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Who is offered a trial of labour after caesarean, and what the offer misses: a retrospective cohort study of 1020 women in central Kazakhstan

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Abstract

Background.

Women with one previous caesarean drive caesarean rates in Kazakhstan and elsewhere, yet whether they labour again depends more on local practice than on case mix. Research on this population predicts who succeeds once labour has been chosen, which presupposes the offer. We asked the prior question, then whether the trials a service supports reach the right women.

Methods.

Retrospective cohort of 1020 consecutive women with one previous lower-segment caesarean delivering a term singleton cephalic pregnancy in Karaganda region, 2017–2022, classified by intended mode of birth as trial of labour after caesarean (TOLAC) or planned repeat caesarean (PRCD). Determinants of the offer were modelled on the full cohort using only pre-decision variables, and the same covariates fitted to vaginal birth among women who laboured, the two sets of weights then being compared by bootstrap. Women were stratified by an unweighted count of four guideline-derived antenatal factors, and stratum-specific vaginal birth rates applied to alternative allocations holding trials, or failed trials, fixed.

Results.

Of 1020 women, 224 (22.0%) laboured; 161 delivered vaginally, a cohort caesarean rate of 84.2%.

Conclusions. Uptake was low and the trials offered were poorly targeted. The failed trials this service already tolerates would have supported two thirds more labouring women and nearly twice the vaginal births. The constraint is neither safety nor the success rate, but where the offer points.

Background

Caesarean delivery reached 21.1% of births worldwide by 2018 and is projected to approach 28.5% by 2030 [1]. The arithmetic behind that trend is unforgiving. Every primary caesarean produces a woman who will face the same decision in her next pregnancy, and in most systems the answer given to her is another operation. The consequences accumulate across a reproductive lifetime and include abnormal placentation, uterine rupture, peripartum hysterectomy and subfertility [2, 3]. Robson group 5, women with a previous caesarean, is the largest single contributor to national caesarean rates across almost all of Europe [4]. It holds the same position in Kazakhstan, where an audit of Almaty facilities found that women with a uterine scar accounted for 37.5% to 46.3% of all caesareans and concluded that vaginal birth should be advised for these women when no contraindication exists [5].

Trial of labour after caesarean (TOLAC) is the only intervention that acts directly on this group. Both the American College of Obstetricians and Gynecologists and the Royal College of Obstetricians and Gynaecologists recommend that women with one previous low-transverse caesarean and no

contraindication be counselled about it [6, 7]. Whether they actually are varies enormously, and not in ways that clinical case mix explains. Among European countries with comparable standards, the proportion of women with a previous caesarean who labour ranges from under 30% in Germany, Ireland and Italy to between 45% and 55% in the Netherlands, Sweden and Finland [8, 9]. The OptiBIRTH investigators, who ran a cluster randomised trial across the low-rate countries, concluded that the difference was cultural rather than clinical. Where vaginal birth is the assumed starting point women labour, and where repeat caesarean is the default they do not [10].

Research effort has gone almost entirely elsewhere. Since the original nomogram of Grobman and colleagues [11] and its race-neutral revision [12], dozens of models have been published to estimate a woman's probability of vaginal birth once she has decided to labour. That literature answers a question that presupposes the offer. In a low-uptake system it is also partly self-confirming, because a model fitted to women whom clinicians selected for labour learns the selection as well as the physiology, and returns high probabilities for the women who were already going to be offered a trial. When the calculator of the Maternal-Fetal Medicine Units Network was introduced into routine care at one American centre, knowing the predicted score made no difference to the vaginal birth rate [13]. Predictive accuracy does not appear to be the scarce input.

Central Asia is absent from this evidence. Kazakhstan delivers around 400,000 women a year, has invested substantially in perinatal services and brought maternal mortality down to among the lowest rates in the region [14]. How often women with a scarred uterus are offered labour there, on what basis that decision is made, and what it costs when they are not, has not been described.

We therefore asked three questions of one regional cohort. Who was offered a trial of labour, and did the criteria that governed the offer match the criteria that governed its outcome? How did women who laboured fare against women delivered without labour? And given the trials the service already supported, and the failed trials it already absorbed, could the same practice have produced more vaginal births had those trials been directed differently?

Methods

Design and setting

This was a retrospective cohort study of consecutive deliveries in the obstetric facilities of Karaganda region, Republic of Kazakhstan, between 1 January 2017 and 31 December 2022. Karaganda is an industrial region of central Kazakhstan with a mixed urban and rural population served by a tiered perinatal network. Reporting follows the STROBE statement for observational studies [15].

Participants

Eligible women had exactly one previous lower-segment transverse caesarean and delivered a live singleton in cephalic presentation at 37 weeks of gestation or later. We excluded classical or unknown uterine incision, more than one previous caesarean, previous uterine rupture, placenta praevia or accreta spectrum, multiple pregnancy, non-cephalic presentation, preterm birth and any recorded absolute contraindication to labour. The cohort comprised 1020 women.

Exposure and outcomes

Intended mode of birth was taken from the antenatal plan recorded before labour. TOLAC included every woman for whom labour was intended, whether spontaneous or induced. Planned repeat caesarean delivery (PRCD) included every woman scheduled for operation without labour. Women whose trial ended in intrapartum caesarean were counted as failed TOLAC and retained in the TOLAC group for intention-to-treat comparisons.

Outcomes were vaginal birth, postpartum haemorrhage of at least 1000 mL, blood transfusion, admission to neonatal intensive care, uterine rupture defined as complete disruption of the uterine wall with clinical consequences, and scar dehiscence identified at operation. A composite maternal morbidity outcome combined haemorrhage, transfusion, rupture and dehiscence.

Separating determinants of the offer from consequences of it

Variables determined after the mode-of-birth decision were excluded from every model of that decision. Planned caesareans in this network are scheduled at 39 weeks, so gestational age at delivery, onset of labour and cervical status at admission are consequences of the delivery plan rather than inputs to it. Conditioning on them produces a familiar artefact in which post-dates gestation appears to cause the offer of labour, when it only marks the women who went into labour before their operating date.

This restriction has a second use. It defines an antenatal covariate set that exists for every woman in the cohort whether she laboured or not: previous vaginal delivery, non-recurrent indication for the index caesarean, interdelivery interval of at least 18 months, body mass index of at least 30 kg/m² and maternal age. Fitting those five covariates to two outcomes, the offer of labour in all 1020 women and vaginal birth among the 224 who laboured, sets the criteria that governed the decision directly against the criteria that governed the result.

To test formally whether the two models weight the covariates differently, rather than relying on visual comparison of the two sets of estimates, we bootstrapped the cohort 4000 times, refitted both models in each resample, and took the difference in log odds ratios covariate by covariate. Percentile intervals and a joint Wald test of the hypothesis that all five weights are identical across the two models are reported.

As a secondary comparison we refitted the vaginal birth model in the 224 labouring women with two admission variables added, Bishop score of at least 6 and spontaneous onset of labour, to quantify how much predictive information becomes available only after the decision has been taken. That model is reported for its C-statistic alone and is used nowhere else.

Antenatal favourability strata

To compare like with like between groups we grouped women by an unweighted count of four antenatal factors that ACOG and RCOG identify as favouring a successful trial of labour [6, 7]: previous vaginal delivery; a non-recurrent indication for the index caesarean, meaning breech, fetal distress, or placental or cord pathology rather than cephalopelvic disproportion or failure to progress; an interdelivery interval of at least 18 months; and a body mass index below 30 kg/m². Each contributes one point, giving a score from 0 to 4.

This is not a fitted model. The four factors were specified from guidelines before analysis and no weight was estimated from these data. The score exists to define comparable strata, not to predict any individual woman's chance, and every counterfactual estimate below therefore rests on observed stratum-specific rates rather than on a model developed here.

Allocation analysis

Under observed practice 224 women laboured, 161 delivered vaginally and 63 did not. We asked what the same service would have produced had those trials been distributed differently, under two constraints.

The first fixes the volume of trials. We allocated 224 trials to women in descending order of favourability, filling the most favourable stratum before moving to the next, and applied the vaginal birth rate observed among women who actually laboured in each stratum. This holds constant what a service consumes when it supports a trial of labour, including staffing, theatre readiness and continuous monitoring, and varies only who receives it.

The second fixes the cost that clinicians act to avoid. A failed trial is the outcome the decision to operate is intended to prevent, and 63 of them occurred. We allocated trials in the same descending order until the expected number of failures reached 63, then recorded how many women could have laboured and how many would have delivered vaginally within that budget.

Confidence intervals came from 2000 bootstrap resamples of the TOLAC group, stratified by favourability, recomputing stratum-specific rates each time.

Both constraints require that within a stratum, women who laboured and women who did not were exchangeable with respect to their chance of vaginal birth. That assumption is unlikely to hold exactly and it fails in a known direction, since unmeasured contraindications, patient refusal and clinician

reluctance all select more favourable women into labour. We therefore repeated both analyses discounting the odds of vaginal birth among never-offered women by a factor δ ranging from 1.0, meaning exchangeable, down to 0.15, and report the value of δ at which each advantage disappears.

Statistical analysis

Continuous variables are summarised as mean and standard deviation and compared with the Mann-Whitney U test. Categorical variables are given as counts and percentages and compared with the chi-squared or Fisher exact test. Proportions of rare events carry Wilson score intervals. Determinants of the offer and of vaginal birth were examined by multivariable logistic regression with all covariates entered together. Outcomes were compared by logistic regression adjusting for age, previous vaginal delivery, obesity, interdelivery interval, indication type, gestational diabetes and hypertensive disorder, reported first as intention-to-treat for the whole TOLAC group and then as-treated for vaginal birth and for failed trial, each against PRCD. No values were missing for any variable used in any analysis; all 1020 records were complete, so no imputation was required and every model is a complete-case analysis of the full cohort. Bishop score and onset of labour exist only for the 224 women who laboured, by definition rather than through loss of data, and are used only in the secondary model noted above.

For the outcome comparisons we report E-values, which give the minimum strength of association, on the risk ratio scale, that an unmeasured confounder would need with both exposure and outcome to explain away the observed effect [16]. Odds ratios were converted to approximate risk ratios by the square-root transformation recommended for common outcomes.

Analyses used Python 3.12 with statsmodels 0.14, scikit-learn and scipy. Two-sided p values below 0.05 were considered significant. Complete analysis code is provided as Additional file 1.

Ethics

The study was reviewed and approved by the Local Bioethics Committee of NJSC Karaganda Medical University, Karaganda, Kazakhstan, under the title “Optimisation of the management of vaginal birth after one caesarean section in Karaganda region”. Details are given in the Declarations.

Results

Uptake

Of 1020 eligible women, 224 (22.0%) laboured and 796 (78.0%) were delivered by planned repeat caesarean (Fig. 1). Among those who laboured, 161 delivered vaginally, giving a cohort-wide vaginal birth rate of 15.8% and an overall caesarean rate of 84.2%.

Women who laboured were younger (29.5 versus 31.5 years, $p < 0.001$), far more likely to have delivered vaginally before (45.1% versus 11.8%, $p < 0.001$) and more likely to have had a non-recurrent indication

for their index caesarean (54.9% versus 39.2%, p < 0.001). Obesity, interdelivery interval, gestational diabetes and hypertensive disorders were distributed almost identically between the groups (Table 1).

Table 1
Baseline characteristics by intended mode of birth (n = 1020)

Characteristic	TOLAC (n = 224)	PRCD (n = 796)	p
Maternal age, years, mean (SD)	29.5 (4.7)	31.5 (5.0)	< 0.001
Interdelivery interval, months, mean (SD)	27.3 (12.5)	24.7 (10.1)	0.005
Previous vaginal delivery, n (%)	101 (45.1)	94 (11.8)	< 0.001
BMI ≥ 30 kg/m², n (%)	65 (29.0)	248 (31.2)	0.60
Interdelivery interval ≥ 18 months, n (%)	166 (74.1)	581 (73.0)	0.80
Non-recurrent indication for index caesarean, n (%)	123 (54.9)	312 (39.2)	< 0.001
Gestational diabetes, n (%)	27 (12.1)	102 (12.8)	0.85
Hypertensive disorder of pregnancy, n (%)	21 (9.4)	89 (11.2)	0.52

BMI body mass index, PRCD planned repeat caesarean delivery, SD standard deviation, TOLAC trial of labour after caesarean. Gestational age at delivery is omitted because planned caesareans were scheduled at 39 weeks, which makes it a consequence of the delivery plan rather than a baseline characteristic.

The offer and the outcome were governed by different variables

Table 2 places the two models side by side. The odds of being offered labour rose more than sixfold with a previous vaginal delivery (adjusted odds ratio [aOR] 6.26, 95% confidence interval [CI] 4.40–8.90) and fell with each additional year of maternal age (aOR 0.92, 95% CI 0.89–0.95). A non-recurrent indication carried modest additional weight (aOR 1.85, 95% CI 1.33–2.56). Interdelivery interval and obesity had no bearing at all on who laboured, with adjusted odds ratios of 1.04 and 1.01 and neither approaching significance.

Fitted to vaginal birth in the women who did labour, the same five covariates behave differently (Fig. 2). An interdelivery interval of at least 18 months raised the odds of vaginal birth two and a half fold (aOR 2.56, 95% CI 1.27–5.16) and obesity roughly halved them (aOR 0.52, 95% CI 0.26–1.02). These are the two factors the decision ignored. Maternal age, which moved the decision more consistently than anything except previous vaginal delivery, did not predict the outcome (aOR 0.94 per year, 95% CI 0.88–1.01, p = 0.11).

Selection was not random. It discriminated, and women offered labour were more favourable than women who were not. What it did was run on a short list of variables, one of which does not predict what happens, while leaving out two that do.

The formal comparison is more cautious than that description. Bootstrapping the difference in log odds ratios between the two models, only interdelivery interval differed significantly (difference in log odds ratio − 0.90, 95% CI − 1.79 to − 0.06, p = 0.035). Differences for obesity (+ 0.67, − 0.16 to 1.47), previous vaginal delivery (+ 0.38, − 0.57 to 1.15), non-recurrent indication (− 0.40, − 1.20 to 0.32) and age (− 0.03, − 0.12 to 0.07) did not reach significance, and the joint test of the hypothesis that all five covariates carry the same weight in both models gave $\chi^2(5) = 9.25$, p = 0.099. With 224 labouring women the prognostic model is imprecise, so this comparison has limited power. We therefore treat the divergence between the two columns of Table 2 as a pattern that is clear for interdelivery interval and suggestive but unproven for the rest.

Table 2
The same antenatal covariates fitted to two outcomes: the offer of labour (n = 1020) and vaginal birth among women who laboured (n = 224)

Covariate	Offer of labour, aOR (95% CI)	p	Vaginal birth, aOR (95% CI)	p
Previous vaginal delivery	6.26 (4.40–8.90)	< 0.001	4.27 (2.09–8.73)	< 0.001
Non-recurrent indication	1.85 (1.33–2.56)	< 0.001	2.76 (1.43–5.33)	0.003
Interdelivery interval ≥ 18 months	1.04 (0.72–1.50)	0.85	2.56 (1.27–5.16)	0.009
BMI ≥ 30 kg/m²	1.01 (0.71–1.43)	0.98	0.52 (0.26–1.02)	0.059
Maternal age, per year	0.92 (0.89–0.95)	< 0.001	0.94 (0.88–1.01)	0.11

aOR adjusted odds ratio, BMI body mass index, CI confidence interval. Both models contain the same five covariates entered together. Variables generated after the mode-of-birth decision, namely gestational age at delivery, onset of labour and cervical status at admission, are excluded from both. Discrimination: offer model C-statistic 0.734, vaginal birth model 0.776. Two covariates that predicted vaginal birth had no association with the offer, and one that drove the offer did not predict vaginal birth.

Nor was the information simply unavailable when the decision was made. The five antenatal covariates alone discriminated vaginal birth with a C-statistic of 0.776. Adding Bishop score and spontaneous onset of labour, the two strongest single predictors in this cohort and the two that cannot be known until a woman is already in or presenting for labour, raised it to 0.849. Most of the signal separating a successful trial from a failed one was sitting in the antenatal record on the day the woman was booked for theatre.

Uptake against the odds of success

Table 3 and Fig. 3 set uptake against what happened to the women who laboured. Vaginal birth rates in the TOLAC group rose steeply across favourability strata, from 35.3% with one favourable factor to 83.1%

with three and 97.1% with four. Uptake rose too, but far more slowly, at 13.7%, 30.0% and 64.8% respectively.

The mismatch is clearest at the extremes. Among the 54 women with all four favourable factors, 35 laboured and 34 of them delivered vaginally, yet 19 women in that stratum went to theatre without labour. Among the 257 women with three favourable factors, where 83.1% of those who laboured gave birth vaginally, 180 were delivered by planned caesarean. Aggregated, uptake was 14.0% (95% CI 10.5–18.5) among women with none or one favourable factor and 36.0% (95% CI 30.9–41.5) among those with three or four ($p < 0.001$). A gradient exists, but it is shallow relative to the gradient in outcomes it ought to follow.

Table 3
Trial of labour uptake and observed vaginal birth rate by number of favourable antenatal factors

Favourable factors	Total women	Underwent TOLAC, n (%)	Vaginal birth among those who laboured, n/N (%, 95% CI)	Delivered by planned caesarean, n
0	36	6 (16.7)	3/6 (50.0, 19–81)	30
1	249	34 (13.7)	12/34 (35.3, 21–52)	215
2	424	72 (17.0)	48/72 (66.7, 55–76)	352
3	257	77 (30.0)	64/77 (83.1, 73–90)	180
4	54	35 (64.8)	34/35 (97.1, 85–99)	19

CI confidence interval, TOLAC trial of labour after caesarean. Favourable factors: previous vaginal delivery; non-recurrent indication for the index caesarean; interdelivery interval ≥ 18 months; BMI < 30 kg/m². The count is unweighted and pre-specified from ACOG and RCOG guidance, and no weights were estimated from these data. Chi-squared test for the association between stratum and uptake, $p < 0.001$. Confidence intervals are Wilson score intervals.

Outcomes

Uterine rupture occurred twice among the 224 women who laboured (0.89%, 95% CI 0.25–3.20) and scar dehiscence four times (1.79%, 95% CI 0.70–4.50). Neither occurred in any of the 796 planned caesarean deliveries, so any scar disruption was more frequent in the TOLAC group (2.68% versus 0%, $p = 0.001$). Both ruptures were managed without maternal or perinatal death.

Postpartum haemorrhage, transfusion and admission to neonatal intensive care were all numerically less common in the TOLAC group and none differed significantly on intention-to-treat (Table 4). After adjustment, composite maternal morbidity was equivalent for TOLAC and planned caesarean (aOR 0.78, 95% CI 0.51–1.19). The as-treated analysis locates the difference (Fig. 4). Women who achieved vaginal birth had lower composite morbidity than women delivered electively (aOR 0.56, 95% CI 0.32–0.96), while women whose trial failed had morbidity that was numerically higher but statistically indistinguishable from planned caesarean (aOR 1.35, 95% CI 0.74–2.46).

The E-value for the composite advantage of vaginal birth was 2.01, falling to 1.17 at the confidence limit: an unmeasured confounder would need to be associated with both delivery route and morbidity by a risk ratio of at least 2.01 to explain the point estimate away. For the failed-trial comparison the E-value at the upper confidence limit was 2.51, so these data cannot exclude a moderate excess of morbidity after a failed trial, only a large one.

That last comparison matters more than its confidence interval suggests, because it is the premise of the whole selection strategy. Restrictive allocation is justified by the belief that a failed trial is materially worse than an operation performed without labour. In these data it was not.

Table 4 Maternal and neonatal outcomes by delivery route Adjusted odds ratios versus planned repeat caesarean, adjusted for age, previous vaginal delivery, BMI ≥ 30 kg/m², interdelivery interval ≥ 18 months, indication type, gestational diabetes and hypertensive disorder:

Comparison	Postpartum haemorrhage	Transfusion	Neonatal intensive care	Composite
TOLAC, intention-to-treat	0.68 (0.40–1.14)	0.64 (0.31–1.32)	0.78 (0.45–1.36)	0.78 (0.51–1.19)
Successful vaginal birth	0.54 (0.28–1.05)	0.40 (0.15–1.09)	0.55 (0.27–1.14)	0.56 (0.32–0.96)
Failed trial of labour	1.05 (0.50–2.20)	1.13 (0.43–2.96)	1.28 (0.60–2.70)	1.35 (0.74–2.46)

PRCD planned repeat caesarean delivery, TOLAC trial of labour after caesarean, VBAC vaginal birth after caesarean. The p values in the upper panel compare TOLAC on intention-to-treat against PRCD. Composite maternal morbidity comprises postpartum haemorrhage, transfusion, uterine rupture or scar dehiscence. Scar events in the TOLAC group: uterine rupture 2/224 (0.89%, 95% CI 0.25–3.20); scar dehiscence 4/224 (1.79%, 95% CI 0.70–4.50); any scar disruption 6/224 (2.68%, 95% CI 1.23–5.72), versus 0/796 (0%, 95% CI 0–0.48) after planned caesarean.

The trials that were offered were also misallocated

Under observed practice, 224 trials produced 161 vaginal births (Table 5, Fig. 5, Fig. 6). Directing those same 224 trials to the most favourable women, meaning all 54 with four favourable factors and 170 of the 257 with three, would have produced 194 vaginal births (95% CI 178–207). That is 33 more, with no additional trial of labour supported and no additional theatre time or monitoring. The vaginal birth rate among labouring women would have risen from 71.9% to 86.5%.

The second constraint is the more consequential. Sixty-three trials failed. Allocating trials in descending order of favourability until the expected number of failures reached 63, the service could have supported 365 trials rather than 224, and 302 women (95% CI 228–374) rather than 161 would have delivered vaginally. The cohort caesarean rate would have fallen from 84.2% to 70.4%. At the rupture rate observed in this cohort's own TOLAC group the expected number of ruptures across 365 trials is 3.3, against the 2 that occurred.

Put plainly, the failed trials the service already absorbed were spent on 224 women when they would have covered 365, and bought 161 vaginal births when they would have bought about 300. The volume of intervention was not the limit. The ordering was.

Table 5
Vaginal births under observed and reordered allocation of trials of labour

Allocation	Trials of labour, n	Expected vaginal births (95% CI)	Vaginal birth rate among labouring women	Cohort caesarean rate	Expected uterine ruptures
Observed practice	224	161 (observed)	71.9%	84.2%	2 (observed)
Same trial volume, prognosis-ordered	224	194 (178–207)	86.5%	81.0%	2.0
All women with ≥ 3 favourable factors	311	266 (244–288)	85.6%	73.9%	2.8
Same number of failed trials (63)	365 (291–437)	302 (228–374)	82.8%	70.4%	3.3
All 1020 women	1020	655 (587–719)	64.2%	35.8%	9.1

CI confidence interval. Trials are allocated in descending order of favourability and stratum-specific vaginal birth rates observed among women who actually laboured are applied. Confidence intervals come from 2000 stratified bootstrap resamples of the TOLAC group. Expected uterine ruptures use the rate observed in this cohort's TOLAC group, 0.89%. Estimates assume within-stratum exchangeability; Table 6 shows their behaviour when that assumption is relaxed.

Sensitivity to unmeasured selection

Both estimates assume within-stratum exchangeability and both would shrink if women never offered labour were systematically less likely to succeed for reasons not recorded. Table 6 and Fig. 7 show how far that can go. Discounting the odds of vaginal birth among never-offered women to 60% of those of clinically identical women who laboured still leaves 178 births under the fixed-volume scenario and 230 under the fixed-failure scenario. The fixed-volume advantage disappears at δ of about 0.40 and the fixed-failure advantage at about 0.37. For the observed allocation to have been efficient, the 796 women delivered without labour would have had to carry an average unrecorded handicap reducing their odds of vaginal birth by roughly 60% relative to women with identical recorded factors who did labour.

Table 6
Sensitivity of reordered allocations to unmeasured selection

δ	Expected vaginal births, same volume (224 trials)	Expected vaginal births, same failed trials (63)
1.00 (exchangeable)	**194**	**302**
0.80	188	272
0.60	178	230
0.50	172	199
0.40	163	167
0.30	151	135

δ is the odds of vaginal birth among never-offered women relative to women in the same favourability stratum who laboured. Observed practice produced 161 vaginal births. Both reordered allocations remain superior to observed practice down to δ of about 0.40.

Discussion

Main findings

Three things follow from these data. At 22%, TOLAC uptake in this region is low even by the standards of European countries considered to have a VBAC problem. The women who did labour did well, with a 71.9% vaginal birth rate, rupture under 1%, composite morbidity after vaginal birth lower than after planned caesarean, and failed trials no worse than planned caesarean. And, the finding we did not anticipate, the small number of trials that were offered was itself misdirected.

That third result changes what the problem is. A service that offers few trials but offers them to the right women is being cautious. A service that offers few trials and misdirects a fifth of them is doing something else. The same 224 trials, reordered, would have yielded 33 more vaginal births at no additional cost of any kind, and the 63 failed trials already absorbed would have supported 365 labouring women.

The filter was not merely narrow. It appears also to have been narrow along an axis that partly missed the target. Previous vaginal delivery, which drove the decision more than any other factor, is a genuine predictor and deserves its weight. Maternal age, the second most consistent driver, predicted the offer sharply and the outcome not at all. Interdelivery interval and body habitus, which did predict the outcome, were invisible to the decision.

We are deliberately careful about how far that reading can be pushed. Formally testing the two sets of weights against each other, only interdelivery interval separates them at conventional significance, and the joint test falls short ($p = 0.099$). Two readings remain compatible with these data: that clinicians

weight prognostic information differently from the way it behaves, or that a prognostic model built on 224 women is simply too imprecise to demonstrate the difference. Distinguishing them needs a larger cohort, and we present the comparison as a hypothesis rather than a finding. Notably, none of this bears on the allocation results below, which use no fitted model at all.

One defence of restrictive practice is that the decisive information arrives too late, and that cervical status and mode of labour onset tell you what the antenatal record cannot. In these data that defence does not hold. Antenatal variables alone reached a C-statistic of 0.776 for vaginal birth, and adding Bishop score and spontaneous onset carried it to 0.849. The increment is real, but it is an increment. Most of what distinguishes a trial that succeeds from one that fails was already legible in the notes.

We would put the point more sharply than the prediction literature usually allows. Building a better calculator for the women who already labour cannot address any of this. The calculator is fitted to a population the filter created, and its two most powerful inputs arrive after the decision it is meant to inform. The scarce resource is not predictive accuracy. It is the offer.

Comparison with other settings

The 71.9% vaginal birth rate matches the 72% to 75% commonly reported and the pooled 74.6% from three European countries in the OptiBIRTH cohort [9]. Uterine rupture at 0.89% is close to the 0.7% observed in the cohort of 17,898 women undergoing trial of labour reported by Landon and colleagues [17], and within the 0.4% to 1.1% range of a recent expert review [18]. Once women here laboured, they did roughly as well as women anywhere.

Uptake is the outlier. Twenty-two per cent sits below the 29% to 36% recorded in Germany, Ireland and Italy before intervention, and less than half the 45% to 55% of the Netherlands, Sweden and Finland [8, 9]. Case mix cannot account for it, since 311 women in this cohort had three or four favourable factors and only 36% of them laboured.

Meta-analyses comparing trial of labour with elective repeat caesarean report higher uterine rupture, neonatal asphyxia and perinatal death with TOLAC alongside no difference in hysterectomy, transfusion or puerperal infection [18, 19]. Our results fit that pattern with one addition. The morbidity advantage of labouring was concentrated entirely in women who succeeded, and a failed trial was not significantly worse than a planned operation. With roughly seven in ten succeeding, the expected value of the offer was favourable.

The high vaginal birth rate deserves a comment of its own, because it is routinely read as a performance indicator. In a system where 22% of eligible women labour, a 72% success rate describes the filter rather than the care. Any service can raise its success rate by narrowing its offer, and the cohort caesarean rate of 84.2% is the price. Success among those who labour and access to labour move in opposite directions, and reporting only the first is how a system can appear to be performing well while forgoing several hundred vaginal births.

What the counterfactuals can and cannot support

The allocation analysis uses no fitted model. Stratum-specific vaginal birth rates observed among women who laboured are applied to women in the same stratum who did not, which avoids extrapolating a regression from one group to another but does not avoid the underlying exchangeability assumption. Women delivered electively may have had contraindications that were never coded, may have declined labour for reasons unrelated to obstetric risk, or may have been cared for by clinicians less willing to support a trial. Each of these pushes the true figures below our estimates.

We therefore present these as magnitudes rather than forecasts, and the sensitivity analysis defines how large the unmeasured handicap would need to be. At δ of 0.6, a substantial penalty, the fixed-volume gain is still 17 births and the fixed-failure scenario still yields 230. Only below δ of about 0.4, meaning never-offered women had under half the odds of vaginal birth of clinically identical women who laboured, does observed practice become defensible as efficient. Whether such a handicap exists is an empirical question this dataset cannot settle. It is also a strong claim for a service to make about its own records, since it amounts to asserting that most of what determined the decision was never written down.

Strengths and limitations

The cohort is consecutive, regionally rather than single-unit based, and includes the 796 women whom studies of this population routinely exclude. Analysing that group is the point, because the question of who is offered labour cannot be answered from the women who accepted it. The stratification uses guideline-derived factors with no weights fitted to these data, and every model of the decision excludes variables generated after it.

The limitations are substantial. The design is retrospective, and intended mode of birth was taken from the antenatal record, so misclassification is possible. We have no data on birthweight, fetal head station, cervical dilatation on admission, method of induction, epidural use, or the detailed written indication for the index caesarean beyond the recurrent versus non-recurrent dichotomy, and several of these influence both the offer and the outcome. We could not identify the individual clinician or the calendar year of the decision, so we cannot say how much of the unexplained variation in the offer is variation between clinicians, which is what the OptiBIRTH experience would predict [10]. Uterine rupture was too rare for multivariable analysis and is reported descriptively; a study powered for rupture would need a cohort several times this size. The favourability strata are coarse, and within-stratum heterogeneity means the reordered allocations are approximations rather than achievable schedules.

Most importantly, we did not measure the thing that most plausibly explains the uptake gap: what women were told, what they understood and what they preferred. Administrative records cannot distinguish a woman who was never offered labour from one who was offered it and declined. A survey or qualitative component would separate the two and is the necessary next step.

Implications

Raising TOLAC uptake is a documented and transferable intervention. OptiBIRTH achieved it through clinician education, local opinion leaders, audit and structured discussion with women, without changing obstetric technique at all [9]. The Kazakh audit that identified women with a uterine scar as the largest contributor to the national caesarean rate reached the same practical conclusion five years ago, recommending vaginal birth for these women in the absence of contraindications [5]. Our data show what happens when that recommendation meets ordinary practice.

They also suggest a cheaper first step than a campaign. Before uptake is raised at all, the trials already being offered could be redirected, and the audit needed to do it requires four variables that every antenatal record already contains. A service unwilling to expand its trial volume can still be asked why 19 women with every favourable feature went to theatre without labour in a network where 34 of 35 comparable women who laboured gave birth vaginally. The safety case in these data is already made. What is missing is a process that puts the question to women whose obstetric profile answers it favourably, rather than only to those who have laboured before.

Conclusions

Among 1020 women in Karaganda region with one previous caesarean, 22% laboured and 72% of those delivered vaginally, with uterine rupture in 0.89% and lower composite maternal morbidity after vaginal birth than after planned repeat caesarean. Selection for labour was driven by previous vaginal delivery and maternal age, ignored interdelivery interval and body habitus, and used one factor that does not predict the outcome while omitting two that do. The same 224 trials, better directed, would have produced 194 vaginal births rather than 161, and the same 63 failed trials would have supported 365 labouring women and about 300 vaginal births. The constraint here is not the success rate and it is not safety. It is the offer, and where it is pointed. These findings are directly relevant to any maternity system that reports a high VBAC success rate alongside low uptake, and the audit that produced them can be repeated anywhere from four variables already held in routine records.

Abbreviations

ACOG
American College of Obstetricians and Gynecologists
aOR
adjusted odds ratio
BMI
body mass index
CI
confidence interval
PRCD
planned repeat caesarean delivery
RCOG

Royal College of Obstetricians and Gynaecologists
STROBE
Strengthening the Reporting of Observational Studies in Epidemiology
TOLAC
trial of labour after caesarean
VBAC
vaginal birth after caesarean.

Declarations

Ethics approval and consent to participate

The study was reviewed and approved by the Local Bioethics Committee of NJSC Karaganda Medical University, Karaganda, Kazakhstan, under the study title “Optimisation of the management of vaginal birth after one caesarean section in Karaganda region”. The approving document is the Letter of Conclusion of the Committee, issued as an Extract from Protocol No. 3 of the Committee meeting held on 4 February 2026, chaired by O. A. Visternichan. The application was registered under reference No. 27 on 21 January 2026 in the name of the principal investigator, Zh. K. Zharova. Because the analysis was retrospective and used de-identified records collected during routine clinical care, the Committee waived the requirement for individual informed consent. All procedures complied with the Declaration of Helsinki. The records analysed were collected during routine clinical care between 2017 and 2022; the retrospective analysis reported here was conceived afterwards, and ethical approval was therefore sought and granted in 2026, before any analysis of the dataset began. A copy of the approval document is available from the corresponding author and has been provided to the editorial office.

Consent for publication

Not applicable.

Competing interests

The authors declare no competing financial or non-financial interests as defined by Springer Nature.

Authors’ information

Not applicable.

Additional files

Additional file 1.

Analysis code (Python). Complete code reproducing every estimate reported in this article, including the two logistic models, the favourability stratification, both allocation counterfactuals, the stratified bootstrap and the sensitivity analysis.

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Author Contribution

ZhZ and DO conceived and designed the study. DO and MM developed the methodology. AU curated the data. MM performed the formal analysis. DO wrote the original draft. ZhZ reviewed and edited the manuscript. ZhZ supervised the work. All authors read and approved the final manuscript and agree to be accountable for all aspects of the work.

Acknowledgement

The authors declare no competing financial or non-financial interests as defined by Springer Nature.

Data Availability

The datasets generated and analysed during the current study are not publicly available because they derive from identifiable perinatal records held by the participating facilities and the conditions of the ethics approval do not permit public deposition, but a de-identified analysis dataset is available from the corresponding author on reasonable request. The complete analysis code is included with this published article as Additional file 1.

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Figures

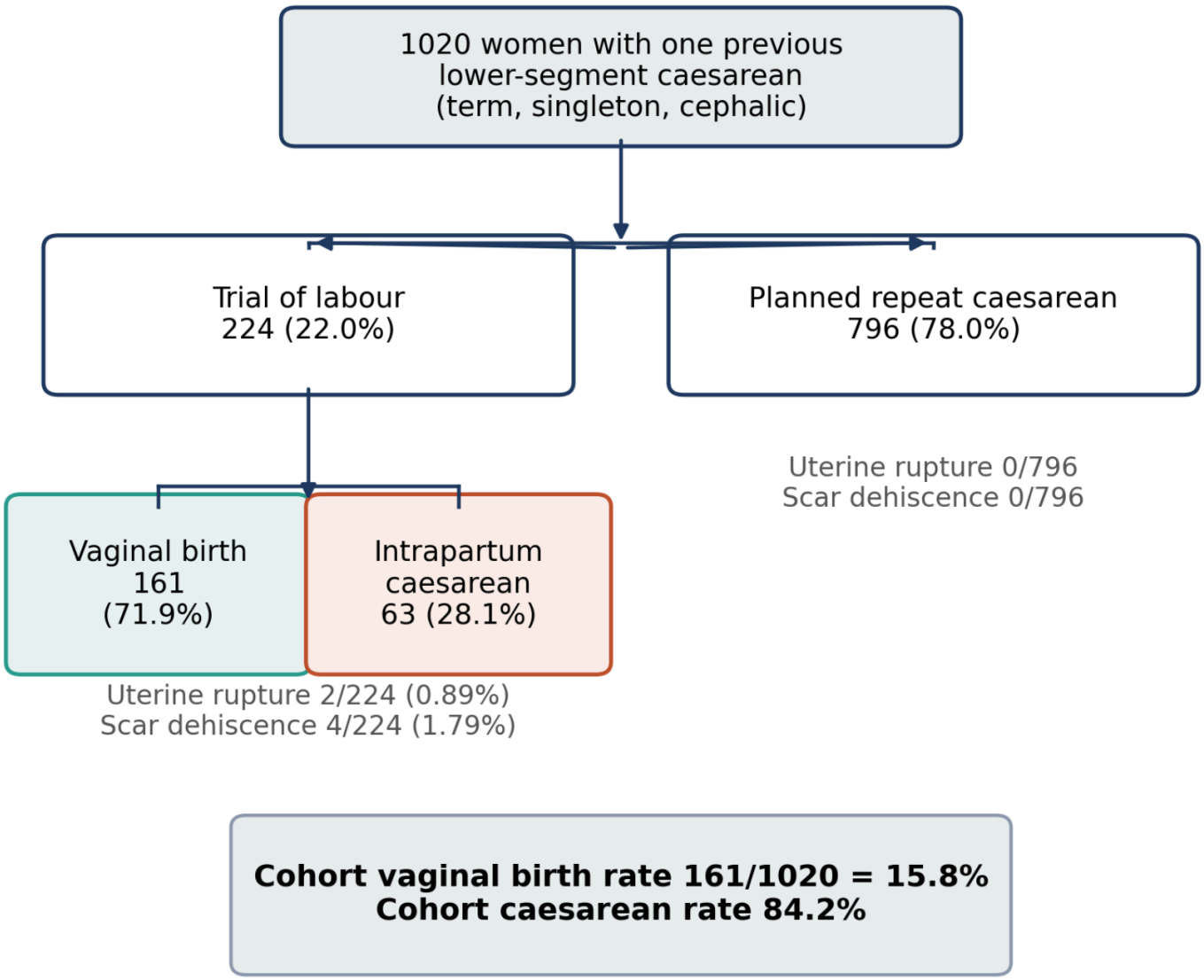

Figure 1

Cohort profile. The 1020 women with one previous lower-segment caesarean, by intended and actual mode of birth, with scar events and cohort-level delivery rates. The offer tracked previous vaginal delivery (adjusted odds ratio 6.26, 95% CI 4.40–8.90) and age (0.92 per year, 0.89–0.95), and was unrelated to interdelivery interval (1.04) or obesity (1.01), both of which predicted vaginal birth among women who laboured (2.56 and 0.52). Comparing the two sets of weights formally, only interdelivery interval differed significantly (p = 0.035; joint test p = 0.099). Composite maternal morbidity was equivalent for TOLAC and PRCD (0.78, 0.51–1.19) and not raised after a failed trial (1.35, 0.74–2.46). Redirecting the same 224 trials to the most favourable women would have yielded 194 vaginal births (178–207), not 161. Holding failed trials at the 63 that occurred, 365 women could have laboured and 302 delivered vaginally (228–374), lowering the caesarean rate to 70.4%.

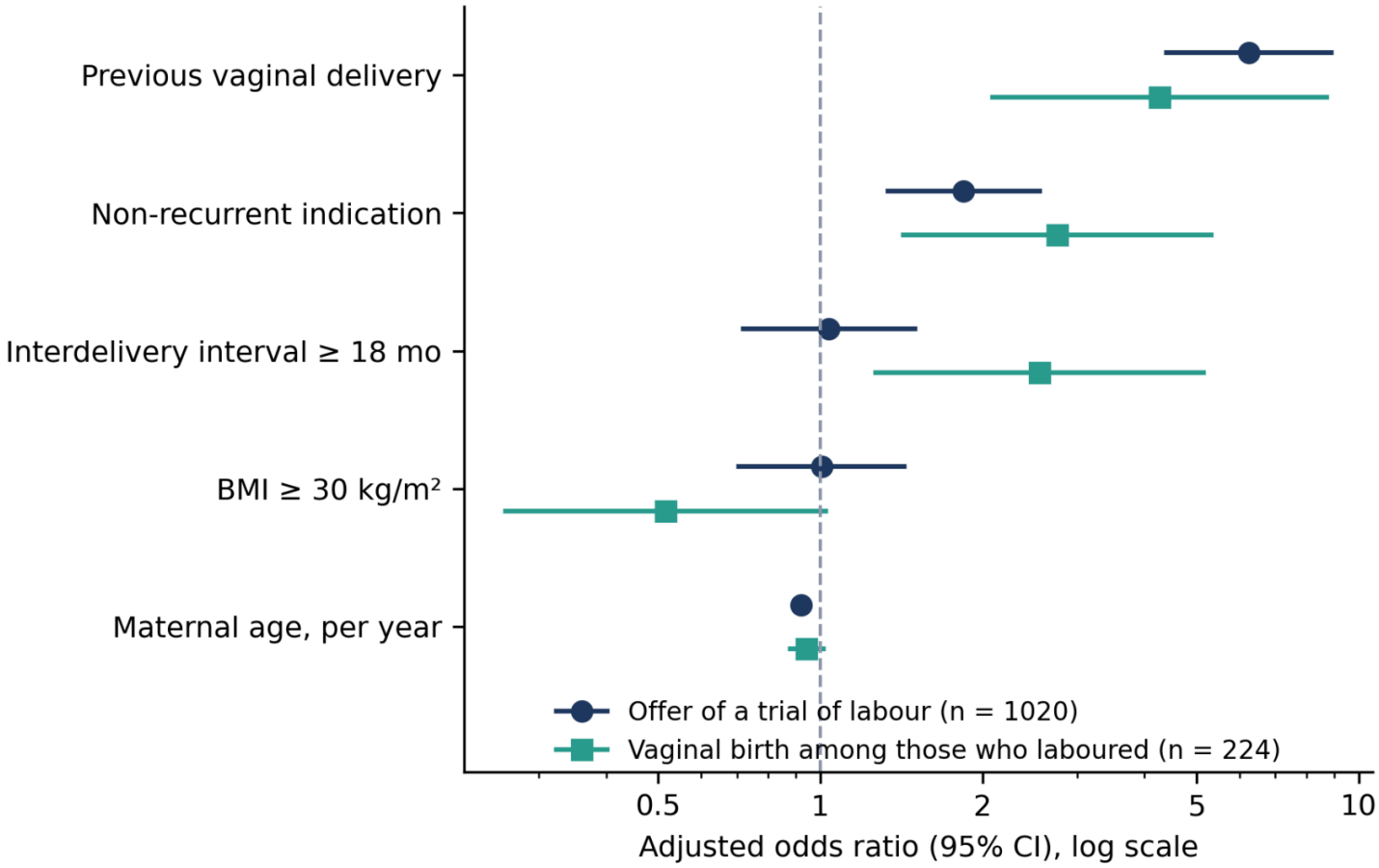

Figure 2

The same five antenatal covariates fitted to two outcomes: the offer of a trial of labour in all 1020 women, and vaginal birth among the 224 who laboured. Adjusted odds ratios with 95% confidence intervals, log scale.

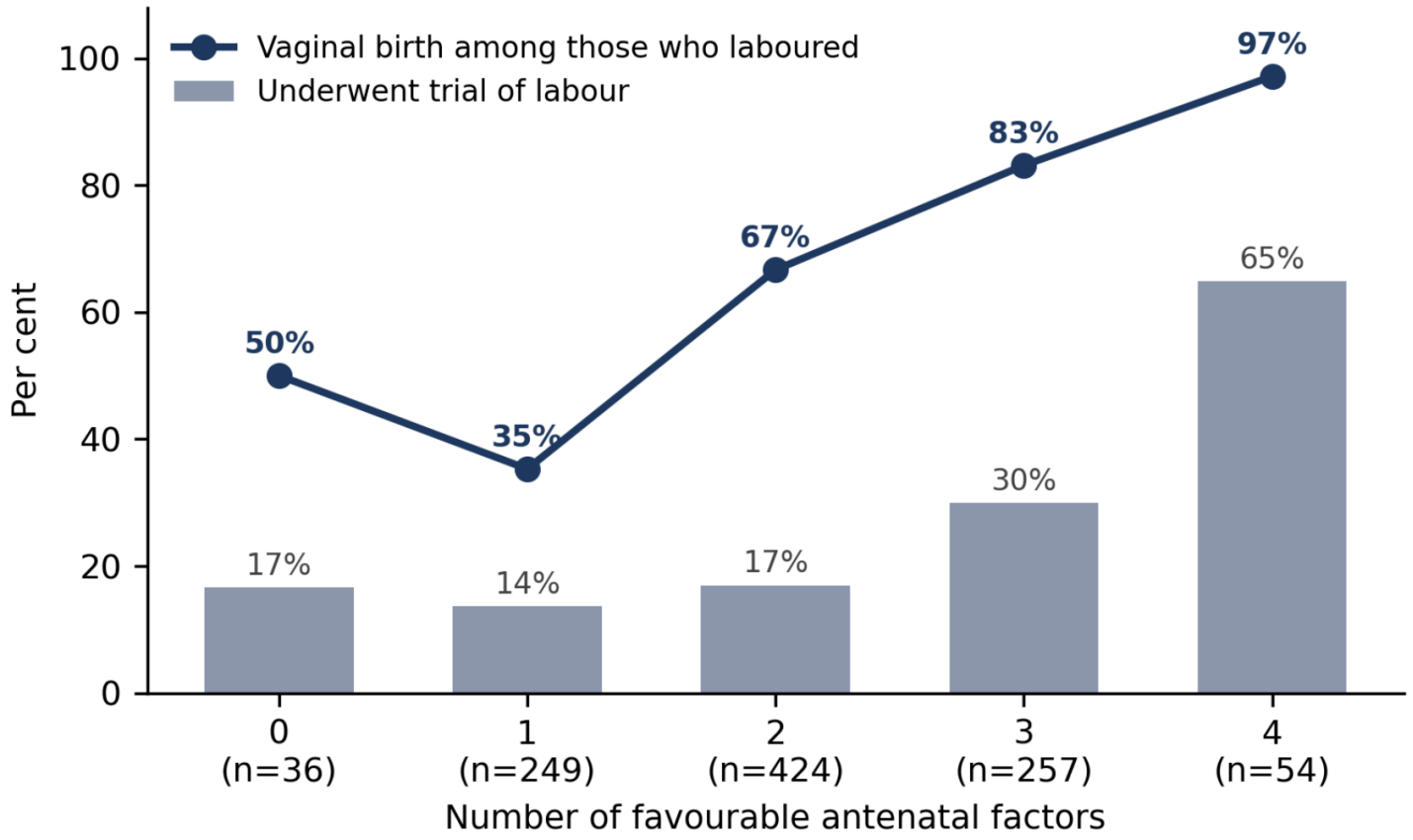

Figure 3

Trial of labour uptake and observed vaginal birth rate by number of favourable antenatal factors. Bars show the proportion of women in each stratum who underwent trial of labour. The line shows the proportion of those women who delivered vaginally.

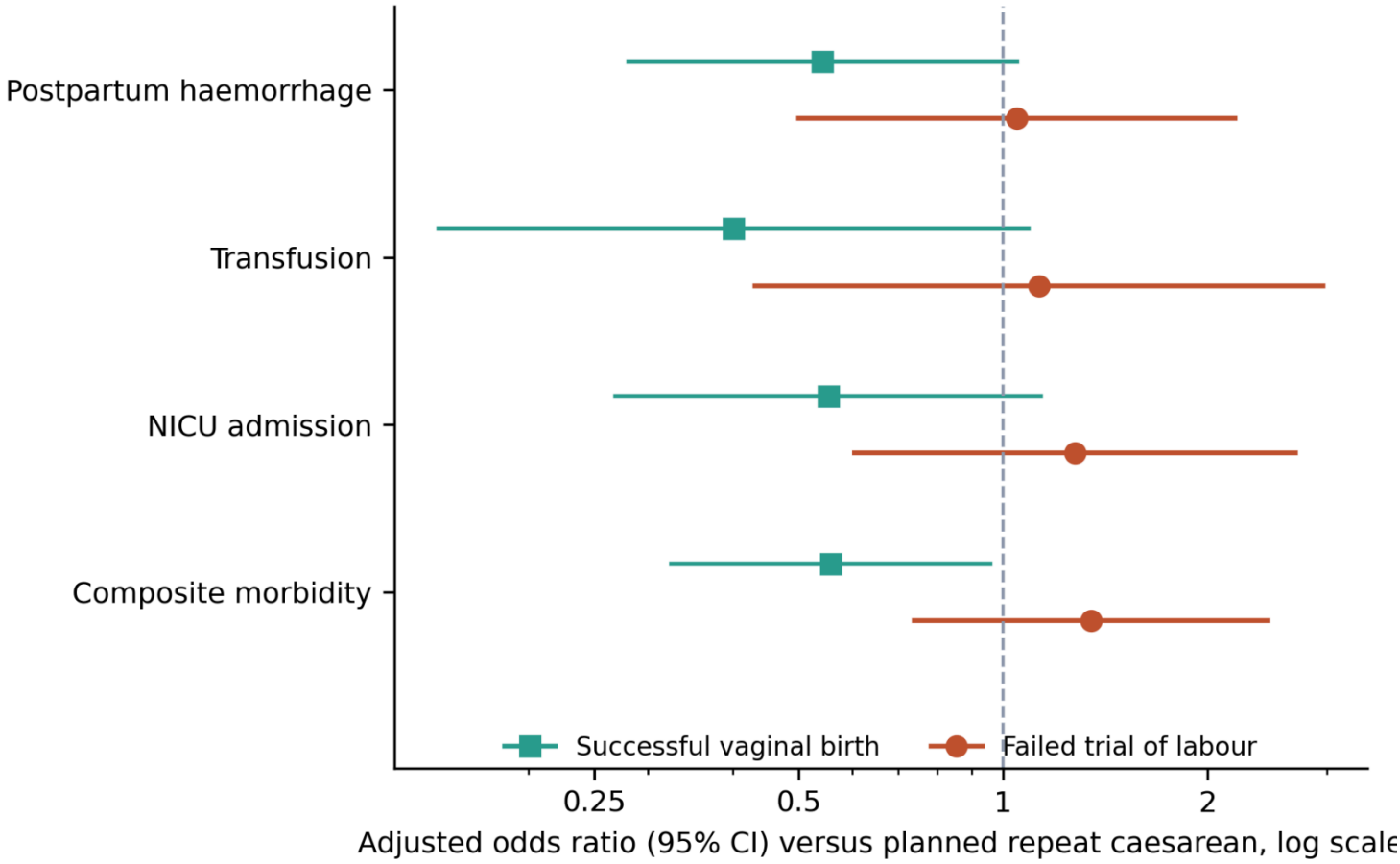

Figure 4

Adjusted odds ratios for maternal and neonatal outcomes versus planned repeat caesarean, shown separately for successful vaginal birth and for failed trial of labour.

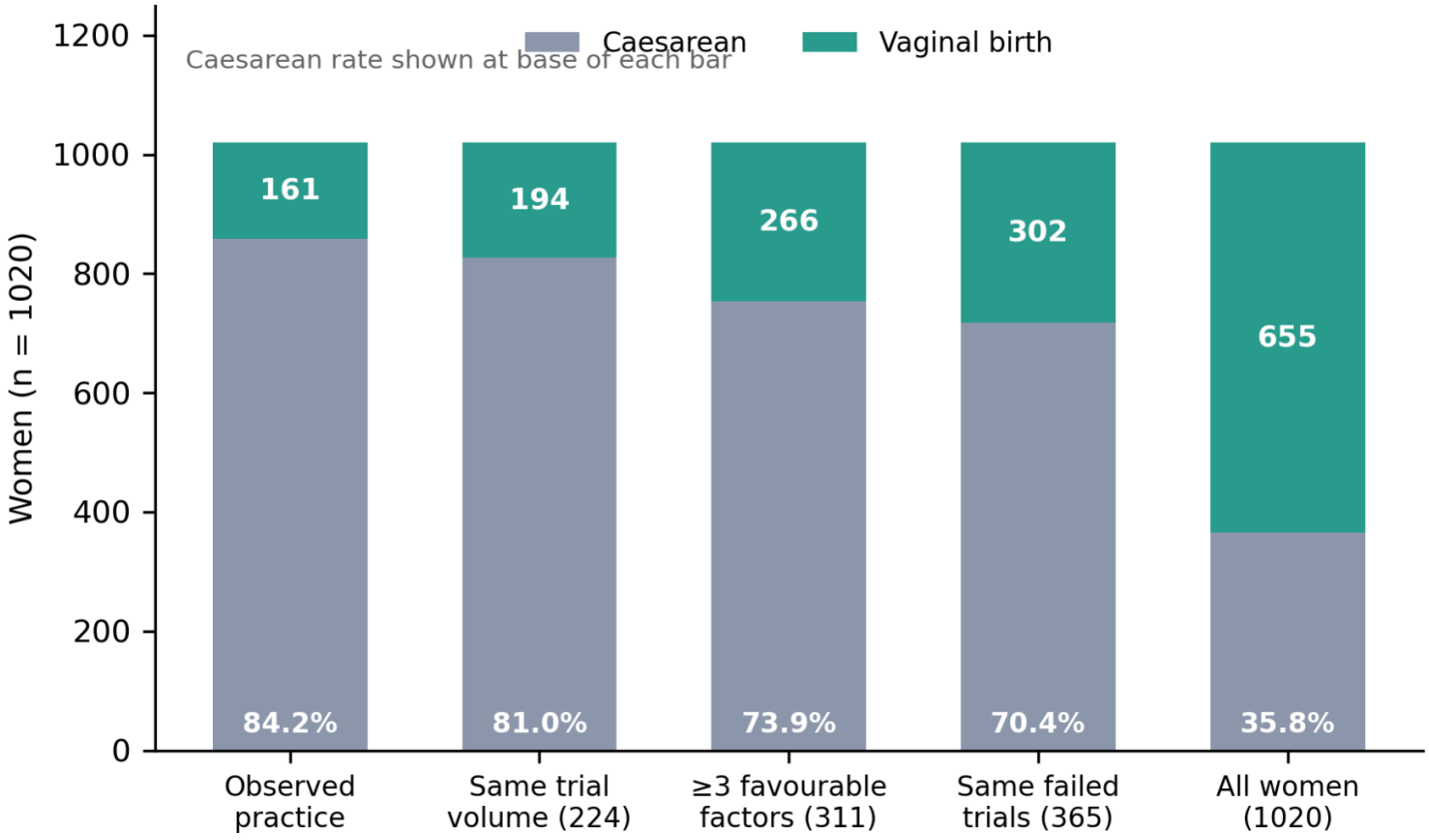

Figure 5

Cohort delivery mode under observed practice and four alternative allocations of trials of labour. Percentages at the base of each bar give the resulting caesarean rate.

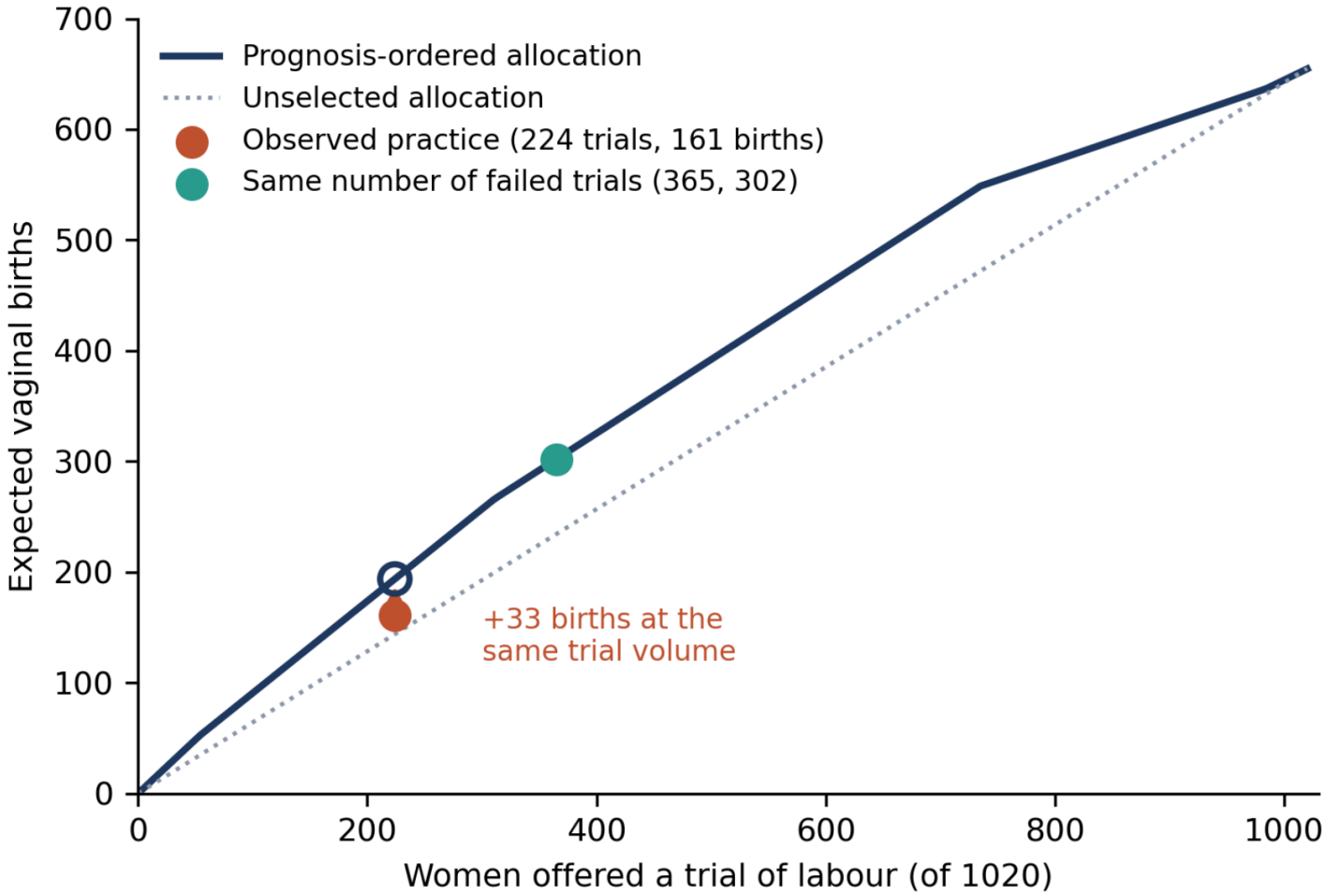

Figure 6

Expected vaginal births against the number of women offered a trial of labour under prognosis-ordered allocation. The observed practice point, 224 trials and 161 births, lies below the frontier. The fixed-failure point, 365 trials and 302 births, marks the allocation consuming the same 63 failed trials.

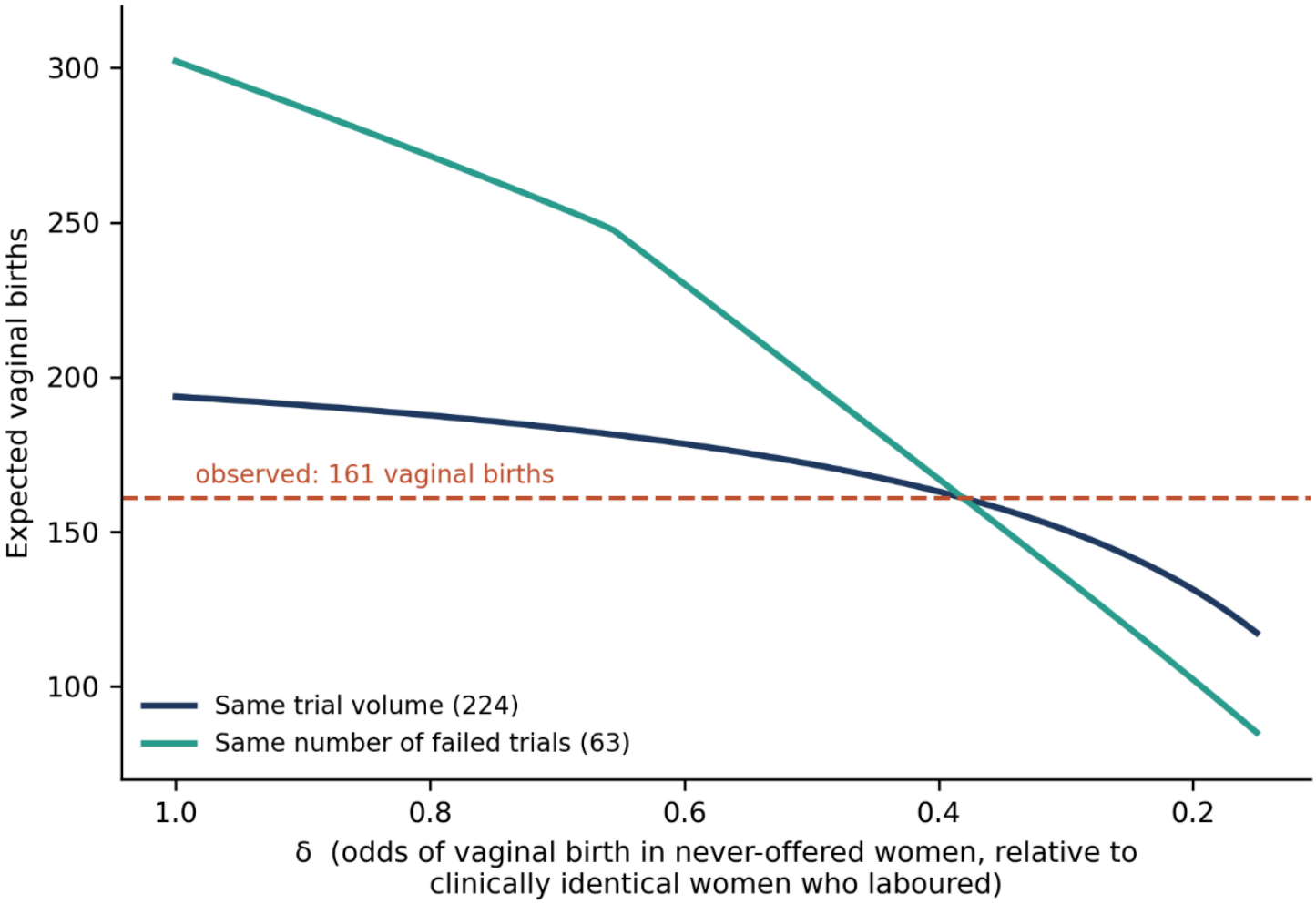

Figure 7

Sensitivity of both reordered allocations to unmeasured selection. δ is the odds of vaginal birth among never-offered women relative to clinically identical women who laboured.

Supplementary Files

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

- Additionalfile2STROBEchecklis.docx
- Additionalfile1analysiscode.py