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**Astrid de Linde**
Department of Health Management and Health Economics,
University of Oslo, Norway

**Jostein Grytten**
Department of Community Dentistry,
University of Oslo, Norway
Division of Obstetrics and Gynecology, Akershus University Hospital, Lørenskog, Norway

**Irene Skau**
Department of Community Dentistry,
University of Oslo, Norway

**Jonas Minet Kinge**
Department of Health Management and Health Economics,
University of Oslo, Norway
Norwegian Institute of Public Health, Bergen, Norway

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# Maternity ward closures and infant health outcomes, maternal health outcomes, and birth procedures*

Astrid de Linde[1,**], Jostein Grytten[2], Irene Skau[2], and Jonas Minet Kinge[1,3]

[1]Department of Health Management and Health Economics, University of Oslo, Oslo, Norway
[2]Department of Community Dentistry, University of Oslo, Oslo, Norway
[3]Centre for Disease Burden, Norwegian Institute of Public Health, Bergen, Norway

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**Abstract**

We analyze the short- and long-term impacts of maternity ward closures on health and education outcomes, hospital procedures, and fertility. Our study makes use of registry data that covers every delivery in Norway from 1981 through 2019. Among those directly experiencing a closure, we find in the short-term a small decline 5-minute Apgar score and increased probability of birth outside institution. This slight drop in Apgar is not reflected in the other available health indicators and we therefore hypothesize it reflects different institutional scoring standards as opposed to a health effect. For long-term outcomes, we find that experiencing a closure as an infant results in a 1 percentage point increase in beginning high school by 16, but does not lead to a change in the likelihood of graduating by age 22. Furthermore, for those infants assigned female at birth, experiencing closures as an infant does not change the likelihood of giving birth as an adult or experiencing negative health conditions during pregnancy. Given that these conditions are themselves potential risk factors for newborn health, our results do not point to evidence for an intergenerational effect of closures. We hypothesize that an effective prenatal screening process and robust health and social services may mitigate the effect of closures and thus account for a limited treatment effect. Our paper is among the first to look at both the short and long-term implications of closures and suggests further avenues to study among labor, education, and health outcomes.

## 1 Introduction

The centralization of Norway's population (Statistics Norway, 1995) has been matched by a centralization of obstetric wards. In the past 40 years, the majority of small units have closed, resulting in a flow of expecting individuals to central and regional hospitals (Grytten et al., 2014). From a health economics perspective, the effects of these closures are uncertain. Proximity may provide more timely care and prevent overcrowding, however improved efficiency and specialization may

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** Corresponding author: a.de.linde@medisin.uio.no

result from consolidation (Avdic et al., 2024). Given the link between early-life exposures and long-term health (Almond and Currie, 2011) and that quality and accessibility are defined care goals for obstetrics (The Norwegian Directorate of Health, 2024), the impact of these closures is a clear policy concern.

Although centralization is a phenomenon seen across high-income countries, prior research into the short-term effects of closures has yielded somewhat varying results (Malouf et al., 2020). Avdic et al. (2024) find that a series of hospital mergers in Sweden resulted in a net negative effect across a range of maternal health outcomes, largely due to crowding, but find a positive effect on infant outcomes. Grytten et al. (2014) similarly consider maternity ward closures in Norway from 1979 through 2004 and find no effect on neonatal or infant mortality. Examining closures at the city level, Lorch et al. (2013) report only a temporary decline in outcomes. Recent papers from Battaglia (2023), Fischer et al. (2024), and Chatterji et al. (2023) look at obstetric unit closures in the US. They conclude closures had no discernible effect on infant health outcomes for the affected population in general. However, Chatterji et al. (2023) report that when race and ethnic background are considered, there is evidence for worsening infant health outcomes. Among maternal health outcomes, Fischer et al. (2024) and Chatterji et al. (2023) find no evidence for worsening health, and Battaglia (2023) points to improved allocation of Cesarean sections.

To our knowledge, the long-term effects of closures on health and education have yet to be examined. Lazuka (2023) considers *openings* of maternity wards in Sweden in the 1930s and 40s and finds compared to home-birth, being born in a hospital results in higher earnings and lower likelihood of unemployment and disability income. Human capital, fetal origins, and early childhood literature similarly suggests access to health services in utero and during infancy can have positive, lasting effects (Almond and Currie, 2011; Almond et al., 2018). Provided that closures lead to early-life health shocks, there could be residual effects on health and education later in the lifecycle.

The goal of this paper is to add to the literature on centralization of care and to identify in Norway whether procedures as well as short- and long-term health outcomes are affected by closures. We begin by establishing an assigned institution for each municipality during a baseline period. After the closures of these baseline institutions, we identify the relevant hospital to which the impacted population goes. To fully capture the dynamics of changing catchment regions, we follow Avdic et al. (2024) and consider two separate treatment groups: the closure group, whose assigned institution closes, and the inflow group, who experience an inflow of patients at their assigned institution after a nearby closure. This distinction allows us to differentiate between the relative impacts of travel distance, hospital resource availability, and crowding.

Using data from the Medical Birth Registry of Norway, we isolate 19 catchment-changing closures and 13 inflow waves during the period 1981-2019. A unique benefit in our study is the extensive timeline, which allows us to compare the effect of closures across nearly four decades and examine, for example, whether there are learning effects from early closures on later closures. The extended timeline additionally allows us to consider long-term outcomes on health and education from early life treatment exposure. Our empirical strategy draws on Callaway and Sant'Anna's (2021) difference-in-differences framework that allows for heterogeneous and dynamic treatment effects. We capitalize on the flexibility of the model in choice of treatment effect aggregation and control group.

We find for the entire period of study among the closure group a small decline in 5-minute Apgar score and an increase in births outside of obstetric institutions, when compared to the not-yet and never treated groups. This modest decline in average Apgar score is not accompanied by an increase in mortality or by signs of a negative long-term impact among our available indicators. We therefore hypothesize that the decline reflects differences in institutional-level practices in assigning scores as opposed to a real health effect. Among the available markers of long-term health, we find no effect in adulthood of experiencing a closure as an newborn.

For the inflow group we find a reduction in the probability of hemorrhage. The lack of evidence for a crowding effect is likely due to the generally small influx in patients following nearby closures. However, even when only larger patient increases are considered, we do not find evidence for negative infant, maternal, or procedure-related effects.

The rest of our paper is as follows. Section 2 provides a brief summary of maternal care in Norway. Empirical frameworks and data are covered in Sections 3 and 4, respectively. In Section 5 we discuss our results and the potential mechanisms behind our findings. Section 6 concludes.

# 2 Background

## 2.1 Organization of Care

Maternal services in Norway are provided free of charge and independent of residency status at both the local and national level (Saunes et al., 2020; The Norwegian Directorate of Health, 2022). General practitioners (GPs) and midwives, who are employed by municipalities, are responsible for prenatal care. Although prenatal check-ups have been a normal part of the care pathway in Norway since the 1950s, uniform guidelines for the visits were not discussed until 1984 (The Ministry of Health and Care Services, 2009; NOU [Official Norwegian reports], 1984). Currently, however, expecting individuals are entitled to nine pregnancy consultations at a minimum, during which the GP or midwife assesses the risk level of the birth and applies for space at the appropriate institution (The Norwegian Directorate of Health, 2022). These institutions are organized by one of four Regional Health Authorities as a part of specialist health care (The Ministry of Health and Care Services, 2009), but are ultimately under the responsibility of the state (Spesialisthelsetjenesteloven [The Specialist Health Service Act], 1999, §2-1). While closures may affect where individuals give birth, they in theory should have no direct impact on prenatal care.

Beginning in 2001, institutions for childbirth have been formally divided across three levels: midwifery-led units (*fødestuer*), hospital maternity wards (*fødeavdelinger*), and specialist clinics (*kvinneklinikker*) (Stortingsforhandlinger [the proceedings of the Storting], 2001; The Norwegian Directorate of Health, 2024). On-hand resources are the defining characteristic of each category, but even within institutions of the same level there can be a range in the number of staff or the types of procedures that can be performed (The Norwegian Directorate of Health, 2019). Midwifery-led units lie at the lower end of the resource spectrum, and thus are only intended for low-risk pregnancies (The Norwegian Directorate of Health, 2024, 2019). As the name suggests, midwives, as opposed to obstetricians, steer these units which may or may not be located within a hospital[1] (The Norwegian Directorate of Health, 2019). In contrast, hospital maternity wards provide 24-7 care with both obstetricians and midwives on staff (The Norwegian Directorate of Health, 2019). Potential complications, such as C-sections, multiple births, and early deliveries, are often considered within the capacity of these wards. However, high-risk cases are referred to specialist clinics

[1] Midwifery-led units can be further differentiated between classic and modified units. Classic units tend to be free-standing and without physician staffing, while modified units are located in hospitals and thereby have access to surgeons and/or gynecologists who can perform emergency c-sections (Børdahl et al., 2006; The Ministry of Health and Care Services, 2009).

at hospitals which house neonatal intensive care units and other high resource services (The Norwegian Directorate of Health, 2019). Although specialist clinics and hospital maternity wards are differentiated by their ability to handle complex deliveries, they are most often the local, "default" institutions for low-risk pregnancies. Under the Patients' Rights Act of 1999, expecting individuals can choose their institution, subject to space availability [2] (The Norwegian Directorate of Health, 2019).

## 2.2 Brief History of Closures

Throughout the mid-20th century, the number of delivery institutions drastically increased, from 11 in 1930 to nearly 200 by 1970 (The Ministry of Health and Care Services, 1998). Yet, by the 1990s, fewer than 100 institutions remained, with the decline mostly driven by the closures of small midwifery-led units and small hospital maternity wards (The Ministry of Health and Care Services, 1998). This downward trend continued through the new millennium and by 2019 just 45 institutions were recorded in the Medical Birth Registry. Despite the national steering of health services, there have been no official mandates to centralize maternal care; instead, economic considerations, including an insufficiently large patient-base, a lack of personnel, and medical advice have all been cited as reasons for the closures (Grytten et al., 2014; The Ministry of Health and Care Services, 1998).

The precise circumstances surrounding closures have varied. In some cases, these closures were tied to the closures of entire health centers or hospitals, while in other cases they were strictly limited to obstetric wards. And in a few instances, the wards did not close but instead drastically reduced the services provided. The notable distinction for our purposes, however, is that in a subset of cases the closures brought about a shift in catchment regions: where we would expect an individual from a given municipality to give birth changes. While we observe around 60 unique institutions dropping from the Medical Birth Registry from 1981-2019, only 19 of these represent a catchment-changing closure[3]. The other departures from the Medical Birth Registry encapsulate for the most part small institutions as well as anomalous situations (e.g., unplanned, emergency births), and are generally too small to alter the treatment opportunities for geographic clusters of individuals. In the remainder of our paper, we refer specifically to catchment-changing cases.

## 2.3 Previous Literature

The net effect of these catchment-altering closures is unclear and likely depends on whether they are experienced directly or indirectly. Moreover, because maternal and infant health are not perfectly linked, the direction of any effects may vary within maternal-infant units (Verma et al., 2021). For expecting individuals whose assigned institution closes, there may be a trade-off between proximity and scale. If closures result in greater travel distances and if greater travel distances increase the likelihood of delayed care, delivery outside of institution, or stress during birth, centralization may threaten infant and maternal health. Prior research, however, on the phenomenon is somewhat limited. A review of five registry-based observational studies on high-income settings from Ames et al. (2022) found that travel time over one hour might slightly raise the risk of eclampsia and induction and might greatly increase the chance of giving birth outside of an institution; the connection though between the 60-minute threshold and a range of other outcomes was uncertain.

[2] At the time of writing, planned home births are not guaranteed under this act and are only provided through the private sector (The Norwegian Directorate of Health, 2023).

[3] Three of these 60 institutions that close are not included in our analysis because they were first downgraded from hospital maternity wards to midwifery-led units.

Asheim et al. (2022) conduct a causal analysis based on movement between catchment regions in Norway and find distance effects in the form of increased birth during transport, but no associated increase in perinatal mortality. Other studies on distance and delivery within high-income countries have found mixed results. Combier et al. (2013), Kwak et al. (2019), and Ravelli et al. (2011) report an association between increased travel time and poorer health outcomes, Blondel et al. (2011) and Örtqvist et al. (2021) link greater travel time to birth outside hospital, and Ovaskainen et al. (2020) and Rodie et al. (2002) tie unplanned out-of-hospital delivery to adverse outcomes. In contrast, Dummer and Parker (2004), Parker et al. (2000), and Pilkington et al. (2014) do not find evidence of an association between travel distance and poor outcomes, and Pirneskoski et al. (2016) report no deaths or other notable poor outcomes in their study of out-of-hospital deliveries assisted by emergency medical services. An additional challenge in interpreting any association between proximity to institutions and maternal and infant health is the potential mitigating effects of improved infrastructure, access to emergency services, accompaniment during transport, and anticipation and adjustment.

However, even if individuals in the directly treated municipalities must travel to a different institution to give birth following a closure, they are generally speaking moving to a larger, higher resourced setting. To the extent that institution size and resource level affects quality, there may be a positive health impact to centralization. But higher resource availability could also result in overtreatment. For low-birth weight and preterm cases, there is strong evidence in favor of referral to top-level hospitals (Helenius et al., 2019). For low-risk pregnancies however, there is less of a consensus. Garcia et al. (2001) and Heller et al. (2002) report improved health outcomes associated with births taking place at academic medical centers and larger institutions, respectively, and Asheim et al. (2022) find evidence for lower perinatal mortality at higher-volume wards. Grunebaum et al. (2022) similarly find an association with poorer outcomes and freestanding birth centers compared to hospital units staffed by doctors or midwives. However, results from Hemminki et al. (2011) and Brocklehurst et al. (2011) suggest that in general outcomes are comparable across hospital levels, and although Snyder et al. (2011) report fewer inappropriate inductions at university hospitals compared to community hospitals, they do not find any difference in at-term c-sections.

For those whose assigned institution experiences an inflow of patients following a closure, there may be a trade-off between crowding and specialization. If any influx of patients is not met with a corresponding increase in staff or physical resources, there could be a decline in the standard or amount of care. But it is also possible that with the same level of staff, care is maintained or improved through a practice-makes-perfect effect. Previous studies on hospital volume do not point to a clear association. Janakiraman et al. (2011) and Snowden et al. (2015) find no evidence of a connection between hospital volume and maternal health outcomes, and Tracy et al. (2006) draw a similar conclusion for maternal and infant outcomes. In contrast, Snowden et al.'s (2012) examination of infant health points to a higher volume being associated with a lower prevalence of birth asphyxia, while Kyser et al. (2012) find poor outcomes at the ends of the volume continuum. While many of the studies consider case-mix, without a correctly specified design, no causal conclusions can be drawn due to endogeneity bias: volume may lead to better care through a practice-makes-perfect effect, but it is also possible that patients gravitate towards high-standard institutions (Gaynor et al., 2005).

# 3 Empirical Framework

## 3.1 Definitions

We classify maternal-infant treatment status in reference to the home municipality's baseline assigned institution. We use 1981-1984 as the baseline period to allow for a sufficient pretreatment period and generate assigned wards based on the modal institution at which residents of a municipality give birth during this time. The closure group includes all maternal-infant pairs who reside in a municipality in which the baseline institution closes[4] The inflow group consists of those whose municipality's assigned institution expands its catchment region following a nearby closure; and the control group contains all maternal-infant pairs from municipalities that neither directly nor indirectly experience a closure. To maintain a consistent treatment classification, we use a slightly modified version of the 2019 municipality divisions. Figure 3 presents municipal treatment assignment. Across the entire period, we observe that approximately 87% of births occur at the assigned institution for the given home municipality.[5]

## 3.2 Empirical Model

To estimate the effect of closures, we employ Callaway and Sant'Anna's (2021) semiparametric difference-in-differences estimator that allows for heterogeneous and dynamic treatment effects. Within the framework we assume that treatment turns on in the first period at the new assigned institution for the closure group and in the first period of an expanded catchment region for the inflow group[6]. Each treatment group is considered separately such that individuals in the closure group are only compared to individuals in the control group or in a later-closure group, and individuals in the inflow group are only compared to individuals in the control group or in a later-inflow group.

Following their design,

$$ATT(g,t) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{E\left[\frac{p_g(X)C}{1-p_g(X)}\right]}(Y_t - Y_{g-1})\right)\right]$$

we identify an average treatment effect for each treated cohort ($g$) at each period ($t$). These individual group ATTs can be flexibly aggregated to underscore different aspects of treatment; in our main results we report an average post-treatment effect for the four years following treatment. Within their notation, $G_g$ is an indicator variable for treatment, equal to one if the observation comes from a closure or inflow municipality and zero otherwise. $C$ is an indicator for being part

[4]We define closed institutions to be those that cease to accept planned deliveries during the period 1985-2015. Institutions are considered closed when they are last observed in the Medical Birth Registry, or earlier if the yearly number of births declines to less than 10% of the annual maximum (in some cases, closures are protracted and involve a period in which only emergency cases are accepted, for example; in these situations we use the effective closure date). All closures and their approximate timing are confirmed through government documents or contemporary media reports.

[5]The value includes births that occur outside of an institution. Because such deliveries are rare, the intention-to-treat value is comparable when conditioned on birth at a ward or hospital.

[6]There are a few institutions with multiple inflow waves; we assign the largest influx of patients which is almost always the first wave.

of the control group; it is equal to one for never- and not-yet-treated individuals and is zero otherwise. $p_g$ represents the probability of being in treatment cohort g, given the value of pre-treatment covariates $(X)$ and treatment status of being never or not-yet treated. $Y_t$ is the outcome from treatment at half-year [7] $t$ and $Y_(g-1)$ is the outcome in the half-year prior to treatment.

We additionally present in the appendix traditional event studies using the equation,

$$y_{ikmt} = \alpha_k + \gamma_m + \lambda_t + \sum_{\tau=-q}^{-1} \theta_\tau D_{kt} + \sum_{\tau=0}^{m} \delta_\tau D_{kt} + X_{it} + \varepsilon_{ikmt}$$

Treatment is normalized to occur at period $\tau$ equal to zero, with $q$ pre- and $m$ post-treatment periods, and to be relative to a null effect at $\tau$ of negative one. Both never treated and not-yet treated observations are included within this negative one $\tau$ point. Thus, we interpret $\theta_\tau$ as the comparative lead effect and $\delta_\tau$ as the comparative treatment effect at period $\tau$.

The key assumption within our set up is that absent any closures, outcomes across obstetric units would have followed similar trends.

## 4 Data

Our primary source of data is the Medical Birth Registry of Norway, which provides a record of all births in the country. Although the registry has been in operation since 1967 (Helsedata [Health data], nd), we start our analysis in 1981, due to inconsistent recording of our variables of interest in the early years of the registry, and follow births through 2019. This timeline allows us to analyze at least four years of data pre and post treatment for each closure and inflow wave. We additionally incorporate education data from the National Education Database and generate travel distance to institutions using the Norwegian Public Roads Administration's (2023) "Route planning for cars."

To study the short-term effects of centralization on infant health, we examine infant and neonatal mortality as well as Apgar score and referral to a neonatal ward. Apgar scores are integer values from 0 to 10 meant to provide an indication of the general health status of the newborn (Casey et al., 2001), and are taken at one, five, and sometimes 10 minutes. Because of its predictive nature (Casey et al., 2001), we focus on five-minute Apgar scores. To evaluate the potential effects of treatment on maternal health, we analyze 3$^{rd}$ and 4$^{th}$ degree lacerations and hemorrhaging, as each can be influenced in part by quality of care (Knight et al., 2009; OECD, 2021; World Health Organization, 2012). Our final set of short-term outcomes falls within the category of birth procedures and includes birth during transport, home birth, birth outside of institution[8], induction, epidural, and c-section. Induction as well as births during transport, at home, and outside of an institution can capture distance effects while c-section and epidural reflect institutional resource availability and allocation. Due to changes in the Medical Birth Registry, we are only able to reliably follow emergency c-sections and 3$^{rd}$ and 4$^{th}$ degree lacerations from 1990 and planned versus unplanned home births, epidural, and induction from 1999. There are a range of maternal

[7] We define periods in terms of half-years as opposed to years in order to better capture that closures occur at different periods throughout the year. We hope to avoid potential spill-overs between pre- and post-treatment periods. We present estimates using treatment year in the Appendix Table 15.

[8] This category includes all births during transport, home births, and births that occur at otherwise closed institutions (i.e., institutions that do not accept planned deliveries)

health characteristics and socio-demographic variables that can influence our studied outcomes (Avdic et al., 2024; Grytten et al., 2014; Johnson and Rehavi, 2016). We do not include these as controls in the models but provide summary statistics across treatment groups and additionally test these variables as predictors of treatment (Tables 1, 2, 3, 4, and 5).

Given the time range of our data, we are also able to explore longer-term outcomes of centralization. Among those born in the first decades of closures, we analyze education level at adulthood, and for those who are assigned female at birth we additionally examine likelihood of giving birth as well as pre- and during pregnancy health indicators included in the Medical Birth Registry (e.g., asthma, arthritis, diabetes, gestational diabetes, renal disease, chronic hypertension, and epilepsy).

Because our identification strategy relies on home municipality, we drop all maternal-infant observations for which municipality residence is unknown. We additionally drop observations for which Oslo is the home municipality, as well as those residing in municipalities in which the assigned institution is downgraded, regardless of whether it eventually closes.

# 5 Results

## 5.1 Descriptives

Table 1 provides an overview of our sample as a whole and across treatment groups. We summarize mean values across maternal background characteristics and risk factors, infant health outcomes, maternal health outcomes, and birth procedures. The control group consists of 940,674 maternal-infant pairs, from 190 municipalities assigned to 27 institutions. The closure group includes 205,022 infants, from 48 municipalities, and 19 baseline institutions. The inflow group includes 684,946 infants, from 104 municipalities at 14 institutions. Table 2 provides a similar summary for pre and post treatment for the closure and inflow groups, and placebo treatment for the control municipalities. To contextualize our 1999 regressions, we additionally include summaries for the period 1999-2019 (Tables 3 and 4).

Following Avdic et al. (2024), we present in Figure 1 the average number of births per institution for each treatment group in the periods pre- and post-treatment. Individuals from the closure group tend to give birth at institutions with approximately 130 births per half-year period on average leading up the closure. They go on to give birth at much larger institutions with around 810 births per half-year on average. In periods prior to treatment, the inflow group is assigned to institutions with around 650 births per half-year on average to 750, such that the influx from the closure group is fairly muted. For the control group, their assigned institutions represent around 450 births before and after potential treatment.

We construct a similar analysis for travel distance (Figure 2)(Asheim, 2023) using each municipality's administrative center as the reference point. For the control and inflow groups, average driving distance only slightly declines over the period from 30 kilometers and 20 kilometers, respectively [9]. While the closure group has a similar pre-treatment driving distance of approximately 20 kilometers to the inflow group, following the new institution assignment the average driving distance increases to around 55 kilometers. [10]

[9] These declines reflect changes in the relative number of births in each municipality as opposed to changes in routes.

[10] These driving distances are based off of current road and infrastructure conditions. It is likely that travel distances are in general shorter now than in the earlier periods included in our analysis due to investments in i.a. tunnels, bridges, and road conditions.

## 5.2 Predictors of Treatment

Treatment in our study is not randomly assigned. Because centralization of care occurred largely in response to centralization of the population, municipalities that experienced closures tend to have smaller baseline institutions. Moreover, because of the political significance of closures, there is the possibility of an anticipation effect: both those receiving and those providing care could be assumed to regard a closure as probable before a formalized decision is announced. From a policy perspective, our results do not rely on randomization of closures and inflows, as long as treatment timing is conditionally random. For this reason, however, our results, similar to Avdic et al. (2024) are not generalizable to all closures but instead closures of similar magnitudes.

We examine the pretreatment trends for a range of maternal health and background indicators as well as the number of births per half year for each municipality (Table 5). The results do not point to changing fertility patterns or maternal risk composition in anticipation of closures.

## 5.3 Closure

### 5.3.1 Short-Term Outcomes

From our Callaway and Sant'Anna (2021) inspired models, we estimate a post-treatment effect for the closure and inflow groups. The results are presented in Tables 7 and 8, with both only not-yet treated and not-yet and never-treated municipalities as the control group. Because some of our outcomes are only available starting in 1999, we separately run regressions for all outcomes using 2002 as the first possible treatment period (Table 8). Emergency c-section and 3$^{rd}$ and 4$^{th}$ degree lacerations, which are consistently registered from 1990 are included with the all-period results (Table 7), with 1993 as the first possible treatment year.

Among markers of infant health, we find no effect on one-month or one-year mortality following the closure of the baseline institution. This null finding is consistent with prior studies on mortality and closures in Norway and Sweden (Avdic et al., 2024; Grytten et al., 2014). However, because of the relative rarity of neonatal and infant death in Norway, we examine five-minute Apgar score as a more sensitive proxy for health status. When considered as a numeric outcome, we see a small decline in the average score following a closure of approximately 0.13 and 0.08 at the 1 % and 5 % level for our all-year and 1999 specifications, respectively. These declines are relative to average scores of 9.49 and 9.63 in the half-year prior to treatment. One explanation for the shift could be the increased access to anesthesia at the new assigned institutions. However, when we run a stratified regression we find a similar drop among those who are coded as not receiving anesthesia (Figure 4). Another possibility is that the drop reflects the subjective nature of Apgar as a metric and slight variations in scoring norms across institution types. Literature in general points to five-minute Apgar scores above six or seven as good, and we test along each of these thresholds: we find no change in the likelihood of receiving a score above either value, suggesting that the decline in average Apgar scores is not indicative of a change in well-being. However, as an additional check, we explore long-term markers of health to rule out later implications from the modest drop in Apgar scores. See Section 5.3.2.

Among maternal health outcomes, we find no change in the likelihood of experiencing either 3$^{rd}$ and 4$^{th}$ degree lacerations or hemorrhage of 500ml or more. One concern surrounding closures of midwifery-led units and small hospital maternity wards was the potential loss of personalized care that smaller-scale institutions are able to provide. Increased rates of c-sections could offset any impact on lacerations, however we find no change in the likelihood of experiencing such interventions across our results.

For our full period procedure-based outcomes, we find some evidence of distance effects. At baseline, home birth and birth outside of institution are very rare, accounting for only 0.08% and 0.6% of births for the closure group, respectively, in the the period prior to treatment. Following closures there is an increase in the likelihood of giving birth at home of 0.20 percentage points and a 0.38 percentage point increase for all birth outside of an institution. The effects are not found in the 1999 specification, however we do see an increase in births during transport. These events are also rare in the period prior to treatment, accounting for just 0.6% of deliveries in the closure group. Following closure though, the probability increases by 0.9 percentage points. Average driving distance increases by over 30 kilometers following closures, and we hypothesize the greater distance works in two ways. First, individuals may substitute from small, closing institutions to home births if they have a preference for less medicalized delivery or more personalized care. We are unable to differentiate between planned and unplanned home births prior to 1999, and among our 1999 outcomes we see no increase in the probability of planned home births when compared to not-yet and never treated. This could reflect a shortage of qualified midwives to attend home births as more institutions close or if home births become more popular among the control group. But when compared to only not-yet treated individuals we do find an increase in likelihood of both planned and unplanned home deliveries. Second, individuals may be unable to perfectly adjust to increased travel distances, for example by utilizing patient hotels, and may give birth outside of the initially planned setting. As the population of eligible users grows (i.e., as more areas become subject to catchment and non-catchment altering closures), there may be fewer ambulance services available and fewer back-up institutions capable of handling non-planned deliveries. This could explain why we find an increase in birth during transport from 1999, and no statistically significant effect on all out of institution births among the same subset of years.

While increased distance could result in procedures to increase control over timing of birth, we do not find evidence for increases in the likelihood of induction or c-section. We hypothesize, assuming that c-sections are appropriately allocated at baseline, that risk level is accurately identified during the extensive prenatal screening process; since centralization should not in theory impact prenatal care, closures do not alter this allocation. Given the resources and expertise required to administer epidurals, we anticipated increased access to epidurals might result in expanded usage among the closure group. Our lack of findings may reflect selection effects to higher resourced hospitals prior to closures.

An additional area of interest is whether centralization affects the decision to have children, and conditioned on having more than one child, the amount of time between deliveries. If centralization is experienced as a negative phenomenon, (potential) parents may be less inclined to have (any) additional children. While we are not able to isolate the possible effect on having zero children, we are able to analyze the effect of closures on the probability of having an additional child. We test along the margin of one to two children, and two to three children, as well as the time between births (Table 10). We find among closure municipalities no change in the number or spacing of children. This suggests in the short-term (i.e., four-year period), centralization does not alter fertility decisions.

#### 5.3.2 Long-Term Outcomes

One benefit of our extensive data timeline is the unique ability to consider long-term outcomes of centralization. Short-term impacts of closures can carry over into adulthood, but even absent an immediately observable effect at birth, an early-life health shock could manifest as poorer outcomes later in life. As a first step, we follow infants who are recorded in the Medical Birth Registry and assigned female at birth to adulthood and examine whether their treatment status as newborns influences their probability as adults of having any children themselves[11]. We find among the closure group no effect when compared to not-yet and never treated, or only not yet-treated (See Table 13 and Figure 9). As a second step, we analyze for those who do go on to have children, whether being exposed to a closure as an infant affects the likelihood of experiencing a range of negative health markers during pregnancy (asthma, arthritis, chronic hypertension, renal disease, diabetes, gestational diabetes, and epilepsy). Our estimates, presented in Table 14 suggest no impact on these markers from early-life exposure to treatment. Given that these health conditions are themselves risk factors for infant health, our results do not suggest an inter-generational effect of closures among our available measures. Our study is among the first to consider these possible long-term and inter-generational effects of centralization.

Although we are not able at this time to study long-term health status for all children recorded in the Medical Birth Registry, we do run a separate analysis to examine whether treatment status as newborns impacts educational achievement as adults (Table 14). 96.83% of the closure group born in the period prior to treatment goes on to begin high school by age 16, and we find a 1.0 percentage point increase in the probability post treatment. However among those who start by 16, we find no difference in the likelihood of graduating by age 22, (75.90% in the half-year pre-treatment) for the closure group when compared to the not-yet and never treated.

### 5.4 Inflow

Because closing institutions tend to have a small patient base, on average the inflow group experiences a fairly modest increase in the number of births at their assigned institution following a nearby closure. We hypothesize that this generally minor change in patient load accounts for the lack of evidence for a crowding effect. The only significant outcomes we find for the inflow group at the 5% level are a decline in the probability of hemorrhage and an increase in the probability of induction.

Greater likelihood of induction could indicate among procedure outcomes a degree of resource constraints. If extended labor and delivery times restrict staff availability, induction could be used to shorten the length of stay in hospital. Given that inductions can be indicated for advanced-age pregnancies (Rosenthal and Paterson-Brown, 1998; Walker et al., 2016), the increased probability could also reflect changing risk profiles among the inflow group. We do not find evidence, however, for higher maternal age (Table 6). As for hemorrhage, lower probability may point to quality gains if even marginal increases in the patient base have a “practice-makes-perfect” effect. Neither crowding nor positive volume effects are indicated across the remaining outcomes, however.

Because of the wide variation in the nature of inflow waves, we run an additional analysis on only those municipalities whose assigned institution experiences an increase in the patient population of over 15% following a nearby closure, and would thereby potentially be subject to crowding. In this sub-analysis we find a very modest increase in average 5-minute Apgar score of 0.026, a decline in the likelihood of experiencing hemorrhaging of 1.32 percentage points, and a decline in the likelihood of receiving a c-section of 1.04 percentage points (Table 9). citetAvdic202 find no

[11]If closures influence timing of childbirth, estimates for later treated cohorts may be biased. We rerun our regressions using different birth-year restrictions. Average mothers’ age at first birth increased from 27.9 in 2004 to 29.8 in 2019(Statistics Norway, 2024).

effect on the Apgar among the inflow group, but instead find signs of improved infant health along fetal stress and infant trauma. Although our estimate is statistically significant at the 5% level, the small magnitude of the effect would not seem to indicate a clear sign of quality associated with increased volume. The estimate for probability of hemorrhage is only slightly higher than in the full sample, but the group average in the half year period prior treatment is smaller at 5.99% compared to 10.49%. As in our main specification, it is possible that the change reflects a "practice-makes-perfect" effect. C-sections should for the most part in theory be determined during the prenatal screening process. Maternal health indicators do not seems to meaningfully change post treatment, with the exception of hypertension, which increases (Table 6). We are not able to observe all risk factors that are associated with c-sections, so it is not possible to determine if a lower probability of c-sections is a positive or negative effect. We also recognize that because we are not able to incorporate staffing levels across wards, we cannot say whether staff to patient ratios are consistent with a crowding effect.

### 5.5 Sensitivity Checks

#### 5.5.1 Treatment Timing

Although institutions close at different points throughout a calendar year, for the sake of tractability we generate closure and inflow treatment time based on the half of the year in which catchment regions change. Because of this rounding, there are some treated observations included in the period prior to treatment and some not-yet treated observations grouped into the first six months of treatment. This overlap is potentially compounded by the protracted nature of closures. However, given that closures are publicized and anticipated among providers and patients, it would seem likely that closure year is set too late as opposed to too early.

To test for this possibility, we rerun our analysis with closure set to one period (6 months) prior. As presented in Tables11 in our all year analysis, we find a slightly smaller decline in 5 minute Apgar score (-0.0869 compared to -0.133) and a slightly higher probability of birth outside institution (0.420 percentage points compared to 0.381 percentage points). The estimate for birth during transport is similar (0.230 percentage points compared to 0.209 percentage points) but is now statistically significant at the 5% level. Lack of personnel and resource concerns are noted as reasons for closures (Grytten et al., 2014; The Ministry of Health and Care Services, 1998), so a possible explanation for this difference is capacity constraints at institutions in the pre-treatment period. Pregnant individuals may have had to unexpectedly travel to larger hospitals if their assigned and planned institution could no longer accommodate them.

Table 12 presents the results for the 1999-2019 specification with treatment set to one period earlier. None of the maternal health or procedure outcomes are significant at the 5% level. We find a similar decline in 5 minute Apgar score (0.0806 compared to 0.0811) and at a lower significance level of 1%. With an earlier treatment period, we do find a 0.117 percentage point increase in probability for neonatal mortality at the 5%. It is possible that we are capturing a similar phenomenon of capacity constraints in the pre-closure period, but since we find no corresponding statistically significant impact on infant mortality, the estimate is not likely a treatment effect of closures.

## 6 Conclusion

The centralization of maternal care is an area of policy concern, particularly as demographic projections point to an increasingly centralized population in both Norway (Leknes and Løkken, 2020) and abroad (OECD and European Commission, 2020). Our analysis is the first to consider a range of both short and long-term outcomes, over nearly four decades, and for both closure and inflow groups. Our findings in general point to very limited effects of centralization. Among the closure group we find in the short term evidence of a distance effect in the form of increased probability of birth during transport or otherwise outside of an institution. While we do also find a decline in 5-minute Apgar score, the magnitude of the decline is very small and more likely reflects institutional norms around scoring as opposed to worsening health. In the long term, we find no evidence of a negative health shock from experiencing closures as a newborn. Across all infants in the closure group, the probability of starting high school by age 16 increases by 1 percentage point, and those who start by 16 have a similar graduation status by age 22. For those treated infants who are assigned female at birth, fertility patterns in adulthood do not seem to be affected nor are there signs of poorer health status during pregnancy. For the inflow group, centralization results on average in a relatively small influx of patients. On a weekly or monthly basis the small change in patient base is unlikely to result in any meaningful resource constraint. But even among cohorts that experience at least a 15% increase in patient load, there is no clear evidence of a crowding effect.

The consistency of a distance effect over time suggests that the ability to adjust is imperfect. Smaller, non-catchment institutions may have helped to mitigate distance effects early in the period of study by taking non-planned, emergency deliveries. We see however that as these institutions also closed, births during transport increased. While these deliveries are rare and we find no evidence for lasting effects, our findings highlight that provisions such as patient hotels might not perfectly compensate for greater travel distance. As maternal risk profiles may also change and as maternity wards continue to close, these distance effects may grow.

There are a number of limitations in our study. Our proxies for infant well-being are restricted to mortality and five-minute Apgar score, the first of which is very rare at baseline and the second of which is subject to clinicians' judgement. Other measures of fetal stress and infant trauma, for example lack of oxygen and birth injuries as studied by Avdic et al. (2024), may better capture the impact of centralization on newborns. Additionally, we are not able to observe the resource availability at institutions, such as the number of nurses, midwives, and doctors per patient. For this reason we cannot rule out that the lack of a crowding effect is due to a shift in resources to handle more patients as opposed to the limited influx in patients.

Despite these challenges, our study is able to uniquely contribute to knowledge on availability of care. Although centralization of health care services is occurring in many countries, our study focuses on the centralization of obstetric wards, holding access to municipal primary care constant. This allows us to separate the effects of closures on deliveries from the effects on prenatal care. Moreover, our rich registry data allows us to consider the effects of treatment across time. Because we begin our analysis in the 1980s, we can isolate long-term and inter-generational impacts of closures.

The extent to which our findings translate to other settings likely depends on a number of factors, including whether care is publicly provided, baseline population health status, the robustness of prenatal care, and the geographic dispersal of the population. Within Norway, our results may provide insight into other sectors of the health system in which primary care provides effective screening and point of contact services and in which risk factors are easily observed. Our preliminary analysis points to areas for further research, both within and outside of maternal care.

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## 7 Tables and Figures

Table 1: Summary Statistics, 1981-2019

| | Control | Closure | Inflow | All |
|---|---|---|---|---|
| *A. Maternal Background Characteristics and Risk Factors* | | | | |
| Age | 28.58 | 28.29 | 28.61 | 28.56 |
| Education above high school (%) | 38.29 | 32.02 | 36.70 | 37.00 |
| Cohabiting (%) | 91.31 | 91.62 | 91.47 | 91.40 |
| Born in high-income country (%) | 90.96 | 91.02 | 90.97 | 90.97 |
| Primiparous (%) | 39.44 | 39.19 | 41.34 | 40.12 |
| Asthma (%) | 3.66 | 3.42 | 3.48 | 3.56 |
| Hypertension (%) | 2.03 | 2.03 | 1.73 | 1.92 |
| Chronic renal disease (%) | 0.83 | 0.81 | 0.76 | 0.80 |
| Rheumatoid arthritis (%) | 0.40 | 0.33 | 0.37 | 0.38 |
| Epilespsy (%) | 0.67 | 0.72 | 0.65 | 0.67 |
| Diabetes (%) | 0.50 | 0.53 | 0.52 | 0.51 |
| Early gestation (%) | 5.38 | 5.56 | 5.45 | 5.43 |
| Birthweight (grams) | 3,528.88 | 3,535.05 | 3,532.27 | 3,530.84 |
| Male (%) | 51.39 | 51.59 | 51.50 | 51.45 |
| *B. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | 9.34 | 9.46 | 9.42 | 9.39 |
| Apgar score at 5 minutes $\geq$8 (%) | 97.72 | 97.68 | 97.96 | 97.81 |
| Apgar score at 5 minutes $\geq$7 (%) | 98.87 | 98.84 | 98.97 | 98.90 |
| Neonatal mortality (%) | 0.20 | 0.21 | 0.18 | 0.19 |
| Infant mortality (%) | 0.37 | 0.36 | 0.34 | 0.36 |
| *C. Maternal Health Outcomes* | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations* (%) | 2.22 | 1.93 | 2.29 | 2.21 |
| Hemorrhage $\geq$ 500ml (%) | 12.05 | 12.21 | 13.46 | 12.59 |
| *D. Birthing Procedures* | | | | |
| Birth during transport (%) | 0.30 | 0.52 | 0.29 | 0.32 |
| C-section (%) | 13.22 | 14.65 | 13.17 | 13.36 |
| Emergency c-section* (%) | 8.67 | 9.18 | 9.19 | 8.92 |
| Home births (all) (%) | 0.44 | 0.44 | 0.39 | 0.42 |
| Birth outside institution+ (%) | 0.81 | 1.06 | 0.71 | 0.80 |
| Municipality count | 190 | 48 | 104 | 342 |
| Infants | 940674 | 205022 | 684946 | 1830642 |
| Number of baseline institutions | 27 | 19 | 14 | 60 |

*Notes*: Data from MBR and NUDB for the years 1981-2019. Each column reports the mean value for the treatment group or entire sample, based on all observations for which information is available. The variable hypertension includes both chronic hypertension and hypertension under pregnancy, while the variable diabetes refers to only pregestational diabetes. We define early gestation as before 37 weeks.

* Variable is presented from 1990-2019.

\+ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 2: Summary Statistics Pre and Post Treatment, 1981-2019

| | Control | | Closure | | Inflow | |
|---|---|---|---|---|---|---|
| | Pre-Treat | Post-Treat | Pre-Treat | Post-Treat | Pre-Treat | Post-Treat |
| *A. Maternal Background Characteristics and Risk Factors* | | | | | | |
| Age | 28.17 | 28.61 | 27.60 | 27.92 | 27.85 | 28.31 |
| Education above high school (%) | 33.46 | 37.62 | 25.81 | 28.37 | 31.43 | 35.28 |
| Cohabiting (%) | 90.22 | 92.51 | 89.51 | 92.37 | 90.41 | 91.82 |
| Born in high-income country (%) | 93.80 | 91.88 | 95.80 | 94.22 | 94.80 | 93.41 |
| Primiparous (%) | 39.58 | 39.26 | 39.09 | 40.17 | 42.38 | 41.21 |
| Asthma (%) | 3.21 | 3.55 | 3.02 | 2.92 | 2.49 | 2.79 |
| Hypertension (%) | 1.97 | 1.92 | 2.38 | 1.81 | 1.70 | 1.72 |
| Chronic renal disease (%) | 0.91 | 0.81 | 1.02 | 0.90 | 0.90 | 0.73 |
| Rheumatoid arthritis (%) | 0.40 | 0.41 | 0.30 | 0.38 | 0.40 | 0.35 |
| Epilespsy (%) | 0.66 | 0.67 | 0.74 | 0.62 | 0.64 | 0.63 |
| Diabetes (%) | 0.43 | 0.49 | 0.38 | 0.49 | 0.37 | 0.48 |
| Early gestation (%) | 5.42 | 5.52 | 5.41 | 5.77 | 5.38 | 5.47 |
| Birthweight | 3,529.03 | 3,534.21 | 3,541.66 | 3,540.28 | 3,518.84 | 3,530.62 |
| Male (%) | 51.44 | 51.44 | 51.49 | 52.01 | 51.53 | 51.24 |
| *B. Infant Health Outcomes* | | | | | | |
| Apgar score at 5 minutes | 9.29 | 9.32 | 9.48 | 9.38 | 9.28 | 9.28 |
| Apgar score at 5 minutes $\geq 8$ (%) | 97.84 | 97.77 | 97.96 | 97.54 | 98.03 | 98.04 |
| Apgar score at 5 minutes $\geq 7$ (%) | 98.91 | 98.90 | 99.00 | 98.64 | 99.01 | 98.97 |
| Neonatal mortality (%) | 0.23 | 0.20 | 0.25 | 0.26 | 0.22 | 0.20 |
| Infant mortality (%) | 0.43 | 0.36 | 0.50 | 0.42 | 0.46 | 0.39 |
| *C. Maternal Health Outcomes* | | | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations* (%) | 2.34 | 2.33 | 1.87 | 2.06 | 2.25 | 2.28 |
| Hemorrhage $\geq$ 500ml (%) | 9.30 | 11.25 | 6.63 | 8.83 | 9.24 | 10.69 |
| *D. Birthing Procedures* | | | | | | |
| Birth during transport (%) | 0.30 | 0.31 | 0.28 | 0.61 | 0.26 | 0.31 |
| C-section (%) | 12.59 | 13.21 | 12.59 | 13.02 | 12.63 | 12.72 |
| Emergency c-section* (%) | 8.17 | 8.29 | 8.64 | 8.50 | 9.16 | 9.16 |
| Home births (all) (%) | 0.40 | 0.48 | 0.19 | 0.36 | 0.34 | 0.50 |
| Birth outside institution$^{+}$ (%) | 0.77 | 0.86 | 0.51 | 1.08 | 0.62 | 0.84 |

*Notes*: Data from MBR and NUDB for the years 1981-2019. Each column reports the mean value in the four years pre or post treatment for the treatment group and placebo treatment for the control group, based on all observations for which information is available. The variable hypertension includes both chronic hypertension and hypertension under pregnancy, while the variable diabetes refers to only pregestational diabetes. We define early gestation as before 37 weeks.

* Variable is presented for treatments from 1993-2019.

$^{+}$ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 3: Summary Statistics, 1999-2019

| | Control | Closure | Inflow | All |
|---|---|---|---|---|
| *A. Maternal Background Characteristics and Risk Factors* | | | | |
| Age | 29.59 | 29.05 | 29.57 | 29.57 |
| Education above high school (%) | 49.38 | 41.94 | 53.12 | 49.66 |
| Cohabiting (%) | 93.75 | 94.25 | 93.80 | 93.77 |
| Born in high-income country (%) | 86.10 | 89.62 | 88.04 | 86.51 |
| Primiparous (%) | 39.00 | 35.59 | 42.39 | 39.37 |
| Asthma (%) | 5.00 | 4.13 | 4.31 | 4.87 |
| Hypertension (%) | 2.23 | 2.28 | 2.56 | 2.28 |
| Chronic renal disease (%) | 0.67 | 0.51 | 0.55 | 0.65 |
| Rheumatoid arthritis (%) | 0.39 | 0.32 | 0.37 | 0.39 |
| Epilespsy (%) | 0.74 | 0.82 | 0.74 | 0.75 |
| Diabetes (%) | 0.68 | 0.71 | 0.67 | 0.68 |
| Early gestation (%) | 5.52 | 5.35 | 5.60 | 5.53 |
| Birthweight | 3,529.36 | 3,565.93 | 3,527.05 | 3,530.35 |
| Male (%) | 51.27 | 51.86 | 51.52 | 51.33 |
| *B. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | 9.44 | 9.59 | 9.57 | 9.46 |
| Apgar score at 5 minutes $\geq 8$ (%) | 97.46 | 97.76 | 97.72 | 97.51 |
| Apgar score at 5 minutes $\geq 7$ (%) | 98.75 | 98.89 | 98.82 | 98.77 |
| Neonatal mortality (%) | 0.12 | 0.15 | 0.10 | 0.12 |
| Infant mortality (%) | 0.20 | 0.21 | 0.16 | 0.19 |
| *C. Maternal Health Outcomes* | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations (%) | 2.21 | 1.81 | 2.34 | 2.21 |
| Hemorrhage $\geq$ 500ml (%) | 17.73 | 17.98 | 20.23 | 18.11 |
| *D. Birthing Procedures* | | | | |
| Epidural (%) | 28.59 | 25.61 | 30.13 | 28.70 |
| Birth during transport (%) | 0.32 | 1.28 | 0.28 | 0.35 |
| C-section (%) | 15.23 | 16.11 | 15.78 | 15.34 |
| Emergency c-section (%) | 9.39 | 9.55 | 10.00 | 9.49 |
| Home births (all) (%) | 0.54 | 0.42 | 0.43 | 0.52 |
| Planned home births (%) | 0.25 | 0.12 | 0.16 | 0.23 |
| Unplanned home births (%) | 0.28 | 0.30 | 0.27 | 0.28 |
| Induction (%) | 17.06 | 17.25 | 16.74 | 17.02 |
| Birth outside institution$^{+}$ (%) | 0.94 | 1.96 | 0.74 | 0.95 |

*Notes*: Data from MBR and NUDB for the years 1999-2019. Each column reports the mean value for the treatment group or entire sample, based on all observations for which information is available. The variable hypertension includes both chronic hypertension and hypertension under pregnancy, while the variable diabetes refers to only pregestational diabetes. We define early gestation as before 37 weeks.

$^{+}$ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 4: Summary Statistics Pre and Post Treatment, 1999-2019

| | Control | | Closure | | Inflow | |
|---|---|---|---|---|---|---|
| | Pre-Treat | Post-Treat | Pre-Treat | Post-Treat | Pre-Treat | Post-Treat |
| *A. Maternal Background Characteristics and Risk Factors* | | | | | | |
| Age | 29.37 | 29.62 | 28.89 | 29.22 | 29.56 | 29.70 |
| Education above high school (%) | 46.07 | 49.95 | 42.01 | 42.78 | 54.37 | 58.20 |
| Cohabiting (%) | 93.02 | 94.16 | 91.47 | 94.78 | 92.04 | 94.00 |
| Born in high-income country (%) | 88.69 | 85.99 | 89.48 | 87.34 | 88.04 | 85.74 |
| Primiparous (%) | 38.82 | 38.99 | 35.18 | 35.19 | 43.93 | 42.52 |
| Asthma (%) | 4.88 | 4.87 | 5.15 | 4.37 | 4.25 | 3.95 |
| Hypertension (%) | 2.29 | 2.17 | 2.12 | 2.50 | 2.78 | 2.66 |
| Chronic renal disease (%) | 0.70 | 0.67 | 0.44 | 0.83 | 0.60 | 0.67 |
| Rheumatoid arthritis (%) | 0.37 | 0.38 | 0.22 | 0.23 | 0.40 | 0.41 |
| Epilespsy (%) | 0.75 | 0.75 | 0.66 | 0.83 | 0.85 | 0.73 |
| Diabetes (%) | 0.68 | 0.73 | 0.60 | 0.37 | 0.65 | 0.76 |
| Early gestation (%) | 5.70 | 5.56 | 5.82 | 5.26 | 5.10 | 5.32 |
| Birthweight | 3,532.97 | 3,529.18 | 3,549.62 | 3,555.80 | 3,509.46 | 3,505.67 |
| Male (%) | 51.25 | 51.11 | 51.52 | 51.47 | 51.89 | 50.58 |
| *B. Infant Health Outcomes* | | | | | | |
| Apgar score at 5 minutes | 9.40 | 9.44 | 9.64 | 9.60 | 9.57 | 9.60 |
| Apgar score at 5 minutes $\geq 8$ (%) | 97.52 | 97.49 | 98.29 | 97.36 | 97.52 | 97.67 |
| Apgar score at 5 minutes $\geq 7$ (%) | 98.78 | 98.80 | 99.25 | 98.42 | 98.71 | 98.68 |
| Neonatal mortality (%) | 0.12 | 0.12 | 0.12 | 0.10 | 0.12 | 0.05 |
| Infant mortality (%) | 0.21 | 0.19 | 0.25 | 0.20 | 0.22 | 0.11 |
| *C. Maternal Health Outcomes* | | | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations (%) | 2.42 | 2.31 | 1.33 | 1.57 | 2.39 | 1.74 |
| Hemorrhage $\geq$ 500ml (%) | 15.53 | 17.51 | 15.57 | 20.63 | 21.86 | 24.23 |
| *D. Birthing Procedures* | | | | | | |
| Epidural (%) | 26.40 | 28.42 | 22.29 | 26.18 | 28.12 | 30.79 |
| Birth during transport (%) | 0.32 | 0.32 | 0.88 | 1.57 | 0.25 | 0.24 |
| C-section (%) | 15.07 | 15.43 | 15.95 | 15.96 | 16.13 | 16.82 |
| Emergency c-section (%) | 9.15 | 9.46 | 9.38 | 9.57 | 10.20 | 10.65 |
| Home births (all) (%) | 0.51 | 0.57 | 0.38 | 0.47 | 0.53 | 0.49 |
| Planned home births (%) | 0.24 | 0.26 | 0.06 | 0.20 | 0.17 | 0.22 |
| Unplanned home births (%) | 0.28 | 0.30 | 0.32 | 0.27 | 0.35 | 0.27 |
| Induction (%) | 15.34 | 17.08 | 15.57 | 18.56 | 17.43 | 19.94 |
| Birth outside institution (%) | 0.92 | 0.98 | 1.42 | 2.40 | 0.78 | 0.75 |

*Notes*: Data from MBR and NUDB for the years 1999-2019. Each column reports the mean value in the four years pre or post treatment for the treatment group and placebo treatment for the control group, based on all observations for which information is available. The variable hypertension includes both chronic hypertension and hypertension under pregnancy, while the variable diabetes refers to only pregestational diabetes. We define early gestation as before 37 weeks.

$^{+}$ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 5: Maternal Background and Municipality Characteristics, Pre Treatment Trends

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Maternal Background & Birth* | | | | |
| Mother's Age | 0.00763 | 0.000603 | 0.00302 | 0.0259 |
| | (0.0175) | (0.0195) | (0.00899) | (0.0175) |
| Civil Status | -0.000526 | -0.000739 | -0.00177* | -0.00126 |
| | (0.00144) | (0.00144) | (0.000977) | (0.00125) |
| High-Income Country | -0.0000920 | 0.00175 | 0.000411 | 0.000435 |
| | (0.00103) | (0.00154) | (0.000336) | (0.000584) |
| Primiparous | -0.000866 | 0.000895 | 0.000263 | 0.0000475 |
| | (0.00184) | (0.00233) | (0.000923) | (0.00104) |
| Asthma | 0.000606 | 0.000125 | 0.000164 | 0.000240 |
| | (0.000680) | (0.00122) | (0.000345) | (0.000464) |
| Hypertension | 0.000463 | 0.000275 | -0.0000927 | -0.000545 |
| | (0.000598) | (0.000647) | (0.000269) | (0.000339) |
| Arthritis | -0.000152 | 0.000142 | -0.000102 | -0.000158 |
| | (0.000190) | (0.000283) | (0.000140) | (0.000173) |
| Epilepsy | 0.0000796 | -0.000302 | -0.0000770 | -0.000618 |
| | (0.000273) | (0.000491) | (0.000191) | (0.000391) |
| Diabetes | 0.0000247 | -0.000133 | -0.0000239 | 0.00000682 |
| | (0.000223) | (0.000248) | (0.000108) | (0.000261) |
| Birthweight | 1.972 | 1.284 | 3.310** | 0.689 |
| | (2.396) | (3.014) | (1.616) | (2.384) |
| Male | -0.00235 | -0.00122 | 0.0000524 | 0.000386 |
| | (0.00152) | (0.00219) | (0.000895) | (0.00134) |
| *B. Municipality* | | | | |
| Births Per Half Year | -0.148 | 0.0111 | 0.365 | 0.462 |
| | (0.182) | (0.214) | (0.386) | (0.426) |

*Notes*: For each indicator we estimate an average pre treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the fours years prior to the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other.
*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

Table 6: Maternal Background, Birth and Municipality Characteristics, Post Treatment Trends

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Maternal Background & Birth* | | | | |
| Mother's Age | -0.221* | -0.360** | 0.0570 | 0.0786 |
| | (0.127) | (0.163) | (0.0477) | (0.0635) |
| Civil Status | 0.00379 | -0.00569 | -0.00674 | -0.00656 |
| | (0.00832) | (0.00951) | (0.00481) | (0.00507) |
| High-Income Country | 0.000456 | -0.00591 | 0.00291 | -0.00128 |
| | (0.00393) | (0.00475) | (0.00370) | (0.00289) |
| Primiparous | 0.0185 | 0.0181 | -0.00206 | 0.00420 |
| | (0.0133) | (0.0162) | (0.00585) | (0.00703) |
| Asthma | -0.00829** | -0.00605 | -0.00124 | -0.000732 |
| | (0.00403) | (0.00538) | (0.00194) | (0.00236) |
| Hypertension | -0.00297 | -0.00551* | 0.00320*** | 0.00252* |
| | (0.00310) | (0.00329) | (0.00124) | (0.00145) |
| Arthritis | 0.00118 | 0.00125 | -0.000330 | -0.000501 |
| | (0.00101) | (0.00153) | (0.000615) | (0.000800) |
| Epilepsy | -0.000905 | -0.00144 | 0.000362 | 0.0000930 |
| | (0.00153) | (0.00202) | (0.00111) | (0.00121) |
| Diabetes | 0.00151 | 0.00202 | 0.000471 | 0.000424 |
| | (0.00122) | (0.00148) | (0.000515) | (0.000691) |
| Birthweight | -5.808 | 4.046 | -3.846 | 6.991 |
| | (12.24) | (16.11) | (7.474) | (8.856) |
| Male | 0.00589 | 0.00864 | -0.00902 | -0.0103 |
| | (0.00984) | (0.0133) | (0.00633) | (0.00865) |
| *B. Municipality* | | | | |
| Births Per Half Year | 0.806 | 1.564 | 0.0305 | -0.378 |
| | (1.187) | (1.528) | (1.283) | (1.494) |

*Notes*: For each indicator we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the fours years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other.
*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

Table 7: Effect of Closures on Infant and Maternal Health and Procedures, 1981-2019

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | -0.133*** | -0.127*** | 0.000104 | 0.0230** |
| | (0.0240) | (0.0319) | (0.0107) | (0.0116) |
| Apgar score at 5 minutes ≥8 | -0.00455 | -0.00353 | 0.00285 | 0.00312 |
| | (0.00444) | (0.00584) | (0.00184) | (0.00203) |
| Apgar score at 5 minutes ≥7 | -0.00290 | -0.00122 | -0.00159 | -0.00123 |
| | (0.00307) | (0.00362) | (0.00111) | (0.00119) |
| Neonatal mortality | 0.00156 | 0.00128 | 0.000297 | 0.00128 |
| | (0.000993) | (0.00156) | (0.000461) | (0.00156) |
| Infant mortality | 0.00121 | 0.00156 | 0.000617 | 0.00125 |
| | (0.00140) | (0.00195) | (0.000688) | (0.000836) |
| *B. Maternal Health Outcomes* | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations$^{+}$ | -0.000334 | 0.00273 | 0.000806 | 0.000608 |
| | (0.00328) | (0.00407) | (0.00152) | (0.00243) |
| Hemorrhage ≥500 ml | 0.00667 | -0.00521 | -0.0122*** | -0.0105** |
| | (0.00524) | (0.00740) | (0.00385) | (0.00487) |
| *C. Birth Procedures* | | | | |
| C-section | -0.00966 | -0.00699 | 0.00183 | 0.00286 |
| | (0.0120) | (0.0137) | (0.00360) | (0.00538) |
| Emergency C-section$^{+}$ | -0.0102 | -0.00592 | 0.00231 | 0.00706 |
| | (0.0112) | (0.0136) | (0.00402) | (0.00730) |
| Home birth (all) | 0.00208** | 0.00292*** | 0.000725 | 0.00145 |
| | (0.000842) | (0.00108) | (0.000833) | (0.000952) |
| Birth during transport | 0.00209 | 0.000833 | 0.000664 | 0.000908 |
| | (0.00146) | (0.00158) | (0.000669) | (0.000784) |
| Birth outside of institution$^{§}$ | 0.00381** | 0.00294 | 0.00117 | 0.00206* |
| | (0.00177) | (0.00216) | (0.00119) | (0.00121) |

*Notes*: For each outcome we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the fours years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other, and no pre-treatment covariates are included.

*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

$^{+}$Estimates are presented for 1990-2019, with 1993 as the first possible treatment year.

$^{§}$ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 8: Effect of Closures on Infant and Maternal Health and Procedures, 1999-2019

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | -0.0811** | 0.000739 | -0.0363* | -0.0440 |
| | (0.0390) | (0.0785) | (0.0199) | (0.0328) |
| Apgar score at 5 minutes ≥8 | -0.00604 | 0.0196 | 0.00534 | -0.00429 |
| | (0.00787) | (0.0161) | (0.00434) | (0.00645) |
| Apgar score at 5 minutes ≥7 | -0.00750 | 0.0103 | -0.00143 | -0.00676** |
| | (0.00468) | (0.00983) | (0.00301) | (0.00288) |
| Neonatal mortality | -0.000410 | -0.0123* | -0.000245 | -0.0123* |
| | (0.00183) | (0.00640) | (0.000315) | (0.00640) |
| Infant mortality | -0.00123 | -0.00654 | 0.000287 | 0.00222 |
| | (0.00226) | (0.00427) | (0.000505) | (0.00247) |
| *B. Maternal Health Outcomes* | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations | 0.00235 | -0.00187 | 0.00162 | 0.00290 |
| | (0.00333) | (0.00598) | (0.00226) | (0.00771) |
| Hemorrhage ≥ 500ml | 0.0173 | 0.0129 | -0.0213*** | -0.0163 |
| | (0.0146) | (0.0233) | (0.00713) | (0.0334) |
| *C. Birth Procedures* | | | | |
| C-section | -0.0170 | 0.0535*** | 0.00400 | -0.0175 |
| | (0.0213) | (0.0202) | (0.00495) | (0.0189) |
| Emergency c-section | -0.0158 | 0.0152 | 0.00626 | -0.00139 |
| | (0.0180) | (0.0152) | (0.00460) | (0.0113) |
| Home birth (all) | 0.00146 | 0.0137*** | -0.00171 | 0.00661** |
| | (0.00251) | (0.00282) | (0.00105) | (0.00312) |
| Planned home birth | 0.000740 | 0.00500** | -0.000935 | 0.00493** |
| | (0.000671) | (0.00213) | (0.000665) | (0.00229) |
| Unplanned home birth | 0.000721 | 0.00942*** | -0.000771 | 0.00168 |
| | (0.00231) | (0.00263) | (0.000800) | (0.00295) |
| Birth during transport | 0.00932** | 0.00869*** | -0.00125 | -0.00596 |
| | (0.00442) | (0.00315) | (0.00120) | (0.00759) |
| Birth outside of institution§ | 0.00915 | 0.00985 | -0.00294* | 0.000542 |
| | (0.00577) | (0.0138) | (0.00155) | (0.0104) |
| Induction | 0.0388 | -0.00704 | 0.0223** | -0.0279** |
| | (0.0271) | (0.0295) | (0.0101) | (0.0124) |
| Epidural | 0.0163 | 0.0371** | 0.0189 | 0.0272** |
| | (0.0222) | (0.0179) | (0.0216) | (0.0117) |

*Notes*: For each outcome we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the four years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other, and no pre-treatment covariates are included.
*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.
§ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 9: Sensitivity Analysis - Effect of Closures on Subset of Inflow Group, 1981-2019

| | Inflow | |
|---|---|---|
| | Not-Yet & Never | Only Not-Yet |
| *A. Infant Health Outcomes* | | |
| Apgar score at 5 minutes | 0.0260** | 0.0270* |
| | (0.0107) | (0.0159) |
| Apgar score at 5 minutes ≥8 | 0.00366* | 0.00605* |
| | (0.00209) | (0.00336) |
| Apgar score at 5 minutes ≥7 | -0.000548 | -0.0000984 |
| | (0.00114) | (0.00152) |
| Neonatal mortality | -0.000221 | 0.000281 |
| | (0.000837) | (0.00123) |
| Infant mortality | -0.000397 | 0.000324 |
| | (0.00129) | (0.00182) |
| *B. Maternal Health Outcomes* | | |
| 3rd and 4th degree lacerations+ | -0.00121 | 0.00118 |
| | (0.00266) | (0.00552) |
| Hemorrhage ≥500 ml | -0.0132** | -0.00418 |
| | (0.00525) | (0.00665) |
| *C. Birth Procedures* | | |
| C-section | -0.0104** | -0.0156*** |
| | (0.00456) | (0.00538) |
| Emergency c-section+ | -0.00802 | -0.0165** |
| | (0.00645) | (0.00766) |
| Home birth (all) | 0.00170* | 0.00214* |
| | (0.000900) | (0.00124) |
| Birth during transport | 0.000969 | -0.000329 |
| | (0.00105) | (0.00142) |
| Birth outside of institution§ | 0.00201 | 0.00215 |
| | (0.00147) | (0.00189) |

*Notes*: We rerun our Callaway and Sant'Anna's (2021) regressions, only including inflow cohorts who experienced an increase in their patient base of at least 15% following a nearby closure.

+Estimates are presented for 1990-2019, with 1993 as the first possible treatment year.

*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

§ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 10: Effect of Closures on Number of Children and Timing

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| 1 to 2 children | -0.0140 | -0.00671 | 0.00374 | -0.00135 |
| | (0.0164) | (0.0203) | (0.00715) | (0.00803) |
| 2 to 3 children | -0.0118 | -0.0277* | -0.0117 | -0.0141* |
| | (0.0121) | (0.0158) | (0.00750) | (0.00853) |
| Months Between Deliveries | 0.144 | -0.892 | -0.487 | -0.367 |
| | (0.730) | (1.005) | (0.421) | (0.457) |

*Notes*: For each outcome we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the fours years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other, and no pre-treatment covariates are included.
*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

Table 11: Sensitivity Analysis - Effect of Closures on Infant and Maternal Health and Procedures, 1981-2019 - Treatment Set One Period Prior

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | -0.0869*** | -0.0524 | -0.00238 | 0.00229 |
| | (0.0240) | (0.0322) | (0.00997) | (0.0114) |
| Apgar score at 5 minutes ≥8 | -0.00146 | 0.0000429 | 0.000564 | -0.000628 |
| | (0.00281) | (0.00364) | (0.00132) | (0.00168) |
| Apgar score at 5 minutes ≥7 | -0.000988 | -0.000274 | 0.000850 | 0.000316 |
| | (0.00293) | (0.00359) | (0.00111) | (0.00102) |
| Neonatal mortality | 0.000559 | 0.0000124 | -0.000536 | 0.0000124 |
| | (0.00107) | (0.00155) | (0.000620) | (0.00155) |
| Infant mortality | 0.00222* | 0.00244 | -0.000491 | -0.000538 |
| | (0.00134) | (0.00175) | (0.000969) | (0.00111) |
| *B. Maternal Health Outcomes* | | | | |
| 3rd and 4th degree lacerations+ | 0.00430 | 0.00507 | -0.00550 | -0.000269 |
| | (0.00324) | (0.00401) | (0.00420) | (0.00421) |
| Hemorrhage ≥500 ml | 0.00915* | -0.00111 | -0.00783* | -0.00731* |
| | (0.00534) | (0.00575) | (0.00416) | (0.00439) |
| *C. Birth Procedures* | | | | |
| C-section | -0.00787 | -0.00784 | -0.000755 | -0.00248 |
| | (0.00873) | (0.0105) | (0.00421) | (0.00475) |
| *C. Birth Procedures* | | | | |
| Emergency C-section+ | -0.0153* | -0.0101 | 0.00390 | 0.00146 |
| | (0.00845) | (0.0123) | (0.00576) | (0.00763) |
| Home birth (all) | 0.00108 | 0.000950 | -0.000682 | -0.0000611 |
| | (0.000839) | (0.000946) | (0.000676) | (0.000900) |
| Birth during transport | 0.00230** | 0.00161 | 0.000388 | 0.000518 |
| | (0.00117) | (0.00110) | (0.000587) | (0.000847) |
| Birth outside of institution§ | 0.00420*** | 0.00383** | -0.000553 | 0.000276 |
| | (0.00150) | (0.00153) | (0.000878) | (0.00100) |

*Notes*: We re-run our Callaway and Sant'Anna's (2021) difference-in-differences framework, setting the time of treatment to one period (6 months) prior. We report the post average treatment effect for the four years following the new treatment date. No pre-treatment covariates are included.

+Estimates are presented for 1990-2019, with 1992 as the first possible treatment year.

*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

§ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 12: Sensitivity Analysis - Effect of Closures on Infant and Maternal Health and Procedures, 1999-2019 - Treatment Set One Period Prior

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | -0.0806*** | 0.0219 | -0.00835 | -0.136*** |
| | (0.0294) | (0.0445) | (0.0153) | (0.0476) |
| Apgar score at 5 minutes ≥8 | -0.00483 | -0.00101 | 0.00177 | -0.0139** |
| | (0.00443) | (0.00407) | (0.00265) | (0.00542) |
| Apgar score at 5 minutes ≥7 | -0.00323 | 0.00946* | 0.00222 | -0.0133*** |
| | (0.00395) | (0.00558) | (0.00258) | (0.00340) |
| Neonatal mortality | 0.00117** | 0.00618*** | -0.000876 | 0.00618*** |
| | (0.000485) | (0.00231) | (0.000706) | (0.00231) |
| Infant mortality | 0.000966 | 0.00839*** | -0.00107 | 0.00410 |
| | (0.00179) | (0.00162) | (0.000816) | (0.00252) |
| *B. Maternal Health Outcomes* | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations | 0.00828* | 0.0141 | -0.00927* | 0.00264 |
| | (0.00443) | (0.0142) | (0.00484) | (0.00703) |
| Hemorrhage ≥500 ml | 0.0206 | -0.0258 | -0.0172* | -0.0194 |
| | (0.0167) | (0.0301) | (0.00906) | (0.0444) |
| *C. Birth Procedures* | | | | |
| C-section | -0.00373 | 0.0164 | 0.00791 | 0.0116 |
| | (0.0198) | (0.0241) | (0.00946) | (0.0179) |
| Emergency c-section | -0.0118 | -0.00149 | 0.00614 | -0.00505 |
| | (0.0151) | (0.0157) | (0.00722) | (0.0112) |
| Home birth (all) | 0.00105 | -0.00174 | -0.000330 | 0.00693** |
| | (0.00163) | (0.00574) | (0.00119) | (0.00316) |
| Planned home birth | 0.000778 | 0.00301 | -0.000983* | 0.00426* |
| | (0.000650) | (0.00232) | (0.000565) | (0.00237) |
| Unplanned home birth | 0.000269 | -0.00368 | 0.000652 | 0.00267 |
| | (0.00158) | (0.00601) | (0.000892) | (0.00308) |
| Birth during transport | 0.00151 | -0.00390 | -0.000250 | 0.00392** |
| | (0.00467) | (0.00574) | (0.00105) | (0.00199) |
| Birth outside of institution§ | 0.00621 | 0.00223 | -0.000961 | 0.00523 |
| | (0.00524) | (0.00472) | (0.00158) | (0.00506) |
| Induction | 0.00320 | 0.0444** | 0.00216 | -0.0175* |
| | (0.0128) | (0.0214) | (0.00519) | (0.0100) |
| Epidural | 0.0356* | -0.0288 | 0.0403*** | 0.0286 |
| | (0.0206) | (0.0202) | (0.0131) | (0.0332) |

*Notes*: We re-run our Callaway and Sant'Anna's (2021) difference-in-differences framework, setting the time of treatment to one period (6 months) prior. We report the post average treatment effect for the four years following the new treatment date. No pre-treatment covariates are included.
*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.
§ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

Table 13: Long-Term Effect of Closures on Likelihood of Giving Birth

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| Likelihood Births 2001 | -0.00259 | 0.00933 | -0.0172** | -0.0186*** |
| | (0.0158) | (0.0174) | (0.00827) | (0.00713) |
| Likelihood Births 1995 | -0.00441 | 0.00289 | -0.0187* | -0.0179** |
| | (0.0192) | (0.0214) | (0.00983) | (0.00843) |
| Likelihood Births 1990 | -0.0115 | -0.00421 | -0.0172* | -0.0110 |
| | (0.0220) | (0.0240) | (0.0103) | (0.0117) |

*Notes*: For individuals assigned female at birth, we examine whether experiencing a direct or indirect closure as an infant impacts the likelihood in adulthood of giving birth. Because treatment could alter maternal age at first birth, we run three iterations of our analysis and vary the included cohorts. For each outcome we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the fours years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other.
*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

Table 14: Long-Term Effect of Closures on Health and Education

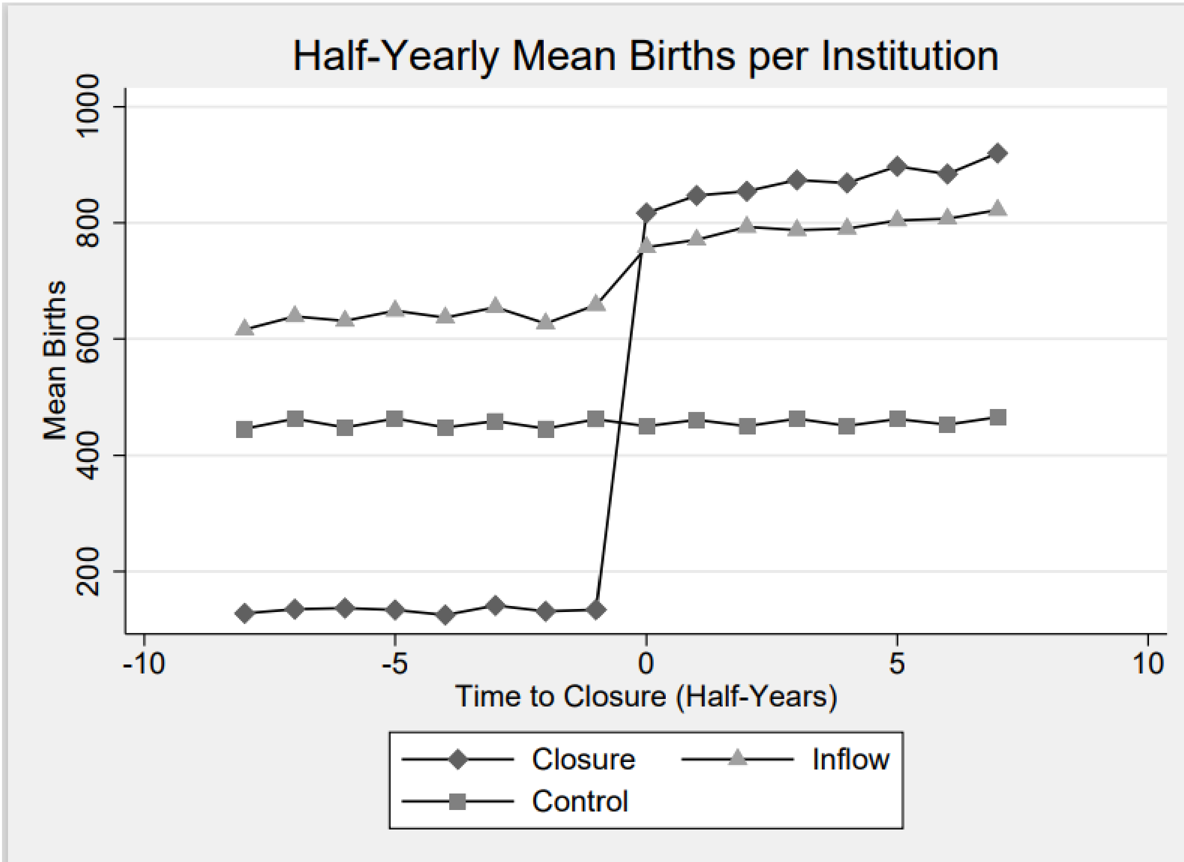
| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Health During Pregnancy* | | | | |
| Asthma | 0.00966 | 0.00971 | 0.0155 | 0.0198 |
| | (0.0110) | (0.0100) | (0.0133) | (0.0158) |
| Chronic hypertension | 0.00207* | 0.00333* | 0.00153 | -0.00112 |
| | (0.00112) | (0.00198) | (0.00298) | (0.00226) |
| Renal disease | 0.00966 | 0.00971 | 0.0155 | 0.0198 |
| | (0.0110) | (0.0100) | (0.0133) | (0.0158) |
| Arthritis | 0.000154 | -0.00196 | -0.00568 | -0.0100** |
| | (0.00396) | (0.00445) | (0.00383) | (0.00482) |
| Epilepsy | -0.00324 | -0.00702 | -0.00192 | -0.000739 |
| | (0.00715) | (0.00806) | (0.00301) | (0.00333) |
| Diabetes+ | 0.00000805 | -0.00185 | 0.00153 | -0.00180 |
| | (0.00547) | (0.00698) | (0.00223) | (0.00283) |
| Gestational Diabetes | 0.000750 | -0.00331 | -0.00706 | -0.00710 |
| | (0.0121) | (0.0145) | (0.00668) | (0.00671) |
| Any health | 0.0125 | -0.00892 | 0.00958 | 0.00746 |
| | (0.0265) | (0.0266) | (0.0147) | (0.0163) |
| Mother's age at first birth | 0.200 | 0.133 | 0.0122 | 0.0376 |
| | (0.170) | (0.166) | (0.137) | (0.128) |
| *B. Education* | | | | |
| Begin High School by 16 | 0.0100*** | 0.0104*** | 0.00136 | 0.00159 |
| | (0.00337) | (0.00359) | (0.00171) | (0.00188) |
| Graduate by 22 | -0.0147 | -0.0181 | -0.00344 | -0.00600 |
| | (0.0111) | (0.0127) | (0.00613) | (0.00755) |

*Notes*: For each outcome we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the fours years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other. Maternal characteristics include observations we identify twice in the registry, first as infants and then as mothers. The education outcome, "Begin High School by 16" includes all infants born before 2003, and the outcome "Graduate by 22" includes all infants born before 1997.

*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

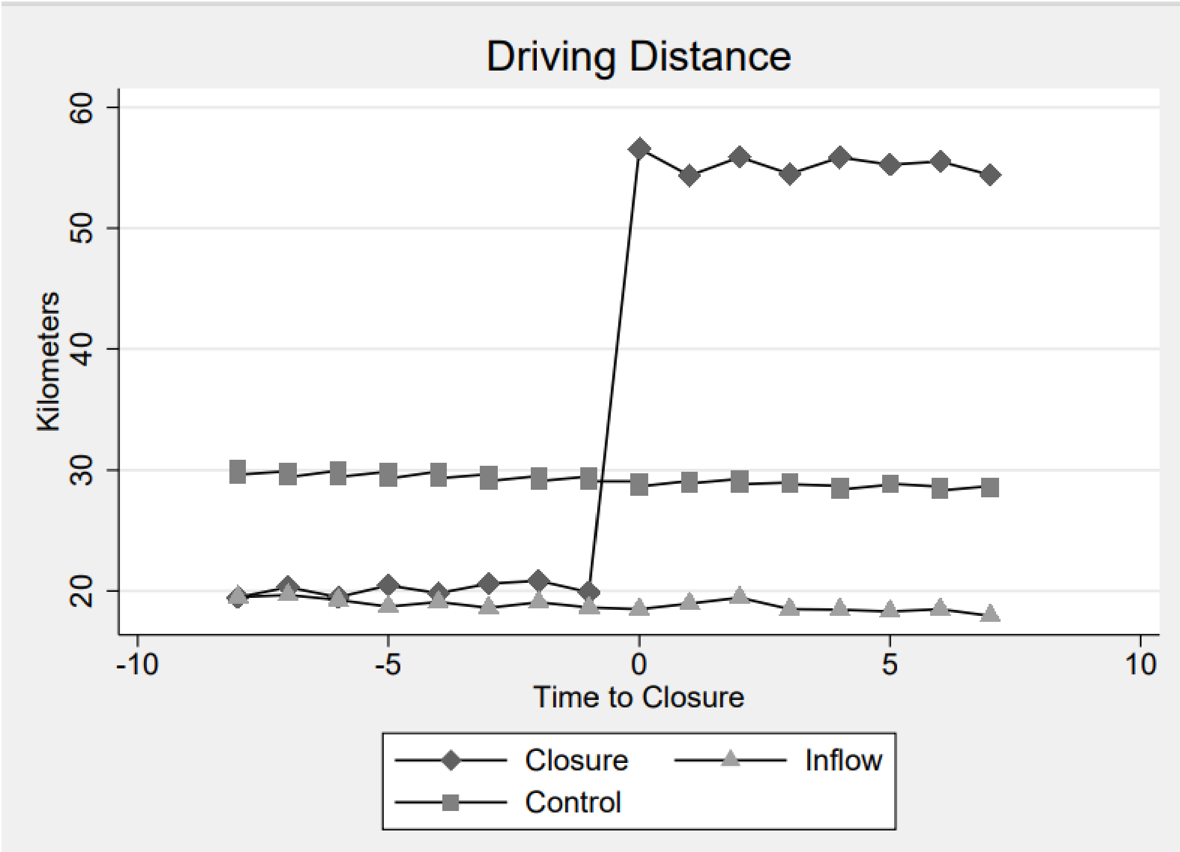
+ Does not include gestational diabetes.

Figure 1



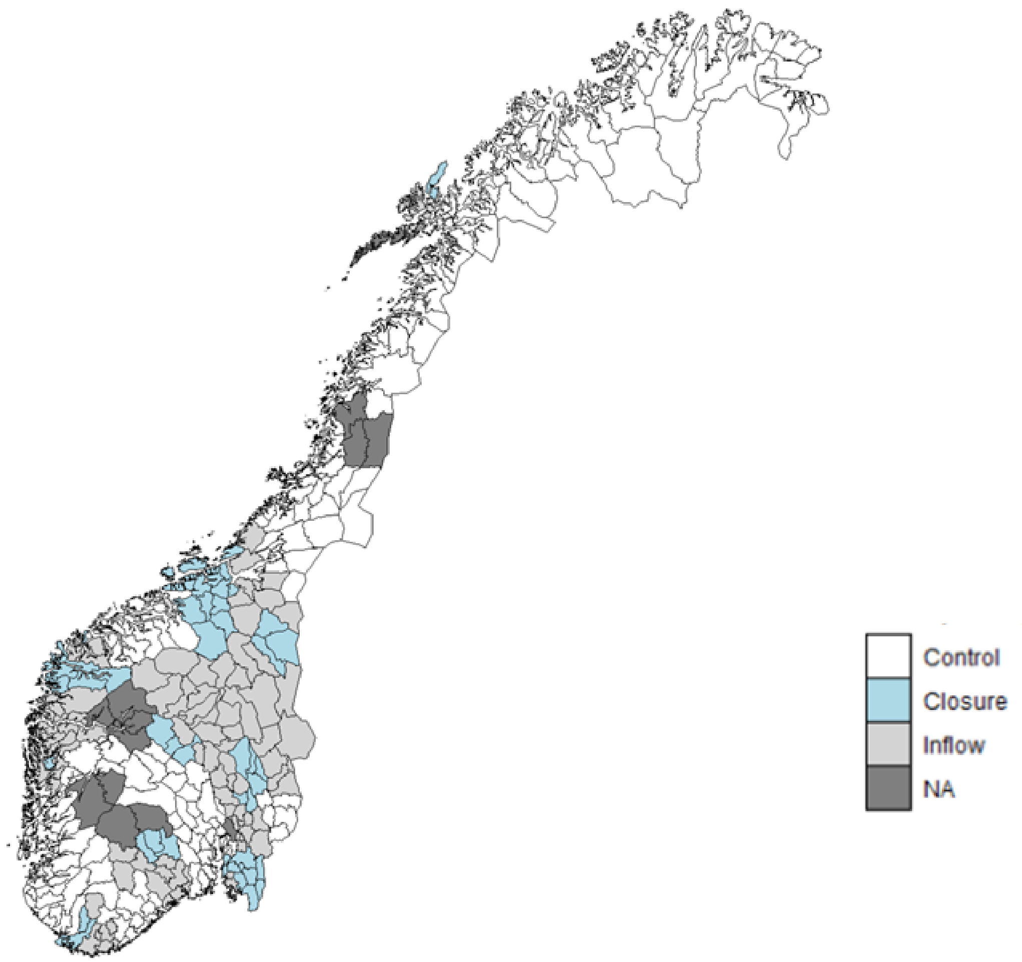

Note: Following Avdic et al. (2024), we plot the average half-yearly births at assigned institution for the closure and inflow groups leading up to and following treatment. We plot the same average for the control group using a placebo closure date. Among the closure group we use all treatment waves, however among the inflow group we define just one treatment wave in the case of multiple nearby closures.

Figure 2



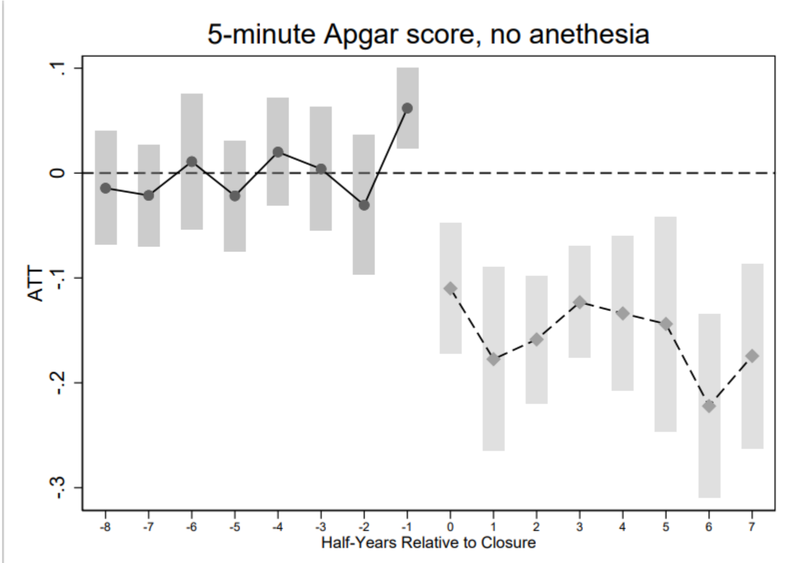

Note: Following Avdic et al. (2024), we plot the mean driving distance from home municipality center to assigned institution for the closure and inflow groups leading up to and following treatment. We plot the same average for the control group using a placebo closure date.

Figure 3




Note: We classify maternal-infant pair's treatment status in reference to their municipality's baseline assigned institution. We use 1981-1984 as the baseline period to allow for a sufficient pretreatment period and generate assigned wards based on the modal institution at which residents of a municipality give birth during this time. The closure group (blue) includes all maternal-infant pairs who reside in a municipality in which the baseline institution closes. The inflow group (light gray) consists of those whose municipality's assigned institution expands its catchment region following a nearby closure. The control group (white) contains all maternal-infant pairs from municipalities that neither directly nor indirectly experience a closure. Municipalities in dark gray are dropped from our analysis. To maintain a consistent treatment classification, we use a slightly modified version of the 2019 municipality divisions.

Figure 4

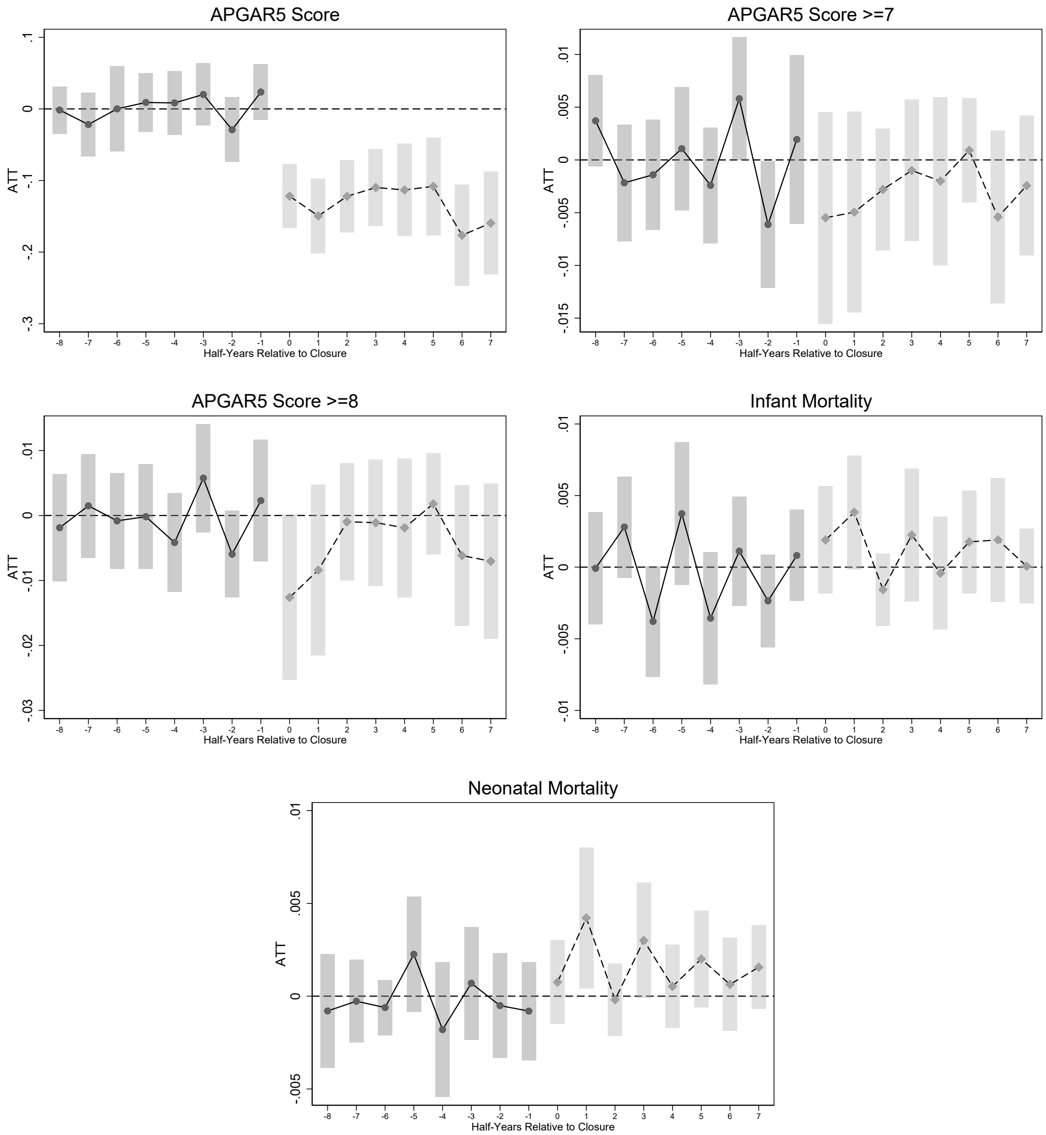



Note: In our main regressions, we find a small but statistically significant decline in 5-minute Apgar score among the closure group. To investigate whether the decline is due to increased access to anesthesia, we stratify the Callaway and Sant'Anna regression by anesthesia. Figure 4 presents an event study of for the closure group in which the not-yet and never treated are used for the control group.

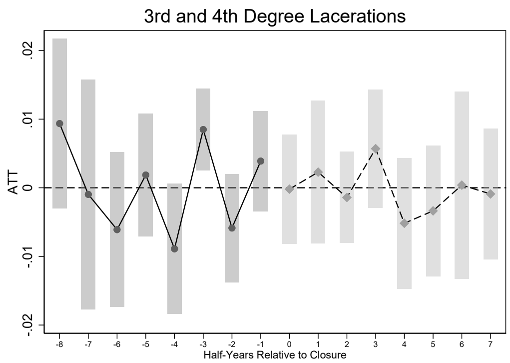
Figure 5

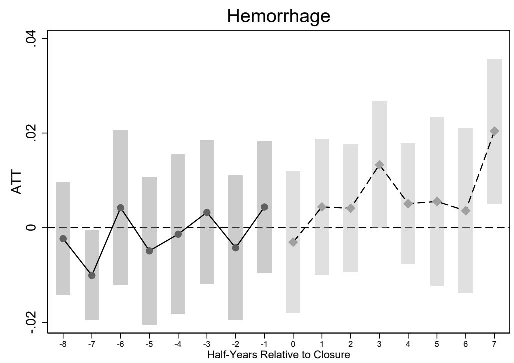
Please see the note below for a brief explanation of the event studies.

## Infant Health Outcomes, Closure Group

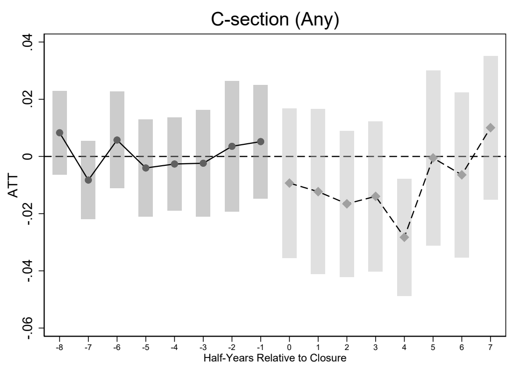


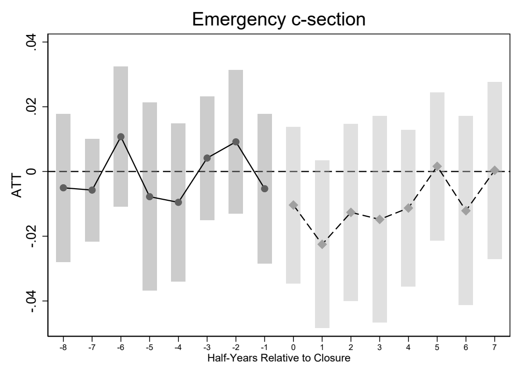
## Maternal Health Outcomes, Closure Group

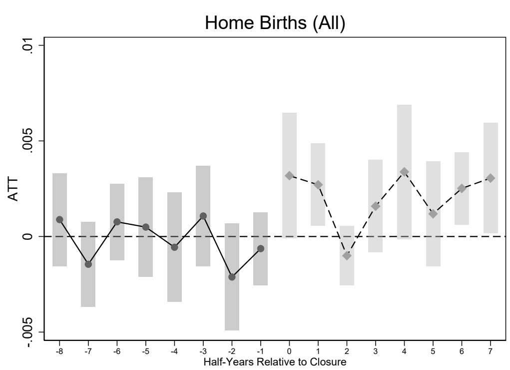


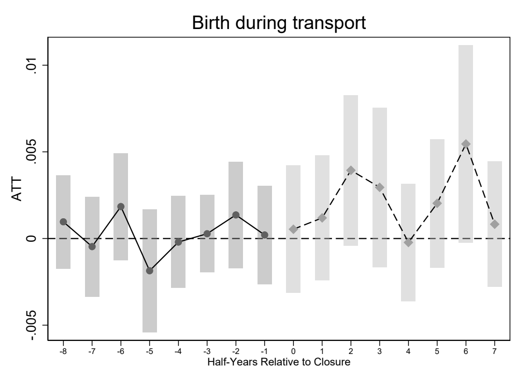



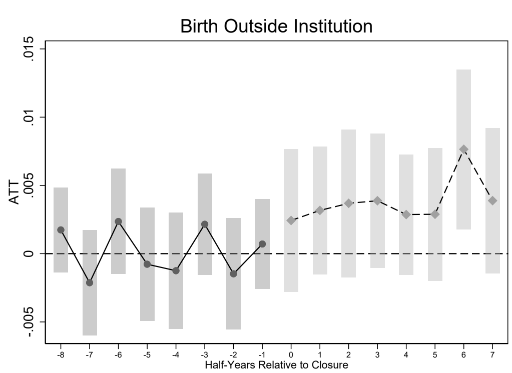

## Procedures, Closure Group



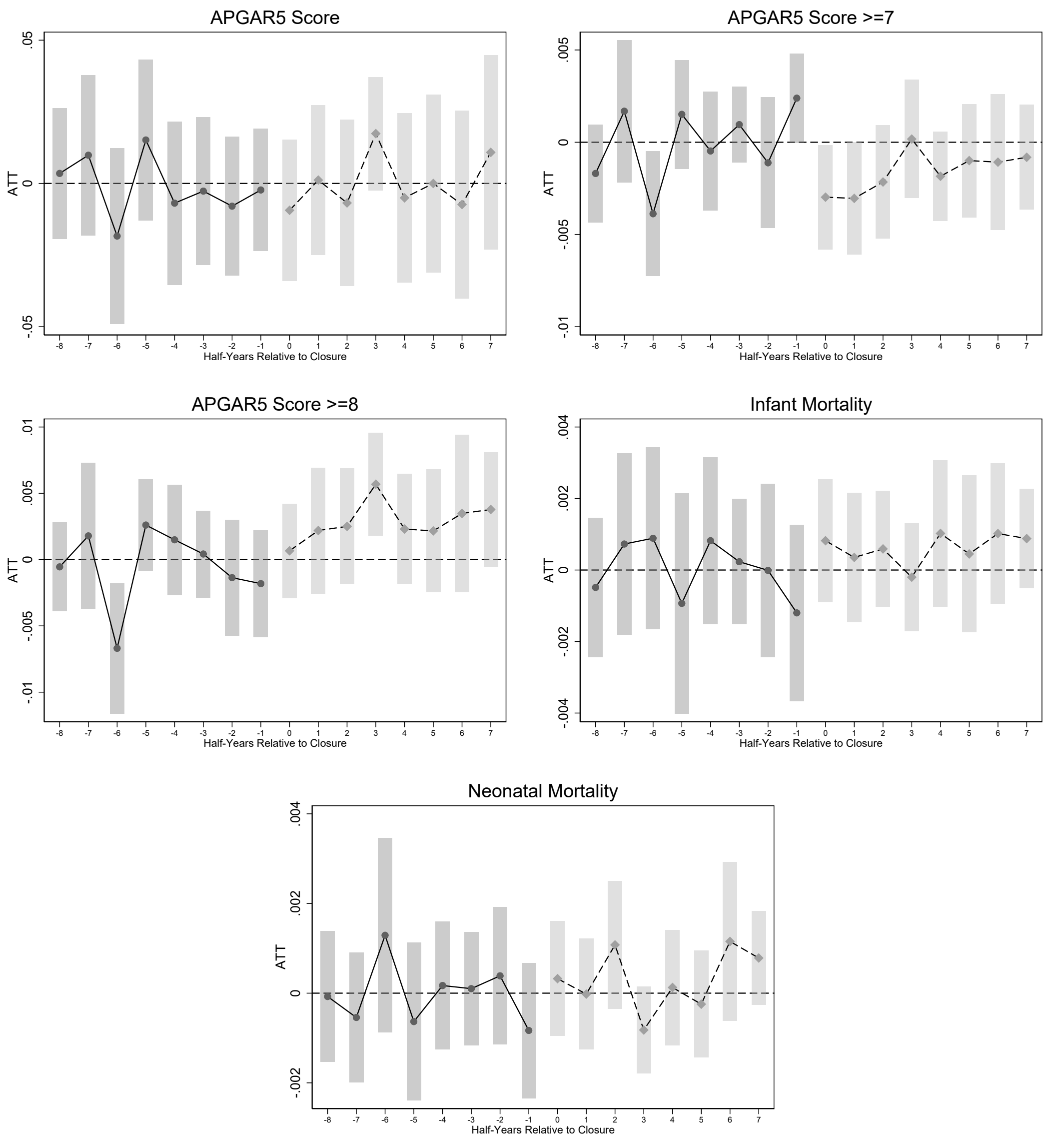



Note: For each of our outcomes, we generate event studies using Callaway and Sant'Anna's (2021) difference-in-differences framework. In the graphs above, the infow group is excluded such that the closure group is compared to not-yet and never treated observations. Relative time zero is the first half-year in which the closure group is assigned to their new institution. Given that catchment changes occur throughout a year, we establish a half yearly catchment assignment by rounding the treatment date based on in which half of the year the catchment change occurs. We use data from the years 1981 to 2019, except for 3rd and 4th degree lacerations and emergency c-section, which we analyze from 1990-2019. We do not incorporate any covariates for these plots. Each coefficient provides the ATT for the treatment-relative period, with 95% confidence intervals.

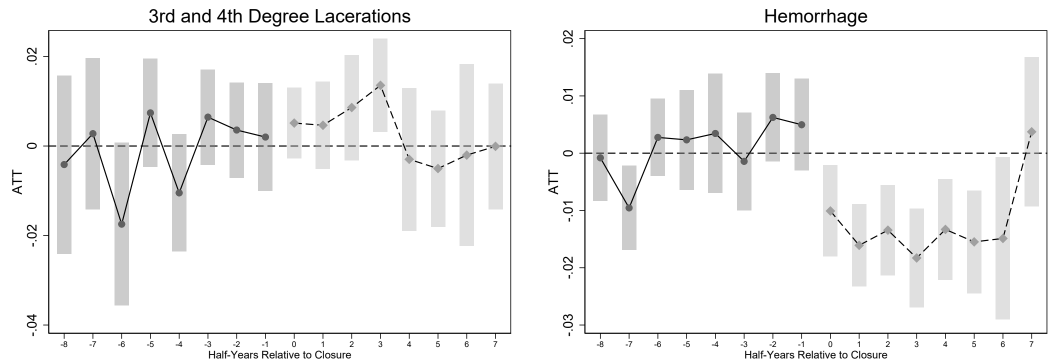
Figure 6

Please see the note below for a brief explanation of the event studies.

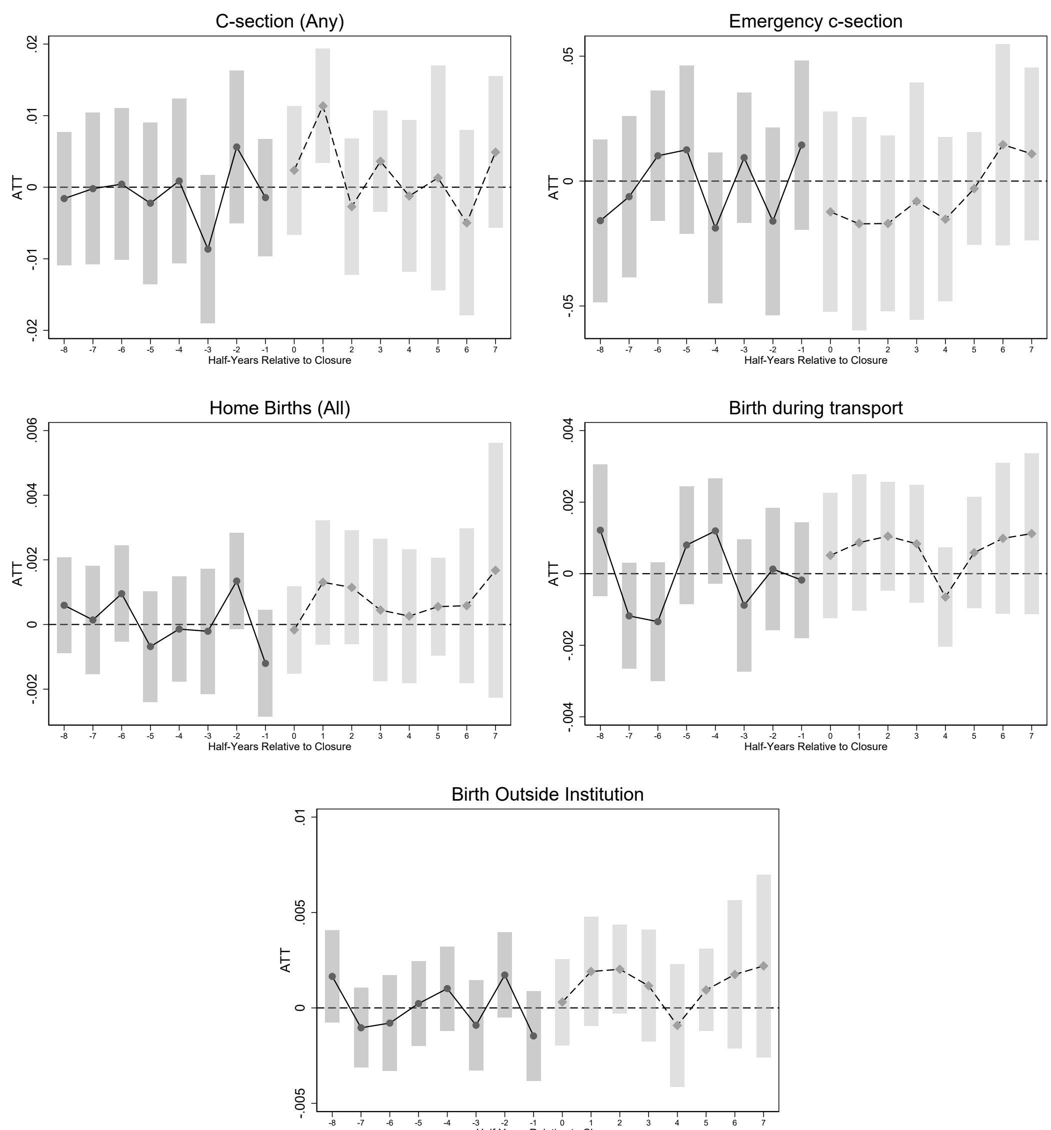
## Infant Health Outcomes, Inflow Group



## Maternal Health Outcomes, Inflow Group




## Procedures, Inflow Group

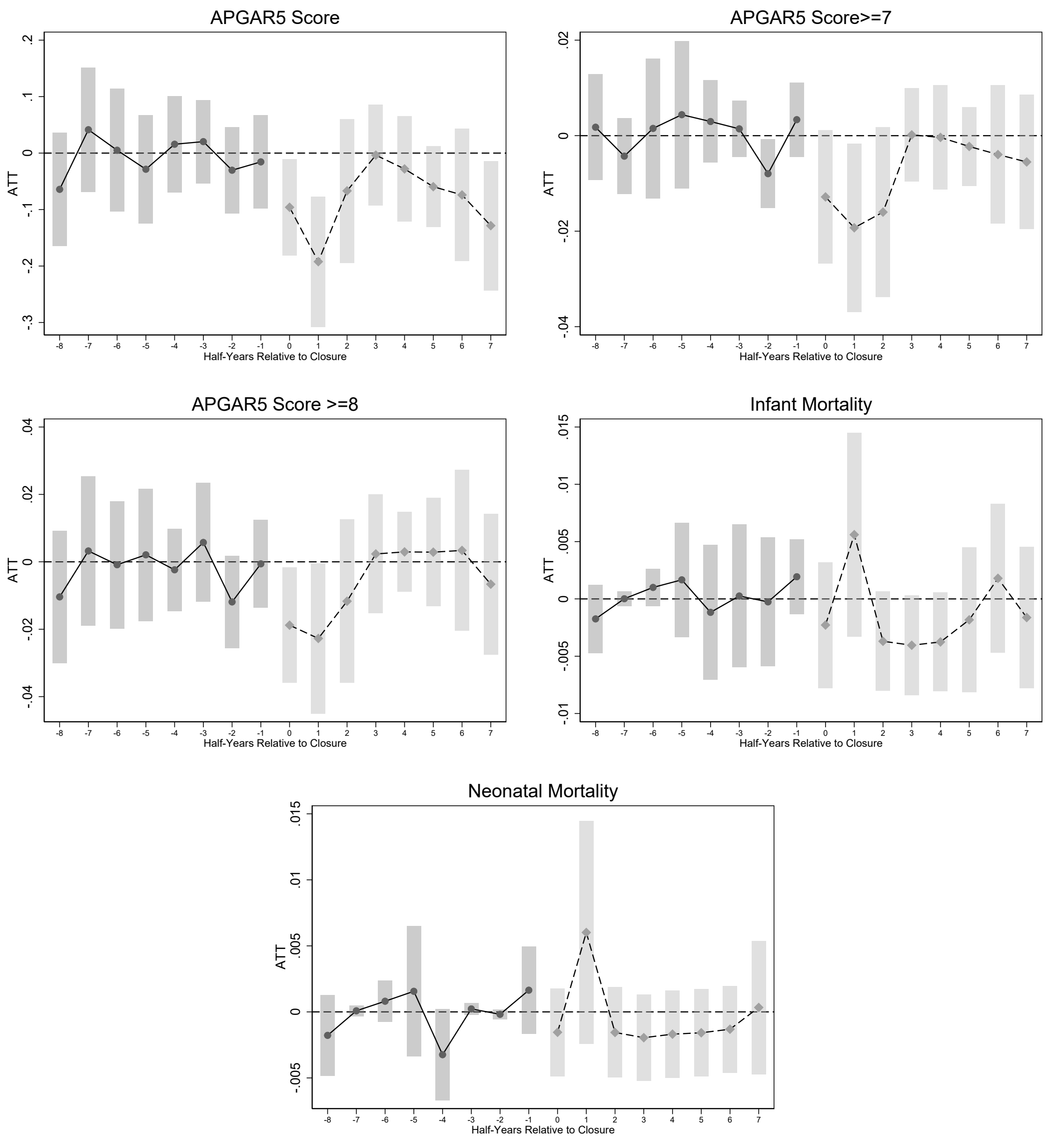


Note: For each of our outcomes, we generate event studies using Callaway and Sant'Anna's (2021) difference-in-differences framework. In the graphs above, the closure group is excluded such that the inflow group is compared to not-yet and never treated observations. Relative time zero is the first half-year in which the inflow group's assigned institution expands its catchment region. Given that catchment changes occur throughout a year, we establish a half yearly catchment assignment by rounding the treatment date based on in which half of the year the catchment change occurs. We use data from the years 1981 to 2019, except for 3rd and 4th degree lacerations and emergency c-section, which we analyze from 1990-2019. We do not incorporate any covariates for these plots. Each coefficient provides the ATT for the treatment-relative period, with 95% confidence intervals.

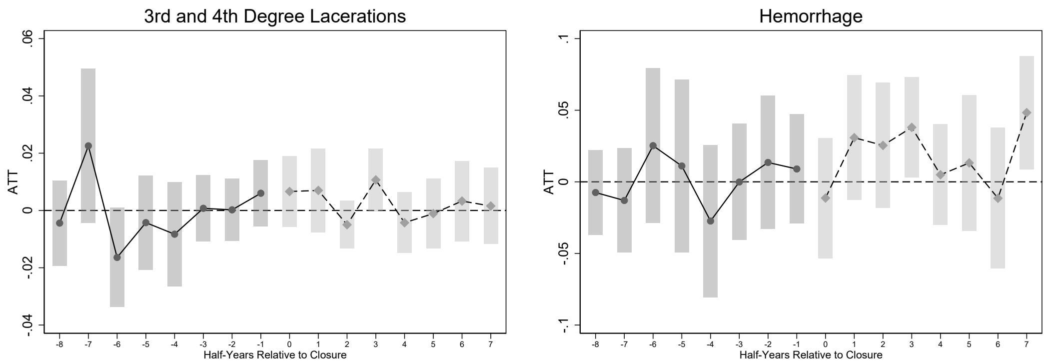
Figure 7

Please see the note below for a brief explanation of the event studies.

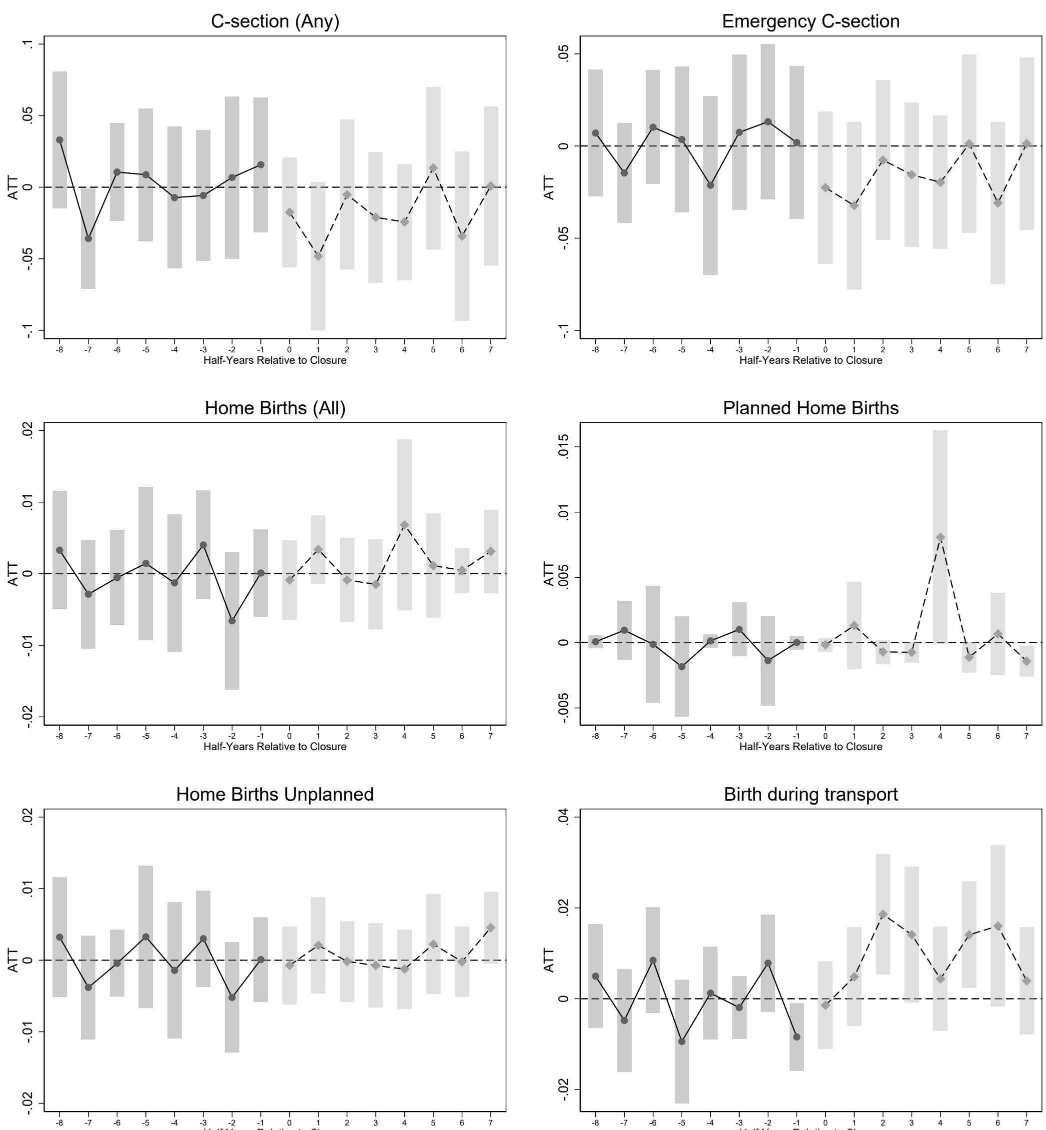
## **Infant Health Outcomes, Closure Group 1999**

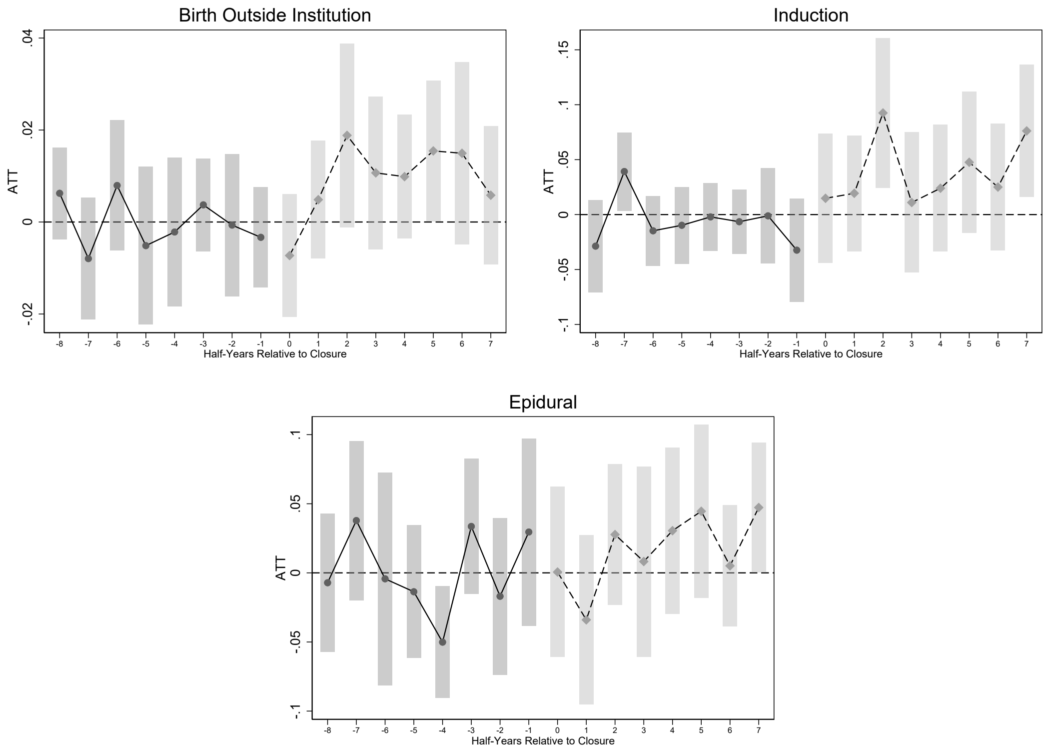


## Maternal Health Outcomes, Closure Group 1999




## Procedures, Closure Group 1999



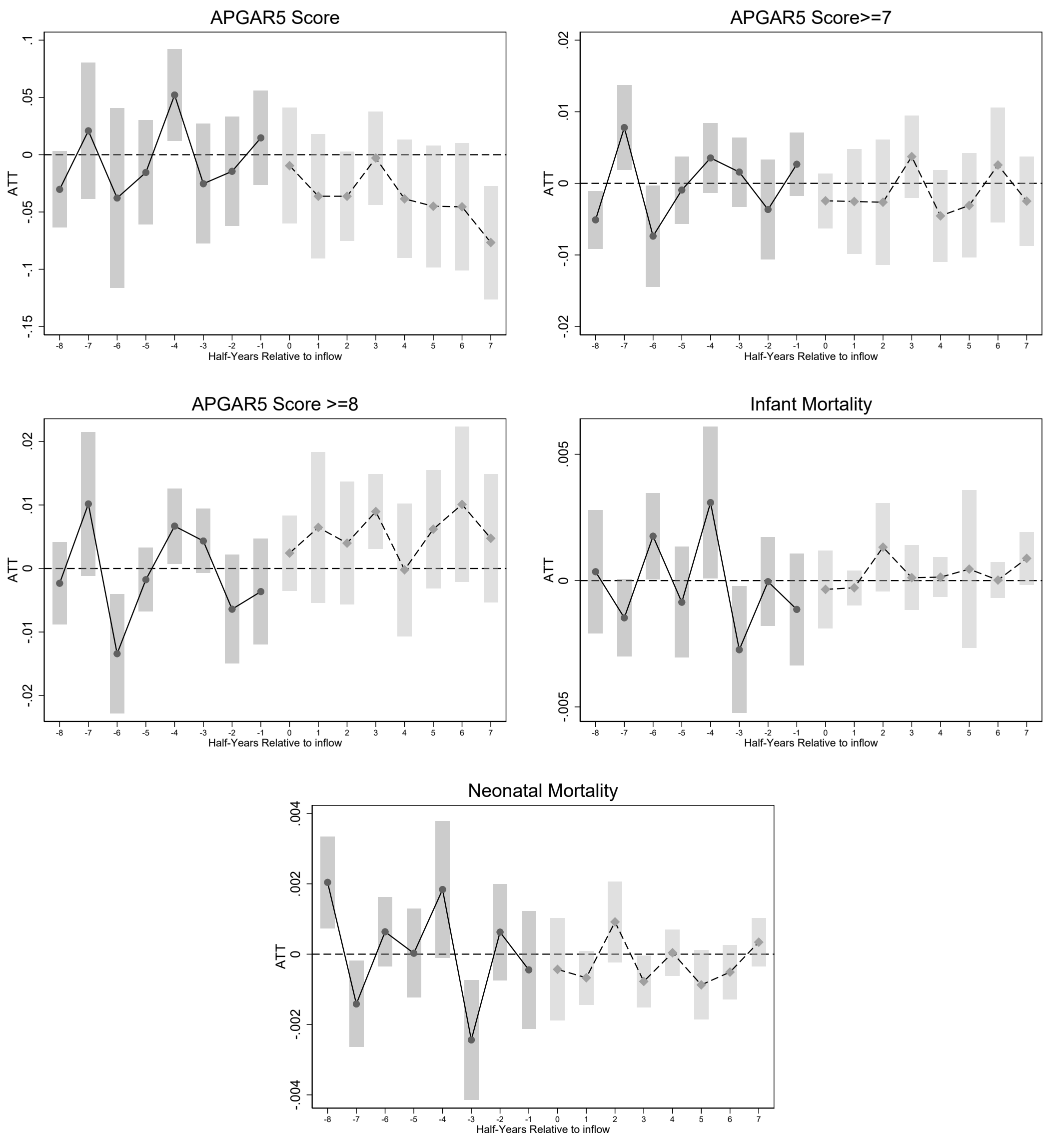



Note: For each of our outcomes, we generate event studies using Callaway and Sant'Anna's (2021) difference-in-differences framework. In the graphs above, the inflow group is excluded such that the closure group is compared to not-yet and never treated observations. Relative time zero is the first half-year in which the closure group is assigned to their new institution. Given that catchment changes occur throughout a year, we establish a half yearly catchment assignment by rounding the treatment date based on in which half of the year the catchment change occurs. We use data from the years 1999 to 2019. We do not incorporate any covariates for these plots. Each coefficient provides the ATT for the treatment-relative period, with 95% confidence intervals.

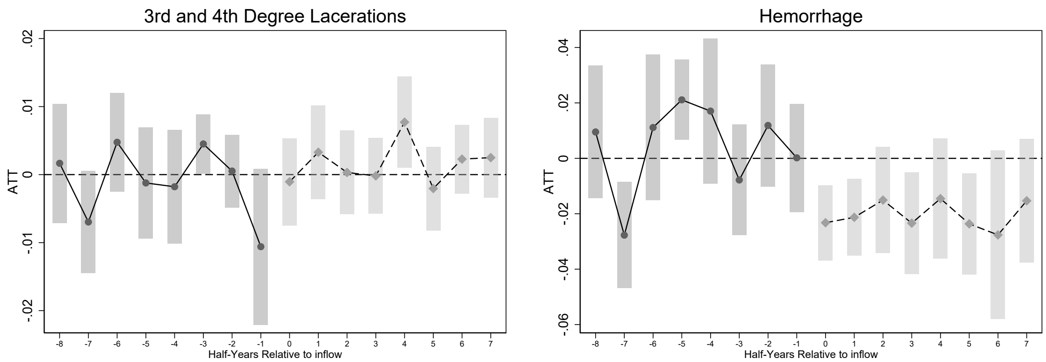
Figure 8

Please see the note below for a brief explanation of the event studies.

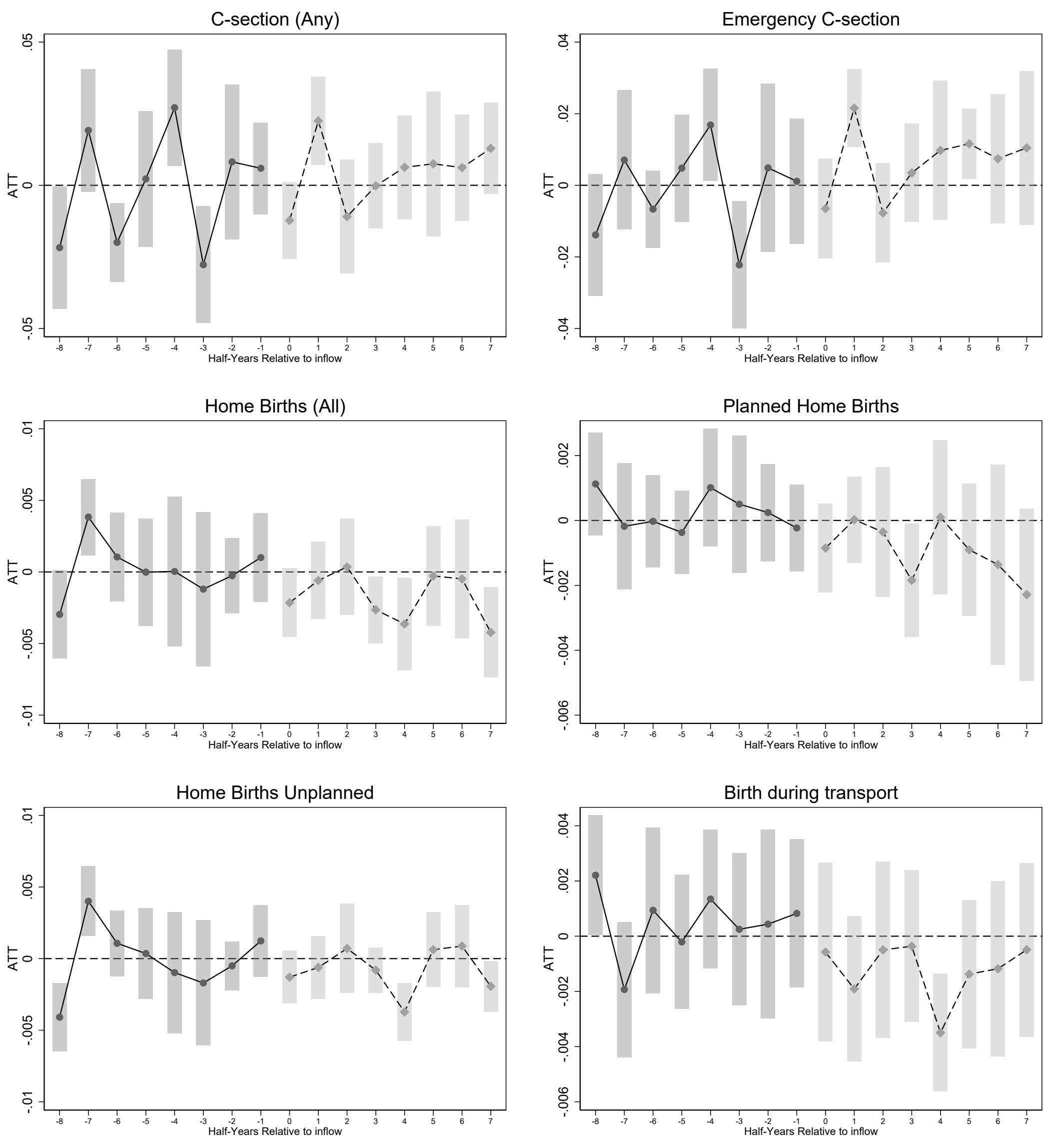
## Infant Health Outcomes, Inflow Group 1999

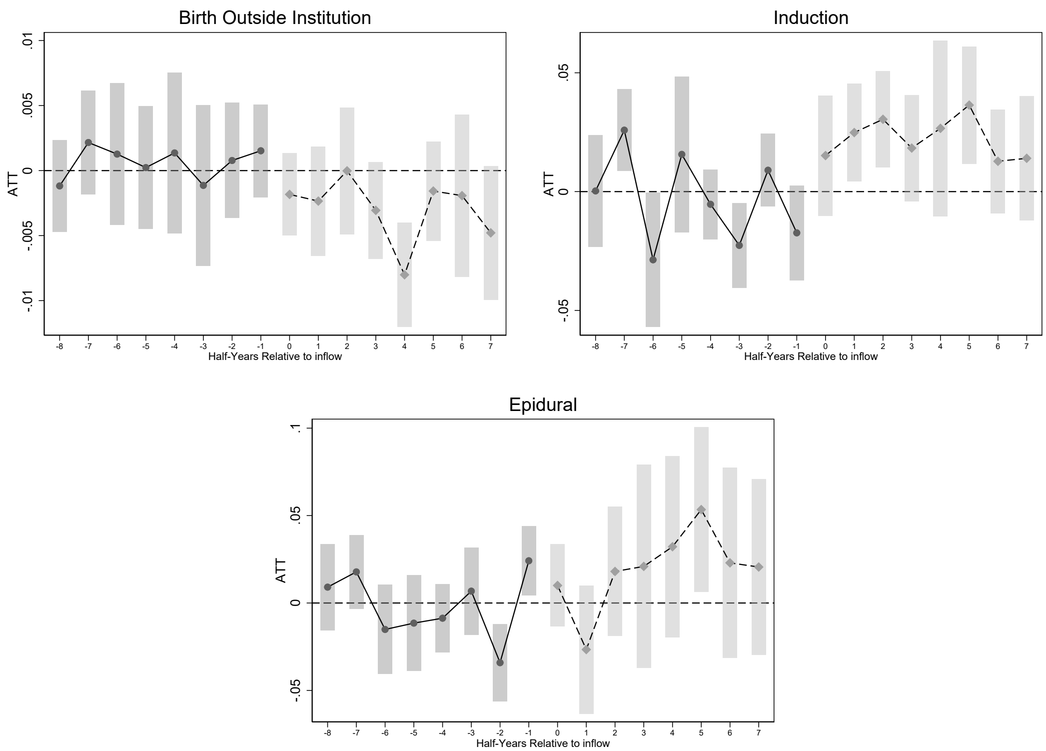


## Maternal Health Outcomes, Inflow Group 1999




## Procedures, Inflow Group 1999





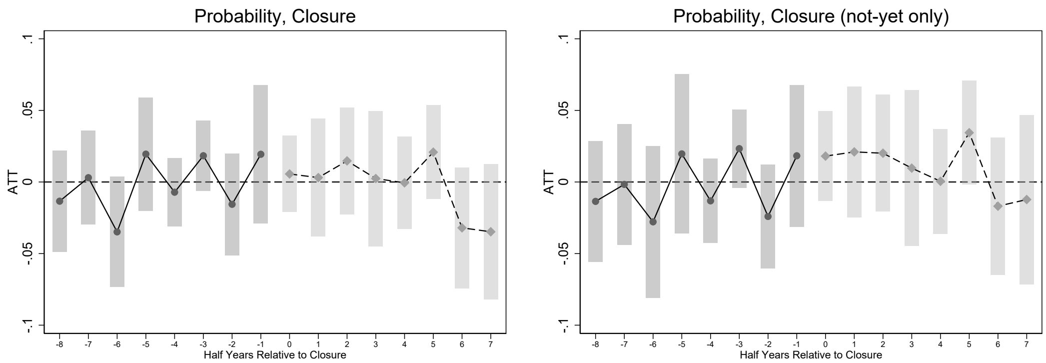

Note: For each of our outcomes, we generate event studies using Callaway and Sant'Anna's (2021) difference-in-differences framework. In the graphs above, the closure group is excluded such that the inflow group is compared to not-yet and never treated observations. Relative time zero is the first half-year in which the inflow group's assigned institution expands its catchment region. Given that catchment changes occur throughout a year, we establish a half yearly catchment assignment by rounding the treatment date based on in which half of the year the catchment change occurs. We use data from the years 1999 to 2019. We do not incorporate any covariates for these plots. Each coefficient provides the ATT for the treatment-relative period, with 95% confidence intervals.

Figure 9

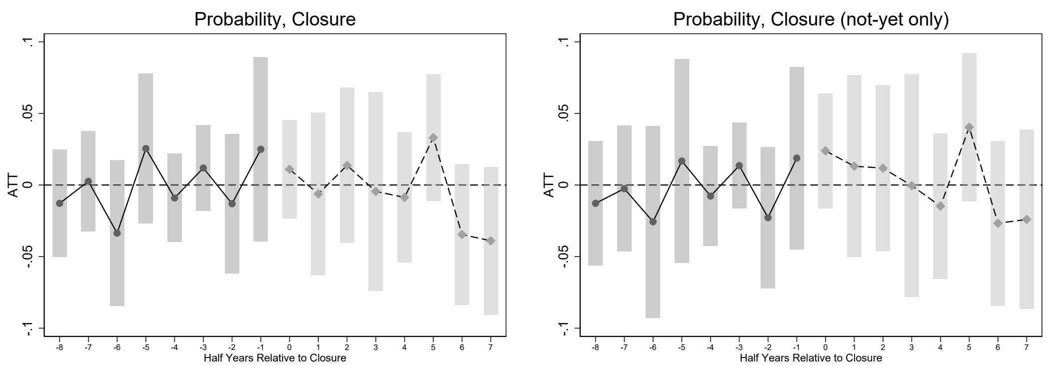
Please see the note below for a brief explanation of the event studies.

## Probability of Giving Birth, Closure Group

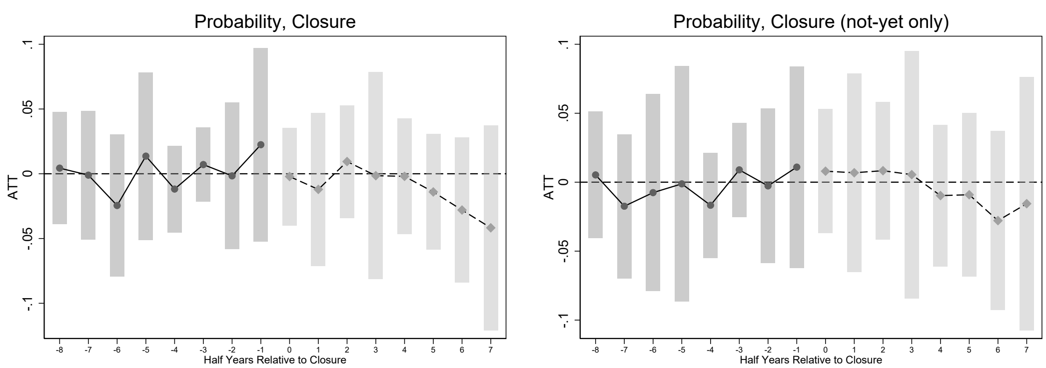
Born 1981-2001




Born 1981-1995

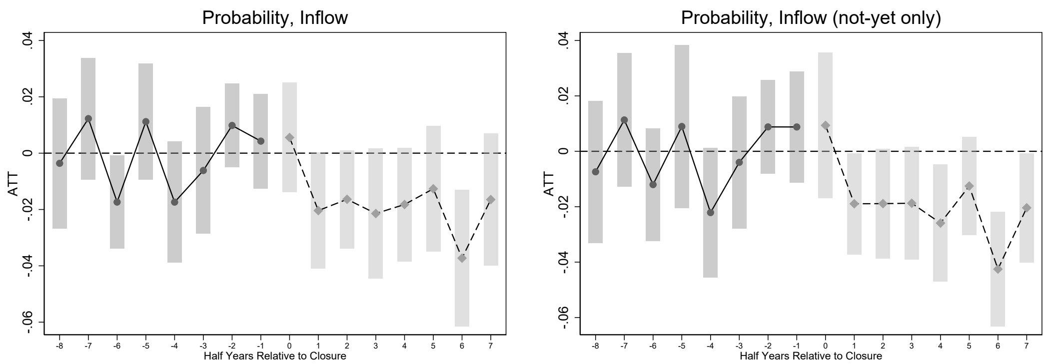



Born 1981-1990

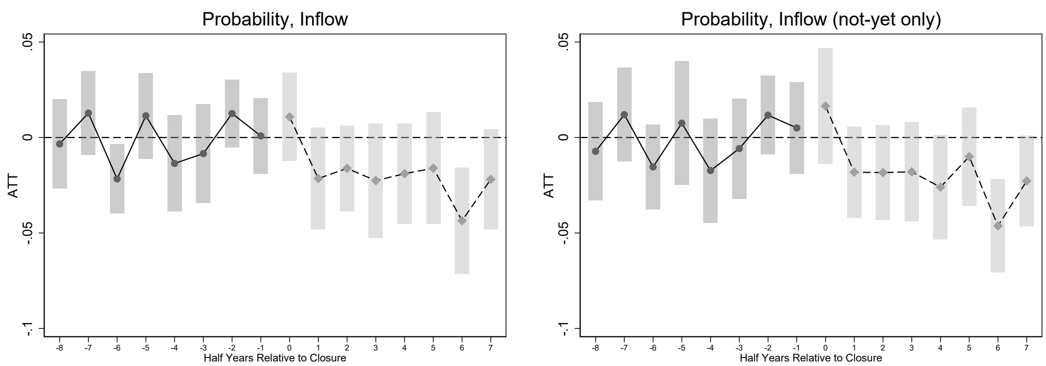



## Probability of Giving Birth, Inflow Group

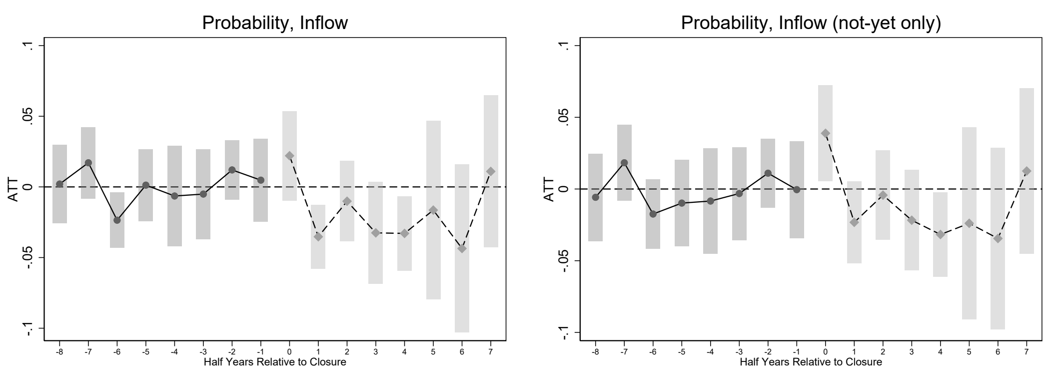
Born 1981-2001




Born 1981-1995




Born 1981-1990




Note: For individuals assigned female at birth, we examine whether experiencing a direct or indirect closure as an infant impacts the likelihood in adulthood of becoming a parent. Because treatment could alter maternal age at first birth, we run three iterations of our analysis and vary the included cohorts. The average mother's age at first birth in 2009, 2014, and 2019, was 28.1, 28.7, and 29.8, respectively. (Statistics Norway, 2024).

# A Appendix Tables

Table 15: Treatment Defined on Yearly Basis - Effect of Closures on Infant and Maternal Health and Procedures, 1981-2019

| | Closure | | Inflow | |
|---|---|---|---|---|
| | Not-Yet & Never | Only Not-Yet | Not-Yet & Never | Only Not-Yet |
| *A. Infant Health Outcomes* | | | | |
| Apgar score at 5 minutes | -0.0783*** | -0.0494 | 0.00466 | 0.0247** |
| | (0.0252) | (0.0332) | (0.0100) | (0.0113) |
| Apgar score at 5 minutes ≥8 | 0.000589 | 0.00262 | 0.00195 | 0.00139 |
| | (0.00306) | (0.00387) | (0.00170) | (0.00222) |
| Apgar score at 5 minutes ≥7 | 0.0000761 | 0.00124 | 0.0000430 | -0.000210 |
| | (0.00204) | (0.00228) | (0.00117) | (0.00143) |
| Neonatal mortality | 0.00107 | 0.000349 | -0.0000462 | 0.000349 |
| | (0.000761) | (0.000975) | (0.000338) | (0.000975) |
| Infant mortality | 0.000360 | -0.0000813 | 0.0000299 | 0.000400 |
| | (0.000981) | (0.00123) | (0.000600) | (0.000839) |
| *B. Maternal Health Outcomes* | | | | |
| $3^{rd}$ and $4^{th}$ degree lacerations$^{+}$ | 0.00174 | 0.00333 | -0.00261 | -0.000571 |
| | (0.00280) | (0.00327) | (0.00184) | (0.00213) |
| Hemorrhage ≥500 ml | 0.0100** | 0.00245 | -0.00924*** | -0.00756* |
| | (0.00492) | (0.00681) | (0.00318) | (0.00389) |
| *C. Birth Procedures* | | | | |
| C-section | -0.00591 | -0.000318 | -0.00275 | -0.00240 |
| | (0.00672) | (0.00831) | (0.00320) | (0.00442) |
| *C. Birth Procedures* | | | | |
| Emergency C-section$^{+}$ | -0.0151* | -0.00366 | 0.000153 | 0.00332 |
| | (0.00771) | (0.0104) | (0.00321) | (0.00512) |
| Home birth (all) | 0.00128** | 0.00176** | 0.000837 | 0.00139* |
| | (0.000622) | (0.000894) | (0.000791) | (0.000799) |
| Birth during transport | 0.00178 | -0.000259 | 0.000307 | 0.000322 |
| | (0.00132) | (0.00141) | (0.000467) | (0.000538) |
| Birth outside of institution§ | 0.00341** | 0.00173 | 0.00116 | 0.00171* |
| | (0.00151) | (0.00180) | (0.00101) | (0.000974) |

*Notes*: For each outcome we estimate an average post treatment effect using Callaway and Sant'Anna's (2021) difference-in-differences framework, for the four years following the adjusted catchment regions. The closure and inflow groups are each compared separately to not-yet and never (control) treated observations. The closure and inflow groups are never compared to each other, and no pre-treatment covariates are included. We establish a yearly catchment assignment by rounding the treatment date based on in which half of the year the closure occurs. We use data from the years 1981 to 2019, unless otherwise noted.

*p<0.1, **p<0.05, ***p<0.01, standard errors in parentheses.

$^{+}$ Estimate is reported for years 1990-2019.

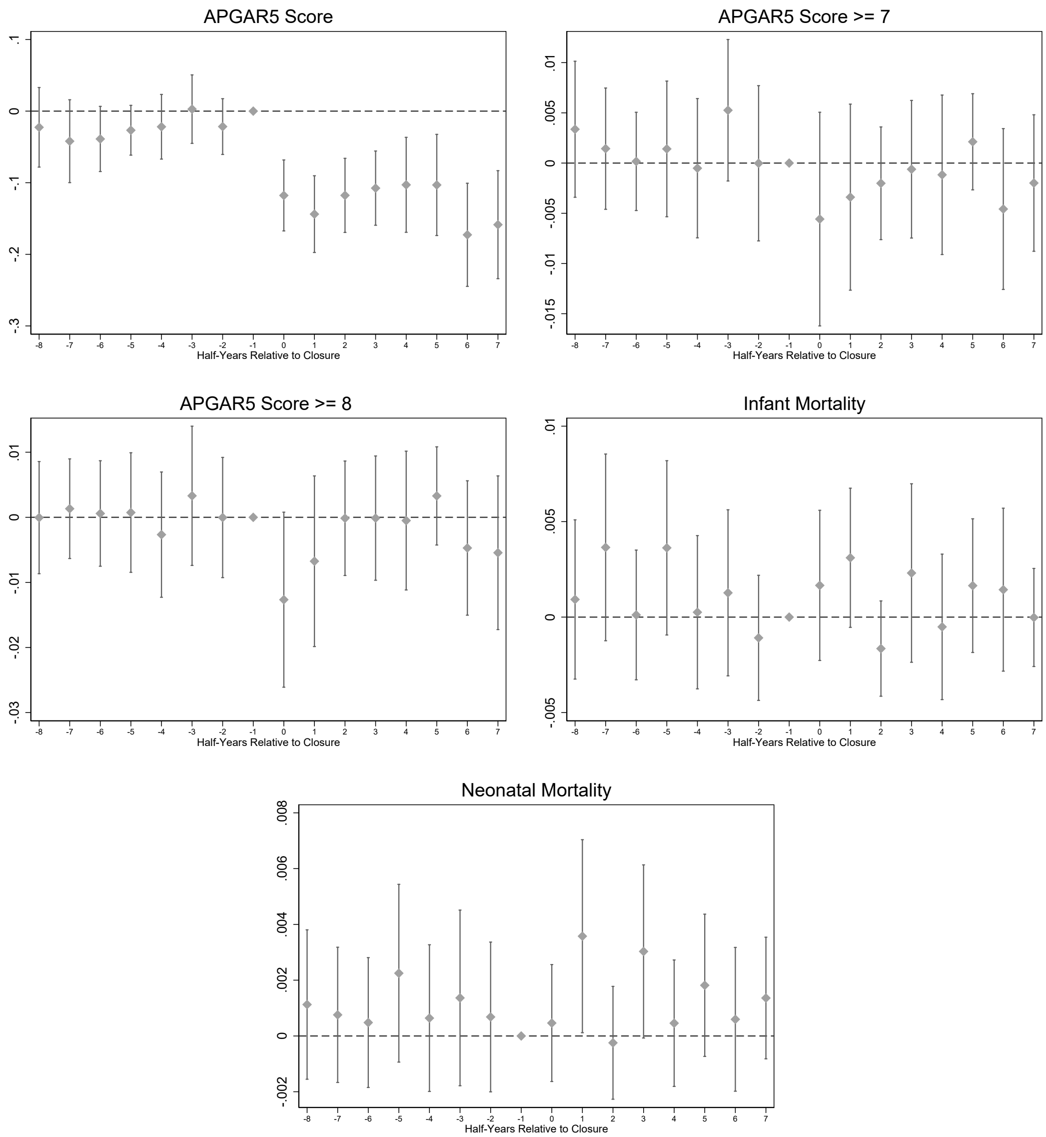
§ Variable includes all births during transport, at home, or at a previously closed institution (i.e., unplanned birth at the institution).

# B Appendix Figures

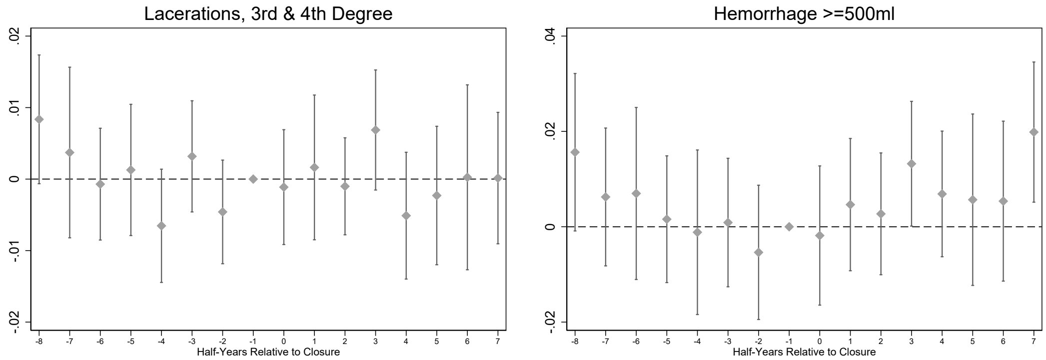
Figure 10

Please see the note below for a brief explanation for the closure group event studies.

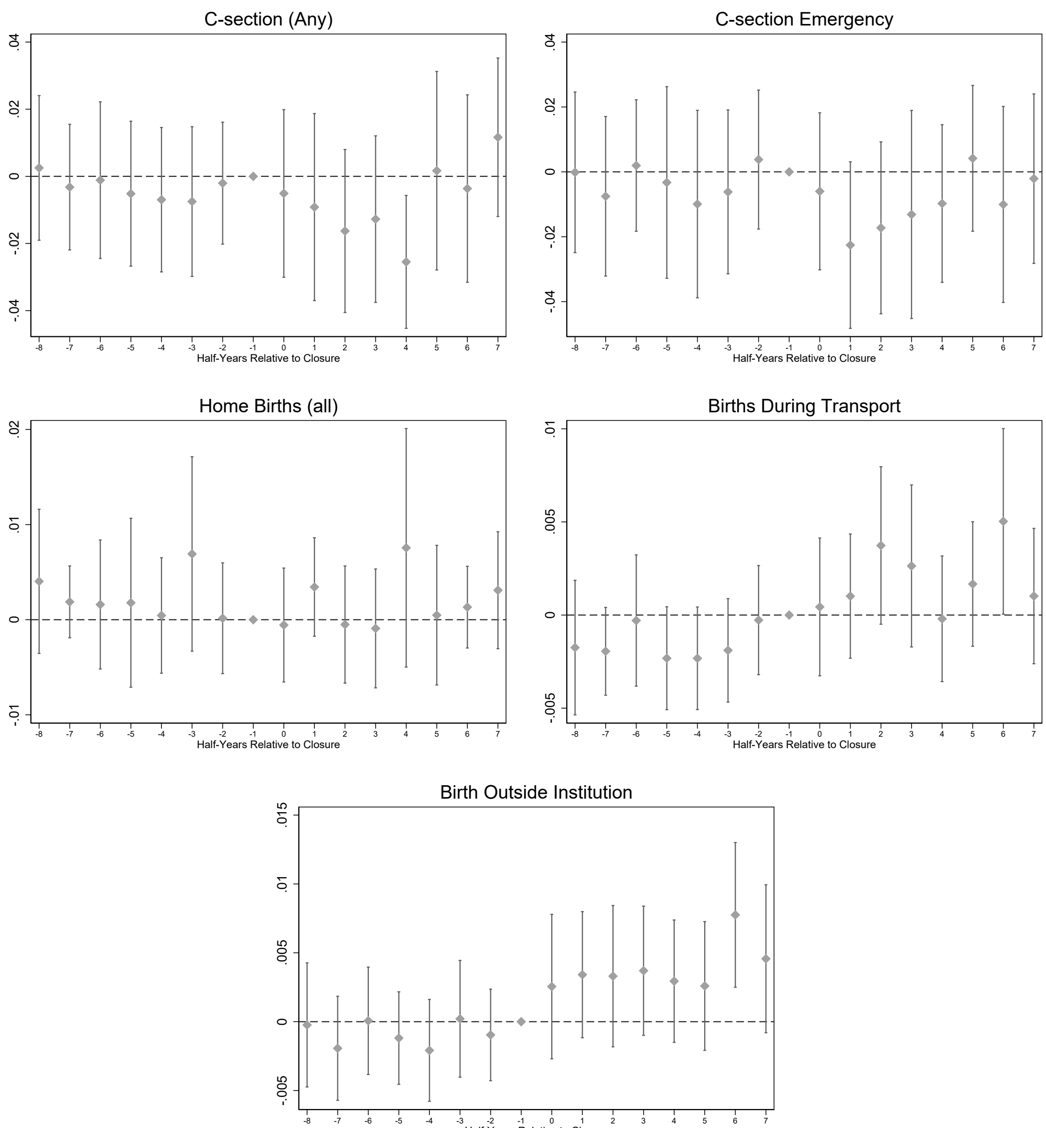
### Infant Health Outcomes, Closure Group



## Maternal Health Outcomes, Closure Group




## Procedures, Closure Group

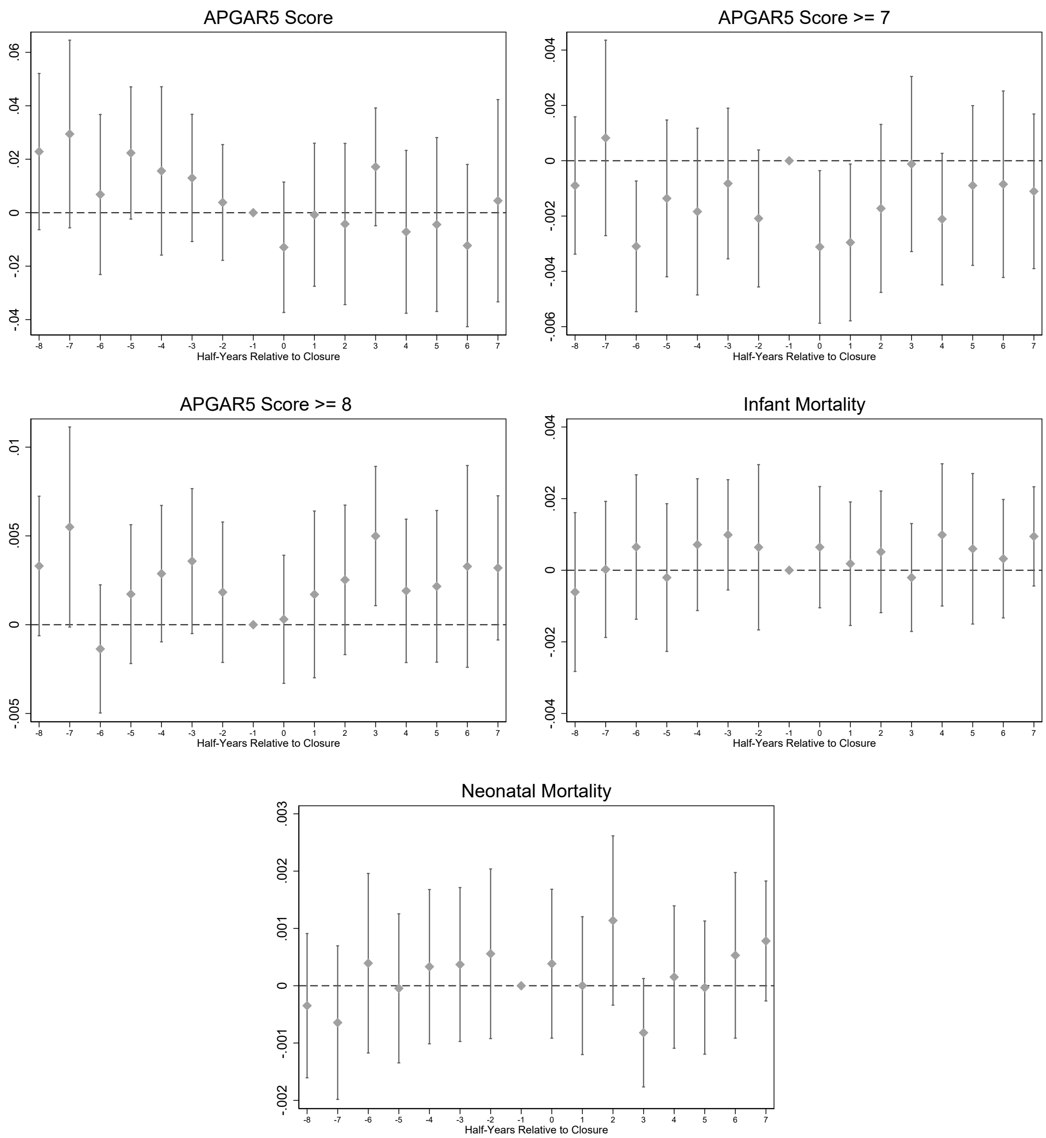


Note: We generate event study plots for the closure group for each outcome within infant health, maternal health, and birth procedures in which the inflow group is excluded and never and not-yet treated units act as the control. Relative time zero is the first half-year in which the closure group is assigned to their new institution. Given that catchment changes occur throughout a year, we establish a half-yearly catchment assignment by rounding the treatment date based on in which half of the year the closure occurs. We use data from the years 1981 to 2019, with the exception of 3rd and 4th degree lacerations and emergency c-sections which are from 1990-2019.
Our endpoints are not bounded, and we can observe at least a four year's pretreatment period for all closure municipalities for all outcomes, except for 3rd and 4th degree lacerations and emergency c-section for which we can observe at least 3 years pretreatment. Additionally, we use 95% confidence intervals clustered at the municipality level and incorporate municipality, half-year, and month fixed effects.
For binary outcomes, we interpret the coefficients as the probability of experiencing the outcome relative to time of negative one.

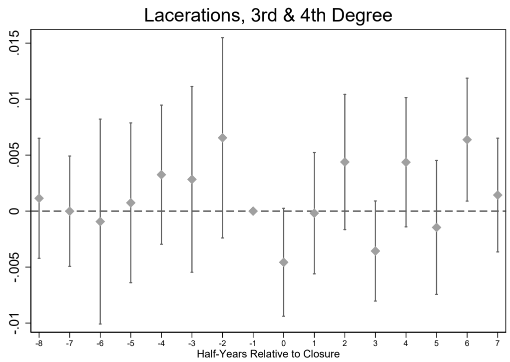
Figure 11

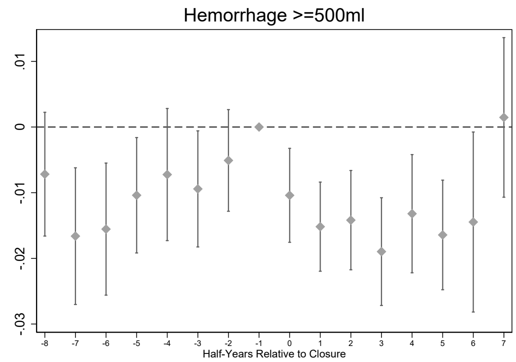
Please see the note below for a brief explanation for the inflow group event studies.

## Infant Health Outcomes, Inflow Group

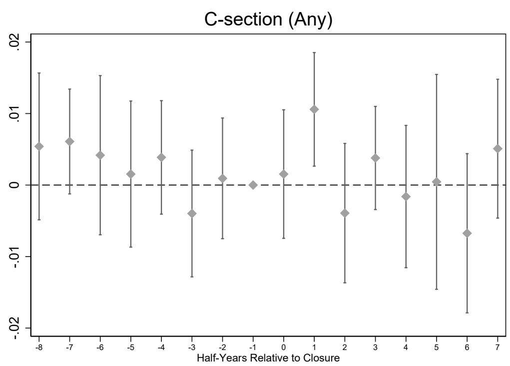


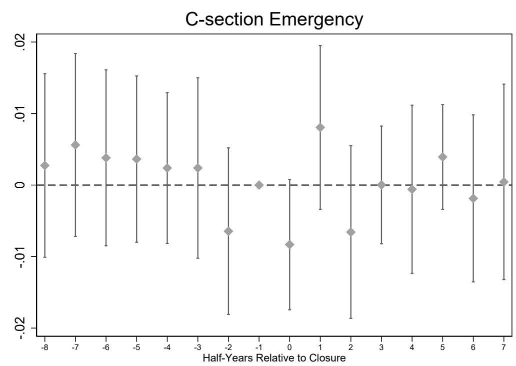
## Maternal Health Outcomes, Inflow Group

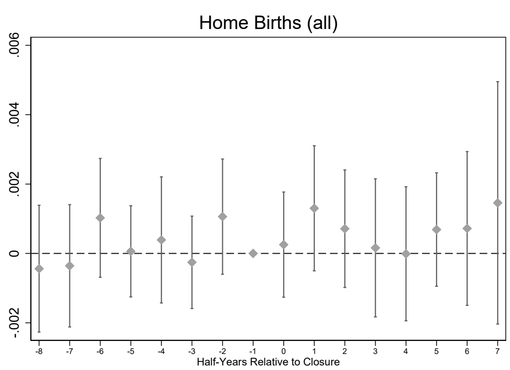


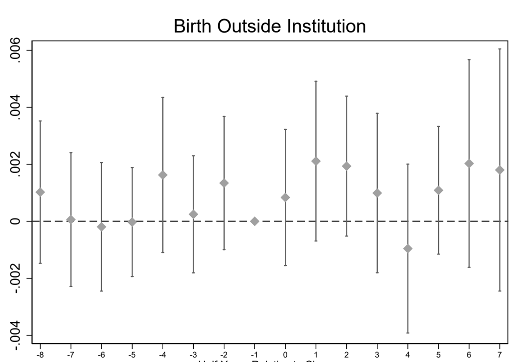

## Procedures, Inflow Group





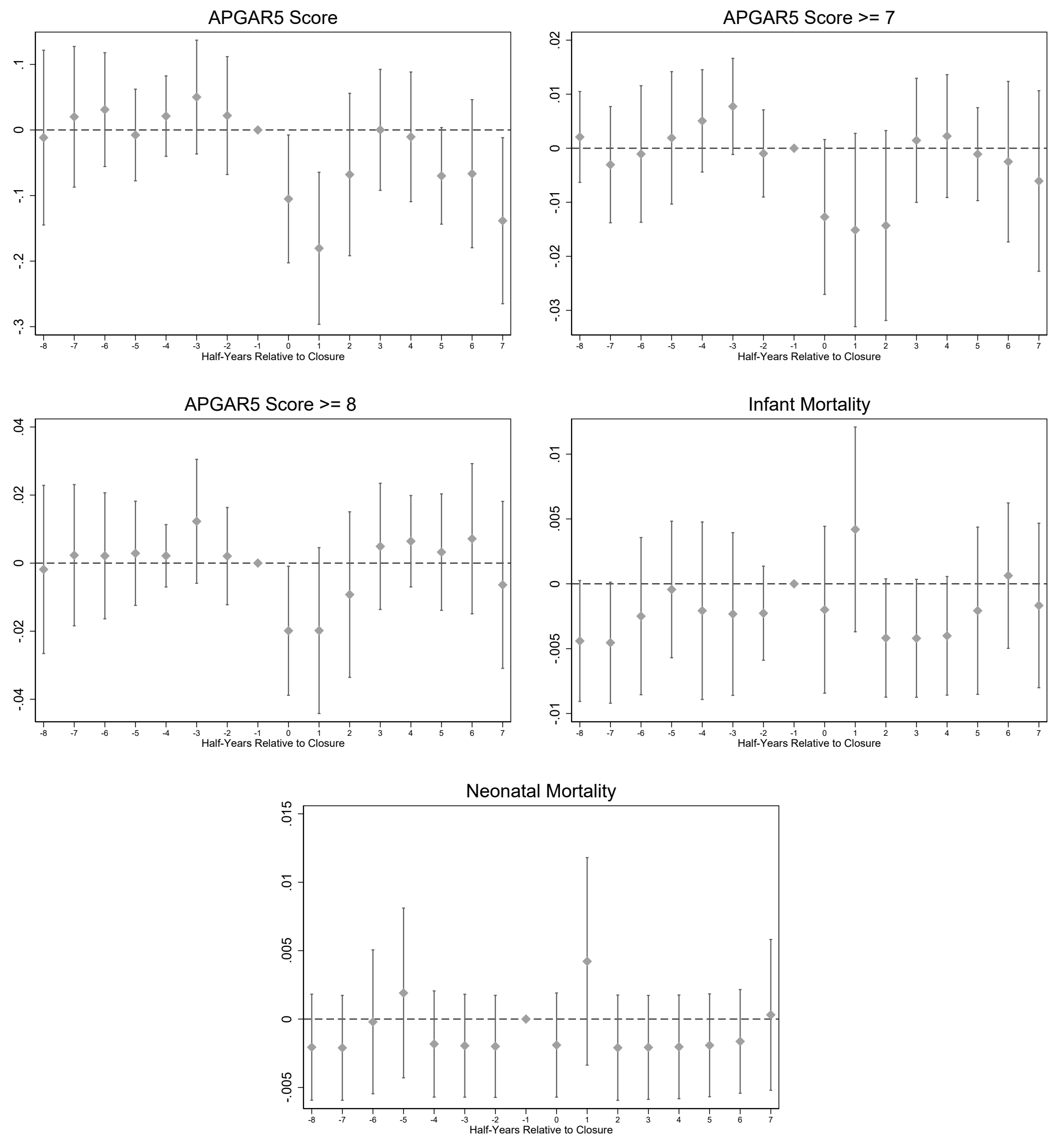


Note: We generate event study plots for the inflow group for each outcome within infant health, maternal health, and birth procedures in which the closure group is excluded and never and not-yet treated units act as the control. Relative time zero is the first half-year in which the inflow group's assigned institution expands its catchment region. Given that catchment changes occur throughout a year, we establish a half-yearly catchment assignment by rounding the treatment date based on in which half of the year the catchment change occurs. We use data from the years 1981 to 2019, with the exception of 3rd and 4th degree lacerations and emergency c-sections which are from 1990-2019.
Our endpoints are not bounded, and we can observe at least a four year's pretreatment period for all inflow municipalities for all outcomes, except for 3rd and 4th degree lacerations and emergency c-section for which we can observe at least 3 years pretreatment. Additionally, we use 95% confidence intervals clustered at the municipality level and incorporate municipality, half-year, and month fixed effects.
For binary outcomes, we interpret the coefficients as the probability of experiencing the outcome relative to time of negative one.

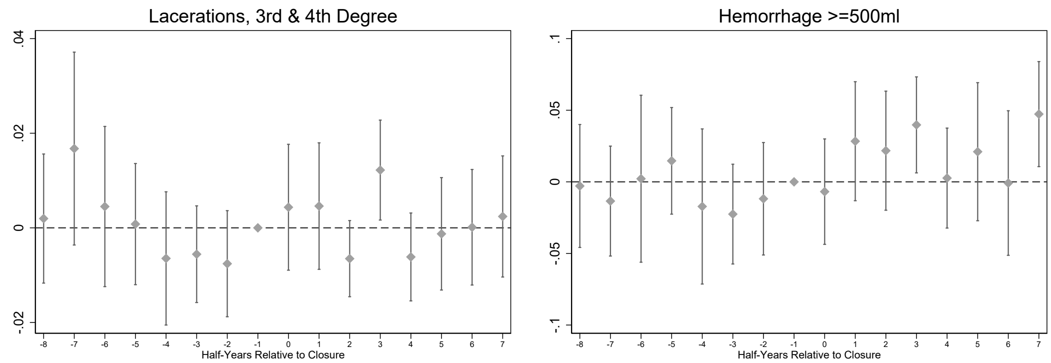
Figure 12

Please see the note below for a brief explanation for the closure group event studies.

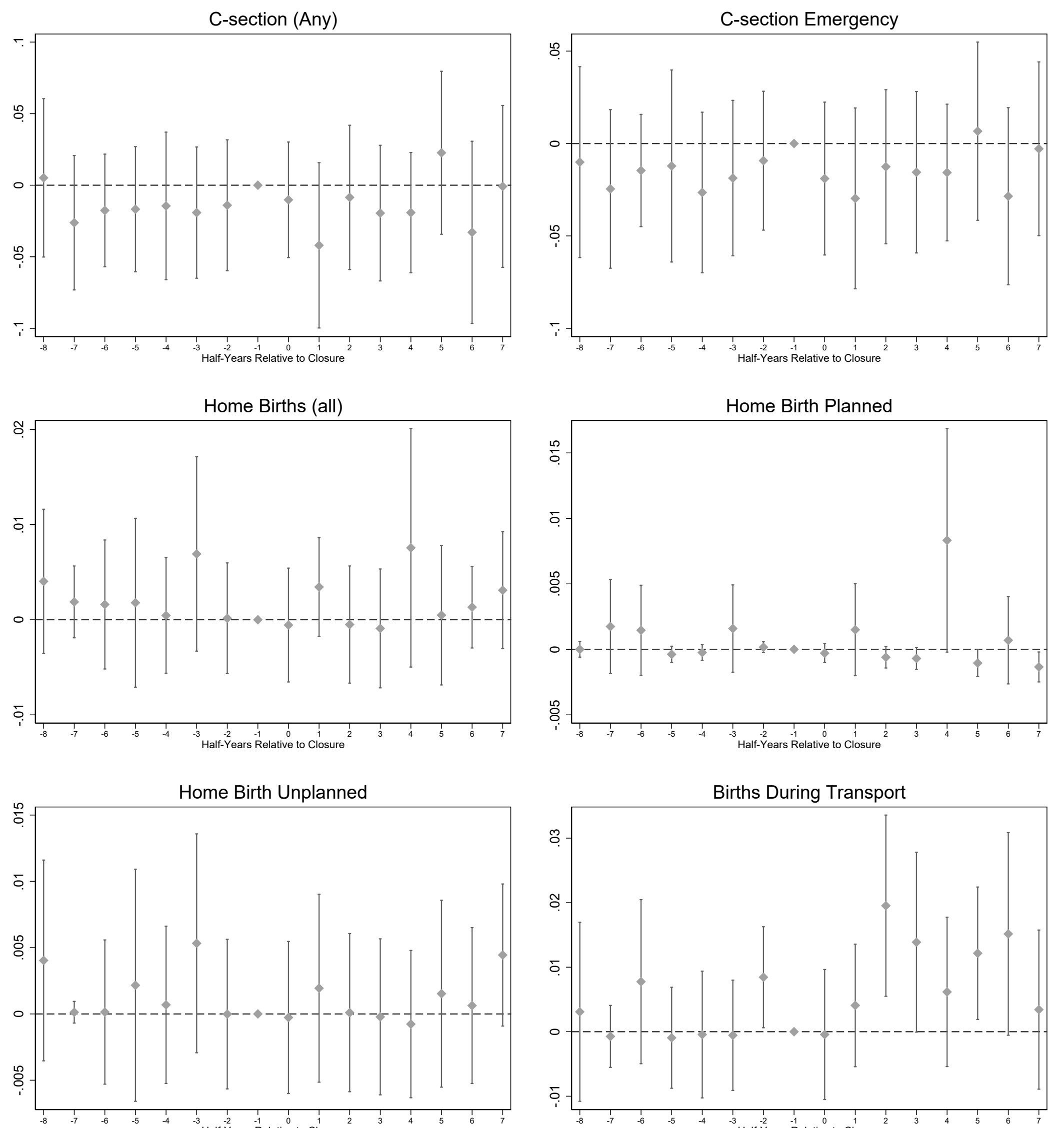
## Infant Health Outcomes, Closure Group, 1999

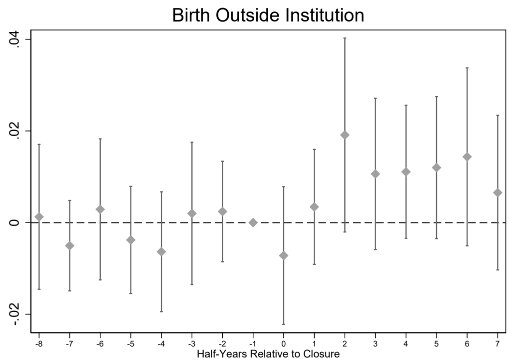


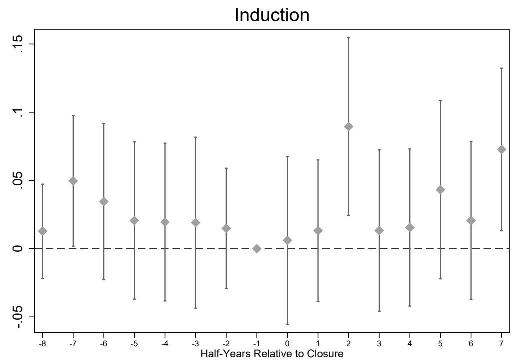
## Maternal Health Outcomes, Closure Group, 1999

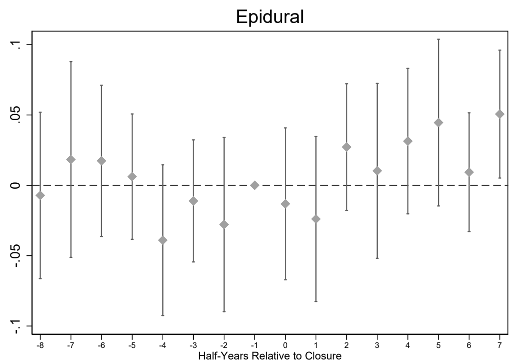



## Procedures, Closure Group, 1999





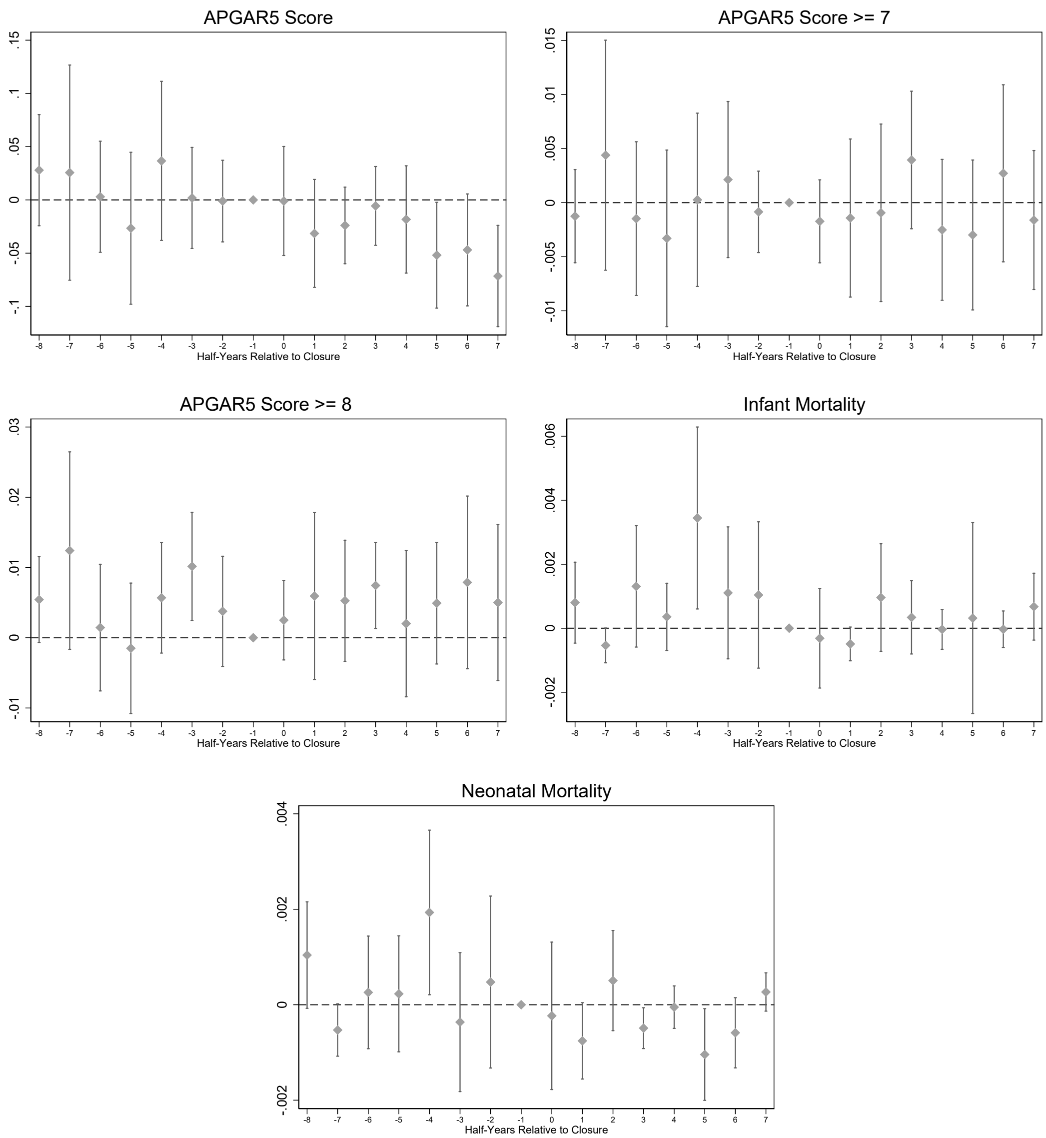




Note: We generate event study plots for the closure group for each outcome within infant health, maternal health, and birth procedures in which the inflow group is excluded and never and not-yet treated units act as the control. Relative time zero is the first half-year in which the closure group is assigned to their new institution. Given that catchment changes occur throughout a year, we establish a half-yearly catchment assignment by rounding the treatment date based on in which half of the year the closure occurs. We use data from the years 1999 to 2019.
Our endpoints are not bounded, and we can observe at least a three and a half year's pretreatment period for all closure municipalities for all outcomes. Additionally, we use 95% confidence intervals clustered at the municipality level and incorporate municipality, half-year, and month fixed effects.
For binary outcomes, we interpret the coefficients as the probability of experiencing the outcome relative to time of negative one.

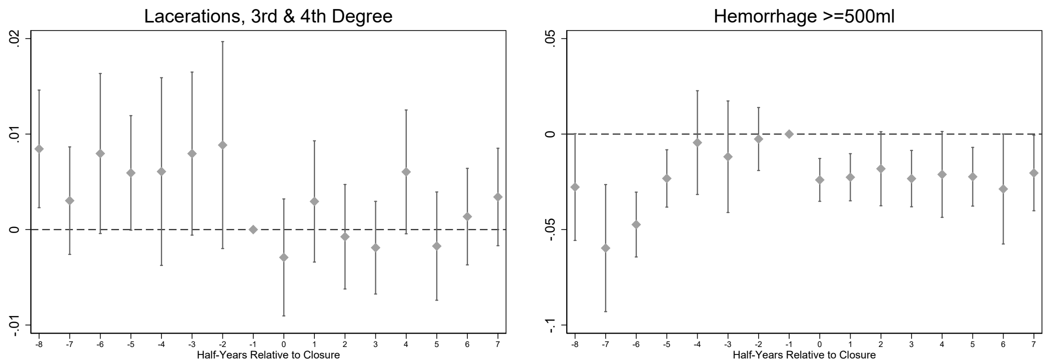
Figure 13

Please see the note below for a brief explanation for the inflow group event studies.

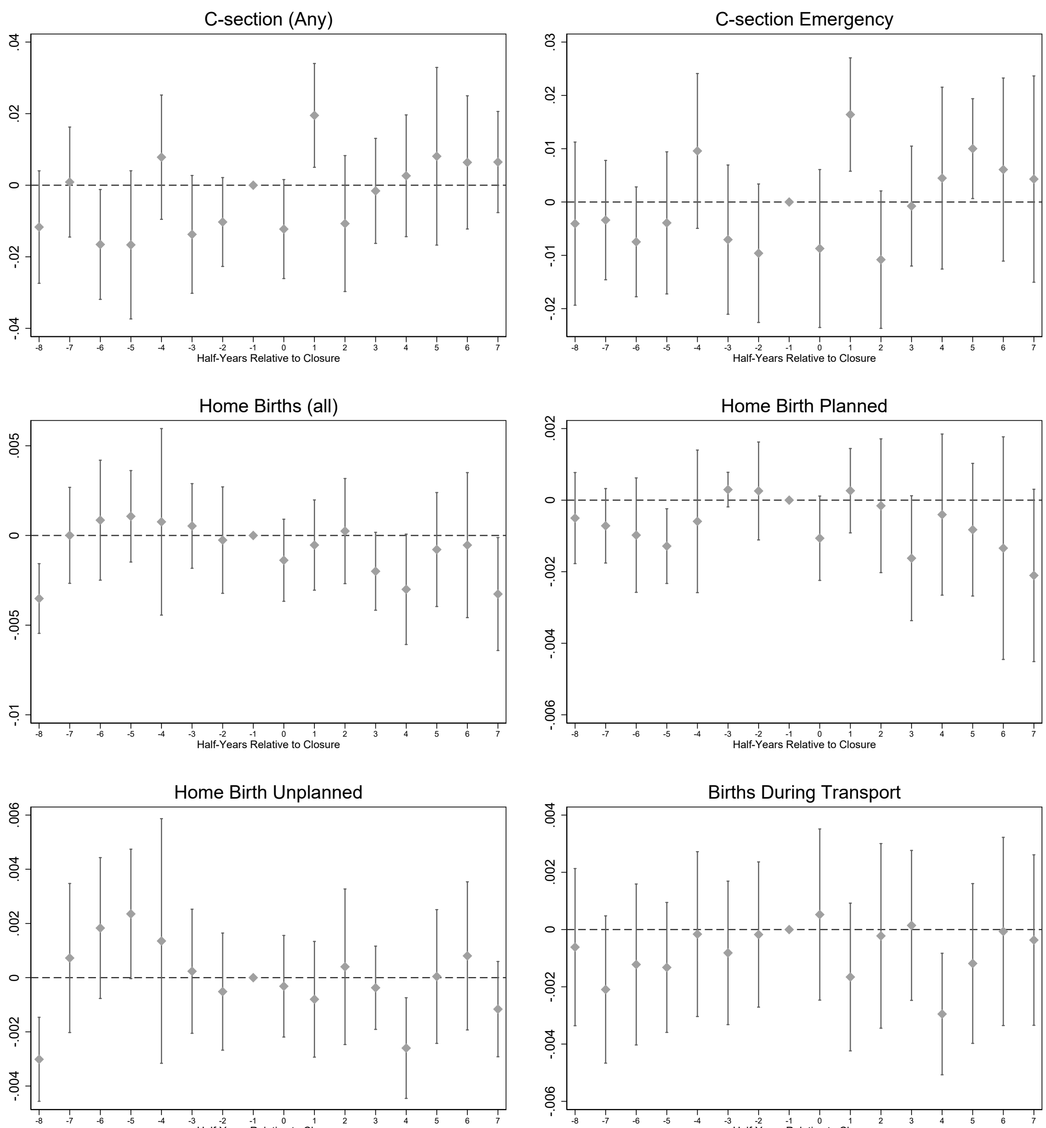
## Infant Health Outcomes, Inflow Group, 1999

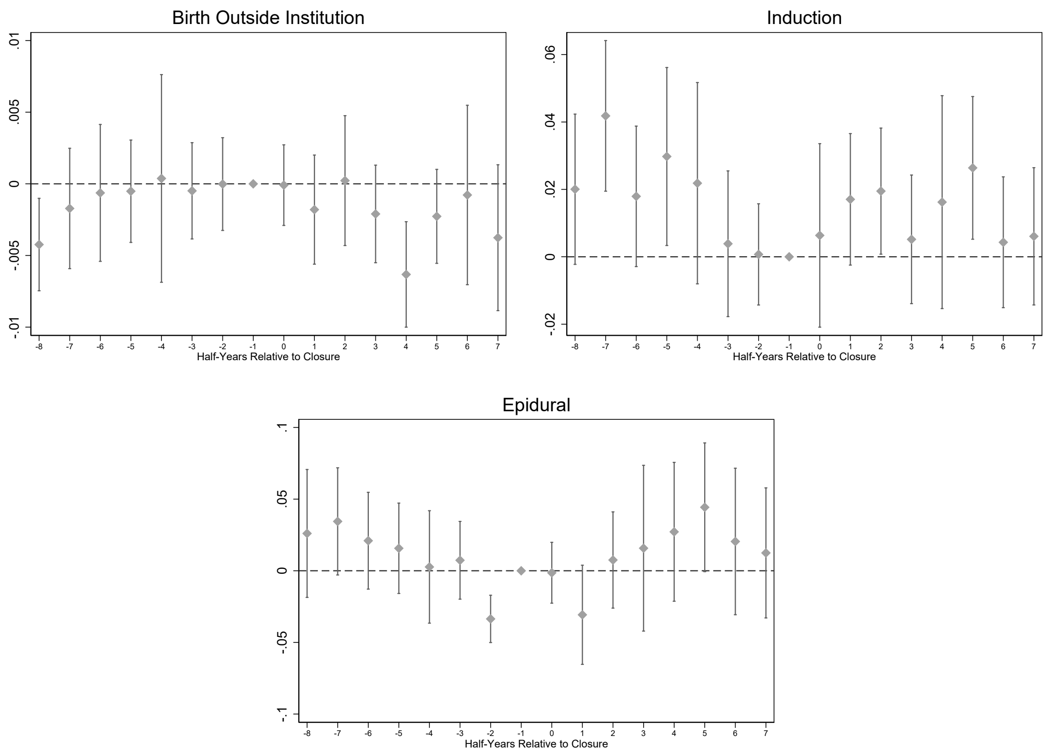


## Maternal Health Outcomes, Inflow Group, 1999




## Procedures, Inflow Group, 1999






Note: We generate event study plots for the inflow group for each outcome within infant health, maternal health, and birth procedures in which the closure group is excluded and never and not-yet treated units act as the control. Relative time zero is the first half-year in which the inflow group's assigned institution expands its catchment region. Given that catchment changes occur throughout a year, we establish a half-yearly catchment assignment by rounding the treatment date based on in which half of the year the catchment change occurs. We use data from the years 1999 to 2019.

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