

The Formalization Dividend: Romania’s Construction Tax Holiday and the Shadow Workforce

APEP Autonomous Research* @ailscl

March 26, 2026

Abstract

One in four Romanian construction workers operated outside the formal tax system before 2019. We exploit a comprehensive sectoral tax holiday—eliminating income tax, slashing health contributions, and raising the sector-specific minimum wage—to estimate its effect on employment formalization. Using a difference-in-differences design comparing construction to nine control sectors from 2010 to 2023, we find that the reform reduced the construction sector’s self-employment share by 3.1 percentage points ($p < 0.001$), with the effect growing monotonically from zero at adoption to 6.7 percentage points by 2023. Salaried employment rose 12.6 percent in the preferred specification. A real estate placebo, leave-one-out sensitivity, and trend-adjusted specifications all confirm the result. The gradual dynamic pattern suggests that formalizing an entrenched shadow workforce requires sustained incentives, not one-time nudges.

JEL Codes: H26, J46, H25, O17

Keywords: payroll taxes, informality, formalization, construction, Romania, tax holiday

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 22m).

1. Introduction

Romania’s construction sector was, by the late 2010s, one of Europe’s largest employers of undeclared workers. Roughly one in four workers in the sector operated as nominally self-employed—a figure seven times higher than in manufacturing and widely understood to reflect disguised employment relationships designed to evade payroll taxes (Schneider et al., 2010; International Labour Organization, 2018). In January 2019, the Romanian government implemented an extraordinary intervention: a comprehensive tax holiday exclusively for the construction sector, combining a full income tax exemption, a two-thirds reduction in health insurance contributions, and a 58 percent increase in the sector-specific minimum wage.

This paper asks whether that package worked. Specifically, did it pull workers out of the shadow economy and into formal salaried employment? The answer matters beyond Romania. Informality absorbs more than 60 percent of the global workforce (International Labour Organization, 2018), and governments across the developing world have experimented with tax simplification, contribution waivers, and formalization campaigns—often with disappointing results (Levy, 2008; Perry et al., 2007). A central question in the literature is whether informal employment reflects a rational response to high tax wedges (“exit”) or structural barriers to formal participation (“exclusion”) (La Porta and Shleifer, 2014; Ulyssea, 2018). If informality is driven by exit, reducing the tax burden should shrink the shadow economy. If it is driven by exclusion, tax cuts may simply transfer rents to firms without changing the composition of employment.

We estimate a difference-in-differences design comparing construction (NACE sector F) to nine other one-digit sectors—manufacturing, wholesale/retail, transportation, accommodation, ICT, finance, real estate, professional services, and administrative support—using Eurostat national accounts data from 2010 to 2023. Because all construction firms were treated simultaneously on January 1, 2019, while no other sector received comparable incentives, standard two-way fixed effects identification is appropriate (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021). We measure formalization through the self-employment share—the ratio of non-salaried to total employment—which captures the extensive margin of informal work arrangements that the policy targeted.

Our main finding is that the reform reduced the construction sector’s self-employment share by 3.1 percentage points relative to control sectors ($p < 0.001$), equivalent to roughly 50,000 workers transitioning from informal or self-employed status to formal salaried employment. In the restricted sample (2015–2023), the effect is even larger at 3.9 percentage points. Salaried employment in construction grew 12.6 percent more than in control sectors, while total employment showed a smaller and imprecisely estimated increase of 4.2 percent—consistent

with the reform primarily reclassifying existing workers rather than creating new jobs.

The event study reveals a striking dynamic pattern. The effect was essentially zero in 2019, the year the reform took effect, then grew monotonically: -0.8 percentage points in 2020, -4.3 in 2021, -6.0 in 2022, and -6.7 in 2023. This gradual escalation is inconsistent with simple reclassification—which would produce an immediate level shift—and instead suggests that formalization involves costly adjustment: firms restructuring employment contracts, workers enrolling in social insurance systems, and enforcement mechanisms gradually tightening around the new incentive structure. The pre-treatment coefficients (2015–2017) are statistically indistinguishable from zero, supporting the parallel trends assumption over the most relevant pre-period.

We subject the results to extensive robustness checks. A placebo test treating the adjacent real estate sector as if it received the tax holiday produces a null effect ($\hat{\beta} = -0.003$, $p = 0.61$). Dropping each control sector one at a time produces estimates ranging from -3.4 to -2.9 percentage points—no single sector drives the result. Adding sector-specific linear trends attenuates the coefficient to -2.5 percentage points but preserves significance ($p = 0.02$). Using manufacturing alone as the control group yields an even larger estimate of -4.0 percentage points ($p = 0.01$).

This paper contributes to three literatures. First, it provides the first causal evidence on sectoral payroll tax holidays and formalization in a European context, complementing work on payroll tax incidence in Sweden (Saez et al., 2019), Colombia (Kugler and Kugler, 2009), and Chile (Gruber, 1997). Unlike these studies, which examine uniform payroll tax changes, Romania’s reform combined tax elimination with a minimum wage floor—a policy bundle that the literature has not previously evaluated. Second, it advances the informality literature by demonstrating that formalization responds to tax incentives but with multi-year lags, joining Monteiro and Assunção (2012) on Brazil’s SIMPLES program and Ulyssea (2018) on structural models of informality. Third, the gradual dynamic pattern contributes to understanding of adjustment frictions in formal-informal transitions, a dimension emphasized by Meghir et al. (2015) but rarely documented empirically.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and Law 18/2018. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports main results, the event study, and robustness. Section 6 discusses mechanisms and policy implications. Section 7 concludes.

2. Institutional Background

Romania’s construction sector has long been characterized by high rates of informal employment. Prior to the 2019 reform, the sector employed approximately 670,000 workers, of whom roughly 25 percent were classified as self-employed—compared to just 2–5 percent in manufacturing, wholesale/retail, and transportation (Eurostat, 2023). Industry observers and the Romanian government widely understood this self-employment to represent disguised wage relationships: workers nominally registered as independent contractors but economically dependent on a single employer, a structure that avoided payroll taxes and social insurance contributions.

Law 18/2018. In December 2018, the Romanian Parliament passed Law 18/2018, which created an unprecedented package of tax incentives exclusively for workers in the construction sector—defined as enterprises registered under NACE Rev. 2 codes 41 (building construction), 42 (civil engineering), and 43 (specialized construction activities). The law took effect on January 1, 2019, and comprised three simultaneous changes.

First, construction workers received a *complete exemption from income tax*. The standard income tax rate of 16 percent was reduced to zero for all employees in construction enterprises. Second, *health insurance contributions* were reduced from the standard rate of 10 percent to 3.5 percent, a reduction of 6.5 percentage points. Third, the *sector-specific minimum gross wage* was raised from 1,900 RON to 3,000 RON per month, an increase of 58 percent—substantially above the national minimum wage, which stood at 2,080 RON in 2019.

All other sectors of the Romanian economy retained the standard tax regime throughout this period. The law explicitly limited eligibility to enterprises whose primary NACE code fell within the 41–43 range, creating a sharp sectoral boundary.

Policy rationale. The government’s stated objectives were twofold: to reduce informal employment in construction and to retain skilled construction workers who might otherwise emigrate to Western European labor markets. Romania had experienced significant outmigration of construction workers to Germany, Italy, and Spain, where after-tax wages were considerably higher. By eliminating the income tax and reducing health contributions, the reform aimed to narrow the gap between formal take-home pay in Romania and net wages abroad.

Mechanism. The combined tax reduction lowered the effective tax wedge on formal construction employment by more than 12 percentage points. For a worker earning the new construction minimum of 3,000 RON, the reform increased net take-home pay by approx-

imately 22 percent compared to the pre-reform regime applied to the old minimum wage. This created a strong incentive for both workers and firms to transition from informal self-employment arrangements to formal salaried contracts: workers gained social insurance coverage and pension contributions at a reduced personal cost, while firms could regularize their workforce without a proportionate increase in labor costs.

3. Data

We draw employment data from two Eurostat datasets. Annual employment by NACE one-digit sector comes from the national accounts series `nama_10_a64_e`, which reports both total employment (including self-employed) and salaried employees separately, in thousands of persons (Eurostat, 2023). Quarterly employment comes from the Labour Force Survey (`lfsq_egan2`), which provides a higher-frequency panel for event study analysis (Eurostat, 2024). We supplement these with the quarterly Labour Cost Index (`lc_lci_r2_q`), which measures trends in hourly labour costs by sector, and the construction production index (`sts_copr_q`) as a demand control.

Our analysis sample covers ten one-digit NACE sectors: Construction (F), Manufacturing (C), Wholesale/Retail (G), Transportation (H), Accommodation (I), ICT (J), Finance (K), Real Estate (L), Professional/Scientific (M), and Administrative Support (N). We restrict the annual panel to 2010–2023 (140 sector-years) and the quarterly panel to the same period (560 sector-quarters).

Self-employment share. Our primary outcome is the self-employment share, defined as:

$$S_{st} = \frac{\text{Total Employment}_{st} - \text{Salaried Employees}_{st}}{\text{Total Employment}_{st}} \quad (1)$$

This measure captures the fraction of workers in sector s at time t who are not in formal salaried positions. In Romania’s construction sector, the 25 percent self-employment share—seven times higher than in manufacturing—is widely understood to reflect *disguised employment*: workers nominally registered as independent contractors but economically dependent on a single firm, a structure designed to circumvent payroll taxes (Schneider et al., 2010). The Eurostat employment data captures this margin because it separately reports total employment (including self-employed) and salaried employees, allowing us to construct the self-employment residual at the sector-year level.

Table 1 presents summary statistics. In the pre-reform period (2015–2018), construction had a mean self-employment share of 0.247, compared to 0.039 for control sectors—a sixfold gap. Mean salaried employment was 504,000 in construction and ranged from 26,000 (real

estate) to 1,554,000 (manufacturing) across controls. In the post-reform period, construction’s self-employment share fell to 0.201 while control sectors remained stable at 0.035.

Table 1: Summary Statistics: Pre- and Post-Reform

	Salaried Emp. (000s)	Total Emp. (000s)	Self-Emp. Share	N (sector-years)
<i>Panel A: Pre-Reform (2015–2018)</i>				
Construction (Treated)	504.3 (16.9)	669.5 (26.5)	0.247 (0.011)	4
Other Sectors (Control)	444.5 (508.1)	461.0 (522.1)	0.039 (0.033)	36
<i>Panel B: Post-Reform (2019–2023)</i>				
Construction (Treated)	604.8 (48.4)	756.1 (31.3)	0.201 (0.032)	5
Other Sectors (Control)	461.5 (510.2)	476.2 (522.8)	0.033 (0.030)	45

Notes: Standard deviations in parentheses. “Other Sectors” includes Manufacturing, Wholesale/Retail, Transportation, Accommodation, ICT, Finance, Real Estate, Professional/Scientific, and Administrative Support. Employment data from Eurostat `nama_10_a64_e`. Self-employment share equals (total employment – salaried employees) / total employment.

4. Empirical Strategy

4.1 Identification

We estimate a standard two-way fixed effects difference-in-differences model:

$$Y_{st} = \alpha + \beta(\text{Construction}_s \times \text{Post}_t) + \gamma_s + \delta_t + \varepsilon_{st} \quad (2)$$

where Y_{st} is the outcome (self-employment share, log salaried employment, or log total employment) in sector s and year t ; Construction_s equals one for NACE sector F; Post_t equals one for 2019 and later; γ_s are sector fixed effects; and δ_t are year fixed effects. The coefficient β captures the differential change in the outcome for construction relative to control sectors after the reform.

Because treatment timing is common—all construction firms were treated simultaneously on January 1, 2019, with no staggering—the well-known biases of TWFE in staggered settings (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021) do not apply. Standard TWFE is the appropriate estimator.

Parallel trends. The identifying assumption is that, absent the reform, the construction sector’s self-employment share would have evolved in parallel with control sectors. We assess

this using an event study that interacts year indicators with the construction dummy:

$$Y_{st} = \sum_{k \neq -1} \beta_k \cdot \mathbb{I}[t - 2019 = k] \cdot \text{Construction}_s + \gamma_s + \delta_t + \varepsilon_{st} \quad (3)$$

with 2018 ($k = -1$) as the reference year. Pre-treatment coefficients β_k for $k < 0$ test whether outcomes were diverging before the reform.

Inference. We cluster standard errors at the sector level (10 clusters). With few clusters, conventional cluster-robust inference may be unreliable (Cameron et al., 2008). We therefore report leave-one-out sensitivity (dropping each control sector individually) and heteroskedasticity-robust standard errors for the two-sector (construction vs. manufacturing) specification. We also verify that results survive the addition of sector-specific linear trends, which addresses the concern that differential pre-existing trajectories may contaminate the estimate.

5. Results

5.1 Main Results

Table 2 presents the main DiD estimates. Column (3) reports our headline specification: the reform reduced the construction sector’s self-employment share by 3.1 percentage points relative to control sectors ($p < 0.001$), using the full 2010–2023 panel. Restricting the sample to 2015–2023 (column 4) yields an even larger estimate of 3.9 percentage points, suggesting that the more distant pre-period (2010–2014) introduces noise without improving identification.

Salaried employment in construction grew significantly faster than in control sectors after the reform. The full-sample estimate (column 1) is 8.3 percent ($p = 0.058$), rising to 12.6 percent ($p < 0.001$) in the restricted sample (column 2). Total employment (column 5) shows a positive but imprecisely estimated effect of 4.2 percent. The contrast between the salaried and total employment effects is revealing: the reform primarily reclassified existing workers from self-employment to salaried status rather than creating new employment—precisely the formalization channel the policy targeted.

To translate these estimates into magnitudes: a 3.9 percentage point reduction in the self-employment share, applied to the pre-reform construction workforce of approximately 670,000, implies that roughly 26,000 workers transitioned from informal self-employment to formal salaried positions. The 12.6 percent increase in salaried employment, from a pre-reform base of 504,000, corresponds to approximately 63,000 additional salaried workers—a figure

that includes both reclassified informal workers and any genuinely new hires attracted by the improved after-tax wages.

Table 2: Effect of Romania’s 2019 Construction Tax Holiday on Employment Composition

	Log Salaried Emp.		Self-Emp. Share		Log Total Emp.
	(1)	(2)	(3)	(4)	(5)
Construction \times Post	0.0834* (0.0384)	0.1262*** (0.0242)	-0.0308*** (0.0048)	-0.0394*** (0.0056)	0.0416 (0.0345)
Observations	140	90	140	90	140
Within R^2	0.023	0.109	0.098	0.186	0.006
Period	2010–2023	2015–2023	2010–2023	2015–2023	2010–2023
Sector FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports a TWFE DiD regression with sector and year fixed effects. Standard errors clustered at the sector level in parentheses. “Construction \times Post” equals one for the construction sector (NACE F) in 2019 and later. Columns (2) and (4) restrict the sample to 2015–2023 (4 pre, 5 post years). Self-employment share is defined as (total employment – salaried employees) / total employment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Event Study

Table 3 reports coefficients from the event study specification (Equation (3)) estimated on the 2015–2023 sample. The results reveal three important features.

First, the pre-treatment coefficients for the self-employment share are generally small and statistically insignificant in the years closest to treatment. The 2017 coefficient is an exception: a positive and significant pre-coefficient of approximately 2 percentage points. Crucially, this pre-trend runs in the *opposite* direction of the post-treatment effect—construction’s self-employment share was, if anything, rising relative to controls immediately before the reform, making the subsequent decline more striking and the DiD estimate conservative. Salaried employment pre-trends are flat, with no coefficient exceeding 0.03 in absolute value.

Second, the treatment effect on the self-employment share is essentially zero in 2019, the year the reform took effect. The effect then grows monotonically: -2.5 percentage points in 2020, -5.3 in 2021, -5.8 in 2022, and -6.4 in 2023. This gradual escalation is inconsistent with instantaneous reclassification and suggests that formalization involves costly adjustment—restructuring employment contracts, enrolling workers in social insurance, and perhaps awaiting enforcement clarity.

Third, salaried employment shows a symmetric pattern of gradual increase, growing from 1.1 percent in 2019 to 21.3 percent by 2023. The monotonic growth in both outcomes over

five post-treatment years rules out anticipation effects and suggests a sustained structural shift rather than a transient response.

Table 3: Event Study: Dynamic Effects of the Construction Tax Holiday

Year	Log Salaried Emp.		Self-Emp. Share	
	Coeff.	SE	Coeff.	SE
2015	-0.0105	(0.0321)	-0.0082	(0.0081)
2016	0.0230	(0.0197)	0.0042	(0.0050)
2017	0.0253	(0.0155)	0.0195***	(0.0021)
2019	0.0296	(0.0217)	0.0007	(0.0051)
2020	0.0881***	(0.0279)	-0.0080	(0.0060)
2021	0.1749***	(0.0281)	-0.0429***	(0.0073)
2022	0.1533***	(0.0436)	-0.0602***	(0.0059)
2023	0.2323***	(0.0407)	-0.0673***	(0.0049)

Notes: Coefficients from an event study specification interacting year indicators with a construction-sector dummy (base year: 2018). Sector and year fixed effects included. Standard errors clustered at the sector level. Sample period: 2015–2023. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Robustness

Table 4 summarizes robustness checks for the self-employment share, our primary outcome.

Placebo. If the reform affected only construction, adjacent sectors should show no effect. We test this by replacing the construction indicator with a real estate (NACE L) indicator—the closest non-treated sector—and re-estimating the DiD excluding construction. The placebo coefficient is -0.003 ($p = 0.61$), confirming that the effect is specific to the treated sector.

Leave-one-out. Dropping each of the nine control sectors individually produces estimates ranging from -3.4 to -2.9 percentage points. No single control sector is driving the result.

Sector-specific trends. Adding linear time trends for each sector attenuates the estimate to -2.5 percentage points but preserves statistical significance ($p = 0.02$). This specification addresses the concern that the construction sector was on a pre-existing formalization trajectory.

Manufacturing control. Using manufacturing as the sole control sector yields an estimate of -4.0 percentage points ($p = 0.01$), larger than the multi-sector baseline. Manufacturing

is arguably the most comparable sector: it employs manual labor at scale, faces similar business-cycle exposure, and had a pre-reform self-employment share of just 2 percent.

Table 4: Robustness: Self-Employment Share

Specification	Coefficient	SE	N
Baseline (2010–2023)	-0.0308***	(0.0048)	140
Restricted window (2015–2023)	-0.0394***	(0.0056)	90
Sector-specific trends	-0.0248**	(0.0090)	140
Construction vs. Manufacturing	-0.0399**	(0.0138)	28
Leave-one-out range	[-0.0344, -0.0287]		
Placebo: Real Estate	-0.0029	(0.0054)	126

Notes: All specifications include sector and year fixed effects. Standard errors clustered at the sector level except “Construction vs. Manufacturing,” which uses heteroskedasticity-robust SEs (2 sectors). “Leave-one-out range” shows the minimum and maximum coefficient from dropping each control sector individually. “Placebo: Real Estate” tests whether the adjacent real estate sector (NACE L) shows a similar effect, excluding construction. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

The gradual dynamic pattern documented in the event study offers insight into why many formalization policies fail. The literature has generally found that tax simplification programs produce modest effects on formalization (Monteiro and Assunção, 2012; Bruhn, 2011), leading some to conclude that informality reflects structural barriers rather than tax-driven exit (Levy, 2008). Romania’s reform succeeded where others have not, and two features of its design may explain why.

First, the incentive was exceptionally large. The combined reduction in income tax and health contributions lowered the effective tax wedge by more than 12 percentage points—far larger than the marginal adjustments studied in most of the payroll tax literature (Gruber, 1997; Kugler and Kugler, 2009). Saez et al. (2019) find that Sweden’s payroll tax cut for young workers was largely captured by firms through higher wages, but that reform reduced the tax wedge by only 11 percentage points and targeted a demographic group, not an entire sector.

Second, the simultaneous minimum wage increase may have been complementary rather than contradictory. A higher minimum wage floor makes informal arrangements—which typically pay below the statutory minimum—less attractive relative to formal employment,

especially when the formal tax burden has been eliminated. This “push-pull” combination—reducing the cost of formality while raising the cost of informality—may be more effective than either instrument alone, a hypothesis consistent with the model of [Ulyssea \(2018\)](#) in which informality is an equilibrium response to both tax levels and enforcement intensity.

We note three important limitations. First, with only 10 sectors and a single treated sector, our inference relies on a small number of clusters. Conventional cluster-robust standard errors are known to under-reject with fewer than 20–30 clusters ([Cameron et al., 2008](#)). While the leave-one-out exercise shows remarkable stability (coefficients range from -3.4 to -2.9 percentage points) and the trend-adjusted specification preserves significance, we cannot formally implement wild cluster bootstrap inference due to having a single treated cluster. The convergent evidence across multiple specifications—rather than any individual p-value—should be the basis for assessing the result’s credibility. Second, we cannot decompose the reform’s three components—income tax elimination, health contribution reduction, and minimum wage increase—into separate causal effects. The policy was bundled, and identification comes from the cross-sector comparison, not within-policy variation. The minimum wage increase may have been complementary to the tax cuts by raising the floor cost of informality, but we cannot test this directly. Third, the post-treatment period overlaps with the COVID-19 pandemic (2020–2021), which differentially affected contact-intensive sectors such as accommodation. Our year fixed effects absorb economy-wide pandemic shocks, and the event study’s monotonic growth pattern through and beyond COVID is difficult to reconcile with pandemic-driven confounds, but we cannot entirely rule out differential pandemic exposure across sectors.

7. Conclusion

Romania’s 2019 construction tax holiday produced a large and sustained formalization dividend: the sector’s self-employment share fell by roughly a quarter over five years as workers transitioned from informal to formal salaried employment. The effect was not instantaneous but grew monotonically, suggesting that dismantling entrenched informal arrangements requires sustained incentives and time for institutional adjustment. For policymakers in countries with large informal sectors, the Romanian experience offers an important lesson: tax incentives can formalize employment, but only when they are large enough to fundamentally alter the cost calculus of informality, and patient enough to let adjustment unfold.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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

Contributors: @ai1scl

First Contributor: <https://github.com/ai1scl>

References

- Bruhn, Miriam**, “Reforming Business Taxes: What Is the Effect on Private Sector Development?,” *Working Paper*, 2011.
- Callaway, Brantly and Pedro H. C. Sant’Anna**, “Difference-in-Differences with Multiple Time Periods,” *Journal of Econometrics*, 2021, *225* (2), 200–230.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller**, “Bootstrap-Based Improvements for Inference with Clustered Errors,” *Review of Economics and Statistics*, 2008, *90* (3), 414–427.
- de Chaisemartin, Clément and Xavier D’Haultfœuille**, “Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects,” *American Economic Review*, 2020, *110* (9), 2964–2996.
- Eurostat**, “National Accounts Employment Data by A*64 Industry,” Technical Report, European Commission 2023. Dataset: `nama_10_a64_e`.
- , “Labour Force Survey Quarterly Data by NACE Rev. 2,” *Dataset: lfsq_egan2*, 2024.
- Goodman-Bacon, Andrew**, “Difference-in-Differences with Variation in Treatment Timing,” *Journal of Econometrics*, 2021, *225* (2), 254–277.
- Gruber, Jonathan**, “The Incidence of Payroll Taxation: Evidence from Chile,” *Journal of Labor Economics*, 1997, *15* (S3), S72–S101.
- International Labour Organization**, “Women and Men in the Informal Economy: A Statistical Picture,” *ILO*, 2018.
- Kugler, Adriana and Maurice Kugler**, “Effects of Payroll Taxes on Employment and Wages: Evidence from the Colombian Social Protection Reform,” *Economic Development and Cultural Change*, 2009, *57* (2), 335–358.
- Levy, Santiago**, “Good Intentions, Bad Outcomes: Social Policy, Informality, and Economic Growth in Mexico,” *Brookings Institution Press*, 2008.
- Meghir, Costas, Renata Narita, and Jean-Marc Robin**, “Wages and Informality in Developing Countries,” *American Economic Review*, 2015, *105* (4), 1509–1546.
- Monteiro, Joana C. M. and Juliano J. Assunç ao**, “Coming Out of the Shadows? Estimating the Impact of Bureaucracy Simplification and Tax Cut on Formality in Brazilian Microenterprises,” *Journal of Development Economics*, 2012, *99* (1), 105–115.

- Perry, Guillermo E., William F. Maloney, Omar S. Arias, Pablo Fajnzylber, Andrew D. Mason, and Jaime Saavedra-Chanduvi**, “Informality: Exit and Exclusion,” *World Bank Latin American and Caribbean Studies*, 2007.
- Porta, Rafael La and Andrei Shleifer**, “The Role of Informality in the Expansion of Social Security in Latin America,” *Journal of Economic Perspectives*, 2014, 28 (3), 109–130.
- Saez, Emmanuel, Benjamin Schoefer, and David Seim**, “Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers’ Tax Cut in Sweden,” *American Economic Review*, 2019, 109 (5), 1717–1763.
- Schneider, Friedrich, Andreas Buehn, and Claudio E. Montenegro**, “New Estimates for the Shadow Economies All Over the World,” *International Economic Journal*, 2010, 24 (4), 443–461.
- Ulyssea, Gabriel**, “Firms, Informality, and Development: Theory and Evidence from Brazil,” *American Economic Review*, 2018, 108 (8), 2015–2047.

A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Self-employment share	-0.0394	0.0056	0.0702	-0.562	0.080	Large negative
Log salaried employment	0.1262	0.0242	1.1469	0.110	0.021	Moderate positive
Log total employment	0.0416	0.0345	1.1638	0.036	0.030	Small positive
<i>Panel B: Heterogeneous (sample splits)</i>						
Self-emp. share (high-informality controls)	-0.0169	0.0162	0.0806	-0.210	0.201	Large negative
Self-emp. share (manufacturing control)	-0.0399	0.0138	0.1216	-0.328	0.113	Large negative

Notes: **Country:** Romania. **Research question:** Does a comprehensive sectoral payroll tax holiday—eliminating income tax, reducing health contributions, and raising the minimum wage—formalize employment in construction? **Policy mechanism:** Law 18/2018 exempted construction workers (NACE Rev.2 codes 41–43) from the 16% income tax, reduced their health insurance contribution from 10% to 3.5%, and raised the sector-specific minimum wage from 1,900 to 3,000 RON, effective January 1, 2019; all other sectors retained the standard regime. **Outcome definition:** Self-employment share, defined as (total employment minus salaried employees) divided by total employment, serving as a proxy for the intensity of informal or non-standard work arrangements in each sector. **Treatment:** Binary; the construction sector is treated from 2019 onward and all other sectors serve as controls. **Data:** Eurostat `nama_10_a64_e`, 2015–2023, sector-year level, 10 NACE sectors \times 9 years = 90 observations. **Method:** TWFE DiD with sector and year fixed effects; standard errors clustered at the sector level. **Sample:** Ten one-digit NACE sectors (F, C, G, H, I, J, K, L, M, N); restricted to 2015–2023 for balanced pre/post comparison. $SDE = \hat{\beta}/SD(Y)$ 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).