Accuracy of body mass index in predicting pre-eclampsia: bivariate meta-analysis

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Objective The objective of this study was to determine the accuracy of body mass index (BMI) (pre-pregnancy or at booking) in predicting pre-eclampsia and to explore its potential for clinical application.

Design Systematic review and bivariate meta-analysis.

Setting Medline, Embase, Cochrane Library, MEDION, manual searching of reference lists of review articles and eligible primary articles, and contact with experts.

Population Pregnant women at any level of risk in any healthcare setting.

Methods Reviewers independently selected studies and extracted data on study characteristics, quality, and accuracy. No language restrictions.

Main outcome measures Pooled sensitivities and specificities (95% CI), a summary receiver operating characteristic curve, and corresponding likelihood ratios (LRs). The potential value of BMI was assessed by combining its predictive capacity for different prevalences of pre-eclampsia and the therapeutic effectiveness (relative risk 0.90) of aspirin.

Results A total of 36 studies, testing 1,699,073 pregnant women (60,584 women with pre-eclampsia), met the selection criteria. The median incidence of pre-eclampsia was 3.9% (interquartile range 1.4–6.8). The area under the curve was 0.64 with 93% of heterogeneity explained by threshold differences. Pooled estimates (95% CI) for all studies with a BMI ≥ 25 were 47% (33–61) for sensitivity and 73% (64–83) for specificity; and 21% (12–31) and 92% (89–95) for a BMI < 25, and 2.7 (1.0–7.3) for BMI ≥ 35 and 0.86 (0.68–1.07) for BMI < 35. The number needed to treat with aspirin to prevent one case of pre-eclampsia ranges from 714 (no testing, low-risk women) to 37 (BMI ≥ 35, high-risk women).

Conclusions BMI appears to be a fairly weak predictor for pre-eclampsia. Although BMI is virtually free of cost, noninvasive, and ubiquitously available, its usefulness as a stand-alone test for risk stratification must await formal cost-utility analysis. The findings of this review may serve as input for such analyses.

Keywords Accuracy, body mass index, likelihood ratio, meta-analysis, pre-eclampsia, sensitivity and specificity.

Introduction

Pre-eclampsia is a major cause of maternal and perinatal morbidity and mortality.1–3 It has a long preclinical phase before signs and symptoms become manifest in the second half of pregnancy. Prediction of pre-eclampsia is important because increased monitoring and administration of early preventative treatment with, for example, aspirin can be more selectively targeted at high-risk women.4

The exact aetiology of pre-eclampsia is still unknown. Several risk factors have been identified including obesity.3 The World Health Organization (WHO) estimates that currently more than 1 billion adults are overweight, of whom at least 300 million are clinically obese.5 From 1999 to 2003, in the total delivery population, the proportion of women who were overweight or obese pre-pregnancy increased from 37.1 to 40.5%.7 This worldwide increase in overweight is likely to cause a rise in pre-eclampsia and other complications, such as gestational diabetes.8 The body mass index (BMI) is an international standard for obesity measurement adjusting bodyweight for height (weight [kg]/height squared [m²]) and an indicator of nutritional status.
We conducted a systematic review to determine the accuracy of BMI (pre-pregnancy or at booking) to predict pre-eclampsia and to explore its potential for clinical application. We meta-analysed the data using a bivariate regression analysis, accounting for (negative) correlation between sensitivity and specificity, and derived likelihood ratios (LRs) thereof.9–12

**Methods**

**Study identification and selection**

We performed an electronic search targeting all procedures used for the prediction of pre-eclampsia. We searched Medline, Embase, Cochrane Library, and MEDION (www.mediondatabase.nl/) from inception to April 2006. The search strategy13 consisted of MeSH or keyword terms related to pre-eclampsia combined with methodological filters for identification of studies on diagnostic tests and aetiology.14–16 We checked reference lists of review articles and eligible primary studies to identify cited articles not captured by electronic searches and contacted experts. Reference Manager 10.0 databases were established incorporating results of all searches.

We selected studies in a three-stage process. First, one reviewer scrutinised titles and/or abstracts of all citations. Second, to ensure independent duplicate selection for this review, we performed a search based on keywords “weight” OR body mass OR obesity OR adipos* OR fat OR quetelet* within the Reference Manager database for citations to be scrutinised by a second reviewer. We obtained full manuscripts of all citations that were selected by at least one of the reviewers. Final in/exclusion decisions were made after independent and duplicate examination of the full manuscripts of selected citations. We included studies if they reported numerical data allowing construction of a 2 × 2 table cross-classifying BMI test results (pre-pregnancy or at booking) and occurrence of pre-eclampsia. We included studies examining pregnant women at any level of risk in any healthcare setting. Language restrictions were not applied. Any disagreements were resolved by consensus or by a third reviewer.

For each included paper, two reviewers independently extracted data on clinical and methodological study characteristics and on test accuracy. Study characteristics consisted of women’s risk classifications, characteristics of the index test and of the reference standard, and details of the outcome, such as onset and severity of pre-eclampsia.

**Assessment of study quality**

Two reviewers independently assessed all included manuscripts for study quality according to an adapted version of QUADAS.17 We considered consecutive or random entry of pregnant women to a cohort as ensuring an appropriate spectrum of patients. Patient selection criteria were considered appropriate when the study reported on parity, singleton/multiple pregnancies, diabetes mellitus, and chronic hypertension. We deemed the reference standard for pre-eclampsia appropriate if it combined persistent systolic blood pressure ≥ 140 mmHg and/or diastolic blood pressure ≥ 90 mmHg with proteinuria ≥ 0.3 g/24 hours or ≥ 1+ dipstick (30 mg/dl in a single urine sample) new after 20 weeks of gestation. Similarly, we deemed the reference standard for superimposed pre-eclampsia appropriate when it combined the development of proteinuria ≥ 0.3 g/24 hours or ≥ 1+ dipstick after 20 weeks of gestation in chronically hypertensive women.18,19 A definition of pre-eclampsia according to recent (from the year 2000 onwards) international guidelines was also considered appropriate.18–20 We also assessed if details of the index test (kg/m², pre-pregnancy or before 20 weeks) and reference test (measurement device, position of patient, and Korotkoff phase), and a description of reasons for withdrawals were adequate.

**Data synthesis**

For each study, we constructed a 2 × 2 table cross-classifying BMI test results and the occurrence of pre-eclampsia. We used receiver operating characteristic (ROC) plots to visualise data. We used a bivariate meta-regression model to calculate pooled estimates of sensitivity and specificity for several BMI cutoff values and to fit a summary ROC (sROC) curve. This method has been extensively described elsewhere.9–12 Briefly, rather than using a single outcome measure per study, such as the diagnostic odds ratio, the bivariate model preserves the two-dimensional nature of diagnostic data in a single model. This model incorporates the correlation that may exist between sensitivity and specificity within studies due to possible differences in threshold between studies. The bivariate model uses a random effects approach for both sensitivity and specificity, allowing for heterogeneity beyond chance due to clinical or methodological differences between studies. In addition, the model acknowledges the difference in precision by which sensitivity and specificity have been measured in each study. This means that studies with a larger number of women with pre-eclampsia receive more weight in the calculation of the pooled estimate of sensitivity, while studies with more women without pre-eclampsia are more influential in the pooling of specificity. We calculated a correlation coefficient (r) to show the strength and shape of the correlation between sensitivity and specificity, and hence the amount of variance that can be explained due to threshold differences (r²). Despite several cutoff values for BMI, for statistical reasons, each study was represented once in the sROC analysis. This was achieved by randomly sampling one 2 × 2 table from each study, ensuring that all areas of the sROC curve were represented. For clinical usefulness, we derived LRs from the estimated sensitivities and specificities.

We defined all subgroup analyses, except one (pre-pregnancy or at booking), a priori requiring that at least 80%
of studies reported clearly on a particular item and each subgroup contained at least three studies. Data on threshold, pre-pregnancy or booking BMI, severity and onset (before or after 34 weeks) of pre-eclampsia, incidence (cutoff 4%), study design, consecutive entry, prospective data collection, and adequacy of reference standard were considered one by one in the subgroup analyses. First, we entered the subgroup characteristic as a covariate in the model. If homogeneity between studies with and without the characteristic was rejected \((P < 0.10)\), the subgroup analysis was performed.

The bivariate models were fitted using the Proc NLMixed (SAS 9.1 for Windows [SAS Institute Inc, Cary, NC, USA]).

**Potential for clinical application**

At a (fixed) relative risk (RR) reduction of aspirin of 10% \((RR = 0.90)\), the absolute treatment effect becomes greater as the absolute risk rises. For a woman with a risk of pre-eclampsia of 40%, aspirin reduces it to 36%, whereas for a woman who has a 4% risk, this is reduced to 3.6%. Testing makes sense if treatment is conditional on obtaining a positive test result. Therefore, the impact of risk stratification using BMI depends on the shift that testing brings about in the pre-test probability distribution. 21 We illustrated this impact with an example of decision-making in clinical practice about the use of aspirin in women at risk of pre-eclampsia, which has been shown to be an effective preventive treatment. 4 We calculated the risk, and hence the therapeutic benefits associated with BMI test results from sensitivity and specificity. The number needed to treat (NNTreat) is the number of women that one needs to treat (with aspirin) to prevent one case of pre-eclampsia and is calculated by \(1/(\text{probability after testing positive} – \text{probability after treatment})\).

**Results**

Figure 1 summarises the process of literature identification and selection. Thirty-six studies met the selection criteria. 22–57 Four papers were found in reference lists, of which one was included. The two papers from Sebire et al. 46,47 reported on the same cohort, and therefore, we considered it as one study. We excluded studies for a variety of reasons. The main reasons for exclusion were no distinction between pre-eclampsia and pregnancy-induced hypertension, insufficient data to construct a \(2 \times 2\) table, and reporting weight or weight gain or a different weight-index test instead of a BMI.

Table 1 summarises study characteristics. We included 23 cohort studies, 11 case–control studies, and one randomised trial. A total of 1 699 073 pregnant women were included, of whom 60 584 (3.6%) developed pre-eclampsia. The median incidence of pre-eclampsia in cohort studies was 3.9% (inter-quartile range 1.4–6.8). For calculation of the median incidence, we excluded case–control studies that did not report the incidence in their source population. The number of women analysed in cohort studies ranged from 281 27 to 561 770, 56 and in case–control studies from 54 37 to 6747. 28 Twenty-seven studies were performed in Western countries. Eleven studies were prospectively designed. Sixteen studies excluded all women with diabetes and/or chronic hypertension.

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**Figure 1.** Process from initial search to final inclusion for studies on BMI in the prediction of pre-eclampsia.

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### Table 1. Study characteristics for individual studies on the predictive accuracy of BMI in predicting pre-eclampsia

<table>
<thead>
<tr>
<th>First author (year)</th>
<th>Country</th>
<th>Type of study</th>
<th>Number of women analysed</th>
<th>Incidence PET (%)</th>
<th>Gestational age (BMI)</th>
<th>Population</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baeten22 (2001)</td>
<td>USA</td>
<td>Retrospective cohort from birth register</td>
<td>96,384</td>
<td>6.8</td>
<td>Pre-pregnancy</td>
<td>IN: nulliparous women</td>
</tr>
<tr>
<td>Bassø23 (2004)</td>
<td>Denmark</td>
<td>Retrospective cohort from birth register</td>
<td>59,074</td>
<td>0.5</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancies</td>
</tr>
<tr>
<td>Bianco25 (1998)</td>
<td>USA</td>
<td>Retrospective cohort</td>
<td>11,926</td>
<td>3.7</td>
<td>Pre-pregnancy</td>
<td>IN: age 20–34 years; EX: multiple pregnancies, extremes of age, BMI between 27 and 34, missing height or pre-pregnancy weight</td>
</tr>
<tr>
<td>Bodnar26 (2005)</td>
<td>USA</td>
<td>Prospective cohort</td>
<td>1179</td>
<td>4.8</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancies; EX: pre-existing medical condition, positive toxicology screen and HIV</td>
</tr>
<tr>
<td>Bowers27 (1999)</td>
<td>USA</td>
<td>Retrospective cohort</td>
<td>281</td>
<td>5.8</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancies; EX: delivery before 36 weeks, pre-existing chronic maternal illness (HIV)</td>
</tr>
<tr>
<td>Carr28 (2005)</td>
<td>USA</td>
<td>Retrospective case–control from birth certificates</td>
<td>6747</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancies; EX: fetal demise, 24 weeks</td>
</tr>
<tr>
<td>Conde-Agudelo29 (2000)</td>
<td>South America</td>
<td>Retrospective cohort from birth register</td>
<td>561,770</td>
<td>5.1</td>
<td>Pre-pregnancy</td>
<td>IN: all pregnancies of at least 20 weeks of gestation</td>
</tr>
<tr>
<td>Enquobahrie30 (2005)</td>
<td>Peru</td>
<td>Retrospective case–control</td>
<td>312</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: Peruvian women</td>
</tr>
<tr>
<td>Goldman32 (1991)</td>
<td>USA</td>
<td>Prospective cohort</td>
<td>305</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: gestational diabetes</td>
</tr>
<tr>
<td>Ioka33 (2003)</td>
<td>Japan</td>
<td>Prospective cohort</td>
<td>488</td>
<td>17.4</td>
<td>Pre-pregnancy</td>
<td>Not reported</td>
</tr>
<tr>
<td>Knust34 (1998)</td>
<td>Netherlands</td>
<td>Prospective cohort</td>
<td>2080</td>
<td>1.4</td>
<td>Pre-pregnancy</td>
<td>IN: nulliparous women, singleton pregnancies; EX: DM, CH, renal disease, obstetric anomaly</td>
</tr>
<tr>
<td>Lee35 (2000)</td>
<td>Taiwan</td>
<td>Retrospective cohort</td>
<td>29,735</td>
<td>1.4</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women; EX: fetal malformations</td>
</tr>
<tr>
<td>Mittendorf36 (1996)</td>
<td>USA</td>
<td>Retrospective case–control</td>
<td>2741</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancies</td>
</tr>
<tr>
<td>Mostello37 (2002)</td>
<td>USA</td>
<td>Retrospective case–control from birth register</td>
<td>4702</td>
<td>5.9</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancies</td>
</tr>
<tr>
<td>Mural38 (1997)</td>
<td>USA</td>
<td>Retrospective case–control</td>
<td>54</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancies</td>
</tr>
<tr>
<td>Nobile39 (2005)</td>
<td>Denmark</td>
<td>Prospective cohort from national birth cohort</td>
<td>54,505</td>
<td>2.1</td>
<td>Pre-pregnancy</td>
<td>Pre-pregnancy participation in telephone interview</td>
</tr>
<tr>
<td>Ogunyemi39 (1998)</td>
<td>USA</td>
<td>Prospective cohort</td>
<td>582</td>
<td>7.9</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancies, black low-income women; EX: delivery &lt; 37 weeks of gestation</td>
</tr>
<tr>
<td>Ostlund40 (2004)</td>
<td>Sweden</td>
<td>Prospective cohort from birth register</td>
<td>430,852</td>
<td>2.8</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancies; EX: DM</td>
</tr>
<tr>
<td>Qiu41 (2004)</td>
<td>USA</td>
<td>Prospective nested case–control</td>
<td>559</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women</td>
</tr>
<tr>
<td>Qiu42 (2006)</td>
<td>Peru</td>
<td>Retrospective case–control</td>
<td>189</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td>IN: women with pre-eclampsia and normotensive controls</td>
</tr>
<tr>
<td>Ramos43 (2005)</td>
<td>USA</td>
<td>Retrospective cohort</td>
<td>22,658</td>
<td>8.3</td>
<td>First visit</td>
<td>IN: singleton pregnancies; EX: fetal anomalies</td>
</tr>
<tr>
<td>Rode44 (2005)</td>
<td>Denmark</td>
<td>Retrospective cohort</td>
<td>8092</td>
<td>2.4</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancies; EX: delivery &lt; 37 weeks</td>
</tr>
<tr>
<td>Ros45 (1998)</td>
<td>Sweden</td>
<td>Retrospective cohort from birth register</td>
<td>2418</td>
<td>5.2</td>
<td>Pre-pregnancy</td>
<td>IN: nulliparous women; EX: &lt;34 years</td>
</tr>
<tr>
<td>Rudra46 (2005)</td>
<td>USA</td>
<td>Retrospective case–control</td>
<td>757</td>
<td>n.r.</td>
<td>Pre-pregnancy</td>
<td></td>
</tr>
</tbody>
</table>

(continued)
Twenty-eight studies reported pre-pregnancy BMI, and seven studies reported BMI at booking. We excluded one study for the meta-analysis because this study excluded women with a BMI between 27 and 34.25

Figure 2 summarises study quality. Patient recruitment, selection criteria, and reasons for withdrawal were poorly reported. Eleven studies used a definition of pre-eclampsia according to current international standards. Fifteen studies reported a definition of pre-eclampsia according to former guidelines. Details of how the reference test was executed were seldom reported. Only one study reported whether women received treatment to prevent pre-eclampsia.48

Figure 3 shows the sROC curve for the included studies. The area under the curve (AUC) was 0.64, which was similar to the AUC representing all data points for all studies (AUC 0.63). The correlation coefficient was −0.965, indicating that 93% of heterogeneity could be explained due to differences in threshold for a positive test. Subgroup analysis on pre-pregnancy BMI versus BMI at booking yielded statistically significant differences \((P < 0.0001)\), showing slightly better performance for pre-pregnancy BMI. However, their clinical relevance seems doubtful (sensitivity\(_{\text{pre-pregnancy}}\) 48.1% (47.9–48.2) versus sensitivity\(_{\text{at booking}}\) 44.9% (44.4–45.5) and specificity\(_{\text{pre-pregnancy}}\) 61.4% (61.4–61.5) versus specificity\(_{\text{at booking}}\) 58.5% (58.5–58.6). The discrepancy between statistical significance and clinical relevance is due to some of the studies having very large sizes.

Below, we report the overall results. Incidence of pre-eclampsia, study design, data collection, and adequacy of reference standard did not further reduce heterogeneity. Analyses on clinical characteristics and consecutive entry were hindered by a lack of reporting. We used cutoff values that were most often used and reported by the Institute of Medicine (IOM) and WHO for categorisation.6,58 Pooled estimates (95% CI) for all studies with a BMI \(\geq 25\) (18 studies) were 47% (33–61) for sensitivity and 73% (64–83) for specificity. For a BMI \(\geq 30\) (19 studies), these estimates were 19% (19–20) and 90% (88–93), and for a BMI \(\geq 35\) (4 studies), the estimates were 21% (12–31) and 92% (89–95). Pooling data on BMI \(\geq 30\) required an adjusted

Table 1. (Continued)

<table>
<thead>
<tr>
<th>First author (year)</th>
<th>Country</th>
<th>Type of study</th>
<th>Number of women analysed</th>
<th>Incidence PET (%)</th>
<th>Gestational age (BMI)</th>
<th>Population</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sebire(^{46,47}) (2001)</td>
<td>UK</td>
<td>Retrospective cohort</td>
<td>325,395</td>
<td>0.8</td>
<td>At booking</td>
<td>IN: unselected population</td>
</tr>
<tr>
<td>Sibai(^{48}) (1997)</td>
<td>USA</td>
<td>Prospective randomised controlled trial</td>
<td>4310</td>
<td>7.6</td>
<td>First visit</td>
<td>IN: normotensive women, singleton pregnancy</td>
</tr>
<tr>
<td>Sebire(^{46}) (2000)</td>
<td>USA</td>
<td>Retrospective cohort</td>
<td>2724</td>
<td>5.3</td>
<td>Pre-pregnancy</td>
<td>IN: singleton pregnancy</td>
</tr>
<tr>
<td>Stone(^{50}) (1994)</td>
<td>USA</td>
<td>Retrospective cohort</td>
<td>19,654</td>
<td>0.4</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancy</td>
</tr>
<tr>
<td>Suzuk(^{51}) (2000)</td>
<td>Japan</td>
<td>Cohort</td>
<td>3446</td>
<td>8.6</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancy</td>
</tr>
<tr>
<td>Thadhani(^{52}) (1999)</td>
<td>USA</td>
<td>Prospective cohort</td>
<td>17,211</td>
<td>0.5</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancy</td>
</tr>
<tr>
<td>van Hoorn(^{53}) (2002)</td>
<td>Australia</td>
<td>Retrospective case-control</td>
<td>448</td>
<td>2.4</td>
<td>At booking</td>
<td>IN: primigravid, singleton pregnancy</td>
</tr>
<tr>
<td>Weiss(^{54}) (2004)</td>
<td>USA</td>
<td>Prospective cohort</td>
<td>16,102</td>
<td>2.4</td>
<td>0–14 weeks</td>
<td>IN: unselected population, singleton pregnancy</td>
</tr>
<tr>
<td>Yamamoto(^{55}) (2001)</td>
<td>Japan</td>
<td>Case–control</td>
<td>1246</td>
<td>8.6</td>
<td>Pre-pregnancy</td>
<td>IN: normotensive women, singleton pregnancy</td>
</tr>
</tbody>
</table>

CH, chronic hypertension; DM, diabetes mellitus; EX, excluded; IN, included; n.r., not reported; PET, pre-eclampsia.

Figure 2. Summary of study quality. Data are presented as 100% stacked bars. Numbers in stacks represent numbers of studies.
model of the bivariate analysis resulting in a very narrow confidence interval for sensitivity. Therefore, we also calculated estimates for BMI $\geq 29$ (seven studies, IOM classification), which were 22% (12–32) for sensitivity and 89% (82–95) for specificity. The corresponding LRs (95% CI) were 1.7 (0.3–11.9) for BMI $\geq 25$ and 0.73 (0.22–2.45) for BMI < 25, 1.9 (0.3–11.9) for BMI $\geq 29$ and 0.88 (0.61–1.29) for BMI < 29, and 2.7 (1.0–7.3) for BMI $\geq 35$ and 0.86 (0.68–1.07) for BMI < 35.

Table 2 shows that the probability of pre-eclampsia after positive BMI testing increases strongly in moderate (say incidence 7%) and high-risk (say 12%) populations to 18 and 29%, respectively, for women with a BMI $\geq 35$. These latter posttest probabilities correspond with NNT$s_{\text{aspirin}}$ of 62 and 37, respectively. BMI results < 35 lead to posttest probabilities of 1.2, 5.9, and 10.7% in populations with a baseline risk of 1.4, 6.8, and 12.2%, respectively. These numbers, in turn, correspond to NNT$s_{\text{aspirin}}$ of 830, 170, and 94.

### Discussion and conclusion

Based on a huge body of evidence, BMI (at any cutoff), pre-pregnancy or at booking, appears to be a fairly weak predictor for pre-eclampsia. Although BMI is virtually free of cost, non-invasive, and ubiquitously available, its usefulness as a stand-alone test for risk stratification must await formal cost-utility analysis. The findings of this review may serve to populate such a model.

Based on the estimates in this review, among low-risk pregnancies without testing, one needed to treat 714 women with aspirin to prevent one case of pre-eclampsia. By contrast, in high-risk women, who in addition have a BMI $\geq 35$, one needed to treat only 37 women to prevent one case. This may seem impressive. However, it is not entirely clear how clinicians may decide on the exact risk level a woman has before modifying it further using BMI. Note also that a single test with modest values of sensitivity implies that many with a negative test result will develop pre-eclampsia and thus, in this example, will not benefit from what appears to be the

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**Table 2. Illustration of potential impact of BMI testing at two cutoff values among pregnant women at different prevalences and numbers of women needed to treat with aspirin to prevent one case of pre-eclampsia**

<table>
<thead>
<tr>
<th>Test result</th>
<th>Prior risk (prevalence of PET, %)*</th>
<th>Probability of PET after testing positive (%)</th>
<th>Treatment effect (RR)</th>
<th>Probability of PET after treatment (%)</th>
<th>NNTreat</th>
</tr>
</thead>
<tbody>
<tr>
<td>No test, no treatment**</td>
<td>1.4</td>
<td>1.4</td>
<td>—</td>
<td>1.4</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>6.8</td>
<td>6.8</td>
<td>—</td>
<td>6.8</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>12.2</td>
<td>12.2</td>
<td>—</td>
<td>12.2</td>
<td>—</td>
</tr>
<tr>
<td>No test, treat all**</td>
<td>1.4</td>
<td>—</td>
<td>0.90</td>
<td>714</td>
<td></td>
</tr>
<tr>
<td></td>
<td>6.8</td>
<td>—</td>
<td>147</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>12.2</td>
<td>—</td>
<td>82</td>
<td></td>
<td></td>
</tr>
<tr>
<td>BMI $\geq 35$; sensitivity 21%; specificity 92%</td>
<td>Test all, treat test positives</td>
<td>1.4</td>
<td>3.6</td>
<td>0.90</td>
<td>3.2</td>
</tr>
<tr>
<td></td>
<td>6.8</td>
<td>16.1</td>
<td>—</td>
<td>14.5</td>
<td>62</td>
</tr>
<tr>
<td></td>
<td>12.2</td>
<td>26.7</td>
<td>—</td>
<td>24.0</td>
<td>37</td>
</tr>
<tr>
<td>BMI $\geq 25$; sensitivity 47%; specificity 73%</td>
<td>Test all, treat test positives</td>
<td>1.4</td>
<td>2.4</td>
<td>0.90</td>
<td>2.2</td>
</tr>
<tr>
<td></td>
<td>6.8</td>
<td>11.3</td>
<td>—</td>
<td>10.2</td>
<td>88</td>
</tr>
<tr>
<td></td>
<td>12.2</td>
<td>19.5</td>
<td>—</td>
<td>17.6</td>
<td>51</td>
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</tbody>
</table>

PET, pre-eclampsia.
*Interquartile ranges of median prevalence in this review used as low and moderate prior risk. 12.2% was used as high prior risk.
**Numbers are equal for all tests regardless of threshold and sensitivity and specificity.
cheap and safe treatment of aspirin. The moderate positive and negative LRs of a BMI > 35 underscore these conclusions. In 2003, O’Brien et al.59 reviewed 13 studies and also reported on the predictive power of BMI for pre-eclampsia. However, they pooled predictive values, an accuracy parameter that is generally considered too sensitive to variations in prevalence.60 Duckitt and Harrington,61 in their review, covering different tests at booking, included ten studies on BMI and reported different summary estimates, without explaining why they selected a RR of pre-eclampsia of 2.47 in their abstract. In addition, their methodology is flawed by the adoption of a cause-effect framework discarding studies with baseline incomparability, although this is unimportant in (noncausal) studies on prediction.62 Finally, a pooled RR (of 2.47) does not allow clinicians to update their pre-test results from such analyses ideally incorporating the costs and benefits of preventive treatment.

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References


