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Between-Firm and Within-Firm Economic Conditions and Workplace-Accident Incidence in Spain, 2006–2009: A Longitudinal Panel Study

Agustín Sánchez-Toledo Ledesma [1,*], Natalia Fernández Laviada [2], Inmaculada Martín Alix [3], Juan José Agún González [4] and Raúl Aguilar Elena [4]

[1] School of Engineering and Technology (ESIT), Universidad Internacional de La Rioja (UNIR), 26006 Logroño, Spain
[2] Doctoral School, Doctoral Programme in Advanced Computing, Energy and Plasmas, University of Córdoba, 14071 Córdoba, Spain
[3] Regional Government of Andalusia (Junta de Andalucía), 41013 Seville, Spain
[4] Faculty of Social and Legal Sciences, Universidad Internacional de Valencia (VIU), 46002 Valencia, Spain
* Correspondence: agustin.sancheztoledo@unir.net

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Abstract: Workplace-accident statistics can change during economic downturns because firms alter employment, activity, work organization, and reporting, yet firm-level evidence often conflates differences between firms with changes occurring within the same firm. This longitudinal study examined associations of net sales and total assets with recorded workplace-accident incidence in 150 Spanish firms observed annually from 2006 to 2009. The balanced firm-year structure was analyzed using firm and calendar-year fixed effects with standard errors clustered by firm. Adjusted within-between decompositions distinguished within-firm changes from between-firm differences. The archived sample included firms that obtained OHSAS 18001 certification at different times and a non-certified comparison group; certification was therefore treated as a time-varying contextual characteristic rather than a uniform sample attribute. Higher average sales and assets characterized firms with lower average accident incidence, but changes in either economic measure within a firm were not associated with corresponding changes in incidence after accounting for workforce and common calendar-year conditions. Recorded incidence was lower in 2009 than in 2006, although this calendar-year contrast cannot identify a causal recession effect. Results were consistent across alternative outcome definitions, complete-sample restrictions, sector-specific time trends, outlier checks, first-difference models, and alternative outcome transformations. The findings show why between-firm and within-firm evidence should be separated when interpreting occupational-injury patterns during macroeconomic disruption. The longitudinal estimates therefore do not support the hypothesis that improvements in firm economic conditions were associated with reductions in accident incidence within firms.

Keywords: occupational accidents; economic cycle; panel data; firm fixed effects; OHSAS 18001; Spain

1. Introduction

Occupational health and safety management systems provide a structured approach to identifying hazards, allocating responsibilities, evaluating performance, and pursuing continual improvement [1,2]. Certification can confirm that an organization has implemented specified management processes, but it does not by itself demonstrate superior injury outcomes. Empirical findings remain mixed: some studies have reported improved safety performance after certification [3–6], whereas others have found no additional reduction after certification or have emphasized selection into certification and variation in implementation [7–9]. A recent systematic review with a systemic analysis likewise found substantial variation in how occupational health and safety management-system performance is defined and evaluated [10]. This distinction is particularly important when recorded accident rates change at the same time as the economic and organizational context in which a management system operates.

Economic downturns can affect occupational-injury indicators through several pathways. Lower production, fewer hours worked, changes in sectoral composition, workforce selection, and altered work intensity can reduce exposure to hazards. Conversely, restructuring, job insecurity, deferred maintenance, reduced preventive investment, and pressure on remaining workers can increase risk. Reporting may also change if workers fear dismissal or if firms modify administrative practices. Evidence from Spain and other countries therefore shows that injury indicators are sensitive to the business cycle, but the direction and mechanism are not uniform across settings, industries, or outcomes [11–17]. Recent studies from China, the United States, and Peru, together with a systematic review, continue to report heterogeneous associations and emphasize the importance of exposure definitions, sector, and organizational context [18–21]. Conceptual work has similarly warned that maintaining occupational safety and health during financial crises depends on interacting organizational and institutional conditions rather than on a single economic mechanism [17].

Spain provides a relevant setting because the 2006–2009 period spans the transition from expansion to severe contraction. National studies documented substantial decreases in recorded occupational injuries during the crisis, particularly in construction and industry [11], and a longer Spanish time series showed a strong relationship between the economic cycle and injury incidence [12]. Such aggregate evidence, however, cannot establish that a firm whose sales or assets declined experienced a corresponding change in its own accident incidence. National and sectoral patterns may reflect changes in which firms and workers remain active, in hours and tasks, or in reporting behavior.

Firm-level financial conditions and occupational injuries may also be bidirectionally related. Stronger economic performance may permit investment in preventive capacity, stable employment, maintenance, and managerial attention. At the same time, accidents can disrupt operations and impair later financial performance [22–24]. Recent longitudinal research in Spanish firms further shows that accident incidence, safety culture, safety performance, and financial outcomes may be jointly related rather than linked by a simple one-way pathway [25,26]. Studies that compare firms at one point in time, or pool repeated observations as if they were independent, cannot separate stable differences between firms from changes within each firm. Larger and more capitalized organizations may differ persistently in sector, technology, safety culture, reporting, workforce composition, or management quality. An inverse pooled association between financial scale and accident incidence can consequently arise even when an increase or decrease in sales within a firm has no corresponding association with that firm's incidence.

Certification adds a further layer to this distinction. OHSAS 18001 was a voluntary certifiable occupational health and safety management standard and a predecessor of ISO 45001. Management-system approaches emphasize policy, worker participation, planning, implementation, evaluation, and improvement [27,28]. In Spain, as elsewhere, third-party certification indicated assessed conformity with the standard's requirements; it was not a regulatory declaration that a firm had achieved a particular accident rate. Research in Spanish small and medium-sized enterprises, certified firms in other national settings, and recent comparative studies has found that substantive safety effort and measured outcomes vary among adopters [3,5–7,9,29,30]. Certified-system effectiveness depends on implementation, organizational attention, learning, audit design, sector, and context [2,5,6,10,31]. It follows that certification status should neither be assumed to homogenize firms nor be used as an explanation for changes that were not directly estimated.

The present study analyzes a four-year firm panel assembled from Spanish occupational-accident, financial, sector, and certification records. Its primary objective was to determine whether net sales and total assets were associated with workplace-accident incidence within the same firm over 2006–2009 after controlling for average workforce, stable firm characteristics, and calendar-year shocks. A second objective was to distinguish these within-firm associations from between-firm differences. A third objective was to describe calendar-year contrasts without interpreting them as causal effects of the Great Recession. This design addresses the methodological need to distinguish between-firm from within-firm evidence and provides a cautious account of what a short firm panel can show.

2. Materials and Methods

2.1. Study Design and Reporting Framework

This was a retrospective longitudinal study of annual organization-level records. The analytical unit was the firm-year. The archived panel contained 150 firms observed in each of four calendar years (2006–2009), giving 600 firm-year observations. Firm identifiers were stable across years. All 150 firms contributed four outcome observations, and no duplicate firm-year keys were present. Reporting was guided by the STROBE recommendations for observational studies [32].

The study was designed to estimate associations, not causal effects. Calendar-year indicators captured conditions common to firms in a given year but were not treated as measures of recession intensity. Likewise, net sales and total assets were treated as firm economic indicators rather than direct measures of the macroeconomic recession.

2.2. Data Sources, Linkage, and Analytical Sample

Three sources were linked by a firm identifier during the doctoral research from which the archived panel originated. First, certification-year information came from records supplied through collaboration with a certification body. Second, annual occupational-accident records and average workforce data came from a Spanish mutual insurance organization. Third, financial statements and the principal economic activity code came from the Sistema de Análisis de Balances Ibéricos (SABI), supplied by Bureau van Dijk.

The thesis documentation describes an initial selection process in which valid OHSAS 18001 certificates were matched to firms insured by the collaborating mutual organization and a non-certified comparison group was added. That documentation reports 126 selected certified firms and 29 non-certified firms at an intermediate stage. The archived final panel available for the present analysis contains 150 firms: 125 had a recorded certification year by 2009 and 25 had no certification year. A systematic cross-check of the retained combined coding frame (1–155) identified five gaps: one corresponded to the 126-firm certified list and four to the 29-firm comparison list. The five corresponding candidates were absent from the final linked accident and SABI records. The archive contains no exclusion flag or contemporaneous note identifying why they were not retained; the discrepancy can therefore be documented but not causally explained. No claim is made that the 150 firms constitute a representative sample of Spanish firms or of all OHSAS-certified firms. The analysis uses the archived 150-firm panel and explicitly acknowledges this provenance gap and the potential for linkage and survivor selection.

Certification was not uniform during follow-up. According to the archived exact-year indicator, 28 firms were certified in 2006, 59 in 2007, 118 in 2008, and 125 in 2009. The remaining firm-years preceded certification or belonged to the comparison group. Certification was consequently treated as a time-varying contextual characteristic and sensitivity covariate, not as the defining exposure or as proof that all firms were certified throughout the study.

2.3. Outcome Measures

The primary outcome was the official annual workplace-accident incidence percentage recorded in the mutual insurer's reports. The source reports define it as accidents with sick leave occurring during working time, excluding relapses, divided by the average number of workers in the calculation period. When the insured period covered fewer than 12 months, the accumulated accident count was extrapolated to December before division. The measure is therefore expressed as recorded accidents per 100 average workers. It does not include a denominator based on actual hours worked.

One source cell used an apostrophe rather than a decimal point (17'78); it was transparently parsed as 17.78. No other primary outcome value was changed. The primary outcome was complete for all 600 firm-years.

The alternative outcome was derived from the archived total number of recorded occupational accidents (accidents_attotal) divided by the average annual workforce (employees_yavg) and multiplied by 100. It is referred to as the all-recorded-accident rate per 100 workers. This derived measure is broader than the official incidence measure and does not reproduce the latter's restriction to accidents with sick leave during working time, exclusion of relapses, or annualization rule. The two outcomes are consequently related but not interchangeable; their Pearson correlation was 0.975. The alternative outcome was used to assess sensitivity to the outcome definition, not as an independent replication.

2.4. Economic, Workforce, Sector, and Certification Variables

Firm economic conditions were represented by annual net sales and total assets, each recorded in thousands of euros. These indicators capture different dimensions of firm activity and financial scale. Because their distributions were strongly right-skewed, natural logarithms were used. One zero value for net sales could not be logged and was treated as unavailable for models using sales. Total assets were strictly positive whenever reported.

The average annual number of workers was used to account for changes in firm workforce and was also log-transformed. It is not a substitute for actual hours worked, overtime, production volume, work intensity, shutdowns, or the composition of tasks. Those exposure variables were not present in the archived spreadsheets or in the annual accident reports available in the study files and could not be reconstructed.

Principal economic activity was available as a NACE Rev. 2 code. Firms were grouped into agriculture (section A), industry (sections B–E), construction (section F), and services (sections G–U), following the European activity classification [33]. The resulting sample comprised one agricultural firm, 47 industrial firms, 64 construction firms, and 38 service firms. Four firms not classified by legacy sector dummy fields were recoverable from their valid primary NACE codes.

Exact-year certification status was taken from the archived safety_cert2 field and certification year. It was used descriptively and as a sensitivity covariate. The present study did not attempt to estimate the causal effect of OHSAS 18001 certification.

2.5. Missing Data and Data Checks

The firm-year panel and both accident outcomes were complete. Net sales and total assets were missing for 5, 9, 18, and 21 firms in 2006, 2007, 2008, and 2009, respectively, for a total of 53 missing values for each measure. The logged sales variable had one additional missing value because of the zero value. No values were imputed. Each model used all firm-years with complete data for its variables, and sample sizes are reported for every model.

The increasing financial missingness could not be assumed to be random. Firm-years missing sales or assets had a higher mean official incidence than those with available financial data and a lower median workforce. A sensitivity analysis therefore restricted the sample to 122 firms with complete sales, assets, and workforce information in all four years. No observation was excluded on the basis of its accident rate. Influence was assessed by repeating the principal models after omitting each firm in turn and by winsorizing the primary outcome at its 99th percentile.

2.6. Statistical Analysis

Descriptive statistics were calculated for the full panel and by calendar year. Accident rates are presented as means and standard deviations; workforce and financial measures are presented as medians and interquartile ranges because of their skewness. Pearson correlations were used only to describe pooled associations and multicollinearity; they were not treated as longitudinal estimates.

The principal models were linear firm fixed-effects regressions with calendar-year fixed effects and standard errors clustered by firm. Firm fixed effects remove all characteristics that do not change during follow-up, including stable sector membership, baseline technology, and other time-invariant differences. Calendar-year effects account for common annual shifts, with 2006 as the reference. Net sales and total assets were analyzed in separate models because their pooled correlation was 0.922 and their joint inclusion with workforce produced variance inflation factors of 8.72 and 7.17. In the separate principal models, variance inflation factors for the economic indicator and workforce were 2.44 for sales and 2.02 for assets. Each model included log average workforce.

To separate within-firm and between-firm information directly, adjusted within-between decompositions were estimated using firm means and annual deviations from those means. For each economic indicator and workforce, the model included the firm's four-year mean (between-firm component) and the annual deviation from that mean (within-firm component), plus calendar-year indicators. These models retain the between-firm contrast while allowing the within-firm coefficient to be compared with the fixed-effects estimate. Standard errors were clustered by firm.

Sensitivity analyses used the all-recorded-accident rate; the 125 firms with a certification date by 2009; exact-year certification status; sector-by-year interactions to allow sector-specific calendar trends; the 122-firm balanced complete economic sample; the winsorized official outcome; log(1 + official incidence); the inverse hyperbolic sine (IHS) of official incidence; first differences adjusted for annual workforce change; and economic-indicator-by-year interactions. Poisson firm fixed-effects models of all recorded accident counts with log workforce as an offset were examined as additional outcome-scale checks. Full coefficients, confidence intervals, *p*-values, analytical sample sizes, all-zero exclusions, and dispersion statistics are reported in the Supplementary Material (Supplementary Tables S1–S3). Because the count models use a broader accident count and a different estimand, they are reported as supplementary evidence rather than as the principal analysis.

The official incidence outcome was right-skewed (skewness 1.53) and contained 83 zero values. The principal linear fixed-effects model was retained because the estimand is an absolute within-firm difference in recorded accidents per 100 workers and clustered inference does not require normally distributed outcomes. Residual distributions, log(1 + incidence), the IHS transformation, outlier sensitivity, and count models were nevertheless examined. The IHS transformation is defined at zero without adding an arbitrary constant. Effect estimates are reported with 95% confidence intervals and two-sided p-values. No automated stepwise selection was used. Given the related specifications, interpretation emphasizes the direction, magnitude, and consistency of confidence intervals rather than isolated significance thresholds.

Analyses were performed using Python 3.12.13, pandas 2.2.3, NumPy 2.3.5, and openpyxl 3.1.5. The within estimator and CR1 firm-clustered covariance were implemented in a reproducible script, with t inference based on the number of firms minus one. Coefficients and clustered standard errors were independently verified against an explicit least-squares firm-dummy formulation; absolute numerical differences were below 1.1×10^{-11}. The statistical scripts and a result-to-source traceability table were retained as audit materials and can be supplied to the Editor on request.

3. Results

3.1. Sample Characteristics and Annual Patterns

All 150 firms contributed an official accident-incidence value and an average workforce value in each year. The official incidence mean was 8.81 accidents per 100 workers in 2006, increased to 9.29 in 2007, and declined to 7.71 in 2008 and 6.49 in 2009 (Table 1; Figure 1). The all-recorded-accident rate showed a similar pattern. Financial medians increased through 2008 and declined in 2009 for net sales, while median total assets increased across the period. Financial availability decreased over time.

The official outcome had a mean of 8.08, median of 6.35, and standard deviation of 7.86 across all firm-years. The corresponding all-recorded-accident values were 8.29, 6.42, and 8.00. The two outcomes' high correlation confirms that they contain largely overlapping information. Certification prevalence increased markedly over time, from 18.7% of firms in 2006 to 83.3% in 2009, which confirms that the sample cannot be described as uniformly certified throughout follow-up.

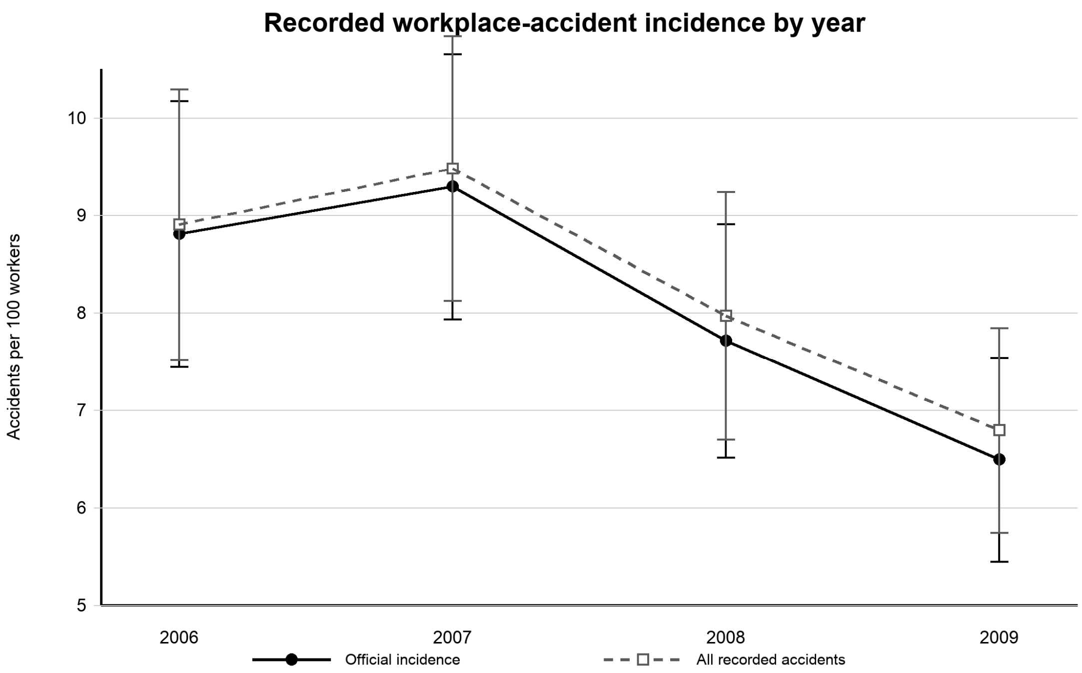

Figure 1. Recorded workplace-accident incidence by calendar year. Note. Points are annual means; error bars are descriptive 95% confidence intervals across firms. The official measure covers accidents with sick leave during working time, excludes relapses, and uses the source report's annualization rule. The alternative rate uses all recorded accidents divided by average workforce. These intervals do not represent the longitudinal fixed-effects analysis.

Table 1. Sample characteristics by calendar year, 2006–2009.

Characteristic	2006	2007	2008	2009	Overall
Official workplace-accident incidence, per 100 workers	8.81 (8.53)	9.29 (8.49)	7.71 (7.47)	6.49 (6.54)	8.08 (7.86)
All recorded accidents, per 100 workers	8.90 (8.67)	9.48 (8.48)	7.97 (7.93)	6.79 (6.56)	8.29 (8.00)
Average annual workforce, median (IQR)	91.8 (32.6–205.8)	93.7 (36.7–248.5)	87.4 (34.2–247.7)	88.0 (31.2–226.8)	91.0 (33.0–232.8)
Net sales, € thousands, median (IQR)	19,803.9 (6435.2–62,576.3)	22,660.2 (7416.6–92,068.0)	23,609.5 (8442.7–99,334.9)	20,890.2 (6469.7–89,024.1)	20,890.2 (6959.2–89,897.4)
Total assets, € thousands, median (IQR)	20,458.6 (4956.9–84,857.2)	20,888.7 (6192.6–104,193.8)	24,959.1 (6422.1–117,811.5)	26,331.1 (6235.4–110,501.5)	22,226.7 (6010.7–104,789.4)
OHSAS 18001 certified in that year, n (%)	28 (18.7)	59 (39.3)	118 (78.7)	125 (83.3)	330 firm-years (55.0)

Note. Accident values are mean (standard deviation); workforce and monetary values are median (interquartile range). Monetary variables are expressed in thousands of euros. The official incidence and all-recorded-accident outcomes are expressed per 100 average workers. Financial sample sizes are 145, 141, 132, and 129 in 2006–2009. IQR, interquartile range; OHSAS, Occupational Health and Safety Assessment Series.

3.2. Principal Firm Fixed-Effects Models

Within firms, net sales were not associated with official accident incidence after adjustment for log workforce and calendar year (coefficient −0.009; 95% CI −2.558 to 2.539; p = 0.994; 546 observations from 147 firms) (Table 2). The confidence interval excluded neither a meaningful negative nor a meaningful positive association. In the same model, the 2009 calendar-year coefficient was −2.478 accidents per 100 workers relative to 2006 (95% CI −4.035 to −0.921; p = 0.002). The 2007 contrast was close to zero, and the 2008 interval included zero. The within R^2 was 0.087.

The total-assets model produced the same substantive conclusion. The within-firm asset coefficient was −0.809 (95% CI −2.934 to 1.316; p = 0.453; 547 observations from 147 firms). The 2009 coefficient was −2.318 (95% CI −3.975 to −0.660; p = 0.006). Log workforce had a positive coefficient in both models, with its confidence interval excluding zero in the asset model but not in the sales model. The workforce coefficient should not be interpreted as an exposure-adjusted causal effect because hours worked, workforce composition, tasks, and production intensity were unavailable.

Table 2. Firm and calendar-year fixed-effects models for official workplace-accident incidence.

Model	Term	*b* (SE)	95% CI	*p*	*n*; Firms	Within R^2
Net sales model	Economic indicator (log)	−0.009 (1.289)	−2.558 to 2.539	0.994	546; 147	0.087
Net sales model	Average workforce (log)	2.887 (1.542)	−0.160 to 5.934	0.063	546; 147	0.087
Net sales model	2007 vs. 2006	0.077 (0.672)	−1.251 to 1.404	0.909	546; 147	0.087
Net sales model	2008 vs. 2006	−1.451 (0.807)	−3.046 to 0.145	0.074	546; 147	0.087
Net sales model	2009 vs. 2006	−2.478 (0.788)	−4.035 to −0.921	0.002	546; 147	0.087
Total assets model	Economic indicator (log)	−0.809 (1.075)	−2.934 to 1.316	0.453	547; 147	0.088
Total assets model	Average workforce (log)	3.073 (1.305)	0.493 to 5.652	0.020	547; 147	0.088
Total assets model	2007 vs. 2006	0.188 (0.674)	−1.143 to 1.520	0.780	547; 147	0.088
Total assets model	2008 vs. 2006	−1.309 (0.797)	−2.885 to 0.266	0.103	547; 147	0.088
Total assets model	2009 vs. 2006	−2.318 (0.839)	−3.975 to −0.660	0.006	547; 147	0.088

Note. Coefficients are absolute changes in recorded accidents per 100 average workers. Both models include firm fixed effects; standard errors are clustered by firm. The reference year is 2006. Economic variables and average workforce are natural-log transformed. Sales and assets are entered in separate models. CI, confidence interval.

3.3. Within-Firm Versus Between-Firm Associations

The adjusted decompositions revealed different within-firm and between-firm patterns (Table 3; Figure 2). For net sales, the within-firm coefficient was 0.014 (95% CI −2.173 to 2.201; p = 0.990), whereas the between-firm coefficient was −1.033 (95% CI −2.040 to −0.026; p = 0.045). For total assets, the within-firm coefficient was −0.654 (95% CI −2.555 to 1.246; p = 0.497), whereas the between-firm coefficient was −0.946 (95% CI −1.781 to −0.111; p = 0.027).

Thus, firms with higher average sales or assets across the period also had lower average recorded incidence, conditional on the model covariates. However, a firm's annual movement above or below its own average sales or assets was not associated with a corresponding incidence change. This contrast indicates that a pooled inverse relationship can be driven principally by differences between firms and should not be interpreted as evidence that improved economic conditions within a firm reduced its accident rate.

Table 3. Adjusted within-firm and between-firm decomposition of economic conditions and official incidence.

Model	Term	*b* (SE)	95% CI	*p*	*n*; Firms	Overall R^2
Net sales model	Within-firm economic component	0.014 (1.107)	−2.173 to 2.201	0.990	546; 147	0.070
Net sales model	Between-firm economic component	−1.033 (0.510)	−2.040 to −0.026	0.045	546; 147	0.070
Net sales model	Within-firm workforce component	2.913 (1.312)	0.319 to 5.507	0.028	546; 147	0.070
Net sales model	Between-firm workforce component	0.429 (0.577)	−0.711 to 1.568	0.458	546; 147	0.070
Net sales model	2007 vs. 2006	0.023 (0.570)	−1.103 to 1.150	0.967	546; 147	0.070
Net sales model	2008 vs. 2006	−1.771 (0.693)	−3.140 to −0.402	0.012	546; 147	0.070
Net sales model	2009 vs. 2006	−2.718 (0.668)	−4.038 to −1.398	<0.001	546; 147	0.070
Total assets model	Within-firm economic component	−0.654 (0.962)	−2.555 to 1.246	0.497	547; 147	0.071
Total assets model	Between-firm economic component	−0.946 (0.422)	−1.781 to −0.111	0.027	547; 147	0.071
Total assets model	Within-firm workforce component	3.042 (1.123)	0.822 to 5.261	0.008	547; 147	0.071
Total assets model	Between-firm workforce component	0.341 (0.500)	−0.648 to 1.330	0.496	547; 147	0.071
Total assets model	2007 vs. 2006	0.169 (0.566)	−0.950 to 1.287	0.766	547; 147	0.071
Total assets model	2008 vs. 2006	−1.619 (0.699)	−3.002 to −0.237	0.022	547; 147	0.071
Total assets model	2009 vs. 2006	−2.550 (0.727)	−3.988 to −1.112	0.001	547; 147	0.071

Note. Each model includes within-firm and between-firm components for the stated economic indicator and average workforce, plus calendar-year indicators. Standard errors are clustered by firm. Coefficients are changes in recorded accidents per 100 average workers. CI, confidence interval.

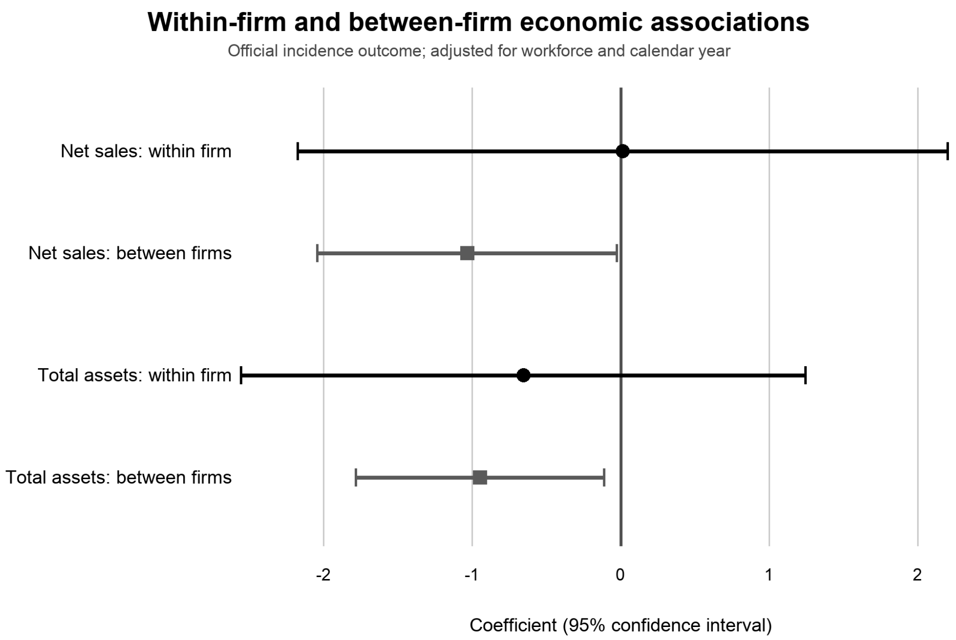

Figure 2. Within-firm and between-firm economic associations with official workplace-accident incidence. Note. Points are adjusted coefficients and bars are 95% confidence intervals. Models include the corresponding within-firm and between-firm workforce components and calendar-year indicators; standard errors are clustered by firm.

3.4. Sensitivity Analyses and Diagnostics

Across sensitivity analyses, within-firm sales changes remained unassociated with corresponding changes in accident incidence (Table 4). The sales coefficient remained near zero or had confidence intervals spanning zero when the alternative all-recorded-accident rate, eventual-certified subset, balanced complete sample, certification adjustment, sector-specific time trends, winsorization, log(1 + incidence), IHS transformation, and first differences were used. In the IHS model, the sales coefficient was −0.074 (95% CI −0.556 to 0.408; $p = 0.762$). Omitting firms one at a time changed the sales point estimate from −0.489 to 0.759 but did not reveal a stable inverse relationship.

The asset results were also consistent with the principal conclusion. Point estimates were generally negative, but all confidence intervals included zero across the listed fixed-effects and first-difference sensitivity analyses. The IHS asset coefficient was −0.305 (95% CI −0.640 to 0.031; $p = 0.075$), and therefore remained imprecise. The complete four-year financial sample produced coefficients of −0.496 for sales and −0.974 for assets, again with intervals spanning zero. Adding exact-year certification status did not alter either economic coefficient; the certification coefficient itself was also imprecise and should not be considered a causal estimate.

Sector-specific calendar trends left the economic coefficients essentially unchanged. Time-invariant sector membership was already absorbed by firm fixed effects; allowing sectors to have different annual patterns did not support an inverse within-firm sales or asset association. Economic-indicator-by-year interactions were imprecise and did not identify a distinct association in 2007, 2008, or 2009. First-difference models adjusted for workforce change also produced intervals spanning zero for changes in sales and assets.

The supplementary Poisson models of all recorded accident counts produced rate ratios above one for sales (RR 1.295; 95% CI 1.129 to 1.485; $p < 0.001$), assets (RR 1.236; 95% CI 1.070 to 1.427; $p = 0.004$), and workforce (RR 1.314; 95% CI 1.168 to 1.478; $p < 0.001$). The sales model excluded 41 observations from 11 all-zero firms from 546 eligible observations in 147 firms; the assets model excluded 42 observations from the same 11 all-zero firms from 547 eligible observations in 147 firms; and the workforce-only model excluded 40 observations from 10 all-zero firms from 600 observations in 150 firms. Full economic and calendar-year coefficients are reported in Supplementary Table S2. These models use a broader accident definition and estimate a multiplicative count-rate association. They should not replace the principal official-incidence models, but they provide additional evidence against characterizing the available data as showing a robust inverse economic association.

Table 4. Sensitivity analyses for within-firm economic associations.

Indicator	Analysis	*b*	95% CI	*p*	*n*; Firms
Net sales	Principal: official incidence	−0.009	−2.558 to 2.539	0.994	546; 147
Net sales	Alternative all-accident rate	0.671	−1.759 to 3.101	0.586	546; 147
Net sales	Eventually-certified firms	0.967	−2.006 to 3.939	0.521	450; 122
Net sales	Balanced complete economic sample	−0.496	−3.015 to 2.023	0.698	488; 122
Net sales	Adjusted for certification status	−0.018	−2.606 to 2.570	0.989	546; 147
Net sales	Sector-specific calendar trends	−0.092	−2.611 to 2.427	0.942	546; 147
Net sales	Official incidence winsorized at 99th percentile	0.018	−2.539 to 2.576	0.989	546; 147
Net sales	log(1 + official incidence)	−0.057	−0.450 to 0.337	0.777	546; 147
Net sales	IHS(official incidence)	−0.074	−0.556 to 0.408	0.762	546; 147
Net sales	First differences adjusted for workforce change	−0.710	−2.951 to 1.530	0.532	393; 143
Total assets	Principal: official incidence	−0.809	−2.934 to 1.316	0.453	547; 147
Total assets	Alternative all-accident rate	−0.492	−2.748 to 1.764	0.667	547; 147
Total assets	Eventually-certified firms	−1.023	−3.729 to 1.682	0.455	451; 122
Total assets	Balanced complete economic sample	−0.974	−3.203 to 1.256	0.389	488; 122
Total assets	Adjusted for certification status	−0.812	−2.948 to 1.325	0.454	547; 147
Total assets	Sector-specific calendar trends	−0.824	−2.984 to 1.335	0.452	547; 147
Total assets	Official incidence winsorized at 99th percentile	−1.053	−3.068 to 0.963	0.304	547; 147
Total assets	log(1 + official incidence)	−0.241	−0.516 to 0.034	0.085	547; 147
Total assets	IHS(official incidence)	−0.305	−0.640 to 0.031	0.075	547; 147
Total assets	First differences adjusted for workforce change	−0.427	−2.443 to 1.590	0.676	394; 143

Note. Unless otherwise specified, analyses are firm and calendar-year fixed-effects models adjusted for log average workforce with standard errors clustered by firm. Coefficients for log(1 + incidence) and IHS-transformed incidence are on their respective transformed scales and are not directly comparable in magnitude with the other rows. The first-difference models estimate associations between annual changes. CI, confidence interval; IHS, inverse hyperbolic sine.

4. Discussion

The central finding is that economic associations differed by level of analysis. Neither within-firm sales nor within-firm assets were associated with official recorded incidence. By contrast, firms with higher average sales or assets had lower average incidence. The distinction is substantive rather than merely technical. It prevents a cross-sectional contrast between different firms from being described as evidence that an improvement in a firm's economic condition lowers its accident rate.

The between-firm inverse association is plausible but not mechanistically identified. Firms with higher average sales or assets may differ in technology, production processes, workforce composition, reporting practices, management quality, safety culture, contracting arrangements, or their capacity to sustain preventive systems. Firm size and financial scale may also be markers for access to resources rather than direct causes. The models accounted for average workforce and calendar year, but they cannot establish which stable differences produced the between-firm pattern. The limited explanatory power reinforces this caution: within R^2 values of 0.087 and 0.088 mean that 91.3% and 91.2%, respectively, of within-firm variance remained unexplained by the economic, workforce, and calendar-year variables. This is consistent with an important role for unobserved organizational and exposure factors. Safety culture, preventive expenditure, management attention, maintenance, training, and worker participation were not measured. A systems interpretation is therefore appropriate only in a limited conceptual sense: accident outcomes arise within an interacting organizational and economic context, but this study did not test a systems model or a mediating safety-culture pathway.

The results are consistent with literature showing that certification alone is an incomplete proxy for substantive safety performance. The realist review by Madsen et al. emphasized that certified systems operate through mechanisms that depend on context [2]. Heras-Saizarbitoria et al. found no simple safety advantage for certification in a large Spanish comparison [3]. More recent longitudinal evaluations report context-dependent findings: effects vary by baseline selection, sector, firm size, audit program, and outcome definition, and some studies find no additional reduction after certification [5,6,9]. A recent systematic review with a systemic analysis likewise highlights persistent variation in performance measurement and organizational fit [10]. The present panel was particularly unsuitable for a blanket "certified firms" description: only 28 of the 150 firms were coded certified in 2006, certification increased during follow-up, and 25 had no certification year by 2009. The analysis therefore uses certification as contextual information and does not attribute the observed calendar-year pattern to OHSAS 18001.

Recorded official incidence was lower in 2009 than in 2006 after controlling for firm fixed effects, workforce, and either sales or assets. This finding accords descriptively with Spanish evidence of declining occupational-injury indicators during the economic crisis [11,12]. Recent panel and time-series studies in the United States and Peru also report heterogeneous dynamic relationships across territories and industries [19,20], while a recent systematic review concludes that the evidence does not support simple general claims about a single economic mechanism [18]. The present result must nevertheless be called a calendar-year association. A 2009 indicator combines every unmeasured condition common to that year, including changes in demand, sectoral activity, hours, employment composition, task mix, reporting, prevention, and regulation. The study has no untreated period or comparison country that would identify a recession effect, and absolute sales or assets do not measure a firm's recession exposure. The absence of significant year-by-economic interactions also does not prove that mechanisms were constant; the short four-year panel provides limited precision for such heterogeneous effects.

Several explanations for declining recorded injuries during downturns remain compatible with the data. Lower hours and reduced production can decrease hazard exposure even after dividing by average workforce. Employment contraction may retain more experienced workers or alter the distribution of high-risk tasks. Moderate accidents may be underreported when job insecurity is high, as suggested by evidence on reporting incentives during recessions [16]. Conversely, resource constraints, restructuring, and intensified work could worsen safety for some firms or workers [22]. Recent research using multidimensional economic indicators similarly indicates that economic development and occupational accidents can move through several indirect pathways rather than a single scale effect [21]. Because actual hours, work intensity, workforce composition, and reporting behavior were unavailable, the present study cannot adjudicate among these mechanisms. It is therefore inappropriate to describe the 2009 result as direct evidence of either improved safety or recession-induced harm.

The positive within-firm workforce coefficient in several specifications also requires restraint. The official outcome already divides accident counts by average workforce, yet annual changes in logged workforce may correlate with unobserved hours, hiring, production intensity, or the composition of work. The coefficient is not a clean "firm size effect," nor does it show that adding employees causes incidence to increase. Its instability across outcomes and specifications reinforces the need to treat it as adjustment information rather than as a principal finding.

The study has practical implications despite the null within-firm economic coefficients. Occupational-safety practitioners and auditors should interpret changes in annual incidence alongside denominators, activity, staffing, hours, task mix, and reporting conditions. A fall in recorded accidents during contraction should not automatically be credited to certification or a management intervention. Conversely, the absence of a within-firm association with sales or assets does not mean that resources are irrelevant to prevention; it means that the available four-year financial changes did not identify such an association. Safety oversight should remain attentive during both contraction and expansion, with particular emphasis on maintaining reporting channels and collecting exposure measures that distinguish fewer accidents from less work.

The principal strengths are the balanced outcome panel, objective firm-level linkage across accident and financial records, explicit modeling of repeated observations, separation of within-firm and between-firm components, clustered uncertainty, and a broad set of sensitivity analyses. Sample and outcome definitions were traced back to the thesis spreadsheets and underlying insurer reports. The analysis also reports the increasing financial missingness rather than silently treating 600 observations as complete in all models.

The limitations are substantial. First, hours worked, overtime, production, shutdowns, task composition, subcontracting, workforce demographics, and safety-culture measures were unavailable. Average workforce is an incomplete exposure denominator. Second, financial missingness increased over time and appeared related to observed firm-year characteristics; balanced complete-case sensitivity cannot eliminate selection bias. Third, the linked sample depends on the collaborating insurer, certification records, and availability in SABI. Firms that closed, failed, left the insurer, or could not be linked are not observed, so survivor and linkage selection cannot be removed. A systematic cross-check of the intermediate 155-firm selection and the archived 150-firm analytical panel identified one missing candidate from the certified list and four from the comparison list, but no contemporaneous exclusion flags or reasons were preserved. Fourth, the four-year window limits power to study heterogeneous slopes and longer-term dynamics. Fifth, the alternative accident rate is highly correlated with the primary outcome and is not an independent validation. Sixth, reverse association and time-varying confounding remain possible: accidents can affect economic performance, and unmeasured organizational changes can affect both. Finally, results from this selected Spanish sample should not be generalized to all firms, all OHSAS adopters, or other economic downturns.

Within these limits, the study contributes a useful methodological and empirical demonstration. It shows that average differences between firms and annual changes within firms can point to different conclusions. Studies of occupational injuries during economic disruption should state which level of association they estimate, use methods that respect repeated observations, and avoid assigning a causal recession mechanism to calendar-year contrasts.

5. Conclusions

In this 2006–2009 panel of 150 Spanish firms, firms with higher average net sales or total assets had lower average recorded workplace-accident incidence, but within-firm changes in sales and assets were not associated with corresponding changes in incidence. Official recorded incidence was lower in 2009 than in 2006, but the design cannot identify this as a causal effect of the Great Recession. The longitudinal evidence therefore does not support the hypothesis that stronger firm economic conditions were associated with reduced accident incidence within firms. The principal contribution is the separation of between-firm and within-firm evidence and the demonstration that sample certification status, exposure measurement, missingness, and calendar-year effects must be described transparently.

Supplementary Materials

The additional data and information can be downloaded at: https://media.sciltp.com/articles/others/2609101023060604/WAH-26070232-SM.pdf. Table S1. Exploratory year-by-economic-indicator interactions in firm and calendar-year fixed-effects models. Table S2. Firm fixed-effects Poisson models for all recorded accident counts. Table S3. Firm and calendar-year fixed-effects models using the inverse hyperbolic sine transformation of official workplace-accident incidence.

Author Contributions

A.S.-T.L.: conceived and designed the study, contributed to the methodology, investigation, data curation, interpretation of the findings, preparation of the figures and tables, and drafted the manuscript. N.F.L.: contributed to the methodology, formal analysis, data curation, interpretation of the findings, and critical revision of the manuscript. I.M.A.: contributed to the investigation, interpretation of the results, and critical revision of the manuscript. J.J.A.G.: contributed to the methodology, supervision, interpretation of the findings, and critical revision of the manuscript. R.A.E.: contributed to the formal analysis, visualization, interpretation of the findings, and critical revision of the manuscript. All authors reviewed and approved the final revised version and agreed to be accountable for the work.

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Institutional Review Board Statement

Ethics approval was not applicable because this study was a secondary analysis of pre-existing, organization-level administrative and commercial records. No workers were recruited, contacted, or individually analyzed, and the analytical panel contained no direct worker identifiers. The study did not involve an intervention or the collection of new human-participant data. Data were handled under the confidentiality and access restrictions described below.

Informed Consent Statement

Not applicable. Because the analysis did not recruit or interact with individual participants and used organization-level secondary records.

Data Availability Statement

The firm-level source data are not publicly available because they derive from proprietary insurer, certification, and commercial databases and are subject to confidentiality and access restrictions. A de-identified aggregate result-to-variable traceability table and the statistical scripts can be supplied to the Editor on request;

neither contains the underlying proprietary records. Access to the underlying records can be considered only if the relevant data owners authorize it and confidentiality can be protected.

Conflicts of Interest

The authors declare no conflict of interest.

Use of AI and AI-Assisted Technologies

During the preparation and revision of this work, the authors used ChatGPT (OpenAI) to assist with English-language editing and to improve the clarity and readability of the manuscript. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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