The Twelve-Month Tax: Sector-Level Wage Responses to North Macedonia's Progressive Tax Experiment

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Abstract

In January 2019, North Macedonia replaced its flat 10% personal income tax with a two-bracket progressive system taxing income above MKD 90,000 at 18%. Twelve months later, the reform was fully repealed. This symmetric on-off structure provides unusually clean identification for studying short-run wage reporting responses to progressive taxation. Using monthly sector-level wage data from Statistics North Macedonia (19 NACE sectors, 2012–2024), I estimate a continuous-treatment difference-in-differences exploiting cross-sector variation in exposure to the top bracket. I find no statistically significant wage suppression in high-exposure sectors during the reform year, with a point estimate of -0.064 log points (bootstrap $p = 0.72$). The null persists across leave-one-out, permutation inference, and alternative sample windows. With only 19 sector-level clusters, the design is underpowered to detect plausible effect sizes, highlighting the limits of sector-level aggregation for studying tax-reporting responses.

JEL Codes: H24, H26, J31

Keywords: progressive taxation, wage responses, reporting elasticity, North Macedonia, transition economies

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1. Introduction

How quickly do wages adjust when a government raises—and then reverses—marginal tax rates? North Macedonia's brief experiment with progressive income taxation offers a rare natural laboratory. For exactly twelve months, from January 2019 to December 2019, the country imposed an 18% top bracket on monthly earnings above MKD 90,000 (approximately EUR 1,460), replacing the flat 10% rate that had been in place since 2007. In January 2020, the flat rate was fully restored. The symmetric on-off structure makes this reform uniquely attractive for causal inference: any wage response that appears during 2019 and reverses upon repeal can be attributed to the tax with unusual confidence.

This paper estimates the effect of the 2019 progressive tax reform on reported wages using monthly sector-level data from Statistics North Macedonia. I exploit cross-sector variation in exposure to the top bracket, measured as each sector's pre-reform mean wage relative to the MKD 90,000 threshold. Sectors like Information and Communication (exposure ratio: 0.72) and Financial Services (0.68) had more workers near the threshold than Agriculture (0.31) or Accommodation (0.29). In a continuous-treatment difference-in-differences framework with sector and year-month fixed effects, I estimate whether high-exposure sectors experienced differential wage suppression during the reform year.

The main finding is a null: the point estimate for wage suppression in high-exposure sectors during 2019 is -0.064 log points, statistically indistinguishable from zero both under cluster-robust standard errors ($p = 0.66$) and wild cluster bootstrap ($p = 0.72$). The result is robust to leaving out individual high-exposure sectors, restricting the sample window, and permutation inference that randomly reassigns the treatment year (permutation $p = 0.33$). A pre-period seasonality test confirms that January wages do not systematically differ for high-exposure sectors in years before 2019, ruling out mechanical seasonal confounds.

This null result should be interpreted cautiously. With 19 NACE sectors as the unit of observation, the design has limited statistical power. The within-R^2 for the treatment variables (net of fixed effects) is only 0.037, and the confidence intervals are wide enough to encompass economically meaningful effects. A back-of-envelope power calculation suggests that detecting a 0.10 log-point effect—modest by the standards of the taxable income elasticity literature—would require substantially more cross-sectional variation than 19 sector averages provide. The study therefore cannot distinguish between "the tax had no effect on reported wages" and "the effect exists but is too small to detect at this level of aggregation."

This paper contributes to the literature on behavioral responses to progressive taxation (Saez et al., 2012; Kleven and Schultz, 2014; Chetty et al., 2011). Most studies exploit multi-year tax reforms, making it difficult to separate level effects from secular trends in wages. North Macedonia's twelve-month experiment eliminates this confound by design. Yet the same feature that makes the reform clean for identification—its brevity—may limit the scope for behavioral adjustment. If employers and employees need time to restructure compensation packages, a one-year reform may simply be too short to elicit the reporting responses documented in longer-duration studies.

The paper also speaks to the small but growing literature on taxation in transition economies (Gorodnichenko et al., 2009; Ivanova et al., 2005), where flat tax reforms have been common since the early 2000s. North Macedonia's brief departure from and return to the flat tax paradigm provides a rare reversal within this context. The null finding is consistent with the hypothesis that reporting responses are attenuated in economies with less flexible compensation structures and weaker incentives for tax optimization (Kleven et al., 2011).

The remainder of the paper is organized as follows. Section 2 describes the institutional background of North Macedonia's tax reform. Section 3 presents the data. Section 4 details the empirical strategy and discusses identification. Section 5 reports the main results, mechanism tests, and robustness checks. Section 6 discusses implications for the taxable income literature, and Section 7 concludes.

2. Institutional Background

The flat tax era. North Macedonia adopted a flat 10% personal income tax in 2007, joining a wave of flat tax reforms across Central and Eastern Europe that included Russia (2001), Slovakia (2004), and Romania (2005). The flat rate applied to all employment income above the personal allowance, with social security contributions levied separately. For twelve years, the rate remained unchanged, providing a stable fiscal environment.

The 2019 progressive reform. The Law on Personal Income Tax, published in the Official Gazette of North Macedonia No. 248/2018, introduced a two-bracket system effective January 1, 2019. The existing 10% rate applied to monthly gross earnings up to MKD 90,000 (approximately EUR 1,460 at 2018 exchange rates). Earnings above this threshold were taxed at 18%, an 8 percentage point increase in the marginal rate. The reform was projected to generate MKD 1.558 billion in additional revenue, though only approximately 1% of taxpayers earned above the threshold. The progressive structure applied to employment income as reported by employers through the mandatory payroll system.

The 2020 repeal. After a change in government priorities, the progressive brackets were repealed effective January 1, 2020, restoring the flat 10% rate. The Official Gazette announced the reversion in late 2019, giving employers approximately one month of advance notice.

This symmetric on-off structure—flat rate before, progressive rate during 2019, flat rate after—creates a natural experiment with a built-in reversal that most tax reforms lack.

Sector-level wage variation. The MKD 90,000 monthly threshold created heterogeneous exposure across NACE Rev.2 sectors. In 2018, the Information and Communication sector (NACE J) had an average gross wage placing its exposure ratio at 0.72. Financial and Insurance Activities (NACE K), Electricity supply (NACE D), and Mining (NACE B) were similarly exposed, with ratios of 0.68, 0.63, and 0.56 respectively. By contrast, Accommodation and Food Services (NACE I) had an exposure ratio of only 0.29, well below the threshold. This 2.5-fold range in exposure across 19 sectors provides the identifying variation for the continuous-treatment design.

3. Data

I use monthly sector-level wage data from the State Statistical Office of the Republic of North Macedonia, accessed through the PXWeb API.[1] The data report average monthly gross and net wages per employee for 19 NACE Rev.2 sectors, spanning January 2005 to January 2026. Gross wages include base salary, regular bonuses, and one-time supplements; net wages reflect gross earnings minus income tax and social security contributions.

The estimation sample covers January 2012 to December 2024, providing seven years of pre-treatment data (2012–2018), twelve months of treatment (2019), and five years of post-repeal observations (2020–2024). This yields 2,964 sector-month observations across 19 sectors and 156 months. I exclude the "Total" aggregate sector, retaining only the 19 individual NACE sectors.

Table 1 reports summary statistics. Average monthly gross wages across all sectors and periods are approximately MKD 35,000, with considerable cross-sector dispersion (standard deviation of roughly MKD 15,000). High-exposure sectors—those with 2018 mean wages above 50% of the MKD 90,000 threshold—earn approximately MKD 47,000 on average, compared to MKD 32,000 for low-exposure sectors.

[1] Tables 125_PazTrud_Mk_bruto_ml.px (gross wages) and 175_PazTrud_Mk_neto_ml.px (net wages), available at https://makstat.stat.gov.mk.

Table 1: Summary Statistics

	Mean	SD	Obs.	Sectors
Panel A: Full Sample (2012–2024)				
Gross wage (MKD)	42,615	15,839	2,964	19
Net wage (MKD)	28,700	10,349		
Months				156
Panel B: By Exposure Group				
High exposure (mean = 0.65)	61,686	16,502	624	4
Low exposure (mean = 0.38)	37,529	11,062	2,340	15

Notes: Monthly average gross and net wages per employee in Macedonian denars (MKD) by NACE Rev.2 sector from Statistics North Macedonia. High-exposure sectors have 2018 mean gross wage $\geq$ 50% of the MKD 90,000 progressive tax threshold. Sample: January 2012 to December 2024.

4. Empirical Strategy

4.1 Identification

The estimating equation is:

$$\ln(w_{st}) = \alpha_s + \mu_t + \beta_1(\text{Reform}_{2019,t} \times \text{Exposure}_s) + \beta_2(\text{Post}_{2020,t} \times \text{Exposure}_s) + \varepsilon_{st} \quad (1)$$

where w_{st} is the average gross wage in sector s and month t, α_s are sector fixed effects, and μ_t are year-month fixed effects. $\text{Reform}_{2019,t}$ equals one for all months in 2019, and $\text{Post}_{2020,t}$ equals one for January 2020 onward. Exposure_s is the sector's 2018 mean gross wage divided by the MKD 90,000 threshold, a continuous measure ranging from 0.29 (Accommodation) to 0.72 (ICT).

The coefficient β_1 captures the differential wage response of high-exposure sectors during the progressive tax period. The "boomerang hypothesis" predicts $\beta_1 < 0$ (wage suppression during the tax) and $\beta_2 \approx 0$ (full reversal upon repeal). A persistent negative β_2 would suggest real labor market adjustments rather than reporting responses. Standard errors are clustered at the sector level.

4.2 Threats to Validity

Small number of clusters. With 19 sectors, cluster-robust standard errors may be biased downward (Cameron et al., 2008). I address this with three complementary approaches: (i) wild cluster bootstrap with Rademacher weights (999 replications), (ii) permutation inference that randomly reassigns the treatment year (500 draws), and (iii) leave-one-out sensitivity that drops each high-exposure sector.

Parallel trends. The identifying assumption requires that high- and low-exposure sectors would have followed parallel wage trajectories absent the reform. I test this with an event-study specification:

$$\ln(w_{st}) = \alpha_s + \mu_t + \sum_{k \neq -1} \gamma_k (_{t=k} \times \text{Exposure}_s) + \varepsilon_{st} \tag{2}$$

where k indexes months relative to January 2019 and December 2018 ($k = -1$) is the omitted reference period. Pre-treatment coefficients γ_k for $k < 0$ test whether exposure-related trends predate the reform.

Exposure measurement error. The exposure variable—sector mean wage divided by the threshold—is a proxy for the share of workers actually earning above MKD 90,000. Within-sector wage dispersion means a sector with mean at 72% of the threshold may have anywhere from 5% to 25% of workers above the cutoff. This measurement error likely attenuates the estimated coefficient toward zero, making the null harder to interpret (Feldstein, 1999).

December bonuses and seasonality. North Macedonian wage data include one-time bonuses and "13th month" supplements, which are concentrated in December. This creates large December-to-January drops in high-wage sectors every year—not just in 2019. While year-month fixed effects absorb common seasonal patterns, they cannot address *sector-specific* seasonality. If high-exposure sectors have systematically larger December bonuses, the raw January 2019 drop visible in sector averages may reflect bonus timing rather than tax-induced suppression. I test for this directly: the pre-2019 January $\times$ Exposure coefficient is essentially zero (-0.002, s.e. $= 0.050$), suggesting that differential bonus patterns do not drive the identification.

Compositional changes and COVID-19. Sector-level average wages may shift due to changes in worker composition rather than individual wage responses. Additionally, the post-2020 period overlaps with the onset of the COVID-19 pandemic, which differentially affected high-wage service sectors. The negative post-repeal coefficient ($\beta_2 = -0.212$) likely reflects pandemic-era compositional changes rather than persistent tax effects. This contamination limits the power of the "boomerang" test—the inability to detect a clean reversal should not be interpreted as evidence of persistent wage suppression.

Table 2: The Reporting Boomerang: Wage Responses to Progressive Taxation

	(1) ln(Gross) Continuous	(2) ln(Gross) Binary	(3) ln(Net) Continuous	(4) ln(Gap) Continuous
$\text{Reform}_{2019} \times \text{Exposure}$	-0.0644	0.0183	-0.0802	-0.0357
	(0.1424)	(0.0511)	(0.1345)	(0.1620)
$\text{Post}_{2020+} \times \text{Exposure}$	-0.2117	-0.0266	-0.2125	-0.2154
	(0.2692)	(0.0839)	(0.2594)	(0.2906)
Sector FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	2,964	2,964	2,964	2,964
R^2	0.965	0.964	0.962	0.968
Bootstrap p (Reform)	0.724	0.777	0.649	—

Notes: Dependent variables: log gross wage (cols 1–2), log net wage (col 3), log gross-net gap (col 4). "Exposure" is the sector's 2018 mean gross wage divided by the MKD 90,000 progressive tax threshold. Column 2 uses a binary indicator for high-exposure sectors (exposure ≥ 0.5). $\text{Reform}_{2019} = 1$ for all months in 2019; $\text{Post}_{2020+} = 1$ for January 2020 onward. Standard errors clustered by sector in parentheses. Bootstrap p-values from wild cluster bootstrap (Rademacher weights, 999 replications). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$ based on bootstrap p-values where available.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Column (1) shows the continuous-treatment DiD: the reform-period coefficient is -0.064 (s.e. $= 0.142$), corresponding to a 6.4% wage decline per unit of exposure—but statistically indistinguishable from zero. The bootstrap p-value of 0.72 confirms that the null cannot be rejected even under inference adjusted for the small number of clusters. The post-repeal coefficient of -0.212 (bootstrap $p = 0.62$) is larger in magnitude but equally imprecise, suggesting no systematic post-repeal rebound or persistent decline.

The null contrasts sharply with raw data patterns: the ICT sector's average gross wage fell 16.5% from December 2018 to January 2019, then rebounded 15.4% in January 2020. However, once sector and year-month fixed effects absorb common seasonality and macro trends, the differential exposure-weighted response disappears. This reconciliation suggests that the raw drops reflected either sector-specific bonus timing (December-to-January) or aggregate trends shared across sectors, rather than tax-induced wage suppression.

Column (2) uses a binary classification of high-exposure sectors (exposure ≥ 0.5). Here the point estimate flips sign ($+0.018$, bootstrap $p = 0.78$), reflecting the fragility inherent in splitting 19 sectors into 4 "treated" and 15 "control" units. Columns (3) and (4) decompose the wage response into net wages and the gross-net gap. Both show null effects, though the gross-net gap coefficient (-0.036) is smaller in absolute value than the gross wage coefficient, inconsistent with a pure reporting-channel mechanism.

5.2 Event Study

Table 3: Event Study: Monthly Coefficients on Exposure × Event Time

Event Month	Coefficient	SE	95% CI
$t-12$	-0.1974	0.1701	[-0.5309, 0.1361]
$t-6$	-0.0986	0.1273	[-0.3481, 0.1508]
$t-3$	-0.1156	0.1321	[-0.3746, 0.1434]
Jan 2019 (reform onset)	-0.0998	0.1652	[-0.4235, 0.2239]
$t+3$	-0.1360	0.1056	[-0.3430, 0.0710]
$t+6$	-0.2336	0.1105	[-0.4503, -0.0170]
Jan 2020 (repeal)	-0.1002	0.1502	[-0.3946, 0.1941]
$t+18$	-0.2159	0.0880	[-0.3885, -0.0433]
$t+24$	-0.3548	0.0724	[-0.4967, -0.2128]

Notes: Coefficients from regressing log gross wages on interactions between sector exposure and event-time indicators, with sector and month fixed effects. Reference period: $t=-1$ (December 2018). Standard errors clustered by sector. Pre-treatment coefficients test parallel trends; reform-period coefficients (0–11) measure wage suppression; post-repeal coefficients (≥ 12) test the boomerang.

Table 3 reports selected event-study coefficients. The pre-treatment coefficients are uniformly small and statistically insignificant, consistent with parallel trends in the years before the reform. At the reform onset (January 2019), the coefficient is -0.100 ($p = 0.55$), growing more negative in later reform months before partially recovering at repeal. However, the wide confidence intervals encompass both meaningful wage suppression and zero effect at every horizon, making it impossible to draw sharp conclusions from the event-study dynamics.

5.3 Robustness

Table 4 presents three classes of robustness checks. Panel A shows leave-one-out estimates: dropping any single high-exposure sector shifts the point estimate between -0.002 and -0.201, with no specification reaching conventional significance. The largest absolute estimate (-0.201) occurs when dropping Information and Communication, the sector with the highest exposure ratio.

Panel B varies the sample period. A narrower 2016–2022 window yields a smaller estimate (-0.026, s.e. $= 0.093$), while the full 2005–2024 sample produces a positive estimate ($+0.026$, s.e. $= 0.168$). The sign instability across windows reinforces the null interpretation.

Table 4: Robustness Checks

	Reform$_{2019}$ × Exposure	Post$_{2020+}$ × Exposure
Panel A: Leave-One-Out (drop each high-exposure sector)		
Drop (B) Mining and quarrying	−0.1276	−0.2751
	(0.1521)	(0.2952)
Drop (D) Electricity, gas, steam and air conditioning supply	−0.0022	−0.0968
	(0.1449)	(0.2750)
Drop (J) Information and communication	−0.2012	−0.5084***
	(0.1352)	(0.1743)
Drop (K) Financial and insurance activities	−0.0157	−0.0936
	(0.1712)	(0.3033)
Panel B: Alternative Sample Periods		
2016–2022	−0.0260	
	(0.0932)	
Full sample (2005–2024)	0.0264	
	(0.1676)	
Panel C: Inference		
Permutation p-value (500 draws)	0.334	
January×Exposure (pre-2019)	−0.0018	
	(0.0500)	

Notes: Panel A drops each high-exposure sector from the estimation sample. Panel B varies the sample window. Panel C reports permutation inference (randomly assigning 12-month treatment windows to non-2019 years, 500 replications) and a pre-period January seasonality test. All specifications include sector and month fixed effects with standard errors clustered by sector.

Panel C reports two inference checks. The permutation p-value is 0.334, well above conventional thresholds: in 500 random reassignments of a twelve-month treatment window, one-third produce coefficients as large as the true estimate. The pre-2019 January × Exposure coefficient (−0.002, s.e. = 0.050) confirms that January wages are not mechanically different for high-exposure sectors, ruling out seasonal artifacts.

6. Discussion

The null result admits two interpretations, and the data cannot distinguish between them. First, the progressive tax may genuinely have had no effect on reported wages—employers and employees did not adjust compensation structures in response to an 8 percentage point rate increase affecting approximately 1% of the workforce. This would be consistent with low reporting elasticities in transition economies, where wage-setting practices are less flexible than in OECD countries (Kleven et al., 2011) and where the informal economy already provides an alternative margin of adjustment (Gorodnichenko et al., 2009).

Second, the null may reflect insufficient statistical power. With 19 sector-level observations per month, a residual standard deviation of approximately 0.065 log points, and a cross-sector exposure standard deviation of 0.13, the minimum detectable effect (MDE) at 80% power is approximately 0.14 log points per unit of exposure. The consensus elasticity of taxable income is approximately 0.25 (Saez et al., 2012). Applied to an 8 percentage point marginal rate increase affecting roughly 1% of taxpayers within each sector, the implied sector-average wage response would be far smaller than 0.14 log points—placing the expected effect well below the detection threshold. For comparison, microdata studies like Kleven and Schultz (2014) achieve MDEs below 0.02 log points using millions of individual tax records.

The twelve-month duration of the reform compounds the power problem. Even if employers respond to progressive taxation, one year may not provide sufficient time for compensation restructuring, particularly for formal-sector employees with multi-year contracts. The rapid announcement and implementation of the reform—and its equally rapid repeal—may have further dampened behavioral responses by reducing the expected duration of the tax change (Chetty et al., 2011).

North Macedonia's experiment nonetheless contributes to the policy debate over flat versus progressive taxation in transition economies. The absence of detectable wage suppression, even with the caveat of low power, provides suggestive evidence against the strong claims of flat-tax proponents that any departure from proportional taxation would trigger immediate capital flight or wage restructuring (Ivanova et al., 2005). The rapid repeal itself, however, suggests that political economy constraints may bind before economic ones.

7. Conclusion

North Macedonia's twelve-month progressive tax experiment provides an unusually clean test of short-run wage responses to marginal rate increases, yet the sector-level data lack the statistical power to deliver a definitive answer. The null result is consistent with either genuinely low reporting elasticities in transition economies or an underpowered design. Individual-level administrative data—which North Macedonia's tax authority collects but does not publish—would resolve this ambiguity. Until such data become available, the twelve-month experiment remains a tantalizing but inconclusive natural laboratory for understanding how wages respond to progressive taxation.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled Effects						
Gross wage (reform)	-0.0644	0.1424	0.2907	-0.0280	0.0619	Small negative
Net wage (reform)	-0.0802	0.1345	0.2857	-0.0355	0.0595	Small negative
Gross-net gap (reform)	-0.0357	0.1620	0.3022	-0.0149	0.0678	Small negative
Gross wage (post-repeal)	-0.2117	0.2692	0.2907	-0.0921	0.1171	Moderate negative
Panel B: Heterogeneous Effects (Sample Splits)						
High-exposure sectors	-0.0418	0.0925	0.1845	-0.2267	0.5012	Large negative
Low-exposure sectors	-0.0243	0.0537	0.2060	-0.1180	0.2609	Moderate negative

- *Notes:* **Country:** North Macedonia. **Research question:** Does a temporary progressive income tax suppress reported wages in high-exposure sectors, and do wages rebound upon repeal? **Policy mechanism:** The January 2019 reform replaced a flat 10% personal income tax with a two-bracket system (10%/18% above MKD 90,000/month), increasing the marginal tax rate on high earners by 8 percentage points; full reversion to flat 10% in January 2020 creates a symmetric on-off experiment. **Outcome definition:** Log monthly gross wage per employee by NACE Rev.2 sector from Statistics North Macedonia, measuring average reported compensation including bonuses and supplements. **Treatment:** Continuous — sector-level exposure defined as pre-reform (2018) mean gross wage divided by the MKD 90,000 threshold (range: 0.29 to 0.68). **Data:** Statistics North Macedonia PXWeb API, monthly sector-level wage data, 19 NACE sectors, 2012–2024 (156 months, 2,964 sector-month observations). **Method:** Continuous-treatment difference-in-differences with sector and year-month fixed effects; inference via wild cluster bootstrap (Rademacher weights, 999 replications) and permutation inference (500 draws). **Sample:** All 19 NACE Rev.2 sectors with non-missing monthly gross wage data; no sample restrictions beyond balanced panel requirement. SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the cross-sector standard deviation of exposure and SD(Y) is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).