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RESEARCH PAPER

Conflict, rockets, and birth outcomes: evidence from Israel's Operation Protective Edge

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Abstract
In summer 2014, southern Israel experienced rocket attacks from the Hamas-ruled Gaza strip on a nearly daily basis for over 50 consecutive days. We exploit this unexpected escalation in the Israeli-Palestinian conflict and variation across localities in Israel in the amount of sirens that warned of rocket attacks to measure the effect of conflict intensity on birth weight and gestation length among pregnant women during this period. In addition to the common notion that conflict intensity induces stress and anxiety, we show changes in prenatal care in response to sirens. This maternal behavioral response varies based on socioeconomic status, which ultimately differentially affected birth outcomes. While mothers ranked high socioeconomically likely had the resources to increase their prenatal care and shield their fetuses from the negative shock of sirens, mothers ranked low socioeconomically did not have these resources and even decreased prenatal care.

Keywords: Birth outcomes; Israeli-Palestinian conflict; prenatal care; prenatal stress

JEL classification: I10; I12

1. Introduction

In summer 2014 Israel entered another round of armed conflict against the Hamas-ruled Gaza strip, as part of a series of violent escalations and military operations taking place intermittently since the Hamas took power there in 2007. This round, during which Israel responded with a military operation called Operation Protective Edge (OPE), was particularly intense. It lasted for over 50 days, which was far greater than previous rounds that lasted at most for three weeks, and included over 4,500 rockets fired from Gaza toward the Israeli civilian population. We exploit this unexpected and relatively intense event, which severely disrupted the civilian population's daily lives in Israel, to examine birth outcomes for women pregnant during the conflict. We utilize regional and temporal variation in its intensity by measuring the number of siren warnings of rockets that pregnant women experienced in their locality of residence during each pregnancy trimester. These sirens provided civilians 15–90 seconds—depending on the locality's proximity to the Gaza strip—to seek shelter that can protect them in case the rocket hits the ground in close proximity to them, which can be life-threatening. We use restricted data on all births from Soroka University Medical Center, the only hospital serving the Negev region in southern Israel and match these data with the number of sirens in each locality by date.

The effects of intrauterine shocks on birth outcomes have been demonstrated with respect to shocks related to nutritional quality [Lunney (1998); Almond and Mazumder (2011)], diseases and flus [Currie and Schwandt (2013)], psychological stress [Lauderdale (2006); Torche (2011); Currie and Rossin-Slater (2011)], and air quality [Currie and Walker (2011); Currie *et al.* (2011)]. Based on evidence of the long-lasting effects of birth outcomes on adult health, human capital accumulation, and welfare [Black *et al.* (2007); Schwandt (2014)], economists have been intensifying their investigation of the effects of prenatal shocks. Given that various negative shocks prevail more among disadvantaged mothers or communities, these determinants can assist in explaining social and economic disparities and their intergenerational persistence. Furthermore, if the disadvantaged are less able to shield themselves against these negative shocks, they can become a source for increased inequality.

We find both beneficial and detrimental effects of sirens on birth outcomes, and most of the differences are based on maternal socioeconomic status. When examining prenatal care responses to siren intensity, a differential response matches the qualitatively different effect of birth outcomes. Mothers ranked high socioeconomically decreased their probability of being categorized as having lack of prenatal care upon admission to the hospital when giving birth, which could explain improved birth outcomes among these mothers in response to sirens; mothers ranked low socioeconomically increased this probability, thus consistent with more adverse birth outcomes in response to sirens among this population. We use our data to rule out other potential channels, such as migration responses following OPE, selection of mothers giving birth, or selection of births due to miscarriages. Some adverse effects of sirens in the results do not specifically match the changes in prenatal care. Thus, our results are also consistent with a detrimental effect of sirens on birth outcomes through the anxiety and stress these produce.

Our outcomes of interest are: birth weight, gestation length, an indicator for low birth weight (<2500 grams), and an indicator for a pre-term birth (<37 weeks gestation). These are the main birth outcomes most frequently evaluated in studies of this nature, and they have been found to have a profound effect on long-term health and welfare.[1]

Our primary regression specifications include locality-year-of-conception fixed effects. These fixed effects not only allow us to control for potential correlation between the intensity of sirens and locality demographics but also for locality-specific demographics that may change over time. This considerably reduces concern that our explanatory variables of interest—the number of sirens mothers experienced in their locality of residence during each pregnancy trimester—are correlated with other maternal demographic characteristics that may also be determinants of birth outcomes. Specifically, the main source of variation in our regressions stems from

[1]In the online Appendix, we also evaluate the relationship between sirens during OPE and the probability of a cesarean section (C-section), a birth defect, an early water break, being small for gestational age (SGA), and the Apgar 5 score.

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comparing between mothers conceiving a child in the same calendar year and residing in the same locality but with different conception dates, which results in different exposure to sirens during a pregnancy.[2] All conception dates within our sample are prior to OPE to avoid concern of selection into pregnancy during OPE itself and afterwards. The underlying assumption is that the conception date is orthogonal to birth outcomes when controlling for seasonality through month of conception fixed effects.

Our identification also relies on the highly plausible assumption that OPE could not have been anticipated prior to June 2014. The escalated conflict was triggered by the kidnapping and murder of three Israeli teenagers by two Hamas members during June 2014, a severe and unexpected event even amidst the ongoing Israeli-Palestinian conflict.

The paper proceeds as follows. In the next section, we propose the potential underlying mechanisms that can produce changes in birth outcomes in response to sirens during OPE. In section 3 we describe our data from Soroka Hospital as well as the data on sirens during OPE. In section 4, we outline our empirical strategy. In section 5 results are presented both for the entire population of births in our sample and with differential effects based on localities' socioeconomic ranking. Robustness checks are provided in section 6, followed by the conclusions in section 7.

2. Potential mechanisms for the effect of sirens on birth outcomes

Numerous studies have demonstrated the importance of intrauterine shocks to mothers in determining birth outcomes. Many of the shocks investigated in these studies alter the physical environment pregnant women are exposed to in terms of environmental factors such as air quality [Currie and Walker (2011); Currie *et al.* (2011); Currie and Schwandt (2016)], nutritional quality [Lumey *et al.* (1993); Almond and Mazumder (2011)], or the availability of prenatal care [Evans and Lien (2005)]. Other shocks relate to the mother's psychological condition while pregnant, which has been largely investigated in terms of maternal stress. Maternal stress has been shown to increase the production of cortisol, which is related to premature delivery [Lockwood (1999); Hobel (2004)]. Several studies consider natural disasters [Torche (2011); Currie and Rossin-Slater (2011)], bereavement [Persson and Rossin-Slater (2018)], or increases in discrimination and racism [Lauderdale (2006)] to show a causal detrimental relationship between stress experienced by mothers during various phases of their pregnancies and birth outcomes several months later.

Civilian stress can also increase due to armed conflict or violent incidents. In accordance with this, numerous studies have found adverse effects in response to stress induced from armed conflict during the early stages of pregnancy. Mansour and Rees (2012) show that mothers in the Palestinian territories who resided in a town with higher fatalities by Israeli security forces during the Second Intifada gave birth 6–9 months later to infants with a low birth weight in higher probabilities. Camacho (2008) investigates land mine explosions in Colombia as a source of stress during pregnancy and finds a detrimental effect on birth weight as a function of the number of land mine explosions during early pregnancy. Quintana-Domeque and

[2]Our specifications also control for month-of-conception fixed effects, thus allowing us to compare between mothers who conceived during the same calendar month but in different years, and as such experienced different exposure to sirens.

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Ródenas-Serrano (2017) investigate the effect of terror attacks in Spain over a period of more than 20 years and find that terror bomb casualties during the first trimester resulted in lower birth weight. Torche and Shwed (2015) exploit Israel's second Lebanon War in 2006, which similarly affected residents of northern Israel with rocket attacks for several weeks as OPE did for southern Israel, and find that exposure to the conflict early in the pregnancy had negative effects on birth outcomes. The authors do not examine localities separately and do not utilize data on sirens, as we do in the present study. Gluck *et al.* (2019) examine maternal stress and birth outcomes in response to OPE as well. However, their study focuses on comparing birth outcomes between mothers residing in close proximity to Gaza who experienced prolonged exposure to sirens prior to OPE and mothers residing in central Israel for whom the exposure to sirens during OPE was a new phenomenon. Their entire sample consists of births that were all exposed to OPE across different trimesters so it is not possible to conclude from the paper regarding the broad effects of OPE, in comparison to pregnancies that did not experience OPE. They find that OPE adversely affected birth outcomes when experienced during the first and second trimester, in comparison to third trimester exposure, and that the effect depended on whether prior to OPE the mother had already experienced sirens. Brown (2018) finds that exposure to greater violent conflict due to drug wars in Mexico during early pregnancy decreased birth weight. Lastly, Eskenazi *et al.* (2007) exploit the stress induced by the September 11 attacks in New York and show increased probability of low birth weight among mothers experiencing the event early in their pregnancy.

While armed conflict or violence can affect birth outcomes through the anxiety and stress they may generate among pregnant women, behavioral responses among pregnant women during armed conflict can also affect birth outcomes. OPE was a conflict lasting less than 2 months, so migration or fertility responses are not relevant for this setting. Nevertheless, we do test whether our results may be driven by migration patterns shortly after OPE and find no evidence for this (see Table B1 in online Appendix).[3] However, changes in prenatal care can be a likely behavioral response to OPE. On the one hand, OPE increased the risk of performing outdoor activities, including routine activities such as medical check-ups and prenatal care. This could have led to a decrease in prenatal care during OPE. On the other hand, it has been shown that pregnant women can increase pro-health behavior—particularly prenatal care—in response to violent conflict, so as to better shield their fetus from the negative shock. Torche and Villarreal (2014) find improved birth weight outcomes in response to greater exposure to homicides due to drug wars in Mexico. Whether prenatal care decreases or increases in response to OPE may depend on pregnant women's ability to limit or minimize the risk of obtaining prenatal care.

A few studies on armed conflict during pregnancy exploit regional and temporal variation in the intensity measures of armed conflict, such as casualties [Mansour and Rees (2012); Quintana-Domeque and Ródenas-Serrano (2017)], explosions [Camacho (2008)], or homicide rates [Brown (2018)]. Our study also exploits such

[3]Migration responses *during* OPE were very minimal. In the localities that were extremely close to Gaza (up to a few kilometers and excluded from our sample), many families left their towns and communities during the peak of the conflict. However, in most other localities, the vast majority of residents remained. There were no official instructions from the IDF or the government to evacuate any residential area during OPE.

variation by assigning to each mother the number of sirens she experienced in her locality of residence during each trimester of her pregnancy.

Environmental factors were not affected in any way by OPE. The combat fighting was confined to the Gaza strip, where Israelis do not reside, and missiles shot at Israel do not produce any environmental pollutants or chemical hazards. While OPE did affect the economy in Israel, this effect was marginal for the vast majority of households with an estimated loss of 0.4% of GDP.

Absences from work increased during OPE, as child care services were not available in many localities and some employees may have had concerns on traveling to reach their work destinations.[4] For pregnant women, this could lead to a substantial decrease in work attendance, as paid parental leave in Israel does not include the prenatal period.[5] However, results on the effects of work attendance on birth outcomes are not conclusive, with evidence that both adverse [Wüst (2015)], beneficial [Vrijkotte *et al.* (2009); Dave and Yang (2020)], or null [Ahammer *et al.* (2020)] birth outcome effects are associated with decreased work attendance. Thus, although we indeed see increased absence from work using data from the Israel Labor Survey (see Figure C1 in online Appendix), we do not reach any conclusions related to this potential mechanism in our main analysis, given the inconclusiveness regarding the effect this may have on birth outcomes.[6]

3. Data

Data for our analysis are primarily from two sources. The first source is restricted confidential data for all births delivered in Soroka University Medical Center (Soroka) between January 2007 and early August 2015. The second source documents all sirens in Israel by locality and date beginning in June 2014 and ending in September 2014.

Soroka University Medical Center (Soroka) is the only hospital serving the Negev region in southern Israel. The hospital is located in Beer Sheva, the largest city in southern Israel, and provides medical services to approximately one million residents of the South, from Kiryat Gat and Ashkelon to Eilat. Soroka is the third largest hospital in Israel, with 1,151 hospital beds. The nearest hospital to Soroka is roughly

[4]Many summer camps and child care facilities in the south did not operate during OPE. While most of the population continued to work regularly during OPE, a large share of adults took (paid or unpaid) time off from work, especially if child care services were unavailable to them. According to Israeli law, if child care services are canceled due to heightened security events for children age 14 and under, one parent can take paid time off from work to stay with their child—unless employed in an "essential" establishment—hospital, security forces, fire department, utilities, etc. A summary of worker's rights during OPE is available in an article (in Hebrew) dated July 2014—https://www.themarker.com/career/1.2372240.

[5]Employed women giving birth in Israel were entitled (as of 2014) to 14 weeks of paid leave, beginning on the child's birth date and without a possibility to transfer the post-natal paid leave to a period prior to giving birth. As a result, antenatal leave in Israel is quite rare.

[6]There is also the possibility that reduced outdoor activities led to improved birth outcomes, as OPE was during the summer and in southern Israel average temperatures during this period are always over 30 degrees Celsius (and sometimes close to 40 or even beyond). See Son *et al.* (2019); Wang *et al.* (2019) for evidence on the adverse effects of exposure to high temperatures during pregnancy on birth outcomes. Empirically, this is not testable within our framework (i.e., we do not have data on time spent outdoors and whether this was affected by the number of sirens pregnant women experienced), except for our evidence on reduced work attendance, which may have also reduced outdoor activities in order to reach work.

an hour drive from Soroka. Thus, for the vast majority of mothers giving birth in the Negev region, Soroka was the only option for delivery.[7] The number of annual births in Soroka ranged from over 12,000 in 2007 to roughly 15,000 in 2014, steadily increasing over the years. Of these, roughly 55% of births are to Arab mothers and 45% are to Jewish mothers. We focus our analysis on Jewish births, due to concern that the locality for some of the Arab mothers is imprecise, as a large fraction of Arabs in southern Israel reside in unrecognized communities that are not documented in the Soroka birth data. During OPE, Soroka was fully functional. The hospital complex has built-in protective shelters in nearly every hallway in case of a missile attack. All labor and delivery rooms (as well as operation rooms) in Soroka are fully protective in case of a missile attack.

Our birth data detail for each birth the month of birth, the month of release from the hospital, mother's age, birth parity, locality, whether she is Jewish or Arab, and a vast range of pre- and post-birth health conditions and outcomes documented by ICD codes. Mother identification numbers are provided, so that it is possible to identify multiple births for the same mother over time.

Due to data confidentiality, only the month of birth and release from the hospital are provided, rather than exact dates. Our treatment variables—the number of sirens experienced by the mother during her pregnancy—are relative to the conception date. In order to derive an approximate date of conception, we first assume that all birthdates are on the 15th of the birth month provided, except in cases where the release month from the hospital proceeds the birth month, which for the most part should reflect births at the end of the month with a release date in the following month. Thus, for those births we assume a birthdate at the end of the birth month provided. This should generate errors of at most two weeks in the exact date of birth.[8] For the conception date, we take the birthdate we derive, subtract from it the gestation length provided in days, and add to that 14 days.

Our main sample is limited to Jewish mothers ages 20–45 with a birth parity of 10 or less and gestation length of at least 180 days. We exclude mothers from localities that are within 7 kilometers of the Gaza strip. These towns and communities—officially termed the Gaza Envelope in Israel—experienced rocket and mortar firings for several years prior to OPE and their residents were also more likely to leave during OPE.

Most of our analysis focuses on the sample of births conceived during 2011, 2013, and the first half of 2014. We avoid potential selection of births that were conceived

[7]Soroka Hospital is a public hospital in Israel. The entire population in Israel is covered by the national health insurance program that includes all hospitals for giving birth in Israel. Delivery at these hospitals is fully covered.

[8]We verified that these births did not overwhelmingly have longer hospitalization stays by calculating for our entire sample of births (2007–2015) the fraction of births with the month of release proceeding the month of birth. Based on our calculation, under the assumption of a uniform distribution of birth dates across all days of the month, this should roughly be 7.6% (a weighted average of regular births occurring in the last 2 days of the month and c-section births occurring in the last 3–4 days of the month). In our calculations for each month of the year, this fraction ranged from 8% to 10.5% (10.5% was for February, the month with the least number of days). Thus, it appears that a small fraction of births with a release month following the birth month are due to longer hospitalization stays. However, the overwhelming majority of these births are likely due to birthdates at the end of the month. We also re-ran the baseline regression specifications without the birthdate adjustment and for the sample of births that excludes births with a release month following the birth month, and the results (not presented) were highly robust to these changes.

during and immediately after OPE, by excluding from our sample all births with a conception date beginning July 1, 2014. We also exclude births conceived during 2012 due to an eight-day military operation during November 2012, which also involved rocket attacks. We thus have two conception years—2013 and 2014 with varying exposure to OPE and sirens during the pregnancy trimesters, along with 2011 conceptions, which complement 2013 control group conceptions, such that the sample includes conceptions that were not exposed to OPE during pregnancy from all calendar months.[9] When using samples from longer time frames for our analysis, we exclude pregnancies exposed to the other military operation in Gaza preceding OPE—Operation Cast Iron in December–January 2008.

Our outcome variables consist of the following post-natal maternal and newborn health conditions: birth weight in grams along with an indicator for low birth weight (LBW) (≤ 2500 grams), and gestation length in days along with an indicator for pre-term delivery (<37 weeks gestation length).[10] We utilize the health conditions available in the Soroka birth data to control for the following pre-birth conditions in our regression analysis: maternal age (categorical: 20–26, 26–30, 30–35, 35–40, 40–45), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment.[11]

Our birth data are merged with data on all sirens by date and locality during OPE and the month preceding OPE. Israel's Home Front Command—an IDF branch responsible for instructing the civilian population on coping with military threats facing Israel—has on its web site detailed documentation of all sirens by date and locality beginning in mid-July 2014. Because we needed data on sirens from as early as June 2014, we complemented the Home Front Command data with documentation of all sirens by date and locality from a provider of a cell phone application that alerts of sirens in Israel.[12]

Israel's Home Front Command divides large cities in Israel into areas such that a single rocket targeted toward the city does not automatically initiate sirens across the entire city but rather just for the areas that are at risk. The data on sirens and maternal residence are just at the locality level. As such, it is likely that the number of sirens in large cities does not reflect the true number of sirens mothers experienced in the locality. For this reason, for the three largest cities in our data—Beer Sheva, Ashkelon, and Ashdod—we divide the total number of sirens by two.[13]

Based on our derived conception date, we assign to each mother the number of sirens in her locality during each trimester of her pregnancy. If exposure to OPE or to sirens affected the birthdate, then the number of sirens a mother experiences is

[9]For 2013, only conceptions through September did not experience OPE during pregnancy.

[10]In the online Appendix, we also present results for 5 additional birth outcomes: cesarean delivery, birth defects, early water prior to delivery, the Apgar score 5 minutes after birth, and being born small for gestational age.

[11]Several pre-birth health conditions could not be included as control variables due to substantial changes in their coding over the course of the sample period. These include all diabetic (including gestational) health conditions, and whether the mother had early births in the past.

[12]We verified that the sirens data from the cell phone application provider was identical to the Home Front Command official data during mutual dates covered to ensure consistency in our assignment of sirens to pregnant mothers during OPE.

[13]We note that if multiple rockets were fired at a large city at the exact time, then this would be registered as a single siren for that city in the siren data, although it may be that all or most areas in the city had a siren.

endogenous to our outcomes of interest. To address this potential violation of our exclusion restriction, we follow [Persson and Rossin-Slater (2018)], and define our treatment variables—the number of sirens experienced during pregnancy trimesters—relative to the expected due date at full term rather than the actual birthdate.

We note that we only observe maternal locality when giving birth, rather than during pregnancy. This does not pose a problem to our analysis as long as migration is minimal (so that the noise in assigning sirens based on locality at birth remains minimal) and migration patterns post-OPE are not systematically correlated with the number of sirens in a locality. We test for this using data from the Israel Central Bureau of Statistics (CBS) on the migration balance for all towns and cities in Israel in 2015. Migration balances are relatively small—2.5–4.8 individuals per 1,000 residents. Furthermore, we do not find a correlation between migration balances per 1,000 residents in Jewish towns and the total number of sirens the town experienced during OPE (coefficient estimates for various regressions were positive with p-values ranging between 0.21 and 0.38). Actual results are presented in Table B1 in online Appendix.

Our birth and siren data are complemented by locality-level data from the CBS. This particularly includes the locality socioeconomic ranking, an official measure in integers ranging between 1 (the lowest) and 10 (the highest), which is based on population characteristics such as the mean age, dependency ratio, the share of families with 4 or more children, educational attainment, employment and retirement, and living standards (mean income, vehicle ownership, and travel abroad). We separately examine effects for mothers residing in localities that are ranked low and high socioeconomically among Jewish localities.

3.1. Descriptive statistics

Table 1 presents summary statistics for pre-birth characteristics and siren exposure for the main sample of births used in our regression analysis—births conceived during 2011, 2013, and the first half of 2014. We test for potential selection of mothers based on exposure to OPE at pregnancy trimesters with t-tests for differences in the mean characteristics between mothers who did not experience OPE and mothers who did experience OPE in any of the trimesters. P-values for these t-tests are presented in columns 3, 5, and 7. Most pre-birth characteristics are balanced between the sub-samples, with the exception of differences in the abortion history and c-section history indicators between mothers experiencing OPE during the third trimester and mothers who did not experience OPE. These are two differences out of 24 differences that are tested for in Table 1, so statistically significant differences for two tests are possible by chance. We further note that there are several p-values for the t-test results presented in Table 1 that are less than 0.2. In total, there are 6 such p-values, which can also result by chance in a series of t-tests.[14]

The last row in Table 1 shows the mean number of sirens experienced by mothers exposed to OPE during each of the pregnancy trimesters. Figure 1 displays how the number of sirens is distributed across births that are exposed to OPE during each

[14]We also conducted the same tests for differences between treatment and control births based on conception months (not presented), resulting in 9 such tests for each pre-birth characteristic because there are no treated births with July–September as conception months. Out of 72 tests (9 months*8 pre-birth characteristics), six resulted in p-values under 0.1.

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Table 1. Summary statistics—pre-birth characteristics

	Did not experience OPE during pregnancy	1st trimester during OPE	*p*-value for *t*-test of difference 1st trim.	2nd trimester during OPE	*p*-value for *t*-test of difference 2nd trim.	3rd trimester during OPE	*p*-value for *t*-test of difference 3rd trim.
Number of births	9,439	977		2,323		2,327	
Mother's age	30.612 (4.964)	30.826 (4.999)	0.200	30.800 (5.045)	0.103	30.788 (5.076)	0.127
Birth number	2.373 (1.431)	2.415 (1.451)	0.385	2.415 (1.471)	0.209	2.402 (1.480)	0.390
Male baby	0.512 (0.500)	0.498 (0.500)	0.416	0.519 (0.500)	0.569	0.526 (0.499)	0.220
Abortion history	0.034 (0.182)	0.034 (0.181)	0.928	0.032 (0.176)	0.555	0.026 (0.159)	0.037
High fertility	0.072 (0.259)	0.076 (0.265)	0.707	0.080 (0.272)	0.185	0.082 (0.275)	0.111
C-section history	0.139 (0.346)	0.141 (0.348)	0.832	0.132 (0.339)	0.406	0.115 (0.319)	0.003
Late pregnancy loss	0.013 (0.114)	0.014 (0.119)	0.778	0.012 (0.111)	0.773	0.012 (0.111)	0.764
Infertility treatment	0.039 (0.193)	0.044 (0.205)	0.403	0.038 (0.191)	0.878	0.037 (0.188)	0.642
Sirens during trimester	0.000	27.339 (15.961)	N/A	24.002 (17.388)	N/A	23.737 (17.456)	N/A

Notes: The sample is limited to all Jewish births in Soroka Hospital conceived during 2011, 2013, and the first half of 2014 with gestation length of at least 180 days and residents of localities in Southern Israel. The number of births is smaller for first trimester exposure to OPE due to the exclusion of all births conceived during OPE to avoid selection concern. Columns 1, 2, 4, and 6 present means with standard deviations in parenthesis. Columns 3, 5, and 7 present *p*-values for a *t*-test of the difference between the means of the relevant trimester during OPE and those that did not experience OPE during pregnancy. The samples for experiencing OPE during the first, second, or third pregnancy trimesters are not mutually exclusive.

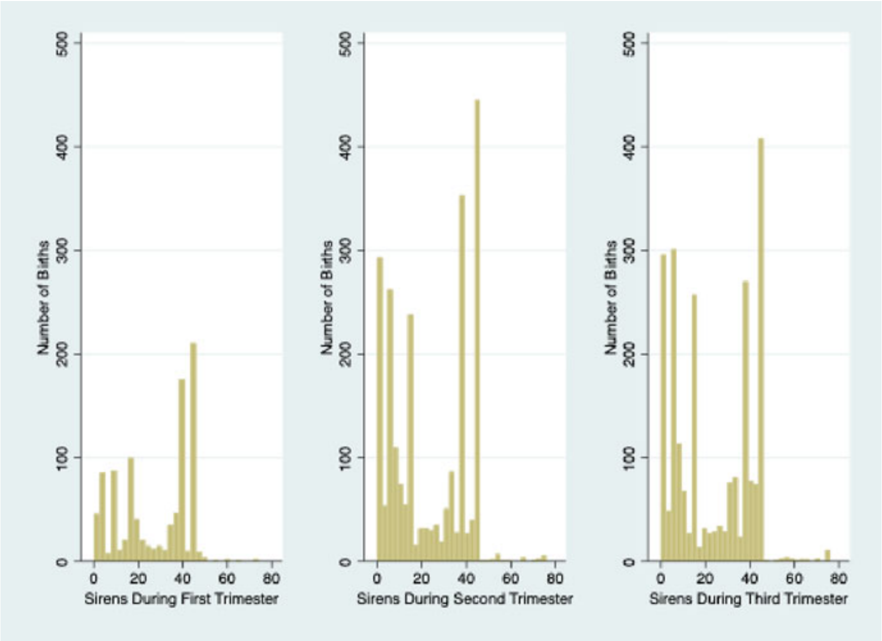

Figure 1. Number of sirens experienced during OPE by pregnancy trimester exposure to OPE.
Notes: The sample is limited to all Jewish births in Soroka Hospital conceived during 2013 and through the first half of 2014 with gestation length of at least 180 days and residents of localities in Southern Israel. Each histogram is limited to births that were exposed to OPE during the relevant trimester. The number of births is smaller for first trimester exposure to OPE due to the exclusion of all births conceived during OPE to avoid selection concern. Total number of localities is 85.

trimester. Bunching at some of the siren values in Figure 1 is due to more births being attributed to larger towns that experienced that amount of sirens. We note though that even within these towns, there is variation across births exposed to OPE in the number of sirens due to different dates for each of the trimesters across births. In Figure D1 in online Appendix, we present the same histograms as in Figure 1, only separately based on the socioeconomic ranking of mothers' locality.

4. Empirical strategy

Our main regression specification examines the linear relationship between sirens experienced during each of the pregnancy trimesters and birth outcomes. This results in the following regression specification:

$$Outcome_{ilym} = \alpha_0 + \sum_{t=1}^{3} \alpha_1^t Sirens_{ilym}^t + \eta X'_{iym} + \rho_m + \theta_{ly} + \varepsilon_{ilym}. \quad (1)$$

Our dependent variables are birth outcomes for mother i in locality l with a child conceived in year and month (y, m). The main coefficients of interest in equation (1)

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are α_1^t for $t \in \{1, 2, 3\}$ that represent trimesters 1,2, and 3, respectively. These three coefficients of interest indicate how birth outcomes change for each additional siren the mother experiences during the specific pregnancy trimester in comparison to mothers who experienced no sirens during their pregnancy.

Equation (1) controls for pre-birth maternal and fetal characteristics [X'_{iym}—mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment].[15] ρ_m are month of conception fixed effects.

The intensity of sirens may be correlated with locality characteristics that may also be determinants of birth outcomes—for example, sirens were relatively more frequent in larger localities and the larger localities in southern Israel are relatively more disadvantaged. Locality fixed effects can control for that. However, if locality characteristics are changing over time and this is correlated with OPE exposure at the end of our sample period, then locality fixed effects cannot control for that. As such, our regression specification is more conservative and controls for locality-year-of-conception fixed effects, θ_{ly}, rather than just locality fixed effects.

Our results will demonstrate the importance of accounting for differential effects of sirens on birth outcomes based on socioeconomic status. Our only source for characterizing maternal socioeconomic status is the socioeconomic ranking of the mother's town, categorized on a scale of 1–10 by the Israel CBS. As such, we utilize a variation of equation (1) by interacting our siren count variables for each trimester with indicator variables for the mother residing in a low socioeconomically ranked town (4 or less in the CBS ranking) or high socioeconomically ranked town (5 or more). This results in the following regression specification:

$$Outcome_{ilym} = \beta_0 + \sum_{t=1}^{3} \beta_1^t Sirens_{ilym}^t * LowSocio_{ly} + \sum_{t=1}^{3} \beta_2^t Sirens_{ilym}^t * HighSocio_{ly} + \eta X'_{iym} + \rho_m + \theta_{ly} + \varepsilon_{ilym}. \quad (2)$$

In equation (2), all variables are as defined in equation (1), only there are now six coefficients of interest—the three siren counts for each trimester interacted with the high and low socioeconomic categorizations.

We argue that our coefficients of interest are capturing a causal relationship between birth outcomes and sirens during OPE. This is under the assumption that absent OPE, birth outcomes would have continued on the same path they were on prior to OPE. Because OPE—especially its exact timing and its length—can be viewed as an exogenous shock in Israel, this identification assumption seems highly reasonable. We stress further that all treated births in our sample were conceived prior to OPE, and that it was not possible to foresee OPE even a month before its onset. Given the locality-year-of-conception fixed effects, we can broadly generalize our identification

[15] The sex of the child can be endogenous due to greater fetal loss among male fetuses in response to prenatal shocks [Sanders and Stoecker (2015); Valente (2015)]. We tested for this by running regressions as in equation (1) with an indicator variable for male chid as the dependent variable and did not find that the amount of sirens had a statistically significant effect on child gender (see Table 4, column 2). Nevertheless, we stress that exclusion of child's sex as a control variable did not change the results.

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strategy to comparing between mothers conceiving during the same year and residing in the same locality, only their conception dates vary by several months. This difference in the conception date generates exogenous variation in the number of sirens each of these mothers experienced. This is while accounting for seasonality of conception by including in our regression specification month of conception fixed effects.[16]

5. Results

5.1. Birth outcomes

Table 2 presents results for our four dependent variables of interest—birth weight (in grams), gestation length (in days), low birth weight, and pre-term delivery—from equation (1). For each dependent variable, the pair of columns shows the coefficient estimates $\alpha_1 - \alpha_3$, with or without birth-specific control variables. The similarity of the results between the two columns is reassuring as this is evidence that our main explanatory variables of interest are not correlated with our control variables.

The results suggest a decline in birth weight in response to sirens experienced during the second and third pregnancy trimesters. With the mean number of sirens during the second and third trimesters being roughly 24 (see Table 1), the mean decrease in birth weight is 17 and 29 grams for the second and third trimesters, respectively. This represents 0.5% and 0.9% decline from the mean birth weight among births in the sample that did not experience OPE.

When evaluating the three other birth outcomes in Table 2, the results suggest *improvements* in response to sirens, rather than adverse effects. There is an increase in gestational length, representing 0.5% and 0.2% of the mean gestation length for births not experiencing OPE in response to first and second trimester sirens, respectively. There is also a statistically significant decline in the probability of being born with low birth weight or pre-term in response to second trimester sirens. Lastly, even the birth weight response to sirens during the first trimester is positive, with a p-value that is marginally statistically significant at 0.11.

We proceed to equation (2), which allows for a differential effect of sirens, depending on mothers' residence socioeconomic ranking. This specification results in six coefficients of interests—for each of the three pregnancy trimesters interacted with indicators for low versus high socioeconomically ranked towns of residence.

The results—exhibited in Table 3—show that births to mothers from localities ranked low socioeconomically mostly experienced detrimental effects in response to sirens. Their birth weight declined by 2.2–4.5% of the mean birth weight for non-OPE mothers in response to sirens in all three trimesters, and their probability for having a low birth weight increased. The only exception to these adverse effects among the low socioeconomic mothers is decreases in the probability of being born pre-term in response to second and third trimester sirens. In contrast, births to mothers from localities ranked high socioeconomically experienced improvements in

[16]An additional source of variation emerges from the conception month fixed effects, ρ_m, in our regression specifications. The inclusion of these implies that siren effects are identified among mothers who conceived during the same calendar month but in different years, resulting in different exposures to sirens, depending on the year of conception. We stress the within locality-year of conception variation more than the calendar month of conception variation, as within locality-year, mothers are much more comparable, thus providing a stronger case against arguments that unobserved maternal characteristics are driving the results.

Table 2. Birth weight and gestation length following sirens during pregnancy

	Birth weight (grams) (mean = 3, 215)		Gestation length (days) (mean = 272.7)	
Sirens—trimester 1	0.698 (0.486)	0.693 (0.432)	0.059*** (0.012)	0.054*** (0.010)
Sirens—trimester 2	−0.768* (0.393)	−0.699* (0.400)	0.030*** (0.010)	0.027*** (0.009)
Sirens—trimester 3	−1.187*** (0.412)	−1.227*** (0.389)	0.002 (0.011)	−0.000 (0.011)
	Low birth weight (mean = 0.065)		Pre-term delivery (mean = 0.128)	
Sirens—trimester 1	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
Sirens—trimester 2	−0.000*** (0.000)	−0.000*** (0.000)	−0.001*** (0.000)	−0.001*** (0.000)
Sirens—trimester 3	0.000 (0.000)	0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
Observations	13,489	13,488	13,489	13,488
Conception month fixed effects	✓	✓	✓	✓
Birth-specific controls		✓		
Locality-conception year FE's	✓	✓	✓	✓

Notes: The number of localities is 85. See section 3 for information on the sample. The coefficient estimates presented are $\alpha_1 - \alpha_3$ from equation (1). Birth-specific controls are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Mean of dependent variable is the mean for non-OPE births in the sample. Standard errors are clustered at the locality level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

response to sirens. Their birth weight increased by 1% of the mean among non-OPE mothers in response to first trimester sirens, and their probabilities of being low birth weight or pre-term declined as well in response to first and second trimester sirens. A single exception here is the birth weight decrease among mothers in localities ranked high socioeconomically in response to sirens during the third trimester.

In Table E1 in online Appendix we evaluate other birth outcomes—namely, c-sections, birth defects, early water breaks, the Apgar 5 score, and being born small for gestational age. Many of the coefficient estimates further demonstrate improvements in birth outcomes among mothers from highly ranked localities in response to sirens during the first and second trimesters, in addition to some adverse effects of sirens among mothers from localities ranked low and high socioeconomically. This lends further support to the results in Table 3 demonstrating improvement in some birth outcomes among mothers from localities ranked high socioeconomically in response to siren exposure.

Our data enable us to include mother fixed effects in our regression specifications [not specified in equation (2)], as the data include a mother identifier. Mother fixed effects have the advantage of controlling for unobserved maternal characteristics that

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Table 3. Birth weight and gestation length following sirens during pregnancy—differential effects based on maternal locality socioeconomic ranking

	Birth weight (grams)	Gestation length (days)	Low birth weight	Pre-term delivery
Sirens—trimester 1—low socio	−5.292*** (0.959)	−0.031 (0.027)	0.004*** (0.001)	−0.000 (0.001)
Sirens—trimester 2—low socio	−3.388** (1.557)	−0.036 (0.041)	0.001 (0.001)	−0.003** (0.001)
Sirens—trimester 3—low socio	−3.046*** (0.762)	−0.005 (0.017)	0.000 (0.001)	−0.002*** (0.000)
Sirens—trimester 1—high socio	1.244** (0.526)	0.063*** (0.013)	−0.000* (0.000)	−0.000 (0.000)
Sirens—trimester 2—high socio	−0.474 (0.446)	0.033*** (0.012)	−0.001*** (0.000)	−0.001*** (0.000)
Sirens—trimester 3—high socio	−1.083** (0.479)	0.000 (0.011)	0.000 (0.000)	−0.000 (0.000)
Observations	13,467	13,467	13,467	13,467
Mean dependent variable—low socio	3,213	272.6	0.0650	0.129
Mean dependent variable—high socio	3,220	272.9	0.0641	0.124
Conception month fixed effects	✓	✓	✓	✓
Birth-specific controls	✓	✓	✓	✓
Locality-conception year FE's	✓	✓	✓	✓

Notes: The number of localities is 84. Low (high) socio indicates that the siren count variable for each trimester is interacted with an indicator variable for the mother's residence being ranked 4 or less (5 or more) in the CBS official socioeconomic ranking of towns. Based on this, 17 towns are ranked low socioeconomically, representing 3,317 of the total number of observations. See section 3 for information on the sample. The coefficient estimates presented are β_1 – β_6 from equation (2), as discussed in section 4. Birth-specific controls are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Mean of dependent variable is the mean for non-OPE births in the sample. Standard errors are clustered at the locality level.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

may be correlated both with our main explanatory variables and our outcome variables, thereby alleviating concern that results are driven, for example, by a different population of pregnant women during OPE. However, mother fixed effects also limit the variation in our analysis to within mothers who were pregnant with a child during OPE and were pregnant with at least one other child not during OPE. This decreased variation can inflate the standard errors of our coefficient estimates. We present results for equation (2) with mother fixed effects in Table F1 in online Appendix. Of the 24 coefficient estimates for the four variables of interest in Table F1 in online Appendix, four are statistically significant at the 10% level. However, only two of these are consistent with the results in Table 3. There are several additional point estimates in Table F1 in online Appendix that are very similar to those in Table 3—low socio first trimester birth weight response, high socio first trimester gestation length response, and first trimester low birth weight response—only the standard errors are substantially larger, and as such, they are not statistically significant.

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5.2. Prenatal care and birth selection due to miscarriages

As discussed in section 2, one important potential channel for birth outcome responses to sirens is changes in prenatal care. Changes in prenatal care can explain both adverse and improved birth outcomes, depending on whether sirens decreased or increased prenatal care.

In the first columns of Table 4 we present results for the same equation as in Table 3, only the dependent variable is an indicator for Lack of Prenatal Care (LOPC). LOPC is defined as reporting less than three prenatal care visits during the entire pregnancy. This is an extreme case of reduced prenatal care, and its percent among Jewish mothers giving birth in Soroka is quite low (<3%). However, it is quite plausible that changes in birth outcomes would only be statistically detectable in response to extreme cases of reduced prenatal care.[17]

Table 4 shows that the probability of LOPC increased for mothers from localities ranked low socioeconomically and experiencing sirens during the first and third trimesters.[18] Thus, decreased prenatal care can explain adverse birth outcomes among mothers from localities ranked low socioeconomically, and this is particularly consistent with lower birth weight and a high probability of low birth weight among low socioeconomically ranked mothers in response to first trimester sirens. In contrast, mothers from localities ranked high socioeconomically decreased their probability of LOPC in response to first and second trimester sirens. Thus, increased prenatal care can explain improved birth outcomes among mothers from localities ranked high socioeconomically that are observed in Table 3, particularly in response to first and second trimester sirens.

The second column of Table 4 examines whether changes in the probability of having a male birth changed in response to sirens. This test serves to examine whether sirens during OPE resulted in a greater probability of miscarriage, which would bias the effect of sirens, as an increase in miscarriages would entail positive selection of births. Because we do not have data on miscarriages, we use the probability for a male birth as a proxy for changes in miscarriages, as it has been shown that male fetuses are more vulnerable to negative intrauterine shocks, and as such, greater miscarriages would result in a lower probability of male births [Kraemer (2000); Sanders and Stoecker (2015); Valente (2015)].

Table 4 shows that the probability for a male birth did not change in response to sirens, except among mothers from localities ranked low socioeconomically in response to third trimester sirens. Because miscarriages in response to shocks towards the end of pregnancy should be relatively rare, we interpret the results for the male dependent variable in Table 4 as evidence that improved birth outcomes in response to sirens are not being driven by an increase in miscarriages.[19]

5.3. Discussion

We observe both detrimental and beneficial birth outcome responses to sirens during OPE. The beneficial effects are almost entirely among mothers who reside in localities

[17]Prenatal care during pregnancy in Israel is fully covered as part of the National Health Insurance Program.

[18]The p-value for the estimated effect of second trimester sirens among mothers from low socioeconomically ranked localities is 0.23.

[19]We note that the lowest p-value for all other coefficient estimates in Table 4 for the male dependent variable is 0.23.

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Table 4. Lack of prenatal care and male probability following siren exposure

	LOPC	Male
Sirens—trimester 1—low socio	0.001** (0.001)	−0.003 (0.002)
Sirens—trimester 2—low socio	0.001 (0.000)	−0.002 (0.002)
Sirens—trimester 3—low socio	0.001*** (0.000)	−0.001* (0.001)
Sirens—trimester 1—high socio	−0.000* (0.000)	−0.000 (0.001)
Sirens—trimester 2—high socio	−0.001** (0.000)	−0.001 (0.001)
Sirens—trimester 3—high socio	0.000 (0.000)	0.000 (0.000)
Observations	13,467	13,467
Mean dependent variable—low socio	0.0297	0.522
Mean dependent variable—high socio	0.0138	0.508
Conception month fixed effects	✓	✓
Birth-specific controls	✓	✓
Locality-conception year FE's	✓	✓

Notes: The number of localities is 84. Low (high) socio indicates that the siren count variable for each trimester is interacted with an indicator variable for the mother's residence being ranked 4 or less (5 or more) in the CBS official socioeconomic ranking of towns. Based on this, 17 towns are ranked low socioeconomically. See section 3 for information on the sample. The coefficient estimates presented are $\beta_1 - \beta_6$ from equation (2). LOPC stands for Lack of Prenatal Care, an indicator variable for mothers who report less than three prenatal care visits during pregnancy. Birth-specific controls are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Mean of dependent variable is the mean for non-OPE births in the sample. Standard errors are clustered at the locality level.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

ranked high socioeconomically. The detrimental effects are more among mothers residing in localities ranked low socioeconomically, but not exclusively. To better understand what may be driving these effects, we empirically test for evidence of two underlying mechanisms. Positive selection of births due to an increased prevalence of miscarriages is not supported by regressions testing for changes in the probability of a male birth. In contrast to this, differential responses in terms of prenatal care across mothers that are high versus low socioeconomically ranked support the differential birth outcome responses to sirens based on socioeconomic ranking.

Our increased prenatal care response and improved birth outcomes results are consistent with Torche and Villarreal (2014), who also find increased prenatal care in response to greater exposure to homicide rates in Mexico, and as a result of this, birth outcomes improved. However, while in Torche and Villarreal (2014) increased prenatal care is concentrated among urban mothers ranked low socioeconomically, our results show increased prenatal care among mothers ranked high socioeconomically and decreased prenatal care among mothers ranked low socioeconomically. This can understandably be attributed to the differences between

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the two sources of exposure to violence. In Torche and Villarreal (2014) exposure to homicides does not pose an immediate threat to the vast majority of pregnant women going about their routine activities—rather, this exposure likely increases mental stress that pregnant women can choose to shield themselves against through greater prenatal care. In contrast, during OPE, the threat of a rocket attack was immediate while being outdoors and performing routine activities, and as such choosing to shield against the stress induced from this threat through greater prenatal care is more difficult, given the risk associated with leaving one's home. In fact, mothers from low socioeconomically ranked towns appeared to have responded to this threat by *decreasing* their prenatal care. In contrast to this, mothers from higher socioeconomically ranked towns did respond to the psychological stress by increasing their prenatal care, and this may be due to greater availability of resources among these mothers to obtain this potential shielding—for example, having a car that can reach a clinic more quickly or someone who can accompany them to the visit.

Lastly, there are still some adverse effects that are observed in Table 2—particularly among low socioeconomically ranked mothers—that our analysis showing changes in prenatal care does not explain. For example, birthweight among mothers in localities ranked low socioeconomically decreased in response to sirens during the second trimester, but we do not observe a change in the probability of LOPC among mothers ranked low socioeconomically in response to second trimester sirens.[20] This leaves the possibility of stress resulting from sirens adversely affecting birth outcomes, with possibly a stronger effect among mothers ranked low socioeconomically.

6. Robustness checks

6.1. Placebo analysis

We examine whether the results in section 5 are due to pre-existing trends. For this, we run the specification in equation (1) but on data that assigns the OPE exposure and siren variables as if OPE occurred in summer 2010 rather than summer 2014. The sample for this is births conceived during the first half of 2010, all of 2011, and 2009 beginning in April.[21] The results presented in Table 5 are reassuring. While the coefficient estimate for third trimester sirens is statistically significant for pre-term birth, all other 11 coefficient estimates are not statistically significant. With the exception of the third trimester birth weight coefficient estimate, with a p-value of 0.18, all other p-values are at least 0.24. Thus, this result may be by chance.

6.2. Testing for selection of mothers giving birth during operation protective edge

One can argue that it is problematic to include births that took place during OPE in Soroka Hospital, as there may be selection of a different population giving birth during this period. As we will show below, this does not appear to be the case. Furthermore, these births are the main sample of births that experienced OPE during their third trimester. Given this, excluding these births would limit

[20]It should be noted that this coefficient estimate is positive with a p-value of 0.23.

[21]There were two additional military operations with rocket attacks toward the civilian population during December–January 2008–2009 and November 2012. Due to this, the above-mentioned sample was selected, such that it would exclude pregnancies exposed to these military operations yet still cover within the control group all conception calendar months.

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Table 5. Placebo analysis—assigning OPE to summer 2010 births

	Birth weight	Gestation length	LBW	Preterm
Sirens—trimester 1	−0.816 (0.693)	0.000 (0.010)	0.000 (0.000)	0.000 (0.000)
Sirens—trimester 2	−0.520 (0.565)	−0.006 (0.010)	0.000 (0.000)	−0.001 (0.001)
Sirens—trimester 3	−0.715 (0.523)	0.004 (0.010)	−0.000 (0.000)	−0.001*** (0.000)
Observations	11,082	11,082	11,082	11,082
R^2	0.05	0.06	0.03	0.05
Mean of dependent variable	3201	272.6	0.0669	0.138
Conception month fixed effects	✓	✓	✓	✓
Birth-specific controls	✓	✓	✓	✓
Locality-conception year fixed effects	✓	✓	✓	✓

Notes: Number of localities is 78. The coefficient estimates presented are $\alpha_1 - \alpha_3$ from equation (1). Birth-specific control variables are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Mean of dependent variable is the mean for non-OPE births in the sample, in the above case births not assigned OPE treatment based on the placebo OPE. Standard errors are clustered at the locality level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

substantially our ability to estimate the effect of third trimester sirens on birth outcomes. Lastly, we note that the effects reported in response to third trimester births are mixed in Table 3. While most coefficient estimates point at worse birth outcomes in response to third trimester sirens, there is also a statistically significant decline in the probability of a pre-term birth among mothers ranked low socioeconomically in response to third trimester sirens. Thus, it is not clear which direction the selection would be, if there were any.

If different mothers came to give birth in Soroka during OPE, then the distribution of birth months over the course of the year of OPE—2014—should be different in comparison to other years. We would expect less births during July–August 2014 when OPE was taking place. We test for this in Table 6 by running regressions with the dependent variable being an indicator for a birth occurring during July–August. These regressions are run for the sample of births occurring in 2007–2009, 2011–2012, and 2014 during January through October, in order to exclude births exposed to other military operations (see section 3 on sample restrictions due to this). Two alternative specifications explore two different explanatory variables of interest. In the first, the main explanatory variable is an indicator for a birth taking place during 2014, thus comparing the probability of birth during July–August 2014 to that of all other years in the regression sample. In the second specification, the main explanatory variable is the total amount of sirens in the mother's locality during OPE divided by 20.[22] The latter regression specification controls also for

[22]The number of sirens is divided by 20 so that the coefficient estimates and their standard errors do not appear as zero.

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Table 6. Testing for different probability of births during July–August 2014

	Dependent variable: July–August birth					
	All localities		Disadvantaged localities		More advantaged localities	
Year of birth is 2014	−0.002 (0.007)		−0.004 (0.007)		−0.002 (0.007)	
Multiple of 20 sirens		0.002 (0.003)		0.010 (0.015)		0.003 (0.006)
Observations	20,881	20,881	4,839	4,839	15,874	15,874
R^2	0.01	0.01	0.01	0.01	0.01	0.01
Mean of dependent variable	0.207	0.207	0.197	0.197	0.211	0.211
Locality fixed effects	✓	✓	✓	✓	✓	✓
Year of birth fixed effects		✓		✓		✓
Birth-specific controls	✓	✓	✓	✓	✓	✓

Notes: The sample includes birth occurring during January–October in 2007–2009, 2011–2012, and 2014. Birth-specific control variables are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Columns (3)–(4) limit the sample to maternal localities that are ranked socioeconomically at level 4 or less. Columns (5)–(6) limit the sample to maternal localities that are ranked socioeconomically at level 5 or more. Standard errors in columns (1), (3), and (5) are clustered at the year of birth level, while in columns (2), (4), and (6) at the locality level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

year-of-birth fixed effects. All regression specifications for Table 6 control for locality fixed effects, linear time trends, and birth-specific controls. Standard errors are clustered in the year of birth level in the former regression and at the locality level in the latter regression.

The results in Table 6 do not show statistically significant changes in the probability of a birth during July–August 2014, in comparison to other years—both in general (first column) and as a function of the number of sirens in the mother's locality (second column). Furthermore, these results hold when examining only the sample of births from mothers either from disadvantaged or more advantaged localities. Thus, the evidence does not suggest more or less births in Soroka during OPE.[23]

6.3. Different specifications

Equations (1) and (2) assume a linear relationship between sirens and birth outcomes. We verify whether this is appropriate for capturing the potential effect of sirens on birth outcomes using two alternative specifications: a non-parametric specification and a non-linear quadratic specification.

[23]Home deliveries are quite rare in Israel. According to data from https://www.yoldot.leidaraka.co.il/, a website promoting natural and home deliveries in Israel, in 2014, there were 796 planned home deliveries, and this is out of roughly 170,000 births in Israel annually. The data also indicate that home deliveries were on a rising trend for several years prior to 2014 (755 in 2013, 721 in 2012, and 716 in 2011), and thus it does not appear that there was an increase in home deliveries due to OPE.

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For the non-parametric specification, we split the siren count variables for each trimester into indicator variables for the following: 1–20 sirens, 21–40 sirens, and 41 + sirens, producing a total of 9 coefficients of interest. The excluded group consists of mothers who did not experience sirens during their pregnancy. This regression specification takes the following form:

$$Outcome_{ilym} = \gamma_0 + \sum_{t=1}^{3}\sum_{k=1}^{3}\gamma_k^t SirensIndic_{ilym}^{t,k} + \eta X'_{iym} + \rho_m + \theta_{ly} + \varepsilon_{ilym}. \qquad (3)$$

All dependent and control variables are as in equation (1). The main coefficients of interest in equation (3) are $\gamma_1^t - \gamma_3^t$ for $t \in \{1, 2, 3\}$ that represent trimesters 1,2, and 3, respectively. These 9 coefficients of interest indicate how birth outcomes change when the mother experienced the respective range of sirens during the specific pregnancy trimester in comparison to mothers who experienced no sirens during their respective trimester.

The results of this specification are outlined in Figure 2 for our four main dependent variables. The coefficient estimates for each siren range and pregnancy trimester are the dots and the vertical lines represent their 95% confidence intervals. The results suggest both adverse and beneficial effects of sirens on birth outcomes. For example, a large amount of sirens during the first trimester appears to positively affect both birth weight and gestation length. On the other hand, a large range of sirens during the third trimester decreases birth weight and gestation length and increases the probability of low birth weight. With the exception of the pre-term dependent variable, which exhibits statistically significant effects in response to the lowest siren range, all other patterns exhibited in Figure 2 are consistent with a linear relationship between sirens and the birth outcome.

Due to some patterns in Figure 2 that may suggest a non-linear relationship between sirens and birth outcomes, we present in Figure 3 results from a specification that allows for a quadratic relationship between sirens and birth outcomes, differentiated by low and high maternal locality socioeconomic ranking (as in equation (2)). The figure presents predicted values from these specifications for various siren counts. First, the results exhibit for birth weight and low birth weight a qualitatively different effect of sirens based on maternal locality socioeconomic ranking, in particular in response to first and third trimester sirens. For the first trimester response, this is highly consistent with the results in Table 3. Second, when the predicted values for positive siren counts are statistically different from those for zero siren counts, the patterns plotted do not overwhelmingly appear non-linear. This lends further support to our linear specifications in equations (1) and (2).

In the online Appendix, we further challenge our regression specification by examining the relationship between sirens per 1,000 locality population and birth outcomes, under the assumption that the effect of a siren may be smaller in larger localities that are more spread. The results are not as precise as those presented in Table 3.

6.4. Different gestation length thresholds

In section 3, we explain that the number of sirens assigned to each mother in constructing the sirens treatment variable is based on the mother's due date rather

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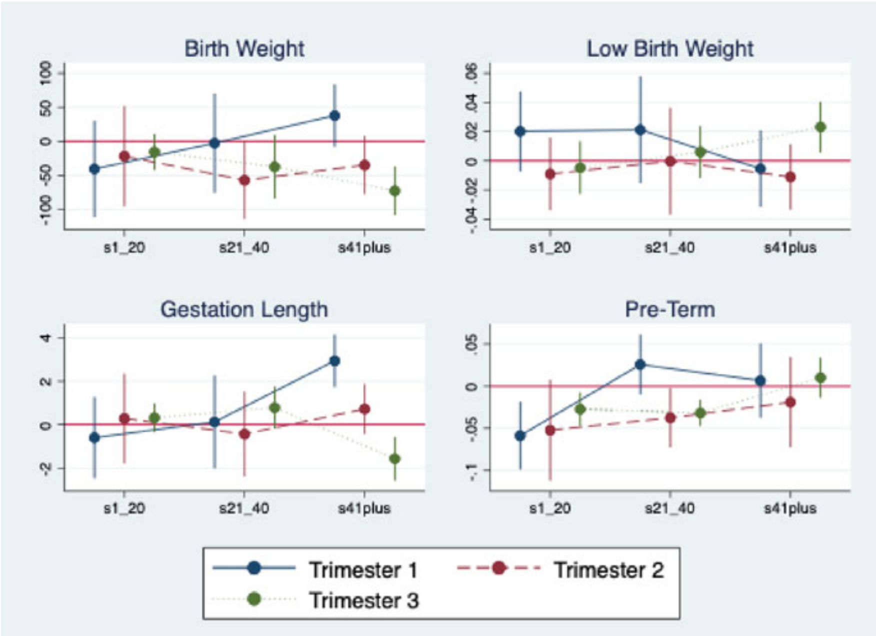

Figure 2. Non-parametric effect of sirens on birth outcomes.
Notes: The sample is limited to all Jewish births in Soroka Hospital conceived during 2011, 2013, and 2014 through June with gestation length of at least 180 days and residents of 85 localities in southern Israel. See section 3 for information on the sample. Each figure plots the coefficient estimates $\gamma_1^t - \gamma_3^t$ for t∊ {1, 2, 3} from a equation (3). Each dot represents the coefficient estimate for the indicator variable on the horizontal axis and the vertical lines branching from the dot are the 95% confidence intervals. The various trimester dots for each of the siren count variables are supposed to be vertically aligned but are not for presentation purposes. Birth-specific control variables are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Standard errors are clustered at the locality level.

than her child's birth date. This addresses the potential endogeneity of the birthdate, given that our variables of interest affect gestational length. Our sample is limited to births with a gestational length of at least 180 days. This results in a small amount of mothers—those who gave birth substantially earlier than their due date—having a large amount of sirens assigned to them despite not experiencing these sirens during their pregnancy. This distortion would primarily be relevant to the number of sirens assigned during the third trimester.

To test whether assignment of sirens based on the mother's due date affected our results in any way, we present regression results in Table 7 for the same specification as in Table 3, only the minimal gestation length threshold varies. The advantage of this more strict sample criterion is that the number of sirens more precisely captures the actual number of sirens mothers experienced in their locality during their third trimester. The disadvantage of this sample restriction is reduced variation in our outcomes of interest. Despite this disadvantage, the results in Table 7 still show statistically significant benefits in response to sirens among mothers ranked high

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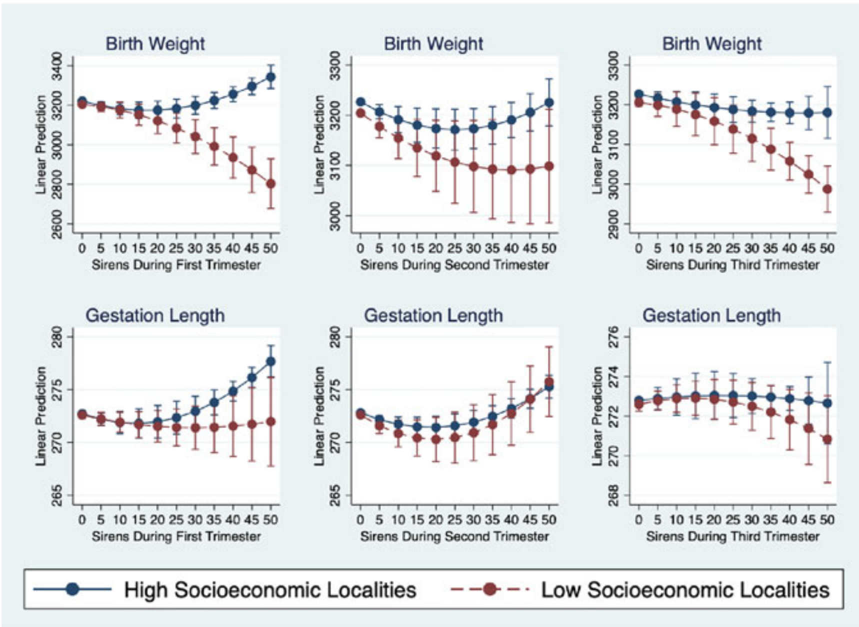
(a) Birth Weight and Gestation Length



(b) Low Birth Weight and PreTerm

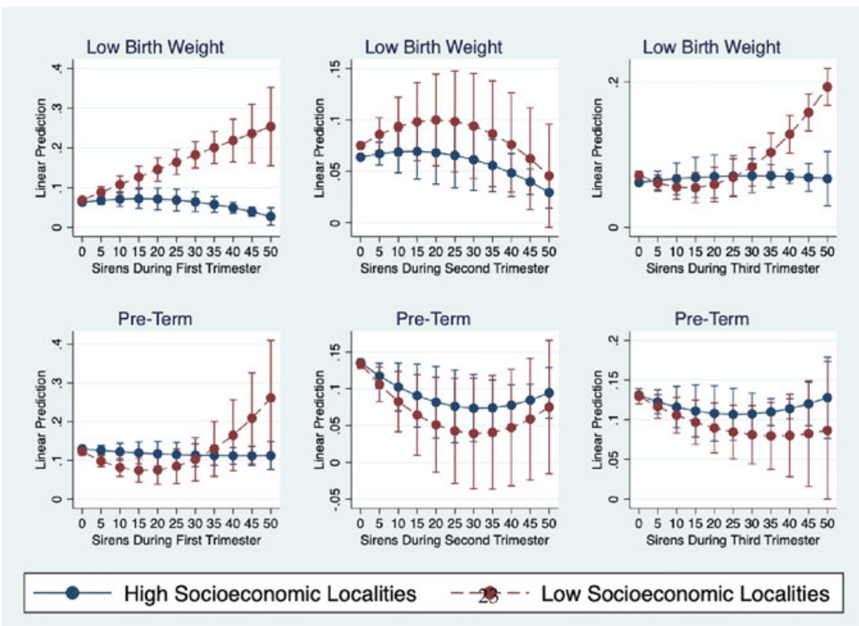


Figure 3. Sirens and birth outcomes—non-linear specification differentiated by socioeconomic status. (a) Birth weight and gestation length. (b) Low birth weight and preterm.

Notes: The figures plot the predictive values and the 95% confidence intervals from results for regression specifications that are similar to equation (2) only the siren variables are quadratic. Siren values are listed on the horizontal axis during the first, second, and third trimester of pregnancy in the left, middle, and right columns, respectively

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Table 7. Operation protective edge and birth outcomes—different gestational length thresholds for sample

Minimal gest. length threshold	180 Days (baseline)	200 Days	220 Days	240 Days	180 Days (baseline)	200 Days	220 Days	240 Days
	Birth weight (grams)				Gestation length (days)			
Sirens—trimester 1—low socio	−5.292*** (0.959)	−3.484** (1.706)	−4.635*** (1.741)	−3.173** (1.541)	−0.031 (0.027)	0.083* (0.042)	0.043 (0.046)	0.046 (0.045)
Sirens—trimester 2—low socio	−3.388** (1.557)	−0.530 (1.027)	−0.649 (0.975)	−0.391 (1.091)	−0.036 (0.041)	0.031 (0.021)	0.024 (0.022)	0.016 (0.023)
Sirens—trimester 3—low socio	−3.046*** (0.762)	−1.138 (0.787)	−0.385 (0.750)	−0.871 (0.653)	−0.005 (0.017)	0.017 (0.019)	0.040*** (0.015)	0.033*** (0.011)
Sirens—trimester 1—high socio	1.244** (0.526)	2.759** (1.088)	2.353** (1.136)	2.065* (1.152)	0.063*** (0.013)	0.093*** (0.013)	0.077*** (0.016)	0.072*** (0.017)
Sirens—trimester 2—high socio	−0.474 (0.446)	0.264 (0.643)	0.107 (0.704)	0.009 (0.726)	0.033*** (0.012)	0.035*** (0.013)	0.025 (0.015)	0.022 (0.016)
Sirens—trimester 3—high socio	−1.083** (0.479)	−0.750 (0.574)	−0.543 (0.546)	−0.650 (0.518)	0.000 (0.011)	0.009 (0.010)	0.014 (0.009)	0.010 (0.008)
Mean of dependent variable	3,215	3,219	3,225	3,241	272.7	272.8	273.1	273.6

	Low birth weight (indicator)				Pre-term delivery (indicator)			
Sirens—trimester 1—low socio	0.004*** (0.001)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	−0.000 (0.001)	0.004** (0.002)	0.004** (0.002)	0.005*** (0.002)
Sirens—trimester 2—low socio	0.001 (0.001)	−0.001** (0.000)	−0.001** (0.000)	−0.001 (0.000)	−0.003** (0.001)	−0.000 (0.001)	−0.000 (0.001)	0.000 (0.001)
Sirens—trimester 3—low socio	0.000 (0.001)	0.000 (0.001)	−0.000 (0.001)	−0.000 (0.001)	−0.002*** (0.000)	−0.001** (0.001)	−0.002*** (0.001)	−0.001** (0.001)
Sirens—trimester 1—high socio	−0.000* (0.000)	−0.001*** (0.000)	−0.001*** (0.000)	−0.001** (0.000)	−0.000 (0.000)	0.001 (0.001)	0.001 (0.001)	0.001* (0.001)
Sirens—trimester 2—high socio	−0.001*** (0.000)	−0.001*** (0.000)	−0.001*** (0.000)	−0.001** (0.000)	−0.001*** (0.000)	−0.001 (0.001)	−0.000 (0.001)	−0.000 (0.001)
Sirens—trimester 3—high socio	0.000 (0.000)	0.000 (0.000)	−0.000 (0.000)	0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
Mean of dependent variable	0.0648	0.0625	0.0593	0.0480	0.128	0.126	0.123	0.111
Observations	13,467	13,397	13,340	13,169	13,467	13,397	13,340	13,169
Conception month fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Birth-specific controls	✓	✓	✓	✓	✓	✓	✓	✓
Locality-conception year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The number of localities is 83–85. See section 3 for information on the sample. The coefficient estimates presented are $\beta_1 - \beta_6$ in equation (2). The first row indicates the minimal gestational length threshold for the sample in the regression. Birth-specific control variables are: mother's age (categorical), child sex, maternal abortion history, birth parity (categorical), high fertility, past cesarean delivery, late pregnancy loss in the past, and infertility treatment. Mean of dependent variable is the mean for non-OPE births in the sample. Standard errors are clustered at the locality level.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

socioeconomically and adverse effects for mothers ranked low socioeconomically in response to sirens during the first trimester.

7. Concluding remarks

As the economics literature has progressed from demonstrating the importance of birth weight for long-term adult outcomes [Behrman and Rosenzweig (2004); Black *et al.* (2007)] to establishing the central role intrauterine shocks play in determining birth weight and other birth outcomes [Almond and Currie (2011)], an abundant research has emerged that evaluates various potential intrauterine shocks and their effect on birth outcomes. When possible, these studies also evaluate the effect of these shocks on long-term adult outcomes or even offspring outcomes. This study evaluates one such shock—Operation Protective Edge in Israel—which resulted in the civilian population experiencing numerous daily siren warnings of rocket attacks over a two-month period.

Our results show that the effect of a conflict on birth outcomes can be both positive and negative, depending on behavioral responses to this shock, in particularly in terms of prenatal care. Furthermore, prenatal care responses—at least in our setting—vary qualitatively based on maternal socioeconomic status. Mothers ranked high socioeconomically likely have the resources to increase pro-health behavior during pregnancy in response to OPE, while mothers ranked low socioeconomically do not appear to have this option and even respond behaviorally in ways that are detrimental to their pregnancies. These differential responses produce qualitatively different birth outcome effects, thus demonstrating the potential for conflict—and possibly other negative shocks that can involve intrauterine exposure—to aggravate further socioeconomically-based inequality.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/dem.2022.18.

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