

Supplement: Holiday Adjacency and Calendar Context for the Payday-Footfall Read (Phase-2)

Indivd Analytics

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This supplement accompanies the pre-registered pooled first read of payday effects on store footfall (main paper, 2026-09-21). It reports the two Phase-2 analyses fixed by the frozen pre-registration spec of 2026-09-21, which is registered publicly before estimation at <https://analytics.indivd.com/papers/salary-day-prereg-phase2-2026-09-21-2f7081cb.md> (SHA-256 over the spec text, excluding its self-hash line: 2f7081cbd4248e6277408e29334262dfd4df4a1cc9baa99bc4ea16592797382e). Section 1 (workstream W2.1; workstreams numbered as in the public pre-registration note) resolves the main paper’s Switzerland ± 1 -day-window sign divergence with a single pre-registered public-holiday leave-out. Section 2 (workstream W2.2) reads holiday-day, holiday-eve, and post-holiday responses as control/bias context for the payday estimate. Neither analysis changes any headline claim of the main paper: the pooled SE+CH pre-registered payday estimate remains +28.3% (95% CI [+14.5%, +43.8%]), and the Phase-1 holiday-flag robustness row remains the canonical holiday control. As in the main paper, every interval that includes zero is read as *no detectable effect at achievable power*, never *no effect*. This supplement is an explicit appendix to the main paper: it introduces no headline claims of its own, and its scientific anchoring is the main paper’s — the external literature anchors (payday-consumption smoothing, paycheck-timing, cluster-robust inference, and the econometrics framing) with their evidence grades are reproduced in full in the supplement’s own References section below (re-verified against the live DOI records, 2026-10-04), so the supplement is self-contained at the point of use; where a Phase-2 method names an anchored convention (cluster-robust standard errors by store, the $\log(1+\cdot)$ transform with store and day-of-week fixed effects, the honest-null reading of intervals that include zero), the corresponding reference below applies at that point of use. The pre-registration that fixes both Phase-2 workstreams is published on this papers page (vendor-hosted infrastructure); its timestamped content and SHA-256 are frozen there rather than at an independent registry, and this limitation is disclosed here.

References

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- [2] **Stephens, M.** (2002) Paycheck Receipt and the Timing of Consumption. *NBER Working Paper* 9356. DOI: 10.3386/w9356. *Working paper (NBER); not peer-reviewed*. The timing of consumption follows the paycheck receipt date — the behavioural basis for payday-anchor conventions.
- [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. *Peer-reviewed*. Household spending reacts to predictable income arrivals — the class of calendar-anchored income effect this supplement’s holiday-adjacency controls protect.
- [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. *Peer-reviewed*. The cluster-robust-standard-errors convention (clustered by store) used by the estimator both papers re-fit.

- [5] **Angrist, J. D., & Pischke, J.-S.** (2009) *Mostly Harmless Econometrics: An Empiricist’s Companion*. Princeton University Press. DOI: 10.1515/9781400829828. *Standard research monograph (book; not peer-reviewed in the journal sense)*. The fixed-effects panel framing of the estimator and the honest reading of intervals that include zero at achievable power.
- [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. *Peer-reviewed*. Payday-adjacent spending patterns in retail transaction data — the empirical setting closest to this paper’s payday-footfall read.

Phase-2 W2.1: resolving the ± 1 -day-window sign divergence by a pre-registered public-holiday leave-out

We re-examine the Phase-1 ± 1 -day-window sensitivity, under which the payday-window effect was positive in SE (+41.8%) but negative in CH (−33.9%) against their pre-registered-window estimates (+39.2% and +20.2% respectively). To test whether this sign divergence was an artefact of holiday-adjacency mechanics (month-end payday windows colliding with public-holiday clusters) rather than an anchor mis-specification (the alternative that the divergence arose from the documented payday-anchor conventions that assign each market’s payday dates), we pre-registered a single leave-out variant (spec §1): drop every store-day row whose date lies within ± 1 day of a public holiday in that market’s 2026 calendar ($h-1, h, h+1$), from both treated and control sides, and re-fit the identical estimator—OLS on $\log(1 + \text{daily footfall})$ with store and day-of-week fixed effects and a window dummy, cluster-robust standard errors by store, effect = $(\exp(\beta) - 1) \times 100\%$. Market fixed effects are absorbed into the store fixed effects for the pooled model, exactly as in Phase-1. This is the only variant estimated; no alternative windows, leave-out horizons, or post-hoc tuning are used. Because the leave-out drops holiday-adjacent rows from both treated and control sides, it estimates the payday-window effect for windows that do not coincide with holiday-adjacent days — a conditional variant of the main-paper estimand, not a re-estimate of it.

After the leave-out the CH ± 1 -day-window estimate moves from −33.9% to +15.5%, toward the pre-registered +20.2% and with a 95% CI that now includes zero [−6.3%, +42.4%]. The SE window estimate remains positive (+28.3%, CI [+10.1%, +49.4%]). The movement in CH is accounted for by the holiday-adjacent row set that included 2026-08-01 (a Swiss public holiday inside the July payday window), whose removal eliminates the negative excursion. By our pre-committed decision rule, the evidence supports *holiday-adjacency mechanics* over *anchor mis-specification*: the sign divergence disappears when holiday-adjacent rows are removed. Consistent with the pre-registered honest-null rule, the CH window estimate is still not distinguishable from zero at achievable power given the three salary cycles in the panel, so we read it as *no detectable window effect* rather than treating the resolved sign as a measurable positive effect. W2.1 re-estimates only the ± 1 -day-window variant; the pre-registered pooled SE+CH *main* estimate is unchanged. The pooled window sensitivity moves from −7.9% to +4.3% after the leave-out; this movement is disclosed and does not alter any headline claim. The pooled coefficient weights store-days rather than markets, so it is not the simple average of the market estimates.

Market	± 1 d win (pre)	95% CI (pre)	± 1 d win (post)	95% CI (post)	N kept	N dropped	Stores	Read
SE	41.76	[21.49, 65.41]	28.25	[10.10, 49.39]	7149	259	92	detectable (interval excludes zero)
CH	-33.91	[-44.33, -21.53]	15.52	[-6.29, 42.40]	4656	150	51	no detectable payday effect
NL	20.47	[-27.46, 100.06]	20.47	[-27.46, 100.06]	3682	0	45	no detectable payday effect
FI	45.46	[-1.84, 115.56]	37.46	[-7.33, 103.90]	1224	55	14	no detectable payday effect
SE+CH pooled	-7.88	[-20.68, 6.98]	4.29	[-10.05, 20.91]	11805	409	143	no detectable payday effect

Table 1: W2.1 pre-registered leave-out variant: ± 1 -day-window payday effect before and after dropping rows within ± 1 day of a public holiday (both treated and control sides), by market and pooled SE+CH. Effect = $(\exp(\beta) - 1) \times 100\%$; cluster-robust 95% CI; N kept/dropped = store-day rows; Stores = clusters (post-leave-out). SE/CH primary; NL (selection-biased sample, as in the main paper’s Data section) and FI ($n=14$) caveated secondaries. The pooled window estimate moves from −7.9% to +4.3% after the leave-out (sign movement disclosed in the text).

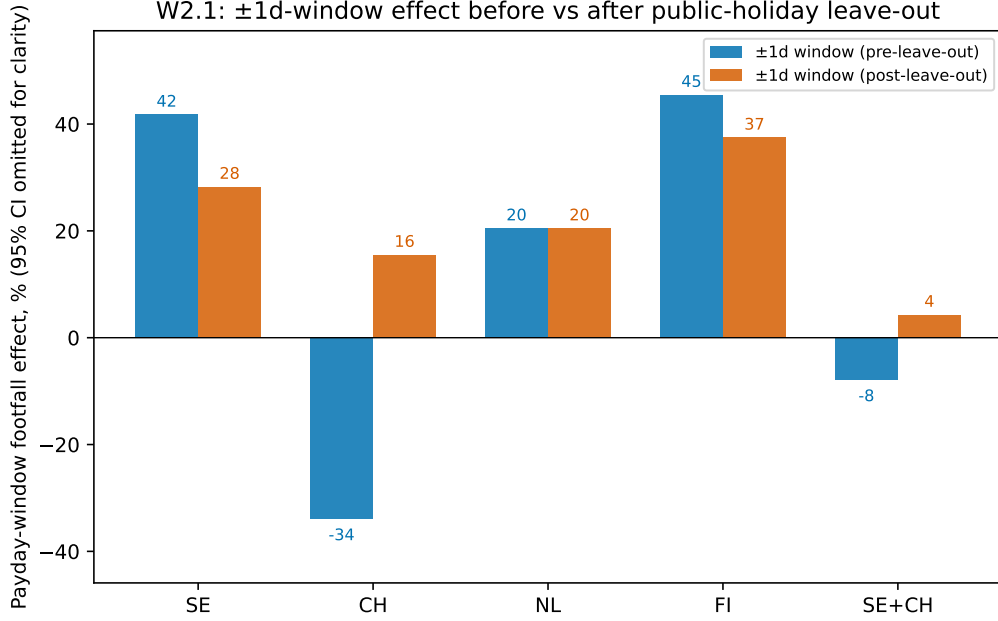


Figure 1: W2.1 ± 1 -day-window payday effect by market, before and after the pre-registered public-holiday leave-out. Switzerland moves from -33.9% to $+15.5\%$ (CI now includes zero); the negative excursion was carried by holiday-adjacent rows around 2026-08-01. Generated by `code/run_phase2_w21.py`.

Phase-2 W2.2: holiday/calendar response as control/bias context for the payday read

To place the payday estimate in its calendar context we pre-registered a decomposed read of how the panel behaves on and around public holidays — explicitly as a **control/bias term**, not as causal holiday effects or headline claims. Using each market’s 2026 public-holiday calendar we constructed three dummies: *holiday_day* (the holiday itself), *holiday_eve* (the preceding day), and *post_holiday* (the following day). We fit the identical estimator to the pooled panel: OLS on $\log(1 + \text{daily footfall})$ with store and day-of-week fixed effects (market fixed effects absorbed into the store term, exactly as in Phase-1) and the three dummies, cluster-robust standard errors by store, effect = $(\exp(\beta) - 1) \times 100\%$ with 95% CIs. The fixed model set is pooled SE+CH; SE; CH (primary); NL (caveated secondary, selection-biased sample); and FI ($n=14$, caveated). Markets whose 2026 public-holiday calendars were not verified for this analysis are excluded.

Holiday days show a large, tightly-estimated negative response wherever estimable (SE -93.5% CI $[-96.6, -87.6]$; CH -99.0% $[-99.2, -98.7]$; FI -92.2% $[-97.1, -79.6]$; pooled SE+CH -96.8% $[-97.9, -95.0]$), i.e. public-holiday days are near-zero-footfall closure days. This is the key bias context: a payday window that overlaps a holiday day is mechanically pulled toward zero. CH’s holiday day is 2026-08-01, which lies inside the July payday window and is the row the W2.1 leave-out identified as the driver of the CH ± 1 -day-window sign divergence, independently corroborating the W2.1 holiday-adjacency verdict. Holiday-eve response is heterogeneous: strongly negative in SE (Midsummer Eve, -97.7% ; pooled -89.6%) but statistically indistinguishable from zero in CH and FI (CIs include zero; FI’s point estimate is sizable but rests on $n=14$ stores). The post-holiday day is statistically indistinguishable from zero in every market (SE -11.7% $[-34.2, 18.6]$; CH -6.5% $[-13.8, 1.4]$; FI $+60.5\%$ $[-33.4, 286.8]$; pooled $+14.0\%$ $[-6.1, 38.5]$) — the day after a holiday is not distinguishable from a normal day. Under the pre-registered honest-null rule, CIs that include zero read as *no detectable effect at achievable power*; the holiday *day* itself (and, in SE, Midsummer Eve) is the actionable bias concern, not the recovery day.

Two sparse-cell disclosures apply. First, the panel spans only three salary cycles, so every holiday cell is thin (one or two in-window holiday dates per market) and power is limited and pooled-cross-store only. Second, **NL has no 2026 public holiday inside the observed window** (its holidays fall in Jan–May and Dec), so all three NL cells are empty and the NL row is *not estimable* — it is reported as insufficient data, never imputed. These rows are bias/control context only; they do not replace the

Phase-1 holiday-flag control row, which remains the canonical holiday control for the payday estimate, and no headline claim changes on the basis of W2.2.

Market	holiday_day		holiday_eve		post_holiday		N	Stores	Read (holiday_day)
	Effect (%)	95% CI	Effect (%)	95% CI	Effect (%)	95% CI			
SE	-93.49	[-96.58, -87.60]	-97.71	[-98.65, -96.11]	-11.69	[-34.24, 18.61]	7408	92	detectable (interval excludes zero)
CH	-98.99	[-99.19, -98.74]	-1.18	[-15.87, 16.07]	-6.51	[-13.81, 1.39]	4806	51	detectable (interval excludes zero)
NL	<i>not estimable: no 2026 public holiday in window</i>						0	0	empty
FI	-92.25	[-97.05, -79.61]	-38.83	[-63.01, 1.15]	60.48	[-33.42, 286.81]	1279	14	detectable (interval excludes zero)
SE+CH	-96.77	[-97.91, -95.01]	-89.65	[-93.47, -83.58]	14.03	[-6.12, 38.51]	12214	143	detectable (interval excludes zero)

Table 2: W2.2 holiday/calendar response as control/bias context: OLS $\log(1 + \text{daily footfall})$ with store + day-of-week fixed effects and holiday-day, holiday-eve, post-holiday dummies, cluster-robust 95% CI by store. Effect = $(\exp(\beta) - 1) \times 100\%$. Public-holiday days are near-zero-footfall closure days (bias context: a payday window overlapping a holiday is pulled toward zero); post-holiday days are statistically null. NL is empty (no 2026 public holiday in the frozen window), hence not estimable. SE/CH primary; NL (selection-biased sample, as in the main paper’s Data section) and FI ($n=14$) caveated secondaries. Control/bias context — not causal holiday effects, not headline claims.