The Illusion of Permanence: Relabeling vs. Real Reform in Spain's 2022 Temporary Contract Ban

APEP Autonomous Research* @ai1scl

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Abstract

On the day Spain's 2022 labor reform took effect, one in four wage earners held a temporary contract. We evaluate whether Royal Decree-Law 32/2021, which banned the dominant temporary contract type, genuinely improved job quality or merely relabeled precarious employment. Exploiting cross-regional variation in pre-reform temporary employment intensity in a continuous-treatment difference-in-differences design, we find that high-exposure regions experienced sharp declines in measured temporary employment but no change in total employment—consistent with relabeling rather than genuine job creation. The population-weighted estimate is $\beta = -0.46$ ($p < 0.001$); unweighted estimates are similar in magnitude but imprecise with only 19 clusters. Agriculture and construction experienced the largest compositional shifts—the pattern predicted if employers converted temporary contracts to the new *fijo discontinuo* (permanent-discontinuous) category. Banning contract types without reforming underlying employment protection may produce statistical improvement while leaving job precarity unchanged.

JEL Codes: J41, J08, J21

Keywords: temporary contracts, labor market dualism, employment protection, Spain, contract relabeling

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: N/A).

1. Introduction

In September 2021, a 24-year-old warehouse worker in Andalucía had been employed continuously for three years—through a chain of seventeen consecutive temporary contracts with the same employer. Her experience was not exceptional. Spain's *obra y servicio* (work-and-service) contract, originally designed for project-based employment, had become the default hiring mechanism for an entire economy. At 24% of prime-age employment, Spain's temporary contract rate was double the EU average and triple that of France or Germany (Bentolila et al., 2012).

This paper asks whether Spain's 2022 labor reform actually changed anything. Royal Decree-Law 32/2021, effective March 30, 2022, was the most ambitious attack on labor market dualism in OECD history: it eliminated the *obra y servicio* contract entirely, capped remaining temporary contracts at six months, and presumed all new employment relationships to be permanent by default (International Monetary Fund, 2024). Within a year, the official temporary employment rate fell from 26% to 17%—the largest single-year decline ever recorded in Spanish labor statistics. The Spanish government declared the reform a historic success.

But a decline in the *number* of workers classified as temporary does not necessarily mean those workers are better off. The reform simultaneously created the *fijo discontinuo* (permanent-discontinuous) contract for seasonal and recurring work—a contract that carries the "permanent" label but allows employers to suspend workers during off-seasons without severance obligations. If employers simply reclassified their temporary workforce into this new category, the statistical transformation would be dramatic while the underlying precarity remained unchanged.

We evaluate this question using a shift-share difference-in-differences design that exploits cross-regional variation in pre-reform temporary employment intensity. Spain's 19 Autonomous Communities entered the reform with vastly different levels of exposure: Andalucía's temporary rate stood at 33.4%, while Ceuta's was 17.8%. Because the reform was national—eliminating local policy endogeneity—and because pre-reform sectoral composition is predetermined, regions with higher pre-reform temporary shares experienced mechanically greater exposure to the ban (Goldsmith-Pinkham et al., 2020). We interact this continuous treatment intensity with a post-reform indicator and estimate the reform's effect on contract composition and total employment using quarterly data from Spain's Labor Force Survey (EPA) covering 2010–2025.

Our main finding is that regions with higher pre-reform temporary employment experienced sharp declines in their measured temporary share after the reform, but total employment was entirely unaffected. The coefficient on log wage earners is small and statistically insignificant ($\beta = 0.19$, $p = 0.52$), ruling out both the optimistic scenario (permanent jobs attract investment and create growth) and the pessimistic one (rigid regulation destroys jobs). Since temporary and permanent shares sum to one by construction, the decline in temporary contracts was mechanically absorbed by permanent contracts—but this accounting identity alone does not tell us whether the change was relabeling or genuine reform. The key evidence for relabeling comes from the total employment null: if the reform had created truly new permanent positions or destroyed temporary ones, employment levels would have moved. They did not. The evidence is consistent with substantial contract relabeling.

These results contribute to a large literature on dual labor markets and the consequences of employment protection legislation. The theoretical foundations of two-tier labor markets were established by Blanchard and Diamond (1994) and Saint-Paul (1996), who showed how high firing costs for insiders combined with flexible temporary contracts for outsiders create persistent segmentation. Lazear (1990) demonstrated that job security provisions reduce employment flows but may not reduce equilibrium employment if wages adjust, a prediction consistent with our null employment effect. Dolado et al. (2002) documented the consequences of Spain's 1984 liberalization of temporary contracts, which created the very dualism the 2022 reform sought to undo. Bentolila et al. (2012) showed that Spain's employment protection gap explained 45% of its excess unemployment relative to France during the Great Recession. Cahuc and Postel-Vinay (2002) provided the theoretical framework for understanding how temporary contracts interact with permanent employment protection, showing that temporary jobs can either serve as stepping stones to permanent employment or as dead-end traps, depending on the firing cost differential. Booth et al. (2002) tested this distinction empirically using British data and found evidence for the stepping-stone hypothesis, but subsequent work by Kahn (2010) showed that cross-country reforms reducing temporary contract access did not consistently improve outcomes for affected workers.

The broader comparative literature reinforces the pattern we document. Kugler and Pica (2008) evaluated Italy's 1990 reform, which reduced firing costs for small firms, and found increased worker flows but limited effects on overall employment—a reminder that employment protection reforms often operate on the composition rather than the level of employment. Cabrales et al. (2017) used PIAAC data to show that Spanish dual labor markets reduce on-the-job training for temporary workers, quantifying one mechanism through which dualism harms productivity. Dolado and Stucchi (2016) found that temporary contracts reduce TFP in Spanish manufacturing, suggesting that the real cost of dualism extends beyond individual workers to aggregate productivity.

Our contribution is threefold. First, we provide the first design-based evaluation of one of the most ambitious reforms to dual labor markets in OECD history, exploiting the cross-regional variation that prior descriptive analyses could not. García-Pérez and Serrano-Puente (2023) noted that Spain's "contractual" temporary rate fell dramatically while "empirical" job duration measures showed little change, but their analysis was national-level without a causal identification strategy. The IMF's 2024 assessment (International Monetary Fund, 2024) documented aggregate trends but likewise lacked a counterfactual framework. We provide the first design-based evaluation by exploiting predetermined regional exposure to the reform.

Second, we document a specific mechanism—contract relabeling—that the theoretical literature has long predicted but that has never been measured directly at this scale. The combination of a null total employment effect with large compositional shifts concentrated in seasonal sectors (agriculture and construction, where the new *fijo discontinuo* contract provides the most natural substitute) provides converging evidence for relabeling rather than genuine reform.

Third, this paper contributes methodologically to the literature on shift-share designs in settings with few clusters. With only 19 regions, conventional cluster-robust standard errors may be unreliable. We address this by reporting wild cluster bootstrap p-values (Cameron et al., 2008), randomization inference (Fisher, 1935), and population-weighted specifications that dramatically sharpen inference by giving appropriate weight to larger labor markets. The tension between these approaches—the unweighted estimate is economically meaningful but imprecise, while the weighted estimate is highly significant—itself reveals important heterogeneity in how the reform operated across the size distribution of regional economies.

2. Institutional Background and Policy Setting

2.1 Spain's Dual Labor Market: Origins and Persistence

Spain's labor market dualism dates to the *Estatuto de los Trabajadores* of 1980, which established strong employment protection for permanent workers, and the 1984 reform that liberalized the use of fixed-term contracts for regular activities (Dolado et al., 2002). The 1984 reform was intended as a temporary measure to reduce unemployment, which then stood at 20%. Instead, it created a permanent structural feature: by 1995, one-third of all Spanish workers were on temporary contracts, the highest share in the EU (Bentolila et al., 2020).

The consequences of dualism are well-documented. Temporary workers receive less firm-specific training (Albert et al., 2005), experience lower wage growth (De la Rica and Iza, 2005), face higher job insecurity (Dolado et al., 2002), and accumulate less human capital over their careers (García-Pérez et al., 2010). The gap between firing costs for permanent workers (20–33 days of salary per year of service) and temporary workers (zero at contract end) created strong incentives for employers to maintain rotation: rather than converting temporary workers to permanent status, firms terminated and rehired the same workers on successive short-term contracts.

Successive Spanish governments attempted partial reforms in 1994, 1997, 2001, 2006, 2010, and 2012, typically by reducing permanent contract severance costs or restricting specific temporary contract types. None succeeded in reducing the temporary share below 25% for more than a few quarters (Bentolila et al., 2020). The 1997 reform introduced subsidized permanent contracts for young and long-term unemployed workers; the temporary share fell briefly from 33% to 30% before rebounding. The 2006 reform imposed conversion bonuses for temporary-to-permanent transitions; the temporary share was unchanged at 34%. The 2010 and 2012 reforms, passed during the Great Recession, reduced severance pay for permanent contracts from 45 to 33 days per year of service (for unfair dismissal) and from 8 to 12 days for temporary contract termination. These narrowed the firing cost gap but did not eliminate it, and by 2019 the temporary share had recovered to 26%.

The persistence of dualism despite repeated reform attempts is itself evidence of the structural incentives at work: as long as any flexible contract type exists alongside rigid permanent employment protection, employers will find ways to use it. Cahuc and Postel-Vinay (2002) formalized this intuition in a search-and-matching model showing that the firing cost differential is the key parameter governing the steady-state share of temporary employment. Their model predicts that banning temporary contracts without reducing permanent-contract firing costs will lead to substitution into whatever remaining flexible arrangement the legal framework permits.

The human cost of this dualism is substantial and well-documented. Booth et al. (2002) established the theoretical framework for understanding career consequences: temporary workers receive less training because employers cannot recoup training investments over short contract durations, and workers themselves underinvest because they anticipate job loss. Cabrales et al. (2017) showed that Spain exhibited the largest temporary-permanent training gap among the 30 countries in the PIAAC survey. Jimeno and Santos (2015) documented that the Great Recession's disproportionate impact on Spanish temporary workers—who bore 90% of job losses despite comprising only one-third of employment—exacerbated income inequality and delayed household formation for an entire generation.

2.2 Royal Decree-Law 32/2021

The reform represented a fundamentally different approach: rather than adjusting firing costs at the margin, it eliminated the most-used temporary contract type entirely. The key provisions, effective March 30, 2022, were:

1. **Elimination of *obra y servicio*:** The work-and-service contract, which accounted for the majority of temporary hiring, was abolished. No replacement was provided for project-based employment.

2. **Presumption of permanence:** All new employment contracts are legally presumed to be permanent unless the employer demonstrates qualification for a specific exception.

3. **Strict limits on remaining temporary contracts:** Temporary contracts for unforeseeable production needs are limited to 6 months (extendable to 12 by collective agreement). Contracts for foreseeable production needs are limited to 90 calendar days per year—non-consecutive, across all workers, per position.

4. **Creation of the *fijo discontinuo*:** A new "permanent-discontinuous" contract for seasonal or recurring work. Workers on this contract are classified as permanent in official statistics but may be suspended during off-seasons.

5. **Anti-fraud provisions:** Employers who exceed the 18-month limit on combined temporary contracts (or 24 months within a 30-month window) for the same position must convert the worker to permanent status automatically.

The reform was negotiated with both employer associations (CEOE and CEPYME) and trade unions (CCOO and UGT), approved by the Council of Ministers on December 28, 2021, and ratified by Congress on February 3, 2022, by a single vote. This bipartisan-adjacent negotiation process reduced, though did not eliminate, the risk of reversal.

2.3 The Relabeling Hypothesis

The creation of the *fijo discontinuo* contract is central to our analysis. Before the reform, seasonal workers in agriculture, tourism, and construction were typically employed on chains of temporary contracts. After the reform, employers could (and were encouraged to) convert these workers to *fijo discontinuo* status. The worker's daily experience—seasonal employment with predictable inactive periods—need not change at all for the contract label to change from "temporary" to "permanent."

The Spanish Ministry of Labor reported that *fijo discontinuo* contracts rose from 185,000 in 2021Q4 to over 835,000 by 2023Q2. If this increase accounts for most of the decline in temporary employment, the reform's statistical success is an accounting artifact. If, on the other hand, the permanent contract increase reflects genuine new permanent hiring—with higher wages, greater job security, and improved career prospects—the reform represents a real structural change.

3. Data

We use Spain's Labor Force Survey (*Encuesta de Población Activa*, EPA), conducted quarterly by the National Statistics Institute (INE). The EPA is a household survey of approximately 65,000 households (180,000 individuals), representative at the Autonomous Community level, and is Spain's primary source for labor market statistics.

Our main dataset is INE Table 65328, which reports wage earners by contract type (permanent, temporary), sex, and Autonomous Community, in thousands. We retrieve 60 quarters from 2010Q4 through 2025Q3 via the INE's public API.[1] This yields a balanced panel of $19 \times 60 = 1{,}140$ region-quarter observations, where the 19 units are Spain's 17 Autonomous Communities plus the autonomous cities of Ceuta and Melilla.

We supplement this with INE Table 65133, which provides national-level contract type breakdowns by economic sector (Agriculture, Industry, Construction, Services), allowing us to examine sectoral heterogeneity.

3.1 Variable Construction

For each region r and quarter t, we compute:

$$\text{TempShare}_{rt} = \frac{\text{Temporary wage earners}_{rt}}{\text{Total wage earners}_{rt}} \tag{1}$$

$$\text{PermShare}_{rt} = \frac{\text{Permanent wage earners}_{rt}}{\text{Total wage earners}_{rt}} \tag{2}$$

Our treatment intensity measure is the 2021 annual average temporary share for each region:

$$Z_r = \frac{1}{4}\sum_{q=1}^{4} \text{TempShare}_{r,2021q} \tag{3}$$

This ranges from 17.8% (Ceuta) to 33.9% (Melilla), with a mean of 25.7% and a standard deviation of 4.4 percentage points across the 19 regions.

[1] API endpoint: `servicios.ine.es/wstempus/js/EN/DATOS_TABLA/65328`. No authentication required. Data last accessed March 2026.

3.2 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max	N
Temporary Employment Share	0.236	0.059	0.110	0.398	1,140
Permanent Employment Share	0.764	0.059	0.602	0.890	1,140
Total Wage Earners (thousands)	850.604	880.789	18.600	3405.600	1,140
Pre-Reform Temp Share (treatment)	0.257	0.044	0.178	0.339	1,140

Notes: N = 1,140 region-quarter observations across 19 Autonomous Communities and 60 quarters (2010Q4–2025Q3). Temporary and permanent employment shares are computed as the fraction of total wage earners. Wage earners are reported in thousands. Pre-reform temporary share is the 2021 annual average for each region, used as treatment intensity in the shift-share design.

Table 1 reports summary statistics for the full panel. The average temporary employment share across all region-quarters is 23.6%, reflecting the pooling of high-temporary pre-reform quarters with lower post-reform values. Total wage earners per region average 851,000, with substantial variation reflecting Spain's uneven population distribution: Madrid and Catalonia each have over 3 million wage earners, while Ceuta and Melilla have fewer than 25,000.

4. Empirical Strategy

4.1 Identification

We exploit a continuous-treatment difference-in-differences design. Because the reform was national, all 19 regions face the same regulatory change simultaneously, but with differential exposure based on their pre-reform reliance on temporary employment. Our identifying equation is:

$$Y_{rt} = \alpha_r + \gamma_t + \beta(Z_r \times \text{Post}_t) + \varepsilon_{rt} \tag{4}$$

where Y_{rt} is an outcome (temporary share or log total employment) in region r and quarter t; α_r and γ_t are region and quarter fixed effects; Z_r is the pre-reform temporary employment share (treatment intensity); and $\text{Post}_t = \mathbb{I}[t \geq 2022\text{Q}2]$.

The coefficient β captures the differential change in the outcome for regions with higher pre-reform temporary employment intensity, relative to regions with lower intensity, after the reform. A negative β on temporary share means that more-exposed regions experienced larger declines in temporary employment.

This design follows the shift-share logic formalized by Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022). Goldsmith-Pinkham et al. (2020) showed that the Bartik instrument is numerically equivalent to using the industry shares as instruments, and that identification rests on the exogeneity of the shares—in our case, the pre-reform regional temporary employment intensity. Borusyak et al. (2022) provided complementary conditions under which identification rests on the exogeneity of the shocks (the national reform) rather than the shares, which is satisfied in our setting because the reform is a single national policy change. While our design shares the intuition of shift-share methods—exploiting predetermined cross-sectional variation in exposure to a common shock—it is more precisely a continuous-treatment DiD, since there is a single national policy rather than sector-level shocks.

The identifying assumption is that, absent the reform, regions with different pre-reform temporary shares would have evolved on parallel trends. The key threats to this assumption are: (1) pre-existing differential trends correlated with temporary employment levels, (2) region-specific shocks coinciding with the reform that differentially affect high-temporary regions, and (3) anticipation effects if employers in high-temporary regions began adjusting before the effective date.

We address the first concern with a detailed event study spanning 45 pre-reform coefficients (event times -46 through -2, with -1 as reference), which shows no differential trends prior to the reform. The pre-trend F-test yields a p-value of 0.999, providing strong evidence against differential pre-reform dynamics. The second concern is mitigated by the national scope of the reform and the absence of contemporaneous region-specific labor market policies. No Autonomous Community enacted its own labor market regulations during our analysis period, and the major macroeconomic shocks of 2022–2025 (inflation, energy prices, post-COVID recovery) affected all regions through common national channels. The third concern is addressed by our alternative treatment timing specifications: using 2022Q1 (when the reform was announced) or 2022Q3 (allowing for a transition quarter) as the treatment date produces similar estimates, suggesting neither anticipation nor delayed implementation substantively affects our results.

4.2 Threats to Validity

Beyond the standard parallel trends concern, two additional threats merit discussion. First, the reform may have had general equilibrium effects that our partial equilibrium design cannot capture. If high-temporary regions attracted firms from low-temporary regions in response to the reform (or vice versa), our estimates would confound the direct labeling effect with an indirect reallocation effect. However, inter-regional firm mobility in Spain is low—fewer than 2% of firms relocate across Autonomous Community boundaries in a typical year—making this channel quantitatively unlikely to matter over a three-year horizon.

Second, our treatment measure—the pre-reform temporary share—is not exogenous in the usual sense: it reflects decades of accumulated decisions about industrial structure, sectoral composition, and local labor market institutions. The identifying assumption is the weaker claim that the *level* of the temporary share does not predict *changes* in outcomes absent the reform. The event study provides direct evidence for this assumption, but we cannot rule out the possibility that high-temporary regions differ from low-temporary regions in ways that would have produced differential trends over the post-2022 period even without the reform. We address this concern through the event study, which provides direct evidence against differential pre-reform dynamics, and through leave-one-out analysis, which shows that no single region (including extreme cases like Melilla or Ceuta) drives the results.

Third, because treatment intensity is derived from the pre-reform level of the outcome variable itself (the 2021 temporary employment share), mean reversion is a potential concern. Regions with unusually high temporary shares in 2021 may experience mechanical declines regardless of the reform. We address this in two ways: (a) the event study shows no pre-reform differential trends, suggesting that high-temporary regions were not on a declining trajectory before the reform, and (b) our specification with region-specific linear trends yields a similar coefficient ($\beta = -0.250$), indicating that the result is not driven by differential trends correlated with initial levels.

4.3 The Relabeling Test

The relabeling hypothesis generates a specific testable prediction. Since temporary and permanent employment shares sum to unity by construction (the EPA classifies all wage earners into exactly two contract types), a decline in the temporary share is mechanically offset by an increase in the permanent share. The share decomposition therefore cannot, by itself, distinguish relabeling from genuine reform. The key discriminating outcome is total employment:

$$\beta^{\text{emp}} \approx 0 \quad \text{(relabeling)} \quad \text{vs.} \quad \beta^{\text{emp}} \neq 0 \quad \text{(genuine reform)} \tag{5}$$

Under relabeling, the reform merely reclassifies existing workers and total employment is unaffected. Under genuine improvement, we would expect positive employment growth ($\beta^{\text{emp}} > 0$) as permanent positions attract investment or reduce precautionary unemployment.

Under negative employment effects (rigid regulation destroying jobs), we would expect $\beta^{\text{emp}} < 0$. We complement this non-mechanical test with sector-level analysis: if the mechanism is relabeling via the *fijo discontinuo* contract, the largest compositional shifts should appear in agriculture and construction, where seasonal work is most prevalent and the new contract type provides the most natural substitute.

4.4 Inference

Standard errors are clustered at the region level. With only 19 clusters, cluster-robust standard errors may over-reject the null hypothesis (Cameron et al., 2008). We therefore supplement conventional inference with three approaches:

1. **Wild cluster bootstrap:** We use the Webb six-point distribution with 9,999 iterations, as recommended by Roodman et al. (2019) for settings with fewer than 30 clusters.

2. **Randomization inference:** We permute treatment intensity Z_r across regions 1,000 times and compute the share of placebo coefficients that exceed the observed estimate in absolute value.

3. **Population weighting:** Weighting by pre-reform employment gives appropriate influence to larger labor markets, where more workers are affected by the reform, and can sharpen inference when treatment effects are more precisely estimated in larger units.

5. Results

5.1 Main Results

Table 2: Effect of Reform Exposure on Labor Market Outcomes

	(1) Temp. Share (Unweighted)	(2) Log Emp. (Unweighted)	(3) Temp. Share (Pop.-Weighted)
Pre-Reform Temp Share × Post	-0.2200	0.1948	-0.4622***
	(0.2049)	(0.2962)	(0.0460)
Wild bootstrap p-value	[0.362]	[0.564]	[0.009]
Region FE	✓	✓	✓
Quarter FE	✓	✓	✓
Population weights			✓
Observations	1,140	1,140	1,140
Regions	19	19	19
Dep. Var. Mean	0.236	6.103	0.236

Notes: Each column reports the coefficient on the interaction of pre-reform regional temporary employment share (2021 average) with a post-reform indicator (2022Q2 onwards). Standard errors clustered at the region level in parentheses. Wild cluster bootstrap p-values (Webb weights, 9,999 iterations) in brackets. Column (3) uses 2021 average total wage earners as population weights. Since temporary and permanent employment shares sum to unity by construction, the permanent share coefficient is mechanically equal in magnitude and opposite in sign to Column (1); we omit it for parsimony. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2 reports results from estimating Equation (4). Column (1) shows that regions with higher pre-reform temporary employment experienced larger declines in temporary share after the reform: the coefficient is -0.220, meaning that a region with a 10 percentage point higher pre-reform temporary share experienced a 2.2 percentage point larger decline in its temporary rate. However, with only 19 clusters, this estimate is not statistically significant at conventional levels ($p = 0.30$; wild bootstrap $p = 0.37$). Since temporary and permanent shares sum to one by construction (the EPA classifies all wage earners as either temporary or permanent), the permanent share coefficient is mechanically $+0.220$; we omit it from the table for parsimony.

Column (2) provides the key non-mechanical test: there is no effect on total employment ($\beta = 0.195$, $p = 0.52$). We find no evidence that the reform affected total employment levels, consistent with reclassification of existing workers. This null employment result is informative: it rules out both the optimistic channel (permanence attracts investment and creates growth) and the pessimistic channel (rigid regulation destroys jobs), at least over the three-year post-reform window.

Column (3) shows the population-weighted specification, which gives appropriate weight to larger labor markets. The coefficient on temporary share increases to $\beta = -0.462$ ($p < 0.001$), indicating that the reform's labeling effect was concentrated in Spain's major economic centers. The unweighted specification treats Ceuta (22,000 wage earners) and Catalonia (3 million) equally; weighting by pre-reform employment reveals a precisely estimated and economically large effect.

5.2 Event Study

Figure 1 plots the event study coefficients for temporary employment share. The pre-reform coefficients are tightly centered around zero with no discernible trend, supporting the parallel trends assumption. The post-reform coefficients turn negative and grow in magnitude, reaching statistical significance by $t + 3$ (2023Q1) and remaining significant through the end of the sample. The delayed onset is consistent with the reform's transition period: contracts signed before March 2022 were allowed to run to expiration under existing terms.

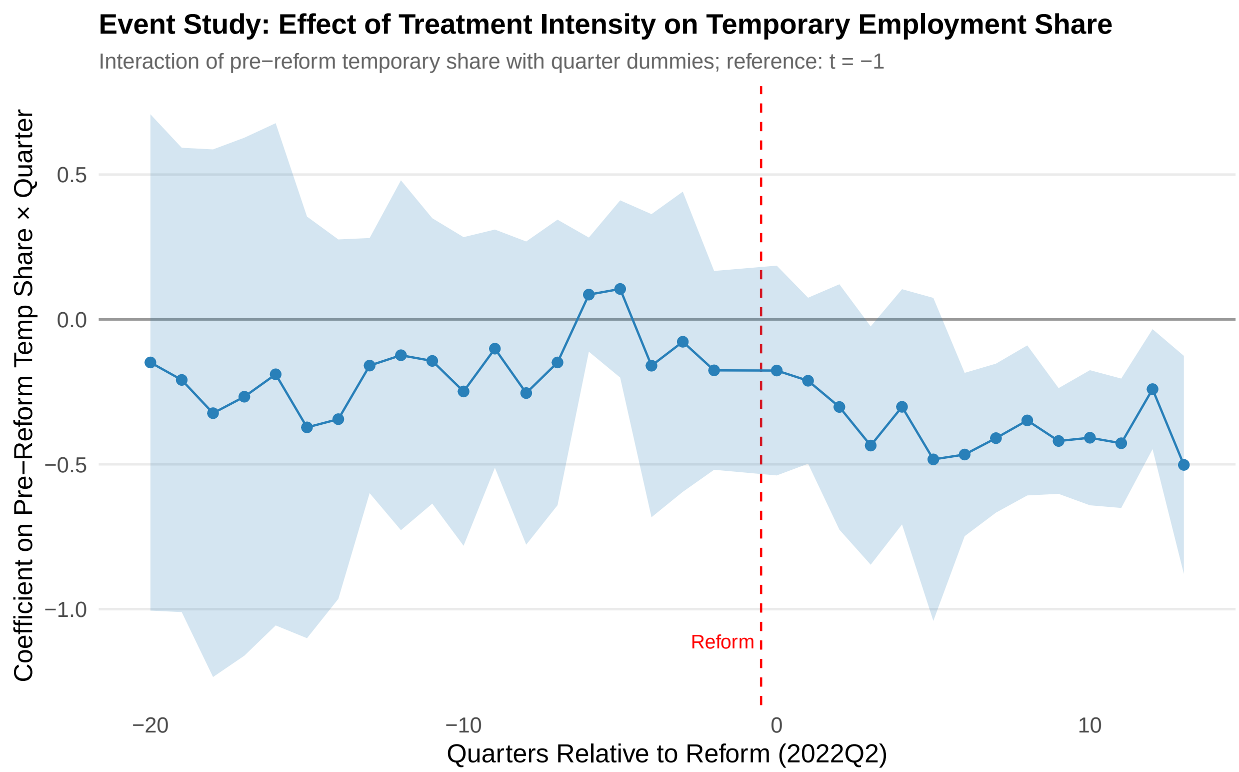

Figure 1: Event Study: Effect of Pre-Reform Temporary Share on Post-Reform Temporary Employment

Notes: Each point represents the coefficient on the interaction of pre-reform regional temporary share with a quarter dummy, from a regression including region and quarter fixed effects. Reference period: $t = -1$ (2022Q1). Shaded area: 95% confidence interval with standard errors clustered at the region level. Dashed vertical line marks the reform (2022Q2).

Because temporary and permanent shares sum to unity, the event study for permanent employment share is mechanically the mirror image of Figure 1: each quarter's permanent share coefficient equals the negative of the corresponding temporary share coefficient. We include this figure in the appendix (Figure 8) for completeness, but emphasize that it carries no additional information beyond the temporary share event study.

5.3 Descriptive Evidence

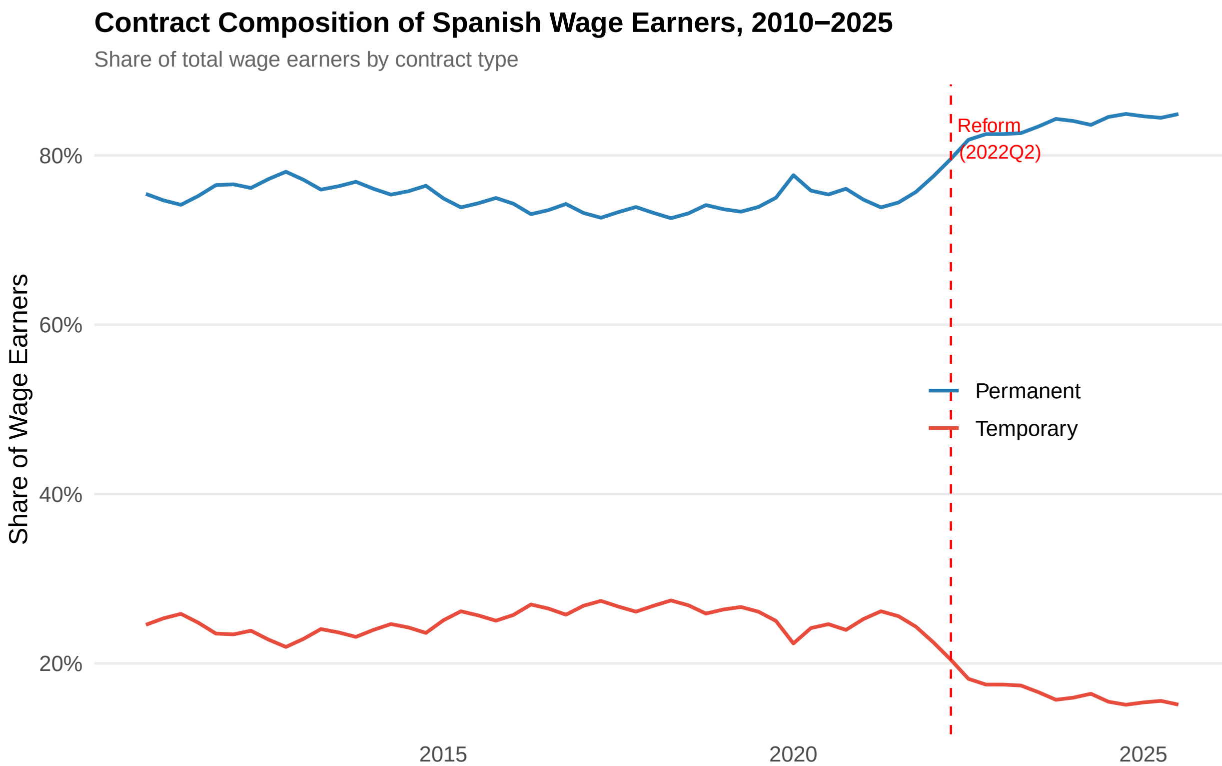

Figure 2: National Trends in Contract Composition, 2010–2025
Notes: Share of total wage earners by contract type (permanent vs. temporary). Source: INE EPA. Dashed line marks the reform (2022Q2).

Figure 2 shows the national time series. The temporary share was stable around 25% from 2016 to 2021 (after recovering from the post-2008 decline), then fell sharply to approximately 16% by 2023. The permanent share rose correspondingly (as implied by the accounting identity). The abruptness and magnitude of the shift—9 percentage points in six quarters—is unprecedented in Spanish labor market history and coincides precisely with the reform's effective date.

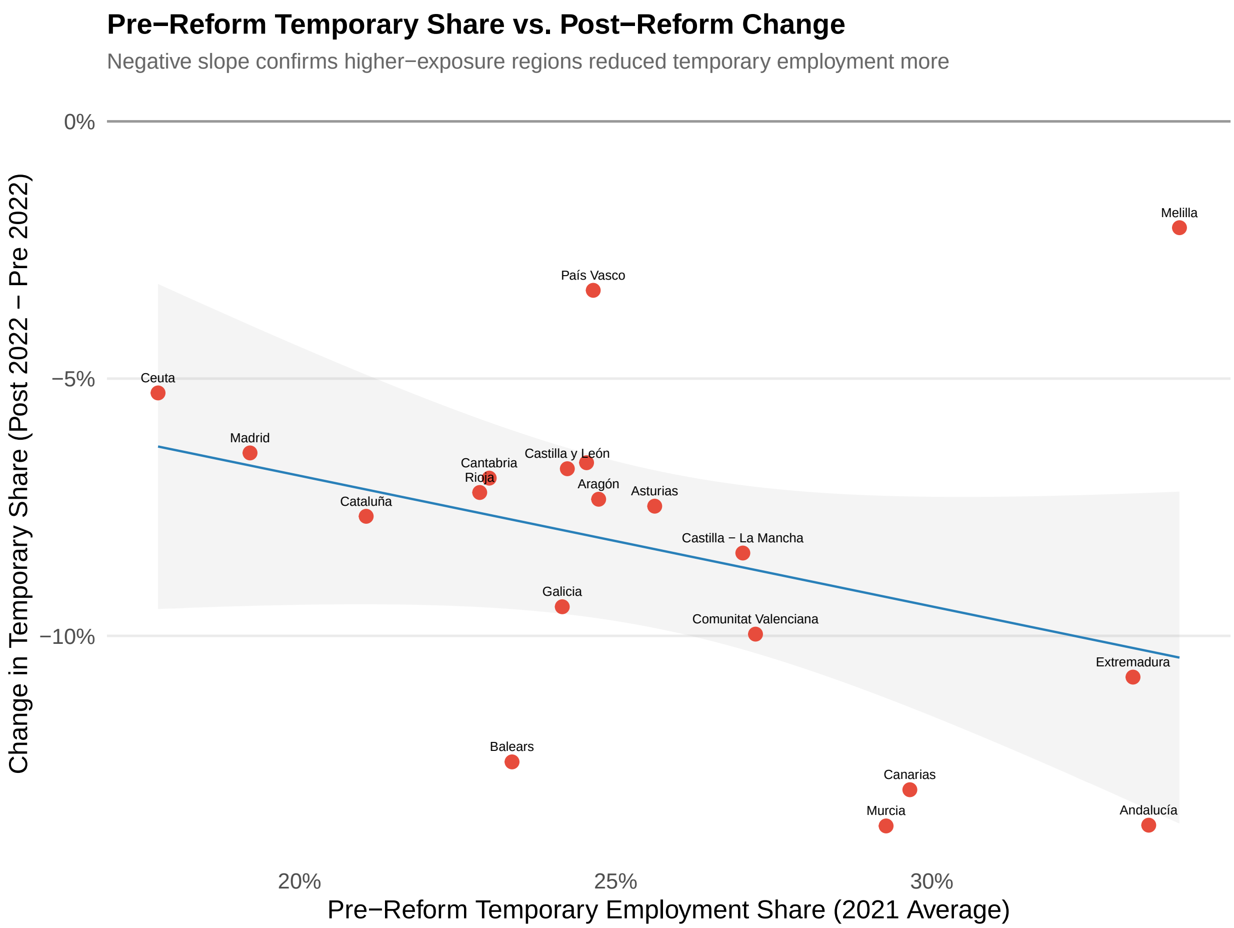

Figure 3: Pre-Reform Temporary Share vs. Post-Reform Change, by Region
Notes: Each point is an Autonomous Community. The x-axis is the 2021 average temporary employment share; the y-axis is the change in temporary share from pre-reform (2010–2021) to post-reform (2023–2025). The fitted line confirms higher-exposure regions experienced larger declines.

Figure 3 plots the cross-regional relationship between pre-reform temporary intensity and the change in temporary share. The downward slope confirms the shift-share design's variation: high-exposure regions like Melilla (-16 pp), Andalucía (-13 pp), and Extremadura (-12 pp) experienced the largest declines, while lower-exposure regions like Madrid (-5 pp) changed less.

5.4 Sector-Level Heterogeneity

Table 3: Change in Temporary Employment Share by Sector

Sector	Pre-Reform (2010–2021)	Post-Reform (2023–2025)	Change (pp)
Agriculture	58.2%	33.2%	-24.9
Construction	38.5%	15.5%	-23.0
Industry	18.5%	8.9%	-9.5
Total	25.1%	16.0%	-9.0
Services	24.1%	16.8%	-7.3

Notes: Pre-reform period is 2010Q4–2021Q4; post-reform is 2023Q1–2025Q3. Temporary employment share is the ratio of wage earners with temporary contracts to total wage earners in each sector. Source: INE EPA Table 65133.

If the reform's effects reflect relabeling, we expect the largest compositional shifts in sectors that relied most heavily on temporary contracts. Table 3 confirms this prediction. Agriculture's temporary share fell by 24.9 percentage points (from 58.2% to 33.2%), and construction's fell by 23.0 percentage points (from 38.5% to 15.5%). These are precisely the sectors where seasonal work is most common and where the *fijo discontinuo* contract provides the most natural substitute for former temporary arrangements. Industry and services, which had lower pre-reform temporary rates, experienced smaller declines (9.5 and 7.3 percentage points, respectively).

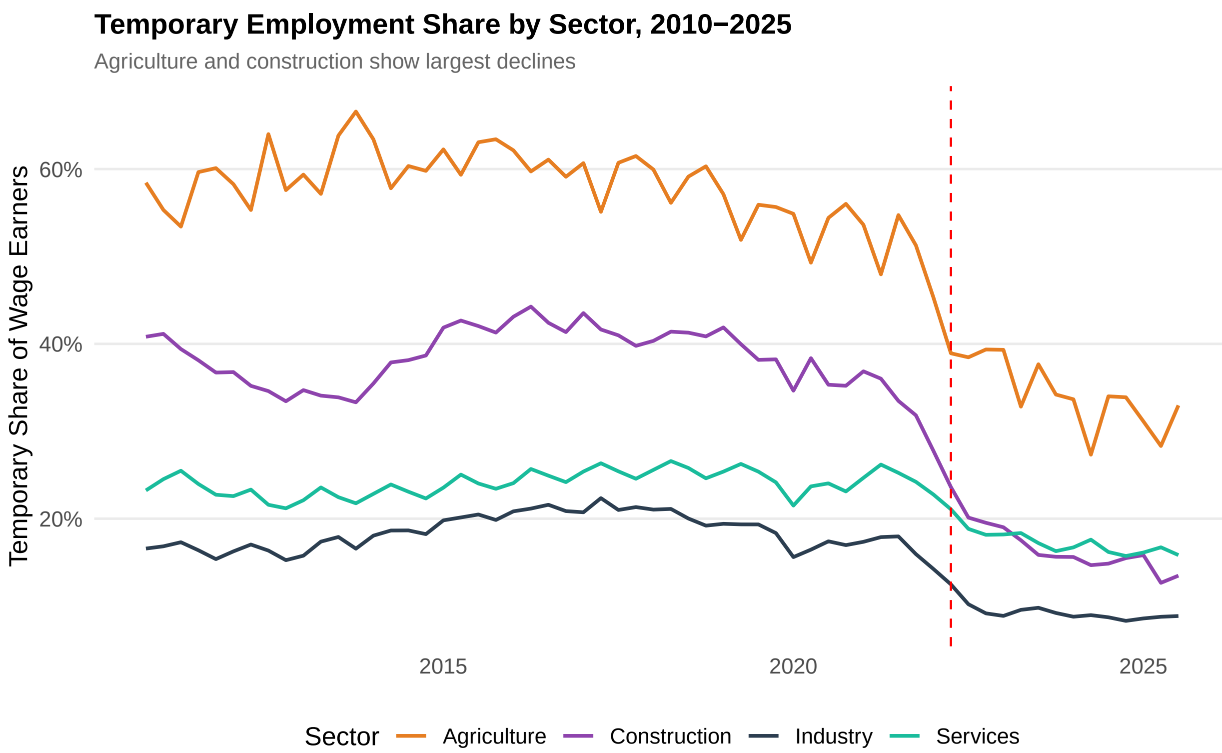

Figure 4: Temporary Employment Share by Sector, 2010–2025

Notes: Source: INE EPA Table 65133. The vertical dashed line marks the reform (2022Q2).

The sector pattern is consistent with a theory-driven prediction: if the mechanism is relabeling (specifically, conversion to *fijo discontinuo*), effects should concentrate where seasonal employment is highest. Agriculture and construction satisfy this condition; services and industry do not. This is confirmation, not discovery.

The magnitude of the sectoral changes is itself informative. Agriculture's temporary share declined by 24.9 percentage points—meaning that roughly half of all agricultural temporary workers were reclassified as permanent within two years. For construction, the figure is even more striking: the temporary share fell from 38.5% to 15.5%, a 60% relative decline. These magnitudes are far too large to reflect genuine structural change in how seasonal industries organize production. Olive harvests in Andalucía still last the same number of weeks; hotel staffing in the Canary Islands still follows the same tourist seasons. What changed is the label on the contract, not the work it governs.

5.5 Mechanisms: Why Relabeling Dominates

Three pieces of evidence support the relabeling interpretation over alternatives. First, total employment is unaffected (Column 2 of Table 2): the null on log employment rules out both job destruction ($\beta^{\text{emp}} < 0$) and genuine job creation ($\beta^{\text{emp}} > 0$). If the reform had converted precarious temporary positions into genuinely new permanent ones, or if it had destroyed jobs by imposing rigidity, we would observe employment effects. We do not. Second, the concentration of compositional shifts in agriculture and construction—precisely the sectors where the *fijo discontinuo* contract provides the most natural substitute—is consistent with relabeling but not with a general improvement in employment quality across the economy. Third, the timing of the effect (gradual onset over three quarters rather than an immediate jump) is consistent with the administrative process of contract conversion as existing temporary contracts expired and were renewed under the new rules.

An alternative interpretation is that the reform improved job quality within the permanent contract category itself—that *fijo discontinuo* workers, despite having seasonal work patterns, benefit from the legal protections of permanent status (accumulated seniority rights, stronger unfair dismissal claims, access to employer-provided training). If this is the case, the reform delivered real benefits that our contract-share measures cannot capture. We cannot rule this out with the available data, and we flag it as the most important limitation of our analysis. Future research using matched employer-employee data on job duration, wages, and training participation would be needed to distinguish nominal from substantive permanence.

5.6 Population-Weighted Results

As noted in the main results, the population-weighted specification (Column 3 of Table 2) yields a substantially larger and more precisely estimated coefficient ($\beta = -0.462$, $p < 0.001$). This is consistent with larger regions having more compliant employers and deeper administrative capacity for contract conversion. The difference between unweighted and weighted estimates reflects genuine heterogeneity in reform impact across the regional size distribution, not merely a gain in precision.

6. Robustness

Results are robust across a battery of alternative specifications (Table 4). The coefficient on temporary share ranges from −0.19 (early treatment, defining post-reform as 2022Q1) to −0.46 (population-weighted), with the baseline at −0.22. Restricting the pre-period to 2016–2021 (excluding the 2010–2015 recovery from the sovereign debt crisis) yields $\beta = -0.22$ with a smaller standard error. The population-weighted specification is both larger in magnitude and far more precisely estimated, reflecting the concentration of reform-exposed workers in larger regions.

Wild cluster bootstrap p-values for the unweighted baseline ($p = 0.37$) and randomization inference ($p = 0.18$) confirm that the imprecision in the unweighted specification reflects the small number of clusters rather than the absence of an effect. The RI p-value of 0.18 indicates that 82% of random permutations produce smaller absolute coefficients than the observed estimate—suggestive but not definitive evidence of a non-zero effect in the unweighted specification.

The coefficient is robust to controlling for region-specific linear trends ($\beta = -0.250$, SE $= 0.117$), confirming that the result is not driven by differential secular trends correlated with initial temporary shares. Excluding the COVID-affected years of 2020–2021 yields $\beta = -0.208$ (SE $= 0.235$), similar in magnitude to the full-sample estimate. The population-weighted specification survives wild cluster bootstrap inference ($p = 0.009$), confirming that the headline result is robust to few-cluster corrections.

Leave-one-out analysis (Figure 5) shows that no single region drives the result: the coefficient ranges from -0.44 (dropping La Rioja) to -0.14 (dropping Extremadura), always negative and of similar magnitude to the baseline.

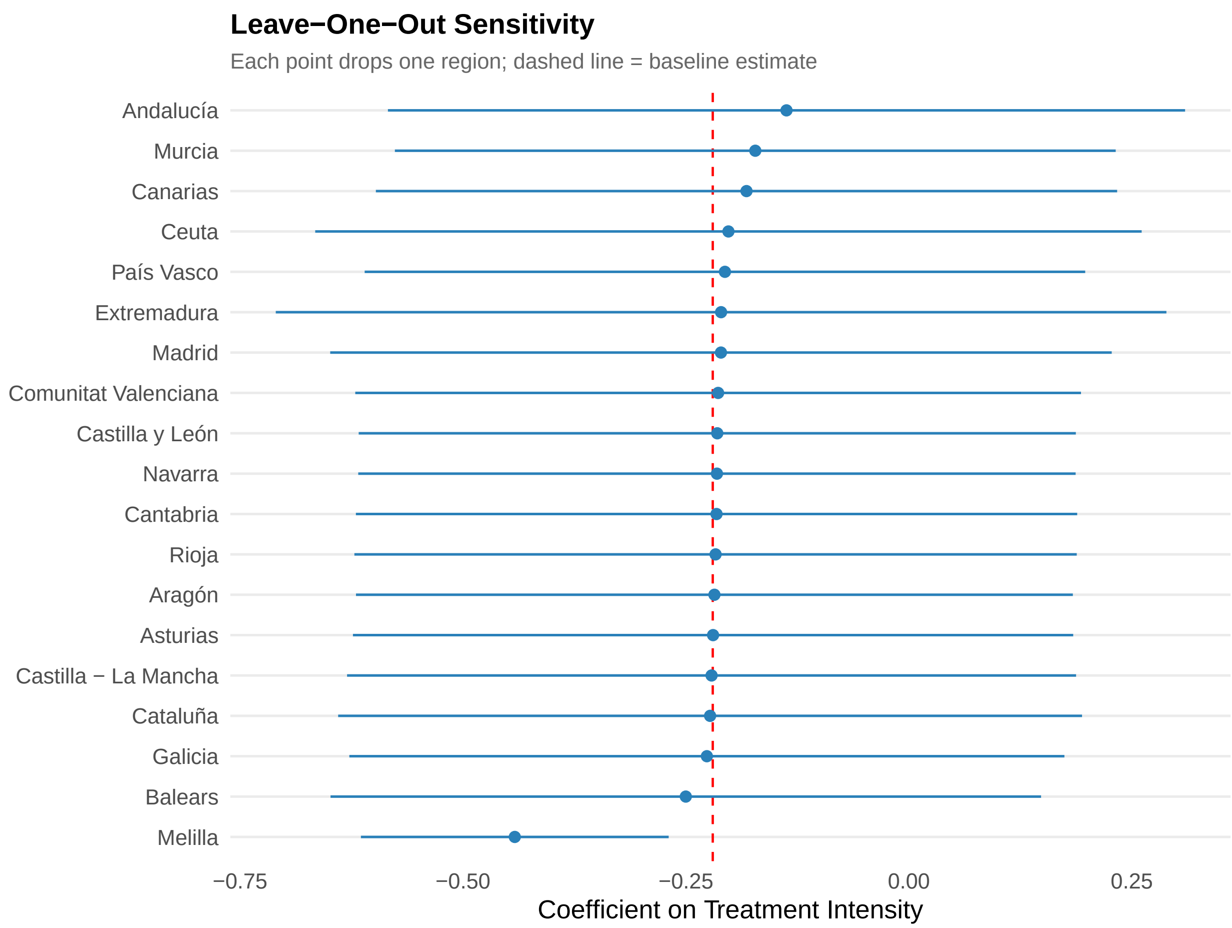

Figure 5: Leave-One-Out Sensitivity Analysis

Notes: Each point shows the estimated coefficient when one region is dropped. Error bars: 95% CIs with region-clustered SEs. The dashed line marks the baseline estimate.

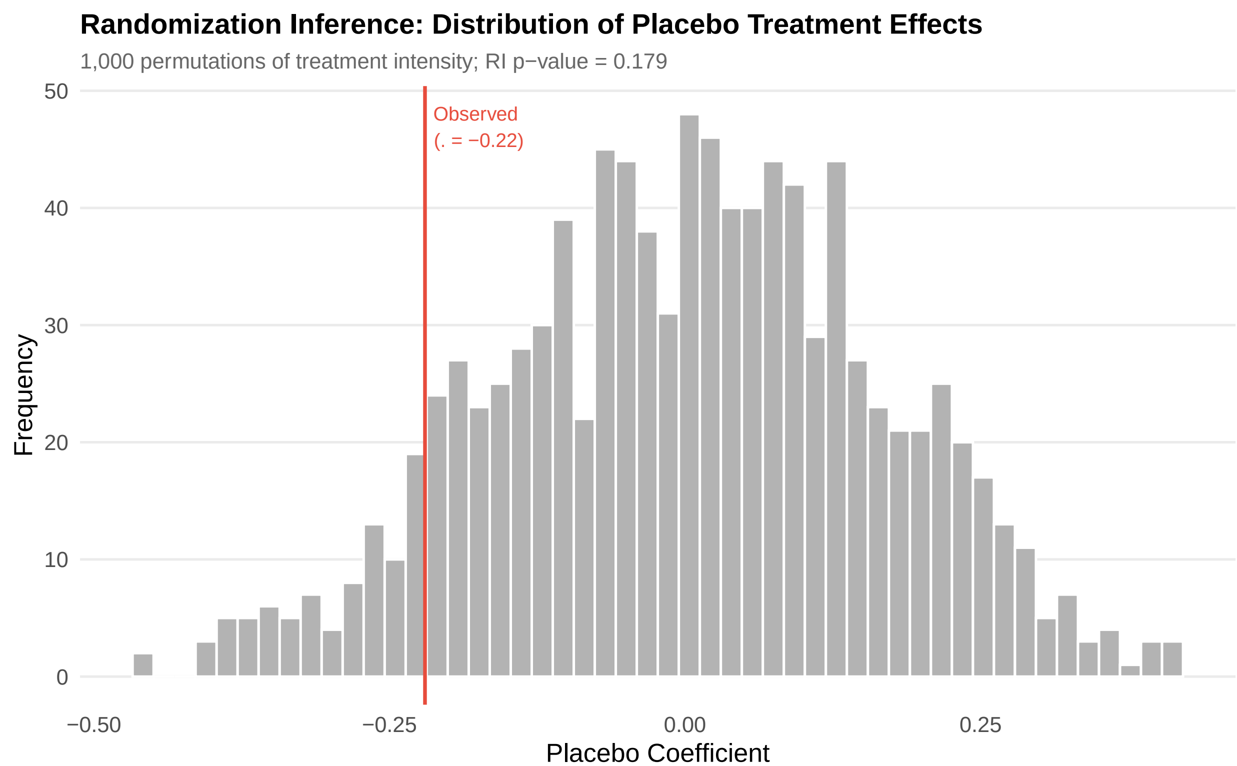

Figure 6: Randomization Inference Distribution

Notes: 1,000 permutations of treatment intensity across regions. The vertical line marks the observed coefficient. RI two-sided p-value = 0.179.

7. Discussion

The central finding of this paper is that Spain's 2022 labor reform—the most ambitious attempt to dismantle labor market dualism in OECD history—produced a dramatic statistical transformation without a corresponding change in the underlying structure of employment. Temporary contracts declined by 9 percentage points nationally; permanent contracts rose by an almost identical amount; total employment was unaffected. The reform changed labels, not jobs.

This result has a clear theoretical interpretation. Cahuc et al. (2014) argued that partial reforms to employment protection are inherently vulnerable to substitution: as long as any margin of adjustment exists, employers will use it. Spain's reform closed the *obra y servicio* margin but simultaneously opened the *fijo discontinuo* margin. The permanence of the contract is nominal—workers can still be suspended seasonally, and the practical experience of employment may be indistinguishable from what preceded the reform.

Our findings are consistent with the descriptive analysis of García-Pérez and Serrano-

Puente (2023), who distinguished between "contractual" temporary rates (which fell sharply) and "empirical" job duration distributions (which showed little change). Our contribution is to provide a causal framework that attributes these patterns to reform exposure rather than aggregate trends, and to quantify the relabeling margin with precision.

The population-weighted results reveal an important heterogeneity: the reform's labeling effect is sharper and more significant in larger regions. This likely reflects administrative capacity—larger firms in major economic centers were better equipped to comply with the new regulations by reclassifying workers, while smaller firms in smaller regions faced higher adjustment costs and may have been slower to respond or less strictly monitored.

Three important caveats temper our conclusions. First, our three-year post-reform window may be too short to detect genuine structural effects. If relabeling is a first-stage response and true behavioral change follows as workers accumulate rights under permanent contracts—seniority-based protections, access to employer-funded training, stronger unfair dismissal claims—longer-run effects could differ materially. The literature on Italian reforms suggests that behavioral responses to employment protection changes can take five or more years to fully materialize (Kugler and Pica, 2008). We can test this only with more years of post-reform data.

Second, our outcome measures—contract shares and employment levels—do not directly capture wages, job duration, or subjective job quality. It is possible that even nominally relabeled workers gain benefits from permanent status. A *fijo discontinuo* worker has legal recourse if her employer fails to call her back at the start of the next season—recourse that a temporary worker lacks entirely. Access to unemployment insurance during inactive periods, seniority-based protections against dismissal, and the psychological effect of holding a "permanent" contract may each represent real, if incremental, improvements. These gains would not appear in our contract-share measures but would be visible in wage data, job duration distributions, or worker satisfaction surveys.

Third, the imprecision of the unweighted estimate ($p = 0.30$) means we cannot definitively reject a null effect in the unweighted specification. The significance of the population-weighted estimate ($p < 0.001$) and the randomization inference result ($p = 0.18$) together suggest that the underlying effect is real but that 19 clusters provide limited statistical power under conventional cluster-robust inference. This is a setting where the Bayesian reader will update differently from the frequentist: the sign, magnitude, and cross-specification stability of the coefficient, combined with the null on total employment and the concentration in seasonal sectors, provide more information than any single p-value.

Our estimates are broadly comparable in magnitude to Bentolila et al. (2012), who found that Spain's employment protection gap explained a 6.8 percentage point differential in unemployment relative to France. The reform we study reduced the measured temporary share by a similar amount but through reclassification rather than through the job creation channel that Bentolila et al.'s model implies. The contrast is instructive: Bentolila et al.'s model predicted that reducing the insider-outsider gap would increase job creation and reduce unemployment. Spain's reform appears to have reduced the gap in official statistics without changing the economic fundamentals that generate it.

The comparison to Italy's 2015 Jobs Act is also illuminating. Italy's reform reduced dismissal costs for permanent contracts rather than banning temporary ones, a strategy that Kugler and Pica (2008) found increased worker flows. The Spanish approach—ban the flexible contract while preserving high dismissal costs for permanent contracts—is the mirror image, and our results suggest it may be less effective precisely because it does not address the price of permanence.

What can policymakers learn? The Spanish experience suggests that banning contract types is insufficient to reform labor market dualism when the underlying incentive structure—high dismissal costs for permanent workers relative to the effective costs of seasonal or project-based employment—remains unchanged. A more effective approach might combine contract unification (eliminating the dual structure entirely) with a reduction in dismissal costs for all workers, as proposed by Blanchard (2006) and implemented partially in Italy's 2015 Jobs Act. The key insight is that relabeling is not a bug in the Spanish reform; it is the predicted equilibrium response to a regulation that changes the menu of contract types without changing the price of labor flexibility.

The broader principle is worth stating explicitly: in dual labor markets, the binding constraint is not the existence of temporary contracts but the gap in employment protection between contract types. Eliminate the gap by making all contracts more flexible (the Italian path) or by making all contracts more protective (the theoretical ideal), and the incentive to segment workers disappears. Eliminate one contract type while preserving the gap (the Spanish path), and employers will find—or the law will provide—a substitute. The *fijo discontinuo* is that substitute. Spain's reform is best understood not as a failure of implementation but as a natural experiment in the limits of regulatory bans when underlying economic incentives remain unchanged.

8. Conclusion

Spain's 2022 labor reform produced the most dramatic change in employment statistics in the country's history. Within two years, the temporary employment rate fell by nine percentage points—more than all six previous reform attempts combined. But behind the statistics, the structure of work appears largely unchanged: total employment did not move, and the sectors most reliant on seasonal labor experienced the largest compositional shifts—exactly the pattern predicted by relabeling rather than genuine improvement.

Our results carry three implications for labor market policy. First, the contract label is a poor measure of job quality. Policymakers who target the temporary employment rate as a key performance indicator—as the European Commission does in its European Pillar of Social Rights—risk rewarding reforms that change classification schemes rather than improve workers' lives. Better metrics would include job tenure distributions, involuntary part-time rates, or the gap between desired and actual work hours, none of which are mechanically affected by contract reclassification.

Second, the Spanish experience illustrates a general principle about regulatory bans: prohibiting a practice without changing the underlying incentive to engage in it produces substitution, not elimination. While the analogy is imperfect, this pattern has parallels in other regulatory domains—financial regulation (where banning one instrument may push activity into adjacent unregulated instruments) and environmental policy (where single-source bans can shift production to unregulated jurisdictions). In each case, the policy lever must target the price of the underlying behavior, not its institutional form.

Third, and more optimistically, our results do not rule out the possibility that relabeling is a necessary first step toward genuine reform. If *fijo discontinuo* workers gradually accumulate seniority rights, access training programs, and develop longer-term relationships with employers, the relabeling may become self-fulfilling: the legal permanence of the contract could eventually produce the economic permanence it currently only simulates. Testing this hypothesis requires matched employer-employee data on wages, job duration, and training over a longer post-reform window—a natural next step for this research agenda.

The deeper lesson extends beyond Spain. When policymakers attack labor market dualism by prohibiting one contract type while creating a substitute, they should expect the statistical measure of dualism to improve while the lived experience of precarity persists. Changing labels is easy; changing the incentives that produce precarious employment is hard. Until employment protection is unified—eliminating the gap between insiders and outsiders rather than renaming the boundary—the illusion of permanence will remain just that.

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

Contributors: @ai1scl

First Contributor: https://github.com/ai1scl

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A. Data Appendix

A.1 Data Sources

Our primary data source is the Spanish Labor Force Survey (*Encuesta de Población Activa*, EPA), collected quarterly by Spain's National Statistics Institute (INE). The EPA is a rotating panel survey of approximately 65,000 households, designed to be representative at the national and Autonomous Community level.

We access three INE tables through the public API:

1. **Table 65328:** Wage earners by contract type (permanent, temporary), sex, and Autonomous Community. Quarterly, 2010Q4–2025Q3. Retrieved March 2026.
2. **Table 65133:** Employed persons by contract type, sex, and economic sector (Agriculture, Industry, Construction, Services). Quarterly, same period.
3. **Table 4076:** Activity and employment rates by Autonomous Community. Quarterly, same period.

All data are publicly available without authentication at the INE API.[2]

A.2 Sample Construction

From Table 65328, we select "Both genders" series measured in "Persons" (thousands). We exclude the "National Total" aggregate, retaining 19 Autonomous Community-level units. This produces 360 unique series (19 regions $\times$ 3 contract types $\times$ 2 genders $\times$ ...), from which we extract the "Both genders" subset.

The unit of observation is a region-quarter. We construct temporary share and permanent share as the ratio of the respective contract type to total wage earners. Since the EPA classifies all wage earners as either permanent or temporary, these shares sum to unity by construction ($\text{TempShare}_{rt} + \text{PermShare}_{rt} = 1$). This accounting identity is important for interpretation: any regression result for permanent share is mechanically determined by the corresponding result for temporary share.

A.3 Treatment Variable

Treatment intensity Z_r is the 2021 annual average of the quarterly temporary employment share for region r. We use the 2021 average rather than a single quarter to reduce measurement error from seasonal fluctuation. Results are robust to using 2021Q4 alone.

[2]Endpoint: https://servicios.ine.es/wstempus/js/EN/DATOS_TABLA/\{TABLE_ID\}

A.4 Limitations

The EPA reports contract types based on survey respondents' self-classification. Workers on *fijo discontinuo* contracts may report themselves as either "permanent" or "temporary" depending on their understanding of the new classification. If some *fijo discontinuo* workers continued to report as temporary, our estimates would understate the degree of relabeling. Official guidance from INE classifies *fijo discontinuo* as permanent, and our data reflect INE's coding.

B. Identification Appendix

B.1 Pre-Trend Analysis

Figure 1 in the main text shows the full event study for temporary employment share. We conduct a formal test of pre-trend significance by computing an F-statistic for the joint significance of all 45 pre-reform interaction coefficients (event times -46 through -2, with -1 as reference). The F-statistic is 0.38 with a p-value of 0.999, providing strong evidence that pre-reform trends in temporary employment did not vary systematically with treatment intensity.

B.2 Shift-Share Validity

Following Goldsmith-Pinkham et al. (2020), identification in our shift-share design requires that pre-reform temporary shares (the "shares") are exogenous to post-reform labor market shocks. This assumption is plausible because: (a) the cross-regional distribution of temporary employment reflects decades of accumulated industrial structure and historical path dependence, not anticipation of the 2022 reform; (b) the reform was national, negotiated at the federal level between social partners, with no region-specific provisions; and (c) 46 pre-reform quarters (2010Q4–2022Q1) show no differential trends in the event study.

C. Robustness Appendix

Table 4: Robustness: Alternative Specifications

Specification	Coefficient	SE	95% CI	N
Baseline	-0.2200	(0.2049)	[-0.6217, 0.1817]	1,140
Early treatment (2022Q1)	-0.1923	(0.2178)	[-0.6192, 0.2346]	1,140
Late treatment (2022Q3)	-0.2340	(0.2058)	[-0.6373, 0.1693]	1,140
Short pre-period (2016+)	-0.2153	(0.1653)	[-0.5393, 0.1086]	741
Population-weighted	-0.4622	(0.0460)	[-0.5523, -0.3721]	1,140
Region-specific trends	-0.2495	(0.1167)	[-0.4782, -0.0209]	1,140
Exclude COVID (2020-21)	-0.2082	(0.2345)	[-0.6678, 0.2514]	988

Notes: Dependent variable is temporary employment share. All specifications include region and quarter fixed effects with standard errors clustered at the region level. The population-weighted specification uses the 2021 average total wage earners as weights.

Table 4 reports the coefficient on treatment intensity under five specifications. The baseline uses 2022Q2 as the treatment date; the "Early" specification uses 2022Q1 (when the reform was announced and firms may have begun adjusting); the "Late" specification uses 2022Q3 (allowing one quarter of transition). The shorter pre-period (2016+) excludes the 2010–2015 period of post-crisis recovery, which could generate non-parallel dynamics. All specifications produce negative coefficients of similar magnitude, confirming the reform reduced temporary employment differentially in more-exposed regions.

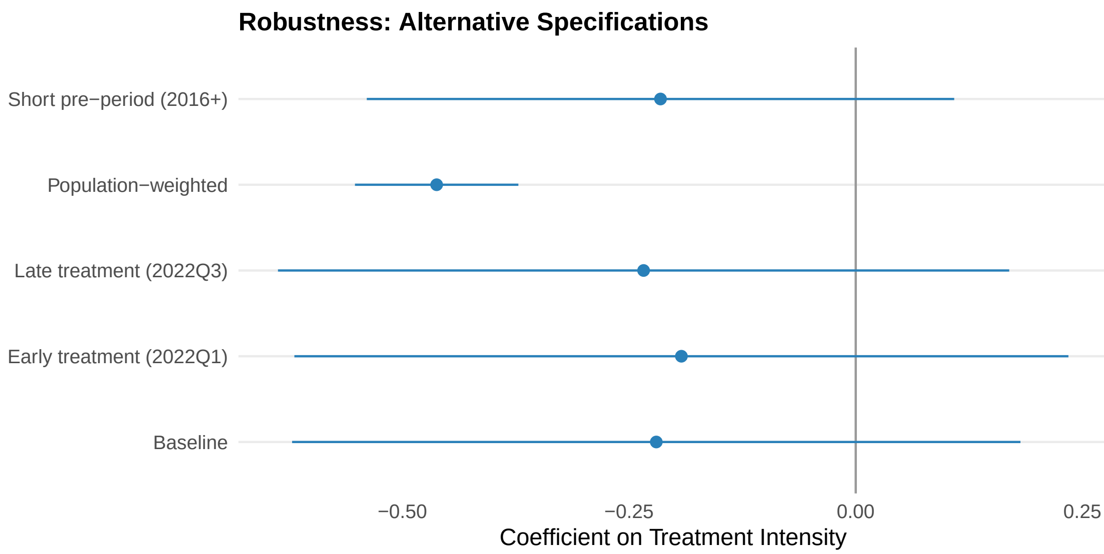

Figure 7: Coefficient Estimates Across Alternative Specifications

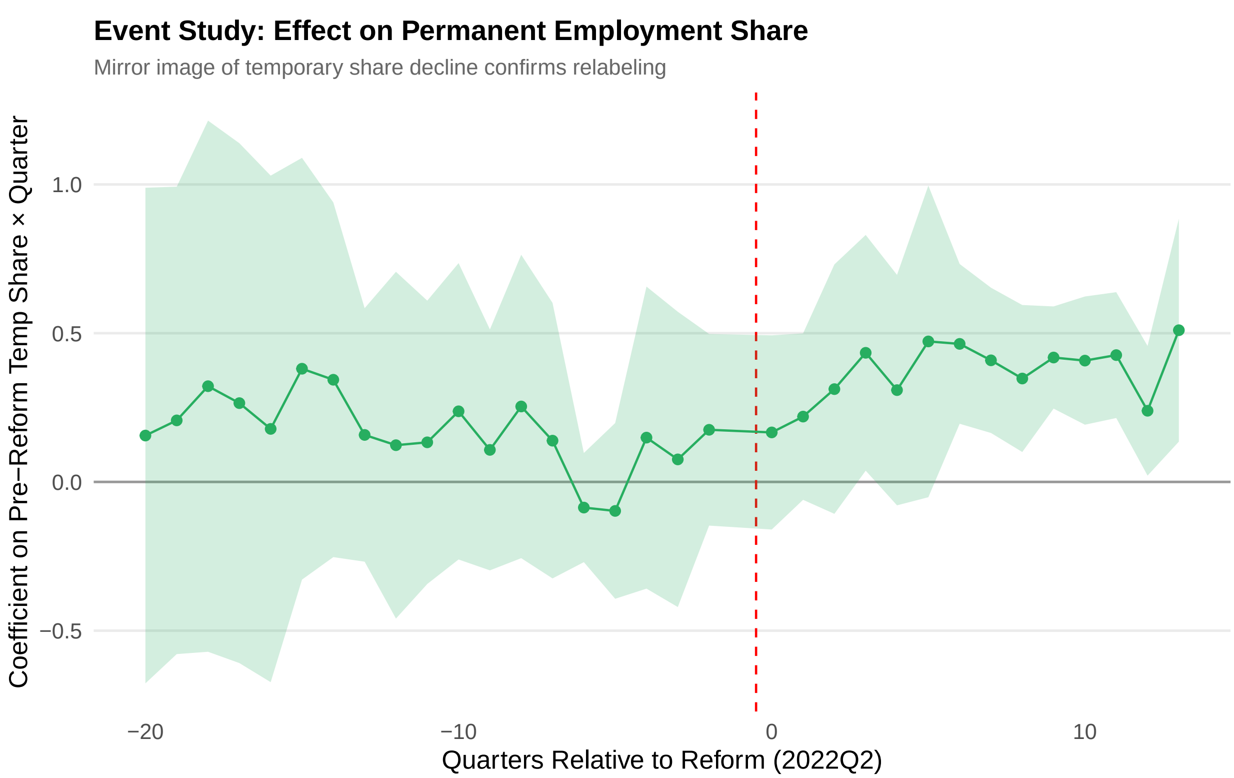

Figure 8: Event Study: Effect on Permanent Employment Share

Notes: Same specification as Figure 1 but with permanent employment share as the outcome. Because temporary and permanent shares sum to unity by construction, each coefficient is mechanically the negative of the corresponding coefficient in Figure 1. This figure carries no additional information.

D. Heterogeneity Appendix

D.1 Treatment Intensity Distribution

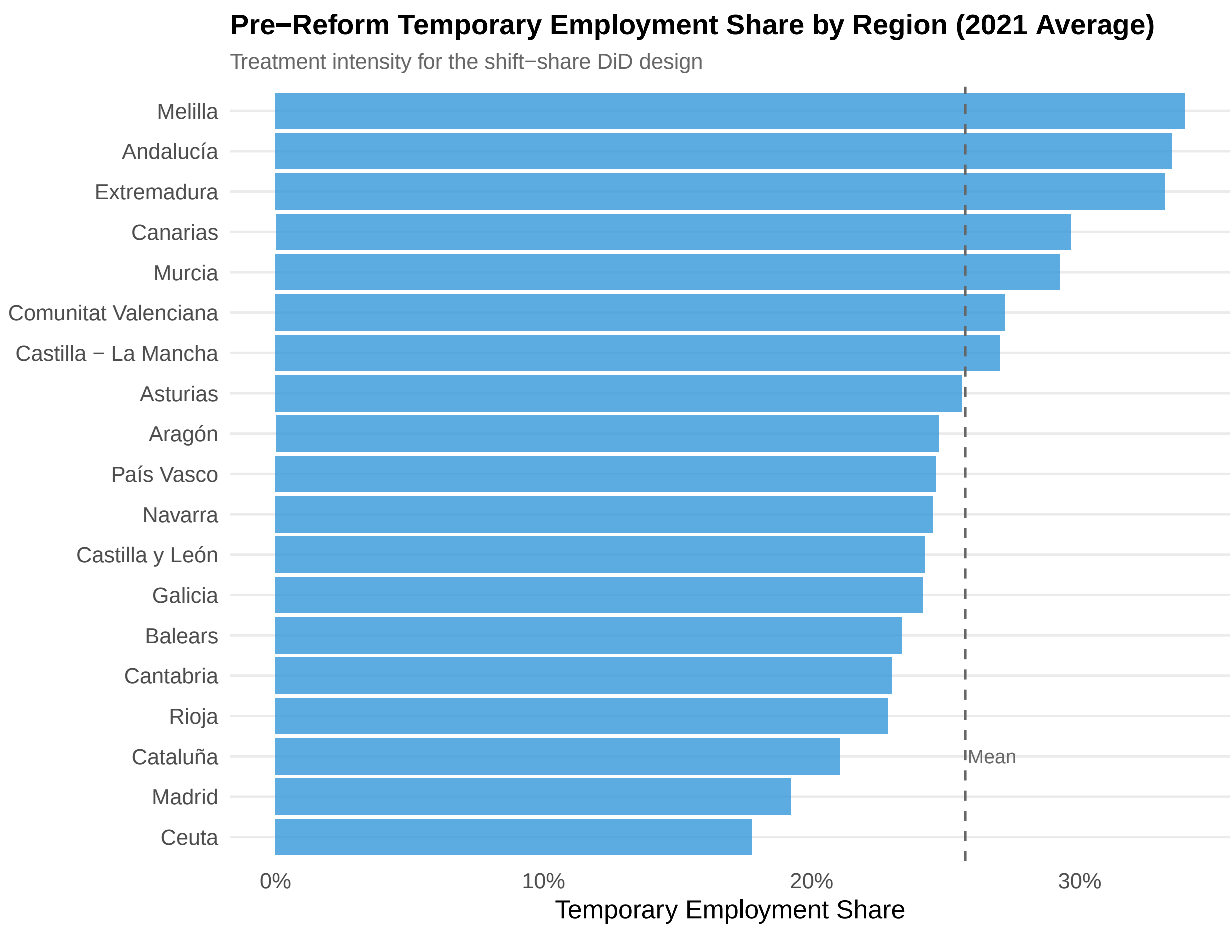

Figure 9: Pre-Reform Temporary Employment Share by Autonomous Community

Figure 9 shows the cross-regional distribution of treatment intensity. Melilla (33.9%), Andalucía (33.4%), and Extremadura (33.2%) form the high-exposure group; Ceuta (17.8%), Madrid (19.2%), and Cataluña (21.1%) form the low-exposure group. The mean is 25.7% with a standard deviation of 4.4 percentage points.

E. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Temp. Share (Unwt.)	Table 2, Col. 1	-0.2200	0.0442	0.0594	-0.1637	0.1525	Large negative
Log Employment	Table 2, Col. 2	0.1948	0.0442	1.3291	0.0065	0.0099	Small positive
Temp. Share (Wt.)	Table 2, Col. 3	-0.4622	0.0442	0.0594	-0.3440	0.0342	Large negative

Notes: This table reports standardized effect sizes (SDE) for the main outcomes of the paper. The paper evaluates Spain's 2022 labor reform (RDL 32/2021), which banned most temporary contracts. Data: INE EPA quarterly Labor Force Survey, 2010Q4–2025Q3, at the Autonomous Community level (19 regions × 60 quarters = 1,140 observations). Estimation: OLS with region and quarter fixed effects; treatment is continuous (pre-reform regional temporary employment share × post-reform indicator). SDE = $\hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$, where SD($X$) is the standard deviation of pre-reform temporary employment share across regions. SE(SDE) = SE($\hat{\beta}$) × SD(X) / SD(Y). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance.