Prenatal exercise (including but not limited to pelvic floor muscle training) and urinary incontinence during and following pregnancy: a systematic review and meta-analysis

Margie H Davenport,[1] Taniya S Nagpal,[2] Michelle F Mottola,[2] Rachel J Skow,[1] Laurel Riske,[1] Veronica J Poitras,[3] Alejandra Jaramillo Garcia,[3] Casey E Gray,[4] Nick Barrowman,[5] Victoria L Meah,[6] Frances Sobierajski,[1] Marina James,[1] Megan Nuspl,[7] Ashley Weeks,[8] Andree-Anne Marchand,[9] Linda G Slater,[10] Kristi B Adamo,[11] Gregory A Davies,[12] Ruben Barakat,[13] Stephanie-May Ruchat[14]

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For numbered affiliations see end of article.

Correspondence to
Dr Margie H Davenport, Program for Pregnancy and Postpartum Health, University of Alberta, Edmonton T6G 2E1, Canada;
mdavenpo@ualberta.ca

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ABSTRACT

Objective To examine the relationships between prenatal physical activity and prenatal and postnatal urinary incontinence (UI).

Design Systematic review with random effects meta-analysis and meta-regression.

Data sources Online databases were searched up to 6 January 2017.

Study eligibility criteria Studies of all designs were included (except case studies) if they were published in English, Spanish or French and contained information on the Population (pregnant women without contraindication to exercise), Intervention (subjective or objective measures of frequency, intensity, duration, volume or type of exercise, alone ["exercise-only"] or in combination with other intervention components [e.g., dietary; "exercise + co-intervention"]), Comparator (no exercise or different frequency, intensity, duration, volume and type of exercise) and Outcome (prenatal or postnatal UI).

Results 24 studies (n=15 982 women) were included. 'Low' to 'moderate' quality evidence revealed prenatal pelvic floor muscle training (PFMT) with or without aerobic exercise decreased the odds of UI in pregnancy (15 randomised controlled trials (RCTs), n=2764 women; OR 0.50, 95% CI 0.37 to 0.68, I^2=60%) and in the postpartum period (10 RCTs, n=1682 women; OR 0.63, 95% CI 0.51, 0.79, I^2=0%). When we analysed the data by whether women were continent or incontinent prior to the intervention, exercise was beneficial at preventing the development of UI in women with continence, but not effective in treating UI in women with incontinence. There was 'low' quality evidence that prenatal exercise had a moderate effect in the reduction of UI symptom severity during (five RCTs, standard mean difference (SMD) −0.54, 95% CI −0.88 to −0.20, I^2=64%) and following pregnancy (three RCTs, 'moderate' quality evidence; SMD −0.54, 95% CI −0.87 to −0.22, I^2=24%).

Conclusion Prenatal exercise including PFMT reduced the odds and symptom severity of prenatal and postnatal UI. This was the case for women who were continent before the intervention. Among women who were incontinent during pregnancy, exercise training was not therapeutic.

INTRODUCTION

Urinary incontinence (UI) is a common complaint in pregnancy with 18%–75% of women affected in late gestation.[1] Risk of UI increases as pregnancy progresses due to altered hormonal status and increased weight of the uterus on the pelvic floor.[1 2] Neurophysiological studies indicate that pregnancy, and specifically vaginal childbirth, may lead to weakening and trauma of pelvic floor muscles leading to increased risk of UI in the postpartum period.[3 4] Some women may experience progressively increasing bladder irritability as the fetus continues to grow and the uterus presses down on the bladder.[5] Elevated progesterone levels during pregnancy have also been suggested to decrease tone of the bladder and urethra.[6] Regardless of the potential mechanism, approximately one-third of pregnant women experience UI after childbirth.[7] Pelvic floor muscle training (PFMT) has the potential to strengthen pelvic floor muscles and therefore may be able to prevent weakening of the periurethral muscles.[7]

In women with UI before pregnancy, maximal vaginal squeeze pressure is reduced and may result in a greater severity of UI symptoms in the perinatal period.[7] Additionally, PFMT (the voluntary contraction and relaxation of the pelvic floor muscles)[8] has been recommended to specifically prevent UI by strengthening pelvic floor muscles to support the pelvic organs including the bladder, bladder neck and urethra during pregnancy.[9] A systematic review and meta-analysis of randomised controlled trials (RCTs) suggested prenatal PFMT did not prevent or treat prenatal UI or decrease symptom severity during pregnancy (prevention: three studies, 307 women; treatment: two studies, 304 women).[10] In contrast, prenatal PFMT reduced the odds of developing postpartum UI by 29%–50% in women who were continent during pregnancy (seven studies, 792 women);[10] these benefits did not extend to women who were incontinent during pregnancy.[10] However, the effectiveness of PFMT alone compared with the impact of other types of exercise (eg, aerobic or resistance training) on prevention and treatment of UI was not investigated.

Although UI is a common side-effect of pregnancy and can be associated with high impact of exercise such as jogging,[11 12] there is a paucity of research examining the effects of whole body exercise compared with, or in conjunction with, PFMT on perinatal UI. Aerobic exercise performed during pregnancy has many known benefits including prevention of excessive gestational weight gain and large for gestational age babies,[13 14] which are important risk factors for prenatal and postnatal UI.[15 16] Perales *et al*'s 2016 systematic review suggested that a combination of aerobic and resistance training during pregnancy may prevent UI.[17] We undertook a systematic review to better understand the effect of (1) PFMT alone, (2) other exercise alone (eg, aerobic and resistance training) and (3) PFMT in conjunction with other exercise.

The current systematic review and meta-analysis is part of a series of reviews that will inform the development of the *2019 Canadian guideline for exercise throughout pregnancy* (herein referred to as the *Guideline*).[18] The purpose was to evaluate the relationship between prenatal exercise and prenatal and postnatal UI prevalence and symptoms.

METHODS

In October 2015, a panel of key researchers, stakeholders and methodologists (ie, the Guidelines Consensus Panel) met to identify priority outcomes for inclusion in the update of the *Guideline*. UI was identified as an 'important' outcome (see process paper for complete list of critical and important outcomes). The Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) statement and checklist were used to guide this systematic review and meta-analysis.[19]

Protocol and registration

Two systematic reviews were undertaken to investigate the impact of prenatal exercise on fetal and maternal health outcomes and records identified through both processes were considered for inclusion in the current review. Each review was registered a priori with the International Prospective Register of Systematic Reviews (PROSPERO; fetal health: CRD42016029869; maternal health: CRD42016032376). Since the relationships between prenatal exercise and maternal/fetal health outcomes are examined in studies related to both maternal and fetal health, records retrieved from the searches for both of these reviews were evaluated for inclusion in the current review.

Eligibility criteria

The participants, interventions, comparisons, outcomes and study design (PICOS) framework was used to guide this review.[20]

Population

The population of interest was pregnant women without contraindication to exercise (as per the CSEP and American Congress of Obstetricians and Gynecologists (ACOG) guidelines).[21 22] Absolute contraindications to exercise are: ruptured membranes, premature labour, persistent second or third trimester bleeding, placenta praevia, preeclampsia, gestational hypertension, incompetent cervix, intrauterine growth restriction, high order pregnancy, uncontrolled type 1 diabetes, hypertension or thyroid disease or other serious cardiovascular, respiratory or systemic disorders. Relative contraindications to exercise are: a history of spontaneous abortion, premature labour mild/moderate cardiovascular or respiratory disease, anaemia or iron deficiency, malnutrition or eating disorder, twin pregnancy after 28 weeks or other significant medical conditions.[21 22]

Intervention (exposure)

The intervention/exposure of interest was objectively or subjectively measured prenatal exercise of any frequency, intensity, duration, volume or type (studies on exercise during labour were excluded). Exercises could be a single session (acute) or chronic (habitual activity). Interventions that consisted of exercise alone (termed 'exercise-only' interventions) or exercise combined with other interventions (eg, diet; termed 'exercise+cointerventions') were considered. Although exercise is a subtype of physical activity, the terms are used interchangeably in this review. Exercise and physical activity were defined as any bodily movement generated by skeletal muscles that resulted in energy expenditure above resting levels.[23] Studies that investigated prenatal exercise during labour were not eligible for inclusion.

Comparison

Eligible comparators were: no exercise; different frequency, intensity, duration, volume or type of exercise.

Outcome

Relevant outcomes were prevalence and symptoms of UI during the prenatal and/or postpartum period (up to 12 months postpartum).

Study design

Primary studies of any design were eligible, with the exception of case studies (n=1), narrative syntheses and systematic reviews.

Information sources

A comprehensive search was created and run by a research librarian (LGS) in the following databases: MEDLINE, EMBASE, PsycINFO, Cochrane Database of Systematic Reviews, Cochrane Central Register of Controlled Trials, Scopus and Web of Science Core Collection, CINAHL Plus with Full-text, Child Development & Adolescent Studies, ERIC, Sport Discus, ClinicalTrials.gov and the Trip Database up to January 6, 2017 (see online supplementary file 1 for complete search strategies).

Study selection and data extraction

The titles and abstracts of all articles identified in the search were screened against the inclusion criteria by two independent reviewers. Abstracts that were selected as eligible at level one by at least one reviewer were retrieved for level two screening as a full text article. Full text articles were screened by two independent reviewers against the study inclusion criteria. When a study was recommended by one or more reviewers for exclusion, further review was conducted by MHD and/or SMR for a final decision. If a decision could not be made, the characteristics of the study were presented to the Guidelines Steering Committee who oversaw the systematic reviews (MHD, MFM, SMR, CG, VP, AJG and NB) for a final decision regarding inclusion/exclusion by consensus. Studies identified by the maternal and fetal search strategies were imported into DistillerSR for deduplication and data extraction and are subsequently considered as one review.

Data extraction was completed in DistillerSR and data extraction tables were created in consultation with methodological experts and the Guidelines Steering Committee. Data from records that met the inclusion criteria were extracted by one person and independently verified by a content expert (MHD, MFM or SMR). For studies where multiple publications exist, the most recent or complete publication was selected as the 'parent' paper; however, relevant data from all publications

were extracted. Extracted data included study characteristics (ie, year, study design, country), characteristics of the population (eg, number of participants, age, pre-pregnancy body mass index (BMI), parity and pregnancy complications including pre-eclampsia, gestational hypertension and gestational diabetes), intervention/exposure (prescribed and/or measured exercise frequency, intensity, time and type, intervention duration, measurement tool) and outcomes (prevalence and symptoms of UI). If data were unavailable for extraction, authors were contacted to request additional information. See supplementary table 1 for included study characteristics.

Quality of evidence assessment

The Grading of Recommendations Assessment, Development and Evaluation (GRADE) framework was used to assess the quality of evidence across studies for each study design and health outcome.

Accordingly, evidence from RCTs was considered 'high' quality and evidence from non-randomised studies was considered 'low quality' unless it was graded down based on concerns with risk of bias, indirectness, inconsistency or imprecision because the presence of these factors reduce the level of confidence in the observed effects. Evidence from all non-randomised intervention and observational studies began with a 'low' quality rating.[24]

The risk of bias associated with each included study was independently assessed by two reviewers. The risk of bias in RCTs and non-randomised intervention studies was assessed following the Cochrane Handbook[25] and risk of bias in observational studies was assessed using the characteristics recommended by Guyatt *et al*,[26] consistent with systematic reviews conducted to support previous health behaviour guidelines.[27] All studies were assessed for potential sources of selection bias, reporting bias, performance bias, detection bias, attrition bias and 'other' sources of bias. Risk of bias across studies was rated as 'serious' when studies with the greatest influence on the pooled result (assessed using weight (%) given in forest plots or sample size in studies that were narratively synthesised) presented 'high' risk of bias. The greatest influence on the pooled result was determined as follows: the studies that had the greatest individual % contribution in the meta-analyses, when taken together, contribute to >50% of the weight of the pooled estimate. Additionally, studies were considered to reflect a serious risk of bias when the sample size of narratively synthesised studies was similar to the total sample size of studies contributing to >50% of the weight of the pooled estimate in the meta-analyses. Given the nature of exercise interventions, it is not possible to blind participants to group allocation, and selection risk of bias was rated as 'low' if this was the only source of bias identified. Performance bias was rated as 'high' when<60% of participants performed 100% of prescribed exercise sessions or attended 100% of counselling sessions (defined as low compliance) or when compliance to the intervention was not reported. Attrition bias was rated as 'high' when >10% of data were missing at the end of the study and intention-to-treat analysis was not used.

Inconsistency across studies was considered serious when heterogeneity was high (I^2 ≥50%) or when only one study was assessed (I^2 unavailable). Indirectness was considered serious when the effect of exercise+cointervention on an outcome was assessed. Imprecision was considered serious when the 95% CI crossed the line of no effect and was wide, such that interpretation of the data would be different if the true effect were at one end of the CI or the other. When only one study was assessed, imprecision was not considered serious, because inconsistency was already considered serious for this reason. Finally, publication bias was assessed if possible (ie, at least 10 studies were included in the forest plot) via funnel plots (see online supplementary file 1). If there were fewer than 10 studies, publication bias was deemed non-estimable and not rated down. If there were no important threats to validity, evidence was eligible to be upgraded if there was a large magnitude of effect, there was evidence of a dose-response gradient in the findings or all plausible confounding factors were accounted for.[24]

Original plans for two people to independently assess the quality of the evidence across each health outcome were amended for feasibility reasons. As such, one reviewer evaluated the quality of the evidence and a second person checked the GRADE tables as a quality control measure. GRADE tables are presented in online supplementary tables 2 and 3.

Evidence synthesis: statistical analysis and narrative synthesis

Statistical analyses were conducted using Review Manager V.5.3. (Cochrane Collaboration, Copenhagen, Denmark). ORs were calculated for all dichotomous outcomes using inverse-variance weighting and a random effects model. As severity of UI was assessed using multiple tools, standardised mean differences (SMDs) were calculated when different tools were used for a single outcome. SMD effect sizes were calculated in Review Manager V.5.3 using Hedges' g method, and significance was set at $p<0.05$. Effect sizes of 0.2, 0.4 and 0.8 were considered small, moderate and large, respectively.[28]

A staged approach was used to determine if there was sufficient evidence from high quality study designs (ie, RCTs) to inform the *Guideline* or if lower quality study designs needed to be examined. If meta-analyses of RCTs contained data from fewer than 2000 women, the impact of prenatal exercise on the specific outcome was examined further using observational evidence (first non-randomised interventions, followed by cohort, cross-sectional and case-control studies). For RCTs and non-randomised intervention studies, sensitivity analysis was performed to evaluate whether the effects were different when examining relationships between exercise-only interventions (including standard care) versus exercise+cointerventions and prevalence or severity of UI. When possible, a priori subgroup analyses were conducted for exercise-only interventions and observational studies. These were: (1) women diagnosed with diabetes (gestational, type 1 or type 2) compared with women without diabetes ('general population'); (2) women with pre-pregnancy overweight or obesity status (mean BMI>25.0 kg/m^2) compared with women with pre-pregnancy normal or underweight status (mean BMI<25 kg/m^2, which may include individuals with BMI>25.0 kg/m^2); (3) women>35 years of age compared with women<35 years of age; (4) previously inactive compared with previously active women (as defined by individual study authors) and (5) type of exercise conducted comparing studies including PFMT, whole body exercise or both and (6) studies including women who were continent or incontinent prior to the intervention. If a study did not provide sufficient detail to allow for inclusion in subanalyses, then a third group called 'unspecified' was created. The per cent of total variability that was attributable to between-study heterogeneity (ie, not to chance) was expressed using I^2.

In order to identify a clinically meaningful decrease, dose response meta-regression[29–31] was carried out by weighted no-intercept regression of mean differences with a random effects model for study, using the metafor package[32] in R V.3.4.1.[33] It was determined that an accepted cut-point for a clinically

meaningful decrease does not exist in the literature. As such, a reduction of 25% was chosen based on expert opinion. Models did not include an intercept term since the mean difference is assumed to be zero when the exercise dose is zero. Restricted cubic splines with knots at the 10th, 50th and 90th percentiles of the explanatory variable[34] were used to investigate whether there was evidence for a nonlinear relationship. Fitting was performed by maximum likelihood, and nonlinearity was assessed using a likelihood ratio test. When the model was statistically significant at $p<0.05$, the minimum exercise dose to obtain a clinically significant benefit was estimated by the minimum value of the explanatory variable at which the estimated OR was less than 0.75. Linear models were presented unless the fit of the spline was significantly better ($p<0.05$). In studies where there were no observed events in the intervention or control groups, data were entered into forest plots, but were considered 'not estimable' and excluded from the pooled analysis as per the recommendation in the Cochrane Handbook.[25]

For outcomes or for subsets of studies where a meta-analysis was not possible, a narrative synthesis of the results was presented by study design, organised around each outcome. Unless otherwise specified, studies were not included in meta-analyses if data were reported incompletely (ie, or for example, SD, SE or number of cases/controls not provided), if data were adjusted for confounding factors or if the study did not include a non-exercise control group). In studies where data were included in the meta-analysis but additional information was available that could not be meta-analysed, the studies were included in both the meta-analysis and narrative synthesis. One study had inconsistencies in the reporting of their data, and the author was contacted to confirm and assure that the data were correctly extracted.[35]

RESULTS

Study selection

A PRISMA diagram of the search results, including reasons for exclusion, is shown in figure 1. The a priori registered study protocol was amended to exclude studies that were not published in English, French or Spanish for feasibility reasons. A comprehensive list of excluded studies is presented in the online supplementary file 1. Consistent with the planned staged approach, when fewer than 2000 participants were included in RCTs, data were considered from other study designs.

Study characteristics

Twenty-four studies (n=15 982 women)[11 35–57] from 12 countries and 4 continents were included in this systematic review. There were 18 RCTs, 2 non-randomised intervention and 4 cohort studies identified. Among the RCTs, 16 were exercise only.[35 37 41 43 51 5738 40 46 48 49 52–56] and 2 were exercise+cointerventions.[36 39] The cointerventions included education about diet[36] and strategies to use during labour (eg, breathing exercises[39]). Among the included exercise interventions, the frequency of exercise ranged from 1 to 7 days per week, the duration of exercise ranged from 8 to 60 min per session, the intensity of exercise ranged from light to moderate and the types of exercise included aerobic exercise and PFMT. Exercise was initiated in the first to the early third trimester (9–30 weeks gestation). UI was assessed using self-reported questionnaires of leakage and severity of UI symptoms,[11 35–52 57] the International Consultation on Incontinence Questionnaire,[11 38–44 52] the Bristol Female Lower Urinary Tract Symptoms questionnaire,[47 53] the Urogenital Distress Inventory-6 and Incontinence Impact Questionnaire-7 (38, 55), the Sandvik's Severity Index,[36] Overactive Bladder Questionnaire,[56] the Urinary Distress Inventory[56] or other self-reported assessments including 3–7-day bladder diaries[37 55] and the 1-hour pad test[35 41 55] or the Questionnaires were based on standardised assessment methods. Additional details regarding the included studies can be found in the online supplementary table 1.

Quality of evidence

Overall, the quality of evidence ranged from 'very low' to 'high' (see online supplementary tables 2 and 3). The most common reasons for downgrading the quality of evidence were (1) serious risk of bias and (2) serious inconsistency. Common sources of bias included poor or unreported compliance and inappropriate treatment of missing data when attrition rate was high. When possible to examine, no evidence of publication bias was observed.

Synthesis of data

Prevalence of prenatal urinary incontinence

There was 'low' quality evidence from 15 RCTs (n=2764) regarding the association between prenatal exercise and prenatal UI. Findings indicated that prenatal exercise resulted in a 50% reduction in the odds of developing prenatal UI compared with no exercise (OR 0.50, 95% CI 0.37 to 0.68, $I^2=60\%$, see online supplementary figure 1).[36–39 41 43 46 48 49 51–54 56 57] The quality of evidence was downgraded from 'high' to 'low' due to serious inconsistency and serious indirectness due to the inclusion of cointerventions. One study that could not be included in the meta-analysis[36] reported a 40% reduction in the odds of developing UI following the exercise intervention after adjusting for baseline urinary leakage (OR 0.60, 95% CI 0.40 to 0.90).

Sensitivity analysis

The pooled estimate for the exercise-only interventions was not significantly different than the pooled estimate for the exercise+cointerventions (p=0.99). Both exercise-only interventions and exercise+cointerventions reduced the odds of developing UI during pregnancy (online supplementary figure 1).

Subgroup analysis

The tests for a priori subgroup analyses performed for exercise-only interventions were not statistically significant (p=0.69) (figure 2, online supplementary figure 3).

Other study designs

Findings from one non-randomised intervention (n=110; OR 0.35, 95% CI 0.10 to 1.20; 'very low' quality evidence, downgraded due to serious risk of bias and serious inconsistency, online supplementary figure 4)[42] and three cohort studies (n=1843; OR 1.05, 95% CI 0.84 to 1.30, $I^2=0\%$; 'very low' quality evidence, downgraded due to serious risk of bias and serious imprecision, online supplementary figure 5)[44 45 47] found no significant relationship between prenatal exercise and odds of prenatal UI.

Severity of prenatal urinary incontinence symptoms

There was 'low' quality evidence from five exercise-only interventions showing a moderate reduction in the severity of prenatal UI symptoms with prenatal exercise (n=465; SMD −0.54, 95% CI −0.88 to −0.20, $I^2=64\%$; online supplementary figure 6).[46 48 52 55 56] The quality of evidence was downgraded

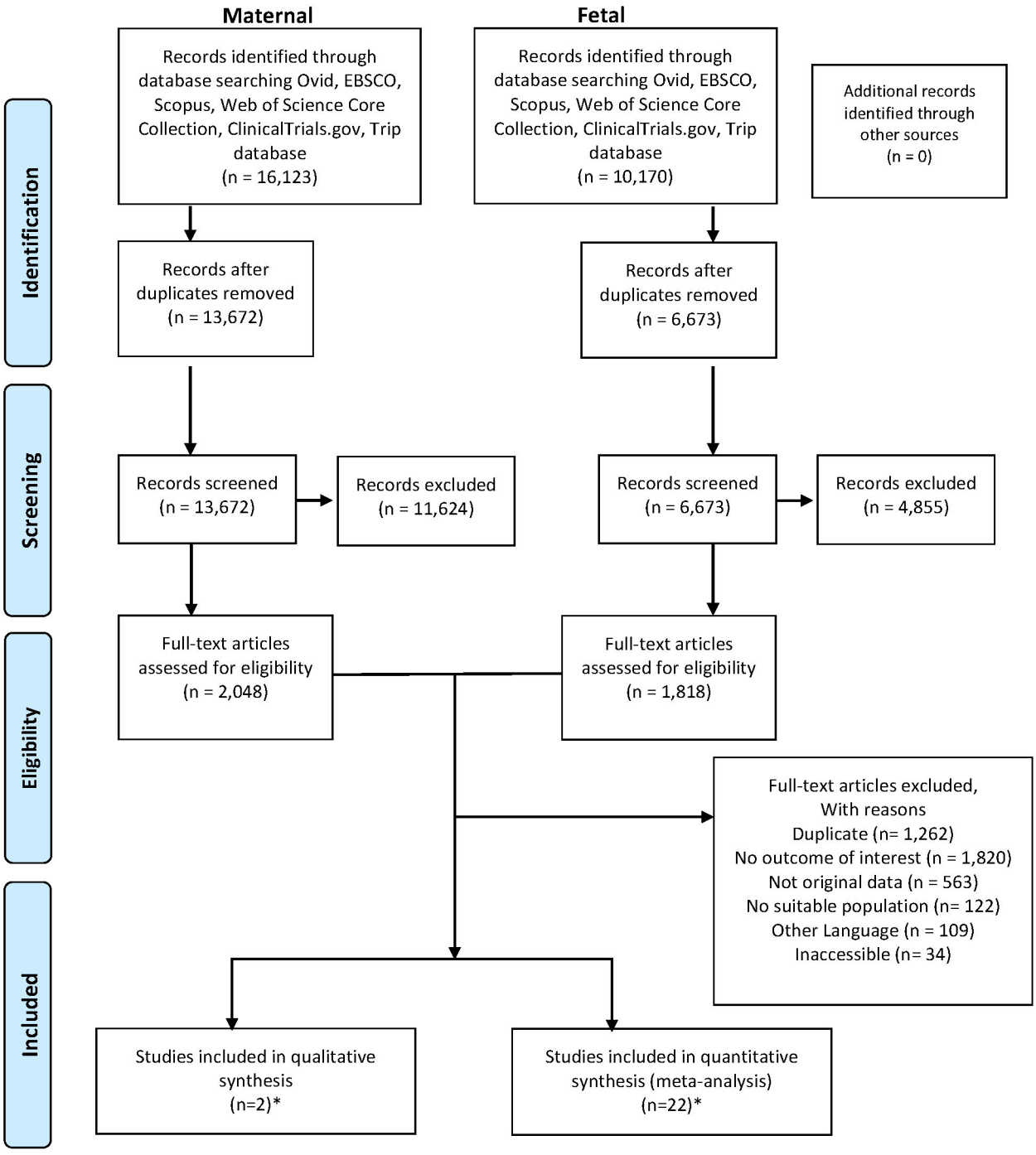

Figure 1 PRISMA flow diagram. *Among the 24 studies included, one study was included in both the qualitative and quantitative synthesis. Twenty-three studies were included in the quantitative synthesis; however, one study[58]) only provided follow-up information for another study[35] and therefore was not considered as an included study. PRISMA, Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

from 'high' to 'low' because of serious risk of bias and serious inconsistency.

Sensitivity analysis

No sensitivity analysis was conducted since there were only exercise-only interventions reporting results on the severity of prenatal UI.

Subgroup analysis

The tests for subgroup differences performed for exercise-only interventions were not statistically significant (see online supplementary figures 5 to 7).

Other study designs

There was 'very low' quality evidence (downgraded due to serious risk of bias, serious inconsistency and serious imprecision) from two non-randomised interventions showing no reduction in the severity of prenatal UI symptoms with prenatal PFMT (n=176; SMD −0.93, 95% CI −2.32 to 0.45, I^2=94%; online supplementary figure 8).[42 50]

Prevalence of postnatal UI

There was 'moderate' quality evidence from 11 RCTs (n=1851)[35 58] showing that exercise-only interventions reduced the odds of developing postpartum UI by 37% (pooled estimate

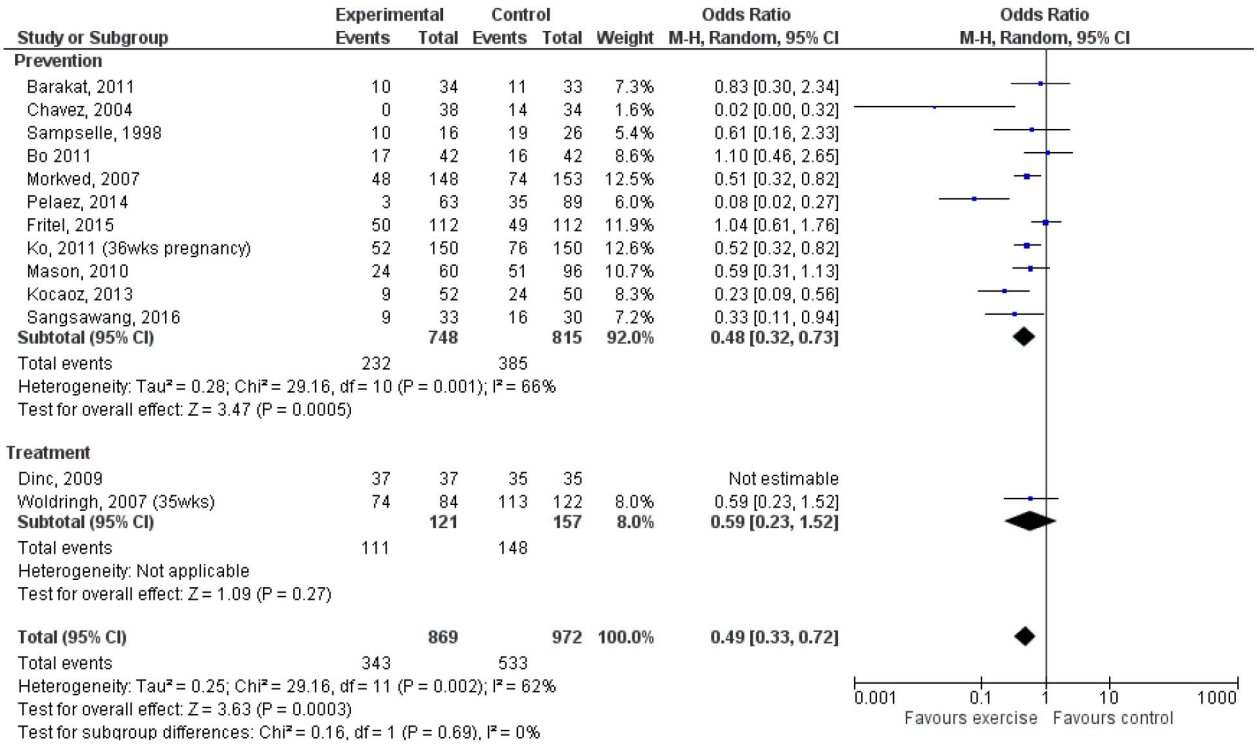

Figure 2 Effects of prenatal exercise compared with control on odds of urinary incontinence during pregnancy (RCTs). Subgroup analyses were conducted with studies including women who were continent ('prevention') and with those including women who were incontinent ('treatment') prior to the intervention. Analysis was conducted using a random effects model. M-H, Mantel-Haenszel method; RCTs, randomised controlled trials.

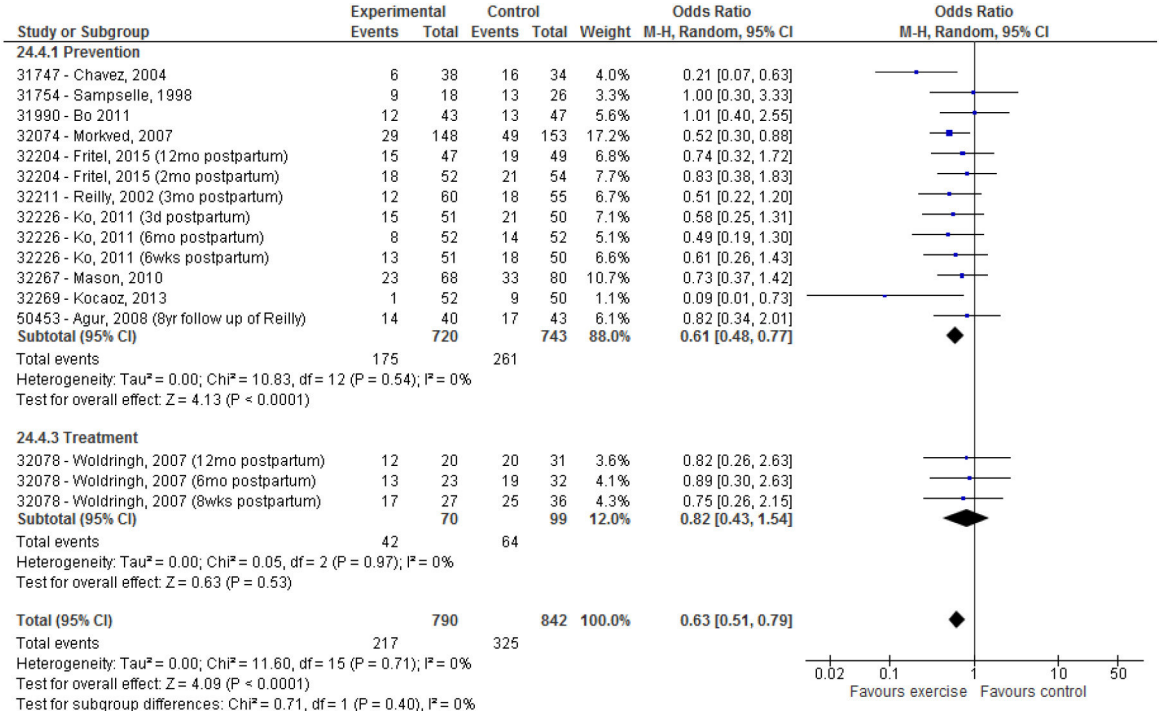

Figure 3 Effects of prenatal exercise compared with control on odds of urinary incontinence during postpartum (RCTs). Subgroup analyses were conducted with studies including women who were continent ('prevention') and with those including women who were incontinent ('treatment') prior to the intervention. Analysis conducted using a random effects model. M-H, Mantel-Haenszel method; RCTs, randomised controlled trials.

based on 10 RCTs; OR 0.63, 95% CI 0.51 to 0.79, I^2=0%; see figure 3).[35 37 41 43 46 49 51 53 54 57] The quality of evidence was downgraded from 'high' to 'moderate' because of serious risk of bias. One study that could not be included in the meta-analysis reported no difference in the prevalence of UI at 6 months postpartum between women who were randomised to a prenatal PFMT intervention (n=108) and those who were randomised to a control group (n=111).[40]

Sensitivity analysis

No sensitivity analysis was conducted since there were only exercise-only interventions reporting results on odds of developing postpartum UI.

Subgroup analysis

The tests for subgroup differences performed for exercise-only interventions were not statistically significant (see figure 3, online supplementary figure 9).

Other study designs

There was 'very low' quality evidence (downgraded due to serious risk of bias and serious inconsistency) from one non-randomised intervention (n=40) indicating that in women who were continent prior to the intervention, prenatal exercise did not reduce the odds of developing postpartum UI (OR 0.09; 95% CI 0.00 to 1.78; see online supplementary figure 11).[35]

There was 'very low' quality evidence (downgraded due to serious risk of bias and serious inconsistency) from one cohort study (n=10 098) that could only be reported narratively. The study showed that women who exercised frequently during pregnancy (various types of exercise on most days of the week) were more likely to develop UI at 6 weeks (OR 1.21, 95% CI 1.06 to 1.39) and 6 months (OR 1.63, 95% CI 1.39 to 1.92) postpartum compared with women who exercised occasionally (various types of exercise once or twice per week).[11]

Severity of postnatal UI symptoms

There was 'moderate' quality evidence from three RCTs (n=284) indicating that prenatal PFMT had a moderate effect in reducing the severity of postpartum UI symptoms (SMD −0.54, 95% CI −0.87 to −0.22, I^2=24%; online supplementary figure 12).[46 55 56] The quality of evidence was downgraded from 'high' to 'moderate' because of serious risk of bias.

Subgroup analyses

Additional a priori subgroup analyses were not conducted as these subgroups were not examined in the included studies.

Meta-regressions

Minimum exercise thresholds required to achieve a clinically meaningful reduction (ie, 25%) reduction in prenatal UI were identified as follows (see online supplementary figures 13–16): 2.2 metabolic equivalents (METs; light intensity), 27.2 min per session, 4 days per week or 554 MET min per week. The results of the meta-regression analyses are presented in the online supplementary file 1 (*Meta-regressions*).

DISCUSSION

PFMT with or without other types of exercise initiated during pregnancy was associated with a decreased risk of developing prenatal and postnatal UI by 50% and 37%, respectively. In women who were continent prior to intervention, PFMT was effective in preventing UI and also reduced symptom severity during pregnancy and into the postpartum period among women who developed UI. Prenatal exercise was not effective in treating women who were incontinent before intervention, but did reduce symptom severity both during and following pregnancy.

A cross-sectional study of 495 women found that 71% reported UI in the last 4 weeks of pregnancy, and they rated this as having a severe impact on their quality of life.[59] The current findings suggest that PFMT is an effective prevention strategy for prenatal and postnatal UI, similar to findings in non-pregnant women.[60–62] PFMT is advocated as the primary treatment and preventative intervention for UI in non-pregnant women as it effectively strengthens the pelvic floor muscles.[60–62] The results of the present systematic review and meta-analysis support the use of PFMT during pregnancy.

There was limited evidence from two studies evaluating the effectiveness of PFMT on the treatment of prenatal UI.[37 56] Although improvements were not observed, compliance with the intervention was low in one intervention (37%)[37] and not reported in the other.[56] Two reviews which combined data from pregnant and non-pregnant populations suggested that compliance to PFMT interventions can treat UI.[60 61] Additionally, a systematic review including 1051 non-pregnant women across 18 RCTs suggested PFMT was 8 times more likely to successfully treat UI than non-activity.[62] As previous literature on UI treatment has focused on non-pregnant populations, additional research investigating the potential of treating UI with PFMT during the perinatal period is warranted.

There have been mixed findings regarding the effects of exercise on prenatal UI. Whole body exercise such as walking has been suggested to strengthen the pelvic floor muscles as well as supporting muscles, such as the lower back, and assist with supporting the increased weight of the uterus.[63] In contrast, there is literature suggesting that high-impact activities including aerobic exercise may weaken pelvic floor muscles by increasing intra-abdominal pressure and this can lead to involuntary leakage in non-pregnant women.[64–66] Results of the current systematic review demonstrated a favourable association between prenatal UI and PFMT, with or without aerobic exercise. No studies that examined the impact of whole body exercise without concurrent

What is already known?

- Urinary incontinence (UI) is common during pregnancy and impacts up to 75% of pregnant women in late gestation and into the postnatal period.
- Exercise, specifically pelvic floor muscle training (PFMT), is an effective way to prevent and treat UI during pregnancy. The impact of prenatal PFMT alone, aerobic exercise alone or a combination of PFMT and aerobic exercise on prenatal and postnatal UI (prevention and treatment) is not yet known.

What are the new findings?

- PFMT with aerobic exercise reduced the odds of prenatal and postnatal UI in women by 50%.
- PFMT without aerobic exercise reduced the odds of prenatal and postnatal UI in women by 37%.
- Among women who were incontinent during pregnancy, exercise training was not therapeutic. PFMT with or without aerobic exercise can reduce the severity of UI symptoms during pregnancy and in the postnatal period.

PFMT on prevalence or severity of UI were identified and thus no conclusions could be made about the effect of whole body exercise without concurrent PFMT on prenatal UI prevention. Additional research examining the impact of non-PFMT exercise during pregnancy as well as the optimal dose of exercise (PFMT and non-PFMT exercise) is warranted.

The strength of our systematic review includes the incorporation of studies looking at PFMT alone and in conjunction with another aerobic exercise, which has not been examined in previous reviews in pregnant and non-pregnant populations. Limitations of the current systematic review include the high heterogeneity that was not reduced with subgroup analysis. However, this is similar to reviews in other populations and may be related to the tools used to evaluate UI[7 10 60 67 68] and or the variability in adherence across studies. A limitation of the evidence base is that a combination of women with and without continence were included in the studies, which may have masked the effectiveness of the interventions. Additionally, no studies evaluated the impact of exercise without PFMT, limiting the ability to draw conclusions on the relationship between prenatal exercise and UI. A limitation of this study is that we were unable to identify evidence-based cut-points for clinically meaningful changes in study outcomes. Accordingly, it is possible that the results may have overestimated or underestimated the relevance of the findings.

In conclusion, this systematic review and meta-analysis demonstrates prenatal PFMT alone or in combination with other forms of exercise was effective in reducing the odds and symptom severity of UI during pregnancy and the postpartum period. Additional high-quality RCTs are needed to evaluate the effectiveness of exercise in treating UI in women with incontinence.

Author affiliations
[1]Program for Pregnancy and Postpartum Health, Physical Activity and Diabetes Laboratory, Faculty of Kinesiology, Sport and Recreation, Women and Children's Health Research Institute, Alberta Diabetes Institute, University of Alberta, Edmonton, Alberta, Canada
[2]R. Samuel McLaughlin Foundation-Exercise and Pregnancy Laboratory, School of Kinesiology, Faculty of Health Sciences, Department of Anatomy and Cell Biology, Schulich School of Medicine and Dentistry, Children's Health Research Institute, The University of Western Ontario, London, Ontario, Canada
[3]Independent researcher, Ottawa, Ontario, Canada
[4]Healthy Active Living and Obesity Research Group, Children's Hospital of Eastern Ontario Research Institute, Ottawa, Ontario, Canada
[5]Clinical Research Unit, Children's Hospital of Eastern Ontario Research Institute, Ottawa, Ontario, Canada
[6]Cardiff School of Sport and Health Sciences, Cardiff Metropolitan University, Cardiff, UK
[7]Alberta Research Centre for Health Evidence, Faculty of Medicine and Dentistry, University of Alberta, Edmonton, Alberta, Canada
[8]School of Interdisciplinary Health Sciences, University of Ottawa, Ottawa, Ontario, Canada
[9]Department of Anatomy, Universite du Quebec a Trois-Rivieres, Trois-Rivieres, Quebec, Canada
[10]John W. Scott Health Sciences Library, University of Alberta, Edmonton, Alberta, Canada
[11]School of Human Kinetics, University of Ottawa, Ottawa, Ontario, Canada
[12]Department of Obstetrics and Gynecology, Queen's University, Kingston, Ontario, Canada
[13]AFIPE Research Group, Technical University of Madrid, Madrid, Spain
[14]Department of Human Kinetics, Universite du Quebec a Trois-Rivieres, Trois-Rivieres, Quebec, Canada

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