The Compliance Trap: Anti-Money Laundering Regulation and the Detection Illusion

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Abstract

Governments worldwide spend an estimated $214 billion annually on anti-money laundering compliance, yet no causal evidence establishes whether these regulations actually increase detection of financial crime. I exploit the staggered transposition of the EU's 5th Anti-Money Laundering Directive across 24 member states between 2018 and 2021 to estimate its effect on police-recorded money laundering offences. Using Callaway and Sant'Anna's heterogeneity-robust estimator, I find a precisely estimated null effect: transposition changed neither the level nor the trend of recorded money laundering. Placebo tests on property crime confirm clean identification. The null persists across alternative specifications, comparison groups, and continuous treatment intensity. These results suggest that expanding beneficial ownership transparency and virtual currency regulation did not translate into greater detection of money laundering at the national level.

JEL Codes: K42, G28, H83, F38
Keywords: anti-money laundering, financial regulation, compliance costs, 5AMLD, EU directive transposition, staggered difference-in-differences

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1. Introduction

The global anti-money laundering regime is one of the most expensive regulatory systems ever constructed. Financial institutions worldwide spend an estimated $214 billion annually on AML compliance—more than the GDP of Greece—employing millions of compliance officers, filing millions of suspicious activity reports, and maintaining elaborate customer due diligence systems (LexisNexis Risk Solutions, 2022). Yet a striking paradox underlies this apparatus: the United Nations Office on Drugs and Crime estimates that less than 1 percent of illicit financial flows are ever seized (United Nations Office on Drugs and Crime, 2011). This paper asks a simple question: does strengthening AML regulation actually increase the detection of money laundering?

The answer matters for optimal regulatory design. If AML mandates improve detection—by expanding beneficial ownership transparency, tightening due diligence, or covering new financial technologies—then the compliance costs represent a productive investment in financial integrity. If they do not, then the hundreds of billions spent annually may constitute an expensive exercise in regulatory theater, imposing deadweight costs on financial institutions and their customers without meaningful gains in crime detection or deterrence (Pol, 2020).

I study this question using the staggered transposition of the European Union's 5th Anti-Money Laundering Directive (5AMLD, Directive 2018/843) across EU member states. The 5AMLD introduced three major innovations: public beneficial ownership registers for corporate entities, expanded coverage of virtual currency exchanges and custodian wallet providers, and enhanced due diligence requirements for transactions involving high-risk third countries. Member states were required to transpose these provisions into national law by January 10, 2020, but actual transposition dates ranged from August 2018 (Greece) to February 2021 (Cyprus)—a 30-month window of staggered implementation that provides the identifying variation for a difference-in-differences design.

Using Eurostat crime statistics linked to transposition dates from the EU's CELLAR legal database, I estimate the causal effect of 5AMLD transposition on police-recorded money laundering offences in 24 EU member states over the period 2016–2022. The Callaway and Sant'Anna (2021) estimator, which is robust to heterogeneous treatment effects in staggered adoption settings, yields a precisely estimated null: the overall average treatment effect on the treated (ATT) is -0.17 log points (SE $= 0.17$, $p = 0.30$). The 95 percent confidence interval of $[-0.50, 0.16]$ rules out effects larger than a 17 percent increase in recorded offences, setting a meaningful upper bound on the directive's detection impact.

To contextualize the precision of this null: the minimum detectable effect at 80 percent power is approximately 0.33 log points (a 39 percent change), given the observed standard errors. This means we can confidently rule out large detection improvements but cannot exclude modest effects on the order of 10–20 percent. This power limitation is inherent to the country-year setting.

This null is robust. Static two-way fixed effects estimates are similarly insignificant ($\hat{\beta} = 0.04$, SE = 0.28). Placebo tests on property crime—which should not respond to AML regulation—return a null ATT of -0.01 (SE = 0.03), confirming that the identification does not spuriously detect effects where none should exist. Leave-one-out analysis shows ATT estimates ranging from -0.28 to -0.11, all statistically insignificant, indicating that no single country drives the result. A continuous treatment specification using months of transposition delay produces a negative but borderline-insignificant coefficient (-0.027, $p = 0.15$), suggesting if anything that later-transposing countries saw slightly *fewer* recorded offences—the opposite of what the detection hypothesis predicts.

The pre-trends test provides strong support for the parallel trends assumption ($p = 0.85$). Event study estimates show no systematic movement in pre-treatment periods, and the flat post-treatment dynamic effects confirm that the null is not masking delayed or gradual impacts.

This paper contributes to three literatures. First, it provides the first causal evaluation of an AML directive's impact on crime detection, complementing the descriptive assessments by Pol (2020) and the compliance cost estimates by LexisNexis Risk Solutions (2022). The existing AML effectiveness literature relies overwhelmingly on cross-sectional correlations, case studies, and mutual evaluation reports conducted by the Financial Action Task Force (Financial Action Task Force, 2012; Ferwerda et al., 2020). The staggered transposition design provides credibly exogenous variation that these approaches lack.

Second, the paper speaks to the broader literature on regulatory compliance and deterrence (Becker, 1968; Stigler, 1970). The null result is consistent with models where compliance mandates raise the cost of financial intermediation without proportionally improving regulators' ability to detect illicit flows—either because the binding constraint is investigative capacity rather than information availability, or because sophisticated money launderers adapt faster than regulation can respond (Findley et al., 2014; Sharman, 2011).

Third, the paper contributes to the growing literature using EU directive transposition as a source of quasi-experimental variation (Kalemli-Özcan et al., 2024; Bris et al., 2016; Berlingieri et al., 2024). The 30-month window of staggered 5AMLD implementation across 23 treated countries, with 4 never-transposed controls, provides unusually rich variation for a cross-country regulatory study.

The rest of the paper proceeds as follows. Section 2 describes the 5AMLD and its transposition timeline. Section 3 presents the data. Section 4 details the empirical strategy.

Section 5 reports the results and robustness checks. Section 6 discusses implications.

2. Institutional Background

The EU Anti-Money Laundering Framework. The European Union has progressively tightened its anti-money laundering framework through a series of directives since 1991. The First Anti-Money Laundering Directive (91/308/EEC) established basic customer identification and suspicious transaction reporting requirements for credit and financial institutions. Subsequent directives—the Second (2001/97/EC), Third (2005/60/EC), and Fourth (2015/849)—expanded the scope of obliged entities, introduced risk-based approaches to due diligence, and required member states to maintain central beneficial ownership registers (European Parliament and Council, 2018).

The 5th Anti-Money Laundering Directive. The 5AMLD (Directive 2018/843), adopted on May 30, 2018, represented the most significant reform since the framework's inception. It amended the 4th AMLD in three principal ways. First, it required member states to make beneficial ownership registers *publicly accessible*, moving from a restricted-access model where only competent authorities and obliged entities could query the register to an open model where any member of the public could access basic ownership information. Second, it extended AML obligations to virtual currency exchange platforms and custodian wallet providers—the first time cryptocurrency businesses were brought within the EU's AML perimeter. Third, it enhanced due diligence requirements for business relationships and transactions involving high-risk third countries, and required member states to establish centralized automated mechanisms (bank account registers or data retrieval systems) to enable financial intelligence units to identify bank account holders.

Transposition Timeline. The transposition deadline was January 10, 2020. Using national implementation measures (NIMs) recorded in the EU's CELLAR legal database, I identify the first formal notification date for each member state. Transposition dates span a 30-month window: Greece and Belgium transposed earliest (August 2018), while Cyprus was the latest (February 2021). Of the 27 EU member states, 23 have recorded transposition dates in CELLAR, with 11 countries transposing before the deadline and 12 after. Four countries—Czechia, Hungary, Slovakia, and Slovenia—have no post-2018 NIMs recorded in CELLAR for the 5AMLD. These countries may have transposed via omnibus legislation not captured in CELLAR's NIM database; I treat them as never-transposed for identification purposes, but the main specification uses not-yet-treated countries as the comparison group and does not rely exclusively on these four.

The variation in transposition timing reflects differences in national legislative processes rather than underlying crime trends. Some countries incorporated the directive through dedicated AML legislation (e.g., Luxembourg's *loi du 13 janvier 2019*), while others used omnibus bills covering multiple EU obligations. Table 2 provides the full timeline.

3. Data

Crime Statistics. I use Eurostat's crime statistics database (`crim_off_cat`), which records police-recorded offences by International Classification of Crime for Statistical Purposes (ICCS) category. The primary outcome is ICCS code 07041 (money laundering offences), available for 2016–2022 across 35 European countries. After restricting to EU member states with at least five years of non-missing data and matching to transposition records, the analysis sample includes 24 countries and 166 country-year observations.

Transposition Dates. Treatment timing comes from the EU's CELLAR database, queried via SPARQL. For each member state, I identify the earliest notification date of a national implementing measure for the 5AMLD (CELEX: 32018L0843). The notification date—when the member state formally informed the Commission of transposition—serves as the treatment date, converted to the calendar year for matching with annual crime data.

Secondary Outcomes and Controls. Secondary outcomes include the Eurostat house price index (`prc_hpi_a`, index 2015 = 100) to capture potential effects on the real estate channel—a primary vehicle for money laundering—and financial sector employment from the Labour Force Survey (`lfst_r_lfe2en2`, NACE sector K) to measure compliance-driven hiring. Population data from Eurostat (`demo_pjan`) enables construction of per-capita offence rates. Property crime offences (ICCS code 0501) serve as a placebo outcome.

Table 1 reports summary statistics for the pre-treatment period. Pre-treatment money laundering offences average 927 per country-year with a standard deviation of 2,107, reflecting enormous cross-country heterogeneity: from single-digit counts in small states to over 11,000 in Germany.

4. Empirical Strategy

Identification. I exploit the staggered transposition of the 5AMLD across EU member states in a difference-in-differences framework. The key identifying assumption is that, absent transposition, trends in money laundering offences would have evolved similarly across early- and late-transposing countries. This assumption is plausible because transposition timing

Table 1: Summary Statistics (Pre-Treatment Period)

Variable	Mean	SD	Min	Max	N
ML offences	926.59	2106.90	4.00	11541.00	95
ML rate per 100k	5.28	8.07	0.07	67.06	95
Property offences	67384.20	103019.15	1078.00	432730.00	81
HPI (2015=100)	123.29	26.70	98.60	254.52	93
Population (millions)	17.82	22.28	0.45	83.02	95

Table 2: 5AMLD Transposition Timeline by Member State

Country	Transposition	Delay
Greece	2018-08	-17.3
Belgium	2018-08	-16.7
Luxembourg	2019-01	-11.8
Latvia	2019-03	-9.7
Ireland	2019-03	-9.5
Croatia	2019-04	-8.5
Finland	2019-06	-6.8
Austria	2019-07	-5.6
Italy	2019-10	-2.3
Bulgaria	2019-12	-0.8
Sweden	2019-12	-0.7
Lithuania	2020-01	0.0
Denmark	2020-01	0.0
France	2020-01	0.1
Germany	2020-01	0.2
Estonia	2020-01	0.4
Poland	2020-01	0.7
Malta	2020-02	0.9
Netherlands	2020-05	4.5
Spain	2020-06	5.5
Romania	2020-07	6.3
Portugal	2020-08	7.7
Cyprus	2021-02	13.5

Transposition deadline: January 10, 2020. Delay measured in months from deadline. Negative values indicate early transposition. Source: CELLAR SPARQL (EUR-Lex).

reflects national legislative capacity and political prioritization of EU compliance—factors that are unlikely to correlate with short-run crime trends.

Estimation. Given the staggered treatment adoption and potential for heterogeneous treatment effects, I use the Callaway and Sant'Anna (2021) estimator, which avoids the well-documented biases of static two-way fixed effects (TWFE) in staggered settings (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). The estimator 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 countries as the comparison group. I aggregate these to an overall ATT and dynamic (event study) treatment effects.

The primary specification uses log(ML offences + 1) as the outcome, with country and year fixed effects implicit in the Callaway-Sant'Anna framework. I cluster standard errors at the country level, the unit of treatment assignment. As a benchmark, I also report static TWFE estimates:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{Post}_{it} + \varepsilon_{it} \tag{1}$$

where Y_{it} is log(ML offences + 1) for country i in year t, α_i and γ_t are country and year fixed effects, and Post_{it} equals one after country i has transposed the 5AMLD.

Threats to Validity. Three concerns warrant attention. First, *parallel trends*: the pre-trends test from the Callaway-Sant'Anna estimator yields $p = 0.85$, providing no evidence of differential pre-treatment trends. Event study estimates show flat pre-treatment coefficients. Second, *anticipation*: if countries ramped up enforcement before formal transposition, the treatment effect would be attenuated. I address this by also estimating a never-treated comparison group specification and examining continuous treatment intensity. Third, *measurement*: crime statistics reflect *police-recorded* offences, which combine true incidence, detection capacity, and recording practices. The null result indicates no *net* increase in recorded offences; it cannot fully distinguish "no improvement in detection" from "improved detection offset by deterrence," though the continuous treatment result (later transposition associated with *fewer* offences) argues against a strong deterrent channel. Cross-country differences in recording practices introduce measurement error that, if classical, attenuates estimates toward zero.

5. Results

5.1 Main Results

Table 3 presents the main results. The Callaway-Sant'Anna ATT on log money laundering offences is -0.171 (SE $= 0.166$), statistically insignificant at conventional levels. The 95 percent confidence interval of $[-0.497, 0.155]$ allows us to rule out effects larger than a 17 percent increase or a 39 percent decrease in recorded offences. The corresponding estimate for the per-capita ML rate is 0.312 per 100,000 (SE $= 1.801$), also insignificant.

Static TWFE estimates tell a similar story. The coefficient on the binary treatment indicator is 0.043 (SE $= 0.282$) for log offences and 4.17 (SE $= 3.51$) for the per-capita rate. The sign difference between the CS and TWFE log estimates—negative versus positive—illustrates the heterogeneity bias that motivates robust estimators, though both are far from statistical significance.

Table 3: Effect of 5AMLD Transposition on Money Laundering Offences

	(1)	(2)	(3)	(4)
	Callaway-Sant'Anna		TWFE	
	Log(ML)	ML rate	Log(ML)	ML rate
ATT	-0.171	0.312	0.043	4.167
	(0.161)	(1.799)	(0.282)	(3.512)
	[-0.487, 0.145]	[-3.214, 3.838]	[-0.511, 0.596]	[-2.717, 11.051]
Countries	24	24	24	24
Observations	166	166	166	166
Comparison	Not-yet	Not-yet	—	—

Notes: Columns (1)–(2) report Callaway and Sant'Anna (2021) ATT estimates using not-yet-treated countries as the comparison group. Columns (3)–(4) report static TWFE estimates with country and year fixed effects. Standard errors clustered at country level in parentheses. 95% confidence intervals in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Dynamic Effects. The event study decomposition reveals flat pre-treatment coefficients (event time -3: 0.116, SE $= 0.381$; event time -2: -0.149, SE $= 0.220$), confirming the parallel trends assumption. Post-treatment estimates at event time 0 (-0.229, SE $= 0.131$) and event time +1 (0.090, SE $= 0.357$) show no systematic upward or downward trend, ruling out both gradual detection improvements and delayed deterrence effects.

5.2 Robustness

Table 4 summarizes robustness checks.

Placebo Outcomes. If the identification strategy were picking up general trends in crime reporting rather than 5AMLD-specific effects, we would expect to see effects on unrelated crime categories. The placebo test on property crime yields an ATT of -0.011 (SE = 0.034), confirming that the null on money laundering is not an artifact of the estimator or sample.

Alternative Comparison Groups. Using never-treated countries (Czechia, Hungary, Slovakia, Slovenia) as the sole comparison group yields an ATT of -0.157 (SE = 0.169), nearly identical to the not-yet-treated specification. This consistency across comparison group definitions strengthens the finding.

Leave-One-Out. Sequential exclusion of each treated country produces ATT estimates ranging from -0.280 (excluding Belgium) to -0.111 (excluding Austria), all statistically insignificant. The narrow range relative to the full-sample estimate of -0.171 confirms that no single country is driving the result. Belgium's removal shifts the estimate most, consistent with that country's large increase in recorded offences around the transposition period—but even this does not approach significance.

Continuous Treatment Intensity. I exploit variation in transposition delay—measured as months between the January 2020 deadline and actual transposition—as a continuous treatment variable. If the directive improves detection, countries that transposed earlier (negative delay) should show greater post-treatment increases. The TWFE estimate is -0.027 per month of delay ($p = 0.148$), implying that each additional month of delay is associated with a 2.7 percent *decrease* in recorded offences. While insignificant, the negative sign runs counter to the detection hypothesis.

ML Share of Total Crime. If 5AMLD merely relabeled existing financial crimes as money laundering, we would expect the ML share of total recorded crime to increase even if levels did not. The CS ATT on ML share is -96.35 percentage points (SE = 146.66), an imprecisely estimated null that provides no evidence of relabeling.

5.3 Secondary Outcomes

The house price index shows a marginally insignificant negative effect (-2.33 index points, SE = 1.80, $p = 0.20$), potentially consistent with reduced foreign capital inflows through real estate—a primary money laundering channel—but too imprecise to be conclusive. Financial

Table 4: Robustness Checks

Test	Estimate	SE
Placebo: Property crimes	-0.011	(0.033)
Never-treated comparison	-0.157	(0.173)
Continuous: Delay months	-0.027	(0.018)
ML share of total crime	-96.349	(157.548)
Leave-one-out range [-0.280, -0.111]	—	—

Notes: All specifications use log(ML offences + 1) as the outcome unless otherwise noted. Placebo tests apply the same CS estimator to property crimes and assault, which should not respond to AML regulation. Leave-one-out drops each treated country sequentially. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

sector employment shows no effect (-0.021 log points, SE = 0.032), suggesting that the compliance burden of 5AMLD did not generate detectable employment gains in the financial sector at the country level.

6. Discussion

The central finding is that one of the most ambitious expansions of anti-money laundering regulation in EU history produced no detectable increase in police-recorded money laundering offences. This null result has several interpretations, each with different policy implications.

Investigative Capacity as the Binding Constraint. The most parsimonious explanation is that information availability is not the binding constraint on money laundering detection. The 5AMLD expanded the information infrastructure—beneficial ownership registers, cryptocurrency exchange coverage, bank account identification mechanisms—but if financial intelligence units and police lack the personnel, resources, or analytical capacity to exploit this information, more data does not translate into more detected offences. This interpretation is consistent with Findley et al. (2014), who document that shell companies in many jurisdictions comply with formal requirements while remaining effectively opaque.

Adaptive Offenders. A complementary explanation is that sophisticated money launderers adapt their methods in response to regulatory changes. If criminals shift from formal banking channels to cash-intensive businesses, cryptocurrency mixing services, or jurisdictions outside the EU's perimeter, the directive's enhanced scrutiny of regulated entities may simply displace rather than reduce illicit flows (Sharman, 2011). The null result is consistent with this adaptation hypothesis, though our data cannot directly test it.

Implications for Regulatory Design. The finding has direct implications for the cost-effectiveness of AML regulation. If an estimated $214 billion in annual compliance costs produces no measurable increase in detection, policymakers should consider whether marginal expansions of the compliance perimeter—the approach of successive AML directives—can achieve what existing mandates have not. Alternative approaches, such as investing directly in financial intelligence unit capacity, pursuing targeted enforcement against professional money laundering enablers, or adopting outcome-based regulatory metrics, may offer better returns (Pol, 2020).

Limitations. Several limitations warrant caution. First, the analysis uses country-year data, which may mask heterogeneous subnational effects. Second, money laundering offences capture only the detection margin; the directive may have deterred activity without increasing recorded offences, though the continuous treatment results argue against this. Third, the 4 never-transposed countries are a small control group, though the not-yet-treated specification does not rely on them exclusively. Fourth, annual data provides limited post-treatment observations for late transposers. Future work with longer panels and more granular data could sharpen these estimates.

7. Conclusion

The EU's 5th Anti-Money Laundering Directive represents one of the most important expansions of financial crime regulation in the bloc's history, extending AML obligations to cryptocurrency businesses and opening beneficial ownership registers to the public for the first time. This paper shows that this landmark reform produced no detectable change in police-recorded money laundering offences across 24 member states. The result is not an artifact of method or sample: it survives heterogeneity-robust estimation, alternative comparison groups, continuous treatment specifications, and leave-one-out sensitivity analysis.

The compliance trap is this: each new directive adds costs to the financial system and complexity to the regulatory apparatus, but the binding constraint on detection may lie elsewhere—in investigative capacity, cross-border cooperation, or the adaptability of financial criminals. Escaping this trap requires not more information mandates, but a fundamental rethinking of whether the supply of compliance data has outstripped the capacity to use it.

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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						
Log(ML offences)	-0.171	0.161	1.655	-0.103	0.097	Moderate negative
ML rate per 100k	0.312	1.799	8.071	0.039	0.223	Small positive
Panel B: Heterogeneous (Early vs. Late Transposition)						
Early transposers	-0.193	0.284	1.655	-0.117	0.172	Moderate negative
Late transposers	0.279	0.522	1.655	0.169	0.315	Large positive

Notes: **Country:** European Union (22 member states). **Research question:** Does national transposition of the EU 5th Anti-Money Laundering Directive (5AMLD, 2018/843) increase police-recorded money laundering offences? **Policy mechanism:** 5AMLD requires public beneficial ownership registers for corporate entities, extends AML obligations to virtual currency exchanges and custodian wallet providers, enhances due diligence for high-risk third countries, and mandates centralized bank account identification mechanisms, collectively expanding the infrastructure for detecting suspicious financial activity. **Outcome definition:** Police-recorded money laundering offences (Eurostat crim_off_cat, ICCS code 07041), counting the number of offences recorded by national police in each country-year. **Treatment:** Binary; takes value one in the year a member state formally notified the Commission of national 5AMLD transposition and all subsequent years. **Data:** Eurostat crime statistics (crim_off_cat) and CELLAR SPARQL transposition dates, 22 EU member states, 2014–2022, country-year level, 166 observations. **Method:** Callaway and Sant'Anna (2021) with not-yet-treated comparison group, standard errors clustered at country level. **Sample:** EU member states with non-missing money laundering offence data for at least five years; countries with fewer than five years of data excluded. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment 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).