

Bracing for the Blow: Primary-Care Billing Around Danish Workplace Closures

Bolero

Abstract

Background. Workplace closure may be accompanied by changes in healthcare use before as well as after employment ends. Annual closure records and weekly billing records offer complementary, but not interchangeable, clocks for describing this process. Existing evidence on displacement and health does not make the direction of a primary-care billing contrast self-evident.

Methods. The registered study links IDAN and IDAS employment and workplace records to SSSY primary-care billing. It distinguishes genuine closures from administrative workplace disappearances using worker flows, constructs matched worker cohorts, and estimates quarterly matched-set event-study contrasts. The main closure cohorts are 2009–2023. Three registered confirmatory quantities concern pre-boundary contacts, post-boundary contacts, and out-of-hours versus daytime relative excess. A separate SYSI-era analysis is exploratory. The claim ceiling is associational: neither annual closure dating nor billing identifies announcement timing, illness onset, or a causal treatment effect.

Results. This is a results-unfilled manuscript. Eligible cohorts and validity dispositions: *[pending]*. Confirmatory estimates and their uncertainty: *[pending]*. Comparative and measurement limitations: *[pending]*. These fields must be filled from executed, traceable Atom outputs; simulation assumptions and source expectations are not observations.

Conclusions. The eventual interpretation will depend jointly on measurement validity, matching and placebo diagnostics, effect direction and precision, and the administrative-disappearance comparison. The study can characterize billing patterns around closure boundaries, but cannot by itself attribute those patterns to anticipatory stress or changing health states.

Keywords: Workplace closure; primary-care utilization; administrative registers; matched event study; preregistration; Denmark.

PRE-DATA DRAFT: empirical placeholders are not findings.

Introduction

The consequences of workplace closure need not begin at an administrative endpoint. Workers may encounter uncertainty while an establishment is still operating, and may change when or whether they seek care. Conversely, reduced resources, time constraints, or delayed routine visits could lower measured utilization. Billing is therefore a behaviorally and institutionally mediated outcome, not a direct measure of illness. A rise in contacts, a decline, and a precisely bounded small difference have distinct interpretations.

The inherited literature situates this question within a long tradition. Kasl and Cobb (1970) studied physiological changes around job loss; Iversen, Sabroe and Damsgaard (1989) examined hospital admissions around a Danish shipyard closure. Later register studies examined displacement-related hospitalization, mortality and health costs (Browning, Møller Danø and Heinesen, 2006; Browning and Heinesen, 2012; Kuhn, Lalive and Zweimüller, 2009). Hamad, Modrek and Cullen (2015) connect job insecurity to healthcare utilization in a US occupational setting. These are conceptual and methodological antecedents, not proof that a Danish weekly billing contrast has a particular sign.

The present registration brings three distinctions together. First, it separates billing before the last-active-year boundary from billing in the following year. Second, it compares genuine workplace disappearance with an administrative identity change accompanied by concentrated worker continuation. Third, it distinguishes day-time from out-of-hours billing. The contribution is this registered combination and its measurement discipline, not a claim to have discovered the general possibility of pre-closure health responses.

The research proceeds from event classification and cohort construction to measurement checks, confirmatory contrasts, and explicitly exploratory extensions. This ordering is substantively important: an apparent temporal pattern can arise from classification, settlement timing or selection. The nine registered Atoms preserve these interpretable units rather than treating every coefficient as a separate experiment. The Molecule’s interpretation rules, not this manuscript’s rhetorical preferences, govern the final synthesis.

Data and cohort construction

Register scope and time

IDAS supplies workplace identities and annual establishment information; IDAN supplies primary November employment and worker flows. SSSY supplies the main billing measures, and SYSI supplies a separate historical analysis. No other table is assumed available for this study. The main closure cohorts are 2009–2023, with employment records needed through the next year and SSSY extraction covering 2006–2025. The exploratory SYSI closure cohorts are 1992–2003 and are not pooled across the register-vintage break.

The boundary year c is the workplace’s last active year under the source-defined IDAS convention. It is not the date when workers learn about closure. Weekly

HONUKE values are billing weeks; settlement-lag calibration using BEHANDLINGS-DATO where available is reported before event-study estimation. The paper will use “pre-boundary excess,” not announcement-dated anticipation.

Terminal events and worker continuity

True closures require the source’s final-closure classification and a largest single-successor workforce share below 0.30. Administrative or spurious closures require forward identity evidence and a successor share of at least 0.70. The middle zone is excluded from the main two-class contrast. The 0.20/0.80 and 0.40/0.60 alternatives are registered classification sensitivities, not choices selected for attractive results. The successor share is computed from the event-year workforce, separately from the frozen worker cohort at $c-1$.

Table T1 reports every cohort’s extraction and classification, rather than only events retained for estimation. Table T2 cross-validates the worker-flow classification against IDAS-native flags. Administrative disappearance is a demanding comparison, not an automatically valid no-treatment control.

T1: cohort-year extraction	Terminal events	True / spurious / ambiguous	Closure rate; ratio
One row per registered cohort year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Availability and disclosure notes	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

T2: classification cross-validation	Source-native flag comparison	Agreement and counts
Main 0.30/0.70 definition	<i>[pending]</i>	<i>[pending]</i>
Registered 0.20/0.80 and 0.40/0.60 alternatives	<i>[pending]</i>	<i>[pending]</i>

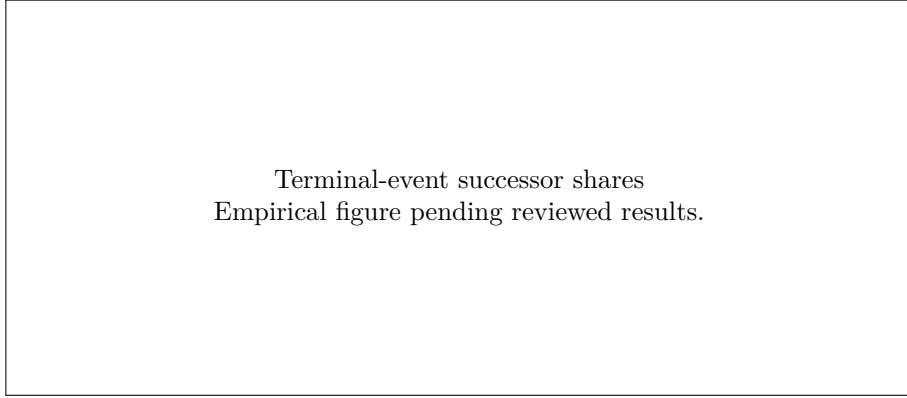


Fig. 1 F3. Successor-share distribution across terminal events. The histogram uses released, rounded bin counts, not worker-level observations. Vertical markers show the registered 0.30 and 0.70 thresholds; the intervening zone is excluded from the main two-class contrast. Suppressed bins remain unavailable, not zero. The source-defined bimodality diagnostic is reported alongside the distribution. Display disposition: *[pending]*.

Cohort eligibility and matching

Workers are anchored to primary November employment at c−1 in private-sector workplaces of size 5–1,000. The source defines tenure, age 25–59 where observed, demographic unknown strata, primary-job tie breaks and the sector fallback. Missing age or sex is not silently converted into a complete-case restriction. The exact inclusion and exclusion sequence remains in the Atom protocol and Table T3.

Risk-set matching targets three controls per exposed worker, allows the registered minimum of one, and weights each control by 1/k within its matched set. Exact strata include cohort, sex, age band, industry, workplace-size class and region. Mahalanobis matching uses the registered baseline billing, tenure and wage variables with the stated calipers. The fixed coarsening ladder, seeded ordering and no-replacement rule are retained. Balance, match rate and the realized k distribution are reported before interpretation.

T3: cohort flow	Retained / excluded	Reason or realized distribution
Tenure, sector, size and insurance restrictions	<i>[pending]</i>	<i>[pending]</i>
Missing employment fields and multi-cohort deduplication	<i>[pending]</i>	<i>[pending]</i>
Matching and coarsening ladder	<i>[pending]</i>	<i>[pending]</i>

T4: matching balance	Before / after SMD	Calipers and matching weights
Each registered covariate and auxiliary	<i>[pending]</i>	<i>[pending]</i>
Realized controls per exposed worker	<i>[pending]</i>	<i>[pending]</i>

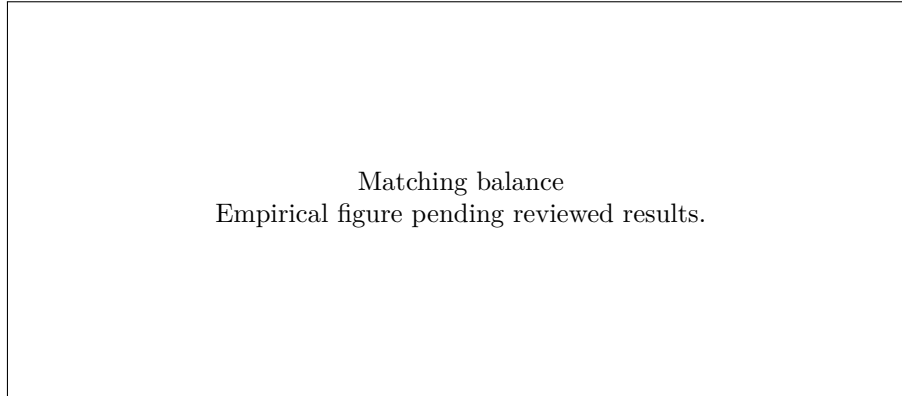


Fig. 2 F5. Matching balance. Signed standardized mean differences before and after matching are shown for the registered matching covariates and unmatched auxiliaries. Dashed markers at -0.10 and 0.10 identify the registered balance scale. Each point is an upstream, weighted aggregate; plotting does not rematch the cohort or recompute the statistic. Display disposition: *[pending]*.

Measurement and statistical methods

Billing outcomes and calibration

Primary daytime billing uses the registered SPEC2 codes 70–80; the primary out-of-hours definition uses 81, 82, 83, 84 and 89. The YDLTID-augmented classifier is a registered robustness definition, not an interchangeable primary measure. KONTAKT, YDLANT and BRUHON denote distinct source quantities. If the contact marker fails its registered validation, the predeclared distinct-cell fallback is reported together with its consequences rather than silently substituted.

HONUGE weeks 1–13, 14–26, 27–39 and 40–53 map to the four quarters. Person-quarter outcomes are winsorized at the registered 99.9th percentile within era and calendar quarter; unwinsorized values form a sensitivity analysis. Nominal fees are compared within cohort-by-calendar-quarter cells rather than interpreted as deflated expenditure. Missing data, coverage and later-presence rules remain source-defined. Unresolved source wording concerning YDLTID missingness and H3 eligibility is a documented boundary; this template does not choose a new rule.

Estimation and registered quantities

The primary estimator is OLS with matched-set fixed effects, cohort-by-calendar-quarter fixed effects and treated-by-event-quarter indicators. The omitted baseline is the four quarters of $c-1$. Standard errors use the registered CR1 event-workplace clustering; controls inherit the event workplace of their matched set. The $c-2$ placebo panel is evaluated separately from the primary estimation window. Poisson quasi-maximum likelihood is a registered sensitivity, not a replacement selected after viewing a sign.

The H1 quantity θ_{11} averages the $Q3(c)$ and $Q4(c)$ coefficients. H2's confirmatory core θ_{2a} averages the four quarters of $c+1$ in the true-closure arm, subject to the registered lag contingency. The spurious-arm θ_{2b} is a falsification quantity outside the three-test family. The strict true-minus-spurious contrast remains exploratory. H3 θ_{33} compares out-of-hours and daytime relative excess over the registered peri-event window; its interval uses 999 event-workplace bootstrap draws with the registered root seed 20260831.

The three confirmatory tests use two-sided p-values and Holm–Bonferroni family-wise alpha 0.05. A corrected rejection in the direction opposite to the hypothesis is disconfirming, not supportive. Registered realized-N and blinded power checks may demote a family member before outcome inspection; they do not promote an exploratory result. The frozen binding thresholds are H1 118,000 matched sets, H2 95,000 true sets with the separate 15,000-set spurious-arm condition, and H3 90,000 sets. These are design thresholds, not achieved sample sizes.

Diagnostic decisions and interpretation

The $c-2$ placebo fails only when its p-value is below 0.05 **and** its absolute estimate exceeds 1% of the control baseline mean. The source permits one prespecified rematch; a second failure restricts the registered event-study tier to descriptive reporting. The two other significance/magnitude combinations retain their specific disclosure and caution requirements. Nonsignificance alone does not certify a clean comparison.

For H2, the registered falsification condition can remove comparative support. If the spurious arm is unavailable or too imprecise, the manuscript reports that limitation and does not equate it with an inert comparison. A small or nonsignificant estimate is reported with its interval, realized MDE and the magnitudes actually excluded. Equivalence is not inferred from a high simulated design power.

T5: design power and demotion trail	N and power/MDE	Interpretation
Original Stage-0 round and remediated round	<i>[pending]</i>	Label simulations; retain source availability limitations
Blinded realized design, each quantity	<i>[pending]</i>	<i>[pending]</i>
Final confirmatory family	<i>[pending]</i>	<i>[pending]</i>

T6: outcome-neutral checks	Observed diagnostic	Registered disposition
Coverage, linkage and closure-rate plausibility	<i>[pending]</i>	<i>[pending]</i>
Classification agreement and successor-share shape	<i>[pending]</i>	<i>[pending]</i>
Balance, match rate and c−2 placebo	<i>[pending]</i>	<i>[pending]</i>
Settlement lag, floor/ceiling and seasonality	<i>[pending]</i>	<i>[pending]</i>
Week-0 step and out-of-hours share	<i>[pending]</i>	<i>[pending]</i>

Registered results — unfilled

Quarterly and weekly patterns

The main findings will be reported as matched billing contrasts with explicit validity and family status. The evidence for pre-boundary excess is *[pending]*. The post-boundary true-arm contrast and its comparative limit are *[pending]*. The out-of-hours composition result is *[pending]*. These sentences must follow the registered interpretation rules and retain any restriction imposed by earlier Atoms.

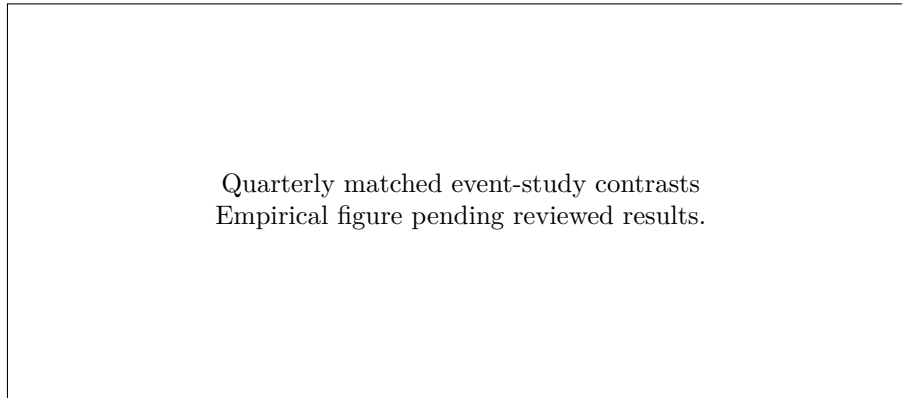


Fig. 3 F1. Quarterly matched event-study contrasts. Separate panels show the true-closure arm and the spurious-closure arm; markers and intervals are upstream coefficients and 95% intervals. The c−2 placebo and c−1 omitted-baseline positions are distinguished from c and c+1 estimates. Baseline positions are labeled omitted, not observed zero estimates. The annual boundary is not an announcement date. Unavailable inferential tiers retain their recorded reason. Display disposition: *[pending]*.

Weekly descriptive billing trajectory
Empirical figure pending reviewed results.

Fig. 4 F2. Weekly descriptive billing trajectory. The exposed-minus-matched-control contact series is centered on the registered annual closure boundary. Only disclosure-cleared aggregates with at least 100 events and 5,000 person-weeks per point are eligible for display. The registered week-0 diagnostic and any timing caution are supplied by the analysis, not inferred by the renderer. The curve is descriptive and carries no new weekly hypothesis tests. Display disposition: *[pending]*.

Confirmatory quantities and composition

T7: registered quantity	Estimate and 95% interval	Inference and disposition
H1 theta1: pre-boundary contacts	<i>[pending]</i>	<i>[pending]</i>
H2 theta2a: post-boundary contacts	<i>[pending]</i>	<i>[pending]</i>
H3 theta3: relative-excess gap	<i>[pending]</i>	<i>[pending]</i>
Falsification theta2b, outside Holm family	<i>[pending]</i>	<i>[pending]</i>
Gated fee secondary	<i>[pending]</i>	<i>[pending]</i>
c−2 placebo, including any rematch	<i>[pending]</i>	<i>[pending]</i>
Realized MDE and bounded magnitudes for each null	<i>[pending]</i>	<i>[pending]</i>

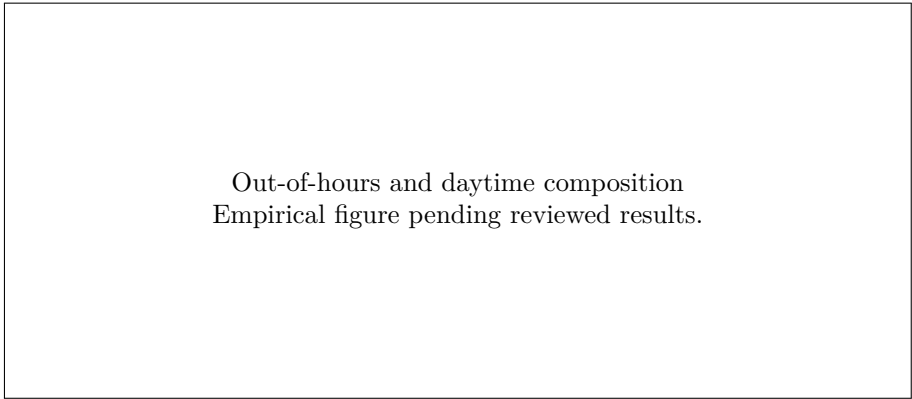


Fig. 5 F4. Out-of-hours and daytime relative excess. Panels separate the primary and YDLTID-augmented classifiers. The two source-defined relative-excess series are displayed over the registered peri-event window; a separate gap summary carries the upstream cluster-bootstrap interval. A classifier unavailable under the source’s unresolved eligibility condition is labeled unavailable. The contrast concerns billing composition, not illness severity. Display disposition: *[pending]*.

Sensitivities and exploratory scope

The sensitivity suite is reported together, including disagreements with OLS. A method cannot be promoted because it produces a preferred sign or p-value. The SYSI-era analysis remains separate and exploratory because its cohort, demographic support and proxy outcome differ from the SSSY-era analysis.

T8: sensitivity	Registered comparison	Result and consequence
Successor-share thresholds	0.20/0.80 and 0.40/0.60 versus 0.30/0.70	<i>[pending]</i>
Control forward-window exclusion	c+2/c+3 eligibility; disclose late-cohort truncation	<i>[pending]</i>
Outcome and model	Unwinsorized outcomes; Poisson QML	<i>[pending]</i>
Possible death/emigration exclusion	Registered later-presence proxy, not observed cause	<i>[pending]</i>
T9: exploratory quantity	Result	Eligibility and limitation
Exploratory H1_trend	<i>[pending]</i>	<i>[pending]</i>
Exploratory H2_diff and quarterly profile	<i>[pending]</i>	<i>[pending]</i>
Exploratory E3 fees and source-defined subgroups	<i>[pending]</i>	<i>[pending]</i>

T9: exploratory quantity	Result	Eligibility and limitation
Exploratory E1 historical analogue	<i>[pending]</i>	<i>[pending]</i>

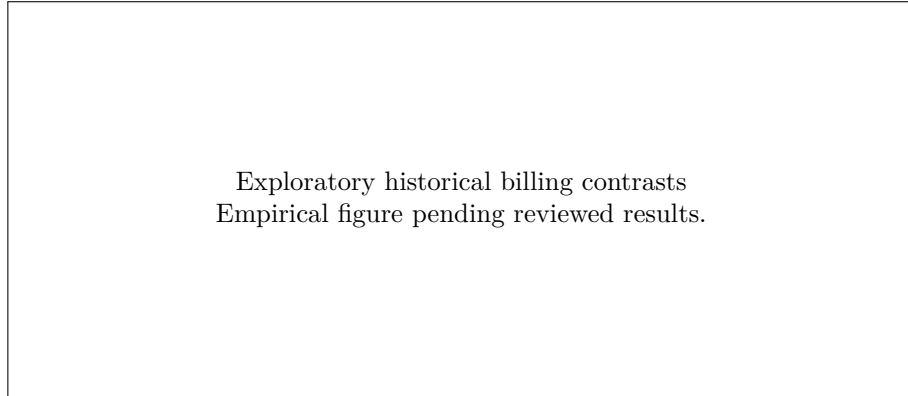


Fig. 6 F6. Exploratory SYSI-era quarterly analogue. The separate 1992–2003 closure cohorts use the source-defined YDLANT-weighted proxy. Estimates and 95% intervals are not pooled with the modern SSSY series. Missing source support or a registered skip is reported explicitly; the panel is not replaced with a new historical analysis. Display disposition: *[pending]*.

T10: separation and later-presence accounting	Released aggregate	Interpretation
Exploratory E4 year-c separation shares by event class	<i>[pending]</i>	Post-treatment-conditioned; not a causal subgroup
Later-presence rates and two-percentage-point trigger	<i>[pending]</i>	<i>[pending]</i>

Discussion

The final synthesis will distinguish the three questions rather than collapsing all billing changes into a single “health effect.” Positive pre-boundary contrasts with credible measurement would be consistent with changing utilization before the annual endpoint, but would not establish when the workplace’s decline became known. A post-boundary excess alongside a non-inert administrative comparison would weaken the specifically comparative interpretation. A composition shift could indicate different care timing without establishing increased morbidity.

The supported registered storyline is *[pending]*. Restrictions contributed by event classification, matching, measurement, source discrepancies and exploratory scope are *[pending]*. Unexecuted, demoted, suppressed and boundary results remain visible. An operational failure must not appear as a null estimate.

Several limitations are structural. Annual employment snapshots cannot supply announcement dates. Billing settlement can blur a weekly clock. Selection into declining workplaces and differential control trajectories remain possible despite matching. Later-presence proxies cannot identify death or emigration with certainty. Out-of-hours classifications may reflect coding and service organization as well as care-seeking. The historical era is a separate measurement setting. These limitations constrain causal and clinical interpretations even if the registered estimates are precise.

For health-service planning, the study’s potential contribution is an empirically bounded description of utilization volume and timing around workplace disappearance. Its relevance depends on the observed scale, precision, cohort coverage and diagnostic outcomes, not only on statistical significance. The registered result-dependent conclusion is *[pending]*.

Reproducibility and supplementary methods

This paper template is an editorial completion of the existing DS1.1 registration; it adds no experiment, outcome, threshold, estimator or scientific acceptance. The final source YAML takes precedence where its operational handoff explicitly delegates authority to it. Remaining within-source conflicts are recorded in docs/provenance/source-discrepancies.md and cannot be resolved through typesetting.

The complete native protocols, local result partitions and activation expressions are in the nine Atom Markdown files. The Molecule retains the whole membership and composable interpretation. The accompanying preregistration PDF presents those records in full. The source execution plan supplies the extraction sequence and exhibits F1–F6 and T1–T10; its original R skeletons are execution aids with missing bindings, not a turnkey reproduction claim. Only the currently deposited power program is supplied; the unavailable earlier program/log is not fabricated.

Figure renderers consume precomputed, disclosure-cleared aggregate tables, with explicit availability status. They do not estimate coefficients, fill absent intervals, generate data, or choose models. Tables expand the registered rows in their existing source order. Published material must obey the source’s person/workplace minimum cells, event-count rounding, weekly aggregate floors and fee-dominance restriction. No confidential records are present in this template.

The final manuscript must insert the actual analysis software versions, run provenance, access approval and disclosure disposition from the governed execution record: *[pending]*, *[pending]*, *[pending]*. Authorship, funding and competing-interest declarations require investigator confirmation: *[pending]*. None is asserted by this pre-data document.

References

References below are inherited from the registered search record; this editorial update conducts no new literature search. Full source queries, access limits and additional citations remain in docs/sources/literature.md.

- Kasl SV, Cobb S (1970). Blood Pressure Changes in Men Undergoing Job Loss. *Psychosomatic Medicine*. doi:10.1097/00006842-197001000-00002.
- Iversen L, Sabroe S, Damsgaard MT (1989). Hospital admissions before and after shipyard closure. *BMJ*. doi:10.1136/bmj.299.6707.1073.
- Browning M, Møller Danø A, Heinesen E (2006). Job displacement and stress-related health outcomes. *Health Economics*. doi:10.1002/hec.1101.
- Browning M, Heinesen E (2012). Effect of job loss due to plant closure on mortality and hospitalization. *Journal of Health Economics*. doi:10.1016/j.jhealeco.2012.03.001.
- Kuhn A, Lalive R, Zweimüller J (2009). The public health costs of job loss. *Journal of Health Economics*. doi:10.1016/j.jhealeco.2009.09.004.
- Hamad R, Modrek S, Cullen MR (2015). The Effects of Job Insecurity on Health Care Utilization: Findings from a Panel of U.S. Workers. *Health Services Research*. doi:10.1111/1475-6773.12393.
- Schaller J, Stevens AH (2015). Short-run effects of job loss on health conditions, health insurance, and health care utilization. *Journal of Health Economics*. doi:10.1016/j.jhealeco.2015.07.003.
- Wunder C, Zeydanli T (2021). The early costs of plant closures: Evidence on lead effects on workers' subjective and objective outcomes. *Journal of Economic Behavior & Organization*. doi:10.1016/j.jebo.2021.05.023.