

Safety organization reform and worker consequences in Denmark

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Abstract

Employer-size rules can alter how workers participate in safety decisions, but a difference between small and large firms is not itself a policy effect. This preregistration studies Denmark's 2010 reform of the work-environment organization, which changed default organizational tiers while retaining substantive duties and introducing other changes. The proposed comparison follows workers defined in 2007 at single-workplace manufacturing enterprises with 20–34 rather than 10–19 November payroll persons. It estimates the difference in effects of the national reform package across these fixed groups, using 2006 as the reference and 2011 as the primary follow-up. Outcomes are annual risk of a recorded principal inpatient injury admission, November primary-or-secondary salaried employment, and wage income capped at 600,000 nominal DKK. Documented 2008 employment ascertainment and 2011 wage-component changes restrict the economic results to literal recorded contrasts and conditional sensitivity; they cannot supply established constant-definition economic policy effects. Exact legal eligibility and committee receipt are not observed. Conditional parallel counterfactual changes, stable ascertainment and no relevant reference-period anticipation are required. Prespecified bounds, clustered inference, familywise error control and sensitivity analyses distinguish precision from identification. No study records have been accessed and all results remain pending. A failed institutional or data comparison will be reported as a boundary, not replaced with an observational claim.

Keywords: Work-environment organization; enterprise size; institutional reform; recorded injury; employment; Denmark; preregistration.

PRE-DATA TEMPLATE: empirical results are pending; any illustrative mock-data figures or tables are not research findings.

Why organizational thresholds matter

ILLUSTRATIVE MOCK DATA — NOT RESULTS. Every figure and table preview is a hypothetical layout demonstration, not a prediction or study finding. Empirical result slots remain pending.

Employer size can change the institutions through which safety concerns are heard and acted upon. Formal representation may convey information that individual workers hesitate to disclose; a separate committee can coordinate strategic decisions across daily work. The same structure may also impose administrative costs or separate responsibility from the people who can act. These competing mechanisms make the consequences of changing an organizational tier an empirical question. They do not predict that every increase or decrease in formal organization improves safety.

Denmark’s 2010 AMO reform offers an institutionally motivated comparison because its organizational incidence differed with enterprise size. Under the prior default, enterprises at twenty employees had a separate safety committee. The new framework allowed a single organizational level through thirty-four employees while retaining daily and strategic duties. The reform was broader than this threshold change. Training, discussion, flexibility and transitional arrangements could affect both sides of the comparison. Accordingly, our question concerns differential consequences of the package, rather than a claim that one size group alone was treated.

The scientific distinction matters for both identification and interpretation. Firms that grow are selected by demand, productivity, workforce composition and prior performance. A cross-sectional size gradient cannot establish what a legal requirement does. We instead freeze enterprise payroll groups before the reform and compare changes for their original workers across a common national policy event. This removes post-reform growth as the definition of treatment, while leaving an explicit assumption about how the groups would otherwise have changed. It is a conditional quasi-experimental comparison, not random assignment at a count cutoff.

[Pedersen, Hannerz and Christensen \(2012\)](#) already completed a Danish size-hospital-injury study and a comparison around the 2002 safety-organization change, following their [2011 protocol](#). They studied a dynamic population of male manual construction workers, measured local workplace size, and included the broader S00–T98 hospital-contact definition. Our proposed extension concerns the different 2010 institutional package, a fixed manufacturing workforce, a legal-identifier payroll proxy and a narrower person-level inpatient endpoint. We also retain economic evidence for the same original workers, with its recording limitations explicit. The policy comparison is not gated by a significant size gradient. Neither hospital linkage nor an institutional size comparison is claimed as new, and the earlier study’s nonrejection is not evidence of equivalence for our target.

The [CAVI final report](#) supplies implementation context: its 2012–2015 case studies examine reform alongside changes in management and work-environment organization. This makes a purely mechanical response to the legal tier unlikely to be an adequate account. It does not provide a representative first stage for our 2011 comparison or establish that reform incidence was absent. Our endpoint evidence can assess worker consequences conditionally, while leaving organizational receipt and mechanism unresolved.

A safety policy can affect more than medically recorded injuries. Employment loss can reduce time exposed to some hazards; wage resources may change even when a worker remains attached to employment. Conversely, a lower recorded injury rate accompanied by preserved work is more compatible with some safety-improvement accounts than a reduction accompanied by substantial work loss. We measure these margins in the same original workforce, without adjusting the injury regression for post-policy employment. Their combination constrains interpretation but does not identify mediation or total welfare.

The institutional change and its measurement

The contemporaneous [L126 memorandum](#) describes the prior default of safety groups at ten employees and committees at twenty. [LOV508/2010](#) principally commenced on 1 October 2010. The implementing [BEK1181/2010](#) commenced on 17 October and specified one organizational level for 10–34 employees and two for 35 or more. The one-level organization still had strategic and daily functions. Existing representatives, training provisions and negotiated alternatives prevent interpreting the date as instantaneous removal of all committees.

The earlier organizational tiers did not necessarily involve different people. Under the old default, one or two safety groups could themselves perform committee functions with the employer ([BEK 575/2001, §12\(4\)](#)). This makes the difference between formal structure and actual representation especially consequential in our single-workplace sample. The comparison cannot isolate a loss of committee members or worker voice.

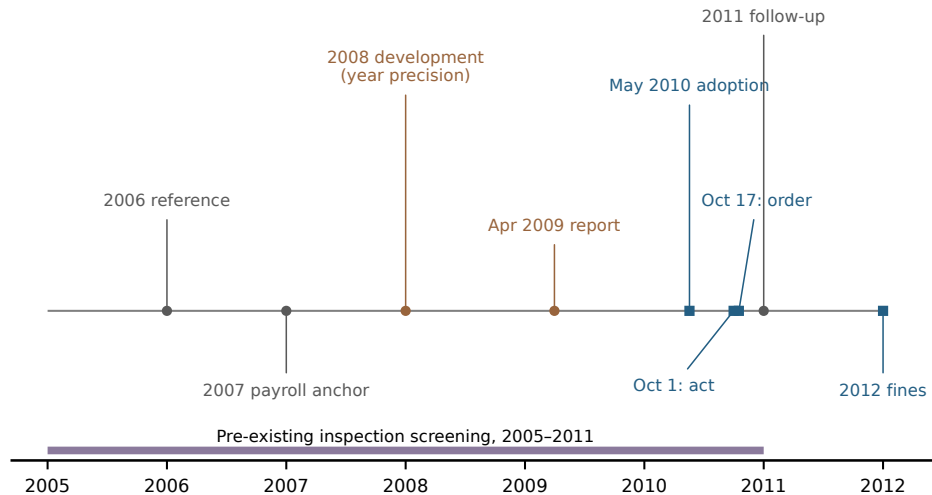
The legal counting rule is consequential. Statutory supervisors and enterprise leaders are excluded, and office/administrative/shop employees face a weekly-hours condition. Temporary off-site and seasonal employment have additional rules. Agreements can alter organizational form without eliminating underlying duties. A November workplace count cannot reconstruct those features. Restricting the study to single-workplace manufacturing avoids making construction-site assignment central, but does not remove every exception or part-time employee.

We therefore use an explicitly imperfect payroll measure. IDAS documents a contemporaneous CVR field from 2007, enabling a proposed job-to-workplace-to-legal-identifier map one year before the corresponding IDAN field begins. All November H/B/N persons are counted once per CVR before restricting to the worker cohort. Workplace ANTNOV is used only as a diagnostic. This avoids both summing jobs as if they were persons and counting only the eventual study participants as if they were the whole employer. Actual coverage and linkage remain runtime questions.

Three conclusions must be kept distinct. An exact statutory RDD using ANTNOV is contradicted by the documented unit and count differences. Actual legal headcount, agreement incidence and receipt are unresolved, not demonstrated absent. A differential effect of the national package across fixed payroll groups remains a coherent target under stated counterfactual assumptions, while an isolated committee-removal effect does not follow from this design. Diagnostic success cannot collapse these three findings into a claim of verified legal assignment.

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A Calendar time and distinct evidence types



B Default organizational tiers (statutory count)

Legal count	Before reform	After reform
10-19	Safety group	One level
20-34	Committee tier	One level
35+	Committee tier	Two levels

Daily and strategic duties persist; agreements and transition apply.
 Old small committees could use existing group members.
 Payroll groups are proxies; legal count and receipt are unobserved.

Fig. 1 Historical policy timing and the proposed comparison. The upper strip places the April 2009 tripartite report, May 2010 adoption, October 2010 act/order commencement, the 2011 follow-up and the later 2012 fine schedule in sequence. The lower comparison states the old default separate committee at 20 and the post-reform one-level organization through 34, with substantive duties retained and negotiated exceptions. Payroll groups are fixed in 2007; 2006 is the outcome reference. These are documented institutional facts and analyst-selected design dates, not observed committee receipt. Source: L126, LOV508 and BEK1181; institutions.csv from atom-000004. Old committees at one or two safety groups could comprise the same members plus the employer; the diagram does not imply removal of distinct representatives. ILLUSTRATIVE MOCK DATA — NOT RESULTS.

Population, estimand and identifying assumptions

The study population consists of workers whose unique primary November job in 2007 belonged to a single-workplace manufacturing CVR with 10–34 November payroll

persons. Manufacturing uses DB03 divisions 15–37 in 2007. Workers must be aged 25–54 on 31 December 2007, have December residence snapshots in 2004–2007, and have valid nonnegative 2006 wage income. Both recorded sexes and unknown sex are retained. Neither health events nor future employer survival define eligibility. The resulting target is workers with documented prehistory, not every worker who ever encountered the institution.

The comparison is 20–34 versus 10–19 payroll persons at baseline. Within common-support cells defined by two-digit industry, sex and age group, the target uses pooled original-worker proportions. This makes the standardization explicit and gives each group the same cell distribution. Cells absent in one group are excluded and reported before outcome analysis. It is not an assurance that remaining employers are otherwise exchangeable.

Let $P=1$ denote the historical 2010 AMO package, including announcement and implementation, and $P=0$ continuation of the prior regime. For fixed group G and common cell s , the target is the pooled-cell average of the policy effect in $G=1$ minus the policy effect in $G=0$. It is estimated by the difference between the groups’ 2006-to-2011 changes. Both groups experience $P=1$. Consequently, an estimated zero differential effect is compatible with equally large beneficial or adverse effects in both groups. No estimate of the absolute national effect or actual committee receipt is promised.

Identification requires equal counterfactual changes within the registered cells absent the reform, no relevant anticipation in 2006, comparable record ascertainment and a stable interpretation of the policy regime, including spillovers. The external event is the law change, not worker illness or an endogenous increase in headcount. Nevertheless, a national date is not sufficient by itself: firms of different initial size may recover differently from the financial crisis, face different demand or financing shocks, or respond differently to concurrent tax changes. [The Ministry of Taxation’s contemporary account](#) documents another major policy change in this period. Industry standardization and fixed worker follow-up do not eliminate these threats. [Kreiner, Leth-Petersen and Skov \(2016\)](#) directly document employer-assisted shifts of wage payment from late 2009 into early 2010. A transition-year wage dip and rebound is therefore not specific evidence of AMO anticipation or response. That source does not establish a lasting 2011 effect or its incidence across our size groups, and our annual records do not reproduce its monthly correction.

Baseline workforce selection also matters. Every selected worker has primary employment in 2007, so that year’s employment rate is mechanically selected. We use 2006 as the reference and retain 2007 as a marked selection anchor, not a clean placebo. Strict pretrend checks compare 2005 with 2004 and 2006 with 2005. The 2008–2010 paths are informative about crisis exposure, anticipation and transition, but are not declared untreated placebo years. Following workers through 2011 avoids censoring those who leave their original employer, while leaving possible selection-induced mean reversion explicit.

The three primary 2011 contrasts share familywise $\alpha=.05$, allocated equally by Bonferroni. Uncertainty is clustered at the original CVR and uses fixed-worker weights; employer changes do not change clusters. Cluster-count, concentration and

variance safeguards can leave a result with bounds but no directional inference. Missing employment and capped wages retain their registered intervals rather than being dropped. Hospital absence means zero only for the endpoint of a recorded Danish admission in a documented complete linkable extract.

A contemporaneous [Danish government report](#) describes a national enterprise safety-screening programme spanning 2005–2011 (physical pp.36–37). Different inspection timing across initial size groups could produce different injury paths without being caused by the 2010 AMO package. The available fields do not establish visit timing. [Seim, Møller and Jensen](#) place the reform-development process in 2008 and emphasize organizational flexibility and integration. Their programme-theory account reinforces both the transition-period designation and the distinction between intended mechanisms and measured implementation.

Sensitivity to identification is reported separately from sampling uncertainty. A fixed grid allows total differential bias of up to 1, 2 or 5 workers with a recorded qualifying injury admission per 1,000 original workers per year; 1, 2 or 5 percentage points in employment; and 5,000, 10,000 or 20,000 DKK in capped wages. For economic outcomes this is one bound on the sum of counterfactual change and measurement drift, including the documented recording breaks, rather than separate allowances for each source of bias. These are illustrative assumptions, not empirically verified bias limits. All activated contrasts are reported regardless of pretrend results, following the concern about selected inference emphasized by [Roth 2022](#). We do not select the comparison after seeing favorable estimates.

Measurement evidence and the comparison population

The first evidence block establishes the denominator and the meaning of the exposure. Table 1 describes the fixed comparison population; Table S1 accounts for its construction. Counts of enterprises and people remain separate because a large workforce in a few employers is not a large number of independent policy units. The reported payroll-size measure is worker-weighted when describing the workers' institutional setting. It is never relabeled statutory headcount.

Table 1. Baseline workers in the common-support comparison.

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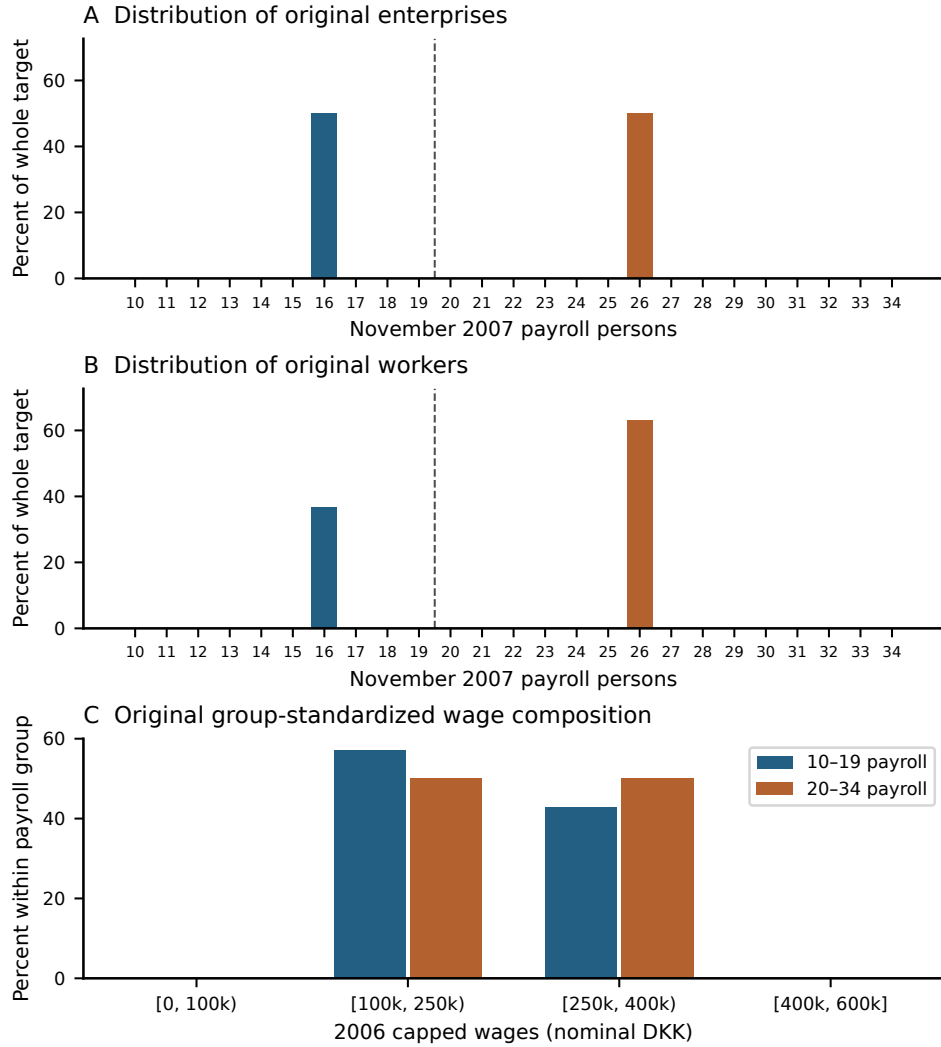
Characteristic	10–19 payroll persons	20–34 payroll persons
Original workers	560	960
Baseline enterprises	40	40
Supported cells	2	2
Mean baseline age	37.0000	37.0000
Recorded female share	0.5000	0.5000
Unknown sex share	0.0000	0.0000
Worker-weighted November payroll size	16.0000	26.0000
Mean 2006 capped wage income	231,408	240,005
Largest enterprise share of group weight	0.0250	0.0250

All empirical cells are pending. Counts are distinct and unweighted; means/shares use the fixed common-cell target except the explicitly defined concentration statistic. Known differences describe the target; no balance p-value establishes exchangeability. The full exclusion, count-scenario and subsequent original-employer observation tables are in SI.

The distributions in Figure 2 show what averages in Table 1 can conceal. Payroll size contributes both a distribution of independent enterprises and a distribution of people; their difference matters for interpreting support and precision. Baseline wage bins reveal composition relevant to differential recession recovery and tax exposure without claiming that the bins reconstruct tax liabilities. Table S13 compares additional earnings standardization against a restricted baseline specification on exactly the same supported workers, keeping sample loss separate from reweighting.

[pending]

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Dashed division marks payroll grouping, not verified legal assignment. No sampling intervals.

Fig. 2 The enterprises and workers behind the comparison. Panels A and B show respectively the shares of all original enterprises and unweighted workers at each integer November 2007 payroll count from 10 to 34. The same percent scale and count axis permit direct comparison; the gap between 19 and 20 marks the registered groups, not a verified legal cutoff. Panel C compares fixed 2006 capped-wage bins across groups using original group-standardized shares. Bin edges are 0,100000,250000,400000,600000 nominal DKK; the top bin includes its upper endpoint. These are descriptive baseline distributions without sampling intervals, not an RDD density test or a distributional treatment effect. Suppressed cells remain marked rather than regrouped. Source: atom-000004 payroll-distribution.csv and baseline-wage-distribution.csv. ILLUSTRATIVE MOCK DATA — NOT RESULTS.

Registered injury and economic results

Recorded inpatient injury is the closest planned health consequence to the safety question, but it is not an occupational-accident measure. The primary endpoint is any inpatient admission in the calendar year with a principal diagnosis in ICD-10 S00–T79, including descendants and excluding medical-care complications and sequelae in T80–T98. Multiple admissions count once. No restriction to acute admission or survival to discharge is invented. Nonprincipal and outpatient-only injuries do not qualify. A person without a Danish record after moving abroad may have injuries elsewhere; the outcome name preserves that limitation.

The dynamic display asks whether the 2011 contrast emerges against already different trajectories and whether its timing is consistent with the institutional account. It cannot distinguish every recession or anticipation mechanism. The main estimate and its adjusted interval appear in Table 2, together with economic consequences measured in the same cohort. Small-effect regions require the entire interval to fit within the prespecified band; a nonsignificant point estimate is insufficient.

[pending]

Table 2. Primary 2011 differential changes relative to 2006.

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Outcome	Lower bound	Upper bound	CI lower	CI upper	Adj. p	Disposition
Any recorded inpatient injury (probability difference)	-0.0010	0.0057	-0.0366	0.0427	Not applicable (mock)	fragile
November H/B employment (probability difference)	-0.0558	-0.0301	-0.1131	0.0260	Not applicable (mock)	fragile
Wages capped at 600,000 DKK (nominal DKK difference)	-8,609	6,838	-13,201	10,374	Not applicable (mock)	fragile

Equal contrast endpoints indicate a point-identified observed-data contrast; unequal endpoints show bounds from missing outcomes. Intervals allocate $\alpha/3$ to each primary endpoint, even if another is unavailable. An adjusted p-value is not supplied for a set-valued contrast. The actual local key and flags determine the permitted wording; numbers and keys are all pending.

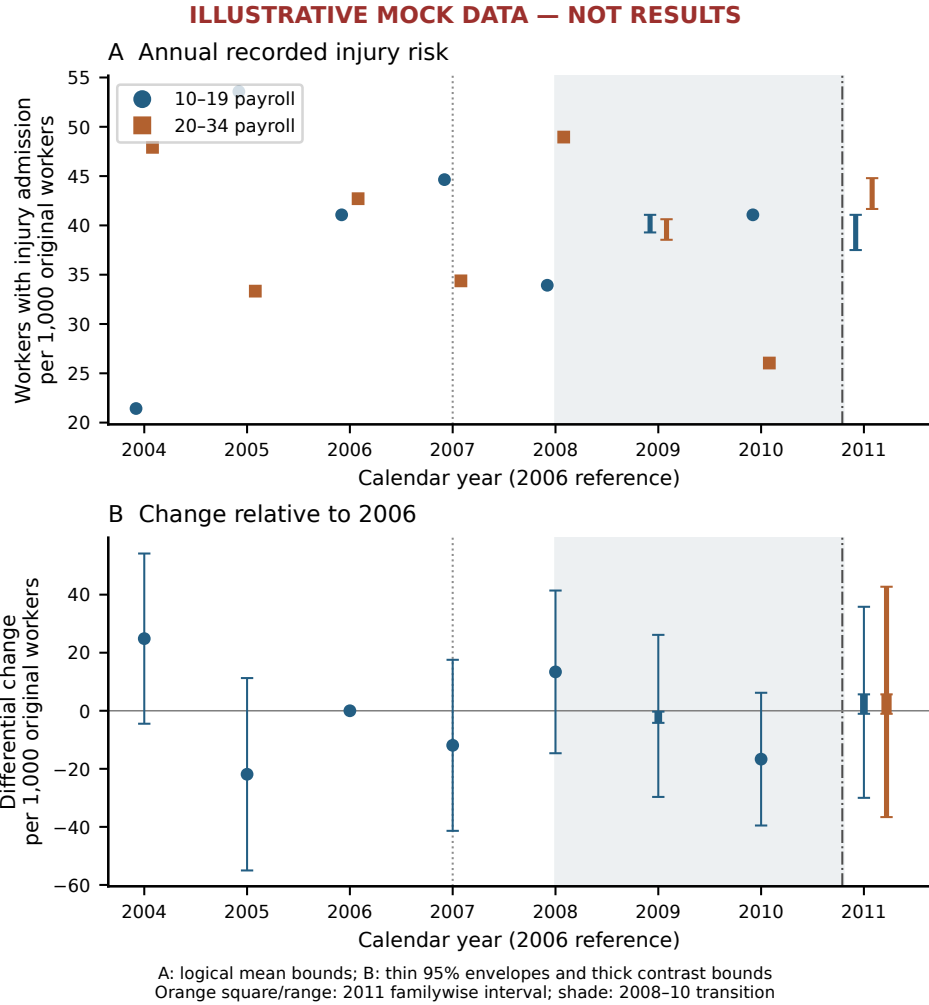


Fig. 3 Recorded inpatient injury levels and differential changes. Panel A shows 2004–2011 standardized annual risk in each original payroll group, expressed as workers with a qualifying admission per 1,000 original workers. Ranges are person-level missingness bounds, not sampling intervals; coincident bounds give a point. Panel B shows differential changes relative to 2006, with 95% pointwise cluster sampling/missingness envelopes and a separately marked 2011 Bonferroni-adjusted envelope. No mid-point is invented for a bounded contrast. The 2007 selection anchor, 2008–2010 transition window and October 2010 implementation are marked. Unavailable evidence is a gap with a reason, never zero. Source: atom-000005 injury-observability.csv, injury-dynamics.csv and injury-summary.csv. ILLUSTRATIVE MOCK DATA — NOT RESULTS.

Table 2b. Numerical regions and measurement restrictions for the primary contrasts.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Outcome	Numerical region and interval sign	Recording status	Scientific restriction flags
Any recorded inpatient injury	imprecise; includes zero	not crossing known economic break	strict pretrend

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Outcome	Numerical region and interval sign	Recording status	Scientific restriction flags
November H/B employment	imprecise; includes zero	known break unquantified	employment 2008 ascertainment
Capped wage income	imprecise; includes zero	known break unquantified	strict pretrend; wage 2011 components

Numerical regions summarize the adjusted recorded-outcome interval before scientific restrictions. A small interval may be strictly positive or strictly negative. Its sign is retained and never described as unchanged or necessarily favorable. The economic recording flags are predetermined by source history; their eventual empirical incidence and magnitude are unknown. The injury status here addresses the economic breaks only and does not certify hospital ascertainment. Detailed definitions appear in SI.

Employment is a recorded primary or secondary November salaried job, not continuous retention or all paid work. N-only jobs can be paid work while failing this narrow endpoint. Wage income is an annual register aggregate rather than hourly compensation; the fixed 600,000 DKK cap defines a bounded target and is not selected from the observed distribution. Job changes, missing residence, death and wage anomalies are accounted for explicitly. Income after a death is not automatically set to zero.

These economic endpoints cross known recording changes. The [official employment history](#) describes the large reduction in the earnings requirement for November classification from 2008; H/B cannot be assumed to measure the same work-attachment threshold before and after that change. The [wage history](#) and [dated component account](#) also identify a change at 2011, including noncompetition compensation previously excluded. We retain the literal records on the original workforce and flag their policy interpretation as fragile whenever valid intervals can be calculated. There is no outcome-selected threshold, survivor restriction or unverified component subtraction. Corrected extracts, early pretrends and the fixed bias exercise do not resolve these differences. This qualification limits economic attribution separately from the injury comparison.

The economic comparison asks what happened to the original workers rather than only those who remained at their employer. Destination shares distinguish original-employer attachment, attachment elsewhere, unresolved employer identity and lack of the registered job witness. These shares can help interpret a joint pattern, but cannot date continuous transitions or identify a legal layoff. Female-minus-male contrasts describe possible distributional differences within separately standardized populations; they are exploratory and do not identify discrimination.

The separate injury and work series do not establish whether the two recorded states belong to the same people. Figure 5 therefore describes their joint distribution in the original workforce before and after the reform window. Four panels retain the four possible binary states; bounds account for unknown records without restricting the population to workers with complete follow-up. Table S14 shows the underlying nine observed states, including unknown injury or employment. The annual injury window and November job snapshot cannot date transitions or establish which event

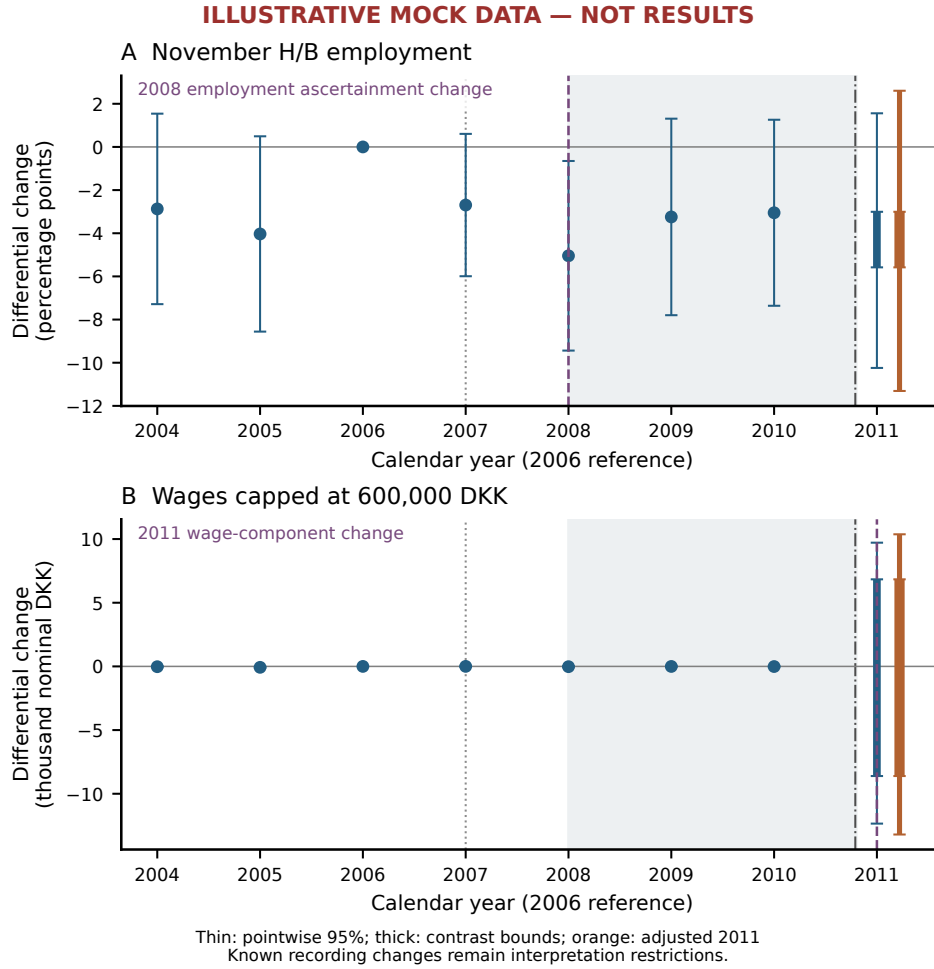


Fig. 4 Work attachment and wage resources of the original workforce. Panel A plots the differential change in November H/B employment in percentage points; panel B plots the differential change in capped nominal annual wages in thousands of DKK. Both use the same original workforce, fixed common-cell weights, 2006 reference and original-CVR clustering as Figure 3. Pointwise 95% envelopes and separately overlaid 2011 familywise-adjusted envelopes are distinguished. 2007 is a selection anchor and 2008–2010 is a transition window. The 2008 H/B ascertainment break and 2011 wage-component break are labeled in their respective panels; neither is an estimated AMO effect. Missing or unsupported intervals are marked, not filled. Source: atom-000006 economic-dynamics.csv and economic-summary.csv. ILLUSTRATIVE MOCK DATA — NOT RESULTS.

came first. Differences across 2006 and 2011 also retain the employment-recording limitation.

[pending]

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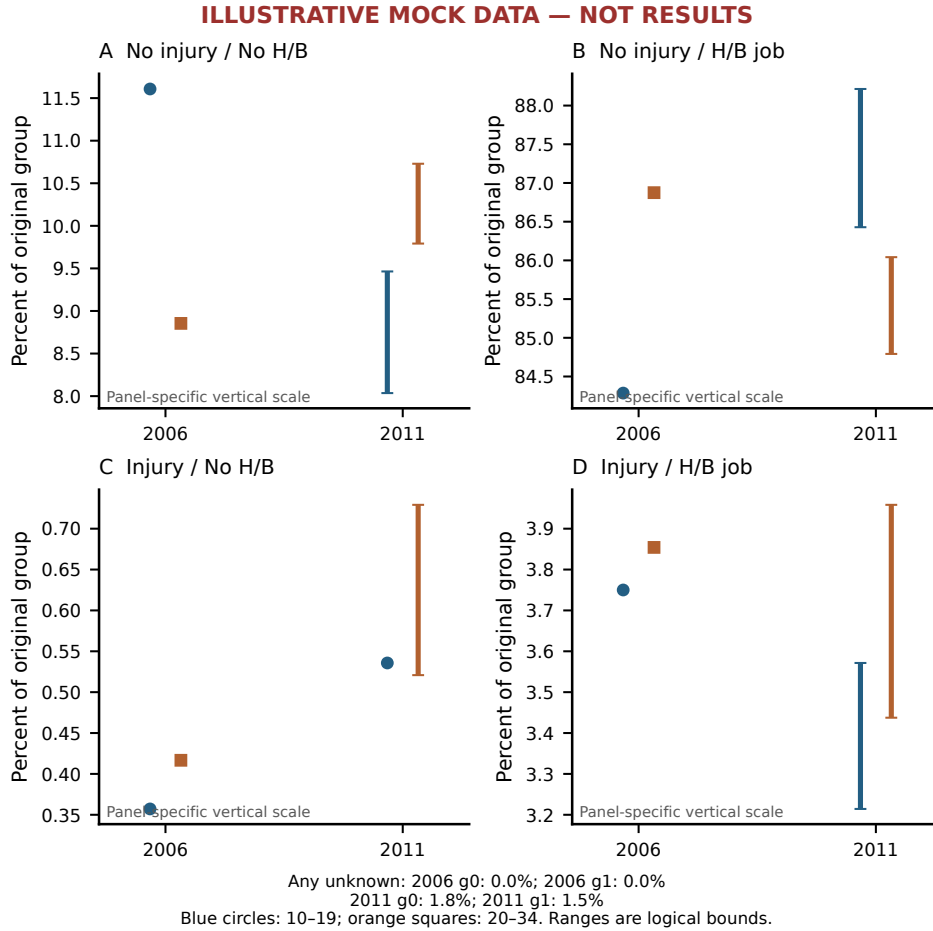


Fig. 5 Injury and employment recorded in the same original workers. Four small multiples show the states injury/no injury crossed with November H/B job/no H/B job in 2006 and 2011. Each panel plots both fixed payroll groups on the same within-panel percent scale; state-specific vertical scales are explicitly labeled, so heights across panels are not directly comparable. Capped vertical ranges are logical missingness bounds; a point appears only when the bounds coincide. The weighted share with either component unknown is stated for every group/year, and Table S14 reports all nine observed states. These are descriptions, not confidence intervals or joint causal effects. Annual injury and November employment have different clocks, and the comparison crosses the documented employment ascertainment change. Source: atom-000006 joint-bounds.csv and joint-states.csv. ILLUSTRATIVE MOCK DATA — NOT RESULTS.

What the comparison can establish

The study is designed to learn about institutional change while preserving the difference between law, administrative exposure and realized organizational practice. Its strongest licensed conclusion would be a differential effect of the 2010 AMO package across the registered pre-reform payroll groups, conditional on the stated assumptions and supported uncertainty. It would not identify the effect of one more

employee, a committee’s actual presence, the removal of worker voice, or the overall national reform. An absence of differential implementation could itself produce a small differential effect, without implying that the policy did nothing.

The main causal vulnerability is a different counterfactual recovery path by initial enterprise size. The interval sensitivity exercise makes explicit how large a net deviation from parallel changes is sufficient to alter a conclusion. It does not prove that the selected bounds are plausible or that a passed pretrend test removes confounding. Concurrent policy and measurement changes remain in the interpretation even if every available diagnostic looks reassuring. An affected comparison is described as fragile when the registered modest-bound or pretrend flags apply. Economic contrasts also retain their predetermined measurement flags regardless of these diagnostics. Their conditional bias envelopes describe assumptions under which a policy interpretation would follow; no selected envelope is evidence that those assumptions hold.

An unresolved health result need not erase computable recorded economic contrasts, but their known recording breaks continue to restrict causal interpretation. Conversely, economic fragility does not automatically invalidate a separately supported injury comparison, and a favorable numerical economic profile cannot validate an unavailable health effect. An institutional boundary is a substantive limitation of this proposed study rather than a statistical null. If no credible policy interpretation survives, the report will say that the causal objective was not achieved. This reporting commitment is part of the design and prevents outcome-driven replacement of the question.

The policy question extends beyond these registers. We do not measure committee membership, votes, training receipt, weekly risk exposure, subjective safety, friendships, or wellbeing. The boundary also supplies no prescription or education register. Occupational accident coding in LPR_ULK was investigated but its completeness and meaning for the target were not established, so hospital injury is not labeled occupational. Household/disposable-income extensions are not required to interpret the selected labor-resource question; their omission is a scope choice, not a claim that IND contains no relevant fields. Later policy windows are not ruled out solely by legacy hospital dates. These limits focus the present question without silently treating unsupported outcomes as observed.

[pending]

Supplementary Information: sources and cohort construction

This supplement supplies the methods and complete supporting displays needed to assess the main argument. It is part of the same manuscript. Exact source fields are incorporated in the Atom registrations through the shared data contract; no field list establishes that the corresponding data were delivered.

IDAN is an employment-relationship register, whereas IDAS describes workplaces. We first map 2007 workplace identifiers to contemporaneous legal identifiers in a complete workplace universe, then count distinct H/B/N persons within each CVR. A person with two jobs in one enterprise contributes one payroll person; a person working at two enterprises contributes to each. Only after constructing those rosters do we apply sector, size and worker restrictions. The mapping never uses a later survivor crosswalk. Conflicting primary keys are not resolved by selecting the record with the largest wage or by silently deleting conflicts.

TYPE before 2008 uses the literal H/B/N job codes; numeric encodings invented from a different source would change the denominator. Later TYPE_2008 reflects the eIndkomst-based job system, and the documented 2018 rerun requires attention to extract vintage. The 2008 change affects the recorded H/B employment endpoint as well as ANTNOV and payroll persistence. A corrected IDA vintage repairs extraction errors, but does not restore the old annual-earnings ascertainment rule. The 2007 exposure roster remains governed by its contemporaneous definition; the later outcome restriction is not automatically a shared-cohort failure. STILL 31 identifies top managers but cannot remove all persons excluded by the law. Annual hours and days do not recover a statutory weekly threshold. Each of these distinctions can change the institutional interpretation even when the files join without error.

Table S1 records construction stages and separate exclusion reasons. Overlapping reason counts must not be summed as a sequential flow. BEF snapshots document residence at dates, not continuous residence or exact emigration. Requiring 2004–2007 snapshots and a valid 2006 wage narrows the baseline population before examining outcomes. Future absence, death or employer disappearance never becomes a reason to remove an otherwise eligible worker.

Table S1. Cohort construction and exclusions.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Stage or reason	Unit	Count
All 2007 H/B/N jobs	jobs	1,680
Jobs with usable contemporaneous legal map	jobs	1,680
Manufacturing enterprises	CVRs	80
Payroll size 10–34	CVRs	80
Single workplace with FILIAL agreement	CVRs	80
Unique primary workers	persons	1,680
Age 25–54	persons	1,520
Required prehistory	persons	1,520
Workers in common support	persons	1,520

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Stage or reason	Unit	Count
Enterprises contributing to support	CVRs	80
Exclusion reason: Orphan job map	source units specified in reason	0
Exclusion reason: Missing legal identifier	source units specified in reason	0
Exclusion reason: Workplace multiplicity disagreement	source units specified in reason	0
Exclusion reason: Conflicting primary jobs	source units specified in reason	0
Exclusion reason: Missing birth date	source units specified in reason	0
Exclusion reason: Missing baseline residence snapshots	source units specified in reason	0
Exclusion reason: Missing/negative baseline wage	source units specified in reason	0
Exclusion reason: Absent common cell support	source units specified in reason	0

Table S2. Legal rule versus available measure.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Feature	Historical rule or institutional concern	Planned measure	Remaining limit
Assignment unit	Enterprise/legal unit	2007 IDAS.CVRNR via contemporaneous LBNR	Actual namespace and universe need validation
Prior organization	Groups at 10; separate committee at 20 by default	Fixed payroll groups 10–19 and 20–34	Payroll size is not legal count
Later organization	One level 10–34; two levels 35+	2010 package timing	Duties retained; no receipt/compliance observation
Supervisory personnel	Leaders/statutory supervisors excluded	STILL 31 sensitivity	Other supervisors unobserved
Office/admin/ shop work	Weekly-hours inclusion rule	Annual hours/days inspected	Annual totals cannot reconstruct weekly eligibility
Off-site/ seasonal work	Special counting/time provisions	Single-workplace manufacturing scope	Exceptions not proved absent
Negotiated structures	Qualifying agreements can vary form	No direct agreement variable	No committee-removal first stage
Transition	Incumbent/training provisions	2011 first full calendar follow-up	Not instantaneous reorganization
Pay transparency	Separate legal count, period and sex/ occupation cells	Historical primary-source reconstruction	No November/ annual-code substitution

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Feature	Historical rule or institutional concern	Planned measure	Remaining limit
Data vintage	2008 job-definition break and later revision	Corrected IDA vintage; H/B/N construction	Actual comparability must be checked

The nearby thirty-five-person region carries other institutional obligations, including pay-transparency and information/consultation rules with their own count definitions. It cannot be labeled an untreated AMO control merely because its default tiers differ. We retain workers whose original employers later cross this region and report original-CVR persistence, without inventing exact incidence of those obligations. The primary contrast remains the two fixed payroll groups below thirty-five in 2007.

Table S2b. Binding treatment of known economic recording changes.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Component	Historical change and registered construction	Predeclared disposition
November H/B employment	Before 2008 an annual earnings requirement of roughly DKK 10,000 governed classification; the new basis greatly reduced that requirement. Retain literal TYPE H/B before 2008 and TYPE 2008 H/B thereafter.	Record employment 2008 ascertainment. With valid uncertainty report fragile plus the numerical region/sign; with unsupported uncertainty report bounds only and the same warning. No post-reform low-earner exclusions or arbitrary common earnings threshold.
Capped LOENMV 13	Employer noncompetition compensation excluded in 2002–2010 changes in 2011; retain each year's recorded aggregate and the fixed cap. Other documented 2011 changes are not assumed irrelevant without evidence.	Record wage 2011 components. Valid uncertainty remains fragile; otherwise bounds only. Neither cap sensitivity nor subtracting a residual income aggregate harmonizes compensation.
Absent/incompatible required component	Source-year dictionary, complete universe, units or required keys demonstrably cannot supply the literal endpoint.	Report unavailable for that component and retain its test allocation; compute the other component on the unchanged workforce. A failed access/run remains operational no result.

These are component decisions rather than a license to certify comparability from a favorable outcome. The revision chooses no source-supported common-definition economic measure: annual job hours cannot restore missing historical classification, post-2008 narrow/broad wage fields lack the fixed 2006 reference, and residual income is not an isolated noncompetition payment. A genuine harmonization would require a disclosed scientific amendment with its source and exact algorithm. Measurement drift is not treated as missing persons; ordinary outcome bounds and recording restrictions

answer different questions. The strict 2004–2006 pretrends cannot diagnose later coding changes. The same restrictions accompany every affected supplementary economic comparison, including caps and sex contrasts.

Table S3 examines count proxies without pretending they recover legal eligibility. For each scenario, the original cohort is intersected with that scenario’s eligible enterprises and groups are reassigned using that scenario alone. Standardization is recomputed within its stated target. The interior comparison uses 12–17 versus 23–30 payroll persons. Results for a scenario without support stay unavailable; no scenario becomes the headline because its estimate looks preferable.

Table S3. Exposure-proxy sensitivity populations.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Scenario	Retained workers	Retained share	Changed- group share	Firms lower group	Firms higher group
Original November H/ B/N	1,520	1.0000	0.0000	40	40
One hypothetical exclusion	1,520	1.0000	0.0000	40	40
Two hypothetical exclusions	1,520	1.0000	0.0000	40	40
Three hypothetical exclusions	1,520	1.0000	0.0000	40	40
Only H/B payroll persons	1,520	1.0000	0.0000	40	40
Remove identified primary top managers	1,520	1.0000	0.0000	40	40
Interior payroll groups	1,520	1.0000	0.0000	40	40

Subsequent original-employer observation is a context diagnostic, not a survival filter. The next table reports the five exhaustive roster categories for each year, separately for each baseline group. The category concerns the original CVR rather than the worker’s current job. An unobservedCVR is not an adjudicated closure. Net headcount decline cannot establish a legal mass-layoff event.

Table S3b. Original-enterprise payroll observation after baseline.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Year	Baseline group	Unob- served	Below 10	10–19	20–34	35+
2008	10–19	0.0000	0.0000	1.0000	0.0000	0.0000
2008	20–34	0.0000	0.0000	0.0000	1.0000	0.0000
2009	10–19	0.0000	0.0000	1.0000	0.0000	0.0000
2009	20–34	0.0000	0.0000	0.0000	1.0000	0.0000
2010	10–19	0.0000	0.0000	1.0000	0.0000	0.0000
2010	20–34	0.0000	0.0000	0.0000	1.0000	0.0000
2011	10–19	0.0000	0.0000	1.0000	0.0000	0.0000
2011	20–34	0.0000	0.0000	0.0250	0.9750	0.0000

Supplementary Information: outcomes and uncertainty

The hospital join links diagnoses many-to-one to a contact and contacts many-to-one to cohort persons using a documented RECNUM namespace. Codes are uppercased and one leading Danish SKS diagnosis prefix D and punctuation are removed before applying ICD-10 families. Contact type 0 and principal diagnosis type A are required. Invalid dates, conflicting identity scopes and incomplete national extracts are diagnosed before establishing absence of an admission. A complete extract supports a zero Danish recorded-admission endpoint, not a statement that a person had no injury anywhere. An inpatient contact with missing or unusable principal diagnosis leaves the person-year unknown unless another qualifying injury establishes one; unexplained diagnosis orphans require resolution of contact coverage rather than automatic noninjury coding.

For economic outcomes, an H/B job witness establishes employment. Otherwise, December residence supports zero for this narrow endpoint; absence of both witnesses remains unknown unless a validated earlier death establishes zero. N-only jobs are retained in the institutional count but do not satisfy the H/B outcome. This distinction is intentional and must be stated in captions and prose. Wage observations are nonnegative finite annual values capped at 600,000 DKK. Missing and anomalous negative values retain an interval from zero to the cap. Known death does not justify deleting a person or assigning zero death-year income.

Let π_s be the pooled original-worker share of cell s and n_{gs} its worker count in group g . Each worker receives signed contrast weight $a_i = (2G_i - 1)\pi_s/n_{gs}$. The 2011 statistic is the weighted sum of $Y_{2011} - Y_{2006}$. For unknown outcomes, the minimum and maximum completion follow the sign of a_i ; if both periods are unknown, the difference ranges from $L - U$ to $U - L$. We never recalculate primary weights on complete cases. The resulting bounds reflect missing outcomes for this contrast, separately from the causal uncertainty about parallel changes.

$$a_i = (2G_i - 1) \frac{\pi_s}{n_{G_i s}}, \quad \hat{\tau}_t = \sum_i a_i (Y_{it} - Y_{i,2006}).$$

$$\hat{\tau}_L = \sum_{a_i \geq 0} a_i d_{iL} + \sum_{a_i < 0} a_i d_{iU}, \quad \hat{\tau}_U = \sum_{a_i \geq 0} a_i d_{iU} + \sum_{a_i < 0} a_i d_{iL}.$$

Here the lower and upper changes are follow-up lower minus reference upper, and follow-up upper minus reference lower. These formulas permit both dates to be unknown and preserve the original target weights.

For each point or bound completion, subtract the group-cell mean change and sum weighted residuals within original CVR. The variance estimate is $F/(F-1)$ times the sum of squared employer contributions, with $F-2$ degrees of freedom. This is an approximation relying on many sufficiently independent employers, not randomization inference. At least 30 employers per group, finite positive variance and limited concentration are required for directional/equivalence inference. No events and zero empirical variance do not yield a zero-width inferential interval. Required common support and the possibility of a one-cell target are disclosed.

$$u_f = \sum_{i \in f} a_i(z_i - \bar{z}_{G_{is}}), \quad \hat{V} = \frac{F}{F-1} \sum_f u_f^2, \quad \text{df} = F - 2.$$

The completion-specific change is denoted by z , its group-cell mean by the barred term, and the number of original enterprises by F . The concentration threshold is a maximum share of 0.10 of total absolute contrast weight. A one-cell target must be explicitly identified; it does not introduce an additional covariate adjustment.

Primary intervals use t critical value at $1 - .05/(2 \times 3)$. Bound endpoints receive the corresponding outer envelope. Point-identified contrasts may also report $\min(3p, 1)$; set-valued contrasts do not receive invented p -values. Dynamic 95% intervals are point-wise and cannot support a simultaneous all-years claim. The six strict pretrend checks retain their own fixed Bonferroni family, including unavailable tests. A missing test stays unresolved; a non-rejection never establishes parallel trends.

Table S4. Strict pretrend checks, all registered comparisons.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Outcome	Change	Envelope lower	Envelope upper	Six-test adjusted p if defined	Flag
injury	2005–2004	-0.0868	-0.0066	0.0137	flagged
injury	2006–2005	-0.0232	0.0669	1.0000	not flagged
employment	2005–2004	-0.0680	0.0448	1.0000	not flagged
employment	2006–2005	-0.0212	0.1019	0.4798	not flagged
wage	2005–2004	-108	11	0.1747	not flagged
wage	2006–2005	10	135	0.0136	flagged

Table S5 separates outcome observability from a substantive outcome. Each cell shows the weighted unknown fraction; the associated output supplies group means/bounds, known counts and anomalies. Hospital-wide missing partitions make a year unavailable rather than treating everybody as a zero or a personwise missing interval. A temporary computational failure is recorded separately as `no_result`.

Table S5. Unknown outcome shares, by fixed group and year.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Year	Injury 10–19	Injury 20–34	Em- ploy- ment 10–19	Em- ploy- ment 20–34	Wage 10–19	Wage 20–34
2004	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
2005	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
2006	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
2007	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
2008	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
2009	0.0018	0.0021	0.0000	0.0000	0.0000	0.0000
2010	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Year	Injury 10–19	Injury 20–34	Em- ploy- ment 10–19	Em- ploy- ment 20–34	Wage 10–19	Wage 20–34
2011	0.0036	0.0031	0.0143	0.0115	0.0143	0.0115

Supplementary Information: sensitivity and alternative accounts

The fixed-bias sensitivity adds an assumed bound on total net counterfactual and measurement discrepancy to the primary sampling/missingness interval: $C(B)=[C_lower-B, C_upper+B]$. It is not an estimate of bias, a bound calibrated from favorable pretrends, or an implementation of the specialized Rambachan–Roth procedure. The breakdown value for excluding zero is $\max(0, C_lower, -C_upper)$. Every point on the grid is shown, whether it strengthens or weakens a preferred interpretation.

A directional or small-effect result that fails to survive the modest bound of .001 injury probability, .01 employment probability or 5,000 DKK receives a fragility flag. These thresholds are chosen interpretation scales and are not externally validated clinical or economic minima. The raw interval and its original region remain visible, so the fragility label does not hide which conclusion changed or imply that the chosen bias actually occurred. For economic outcomes, surviving any of these bounds does not clear the known recording-break restriction; the grid does not estimate the size of reclassification.

Table S6. Complete fixed-bias grid.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Outcome	Net bias bound B	Envelope lower	Envelope upper
injury	0	-0.0366	0.0427
injury	0.001	-0.0376	0.0437
injury	0.002	-0.0386	0.0447
injury	0.005	-0.0416	0.0477
employment	0	-0.1131	0.0260
employment	0.01	-0.1231	0.0360
employment	0.02	-0.1331	0.0460
employment	0.05	-0.1631	0.0760
wage	0	-13,201	10,374
wage	5000	-18,201	15,374
wage	10000	-23,201	20,374
wage	20000	-33,201	30,374

Fracture diagnoses restrict the injury definition to principal S02,S12,S22,S32,S42,S52,S62,S72,S82,S92 families. This is a specificity comparison, not occupational validation. The injury-or-death sensitivity additionally requires

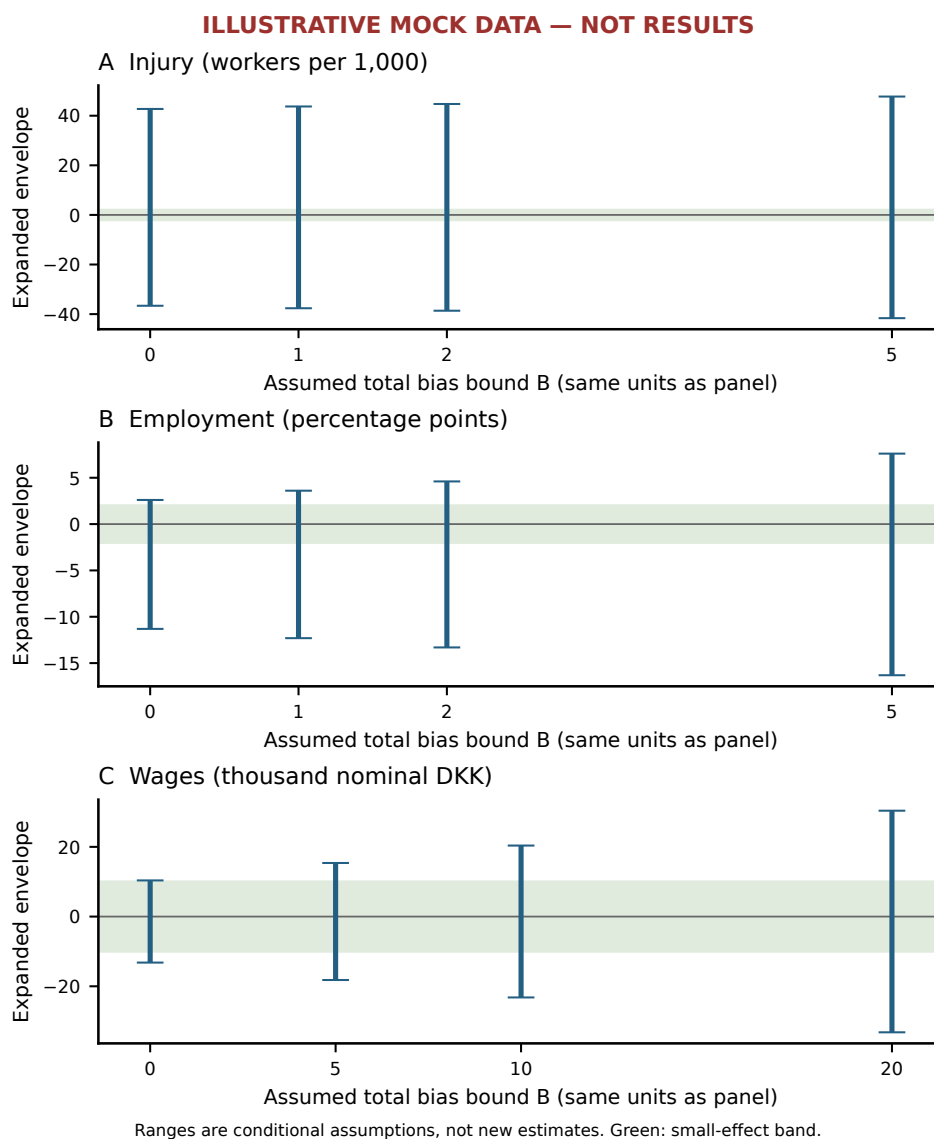


Fig. S1 Dependence on assumed bounds for counterfactual and measurement bias. Three panels plot the adjusted primary confidence envelope expanded by each registered net-bias bound B, in workers with a recorded qualifying injury admission per 1,000 original workers per year, employment percentage points and thousands of nominal DKK. The zero line and prespecified small-effect bands are shown. Wider envelopes reflect a stronger allowance for unidentified differential change, not another empirical estimate or a posterior probability. Source: atom-000005 injury-bias.csv and atom-000006 economic-bias.csv. ILLUSTRATIVE MOCK DATA — NOT RESULTS.

an explicit death clock for its 2006 reference, which the scientific handoff leaves unresolved. Its row therefore remains unresolved even if historical DODSAASG completeness is established; independent death accounting remains available when its sources are valid. The wage-cap comparisons distinguish dependence on the fixed cap from claims about the uncapped observed-record population. None replaces the primary endpoint.

Table S7. Outcome-definition comparisons.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Comparison	Contrast lower	Contrast upper	95% lower	95% upper
Principal fractures	-0.0010	0.0057	-0.0300	0.0358
Injury or death	Unavailable (mock)	Unavailable (mock)	Unavailable (mock)	Unavailable (mock)
Wage cap 400,000 DKK	-5,752	4,546	-8,118	6,909
Wage cap 800,000 DKK	-11,466	9,129	-16,840	12,838
Uncapped observed-record wages	-39	-39	-84	5

Table S7b. Complete exposure-scenario outcome matrix.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Exposure scenario	Outcome	95% envelope lower	95% envelope upper
main	injury	-0.0300	0.0358
main	employment	-0.1024	0.0156
main	wage	-12,345	9,715
minus 1	injury	-0.0300	0.0358
minus 1	employment	-0.1024	0.0156
minus 1	wage	-12,345	9,715
minus 2	injury	-0.0300	0.0358
minus 2	employment	-0.1024	0.0156
minus 2	wage	-12,345	9,715
minus 3	injury	-0.0300	0.0358
minus 3	employment	-0.1024	0.0156
minus 3	wage	-12,345	9,715
hb only	injury	-0.0300	0.0358
hb only	employment	-0.1024	0.0156
hb only	wage	-12,345	9,715
no primary topmanager	injury	-0.0300	0.0358
no primary topmanager	employment	-0.1024	0.0156
no primary topmanager	wage	-12,345	9,715
interior	injury	-0.0300	0.0358
interior	employment	-0.1024	0.0156
interior	wage	-12,345	9,715

Table S8 accounts for 2011 attachment among all original workers. A known same-CVR witness takes priority, followed by another known employer and then an

H/B record with unresolved employer identity. Among those without H/B, validated death before November, December residence and lack of both witnesses distinguish the remaining categories. These categories cannot establish uninterrupted retention, exact emigration or firm closure. When death coverage is unavailable, its row says so and the remaining accounting categories retain their observed definitions.

Table S8. Original-worker destination accounting in 2011.

ILLUSTRATIVE MOCK DATA — NOT RESULTS		
Category	10–19 baseline group	20–34 baseline group
Original enterprise H/B witness	0.9000	0.8854
Other known enterprise H/B witness	0.0000	0.0000
H/B witness, employer identity unresolved	0.0000	0.0000
No H/B, residence witness	0.0857	0.1031
Validated death before November	Unavailable (mock)	Unavailable (mock)
No H/B or residence/death classification	0.0143	0.0115

Table S9. Exploratory female-minus-male differential contrasts.

ILLUSTRATIVE MOCK DATA — NOT RESULTS						
Outcome	Con- trast lower	Con- trast upper	95% lower	95% upper	Female targetN	Male targetN
employment	0.0024	0.0539	-0.0815	0.1375	760	760
wage	-15,427	15,465	-22,131	22,146	760	760

The sex comparison uses separate common-industry/age standardization within each sex and accounts for employer-level covariance when differencing contrasts. It does not compare significance stars. Unknown recorded sex remains in the primary target but not this exploratory difference. The differing standardized populations prevent treating the result as a pure individual sex interaction or as proof of unequal treatment.

Table S10. Primary inference support and design precision.

ILLUSTRATIVE MOCK DATA — NOT RESULTS						
Endpoint	CVRs 10–19	CVRs 20–34	SE lower com- ple- tion	SE upper comple- tion	Illustra- tive 80% de- tectable differ- ence	Flags
injury	40	40	0.0145	0.0152	Not applicable (mock)	strict pretrend

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Endpoint	CVRs 10–19	CVRs 20–34	SE lower completion	SE upper completion	Illustrative 80% detectable difference	Flags
employment	40	40	0.0234	0.0229	Not applicable (mock)	employment 2008 ascertainment
wage	40	40	1,877	1,445	Not applicable (mock)	strict pretrend; wage 2011 components

The largest enterprise share of absolute contrast weight in the pooled target is *[pending]*. The registered concentration guard uses this pooled denominator (total absolute signed weight equals two). The detectable difference is a local-alternative approximation based on a valid point-identified standard error, not achieved power or evidence that an effect exists. It is unavailable for set-valued contrasts and unsupported variance regimes.

Table S11. Registered outcome levels before and after the reform.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Endpoint	Year	Baseline group	Mean lower	Mean upper	Known observations
injury	2006	10–19	0.0411	0.0411	560
injury	2006	20–34	0.0427	0.0427	960
injury	2011	10–19	0.0375	0.0411	558
injury	2011	20–34	0.0417	0.0448	957
employment	2006	10–19	0.8804	0.8804	560
employment	2006	20–34	0.9073	0.9073	960
employment	2011	10–19	0.9000	0.9143	552
employment	2011	20–34	0.8854	0.8969	949
wage	2006	10–19	231,408	231,408	560
wage	2006	20–34	240,005	240,005	960
wage	2011	10–19	232,950	241,521	552
wage	2011	20–34	241,509	248,384	949

Means retain the fixed standardization; known observation counts are unweighted. Equal endpoints indicate a known mean, not a sampling confidence interval. These levels contextualize differential changes without turning a raw group gap into a causal result. Injury and employment use probabilities; wages use nominal DKK.

Table S12. Optional death accounting and temporal integrity.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Metric	10–19 baseline group	20–34 baseline group
Death registered by end2011	Unavailable (mock)	Unavailable (mock)
Injury admission after an earlier recorded death	Unavailable (mock)	Unavailable (mock)

Death accounting requires validated historical completeness. Its absence does not become zero deaths or invalidate an otherwise complete Danish hospital-record end-point. Contradictory dates trigger the registered integrity and observability branches, never selective removal of inconvenient workers.

Baseline earnings composition is a predetermined sensitivity to unequal resources and associated exposure to other shocks. Coarse bins use valid 2006 capped wages at 100000, 250000 and 400000 DKK. These cut points are analyst categories, not tax brackets. Fine support requires both groups in each original industry/sex/age $\times$ wage-bin cell. Both specifications in Table S13 use exactly the same retained workers R: one standardizes the original cells again within R, the other standardizes the finer cells. The unchanged primary estimate uses the original target. Thus a comparison with the primary can involve support loss, while the paired specifications isolate the additional standardization within R. Neither agreement nor divergence identifies an omitted confounder, clears known recording breaks, or selects the headline model.

Table S13a. Support and weights for the paired composition comparison.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Specification	Group	Work- ers	CVRs	Retained fraction	Cells	Largest firm weight share
restricted base	g0	560	40	1.0000	2	0.0250
restricted base	g1	960	40	1.0000	2	0.0250
restricted base	pooled	1,520	80	1.0000	2	0.0125
earnings	g0	560	40	1.0000	4	0.0250
standardized earnings	g1	960	40	1.0000	4	0.0250
standardized earnings	pooled	1,520	80	1.0000	4	0.0125
standardized						

Counts are unweighted; group concentration divides by group absolute weight one, pooled concentration by total absolute contrast weight two. Retention divides R by the corresponding original population. No support is unavailable, not evidence of equivalence.

Table S13b. Paired baseline-composition sensitivity, 2011 minus 2006.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Endpoint	Specifica- tion	Lower	Upper	95% envelope lower	95% envelope upper	Disposition and limitation
injury	restricted base	-0.0010	0.0057	-0.0300	0.0358	strict pretrend
injury	earnings standardized	-0.0005	0.0058	-0.0297	0.0360	strict pretrend
employ- ment	restricted base	-0.0558	-0.0301	-0.1024	0.0156	employment 2008 ascertainment
employ- ment	earnings standardized	-0.0557	-0.0304	-0.1021	0.0151	employment 2008 ascertainment
wage	restricted base	-8,609	6,838	-12,345	9,715	strict pretrend; wage 2011 components
wage	earnings standardized	-8,413	6,785	-12,058	9,730	strict pretrend; wage 2011 components

The confidence envelopes are pointwise robustness summaries, not additional confirmatory tests or a test of equality between specifications. Unsupported uncertainty remains a bound with its reason. All economic rows retain their known-definition-break warning; no result from this table changes a primary local state.

The joint-state module uses the original group-standardized weights in 2006 and 2011. Injury and employment are each coded 0, 1 or U (unknown), yielding nine mutually exclusive observed cells. Table S14 reports the entire denominator, including unknown states. The binary-state lower bound requires both components observed and equal to the specified state; the upper bound admits any worker whose known component is compatible. No independence of missingness or between outcomes is assumed. The four upper bounds need not sum to one and cannot all be attained simultaneously. Figure 5 plots these bounds without midpoint estimates or sampling intervals. A whole-source absence makes the affected year unavailable, rather than assigning all people to U.

Table S14a. Joint recorded states in the original workforce (standardized proportions).

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Annual injury	November H/B job	2006, 10–19	2006, 20–34	2011, 10–19	2011, 20–34
0	0	0.1161	0.0885	0.0804	0.0979
0	1	0.8429	0.8687	0.8643	0.8479
0	U	0.0000	0.0000	0.0143	0.0094
1	0	0.0036	0.0042	0.0054	0.0052
1	1	0.0375	0.0385	0.0321	0.0344
1	U	0.0000	0.0000	0.0000	0.0021
U	0	0.0000	0.0000	0.0000	0.0000
U	1	0.0000	0.0000	0.0036	0.0031

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Annual injury	November H/B job	2006, 10–19	2006, 20–34	2011, 10–19	2011, 20–34
U	U	0.0000	0.0000	0.0000	0.0000

Table S14b. Joint-state observability and original denominators.

ILLUSTRATIVE MOCK DATA — NOT RESULTS

Year	Payroll group	Original workers	Known on both outcomes	Any unknown (weighted share)
2006	g0	560	560	0.0000
2006	g1	960	960	0.0000
2011	g0	560	550	0.0179
2011	g1	960	946	0.0146

Counts are unweighted and shares use fixed group weights. A validated available year has nine observed shares summing to one before disclosure suppression. These are finite-cohort descriptions, without p-values, effect classifications or causal timing claims. Their optional availability is independent of the injury Atom’s local judgment; the module requires the identical injury construction at person level, not its aggregate estimate. It cannot infer joint behavior from separate marginal output files.

Supplementary Information: interpretation, provenance and references

Local result regions separate evidenced data gaps, operational nonresults, unsupported cluster inference, fragility, small effects, directional contrasts and imprecision. Small-effect bands are $\pm.002$ for injury, $\pm.02$ for employment and $\pm 10,000$ DKK for capped wages. Small takes precedence when an interval lies entirely inside the band, even if its sign is positive or negative. Economic numerical profiles preserve both component keys and interval signs: gain/loss requires at least one higher/lower component while the other has the same classification or is small. A small component can have the opposite sign, so neither profile implies weakly upward/downward movement of both margins. These remain base profiles beneath the realized fragile/partial/data_gap/unresolved economic state; known recording breaks prohibit an unflagged causal-economic success state in this version. The Molecule registration is the authority for composing the corresponding prose.

The neutral title applies to every result. The final abstract and conclusion must name the realized differential target, interval or boundary, and all surviving scientific restrictions. The full component-sensitive wording and operational distinctions are registered in the result map and protocol; they are not alternative empirical conclusions printed together in this manuscript.

This is Blueprint 1 of the authorized pre-data institutional study. Baseline distributions, a paired earnings-composition comparison and descriptive same-person

injury/employment states were added within the existing three experiments before any study outcome access. The concise amendment record identifies the retained baseline and knowledge history. Published findings and historical designs are background, never results for this cohort. All empirical slots remain pending.

Institutional references: [L1262009/10](#); [LOV508/2010](#); [BEK1181/2010](#). Exact sections, access limits and historical source versions are documented in the institutional source record.

The complete source record is maintained in the accompanying literature file. The principal study and methodological references are:

- Pedersen, B. H., Hannerz, H., Christensen, U., and Tüchsen, F. (2011). Enterprise size and risk of hospital treated injuries among manual construction workers in Denmark: a study protocol. *Journal of Occupational Medicine and Toxicology*, 6, 11. DOI: 10.1186/1745-6673-6-11.
- Pedersen, B. H., Hannerz, H., and Christensen, U. (2012). The effect of enterprise size on the risk of hospital treated injuries among all male manual construction workers in Denmark, 2000–2006. *Safety Science Monitor*, 16(1), article 2.
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- Kreiner, C. T., Leth-Petersen, S., and Skov, P. E. (2016). Tax reforms and intertemporal shifting of wage income: evidence from Danish monthly payroll records. *American Economic Journal: Economic Policy*, 8(3), 233–257. DOI: 10.1257/pol.20140233.
- Roth, J. (2022). Pretest with caution: event-study estimates after testing for parallel trends. *American Economic Review: Insights*, 4(3), 305–322. DOI: 10.1257/aeri.20210236.
- Rambachan, A., and Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, 90(5), 2555–2591. DOI: 10.1093/restud/rdad018. This study uses a simpler fixed total-bias calculation, not that procedure.