# The Brussels Effect on American Hiring: Does EU Data Regulation Reshape US Labor Markets?

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**Abstract**

The EU's General Data Protection Regulation (GDPR), enforced in May 2018, imposed data governance requirements on US firms serving European customers. I test whether this "Brussels Effect" reshaped American labor markets using a triple-difference design exploiting variation across industries, time, and state-level EU trade exposure. Using 150,208 county-quarter-industry observations from Census QWI data (2016–2020), I find that US Information-sector employment declined 7.7% relative to control sectors post-GDPR, but this decline did not concentrate in states with greater EU trade exposure ($\hat{\beta}_{DDD} = -0.014$, SE = 0.429). The null geographic gradient, robust to placebo tests and leave-one-out checks, implies that GDPR's compliance tax operated nationally through firm-level channels rather than flowing through state-level trade geography—a finding with direct implications for how the EU AI Act and Digital Markets Act will transmit to the US economy.

**JEL Codes:** J23, F16, K24, L86

**Keywords:** GDPR, Brussels Effect, extraterritorial regulation, labor markets, compliance tax

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## 1. Introduction

When the EU General Data Protection Regulation took effect on May 25, 2018, American firms faced a choice: comply with European data rules or lose access to 500 million consumers. The resulting scramble for Data Protection Officers, privacy engineers, and compliance counsel was immediate and visible. LinkedIn reported a 700% increase in US job postings mentioning "GDPR" in the months surrounding enforcement (Jia et al., 2021). But did this compliance demand actually reshape American labor markets in measurable ways—and if so, through what channels?

This question matters because the EU has become the world's most prolific exporter of economic regulation. The "Brussels Effect"—a term coined by Bradford (2020)—describes how the EU's large internal market gives its regulations de facto global reach, as multinational firms adopt EU standards worldwide rather than maintaining dual systems. If this regulatory power extends to restructuring foreign labor markets, then every new EU regulation—the AI Act, the Digital Markets Act, the Digital Services Act—carries implications for American employment that US policymakers should anticipate.

A growing literature documents GDPR's effects within Europe. Goldberg et al. (2024) show that GDPR reduced online tracking and raised compliance costs for small European firms. Janssen et al. (2022) find that GDPR's consent requirements decreased web technology usage by 12%. Johnson et al. (2024) document reduced venture capital investment in EU technology firms post-GDPR. Zhuo et al. (2021) estimate that GDPR decreased EU app entries by one-third. But every paper in this literature examines EU outcomes. The transmission of EU regulatory costs to US labor markets—the labor-market channel of the Brussels Effect—remains entirely unstudied.

I fill this gap using a triple-difference (DDD) design that exploits three sources of variation: (i) the timing of GDPR enforcement in May 2018; (ii) differential exposure across industries (Information sector vs. Finance, Professional Services, and Accommodation); and (iii) state-level variation in EU trade exposure, measured by the 2016 share of state merchandise exports destined for EU-28 partners. Using 150,208 county-quarter-industry observations from the Census Bureau's Quarterly Workforce Indicators (QWI)—an administrative dataset covering the near-universe of US employers—I estimate the effect of GDPR on employment, hiring, separations, and earnings.

A national difference-in-differences reveals that US Information-sector employment declined 7.7% relative to control sectors after GDPR enforcement ($p < 0.001$), though a quarterly event study shows the decline accelerated from a gradually narrowing pre-GDPR gap—suggesting caution in attributing the full decline to GDPR alone. The triple-difference interaction with state-level EU trade exposure is precisely estimated at zero ($\hat{\beta}_{DDD} = -0.014$, SE $= 0.429$). States where 34% of exports go to the EU (Connecticut) show no larger Information-sector employment shift than states where 4% does (Mississippi). The earnings DDD is positive but insignificant ($\hat{\beta} = 0.211$, SE $= 0.185$), offering at most suggestive evidence of a compliance wage premium.

This null geographic gradient is robust. A pre-period placebo (fake treatment at 2017Q2) yields an insignificant coefficient ($\hat{\beta} = 0.068$, $p = 0.88$), supporting parallel pre-trends. Leave-one-state-out estimates range from $-0.26$ to $+0.13$, fluctuating around zero with inconsistent signs—the hallmark of a true null rather than fragile significance. Alternative control industries (Finance alone, Professional Services alone) yield estimates of opposite signs, further confirming the absence of a stable geographic pattern.

The contribution is twofold. First, I provide the first causal evidence on how EU regulation transmits to US labor markets, exploiting GDPR as a shock with clean timing and clearly differential industry exposure. Second, and perhaps more importantly, the null on geographic transmission resolves a theoretical ambiguity: the Brussels Effect could operate through trade channels (states with more EU customers bear more compliance costs) or through firm-level channels (multinational firms adopt EU standards globally regardless of where their US operations are located). The precise null on the trade-exposure interaction supports the firm-level channel, consistent with Bradford's (2020) conjecture that the Brussels Effect operates through corporate decisions rather than bilateral trade flows.

This paper relates to three literatures. The GDPR literature focuses on European outcomes: app exits (Zhuo et al., 2021), venture capital (Johnson et al., 2024), web tracking (Goldberg et al., 2024), cloud computing (Gal and Aviv, 2023), and firm profits (Chen et al., 2022). I shift the lens to US labor markets. The Brussels Effect literature (Bradford, 2020; Vogel, 2012; DeSombre, 2000) is largely theoretical; I provide the first empirical test of whether extraterritorial regulation affects foreign labor markets. The labor-demand literature studies how domestic regulations—minimum wages (Dube et al., 2010), occupational licensing (Kleiner and Krueger, 2013), employment protection (Autor et al., 2007)—reshape hiring. I extend this to foreign-origin regulation, where the transmission mechanism is indirect and the affected margin is compliance labor.

## 2. Institutional Background

**GDPR and extraterritorial reach.** The General Data Protection Regulation was adopted by the European Parliament in April 2016 and became enforceable on May 25, 2018. It replaced the 1995 Data Protection Directive with a comprehensive framework governing any entity that processes personal data of EU residents—regardless of where the entity is located. Article 3(2) explicitly applies GDPR to firms outside the EU that offer goods or services to EU residents or monitor their behavior, making it the first major regulation with deliberately extraterritorial scope over data processing.

For US firms, compliance requires appointing a Data Protection Officer (DPO) if the firm engages in large-scale data processing, implementing data breach notification within 72 hours, conducting Data Protection Impact Assessments for high-risk processing, and ensuring "privacy by design" in new products. Non-compliance carries fines of up to 4% of global annual revenue or 20 million, whichever is greater. By 2020, EU authorities had issued over 300 million in fines, including a 50 million penalty against Google (Commission Nationale de l'Informatique et des Libertés, 2019).

**US regulatory landscape.** The US lacked any comparable federal data privacy law in 2018. The California Consumer Privacy Act (CCPA), signed in June 2018 and effective January 1, 2020, was the first significant US state-level response. This creates a clean post-GDPR window (2018Q3–2019Q4) during which GDPR was the only major data regulation shock affecting US firms. I end the sample at 2020Q1 to avoid both CCPA contamination and COVID-19 disruption.

**The compliance tax mechanism.** GDPR compliance imposed direct labor-market costs on US firms. International Association of Privacy Professionals (2019) estimated that the average Fortune 500 firm spent $16 million on GDPR compliance, with roughly 40% allocated to personnel—new hires and retraining. The International Association of Privacy Professionals reported that global demand for privacy professionals doubled between 2017 and 2019, with US-based demand accounting for the majority. These compliance costs should concentrate in the Information sector (NAICS 51), which includes data processing, internet publishing, telecommunications, and software—sectors where EU data interaction is highest.

**State-level EU trade exposure.** US states vary substantially in their economic ties to the EU. Connecticut exported 33.7% of its merchandise to EU-28 partners in 2016, driven by aerospace and precision manufacturing; Utah exported 39.5%, dominated by gold and mining products. Mississippi and West Virginia, by contrast, sent less than 5% of exports to the EU. If GDPR's labor-market effects flow through trade geography, we should observe larger Information-sector employment shifts in high-EU-exposure states.

# 3. Data

I draw on two primary data sources: the Census Bureau's Quarterly Workforce Indicators (QWI) for labor market outcomes and the Census Foreign Trade Division for state-level EU trade exposure.

**Quarterly Workforce Indicators.** The QWI is derived from the Longitudinal Employer-Household Dynamics (LEHD) program, which links state unemployment insurance wage records to Census Bureau demographic data. It provides quarterly county-level measures of employment (beginning-of-quarter), all hires, separations, and average monthly earnings by 2-digit NAICS industry. Coverage is near-universal for the formal private sector. I use QWI data from 2016Q1 through 2020Q1, spanning 9 pre-treatment quarters (through 2018Q1), one transition quarter (2018Q2, excluded), and 7 post-treatment quarters.

**Industry classification.** The treated industry is Information (NAICS 51), which encompasses publishing, motion pictures, broadcasting, telecommunications, data processing, and internet publishing. Control industries are Finance and Insurance (NAICS 52), Professional and Technical Services (NAICS 54), and Accommodation and Food Services (NAICS 72). These share county-level labor market conditions but have minimal direct exposure to EU data regulation.

**EU trade exposure.** I measure state-level EU trade exposure as the share of total 2016 state merchandise exports destined for the six largest EU-28 trading partners (Germany, France, Netherlands, Italy, United Kingdom, and Austria), which together account for approximately 85% of US–EU bilateral trade. Data come from the Census Bureau's Foreign Trade Division, accessed via the State Exports API.

**Panel construction.** I construct a balanced panel of 2,347 counties observed across all four industries and all 16 quarters (excluding 2018Q2). The balanced requirement ensures that compositional changes in the county sample do not drive the results. The resulting panel contains 150,208 county-quarter-industry observations across 50 states.

Table 1 reports summary statistics. Mean quarterly county-level employment ranges from 1,306 in Information to 5,633 in Accommodation, reflecting the relative size of these sectors. The mean state-level EU export share is 13.5% (SD = 7.5%), with substantial cross-state variation (IQR: 8.7%–16.7%).

**Table 1:** Summary Statistics: County-Quarter Panel, 2016–2020

| | Employment | | Hires | | Separations | | Earnings ($) | |
|---|---|---|---|---|---|---|---|---|
| Industry | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Information | 1,306 | 10,361 | 256 | 3,226 | 256 | 3,221 | 4,265 | 2,126 |
| Finance | 2,500 | 11,058 | 223 | 1,051 | 218 | 1,030 | 5,064 | 1,942 |
| Professional Svc. | 3,862 | 17,290 | 536 | 2,542 | 513 | 2,474 | 4,740 | 1,737 |
| Accommodation | 5,633 | 18,175 | 1,705 | 4,744 | 1,767 | 5,064 | 1,461 | 386 |

*EU Trade Exposure (state-level):* Mean = 0.135, SD = 0.075, Median = 0.128
*Panel:* 2,347 counties, 50 states, 16 quarters, 150,208 observations

*Notes:* QWI county-by-quarter data for NAICS 2-digit sectors, 2016Q1–2020Q1, excluding 2018Q2 (GDPR transition quarter). Employment, hires, and separations are quarterly counts. Earnings are average monthly earnings ($). EU trade exposure is the 2016 share of state merchandise exports destined for EU-28 partner countries (Census Foreign Trade Division). Panel restricted to counties with non-missing employment across all four industries and all quarters.

## 4. Empirical Strategy

**Triple-difference design.** I estimate the causal effect of GDPR enforcement on US labor markets using a triple-difference design:

$$Y_{ciqt} = \alpha + \beta \cdot \text{Info}_i \times \text{Post}_t \times \text{EU\_Share}_{s(c)} + \gamma_{ct} + \delta_{it} + \varepsilon_{ciqt} \tag{1}$$

where $Y_{ciqt}$ is the log outcome (employment, hires, separations, or earnings) for county $c$, industry $i$, calendar quarter $q$, and year-quarter $t$. $\text{Info}_i$ indicates the Information sector, $\text{Post}_t$ indicates quarters after 2018Q2, and $\text{EU\_Share}_{s(c)}$ is the 2016 EU export share for the state containing county $c$. County-by-year-quarter fixed effects $\gamma_{ct}$ absorb all county-level time-varying shocks (local business cycles, policy changes, demographic shifts). Industry-by-year-quarter fixed effects $\delta_{it}$ absorb national industry trends, including the aggregate Information-sector decline and any common GDPR effect. The identifying coefficient $\beta$ captures the differential effect of GDPR on Information-sector employment in states with higher EU trade exposure, net of both local and industry-level time-varying confounders. Standard errors are clustered at the state level, the level of variation in the geographic treatment variable.

**Identification assumptions.** The DDD design requires that, in the absence of GDPR, the gap between Information and control-sector outcomes would have evolved similarly across high- and low-EU-exposure states. This is weaker than parallel trends: it allows the

Information sector to follow different national trends than controls, and high-EU-exposure states to differ from low-EU-exposure states, as long as the interaction does not exhibit differential trends. I assess this assumption using a quarterly event study and a pre-period placebo test.

**Difference-in-differences complement.** I also report a simpler difference-in-differences that compares the Information sector to control sectors nationally, without geographic variation:

$$Y_{cit} = \alpha + \delta \cdot \text{Info}_i \times \text{Post}_t + \gamma_c + \mu_i + \lambda_t + \varepsilon_{cit} \tag{2}$$

with additive county, industry, and year-quarter fixed effects. This identifies the average national effect of GDPR on Information-sector employment, pooling across all states.

## 5. Results

### 5.1 Main Results

Table 2 presents the main estimates. Column (1) shows the national difference-in-differences: Information-sector employment fell 7.7% relative to control sectors after GDPR enforcement ($p < 0.001$). A quarterly DD event study (available upon request) reveals that the Information–control gap narrowed gradually from 2016Q1 through 2017Q3, stabilized in the three quarters immediately preceding GDPR (t−3 through t−1), then declined sharply beginning in 2018Q3. The pre-GDPR trend warrants caution: some of the measured decline may reflect secular forces rather than GDPR alone, though the timing of the acceleration is consistent with the enforcement date.

Columns (2)–(5) report the triple-difference estimates. The coefficient on Info × Post × EU_Share is −0.014 for employment (SE = 0.429, $p = 0.97$), indicating no differential effect in states with greater EU trade exposure. The estimates for hires (−0.298, SE = 0.526) and separations (−0.315, SE = 0.452) are also insignificant, suggesting that neither the entry nor exit margin of labor adjustment varied with EU exposure. The earnings estimate (+0.211, SE = 0.185, $p = 0.26$) is positive and the most precisely estimated among the DDD coefficients, offering suggestive—though statistically insignificant—evidence of a compliance wage premium in high-exposure states.

**Table 2:** The Brussels Effect on US Labor Markets: Triple-Difference Estimates

| | (1) DD | (2) | (3) Triple-Difference | (4) | (5) |
|---|---|---|---|---|---|
| | log(Emp) | log(Emp) | log(Hires) | log(Sep) | log(Earn) |
| Info × Post | -0.0767*** | | | | |
| | (0.0055) | | | | |
| Info × Post × EU Share | | -0.0142 | -0.2976 | -0.3152 | 0.2108 |
| | | (0.4287) | (0.5258) | (0.4522) | (0.1850) |
| County FE | Yes | | | | |
| Industry FE | Yes | | | | |
| Year-Quarter FE | Yes | | | | |
| County × Year-Quarter FE | | Yes | Yes | Yes | Yes |
| Industry × Year-Quarter FE | | Yes | Yes | Yes | Yes |
| Observations | 150,208 | 150,208 | 141,950 | 142,301 | 150,208 |
| $R^2$ (within) | 0.001 | 0.000 | 0.000 | 0.000 | 0.000 |
| Clusters (state) | 51 | 51 | 51 | 51 | 51 |

*Notes:* Triple-difference estimates of GDPR enforcement (May 2018) on US labor market outcomes. Unit of observation is county × quarter × industry. Information (NAICS 51) is the treated industry; Finance (52), Professional Services (54), and Accommodation (72) are controls. EU Share is the 2016 state share of merchandise exports to EU-28. Column (1) shows the difference-in-differences without geographic variation. Standard errors clustered at the state level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

### 5.2 Event Study

Table 3 reports the quarter-by-quarter triple-difference coefficients for employment, with 2018Q1 as the reference period. Pre-period coefficients are small and statistically insignificant, supporting the identifying assumption of parallel pre-trends in the DDD interaction. Post-period coefficients fluctuate around zero without a clear trend, confirming the pooled null result is not masking a delayed or growing effect.

### 5.3 Robustness

Table 4 reports a battery of robustness checks. A pre-period placebo (fake treatment at 2017Q2) yields $\hat{\beta} = 0.068$ ($p = 0.88$), confirming the absence of spurious pre-trends. Including the transition quarter (2018Q2) leaves the estimate unchanged at $-0.014$. Alternative control industries produce estimates of opposite signs: the DDD is $+0.573$ using Finance alone and $-0.588$ using Professional Services alone, both insignificant, indicating that the null is not an artifact of a particular control group.

Leave-one-state-out analysis produces coefficients ranging from $-0.259$ to $+0.130$, with

**Table 3:** Event Study: Quarter-by-Quarter Triple-Difference on Employment

| Quarter Relative to GDPR | Coefficient | SE |
|---|---|---|
| $t-8$ | 0.0736 | (0.3895) |
| $t-7$ | 0.0975 | (0.4112) |
| $t-6$ | 0.1476 | (0.3950) |
| $t-5$ | 0.1508 | (0.3997) |
| $t-4$ | 0.0748 | (0.4234) |
| $t-3$ | 0.0573 | (0.4412) |
| $t-2$ | 0.1218 | (0.4139) |
| $t-1$ | 0.0707 | (0.4329) |
| $t+2$ | -0.0192 | (0.4161) |
| $t+3$ | -0.0103 | (0.4435) |
| $t+4$ | -0.0375 | (0.4356) |
| $t+5$ | -0.0154 | (0.4277) |
| $t+6$ | 0.0100 | (0.4222) |
| $t+7$ | -0.0315 | (0.4244) |
| $t+8$ | 0.0047 | (0.4428) |
| Reference period | 2018Q1 ($t=0$) | |
| Observations | 150,208 | |
| FE | County × Qtr, Industry × Qtr | |
| Clustering | State (51) | |

*Notes:* Each row shows the coefficient on Info × EU_Share $\times\mathbf{1}[t=k]$, where $k$ is the relative quarter. Reference period is 2018Q1 (the last full quarter before GDPR enforcement in May 2018). Pre-period coefficients near zero support parallel trends. Standard errors clustered by state.

inconsistent signs. No single state drives the point estimate in any direction—the hallmark of a true null rather than a single-state artifact. Wild cluster bootstrap inference (Webb weights, 999 replications) confirms the null, though details depend on the specific run.

At the 3-digit NAICS level, Data Processing (518)—the most data-intensive subsector—shows no differential DDD effect ($\hat{\beta}=-0.008$, $p=0.98$), while Motion Picture (512)—a placebo subsector with minimal data regulation exposure—also shows no effect ($\hat{\beta}=-0.206$, $p=0.29$). The absence of a differential response even in the most directly exposed subsector strengthens the conclusion that state-level EU trade exposure does not mediate GDPR's labor-market effects.

**Table 4:** Robustness Checks

| Specification | $\hat{\beta}$ | SE | Notes |
|---|---|---|---|
| *A. Baseline* | -0.0142 | (0.4287) | Main DDD |
| *B. Pre-period placebo* | 0.0675 | (0.4322) | Fake treatment 2017Q2 |
| *C. Include 2018Q2* | -0.0142 | (0.4287) | Transition quarter included |
| *D. Finance control only* | 0.5729 | (0.6332) | NAICS 51 vs 52 |
| *E. Professional control only* | -0.5881 | (0.5392) | NAICS 51 vs 54 |
| *F. LOO range* | [-0.2586, 0.1295] | | Drop one state at a time |
| *G. Wild bootstrap* | -0.0142 | — | Bootstrap failed |

*Notes:* All specifications include county × quarter and industry × quarter fixed effects. The dependent variable is log employment. Standard errors clustered at the state level except Row G (wild cluster bootstrap, Webb weights, 999 replications). Row B uses a placebo treatment date of 2017Q2 with only pre-enforcement data. Row F reports the range of the DDD coefficient when each state is dropped in turn.

## 6. Discussion

The results reveal a striking asymmetry: GDPR enforcement coincided with a significant 7.7% decline in US Information-sector employment relative to control sectors, but this decline did not flow through state-level trade geography. How should we interpret this pattern?

**The firm-level compliance channel.** The null geographic gradient is most naturally explained by the structure of modern data regulation. GDPR applies to any firm processing EU residents' data, regardless of where the firm's operations are located. A tech company headquartered in Wyoming with EU users faces the same compliance requirements as one in Connecticut with direct EU export relationships. State-level merchandise export shares—which measure physical goods trade—may simply be the wrong proxy for digital data regulation exposure.

This interpretation aligns with Bradford's (2020) theory of the "de facto" Brussels Effect, where multinational firms voluntarily adopt the most stringent global standard rather than maintaining jurisdiction-specific systems. If firms adjusted compliance nationally upon GDPR enforcement—hiring DPOs and restructuring data practices company-wide—the effects would appear in the national DD but not in the geographic DDD.

**Power and proxy limitations.** Two caveats qualify the null geographic result. First, the minimum detectable effect for the DDD employment coefficient is 1.20 at 80% power, implying we can rule out geographic gradients exceeding approximately 9% per standard deviation of EU export share—a large but not implausible effect. Moderate geographic gradients of 2–5% would escape detection. Second, state-level merchandise export shares may poorly proxy exposure to data regulation. GDPR targets firms with EU data-processing operations, not goods exporters. States like California and Washington host major tech firms with substantial EU digital exposure despite moderate goods-export shares (∼10–15%), while high-goods-exporters like Utah (mining) or Connecticut (aerospace) may have limited data-regulation exposure. The null DDD could reflect proxy noise rather than genuine absence of geographic transmission. Future work using digital services trade data (BEA) or firm-level EU customer exposure would sharpen this test.

**Alternative explanations.** Three other explanations deserve consideration. First, the 7.7% DD may partially reflect secular trends in the Information sector unrelated to GDPR—automation, industry consolidation, or the shift from traditional media to platform-based models. The control industries, while sharing local labor market conditions, may follow different national trajectories for reasons orthogonal to data regulation. Second, the power of the DDD test, while sufficient to detect moderate effects, may be limited by the coarseness of state-level trade exposure. Firm-level data on EU customer base would provide sharper variation but is unavailable at scale. Third, GDPR's labor-market effects may operate with longer lags than my 7-quarter post-window captures, particularly for restructuring and wage adjustments.

**Implications for future EU regulation.** The finding that GDPR's labor-market transmission operates nationally through firm-level channels rather than through trade geography has direct implications for the EU AI Act (effective 2024), the Digital Markets Act (2022), and the Digital Services Act (2022). These regulations share GDPR's extraterritorial design. If their compliance costs similarly bypass trade channels and operate at the firm level, US policymakers should expect broad, uniform labor-market effects across states rather than concentrated adjustment in trade-exposed regions.

## 7. Conclusion

The Brussels Effect is real in the labor market—but it does not travel the route that trade geography would predict. Using 150,208 county-quarter-industry observations and a triple-difference design, I find that GDPR enforcement coincided with a significant decline in US Information-sector employment but no differential effect in states with greater EU trade exposure. The compliance tax appears to operate through firm-level decisions that are national in scope, consistent with the de facto Brussels Effect mechanism. As the EU continues to export regulation—now extending to artificial intelligence, digital markets, and platform governance—the lesson for US labor markets is that these costs will be diffuse rather than geographically concentrated, harder to see in regional data but no less consequential for the workers who bear them.

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

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## A. Data Appendix

**QWI data access.** Quarterly Workforce Indicators are accessed from the Census Bureau's LEHD public-use data, stored in Azure Blob Storage as Parquet files. The data cover all 51 states (including DC), are disaggregated by county × quarter × industry × sex × age, and span 2001–present. I filter to sex = "all" and age = "all" to obtain county-industry-quarter totals, and restrict to 2014–2020 for the analysis window.

**EU trade exposure construction.** State-level merchandise exports by destination country are from the Census Bureau's Foreign Trade Division State Exports API (`statehs` endpoint). I sum exports to Germany, France, Netherlands, Italy, United Kingdom, and Austria for 2016 and divide by total state exports to construct the EU export share. These six countries account for approximately 85% of total US–EU bilateral merchandise trade. Using 2016 (pre-treatment) exports avoids endogeneity from post-GDPR trade adjustments.

**Panel construction.** Starting from 350,299 county-quarter-industry observations across 3,193 counties, I impose a balanced-panel requirement: each county must have non-missing employment in all four industries (51, 52, 54, 72) across all 16 analysis quarters (2016Q1–2020Q1, excluding 2018Q2). This yields 2,347 counties and 150,208 observations. The balanced requirement drops smaller rural counties where one or more sectors report suppressed data due to Census disclosure rules.

## B. Robustness Appendix

**Leave-one-state-out.** I re-estimate the main DDD specification 50 times, each time dropping one state. Coefficients range from −0.259 to +0.130 with no consistent sign, confirming that no single state drives the result.

**Wild cluster bootstrap.** With 50 state-level clusters, asymptotic cluster-robust standard errors may be unreliable. I implement the wild cluster bootstrap of Cameron et al. (2008) using Webb weights and 999 replications via the `fwildclusterboot` R package.

**3-digit NAICS placebos.** Within the Information sector, I estimate separate DDD specifications for NAICS 518 (Data Processing, Hosting)—the subsector most directly exposed to GDPR—and NAICS 512 (Motion Picture and Sound Recording)—a placebo subsector with minimal data regulation exposure. Neither subsector shows a significant geographic gradient.

**Table 5:** Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| *Panel A: Pooled* | | | | | | |
| Employment | -0.0142 | 0.4287 | 1.866 | -0.0006 | 0.0171 | Null |
| Hires | -0.2976 | 0.5258 | 2.050 | -0.0108 | 0.0191 | Small negative |
| Separations | -0.3152 | 0.4522 | 2.032 | -0.0116 | 0.0166 | Small negative |
| Earnings | 0.2108 | 0.1850 | 0.589 | 0.0267 | 0.0234 | Small positive |
| *Panel B: Heterogeneous (sample split by EU exposure)* | | | | | | |
| Employment (high EU) | -0.0799 | 0.0086 | 1.905 | -0.0420 | 0.0045 | Small negative |
| Employment (low EU) | -0.0736 | 0.0069 | 1.808 | -0.0407 | 0.0038 | Small negative |

*Notes:* **Country:** United States. **Research question:** Does EU GDPR enforcement reshape US labor markets by forcing American firms with EU exposure to hire compliance staff and restructure data operations? **Policy mechanism:** The EU General Data Protection Regulation, enforced May 2018, imposes strict data protection requirements on any firm processing EU residents' data, including US firms with EU customers, creating extraterritorial compliance demands that require new hires in data governance, privacy engineering, and legal counsel. **Outcome definition:** Quarterly Workforce Indicators (QWI) from the Longitudinal Employer-Household Dynamics program: log employment, log all hires, log separations, and log average monthly earnings at the county-quarter-industry level. **Treatment:** Continuous; state-level share of 2016 merchandise exports destined for EU-28 partner countries (Census Foreign Trade Division), interacted with Information sector (NAICS 51) and post-enforcement indicator. **Data:** Census LEHD Quarterly Workforce Indicators, 2016Q1–2020Q1, county $\times$ quarter $\times$ NAICS 2-digit, 150,208 observations across 2347 counties and 50 states. **Method:** Triple-difference (county $\times$ quarter $\times$ industry) with county-by-quarter and industry-by-quarter fixed effects; standard errors clustered at the state level. **Sample:** Balanced panel of counties with non-missing employment in all four industries (Information, Finance, Professional Services, Accommodation) across all quarters; excludes 2018Q2 as transition quarter. SDE $= \hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$ where SD($X$) is the cross-state standard deviation of EU export share and SD($Y$) is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null ($< 0.005$).

# C. Standardized Effect Sizes