Background: It is unclear whether confounding accounts for the increased risk of preterm birth and small for gestational age (SGA) birth in opioid analgesic exposed pregnancies.

Methods: Using universal coverage health data for Ontario, we assembled a cohort of mother–infant pairs without opioid use disorder (627,172 pregnancies and 509,522 women). We estimated risk ratios (RRs) between opioid analgesics and preterm birth, SGA birth, and stillbirth; neonatal abstinence syndrome was a secondary outcome. We used high-dimensional propensity scores and sensitivity analyses for confounding adjustment.

Results: 4% of pairs were exposed, mainly to codeine (2%), morphine (1%), and oxycodone (1%). Compared with unexposed, the adjusted risk of preterm birth was higher with any (1.3, 95% confidence interval [CI] = 1.2, 1.3), first- (RR: 1.2, 95% CI = 1.2, 1.3), and second-trimester (RR: 1.3, 95% CI = 1.2, 1.4) opioid analgesic exposure. Preterm birth risk was higher for first- and second-trimester codeine, morphine, and oxycodone exposure, and for third-trimester morphine. There was a small increase in SGA with first-trimester exposure to any opioid analgesic or to codeine. Exposed pregnancies had an elevated stillbirth risk with any (RR: 1.6, 95% CI = 1.4, 1.8), first- and second-trimester exposure. Few infants had neonatal abstinence syndrome (N = 143); the risk was higher in exposed (RR: 3.6, 95% CI = 2.1, 6.0). In sensitivity analyses of unmeasured confounding, an elevated risk in exposed pregnancies persisted for preterm birth but not SGA.

Conclusions: Opioid analgesic-exposed pregnancies had a small increased risk of preterm birth and possibly stillbirth after accounting for confounding by indication and sociodemographic factors.

Keywords: Opioid analgesics; Pregnancy; Preterm birth; Small for gestational age birth; Stillbirth; Neonatal abstinence syndrome; Confounding
including demographic factors on both the individual and neighborhood levels, reproductive history, maternal history of illness, and medication use. The authors concluded that, while associations between any opioid analgesic exposure and preterm birth and SGA birth could largely be explained by confounding, a small increased risk could not be ruled out.13

We, therefore, sought to examine whether the small increased risk of preterm birth could be entirely explained by confounding in our population-based study, and further, to estimate associations by morphine equivalent dose and trimester of exposure. Using a large contemporary database of universal healthcare insurance, comprehensive data on all narcotic prescriptions during pregnancy, and probabilistic bias analysis of unmeasured confounding, we report on the risk of preterm birth, SGA birth, and stillbirth after prenatal opioid analgesic exposure. Neonatal abstinence syndrome (NAS) was a secondary infant outcome.

METHODS

Study Cohort

We followed a population-based cohort of pregnancies using the administrative health data sources in the single-payer healthcare system in Ontario. Universal coverage for physician care and hospital services is provided to all Ontario residents through the Ontario Health Insurance Program (OHIP). Datasets were linked by encoded identifiers and analyzed at ICES (www.icles.on.ca). ICES is an independent, nonprofit research institute whose legal status under Ontario’s health information privacy law allows it to collect and analyze healthcare and demographic data for health system evaluation and improvement. ICES maintains a validated database of pregnancies, pregnancy outcomes, and mother–infant linkage from these healthcare data, the MOMBABY database. Infants in MOMBABY were matched to mothers using a unique maternal-infant matching number.

The study cohort included mother–infant pairs with an estimated date of confinement after April 7, 2013—which corresponded to 280 days after the Narcotics Monitoring System database (NMS, described below) was established—through March 31, 2018 to prevent over-selecting preterm and SGA births.14 Deliveries before April 7, 2013 or after March 31, 2018 were eligible provided the estimated data of confinement fell within the study period. To reduce confounding, we excluded women with a diagnosis of opioid use disorder or an opioid overdose within 2 years before delivery (International Classification of Diseases [ICD]-10: F11.1X, F11.2X, F11.9X)2,15,18 and those treated with methadone or buprenorphine for opioid use disorder.

Prenatal Opioid Analgesic Exposure

We searched maternal records for prenatal opioid analgesic prescriptions (butorphanol, buprenorphine for pain, codeine, fentanyl, hydrocodone, meperidine, methadone for pain, morphine, opium, oxycodone, pentazocine, tapentadol, and tramadol) in the NMS database. The NMS is part of the Ontario Narcotics Strategy to address misuse of prescription narcotics and other controlled substances. The NMS database contains information (medication, prescription date, fill date, dose, and quantity) for all community pharmacy-dispensed prescriptions for narcotics, controlled substances, and other monitored drugs, irrespective of whether the prescription was paid for under the publicly funded drug program, private insurance, or cash. The prescription fill date must have overlapped the pregnancy period to be considered prenatal medication. For most pairs, the pregnancy period was defined using the maternal obstetric gestational age in the MOMBABY dataset abstracted from the maternal delivery record. For infants for whom the maternal obstetric gestational age variable was missing in MOMBABY (N = 599), we used the gestational age variable in MOMBABY from the infant record. For infants who had neither variable in MOMBABY (N = 1,166), we followed the validated algorithm for administrative healthcare data and imputed 39 weeks for births without an ICD-10 preterm indicator and 35 weeks for those with a preterm indicator.17 We classified opioid analgesic exposure as any use versus no use, and first trimester (conception to <14 weeks gestation), second trimester (14 weeks gestation to <27 weeks gestation), and third trimester (27 weeks gestation to delivery) versus no use; women had an indicator for each trimester of exposure. We considered opioid analgesics as a single class and by specific agents (e.g., codeine) where feasible. In sensitivity analyses, we determined the daily dose of the opioid analgesic dispensed in milligram (mg) of morphine equivalents and then multiplied this by the number of days supplied in pregnancy. We classified the total cumulative morphine equivalent dose over pregnancy as none, >0–75 mg, 76–150 mg, 151–300 mg, and >300 mg.

Birth Outcomes

We identified study outcomes from the MOMBABY database; the hospital discharge abstracts database (DAD)—mandatory submissions from hospitals to in the Canadian Institute for Health Information; the OHIP database—the physician fee-for-service claims file; and the National Ambulatory Care Reporting System database—mandatory submissions from hospitals for emergency department visits. The MOMBABY dataset was used to identify preterm deliveries (≥20 weeks gestation to <37 weeks gestation) and stillbirths (fetal demise ≥20 weeks gestation). Preterm birth was further classified as provider-initiated or spontaneous following the approach used with Canadian administrative health data.18 SGA births were identified using the infant’s birthweight from MOMBABY and the 10th percentile Canadian weight cut-offs for gestational age and sex.19 NAS was a secondary outcome and was identified in the DAD (ICD-9: 779.5, 292.0 and 760.72 and ICD-10: P961, P962, P04.4).2,16,20
Confounding

A priori confounders included maternal age, parity, socioeconomic status determined by the woman’s neighborhood income quintile, diabetes, Elixhauser comorbidity score, obesity, hypertension, pain, prescribed prenatal benzodiazepines or barbiturates, and year of delivery. Maternal socioeconomic status at delivery was determined using postal codes to rank average neighborhood income among other neighborhoods in the census area and was classified as household size-adjusted income in quintiles. Data on other prescribed prenatal psychotropic medications in NMS were only available for benzodiazepines and barbiturates.

To ensure the similarity of mother–infant pairs exposed to opioid analgesics and those unexposed, we generated a high-dimensional propensity score (HDPS) for all pairs in the cohort.21 The HDPS approach used a computer algorithm to empirically identify candidate covariates, prioritize covariates, and integrate them into a propensity score. We drew potential covariates from the healthcare claims data in the year before pregnancy (physician visits, emergency department and inpatient diagnostic codes, and prescription records), in addition to forcing inclusion of the a priori confounders. We generated a separate HDPS for each trimester of exposure. The HDPS procedure was developed for use in pharmacoepidemiologic studies with administrative healthcare data and has been used with ICES data.22,23 A complete list of confounders and data elements used in the HDPS can be found in the eTable; http://links.lww.com/EDE/B787.

Statistical Analysis

Because of the large study size, we assessed differences between maternal characteristics by opioid analgesic exposure using standardized differences; we deemed a difference of greater than 0.10 to be important.24 We used generalized linear models to estimate the risk ratio (RR) between opioid analgesic exposure and each study outcome. We estimated unadjusted associations, and then adjusted models using the a priori confounders alone—for comparison only—and the inverse probability treatment weighting with propensity scores described above. We stabilized the HDPS to improve precision.25 Because preterm deliveries in the second trimester in the unexposed group were not at risk of preterm birth in the third trimester, we excluded them from models of third-trimester exposure and preterm birth. Inclusion of second-trimester births in the unexposed group could increase the denominator of the RR and artificially underestimate the relative risk with third-trimester exposure.26

We conducted several sensitivity analyses for SGA and preterm birth. These included: (1) classifying exposure as cumulative morphine equivalent dose in pregnancy (to estimate associations according to the amount of opioid analgesic exposure), (2) excluding mother–infant pairs that were not singleton pregnancies (to assess a possible influence of multiples), (3) including siblings only (to further examine confounding), (4) restricting to one pregnancy per woman (to assess whether statistical independence was violated), (5) modeling prenatal opioid analgesic exposure as a time-dependent variable (to examine possible misclassification of exposure time), and (6) probabilistic bias analysis (to assess the effect of possible unmeasured confounding). For the probabilistic bias analysis, we followed the method of Lash et al27 and created a dichotomous variable to represent the unmeasured confounder (i.e., variables unavailable in the ICES data that predicted both opioid analgesic use and risk of the particular outcome). We then selected the prevalence of the unmeasured confounder from a uniform distribution between 1% and 5% for women unexposed to opioid analgesics and who did not have the particular pregnancy outcome (i.e., preterm birth, SGA birth) and between 2–10% for women exposed to opioid analgesics and whom did not have the pregnancy outcome; women who had the pregnancy outcome had an additional 2–5% prevalence. Plausible values for the unmeasured confounder prevalence were informed by the prior Swedish study.13 Psychotropic medications had the greatest difference between opioid analgesic users and nonusers in the Swedish study and were incompletely measured in our study (i.e., NMS had data only on benzodiazepines and barbiturates); we used the distribution in the Swedish cohort to inform the likely distribution of the unmeasured confounder in our bias analysis. To perform a single reconstruction of the data, we conducted a Bernoulli trial for all women, based on their probabilities, to assign whether they had the unmeasured confounder.27 We then subjected the reconstructed dataset to generalized linear models, with the model now containing the unmeasured confounder. We repeated the process 1,000 times and calculated bias-corrected RRs and 95% CIs as the median, and 2.5th and 97.5th percentiles of the distribution, respectively. To create the Bayesian prior distribution used in the Lash et al method,27 we assigned 50% probability to the null effect and 50% to the sensitivity analysis result of Sujan et al.13 (0.99, 95% CI = 0.85, 1.14 for preterm birth and 0.91, 95% CI = 0.70, 1.19 for SGA birth). We compared the results from the probabilistic bias analysis with the RR and 95% CIs from the HDPS models.

Ethics

The Queen’s University Health Sciences Research Ethics Board approved this study.

RESULTS

During the study period, there were 651,180 births in Ontario. After excluding women without OHIP coverage (N = 357), a history of opioid dependence (N = 23,527), age >50 (N = 113) or a pregnancy with more than three fetuses (N = 11), we included 627,172 (96%) of the pregnancies in the study cohort. The 627,172 pregnancies occurred among 509,522 women (N = 399,234 women with one pregnancy; N = 103,189 with two pregnancies, N = 6,844 with three
pregnancies, N = 247, with four pregnancies, and N = 8 with five pregnancies). Of the 627,172 pregnancies 616,442 (98%) were singletons, 10,538 (2%) twins, and 192 (<1%) triplets.

A total of 25,755 (4%) pregnancies were exposed to prenatal opioid analgesics including codeine (N = 14,701), morphine (N = 6,802), oxycodone (N = 5,454), tramadol (N = 1,123), meperidine (N = 148), fentanyl (N = 91), and other opioid analgesic (N = 76). The total morphine equivalent dose during pregnancy among exposed women was >0–75 mg: 23%, 76–150 mg: 41%, 151–00: mg 19%, and >300 mg: 18%. The characteristics of the women by prenatal opioid analgesic use are shown in Table 1. Women who used opioid analgesics prenatally were more likely to have used opioid analgesics in the year before pregnancy (34% vs. 10%), to have a diagnosis of pain in the year before pregnancy (24% vs. 10%), to have slightly more comorbidities, and were more likely to have a prenatal prescription for benzodiazepines or barbiturates (7% vs. 1%).

Table 2 shows the unadjusted and adjusted associations between any prenatal opioid analgesic exposure and the study outcomes. After adjusting for a priori confounders, pregnancies exposed to opioid analgesics had an elevated risk of preterm birth (RR: 1.4, 95% CI = 1.3, 1.4) and stillbirth (RR: 1.5, 95% CI = 1.3, 1.7) compared with unexposed pregnancies. There was no association between any exposure and SGA birth. Although the number of infants with NAS was small, as expected, the risk was considerably higher in infants exposed to opioid analgesics (RR: 6.8, 95% CI = 4.6, 10). We observed greater attenuation of the estimated RR when weighting by HDPS compared with adjusting using a priori confounders alone; for preterm birth, the estimate was RR: 1.3, 95% CI = 1.2, 1.3 and for NAS, RR: 3.6, 95% CI = 2.1, 6.0. The proportion of preterm births that were spontaneous—as opposed to provider-initiated—was similar in the opioid analgesic exposed (N = 1,850, 69%) and unexposed (N = 31,469, 73%) groups. Opioid analgesic-exposed pregnancies had an elevated stillbirth risk with any exposure vs. none (RR: 1.6, 95% CI = 1.4, 1.8) adjusted using HDPS.

Estimating associations between opioid analgesics by trimester of exposure and adverse infant outcomes (Table 3) showed that the risk of preterm birth was elevated for first (RR: 1.2, 95% CI = 1.2, 1.3) and second (RR: 1.3, 95% CI = 1.2, 1.4) trimester exposure compared with no exposure. There was a small increase in SGA birth in first trimester exposed compared with unexposed pregnancies (RR: 1.1, 95% CI = 1.0, 1.2). The number of stillbirths was small, yet an increased risk was estimated with first (RR: 1.5, 95% CI = 1.3, 1.8) and second (RR: 1.4, 95% CI = 1.1, 1.7) trimester exposure. We also estimated associations with specific analgesics (Table 4). The risk of preterm birth was higher for first- and second-trimester exposure to codeine, morphine, and oxycodone, and for third-trimester morphine exposure. The small increase in SGA birth in first-trimester exposed compared with unexposed pregnancies was suggested with codeine and morphine. The number of stillbirths was relatively small to examine associations by specific agents.

In sensitivity analyses (Table 5), when we examined associations by morphine equivalent dose, the RRs for preterm birth were higher for doses above 300 mg compared with unexposed than were the RRs estimated for other exposure categories. We observed associations between stillbirth and the two lowest dose categories. Dose was associated with duration of use: women who used opioid analgesics for more than one trimester tended to have a higher morphine equivalent dose. Other sensitivity analyses for preterm birth suggested that the estimated RRs between any opioid analgesic exposure and trimester of exposure were similar to those of the primary analyses when excluding: (1) mother–infant pairs that were not singleton pregnancies, (2) infants without a sibling, and (3) >1 pregnancy during the study period per woman. When we modeled prenatal opioid analgesic exposure as time-dependent, the hazard ratio of preterm birth for any opioid analgesic exposure versus none was 1.5, 95% CI = 1.5, 1.6 supporting our primary analyses. Although results of bias analysis of possible unmeasured confounding were attenuated compared with HDPS adjusted estimates, a small increase in the risk of preterm birth with any, first-, or second-trimester opioid analgesic exposure persisted compared with no exposure. In sensitivity analyses for SGA there was no association with morphine equivalent dose. Bias analysis suggested that the higher SGA risk associated with opioid analgesic exposure could be explained by confounding. Due to the confounder distribution and the HDPS adjusted SGA estimate close to 1, the bias analysis results moved the RR towards the left of one suggesting an unrealistic protective effect. Finally, in sensitivity analysis that was performed for stillbirth, the increased risk with first and second-trimester exposure persisted.

**DISCUSSION**

In this population-based study of births to women without a documented history of opioid dependence, those exposed to opioid analgesics prenatally had a small increased risk of preterm birth, SGA birth, and stillbirth after accounting for confounding by indication and sociodemographic factors using HDPS. Bias analyses to further adjust for possible unmeasured confounding were attenuated compared with those adjusted using the HDPS, but a small increase in the risk of preterm birth persisted with any exposure to opioid analgesics, and first-, and second-trimester exposure, compared with no exposure. Like previous studies, our association was partially explained by unmeasured confounding. Results of sensitivity analyses for SGA birth generally suggested that the higher risk associated with opioid analgesic exposure could be explained by confounding.

**Preterm Birth**

Consideration of confounding by indication is needed when assessing the safety of prenatal opioid analgesics.
Women who use opioid analgesics for pain in pregnancy may have other important risk factors associated with both treatment indication and the risk adverse pregnancy outcomes. Our findings suggest a small increased risk of preterm birth after first- or second-trimester exposure and that unmeasured confounding are unlikely to account for the observed association. Our results add to the Swedish population-based cohort study, which noted that confounding accounted for some, but perhaps not all, of the increased risk. 13 Earlier population-based studies in Sweden and Norway estimated small (~10%) increases in preterm birth risk.

### TABLE 1. Characteristics of 627,172 Pregnancies in the Ontario Cohort by Prenatal Opioid Analgesic Exposure

| Characteristic | Exposed to Prenatal Opioid Analgesics (N = 25,755) | Unexposed to Prenatal Opioid Analgesics (N = 601,417) | Standardized Difference |
|----------------|-----------------------------------------------|-------------------------------------------------|------------------------|
| Trimester of exposure, n (%)                     |                                               |                                                |                        |
| First                                       | 12,284 (48)                      | N/A                 | N/A                  |
| Second                                      | 9,357 (36)                       | N/A                 | N/A                  |
| Third                                       | 9,488 (37)                       | N/A                 | N/A                  |
| Total prenatal morphine equivalent of opioid analgesic, n (%)* |                                               |                                                |                        |
| >0–75 mg                                    | 5,807 (23)                       | N/A                 | N/A                  |
| 76–150 mg                                   | 10,505 (41)                      | N/A                 | N/A                  |
| 151–300 mg                                  | 4,884 (19)                       | N/A                 | N/A                  |
| >300 mg                                     | 4,554 (18)                       | N/A                 | N/A                  |
| Opioid analgesic use in the year before pregnancy |                                               |                                                |                        |
| Any                                         | 8,767 (34)                       | 60,712 (10)         | 0.60                 |
| Mean duration of analgesic use in the year before pregnancy (weeks) ± SD | 5.8 ± 14                         | 0.4 ± 2.8           | 0.52                 |
| Singleton pregnancy                         | 25,205 (98)                      | 593,237 (98)        | 0.03                 |
| Maternal age at delivery                    |                                               |                                                |                        |
| <20                                         | 549 (2)                          | 12,244 (2)          | 0.10                 |
| 20–24                                       | 3,377 (13)                       | 61,573 (10)         | 0.12                 |
| 25–29                                       | 6,891 (27)                       | 161,054 (27)        | 0.10                 |
| 30–34                                       | 8,723 (34)                       | 224,000 (37)        | 0.10                 |
| ≥35                                         | 6,215 (24)                       | 142,546 (24)        | 0.10                 |
| Year of delivery                            |                                               |                                                |                        |
| 2013                                        | 4,477 (17)                       | 93,753 (16)         | 0.10                 |
| 2014                                        | 5,638 (22)                       | 120,309 (20)        | 0.10                 |
| 2015                                        | 5,300 (21)                       | 119,916 (20)        | 0.10                 |
| 2016                                        | 5,032 (20)                       | 120,932 (20)        | 0.10                 |
| 2017                                        | 4,401 (17)                       | 121,074 (20)        | 0.10                 |
| 2018                                        | 907 (4)                          | 25,433 (4)          | 0.10                 |
| SES quintile                                |                                               |                                                |                        |
| 1–2                                         | 12,041 (47)                      | 256,080 (43)        | 0.08                 |
| 3                                           | 5,216 (20)                       | 123,450 (21)        | 0.08                 |
| 4                                           | 4,923 (19)                       | 123,628 (21)        | 0.08                 |
| 5                                           | 3,575 (14)                       | 98,259 (16)         | 0.08                 |
| Maternal pain diagnosis, year before pregnancy |                                               |                                                |                        |
| Any                                         | 6,144 (24)                       | 58,749 (10)         | 0.39                 |
| Low back pain                               | 4,891 (19)                       | 47,234 (8)          | 0.33                 |
| Migraine                                    | 882 (3)                          | 5,376 (1)           | 0.17                 |
| Chronic                                     | 747 (3)                          | 5,295 (1)           | 0.15                 |
| Limb                                        | 354 (1)                          | 2,951 (1)           | 0.09                 |
| Facial                                      | 64 (0)                           | 917 (0)             | 0.02                 |
| Other                                       | 391 (2)                          | 1,930 (0)           | 0.13                 |
| Maternal diabetes                           | 824 (3)                          | 9,797 (2)           | 0.10                 |
| Maternal obesity                            | 1,168 (5)                        | 14,520 (2)          | 0.12                 |
| Maternal hypertension                       | 901 (4)                          | 12,938 (2)          | 0.08                 |
| Elixhauser comorbidity score ≥1              | 457 (2)                          | 4,263 (1)           | 0.11                 |
| Prescribed prenatal benzodiazepines or barbiturates | 1,723 (7)                      | 8,424 (1)           | 0.27                 |
| Prior live birth                            | 4,509 (18)                       | 111,258 (19)        | 0.03                 |

*Morphine equivalent dose could not be determined for five women.
SES, socioeconomic status.
increases in the risk of preterm birth with second- or third-trimester opioid analgesic exposure and prenatal codeine exposure compared with no exposure. The opportunity for exposure is diminished for earlier deliveries. To address this potential bias, we excluded second-trimester deliveries from models of third-trimester exposure and preterm birth and also performed a sensitivity analysis with time-dependent opioid analgesic exposure. The risk of preterm birth was higher for first- and second-trimester exposure to codeine, morphine, and oxycodone compared with no exposure. The most common opioid analgesic prescriptions were codeine, morphine, and oxycodone; therefore, we were only able to estimate associations with these specific agents.

**Opioid Dependence**

We addressed possible confounding in the assembly of our cohort by excluding women with opioid dependence recorded in the administrative health data. Some of these high-risk women, however, likely could not be identified with administrative data alone. Opioid analgesic use in the year before pregnancy was identified in 34% of the exposed and 10% of the unexposed groups. Although this may represent women with chronic pain, it may include those with undocumented opioid dependence. The small number of infants with NAS diagnosed in the unexposed group (N = 37, 0.006%) likely indicates the use of illicit opioids and/or misuse of prescription opioids or possibly NAS signs from a nonopioid (e.g., selective serotonin reuptake inhibitor). The HDPS adjusted risk of NAS was almost four-fold higher in exposed compared with unexposed infants.

**Confounding Adjustment**

The HDPS approach uses an algorithm to empirically identify covariates—in addition to priori variables—among

### TABLE 2. Association Between Any Prenatal Opioid Analgesic Exposure and Birth Outcomes

| Outcome | Opioid analgesic exposure | No. of infants | No. of outcomes | Unadjusted | Adjusted for priori confounders | Adjusted with HDPS |
|---------|---------------------------|----------------|----------------|------------|---------------------------------|-------------------|
| Preterm birth | None | 601,417 | 43,213 | 1.0 | 1.0 | 1.0 |
| | Any | 25,755 | 2,693 | 1.5 (1.4, 1.5) | 1.4 (1.3, 1.4) | 1.3 (1.2, 1.3) |
| SGA birth | None | 589,133 | 57,255 | 1.0 | 1.0 | 1.0 |
| | Any | 25,064 | 2,402 | 1.0 (1.0, 1.0) | 1.0 (1.0, 1.0) | 1.0 (0.9, 1.0) |
| Stillbirth | None | 601,047 | 3,536 | 1.0 | 1.0 | 1.0 |
| | Any | 25,725 | 235 | 1.6 (1.4, 1.8) | 1.5 (1.3, 1.7) | 1.6 (1.4, 1.8) |
| NAS | None | 601,417 | 106 | 1.0 | 1.0 | 1.0 |
| | Any | 25,755 | 37 | 8.2 (5.6, 12) | 6.8 (4.6, 10) | 3.6 (2.1, 6.0) |

*Outcome data unavailable on SGA for 12,975 infants and stillbirth for 400 infants.

### TABLE 3. Association Between Trimester of Opioid Analgesic Exposure and Birth Outcomes

| Outcome | Trimester of opioid analgesic exposure | Number of infants | Number of outcomes | Unadjusted | Adjusted with HDPS |
|---------|----------------------------------------|-------------------|-------------------|------------|-------------------|
| Preterm birth | None | 601,417 | 43,213 | 1.0 | 1.0 |
| | First | 12,284 | 1,420 | 1.6 (1.5, 1.7) | 1.2 (1.2, 1.3) |
| | Second | 9,357 | 1,166 | 1.7 (1.6, 1.8) | 1.3 (1.2, 1.4) |
| | Third | 9,488 | 951 | 1.4 (1.3, 1.5) | 1.0 (1.0, 1.1) |
| SGA birth | None | 589,133 | 57,255 | 1.0 | 1.0 |
| | First | 11,929 | 1,260 | 1.1 (1.0, 1.2) | 1.1 (1.0, 1.2) |
| | Second | 9,081 | 889 | 1.0 (1.0, 1.1) | 1.0 (0.9, 1.0) |
| | Third | 9,258 | 851 | 1.0 (0.9, 1.0) | 0.9 (0.8, 1.0) |
| Stillbirth | None | 601,047 | 3,536 | 1.0 | 1.0 |
| | First | 12,273 | 115 | 1.6 (1.3, 1.9) | 1.5 (1.3, 1.8) |
| | Second | 9,345 | 89 | 1.6 (1.3, 2.0) | 1.4 (1.1, 1.7) |
| | Third | 9,477 | 71 | 1.3 (1.0, 1.6) | 1.1 (0.8, 1.4) |
| NAS | None | 601,417 | 106 | 1.0 | 1.0 |
| | First | 12,284 | 25 | 12 (7.5, 18) | 2.2 (1.0, 4.6) |
| | Second | 9,357 | 22 | 13 (8.4, 21) | 1.1 (0.5, 2.8) |
| | Third | 9,488 | 29 | 17 (12, 26) | 4.7 (2.4, 9.3) |

*Outcome data unavailable on SGA for 12,975 infants and stillbirth for 400 infants.

*Those exposed to opioid analgesics in >1 trimester and had the outcome are shown for each trimester of exposure.
the vast administrative data elements and integrate them into a propensity score.21 Adjusting for large numbers of covariates ascertained from patients’ healthcare claims data with HDPS may improve control of confounding as these variables may collectively be proxies for unobserved factors.21 RRs for preterm birth and NAS adjusted for a priori confounders were further attenuated by HDPS adjustment. Given the suspected direction of confounding to upwardly bias the estimated RR, the HDPS may better adjust for confounding. It must also be considered, however, that the HDPS is a computer-automated selection algorithm and thus causal intermediates may have been included.25 We attempted to prevent against this by only including data elements in the year before pregnancy. If causal intermediates were inadvertently included our RRs could be underestimated. Regardless, our findings suggest a small increased risk of preterm birth in opioid analgesic exposed pregnancies. There is greater opioid prescribing in Canada than Sweden,29 yet our study findings and those of the former Swedish study—studies were done in different contexts of opioid use—had similar estimates, suggesting the robustness of our results.

**SGA Birth**

Our results suggested a small increase in SGA birth with the first trimester exposed compared with unexposed pregnancies for any opioid analgesic exposure and for codeine and morphine. A small increased risk with first-trimester exposure to any analgesic persisted in sensitivity analyses, except for bias analysis of confounding; the latter suggested that confounding explained the small increased risk.

**Stillbirth**

Pregnancies exposed to prenatal opioid analgesics in the first and second trimester had an elevated risk of stillbirth. The number of stillbirths in our study was small (235 in exposed pregnancies, 3,536 in unexposed) which limited the precision of associations with specific agents. The risk of stillbirth from prenatal opioids for pain has not previously been studied and therefore we could not incorporate estimates from prior

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**TABLE 4. Association Between Trimester of Specific Opioid Analgesic Exposures and Birth Outcomes**

| Outcome | Trimester of Opioid Analgesic Exposure | Number of Infants | Number of Outcomes | Unadjusted | Adjusted with HDPS |
|---------|----------------------------------------|-------------------|-------------------|------------|--------------------|
| **Preterm birth** | None | 601,417 | 43,213 | 1.0 | 1.0 |
| | First codeine | 6,737 | 726 | 1.5 (1.4, 1.6) | 1.2 (1.1, 1.3) |
| | Second codeine | 5,111 | 579 | 1.6 (1.5, 1.7) | 1.3 (1.2, 1.4) |
| | Third codeine | 4,919 | 428 | 1.2 (1.1, 1.3) | 1.0 (0.9, 1.1) |
| | First morphine | 2,528 | 309 | 1.7 (1.5, 1.9) | 1.3 (1.1, 1.4) |
| | Second morphine | 2,546 | 348 | 1.9 (1.7, 2.1) | 1.4 (1.3, 1.6) |
| | Third morphine | 2,716 | 323 | 1.6 (1.5, 1.8) | 1.3 (1.1, 1.4) |
| | First oxycodone | 2,984 | 404 | 1.9 (1.7, 2.1) | 1.4 (1.2, 1.5) |
| | Second oxycodone | 2,010 | 298 | 2.1 (1.9, 2.3) | 1.4 (1.2, 1.6) |
| | Third oxycodone | 2,035 | 231 | 1.6 (1.4, 1.8) | 1.0 (0.9, 1.2) |
| **SGA birth** | None | 601,417 | 57,255 | 1.0 | 1.0 |
| | First codeine | 6,529 | 685 | 1.1 (1.0, 1.2) | 1.1 (1.0, 1.2) |
| | Second codeine | 4,966 | 481 | 1.0 (0.9, 1.1) | 1.0 (0.9, 1.1) |
| | Third codeine | 4,797 | 418 | 0.9 (0.8, 1.0) | 0.9 (0.8, 1.0) |
| | First morphine | 2,456 | 267 | 1.1 (1.0, 1.3) | 1.1 (1.0, 1.3) |
| | Second morphine | 2,470 | 225 | 0.9 (0.8, 1.1) | 0.9 (0.8, 1.0) |
| | Third morphine | 2,659 | 249 | 1.0 (0.9, 1.1) | 0.9 (0.8, 1.0) |
| | First oxycodone | 2,893 | 306 | 1.1 (1.0, 1.2) | 1.0 (0.9, 1.1) |
| | Second oxycodone | 1,943 | 215 | 1.1 (1.0, 1.3) | 0.9 (0.8, 1.1) |
| | Third oxycodone | 1,973 | 203 | 1.1 (0.9, 1.2) | 0.9 (0.8, 1.0) |
| **Stillbirth** | None | 601,417 | 3,536 | 1 | 1.0 |
| | First codeine | 6,732 | 76 | 1.9 (1.5, 2.4) | 1.6 (1.3, 2.1) |
| | Second codeine | 5,106 | 54 | 1.8 (1.4, 2.4) | 1.2 (0.9, 1.7) |
| | Third codeine | 4,915 | 42 | 1.5 (1.1, 2.0) | 1.2 (0.9, 1.7) |
| | First morphine | 2,524 | 25 | 1.7 (1.1, 2.5) | 1.4 (0.9, 2.2) |
| | Second morphine | 2,543 | 18 | 1.2 (0.8, 1.9) | 1.2 (0.7, 1.9) |
| | Third morphine | 2,713 | 14 | 0.9 (0.5, 1.5) | 0.7 (0.4, 1.2) |
| | First oxycodone | 2,981 | 22 | 1.3 (0.8, 1.9) | 1.4 (0.9, 2.1) |
| | Second oxycodone | 2,005 | 21 | 1.8 (1.2, 2.7) | 1.5 (0.9, 2.4) |
| | Third oxycodone | 2,030 | 17 | 1.4 (0.9, 2.3) | 1.2 (0.7, 1.9) |

*Outcome data unavailable on SGA for 12,975 infants and stillbirth for 400 infants.*
studies in our bias analysis. Elevated risks, however, are documented in pregnant women treated prenatally with opioid agonists for opioid dependence.2,30–32

Strengths and Limitations
A strength of our population-based study includes detailed records of opioid analgesic prescriptions regardless of out-of-pocket, private insurance, or drug beneficiary coverage. Only a small proportion of NMS records for the Ontario population (<3%) could not be linked to the ICES data due to missing patient identifiers. Our contemporary data included 627,172 pregnancies from 2013 through 2018. We used ICES validated measures of preterm birth and stillbirth. SGA birth and NAS were based on coding and algorithms used in prior studies to minimized misclassification. Limitations of our study include the use of an unexposed group of mother–infant pairs unexposed to any analgesic as well as pairs exposed to an analgesic other than opioids. Using this combined reference group, we would expect to estimate a RR that falls between those estimated using either reference group separately. Another limitation is that we had information on the date the opioid analgesic prescription was written and the date it was filled—the latter was used to define our exposed group—but could not confirm whether the woman actually used the medication; such misclassification would be expected to underestimate the association with opioid analgesics. ICES data do not consistently include pregnancy losses before 20 weeks, and this could be related to exposure and our study outcomes. Finally, detailed race–ethnicity and smoking data were unavailable in ICES data for adjustment.

Summary
Prenatal opioid analgesic exposure and adverse pregnancy outcomes are ongoing concerns.32,33 Our approach to control confounding did not fully attenuate the small increased risk of preterm delivery in opioid analgesic exposed pregnancies and exemplifies the importance of adjustment for maternal characteristics to reduce confounding bias. Our findings for opioid analgesic exposure during pregnancy show that the risk of preterm birth was higher with a greater morphine equivalent dose, suggested a possible association with stillbirth, and confirmed an increased risk of NAS. These results add to an accumulating body of evidence consistent with the hypothesis that opioid treatment for pain in pregnancy may carry risks to the fetus, which will be important to women and clinicians in selecting treatment.

ACKNOWLEDGMENTS
This study was supported by ICES, which is funded by an annual grant from the Ontario Ministry of Health (MOH). This study also received funding from the Eunice Kennedy Shriver National Institutes of Child Health and Human Development under grant number 1R03HD099617-01 (S.B.B.). Parts of this material are based on data and information compiled and provided by MOH, the Canadian Institute of Hospital Information, and the Registrar General through
Service Ontario. The analyses, conclusions, opinions, and statements expressed herein are solely those of the authors and do not reflect those of the funding or data sources; no endorsement is intended nor should be inferred.

The dataset from this study is held securely in the coded form at ICES. Although data sharing agreements prohibit ICES from making the dataset publicly available, access may be granted to those who meet pre-specified criteria for confidential access, available at www.ices.on.ca/DAS. The full dataset creation plan and underlying analytic code are available from the authors upon request, understanding that the programs may rely upon coding templates or macros that are unique to ICES.

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