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# Birth timing and delivery-mode differences in next-day neonatal weight loss: a retrospective nursing-record study

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## Abstract

# Background

Routine next-calendar-day weighing does not ensure equal postnatal observation intervals. We examined whether delivery-mode differences in recorded neonatal weight loss changed after accounting for birth-clock time and record source.

# Methods

This retrospective study used 4801 nursing-record rows from a Beijing maternity hospital. Investigators confirmed routine next-calendar-day morning weighing, rather than individual 24-hour measurements; individual weighing timestamps were unavailable. Percentage weight loss was calculated from birthweight and next-day weight after investigator-confirmed corrections. Median regressions used the same complete-record sample. A non-cyclic birth-clock spline with source adjustment was the workflow-aligned reference; cyclic and linear alternatives, missingness bounds and paired birth-date cluster resampling assessed robustness.

# Results

Numeric weight pairs were available for 4382 records (91.27%); median loss was 2.71% (interquartile range 1.47–4.14). Morning births were more frequent after cesarean than vaginal delivery. In 4376 complete records, the cesarean–vaginal conditional median difference was 0.70 percentage points (95% confidence interval 0.55–0.84) after sex and birthweight adjustment, 0.67 (0.53–0.81) with source adjustment alone, and 0.20 (0.06–0.33) with spline and source adjustment. Cyclic and linear alternatives yielded differences of 0.19 and 0.38 points. In 3608 known-date records, the base-minus-spline/source coefficient difference was 0.510 points (95% paired date-cluster bootstrap interval 0.399–0.641; 1000 replications). Results were similar after reducing duplicate candidates, while weight-pair availability differed by delivery mode and transfer/discharge notation.

# Conclusions

Accounting for birth-clock time reduced delivery-mode differences in next-day recorded weight loss, although residual estimates depended on model form. Routine calendar-day observations should not be treated as measurements at a uniform postnatal age. These findings support timing-aware interpretation of nursing records, not causal attribution or a 24-hour clinical reference standard.

## 1. Background

Neonatal weight change is interpreted alongside feeding, elimination and clinical examination, not as an isolated percentage. The Academy of Breastfeeding Medicine includes weight and feeding assessment in birth-hospitalization discharge planning [1]. Prospective observations also show that early weight loss and subsequent recovery differ by feeding method [2]. Both the circumstances of measurement and the interval from birth therefore matter when comparing newborns.

Hour-specific weight-loss nomograms were developed in 108907 exclusively breastfed newborns and stratified by delivery mode [3]. Separate nomograms were developed for formula-fed newborns, and the breastfeeding nomograms were externally evaluated [4, 5]. These studies support age-specific assessment; they do not make a field labelled “next-day weight” equivalent to a measurement at 24 hours of life. A calendar-day entry can combine substantially different birth-to-weighing intervals.

Birth-clock time and postnatal age are distinct. When follow-up weights are obtained during a common next-day morning round, children born at different clock times may have different observation intervals. Delivery groups may also differ in their birth-time composition. Previous research has examined birth-time groups and neonatal weight loss [6], and healthcare processes are known to influence associations in routinely collected records [7]. A clock-associated pattern alone cannot distinguish measurement timing from feeding, delivery circumstances or other mechanisms.

We examined delivery-mode differences in recorded birth-to-next-day weight loss under an investigator-confirmed morning-weighing routine. Our objectives were to describe the recorded distribution and availability, compare sequentially adjusted conditional median differences on an identical sample, and test sensitivity to time-model specification and source-data features. The question was whether a routine-record comparison changes when available timing information is considered, not whether one delivery mode causally increases weight loss or whether these records define a normal range.

## 2. Methods

# 2.1 Study design, setting and analytic population

This retrospective secondary analysis used newborn nursing records from Shunyi Women's and Children's Hospital of Beijing Children's Hospital, Beijing, China. The supplied extract was labelled as covering 1 January 2024 to 27 July 2025 and contained 4801 source rows. The parent project concerned first documented meconium passage. Its protocol specified gestational age above 37 weeks and no prenatally identified fetal abnormality, and excluded direct neonatal-care transfers or transfers before meconium documentation on the obstetric ward. A complete birth-to-enrolment register and record-level verification of these criteria were unavailable.

The analytic unit was a source row, not an independently verified unique infant. Verified infant identifiers and mother–infant linkage were absent. All rows contributed to completeness accounting; paired numeric weights defined the descriptive sample independently of meconium-time availability. Complete delivery-mode, sex and birth-clock information defined the common regression sample. This exploratory

study used the available extract without a prospective sample-size calculation or prospective registration. Reporting followed STROBE and RECORD [8, 9]; Additional file 2 maps the reporting items and unresolved information.

## 2.2 Weighing routine and outcome definition

During manuscript development, after the initial analyses, investigators confirmed that subsequent weights were routinely obtained during a relatively fixed morning nursing round on the next calendar day, rather than at exactly 24 hours after each birth. This clarification describes the reported workflow, not a record-by-record timestamp audit. Individual weighing dates and clock times, adherence and exceptions could not be verified from the extract. No specific morning clock time or unobserved measurement age was assigned. Scale, calibration, clothing status and repeat-reading procedures were also unavailable.

The principal outcome was recorded percentage loss, calculated as 100 × (birthweight − next-day weight) / birthweight. Positive values indicate loss and negative values gain; both source weights were in kilograms. Absolute gram differences were summarized, and a companion model used next-day weight in grams. Recorded losses of at least 5%, 7% and 10% were described as numerical thresholds only, not diagnoses of dehydration, feeding failure or treatment need. None was interpreted as an age-specific clinical cutoff.

## 2.3 Data preparation and covariates

The analysis used the corrected V4 dataset, dated 21 September 2026. A feasibility-stage rule had flagged 23 pairs with recorded loss above 15% or gain above 10%. Investigators reported source re-review and confirmed corrections to two birthweights and 21 next-day weights. The prior values and change log were retained; other weights were unchanged. These were investigator-confirmed corrections, not statistical imputation. Analysts checked the standardized analysis weights against all workbook rows but did not independently audit the underlying clinical charts. Computational agreement does not establish clinical validity [10].

Covariates were delivery mode, sex, birthweight and birth-clock time. Previously resolved sex/delivery-column reversals were retained without a second exchange. Valid clock strings and fractional-day values were parsed directly; an existing fully parsed birth datetime supplied two otherwise missing clocks. Five paired records remained clock-unresolved. Birth time was represented as hours from local midnight, not hours of life.

Four mutually exclusive source strata combined explicit year and a prior coding-normalization batch: 2024 outside that batch, 2024 within the batch, 2025 within the batch, and unresolved birth year. All explicit 2025 rows belonged to that batch, so year and batch effects could not be separated. Strata indicate record provenance, not treatment or verified changes in care. Transfer/discharge wording and multiple-birth-like labels were retained as sensitivity flags. Two exact-content duplicate candidate pairs were identified from trimmed non-identifier source fields; primary analyses retained both members because patient identity was not verified.

Individual gestational age, feeding type and intake, maternal fluids, parity, labour exposure and elective versus emergency cesarean status were unavailable. The first-hour feeding field was almost constant and could not identify exclusive breastfeeding. Meconium, temperature and bilirubin were not adjustment variables because their temporal relationship to weighing was unresolved. Birthweight did not substitute for gestational age, and these covariates were not treated as a sufficient causal adjustment set.

## 2.4 Descriptive analyses and sequential models

Distributions were summarized using medians, interquartile ranges (IQRs) and selected percentiles, with variable-specific denominators. Threshold proportions used Wilson 95% confidence intervals (CIs). Distributional intervals and the unadjusted difference between delivery-group medians used 2000 row-bootstrap replications; hourly-bin summaries used 1000 replications per bin. Threshold membership used decimal arithmetic. Seeds and interpolation conventions are specified in Additional file 1 and the code.

Median regression estimated cesarean–vaginal differences in the conditional median of recorded loss [11]. All principal models used the same complete records. M0 adjusted for sex and birthweight centred at 3.3 kg; M0 plus source added the three source indicators without clock terms. S1 added a non-cyclic linear spline of birth clock to M0, and S2 added source indicators to S1. For birth-clock hour b, the spline basis was (b − 12), max(b − 6, 0), max(b − 12, 0) and max(b − 18, 0).

S2 was designated the workflow-aligned reference after workflow clarification: calendar-day follow-up does not require periodic continuity across midnight. This was a retrospective analytical decision, not a preregistered choice or selection by statistical significance. The original cyclic specifications were retained as H1 (M0 plus a 24-hour sine/cosine pair) and H2 (H1 plus source). L2 used linear clock time and source. Comparing identical samples prevents changing sample membership from explaining coefficient differences; it does not make the differences causal or identify a mediated proportion.

Quantile fits used robust asymptotic standard errors and an iteration tolerance of $10^{-9}$. The maximum iteration limit was increased from 30000 to 300000 for all fits during convergence verification; the 75th-quantile sensitivity fit required the increased limit. Rank and convergence were checked, with independent linear-programming checks of the S2 median fit and the 75th-quantile objective (Additional file 1). Pseudo-$R^2$ measures improvement in quantile loss, not variance explained. Because birthweight is embedded in the percentage-loss denominator [12], its coefficient was not assigned a biological interpretation; a gram-scale outcome model provided a complementary check.

## 2.5 Robustness and missing observations

S2 was refitted after reducing one record from each duplicate candidate pair, omitting multiple-birth-like labels or transfer/discharge notes, and adding quadratic birthweight. Other analyses used the 25th and 75th quantiles, alternative spline knots at 8 and 16 hours, known-date records, and next-day weight in grams. Pre-correction results were retained as an audit comparison. Availability was examined by

delivery mode, source and birth-clock block; source-by-clock cell counts assessed empirical support, not exchangeability.

For a direct paired comparison of coefficients, complete records with known birth dates were resampled by birth-date clusters in 1000 replications. M0 and S2 were fitted to the same resampled records each time, and M0 minus S2 was retained. Iterative-fit warnings triggered a linear-programming fallback rather than discarding replications. Percentile intervals quantify sampling uncertainty in this model comparison; birth-date clustering does not fully address unrecognized twins or repeated infants.

Missing weight pairs were not imputed. Deterministic bounds assigned all unavailable outcomes below or above the observed values to bound the full-extract median and, separately, each delivery-group median. Group-specific bounds were combined for the unadjusted median difference; analogous assignments bounded the proportion with at least 5% recorded loss. These are identification bounds, not CIs or adjusted-effect bounds, and do not cover births absent from the workbook. All analyses were exploratory; no family-wide confirmatory significance claim was made.

## 2.6 Reproducibility and AI assistance

Analyses were developed using Python 3.13.5, NumPy 2.3.5, SciPy 1.17.0 and statsmodels 0.14.6. R9 convergence verification used Python 3.12.14 with the same NumPy, SciPy and statsmodels versions. Executable scripts, fixed seeds, aggregate outputs, diagnostics and input checksums accompany the article (Additional file 3). Patient-level inputs and row-level correction locators remain restricted. ChatGPT assisted with question refinement, analysis-code preparation, numerical checks, literature-supported drafting and editing. Calculations were obtained by executing the supplied code, not by treating generated text as observed data. Human authors retain responsibility for source verification, interpretation and final approval.

# 3. Results

## 3.1 Record availability

Of 4801 source rows, 4382 contained numeric weight pairs (91.27%) and 419 did not. Five paired rows lacked a resolvable birth clock and one additional row lacked sex, leaving 4376 for every principal model. Twenty-three investigator-confirmed corrections were included without changing these denominators. Table 1 summarizes paired and unpaired records; Tables S1 and S14 provide accounting.

Table 1
Source records with and without paired weights

| Characteristic | Weight pair available | Weight pair unavailable |
|---|---|---|
| Source records | 4382 | 419 |
| Male sex, n/observed (%) | 2227/4381 (50.83) | 221/419 (52.74) |
| Cesarean delivery, n (%) | 2263 (51.64) | 98 (23.39) |
| Birthweight, mean (SD), kg | 3.36 (0.40) | 3.35 (0.41) |
| Records with birthweight, n | 4382 | 418 |
| Transfer/discharge note, n (%) | 8 (0.18) | 117 (27.92) |
| Multiple-birth-like label, n (%) | 16 (0.37) | 0 (0.00) |
| 2024, other source rows, n (%) | 1981 (45.21) | 120 (28.64) |
| 2024, normalization batch, n (%) | 263 (6.00) | 52 (12.41) |
| 2025, normalization batch, n (%) | 1369 (31.24) | 196 (46.78) |
| Unresolved birth year, n (%) | 769 (17.55) | 51 (12.17) |
| Resolvable birth clock, n | 4377 | 418 |

Denominators are source rows, not verified unique infants. Percentages use the column total unless an observed denominator is shown. Birthweight means use 4382 and 418 records. Source strata combine explicit year and prior coding-normalization provenance; they are not clinical interventions. Notations are sensitivity flags, not verified transfer or multiple-birth outcomes.

Weight-pair availability was 2119/2440 (86.84%) after vaginal delivery and 2263/2361 (95.85%) after cesarean delivery. Only 8/125 records with transfer/discharge wording had pairs, compared with 4374/4676 without such wording. In the 00:00–<06:00 birth block, availability was 338/494 (68.42%) after vaginal delivery and 115/125 (92.00%) after cesarean delivery. Thus, observation was selective; neither transfer timing nor reason could be established. Availability and distribution also differed by source stratum (Tables S11–S13).

# 3.2 Recorded weight-loss distribution

Median recorded loss was 2.71% (95% bootstrap CI 2.63–2.79), with IQR 1.47–4.14% and median absolute loss of 90 g. There were 109 gains and 152 unchanged pairs. The 95th and 99th percentiles were 6.02% and 7.31%; these are sample summaries, not clinical limits. Values lay on a 10-g recording grid, which does not establish scale accuracy.

Vaginal and cesarean medians were 2.34% (IQR 1.20–3.79) and 3.04% (1.71–4.41). Their unadjusted median difference was 0.70 percentage points (bootstrap CI 0.53–0.84). Across all pairs, 588 (13.42%) had at least 5% recorded loss, 69 (1.57%) at least 7%, and 8 (0.18%) at least 10% (Table 2).

Table 2
Distribution of recorded neonatal weight loss

| Statistic | All pairs | Vaginal | Cesarean |
|---|---|---|---|
| Numeric weight pairs, n | 4382 | 2119 | 2263 |
| Median loss, % | 2.71 | 2.34 | 3.04 |
| Interquartile range, % | 1.47–4.14 | 1.20–3.79 | 1.71–4.41 |
| Median absolute loss, g | 90 | 80 | 100 |
| Recorded loss ≥ 5%, n (%) | 588 (13.42) | 224 (10.57) | 364 (16.08) |
| 95% CI for proportion, % | 12.44–14.46 | 9.33–11.95 | 14.63–17.66 |
| Recorded loss ≥ 7%, n (%) | 69 (1.57) | 33 (1.56) | 36 (1.59) |
| 95% CI for proportion, % | 1.25–1.99 | 1.11–2.18 | 1.15–2.19 |
| Recorded loss ≥ 10%, n (%) | 8 (0.18) | 4 (0.19) | 4 (0.18) |
| 95% CI for proportion, % | 0.09–0.36 | 0.07–0.48 | 0.07–0.45 |
| Recorded gain, n | 109 | 65 | 44 |
| No recorded change, n | 152 | 89 | 63 |

Loss = 100 × (birthweight − next-day weight) / birthweight. Positive values indicate loss and negative values gain. Individual weighing age is unavailable; thresholds are numerical summaries, not diagnoses or treatment criteria. Wilson intervals are pointwise. The 23 investigator-confirmed corrections are included in the paired sample.

## 3.3 Birth timing and sequential delivery-mode differences

Among clock-resolved paired records, births at 06:00–<12:00 comprised 1227/2261 cesarean records (54.27%) and 436/2116 vaginal records (20.60%); corresponding evening proportions at 18:00–<24:00 were 10.08% and 33.60% (Fig. 1). Recorded-loss medians were higher in morning than evening birth groups for both delivery modes: 3.31% versus 1.76% after vaginal delivery, and 3.44% versus 1.64% after cesarean delivery. These are between-record comparisons by birth clock, not within-infant weight trajectories (Fig. 2; Table S8).

In the common 4376-record sample, M0 estimated a cesarean–vaginal conditional median difference of 0.70 percentage points (95% CI 0.55–0.84). Source adjustment alone yielded 0.67 (0.53–0.81). S1 yielded 0.20 (0.04–0.35), and the S2 reference yielded 0.20 (0.06–0.33). Most of the numerical reduction therefore occurred with clock adjustment rather than source adjustment alone. However, H2 gave 0.19 (0.05–0.33) and L2 gave 0.38 (0.26–0.51), demonstrating sensitivity of the residual contrast to model form (Table 3; Fig. 3).

Table 3
Delivery-mode contrasts under alternative timing adjustments

| Model | Adjustment beyond M0 | n | Difference (95% CI), pp | Pseudo-$R^2$ |
|---|---|---|---|---|
| M0 | None | 4376 | 0.70 (0.55 to 0.84) | 0.0157 |
| M0 + source | Source strata | 4376 | 0.67 (0.53 to 0.81) | 0.0426 |
| S1 | Non-cyclic spline | 4376 | 0.20 (0.04 to 0.35) | 0.0664 |
| S2 (reference) | Spline + source strata | 4376 | 0.20 (0.06 to 0.33) | 0.0983 |
| H1 | Cyclic clock | 4376 | 0.21 (0.06 to 0.37) | 0.0599 |
| H2 | Cyclic clock + source | 4376 | 0.19 (0.05 to 0.33) | 0.0925 |
| L2 | Linear clock + source | 4376 | 0.38 (0.26 to 0.51) | 0.0935 |

All models use the same 4376 records; delivery mode and sex are included throughout, with birthweight centred at 3.3 kg. The reference source is explicit 2024 outside the prior normalization batch. Spline knots are at 6, 12 and 18 clock hours; the harmonic representation uses one 24-hour sine/cosine pair. Intervals use robust asymptotic standard errors. Differences (pp) are in percentage points, not relative percentages. Pseudo-$R^2$ measures improvement in quantile loss. S2 was selected retrospectively on workflow grounds. Coefficient attenuation is not a mediation estimate.

## 3.4 Robustness and paired coefficient comparison

The S2 delivery contrast remained similar after duplicate-candidate reduction (0.198 points), omission of multiple-birth-like labels (0.187), omission of transfer/discharge notes (0.207), quadratic birthweight adjustment (0.204), or alternative spline knots (0.211). The 25th- and 75th-quantile contrasts were 0.197 and 0.322 points. The next-day-weight model estimated − 6.91 g (95% CI − 11.41 to − 2.41) after cesarean versus vaginal delivery, conditional on birthweight and the other covariates. This small group difference does not establish clinical importance (Tables S4–S5).

In 3608 complete known-date records spanning 571 birth dates, M0 and S2 estimates were 0.700 and 0.190 points. Their paired difference was 0.510 points, with a 95% date-cluster bootstrap interval of 0.399–0.641. The S2 bootstrap interval was 0.013–0.364. Fifteen of 2000 iterative fits required the linear-programming fallback; no replication was discarded. Independently solving the full-sample S2 model reproduced its delivery coefficient within 0.000001 points (Table S7).

## 3.5 Correction audit and missing-outcome bounds

The 23 investigator-confirmed corrections changed the overall median from 2.72% to 2.71% and the S2 contrast from 0.197 to 0.199 points. In contrast, records with at least 10% loss decreased from 22 to 8, and the observed range changed from − 48.40–69.33% to − 7.84–14.70%. Central estimates were therefore substantially less affected than the unverified pre-correction tail (Tables S1–S2 and S6).

Assigning the 419 unavailable outcomes to opposite extremes bounded the full-extract median at 2.47–2.95%. Vaginal and cesarean median bounds were 2.01–2.74% and 2.92–3.17%, yielding an unadjusted

median-difference bound of 0.19–1.16 points. The full-extract proportion with at least 5% loss was bounded at 12.25–20.97%. These bounds address missing outcomes only, not measurement validity or unmeasured confounding (Table S9).

## 4. Discussion

# 4.1 Principal findings

This study shows that a delivery-mode comparison based on routine next-day weights can change materially when birth-clock composition is considered. Source adjustment alone left the contrast close to the base estimate, whereas flexible clock adjustment reduced it. The reduction occurred on identical records and persisted in paired resampling and alternative analyses. Nevertheless, the residual magnitude depended on model form. The evidence concerns conditional associations in selected nursing records, not a causal effect of delivery mode or the proportion of an effect explained by weighing practice.

The reported morning-weighing routine provides a concrete reason to question equal observation intervals. It does not establish each infant's actual weighing age. Birth-clock adjustment incorporates information available in the extract; it cannot reconstruct missing timestamps or be relabelled as exact postnatal-age adjustment. The contribution is the empirical comparison of delivery-mode estimates within this local nursing-record dataset as birth-clock information is incorporated.

# 4.2 Relationship to neonatal weight research

Existing nomograms characterize weight loss by actual postnatal age and feeding group [3–5]. Our median should not be benchmarked directly against their 24-hour values because elapsed intervals and feeding composition are unresolved. A lower recorded median does not demonstrate better feeding or a healthier population. One follow-up field also cannot describe maximal loss, time to regain birthweight or longer-term recovery [2].

Zia and colleagues examined day- versus night-time birth in 2044 breastfeeding newborns using specified postnatal windows [6]. They reported more threshold-defined weight loss among night-time births, whereas our morning births had higher median loss than evening births; differences in birth-time grouping, outcome definition and observation windows preclude direct comparison. Birth-time associations are therefore not novel in themselves. The present analysis instead compares source-only and alternative clock-adjusted estimates within a reported calendar-day workflow. Differences in populations, windows and ascertainment prevent either study from identifying an intrinsic circadian mechanism. Studies of first-day loss and later nadir, and of postnatal weight patterns with subsequent breastfeeding or growth, contain longitudinal outcomes that our extract does not provide [13, 14]. We consequently did not develop a prognostic score or estimate diagnostic accuracy.

# 4.3 Measurement context and alternative explanations

Under the reported routine, morning and evening births would generally have different intervals before next-day weighing, if the routine was followed. This is compatible with the observed birth-clock pattern, but adherence, exceptions and individual intervals were not verified. Feeding initiation, maternal fluids, labour exposure and delivery indications may also differ across birth times. An observational study linked intrapartum fluid exposure with neonatal output and weight loss [15], while a randomized fluid-management trial did not demonstrate a clear reduction in its primary weight-loss outcome [16]. Such evidence supports considering multiple mechanisms rather than selecting a single unmeasured explanation.

Birth timing may reflect care pathways associated with delivery mode, not merely a conventional baseline confounder. Conditioning on it therefore changes the descriptive comparison without isolating a total delivery effect. Similarly, source indicators may absorb differences in recording practice or case composition; they do not identify improvements in care. The larger estimate under linear clock adjustment is important evidence of uncertainty, not a result to omit in favour of the preferred spline.

## 4.4 Implications for clinical interpretation and nursing data

These results do not justify different feeding advice by birth clock, a new intervention threshold or a changed weighing schedule. The residual gram-scale difference was small, and no clinical outcome was evaluated. Clinical assessment should continue to integrate feeding, elimination, examination and age-appropriate interpretation [1]. A precisely estimated group coefficient should not be confused with an actionable individual difference.

The practical implication for research and documentation is to retain the date and clock time of every weight alongside the value, rather than rely on a calendar-day label. Where timestamps are available, comparisons can use actual elapsed age and assess adherence to a defined observation window. Where they are not, available timing information and source composition should be reported, with sensitivity to model form made visible. Missing observations and transfer-related gaps also require explicit denominator accounting. These are data-collection implications, not interventions tested by this study. Earlier methodological work also highlighted the ambiguity of day-based neonatal observations and recommended specifying elapsed hours [17].

## 4.5 Strengths and limitations

Strengths include a common sample across sequential models, a source-only comparator, non-cyclic and cyclic time representations, paired date-cluster resampling, numerical verification and transparent missingness accounting. The correction audit further distinguishes stability of central estimates from sensitivity of threshold counts. Neither a stable median nor reproducible code validates every clinical measurement.

The principal limitation remains the lack of actual follow-up weighing timestamps. Workflow confirmation was retrospective and could not establish record-level adherence. Gestational age, feeding type, maternal fluids and delivery indications were unavailable; adjustment cannot exclude residual

confounding. Parent-project selection and unequal weight availability, especially among records with transfer/discharge wording, limit generalizability. The denominator represents the supplied workbook, not all hospital births or independently confirmed healthy term infants.

Verified identifiers and family linkage were absent, and duplicate-candidate sensitivity or birth-date clustering cannot resolve all dependence. The 23 corrections rely on investigators' reported re-review; analysts did not independently validate charts, and the screening rule does not prove that all remaining values are accurate. Source-year overlap was incomplete. Finally, this was a single-centre, post hoc exploratory analysis without external validation or clinical outcomes. The reported intervals capture sampling uncertainty under each specification, not the full uncertainty from measurement, selection and model choice.

# 5. Conclusions

In routine next-calendar-day neonatal weight records, accounting for birth-clock time reduced delivery-mode differences, but the remaining estimates varied with the time model. Calendar-day labels should not be treated as a uniform postnatal measurement age. Recording actual weighing timestamps and acknowledging selective observation would strengthen interpretation; the present estimates are not causal effects or a 24-hour clinical reference standard.

# Abbreviations

AI
artificial intelligence
CI
confidence interval
IQR
interquartile range
SD
standard deviation
pp
percentage points
RECORD
REporting of studies Conducted using Observational Routinely-collected health Data
STROBE
Strengthening the Reporting of Observational Studies in Epidemiology.

# Declarations

**Ethics approval and consent to participate**

This retrospective secondary analysis was covered by the approval of the Medical Ethics Committee of Beijing Shunyi District Maternal and Child Health Care Hospital for the parent observational project on first documented meconium passage (approval no. 2024-043-1; 15 October 2024). A continuing-review decision dated 30 December 2024 approved continuation through December 2025. Written informed consent from parents or legal guardians was obtained in accordance with the approved procedures. The approved protocol and consent arrangements covered this secondary analysis of birthweight and next-day weight and all historical records included in the January 2024 to July 2025 extract, including those predating the initial approval.

**Consent for publication**

Not applicable. This article reports aggregate analyses and contains no identifiable individual information or images.

# Competing Interests

Yifan Jia is employed by Beijing Yidu Cloud Technology Co., Ltd. The authors declare no other competing interests.

# Related manuscripts

Two companion manuscripts have been prepared from the same underlying extract to examine first documented meconium passage and low-temperature record findings. The weight-pair sample overlaps in 2643 records with the meconium analysis and 4364 records with the temperature-complete analysis. The present article addresses a distinct weight endpoint and timing-adjusted delivery-mode question; these manuscripts do not represent independent cohorts. The companion manuscripts are available to the editor for assessment of overlap.

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# Author Contribution

WD contributed to conceptualization, investigation, data curation and project administration, and drafted the manuscript. YJ contributed to methodology, software, formal analysis and visualization, and drafted the manuscript. YaL (Yang Liu) contributed to clinical interpretation, validation and critical revision of the manuscript. YiL (Yilin Liu) contributed to data curation, validation and critical revision of the manuscript.

HL contributed to conceptualization, resources, supervision and critical revision of the manuscript. All authors read and approved the final manuscript and agree to be accountable for the work.


# Acknowledgements

Not applicable.


# Data Availability

Aggregate data and executable analysis code accompany the article in Additional file 3. Patient-level nursing records and row-level audit material are not publicly available because they contain sensitive neonatal information and require institutional authorization. Enquiries may be addressed to the corresponding author, Hairong Liu (hlb59612512@sina.com). Any access is subject to institutional policy and applicable consent and confidentiality requirements; access cannot be guaranteed. No public individual-level repository is claimed.

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# Figures

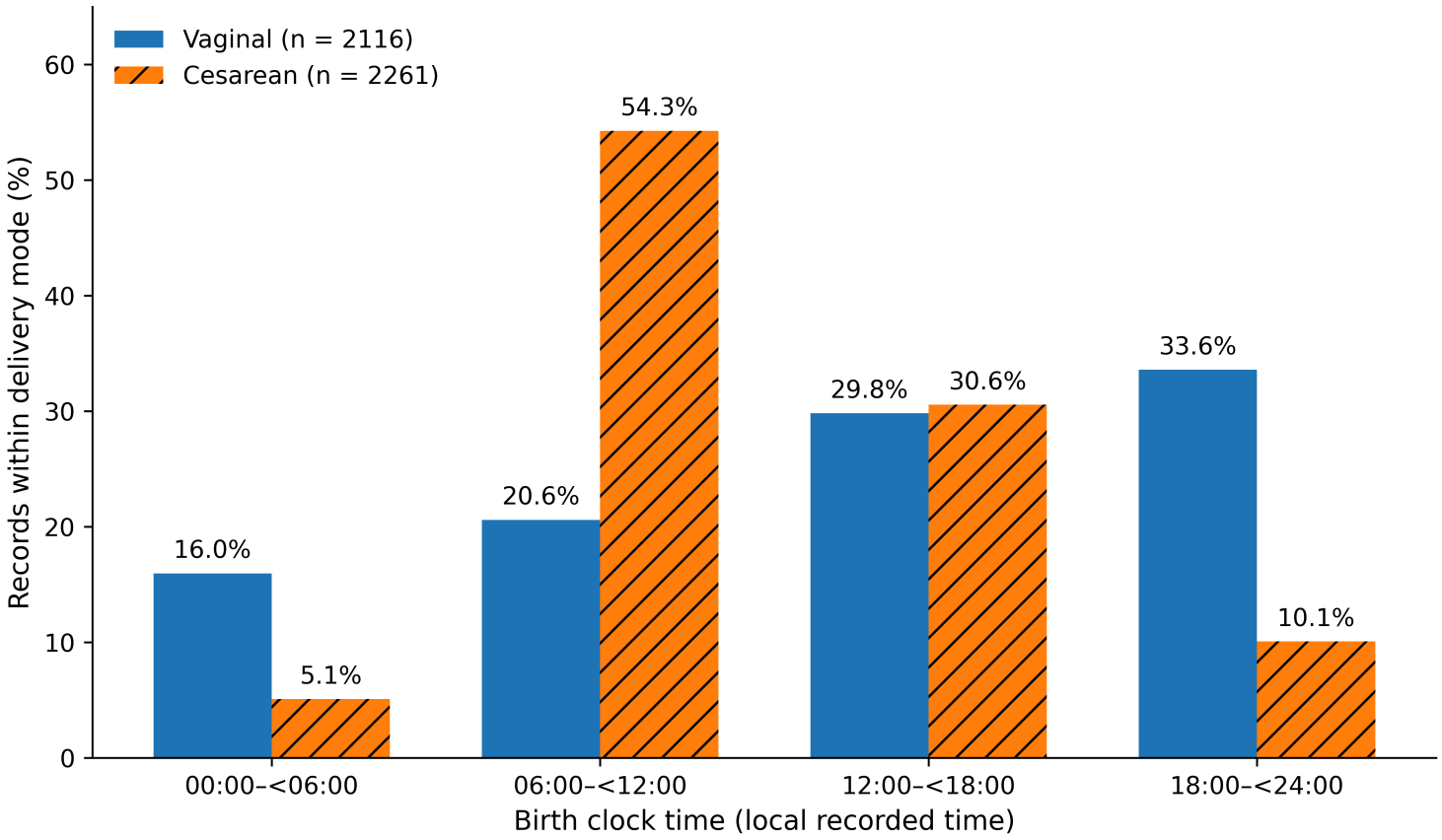



**Figure 1**

**Birth-clock distribution by delivery mode.**

Percentages are calculated within 2116 vaginal and 2261 cesarean records with paired weights and a resolvable birth clock. Each series totals 100%. Five paired records lacked resolvable birth clocks. The axis describes time of birth, not age at weighing. Different morning and evening compositions motivate the common-sample adjusted comparisons.

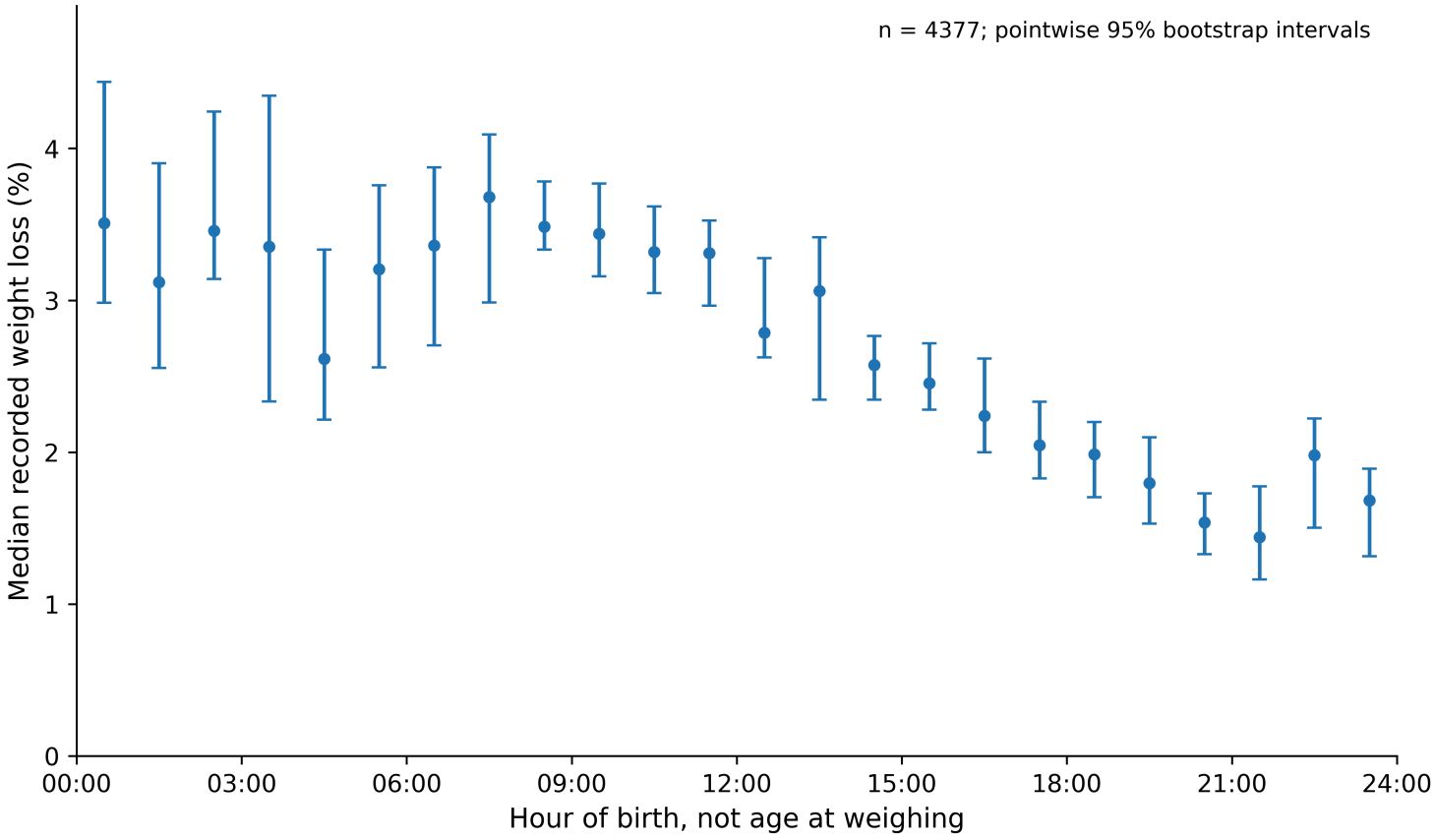



**Figure 2**

**Recorded weight loss by birth-clock hour.**

Points are medians for hourly birth-clock bins among 4377 paired records with resolvable clocks, irrespective of sex availability. Error bars are pointwise 95% percentile bootstrap intervals from 1000 replications per bin. These are different records grouped by birth time, not repeated observations along an infant's postnatal trajectory. Individual weighing timestamps were unavailable; bin denominators are supplied in Additional file 3.

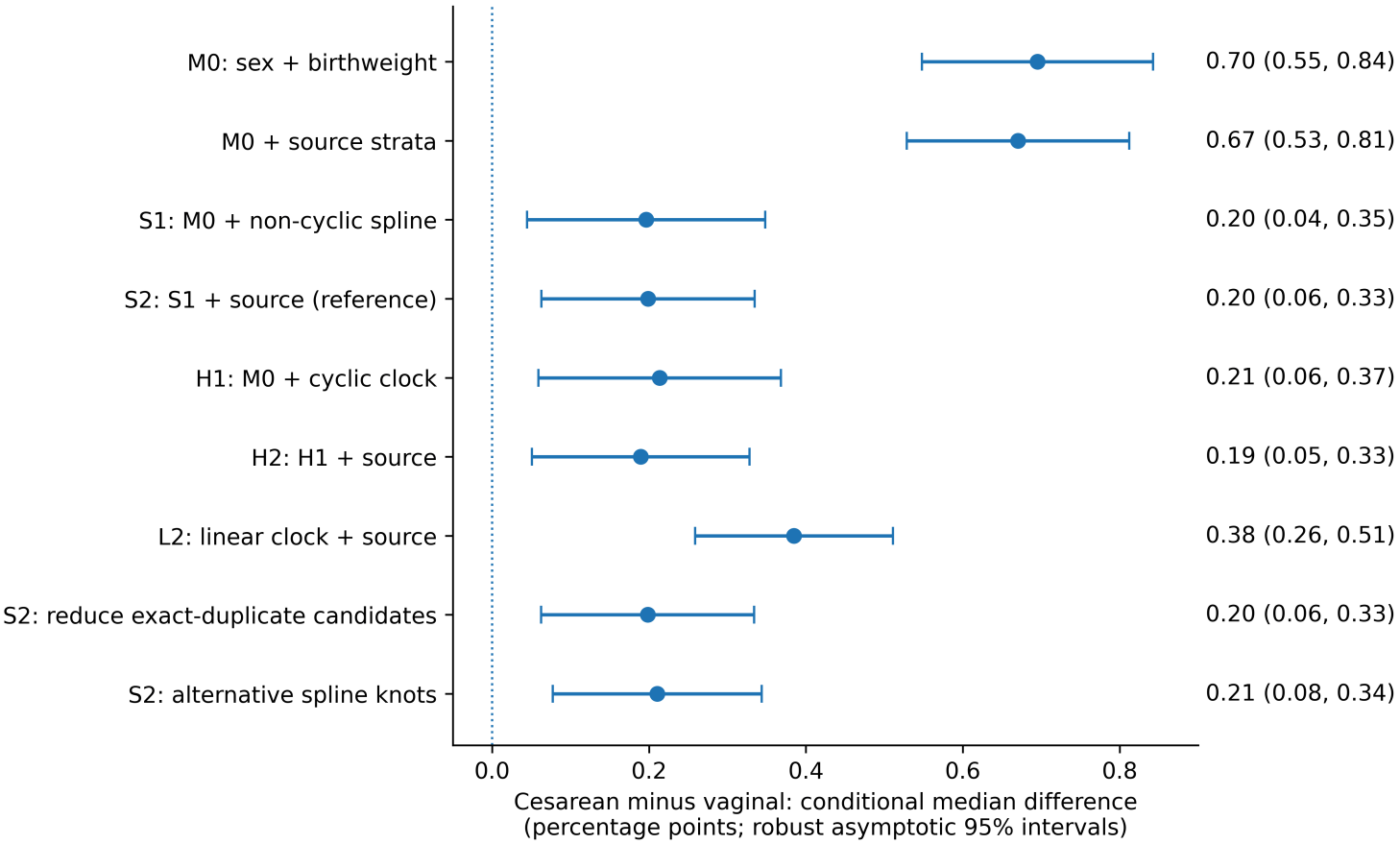



**Figure 3**

**Delivery-mode contrasts under alternative timing adjustments.**

Points and error bars show cesarean–vaginal conditional median differences and robust asymptotic 95% confidence intervals in percentage points. Common-sample models use 4376 records; duplicate-candidate reduction uses 4374. S2 is the non-cyclic spline/source reference, designated after workflow clarification. The figure displays model-specific sampling intervals, not the paired date-cluster interval or full uncertainty across model choices. Neither attenuation nor residual association identifies a causal effect.

## Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- 04AdditionalFile1SupplementR9.docx
- 05AdditionalFile2STROBERECORDR9.docx
- 06AdditionalFile3CodeandAggregatesR9.zip