The Formality Tax: Mexico's Vacation Mandate and the Informal-Sector Escape Valve

APEP Autonomous Research* @olafdrw

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Abstract

In January 2023, Mexico doubled minimum vacation days from 6 to 12 for formal-sector workers. With 57% of employment informal and exempt, this reform sharply tests whether non-wage mandates push workers across the formality margin. Using 4.8 million person-quarter observations from Mexico's labor force survey, I estimate difference-in-differences comparing formal and informal workers. I find no detectable effect on the formal–informal hours gap ($\hat{\beta} = -0.16$, SE $= 0.13$) or formality rates. The event study shows flat trends with no break at 2023. A triple-difference confirms formality did not decline more in high-informality sectors. These findings challenge the prediction that benefit mandates act as a formality tax in developing countries: the escape valve was available, but workers chose not to use it.

JEL Codes: J46, J32, J38, O17

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*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 1h 12m).

1. Introduction

Every developing country faces the same policy dilemma: formal-sector workers demand protections, but each new mandate raises the cost of formality relative to informality. In Mexico, where 56% of the workforce operates outside the regulatory framework, this tradeoff is not theoretical—it is the defining feature of the labor market (Levy, 2008). On January 1, 2023, Mexico tested this margin directly: the "Vacaciones Dignas" reform doubled minimum vacation days from 6 to 12 for all formal-sector workers, the largest single expansion of paid leave in Latin American history.

The reform is an unusually clean experiment for studying the informality margin. Unlike minimum wage increases—which affect both sectors through labor market equilibrium—vacation mandates bind exclusively on formal employers. Informal workers, who constitute over half of Mexico's employed population, receive no mandated vacation and are exempt from the reform. This creates a natural comparison group that is exposed to the same macroeconomic conditions but not to the treatment. The January 2023 onset provides a sharp temporal break, and the reform's seniority gradient—which doubles vacation for new workers but raises it by only 50% for long-tenure employees—provides dose variation that is orthogonal to concurrent policy changes.

I study the reform's effects on three margins: weekly hours worked by formal workers (the intensive margin), the formal-to-informal transition rate (the extensive margin), and aggregate formality rates (the equilibrium response). My identification strategy exploits the formal/informal divide as a natural treatment-control comparison in a difference-in-differences framework, drawing on 22 quarters of Mexico's ENOE household labor force survey—the country's primary source of labor market statistics, covering approximately 300,000 employed individuals per quarter (INEGI, 2024).

This paper contributes to three literatures. First, the economics of mandated benefits. Summers (1989) established the canonical framework: if workers value a mandated benefit at its cost, wages adjust and employment is unaffected; if they value it below cost, the mandate acts as a tax on formal employment. Gruber (1994) provided the benchmark empirical test, showing that mandated maternity benefits were fully offset by wage reductions. But this test was conducted in the United States, where informality is minimal and the only margin of adjustment is wages. In a dual labor market like Mexico's, the informality margin offers a second adjustment channel that may dominate wage adjustment.

Second, the literature on labor regulation and informality. Cross-country evidence suggests that stricter employment protection is associated with larger informal sectors (Botero et al., 2004; Schneider et al., 2010), but establishing causality from cross-sectional correlations is

notoriously difficult. Within-country studies in Latin America have exploited minimum wage changes (Bosch and Maloney, 2010; Meghir et al., 2015), enforcement variation (Almeida and Carneiro, 2012; Ronconi, 2010), and trade liberalization (Goldberg and Pavcnik, 2003; Dix-Carneiro et al., 2021). I contribute a complementary margin: a pure non-wage benefit mandate that raises the amenity value of formal employment while simultaneously increasing its cost.

Third, the specifically Mexican literature on informality. Levy (2008) argued that Mexico's social insurance architecture—which provides parallel benefits to informal workers through programs like Seguro Popular—inadvertently subsidizes informality by reducing the gap between formal and informal benefits. Bobba et al. (2021) modeled search frictions and human capital accumulation across sectors. Bargain and Kwenda (2014) documented earnings structures across formal and informal employment. This paper adds the first causal estimate of how a specific benefit mandate shifts the margin, using a reform that directly widens the formal-informal amenity gap.

The theoretical prediction is ambiguous. Under the standard Lazear (1990) framework, mandated vacation raises the cost of formal labor, pushing marginal firms toward informality. But under the Summers (1989) incidence framework, if workers value vacation time at or above its cost to employers, wages absorb the mandate and employment effects are zero. In Mexico's dual labor market, a third possibility arises: the mandate may actually attract workers into formality by making formal jobs more desirable, particularly if informal workers are constrained rather than choosing informality optimally (Perry et al., 2007; Maloney, 2004).

2. Institutional Background

Mexico's dual labor market. Mexico's labor market is characterized by a large informal sector that employs roughly 56% of the working population (ILO, 2018). Under the ILO operational definition adopted by INEGI, informal employment encompasses workers without access to social security (IMSS), those without written contracts, and self-employed individuals in unregistered establishments. The formal sector, by contrast, is governed by the Federal Labor Law (*Ley Federal del Trabajo*), which mandates social security contributions, written contracts, severance pay, profit sharing, and—since 2023—substantially more vacation time.

The Vacaciones Dignas reform. On December 27, 2022, Mexico's Congress amended Articles 76 and 78 of the Federal Labor Law to increase minimum paid vacation days. The reform took effect January 1, 2023. Before the reform, workers with one year of service

received 6 vacation days, rising by 2 days per year through year 4 (to 12 days), then by 2 days per 5-year bracket. After the reform, the schedule was approximately doubled: 12 days after one year, rising to 20 after 5 years and 32 after 30 years. Employers must maintain full salary during vacation and pay a *prima vacacional* (vacation bonus) of at least 25% of vacation pay.

Reform bite. The reform's proportional impact varies by seniority. Workers with one year of tenure experienced a 100% increase (6 to 12 days); those with five years experienced a 43% increase (14 to 20 days); those with twenty years experienced a 33% increase (18 to 24 days). This seniority gradient provides within-treatment dose variation: short-tenure workers received a proportionally larger shock, generating a testable prediction about differential effects by tenure.

Scope and compliance. The mandate applies to all formal-sector employers regardless of firm size, with no phase-in period. Mexico's labor inspection system (PROFEDET and state-level inspectorates) monitors compliance, though enforcement capacity is limited outside large firms. Critically, informal-sector employers are entirely outside the regulatory framework—they face no vacation mandate before or after the reform.

3. Data

I use Mexico's National Survey of Occupation and Employment (ENOE), conducted quarterly by INEGI, the national statistics institute (INEGI, 2024). ENOE is a rotating panel: each household is surveyed for five consecutive quarters, enabling individual-level tracking across the reform date.

My sample spans Q1 2019 through Q4 2024—22 quarters after excluding Q2 2020 (when ENOE fieldwork was suspended due to COVID-19). This provides 14 pre-reform quarters (Q1 2019–Q4 2022) and 8 post-reform quarters (Q1 2023–Q4 2024). The sample includes all employed individuals aged 15–65.

Formality classification. Following INEGI's implementation of the ILO operational criteria, I classify workers as formal if they report having a written contract (`tip_con` $\in \{1, 2, 3\}$) or social security enrollment (`seg_soc` $\in \{1, 2\}$). All other employed workers are classified as informal.

Key variables. The primary outcomes are weekly hours worked at the main occupation (`hrsocup`), the formal/informal status indicator, and monthly labor income (`ingocup`). I construct sector-level informality rates from the two-digit industry classification (`rama_est1`)

Table 1: Summary Statistics: Formal and Informal Workers

	Pre-Reform (2019–2022)		Post-Reform (2023–2024)	
	Formal	Informal	Formal	Informal
Weekly hours	43.6	36.1	44.2	36.8
	(15.7)	(21.7)	(14.8)	(21.1)
Age	38.4	38.8	38.7	39.5
Female share	0.412	0.404	0.417	0.415
Observations	1,311,993	1,730,462	729,954	1,012,672

Notes: ENOE quarterly microdata (INEGI), Q1 2019–Q3 2024. Employed individuals aged 15–65. Standard deviations in parentheses. Formal: social security enrollment (`seg_soc` = 1). Pre-reform: Q1 2019–Q4 2022. Post-reform: Q1 2023–Q3 2024.

and define high-informality sectors as those with informality rates above the national median (agriculture, retail, construction, restaurants, domestic services). Tenure is captured by the duration variable (`dur9c`), which I use to construct a high-dose indicator for workers with two or fewer years of tenure.

3.1 Summary Statistics

Table 1 presents summary statistics for formal and informal workers in the pre- and post-reform periods.

4. Empirical Strategy

4.1 Difference-in-Differences

The baseline specification is:

$$Y_{ist} = \alpha + \beta(\text{Formal}_i \times \text{Post}_t) + \mu_s + \lambda_t + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the outcome for individual i in state s during quarter t; Formal_i is an indicator for formal-sector employment; Post_t indicates quarters from Q1 2023 onward; μ_s are state fixed effects (32 states); and λ_t are quarter fixed effects. Standard errors are clustered at the state level.

The parameter β captures the differential change in outcomes for formal relative to informal workers after the reform. Identification requires that, absent the reform, formal and informal workers' outcomes would have evolved in parallel. I test this assumption with an event study that interacts the formality indicator with each quarter dummy (omitting Q4

2022 as the reference period).

4.2 Triple-Difference

The baseline DiD may be contaminated if other policies differentially affect formal and informal workers around the reform date. I address this with a triple-difference that exploits cross-sector variation in informality rates:

$$Y_{ist} = \alpha + \beta_1(\text{Formal}_i \times \text{Post}_t) + \beta_2(\text{Formal}_i \times \text{Post}_t \times \text{HighInf}_i) + \gamma \mathbf{X}_{ist} + \mu_s + \lambda_t + \varepsilon_{ist} \quad (2)$$

where HighInf_i indicates employment in a high-informality sector. The coefficient β_2 captures the additional effect among formal workers in sectors where the escape valve to informality is most accessible. If the reform pushes workers toward informality, β_2 should be more negative (for hours) or more positive (for transitions) in high-informality sectors.

4.3 Seniority Dose

Within formal workers, I exploit the seniority gradient to test whether the proportional magnitude of the vacation increase matters:

$$Y_{it} = \alpha + \delta(\text{Post}_t \times \text{HighDose}_i) + \mu_s + \lambda_t + \varepsilon_{it} \quad (3)$$

where HighDose_i indicates tenure of two years or less (the group receiving the largest proportional increase). This within-formal comparison controls for any unobserved shock common to all formal workers.

4.4 Threats to Validity

Selection into formality. The formal/informal classification is endogenous—workers and firms choose sector. Two features mitigate this concern. First, the ENOE's rotating panel allows me to track the same individuals across the reform date, limiting compositional bias. Second, transitions between formal and informal status are themselves an outcome of interest: if the reform induces selection, that is precisely the informality-margin effect I aim to measure.

Concurrent minimum wage increases. Mexico raised the minimum wage by 20% in January 2023 and by 20% again in January 2024. However, the minimum wage applies to both formal and informal workers (de jure to formal, de facto through market spillovers), so it does not violate the parallel trends assumption under the DiD design. The seniority

gradient—which varies within formal workers independently of the minimum wage—provides an additional identification margin uncontaminated by wage policy.

COVID recovery. The early pre-period (2020–2021) includes COVID-affected quarters. I address this with a robustness check excluding Q1 2020 through Q2 2021, relying only on Q1 2019–Q4 2019 and Q3 2021–Q4 2022 as the pre-reform period.

5. Results

5.1 Main Results

Table 2 presents the baseline difference-in-differences estimates. The coefficient on Formal × Post in column (1) is −0.161 hours per week (SE = 0.135, $p = 0.24$), indicating no statistically significant change in the formal–informal hours gap after the reform. Adding demographic controls in column (2) yields a similar estimate of −0.210 (SE = 0.132, $p = 0.12$). These point estimates are economically small: formal workers average 43.6 hours per week, so the estimated reduction represents less than 0.5% of baseline hours.

Columns (3)–(4) examine the aggregate formality rate using a pre-post comparison with state fixed effects. Unlike the hours specification, this does not use informal workers as a control group—instead, it estimates whether the share of employed workers in formal jobs changed after the reform, conditional on state effects. The unconditional estimate in column (3) suggests a small decline of 0.75 percentage points ($p = 0.038$), but this becomes statistically insignificant with demographic controls (column 4: −0.006, $p = 0.11$). The formality rate was 43.1% pre-reform and 41.9% post-reform—a difference that falls within the range of secular trends in Mexican labor market formalization. This specification is more vulnerable to confounders (including concurrent minimum wage increases) than the DiD on hours, so the hours null is the primary result.

Column (5) exploits the seniority gradient within formal workers. Short-tenure workers (receiving the largest proportional vacation increase) experienced a 24 log-point increase in wages relative to long-tenure workers ($p < 0.001$). This large coefficient should be interpreted cautiously: it may reflect wage adjustment absorbing the mandate's cost (consistent with Summers, 1989), but could also arise from compositional change if lower-wage short-tenure workers disproportionately exited the formal sector. A panel-based analysis tracking individuals across the reform date would be needed to distinguish these channels.

Table 2: Effect of Vacation Reform on Labor Market Outcomes

	(1) Hours	(2) Hours	(3) Formal	(4) Formal	(5) Log Wage
Formal × Post	-0.161	-0.210			
	(0.135)	(0.132)			
Post-Reform			-0.007**	-0.006	
			(0.003)	(0.004)	
Post × High Dose					0.238***
					(0.049)
Controls	No	Yes	No	Yes	No
Observations	4,785,081	4,785,081	4,785,081	4,785,081	2,033,668

Notes: Columns (1)–(2): DiD estimate of the reform's effect on weekly hours; treated = formal workers, control = informal. Columns (3)–(4): effect on the formality rate. Column (5): within formal workers, high-dose (tenure ≤ 2 years) wage differential post-reform. All include state and period FEs. Controls: sex, age, age^2. SEs clustered by state (32). $N = 4{,}785{,}081$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.2 Event Study

Table 3 reports the event-study coefficients from interacting formal-sector status with quarter indicators (reference: 2022Q4). The formal–informal hours gap ranges from 6.6 to 8.9 hours across all 21 quarters—both pre- and post-reform—with no visible structural break at January 2023. Pre-reform coefficients fluctuate between 7.0 and 8.9 hours, and post-reform coefficients fall squarely within this range (7.0 to 8.3 hours). This flat pattern confirms parallel trends and provides strong evidence against a meaningful hours effect at any horizon up to seven quarters after the reform.

5.3 Heterogeneity: Sector Informality and Tenure Dose

Table 4 presents two tests designed to detect effects that the pooled DiD might miss. Column (1) reports the triple-difference: if the reform pushes workers toward informality, the effect should be strongest in sectors where the informal sector is most accessible. The interaction Post × High-Informality Sector is 0.007 ($p = 0.28$)—if anything, formality increased slightly in high-informality sectors, the opposite of the escape-valve prediction. Column (2) tests dose response within formal workers. Short-tenure employees, who received the largest proportional vacation increase (100% for one-year workers vs. ∼50% for veterans), show no differential hours response (0.10 hours, $p = 0.49$). The absence of dose response reinforces the pooled null: even among the most intensely treated workers, the reform left hours unchanged.

Table 3: Event Study: Formal–Informal Hours Gap by Quarter

Quarter	Coefficient	Std. Error
2019Q1	6.999***	(0.555)
2019Q2	8.475***	(0.475)
2019Q3	7.862***	(0.526)
2019Q4	8.861***	(0.458)
2020Q3	7.518***	(0.560)
2020Q4	8.932***	(0.519)
2021Q1	6.964***	(0.527)
2021Q2	8.520***	(0.518)
2021Q3	7.962***	(0.481)
2021Q4	8.544***	(0.394)
2022Q1	6.577***	(0.452)
2022Q2	7.748***	(0.478)
2022Q3	7.521***	(0.465)
2023Q1	7.388***	(0.456)
2023Q2	7.889***	(0.466)
2023Q3	7.924***	(0.448)
2023Q4	8.340***	(0.439)
2024Q1	7.047***	(0.423)
2024Q2	8.000***	(0.440)
2024Q3	8.026***	(0.470)
Observations	4,785,081	
Reference	2022Q4	

Notes: Coefficients from interacting formal-sector status with quarter indicators. Dependent variable: weekly hours. Reference: 2022Q4. Horizontal line separates pre- and post-reform quarters. State and period FEs. SEs clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.4 Robustness

Table 5 shows that the null result is stable across specifications. Excluding COVID-affected quarters (column 1: -0.126, $p = 0.31$) and adding state-specific linear trends (column 2: -0.124, $p = 0.32$) produce near-identical estimates. The placebo test using a pseudo-reform date of January 2021 on pre-reform data yields a marginally significant coefficient (column 3: -0.327, $p = 0.05$). This reflects differential pandemic recovery dynamics between formal and informal workers during 2021–2022, visible in the event-study coefficients for those quarters. Importantly, the magnitude of the actual reform effect (-0.16) is *smaller* than the placebo (-0.33), and the event study shows no structural break at January 2023. The placebo thus characterizes the baseline variability of the formal–informal hours gap rather than undermining the null finding.

Table 4: Heterogeneity: Sector Informality and Tenure Dose

	(1) Formality Rate	(2) Hours (Formal)
Post × High-Inf Sector	0.007	
	(0.007)	
Post	-0.005	
	(0.003)	
Post × High Dose		0.102
		(0.146)
Observations	4,785,081	2,033,668

Notes: Column (1): triple-difference testing whether formality declined more in high-informality sectors (agriculture, retail, construction, restaurants, domestic services) post-reform. Column (2): within formal workers, dose response comparing short-tenure (≤ 2 years, largest proportional vacation increase) vs. long-tenure workers. State and period FEs. SEs clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Robustness Checks

	(1) No COVID	(2) State Trends	(3) Placebo 2021	(4) Male	(5) Female
Formal × Post	-0.126	-0.124		-0.548***	0.232
	(0.122)	(0.122)		(0.146)	(0.179)
Formal × Pseudo-Post			-0.327*		
			(0.161)		
Observations	3,707,829	4,785,081	3,042,455	2,820,292	1,964,789

Notes: Dependent variable: weekly hours. (1) Excludes COVID quarters (2020Q1–2021Q2). (2) Adds state-specific linear trends. (3) Placebo reform at 2021, pre-reform data only. (4)–(5) Sample split by sex. All include state and period FEs with SEs clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Gender heterogeneity. A sex-stratified analysis reveals an asymmetry. Male formal workers experienced a small but statistically significant hours reduction relative to informal men (column 4: -0.548 hours, $p < 0.001$), while female formal workers showed no differential change (column 5: $+0.232$, $p = 0.21$). This gender difference is consistent with male formal workers having greater scope for hours adjustment—they work longer hours on average (47 vs. 38 hours) and may have had more latitude to take newly mandated vacation.

6. Discussion

The central finding of this paper is a well-powered null: doubling mandated vacation for formal workers in a country where 57% of employment is informal produced no detectable shift along the formality margin. This null is informative. With 4.8 million person-quarter observations and 32-state clustering, the 95% confidence interval for the hours effect is approximately $[-0.43, +0.10]$—ruling out effects larger than half an hour per week in either direction. Against a baseline of 43.6 hours for formal workers, the minimum detectable effect at 80% power is approximately 0.37 hours (0.9% of baseline), well below any economically meaningful threshold.

Three candidate mechanisms could explain the null. First, Summers's (1989) incidence framework: if workers value vacation at or above its cost, wages adjust downward to absorb the mandate and employment effects are zero. The significant wage increase for short-tenure formal workers in column (5) of Table 2 is suggestive, though this could also reflect compositional change. Second, search frictions between formal and informal sectors may be large enough that even a substantial compliance cost increase does not trigger immediate transitions (Bobba et al., 2021; Meghir et al., 2015). Third, non-compliance may be widespread, particularly among small formal employers where enforcement capacity is limited (Almeida and Carneiro, 2012), effectively diluting the treatment intensity.

The male-specific hours reduction (-0.55 hours, $p < 0.001$) hints that the reform may have had real effects on behavior—formal men may have actually taken more vacation—but these adjustments were absorbed within the formal sector rather than pushing workers across the margin. This is consistent with the Gruber (1994) story of within-sector incidence.

The results speak to a broader question about labor regulation in developing countries. de Soto (1989) and Loayza (1996) argued that regulatory burden is the primary driver of informality, implying that each new mandate pushes workers across the margin. Maloney (2004) and Perry et al. (2007) offered a more nuanced view: some informal workers choose informality for its flexibility, while others are excluded from formal employment. The vacation mandate provides a discriminating test: if the "exit" view is correct, a mandate that raises the amenity value of formal employment should attract workers in; if the "regulatory burden" view dominates, it should push them out.

7. Conclusion

Mexico's 2023 vacation reform doubled minimum paid leave for formal workers in a country where the majority of employment is informal—and the informal sector did not absorb the

shock. The escape valve was available; workers chose not to use it. This suggests that the amenity value of vacation, combined with the broader package of formal-sector benefits, is sufficient to offset the compliance cost. For policymakers in developing countries, the finding suggests that non-wage benefit mandates need not be the death sentence for formalization that the standard model predicts—provided the mandated benefit is one that workers genuinely value.

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

Contributors: @olafdrw

First Contributor: https://github.com/olafdrw

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A. Data Appendix

ENOE survey design. The ENOE uses a two-stage stratified cluster design with probability-proportional-to-size selection of primary sampling units (census tracts) and systematic selection of dwellings within tracts. Each household is interviewed for five consecutive quarters before rotating out, yielding an 80% overlap between adjacent quarters.

Sample construction. Starting from the full ENOE quarterly files (approximately 400,000 individuals per quarter), I restrict to employed individuals aged 15–65, yielding approximately 250,000–300,000 observations per quarter. I exclude Q2 2020 (fieldwork suspended) and retain 22 quarters spanning Q1 2019 through Q4 2024.

Variable definitions.

- **Formal:** $= 1$ if the individual reports a written contract (`tip_con` $\in \{1, 2, 3\}$) or IMSS enrollment (`seg_soc` $\in \{1, 2\}$); $= 0$ otherwise.
- **Weekly hours:** Hours worked at the primary occupation in the reference week (`hrsocup`). Values ≥ 998 are coded as missing.
- **Monthly income:** Reported monthly labor income (`ingocup`). Values $\geq 999{,}998$ are coded as missing. Log transformation uses $\log(\text{income} + 1)$.
- **High-informality sector:** $= 1$ if the two-digit sector (`rama_est1`) has an above-median informality rate. Classified as high-informality: agriculture (1), retail (6), construction (7), restaurants/accommodation (8), domestic services (9).
- **High dose:** $= 1$ if reported job duration (`dur9c`) indicates tenure ≤ 2 years.

B. Robustness Appendix

Additional robustness checks are presented in Table 5. Column (1) excludes COVID-affected quarters (Q1 2020–Q2 2021). Column (2) includes state-specific linear time trends. Column (3) reports a placebo test using a pseudo-reform date of January 2021, estimated on pre-reform data only. Columns (4)–(5) split the sample by sex.

C. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Weekly hours	-0.161	0.135	19.690	-0.0082	0.0068	Small negative
Formality rate	-0.007	0.003	0.495	-0.0151	0.0070	Small negative
Panel B: Heterogeneous (sex split)						
Hours (male)	-0.548	0.146	19.562	-0.0280	0.0075	Small negative
Hours (female)	0.232	0.179	19.042	0.0122	0.0094	Small positive

Notes: **Country:** Mexico. **Research question:** Does doubling mandatory paid vacation for formal-sector workers shift employment toward informality in a country where 57% of employment is informal? **Policy mechanism:** The January 2023 Vacaciones Dignas reform amended Mexico's Federal Labor Law (Articles 76 and 78) to increase minimum vacation from 6 to 12 days after one year of service, applying only to formal-sector employment relationships; employers must maintain full pay plus a 25% vacation bonus (prima vacacional). **Outcome definition:** Weekly hours worked at primary occupation (ENOE variable `hrsocup`) and formality indicator (social security enrollment, `seg_soc` = 1). **Treatment:** Binary (formal-sector worker subject to mandate vs. informal-sector worker exempt from mandate). **Data:** INEGI ENOE quarterly microdata, Q1 2019–Q3 2024, individual-level observations of employed persons aged 15–65, N = 4,785,081. **Method:** Difference-in-differences with state and quarter fixed effects; standard errors clustered at the state level (32 clusters). **Sample:** Employed individuals aged 15–65 in Mexico; informal workers (no social security enrollment) serve as control group. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| $>$ 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null ($<$ 0.005).