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ABSTRACT

Mergers and Birth Outcomes: Evidence from Maternity Ward Closures*

Evidence suggests that hospital mergers can reduce costs but less is known about their effects on patient outcomes. We study how a wave of mergers that led to the shutdown of one third of all Swedish maternity wards affected the health of mothers who gave birth and their newborns. Applying a difference-in-differences approach to register data on all births in Sweden over two decades, we show that the closures negatively affected the health of mothers, while effects on infant health were small and insignificant. The adverse effects on mothers are mainly driven by crowding effects at remaining wards rather than by increased distance to the wards. Moreover, the closures reduced the use of C-sections for high-risk births.

JEL Classification: D24, I11, I18, J13, R41

Keywords: quality of care, hospital closure, birth outcomes

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1 Introduction

How to tackle increasing health care expenditures is a key policy question in most of the industrialized world. To increase efficiency, a popular policy has been to merge hospitals and concentrate health care to a smaller number of (larger) hospitals.[1] While hospital mergers have been shown to cut costs in some cases (Dranove and Lindrooth, 2003; Schmitt, 2017), less is known about their effects on patient outcomes. Mergers can have beneficial effects through learning-by-doing effects and increased specialization, when remaining non-closed units take on more patients, but may also affect patient outcomes negatively, through increased travel distances and longer queues. This is particularly the case for patients that require immediate care, such as mothers giving birth and heart attack patients.[2]

Estimating the effect of mergers on patient outcomes has proven difficult for several reasons, however. First, patient groups exposed to mergers may differ in unobservable ways from unexposed patient groups. Second, mergers that takes place through shutting down smaller or less efficient units can change the patient composition at remaining hospitals in ways that are difficult to fully account for in an empirical analysis.

In this paper, we provide new evidence on the effects of health care mergers on patient outcomes in a public health care setting. For this, we study the impact of a series of closures of maternity wards in Sweden between 1990 and 2004 on health outcomes of mothers and their newborns. The merger policy led to the closure of mainly smaller maternity wards that were merged to larger ones, which resulted in a nationwide reduction of the number of wards by more than one-third. To study the effects of the closures, we use rich administrative data on the universe of births in Sweden, including individual-level information on infant and maternal health outcomes. Moreover, the data allows us to construct measures of the case load at all maternity wards and on the driving distance to the maternity wards, allowing us to analyze the role of patient congestion and distance when wards shut down.

[1] Merger may take several forms. Dranove and Lindrooth (2003) distinguish between mergers where several hospitals share ownership and operate under a single licence but maintain separate physical facilities and mergers leading to closures of hospitals. We focus on the latter type of mergers in this paper.

[2] In private health care markets, mergers can also affect patient outcomes through reduced competition between health care providers and through higher prices (Keeler *et al.*, 1999; Dafny, 2009). Such effects can be ruled out in our public health care context.

We adopt an empirical design that helps us to overcome the empirical challenges mentioned above. First, the institutional context of the public and universal Swedish health care system allows us to define clear treatment and control groups, who were either exposed or unexposed to mergers of maternity wards. We can do so since Swedish women are assigned to a maternity ward mainly based on their place of residence. We then use a difference-in-differences approach and compare changes in outcomes between treated and untreated women, allowing us to difference out the influence of time-invariant unobserved factors. Second, since we have information on the catchment area of each ward, we can fully account for changes in the composition of patients by focusing solely on patients from the same ward catchment area before and after a closure of an maternity ward.

We start our empirical analysis by comparing health outcomes of all newborns and mothers in regions experiencing a closure to the corresponding changes in “control” regions where no closure took place. This gives us the “net” effect of closures since it captures the effects among all women in the treated regions, irrespective of whether the women were directly facing a closure in their particular catchment area or not. Our results show that closures of maternity wards had an overall negative effect on mothers, in terms of an increased risk of obstetric trauma, while the estimated effects on newborns were small and insignificant.

We then focus on the effects for two different subgroups in the treated regions. First, we focus on the women who experienced a closure of the maternity ward in their catchment area. This group was affected in multiple ways. Their distance to their nearest maternity ward more than doubled and their “new” designated ward was twice as large in terms of its annual number of births. We find no indication that mothers or newborns exposed to a closure in their catchment area were worse off in terms of health outcomes than mothers and newborns in control regions. This suggests that any negative effects from greater distance, and potential crowding at the remaining fewer wards, are offset by positive effects of being treated at a larger ward of, presumably, higher quality.

Second, we focus on women giving birth who were not directly exposed to a closure but whose maternity wards experience an inflow of women from other catchment areas in the same region where a closure took place. The former group faces no change in the distance to their ward, or in the quality of it, but they are exposed to an increased case load at their ward. A closure increased the risk of obstetric trauma for these women and we obtain some evidence that the APGAR scores of their newborns were negatively affected. These results suggest that the increased case load from the additional inflow of patients caused patient congestion that might have adversely affected the health of this group of mothers and infants and that such effect outweighs any learning-by-doing or scale effects.

We use supplementary data to study some potential important mechanisms behind our results. To further evaluate whether the effects are driven by patient congestion and increased case load at the maternity wards that remain after a merger, we use data on the number of employed midwives. The number of births per midwife increased after a closure, implying that the wards did not fully compensate for the increase in case load by employing additional midwifes. We also directly assess the impact of changes in the distance to the nearest ward but find no evidence that the distance to the ward matters. Finally, we study if closures affect how treatments are allocated to low- and high-risk births and find that women who had a high predicted likelihood of obtaining a C-section (based on data from regions unaffected by maternity ward closures) were less likely to obtain one after a closure, suggesting an increased mismatch in the allocation of medical treatments to patients after the closures.

We address several threats to our difference-in-differences design. Event study estimates show that our results are not driven by pre-existing trends that differ across groups. Moreover, closures do not affect fertility patterns, the composition of mothers giving birth, or the composition of midwifes at the wards that remain after a merger.

Our paper relates to several strands of literature. A small literature has studied how mergers through hospital closures in public health care systems affect patient health outcomes, using quasi-experimental designs. Avdic (2016) estimates the effect of hospital closures in Sweden on deaths from heart attacks and finds that an increased distance following closures increases mortality. Avdic *et al.* (2014) report improvements in cancer surgery survival after closures of cancer surgery wards in Sweden, but Gaynor *et al.* (2012), who study hospital mergers in the UK, find no evidence that the mergers improved patient outcomes. Related to our study, Grytten *et al.* (2014) study the effect of local hospital closures on child outcomes in Norway but find no significant effects on neonatal and infant mortality.

Our paper also relates to the literature that study the effect of mergers in competitive health care systems, such as in the U.S. Merger decisions in such contexts reflect private decisions by firms that affect the level of competition in the market, meaning that the results may not be directly transferable to a public health care context. Moreover, only in some cases did the mergers involve *closures* of entire wards, which is the focus of our analysis. Most of the literature finds limited effects of the mergers on costs and prices (see, e.g., Dranove and Lindrooth, 2003; Harrison, 2011; Vogt and Town, 2006; Dafny, 2009). Joynt *et al.* (2015) report that hospital closures in the U.S. led to decreases in the use of inpatient care but did not affect mortality or hospitalization rates. However, Buchmueller *et al.* (2006) estimates the effect of hospital closures in Los Angeles county on deaths from heart attacks and injuries and finds that an increased distance following closures increases mortality. Lorch *et al.* (2013) studied the effect of obstetric unit closures on neonatal and perinatal mortality in Philadelphia and found short-term adverse effects that faded out over time.

More generally, our paper relates to the literature on disparities in health at birth. A recent literature highlights the long-term economic implications of such disparities (see, e.g., Heckman, 2007; Currie and Almond, 2011). Early life health interventions, such as improved prenatal and neonatal care, have shown effective in improving short and long-run outcomes (see, e.g., Almond *et al.*, 2010; Bharadwaj *et al.*, 2013). There is much less evidence on how the organization of maternal care health in itself matters for early child and women health outcomes. Understanding the effect of organizational changes is important for policy, as an inefficient organization means that there are potentially unrealized gains in health that do not necessarily rely on investments in new and expensive technology.

The paper proceeds as follows. Section 2 describes the relevant institutional framework in Sweden. In section 3, we describe the closures of maternity wards and discuss our empirical strategy. Section 4 presents our data. Section 5 presents our main results, reports results from robustness checks and present several analyses with the aim to study mechanisms in more detail. Section 6 concludes.

2 Institutional context

Planned home births are very rare in Sweden and almost all births take place in one of the approximately 80 maternity wards located across the country.[3] Birth deliveries, basic neonatal care and postnatal monitoring of mothers and newborns are performed at hospital maternity wards. If a delivery is carried out without complications, the mother and her newborn child are transferred to a post-natal ward (BB-avdelning), where the hospital staff performs a health examination and assists with information on, for example, breastfeeding.

A midwife typically assists the birth without the active involvement of a physician, unless a delivery is expected to involve significant complications or surgery. The midwife occupation in Sweden is, since the 1950's, a licensed nurse education with an orientation in reproductive and perinatal care. To become a midwife in Sweden, a candidate first has to complete a three year general post-secondary education and, after completing one year of vocational training, an additional 1.5 years specialist education.

Pregnancies are continuously monitored through visits to midwife wards located in health care centers in each of the 290 municipalities of Sweden. The monitoring consists of a set of systematic health check-ups (medical examination, anamnesis, ultrasound, etc.) at different stages of the pregnancy and are complemented with information and consulting for both parents to prepare for the birth and parenthood. If potential complications or other types of risk births are detected, such as, e.g., pre-eclampsia, maternal diabetes or multiple births, the expectant woman may be referred to a nearby hospital to be examined by an obstetrician.

Swedish hospitals, and the maternity wards, are owned, managed, and financed by the public sector, which comprises three tiers; the national, regional and local levels. The responsibility for health care, regulated by the Swedish Health Services Act (1982:763), mainly takes place at the regional level. The regional county councils are the major financiers (via direct taxes raised from the residents) and providers of Swedish health care. There are 21 county councils in total, and each council is obliged by law to provide its

[3] Lindgren *et al.* (2008) notes that from over 1.2 million births over the time period 1992–2004, only 1,600 births were planned home deliveries. The main reason is that home deliveries are neither recommended by health care authorities nor covered by the public health insurance.

residents with equal access to health services and quality of care. Each county council is free to set its own patient fees, but a national cap on co-payments limits the total amount that a patient has to pay out-of-pocket each calendar year.[4] The county councils have, since the end of the 1990's, been allowed to contract with private providers in so-called purchaser-provider split (PPS) models, but most inpatient health care services, including birth deliveries, are still mainly performed by public agents.

For the purpose of our study, an important institutional feature of the Swedish public health care system is that patients are assigned a health care provider based on area of residence. Since health care in Sweden is mainly funded by direct taxes, there exist no individual contractual agreements between providers and recipients of care. Instead, mutually exclusive catchment areas and place of residence determine the specific hospital a patient will be assigned to. This setting ensures that each patient always has a designated hospital, which can be identified by using hospital admission data linked to information on where the patient lives.

3 Maternity ward closures and empirical framework

Throughout the 1990s and beginning of the 2000s, a wave of closures of maternity wards swept over Sweden from an alleged need to reduce costs and increase efficiency in the health care sector. The reorganization was triggered by the economic crisis starting in 1991 and was further reinforced by a new law that stipulated that the regional counties were not allowed to run deficits in their annual budgets. These factors led county councils to explore new cost control strategies, including the relocation of many specialized services from smaller (typically rural) to larger (typically urban) hospitals. The hospitals in which closing maternity wards were situated were not entirely shut down, but were instead turned into health care centers providing specialized medical services for common diseases and elective standardized treatments. As a consequence of this wave of mergers, the number of maternity wards in Sweden dropped by around one-third over a period of roughly one decade (FFCC,

[4] In Stockholm, a visit to a doctor in primary care costs 200 SEK ($25) as of 2017.

2002).[5] Figure 1 provides a timeline of the geographical location and timing of the closures we study in this paper.

All closures of maternity wards were decided by the politicians in the county councils and were often preceded by extensive public and political debate. Commonly used arguments in favor of the closures included cost savings and closures of low-quality wards. Because of case-mix differences across wards, it is difficult to assess ward quality differences, but we do observe in the data that the closed wards on average were much smaller than the remaining wards (see Section 5.1).

[Figure 1 about here]

Merging maternity wards may impact maternal and neonatal health in several ways. First, a closure increases the distance to the nearest ward for some women, since they have to travel to another ward located farther away. If the onset of labor arises unexpectedly, increased travel times may increase the riskiness of the delivery (see, e.g., Viisainen *et al.*, 1999). Second, a closure means that remaining maternity wards in the region have to take on the patient population previously assigned to the closed maternity ward, increasing the remaining wards' case load. This can affect the productivity of the medical staff, due to learning-by-doing effects and specialization effects (see, e.g., Halm *et al.*, 2002). However, the inflow of additional patients may also cause patient congestion problems if staffing and facilities are not fully adjusted to accommodate the increased case load. This can affect patient outcomes negatively, due to increased waiting times or higher workload. Finally, if the closed (smaller) wards are closed because of poor quality, transferring mothers from such lower-quality wards to a higher-quality wards can have positive health effects for these patients. Thus, the net impact of maternity ward closures is a priori ambiguous and depends on the relative strength of the various underlying mechanisms. In this paper, we study both the overall effect and the separate mechanisms.[6]

[5] We classify a ward as closed if the yearly number of births at the hospital decreased by more than 90 percent during a single year. Applying this rule, we identify a total of 18 closures of maternity wards between 1990 and 2004. To validate that the closures were not simply an artifact of incomplete data, we further complement this information with other closure sources such as official documents, media coverage and research reports. No openings of maternity wards occurred during the studied time window.

[6] Since health monitoring of pregnant women and their fetus typically takes place at

In our empirical analyses, we will exploit the institutional feature that area of residence determines the assignment to wards in order to divide mothers into treatment and control groups. We define three types of catchment areas: (i) closure areas, which were subject to a maternity ward closure, (ii) inflow areas, with a remaining ward which were subject to an inflow of patients from the closure areas, and (iii) control areas, which were entirely unaffected by the closures. We identify the inflow areas by tracking patient flows following a closure.

In Figure 2, we illustrate the mapping of mothers to catchment areas. The figure shows a map of Sweden and reports the share of pregnant women in each municipality admitted to their designated maternity ward when giving birth. The overwhelming majority (93%) of patients comply with the assignment, implying that our method of mapping mothers to wards works well. Reasons for not complying could be temporary departures from home, such as vacations, and because the designated ward faces a temporary capacity constraint. It should also be noted that we assign patients to treatment and control groups based on their catchment area and not based on the ward were they actually give birth in our analyses. Moreover, we examined if the closures affected the fraction giving birth at their designated ward, but found no evidence of this.

[Figure 2 about here]

In our empirical analysis, we start by comparing the outcomes of women giving birth and their infants in regions subject to a closure of a ward (both closure and inflow areas in regions where a closure took place) to women and infants in unaffected (control) regions where no closure took place. This will give us the net effect of closures on the health of women and infants. We use panel data to reduce concerns of patient sorting into different catchment areas by comparing changes in outcomes between closure/inflow and control areas in a difference-in-differences empirical design. We assess the underlying common trends assumption in two ways. First, we provide event-study estimates for the health outcomes of interest before and after the closure and, second, we adjust for a rich set of control variables known to be related to birth outcomes, especially risk factors for complications at birth. In addition, we look for

primary care centers outside of the hospital, such monitoring and related treatments should not be affected by the maternity ward closures.

changes in the patient composition by analyzing effects on placebo outcomes, such as maternal age.

Our baseline model for a specific health outcome y_{isrt} for individual i residing in catchment area s within county r in year t is

$$y_{isrt} = \alpha + \beta_C C_{st} + \lambda_s + \lambda_t + (t \times \lambda_r) + X'_{it}\beta_X + \varepsilon_{isrt}, \tag{1}$$

where C_{st} is an indicator variable for whether the individual was affected by a ward closure in year $t \geq T_c$, where T_c is the year of the closure. We control for time-invariant differences across catchment areas through local area fixed effects, λ_s, nation-wide trends in maternal and infant health through calendar year fixed effects, λ_t, and pre-birth health characteristics, defined by the vector X'_{it}. We also include regional linear time trends, $(t \times \lambda_r)$, to adjust for any regional-specific variation in outcome trends. The main parameter of interest, β_C, captures the average effect of the closure for all follow-up years. We mainly estimate (1) by OLS, but obtain similar results if we use a non-linear logit model for our binary outcomes. Standard errors are clustered at the parish level.

In a second step, we estimate separate regression models for mothers in closure areas and inflow areas, in both cases comparing their outcomes to those of mothers in regions where no closures took place. This allows us to study which group of women that are affected by the closures, which, in addition, may provide insights into the mechanisms at play.

4 Data

We use data from several Swedish administrative registers. First, the Intergenerational Register (IGR) contains linked personal identifiers between parents and their children for the entire Swedish population. We link this information to data from the National Patient Register (NPR) and the Medical Birth Registry (MBR), which includes relevant pregnancy- and birth-related information for mothers and infants, respectively. The NPR contains detailed medical information on cause for admission and any co-morbidities, and information on medical procedures, such as type of surgery and complementary treatments. Since we can follow individuals over time, we can create detailed measures of the medical history of mothers. The MBR provides complementary health information on newborns, such as APGAR scores, birth weight, risk factors, and any pre- or post-natal complications. Finally, we augment the clinical information with socioeconomic and demographic background characteristics from the population-based LOUISE register, containing information on, for instance, marital status and place of residence.

In our analyses, we focus on births taking place in Sweden between 1990 and 2004. Since almost all births in Sweden takes place at hospitals, our data covers more or less the universe of births during this time period.[7] In our analyses, we control for a rich set of covariates known to be related to birth outcomes (see, e.g., Dubay *et al.*, 1999, 2001; Currie and MacLeod, 2006; Shurtz, 2014). This includes socioeconomic characteristics (e.g., age, foreign born and marital status), medical history (e.g., tumors, obesity and heart diseases), pregnancy-specific conditions (e.g., diabetes, anemia and early onset birth) and delivery-specific conditions.

We also exploit geo-coding data in our analyses to determine the distance from an individual's place of residence to her designated maternity ward. The data are obtained from LOUISE and are computed using three different methods: minimum distance, travel distance, and travel time. The two latter distance measures are computed using the Google® Maps API software.[8,9]

To measure infant health, we use information on APGAR score after one, five and ten minutes, infant mortality, and commonly occurring birth traumas.[10] For infant mortality, we include neonatal and perinatal death mor-

[7]To identify births in the National Patient Register (NPR) we select all inpatient records with a main ICD-10 diagnosis identifying a single spontaneous (O80), forceps or vacuum extractor assisted (O81), cesarean section (O82), other assisted birth (O83), and multiple delivery (O84), respectively. We have validated our final sample of births by comparing it to official Swedish birth statistics.

[8]The coordinates are based on the RT-90 standard and computed using the transverse mercator map projection. In contrast to the standard projection, the transverse projection takes into account that the world is shaped as an ellipsoid by using so-called geodetic datums in order to deliver improved accuracy positioning measurements. According to the Swedish Ordnance Survey, the RT-90 measurements cover approximately 3,800 triangular points over the country with a relative distance accuracy of 1-2 ppm (mm/km).

[9]The coordinates used in the analysis are midpoints in the Small Areas for Market Statistics (SAMS) classification, created by Statistics Sweden in January 1994 and last revised in 2003. The classification is based on registered property names (NYKO) in the larger municipalities and on electoral districts in the smaller. The total number of SAMS districts in Sweden are about 9,200. The SAMS division has remained largely intact over time and any revisions are minor adjustments have been made to adjust the boundaries of updated municipal borders.

[10]The APGAR score is based on the heart rate, respiratory effort, reflex irritability, muscle tone and the color of the infant. For each sign the newborn is given a rate of either

tality, referring to deaths within the first 28 days of life. The main maternal health outcomes included in the analysis are various types of obstetric trauma, capturing the severeness and prevalence of complications at birth (see, e.g., Iizuka, 2013). Perineal lacerations is a common indicator for comparing health care quality across countries (cf., OECD, 2011) and is classified into four categories of increasing severity.[11] We also include other obstetric trauma as a residual category.[12]

Table 1 shows descriptive statistics on the variables used in our analyses.

[Table 1 about here]

5 Results

5.1 Main results

We start our empirical analysis by illustrating how closures of maternity wards affect the case load (annual number of births) at remaining wards in the same region and the average distance to the nearest ward.[13] In Panel A of Figure 3, we see that relatively small wards are merged with larger wards and after a closure, individuals residing in closure areas are giving birth at wards with twice as many births. Mothers giving birth in inflow areas also experience an increased case load, due to the inflow of additional births from the closed wards, but the increase is more modest. As expected, we observe no increased case load for wards in control regions. Next, panel (b) shows that the average distance to the ward for individuals in the closure group more than doubles after the closure, increasing from 14 to 32 kilometers. The average distances

0, 1 or 2 from worst to best, which is then summed up to a sum between 0 to 10. A rule of thumb is that scores of 7 and below are considered as low (see, e.g., Carlo, 2011).

[11]According to the Agency for Health care Research and Quality (AHRQ), first and second degree perineal lacerations are the most common complicating condition for vaginal deliveries in the U.S. (cf., Moore *et al.*, 2014). The more severe third and fourth degree lacerations are used as AHRQ patient safety indicators.

[12]This group includes rupture of uterus, laceration of cervix, haematoma of pelvis and other obstetric injury to the pelvic organs. We do not consider maternal mortality here as it is an extremely uncommon outcome in Sweden during the time period we study.

[13]In practice, for each closure we observe a closure year. To compare with control areas, we sample the outcomes in all control areas before and after this specific closure year. Since, the closure occur in different years we do this for all actual closures years in our data. We then average over all closure years. Thus, year zero is the actual closure year for the closure and the inflow areas and the *potential* closure year for the control areas.

are unaffected for individuals in the inflow areas and in the control regions, as expected.

[**Figure 3 about here**]

We next report our main results: the effect of closures on maternal and infant health outcomes, using the model described by equation (1). Column 1 of Table 2 reports the net effects of the closures, where the treatment group contains individuals in both the closure and inflow areas. For infant health (Panel A), we find no significant effects for any of the three APGAR scores (1, 5 and 10 minutes) or for infant mortality.[14] Instead, we find a sizable and statistically significant increase in the probability of maternal trauma by 2 percentage points (Panel B). Compared to the mean trauma rate of individuals residing in the control regions, this corresponds to a 30 percent increase. When we estimate separate effects for different types of trauma, we see that the effect on trauma is primarily driven by an increase in the less severe traumas (first and second degree lacerations). Further below, we will examine whether these effects are mainly driven by women in closure or inflow areas.

Columns 2-3 of Table 2 show estimates from two robustness analyses. Our main model in Column 1 includes linear regional trends, but in Column 2 we exclude these trends. In Column 3, we use a logit model instead of a linear probability model for our binary outcomes, since both infant mortality and the maternal traumas are rare events. The results from these robustness analyses do not change our conclusions to any important extent. If anything, we find stronger negative health effects when the regional trends are excluded from the model; somewhat larger negative effects for mothers and also an indication of negative effects on APGAR 10. Although the latter effect is significant, the magnitude of the effect is small.[15]

We next investigate if the effect of closures are different for mothers and children residing in closure and inflow areas. The results in Columns 4-5 of Table 2 show that the increased risk of maternal trauma are only observed on mothers residing and giving birth in inflow areas. Furthermore, the zero effect

[14] We also obtain insignificant estimates for 7-days mortality.

[15] Two minor differences are also that for 3–4 degree lacerations, the effect is significant at the 10-percent level with trends but insignificant without the trends. For APGAR 1 scores, we find a significant, positive, but tiny effect without trends.

on infant health (APGAR) in column (1) masks some heterogeneity across groups; for mothers residing in inflow areas, we obtain small, significant, and negative effects on infant APGAR scores, while no effects are observed for mothers in closure areas.[16]

[**Table 2 about here**]

The results above shed light on the different mechanisms at play. The negative effects observed for mothers residing in inflow areas indicate that crowding effects, due to the inflow of additional births at their wards, may be important. Recall that this group does not face any change in the distance to their ward and they remain at the same ward, so that they mainly is affected by a greater case load at their wards. Mothers in closure areas are affected both by potential crowding effects, as they transfer to the remaining wards, and by an increased distance to their ward. They are also transferred to larger wards of presumably higher quality, however. The absence of negative health effects for this group suggests that the positive effects of being transferred to a larger, and potentially better, ward outweigh any negative effects from increased distance and higher case load.

5.2 Threats to identification

Before analyzing potential mechanisms in detail, we consider some potential threats to our identification strategy. Figure 4 provides event-study estimates of year-by-year effects where we focus on on maternal trauma (panels a and b) and APGAR 1 (panels c and d). To this end, we estimate model (1) but allow for separate effects for each year before and after the closure. The magnitude of the change in each outcome is indexed by the value in the year prior to the closure. Reassuringly, for both closure areas (panels a and c) and inflow areas (b and d), we see no differential pre-trends, suggesting that the common trends assumption underlying our difference-in-differences design is supported. After the actual closure, there is a tendency towards increased maternal trauma in both closure and inflow areas, but for the closure group the effect is only significant for the first year after the closure, while the effect

[16] Results from estimating the model on only the subgroup of low birth-weight infants (<2500g), as an indicator for high-risk births, does not yield any statistically significant estimates.

is significant for almost all years in the inflow group. We see a similar but less distinct pattern for the APGAR score after 1 minute.

[Figure 4 about here]

Another concern is that closures of wards may affect fertility patterns through, for instance, affecting the perceived risk of giving birth. Moreover, closures of wards may trigger women to move to areas where wards remains. Such responses to closures could change the composition of women giving birth and complicate the interpretation of our estimates. To check for composition changes, Figure 5 displays event-study estimates for the number of births and maternal age before and after the closure. There is no evidence for any compositional changes over time for mothers in closure areas (panel a and c) or for mothers in inflow areas (panel b and d).

[Figure 5 about here]

5.3 Mechanisms

We next study some potential mechanisms behind our results. First, we focus on the effect of the distance to the nearest maternity ward. For this, we exploit detailed geographical information for women who resided in the closure areas and compute the distance to their nearest ward before and after the closure.

To identify the effect of distance, we exploit that the change in distance to the nearest ward for mothers in closure areas depends on where they live *within* the closure area. By comparing mothers in a closure area who end up giving birth at the same remaining ward after the closure, but who face differential changes in the distance, we are able to control for other effects of the closures, such as crowding and quality effects. In practice, we achieve this by interacting hospital and time fixed effects, i.e. by allowing for differential hospital fixed effects in each year, which will adjust for any general differences between the wards as well as any closure effects that are unrelated to the distance to the ward. To control for pre-existent differences between mothers in different local (closure) areas, we also include local area fixed effects.[17] The estimated distance effects are presented in Table 3 for both the full sample

[17]The full regression model for mothers in the closure areas includes local area fixed effects, separate ward fixed effects for each year, regional linear trends and maternal characteristics (see Table 1).

and a low birth-weight sample (to study potential heterogeneity for more risky births). Interestingly, the estimated coefficients do not reveal any important effects of the distance to the maternity ward.[18] This is an important result, since the increase in distance to a birth clinic is often one major public concern when clinics close. The fact that we find no significant effect of the distance to the ward is also one explanation to why we see no negative health effects for mothers and newborns in areas where wards close.

[Table 3 about here]

In Figure 3 above we observed that closures led to an increased case load at remaining wards, possibly resulting in longer waiting times and increased staff workload. Such effects could be offset, however, by an increase in the number of midwives at the remaining wards, as some midwives from closed wards may transfer to the remaining nearby wards. To evaluate changes in the workload per midwife at the remaining wards, we have collected data on the number of midwives working in each ward in each year. We then use the number of births per midwife as our outcome variable in model (1) and study the impact of closures. As shown in Figure 6, the number of births per midwife increases following the closure. The increase is around 2 additional births per midwife, which amounts to an increase of 6.4%.[19] Even if this is a rather modest increase, crowding effects may still be important, as closures can result in other resource constraints, besides in the number of midwives.

[Figure 6 about here]

That some midwives transfer from closed to remaining wards raises the concern that the composition of midwifes changes. If the remaining wards recruit the more skilled midwives from the closing wards, this may have a direct effect on the outcomes of mothers and their babies. We therefore run regressions on the effect of closures on the characteristics of the midwives at remaining wards. We focus on the age and the annual earnings of midwifes, assuming that age is a proxy for experience and that earnings capture productivity. As shown in Figure 7 we find no effect of closures on these outcomes,

[18] We also found insignificant effects when we instead used travel time and the minimum distance to the ward as well as when dividing distance into different bins.

[19] The average number of births per midwife is 31.2. This relatively low number reflects that many midwifes are part-time employed and that they have other tasks besides deliveries.

suggesting that changes in the composition of midwifes, at least in terms of experience and productivity, cannot explain our results.

[**Figure 7 about here**]

Finally, we examine if closures, and the increase in the case load that follows, affect treatment decisions. We focus on the use of Cesarean sections (C-sections) for low-risk and high-risk births and study if the C-section rate is affected by closures. For this, we follow Currie and MacLeod (2017) and construct a measure of the mothers' appropriateness for a C-section. We use data from the control regions, which were totally unaffected by any maternity ward closures, to estimate the likelihood (in a logistic regression) of obtaining a C-section as a function of the observable characteristics described in Table 1. The propensity of C-sections in these areas control areas provide a benchmark for the appropriateness of a C-section based on the observable characteristics of the expectant woman.[20] We then use the estimated parameters from this model to predict the appropriateness (or "risk") of a C-section delivery for all mothers at the remaining wards before and after these wards experience an inflow of additional births after the closure of a nearby ward, and relate this measure to the actual C-section rates before and after a closure occurred.

The results are presented in Figure 8, where we relate the share of actual C-sections before (circles) and after (triangles) the closure to the predicted appropriateness of a C-section in bins of 0.05. We have also added a 45 degree line to the figure, reflecting the decisions made by the wards in the control regions. In the figure, better decision-making at the remaining wards would correspond to a smaller share of actual C-sections for low appropriateness patients and a higher share of actual C-sections for high appropriateness patients, reflecting a reallocation of C-sections from low-risk cases to high-risk cases. Figure 8 shows that before the closures (circles), the remaining wards had a slightly higher C-section rate for low-risk cases and a slightly lower rate for high-risk cases. After the closure we see an overall shift downwards in the C-section rate, but this decline is sharpest for women who, based on their observed characteristics, were the most appropriate to receive a C-section. After the closure, these high-risk cases are less likely to obtain a C-section. This

[20] Since the popularity of C-sections have generally increased over time, we also include year fixed effects in these regressions to account for such trends.

can be interpreted as evidence of an increased mismatch in the allocation of medical treatments to patients after the closures.[21]

[Figure 8 about here]

6 Concluding remarks

We have studied how mergers of maternity wards affect the outcomes of the mothers and their newborns. Our results show that the net effect of merging maternity wards is negative for mothers but small and insignificant for their newborns. The negative health effects are driven by increased maternal trauma rate among mothers in the inflow areas, who do not face a closure of their own maternity wards themselves, but whose wards face an inflow of additional women from areas where wards closed. We find no significant health effects for mothers and newborns in the closure areas, who due to the closures are reallocated from a smaller rural ward to a larger remaining ward. One likely explanation is that the positive effects of being treated at a larger ward of, presumably, higher quality outweighs any negative effects of increased distance and case load.

We provide additional empirical evidence on potential mechanisms. First, the closures lead to a somewhat larger number of births per midwife at the remaining wards, lending some support to a crowding mechanism. Second, we find no significant effect of the distance to the ward, which partly explains why we see no negative health effects for mothers and newborns in areas where wards close. It should be noted, however, that although the average distance doubled, the mothers were on average still not very far from the nearest maternity clinic after the closure (32 kilometres). Larger changes in distances could be more detrimental to the health of mothers and their newborns. Third, closures affected treatment decisions, such that high-risk cases at the remaining wards are less likely to receive a C-section after the closure, possibly due to crowding at these remaining wards.

[21] Since C-sections also create scars, we have examined if the overall decline in the C-section rate at the remaining wards can explain the increase in the 1–2 lacerations, suggesting a substitution from C-section scars to 1–2 degree lacerations. To investigate this, we have estimated model (1), using a binary indicator of maternal trauma and/or a C-section. For this outcome measure, we also see a sharp increase at the remaining wards following a closure, meaning that the increase in maternal trauma is not entirely explained by substitution away from C-sections.

In conclusion, we provide evidence of a negative net effect of closing maternity wards on women's health, at least in the short-run. In contrast to common arguments, these adverse effects do not arise due to increased distance to the wards, but rather through crowding at the remaining wards to which patients in the areas with closed wards were assigned. These negative health effects should be taken into account when weighing the pros and cons of concentrating maternal health services to a smaller number of (larger) wards.

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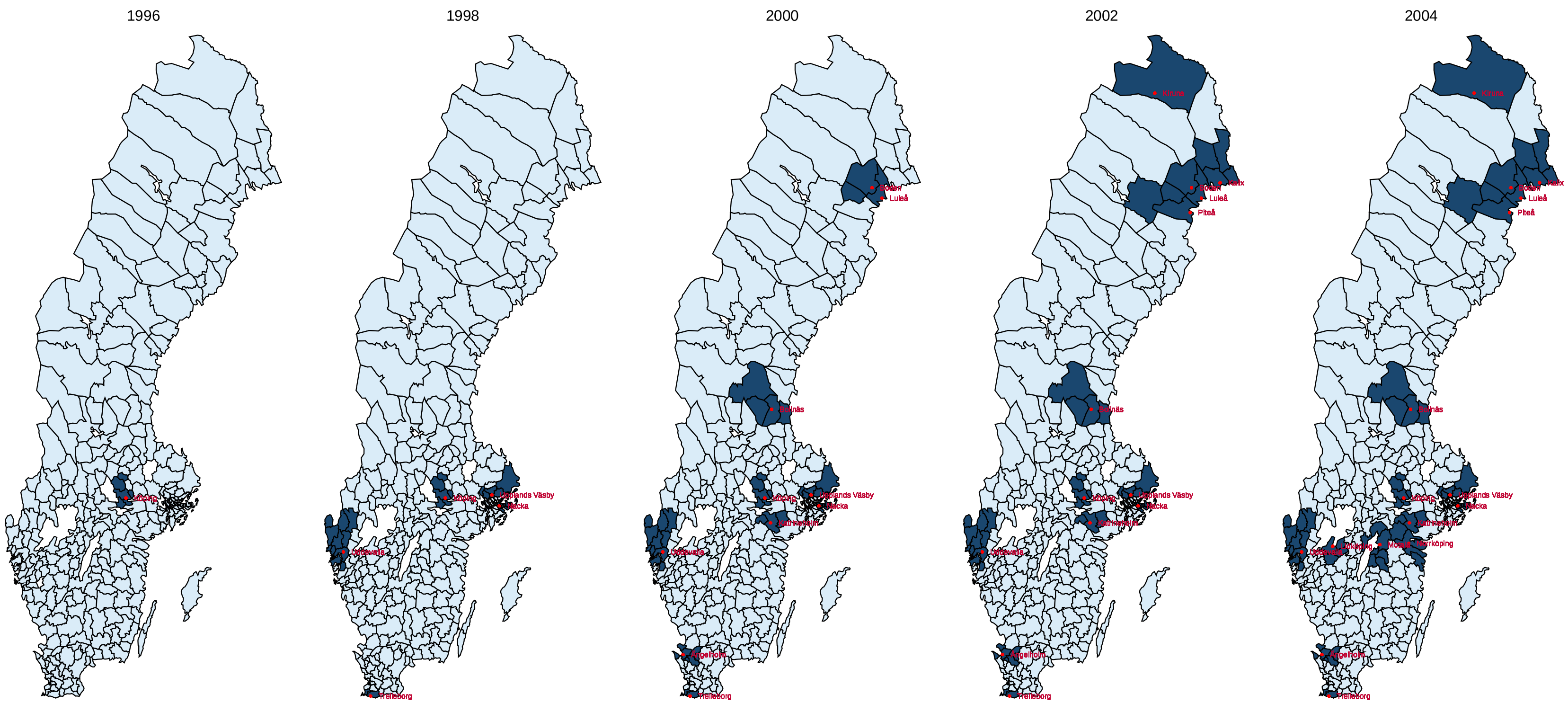
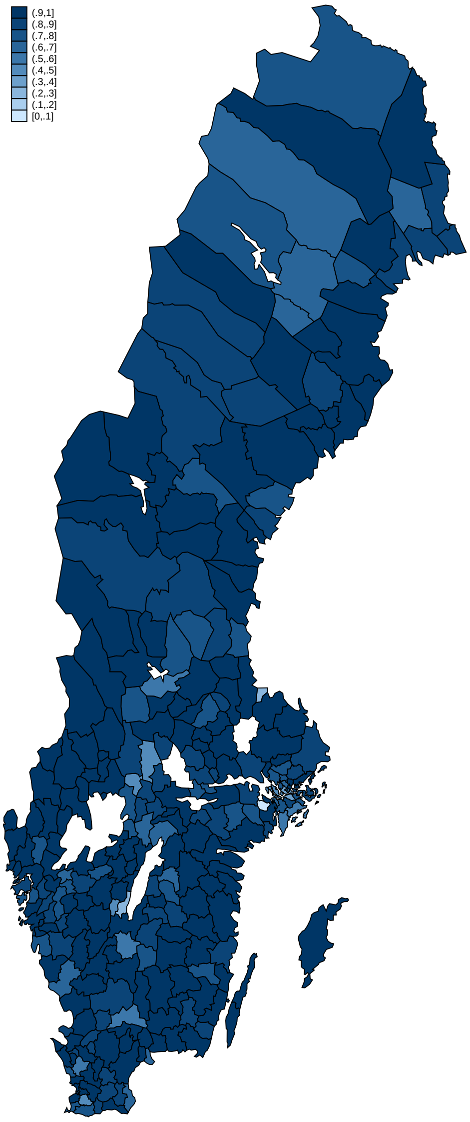
Tables and Figures

FIGURE 1.
Municipals affected by maternity ward closures, 1990–2004



NOTE.— Dark blue areas correspond to municipals affected by a nearby maternal ward closure. The red text indicate the location and name of the closed ward.

FIGURE 2.
Share of births occurring at designated hospitals in Sweden by municipal



NOTE.— A designated hospital is defined for each individual as the hospital in which most of the births in the municipal that the individual resides in takes place. Data is aggregated for years 1990–2004.

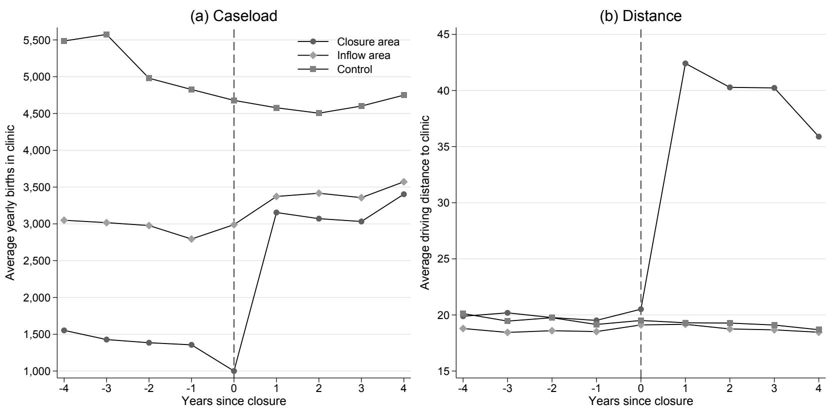
Table 1.

Descriptive sample statistics

Variable	Sample			
	All	Control areas	Inflow areas	Closure areas
Maternal characteristics				
Age	29.13	28.90	29.52	28.89
Cohabiting (%)	88.25	89.10	87.20	88.17
Earnings before tax	58,696.90	54,789.04	64,310.12	56,552.91
Tumor (%)	0.03	0.03	0.03	0.02
Substance Dependence (%)	0.00	0.00	0.01	0.00
Obesity (%)	0.03	0.04	0.02	0.03
Heart Diseased (%)	0.11	0.12	0.10	0.10
Respiratory Disease (%)	0.17	0.20	0.16	0.14
Diabetes (%)	0.78	0.87	0.73	0.67
Pregnancy- and delivery-specific conditions				
STD (%)	0.05	0.04	0.06	0.04
Rhesus Incompatibility (%)	0.11	0.11	0.11	0.12
Umbilical Cord (%)	0.42	0.37	0.44	0.51
Anemia (%)	3.30	3.36	3.51	2.70
Early Onset delivery (%)	5.00	5.11	4.94	4.70
Prolonged Pregnancy (%)	3.60	3.45	3.80	3.36
Labor Dystocia(%)	9.15	9.	9.18	9.25
Placenta (%)	2.55	2.62	2.55	2.42
Hypertension (%)	4.45	4.50	4.50	4.31
Child outcomes				
Apgar at minute 1	8.70	8.71	8.69	8.70
Apgar at minute 5	9.73	9.72	9.74	9.76
Apgar at minute 10	9.87	9.86	9.87	9.88
Infant mortality 0-7 days (%)	0.23	0.23	0.23	0.24
Infant mortality 0-28 days (%)	0.29	0.29	0.28	0.29
Maternal Outcomes				
Trauma during Delivery (%)	7.73	6.95	9.04	7.11
1st or 2nd Deg. Perinea (%)	4.21	3.59	5.19	3.84
3rd 4th Deg. Perineal (%)	2.46	2.33	2.71	2.20
Other Trauma (%)	1.18	1.10	1.29	1.18
Number of Births	1,322,967	586,337	516,783	219,847

Note.— The table reports mean values for each variable by sample. See the text for variable and sample definitions. Earnings are measured in Swedish crowns (SEK). One crown corresponds to around 0.1 euro in 2015.

FIGURE 3.
Maternity ward closures, caseloads and distance, by time since closure and catchment area



NOTE.— Lines refer to group averages for closing, inflow and control catchment areas by time from closure (see text for definitions).

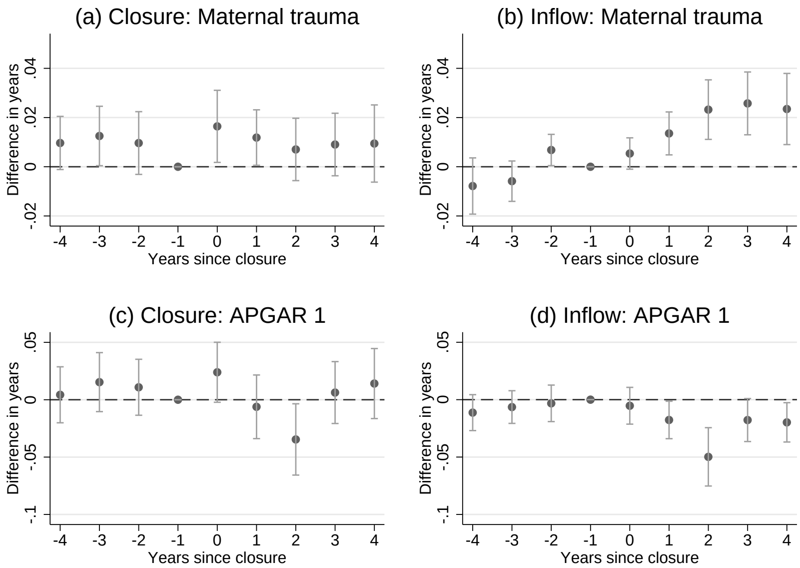
Table 2.
Estimated impact of a maternity ward closure on infant and maternal health

	Closure and Inflow			Closure	Inflow
	Main (1)	No trends (2)	Logit (3)	(4)	(5)
Panel A: Infant health					
APGAR 1	0.011	0.022***		0.0067	-0.012*
	(0.0087)	(0.0056)		(0.016)	(0.0069)
Control mean:	8.71	8.71		8.71	8.71
APGAR 5	-0.0019	-0.0034		-0.0088	-0.019***
	(0.0059)	(0.0041)		(0.0094)	(0.0051)
Control mean:	9.72	9.72		9.72	9.72
APGAR 10	-0.0084	-0.0090***		-0.0067	-0.018***
	(0.0058)	(0.0032)		(0.0077)	(0.0045)
Control mean:	9.86	9.86		9.86	9.86
Child mortality	-0.00041	-0.00014	-0.19	-0.00033	0.000022
	(0.00039)	(0.00020)	(0.13)	(0.00033)	(0.00027)
Control mean:	0.0029	0.0029	0.0029	0.0029	0.0029
Panel B: Maternal health					
Trauma	0.021***	0.028***	0.30***	0.0011	0.020***
	(0.0038)	(0.0046)	(0.047)	(0.011)	(0.0048)
Control mean:	0.069	0.069	0.069	0.069	0.069
1-2 degree laceration	0.015***	0.026***	0.45***	0.0013	0.018***
	(0.0032)	(0.0040)	(0.079)	(0.011)	(0.0039)
Control mean:	0.035	0.035	0.035	0.035	0.035
3-4 degree laceration	0.0021*	-0.00062	0.014	-0.0010	-0.0011
	(0.0012)	(0.00076)	(0.043)	(0.0027)	(0.00099)
Control mean:	0.023	0.023	0.023	0.023	0.023
Other trauma	0.0043***	0.0046***	0.36***	-0.00036	0.0030**
	(0.0016)	(0.00090)	(0.095)	(0.0011)	(0.0013)
Control mean:	0.011	0.011	0.011	0.011	0.011
Time trends	✓		✓	✓	✓
Observations	1,288,790	1,288,790	1,288,790	755,241	1,107,495

Note.— Swedish data for the period 1990-2004. Reported coefficients are the estimated β_C from estimating model (1) for different samples. All models adjust for local area fixed effects, year fixed effects, maternal socioeconomic characteristics and maternal pre-pregnancy health measures (see Table 1). Standard errors clustered at the parish level in parentheses. *p<0.1 **p<0.05 ***p<0.01.

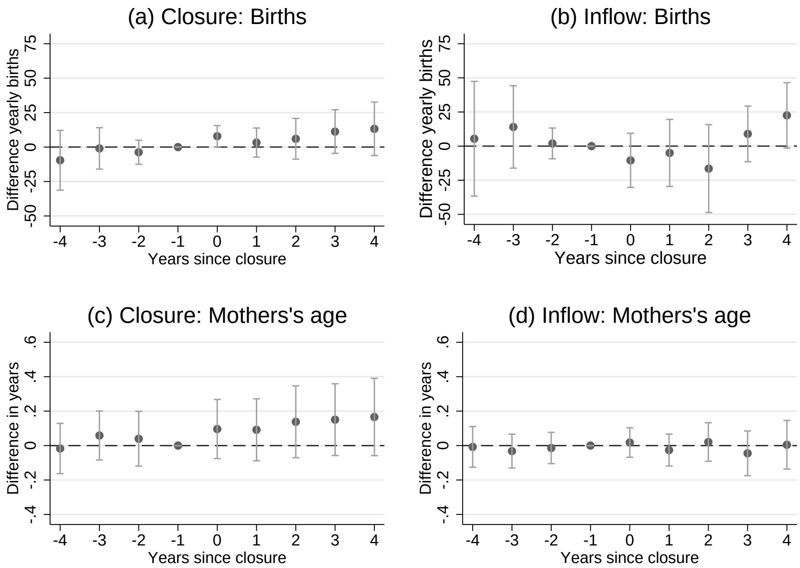
Figure 4.

Event study of a maternity ward closure on infant and maternal health



Note.— Swedish data for the period 1990-2004. Reported estimated coefficients β_C from model (1) separately for each year from the year a closure occurred: $-m, -m-1, ..., m-1, m$. All models adjust for local area fixed effects, year fixed effects, regional linear trends, maternal socioeconomic characteristics and maternal pre-pregnancy health measures (see Table 1). Standard errors clustered at the parish level.

FIGURE 5.
Event study of a maternity ward closure on annual births and maternal age



NOTE.— Swedish data for the period 1990-2004. Reported estimated coefficients β_C from model (1) separately for each year from the year a closure occurred: $-m, -m-1, ..., m-1, m$. All models adjust for local area fixed effects, year fixed effects, regional linear trends, maternal socioeconomic characteristics and maternal pre-pregnancy health measures (see Table 1). Standard errors clustered at the parish level.

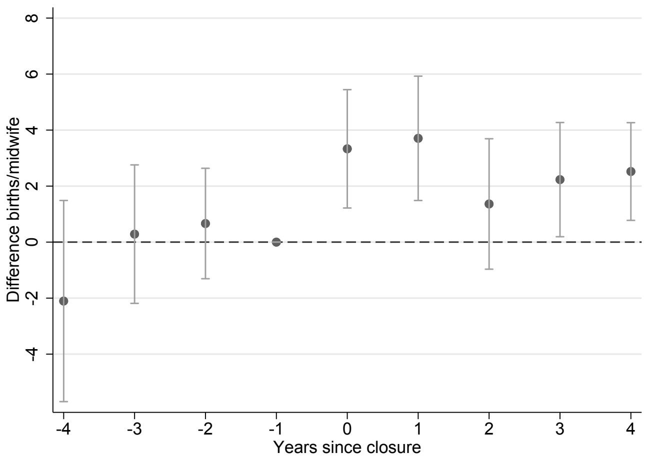
Table 3.
Estimated impact if the distance to the maternity ward on infant and maternal health.

	APGAR 1		Maternal trauma	
	All	Low birth weight	All	Low birth weight
	(1)	(2)	(3)	(4)
		Distance in kilometers		
Distance (km)	0.0048*	-0.0037	0.00012	-0.0098
	(0.0025)	(0.018)	(0.0021)	(0.0064)
		Distance in categories		
10-30 kilometers	0.0013	0.010	-0.0078*	-0.011
	(0.0050)	(0.029)	(0.0044)	(0.0091)
30-60 kilometers	0.014**	0.046	-0.00080	0.0025
	(0.0073)	(0.047)	(0.0058)	(0.018)
60+ kilometers	0.020	-0.099	-0.0013	0.040
	(0.018)	(0.12)	(0.012)	(0.033)
Observations	757,159	32,099	757,159	32,099

Note.— Swedish data for the period 1990-2004. The sample consists of individuals in the closure ares. The model also includes local area fixed effects, separate ward fixed effects for each year, regional linear trends and maternal characteristics (see Table 1). Standard errors clustered at the parish level in parentheses. *p<0.1 **p<0.05 ***p<0.01.

FIGURE 6.

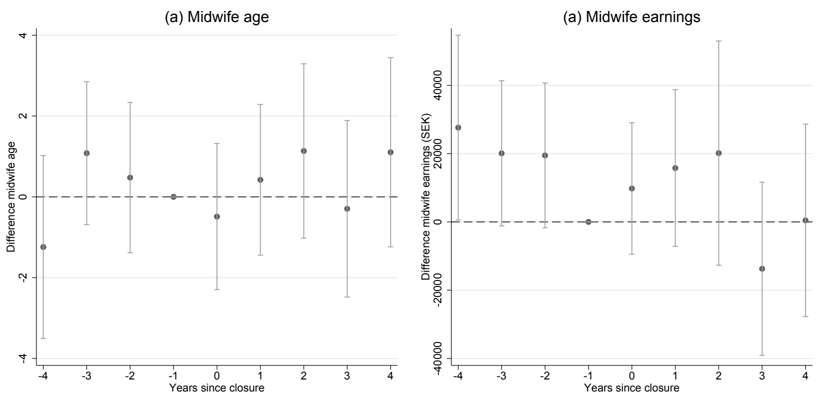
Event study of a maternity ward closure on births per midwife in the inflow areas.



NOTE.— Swedish data for the period 1990-2004. Reported estimated coefficients β_C from model (1) separately for each year from the year a closure occurred: $-m, -m-1, ..., m-1, m$. All models adjust for local area fixed effects, year fixed effects, regional linear trends, maternal socioeconomic characteristics and maternal pre-pregnancy health measures (see Table 1).

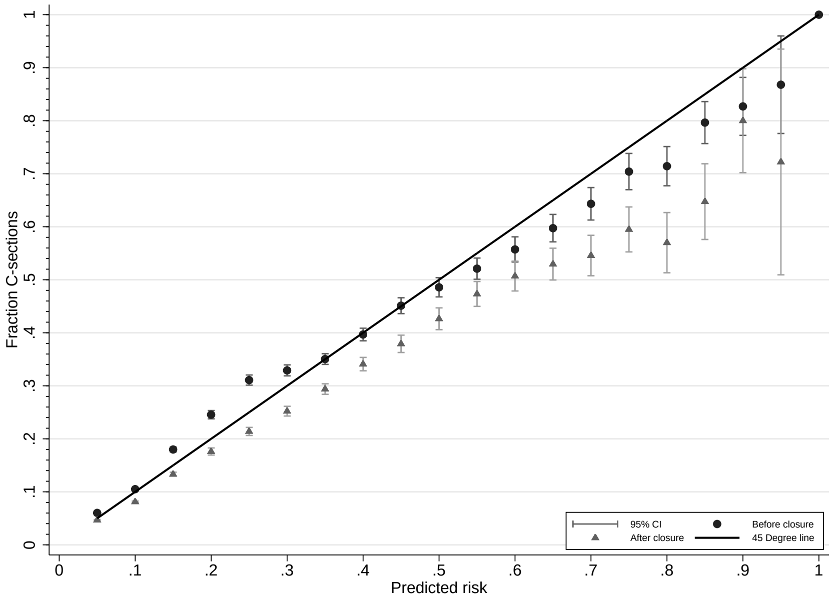
FIGURE 7.

Event study of a maternity ward closure on average midwife characteristics



NOTE.— Swedish data for the period 1990-2004.
Reported estimated coefficients β_C from model (1) separately for each year from the year a closure occurred: $-m, -m-1, ..., m-1, m$. All models adjust for local area fixed effects, year fixed effects, regional linear trends, maternal socioeconomic characteristics and maternal pre-pregnancy health measures (see Table 1). Standard errors clustered at the parish level.

Figure 8.
Fraction actual Cesarean section by predicted risk



Note.—The propensity score estimation is performed using a logit model with the full set of regressors in Table 1 for individuals belonging to a remaining ward before and after a nearby closure occurred.