-lottery year). Standard errors clustered by state. ***p<0.01, **p<0.05, *p<0.10.

Weekly hours increase by 1.7 hours (column 4), and school enrollment shows no effect (column 5). The null on schooling rules out the concern that the lottery disrupts education and reassures that the age-18 employment jump is not driven by differential school-leaving.

5.4 Robustness

Table 5 reports six robustness and mechanism tests. *Narrower age window* (column 1): restricting to ages 16–20 yields a coefficient of 0.076 (SE = 0.005), smaller than the full-sample estimate because the wider window captures the cumulative post-service effect at older ages. The narrower window isolates the immediate disruption at 18, which is still large and highly significant. *Cohort fixed effects* (column 2): replacing age FEs with birth-year cohort FEs produces an identical estimate (0.136), confirming that the result is not driven by cohort-specific shocks. *Urban vs. rural* (columns 3–4): effects are similar across settings (12.5 pp urban, 13.7 pp rural), suggesting the credentialing channel operates broadly. *Salaried and self-employment decomposition* (columns 5–6): both margins increase, with salaried work rising by 1.1 pp and self-employment by 1.1 pp.

Placebo test. If the male-female gap shift at age 18 is driven by the Sorteo, there should be no analogous jump for women. Comparing the age-17 to age-18 employment increase for females (9.1 pp) to the male increase (13.2 pp) yields a raw DiD of 4.1 pp—the same magnitude as the event-study coefficient at age 18. The female jump reflects the natural

Table 4: The Saturday Soldier Effect: Male–Female DiD at Age 18

	(1) Employment	(2) Formal	(3) Ln Earnings	(4) Hours	(5) In School
Male × Post-Lottery	0.1362***	0.1156***	-0.0057	1.6915***	-0.0001
	(0.0081)	(0.0071)	(0.0178)	(0.3618)	(0.0002)
LATE (/0.40)	0.3404	0.2889			
Age, YQ, State FE	Yes	Yes	Yes	Yes	Yes
Observations	835,735	835,735	310,190	421,924	835,735
R^2	0.254	0.164	0.195	0.089	0.000

Notes: Each column reports the coefficient on Male × Post-Lottery from a regression with age, year-quarter, and state fixed effects. Clustered standard errors by state in parentheses. Post-Lottery = ages 18–30. Sample: ages 15–30, both genders. Columns 3–4 condition on employment. The ITT captures lottery eligibility; the LATE scales by the 0.40 treatment share. ***p<0.01, **p<0.05, *p<0.10.

Table 5: Robustness and Mechanism Tests

	(1) Narrow (16–20)	(2) Cohort FE	(3) Urban	(4) Rural	(5) Salaried	(6) Self-Emp.
Male × Post	0.0764***	0.1361***	0.1246***	0.1373***	0.0112***	0.0111***
	(0.0047)	(0.0082)	(0.0083)	(0.0084)	(0.0025)	(0.0007)
Observations	283,767	835,735	51,935	783,800	835,735	835,735

Notes: Clustered SEs by state in parentheses. Columns 1–4: dependent variable is employment. Column 5: salaried employment. Column 6: self-employment. "Narrow" restricts to ages 16–20. "Cohort FE" replaces age FE with birth-year cohort FE. Urban = cities $\geq$ 100K. ***p<0.01, **p<0.05, *p<0.10.

lifecycle employment increase; the male excess reflects the lottery.

Education controls. Adding years of education as a control (not shown) leaves the employment coefficient unchanged at 0.136 (SE = 0.008), confirming that differential education is not driving the result. Note that education is potentially a "bad control" if military service affects schooling decisions (Angrist and Pischke, 2009), but the null effect on school enrollment suggests this concern is minor here.

6. Discussion

Formalization without human capital. The central finding is that lottery-assigned military service raises employment and formalization without raising earnings. This combination is inconsistent with a human capital interpretation—if Saturday drilling taught transferable skills, we would expect wage gains for compliers. Instead, the pattern points to a credential-barrier model: the *cartilla militar* removes a bureaucratic obstacle to formal employment, allowing men who would otherwise work informally to access documented positions. In the framework of Levy (2008), the Sorteo functions as an involuntary formalization shock.

Comparison to other conscription studies. The international evidence on conscription predominantly finds negative earnings effects. Angrist (1990) estimates that Vietnam-era military service reduced earnings by 15% for white veterans, and Angrist et al. (2011) finds 5% annual earnings reductions 10 years after service. In Europe, Bauer et al. (2012) finds negative earnings effects in Germany, while Card and Cardoso (2012) finds small negative effects in the Netherlands. The Mexican result differs because the treatment is qualitatively different: six hours per week of Saturday training, rather than 12–24 months of full-time service. The positive formalization finding is unique to the developing-country context, where the distinction between formal and informal work is a first-order margin.

Policy implications. The credentialing interpretation suggests that Mexico's military lottery has an unintended formalization benefit. If the cartilla requirement is the operative channel, then policy reforms that either (a) extend the credential to all registrants regardless of ball color, or (b) eliminate the cartilla requirement for formal employment, would affect the formalization margin. The 2025 expansion to 95% active assignment effectively implements option (a), and future microdata will test whether the employment gap widens proportionally.

Limitations. This analysis has four main limitations. First, the DiD approach identifies the combined effect of all gender-specific age-18 events, not only the lottery. While I argue that other age-18 thresholds (legal majority, voting) are gender-neutral and show supporting evidence from pre-trends and the schooling placebo, gender-specific lifecycle changes at 18 (e.g., social norms about male breadwinning) could contribute to the estimated gap. A within-male design exploiting cross-municipality variation in treatment intensity, or individual-level lottery data when available, would provide cleaner identification. Second, the 40% treatment share used for LATE scaling is approximate—the exact share varies by year and state, and without individual assignment data, compliance rates are unverifiable. Third, without individual-level lottery data, I cannot distinguish the effects of active Saturday service from the effects of holding a white-ball cartilla credential—or from the act of registration itself. Fourth, the analysis covers only 2018–2019 (eight quarters). ENOE microdata from 2005–2017 use the same URL structure and could substantially extend the sample; post-2020 data require the redesigned ENOEN format. The current window is sufficient for cross-sectional identification but limits assessment of long-run persistence and business-cycle robustness.

7. Conclusion

Mexico's Sorteo Militar has randomly assigned one million young men per year to Saturday military training since 1942. This paper provides the first evidence that this lottery affects labor markets. The mechanism is formalization, not skill acquisition: the military credential unlocks access to formal-sector jobs, raising employment by 13.6 percentage points without changing wages. The finding inverts the standard conscription narrative from "service destroys civilian careers" to "service opens formal doors"—a distinction that matters in economies where the formal-informal margin is the central labor market divide.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Employment	0.1362	0.0081	0.4972	0.2739	0.0162	Large positive
Formal Emp.	0.1156	0.0071	0.4198	0.2752	0.0169	Large positive
Ln Earnings	-0.0057	0.0178	0.6822	-0.0084	0.0260	Small negative
Weekly Hours	1.6915	0.3618	15.8363	0.1068	0.0228	Moderate positive
In School	-0.0001	0.0002	0.0150	-0.0043	0.0136	Null

Notes: Standardized effect sizes from the Male × Post-Lottery ITT specification (Table 4). SDE $= \hat{\beta}/\mathrm{SD}(Y)$. Classification based on SDE magnitude: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005). Classification refers to effect magnitude, not statistical significance. Data: INEGI ENOE quarterly labor force survey, pooled 2018–2019. Sample: ages 15–30, both genders (N = 835,735). Identification: male–female DiD at the Sorteo Militar eligibility threshold (age 18). Treatment share ≈ 0.40 (pre-2025).

A. Data Appendix

Data source. All microdata come from INEGI's Encuesta Nacional de Ocupación y Empleo (ENOE), accessed via direct download from https://www.inegi.org.mx/programas/enoe/15ymas/. I download the quarterly ZIP files for 2018Q1 through 2019Q4, each containing the SDEMT (sociodemographic) module. The SDEMT file contains both demographic variables (age, sex, education) and derived labor market variables (employment status, earnings, hours, social security access, occupational position).

Sample restrictions. Starting from 1,049,626 person-quarter observations aged 15–35, I restrict to ages 15–30 and drop observations with missing sex, yielding 835,735 observations.

Variable definitions. *Employed*: clase1 = 1. *Formal*: seg_soc = 1 (has social security access), set to 0 for non-employed. *Salaried*: pos_ocu = 3, set to 0 for non-employed. *Monthly earnings*: ingocup, conditional on employment and positive values. *Weekly hours*: hrsocup, conditional on employment and positive values. *Years of education*: anios_esc. *In school*: cs_p13_1 = 1. *State*: 32 states from the *ent* variable.

B. Standardized Effect Sizes