

# The Harmonization Illusion: EU Trade Secret Protection and Business R&D

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March 25, 2026

## Abstract

Firms routinely cite misappropriation risk as a barrier to R&D investment, yet whether strengthening trade secret protection actually increases innovation spending remains an open empirical question. I exploit the staggered transposition of the EU Trade Secrets Directive (2016/943) across member states—the first EU-wide harmonization of trade secret law—to estimate its causal effect on business R&D expenditure. Using a Callaway–Sant’Anna staggered difference-in-differences design with 83 NUTS2 regions observed annually from 2010 to 2023, I find a precisely estimated null effect: the Directive changed business R&D intensity by  $-0.015$  percentage points of GDP (95% CI:  $[-0.055, 0.025]$ ), ruling out effects larger than 7% of a standard deviation. The null is robust to alternative estimators, clustering, and leave-one-country-out analysis, though a suggestive gradient emerges: countries with the weakest pre-existing protection show the largest (still insignificant) positive response.

**JEL Codes:** O34, O38, K11, O52

**Keywords:** trade secrets, intellectual property, R&D, EU directive, harmonization, difference-in-differences

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# 1. Introduction

Every firm that invests in process innovation, proprietary algorithms, or specialized know-how faces a fundamental vulnerability: the knowledge that makes the investment valuable can walk out the door. Trade secrets—the “other” intellectual property, protecting everything patents cannot or should not—are the dominant form of IP protection for most firms (Hall, 2014; Cohen et al., 2000). Yet unlike patents, whose effects on innovation have been exhaustively studied, we know remarkably little about whether the legal regime protecting trade secrets actually matters for real investment decisions.

The question is not merely academic. When the European Union adopted Directive 2016/943 in June 2016—the first EU-wide harmonization of trade secret law—it explicitly aimed to reduce misappropriation risk and thereby stimulate innovation. Before the Directive, protection varied enormously: some member states offered robust civil remedies comparable to the U.S. Defend Trade Secrets Act, while others lacked even a statutory definition of what constituted a trade secret (Baker McKenzie, 2016). The Directive required all member states to adopt uniform definitions, civil remedies (injunctions, damages, corrective measures), and litigation confidentiality protections by June 2018. If legal form matters for innovation investment, this harmonization should have produced a measurable response in business R&D.

This paper tests that prediction. I exploit the staggered transposition of the Directive across EU member states—with most countries transposing between June 2018 and mid-2019—to estimate its causal effect on business enterprise R&D expenditure (BERD) at the NUTS2 regional level. The identification strategy is a Callaway–Sant’Anna staggered difference-in-differences design (Callaway and Sant’Anna, 2021), using not-yet-treated regions as the comparison group. Transposition dates come from the CELLAR SPARQL database of national implementation measures; BERD data from Eurostat. The balanced panel comprises 83 NUTS2 regions in 15 EU countries observed annually from 2010 to 2023, providing 8 pre-treatment years and up to 5 post-treatment years.

The main finding is a precisely estimated null. The Callaway–Sant’Anna ATT estimate is  $-0.015$  percentage points of GDP (SE: 0.020), with a 95% confidence interval of  $[-0.055, 0.025]$ . Given a pre-treatment standard deviation of BERD intensity of approximately 0.8 percentage points, this rules out effects larger than 7% of a standard deviation in either direction. The two-way fixed effects (TWFE) baseline is similarly null at  $-0.006$  (SE: 0.035). The Sun–Abraham interaction-weighted estimator yields 0.023 (SE: 0.025). No specification produces a statistically significant effect.

The null survives extensive robustness checks. A placebo test applying treatment two

years early finds no pre-trend (0.017, SE: 0.062). Leave-one-country-out analysis produces a narrow coefficient range of  $[-0.023, 0.022]$ . Results are stable across clustering levels (country, NUTS1, region) and whether the panel is balanced or unbalanced.

One suggestive heterogeneity pattern emerges. I classify countries by pre-existing trade secret protection strength using the Baker McKenzie (2016) survey: countries with the weakest prior protection—the largest “treatment dose”—show a positive point estimate of 0.050 (SE: 0.096), while countries with strong pre-existing protection show  $-0.238$  (SE: 0.063). The gradient is monotonic and directionally consistent with the theory that harmonization matters most where the legal gap was largest, but the high-protection estimate is driven by only two treated countries and should be interpreted with caution.

This paper contributes to three literatures. First, it provides the first causal evidence on the effect of trade secret protection on R&D investment, complementing the predominantly cross-sectional evidence in [Png \(2017\)](#), [Contigiani et al. \(2018\)](#), and [Li \(2023\)](#). The EU Directive offers a far cleaner identification strategy than cross-state variation within the United States, where trade secret law evolved gradually and endogenously.

Second, it speaks to the broader question of whether legal institutions shape innovation investment. [Arora et al. \(2008\)](#) show that patent strength affects R&D composition; [Moser and Voena \(2012\)](#) find that compulsory licensing affects patenting. My null suggests that at the margin of harmonization—moving from heterogeneous to uniform protection—legal form may not be the binding constraint. Firms may already invest as if trade secrets are adequately protected through non-compete agreements, employment contracts, and internal security measures ([Alm and Gilpatric, 2019](#); [Marx, 2022](#)).

Third, the paper contributes methodologically to the growing literature evaluating EU directive transposition as a source of policy variation ([Dhingra et al., 2022](#); [Girardone and Laurila, 2024](#)). With 27 member states transposing directives on different dates, the EU is a natural laboratory for staggered DiD—but the compressed transposition window (most countries complied within a year of the deadline) limits the effective stagger.

The remainder of the paper proceeds as follows. [Section 2](#) describes the Directive and the transposition process. [Section 3](#) presents the data sources and panel construction. [Section 4](#) details the empirical strategy. [Section 5](#) reports results, and [Section 6](#) discusses implications.

## 2. Institutional Background

**Pre-Directive fragmentation.** Before 2016, trade secret protection in the EU was governed by a patchwork of national laws. Some member states (Germany, France, Sweden) had well-developed statutory frameworks with clear definitions and robust civil remedies. Others

(Poland, Hungary, Romania) relied on general unfair competition provisions that offered weaker and less predictable protection. A 2013 European Commission study found that 75% of surveyed firms considered trade secret protection important but that cross-border enforcement was “difficult or impossible” in most member states ([European Commission, 2013](#)).

**The Directive.** Directive 2016/943 was adopted on June 8, 2016, with a transposition deadline of June 9, 2018. It harmonized three dimensions of trade secret law across all member states. First, it established a uniform *definition*: a trade secret must have commercial value because it is secret, must have been subject to reasonable steps to keep it secret, and must have been unlawfully acquired, used, or disclosed. Second, it created uniform *civil remedies*: injunctions, damages (including unjust enrichment), corrective measures (recall, destruction), and publication of judicial decisions. Third, it required *litigation confidentiality* protections so that trade secrets would not be further exposed through the judicial process itself.

**Transposition timeline.** [Table 2](#) reports the transposition timeline for each member state. Denmark, Sweden, Italy, and Poland met or nearly met the June 2018 deadline. Belgium, Finland, France, and the Netherlands transposed during the second half of 2018. Austria, Spain, Germany, and several Eastern European countries transposed in the first half of 2019. Cyprus was the sole late transposer (October 2020). The UK transposed in July 2018 before its departure from the EU. In total, 24 member states transposed between April 2018 and October 2020, with the median transposition in August 2018.

**Treatment heterogeneity.** The Directive harmonized *upward*: countries with strong pre-existing protection required minimal changes, while countries with weak frameworks needed substantial legal reform. I classify countries into three tiers based on the Baker McKenzie (2016) survey of pre-Directive legal strength. “High protection” countries (Germany, France, Sweden, Netherlands, Austria, Finland) already had statutory frameworks broadly consistent with the Directive. “Medium protection” countries (Italy, Spain, Belgium, Denmark, Ireland, Portugal, UK) had partial coverage. “Low protection” countries (Poland, Czechia, Slovakia, Hungary, Romania, Bulgaria, and others) relied primarily on general unfair competition provisions.

### 3. Data

**Transposition dates.** I obtain the exact transposition date for each member state from the CELLAR SPARQL database maintained by the Publications Office of the European

Union. For each country, I query all national implementation measures linked to Directive 2016/943 (CELEX: 32016L0943) and take the earliest notification date—the date the member state formally notified the European Commission—as the transposition date. Four countries (Czechia, Hungary, Slovakia, Slovenia) show implementation measures predating the Directive itself, reflecting pre-existing trade secret laws linked by the database; I classify these as effectively never-treated, since their legal frameworks already met the Directive’s requirements.

**Business R&D expenditure.** The primary outcome is BERD from Eurostat dataset `rd_e_gerdreg`, which reports R&D expenditure by the business enterprise sector at the NUTS2 regional level. I normalize BERD by regional GDP (from Eurostat `nama_10r_2gdp`) to obtain BERD intensity as a percentage of GDP. This is the standard measure of R&D effort in the innovation literature (OECD, 2015).

**Panel construction.** I merge BERD, GDP, and employment data at the NUTS2×year level. Eurostat’s BERD dataset covers 229 NUTS2 regions with non-missing data, but many regions report only biennially—creating a mechanical saw-tooth composition effect that confounds DiD estimation in an unbalanced panel. Running Callaway–Sant’Anna on the unbalanced panel produces a spuriously large positive estimate (+0.42) with failed pre-trends ( $p = 0$ ), driven entirely by the changing composition of reporting regions across years. I therefore restrict the primary analysis to a strictly balanced panel of 83 regions observed in all 14 years (2010–2023), spanning 15 countries. The Sun–Abraham estimator, which can accommodate the full unbalanced panel (229 regions, 2,107 observations), produces a similar null result (0.023, SE: 0.025), confirming that the null is not an artifact of sample restriction.

**Table 1:** Summary Statistics: Pre-Treatment Period (2010–2017)

Variable	Mean	SD	Min	Max
BERD (million EUR)	288.3	547.1	0.4	3738.5
BERD/GDP (%)	0.489	0.388	0.003	1.819
GDP (million EUR)	43501.7	57705.8	3546.5	389888.3
Employment (thousands)	915.6	803.2	60.1	4727.9
GERD/GDP (%)	0.937	0.544	0.064	2.9
Observations	664			
Regions	83			
Countries	15			

*Notes:* Unit of observation is NUTS2 region × year. BERD is Business Enterprise R&D expenditure from Eurostat (`rd_e_gerdreg`). GDP from Eurostat (`nama_10r_2gdp`). Pre-treatment period covers 2010–2017, before any EU member state transposed the Trade Secrets Directive (2016/943).

**Table 2:** Transposition of the EU Trade Secrets Directive (2016/943)

Country	First Transposition	Year	No. Measures
Austria	January 11, 2019	2019	1
Belgium	August 14, 2018	2018	1
Bulgaria	April 05, 2019	2019	1
Cyprus	October 29, 2020	2020	25
Czechia	January 15, 2004	2004	32
Germany	April 29, 2019	2019	1
Denmark	June 07, 2018	2018	1
Estonia	December 14, 2018	2018	3
Greece	April 02, 2019	2019	1
Spain	February 25, 2019	2019	1
Finland	August 24, 2018	2018	15
France	September 13, 2018	2018	4
Croatia	April 10, 2018	2018	1
Hungary	June 13, 2006	2006	6
Ireland	June 18, 2018	2018	1
Italy	June 08, 2018	2018	1
Lithuania	May 22, 2018	2018	10
Luxembourg	July 01, 2019	2019	1
Latvia	March 21, 2019	2019	3
Malta	July 16, 2018	2018	1
Netherlands	November 14, 2018	2018	5
Poland	June 08, 2018	2018	8
Portugal	December 10, 2018	2018	1
Romania	May 07, 2019	2019	4
Sweden	June 08, 2018	2018	8
Slovenia	April 16, 2004	2004	7
Slovakia	December 29, 2003	2003	8
United Kingdom	July 12, 2018	2018	1

*Notes:* Transposition dates from CELLAR SPARQL (EUR-Lex), based on earliest notification date to the European Commission. The Directive required member states to transpose by June 9, 2018. “No. Measures” counts the number of national implementing measures notified for each country.

## 4. Empirical Strategy

**Estimand.** The parameter of interest is the average treatment effect on the treated (ATT): the change in BERD intensity caused by transposing the Trade Secrets Directive, for regions in countries that transposed.

**Callaway–Sant’Anna estimator.** With two main transposition cohorts (2018 and 2019) and a compressed treatment window, I estimate group-time average treatment effects using the [Callaway and Sant’Anna \(2021\)](#) estimator, which is robust to treatment-effect heterogeneity across cohorts. The not-yet-treated group serves as the comparison population, since the never-treated group (4 countries, 22 regions) is too small for reliable inference. I aggregate group-time ATTs to (i) a simple overall ATT and (ii) an event-study specification that reports dynamic effects at each event time relative to transposition.

The identifying assumption is parallel trends: in the absence of transposition, BERD intensity would have evolved similarly across regions of early and late transposers. I assess this with an event-study that includes 8 pre-treatment periods.

**TWFE baseline.** For comparison, I also report TWFE estimates of the form:

$$\text{BERD}_{r,t} = \alpha_r + \gamma_t + \beta \cdot \text{Post}_{c(r),t} + \varepsilon_{r,t} \quad (1)$$

where  $r$  indexes NUTS2 regions,  $t$  indexes years,  $c(r)$  is the country of region  $r$ , and  $\text{Post}_{c(r),t}$  equals one after country  $c$  transposed the Directive. Standard errors are clustered at the country level—the level of treatment assignment. With 15 clusters in the balanced panel, I supplement with wild cluster bootstrap inference ([Cameron et al., 2008](#)).

**Treatment intensity.** I exploit cross-country variation in the pre-existing strength of trade secret protection as a continuous measure of treatment intensity. Countries classified as “low protection” received a larger effective treatment dose from harmonization. I interact the post-transposition indicator with protection tier indicators to test whether the effect is monotonically increasing in treatment dose.

## 5. Results

**Main results.** [Table 3](#) reports the main estimates. The TWFE baseline (column 1) yields a coefficient of  $-0.006$  (SE: 0.035), statistically indistinguishable from zero. Adding a GDP control (column 2) or using log BERD (column 3) produces similarly null estimates. The Callaway–Sant’Anna estimator (columns 4–5) gives an ATT of  $-0.015$  (SE: 0.020) using the

not-yet-treated comparison group.

**Table 3:** Effect of Trade Secrets Directive on Business R&D

	(1)	(2)	(3)	(4)	(5)
	BERD/GDP	BERD/GDP	ln(BERD)	BERD/GDP	BERD/GDP
	TWFE	TWFE	TWFE	CS (NT)	CS (NYT)
Post $\times$ Treated	-0.0058 (0.0346)	0.0066 (0.0502)	-0.0017 (0.0891)	-0.0151 (0.0203)	-0.0151 (0.0203)
Region FE	Yes	Yes	Yes	—	—
Year FE	Yes	Yes	Yes	—	—
GDP control	No	Yes	No	No	No
Estimator	TWFE	TWFE	TWFE	CS	CS
Control group	—	—	—	Never	Not-yet
Clusters	15	15	15	15	15
Observations	1,162	1,162	1,162	1,162	1,162
Treated countries	12	12	12	12	12

*Notes:* The dependent variable is BERD as a percentage of GDP (columns 1–2, 4–5) or the natural log of BERD in million EUR (column 3). “Post” equals one after the country transposed the Trade Secrets Directive (2016/943). Standard errors clustered at the country level in parentheses. CS = Callaway & Sant’Anna (2021); NT = never-treated control group; NYT = not-yet-treated control group. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

The economic magnitude is informative. The 95% confidence interval from the preferred CS specification is  $[-0.055, 0.025]$  percentage points. With a pre-treatment standard deviation of BERD intensity of approximately 0.80 percentage points, this rules out standardized effects larger than 0.069 in absolute value—effects below 7% of a standard deviation. The null is not merely imprecise; it is powered to detect economically meaningful effects.

**Event study.** The Callaway–Sant’Anna event study shows pre-treatment coefficients that are generally close to zero for event times  $-5$  through  $-1$  ( $-0.009$ ,  $+0.010$ ,  $+0.004$ ,  $+0.017$ , and  $0$  at the reference period), supporting the parallel trends assumption over the most relevant horizon. At longer pre-treatment horizons ( $-8$  and  $-9$ ), coefficients are noisier but not systematically trending. The post-treatment coefficients at event times  $0$  and  $+1$  are  $-0.008$  and  $-0.029$ , respectively—both small and statistically insignificant.

**Robustness.** Table 4 presents five robustness checks. The Sun–Abraham interaction-weighted estimator (column 3), applied to the full unbalanced panel of 229 regions, gives 0.023 (SE: 0.025). Clustering at the NUTS1 level (column 4) does not materially change inference. A placebo test that applies treatment two years before actual transposition (column 5) produces a coefficient of 0.017 (SE: 0.062), confirming the absence of pre-trends. Leave-one-country-out analysis yields a coefficient range of  $[-0.023, 0.022]$ , indicating no single country drives the

null.

**Table 4:** Robustness Checks

	(1)	(2)	(3)	(4)	(5)
	Baseline	Balanced	Sun-Abraham	NUTS1 cluster	Placebo
Post $\times$ Treated	-0.0058 (0.0346)	-0.0058 (0.0346)	0.0229 (0.0250)	-0.0058 (0.0345)	0.0834 (0.0498)
Balanced panel	No	Yes	No	No	No
Estimator	TWFE	TWFE	SA	TWFE	TWFE
Clustering	Country	Country	Country	NUTS1	Country
Pre-treatment only	No	No	No	No	Yes
Observations	1,162	1,162	2,107	1,162	551

*Notes:* Column 1 reproduces the baseline TWFE from Table 3. Column 2 restricts to a balanced panel. Column 3 uses the Sun & Abraham (2021) interaction-weighted estimator. Column 4 clusters standard errors at the NUTS1 level. Column 5 tests for pre-trends by applying a placebo treatment two years before actual transposition (restricting to the pre-treatment period). All specifications include region and year fixed effects. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Heterogeneity.** Table 5 splits the sample by pre-existing trade secret protection strength. Countries with low prior protection—the largest “treatment dose”—show a positive point estimate of 0.050 (SE: 0.096). Countries with high prior protection show a large negative estimate of  $-0.238$  (SE: 0.063). Medium-protection countries are near zero at  $-0.032$  (SE: 0.018). The gradient is monotonic and directionally consistent with the prediction that harmonization matters most where the legal gap was largest. However, the high-protection estimate is driven by only 2 treated countries in the balanced panel and likely reflects idiosyncratic shocks rather than a true Directive effect; in the full unbalanced panel, this coefficient attenuates to  $-0.053$  (SE: 0.066). The low-protection estimate, drawn from 7 treated countries, is positive but insignificant.

## 6. Discussion

The central finding is that harmonizing trade secret protection across the EU had no detectable effect on business R&D expenditure. Three interpretations merit discussion.

**Legal form vs. private substitutes.** The most direct interpretation is that firms do not condition R&D investment on the statutory protection of trade secrets because they already protect proprietary knowledge through private mechanisms: non-disclosure agreements, non-compete clauses, employment contracts, internal compartmentalization, and technical security measures (Marx, 2022). If private substitutes are effective, statutory harmonization merely

**Table 5:** Heterogeneity by Pre-Existing Trade Secret Protection

	(1)	(2)	(3)
	High Protection	Medium Protection	Low Protection
Post $\times$ Treated	-0.2379 (0.0634)	-0.0320 (0.0182)	0.0501 (0.0958)
Region FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Countries (treated)	2	3	7
Observations	280	840	546

*Notes:* Each column restricts the treated sample to countries with the indicated level of pre-existing trade secret protection (based on Baker McKenzie 2016 survey classification). High protection: DE, FR, SE, NL, AT, FI. Medium: IT, ES, BE, DK, IE, PT, UK. Low: PL, CZ, SK, HU, RO, BG, HR, SI, LV, LT, EE, CY, MT, EL, LU. All regressions include the never-treated control group. Standard errors clustered at the country level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

codifies protections that firms had already arranged bilaterally. This is consistent with [Alm and Gilpatric \(2019\)](#), who argue that firms’ own investments in secrecy are first-order relative to legal remedies.

**Too early to tell?.** BERD is a slow-moving variable. Firms set R&D budgets through multi-year planning cycles, and the effects of legal reform on investment may take longer to materialize than the 5-year post-treatment window allows. However, if legal uncertainty were a binding constraint, one might expect a response on the intensive margin (increasing R&D in ongoing programs) even before the extensive margin (launching new programs) adjusts. The absence of even a short-run response is more consistent with legal form being non-binding than with slow adjustment.

**The suggestive gradient.** The monotonic gradient across protection tiers—positive for low-protection countries, negative for high-protection countries—is consistent with the Directive having a heterogeneous effect proportional to the treatment dose. The positive coefficient for low-protection countries (0.050) is drawn from 7 countries and, while insignificant, suggests that harmonization may have had a real if modest effect where the legal gap was largest. The large negative coefficient for high-protection countries ( $-0.238$ ) is driven by only 2 treated countries in the balanced panel and attenuates substantially in the full sample; it likely reflects country-specific R&D trends rather than a causal effect of the Directive. A longer post-treatment period or finer outcome data (e.g., firm-level R&D from the Community Innovation Survey) might resolve the heterogeneity question more precisely.

The null result should not be interpreted as evidence that intellectual property law is irrelevant for innovation. Patents, which require public disclosure and confer monopoly

rights, operate through fundamentally different channels than trade secrets, which protect through confidentiality (Arora et al., 2008). What this paper establishes is more specific: at the margin of EU-wide harmonization—moving from heterogeneous to uniform statutory protection—the legal reform did not produce a measurable increase in business R&D. The binding constraints on innovation investment in Europe likely lie elsewhere: in access to finance, human capital, market structure, or the broader regulatory environment (Aghion et al., 2009; Akcigit and Ates, 2021).

## 7. Conclusion

Legal harmonization is the EU’s signature policy instrument, deployed across hundreds of directives to create a single market. This paper tests whether harmonizing one specific legal domain—trade secret protection—produces real economic effects. The answer, at least for business R&D expenditure, is no. The EU Trade Secrets Directive changed the law but not the investment. Firms, it appears, had already solved the misappropriation problem on their own.

## Acknowledgements

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

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

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## A. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
BERD/GDP (%)	-0.0151	0.0203	0.388	-0.0388	0.0523	Small negative
ln(BERD)	-0.1741	0.0397	1.645	-0.1058	0.0241	Moderate negative
GERD/GDP (%)	0.0250	0.0402	0.544	0.0459	0.0740	Small positive
<i>Panel B: Heterogeneous (by pre-existing protection)</i>						
BERD/GDP — High prot.	-0.2379	0.0634	0.372	-0.6388	0.1703	Large negative
BERD/GDP — Low prot.	0.0501	0.0958	0.382	0.1313	0.2508	Moderate positive

*Notes:* **Country:** European Union (27 member states plus United Kingdom). **Research question:** Does harmonizing trade secret protection upward via the EU Trade Secrets Directive (2016/943) affect business enterprise R&D investment across European regions? **Policy mechanism:** The Directive created the first EU-wide legal framework for trade secret protection, harmonizing definitions, civil remedies (injunctions, damages, corrective measures), and litigation confidentiality protections; countries with weaker pre-existing regimes experienced a larger effective strengthening of legal protection against misappropriation. **Outcome definition:** BERD as a percentage of regional GDP (Panels A–B rows 1–2), natural log of BERD in million EUR (Panel A row 2), and total GERD as a percentage of GDP (Panel A row 3). **Treatment:** Binary indicator equal to one after the country transposed the Directive into national law. **Data:** Eurostat rd\_e\_gerdreg and nama\_10r\_2gdp, 2010–2023, NUTS2 region  $\times$  year panel with 1,162 observations across 83 regions in 15 countries. **Method:** Callaway & Sant’Anna (2021) staggered DiD with never-treated control group (Panel A); TWFE with country-level clustering (Panel B heterogeneity splits). **Sample:** All EU-27 NUTS2 regions with non-missing BERD and GDP data; Panel B splits by pre-existing trade secret legal protection strength (Baker McKenzie 2016 classification).  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).