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Abstract: Focusing on hazardous chemical cleanups under the US Resource Conservation and Recovery Act (RCRA), we employ a reverse difference-in-differences design to estimate the effects of cleanup on birth outcomes. Data on the population of births in North Carolina from 1990-2019 are linked to cleanups at contaminated sites across the state. We find robust evidence that for children born to mothers residing within 250 meters, cleanup leads to an almost one week increase in gestational age, and a 6 to 8 percentage point reduction in the risk of preterm birth. Cleanup may also lead to improvements in birthweight, but these results are not statistically significant across all models. Assessments of the post-treatment trends and demographic sorting support a causal interpretation of the results. We illustrate how these quantified improvements in newborn health can be monetized to inform local land use and cleanup decisions, as well as future regulations under RCRA.

JEL Codes: D62, I18, Q53

Keywords: birth, children's health, cleanup, exposure, hazardous, health, RCRA

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I. INTRODUCTION

The Resource Conservation and Recovery Act (RCRA) requires facilities to investigate and clean up releases of hazardous chemicals. Such activities are collectively referred to as Corrective Actions. Releases of hazardous chemicals can potentially yield extensive adverse effects in the United States, with over 31 million people (9% of the US population) living within one mile of a RCRA Corrective Action (CA) site (EPA, 2023a). Despite RCRA being a cornerstone of US environmental policy for nearly 50 years, benefit-cost analyses for most US Environmental Protection Agency (EPA) regulations under the authority of RCRA do not quantify the benefits to residents living near these facilities (Guignet and Nolte, 2024).

A critical step in the monetization of benefits is to first quantify the effects, and that is the objective of our study. Focusing on RCRA facilities in North Carolina where a CA has occurred, we implement a reverse difference-in-differences (RDID) design (Kim and Lee 2019; von Hinke and Sørensen 2023), where the treated and control groups are *different* prior to treatment, but then become similar after treatment. Our approach is coupled with multivariate regression modelling and exact covariate matching, to estimate the effects of hazardous chemical cleanups on birth outcomes. We circumvent recent criticisms against conventional difference-in-differences (DID) models in the face of staggered treatment events by utilizing a natural control group specific to each site and treatment (i.e., cleanup) – children born to mothers who live in the same neighborhood and around the same CA sites, but who live far enough away from the site so that they are not exposed to the released chemicals, nor affected by the subsequent cleanup. The appropriate distance threshold between the treated and control groups is determined based on econometric examination of the conditional pre- and post-treatment distance gradients (Linden and Rockoff 2008; Muehlenbachs et al. 2015; Haninger et al. 2017; Guignet et al. 2023b). The presence of a natural control group corresponding to *each* site and treatment event allows for a naturally stacked DID approach. Stacked DID designs have been touted as one approach to circumvent the "negative weighting" concerns associated with staggered treatment events and conventional DID estimation via two-way fixed effects models (Goodman-Bacon 2021, Roth et al. 2023).

We set out to answer two main questions. First, do cleanups and exposure mitigating activities associated with RCRA Corrective Actions lead to improvements in birth outcomes for children whose mothers reside near the sites? Second, what is the spatial extent of any such health improvements?

We find that cleanup leads to localized improvements in newborn health, extending only to children born to mothers who lived within 250 meters of a CA site. The strongest evidence is in terms of gestational age and preterm birth, suggesting that cleanup leads to a 0.8 to 0.9 week increase in gestation, and a 6 to 8 percentage point decrease in the risk of preterm birth. Supplemental diagnostics confirm that post-cleanup sorting of different demographic groups is likely not driving the results, and that the evidence is overall consistent with a causal interpretation. We also find consistent (but sometimes statistically insignificant) evidence regarding increases in birthweight and reductions in the risk of low and very low birthweight. Such improvements in birth outcomes can yield benefits in terms of longer-term health and increased human capital

(Currie 2011). Both increases in gestational age (Crump et al. 2011; Boyle et al. 2012) and birthweight (Black et al., 2007; Belbasis et al. 2016; Xie et al. 2017; Baguet and Dumas 2019; Ludvigsson et al. 2018; WHO 2022) are associated with later-in-life improvements in health, education, and labor outcomes. Given the extremely localized nature of the estimated health effects and minimal use of local groundwater around our study sites, we conjecture that re-suspension of contaminated particles into the air, and mothers' subsequent inhalation, ingestion, and/or dermal contact with these particles is a plausible exposure pathway. Vapor intrusion into homes and subsequent inhalation of hazardous vapors is also possible.

This study offers several policy-relevant contributions to the literature. First, we add to a growing quasi-experimental literature that quantifies the effects of exposure to hazardous chemicals on children's health (e.g., Currie et al., 2011, 2015; Rau et al., 2015; Klemick et al. 2020; Bui et al., 2022). Second, our quantified estimates of improvements in newborn health pave the way for monetization of this critical endpoint. A review of economic analyses for recent EPA regulations under the authority of RCRA finds that, in most cases, the only monetized benefits were avoided cleanup costs experienced by the regulated facilities, and/or cost-savings to regulators. Benefits accruing to residents living near hazardous chemical facilities are often discussed only qualitatively (Guignet and Nolte 2024). Third, our study provides two methodological contributions. To our knowledge, it is one of only a few applications demonstrating the RDID design (Kim and Lee 2019; von Hinke and Sørensen 2023). Additionally, we illustrate how commonly applied spatial DID designs, where there is a natural control group corresponding to each treatment event, allow researchers to circumvent potential biases when estimating the effects of treatment events that are staggered over time (Goodman-Bacon 2021, Roth et al. 2023).

II. METHODOLOGY

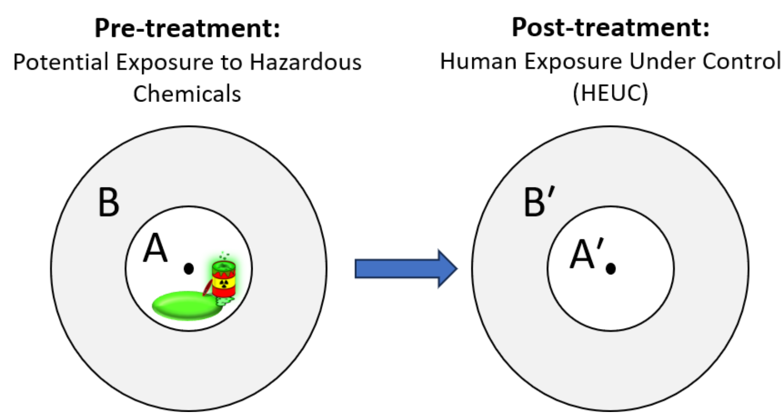
II.A. Reverse Difference-in-differences (RDID)

Difference-in-differences (DID) has emerged as one of the most prominent methodologies for causal inference in the social sciences (Roth et al. 2023). Recently formalized by Kim and Lee (2019), a much less studied variant of the DID approach is the reverse difference-in-differences (RDID) design. In a conventional DID setting, the treated and control groups are as similar as possible before the treatment event, but then post-treatment the two groups become different due to the treatment. In a RDID setting, however, the treated and control groups are *different* prior to treatment, but then become similar after treatment. To our knowledge there are only two published applications of the RDID approach. Kim and Lee (2019) assess the impact of work-hour limits on work hours and wages in South Korea. Initially work-hour limits were only in place for a subset of firms. This control group was compared to a "treated" group of firms that did not initially face work-hour limits, but where such limits were put in place one year later. von Hinke and Sørensen (2023) used the RDID approach to examine how exposure *in utero* and in infancy to an extreme air pollution event – the 1952 London smog – affected cognitive ability and respiratory health later in life. Their identification strategy compared those exposed to the London smog while *in utero* and infancy to those born in the same areas, but after the smog event. This difference was in turn compared to a control group of individuals born during the same time period, but in areas unaffected by the smog.

Similar to von Hinke and Sørensen (2023), in our current context we have a treated group of exposed individuals – i.e., children born to mothers living near a chemical facility and who are thus potentially exposed to hazardous chemicals. This set of individuals is denoted as group A in Figure 1. We have a control group of children born to mothers living in the same neighborhood, and around the same chemical facility, but who live far enough away so that they are not exposed to hazardous chemicals (denoted as group B). These two groups are different prior to treatment – one is potentially exposed to hazardous chemicals, and the other is not.

We define treatment as the completion of cleanup and exposure mitigation activities. More formally, we define treatment as a Corrective Action (CA) site receiving a Human Exposure Under Control (HEUC) determination by regulators.[1] The HEUC determination is a formal milestone of the RCRA Corrective Action (or cleanup) process, and it is assigned when the responsible party sufficiently demonstrates that there is no longer a risk of human exposure. After the HEUC, exposure to hazardous chemicals is eliminated and the treated and control groups are now hypothesized to be similar; as depicted by groups A' and B' in Figure 1.

Figure 1. Depiction of the Reverse Difference-in-differences (RDID) Design.



The identification strategy outlined in Figure 1 is similar to a typical DID setting. We are estimating the change or difference in average outcomes among the treated group, before and after treatment $(A' - A)$. This first difference could also capture other trends that coincide with the HEUC event, and so a second differencing subtracts out the average change over the same time

[1] In North Carolina, the Department of Environmental Quality (NC DEQ) operates the CA program and makes such determinations. Although the US EPA provides oversight and ensures that federal standards are met, 44 states are authorized to operate their CA programs (EPA 2025).

period for children born to mothers in the same neighborhood and around the same chemical facility, but who are far enough away that they are not exposed to hazardous chemicals $(B' - B)$. As in a typical DID framework, our RDID estimator can be summarized as $(A' - A) - (B' - B)$. The difference between the DID and RDID approaches primarily comes into play when we assess parallel trends and the plausibility for causal inference, which we return to in Section IV.C.

II.B. Empirical Model

We implement the RDID approach within a regression model framework, as shown by equation (1). The birth outcomes of interest are denoted as Y_{ijt}. In our primary models, the outcomes of interest are continuous measures of gestational age (measured in weeks) and birthweight (measured in grams). To examine the robustness of our results we also estimate linear probability models, where Y_{ijt} represents binary indicators denoting preterm birth (PTB), low birth weight (LBW), and very low birthweight (VLBW).[2]

The unit of observation is the birth of child *i* to a mother living near chemical facility *j*, and who was conceived in period *t*. We control for $\boldsymbol{x}_{ijt}$, which is a vector of individual-level characteristics of the child (race or ethnicity, sex at birth, and the month of conception), and of the parents (education, mother's smoking and marital status, mother's age, birth order, and proxies for income, namely participation in Medicaid and in the US Department of Agriculture's Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)).

$$(1) \qquad Y_{ijt} = \boldsymbol{x}_{ijt}\boldsymbol{\beta} + \alpha_j HEUC_{jt} + \lambda_j \mathbb{1}\left(d_{ij} \leq D\right) + \gamma\left\{\mathbb{1}\left(d_{ij} \leq D\right) \times HEUC_{jt}\right\} + \phi_{jt} + \varepsilon_{ijt}$$

The "treatment" event of interest is verification by regulators that human exposure is under control $\left(HEUC_{jt}\right)$ at chemical facility *j*. This treatment event indicator equals one if the child was conceived after the HEUC determination, and is zero otherwise. We also include a treated group indicator $\mathbb{1}\left(d_{ij} \leq D\right)$, which equals one when child *i* is born to a mother who lives within a distance *D* from chemical facility *j*, and is zero otherwise. This indicator denotes the treated group, both pre- and post-treatment.

Critical to our identification strategy is the inclusion of site-by-conception year fixed effects (ϕ_{jt}). These fixed effects account for site-specific factors and temporal trends that could otherwise

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[2] For the binary outcomes, linear probability models are estimated instead of common nonlinear models (e.g., probit or logit). This is primarily due to our inclusion of high-dimensional fixed effects to control for spatially and temporally correlated confounders. These overlapping fixed effects cannot be simply differenced or conditioned out in nonlinear models, leading to an incidental parameters problem (Lancaster 2000; Wooldridge 2010, page 612). In such cases, the linear probability model provides a reasonable approximation when the objective is to estimate partial effects on the probability of the outcome of interest (Wooldridge 2010, pp. 563), as is the case here. Linear probability models also accommodate a mix of continuous and categorical variables and offer easily interpretable marginal effects (Angrist and Pischke 2009).

confound our results. The inclusion of ϕ_{jt} also allows us to circumvent criticisms of DID applications when the treatment events are staggered over time (Goodman-Bacon 2021, Roth et al. 2023), and therefore we do not need to implement models developed to address these criticisms (e.g., Callaway and Sant'Anna 2021, Wooldridge Forthcoming). In our setting, we do have staggered treatment events. We observe cleanup and HEUC determinations at different chemical facilities, and that occur at different points in time. In this and other staggered DID settings, analysts are effectively pooling numerous sub-experiments, and are estimating an overall average treatment effect on the treated (ATT) that is a weighted average of the ATTs across the sub-experiments. The crux of the staggered DID criticism is that the conventional two-way fixed effects (TWFE) model compares treated observations to not-yet- and never-treated observations, which act as control group observations. However, the TWFE model also implicitly makes "forbidden" comparisons to already-treated observations, which can lead to negative weighting of the ATTs from some sub-experiments. In cases where the ATT is varying over time this can bias the overall average ATT estimate of interest (Goodman-Bacon 2021, Roth et al. 2023). We cannot rule out the possibility of time varying treatment effects in our current setting. For example, residual parental health effects and within-body chemical burden could remain after the HEUC determination, but may continue to diminish over time.

In our current context, these "forbidden" comparisons would involve cross-site comparisons. However, our estimation approach avoids such concerns because each chemical facility includes a natural control group that is *specific* to the site and HEUC event – children born around the same CA site, but who were too far away to be exposed to the hazardous chemicals. These individuals "experience" a specific treatment event but are not exposed to the treatment. The typical TWFE model does not contain control observations that are linked to a specific sub-experiment, as we have here. The availability of site-specific control groups allows us to identify ϕ_{jt} in equation (1), and thus absorb any cross-site variation over time, including variation based on "forbidden" comparisons. Due to the inclusion of ϕ_{jt}, identification of the overall ATT in our analysis is based solely on within-site and year variation. When considering only within-site, the treatment is not staggered over time, and so there are no "forbidden" comparisons of treated observations to already-treated observations.[3]

Our empirical framework is more akin to a stacked DID design (Cengiz et al. 2019; Deshpande and Li 2019; Fadlon and Nielsen, 2021), where analysts construct a counterfactual group specific to each sub-experiment and then stack the datasets to estimate a pooled model. We refer to our setting as a naturally stacked DID design because we do not need to construct a counterfactual group for each sub-experiment, one is inherently present by our comparison of children living nearest to the CA sites to those living around the same sites but farther away. The stacked DID method has been suggested as one approach to address concerns regarding staggered treatment

[3] This feature is not unique to our study, and in fact such spatial DID approaches have been widely used, particularly in the hedonic property value literature (e.g., Linden and Rockoff 2008, Muehlenbachs et al. 2015, Haninger et al. 2017, Guignet et al. 2023a, 2023b, Guignet and Nolte 2024, Cassidy et al. 2024). Basu et al. (2025) recently applied this same identification strategy in their analysis of residential sorting and pollution exposure among older adults.

events (Goodman-Bacon 2021, Roth et al. 2023, Basu et al. 2025). To further align with the stacked DID framework, in our most flexible models we interact the post-treatment indicator $HEUC_{jt}$ and treated zone indicator $\mathbb{1}(d_{ij} \leq D)$ with site-specific indicators (hence the j subscript on the α_j and λ_j coefficients in equation (1)). In theory, we could estimate a separate regression model for each of the chemical facilities, but as demonstrated in Section III, the sample sizes (particularly for the treated group) are quite small around some of the sites. Instead, we pool the data across sites and estimate the average effect of cleanups on birth outcomes within the same regression model. Doing so provides more power to statistically estimate the average effect of the HEUC events.

Of primary interest in equation (1) is the interaction term between the treated group and the post-treatment event, $\mathbb{1}(d_{ij} \leq D) \times HEUC_{jt}$. The corresponding coefficient to be estimated, γ, is the weighted-average of the ATTs across the sites. All else constant, γ captures the average incremental effect of cleanup on gestational age or birthweight among children born to mothers who live near the chemical facilities. Our primary hypothesis is $\gamma > 0$, which would imply that cleanup and the subsequent HEUC determination increase gestational age and birthweight. In our linear probability models of PTB, LBW, and VLBW, the primary hypothesis is $\gamma < 0$, which would imply a reduction in the risk of these conditions.

The other parameters to be estimated include $\boldsymbol{\beta}$, α_j, λ_j, and ϕ_{jt}. The unobserved disturbance term ε_{ijt} is allowed to be correlated for children within the same neighborhood (i.e., within the same Census block group).

II.C. Pre-regression Matching

To further assess the robustness of our results, we also estimate a series of regression models using a matched sample. An exact covariate matching algorithm is used to create a more comparable set of treated and control units. Treated observations (births within close proximity to the chemical facility, i.e., $\mathbb{1}(d_{ij} \leq D) = 1$) and control observations (births farther away from the same chemical facility, i.e., $\mathbb{1}(d_{ij} \leq D) = 0$) are matched if they simultaneously fulfill all three of the following conditions: (i) they are nearest to the same chemical facility[4], (ii) they are conceived in the same year and month, and (iii) both are conceived either pre- or post-HEUC. The motivation of our matching algorithm is to provide a more balanced sample over time and across sites, and thus better control for any remaining unobserved confounders that may be correlated over space and time.[5]

[4] As discussed in Sections III and IV.A, in our application this means that the treated and control observation are both within 1,000 meters of the same chemical facility.

[5] We assessed the possibility of also matching based on sociodemographic characteristics (e.g., gender, race or ethnicity, parental education, and mother's age), but doing so even for one of these characteristics resulted in too few

The sample is pruned and re-weighted so that the distributions of the treated and control groups across these dimensions are the same. Each matched treated observation is given a weight of one if it is matched to at least one control observation. A control observation can be given a weight greater than one if it was matched to more than one treated observation, or could have a weight less than one if there are many similar control observations matched to the same treated observation. In essence, the matching algorithm constructs a counterfactual based on a weighted average of control observations. Treated and control observations that were not matched simultaneously based on these three dimensions are given a weight of zero and discarded from the matched sample. Although we are performing exact matching, we employ this matching procedure using a Coarsened Exact Matching (CEM) algorithm (Blackwell et a. 2009; Iacus et al. 2012). Regression models based on equation (1) are then estimated using the weighted sample, controlling for the same set of sociodemographic characteristics and other covariates.

III. DATA

III.A. Data Sources and Background

This research is conducted under an agreement with the Children's Environmental Health Initiative (CEHI) at the University of Illinois-Chicago and a protocol approved by the University of Illinois-Chicago Institutional Review Board. The data are from the Vital Statistics Department of North Carolina State Center for Health Statistics (NCSCHS), and are subsequently compiled and maintained by CEHI. The data include individual-level observations for all live births in NC from 1990-2019 and contain information on the date of birth, birthweight, gestational age, race and ethnicity, and parental characteristics. We use the mother's place of residence and baby's date of conception to spatially and temporally link each individual birth record to chemical facilities regulated under RCRA and the cleanup activities at these sites. We assume that a mother lives at the same address throughout the pregnancy, but only observe the mother's place of residence at the time of the child's birth.

Data on all 2,447 RCRA facilities in NC were obtained from RCRAInfo, EPA's comprehensive database of facilities handling hazardous chemicals. Geographic coordinates of the RCRA facilities come from EPA's Facility Registry Service. We first draw focus to the 34 RCRA facilities in North Carolina where a CA investigation was opened, and where that investigation identified a contamination release severe enough to require intervention to protect human health.[6]

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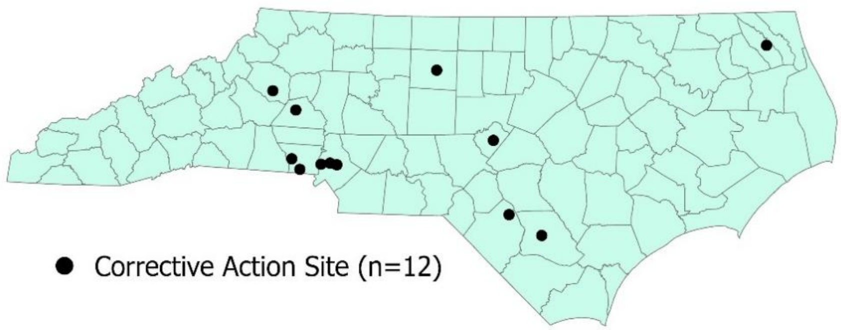
matches and an estimating sample that was too small for statistical analysis. Nonetheless, we control for these key sociodemographic characteristics by including them in $\boldsymbol{x}_{ijt}$ when estimating equation (1).

[6] We define such sites as those where active remediation technologies, physical controls, and/or institutional controls were deemed necessary. Such cases are identified in the RCRAInfo database based on the following event codes: CA550RC (remedy construction); CA770GW and CA770NG (groundwater and nongroundwater controls); and CA772EP, CA772GC, CA772ID, and CA772PR (institutional controls).

Diagnostic analysis suggests that any effects on newborn health are extremely localized, and thus the treated group zone extends only 250 meters from a site, on average (see Section IV.A for details). To facilitate a clean quasi-experiment and thus more valid quantification of any health improvements associated with cleanup, we further draw focus to the 12 CA sites where births were observed within the treated group zone (i.e., 0-250 meters) both before and after the HEUC determination. The location of the 12 CA sites analyzed are shown in Figure 2.

Figure 2. Map of 12 Corrective Action Sites in North Carolina.



Review of the case files from the North Carolina Department of Environmental Quality (NCDEQ) confirm that in all 12 cases there was a chemical spill or release at the facility, and that physical remediation of the pollutants was undertaken. Remediation methods included excavation of contaminated soil, soil aeration and vapor extraction, groundwater treatment, and the installation of physical barriers to minimize the migration of contaminants. Some barriers contained reactive materials to chemically neutralize pollutants. Active monitoring of soil and groundwater and some element of natural attenuation were included in the remediation activities at all sites.

According to the available North American Industry Classification System (NAICS) codes in RCRAInfo, most of the 12 facilities are involved in manufacturing activities (8 facilities), followed by transportation (6), waste management (5), general services (3), and construction (1). Half of the sites are recorded as having more than one NAICS categorization. Based on the individual case files from the NCDEQ, the most common contaminants released included heavy metals (e.g., lead, nickel, chromium, cadmium), volatile organic compounds (e.g., benzene, toluene, and ethylbenzene), and other toxic chemicals such as arsenic and sulfuric acid.[7]

When routine monitoring of soil, groundwater, and (when applicable) surface water consistently suggest that migration of pollutants to human and environmental endpoints is no longer a concern, then the regulators make a HEUC determination. Testing and related cleanup activities may continue after the HEUC determination, but after this event the site is generally deemed safe to surrounding populations.

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[7] The NCDEQ case files can be accessed at https://edocs.deq.nc.gov/WasteManagement/Browse.aspx.

In an ideal quasi-experiment, the HEUC event would denote a discrete decrease in contamination and human exposure, however that is not necessarily the situation. In most (if not all) cases, cleanup entails the use of controls that substantially reduce contamination prior to the HEUC event, and the reduction in chemical exposures could occur gradually. For example, remediation technologies like pumping and treating groundwater, vapor extraction, and chemical neutralization lead to gradual reductions in pollution. In some cases, continued monitoring after initial cleanup activities may reveal additional contamination, and as a result additional remediation activities may then be implemented. Such sequencing of cleanup activities can occur over many years. Among the 12 chemical facilities in our study, the cleanup process took 6 to 15 years from the opening of the CA to the time the facility received a HEUC determination. This makes it less than ideal for a DID approach because children in our treated group may have experienced some of the health improvements from cleanup during the pre-treatment period. In this situation, our RDID design may underestimate the health improvements.

Nonetheless, the spatiotemporal variation across the 12 CA sites and HEUC determinations lend support to our quasi-experimental design. These facilities are located across the State (see Figure 2), and the HEUC events occur at different times during our 1990-2019 study period, with the first HEUC determination being made in 1998, and the last occurring in 2014 (see Figure A1 in the Appendix). Residual confounding factors that are specific to a particular site or year are minimized by analyzing numerous sites and HEUC events.

III.B. Summary Statistics

Our RDID analysis focuses on the n=8,178 live births from 1990-2019 in North Carolina, where the mother lived within 1,000 meters of one of the twelve CA facilities analyzed.[8] Summary statistics are provided in Table 1. The average newborn weighs 3,126 grams at birth, and was *in utero* for 38.4 weeks. About 12.7% and 2.5% of babies are designated as low birth weight (LBW) or very low birth weight (VLBW), meaning that they were below 2500 or 1500 grams at birth, respectively. About 12.5% of the newborns are designated as a preterm birth (PTB), meaning that they were *in utero* for less than 37 weeks. Just over half of the children were male (51%). Most children were a singleton birth, with only 3% of our sample corresponding to a plural birth (twins or triplets). About 24% of the newborns in our sample did not have a race or ethnicity listed, but among those who did, about 25% were White, 40% Black, 14% Hispanic, and 20% were noted as another race or ethnicity. The relatively small percent of children who are White in this sample of newborns living within 1,000 meters of a chemical facility, compared to the 49% White among

[8] Although our dataset started with the population of births in North Carolina during this period, we restrict attention to this subset for the main analysis (see Section IV.A for details).

the broader population of live births in NC during this period, highlights the potential disparities in terms of where these chemical facilities are located.[9]

Table 1. Descriptive Statistics.

Variable	Obs	Mean	Std. dev.	Min	Max
Birthweight (grams)	8,173	3126	648	0	5982
Gestational Age (weeks)	8,178	38.42	2.73	15	47
Low Birth Weight	8,173	0.127	0.333	0	1
Very Low Birthweight	8,173	0.025	0.155	0	1
Preterm Birth	8,178	0.125	0.331	0	1
White	6,202	0.251	0.433	0	1
Black	6,202	0.409	0.492	0	1
Hispanic	6,202	0.136	0.343	0	1
Other race/ethnicity	6,202	0.204	0.403	0	1
Missing: Race/Ethnicity	8,178	0.242	0.428	0	1
Male	8,178	0.511	0.500	0	1
Plural birth	8,178	0.031	0.174	0	1
Parents no college	7,858	0.887	0.317	0	1
Missing: Parents no college	8,178	0.039	0.194	0	1
Mom 15-24 years	8,117	0.519	0.500	0	1
Mom 35-44 years	8,117	0.080	0.271	0	1
Missing: Mom age	8,178	0.007	0.086	0	1
Smoked	7,875	0.157	0.364	0	1
Missing: Smoked	8,178	0.037	0.189	0	1
Second birth	8,178	0.300	0.458	0	1
Third birth	8,178	0.179	0.383	0	1
Fourth birth	8,178	0.082	0.275	0	1
≥ Fifth birth	8,178	0.055	0.228	0	1
Not married	8,177	0.635	0.481	0	1
Missing: Not married	8,178	0.000	0.011	0	1
WIC	2,481	0.602	0.490	0	1
Missing: WIC	8,178	0.697	0.460	0	1
Medicaid	2,479	0.641	0.480	0	1
Missing: Medicaid	8,178	0.697	0.460	0	1

Note: All variables are binary indicators, unless otherwise noted in parentheses.

When data on parental education is available, we see about 89% of the children near these facilities were born to parents with no college education. About 52% of the mothers were between 15-24 years of age at the time of the child's birth, followed by mothers between 25-34 years (the omitted

[9] See Brodin and Guignet (2024) for an in-depth, nationwide distributional analysis of the RCRA Corrective Action program.

category), and then mothers between 35-44 years. Just under 16% of the mothers reported smoking during pregnancy, and 63% of mothers were not married at the time of the baby's birth. Although we do not directly observe income in the birth records data, for births in 2011 and after, we have proxy information based on whether the mother participated in the USDA's nutritional supplement WIC program, and was enrolled in Medicaid, both of which indicate low income. Among the observations where these data are available, we see participation rates of 60% and 64%, respectively.

IV. RESULTS

IV.A. Determining the Spatial Extent of Newborn Health Effects

To determine the spatial cutoff between the treated and control groups we adopt a procedure often used to examine the effects of local disamenities on house prices. It was first introduced by Linden and Rockoff (2008), and subsequently refined by Muehlenbachs et al. (2015), Haninger et al. (2017), Guignet and Nolte (2024), and others. A regression model similar to equation (1) is estimated, which allows us to estimate the pre- and post-HEUC gradients with respect to distance to the site, conditional on all observed characteristics. In theory, the conditional distance gradients will be different closer to the site, but then converge at some distance *D*. This distance *D* is the assumed cutoff between the treated and control groups. The regression model to be estimated is:

$$(2) \qquad Y_{ijt} = \boldsymbol{x}_{ijt}\boldsymbol{\beta}_1 + \boldsymbol{PreHEUC}_{it}\boldsymbol{\theta}_{pre} + \boldsymbol{PostHEUC}_{it}\boldsymbol{\theta}_{post} + \tau_t + \upsilon_j + \varepsilon_{ijt}$$

where $\boldsymbol{PreHEUC}_{it}$ is a vector of indicator variables denoting whether the mother of child i lived in different distance bins from the nearest chemical facility *j*, and whether time *t* (when the child was conceived) was before the HEUC determination for facility *j*. The distance bins are measured in 250-meter increments starting with 0-250m, 250-500m, and so on. The farthest distance bin is omitted for identification. Similarly, $\boldsymbol{PostHEUC}_{it}$ is a vector of indicators denoting proximity to chemical facility *j* but indicates whether child *i* was conceived after the HEUC event. The vectors to be estimated, $\boldsymbol{\theta}_{pre}$ and $\boldsymbol{\theta}_{post}$, capture the conditional pre- and post-treatment distance gradients in a flexible fashion. In this diagnostic exercise, we include separate site and conception year fixed effects (υ_j and τ_t, respectively), rather than site-by-year fixed effects (ϕ_{jt}), as done in the main regression model shown in equation (1).

We estimate equation (2) using a broader sample of children born to mothers living within 5 km of one of the original 34 CA sites in North Carolina.[10] Estimates of $\boldsymbol{\theta}_{pre}$ and $\boldsymbol{\theta}_{post}$ are presented in Figure 3, thus showing the pre- and post-HEUC conditional distance gradients for gestational age and birthweight (Panels (a) and (b), respectively). Both graphs suggest that prior to the HEUC

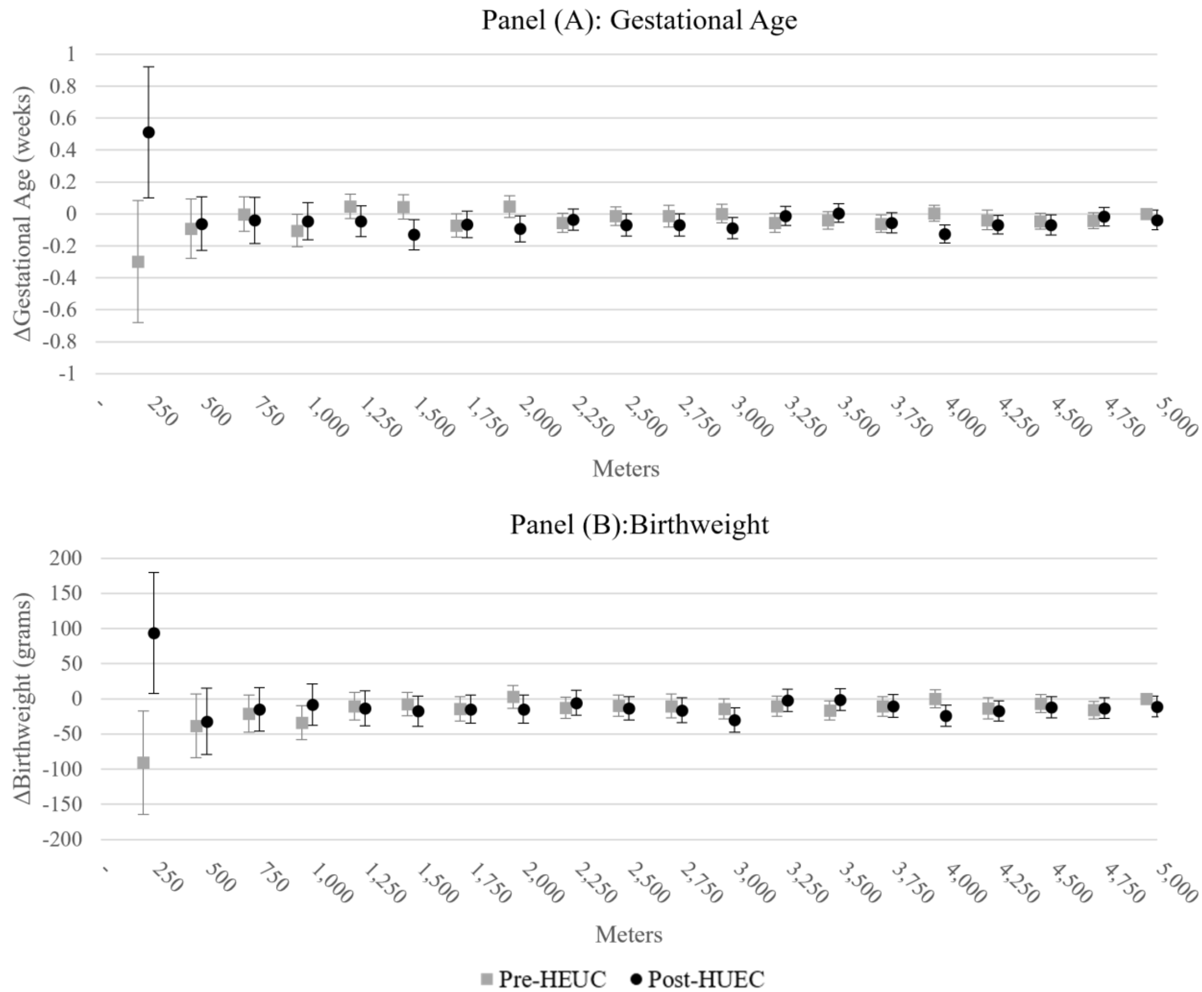
[10] Earlier analysis included births out to 10 km and produced the same result in terms of the estimated spatial extent of the effects of cleanup on infant health.

determination, children born to mothers living nearest the chemical sites (i.e., within 250 meters) experienced a lower birthweight and gestational age, on average, but this negative association dissipates farther from the CA sites, moving towards zero in the subsequent distance bins. The post-HEUC gradients suggest that, on average, newborns nearest the site experience a greater gestation period and birthweight. The post-HEUC gradient converges towards the pre-HEUC gradient beyond 250 meters.

Wald tests confirm that the pre- and post-HEUC distance gradients corresponding to the 0-250 m distance interval are statistically different (p = 0.001 for both the gestational age and birthweight estimates). Therefore, we define the treated group as children born to mothers who lived within 0-250 of a CA site. We assume a control group of infants born to mothers who lived between 250-1000 meters from a site. These definitions were informed by the results in Figure 3, as well as consideration of the tradeoffs between a larger sample size when extending the outer boundary of the control group, versus the possibility of introducing additional spatially correlated confounders. For the control group distance bins (250-500; 500-750; and 750-1,000 meters), we fail to reject the null hypotheses that the pre- and post-HEUC estimates are statistically equivalent, supporting the assumption that newborns in the broader 250-1,000 meter zone serve as a reasonable control group. We arrive at the same conclusion when re-estimating equation (2) with binary health outcome variables (see Figure A2 in the Appendix).

Given that any potential improvements in birth outcomes are very local in nature, the sample size, particularly of the treated group, is relatively small. Focusing on the 12 (out of the original 34) CA sites where there are observed births within the 0-250 meter treated zone, both before and after the HEUC event, we see a total of just 344 observations (225 births pre-HEUC and 119 post-HEUC). The number of control group births within 250-1000 meters of these same 12 sites is 7,834 (with 5,394 and 2,440, before and after the HEUC determination, respectively). When using our matched sample to better balance our treated and control groups across sites and over time, the sample size is reduced even further, resulting in just 66 treated and 6,804 control observations. The small number of identifying observations in our analysis is an important caveat to keep in mind when interpreting the results.

Figure 3. Conditional Pre- and Post-HEUC Distance Gradients.



Note: Figure displays estimates of $\boldsymbol{\theta}_{pre}$ and $\boldsymbol{\theta}_{post}$ from regression models of equation (2). The vertical lines denote the 95% confidence interval around each estimate.

IV.B. RDID Regression Results

Using the sample of births within 1,000 m of one of the 12 CA sites, we first estimate variants of equation (1) where gestational age (measured in weeks) is the dependent variable. ATT estimates (i.e., γ in equation (1)) for the models of gestational age are presented in Table 2. The first model includes the full suite of covariates, conception month and year fixed effects, and separate time-invariant site fixed effects. The coefficient corresponding to *0-250m* × *post-HEUC* is positive and statistically significant, suggesting that children living within 0-250m of a site experienced an average increase in gestation of about 0.89 weeks if they were conceived after cleanup and when human exposure is determined to be under control at the chemical facility. Considering that the average gestation period in our sample is 38.4 weeks, this corresponds to a 2.3% increase in the time a child has to develop *in utero*.

Model 2 in Table 2 includes the site-by-year fixed effects, which as discussed in Section II.B are important for our identification strategy and ability to circumvent criticisms regarding the staggered treatment events over time. Model 2 suggests a similarly sized and statistically significant 0.76 week increase in gestational age after the HEUC determination. Model 3 introduces additional interaction terms to allow the post-HEUC and 0-250 meter treated zone associations to vary across the 12 CA sites. Model 3 yields a similar result, suggesting 0.91 week increase. Although Model 3 is the most thorough in controlling for site-specific factors, we do have some concerns related to the small number of just 344 treated observations (only 119 of which occur post-HEUC). Dividing those identifying observations across the 12 sites when estimating site-specific interaction effects with the *0-250 meter* treated zone indicator and the *post-HEUC* indicator (in addition to the inclusion of the site-by-year fixed effects) results in a loss of statistical power. Nonetheless, the results from Model 3 are robust and of the greatest magnitude, at least in terms of gestational age. Model 4 is the same as Model 1, but utilizes the matched sample discussed in Section II.C. We employ a specification similar to Model 1 here because the matched sample is notably smaller than the full sample included in Models 1-3. The results from Model 4 are similar to the earlier models, suggesting an almost 0.80 week increase in gestational age after the HEUC determination.

The full results including all covariates are provided in Table A1 of the Appendix, and generally align with expectations. For example, a newborn who is Black or of another race/ethnicity tends to have a shorter gestation period relative to a White newborn, all else constant. Plural births, as well as children born to parents with no college education, or to a mother who reports smoking during pregnancy, also experience a shorter gestation period.

Table 2. RDID Gestational Age Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters × post-HEUC	0.8890***	0.7649***	0.9085***	0.7978**
	(0.2490)	(0.2591)	(0.3283)	(0.3775)
Additional covariates	X	X	X	X
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8178	8173	8173	1308
Adjusted R-squared	0.094	0.089	0.087	0.141

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is gestational age (in weeks). See Table A1 in the Appendix for the full results

We find similar results when estimating variants of these same four specifications, but where a binary indicator of PTB is the dependent variable. As shown in Table 3, Model 1 suggests a 6.0 percentage point decrease in the risk of PTB following cleanup. Models 2 through 4 suggest similar reductions in the risk of PTB, ranging from 5.9-7.8 percentage points.[11] Considering that the average risk of PTB in our sample is 12.5%, these results suggest that cleanup leads to a staggering 47-63% reduction in the risk of PTB.

The full results for the PTB models are provided in Table A2 in the Appendix. The estimated associations pertaining to the independent variables that are not of primary interest suggest a similar story as in the models of gestational age.

Table 3. RDID Preterm Birth (PTB) Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters × post-HEUC	-0.0599**	-0.0585**	-0.0697**	-0.0784*
	(0.0291)	(0.0287)	(0.0314)	(0.0412)
Additional covariates	X	X	X	X
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X

[11] Although we have concerns with nonlinear binary model specifications due to our inclusion of high-dimensional spatial and temporal fixed effects, we do re-estimate Models 1 through 3 for PTB using Chamberlain's (1980) Fixed Effect Logit model. Fixed Effect Logit variants of Model 4 could not be estimated because the matching weights were not constant across all observations pertaining to each site (i.e., were not the constant within the same fixed effect). The PTB results are consistent in sign, but the relationship between PTB and the HEUC determination is only significant for Model 1 (p = 0.081). The estimated relationship from Models 2 and 3 are marginally insignificant (p = 0.140 and p = 0.150, respectively). It is important to note that only one dimension of the high-dimensional fixed effects (in our case the site or site-by-year fixed effects, depending on the model) could be conditioned out using the Fixed Effect Logit specification. The other fixed effects were accounted for by including a series of indicator variables. As such, the statistically insignificant results could be at least partly driven by the incidental parameters problem (Lancaster 2000; Wooldridge 2010, page 612); hence our preference for the linear probability models in the main analysis.

Observations	8178	8173	8173	1308
Adjusted R-squared	0.086	0.080	0.079	0.141

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is a binary indicator, equal to one for children designated as a preterm birth (i.e., *in utero* for less than 37 weeks), and zero otherwise. See Table A2 in the Appendix for the full results.

We next turn to the models focused on measures of birthweight. Table 4 shows the results of the models of birthweight (measured in grams). The coefficient corresponding to *0-250m × post-HEUC* in Model 1 is positive and statistically significant, suggesting that children living within 0-250m of a site experienced an average gain in birthweight of 164 grams if they were conceived after cleanup and when human exposure was determined to be under control at the chemical facility. Considering that the average newborn in our sample weighed 3,126 grams at birth, this corresponds to a notable 5.3% increase in birthweight. Model 2 suggests a similarly sized and marginally significant 125 gram increase in birthweight after the HEUC determination. Models 3 and 4 suggest results that are similar in magnitude, but are statistically insignificant. For both Models 3 and 4, we have some concerns regarding the relatively small number of identifying observations and low statistical power, which may be at least partly driving the statistically insignificant results.

Table 4. RDID Birthweight Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters × post-HEUC	164.4512***	125.0005*	110.3970	117.4240
	(62.1264)	(65.6349)	(80.2677)	(88.8404)
Additional covariates	X	X	X	X
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8173	8168	8168	1307
Adjusted R-squared	0.144	0.142	0.141	0.203

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is birthweight (in grams). See Table A3 in the Appendix for the full results.

Estimates from the linear probability models of the risk of LBW and VLBW are consistent in sign, but the results are mixed in terms statistical significance. As shown in Table 5, Model 1 suggests a marginally significant 4.4 percentage point reduction in the risk of LBW. Models 2 through 4 suggest a reduction in risk that is similar in magnitude, but the estimates are statistically insignificant. When re-estimating these models with VLBW as the dependent variable (Table 6), we find evidence that cleanup and the HEUC event may lead to a 3-percentage point reduction in the risk of VLBW, but this result is statistically significant only in Models 1 and 2. The point estimates are similar in Models 3 and 4, but are statistically insignificant. Again, this statistically insignificant result may, at least partly, be driven by a lack of statistical power. Model 3 could be allowing for too many site-specific parameters given the small number of treated observations around each individual site, and Model 4 is estimated using the much smaller, matched sample.[12]

The full results for the birthweight, LBW, and VLBW models are provided in Tables A3, A4 and A5 in the Appendix. The estimated associations between the independent variables that are not of primary interest and each birth outcome suggest a similar story as in the earlier models. For example, birthweight tends to be lower for mothers who report smoking during pregnancy, and the risk of LBW and VLBW are higher.

Table 5. RDID Low Birthweight (LBW) Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters × post-HEUC	-0.0444*	-0.0326	-0.0308	-0.0425
	(0.0265)	(0.0257)	(0.0328)	(0.0428)
Additional covariates	X	X	X	X
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8173	8168	8168	1307
Adjusted R-squared	0.109	0.104	0.103	0.170

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is a binary indicator, equal to one for children designated as low birthweight (i.e., less than 2500 grams), and zero otherwise. See Table A4 in the Appendix for the full results.

[12] Again, we have concerns with nonlinear binary model specifications due to our inclusion of high-dimensional spatial and temporal fixed effects, but for completeness we re-estimate the LBW and VLBW models using Chamberlain's (1980) Fixed Effects Logit specification. The results are consistent in sign, suggesting a negative average effect from the HEUC event, but are statistically insignificant across all Logit model specifications.

Table 6. RDID Very Low Birthweight (VLBW) Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters × post-HEUC	-0.0335**	-0.0312*	-0.0282	-0.0368
	(0.0157)	(0.0165)	(0.0192)	(0.0225)
Additional covariates	X	X	X	X
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8173	8168	8168	1307
Adjusted R-squared	0.030	0.025	0.023	0.042

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is a binary indicator, equal to one for children designated as very low birthweight (i.e., less than 1500 grams), and zero otherwise. See Table A5 in the Appendix for the full results

IV.C. Assessing a Causal Interpretation

Overall, the RDID regression results suggest that the cleanup of hazardous chemicals through RCRA's Corrective Action program is associated with improvements in birth outcomes, particularly in terms of gestational age and reduced risk of preterm births. We take several steps to control for possibly confounding factors and best identify a plausibly causal relationship. We account for numerous individual-level characteristics, include high-dimensional spatiotemporal fixed effects, implement a RDID identification strategy, and in some models employ exact covariate matching. We next conduct three supplemental analyses to assess the appropriateness of a causal interpretation of our findings. We first implement an event study and examine whether the trends across the treated and control groups are parallel. We then compare the observed characteristics across the treated and control groups to assess the degree to which the two are similar, and hence that our assumed counterfactual group is reasonable. Finally, we estimate a series of simple DID regression models to assess whether post-treatment sorting across socioeconomic groups could be confounding our results.

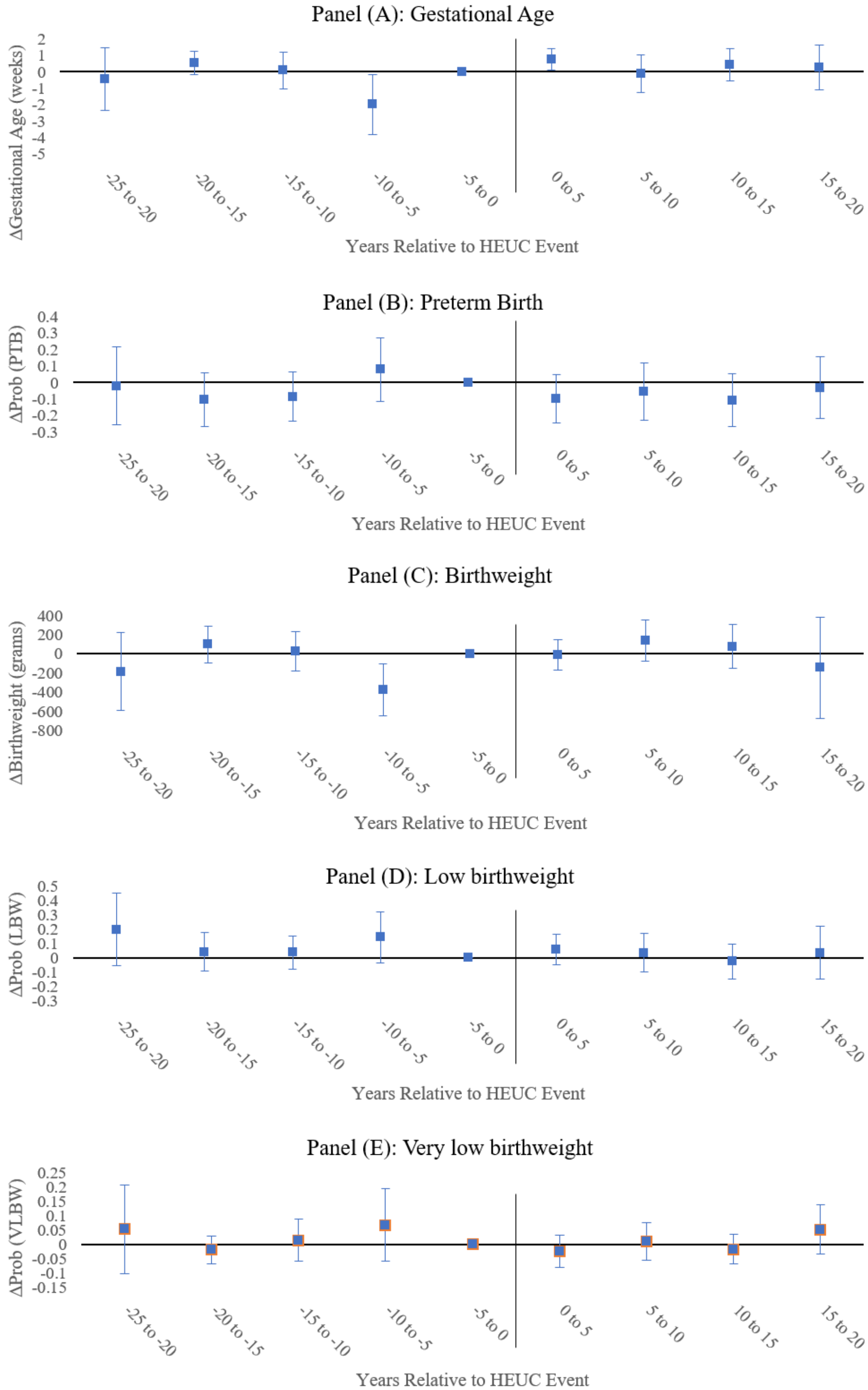
In a conventional DID setup, having parallel pre-treatment trends is generally considered a necessary (but not necessarily sufficient) condition for a causal interpretation of the treatment effect estimates (Angrist and Pischke 2009, Roth 2022). Within a RDID framework, however, it is the post-treatment trends that must be parallel for a plausibly causal interpretation (Kim and Lee 2019; von Hinke and Sørensen 2023). At first, this may seem counterintuitive, but in a RDID setting the treated and control groups are dissimilar in terms of hazardous chemical exposures prior to treatment, and so there is no reason to suspect the pre-treatment trends in the outcomes of interest are similar across the two groups. The treatment – minimizing chemical exposure in our case – then makes the treated and control groups similar. If the trends are parallel after treatment, it suggests that the chemical exposure was the only factor deterring the pre-treatment trends from being parallel, supporting a causal interpretation of the ATT estimates.

To assess whether the post-treatment trends are parallel, we conduct an event study where we estimate a variant of equation (1) that includes 5-year incremental lead and lag indicator variables for $HEUC_{jt}$ and $\mathbb{1}(d_{ij} \leq D) \times HEUC_{jt}$. Coefficients corresponding to the latter term capture the incremental difference in the outcome of interest between the treated and control groups, and allow for this association to vary over time relative to the HEUC event. The regression model is estimated for each of the five birth outcomes. The results for each outcome are plotted in Figure 4. Panel (a) of Figure 4, for example, focuses on gestational age and demonstrates that prior to the HEUC determination the trends are not parallel. The estimated associations displayed in Figure 4 reflect the incremental difference between the treated and control group, and so the point estimates being statistically equal (i.e., a constant difference between the treated and control groups at each point in time) would suggest a parallel trend. Based on an F-test we reject the null hypothesis that the pre-treatment estimates are equal (p = 0.060), suggesting that the pre-treatment trends are not parallel. In particular, we can clearly see a decrease in gestational age around 5 to 10 years before the HEUC event. Anecdotally, this corresponds to the time in which the CA investigations were opened at many sites. Recall that the time between a CA investigation opening and the HEUC determination ranges from 6 to 15 years in our study. Although the contamination issues often date back much further and are usually linked to historical activities, an additional release, migration of chemicals, and/or new discovery of exposure often leads to an investigation being opened. Exposure mitigation and cleanup activities are put in place shortly after an investigation is opened and risks to human health and the environment are identified. The observed decrease in newborn health 5 to 10 years before the HEUC determination is consistent with the general story around many of these sites.

We observe much less fluctuation in the post-treatment trends for gestational age. The post-HEUC point estimates in Panel (a) of Figure 4 are more similar in magnitude, and the 95% confidence intervals largely overlap. We fail to reject the null hypothesis that the post-treatment estimates are equal (p = 0.486), which is consistent with the post-treatment trends being parallel. We see similar patterns when looking at the event study results for the other health outcomes, as shown in the other panels in Figure 4. The pre-treatment trends often suggest a decrease in health around 5 to 10 years before the HEUC event. More importantly, a series of F-tests again suggest that the post-treatment trends are parallel.[13] Overall, the evidence is consistent with a causal interpretation of the ATT estimates from the main analysis.

[13] The corresponding p-values for each of these parallel post-treatment trends tests are p = 0.658 for PTB, 0.565 for birthweight, p = 0.447 for LBW, and p = 0.217 for VLWB.

Figure 4. Event study analysis of parallel trends.



Note: Estimates from event study variant of equation (1). The plotted coefficient estimates correspond to interaction terms between 5-year increment lead and lag indicators relative to the HEUC event, and the 0-250 meter treated group indicator, and thus reflect the incremental difference in health outcomes between the treated and control group at each 5-year time period, all else constant. The vertical lines denote the 95% confidence intervals.

We next assess the comparability of our treated and control groups in terms of observed characteristics. Our identification strategy relies on the assumption that infants born to mothers residing within 0-250 meters of a chemical facility (the treated group) are similar to infants born to mothers residing within 250-1,000 meters of the same chemical facilities in terms of characteristics besides chemical exposure. Although we condition on numerous covariates in the regression models, assessing the similarity between the two groups based on observed characteristics can shed light on the plausibility that the two groups are also similar based on unobserved characteristics. We conduct a series of two-sample t-tests to assess the comparability of the two groups. As shown in Table A6 in the Appendix, the two groups are statistically similar, on average, with respect to 13 out of the 16 observed characteristics. There is a marginally significant difference in the percent of children born to parents with no college education, but the magnitude of this difference is small – 88.8% versus 85.1%. There is also a marginally significant difference in the percent of children who correspond to their mother's third birth, but this statistical difference seems sporadic because there are no clear patterns nor statistically significant differences among the other birth order indicators. The only significant difference that could confound our comparison is that the farther out control group has a greater proportion of Hispanics (13.9%) compared to the treated group (7.8%). Otherwise, our treated and control groups are quite similar in terms of observed characteristics, and we control for ethnicity in all the models.

For our final supplemental analysis to assess the appropriateness of a causal interpretation of the main RDID results, we examine whether there is any systematic demographic sorting in response to cleanup activities. Such sorting behavior has been observed in similar contexts (e.g., Gamper-Rabindran and Timmins 2011), but in a nationwide analysis Cassidy et al. (2024) specifically looked at RCRA CA sites and cleanups and found no evidence of such sorting. Focusing on North Carolina, we are particularly concerned about whether gentrification could be driving our results. If more educated, wealthier people move near these chemical facilities after chemical exposures are eliminated, then that could be driving the estimated improvements in health, rather than the changes in exposure. To assess whether any demographic sorting occurred in our data, we estimate a series of regression models similar to equation (1), but where the outcomes of interest are racial and ethnic indicators, whether the parents were college educated, and whether the household was enrolled in Medicaid. The models only include $HEUC_{jt}$, $\mathbb{1}(d_{ij} \leq D)$, and $\mathbb{1}(d_{ij} \leq D) \times HEUC_{jt}$ as independent variables. No other covariates are included because we are only interested in statistical associations in this supplemental exercise, and not necessarily a causal interpretation. More specifically, we simply want to assess whether there is any systematic sorting of certain types of households after cleanup, and more specifically, whether those patterns differ across the treatment and control groups.

We re-estimate variants of our preferred model specifications, Models 2 and 4. Due to missing values for some of the demographic characteristics, we redo the matching algorithm prior to estimating each variant of Model 4. As shown in Table A7 in the Appendix, the coefficients corresponding to the *0-250 meters × post-HEUC* interaction term are largely insignificant, suggesting that there is no systematic sorting that would confound our interpretation of the ATT estimates from the main analysis. There are two exceptions to this conclusion, however, both based on variants of Model 2. First, the results suggest that there is a relative increase in newborns who are Hispanic in the treated zone after the HEUC event. Given that being Hispanic in our analysis is associated with negligible changes, or sometimes even worsened birth outcomes (see Tables A1 through A5 in the Appendix), this type of sorting would not suggest a gentrification story that confounds the interpretation of our main findings. Another variant of Model 2 suggests that after the HEUC event, there is a decrease in newborns within the 0-250 meter zone whose parents are less educated. This finding is consistent with a gentrification story, suggesting that more educated parents could be moving near the site after cleanup. Neither of these patterns emerge when pre-regression matching procedures are utilized (variants of Model 4). And perhaps most importantly, we see no evidence of sorting based on Medicaid participation, which is perhaps the most direct proxy for income in the data. Overall, we conclude that demographic sorting is likely not a primary driver of the estimated post-cleanup improvements in birth outcomes from our RDID analysis, but this is an important caveat to keep in mind when interpreting the results.

V. DISCUSSION

The Resource Conservation and Recovery Act (RCRA) has been in place since 1976 and is a cornerstone of environmental policy in the US; and yet benefit-cost analyses for most regulations under the authority of RCRA do not quantify the benefits to the primary groups that they are intended to protect – people living in the communities around these hazardous chemical facilities (Guignet and Nolte, 2024). A critical step for benefit-cost analysis, and for welfare analysis more broadly, is to first quantify the effects. We carry out this step, and find localized improvements in birth outcomes for children whose mothers lived within 250 meters of a chemical facility.

Finding such localized health effects is not necessarily surprising in our context because the consumption of contaminated groundwater is likely not an exposure pathway of concern, at least not among the CA sites analyzed in our study. The vast majority of people living around the 12 CA sites in our study lived within a public water system (PWS) service area, and likely relied on these public systems for their potable water.[14] In contrast to private groundwater wells, public water systems typically draw on water sources far away from one's home and nearby CA sites, and are likely not contaminated by hazardous chemicals from nearby sites. Only 1.24% of our sample of newborns potentially relied on private groundwater wells for their potable water (i.e., lived outside the PWS service area), and none of the children living within 250 meters of a CA site relied on private groundwater wells. Similar analyses of other CA sites where local populations rely on private groundwater wells could find much farther-reaching health effects.

Given the extremely localized nature of the estimated health effects and minimal use of local groundwater in our study area, we speculate that re-suspension of contaminated particles into the air and mothers' subsequent inhalation, ingestion, and/or dermal contact with these particles is a

[14] PWS service area boundaries were obtained from the EPA (2023b). Using Geographic Information Systems (GIS), we determined whether each mother's place of residence was located within a PWS service area.

plausible exposure pathway. Vapor intrusion into homes and subsequent inhalation of hazardous fumes is also possible. The potential for soil vapor intrusion and/or the implementation of soil vapor extraction systems were documented at 4 of the 12 CA sites. Detailed, site-specific monitoring and exposure analysis would be needed to firmly identify the mechanisms by which populations near RCRA sites came in contact with hazardous chemicals.

Nonetheless, we find that children born to mothers living within 250 meters of a CA site experienced an average 110 to 164 gram improvement in birthweight after cleanup was completed, as established by the official HEUC determination made by regulators. This corresponds to a 3-5% increase in birthweight, although the results are not significant across all model specifications. The results are also mixed when analyzing alternative binary measures of birthweight. Models of the risk of a low birthweight (LBW) suggest a decrease in risk post-HEUC, but this result is only statistically significant in the simplest of the four model specifications. Models of the risk of very low birthweight (VLBW) suggest a marginally significant 3 percentage point decrease in risk, on average; but again this result is statistically significant in only two of the four models.

We find stronger, and more robust evidence when examining the effects of cleanup on gestational age. Across all model specifications, we find that after the HEUC determination, gestational age increased by an average of almost a week (0.8 to 0.9 weeks). This corresponds to a roughly 2% increase in the amount of time that a child has to develop *in utero*. Models of a binary preterm birth (PTB) indicator reveal a statistically significant 6 to 8 percentage point average decrease in risk after the HEUC event.

To illustrate the potential magnitude of the monetized newborn health benefits from hazardous chemical cleanups, we apply unit value estimates for reductions in the risk of PTB and VLBW. We focus on these two health outcomes because unit value estimates were available from recently released studies that were sponsored by federal and international government organizations. The first study was by Abt Associates (2022), and was conducted to aid the US EPA in benefit-cost analyses. Abt Associates (2022) estimated the incremental cost-of-illness (COI) for the average PTB.[15] The COI estimates include the expected costs for birth-related and subsequent inpatient hospital visits during the first two years of a child's life. Based on Abt Associates' estimates, the cost-savings for an avoided PTB case is $13,894 (2024$ USD).[16] The expected avoided costs from a reduction in the risk of PTB can be used as a proxy to estimate the monetized benefits,

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[15] Abt Associates (2022) also calculated COI estimates for changes in continuous birthweight and the risk of LBW. We focus on PTB in this exercise because applying their COI estimates for continuous birthweight would be more complicated. Although their COI estimates for changes in continuous birthweight could be applied, doing so requires information of the baseline birthweight distribution, and this is beyond the scope of what we wanted to do for this illustrative exercise. Furthermore, our estimated improvements in birthweight and the risk of LBW, were statistically significant in half or less than half of the models, and so monetizing these mixed results may not be as interesting of an example. Additionally, we have more theoretically valid willingness-to-pay estimates for reduced VLBW risks, which we discuss next.

[16] Our COI estimate is calculated by multiplying Abt Associates' (2022) annual inpatient costs by two (to account for the first two years of life), and adding the birth-related hospital costs. We then convert their estimates to 2024$ USD using the US Bureau of Labor Statistics' (BLS) annual urban consumer price index (BLS 2025).

particularly when more theoretically appropriate willingness to pay (WTP) estimates for ex ante benefits analysis are not available (EPA 2014).

The Organisation for Economic Co-operation and Development (OECD) recently conducted a series of international stated preference studies focused specifically on reduced morbidity risks from reductions in hazardous chemical exposures. Under these broader efforts, Ščasný et al. (2023) estimated prospective parents' WTP for reduced risks of VLBW. Based on their results, the value for (an avoided) statistical case (VSC) of VLBW is $1,488,832 (2024$ USD).[17]

Whether based on COI or WTP, we can use the unit value estimates to illustrate the newborn health benefits from remediating hazardous chemical releases at RCRA sites. We estimate the household-level per child benefits of cleanup and the HEUC determination by taking the product of the unit value estimate (M) and our estimated reduction in risk (ΔR), as shown:

(3) $$HH\ Benefit = M \times \Delta R$$

$HH\ Benefit$ is the benefit per child to the average household that lives within 250 meters of a CA site *and* is planning to (or will) have one or more children.

We can also calculate aggregate benefits. For example, we can estimate the average annual benefit from the cleanup and HEUC determination at the 12 CA sites analyzed by multiplying the household per child benefit from equation (3) by the average number of conceived children within 250 meters of a site each year (N). More formally:

(4) $$Total\ Annual\ Benefit = M \times \Delta R \times N$$

The monetized benefits are presented in Table 7, and are based on the estimated reductions in risk from our preferred specifications, Models 2 and 4. First focusing on preterm birth, our results suggested a reduction of about 5.9 or 7.8 percentage points in the risk of PTB, depending on the model (see Table 6). Applying the COI estimate suggests that the average affected household experiences a benefit from cleanup and the HEUC determination of $813 to $1089 per child.[18] We emphasize that the affected households are those who live within 250 meters and who will have a child. There are 11.34 children conceived each year, on average, whose mother lived within 250 meters of one of the 12 CA sites. Plugging this in for N in equation (4) yields a total annual benefit from the HEUC event at these 12 CA sites of $9,217 to $12,352.

Turning to the estimated reductions in the risk of VLBW, and applying Ščasný et al.'s (2023) WTP estimate, we find that the benefit of cleanup to the average affected household is $46,414 or $54,783 per child, depending on the model. We emphasize that the latter result from the Model 4 specification is statistically insignificant. Again, these WTP estimates would only apply to

[17] This estimate is based on Ščasný et al.'s (2023) US-specific VSC estimate of $1,389,000 (2022$ USD). We convert this to 2024$ USD using the BLS's annual urban consumer price index (BLS 2025).
[18] All monetized values are presented in 2024$ USD.

households living within 250 meters of a CA site and who plan to have a child. Aggregating the estimated benefits from reduced VLBW risks across the 12 sites, as per equation (4), suggests an average annual benefit of $526,334 to $621,243.[19] The estimated benefits from reduced PTB and VLBW risks should not be summed. Doing so would likely result in at least partial double counting.

Table 7. Illustrative Monetized Benefits (2024$ USD).

	PTB		VLBW	
	(2)	(4)	(2)	(4)
Change in Risk[a]	-0.0585**	-0.0784*	-0.0312*	-0.0368
	[-0.1147 - -0.0023]	[-0.1994 - 0.0024]	[-0.0635 - 0.0011]	[-0.0809 - 0.0073]
Household Benefit	$813**	$1089*	$46,414*	$54,783
	[32 - 1,594]	[-33 - 2,211]	[-1,666 - 94,494]	[-10,899 - 120,466]
Total Annual Benefit	$9,217**	$12,352*	$526,334*	$621,243
	[359 - 18,074]	[-371 - 25,075]	[-18,891 - 1,071,558]	[-123,597 - 1,366,083]

Note: *p<0.10, ** p<0.05, *** p<0.01. The 95% confidence intervals are in brackets. (a) Estimated benefits from reductions in the risk of preterm birth (PTB) and very low birth weight (VLBW) are based on Models (2) and (4) in Tables 6 and 4, respectively.

Although the annual benefits across the 12 CA sites analyzed seem relatively small, especially considering that the total remediation costs at just one RCRA CA site can often be a few to several million dollars (see footnote 23 in Guignet and Nolte (2024)), we emphasize that these are annual benefits and that this is only one set of health endpoints. There are numerous other benefit endpoints to consider. In addition, the estimated health effects in our study are very localized, thus affecting a small number of households. Our finding of such localized effects is at least partly driven by the fact that residents do not use the groundwater near these facilities as their potable water source. The potential health effects to surrounding communities could be much farther reaching at other CA sites where consumption of contaminated groundwater is a viable exposure pathway. In such cases, we would expect larger aggregate benefits from cleanup (i.e., the *N* in equation (4) would be greater).

[19] The WTP estimates from Ščasný et al.'s (2023) are based on a representative sample of the US population of adults over 18 years of age, and who are of childbearing age, in a relationship, and plan to have a(nother) biological child within the next five years. When applying Ščasný et al.'s estimates we are assuming that our target population in North Carolina holds a similar value. However, our target population may include unplanned pregnancies. For example, our sample includes mothers under the age of 18 (almost 21% of mothers within 250 meters of a CA site were between 15 and 19 years of age). The preferences and income of households in our sample could be different from Ščasný et al.'s nationally representative sample, but we do not think it is warranted to exclude these households from our illustrative benefit calculations. Doing so would assume that these households place a value of $0 on avoiding the adverse birth outcomes.

VI. CONCLUSION

Our study set out to (i) estimate the effects of cleanups at RCRA-regulated chemical facilities on newborn health, and (ii) determine the spatial extent of any newborn health effects. Focusing on Corrective Action (CA) investigations at chemical facilities across North Carolina and using individual-level birth certificate data over a 30-year study period (1990-2019), we employed a Difference-in-differences (DID) methodology that features two methodologically novel features. First, our empirical setting is better accommodated by a Reverse Difference-in-difference (RDID) framework, which to our knowledge has only been applied in two published studies (Kim and Lee 2019; von Hinke and Sørensen 2023) but may fit a variety of similar contexts in environmental economics and beyond. Our study contributes to this nascent literature, and aids in furthering the potentially widely viable RDID methodology. Second, our RDID design features a naturally stacked DID setup, which allows us to circumvent recent concerns around staggered treatment events over time (Goodman-Bacon 2021, Roth et al. 2023).

Our analysis reveals localized improvements in newborn health following cleanup and the official Human Exposure Under Control (HEUC) determination. Children born to mothers living within 250 meters of a CA site experienced an average 110 to 164 gram improvement in birthweight, and a 4 and 3 percentage point decrease in the risk of low birthweight and very low birth weight, but these results were not statistically significant across all models. In contrast, we find robust evidence of newborn health improvements in terms of gestational age, suggesting that cleanup and the HEUC determination led to a 0.8 to 0.9 week increase in the gestation, and a significant 6 to 8 percentage point average decrease in the risk of preterm birth. Considering that the average risk of a preterm birth in our sample is 12.5%, the latter is particularly notable because it suggests that cleanup cuts the risk of preterm birth in half, on average.

Focusing on preterm birth and very low birth weight, we applied COI and WTP estimates from the literature to illustrate how our quantified health improvements from cleanups can be used for benefits analyses. Doing so suggested that the average affected household would benefit \$813 to \$1089 per child from the reduced risk of a PTB due to cleanup and the HEUC determination. The corresponding benefit for the reduced risk of VLBW is \$46,414 to \$54,783 per child, but only the former estimate is statistically significant.

The estimated newborn health effects from reduced chemical exposure are important in their own right, but also signal potential longer term, later-in-life benefits (Currie 2011) that we do not capture here. Studies have linked increases in both birthweight (e.g., Black et al., 2007; Belbasis et al. 2016; Xie et al. 2017; Ludvigsson et al. 2018; Baguet and Dumas 2019; WHO 2022) and gestational age (e.g., Crump et al. 2011; Boyle et al. 2012) to later improvements in health, education, and labor outcomes.

We went to great lengths in the empirical analysis to minimize the influence of potentially confounding factors, and undertook several checks to assess the plausibility of a causal interpretation of our results. Our regression models included numerous, individual-level covariates and spatiotemporal fixed effects, and a RDID identification strategy that utilized infants born to mothers living in the same neighborhoods and around the same chemical facilities as a control group. We investigated the robustness of the results across numerous alternative birth outcomes, and in some models employed an exact covariate matching to yield more balanced and comparable treated and control groups across sites and over time. In our supplemental diagnostic analyses, we confirmed that post-treatment sorting is likely not driving the results, that the treated and control groups are similar in terms of most observed characteristics, and that the post-treatment trends are parallel. Although causality can never be unambiguously claimed in analyses of observational data like ours, the evidence is overall consistent with a causal interpretation of the results.

There are several caveats to keep in mind. For example, to facilitate a clean quasi-experimental comparison, we focused on just 12 RCRA CA sites in North Carolina, where we observed treated births (i.e., within 0-250 meters of a CA site) both before and after the HEUC determination. There is surely a high degree of heterogeneity across chemical facilities in terms of surrounding residential development and populations; types of industrial activities; the types and volumes of chemicals used and released; the direction, speed, and extent of exposure pathways; etc. Caution is warranted when extrapolating our average estimates to other sites, both within North Carolina and beyond. It is also important to keep in mind that although our data started with the population of live births across the State from 1990-2019, the number of identifying observations is small. We observe only 119 post-HEUC births within the 0-250 meter treated group bin. The small number of identifying observations is due to the extremely localized nature of the estimated health effects. We interpret our finding of statistically significant improvements in gestational age and reduced risks of preterm birth, despite the small number of identifying observations, as evidence of the strength of the responses in newborn health from cleanup. At the same time, our often marginally significant or statistically insignificant estimates of the effects of cleanup on measures of birthweight may be, at least partly, driven by the small sample size. The sign and magnitude of those results are consistent with the overall finding that cleanup leads to localized improvements in newborn health. An additional limitation is that our data only includes live births. If chemical exposure and the resulting health effects lead to an increased risk of miscarriage or stillbirth, then this would not be captured in our analysis. A final caveat is that cleanup at these sites often takes years, and even decades. Our HEUC treatment event is not a discrete change in actual exposure, but instead corresponds to the official determination on paper of when there is no longer a risk of human exposure. Our estimated health improvements could be considered under-estimates, at least to the extent that chemical exposure was being reduced and subsequent health benefits realized, prior to the official HEUC determination.

Despite these caveats, our quantified health effects can inform local cleanup and land use decisions, and create a path for expanding what benefits are quantified in benefit-cost analyses of future regulations under RCRA and similar programs addressing chemical incidents and cleanups.

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APPENDIX

Figure A1. Human Exposure Under Control (HEUC) determinations at each Corrective Action Site by Year.

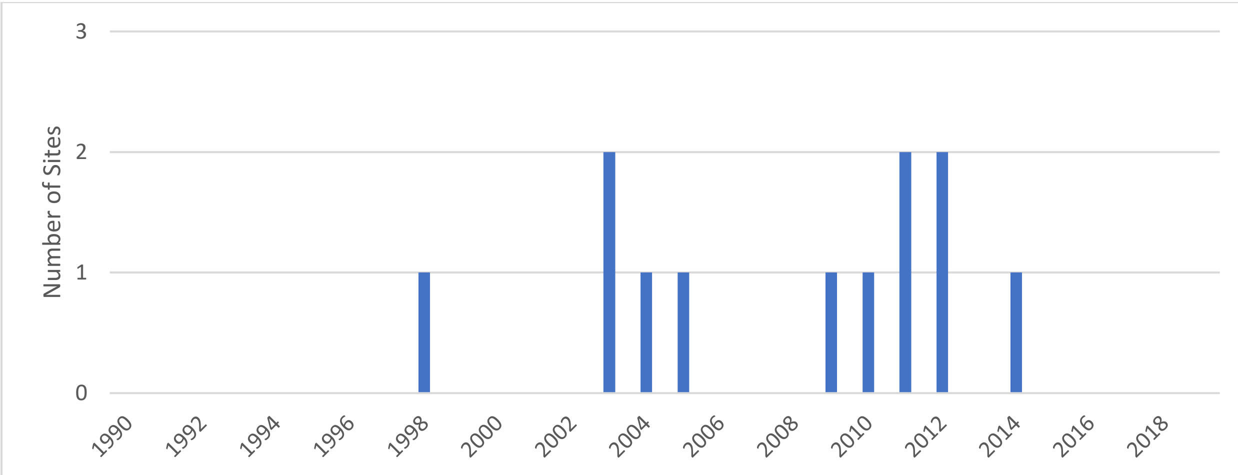

Figure A2. Conditional Distance Gradients: Binary Infant Health Outcomes.

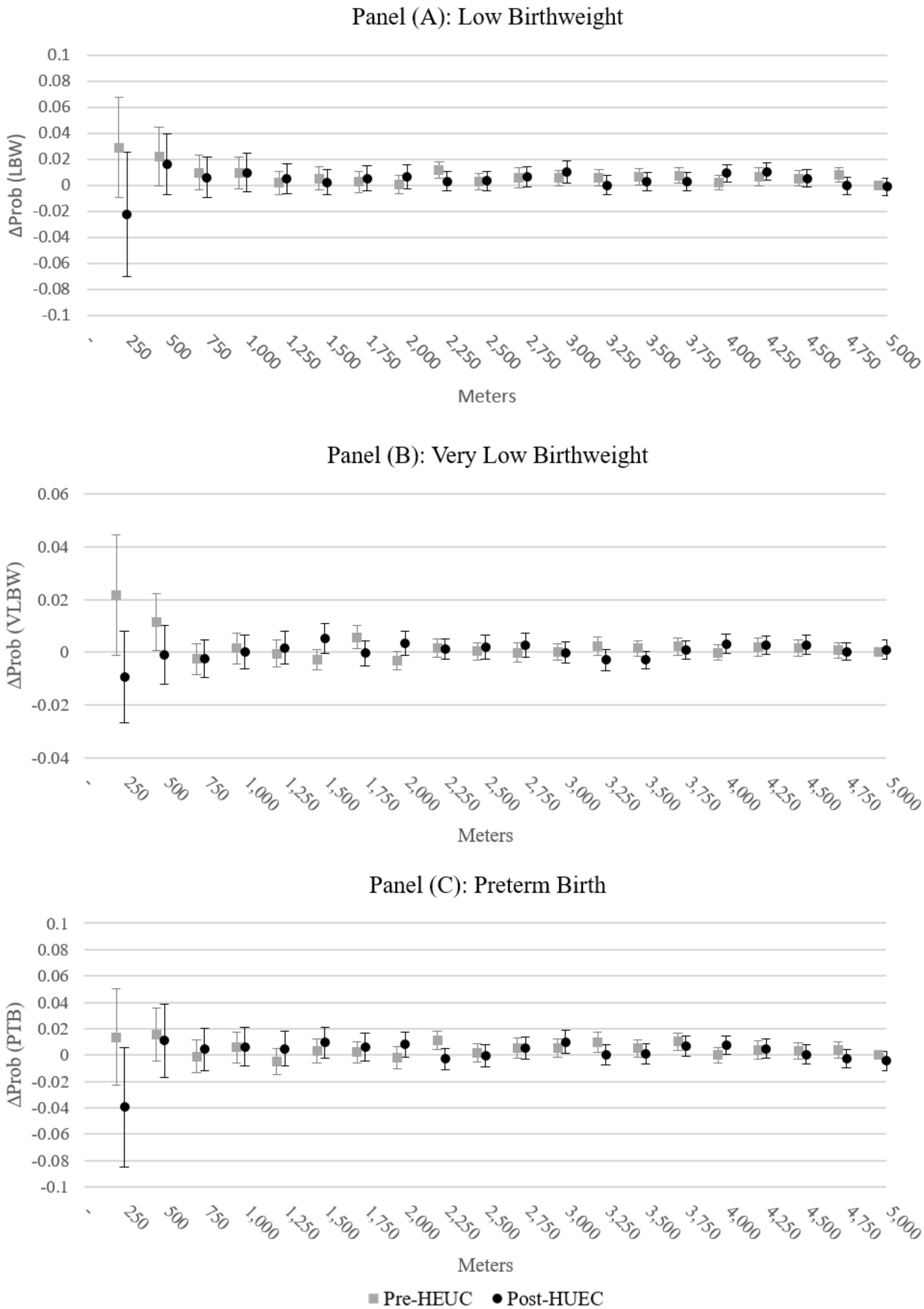

Table A1. RDID Gestational Age Full Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters	-0.2558	-0.2768		-0.3008
	(0.1889)	(0.1974)		(0.2036)
post-HEUC	0.1060	0.3419		0.0715
	(0.1263)	(0.3883)		(0.4278)
0-250 meters × post-HEUC	0.8890***	0.7649***	0.9085***	0.7978**
	(0.2490)	(0.2591)	(0.3283)	(0.3775)
Black	-0.5018***	-0.4833***	-0.4841***	-1.1919***
	(0.1115)	(0.1093)	(0.1108)	(0.3145)
Hispanic	-0.1741	-0.1803	-0.1781	-0.7119**
	(0.1076)	(0.1172)	(0.1167)	(0.2967)
Other race/ethnicity	-0.3905***	-0.4032***	-0.4050***	-1.1541***
	(0.1052)	(0.1100)	(0.1113)	(0.4279)
Missing: Race/Ethnicity	-0.5997***	-0.6101***	-0.6117***	-1.3396***
	(0.1160)	(0.1173)	(0.1186)	(0.3828)
Male	-0.0731	-0.0792	-0.0800	-0.1068
	(0.0614)	(0.0610)	(0.0617)	(0.1910)
Plural birth	-4.1896***	-4.3130***	-4.3094***	-4.8311***
	(0.3254)	(0.3255)	(0.3254)	(0.9617)
Parents no college	-0.2764**	-0.2812**	-0.2836**	-0.6437**
	(0.1250)	(0.1320)	(0.1317)	(0.2617)
Missing: Parents no college	-0.5526	-0.5149	-0.5063	-0.3481
	(1.1462)	(1.0603)	(1.0581)	(0.9365)
Mom 15-24 years	0.1228	0.1318	0.1310	0.8528***
	(0.0823)	(0.0861)	(0.0869)	(0.2766)
Mom 35-44 years	-0.1021	-0.1209	-0.1231	-0.2943
	(0.1270)	(0.1314)	(0.1312)	(0.3440)
Missing: Mom age	-0.1760	-0.1713	-0.1717	1.6704**
	(0.3390)	(0.3366)	(0.3377)	(0.8196)
Smoked	-0.3257***	-0.3089***	-0.3093***	-0.4764
	(0.0942)	(0.0973)	(0.0973)	(0.3519)
Missing: Smoked	0.6105	0.5473	0.5499	-0.3207
	(1.1934)	(1.1251)	(1.1240)	(1.0454)
Second birth	-0.1916***	-0.1609**	-0.1638**	-0.0804
	(0.0690)	(0.0740)	(0.0741)	(0.1788)
Third birth	-0.2552***	-0.2421***	-0.2432***	0.0453
	(0.0768)	(0.0799)	(0.0806)	(0.2565)
Fourth birth	-0.3847***	-0.3735***	-0.3768***	0.0269
	(0.1160)	(0.1176)	(0.1177)	(0.3252)
≥ Fifth birth	-0.4082**	-0.3420**	-0.3433**	0.6566*
	(0.1603)	(0.1696)	(0.1690)	(0.3882)
Not married	-0.1247	-0.1069	-0.1113	-0.0847

	(0.0809)	(0.0828)	(0.0827)	(0.2419)
Missing: Not married	-0.4414*	1.1670	1.1588	0.0000
	(0.2300)	(0.8281)	(0.8301)	(.)
WIC	0.3368**	0.3625**	0.3632**	0.9168*
	(0.1471)	(0.1464)	(0.1466)	(0.4711)
Missing: WIC	-0.4070	-0.3778	-0.4161	0.2889
	(0.6018)	(0.6865)	(0.6988)	(1.0378)
Medicaid	0.1053	0.1608	0.1641	-0.4693
	(0.1460)	(0.1595)	(0.1601)	(0.4059)
Missing: Medicaid	0.4307	0.4822	0.4797	0.1707
	(0.5984)	(0.6768)	(0.6839)	(1.1312)
Constant	39.3032***	39.1381***	39.2664***	39.4900***
	(0.3680)	(0.3729)	(0.3736)	(0.7350)
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8178	8173	8173	1308
Adjusted R-squared	0.094	0.089	0.087	0.141

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is gestational age (in weeks). All independent variables are binary indicators.

Table A2. RDID Preterm Birth (PTB) Full Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters	0.0119	0.0166		0.0334
	(0.0199)	(0.0208)		(0.0227)
post-HEUC	0.0034	-0.0156		-0.0062
	(0.0156)	(0.0482)		(0.0652)
0-250 meters × post-HEUC	-0.0599**	-0.0585**	-0.0697**	-0.0784*
	(0.0291)	(0.0287)	(0.0314)	(0.0412)
Black	0.0327**	0.0333**	0.0332**	0.0876**
	(0.0128)	(0.0129)	(0.0131)	(0.0350)
Hispanic	0.0166	0.0210	0.0211	0.0965**
	(0.0149)	(0.0157)	(0.0157)	(0.0420)
Other race/ethnicity	0.0116	0.0155	0.0154	0.0776*
	(0.0127)	(0.0133)	(0.0134)	(0.0399)
Missing: Race/Ethnicity	0.0500***	0.0536***	0.0535***	0.1225***
	(0.0150)	(0.0153)	(0.0154)	(0.0399)
Male	0.0091	0.0095	0.0092	0.0183
	(0.0074)	(0.0073)	(0.0073)	(0.0232)
Plural birth	0.5094***	0.5185***	0.5173***	0.5796***
	(0.0349)	(0.0349)	(0.0349)	(0.0938)
Parents no college	0.0270*	0.0265*	0.0263*	0.0713**
	(0.0151)	(0.0157)	(0.0158)	(0.0306)
Missing: Parents no college	0.0823	0.0687	0.0669	0.3432*
	(0.0894)	(0.0967)	(0.0968)	(0.1849)
Mom 15-24 years	-0.0067	-0.0059	-0.0054	-0.0623**
	(0.0094)	(0.0096)	(0.0097)	(0.0265)
Mom 35-44 years	-0.0096	-0.0069	-0.0064	-0.0089
	(0.0152)	(0.0158)	(0.0158)	(0.0379)
Missing: Mom age	0.0588	0.0636	0.0636	-0.0594
	(0.0480)	(0.0496)	(0.0497)	(0.0670)
Smoked	0.0387***	0.0354***	0.0353***	0.0742**
	(0.0118)	(0.0122)	(0.0123)	(0.0360)
Missing: Smoked	-0.0388	-0.0222	-0.0213	-0.2076
	(0.0980)	(0.1065)	(0.1067)	(0.1890)
Second birth	0.0005	-0.0012	-0.0010	-0.0176
	(0.0081)	(0.0087)	(0.0087)	(0.0241)
Third birth	0.0040	0.0034	0.0038	-0.0312
	(0.0105)	(0.0110)	(0.0110)	(0.0304)
Fourth birth	0.0412***	0.0413***	0.0418***	-0.0114
	(0.0149)	(0.0150)	(0.0150)	(0.0376)
≥ Fifth birth	0.0435**	0.0422*	0.0427*	-0.0680
	(0.0212)	(0.0219)	(0.0219)	(0.0450)
Not married	0.0246**	0.0223**	0.0226**	0.0114

	(0.0103)	(0.0106)	(0.0106)	(0.0245)
Missing: Not married	-0.0520**	-0.1591***	-0.1574***	0.0000
	(0.0241)	(0.0481)	(0.0485)	(.)
WIC	-0.0392**	-0.0403**	-0.0404**	-0.1182**
	(0.0156)	(0.0164)	(0.0164)	(0.0535)
Missing: WIC	-0.0330	-0.0351	-0.0296	-0.2320**
	(0.0438)	(0.0522)	(0.0534)	(0.0992)
Medicaid	-0.0149	-0.0217	-0.0220	0.0456
	(0.0191)	(0.0197)	(0.0197)	(0.0414)
Missing: Medicaid	-0.0452	-0.0537	-0.0523	-0.1284
	(0.0338)	(0.0423)	(0.0433)	(0.1154)
Constant	0.0933**	0.1078**	0.0988**	0.2458**
	(0.0415)	(0.0439)	(0.0420)	(0.1014)
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8178	8173	8173	1308
Adjusted R-squared	0.086	0.080	0.079	0.141

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is a binary indicator, equal to one for children designated as a preterm birth (i.e., *in utero* for less than 37 weeks), and zero otherwise. All independent variables are binary indicators.

Table A3. RDID Birthweight Full Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters	-57.0938	-57.7150		-47.8427
	(39.1015)	(40.8152)		(50.3419)
post-HEUC	30.7765	130.2504		154.1671
	(29.3475)	(87.9825)		(110.4513)
0-250 meters × post-HEUC	164.4512***	125.0005*	110.3970	117.4240
	(62.1264)	(65.6349)	(80.2677)	(88.8404)
Black	-280.2240***	-271.7354***	-271.3556***	-427.2834***
	(24.7012)	(24.8584)	(25.0899)	(74.7313)
Hispanic	-83.6063***	-84.8407***	-82.6124***	-211.9060***
	(26.4104)	(28.4458)	(28.2976)	(75.3924)
Other race/ethnicity	-211.9341***	-208.1100***	-207.7227***	-412.3379***
	(25.7615)	(27.9025)	(28.0416)	(95.3206)
Missing: Race/Ethnicity	-289.4577***	-281.4315***	-281.2579***	-486.6319***
	(25.9703)	(27.2738)	(27.4002)	(85.7015)
Male	103.9291***	104.1050***	104.2239***	96.2285**
	(12.7829)	(12.9722)	(13.0350)	(46.1184)
Plural birth	-1025.0927***	-1043.8657***	-1044.6878***	-1102.6581***
	(53.9279)	(55.8800)	(56.0546)	(148.5574)
Parents no college	-100.0102***	-101.5395***	-102.6591***	-145.7058**
	(27.7884)	(29.1439)	(29.2325)	(72.3100)
Missing: Parents no college	-186.7590	-167.2425	-168.0220	-112.3624
	(194.9381)	(187.6598)	(188.0081)	(259.2772)
Mom 15-24 years	-16.0518	-18.3433	-18.4358	74.0224
	(18.1334)	(19.1499)	(19.0895)	(51.2844)
Mom 35-44 years	-4.0750	-5.2402	-4.8649	-58.1222
	(29.3726)	(30.0840)	(29.9964)	(72.8817)
Missing: Mom age	-142.5147**	-151.7646**	-152.1213**	256.1181
	(69.8619)	(67.0090)	(66.9069)	(212.9595)
Smoked	-212.7797***	-205.5970***	-205.7531***	-238.6378***
	(20.4064)	(20.8633)	(20.9399)	(67.9316)
Missing: Smoked	111.8521	90.6235	92.5746	-211.4604
	(213.6967)	(206.4856)	(206.9778)	(293.5349)
Second birth	65.6913***	70.5244***	69.4871***	75.0071*
	(16.5059)	(17.2467)	(17.2454)	(42.8083)
Third birth	62.6198***	67.4462***	66.3186***	113.6084**
	(21.5606)	(21.4307)	(21.5753)	(55.1763)
Fourth birth	57.8700**	62.9846***	61.7574***	173.3839***
	(22.6607)	(23.0042)	(23.0852)	(62.7089)
≥ Fifth birth	95.1407***	101.6716***	101.7160***	451.0519***
	(35.5182)	(37.7004)	(37.6518)	(98.2166)
Not married	-37.7024**	-33.0000*	-34.4836*	-27.2159

	(18.5771)	(18.8283)	(18.7201)	(55.0883)
Missing: Not married	-346.0038***	-78.9715	-80.4339	0.0000
	(56.5864)	(93.6628)	(93.8913)	(.)
WIC	61.5788**	54.5717*	54.4600*	252.0527***
	(28.2011)	(29.0105)	(29.1693)	(88.4588)
Missing: WIC	-185.0651	-217.4368	-225.1606	233.6796
	(174.0992)	(189.8621)	(191.4368)	(211.7911)
Medicaid	5.4855	17.5059	17.9297	-131.9629*
	(32.7404)	(35.0223)	(35.2825)	(78.4166)
Missing: Medicaid	160.5469	190.0834	185.2244	4.1328
	(187.7460)	(202.4730)	(203.9231)	(235.7041)
Constant	3411.7762***	3373.0842***	3422.3620***	3274.0555***
	(88.1437)	(91.0191)	(87.5804)	(196.1728)
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8173	8168	8168	1307
Adjusted R-squared	0.144	0.142	0.141	0.203

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is birthweight (in grams). All independent variables are binary indicators.

Table A4. RDID Low Birthweight (LBW) Full Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters	0.0183	0.0246		0.0350
	(0.0191)	(0.0204)		(0.0271)
post-HEUC	-0.0299*	-0.1027**		0.0043
	(0.0155)	(0.0518)		(0.0442)
0-250 meters × post-HEUC	-0.0444*	-0.0326	-0.0308	-0.0425
	(0.0265)	(0.0257)	(0.0328)	(0.0428)
Black	0.0569***	0.0552***	0.0553***	0.0911***
	(0.0115)	(0.0118)	(0.0119)	(0.0316)
Hispanic	0.0123	0.0132	0.0131	0.0498
	(0.0147)	(0.0153)	(0.0153)	(0.0307)
Other race/ethnicity	0.0407***	0.0406***	0.0410***	0.1008**
	(0.0122)	(0.0128)	(0.0128)	(0.0415)
Missing: Race/Ethnicity	0.0686***	0.0681***	0.0681***	0.1327***
	(0.0131)	(0.0138)	(0.0139)	(0.0339)
Male	-0.0179**	-0.0187***	-0.0188***	-0.0026
	(0.0070)	(0.0071)	(0.0071)	(0.0236)
Plural birth	0.5735***	0.5807***	0.5806***	0.6764***
	(0.0329)	(0.0322)	(0.0323)	(0.0642)
Parents no college	0.0392***	0.0391***	0.0404***	0.0704**
	(0.0134)	(0.0137)	(0.0138)	(0.0351)
Missing: Parents no college	0.0855	0.0812	0.0801	0.2476
	(0.0873)	(0.0950)	(0.0954)	(0.1851)
Mom 15-24 years	-0.0067	-0.0051	-0.0054	-0.0251
	(0.0094)	(0.0098)	(0.0098)	(0.0238)
Mom 35-44 years	0.0053	0.0063	0.0066	0.0602*
	(0.0138)	(0.0144)	(0.0144)	(0.0339)
Missing: Mom age	0.0382	0.0437	0.0436	-0.0807
	(0.0437)	(0.0438)	(0.0437)	(0.0518)
Smoked	0.0741***	0.0737***	0.0738***	0.0800**
	(0.0130)	(0.0127)	(0.0127)	(0.0347)
Missing: Smoked	-0.0086	0.0049	0.0062	-0.0906
	(0.0917)	(0.1014)	(0.1019)	(0.1893)
Second birth	-0.0215**	-0.0236**	-0.0236**	-0.0292
	(0.0090)	(0.0095)	(0.0095)	(0.0261)
Third birth	-0.0241**	-0.0252**	-0.0249**	-0.0542*
	(0.0096)	(0.0102)	(0.0102)	(0.0307)
Fourth birth	-0.0192	-0.0210	-0.0208	-0.0376
	(0.0151)	(0.0150)	(0.0151)	(0.0316)
≥ Fifth birth	-0.0193	-0.0189	-0.0192	-0.1292***
	(0.0189)	(0.0198)	(0.0199)	(0.0492)
Not married	0.0160*	0.0138	0.0141	0.0017

	(0.0092)	(0.0096)	(0.0096)	(0.0240)
Missing: Not married	-0.0462*	-0.1065***	-0.1062***	0.0000
	(0.0260)	(0.0383)	(0.0383)	(.)
WIC	-0.0417**	-0.0418**	-0.0414**	-0.1056**
	(0.0162)	(0.0174)	(0.0175)	(0.0442)
Missing: WIC	-0.0011	-0.0198	-0.0150	-0.0572
	(0.0531)	(0.0661)	(0.0671)	(0.0994)
Medicaid	0.0235	0.0210	0.0203	0.1112***
	(0.0192)	(0.0199)	(0.0199)	(0.0406)
Missing: Medicaid	-0.0094	-0.0081	-0.0061	-0.0001
	(0.0448)	(0.0568)	(0.0579)	(0.1224)
Constant	0.0543	0.0913*	0.0542	0.0154
	(0.0479)	(0.0506)	(0.0489)	(0.0936)
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8173	8168	8168	1307
Adjusted R-squared	0.109	0.104	0.103	0.170

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is a binary indicator, equal to one for children designated as low birthweight (i.e., less than 2500 grams), and zero otherwise. All independent variables are binary indicators.

Table A5. RDID Very Low Birthweight (VLBW) Full Regression Model Results.

	(1)	(2)	(3)	(4)
0-250 meters	0.0220*	0.0224**		0.0287**
	(0.0114)	(0.0109)		(0.0126)
post-HEUC	0.0037	-0.0010		-0.0302
	(0.0065)	(0.0245)		(0.0249)
0-250 meters × post-HEUC	-0.0335**	-0.0312*	-0.0282	-0.0368
	(0.0157)	(0.0165)	(0.0192)	(0.0225)
Black	0.0209***	0.0198***	0.0198***	0.0395**
	(0.0059)	(0.0065)	(0.0065)	(0.0175)
Hispanic	0.0070	0.0078	0.0080	0.0172
	(0.0056)	(0.0067)	(0.0067)	(0.0146)
Other race/ethnicity	0.0148**	0.0146**	0.0145**	0.0467**
	(0.0058)	(0.0063)	(0.0064)	(0.0230)
Missing: Race/Ethnicity	0.0256***	0.0255***	0.0253***	0.0402*
	(0.0059)	(0.0065)	(0.0065)	(0.0226)
Male	0.0031	0.0041	0.0038	0.0005
	(0.0035)	(0.0036)	(0.0036)	(0.0103)
Plural birth	0.1375***	0.1427***	0.1437***	0.1176*
	(0.0284)	(0.0285)	(0.0286)	(0.0664)
Parents no college	0.0137**	0.0129*	0.0126*	0.0222
	(0.0068)	(0.0075)	(0.0075)	(0.0136)
Missing: Parents no college	0.0342	0.0305	0.0301	-0.0392
	(0.0588)	(0.0502)	(0.0502)	(0.0490)
Mom 15-24 years	-0.0064	-0.0073	-0.0075	-0.0383***
	(0.0051)	(0.0054)	(0.0054)	(0.0126)
Mom 35-44 years	0.0069	0.0063	0.0063	0.0199
	(0.0075)	(0.0075)	(0.0075)	(0.0202)
Missing: Mom age	0.0050	0.0009	0.0007	-0.0513***
	(0.0237)	(0.0223)	(0.0223)	(0.0191)
Smoked	0.0100*	0.0090*	0.0089*	0.0253
	(0.0054)	(0.0053)	(0.0053)	(0.0219)
Missing: Smoked	-0.0487	-0.0411	-0.0411	0.0543
	(0.0619)	(0.0541)	(0.0542)	(0.0496)
Second birth	-0.0001	-0.0010	-0.0010	-0.0093
	(0.0039)	(0.0043)	(0.0043)	(0.0096)
Third birth	-0.0033	-0.0037	-0.0038	-0.0170
	(0.0047)	(0.0048)	(0.0048)	(0.0125)
Fourth birth	-0.0052	-0.0056	-0.0058	-0.0149
	(0.0066)	(0.0070)	(0.0070)	(0.0179)
≥ Fifth birth	-0.0077	-0.0114	-0.0114	-0.0637***
	(0.0097)	(0.0103)	(0.0103)	(0.0185)
Not married	0.0049	0.0041	0.0044	0.0114

	(0.0049)	(0.0049)	(0.0048)	(0.0121)
Missing: Not married	0.0019	-0.0024	-0.0018	0.0000
	(0.0120)	(0.0109)	(0.0109)	(.)
WIC	-0.0166**	-0.0167**	-0.0170**	-0.0606*
	(0.0084)	(0.0084)	(0.0084)	(0.0316)
Missing: WIC	-0.0009	0.0039	0.0040	-0.0549
	(0.0187)	(0.0265)	(0.0269)	(0.0535)
Medicaid	-0.0044	-0.0040	-0.0034	0.0356
	(0.0091)	(0.0102)	(0.0103)	(0.0250)
Missing: Medicaid	-0.0089	-0.0209	-0.0206	0.0240
	(0.0146)	(0.0219)	(0.0223)	(0.0558)
Constant	0.0006	0.0090	0.0096	0.0206
	(0.0197)	(0.0212)	(0.0205)	(0.0256)
Spatiotemporal intercepts:				
Month	X	X	X	X
Year	X	X	X	X
Site	X	X	X	X
Site × Year		X	X	
Site × 0-250 meters			X	
Site × post-HEUC			X	
Matched sample				X
Observations	8178	8173	8173	1308
Adjusted R-squared	0.016	0.010	0.009	0.055

Note: *p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at 2000 block group level. Dependent variable is a binary indicator, equal to one for children designated as very low birthweight (i.e., less than 1500 grams), and zero otherwise. All independent variables are binary indicators.

Table A6. Two-sample t-tests comparing control and treated groups.

	Control (250-1,000m)	Treated (0-250m)	
	Mean	Mean	t-stat
Black	0.409	0.399	0.32
Hispanic	0.139	0.078	3.54***
Other race/ethnicity	0.204	0.209	-0.19
Male	0.511	0.517	-0.24
Plural birth	0.031	0.035	-0.37
Parents no college	0.888	0.851	1.87*
Mom 15-24 years	0.519	0.531	-0.43
Mom 35-44 years	0.081	0.061	1.46
Smoked	0.157	0.158	-0.05
Second birth	0.300	0.302	-0.10
Third birth	0.180	0.142	1.95*
Fourth birth	0.082	0.078	0.27
≥ Fifth birth	0.055	0.041	1.34
Not married	0.635	0.642	-0.28
WIC	0.604	0.567	0.73
Medicaid	0.643	0.600	0.87

Note: *p<0.10, ** p<0.05, *** p<0.01.

Table A7. Regression models to investigate demographic sorting.

	White (2)	White (4)	Black (2)	Black (4)	Hispanic (2)	Hispanic (4)	No College (2)	No College (4)	Medicaid (2)	Medicaid (4)
0-250 meters	-0.0066	-0.0128	0.1034	0.1067	-0.0872**	-0.1018**	0.0371**	0.0457	-0.0997	-0.1625
	(0.0384)	(0.0479)	(0.0825)	(0.1020)	(0.0412)	(0.0504)	(0.0180)	(0.0279)	(0.1700)	(0.1590)
post-HEUC	0.0026	0.1752*	0.0002	-0.1263	0.0358	-0.0562	0.0167	-0.0901	-0.0516	-0.1057
	(0.0709)	(0.0977)	(0.0659)	(0.0932)	(0.0385)	(0.0747)	(0.0503)	(0.0710)	(0.0928)	(0.1199)
0-250 meters × post-HEUC	-0.0171	-0.0372	-0.1452	-0.1430	0.0997**	0.0542	-0.0923**	-0.0434	0.1259	0.2610
	(0.0694)	(0.0939)	(0.0919)	(0.1178)	(0.0470)	(0.0646)	(0.0465)	(0.0860)	(0.1705)	(0.1724)
Constant	0.2495***	0.1881***	0.4072***	0.4805***	0.1238***	0.1670***	0.8813***	0.8699***	0.6857***	0.6088***
	(0.0304)	(0.0438)	(0.0296)	(0.0485)	(0.0166)	(0.0394)	(0.0168)	(0.0266)	(0.0830)	(0.1200)
Spatiotemporal intercepts:										
Month	X	X	X	X	X	X	X	X	X	X
Year		X		X		X		X		X
Site		X		X		X		X		X
Site × Year	X		X		X		X		X	
Matched sample:		X		X		X		X		X
Observations	6194	798	6194	798	6194	798	7850	1262	2478	383
Adjusted R-squared	0.363	0.325	0.225	0.278	0.111	0.101	0.200	0.249	0.138	0.211

Note: *p<0.10, ** p<0.05, *** p<0.01.