Public Health Policy At Scale: Impact of a Government-sponsored Information Campaign on Infant Mortality in Denmark*

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Abstract

We evaluate the impact of a nationwide public health intervention on deaths from sudden infant death syndrome (SIDS), using population data from Denmark in a regression discontinuity research design. The information campaign–implemented primarily through a universal nurse home visiting program–reduced infant mortality by 17.6 percent and saved between 11.5-14.5 lives over 10,000 births. The estimated effect sizes are 11-14 times larger among low birthweight and preterm infants relative to the overall population. Improvement in infant mortality is concentrated among those with lower socio-economic status and with limited access to health information, thereby reducing health inequities at birth in Denmark.

JEL Classification: I12, I24, I18

1 Introduction

Medical innovations and public health achievements pioneered during the 20th century led to an increase in life expectancy from 32 to over 66 years, an improvement unprecedented in human history.[1] Decline in infant mortality accounts for the largest share in explaining the rise in longevity, brought about by filtering and chlorinating water supplies, sanitation systems, mass vaccination, sulfanilamides and antibiotics, improvements in food safety and nutrition, greater access to health services, and advances in medical technology (Alsan and Goldin 2019; Anderson et al. 2020b, 2021; Cutler et al. 2006; Cutler and Miller 2005; Horton and Steckel 2013; Meckel 1998; Miller and Goldman 2011; Ward and Warren 2006). Equally important have been the diffusion of knowledge gained from these scientific advancements to mass populations and adoption of behaviors promoting infant health, which usually requires well-coordinated, large scale government led campaigns.[2]

Acknowledging the crucial role of increased medical knowledge in obstetrics and pediatrics in reducing preventable deaths among infants, developed countries established public health communication strategies to diffuse practical knowledge to new parents. In this study, we investigate the impact of government-directed and sponsored efforts to communicate newly emerged medical knowledge on infant mortality in Denmark. In 1991, the Danish government issued a new set of guidelines regarding risk factors for sudden infant death syndrome (SIDS), recommending that infants sleep either on their back or side, which went against the then-existing recommendation that had encouraged sleeping on the stomach (Guldager et al. 1990).[3] To communicate the new guidelines to new parents and lower the prevalence of risk factors for SIDS, the government launched a nationwide information campaign in December 1991, primarily using the Danish home nurse visiting program. While information on the new guidelines was also disseminated through the medias and health professional (mainly at maternity wards), an evaluation from 1993 shows that the 62 percent of parents rate the home visiting nurses as the most important source of information with regards to the guideline on sleeping position (Møller et al. 1994). Established in 1937, the home nurse visiting program involves multiple home visits throughout the first year after birth, with more regular visits in the first several weeks after birth. Postnatal home visits by public health nurses administered under the program provided an ideal tool to communicate the updated guidelines due to its capacity to reach all new parents and build a trust-based relationship through repeated visits of the same nurse.[4]

We use a regression discontinuity design that involves comparing all-cause and cause-specific

[1]https://ourworldindata.org/life-expectancy.

[2]The causal estimates that reflect the contribution of at-scale diffusion of scientific knowledge such as the germ theory of disease have been difficult to document due to lack of data, see Cutler et al. (2006); Deaton (2006) for a summary of historical determinants of mortality.

[3]The new guidelines also included recommendations against overheating of infants and exposure to smoking during pregnancy and after birth. However, these additional recommendations were not as central to the guidelines as the sleep position, and parents were not provided with any direction about how to accommodate them.

[4]See Appendix A.2 for a copy of the letter from the National Board of Health to all GPs, maternity wards, hospitals, midwifes, and home visiting nurses dated the 10th of December 1991.

mortality risk for monthly birth cohorts who were born just before and after the unanticipated change in government recommendation and the subsequent information campaign to change parental practices concerning the sleep position of their newborns. Although exact causes are unknown, the majority of SIDS deaths occur before the age of 4 months, which provides an opportunity to observe the immediate impact of changing sleeping environment on infant health (American Academy of Pediatrics 2011).

We show that the nationwide information campaign was highly effective in reducing infant mortality, especially among infants with poor baseline health and lower socioeconomic status. Our estimates suggest that the campaign reduced infant mortality rate by 17.6 percent, which can explain around one quarter of the overall decrease in infant mortality over the past 40 years in Denmark. Decrease in deaths from SIDS and unknown causes fully account for the overall drop in infant mortality. We estimate that the intervention reduced infant mortality for low birthweight and preterm children by 1.5 and 1.8 percentage points, which is 11 and 14 times larger than its impact on the overall population, respectively. Furthermore, the intervention was most effective in improving the health of infants from immigrant or lower educated mothers. These results underscore the effective role that an information-based, large scale public health intervention can play in narrowing early life health disparities.

Our analysis contributes to two strands of literature. First is the literature investigating the impact of public information campaigns on health behaviors. These investigations focus on the role of information diffusion on influencing the public opinion and health behavior, such as energy conservation, immunization, breastfeeding, dietary habits, smoking and alcohol consumption, etc. (Weiss and Tschirhart 1994; Olds et al. 2007). However, most of the existing evidence come from contexts in which the campaigns are limited in scope or targeted at specific groups. Second, we contribute to the literature investigating the interplay among public health interventions, mortality transition, and health inequalities in high income countries (Cutler and Miller 2005; Cutler et al. 2006; Watson 2006; Wüst 2012; Moehling and Thomasson 2014; Komisarow 2017; Alsan and Goldin 2019; Anderson et al. 2019; Feigenbaum et al. 2019; Anderson et al. 2020a).

While prior literature documents the declines in SIDS deaths through public health policy campaigns with varying success in Scandinavia and other high income countries, these analyses are typically based on crude trend evaluations and case control studies relying on small samples (Wennergren et al. 1997; Hauck and Tanabe 2008; MacDorman et al. 2013; Goldstein et al. 2016). As we show in the empirical section, changing measurement error in SIDS classification over time and the contemporaneous declining trends in infant mortality cannot be accounted for by a descriptive investigation. Thus, the inferences drawn from these studies reflect an incomplete picture of the true effect of specific interventions on infant survival. Our research design overcomes these challenges by isolating the impact of a specific nationwide information campaign within a narrow time frame and using outcomes that are not prone to typical diagnostic challenges to classifying SIDS (Hauck and O Tanabe 2010; Hauck and Tanabe 2008). Perhaps more importantly, we leverage population-level data to precisely estimate the impact of an at-scale intervention on vulnerable subpopulations, which shows evidence of substantial benefits in targeting those with poor baseline health, lower socioeconomic status, and thus limited access to useful medical information. Countries with constrained resources or unequal access to health knowledge might benefit from this type of targeted interventions to reduce infant mortality among susceptible populations and improve early life health disparities. Our findings are particularly relevant for the United States, that currently has a higher infant mortality rate than European countries driven by higher postneonatal mortality (1–12 months after birth), a period in which SIDS is still the leading cause of death (Carlin and Moon 2017; Chen et al. 2016).

2 Country Setting

2.1 Infant Mortality and SIDS in Denmark

Infant mortality in Denmark decreased from 134.2 over 1000 live births in 1901 to 20 in 1962, and eventually to only 3.2 in 2019.[5] Until the 1950s, unexplained deaths constituted a small fraction of the overall infant mortality, but as deaths due to infections and other major causes continuously declined over the second half of the century, unexplained infant deaths slowly shifted from the periphery to the center of public health policy (Helweg-Larsen and Guldager 2001b). Formally defined as a cause of death in 1969, reported SIDS rates steadily increased over the next 20 years in many developed countries, including Denmark, eventually making SIDS the most significant post-neonatal risk for infant mortality in industrialized countries (De Jonge et al. 1989; Dwyer and Ponsonby 2009). Research in Denmark suggests that the changes in cause of death classifications and the previous guidelines that recommended sleeping on stomach might explain the rise of SIDS deaths (Helweg-Larsen et al. 1992).

While the association between prone sleeping position and SIDS was known as early as the 1970s, the full recognition of medical community and the following policy action did not take place until early 1990s, after multiple case control, cohort, and observational studies from the UK, Netherlands, Australia, and New Zealand (De Jonge et al. 1989; Dwyer et al. 1991; Fleming et al. 1990; Mitchell et al. 1991). This prompted a series of information campaigns across developed countries, stressing prone sleep position as a high risk factor for SIDS and thus recommending supine sleeping. In Denmark, the recognition of this new scientific information occurred in December 1991 when the National Board of Health, in a reversal of its previous position, issued revised guidelines, recommending that infants sleep either on their back or side. Concomitantly, the government launched an information campaign with a key role assigned to the home visiting nurse program. The news on the campaign was also also circulated in the media, initiated through a press release from the National Board of Health (Guldager n.d.; Helweg-Larsen and Guldager

[5]Estimates between 1901 to 1962 come from Matthiessen (1967) and 2019 from World Bank Open Data (`https://data.worldbank.org/indicator/SP.DYN.IMRT.IN?locations=DK`.

2001a).[6] Specifically, the Danish National Board of Health published a special issue in the Public Health Nurses trade magazine in early 1992, which translated the new evidence on preventive measures related to SIDS and the content of the new guidelines.[7] It also included a description of the public health nurses' role in the implementation of the guideline related to sleeping position. The special issue was delivered to all public health nurses and obstetric nurses together with a pamphlet for the parents that included a list of the recommendations regarding the importance of sleeping on the back as well as avoidance of smoking and overheating. The parents were informed of the new guidelines through material delivered by the healthcare personnel, mainly the public health nurses.[8]

While we do not have data to assess the impact of the campaign on parental practices concerning the sleep position of their babies, the Danish National Board of Health evaluated the information campaign in five regions in Denmark in 1993 and determined that the vast majority of parents complied with the new sleeping guidelines. A survey conducted by the evaluation team indicated that while almost all parents had been aware of the importance of supine sleeping position, only about half of the parents had been informed about the guidelines on smoking and overheating. Furthermore, the majority of the parents reported that they had received the information from the public health nurse or the public media. By 1993, approximately 13 percent of infants of parents with more than one child were sleeping on their stomach, corresponding to a 38.2 percentage points decrease from the pre-1991 levels (Møller et al. 1994).[9]

3 Data

We use several Danish population registries that are linked through a unique identifier to construct a data set of all live births from 1973 through 2006. Information on birth weight, gestational age, and parity are obtained from the Danish Medical Birth Registry (DMBR).[10] Maternal age and immigrant status are also obtained from DMBR. Mortality data including the exact date and cause of death come from the Danish Registry of Cause of Death (Helweg-Larsen 2011).[11] Information on parental education comes from the Danish Education Registry. We categorize mothers to those with (i) basic education (12 years or less of formal education), (ii) vocational education (vocational

[6] However, the media coverage of the information campaign appears limited. A search of the newspaper archives of the Danish nationwide media identified 15 SIDS-related articles in 1991, only two of which were related to the new guidelines on sleeping. There were ten SIDS-related articles in 1992, only one of which mentioned sleep position as an important factor for SIDS.

[7] In Danish: Fagtidsskriftet Sundhedsplejersken.

[8] This material included the pamphlet aimed at the parents and two publications "Barn i vente" (in English: Expecting a child) and "Sunde børn" (in English: "Healthy Children").

[9] Using data on smoking behavior that are available beginning 1991, appendix Figure B1 shows that smoking during pregnancy continually decreased around the time of the change in guidelines with no apparent discontinuity in December 1991. We performed a regression discontinuity analysis using a bandwidth of 12 months on smoking behaviour during pregnancy and did not find a statistically robust effect of guidelines on smoking.

[10] We exclude observations with no information or unrealistic values for birthweight and height.

[11] Unique identifiers in the registries link parents and children.

training equivalent to high school, and (iii) further education (women with any post-high school degree). We provide a detailed description of the variables used in the analysis in Appendix Table C1.

4 Empirical Design

We use a regression continuity design based on birth month and year cutoff, which creates a quasi-random variation in exposure to the drastically different sleep guidelines provided by the government. In particular, our empirical strategy anchors on the notion that children who were born after December 1991 were exposed to an at-scale information campaign that aimed to reverse the sleep practice of newborns in Denmark.

Formally, our research design can be expressed by the following empirical specification:

$$y_i = \alpha + \tau d_i + f(s_i) + \epsilon_i \tag{1}$$

$$\forall s_i \in (c - h, c + h)$$

where y_i indicates mortality outcome for infant i, d_i is a binary treatment indicator for cohorts born in January 1992 and later, and s_i is the running variable and calculated as the number of months between the child's birth month and year and the end of December 1991. We fit two continuous functions $f(s_i)$ on each side of the regression sample, which includes infants who were born h months before and after the change in guidelines, using an automated routine of optimal bandwidth selection that minimizes the mean-squared error (MSE) following Calonico et al. (2019). We also present results from a wide range of alternative bandwidths to test the sensitivity of our estimates with respect the bandwidth choice. Standard errors are clustered at the birth month-year level to account for within birth cohort correlations in outcomes. Finally, we use a uniform kernel to weight the observations in our regression sample.

In this setting, τ captures the intent-to-treat (ITT) effects of a change in sleep position achieved through a nationwide public information campaign. Causal interpretation of the estimated τ further hinges on the following assumptions: (i) quasi-random assignment, i.e. cohorts who were born just before and after the change in guidelines are exposed to different sleep environments but are otherwise comparable in their pre-treatment characteristics; and (ii) exclusion restriction, i.e., there are no other policies that were implemented around the same time and could also generate similar discontinuous mortality risk across birth cohorts born around December 1991. We take advantage of the population-level administrative data to assess the validity of these assumptions. First, we analyze whether a set of predetermined covariates including strong predictors of infant mortality are continuous around the threshold to confirm quasi-random assignment of exposure to policy. Second, we use cause-specific mortality data to confirm that any sharp drop in infant mortality is exclusively driven by SIDS and other unclassified deaths and test whether excluding those leads to a null treatment effect. This provides a powerful empirical test on exclusion restriction, given the statistical power provided by the population-level data, as well as the existing medical evidence that having infants sleep on their back is the most important risk-reducing practice against SIDS (Dwyer and Ponsonby 2009).

Although the newly emerged medical evidence was known even before December 1991, we expect the government reach-out effort to have created meaningful exposure differences in sleep practice among birth cohorts who were born before and after the information campaign for several reasons. First, as discussed in Section 2, the focus of the reach-out efforts by the public health authorities was pregnant women and parents who gave birth after the change in childcare guidelines. Following the change in policy, parents received the updated instructions on sleep position for infants mainly from the health personnel at the maternity wards and through home visits by a public nurse (Iversen 2017; Møller et al. 1994). Second, as documented in experimental interventions to promote healthy habit formation, changing an existing health behavior is often challenging without the right incentives (Loewenstein et al. 2016; Hussam et al. 2017). This is particularly salient in our setting, where getting parents to comply with the guidelines may be more challenging given that sleeping on their stomach is viewed as more comfortable for many infants as they wake up less frequently at night. (Horne et al. 2001; Oster 2020, pp. 111-112).

The τ may be be biased towards zero if children who were born before December 1991 benefited from the information campaign as much as those who were born after the policy change. An infant who was born in October 1991, could be affected by the policy when she was 2 months old but would contribute to the estimates in our control group. In that case, τ would reflect a lower bound estimate of the true treatment effect, which includes policy spillovers on non-targeted group. Since SIDS risk is highest in the first four months after birth (American Academy of Pediatrics 2011), we offer a direct empirical test to assess the potential bias due to policy spillover by comparing the estimated τ from a regression sample that includes the full population to the one that excludes four cohorts who were born between September 1991-December 1991. If the full sample estimates for τ includes a downward biased due to policy contamination among earlier cohorts, the "donut" RD estimates is expected to be considerably larger in magnitude for infant mortality and remain statistically zero once the outcome excludes deaths due to SIDS and other unclassified causes.

5 Results

5.1 Descriptive Analysis and Preliminary Tests

We begin with a descriptive analysis of our main outcomes in Figure 1, and provide the changes in all-cause infant mortality and SIDS rates over 10,000 live births for infants born between 1973 and 2006. With the exception of 1980s, infant mortality rate continuously fell throughout the analysis period. The drop in the rate is particularly dramatic in early 1990s during which it plummeted from around 80 per 10,000 in its 1980 levels to 50 per 10,000, corresponding to a 35-40 percent decline within only four birth cohorts between 1991 and 1994.

Figure 1 Panel B shows (i) the decline in infant mortality during the early 1990s shown in Figure 1, Panel A, is primarily driven by an abrupt decrease in SIDS deaths, and (ii) for infants born between the early 1970s and 1990, the SIDS rate increases gradually despite a decreasing trend in overall infant mortality. For the most part, this increase is attributable to improved classification of cause of death, though epidemiological literature also points to the previous clinical guidelines that advised the parents to put their baby to sleep on their stomach during 1980s (Helweg-Larsen and Guldager 2001b). Crude trend suggests that birth cohorts born after the new guidelines were introduced in 1991 experienced a 3 to 4-fold decrease in SIDS-related mortality risk, which remained stable near zero for cohorts born in the 2000s. Due to confounding secular trends in infant mortality and the time-varying measurement error in SIDS classification, however, it is difficult to quantify the true effect of a policy on infant mortality through a trend analysis without additional strong assumptions.

To assess the internal validity of our research design, we first test whether cohorts born right before and right after introduction of new sleep instructions differ in their observed characteristics. In Table 1, we report the RD estimates using Equation 1, which reflects the differences in observed predetermined characteristics of the treatment and control groups for a wide range of bandwidths. The first column shows results of the continuity tests for the optimal bandwidth obtained using the Calonico et al. (2019) routine. The estimates in the remaining columns were obtained from samples restricted from 24 to 72 months below and above the index threshold cohort of December 1991. Given the very large size of our sample, the estimates from the balancing tests are unsurprisingly precise at conventional levels. However, the coefficient sizes are inconsequentially small in all cases. Thus, we caution against solely relying on statistical significance in interpreting the results of the balancing tests.

The results in Table 1 support the assumption of quasi-random assignment of the treatment status around the vicinity of the RD threshold after accounting for the secular linear trends on either side of it. The residual differences between the treatment and control groups are small and most of them are not statistically significant, despite huge sample sizes. The only consistent difference that we can detect is on birth order coefficients, which suggest that the treatment group's birth order is 0.018 higher, on average, than the control group, which has a mean birth order of 1.76 (Table 1, column 1). While the sign of the coefficient suggests a *negative* selection into treatment, its size is not clinically meaningful enough to have any impact on infant mortality.[12]

In Appendix Figure B2, we report the graphical representation of these estimates generated using the optimal-bandwidth. As revealed by the figure, the treatment assignment does not show any meaningful jumps around the policy threshold. The visual inspection also confirms the findings in Table 1, showing that month-year birth cohorts who were born around the drastic policy change have very similar baseline health and maternal characteristics. Given that some of

[12]As expected, controlling for these variables makes no difference to our reported results. See Table C2.

these covariates are strong predictors of infant mortality, the balance of these covariates across birth cohorts supports the internal validity of our research design.

Note that because we use population-level administrative data, there is no measurement error in our running variable that could result from self-reported date of birth. Given the cohort-based structure, it is also inconceivable that some parents might have manipulated the timing of their childbearing in our context. Therefore, endogenous selection to analysis sample or bunching near the cut-off do not constitute threats to the validity of our research design. These arguments are also supported by Figure B3, which shows no change in population composition around the threshold that could explain the dramatic drop in mortality.[13]

5.2 RD Estimates on All-cause and SIDS-specific Mortality

Figure 2 shows the RD graphs from an analysis sample restricted by Calonico et al. (2019)'s MSE-optimal bandwidth for our main outcomes. In panel (a), the SIDS mortality rate exhibits a clear break towards zero immediately following the change in guidelines. Panel (b) includes deaths from SIDS and unclassified causes, and here a similar break is observed. We quantify these breaks in the first column of Table 2, which indicates a decrease of 11.5 SIDS deaths per 10,000 live births from a control group mean of 17.8 SIDS deaths per 10,000 live births. Effect sizes are robust to various sample restrictions and range between 10.5 and 12.2 based on the distance in birth months to the index cohort, which varies between 24 and 72.

All-cause infant mortality in panel (c) of Figure 2 also shows a clear break in infant mortality immediately after the change in recommended sleep position. The coefficient size indicates that 13.5 deaths per 10,000 live births were averted due to change in public health guidelines, which is slightly more than the estimates for the SIDS-specific mortality rate (Table 2, column 1). The point estimates are remarkably robust to bandwidth selection and vary between 12.9 and 14.5, despite the dramatic change in sample sizes. The relative effect sizes are large and indicate a 17.6 percent decrease in infant mortality compared to the control mean of 76.68 deaths per 10,000 live births. In Figure 2, we provide similar evidence for under-five mortality, which shows a reduction of 15 deaths over 10,000 live births (16.5 percent) induced by the government-led information campaign against SIDS.

In Figure 2, we also provide RD estimates for infant and child mortality from all causes except SIDS and other uncategorized deaths. The rationale is to eliminate the possibility that any other changes in medical knowledge or technology that occurred during the same period might have differentially affected the health of infants in our treatment group, causing spurious correlation between the new guidelines and infant mortality. Non-SIDS-related infant and child mortality rates in Figure 2 (e) and (f) both exhibit a continuous downward secular trend with no sign of a

[13]Because we use population data, at any randomly assigned threshold between -100 and 100 in the lower panel of Figure B3, a standard McCrary (2008) test is more than 60 percent likely to reject the null hypothesis of no break. As a result, we avoid using a formal test of break. The results of this simulation are not reported, but they are available from the authors upon request.

break around the period of change in sleep recommendation. The RD treatment effect estimates in Table 2 indicate that these secular trends are fully captured by the local linear trend fits and report precisely estimated null effects for both infant and child mortality of all known causes but SIDS and other uncategorized deaths. These estimates constitute a powerful test of the exclusion restriction because known causes of deaths excluding SIDS still constitute the majority of infant deaths and show no sign of change among cohorts who were exposed to the new sleep guidelines. Therefore, we conclude that our main estimates on infant and child mortality are entirely driven by the government-led public health information campaign aimed at changing the sleep position among newborns.

5.3 Subpopulation Analysis

Given the well-documented differences in mortality risk based on health at birth and socio-economic factors, we expect the provision of health knowledge to the population to also differ by baseline health and socio-economic background. In particular, we expect that the new guidelines are more likely to be binding for infants with a high baseline mortality risk and those born to parents who are less equipped with health information to begin with. Accordingly, the updated advice on infant sleep position for parents whose babies otherwise have a high risk of mortality should have a stronger response to policy.

Table 3 provides the RD treatment estimates for these subgroups. The results indicate substantial benefits of changing the sleep position for infants with low birthweight and preterm infants. The first panel in Table 3 suggests that the rapid diffusion of new health knowledge saved between 119 and 182 lives per 10,000 births among those with low birth weight and 137 and 198 per 10,000 lives among preterm infants. These are very large absolute effects, and translate into a decrease in the infant mortality rate by 17.3 and 32 percent compared to the baseline mean among the control infants, respectively. These estimates indicate that the information campaign was particularly effective among parents of infants who were born with poor health. The large absolute and relative coefficient sizes further underscore that the policy was also effective in narrowing the early life health disparities that start at birth.

Panel II of Table 3 provides the RD estimates for boys and first children only. These results suggest slightly larger effects on boys and somewhat larger effects on second and higher birth order children. The estimates in Panel II align with the existing medical literature that documents the differential infant mortality risk of all major causes in favor of girls and earlier born siblings. Effect sizes indicate from 13.2-18 saved lives per 10,000 boys and a marginally significant 9.2-11.2 saved lives per 10,000 first-borns.

Panel III of Table 3 report the estimated coefficients by maternal characteristics. When grouped by mother's education, the subgroup estimates show that the estimated effects of the information campaign on infant mortality are entirely driven by mothers with relatively few years of education or a vocational education. None of the estimates for infants with mothers with any post-high school degree are statistically different from zero, and coefficient sizes are small. The same coefficients for infants with a mother with basic or vocational education, however, show a consistent improvement in mortality with similar effect sizes; both are slightly larger than the estimated aggregate ITT effects in Table 2. We conclude that the information campaign was most effective among mothers with relatively fewer years of education. Estimates in Table 3 suggest large reductions in infant mortality among children of immigrant mothers. For example, the estimate from the sample with MSE-optimal bandwidth shows 35.9 averted deaths per 10,000 births, corresponding to a 42.8 percent decrease in infant mortality among this demographic group. We show the RD graphs that depicts these estimates in Appendix Figures B4 and B5. Overall, the subgroup analysis shows that providing newly emerged health knowledge was particularly beneficial for those who were susceptible to adverse health outcomes as well as for those with lower socio-economic status and limited access to information.

5.4 Robustness Tests

In addition to a wide range of bandwidth intervals, we further assess the robustness of our estimates to controlling for covariates,[14] the kernel used to weight our regressions as well as the the form of the control function in Appendix Tables C2, C3, and C4, respectively. We conduct these robustness checks for all outcomes and bandwidths that are reported in Table 2, which essentially replicates our main results with different regression parameters. To ease the comparison of sensitivity estimates, Figure B6 shows a graphical representation of 120 treatment effect estimates across six outcomes, five bandwidths, and four specifications. The gray area in Appendix Figure B6 depicts the MSE-optimal bandwidth range, which constitutes our preferred specifications.

As illustrated in Appendix Table C2, our estimates are also robust to controlling for a set of covariates, with the exception of a loss in precision in few specifications with very small bandwidths due to reduced sample size and the use of birth of month fixed-effects. The results in Appendix Table C3 indicate that using a triangle kernel to weight our regressions makes little difference in our estimates. In Appendix Table C4, we show that the use of quadratic control function, as is typical in RD studies, provide similar but less precise point estimates. In Appendix Figure B6, we show that none of the choices that we make in our regression framework produce statistically different estimates. In addition, we show that the point estimates are particularly robust for the Calonico et al. (2019)'s MSE-optimal bandwidth choices. We interpret these results as a further confirmation of the internal validity of our analysis.

To further test the sensitivity of our results to misclassification in the treatment status of these babies due to policy spillover to babies born before December 1991, as explained in section 4, we obtained our regression discontinuity estimates excluding the birth cohorts born between September 1991 and December 1991. As shown in Appendix Table C5, these results are statistically

[14]Covariate-adjusted specifications control for gender, month of birth, and birth order fixed-effects, birth weight (in natural logarithm), gestation (in weeks), dummy variables for mother's education and immigrant status.

identical and similar in magnitude to our full sample estimates. Importantly, we find null effects from the same sample when the outcome excludes SIDS and other unclassified deaths, which suggests that in the absence of the policy change, mortality would still be continuous even after removing four monthly birth cohorts around the threshold.

Finally, it could be argued that some mothers might have quit smoking in response to the information campaign, which might have then increased the gestational age of their babies, resulting in a potential endogeneity in the date of birth for children who were born after the change in guidelines. As discussed earlier and shown in appendix Figure B1, smoking does not appear to play a role in explaining our results. In Appendix Table C6, we report estimates from a fuzzy regression discontinuity design where we construct the running variable using the *expected* birth month-year[15] and use the expected policy exposure as an instrument for the actual policy exposure. Despite adding a fair amount of measurement error to the running variable, our results are still robust to treating the birthdate as endogenous.

6 Conclusion

This paper demonstrates that basic health advice delivered to public primarily via a universal home visiting program had a profound effect on reducing infant mortality in Denmark. Our results show that the public health information campaign is responsible for one quarter of the decline in infant mortality over the past four decades.[16] The decrease in infant mortality is entirely driven by a drop in SIDS and other unclassified deaths, which is consistent with the fact that the information campaign mainly aimed at reducing prone infant sleep position – the primary risk factor for SIDS. Furthermore, we find substantial heterogeneity in the effectiveness of the public information campaign by health at birth and socioeconomic background. In particular, infants with a poorer baseline health and those born to socioeconomically more deprived parents experienced profoundly stronger benefits in terms of the prevalence of SIDS-related deaths.

Despite the breakthroughs in medical technology and developments in new treatments, a significant number of children continue to die every year, not because of a lack of access to advanced technology or effective treatments, but due to continued infant-care practices that place children at a higher risk of death. For example, SIDS, which can be prevented by simple parental actions, constitutes the leading cause of death among infants between one month and one year of age in the United States. It is also one of the leading sources of ethnic and racial inequality in child survival. In 2015, for example, prevalence of nonsupine sleep position among black children was almost 38 percent, that is 16 percentage points higher than their white peers (Bombard et al. 2018). Not surprisingly, ethnic and racial disparities in child care are also directly reflected in SIDS rates. In fact, similar persistent and even growing divergencies in health by socioeconomic status

[15] Calculated by adding 280 to the date of conception.

[16] Between 1979 and 2019, infant mortality decreased from 8.5 to 3.2 per 1,000 in Denmark, whereas our preferred estimate of the policy impact is 1.35, which roughly corresponds to $\frac{1.35}{(8.5-3.2)} \approx 25\%$ of the overall decrease.

are emerging in developed countries, including those with universal health insurance programs (Elo and Preston 1996; Pappas et al. 1993; Mackenbach et al. 2003). Due to incremental changes in medical technology, health behaviors are playing a decisive role in increasing health disparities. The results of this paper represent a clear demonstration that well-targeted behavioral interventions implemented at scale can effectively reduce health disparities that tend to persist despite better technology and access to healthcare.

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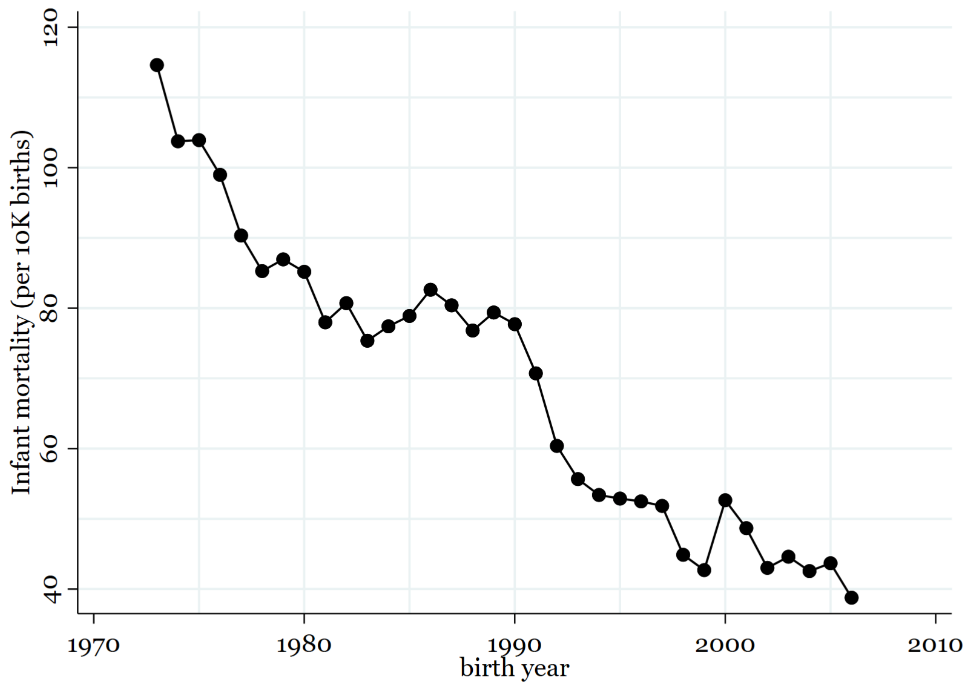
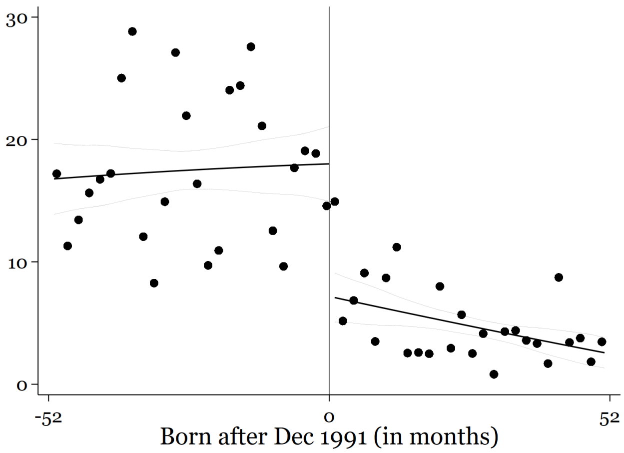
Figure 1: Trends in infant mortality



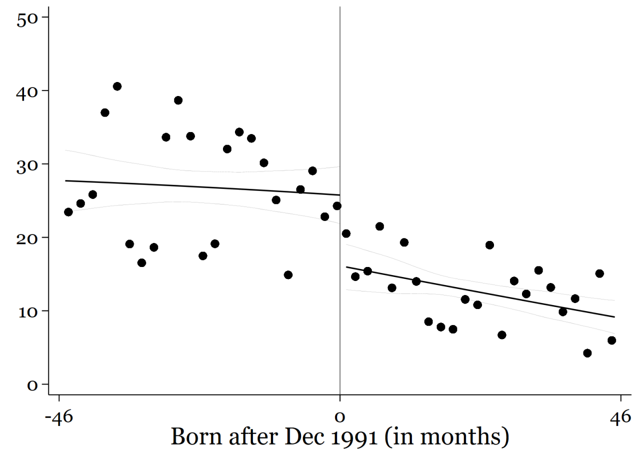
(a) All-cause infant mortality rate

(b) SIDS mortality rate

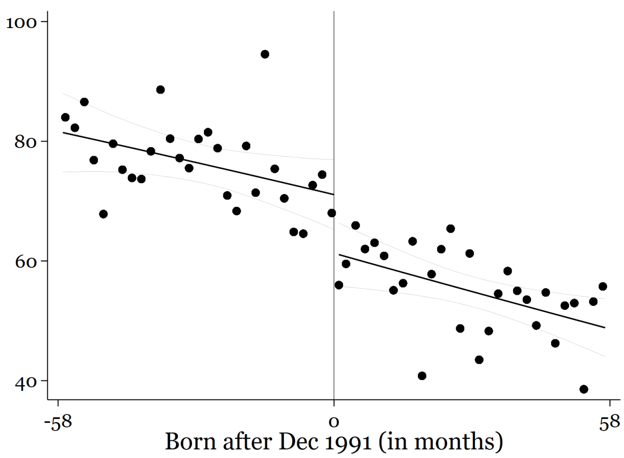
Figure 2: RD estimates on Infant and Child Mortality



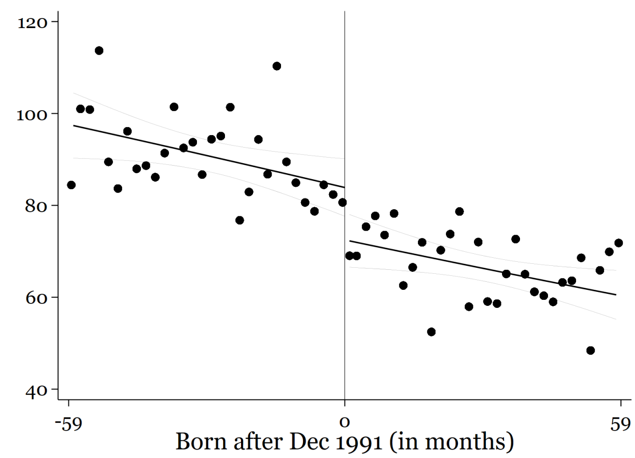
(a) SIDS mortality



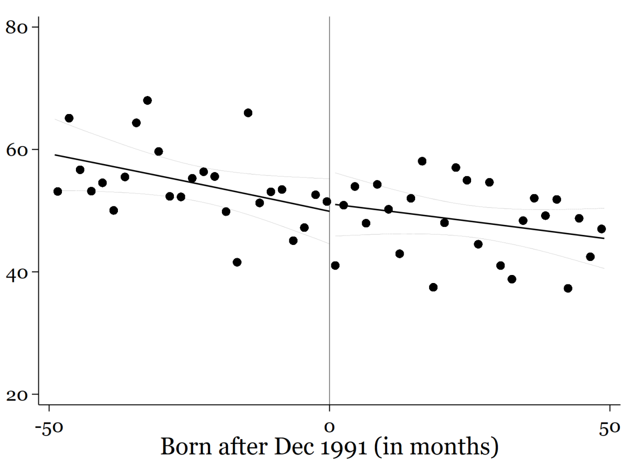
(b) SIDS + all unclassified mortality



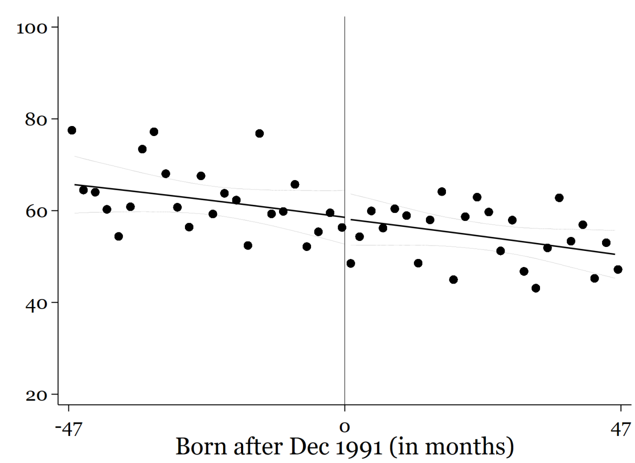
(c) Infant mortality



(d) Child mortality



(e) Infant mortality excluding SIDS + unknown



(f) Child mortality excluding SIDS + unknown

Table 1: Balance of Covariates

	MSE-optimal	±24	±36	±48	±60	±72
Female						
Born after Dec. 1991	-0.001	-0.003	-0.003	-0.001	-0.001	-0.001
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Bandwidth	57	24	36	48	60	72
Observations	621064	267863	399017	527257	650958	773819
Control group mean	0.49	0.49	0.49	0.49	0.49	0.49
Birth order						
Born after Dec. 1991	0.018***	0.033***	0.023***	0.019***	0.027***	0.029***
	(0.005)	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)
Bandwidth	46	24	36	48	60	72
Observations	508338	268228	399591	528021	651926	774948
Control group mean	1.74	1.75	1.74	1.74	1.74	1.74
Low birthweight						
Born after Dec. 1991	0.002	-0.002	0.001	0.002	0.000	0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Bandwidth	47	24	36	48	60	72
Observations	515665	266927	397656	525644	649021	769978
Control group mean	0.05	0.05	0.05	0.05	0.05	0.05
Preterm birth						
Born after Dec. 1991	0.003*	0.001	0.002	0.003**	0.001	0.004***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Bandwidth	39	24	36	48	60	72
Observations	426422	265318	395363	522976	645101	762073
Control group mean	0.05	0.05	0.05	0.05	0.05	0.05
Mother's age at birth						
Born after Dec. 1991	0.037	0.011	0.032	0.032	0.063***	0.066***
	(0.026)	(0.037)	(0.03)	(0.026)	(0.023)	(0.021)
Bandwidth	49	24	36	48	60	72
Observations	537683	268228	399591	528021	651926	774948
Control group mean	27.79	27.94	27.87	27.8	27.74	27.67
Mother with basic education						
Born after Dec. 1991	0.002	-0.003	0.001	-0.001	0.000	-0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Bandwidth	43	24	36	48	60	72
Observations	474641	267581	398629	526681	650208	772744
Control group mean	0.33	0.32	0.33	0.34	0.35	0.35
Mother with further education						
Born after Dec. 1991	-0.001	-0.002	-0.002	0	-0.001	-0.004**
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Bandwidth	44	24	36	48	60	72
Observations	485728	267581	398629	526681	650208	772744
Control group mean	0.26	0.26	0.26	0.26	0.26	0.26
Mother immigrant						
Born after Dec. 1991	0.000	-0.002	0.001	0.002	0.001	-0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Bandwidth	33	24	36	48	60	72
Observations	366969	267581	398629	526681	650208	772744
Control group mean	0.08	0.08	0.08	0.08	0.07	0.07

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on predetermined covariates. See Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

Table 2: Effect of Guideline Changes on Infant and Child Mortality

	MSE-optimal	±24	±36	±48	±60	±72
Infant Mortality (per 10K births)						
Born after Dec. 1991	-13.465***	-14.523**	-11.481**	-12.953***	-13.721***	-13.858***
	(4.029)	(6.298)	(5.127)	(4.428)	(3.971)	(3.63)
Bandwidth	58	24	36	48	60	72
Observations	632449	268228	399591	528021	651926	774948
Control group mean	76.68	74.19	75.88	76.1	76.9	77.78
SIDS Mortality (per 10K births)						
Born after Dec. 1991	-11.507***	-10.551***	-10.34***	-11.511***	-11.403***	-12.165***
	(1.746)	(2.656)	(2.123)	(1.826)	(1.624)	(1.459)
Bandwidth	52	24	36	48	60	72
Observations	568106	268228	399591	528021	651926	774948
Control group mean	17.8	17.77	17.92	17.87	17.95	17.64
Infant mortality excluding SIDS						
Born after Dec. 1991	-0.84	-4.457	-0.171	-0.595	-1.077	-0.056
	(3.859)	(5.547)	(4.538)	(3.931)	(3.528)	(3.233)
Bandwidth	50	24	36	48	60	72
Observations	547850	268228	399591	528021	651926	774948
Control group mean	54.99	52.59	54.41	54.89	55.62	56.86
Child Mortality (per 10K births)						
Born after Dec. 1991	-15.002***	-13.85**	-12.73**	-13.863***	-14.694***	-13.809***
	(4.377)	(6.872)	(5.593)	(4.83)	(4.345)	(3.97)
Bandwidth	59	24	36	48	60	72
Observations	641982	268228	399591	528021	651926	774948
Control group mean	91.09	87.97	89.52	89.85	91.36	92.66
Child mortality excluding SIDS						
Born after Dec. 1991	-2.52	-6.853	-2.272	-2.565	-2.568	-0.728
	(4.239)	(5.949)	(4.846)	(4.194)	(3.771)	(3.452)
Bandwidth	47	24	36	48	60	72
Observations	517997	268228	399591	528021	651926	774948
Control group mean	63.29	61.44	62.92	63.23	64.44	65.95

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant and child mortality. See the Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level, and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

Table 3: RD Effects of Guideline Changes on Infant Mortality: Subgroup Analysis

	MSE-optimal	±24	±36	±48	±60	±72
I. Child's Health at birth						
Low birth weight						
Born after Dec. 1991	-152.688***	-124.568	-130.499**	-181.119***	-143.758***	-118.74**
	(50.247)	(79.842)	(65.884)	(56.763)	(51.136)	(47.114)
Bandwidth	63	24	36	48	60	72
Observations	34823	13654	20399	26956	33330	39629
Control group mean	670.67	666.15	680.64	654.9	666.71	684.84
Preterm						
Born after Dec. 1991	-182.749***	-198.198***	-151.493**	-189.141***	-144.447***	-136.899***
	(55.933)	(73.098)	(60.583)	(52.081)	(46.986)	(43.816)
Bandwidth	42	24	36	48	60	72
Observations	25392	14774	21825	29106	36138	42190
Control group mean	634.19	619.86	644.29	626.14	645.3	667.86
II. Child demographics						
Male						
Born after Dec. 1991	-16.238***	-14.798	-13.205*	-17.968***	-16.034***	-16.28***
	(5.659)	(9.355)	(7.613)	(6.53)	(5.89)	(5.362)
Bandwidth	65	24	36	48	60	72
Observations	361019	137629	204745	270868	334693	397824
Control group mean	87.96	85.03	86.87	85.31	87.65	88.04
First child						
Born after Dec. 1991	-9.413	-11.225	-11.219	-10.08	-9.568*	-9.218*
	(6.07)	(9.013)	(7.365)	(6.386)	(5.71)	(5.242)
Bandwidth	53	24	36	48	60	72
Observations	266899	124396	184866	243696	300933	356996
Control group mean	71.34	70.28	70.34	71.31	71.92	73.15
III. Maternal characteristics						
Mother has basic education						
Born after Dec. 1991	-15.933**	-21.813*	-14.872	-18.684**	-18.445**	-15.639**
	(7.398)	(13.179)	(10.557)	(9.071)	(8.124)	(7.45)
Bandwidth	73	24	36	48	60	72
Observations	243671	82862	123289	163212	202270	240797
Control group mean	96.83	98.94	97.85	95.25	95.31	97.01
Mother has HS/ voc. education						
Born after Dec. 1991	-19.527***	-20.572**	-22.578***	-19.527***	-17.228***	-17.622***
	(6.505)	(9.118)	(7.549)	(6.505)	(5.832)	(5.326)
Bandwidth	48	24	36	48	60	72
Observations	224291	114605	170290	224291	275286	325448
Control group mean	71.02	69.87	70.6	71.02	71.87	71.85
Mother has further education						
Born after Dec. 1991	3.788	5.007	11.738	5.557	-1.182	-4.846
	(7.765)	(10.704)	(8.747)	(7.645)	(6.889)	(6.292)
Bandwidth	46	24	36	48	60	72
Observations	134018	70114	105050	139178	172652	206499
Control group mean	57.71	50.8	56.71	58.93	59.92	60.35
Mother is an immigrant						
Born after Dec. 1991	-35.922**	-16.319	-37.257*	-35.043**	-28.682**	-21.664*
	(16.771)	(23.731)	(19.171)	(16.024)	(14.278)	(13.065)
Bandwidth	45	24	36	48	60	72
Observations	41882	22816	33788	44481	55500	66736
Control group mean	83.84	97.25	89.55	81.31	80.99	82.57

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant mortality for subgroups. See Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

Appendix A Additional Material on Home Visiting Program

A.1 Danish Home Visiting Program

The Danish Home Visiting Program was established by the National Board of Health under legislation enacted in 1937. Initially established as a recommended program, the 1937 legislation was later integrated into the Public Health Nursing Services Act in 1963, which stated that municipalities "ought" to establish the service, but still did not make it compulsory (Kamerman and Kahn 1993). Although most local jurisdictions adopted the program, it was not until 1973 that it became mandatory, after which time all municipalities had to offer visits to new families.[17] From 1973 through 1995, the organization of the public health nurses was assigned to a leading public health nurse in the region.[18] According to the guidelines, first-time parents could be visited by a public health nurse around nine times between the time of birth and the start of school, while parents with more than one child could receive up to seven visits, depending on their needs (Danish National Board of Health 1985; Danish Nurses' Organization 2018). The public health nurses reaches almost 100 percent of the population.

The program reached almost all newborn children by 1962 and involved multiple home visits in the first year after birth, with more regular visits in the first few weeks after birth. At its initial stages, visiting public health nurses routinely collected anthropometric measurements, provided physical examination, and, if necessary, physician referral, and informed parents about the pediatric guidelines on infant nutrition and child care (Matthiessen 1967). The program has evolved over time, but its core services remain the same, i.e. informing parents about childcare guidelines, which are routinely updated with new medical evidence. One such important update occurred in December 1991, with a drastic change in the recommended sleeping position for infants from "on the stomach" to "on the back or the side", which was shown in case control studies to be highly effective in mitigating the SIDS risk (Fleming et al. 1990; Mitchell et al. 1991; Ponsonby et al. 1993; Dwyer et al. 1995). Postnatal home visits by public health nurses provided an ideal tool to communicate the updated guidelines due to its capacity to reach all new parents and build a trust-based relationship through repeated visits of the same nurse.

The public health nurses have two distinct roles: (i) to offer care and support to the families and promote health, with a specific focus on breastfeeding (ii) to measure the children and check that the parents follow guidelines and report back to the authorities if they notice any maltreatment (Sixhøj 2001). While it can be difficult to unite these two roles, this problem seems partly to be solved by focusing on health promotion and repeated visits by the same public health nurse to the family, which nurtures a relationship of trust between the families and the public health nurses.

Previous evidence suggests that home visiting programs assumed an important role in

[17] See *Lov om Sundhedsplejerske Ordningerne, Lov nr 409 af 13. juni 1973* (n.d.) for the corresponding law article.
[18] At that time there were 14 regions in the country.

improving infant health such that their impact went beyond early childhood to result in better adult health, education, and earnings (Bhalotra et al. 2017; Bütikofer et al. 2019; Hjort et al. 2017; Moehling and Thomasson 2014; Wüst 2012). While there is consensus on the health benefits of postnatal home visits by public health nurses, the existing studies are limited in explaining which component of these programs (e.g., basic medical services, nutrition advice, or public health information) makes them so valuable.[19]

[19]For example, Hjort et al. (2017) documents a clear decrease in infant mortality induced by Denmark's Home Visiting Program in 1937, but it is not clear whether the referral of sick children to doctors, hygienic home environments, breastfeeding advice, or a combination of all services drove these changes.

A.2 Letter of the Danish National Board of Health

SUNDHEDSSTYRELSEN

Den 10 DEC. 1991

J.nr. 5300-6-91

B.nr.

Lokalnr.:

Praktiserende læger
Børneafdelinger/Neonatal afdelinger
Barselsgange/Obstetriske afdelinger
Landets Sygehuse
Ledere af den kommunale sundhedspleje
Amtsjordemødre/ledende jordemødre
Embedslægeinstitutioner
Foreningen forældre og fødsel
Amtssundhedsplejersker
Sygehusforvaltningen
Speciallæger i børnesygdomme

Risiko for pludselig uventet spædbarnsdød kan måske reduceres

Sundhedsstyrelsen har på baggrund af nyligt offentliggjorte undersøgelser fundet anledning til at ændre de tidligere anbefalinger om spædbørns sovestillinger. Det anbefales fremover som hovedregel ikke at placere sovende spædbørn på maven. Dette gælder kun indtil barnet selv er i stand til at vende sig.

Hvert år registreres ca. 100 tilfælde af pludselig uventet spædbarnsdød i Danmark svarende til 1-2 pr. 1000 levende fødte børn, overvejende i 1-5 måneders alderen.

Undersøgelser fra New Zealand, Australien, England og Holland tyder på en øget forekomst af pludselig uventet død hos spædbørn, der sover på maven. Der foreligger endvidere oplysninger fra New Zealand og England, der tyder på, at forekomsten af pludselig uventet spædbarnsdød kan reduceres ved, at spædbørnene undgår mavelejring under søvn.

Mekanismen for den formodede risiko ved lejring på maven er ukendt, men er muligvis relateret til flere faktorer, herunder forstyrrelser i temperaturregulationen, især hvis barnet anbragt i maveleje er klædt for varmt på, sover i et meget varmt lokale, eller hvis stofskiftet er øget på grund af infektion. I den forbindelse skal det oplyses, at størstedelen af varmeafgivelsen hos normalt tildækkede spædbørn sker fra ansigtet.

Der er ikke videnskabeli...

Appendix B Additional Figures

Figure B1: Smoking during the First Trimester of Pregnancy by Birth Cohorts

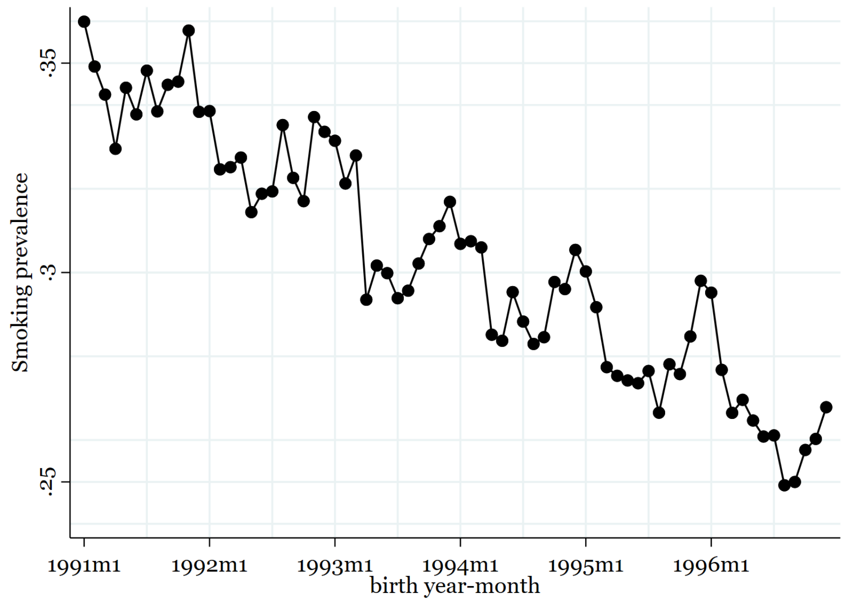
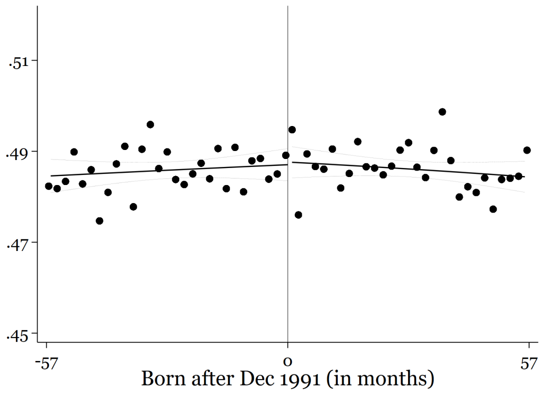
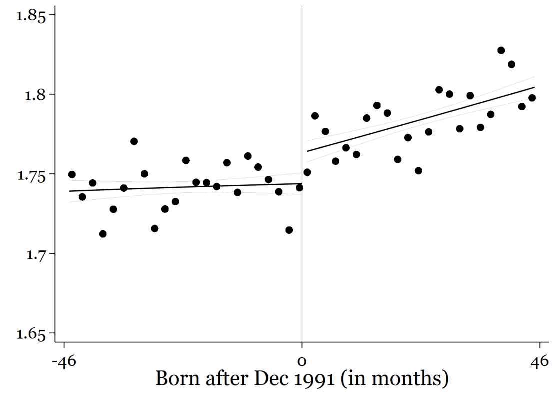


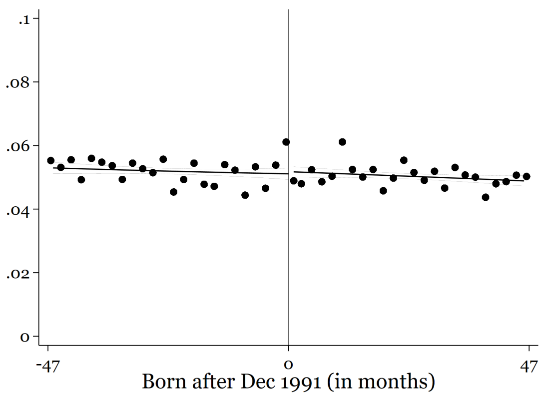
Figure B2: Balance of Covariates



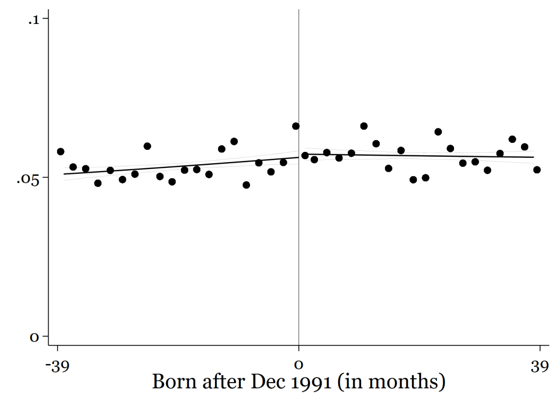
(a) Female



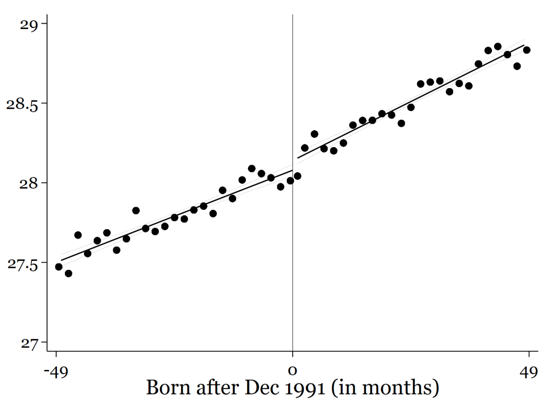
(b) Birth order



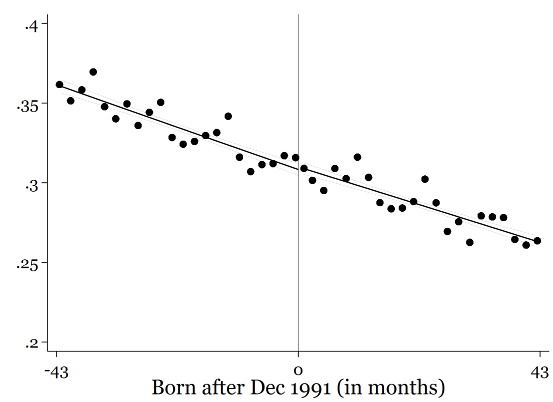
(c) Low birthweight



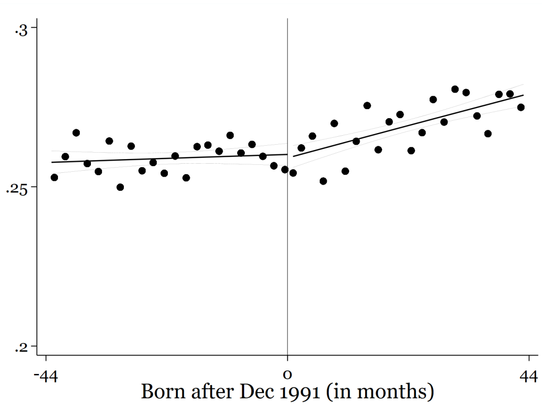
(d) Preterm birth



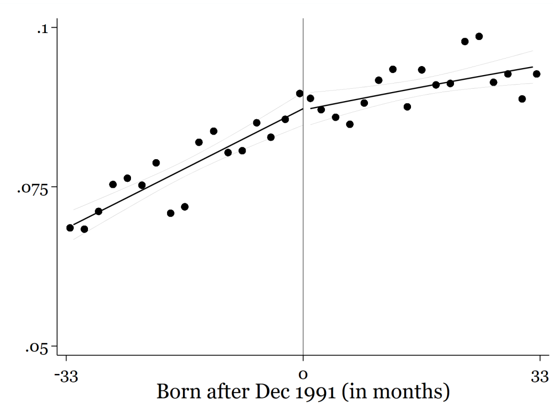
(e) Mother's age at birth



(f) Mother has basic education



(g) Mother with further education



(h) Mother is immigrant

FIGURE B3: DISTRIBUTION OF OBSERVATIONS AROUND THE RD CUTOFF

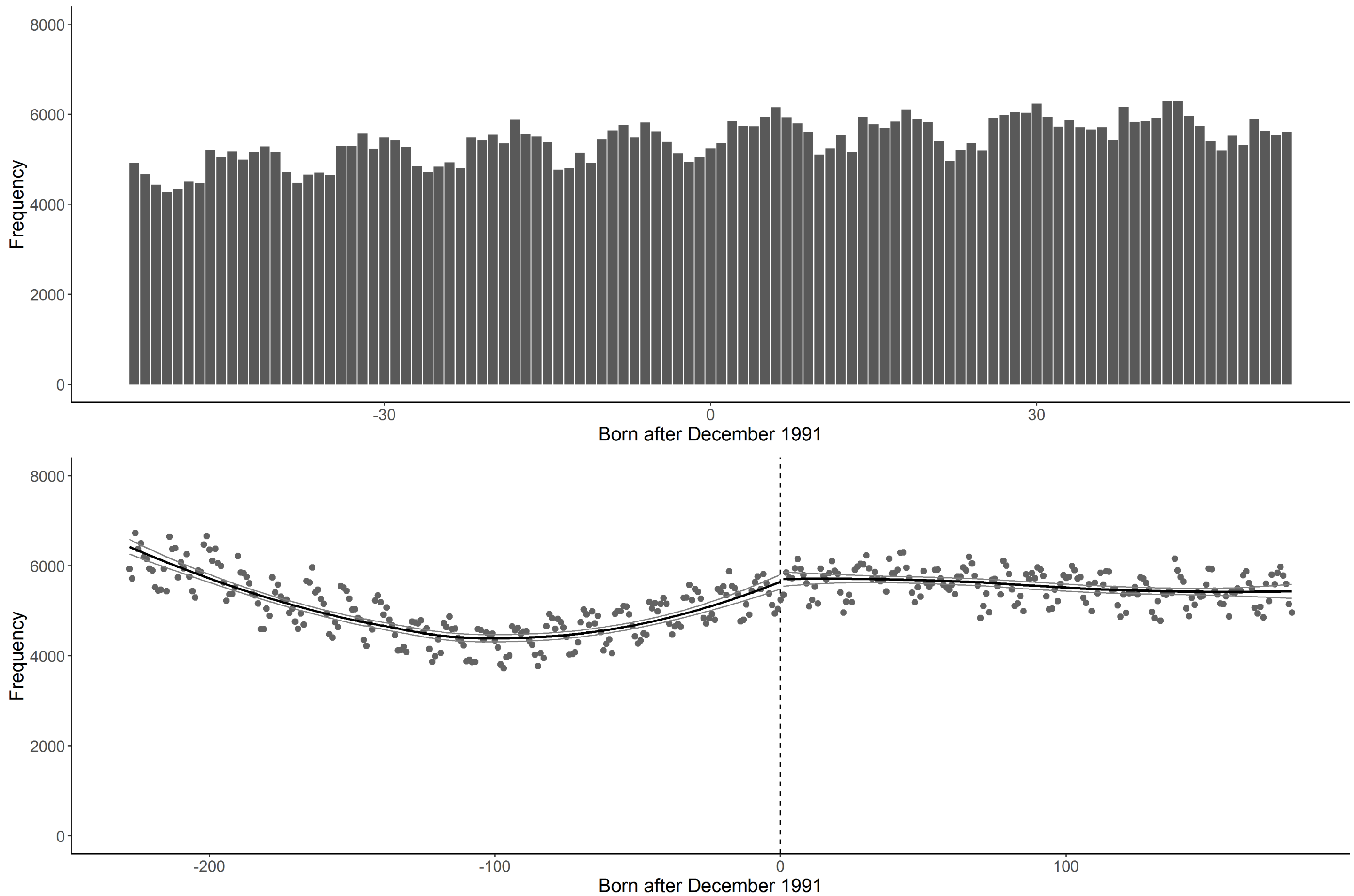
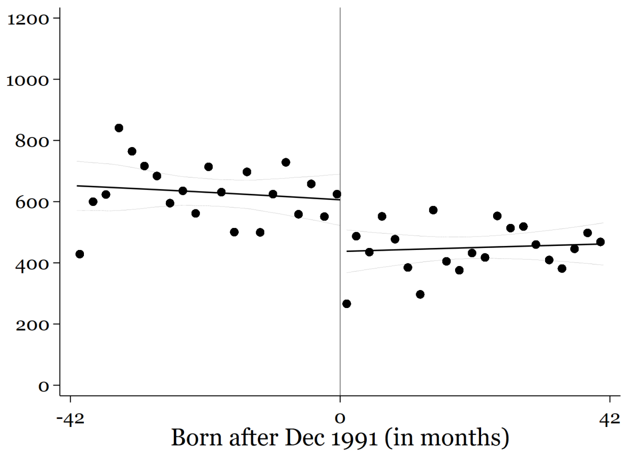
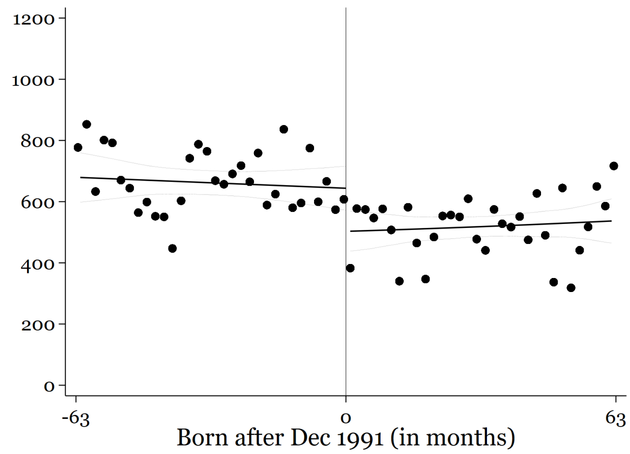


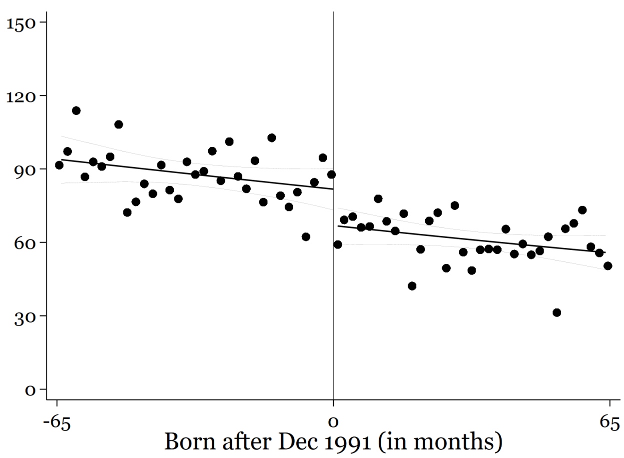
Figure B4: RD estimates on Infant Mortality: Subgroups



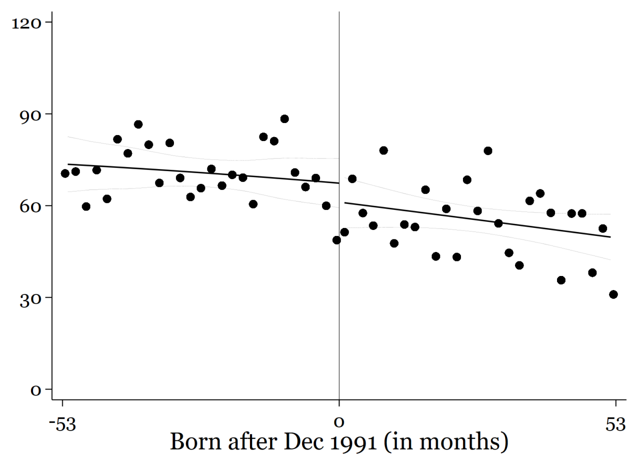
(a) Preterm Birth



(b) Low Birthweight

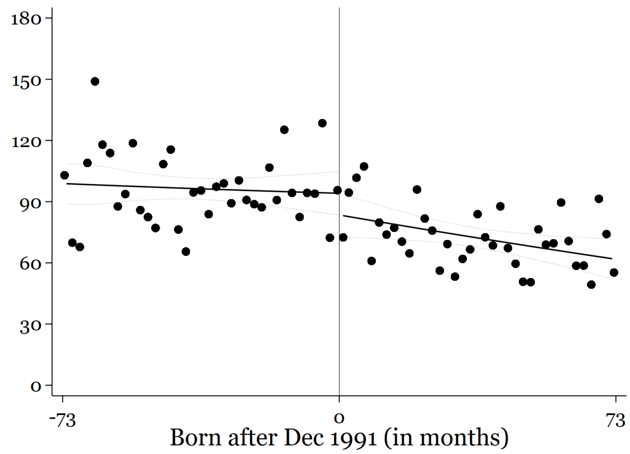


(c) Male

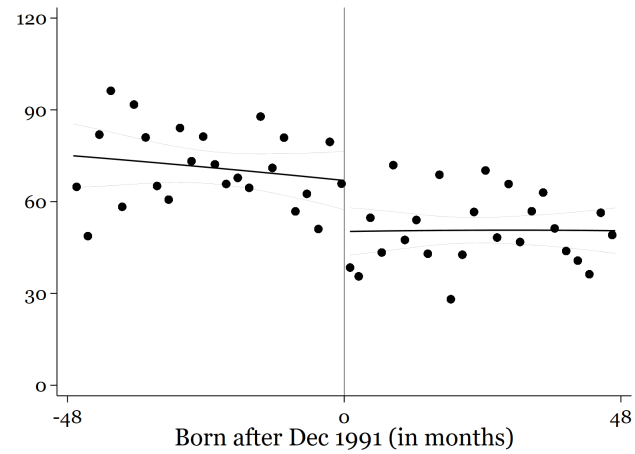


(d) First Child

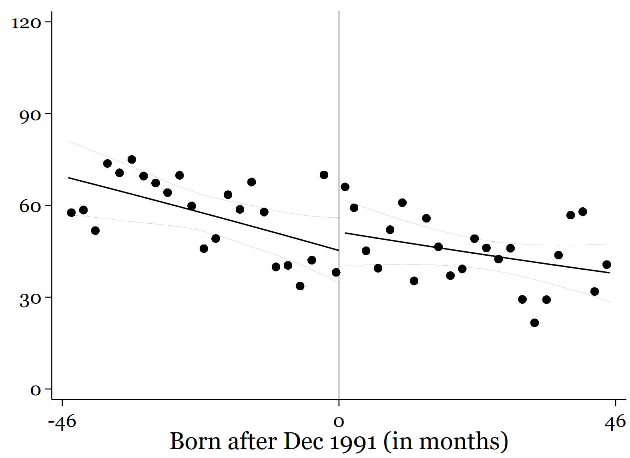
Figure B5: RD Estimates on Infant Mortality: Subgroups



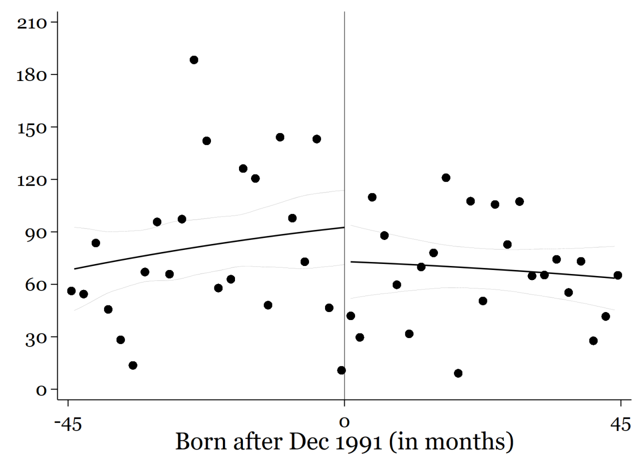
(a) Mother with Basic Education



(b) Mother with HS/Voc Education

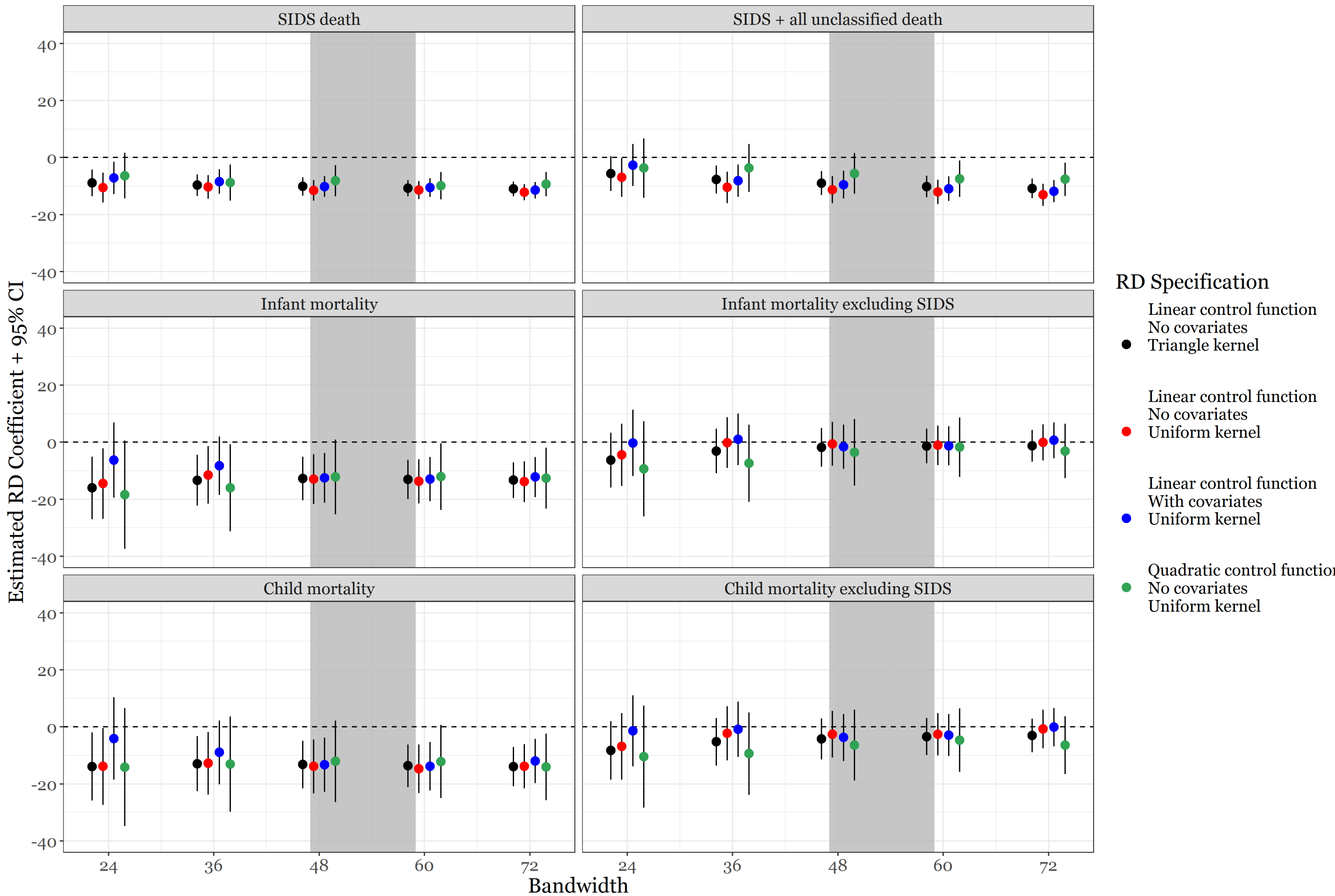


(c) Mother with Further Education



(d) Mother Immigrant

Figure B6: RD treatment effect sensitivity



Appendix C Additional Tables

Table C1: Variable Definitions

Variable name	Definition
Infant mortality	dummy variable that equals 1 if the child is registered in the Danish Death Registry before reaching age 1.
Infant mortality excluding SIDS	dummy variable that equals 1 if the child is registered in the Danish Death Registry before reaching age 1 **excluding** those registered with the following ICD codes that indicate the primary cause of death: 795.0 795.1, 795.9 (ICD-8), and R95.0 and R95.9 (ICD-10).
Infant mortality excluding SIDS and all other unclassified mortality	dummy variables that equal 1 if the child is registered in the Danish Death Registry before reaching age 1 **excluding** those registered with the following ICD codes that indicate the primary cause of death: 795.0 795.1, 795.9, 796.0, 796.9 (ICD-8), and R95.0 R95.9, R96.0, and R99.9 (ICD-10).
SIDS mortality	dummy variables that equal 1 if the child is registered in the Danish Death Registry before reaching age 1 with the following ICD codes that indicate the primary cause of death: 795.0 795.1, 795.9 (ICD-8), and R95.0 and R95.9 (ICD-10).
Child mortality	dummy variable that equal 1 if the child is registered in the Danish Death Registry before reaching age 5.
Child mortality excluding SIDS	dummy variable that equal 1 if the child is registered in the Danish Death Registry before reaching age 5 **excluding** those registered with the following ICD codes that indicate the primary cause of death: 795.0 795.1, 795.9 (ICD-8), and R95.0 and R95.9 (ICD-10).

Child mortality excluding SIDS and all other unclassified mortality	dummy variables that equal 1 if the child is registered in the Danish Death Registry before reaching age 5 **excluding** those registered with following ICD codes that indicate the primary cause of death: 795.0 795.1, 795.9, 796.0, 796.9 (ICD-8), and R95.0 R95.9, R96.0, and R99.9 (ICD-10).
Female	Dummy variables that equal 1 if the child is female. Information from the Danish medical birth registry.
Birth order	Birth order of the child. Information from the Danish medical birth registry.
Low birth weight	dummy variable that equal 1 if the child is registered with a birth weight below 2500 gram in the Danish medical birth registry.
Preterm birth	dummy variable that equal 1 if the child is registered with a gestational age of less than 37 weeks in the Danish medical birth registry.
Mother's age at birth	Mother's age in years at birth. Information from the Danish medical birth registry.
Mother immigrant	dummy variable that equals 1 if the mother is first or second generation immigrant. This group includes both immigrants and descendants of immigrants.
Mother's education	Mother's education is the educational attainment of the mother. The level is categorized into three groups: Basic education includes less than 12 years of schooling. Vocational training includes all vocational training educations and high school. Further education includes all short, medium, and long further education.

Table C2: RD effects on Infant and Child Mortality: With Covariates

	MSE-optimal	±24	±36	±48	±60	±72
Infant Mortality (per 10K births)						
Born after Dec. 1991	-12.719***	-6.275	-8.226	-12.547***	-12.988***	-12.241***
	(4.011)	(6.736)	(5.227)	(4.446)	(3.96)	(3.607)
Bandwidth	58	24	36	48	60	72
Observations	632449	268228	399591	528021	651926	774948
Control group mean	76.68	74.19	75.88	76.1	76.9	77.78
SIDS Mortality (per 10K births)						
Born after Dec. 1991	-11.297***	-7.15**	-8.448***	-10.192***	-10.556***	-11.474***
	(1.756)	(2.884)	(2.198)	(1.861)	(1.644)	(1.472)
Bandwidth	52	24	36	48	60	72
Observations	568106	268228	399591	528021	651926	774948
Control group mean	17.8	17.77	17.92	17.87	17.95	17.64
Infant mortality excluding SIDS						
Born after Dec. 1991	-2.057	-0.278	0.991	-1.626	-1.257	0.637
	(3.83)	(5.924)	(4.62)	(3.941)	(3.511)	(3.207)
Bandwidth	50	24	36	48	60	72
Observations	547850	268228	399591	528021	651926	774948
Control group mean	54.99	52.59	54.41	54.89	55.62	56.86
Child Mortality (per 10K births)						
Born after Dec. 1991	-13.98***	-4.079	-8.946	-13.248***	-13.835***	-11.937***
	(4.376)	(7.361)	(5.711)	(4.857)	(4.34)	(3.951)
Bandwidth	59	24	36	48	60	72
Observations	641982	268228	399591	528021	651926	774948
Control group mean	91.09	87.97	89.52	89.85	91.36	92.66
Child mortality excluding SIDS						
Born after Dec. 1991	-3.874	-1.394	-0.813	-3.713	-2.867	-0.103
	(4.262)	(6.361)	(4.939)	(4.209)	(3.758)	(3.429)
Bandwidth	47	24	36	48	60	72
Observations	517997	268228	399591	528021	651926	774948
Control group mean	63.29	61.44	62.92	63.23	64.44	65.95

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant and child mortality after controlling for gender, month of birth, and birth order fixed-effects, birth weight (in natural logarithm), gestation (in weeks), dummy variables for mother's education and immigrant status. See Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level, and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

Table C3: RD effects on Infant and Child Mortality: Triangle Kernel

	MSE-optimal	±24	±36	±48	±60	±72
Infant Mortality (per 10K births)						
Born after Dec. 1991	-12.927***	-16.041***	-13.339***	-12.699***	-13.026***	-13.33***
	(3.554)	(5.596)	(4.521)	(3.904)	(3.496)	(3.195)
Bandwidth	58	24	36	48	60	72
Observations	621988	257938	389176	517997	641982	765480
Control group mean	76.82	74	75.64	76.23	76.58	77.54
SIDS Mortality (per 10K births)						
Born after Dec. 1991	-10.47***	-8.928***	-9.725***	-10.147***	-10.771***	-11.024***
	(1.576)	(2.416)	(1.92)	(1.642)	(1.459)	(1.322)
Bandwidth	52	24	36	48	60	72
Observations	557913	257938	389176	517997	641982	765480
Control group mean	17.7	17.91	17.51	17.95	17.91	17.61
Infant mortality excluding SIDS						
Born after Dec. 1991	-1.726	-6.343	-3.117	-1.838	-1.361	-1.291
	(3.38)	(4.902)	(3.975)	(3.443)	(3.091)	(2.829)
Bandwidth	50	24	36	48	60	72
Observations	537683	257938	389176	517997	641982	765480
Control group mean	54.85	52.51	54.61	54.92	55.3	56.64
Child Mortality (per 10K births)						
Born after Dec. 1991	-13.554***	-13.919**	-12.898***	-13.177***	-13.624***	-13.907***
	(3.844)	(6.094)	(4.927)	(4.256)	(3.815)	(3.487)
Bandwidth	59	24	36	48	60	72
Observations	632449	257938	389176	517997	641982	765480
Control group mean	91.12	87.1	89.36	89.95	91.09	92.29
Child mortality excluding SIDS						
Born after Dec. 1991	-4.28	-8.257	-5.214	-4.174	-3.42	-3.034
	(3.713)	(5.242)	(4.251)	(3.678)	(3.301)	(3.021)
Bandwidth	47	24	36	48	60	72
Observations	508338	257938	389176	517997	641982	765480
Control group mean	62.68	61.05	63.17	63.29	64.11	65.67

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant and child mortality using a triangle kernel to weight the regression sample. See Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

Table C4: RD Effects on Infant and Child Mortality: Quadratic Control Function

	MSE-optimal	±24	±36	±48	±60	±72
Infant Mortality (per 10K births)						
Born after Dec. 1991	-12.147**	-18.418*	-16.046**	-12.222*	-12.059**	-12.573**
	(6.061)	(9.67)	(7.788)	(6.685)	(5.972)	(5.455)
Bandwidth	58	24	36	48	60	72
Observations	632449	268228	399591	528021	651926	774948
Control group mean	76.68	74.19	75.88	76.1	76.9	77.78
SIDS Mortality (per 10K births)						
Born after Dec. 1991	-8.963***	-6.362	-8.833***	-8.159***	-9.888***	-9.352***
	(2.63)	(4.078)	(3.224)	(2.756)	(2.442)	(2.193)
Bandwidth	52	24	36	48	60	72
Observations	568106	268228	399591	528021	651926	774948
Control group mean	17.8	17.77	17.92	17.87	17.95	17.64
Infant mortality excluding SIDS						
Born after Dec. 1991	-2.954	-9.369	-7.409	-3.576	-1.755	-3.087
	(5.82)	(8.517)	(6.893)	(5.935)	(5.306)	(4.857)
Bandwidth	50	24	36	48	60	72
Observations	547850	268228	399591	528021	651926	774948
Control group mean	54.99	52.59	54.41	54.89	55.62	56.86
Child Mortality (per 10K births)						
Born after Dec. 1991	-11.516*	-14.115	-13.062	-12.029*	-12.161*	-14.063**
	(6.585)	(10.552)	(8.495)	(7.291)	(6.536)	(5.965)
Bandwidth	59	24	36	48	60	72
Observations	641982	268228	399591	528021	651926	774948
Control group mean	91.09	87.97	89.52	89.85	91.36	92.66
Child mortality excluding SIDS						
Born after Dec. 1991	-6.773	-10.425	-9.385	-6.424	-4.683	-6.406
	(6.402)	(9.134)	(7.361)	(6.33)	(5.671)	(5.186)
Bandwidth	47	24	36	48	60	72
Observations	517997	268228	399591	528021	651926	774948
Control group mean	63.29	61.44	62.92	63.23	64.44	65.95

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant and child mortality using a quadratic control function fit on each side of the discontinuity threshold. See Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

TABLE C5: DONUT RD EFFECTS ON INFANT AND CHILD MORTALITY

(per 10K births)	Infant Mortality	SIDS Mortality	Infant Mortality exc. SIDS	Child Mortality	Child Mortality exc. SIDS
Born before 1992	-11.86***	-11.5***	0.02	-14.3***	-1.57
	(4.27)	(1.901)	(4.136)	(4.637)	(4.615)
MSE-optimal BW	60	51	52	61	48
Observations	631418	537405	547598	641056	507513
Control group mean	76.74	17.55	55.1	91.48	63.35

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant and child mortality excluding the four monthly birth cohorts who were born between September 1991 and December 1991. See Appendix Table C1 for the variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

TABLE C6: RD 2SLS EFFECTS ON INFANT AND CHILD MORTALITY

(per 10K births)	Infant Mortality	SIDS Mortality	Infant Mortality exc. SIDS	Child Mortality	Child Mortality exc. SIDS
Born before 1992	-9.43**	-11.04***	1.38	-10.41**	-0.75
	(4.302)	(1.879)	(3.571)	(4.933)	(4.15)
MSE-optimal BW	53	48	60	48	51
Observations	572595	522750	645227	522750	552291
Control group mean	77.06	17.85	55.87	90.52	63.69
First stage Estimates			Born before 1992		
Expected birthday before 1992	0.9797	0.9778	0.982	0.9778	0.979
	(0.0003)	(0.0003)	(0.0002)	(0.0003)	(0.0003)
t-statistic	3617.18	3291.42	4091.86	3291.42	3483.49

Notes: Table shows the regression discontinuity estimates for the effect of public health information campaign on infant and child mortality using a two-stage least squares estimator where the running variable is the expected birth month-year of the child, which is calculated by 280 days after the date of conception. The actual exposure to policy is instrumented by expected exposure, a dummy variable that indicates exposure based on the expected birth date. See Appendix Table C1 for other variable definitions. Each column indicates the bandwidth used to restrict the regression sample. Standard errors are clustered at the month-year cohort level and are shown in parentheses. Significance levels are indicated by *** < 0.01, ** <0.05, and * <0.1.

Table C7: Descriptive statistics

	Mean	Std. dev.	*N*
Child characteristics			
Female	0.49	0.5	2141226
Birth order	1.78	0.9	2143696
Birth weight (in gr)	3383	609	2126173
Gestation (weeks)	39.58	1.97	1724205
Low birthweight	0.05	0.23	2126173
Born preterm	0.05	0.23	1724205
Maternal characteristics			
Age at birth	27.95	4.95	2143696
Basic education	0.33	0.47	2128679
HS/Vocational education	0.39	0.49	2128679
Higher education	0.27	0.45	2128679
Immigrant	0.08	0.28	2128679
Mortality outcomes			
SIDS mortality (over 10K)	7.79	279.09	2143696
SIDS + unknown mortality (over 10K)	9.60	309.69	2143696
Infant mortality (over 10K)	68.88	827.06	2143696
Child mortality (over 10K)	82.52	904.66	2143696

Notes: Table shows sample descriptive statistics for the variables used in the analysis. See Appendix Table C1 for the variable definitions.