The Prosecution Gap: Cross-Border Enforcement Cooperation and Crime in the European Union

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Abstract

Every year, tens of thousands of cross-border criminal cases in the European Union stall because evidence held in one member state cannot reach prosecutors in another within the timeframe required for effective prosecution. The European Investigation Order (Directive 2014/41/EU) replaced this fragmented mutual legal assistance system with binding 90-day evidence requests, compressing average processing times from over a year to three months. I exploit the staggered transposition of the EIO across 25 EU member states between 2016 and 2018 to estimate its effect on cross-border crime. Using Callaway-Sant'Anna estimators and Eurostat police-recorded offences data, I find no evidence that the EIO reduced fraud, drug offences, or theft. A triple-difference comparing cross-border against domestic crimes reveals that reported cross-border offences *increased* relative to domestic crimes after transposition—consistent with a detection channel rather than deterrence.

JEL Codes: K14, K42, F55, H77

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1. Introduction

A French prosecutor investigating a fraud ring operating across four EU member states faces a paradox: she can identify suspects, trace financial flows, and build a case—but the evidence she needs sits in databases in Belgium, Germany, and the Netherlands, governed by four different legal systems, accessible only through diplomatic channels that take 10 to 18 months to produce results (Vernimmen-Van Tiggelen and Surano, 2015). By the time the evidence arrives, statutes of limitations may have expired, witnesses have moved, and co-defendants have destroyed records. This is the *prosecution gap*: the jurisdictional void between where cross-border crime occurs and where evidence can be gathered.

The European Investigation Order (EIO), adopted as Directive 2014/41/EU, represents the EU's most ambitious attempt to close this gap. It replaced the fragmented system of mutual legal assistance treaties and the 2008 European Evidence Warrant with a single instrument: a binding request that compels the receiving state to gather and transfer evidence within 90 days (Mangiaracina, 2014). The reform was dramatic. Before the EIO, cross-border evidence gathering relied on letters rogatory—diplomatic requests processed through justice ministries with no enforceable deadlines. The EIO shifted this to direct prosecutor-to-prosecutor communication with binding timelines, creating what legal scholars have called a "paradigm shift" in judicial cooperation (Allegrezza, 2019).

This paper asks whether the EIO reduced cross-border crime in the EU. The theoretical case for deterrence rests on Becker (1968): if cross-border enforcement cooperation raises the probability of conviction, rational offenders should reduce criminal activity. The EIO's mechanism operates through this conviction-probability channel by making evidence admissible and timely, a less-studied margin than the probability-of-apprehension channel that dominates the policing literature (Chalfin and McCrary, 2017; Draca et al., 2011).

I exploit the staggered transposition of the EIO across EU member states to estimate its causal effect on crime. The directive's transposition deadline was May 22, 2017, but only two member states transposed on time; thirteen faced infringement proceedings from the European Commission for late implementation, with transposition dates ranging from early 2016 to September 2018. Denmark and Ireland exercised their opt-out from Area of Freedom, Security and Justice measures, providing a never-treated control group. This variation yields a natural experiment: countries that gained the ability to compel cross-border evidence at different times, while facing the same EU-wide crime trends.

Using police-recorded offences from Eurostat's International Classification of Crime for Statistical Purposes (ICCS) database covering 27 EU member states from 2008 to 2022, I employ the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator.

This avoids the well-documented biases of two-way fixed effects (TWFE) in settings with staggered treatment timing (Goodman-Bacon, 2021; Sun and Abraham, 2021). The design compares not-yet-treated countries against treated countries, exploiting the 16-month spread in transposition timing.

The headline result is a well-powered null: I find no evidence that the EIO reduced any category of cross-border crime. The Callaway-Sant'Anna aggregate treatment effect on fraud is -0.059 (SE $= 0.109$), on drug offences 0.058 (SE $= 0.229$), and on theft 0.020 (SE $=$ 0.088)—all statistically indistinguishable from zero. The TWFE estimates, while slightly different in magnitude, confirm the null across all specifications. These estimates survive randomization inference (RI $p = 0.24$ for fraud) and are robust to excluding the COVID-19 pandemic years (2020–2022).

More revealing than the category-by-category nulls is the triple-difference. The EIO should disproportionately affect crime categories where cross-border evidence gathering is most relevant—fraud, drug trafficking, theft—as these offences frequently involve financial records, supply chains, or goods dispersed across jurisdictions. Domestic crimes like homicide and serious assault, which predominantly rely on local forensic and witness evidence, should be less affected. The triple interaction (cross-border crime $\times$ post-transposition $\times$ treated country) is $+0.683$ (SE $= 0.146$, $p < 0.001$): reported cross-border offences *increased* relative to domestic crimes after EIO transposition. This pattern is inconsistent with deterrence but *suggestive* of a detection channel: improved enforcement cooperation may enable prosecutors to identify and record more cross-border cases that previously went undetected or uninvestigated.

This paper makes three contributions. First, it provides the first causal estimates of the EIO's effect on crime. Despite being the EU's flagship instrument for criminal justice cooperation, no empirical economics paper has evaluated the EIO or any other EU mutual recognition instrument's deterrence effects. The legal literature discusses implementation challenges (Tosza, 2020; Ruggeri, 2019) but provides no quantitative evidence. Second, the null finding on deterrence contributes to the broader evidence that enforcement-infrastructure reforms may not deter crime through the conviction-probability channel (Helland and Tabarrok, 2007; Agan et al., 2023), particularly when the primary margin they affect—evidence gathering timelines—is invisible to potential offenders. Third, the detection channel finding connects to the growing literature on how law enforcement capacity shapes *measured* crime rather than actual criminal behavior (Chalfin and McCrary, 2017; Mello, 2019).

The remainder of the paper is organized as follows. Section 2 describes the institutional background of the EIO. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses mechanisms and implications.

2. Institutional Background

The prosecution gap before the EIO. Before 2017, cross-border criminal investigations in the EU relied on a patchwork of bilateral mutual legal assistance (MLA) treaties, the 2000 EU Convention on Mutual Legal Assistance, and the 2003 Framework Decision on the freezing of property. These instruments shared a common feature: they operated through diplomatic or ministerial channels with no enforceable deadlines. A letter rogatory—the traditional mechanism for requesting foreign evidence—typically required routing through central authorities in both the requesting and receiving states, with average processing times of 10 to 18 months (Vernimmen-Van Tiggelen and Surano, 2015). For time-sensitive investigations involving financial records, digital evidence, or witness testimony, these delays were often fatal to the case.

The European Investigation Order. Directive 2014/41/EU, adopted on April 3, 2014, replaced this fragmented system with the European Investigation Order: a judicial decision issued by an authority of one member state to have specific investigative measures carried out in another member state to obtain evidence (Mangiaracina, 2014). The key innovations were threefold: (1) a single, comprehensive instrument replacing multiple overlapping frameworks; (2) direct communication between judicial authorities rather than through diplomatic channels; and (3) binding 90-day deadlines for execution, with a 30-day deadline for the initial recognition decision.

Staggered transposition. The directive required transposition into national law by May 22, 2017. In practice, implementation varied dramatically. Only a handful of states met the deadline; the European Commission opened infringement proceedings against thirteen member states for late transposition. The earliest transpositions occurred in 2016 (Hungary, Romania, Czechia, and France transposed pre-deadline measures), while the latest—Luxembourg—did not complete transposition until September 2018. Denmark and Ireland exercised permanent opt-outs from the Area of Freedom, Security and Justice under their respective EU Treaty protocols and never implemented the EIO. This variation in timing and the opt-out structure provides the identifying variation for this study.

3. Data

Crime data. I use Eurostat's `crim_off_cat` dataset, which reports police-recorded offences classified under the International Classification of Crime for Statistical Purposes (ICCS). The data cover 27 EU member states from 2008 to 2022 at annual frequency. I focus on

Table 1: Summary Statistics: Crime Rates per 100,000 Population

Category	Type	Mean	SD	Min	Max	N
Drug offences	Cross-border	214.0	237.7	9.0	1222.4	405
Fraud	Cross-border	402.8	529.5	24.8	2976.3	182
Theft	Cross-border	1316.5	1039.3	56.8	5452.6	404
Homicide	Domestic	1.4	1.2	0.3	8.9	402
Robbery	Domestic	56.7	49.8	5.3	248.5	405
Serious assault	Domestic	83.1	141.6	0.9	722.3	397

Notes: Police-recorded offences per 100,000 population from Eurostat (`crim_off_cat`), 2008–2022. Cross-border crimes (fraud, drug offences, theft) are those where the European Investigation Order mechanism is most relevant. Domestic crimes (homicide, serious assault) serve as placebos.

five ICCS categories: three cross-border crime types where the EIO mechanism is most relevant—fraud (ICCS0701), drug offences (ICCS0601), and theft (ICCS0502)—and two domestic crime types that serve as placebos—homicide (ICCS0101) and serious assault (ICCS020111). These categories were selected because cross-border crimes involve evidence dispersed across jurisdictions (financial records, supply chain intelligence, stolen goods tracking), while homicide and assault investigations typically depend on local forensic and witness evidence.

Transposition dates. I construct the treatment variable from the CELLAR SPARQL endpoint of EUR-Lex, which records national implementation measures (NIMs) for each EU directive. For Directive 2014/41/EU (CELEX: 32014L0041), I extract the earliest notification date per member state, filtering to measures dated from 2016 onward to exclude predecessor instruments. This yields transposition dates for all 25 participating member states.

Population. Annual population data from Eurostat's `demo_pjan` dataset are used to construct crime rates per 100,000 population.

Panel construction. The analysis panel contains 2,195 country×year×crime-category observations: 27 countries over 15 years across 5–7 crime categories. The treatment variable is a binary indicator equal to one from the year a country transposed the EIO. Three cohorts emerge: 2016 (4 countries), 2017 (15 countries), and 2018 (6 countries), with Denmark and Ireland as the never-treated group.

4. Empirical Strategy

Callaway-Sant'Anna staggered DiD. The primary specification uses the Callaway and Sant'Anna (2021) estimator, which computes group-time average treatment effects $ATT(g,t)$ for each cohort g (defined by transposition year) and calendar year t, using not-yet-treated units as the comparison group. This avoids the negative weighting and contamination issues that plague two-way fixed effects in staggered settings (Goodman-Bacon, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020). I estimate doubly-robust treatment effects, which remain consistent if either the outcome regression or the propensity score model is correctly specified.

The identifying assumption is that, conditional on group and time fixed effects, crime trends would have been parallel across countries with different transposition dates. I test this with event-study plots examining pre-treatment dynamics.

TWFE comparison. For transparency and to assess staggered-adoption bias, I also report standard TWFE estimates:

$$\log(\text{Rate}_{it} + 0.01) = \alpha_i + \gamma_t + \beta \cdot \text{EIO}_{it} + \varepsilon_{it} \tag{1}$$

where Rate_{it} is offences per 100,000 population in country i and year t, α_i and γ_t are country and year fixed effects, and EIO_{it} is a binary indicator for transposition. Standard errors are clustered at the country level.

Triple-difference. The most demanding specification compares cross-border crimes against domestic crimes, before and after transposition, in transposing versus non-transposing countries:

$$\log(\text{Rate}_{ict}) = \alpha_i + \gamma_t + \delta_c + \beta_{\text{DDD}} \cdot (\text{Cross-border}_c \times \text{Post}_t \times \text{Treated}_i) + \mathbf{X}'\boldsymbol{\theta} + \varepsilon_{ict} \tag{2}$$

where c indexes crime category and $\mathbf{X}$ includes all lower-order interactions. This design absorbs any country-specific shocks affecting all crime (e.g., economic recessions, migration waves) and any crime-specific trends (e.g., shifts to online fraud globally), isolating the *differential* effect on cross-border versus domestic crime in countries that transposed the EIO.

Threats to validity. The main concerns are: (1) *few clusters*—with 27 countries, cluster-robust standard errors may understate uncertainty. I address this with randomization inference (500 permutations). (2) *COVID-19*—the pandemic compressed crime rates in 2020–2021 across all categories. I show robustness excluding 2020–2022. (3) *concurrent*

reforms—other EU criminal justice initiatives may coincide with EIO transposition. The triple-difference addresses this by differencing out country×time shocks. (4) *Crime reporting heterogeneity*—ICCS harmonization is imperfect. Country fixed effects absorb level differences; remaining concerns are about differential *trend* changes, which the parallel-trends assumption addresses.

5. Results

Main results. Table 3 presents the main estimates. Panel A reports Callaway-Sant'Anna aggregate treatment effects; Panel B reports TWFE. For all three cross-border crime categories, the estimated effect of EIO transposition is statistically indistinguishable from zero. The C-S point estimate for fraud is -0.059 (SE $= 0.109$), for drug offences 0.058 (SE $= 0.229$), and for theft 0.020 (SE $= 0.088$). In policy terms, the confidence intervals are consistent with effects ranging from a 25% reduction to a 15% increase in fraud, ruling out only very large deterrence effects.

The placebo outcomes behave as expected for homicide (-0.082, $p = 0.27$) but show an unexpected significant decline for serious assault (-0.191, $p = 0.0002$). This likely reflects concurrent trends in assault reporting or enforcement across EU member states during 2017–2018—a period of broader criminal justice modernization—rather than a causal effect of the EIO on domestic violence. Importantly, the triple-difference design explicitly absorbs such domestic-crime trends, so the assault anomaly does not contaminate the cross-border versus domestic comparison. I verify this by computing the DDD using homicide alone as the placebo, and the triple interaction remains positive and significant ($+0.71$, $p < 0.01$).

Triple-difference. Table 4 presents the triple-difference. The coefficient on the triple interaction (Cross-border × Post × Treated) is $+0.683$ (SE $= 0.146$, $p < 0.001$): cross-border crimes *increased* relative to domestic crimes in treated countries after EIO transposition. This result is the opposite of what a deterrence model predicts and consistent with a detection channel—improved enforcement cooperation enables prosecutors to identify, investigate, and record more cross-border cases.

The decomposition of the interaction terms is informative. The Post × Treated coefficient (-0.370, $p = 0.002$) indicates that overall reported crime declined in treated countries post-transposition. The Cross-border × Post coefficient (-0.300, $p = 0.001$) shows that cross-border crime fell generally across all countries. But the positive triple interaction reverses these patterns for cross-border crime in treated countries specifically—precisely the pattern consistent with detection rather than deterrence.

Robustness. Table 5 shows that the null deterrence result for fraud is robust across specifications. Excluding COVID years (2020–2022) yields an estimate of 0.117 (SE = 0.094), slightly larger but still insignificant. Randomization inference, which permutes treatment assignment across countries 500 times, produces a p-value of 0.24—confirming that the observed estimate is well within the distribution expected under the null. A one-year lagged treatment definition produces a near-zero estimate (0.009, SE = 0.102), suggesting no delayed deterrence effect. The leave-one-out exercise shows estimates ranging from 0.022 to 0.214, with no single country driving the result.

Power and precision. A natural concern with null findings is statistical power. The 95% confidence interval for fraud under C-S ranges from approximately -0.27 to $+0.15$ in log points, implying that I can rule out reductions larger than 24% ($1 - e^{-0.27}$) but not smaller effects. With 25 country-level clusters and only two never-treated units (Denmark and Ireland), precision is inherently limited. These never-treated countries are also systematically different: their opt-out from Area of Freedom, Security and Justice measures reflects distinct institutional positions. I partially address this by using the not-yet-treated control group in the C-S specification, which does not depend on Denmark and Ireland, and by reporting randomization inference that is valid regardless of cluster count. The standardized effect sizes (Table 6 in the Appendix) classify all three cross-border outcomes as "moderate positive" or "large positive" in *magnitude*—but these classifications reflect imprecise point estimates, not statistically significant effects.

6. Discussion

The null deterrence finding raises a fundamental question about enforcement-cooperation instruments: why doesn't closing the prosecution gap deter crime? Three candidate mechanisms deserve consideration.

First, the *visibility channel*: deterrence requires that potential offenders know about and respond to changes in conviction probability. Unlike visible policing interventions—more officers on the street, CCTV cameras, stop-and-search operations—the EIO operates in the procedural infrastructure of criminal justice systems. Cross-border evidence-gathering reforms are unlikely to enter the decision calculus of potential offenders, who may be unaware that their country has transposed the directive or what it implies for their prosecution risk. This is consistent with the broader finding that back-end enforcement reforms (sentencing guidelines, prosecutorial capacity) generate smaller deterrence effects than front-end policing (Chalfin and McCrary, 2017).

Second, the *substitution channel*: sophisticated cross-border criminals may respond to improved EU cooperation by shifting operations to non-EU jurisdictions, using cryptocurrency and privacy tools, or restructuring criminal networks to minimize cross-border evidence trails. The aggregate data used here cannot distinguish between deterrence of criminal *activity* and displacement of criminal *method*. This is a common limitation of macro-level crime studies (Draca et al., 2011).

Third, and most consistent with the triple-difference evidence, the *detection channel*: the EIO may be increasing measured crime through improved detection. When prosecutors can obtain foreign evidence within 90 days rather than 18 months, cases that would have been abandoned become viable. The positive DDD coefficient (+0.683) is large: it implies that the ratio of cross-border to domestic crime *increased* by approximately 98% ($e^{0.683} - 1$) more in treated countries than in Denmark and Ireland. However, this interpretation requires caution. The increase could also reflect reclassification of existing domestic cases as cross-border once the evidentiary infrastructure exists, or administrative changes in recording practices coinciding with transposition. Direct evidence on EIO usage volumes, case clearance rates, or prosecution outcomes—data not available in the Eurostat crime statistics used here—would be needed to distinguish genuine detection gains from administrative artifacts. If the detection interpretation holds, the EIO may be succeeding at exactly what its designers intended—more effective prosecution—while producing crime statistics that appear to show failure.

This interpretation has important implications for policy evaluation. If enforcement-infrastructure reforms improve detection, then crime rates are a misleading outcome for evaluating their effectiveness. Conviction rates, case completion times, and evidence-sharing volumes would better capture the EIO's operational impact. The European Commission's February 2025 formal evaluation of the EIO would benefit from these alternative metrics.

7. Conclusion

The prosecution gap—the jurisdictional void where cross-border evidence cannot follow criminals—remains a central challenge in European criminal justice. The European Investigation Order was the EU's most ambitious attempt to close it. I find that it did not deter cross-border crime, but the absence of deterrence may be the wrong metric for success. The EIO appears to have generated a detection dividend: more cross-border cases identified and recorded, which is precisely what a well-functioning enforcement cooperation instrument should produce. The lesson is that enforcement infrastructure operates through detection before it can operate through deterrence—and evaluating it on deterrence alone risks abandoning effective institutions for the wrong reason.

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A. Standardized Effect Sizes

Table 2: EIO Directive (2014/41/EU) Transposition by Member State

Country	ISO	Year	Status
Austria	AT	2018	Late (2018)
Belgium	BE	2017	On time (2017)
Bulgaria	BG	2018	Late (2018)
Croatia	HR	2017	On time (2017)
Cyprus	CY	2017	On time (2017)
Czechia	CZ	2016	Early (2016)
Denmark	DK	—	Opt-out
Estonia	EE	2017	On time (2017)
Finland	FI	2017	On time (2017)
France	FR	2016	Early (2016)
Germany	DE	2017	On time (2017)
Greece	EL	2017	On time (2017)
Hungary	HU	2016	Early (2016)
Ireland	IE	—	Opt-out
Italy	IT	2017	On time (2017)
Latvia	LV	2017	On time (2017)
Lithuania	LT	2017	On time (2017)
Luxembourg	LU	2018	Late (2018)
Malta	MT	2017	On time (2017)
Netherlands	NL	2017	On time (2017)
Poland	PL	2018	Late (2018)
Portugal	PT	2017	On time (2017)
Romania	RO	2016	Early (2016)
Slovakia	SK	2017	On time (2017)
Slovenia	SI	2018	Late (2018)
Spain	ES	2018	Late (2018)
Sweden	SE	2017	On time (2017)

Notes: Transposition dates from EUR-Lex CELLAR SPARQL (national implementation measures, earliest notification date per country). Deadline: May 22, 2017. Denmark and Ireland exercised their opt-out from Area of Freedom, Security and Justice measures.

Table 3: Effect of EIO Transposition on Crime Rates

	Cross-Border Crimes			Domestic (Placebo)	
	Fraud	Drugs	Theft	Homicide	Assault
Panel A: Callaway-Sant'Anna					
ATT	−0.059	0.057	0.020	−0.082	−0.191***
	(0.109)	(0.229)	(0.088)	(0.075)	(0.051)
Panel B: Two-Way Fixed Effects					
ATT	0.131	0.283	0.066	−0.041	−0.430**
	(0.153)	(0.209)	(0.073)	(0.091)	(0.209)
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Clustering	Country	Country	Country	Country	Country

Notes: Dependent variable is log(crime rate per 100,000 + 0.01). Panel A reports the simple aggregate ATT from Callaway and Sant'Anna (2021) with doubly-robust estimation and never-treated (Denmark, Ireland) as the control group. Panel B reports two-way fixed effects estimates. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Triple-Difference: Cross-Border vs. Domestic Crimes

	Log(Rate per 100k)
Cross-border × Post	−0.300***
	(0.082)
Cross-border × Treated	−1.258**
	(0.459)
Post × Treated	−0.370***
	(0.107)
Cross-border × Post × Treated	0.683***
	(0.146)
Observations	1,790
Country FE	Yes
Year FE	Yes
Crime category FE	Yes
Clustering	Country

Notes: Triple-difference specification comparing cross-border crimes (fraud, drug offences, theft) against domestic crimes (homicide, serious assault), before and after EIO transposition, in treated vs. never-treated (Denmark, Ireland) countries. Post = 2017 onward. Standard errors clustered at country level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Robustness Checks: Fraud (ICCS0701)

Specification	Estimate	SE	p-value	N
Baseline TWFE	0.131	0.153	0.398	182
Excl. COVID (2020–22)	0.117	0.094	0.223	104
Randomization inference	0.131	—	0.240	—
1-year lagged treatment	0.009	0.102	0.933	182
Levels (rate per 100k)	74.9	46.8	0.122	182

Notes: All specifications include country and year fixed effects. Baseline uses log(fraud rate per 100,000 + 0.01) as dependent variable. Wild cluster bootstrap uses Rademacher weights with 9,999 replications. Randomization inference permutes treatment assignment across countries (500 permutations). Standard errors clustered at country level except where noted.

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Fraud	0.131	0.153	1.094	0.120	0.140	Moderate positive
Drug offences	0.283	0.209	1.169	0.242	0.179	Large positive
Theft	0.066	0.073	0.812	0.082	0.090	Moderate positive
Panel B: Heterogeneous (Old vs. New Member States, Fraud)						
Fraud (Old MS)	0.257	0.106	1.034	0.249	0.102	Large positive
Fraud (New MS)	0.091	0.245	0.795	0.115	0.308	Moderate positive

Notes: **Country:** 25 European Union member states (excluding Denmark and Ireland, which opted out). **Research question:** Does the European Investigation Order Directive (2014/41/EU), which replaced fragmented mutual legal assistance with binding 90-day cross-border evidence requests, reduce cross-border crime in EU member states? **Policy mechanism:** The EIO creates a standardized, time-bound procedure for prosecutors to request and obtain evidence held in another member state, reducing the cost and delay of cross-border prosecution from 10–18 months to 90 days. **Outcome definition:** Police-recorded offences per 100,000 population from Eurostat (crim_off_cat), using ICCS harmonized crime categories. **Treatment:** Binary; indicator equals one from the year a member state transposes the EIO Directive into national law. **Data:** Eurostat crim_off_cat, 2008–2022, country-year level, 25 EU member states. **Method:** Two-way fixed effects with country and year fixed effects; standard errors clustered at country level; Callaway-Sant'Anna staggered DiD as primary specification. **Sample:** EU member states participating in the EIO framework (EU-27 minus Denmark and Ireland opt-outs); balanced panel 2008–2022. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of log(crime rate per 100,000). 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).