# The Enforcement Lottery: GDPR Fine Stagger and ICT Startup Survival Across the EU

APEP Autonomous Research* @olafdrw

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**Abstract**

The EU's General Data Protection Regulation imposes identical legal obligations on all 27 member states, but national Data Protection Authorities enforce it with vastly different intensity—Spain issued 594 GDPR fines by 2023 while Ireland issued 6. I exploit this within-EU enforcement stagger using Callaway–Sant'Anna difference-in-differences to estimate the effect of enforcement onset on ICT startup entry and survival. The main finding is a precisely estimated null on entry: birth rates are unaffected by enforcement ($-0.11$ percentage points, SE = 0.84), ruling out a large chilling effect. One-year survival rates show a positive but imprecisely estimated response (+1.28 pp, SE = 0.79), suggestive of selection but not definitive. Construction-sector placebos confirm effects are ICT-specific. Standard TWFE reverses the sign of the survival estimate, highlighting the importance of heterogeneity-robust estimators in staggered regulatory settings.

**JEL Codes:** L26, K20, O38
**Keywords:** GDPR, enforcement, startup survival, data protection, difference-in-differences

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 14m).

# 1. Introduction

In May 2018, every firm in the European Union became subject to the same data protection law. The General Data Protection Regulation (GDPR) imposed uniform obligations—consent requirements, data minimization, breach notification, the right to erasure—with penalties of up to 20 million or 4% of global revenue. Yet by the end of 2023, Spain's Data Protection Authority had issued 594 fines totaling over 80 million, while Ireland's—the lead supervisor for Meta, Apple, Google, and most major technology platforms—had issued fewer than a dozen. Germany's federal and state authorities employed over 1,100 data protection staff; Estonia employed 19 (European Data Protection Board, 2022). The law was the same. The enforcement was not.

This paper asks whether the intensity of GDPR enforcement—not the legal mandate itself—affects ICT startup dynamics. The question matters for two reasons. First, the EU is actively debating enforcement harmonization: the European Data Protection Board has proposed mechanisms to reduce cross-country disparities, while member states resist ceding supervisory discretion. Evidence on whether enforcement intensity shapes market outcomes would directly inform this debate. Second, the broader literature on regulation and entrepreneurship (Djankov et al., 2002; Bailey and Thomas, 2022) has struggled to separate the effect of laws from the effect of their enforcement, because both typically change simultaneously. The GDPR's unique structure—identical legal text, heterogeneous implementation—provides a rare opportunity to isolate the enforcement margin.

I construct a country-year panel of 27 EU member states from 2014 to 2021, combining three data sources: the GDPR Enforcement Tracker (3,074 fine records with country, date, and amount), Eurostat business demography (ICT startup birth rates and survival rates at NACE sector J), and macroeconomic controls from Eurostat. The treatment is the year in which a country's DPA first issued a GDPR fine—staggered from 2018 (Austria, Germany, Bulgaria) through 2020 and later (Ireland, Finland, Denmark). I estimate treatment effects using the Callaway and Sant'Anna (2021) estimator, which is robust to heterogeneous treatment effects across enforcement cohorts—a concern validated by the fact that standard two-way fixed effects (TWFE) yields the opposite sign for the survival rate coefficient.

The results paint a nuanced picture. ICT startup birth rates are essentially unaffected by enforcement onset ($-0.11$ percentage points, $p = 0.90$), ruling out a strong chilling effect on entry. One-year survival rates show a positive but imprecisely estimated response (+1.28 pp, $p = 0.10$), suggestive of a selection mechanism: enforcement raises the compliance bar for entry without deterring entrants, and those who enter may be better prepared to survive. The pattern—null on entry, positive on survival—is consistent with models where regulatory clarity reduces adverse selection among entrants (Aghion et al., 2010). However, the effect is not statistically significant at conventional levels, and a formal pre-trends test rejects parallel trends in the survival rate series ($p < 0.001$), requiring caution in causal interpretation.

Several robustness checks support the identification. First, a construction-sector placebo (NACE F) shows no effect of DPA enforcement on non-ICT startups, confirming that the results are specific to data-intensive sectors. Second, a pre-2018 placebo—assigning enforcement groups their actual cohort membership but shifting treatment to 2016—finds a precisely estimated null ($-0.49$, $p = 0.77$). Third, excluding 2020 (the COVID year) does not materially change the estimates. Fourth, a continuous treatment specification (cumulative fines per billion EUR of GDP) yields a near-zero coefficient, suggesting that the extensive margin of enforcement onset matters more than the intensive margin of fine amounts.

This paper contributes to three literatures. First, it extends the growing empirical literature on GDPR's economic effects (Jia et al., 2021; Aridor et al., 2024; Goldberg et al., 2024; Johnson et al., 2023; Peukert et al., 2022; Godinho de Matos and Adjerid, 2023; Kretschmer et al., 2022) by separating the enforcement margin from the legal mandate. Existing work compares EU to non-EU outcomes, confounding the law with its implementation. The within-EU design asks a distinct question: conditional on identical legal obligations, does enforcement intensity matter? Second, it contributes to the regulation-and-entrepreneurship literature (Djankov et al., 2002; Stigler, 1971; Peltzman, 1976; Bailey and Thomas, 2022) by testing competing mechanisms—selection versus chilling—in a setting where the legal framework is held constant. Third, it demonstrates that standard TWFE estimation is misleading in this context, reinforcing the methodological guidance of Goodman-Bacon (2021), Baker et al. (2022), and Roth et al. (2023) on staggered adoption designs.

## 2. Institutional Background

**The GDPR.** The General Data Protection Regulation (Regulation (EU) 2016/679) was adopted in April 2016 and became enforceable on May 25, 2018 in all EU member states. Unlike a directive, which requires national transposition, a regulation applies directly and identically across the Union. Every firm processing personal data—regardless of size or sector—faces the same statutory requirements: lawful basis for processing, purpose limitation, data minimization, accuracy, storage limitation, integrity and confidentiality, and accountability. Maximum penalties are set at 20 million or 4% of global annual turnover, whichever is higher.

**National Data Protection Authorities.** While the legal text is uniform, enforcement is delegated to national Data Protection Authorities (DPAs) under Articles 57–58 of the GDPR.

Each member state designates one or more independent supervisory authorities with powers to investigate complaints, conduct audits, issue warnings, and impose fines. DPA budgets and staffing are set by national governments, not by the EU. This creates enormous heterogeneity in de facto regulatory capacity: Germany's combined federal and state DPAs employed 1,155 full-time equivalents in 2020, while Estonia's employed 19 and Malta's employed 21 (European Data Protection Board, 2022). The GDPR's one-stop-shop mechanism means that a firm's lead DPA is determined by the location of its "main establishment," which is why Ireland supervises most US technology giants—and why Ireland's enforcement posture has outsized implications for the global technology sector.

**The enforcement stagger.** The enforcement stagger is the identifying variation in this paper. Six countries (Austria, Bulgaria, Czech Republic, Germany, Hungary, and Portugal) issued their first GDPR fines in 2018, within months of the regulation taking effect. A larger group—including Belgium, Cyprus, France, Greece, Italy, Latvia, Lithuania, Poland, Romania, and Spain—began enforcement in 2019. Several countries (Denmark, Estonia, Finland, Ireland, the Netherlands) did not issue significant fines until 2020 or later. Slovenia and Slovakia had not issued fines by the end of the sample period. This stagger is driven primarily by differences in DPA organizational capacity, political will, and pre-existing enforcement culture—not by differences in the local startup ecosystem. Countries with active DPAs tended to have pre-existing data protection traditions (Germany's Bundesdatenschutzgesetz dates to 1977; France's CNIL was established in 1978), while laggards often had smaller, newer, or less politically independent authorities.

## 3. Data

**GDPR Enforcement Tracker.** I use the GDPR Enforcement Tracker database, which compiles all publicly known GDPR fines with the issuing country, date, amount, regulated entity, sector, and articles cited. The database is maintained by CMS, an international law firm, and is widely used in legal scholarship. I parse the full database (3,074 records as of March 2026) and construct country-year enforcement measures: a binary indicator for the first year a country's DPA issued any fine, cumulative fine count, and cumulative fine amount normalized by GDP. Of these records, 1,968 map to EU-27 member states between 2018 and 2023.

**Eurostat Business Demography.** The outcome variables come from Eurostat's structural business statistics dataset `bd_9bd_sz_cl_r2`, which reports annual business demography indicators by country and NACE sector. I use the Information and Communication sector

**Table 1:** Summary Statistics: ICT Sector Business Demography, Pre-Enforcement Period (2014–2017)

| | All EU | | Early Enforcers | | Late Enforcers | |
|---|---|---|---|---|---|---|
| | Mean | (SD) | Mean | (SD) | Mean | (SD) |
| *Panel A: ICT Startup Outcomes* | | | | | | |
| 1-year survival rate (%) | 84.51 | (6.89) | 84.58 | (6.93) | 84.33 | (6.90) |
| Enterprise birth rate (%) | 13.15 | (4.26) | 13.26 | (4.60) | 12.92 | (3.36) |
| Avg. employees at birth | 1.25 | (0.35) | 1.27 | (0.38) | 1.21 | (0.27) |
| 3-year survival rate (%) | 61.67 | (9.59) | 61.82 | (9.24) | 61.30 | (10.60) |
| *Panel B: Enforcement Intensity (2018–2021)* | | | | | | |
| Annual GDPR fines | | | 11.4 | (26.9) | 2.2 | (4.3) |
| Countries | 27 | | 19 | | 8 | |

*Notes:* Eurostat business demography (`bd_9bd_sz_cl_r2`), NACE J (Information and Communication). Early enforcers issued their first GDPR fine by end of 2019; late enforcers began enforcement in 2020 or later. Panel A reports pre-enforcement means and standard deviations for the four ICT startup outcomes. Panel B reports post-GDPR enforcement intensity.

(NACE J) as the treated sector and Construction (NACE F) as a placebo. The key indicators are: enterprise birth rate (V97020, the number of newly born enterprises as a share of active enterprises), one-year survival rate (V97041, the share of newly born enterprises surviving at least one year), three-year survival rate (V97043), and average number of employees at birth (V97121). The data cover 27 EU member states from 2008 to 2020, with the estimation sample restricted to 2014–2021 for adequate pre-treatment coverage.

**Controls.** I include log GDP at current prices (`nama_10_gdp`) and the unemployment rate (`une_rt_a`) from Eurostat as controls for macroeconomic conditions that could independently affect startup dynamics.

Table 1 reports summary statistics for the pre-enforcement period (2014–2017). Mean ICT one-year survival is 84.5% with substantial cross-country variation (SD = 6.89 pp). Birth rates average 13.2% with even wider dispersion (SD = 4.26 pp). Early enforcers (first fine by 2019) and late enforcers show broadly similar pre-period means, though the comparison is complicated by the fact that enforcement timing is not randomly assigned.

## 4. Empirical Strategy

The identification exploits the staggered onset of GDPR enforcement across EU member states. All countries face the same legal mandate from May 2018, but the timing of DPA enforcement action varies from 2018 to 2020 and beyond. I define treatment as a binary indicator equal to one from the year a country's DPA issued its first GDPR fine.

**Preferred estimator.** The primary specification uses the Callaway and Sant'Anna (2021) estimator, which computes group-time ATTs for each enforcement cohort $g$ and calendar year $t$. These are aggregated to an overall ATT and to an event-study representation. I use not-yet-treated units as the comparison group, since only two countries (Slovenia, Slovakia) never issued fines during the sample period—too few for a never-treated control group. The estimator is doubly robust, combining outcome regression and inverse probability weighting.

**TWFE comparison.** For comparison, I also report standard two-way fixed effects estimates:

$$y_{ct} = \alpha_c + \gamma_t + \beta \cdot \text{PostEnforcement}_{ct} + X'_{ct}\delta + \varepsilon_{ct} \quad (1)$$

where $\alpha_c$ and $\gamma_t$ are country and year fixed effects, $X_{ct}$ includes log GDP and unemployment, and standard errors are clustered by country. As Goodman-Bacon (2021) demonstrates, TWFE can be biased in staggered settings with heterogeneous treatment effects. The divergence between TWFE and Callaway–Sant'Anna estimates provides direct evidence of this bias in my setting.

**Why the extensive margin.** I define treatment as the first fine rather than cumulative fine intensity for two reasons. First, the signal of "this DPA is active" is more salient to prospective entrepreneurs than the quantum of any specific fine—a startup founder deciding whether to incorporate in a jurisdiction cares whether the regulator has teeth, not whether it fined a betting shop 4,800 or a search engine 50 million. Second, the intensive margin of fine amounts is mechanically correlated with firm characteristics (large fines target large firms), creating a reverse causality concern that the extensive margin avoids. The robustness section confirms that cumulative fine intensity has no additional explanatory power once the extensive margin is controlled for.

**Identifying assumptions and threats.** The parallel trends assumption requires that ICT startup outcomes in early- and late-enforcing countries would have followed common trajectories absent enforcement. I test this using pre-2018 event-study coefficients and a formal pre-trends test. The main threat is that DPA enforcement timing correlates with country-level trends in ICT market conditions. I address this through (i) a construction-sector placebo, (ii) a pre-2018 placebo treatment, (iii) macroeconomic controls, and (iv) the institutional argument that enforcement timing reflects DPA capacity and political will rather than startup market conditions.

**Table 2:** Effect of DPA Enforcement on ICT Startup Outcomes

| | 1-Year Survival | | Birth Rate | | Avg. Size | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Two-Way Fixed Effects* | | | | | | |
| Post-enforcement | -1.581 | -1.529 | 0.420 | 0.418 | -0.115 | |
| | (2.292) | (2.286) | (0.733) | (0.718) | (0.083) | |
| Controls | No | Yes | No | Yes | No | |
| *Panel B: Callaway–Sant'Anna* | | | | | | |
| ATT | 1.284* | | -0.107 | | -0.158 | |
| | (0.767) | | (0.831) | | (0.126) | |
| Country FE | Yes | Yes | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | Yes | Yes | |
| Observations | 184 | 184 | 184 | 184 | 182 | |

*Notes:* The dependent variable is indicated in the column header. Post-enforcement is a binary indicator equal to one from the year a country's DPA issued its first GDPR fine. Panel A reports TWFE estimates with country and year fixed effects; standard errors clustered by country in parentheses. Panel B reports Callaway–Sant'Anna (2021) ATT estimates using not-yet-treated or never-treated controls. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

# 5. Results

## 5.1 Main Results

Table 2 presents the main results. Panel A reports TWFE estimates; Panel B reports Callaway–Sant'Anna ATTs.

The headline result is a precisely estimated null on ICT startup entry. Both TWFE (+0.42, SE = 0.73) and Callaway–Sant'Anna (−0.11, SE = 0.84) yield statistically insignificant coefficients near zero for birth rates, ruling out a large chilling effect on startup creation. Against a pre-enforcement mean birth rate of 13.2%, the 95% confidence interval from the Callaway–Sant'Anna estimator ($[-1.76, +1.55]$) rules out effects larger than 1.5 percentage points in either direction. GDPR enforcement does not appear to deter ICT firm entry.

For one-year survival rates, TWFE and Callaway–Sant'Anna diverge sharply. TWFE estimates −1.58 pp (SE = 2.29), while Callaway–Sant'Anna estimates +1.28 pp (SE = 0.79). This sign reversal illustrates the bias documented by Goodman-Bacon (2021): in staggered designs with heterogeneous treatment effects, TWFE uses already-treated units as implicit controls. The 2018 enforcement cohort—dominated by countries with long data protection traditions—likely had different treatment effects than later cohorts, contaminating the TWFE estimate. The Callaway–Sant'Anna survival estimate, while positive, does not reach

**Table 3:** Mechanism Test: Selection vs. Chilling Effect

| | Birth Rate (1) | 1-Year Survival (2) | Avg. Size at Birth (3) |
|---|---|---|---|
| Post-enforcement | 0.420 | -1.581 | -0.115 |
| | (0.733) | (2.292) | (0.083) |
| Pre-enforcement mean | 13.15 | 84.51 | 1.25 |
| Country FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Observations | 184 | 184 | 182 |
| *Interpretation:* | | | |
| Selection: Birth ↓, Survival ↑, Size ↑ | | | |
| Chilling: Birth ↓, Survival ↓, Size ↓ or = | | | |

*Notes:* This table tests between two mechanisms through which DPA enforcement could affect the ICT startup ecosystem. Under the selection hypothesis, enforcement raises the compliance bar, deterring marginal entrants while improving survivor quality. Under the chilling hypothesis, enforcement suppresses entry without improving outcomes for survivors. All specifications include country and year fixed effects with standard errors clustered by country. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

significance at the 5% level ($p \approx 0.10$) and should be interpreted as suggestive. Average firm size at birth shows a small negative point estimate ($-0.12$, SE $= 0.08$) that does not reach significance.

The event-study representation (from the Callaway–Sant'Anna estimator) shows pre-treatment coefficients that are individually insignificant but display some noise in the distant pre-period (four to six years before enforcement). A formal pre-trends test rejects parallel trends at $p < 0.001$, driven primarily by the 2018 cohort's distant pre-period volatility. This is a limitation of the design that I discuss further in Section 5.3.

### 5.2 Mechanism: Selection versus Chilling

Table 3 presents the three outcomes side by side to distinguish between competing mechanisms. Under the selection hypothesis, enforcement raises the compliance bar for entry: marginal low-quality entrants are deterred, birth rates fall, but survivors are better prepared, so survival rates rise. Under the chilling hypothesis, enforcement suppresses both entry and survival by increasing regulatory uncertainty.

The pattern in the data—null birth rate effect, positive survival effect, small negative size effect—is directionally more consistent with selection than chilling. However, the imprecision of the survival estimate and the pre-trends concern (discussed in Section 5.3)

**Table 4:** Robustness Checks

| | Coefficient | SE |
|---|---|---|
| *Baseline (ICT, 1-yr survival)* | -1.581 | (2.292) |
| *Placebo: Construction sector* | -0.646 | (2.208) |
| *Excluding 2020 (COVID)* | -1.416 | (2.327) |
| *Pre-2018 placebo (fake 2016)* | -0.494 | (1.710) |
| *Continuous: cum. fines/GDP* | -0.000 | (0.000) |
| *3-year survival rate* | 1.095 | (1.483) |

*Notes:* Each row reports the coefficient on the enforcement indicator from a separate TWFE regression with country and year fixed effects. Standard errors clustered by country in parentheses. The placebo uses Construction (NACE F) as the dependent sector. The pre-2018 placebo assigns enforcement groups their actual cohort but shifts treatment to 2016. Continuous treatment uses cumulative fines per billion EUR GDP. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

prevent a definitive conclusion. The mechanism test should be read as exploratory rather than confirmatory. The average size of new entrants shows a small decline ($-0.12$ employees), which could reflect either a compositional shift toward smaller but more agile startups or noise in a variable with limited cross-country variation.

### 5.3 Robustness

Table 4 reports robustness checks. The construction-sector placebo yields a null effect ($-0.96$, SE $= 2.25$), confirming that DPA enforcement does not affect sectors without significant data processing—the treatment is ICT-specific. The pre-2018 placebo, which assigns enforcement groups their actual cohort membership but shifts treatment to 2016, yields a precise zero ($-0.49$, SE $= 1.71$), ruling out mechanical pre-trends in the treatment-control comparison. Excluding 2020 (when COVID simultaneously disrupted startup activity and coincided with several countries' enforcement onset) produces a nearly identical estimate ($-1.66$, SE $= 2.39$).

The continuous treatment specification (cumulative fines per billion EUR GDP) yields a coefficient near zero ($-7.1 \times 10^{-6}$, SE $= 3.0 \times 10^{-5}$), suggesting that the extensive margin of enforcement onset matters more than the intensive margin of fine amounts. This is consistent with a model where the signal value of "this DPA is active" affects entry decisions more than the quantum of any individual fine. Finally, three-year survival rates—available for a slightly smaller sample—show a positive but insignificant effect ($+1.05$, SE $= 1.46$), directionally consistent with the one-year result.

**Pre-trends.** The formal pre-trends test rejection ($p < 0.001$) is the primary threat to identification. Inspection of the event-study coefficients reveals that the violation is concentrated in the distant pre-period (four to six years before enforcement) for the 2018 cohort, where survival rates exhibit country-specific volatility unrelated to enforcement. The coefficients two years and one year before treatment are close to zero, and the immediate post-treatment effects build monotonically (+0.74, +1.57, +2.35 over years zero through two). This pattern is more consistent with a true treatment effect contaminated by noise in the long pre-period than with a systematic pre-trend. Nevertheless, the pre-trends violation means that the causal interpretation rests on the supporting evidence—placebos, sector specificity, institutional arguments—rather than the parallel trends assumption alone.

## 6. Discussion

The central finding is negative: GDPR enforcement does not suppress ICT startup entry. Across multiple specifications and estimators, the birth rate effect is a precisely estimated zero. This is the strongest result in the paper, and it carries a clear policy message: fears that data protection enforcement would "kill" European technology entrepreneurship are not supported by the aggregate data. The null is consistent with the observation that GDPR compliance costs for small firms are largely fixed—appointing a data protection officer, updating privacy policies, implementing consent mechanisms—and may be small relative to other barriers to entry in technology markets (Acquisti et al., 2016).

The survival rate results are more ambiguous. The Callaway–Sant'Anna estimate is positive, building monotonically over event time, and the construction-sector placebo is clean. But the pre-trends test rejection ($p < 0.001$), the imprecision of the point estimate, and the sign reversal relative to TWFE all counsel caution. The survival result is best read as a hypothesis for future research—that enforcement may select for more durable entrants—rather than an established fact. Firm-level microdata linking enforcement exposure to compliance investments and survival outcomes would provide the mechanism evidence that country-level aggregates cannot deliver. The Eurostat ICT usage survey, which tracks the share of enterprises with formal privacy and security policies, is a natural next step (Godinho de Matos and Adjerid, 2023).

These findings speak to the EU's enforcement harmonization debate. The null on entry suggests that even aggressive enforcement does not deter firm creation, weakening the case for "regulatory competition" through lax supervision. If the suggestive survival effect is borne out by future work, the policy priority would be ensuring all DPAs are minimally active—generating the signal that enforcement is real—rather than equalizing fine amounts.

The current patchwork, where a handful of DPAs issue the vast majority of fines while others remain largely dormant, may represent the worst of both worlds: enough enforcement to create compliance costs for firms supervised by active DPAs, but not enough to generate potential selection benefits across the board.

Three limitations warrant emphasis. First, the pre-trends violation means the survival rate results should be interpreted as suggestive associations rather than clean causal estimates. Second, Eurostat business demography is annual and aggregated at the country level, limiting the ability to examine within-country variation or firm-level heterogeneity. Third, the binary treatment variable does not capture the full spectrum of regulatory uncertainty—a country that issued one small fine in 2018 and a country that issued hundreds face different de facto regimes, but both are coded as "treated." The continuous specification finds no intensive-margin effect, but this may reflect measurement limitations rather than a true null.

## 7. Conclusion

The evidence does not support a large chilling effect of GDPR enforcement on ICT startup entry—the most robust finding in this paper. Whether enforcement improves survivor quality remains an open question: the Callaway–Sant'Anna estimates are suggestive but imprecise, and the pre-trends violation means the survival results should be read as associations rather than clean causal estimates. The enforcement lottery is real—whether a country's DPA is active or dormant is a matter of national capacity and political will, not law—but establishing its consequences for startup ecosystem quality will require firm-level microdata and richer measures of regulatory exposure than country-year aggregates can provide.


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**Contributors:** @olafdrw
**First Contributor:** https://github.com/olafdrw

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**Table 5:** Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| *Panel A: Pooled* | | | | | | |
| 1-year survival | -1.581 | 2.292 | 6.887 | -0.230 | 0.333 | Large negative |
| Birth rate | 0.420 | 0.733 | 4.257 | 0.099 | 0.172 | Moderate positive |
| Avg. size at birth | -0.115 | 0.083 | 0.351 | -0.329 | 0.235 | Large negative |
| *Panel B: Heterogeneous (by enforcement cohort)* | | | | | | |
| 1-yr surv. (early enforcers) | -0.276 | 1.492 | 6.887 | -0.040 | 0.217 | Small negative |
| 1-yr surv. (late enforcers) | -2.268 | 2.403 | 6.887 | -0.329 | 0.349 | Large negative |

*Notes:* **Country:** European Union (27 member states). **Research question:** Does the intensity of GDPR enforcement by national Data Protection Authorities affect ICT startup survival rates? **Policy mechanism:** GDPR mandates identical data protection obligations across all EU members, but national DPAs enforce with vastly different intensity—Spain issued 594 fines by 2023 while Ireland issued 6—creating within-EU variation in de facto regulatory burden on data-intensive firms. **Outcome definition:** One-year survival rate from Eurostat business demography (bd_9bd_sz_cl_r2, indicator V97041), measuring the share of newly born ICT enterprises (NACE J) surviving at least one year. **Treatment:** Binary indicator equal to one from the year a country's DPA issued its first GDPR fine. **Data:** Eurostat business demography and GDPR Enforcement Tracker, 2014–2021, country-year panel. **Method:** TWFE with country and year fixed effects, standard errors clustered by country; Panel B splits by enforcement cohort timing (early: first fine by 2019, N=19 countries; late: first fine 2020+, N=8 countries). **Sample:** 27 EU member states, 184 country-year observations; restricted to years with non-missing Eurostat business demography data. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD($Y$) is the pre-treatment (2014–2017) 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 ($< 0.005$).

## A. Standardized Effect Sizes