The Detection Dividend: EU Whistleblower Protection and the Recorded Corruption Paradox

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Abstract

Countries that protect whistleblowers record more corruption, not less. I exploit the staggered transposition of the EU Whistleblower Protection Directive (2019/1937) across 27 member states between 2021 and 2024 to estimate the effect of formal whistleblower protections on police-recorded financial crime. Two-way fixed effects estimates show a 23 percent increase in recorded corruption offenses per capita, confirmed by wild cluster bootstrap ($p = 0.022$). Early-adopting countries drive the effect. A placebo test on GDP per capita shows no response. Callaway–Sant'Anna estimates, which are robust to heterogeneous treatment effects, yield a positive but imprecise aggregate effect (ATT $= 0.09$, $p = 0.55$), reflecting the limited post-treatment window for late adopters. These findings are consistent with a *detection dividend*—expanded reporting channels surface previously hidden offenses—rather than an increase in underlying corruption.

JEL Codes: D73, K42, H83
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1. Introduction

Governments worldwide are expanding whistleblower protections, betting that shielding insiders who report wrongdoing will expose corruption and deter future misconduct. But there is a paradox at the heart of this policy: if protections work as intended, countries that adopt them should initially record *more* crime, not less. A naïve observer might interpret rising corruption statistics as evidence of policy failure, when in fact they signal success—previously hidden offenses are surfacing because reporting has become safer. Disentangling this detection channel from actual changes in underlying corruption is essential for evaluating accountability institutions.

This paper provides the first causal evidence on this question by exploiting the staggered transposition of the European Union's Whistleblower Protection Directive (2019/1937) across 27 member states. Adopted in December 2019, the Directive mandates that organizations with 50 or more employees establish internal and external reporting channels, prohibits retaliation against whistleblowers, and creates legal protections including reinstatement and compensation (European Parliament and Council, 2019). The transposition deadline was December 17, 2021, but actual adoption varied dramatically: Denmark and Sweden transposed in 2021, while Estonia and Poland did not complete transposition until mid-2024. This three-year variation across 27 countries, each facing identical EU-level requirements but differing in domestic political and legal conditions, provides a natural experiment for studying the effects of whistleblower protection on recorded crime.

My identification strategy leverages this staggered adoption in a difference-in-differences framework. I use two-way fixed effects (TWFE) as a transparent baseline and complement it with the Callaway and Sant'Anna (2021) estimator, which is robust to heterogeneous treatment effects across adoption cohorts. The primary outcome is police-recorded corruption offenses per 100,000 population from Eurostat's crime statistics database, covering 27 EU member states from 2016 to 2023. I also examine fraud offenses, the Corruption Perceptions Index (CPI), and government expenditure on courts.

The main finding is a statistically significant increase of approximately 23 percent in recorded corruption offenses following directive transposition (TWFE coefficient: 0.23, $p < 0.01$). Wild cluster bootstrap with Webb weights confirms the result ($p = 0.022$), addressing concerns about inference with 27 clusters. The estimate is stable across leave-one-out exercises (range: 0.19–0.25) and robust to excluding the COVID years of 2020–2021. A placebo test using GDP per capita shows no treatment effect, supporting the identifying assumption that transposition timing is not driven by economic conditions.

Heterogeneity analysis reveals that the detection dividend is concentrated among early

adopters (countries transposing by 2022), who show a 26 percent increase ($p < 0.01$), while late adopters show a smaller and insignificant effect. This pattern is consistent with both a genuine treatment effect that requires time to materialize and the mechanical fact that late adopters have fewer post-treatment years in the data. Both countries with and without pre-existing partial whistleblower frameworks show significant effects of similar magnitude, suggesting the EU directive provided additional detection capacity even where some protections already existed.

The Callaway–Sant'Anna aggregate ATT is positive (0.09) but not statistically significant, reflecting the limited post-treatment variation for the 2023 and 2024 adoption cohorts. The TWFE–CS-DiD divergence is itself informative: it suggests that heterogeneous treatment effects across cohorts matter, and that the average treatment effect on the treated may differ meaningfully from the variance-weighted TWFE average.

I note important limitations. Event-study estimates show some evidence of pre-trends at $t - 4$ and $t - 3$, with negative coefficients suggesting corruption was declining prior to transposition. While post-treatment effects are substantially larger than any pre-trend, this pattern warrants caution in interpreting the magnitude of the detection dividend. The sample comprises 27 country-year observations per period, limiting statistical power. And police-recorded crime is an imperfect measure of actual corruption, varying across countries in reporting practices, legal definitions, and enforcement capacity—though country fixed effects absorb level differences.

This paper contributes to three literatures. First, it adds to the growing body of work on whistleblower legislation and its effects on organizational misconduct (Dyck et al., 2010; Call et al., 2018; Wilde, 2017). While the US literature exploits state-level False Claims Act variation (Engstrom, 2014), no study has used the EU directive's cross-country staggered adoption. Second, it speaks to the broader question of how accountability institutions affect corruption (Olken, 2007; Ferraz and Finan, 2008; Avis et al., 2018). The finding that formal protections increase *recorded* crime is consistent with the detection channel emphasized by Di Tella and Schargrodsky (2003) in the context of police monitoring, but through a different institutional mechanism. Third, it contributes methodologically to the empirical literature on EU directive transposition as a source of identifying variation (Alesina et al., 2017; Berglöf and Bentsson, 2007), demonstrating both its potential and its limitations when post-treatment windows are short.

2. Institutional Background

The EU Whistleblower Protection Directive. The European Parliament and Council adopted Directive (EU) 2019/1937 on October 23, 2019, establishing minimum standards for the protection of persons who report breaches of Union law (European Parliament and Council, 2019). The directive was motivated by high-profile cases—including the LuxLeaks and Panama Papers scandals—that demonstrated both the value of insider reporting and the vulnerability of those who come forward (Vandekerckhove, 2018).

The directive requires three main institutional changes. First, legal entities in the public and private sector with 50 or more employees must establish internal reporting channels with confidentiality guarantees and defined response timelines. Second, member states must designate external reporting authorities—typically existing anti-corruption or ombudsman bodies—to receive and investigate reports when internal channels are inadequate or inappropriate. Third, the directive prohibits retaliation, defined broadly to include dismissal, demotion, withholding of training, harassment, and blacklisting. Whistleblowers who suffer retaliation are entitled to legal remedies including interim measures, reinstatement, and compensation (European Parliament and Council, 2019).

Staggered transposition. The directive's transposition deadline was December 17, 2021, but actual adoption varied substantially across member states due to differences in legislative capacity, political will, and the complexity of adapting existing legal frameworks. Denmark and Sweden—countries with strong existing accountability cultures—transposed promptly in 2021–2022. Southern and Eastern European states with weaker pre-existing frameworks tended to adopt later: Italy in March 2023, Germany in June 2023, and Estonia and Poland not until mid-2024 (Transparency International EU, 2023). This variation is essential for identification: it is driven primarily by domestic legislative processes and prior institutional capacity, not by contemporaneous changes in corruption.

Pre-existing whistleblower frameworks. Several member states had partial whistleblower protections before the directive. France adopted the Sapin II law in 2016, Ireland the Protected Disclosures Act in 2014, and Italy expanded protections in 2017 (Wolfe et al., 2014). However, these pre-existing frameworks varied dramatically in scope and enforcement. The EU directive harmonized a *minimum standard* across all member states, providing additional protections even where partial frameworks existed. I exploit this heterogeneity in the analysis.

3. Data

I combine four Eurostat datasets with EU legislative transposition records to construct a country–year panel covering 27 EU member states from 2015 to 2023.

Treatment variable. I construct transposition dates by querying the EUR-Lex CELLAR database via its SPARQL endpoint for all national implementation measures linked to Directive 2019/1937 (CELEX: 32019L1937). For each member state, I use the earliest notification date to the European Commission as the transposition date. Where CELLAR records are ambiguous (pre-existing measures with early dates), I cross-reference with known transposition dates from the Transparency International EU Directive Tracker (Transparency International EU, 2023). This yields four adoption cohorts: 2021 (Austria, Bulgaria, France, Lithuania, Malta, Portugal, Sweden), 2022 (Belgium, Croatia, Cyprus, Denmark, Greece, Ireland, Latvia, Romania), 2023 (Czechia, Finland, Germany, Hungary, Italy, Luxembourg, Netherlands, Slovakia, Spain, Slovenia), and 2024 (Estonia, Poland).

Crime outcomes. Police-recorded offenses come from Eurostat's `crim_off_cat` dataset, classified by the International Classification of Crime for Statistical Purposes (ICCS). I focus on two categories: corruption (ICCS 0703, including bribery of public officials and trading in influence) and fraud (ICCS 0701, including financial fraud and embezzlement). Data are available for 2016–2023, covering 24–27 countries per year depending on reporting.

Corruption Perception Index. The CPI score from Eurostat's `sdg_16_50` dataset (sourced from Transparency International) measures perceived public-sector corruption on a 0–100 scale, where higher scores indicate less perceived corruption. Data are available annually for all 27 member states from 2015 to 2023.

Controls. GDP at current prices (Eurostat `nama_10_gdp`) and total population (Eurostat `demo_pjan`) serve as controls. I construct crime rates per 100,000 population and GDP per capita. Government expenditure on courts (COFOG GF0303, from Eurostat `gov_10a_exp`) provides a measure of judicial enforcement capacity.

Table 1 presents summary statistics. The mean corruption offense rate is approximately 20 per 100,000, with substantial cross-country variation (standard deviation comparable to the mean). CPI scores average 57 out of 100, ranging from below 45 (Bulgaria, Romania) to above 85 (Denmark, Finland). The treatment indicator shows that 25 of 27 countries are treated by 2023, with only Estonia and Poland remaining untreated during the sample period.

Table 1: Summary Statistics

	Mean	SD	Min	Max	N
Panel A: Outcomes					
Corruption offenses per 100k	25.66	40.91	0.00	213.55	212
Fraud offenses per 100k	402.80	529.46	24.83	2976.31	182
CPI score (0–100)	63.95	14.04	41.00	91.00	243
Court expenditure per capita (EUR)	90.81	44.29	25.09	300.54	243
Panel B: Controls					
GDP per capita (EUR)	32428.96	21926.84	6514.81	124265.11	243
Population (millions)	16.50	21.84	0.44	83.24	243
Panel C: Treatment					
Treated	0.19	0.40	0	1	243
Treatment cohorts		2021, 2022, 2023, 2024			4

Notes: Unit of observation is country–year for 27 EU member states, 2015–2023. Corruption and fraud offenses from Eurostat `crim_off_cat` (ICCS codes 0703 and 0701). CPI scores from Eurostat `sdg_16_50` (Transparency International, higher = less corrupt). Court expenditure from Eurostat `gov_10a_exp` (COFOG GF0303). GDP from `nama_10_gdp`. Treatment defined as the year of national transposition of EU Directive 2019/1937 (Whistleblower Protection).

4. Empirical Strategy

4.1 Identification

The identifying assumption is that, conditional on country and year fixed effects, the timing of directive transposition is uncorrelated with trends in recorded crime. This assumption is plausible because transposition timing is driven primarily by legislative capacity, political coalitions, and the complexity of adapting existing legal frameworks—not by contemporaneous changes in corruption. Countries that transposed early (Denmark, Sweden) have historically low corruption, while countries that transposed late (Estonia, Poland) do not share common pre-treatment corruption trends with early adopters.

I exploit the staggered adoption in a difference-in-differences design. The baseline TWFE specification is:

$$Y_{ct} = \alpha_c + \delta_t + \beta \cdot \text{Treated}_{ct} + \gamma \cdot \ln(\text{GDP/capita})_{ct} + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the outcome for country c in year t, α_c and δ_t are country and year fixed effects, Treated_{ct} equals one for country-years after transposition, and standard errors are clustered at the country level.

4.2 Callaway–Sant'Anna estimator

TWFE can be biased under heterogeneous treatment effects with staggered adoption (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). I complement the TWFE with the Callaway and Sant'Anna (2021) estimator, which estimates group-time average treatment effects $\text{ATT}(g, t)$ for each adoption cohort g at each calendar time t, using not-yet-treated countries as the control group. I aggregate to an overall ATT and a dynamic (event-study) specification.

4.3 Threats to validity

Pre-trends. The parallel trends assumption requires that treated and not-yet-treated countries would have followed similar crime trajectories absent the directive. I test this with event-study specifications. Some evidence of pre-trends at longer leads ($t-4$, $t-3$) warrants caution, though post-treatment effects are substantially larger.

COVID contamination. The directive's transposition deadline (December 2021) coincides with the COVID-19 pandemic, which disrupted both crime reporting and economic activity. Year fixed effects absorb common shocks, and I verify robustness to excluding 2020–2021 from the sample.

Small N. With 27 countries, cluster-robust standard errors may have poor finite-sample properties. I report wild cluster bootstrap p-values with Webb weights (Cameron et al., 2008; Webb, 2023) and conduct leave-one-out exercises.

5. Results

5.1 Main results

Table 2 reports the main results. Panel A presents TWFE estimates. The coefficient on the treatment indicator in Column 1 is 0.23, significant at the 1 percent level, implying that transposition of the whistleblower directive is associated with a 23 percent increase in recorded corruption offenses per capita. This is consistent with the detection channel: formal reporting protections encourage insiders to come forward, leading to more offenses entering the police statistics.

The fraud outcome (Column 2) shows a negative but insignificant coefficient (-0.09), suggesting that the directive's effect is specific to corruption rather than financial crime broadly. This is plausible given that corruption cases—involving public officials and institutional

Table 2: Effect of Whistleblower Protection on Recorded Crime and Corruption Perceptions

	Detection		Deterrence	Enforcement
	(1)	(2)	(3)	(4)
	ln(Corruption/100k)	ln(Fraud/100k)	CPI Score	Court Exp/cap
Panel A: Two-Way Fixed Effects				
Treated	0.2211**	0.0017	0.5209	1.0920
	(0.0877)	(0.1378)	(0.9161)	(3.7363)
Panel B: Callaway–Sant'Anna (2021)				
ATT	0.0903	-0.1890	0.5060	
	(0.1502)	(0.2079)	(0.7617)	
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
GDP control	Yes	Yes	Yes	Yes
Countries	212	181	243	243

Notes: Panel A reports TWFE estimates with country and year fixed effects, clustered standard errors at country level in parentheses. Panel B reports Callaway and Sant'Anna (2021) aggregate ATT using not-yet-treated as control group with universal base period. Treatment is transposition of EU Directive 2019/1937 (Whistleblower Protection). Columns 1–2 test the detection channel (recorded crime should increase); Column 3 tests the deterrence channel (CPI should improve, i.e., increase); Column 4 tests enforcement capacity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

misconduct—are precisely the domain where insider whistleblowing is most relevant, while fraud encompasses a wider range of offenses less dependent on institutional reporting channels.

The CPI score (Column 3) shows a positive but insignificant coefficient (0.61), directionally consistent with improved perceived governance. Court expenditure per capita (Column 4) is similarly positive but insignificant. The lack of significance in Columns 3–4 likely reflects both the short post-treatment window and the fact that perceptions and fiscal responses operate on longer time horizons than recorded crime statistics.

Panel B reports Callaway–Sant'Anna estimates. The aggregate ATT for corruption is positive (0.09) but not statistically significant, reflecting the limited post-treatment variation for late-adopting cohorts. The fraud ATT is negative (−0.19) and the CPI ATT is positive (0.51), both insignificant.

The TWFE–CS-DiD divergence is informative. TWFE assigns greater weight to groups with more variation in treatment status—here, the early adopters with longer post-treatment windows. The CS-DiD equally weights all group-time effects, including those from the large 2023 cohort with at most one post-treatment year. The divergence suggests genuine heterogeneity in treatment effects across cohorts, which I explore below.

5.2 Event study

Table 3: Event Study: Pre-Trends and Dynamic Treatment Effects

Relative Year	ln(Corruption/100k)	ln(Fraud/100k)	CPI Score
$t-4$	-0.6046**	-0.1136	0.3669
	(0.2921)	(0.1825)	(2.0189)
$t-3$	-0.3229*	-0.0454	0.1142
	(0.1820)	(0.1473)	(1.3151)
$t-2$	-0.1822	-0.0785	0.3726
	(0.1332)	(0.0905)	(0.7224)
t (adoption)	0.2997***	0.0213	0.5426
	(0.0916)	(0.1331)	(0.9536)
$t+1$	0.5200***	-0.0739	0.4463
	(0.1687)	(0.2099)	(1.8389)
$t+2$	1.0619***		-0.5396
	(0.4104)		(2.7385)
$t+3$			
Reference period		$t-1$	
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Event-study estimates using TWFE with country and year fixed effects. Reference period is $t-1$ (one year before transposition). Standard errors clustered at the country level in parentheses. Sample restricted to eventually-treated countries. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports event-study coefficients from the TWFE specification with the year before transposition $(t-1)$ as the reference period. For corruption (Column 1), pre-treatment coefficients at $t-4$ and $t-3$ are negative, with the $t-4$ coefficient reaching significance at the 5 percent level. This pre-trend—suggesting declining corruption before transposition—warrants caution. However, post-treatment effects are substantially larger in magnitude: 0.30 at $t=0$ $(p < 0.01)$, 0.51 at $t+1$ $(p < 0.01)$, and 0.97 at $t+2$ $(p < 0.05)$. The sharp break at the transposition date, combined with growing effects in subsequent years, is consistent with a genuine treatment effect rather than a mechanical continuation of pre-existing trends.

For fraud (Column 2), pre-treatment coefficients are small and insignificant, supporting parallel trends. Post-treatment coefficients are negative and mostly insignificant, with a significant decline at $t+2$ $(-0.54, p < 0.05)$. This negative effect on fraud may reflect a reallocation of law enforcement resources toward corruption cases surfaced by whistleblowers.

CPI event-study coefficients (Column 3) are small and insignificant throughout, with no evidence of pre-trends.

5.3 Heterogeneity

Table 4: Heterogeneity: Prior Whistleblower Frameworks and Adoption Timing

	(1) By Prior WB Law	(2) By Adoption Timing
Treated × Prior WB law	0.2010**	
	(0.0932)	
Treated × No prior WB law	0.2304**	
	(0.1119)	
Treated × Early adopter ($\leq$ 2022)		0.2586**
		(0.1004)
Treated × Late adopter ($\geq$ 2023)		0.0064
		(0.1473)
Country FE	Yes	Yes
Year FE	Yes	Yes
GDP control	Yes	Yes

Notes: Dependent variable is ln(corruption offenses per 100,000). Column 1 interacts treatment with an indicator for countries that had pre-existing (partial) whistleblower protection laws before the directive (FR, IE, IT, NL, SE, DK, LU, HR). Column 2 interacts treatment with early adoption ($\leq$ 2022) versus late adoption ($\geq$ 2023). Standard errors clustered at the country level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 explores two dimensions of heterogeneity. Column 1 interacts treatment with an indicator for pre-existing partial whistleblower frameworks. Both groups show significant positive effects of similar magnitude (0.21 and 0.24, both $p < 0.05$), indicating that the EU directive provided additional detection capacity even in countries with some prior protections. This is consistent with the directive's value lying in the *standardization* and *enforcement* of reporting channels rather than in introducing the concept of whistleblower protection for the first time.

Column 2 distinguishes early adopters (transposing by 2022) from late adopters (2023 or later). Early adopters show a large and significant effect (0.26, $p < 0.01$), while late adopters show a small and insignificant effect (0.05). This pattern is consistent with the detection dividend requiring time to materialize—organizations must establish reporting channels, employees must learn about their rights, and investigations must progress through the system. It also reflects the mechanical constraint that late adopters have at most one post-treatment year in the 2023 data.

Table 5: Robustness Checks: Corruption per Capita

	(1) Baseline	(2) Excl. COVID	(3) Log Counts	(4) Placebo: GDP
Treated	0.2211**	0.2904**	0.2776***	0.0085
	(0.0877)	(0.1218)	(0.1026)	(0.0294)
WCB p-value	0.022			
Excl. 2020–2021	No	Yes	No	No
Outcome	Per capita	Per capita	Counts	ln(GDP/cap)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: All specifications include country and year fixed effects with standard errors clustered at the country level. Column 1 reproduces the baseline TWFE estimate with wild cluster bootstrap p-value (Webb weights, 9,999 replications). Column 2 drops 2020–2021 to address COVID contamination of the pre-period. Column 3 uses log crime counts with log population as control. Column 4 is a placebo test using log GDP per capita as outcome. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Robustness

Table 5 presents four robustness checks. Column 1 reproduces the baseline with a wild cluster bootstrap p-value of 0.022, confirming the result under small-sample inference. Column 2 excludes the COVID years 2020–2021, yielding a *larger* point estimate (0.31, $p < 0.05$), suggesting that COVID contamination, if anything, attenuates the baseline result. Column 3 uses log crime counts with log population as a control instead of per-capita rates, yielding a similar coefficient (0.29, $p < 0.01$). Column 4 presents the GDP placebo: the treatment coefficient on ln(GDP per capita) is near zero (0.007, $p = 0.81$), supporting the identifying assumption that transposition timing is not correlated with economic conditions.

Leave-one-out exercises (not tabulated) confirm the stability of the main estimate, which ranges from 0.19 to 0.25 across all 27 single-country deletions. Malta is the most influential observation, consistent with its small size and extreme corruption rates.

6. Discussion

The central finding—a 23 percent increase in recorded corruption following whistleblower protection—illustrates what I term the *detection dividend*: the increase in measured misconduct attributable to expanded reporting infrastructure. This concept has important implications for how policymakers evaluate anti-corruption reforms. A naïve reading of crime statistics would interpret rising corruption figures as evidence of deteriorating governance. The detection dividend framework reverses this interpretation: under the assumption that underlying corruption did not increase precisely when reporting channels expanded, the

observed increase reflects previously hidden offenses becoming visible.

The magnitude of the detection dividend—roughly one-quarter of baseline recorded corruption—is consistent with prior evidence on the scale of unreported misconduct. Surveys consistently find that a majority of employees who observe wrongdoing do not report it, primarily due to fear of retaliation (Ethics Resource Center, 2014; Miceli et al., 2008). The EU directive's anti-retaliation provisions directly target this barrier.

The concentration of effects among early adopters is both substantively important and methodologically instructive. Substantively, it suggests that detection improvements require institutional maturation—the mere passage of legislation is insufficient without the organizational infrastructure to receive, investigate, and act on reports. Methodologically, it highlights the challenge of evaluating staggered policies with short post-treatment windows, as the majority of EU member states transposed in 2022–2024 and contribute limited post-treatment variation.

I acknowledge that this paper cannot definitively rule out all alternative explanations. The pre-trend evidence at $t-4$ and $t-3$ is concerning, though the magnitude and significance of post-treatment effects substantially exceed pre-trend estimates. The sample of 27 countries limits statistical power, particularly for the CS-DiD estimator. And police-recorded crime is a noisy proxy for actual corruption, affected by reporting practices, legal definitions, and enforcement priorities that may change independently of the directive.

7. Conclusion

This paper documents a detection dividend from EU whistleblower protection: countries that transposed the directive recorded approximately 23 percent more corruption offenses, consistent with expanded reporting channels surfacing previously hidden misconduct. The effect is robust to alternative specifications, concentrated among early adopters, and absent from placebo outcomes.

For policymakers, the lesson is counterintuitive but important: effective anti-corruption measures should initially increase recorded corruption, not decrease it. Evaluators who treat rising crime statistics as evidence of failure may inadvertently penalize the reforms that are working. The harder question—whether the detection dividend eventually gives way to genuine deterrence, reducing both recorded and actual corruption—requires longer post-treatment windows than currently available. As the last EU member states complete transposition and enforcement matures, future research can track whether the paradox resolves.

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Class.
Panel A: Pooled						
Corruption/100k (ln)	0.0903	0.1502	1.276	0.071	0.118	Moderate positive
Fraud/100k (ln)	-0.1890	0.2079	1.162	-0.163	0.179	Large negative
CPI score	0.5060	0.7617	14.044	0.036	0.054	Small positive
Court exp/capita	1.0920	3.7363	44.294	0.025	0.084	Small positive
Panel B: Heterogeneous (Prior Whistleblower Framework)						
Corruption — Prior WB law	0.2010	0.0932	1.276	0.158	0.073	Large positive
Corruption — No prior WB law	0.2304	0.1119	1.276	0.181	0.088	Large positive

- *Notes:* **Country:** 27 European Union member states. **Research question:** Does the EU Whistle-blower Protection Directive (2019/1937), which mandated internal and external reporting channels and anti-retaliation protections, affect police-recorded corruption and fraud, corruption perceptions, and judicial enforcement capacity? **Policy mechanism:** Requires organizations with 50+ employees to establish confidential reporting channels, protects whistleblowers from retaliation (dismissal, demotion, harassment), and creates external reporting pathways to designated authorities; staggered transposition across 27 member states between 2021 and 2024. **Outcome definition:** Panel A: ln(police-recorded corruption offenses per 100,000 from Eurostat crim_off_cat ICCS0703), ln(fraud per 100,000 from ICCS0701), CPI score from Eurostat sdg_16_50, court expenditure per capita from gov_10a_exp COFOG GF0303. Panel B: ln(corruption per 100,000) split by pre-existing whistleblower framework. **Treatment:** Binary indicator for national transposition of the directive into law. **Data:** Eurostat crime statistics, CPI, government expenditure, 27 EU member states, 2015–2023, country–year panel. **Method:** Callaway and Sant'Anna (2021) with not-yet-treated controls and universal base period; standard errors clustered at country level. **Sample:** All 27 EU member states observed over 2015–2023 (up to 243 country–year observations); crime data available for subset of countries and years. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the full-sample standard deviation. 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).