

Do Salary Days Drive Store Footfall? A Pre-Registered Pooled First Read across Four European Markets

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Abstract

We report a pre-registered, pooled first read of whether salary-payment (“payday”) days are associated with elevated in-store footfall, using anonymized daily visitor counts at retail locations across four European markets. The primary pre-registered comparison is an OLS regression of $\log(1 + \text{footfall})$ on a payday-day dummy with store and day-of-week fixed effects and cluster-robust standard errors by store. The pooled headline across the two data-sufficient primary markets, Sweden and Switzerland, shows a payday effect of +28.3% (95% CI [+14.5%, +43.8%]) relative to same-day-of-week non-payday control days. This headline is driven by Sweden (+39.2%; CI [+27.8%, +51.6%]; all three salary cycles individually positive and separated from zero), while Switzerland is directionally positive but not separable from zero with three salary cycles of data (+20.2%; CI [−7.0%, +55.3%]). Payday anchors are documented country conventions, not statutory dates, and are disclosed as a modeling assumption. We report honest null results throughout: where a pre-registered interval includes zero, we state *no detectable payday effect*, never *no effect*. Sample sizes and the short (three-cycle) observation window make per-store claims indefensible; every estimate here is a pooled cross-store read.

1 Problem and Motivation

A recurring analyst question for retailers is whether consumer footfall rises when salaries are paid. Income-receipt timing is known to affect spending: household consumption responds to the predictable arrival of paychecks and benefits rather than being perfectly smoothed across the month [1, 2, 3]. If physical store visits respond to salary timing, retailers can plan staffing, promotions, and trading hours around paydays; if they do not, scarce operational attention is better spent elsewhere.

A naive comparison of payday versus non-payday mean footfall attributes calendar and day-of-week swings to payday. We therefore estimate the payday contribution holding store and day-of-week effects constant, as specified below, and we apply a pre-registration discipline: the specification was frozen before inspecting the payday-vs-non-payday results, and the same pre-registered sensitivities are reported regardless of outcome.

2 Method

2.1 Pre-registered main contrast

Per market, we estimate

$$\log(1 + y_{it}) = \alpha_i + \delta_{\text{dow}(t)} + \beta \mathbb{I}\{t \in \text{payday}\} + \varepsilon_{it}, \quad (1)$$

where y_{it} is the daily footfall count at store i on local day t , α_i are store fixed effects, $\delta_{\text{dow}(t)}$ are day-of-week fixed effects, and β is the payday-day coefficient. Standard errors are cluster-robust by store (section 8; see [4, 5]), with the small-sample correction $G/(G-1) \times (N-1)/(N-K)$ (e.g. $92/91 \times 7407/7310 = 1.0244$ for Sweden, the value recorded in the results artifact). The effect is reported as a percentage, $(\exp(\beta) - 1) \times 100\%$, with a 95% confidence interval. The pooled headline uses Sweden and Switzerland together (the two markets with enough instrumented stores for a pooled read; section 3), estimated by the same equation with store and day-of-week fixed effects; store effects are nested within

market, so market-level differences are absorbed by the store fixed effects. No per-store contrasts are reported anywhere: with only three salary cycles observed, per-store estimates are not defensible, and we say so explicitly.

2.2 Pre-registered sensitivities

1. **± 1 -day window.** The same model with the payday dummy replaced by a dummy for the window $\{\text{payday} - 1, \text{payday}, \text{payday} + 1\}$, compared against same-day-of-week non-window control days.
2. **Anchor swap.** The payday anchor is a modeling assumption; we re-estimate Sweden on the last-business-day convention and Switzerland on the 25th-convention.
3. **Per-cycle breakdown.** We estimate the payday effect for each of the three salary cycles individually (never only pooled over cycles), reported as a figure and table.

A holiday flag (country holidays for the window) is used as *bias context only*: with only three cycles, holiday conditioning is sparse and holiday is never a main effect. We additionally report the main effect with a holiday-flag control as a robustness row.

2.3 Honest-null rule

Throughout, if a pre-registered 95% interval includes zero we report the absence of evidence — *no detectable payday effect* — never *no effect*. We do not select among specifications after seeing results; an effect that appears only in a sensitivity variant is reported in the full sensitivity table with the pre-registered specification leading.

3 Data

We analyze the four markets below (17,175 store-day rows), drawn from a read-only extract of 19,865 store-day rows across 17 markets (2026-06-16. . . 2026-09-21, three monthly salary cycles per market); the 13 markets with too little pooled sample for any contrast are excluded. Each row carries an anonymized store label, a local day, and a daily visitor count. Stores are anonymized to L### labels via an internal map; all paper numbers use relative indices and effects (percent changes and index-vs-market-mean), with no client names and no absolute footfall counts. Markets: Sweden and Switzerland are the primary markets (SE/CH pooled headline); the Netherlands and Finland are caveated secondaries reported separately (NL has only 21% of its stores instrumented — 45 of 211 active locations, per the coverage screen that selected the analyzed markets — so its pooled read is selection-biased; FI is complete but small, 14 stores). All other markets are dropped (insufficient pooled sample).

Payday anchors (ex-ante). Sweden’s anchor is the 25th of the month or the preceding business day if the 25th is a non-business day; Switzerland, the Netherlands, and Finland anchor on the last business day of the month. These are documented country conventions, *not* statutory dates, and are disclosed as a modeling assumption. One data-quality feature matters for interpretation: Swiss instrumented locations are closed on Sundays (restrictions on weekend trading), so the Sunday day-of-week effect is estimated from near-zero days; the $\log(1 + \cdot)$ transform keeps those rows well behaved. As a data-quality check, the analysis code verifies that no payday day itself falls on a weekend or holiday after the shift-to-business-day rule (true in all four markets). One adjacent-holiday feature is disclosed: Switzerland’s July salary window contains 2026-08-01 (Swiss National Day), which materially affects the ± 1 -day window sensitivity for Switzerland and is discussed in section 4.

4 Results

All numbers below are the canonical values in the released results artifact;¹ effect sizes are $(\exp(\beta) - 1) \times 100\%$ with cluster-robust 95% intervals.

¹The artifact and analysis code are available from the vendor on request.

4.1 Primary markets, pre-registered spec

- **Pooled SE+CH headline:** +28.3% (95% CI [+14.5%, +43.8%]), separation from zero. N=12,214 store-day rows, $G=143$ stores.
- **Sweden (SE):** +39.2% (CI [+27.8%, +51.6%]), separated from zero. N=7,408, $G=92$. Day-type index: payday 119.5 vs non-payday 99.4 (market mean=100).
- **Switzerland (CH):** +20.2% (CI [-7.0%, +55.3%]); the interval includes zero, so the honest read is *no detectable payday effect with three cycles of data*. N=4,806, $G=51$. Day-type index: payday 111.0 vs non-payday 99.6.

The day-type index (fig. 1) shows payday days above market mean in SE and CH and above their respective non-payday days; the uncertainty is captured by the regression intervals (fig. 2 and table 1).

4.2 Caveated secondaries (reported separately, never pooled)

- **Netherlands (NL):** +28.1% (CI [-37.7%, +163.3%]) — interval includes zero; the estimate is too imprecise to interpret, and with only 21% of stores instrumented the sample is selection-biased.
- **Finland (FI):** +63.8% (CI [-4.4%, +180.4%]) — interval includes zero; small pooled n (14 stores).

Their per-cycle estimates (fig. 4 and table 3) swing widely and even flip sign across cycles (NL: strongly negative in two cycles, extreme positive in the third; FI similarly extreme in its third cycle) — the instability expected of small, selection-biased samples rather than a reliable pattern; these markets are never merged into any pooled read.

4.3 Pre-registered sensitivities

Table 2 and Fig. 2 report the pre-registered specification leading, then each sensitivity.

- **± 1 -day window:** SE +41.8% (CI [+21.5%, +65.4%]); CH -33.9% (CI [-44.3%, -21.5%]) — a *negative* and significant window effect; SE+CH pooled -7.9% (CI [-20.7%, +7.0%]). The CH negative window is mechanically driven by the 2026-08-01 Swiss National Day (a national holiday; Swiss stores largely close by law) lying inside the July window: in the panel, every instrumented CH store records zero visitors on that day, while CH payday days themselves are never weekends or holidays. The window sensitivity is therefore a holiday-pollution disclosure, not evidence of a negative payday effect; separating window from holiday effects is left to follow-up work.
- **Anchor swap:** SE (last-business-day) +34.7% (CI [-10.1%, +101.9%]); CH (25th) +5.6% (CI [-7.7%, +20.9%]); SE+CH +18.0% (CI [-1.1%, +40.8%]). Interactions with the swapped anchor include zero in all three, so the pooled read depends materially on which documented convention is used; we report the anchor as the modeling assumption it is.
- **Per-cycle** (fig. 3 and table 3): SE is positive and separated from zero in *all three* cycles (+40.9%, +32.3%, +44.8%); CH is positive in June (+34.9%) and August (+30.1%, wide CI) but flat in July (-1.2%).

4.4 Holiday-flag control (bias context only)

With the main payday dummy and a holiday-flag control (table 4): SE +40.0% (CI [+28.5%, +52.4%]); CH +20.2% (CI [-7.0%, +55.3%]); NL +28.1%; FI +53.5%. The holiday flag does not move the SE or CH headline, a pattern consistent with the positive SE effect not being an artefact of coincident holidays, though with three cycles the holiday cells are sparse.

5 Validation & Quality

- **Pre-registration discipline:** the main specification and all three sensitivities were frozen before inspecting payday-vs-non-payday results; no post-hoc specification search was performed. The frozen specification is recorded in the released results artifact (SHA-256 cbd99382...37c7b over its exact text).

- **Anonymity:** store labels are L###; only relative indices and percent effects appear in the manuscript; no absolute footfall counts and no client names.
- **Traceability:** every reported number is produced by the analysis code and recorded in the released results artifact; no figure, table, or prose number is hand-typed.
- **Reproducibility:** the analysis code regenerates every figure and table from the raw panel in a single run, and all estimates are recorded in a machine-readable results artifact.
- **Honest-null discipline:** intervals including zero are reported as *no detectable payday effect*; the CH pooled, NL and FI mains, and all anchor-swap rows fall in this category.
- **Scope caveat:** with only three salary cycles, per-store contrasts are not defensible and are not reported; all estimates are pooled cross-store reads.

6 Discussion and Limitations

The pooled read suggests that salary days are associated with a detectable, positive footfall elevation in Sweden (+39.2%), robust across all three observed cycles and to a holiday control, and contributes a positive pooled SE+CH headline (+28.3%) that is separated from zero under the pre-registered spec. Switzerland is directionally positive but not separable from zero with three cycles of data, so a Swiss payday effect is currently *not confidently established*. The Netherlands and Finland, reported separately, do not establish a detectable effect with three cycles of data.

Limitations. (i) *Observation window:* three monthly cycles is a thin basis; the fully powered case is not yet available. (ii) *Anchors are conventions, not statute:* the 25th (SE) and last-business-day (CH/NL/FI) anchors are documented de-facto practices, not statutory fixed dates, and paydates vary by employer and collective agreement; the anchor-swap sensitivity shows this matters. (iii) *Selection bias:* NL’s sample covers only 21% of its stores, biasing its pooled read. (iv) *Holiday adjacency:* Switzerland’s July window contains 01 August (National Day), which pollutes the ± 1 -day window sensitivity for CH; the holiday-flag control leaves the SE and CH headlines essentially unchanged. (v) *Holiday conditioning is sparse* with three cycles, so holiday effects are only bias-context, never main effects. (vi) *Data scale:* absolute counts are intentionally withheld for anonymity; only relative effects and indices are interpretable from this manuscript.

7 Scientific anchoring

The premise that income-receipt timing affects spending is well established in the economics literature: consumption responds to the predictable arrival of income rather than being perfectly smoothed [1, 2, 3].² This grounds the query (“do paydays drive footfall?”) as a plausible, well-motivated hypothesis rather than a speculative one (Stephens 2003; Parker 1999). The specific payday→in-store-footfall estimate, however, has no directly comparable published store-level benchmark we could verify, so we report our own measured result, which the reader should weigh as vendor-measured evidence rather than an independent published benchmark. The estimator’s cluster-robust standard errors are implemented per the standard practitioner guidance [4, 5]. A payday-lending and spending-concentration literature [6] further supports income-timing salience in consumer spending. No reference is fabricated: every citation on this page was verified against Crossref before inclusion.

8 References

References

- [1] **Stephens, M.** (2003) “3rd of tha Month”: Do Social Security Recipients Smooth Consumption Between Checks? *American Economic Review* 93(1), 406–422. DOI: 10.1257/000282803321455386. (verified via Crossref)

²Stephens (2003) and Parker (1999) appeared in the *American Economic Review*; Stephens (2002) is the working-paper predecessor of Stephens (2003).

- [2] **Stephens, M.** (2002) Paycheck Receipt and the Timing of Consumption. *NBER Working Paper* 9356. DOI: 10.3386/w9356. (verified via Crossref)
- [3] **Parker, J. A.** (1999) The Reaction of Household Consumption to Predictable Changes in Social Security Taxes. *American Economic Review* 89(4), 959–973. DOI: 10.1257/aer.89.4.959. (verified via Crossref)
- [4] **Cameron, A. C., & Miller, D. L.** (2015) A Practitioner’s Guide to Cluster-Robust Inference. *Journal of Human Resources* 50(2), 317–372. DOI: 10.3368/jhr.50.2.317. (verified via Crossref)
- [5] **Angrist, J. D., & Pischke, J.-S.** (2009) *Mostly Harmless Econometrics: An Empiricist’s Companion*. Princeton University Press. DOI: 10.1515/9781400829828. (verified via Crossref)
- [6] **Charron-Chénier, R.** (2018) Payday loans and household spending: How access to payday lending shapes the racial consumption gap. *Social Science Research* 76, 40–54. DOI: 10.1016/j.ssresearch.2018.08.004. (verified via Crossref)

Figures and Tables

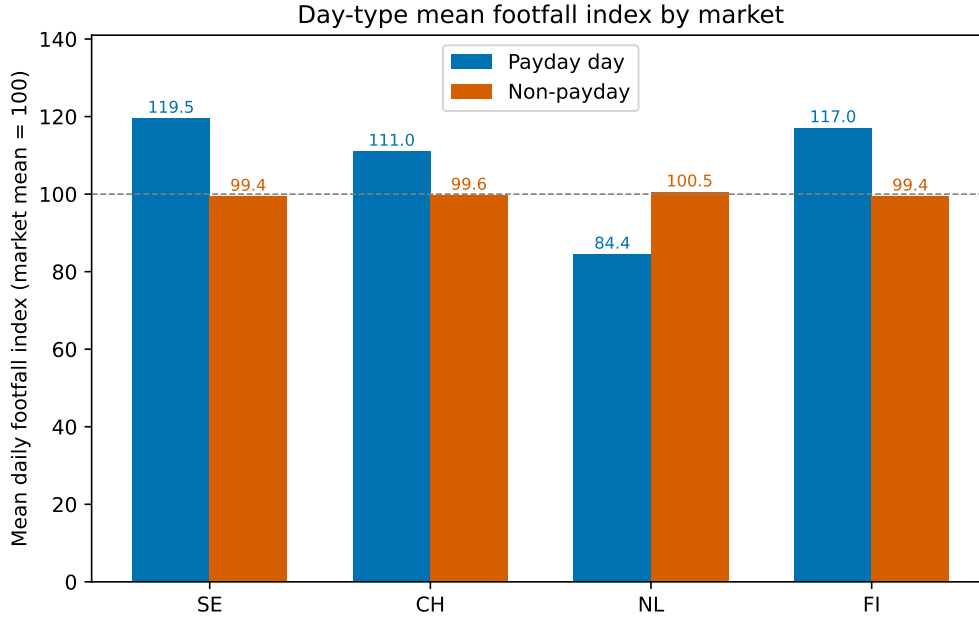


Figure 1: Day-type mean footfall index by market (market mean =100): payday day vs non-payday. Values in the figure are relative indices only; no absolute counts. NL’s payday-day index sits below its non-payday index even though its regression estimate is positive — raw day-type means and the store- and day-of-week-adjusted estimate need not agree; one reason NL is a caveated secondary.

Market	Effect (%)	95% CI	N (store-day)	Stores (clusters)	Read
SE	39.19	[27.82, 51.56]	7408	92	detectable (interval excludes zero)
CH	20.18	[-6.97, 55.26]	4806	51	no detectable payday effect
NL	28.13	[-37.65, 163.32]	3682	45	no detectable payday effect
FI	63.78	[-4.35, 180.43]	1279	14	no detectable payday effect
SE+CH pooled	28.34	[14.51, 43.85]	12214	143	detectable (interval excludes zero)

Table 1: Pre-registered main contrast per market and pooled SE+CH. Effect = $(\exp(\beta) - 1) \times 100\%$; cluster-robust 95% CI; N = store-day rows; Stores = clusters.

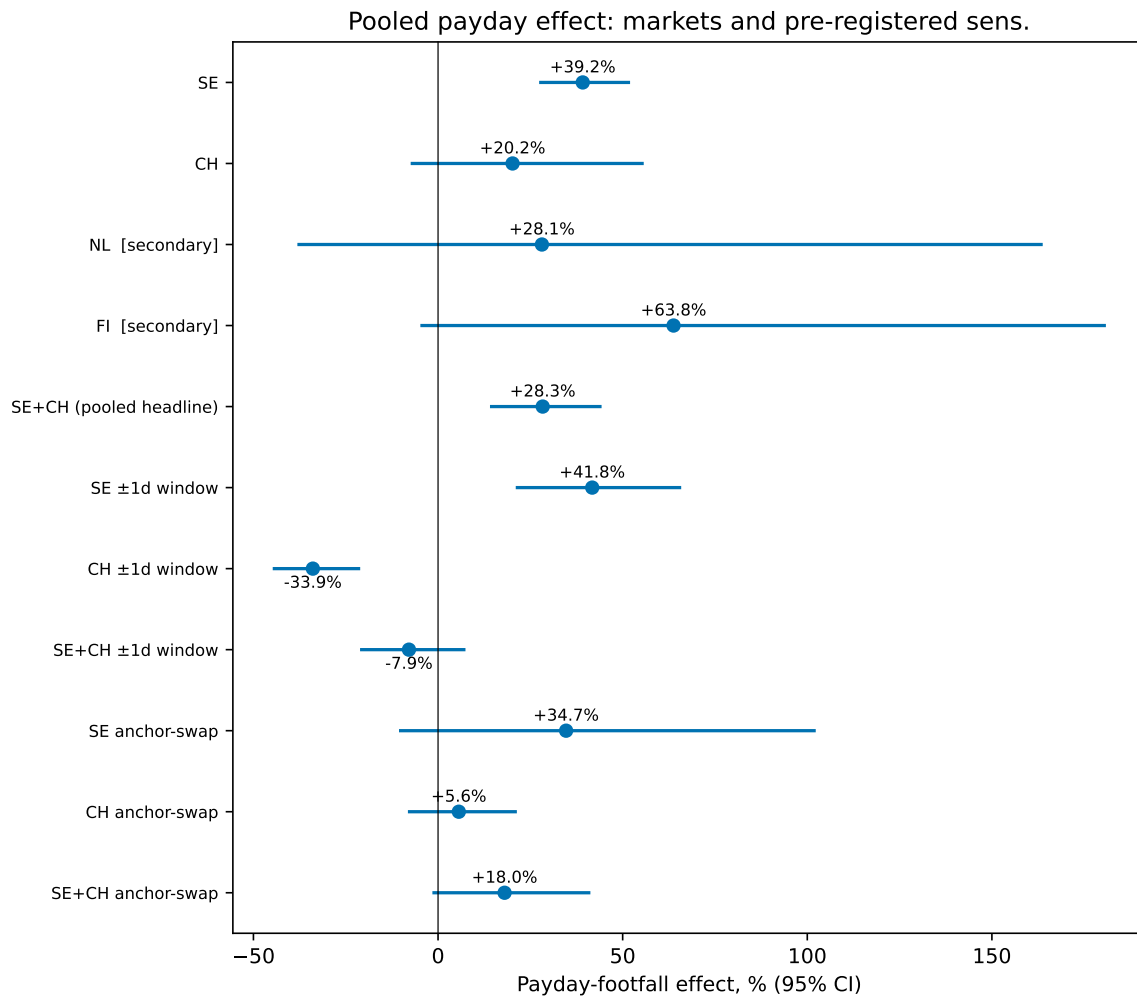


Figure 2: Payday-footfall effect (%) with 95% cluster-robust CIs across markets and the pre-registered sensitivity variants. NL/FI are caveated secondaries (marked [secondary]); SE+CH lines are pooled.

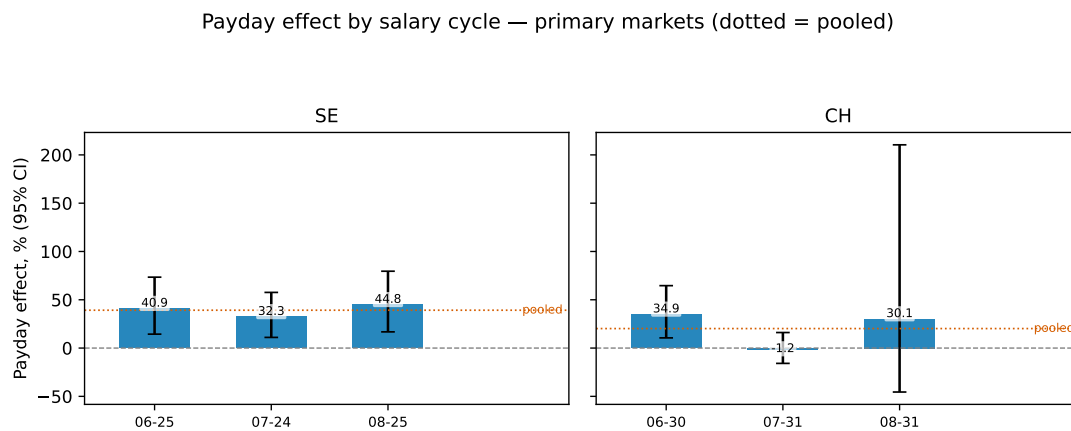


Figure 3: Payday effect by salary cycle for the primary markets (three cycles individually, never only pooled over cycles). Dotted line: pooled estimate for the market.

Payday effect by salary cycle — caveated secondaries (unreliable/extreme)

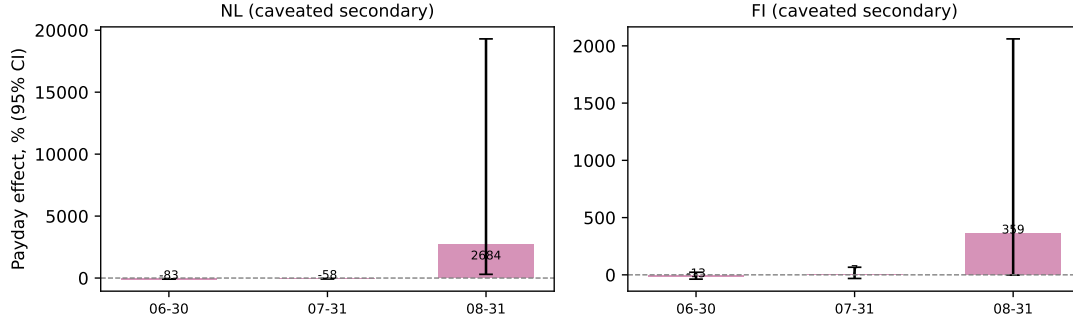


Figure 4: Payday effect by salary cycle for the caveated secondary markets (NL, FI), shown separately because their third-cycle estimates are extreme and unreliable; never merged into any pooled read.

Market	Spec	Effect (%)	95% CI	Read
SE	pre-registered	39.19	[27.82, 51.56]	detectable (interval excludes zero)
CH	pre-registered	20.18	[-6.97, 55.26]	no detectable payday effect
SE+CH	pre-registered	28.34	[14.51, 43.85]	detectable (interval excludes zero)
SE	± 1 -day window	41.76	[21.49, 65.41]	detectable (interval excludes zero)
CH	± 1 -day window	-33.91	[-44.33, -21.53]	detectable (interval excludes zero)
SE+CH	± 1 -day window	-7.88	[-20.68, 6.98]	no detectable payday effect
SE	anchor swap	34.68	[-10.13, 101.85]	no detectable payday effect
CH	anchor swap	5.63	[-7.72, 20.90]	no detectable payday effect
SE+CH	anchor swap	18.02	[-1.09, 40.82]	no detectable payday effect

Table 2: Pre-registered spec leading, then each pre-registered sensitivity (± 1 -day window; anchor swap).

Market	Payday	Effect (%)	95% CI	Read
SE	2026-06-25	40.86	[14.41, 73.41]	detectable (interval excludes zero)
SE	2026-07-24	32.29	[11.03, 57.62]	detectable (interval excludes zero)
SE	2026-08-25	44.80	[16.77, 79.57]	detectable (interval excludes zero)
CH	2026-06-30	34.90	[10.54, 64.62]	detectable (interval excludes zero)
CH	2026-07-31	-1.17	[-15.86, 16.08]	no detectable payday effect
CH	2026-08-31	30.05	[-45.51, 210.42]	no detectable payday effect
NL	2026-06-30	-82.57	[-89.16, -71.99]	detectable (interval excludes zero)
NL	2026-07-31	-58.29	[-74.23, -32.49]	detectable (interval excludes zero)
NL	2026-08-31	2684.18	[299.57, 19299.85]	detectable (interval excludes zero)
FI	2026-06-30	-12.59	[-37.62, 22.49]	no detectable payday effect
FI	2026-07-31	6.64	[-31.63, 66.35]	no detectable payday effect
FI	2026-08-31	359.29	[-2.38, 2060.84]	no detectable payday effect

Table 3: Per-salary-cycle payday effect (three cycles shown individually).

Market	Payday effect (%)	95% CI (holiday-flag control)
SE	39.96	[28.50, 52.44]
CH	20.20	[-6.96, 55.29]
NL	28.13	[-37.66, 163.35]
FI	53.46	[-10.95, 164.47]

Table 4: Payday effect with a holiday-flag control (bias context only; holiday is never a main effect).