Long-term causal effects of access to institutional delivery service on dementia risk

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January 10, 2023

Abstract

This paper analyzes the early-life determinants of later-life dementia risk. We exploit a Swedish intervention that affected supply of hospital maternity wards during the 1926–46 period. Using exogenous variation in the supply of maternity wards to instrument the likelihood of institutional delivery, we find that delivery in hospital has substantial effects on late-life dementia. Exploiting the longitudinal dimension of our data, we are able to show that an improvement in cognitive abilities is visible already in childhood, and also manifest in the task contents of mid-life occupations. We argue that a decrease in child morbidity due to better treatment of complications is a likely mechanism. We also study the selection into treatment and argue that there seems to be very strong selection into treatment, which implies that the estimated LATE would seriously overstate the population-level treatment effect.

Keywords: institutional delivery; old-age dementia; difference-in-discontinuities

JEL classification: I18, I13, N34

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‡We are grateful to Sonia Bhalotra, Veronica Grembi, Reyn van Ewijk, James Fenske, Søren Rud Kristensen, Daniel Kühnle, Andreea Mitrut, Hanna Mühlrad, Bettina Siflinger and Yuejun Zhao for their comments and suggestions, Lukas Hörnig for excellent research assistance.

1 Introduction

In the light of limited progress regarding disease-modifying medical treatments for dementia, the Lancet 2020 commission highlights the importance of prevention targeting modifiable risk factors Livingston et al. [2020]. The commission report states that very early life risk factors – such as poor health or nutrition of pregnant mothers, pregnancy difficulties, early life malnutrition, and survival with heavy infection burdens during pregnancy and early childhood – can be important for developing late-life dementia, especially in low and middle income countries. Several studies show that age-specific dementia incidence rates significantly declined over the past 20—30 years Wu et al. [2017], Langa et al. [2017], Nichols et al. [2019].This trend coincides with increases in educational attainment, improvements in cardiovascular disease risk, but also massive improvements in the early life circumstances of today's elderly in high-income countries.

A life cycle perspective suggests that the process of dementia development partially origins from conditions in very early life [Barnett et al., 2013, Fratiglioni et al., 2020]. Meta analyses show association with dementia for a wide range of very early life proxy variables [Wang et al., 2019, Beydoun et al., 2014]. Preterm births are associated with mild cognitive impairment (MCI) [Blencowe et al., 2013, Heinonen et al., 2015]. Low birth weight, short leg, arm and body length, and small head circumference at birth are identified as risks for poor late-life cognitive functioning [Huang et al., 2008, Raikkonen et al., 2013, Short and Baram, 2019, Mosing et al., 2018]. Recent empirical evidence shows that birth complications leading to cerebral palsy substantially increase dementia risk [Mahmoudi et al., 2022, Smith et al., 2021]. Research thus supports the importance of very early life health for late life cognitive health and suggests that public health interventions targeting early-life health can modify dementia risk. There is however a lack of empirical research that investigates whether public interventions may alleviate such adverse effects of early life shocks. In this paper, we study the potential of access to institutional delivery service to alter early life environment and the subsequent risk of dementia. Using a quasi-experimental approach, we evaluate the long-term effects of openings and extensions of maternity wards as an exogenous variation in the access to institutional delivery service. The main alternative to a institutional delivery at the time was home births assisted by a skilled and licensed midwife. To evaluate late-life dementia risk, we link data on historical maternity ward openings and extensions to individual level data for cohorts born 1930–1946 in

Sweden. Data from administrative registers on inpatient health and cause of death is used to identify dementia cases in late life.

Hospital delivery had the potential to improve the health of the infants given the fact that they expand the level of available health care. In the period we consider, less than 5 per cent of home deliveries would involve a medical procedure, which typically involved calling a doctor. As we will show, the opening or extension of a maternity ward led to a near doubling of that rate in the overall population of home and hospital births. However despite this clear move in the direction of an increased medicalisation of childbirth, it remains an open issue whether deliveries in hospital are beneficial enough to leave a mark on later-life outcomes in the wider population, for four reasons. First, serious complications that possibly harm the baby for life – like e.g. uterine rupture, shoulder dystocia or chorioamnionitis – affect only a minority of births.[1] Second, considering the general inadequacy of prenatal care at the time, the mothers who decided to give birth in hospital might not have been selected based on risk – thus leaving a large proportion of risky births untreated. Third, the advantage of treating these conditions in hospital might not have been large enough to make a difference in the aggregate. This is manifested already in the fact that some of them represent serious risks to the baby even today.[2] In addition, previous literature has identified access to modern medical technologies as a key mediator behind improved child outcomes [Daysal et al., 2015]; these were not available during the period we cover. Fourth, most complications that risk harming the baby also reduce their survival chances. Thus, even if delivery in hospital is beneficial to the child's life prospects, a survival selection might still make it hard to detect effects on adult outcomes [cf. Almond and Currie, 2011, Floris et al., 2019].

Our main finding is however that the impact of institutional delivery on dementia risk is robustly large. Depending on specification, the intent-to-treat effect is estimated at 0.5-0.9 percentage points, which corresponds to a reduction in dementia risk by around 10 per cent. The estimated local average treatment effect at 4-7 percentage points is remarkably large, and suggests strong selection into treatment based on perceived benefits. Exploiting the longitudinal character of our data, we are able to estimate the impact of the intervention on indicators of

[1] The incidence of uterine rupture, which increases the risk for brain damage and death, is estimated at 0.053% for a combination of low- and high-income countries [Justus Hofmeyr et al., 2005]. Shoulder dystocia, which has similar risks associated, is estimated at 0.5% [Menticoglou, 2018]. Chorioamnionitis has an estimated incidence of less than 1%.

[2] Umbillical cord collapse, which has an incidence of 0.1-0.6%, leads to large increases in perinatal mortality even today [of Obstetricians and Gynaecologists, 2014].

cognitive performance earlier in life, such as primary school performance or occupation. We consistently find evidence consistent with a significant improvement in cognitive functioning.

We additionally contribute to a very small literature within economics on the selective adoption of medical innovation [Glied and Lleras-Muney, 2008, Korda et al., 2011]. The shift of risky births to hospitals suggests substantial self-selection is taking place, which is remarkable given that only a small minority of expectant mothers would see a doctor before giving birth [cf. Bhalotra et al., 2017]. Using individual-level information on place of delivery, we investigate this issue further. We conclude that compliers are indeed negatively selected with regard to their school performance, sickness absence and survival chances – whereas they are positively selected with regard to parental SES.

Our results are insensitive to a number of robustness checks and alternative specifications. In order to assess the implications of selection bias from reduced mortality, we bound the estimated effects on human capital outcomes, and find substantial human capital gains even under the most extreme assumption of positively selected survivors. Results are also robust to adjustments for multiple hypothesis testing, to the inclusion of covariates, and to a wide range of bandwidth choices. There is moreover no evidence suggesting that manipulation of birth dates has taken place. We also study the implications of a 1939 change in abortion law, and conclude that it is inconsequential for our estimates.

The rest of the paper is organised as follows: The next section gives the institutional background on the transition from home births to institutional deliveries in Sweden. Section 3 describes the data and sample selection and presents the empirical strategy. Section 4 shows the main results and also investigates potential mechanisms. Section 5 provides an analysis of selection into treatment and Section 7 concludes.

2 Background

Compared to other developed countries, the transition from home births to institutional deliveries happened relatively early in Sweden. While the transition took place in the 1950's in countries like the United Kingdom and Norway, in Sweden most of it happened in the 1920–40 period. This means that the transition was early even in the comparison to the United States [Devitt, 1977]. At the turn of the 20th century, there had been only seven specialized maternity hospitals or separate maternity wards throughout the whole country. In total 216 beds were

available for childbirth and only 4-5% of all deliveries happened in institutions [Vallgårda, 1996]. Already in 1940, within just one generation, with 70% the majority of expectant mothers gave birth in a maternity institution [Royal Commission on Population Issues, 1945]. The rapid transition is illustrated in Figure 1.

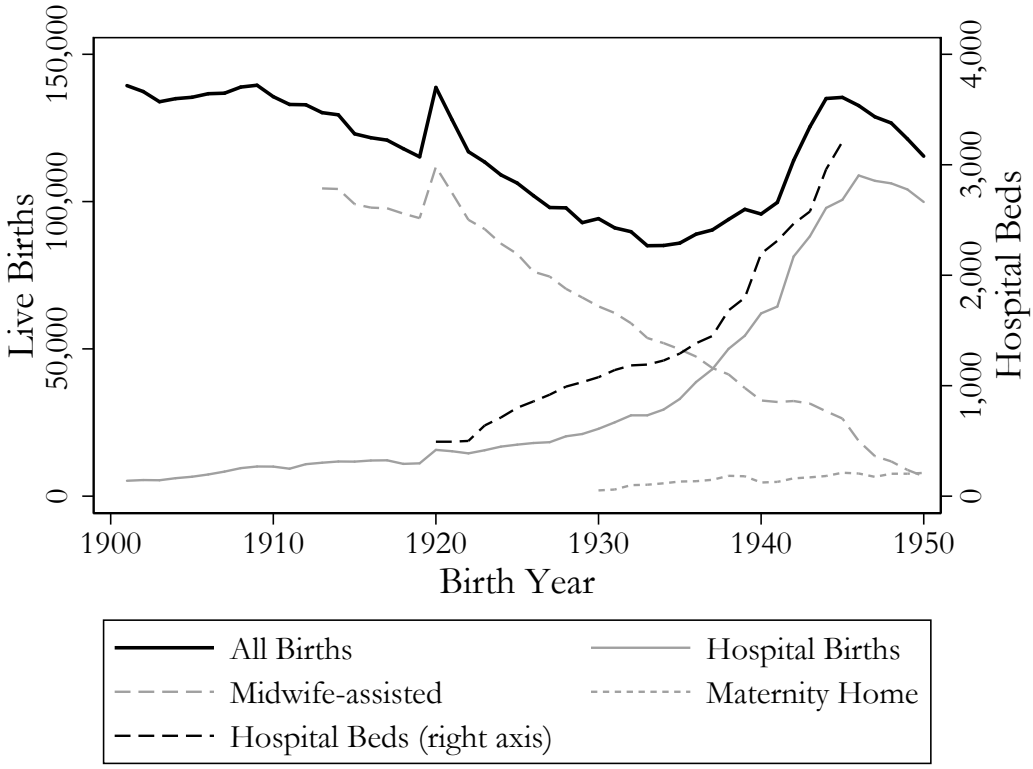


Figure 1: Live Births and Hospital Beds in Sweden, 1901–50.

Sources: National Board of Health (1900–1912), National Board of Health (1913–1950). Information on midwife-assisted births available from 1912, hospital beds from 1920 until 1945, births in birth centres from 1930.

The main alternatives to delivery in hospital were home births assisted by a midwife, and health centers like maternity homes, which were either independent or run by local authorities. As Figure 1 makes clear, home births remained the quantitatively most important alternative during the entire 1900–1950 period, whereas e.g maternity homes represented a small fringe, which never accounted for more than 7 per cent of all births or 13 per cent of all institutional births. Both alternatives had in common that they provided less resources in terms of medical staff; in less than 3% of midwife-assisted births a physician was eventually present [National Board of Health, 1937].

2.1 Institutional Context

Despite a rapid transformation of the health care sector, the institutional context remained relatively stable throughout the period we consider. Hospital care was mainly the responsibility of the 24 regional authorities (*landsting*) and independent cities (of which there were 6 in total). The national government was responsible for military hospitals (which sometimes delivered care also to civilians) and three academic hospitals.[3] By 1930, the national government's involvement in the funding of hospitals run by regional authorities and cities was limited to some specialisations [Royal Commission on Health Care, 1934].

In the period around 1920, the responsibility for home births shifted from municipalities to regional authorities. A 1919 law stipulated that the country be divided into 1,500 midwife districts, each one with their own salaried midwife. The funding relied on contributions from the national government, regional authorities, and municipalities [Royal Commission on Health Care, 1934].

Decisions regarding the hospital sector were taken by elected politicians in regional councils, or, in the case of independent cities, by directly elected city councils. Midwife districts were run by regional midwifery boards, which typically consisted of the chief medical officer of the region (or the city physician, in the case of independent cities), two representatives of the county administrative board and two representatives of the county council (or the city council) [Royal Commission on Population Issues, 1945].[4] All three levels of government raised taxes to cover their operating costs. The National Board of Health was a national oversight authority which monitored activities in all parts of the health care sector [Royal Commission on Health Care, 1934].

2.2 Policy Changes

Promotion of maternity wards. The transition from home births to institutional delivery was the result of a deliberate shift in national policies regarding childbirth. It occurred stepwise during the 1900-1950 period and was driven by a combination of social and medical concerns. The national Government was a key driving force behind the expansion of the maternity in-

[3] Additionally, some care institutions run by the state pension fund were typically focused on some specific conditions.

[4] Later in the period, the responsibilities of the midwifery boards were transferred to a general administrative body within the county councils.

stitutions and the parallel transition to institutional delivery. Large-scale emigration and declining fertility rates had given rise to concerns that the Swedish population would decline. Therefore, providing the best possible conditions for expectant mothers was considered an important priority [Vallgårda, 1996]. It was expected that giving birth in a hospital had significant health advantages compared to births at home with limited medical resources and under the cramped and crowded housing conditions of the era .

The 1901 Hospital law explicitly stated that only risky births requiring operations that could not be carried out in the mother's home should be referred to hospital [Royal Commission on Midwifery, 1942]. During the 1910s and 1920s, experts and policy makers began to debate how to improve care and support around childbirth. Views started to shift towards a new consensus that institutional deliveries were desirable. However it remained controversial which type of institutions would be best suited to provide good and equitable conditions for safe delivery. One side favored large and centralised institutions in city hospitals. They emphasised the possibility to build up substantial expertise, and synergies between specialisations like obstetrics and gynaecology. Other experts advocated small-scale birth centers, emphasising Sweden's low population density as a main obstacle for a strong centralization, as well as a higher risk of general infections in hospitals [Royal Commission on Health Care, 1934]. In the end, the proponents of large-scale solutions had their way, which is depicted in Figure 1.

The public debates surrounding the expansion of the hospital sector and the mode of delivery also paid considerable attention to expectant mothers' preferences and views. In the early phase this discussion centered more around the question of why women chose home births instead of hospital deliveries. In 1929 a Royal Commission investigated the reasons for an *underutilization* of hospital births were. It concluded that insufficient supply of maternity wards, the lack of knowledge and fees were the major obstacles [Socialdepartementet, 1929]. In 1941, an influential Royal Commission for population issues identified further factors driving the desire to give birth in hospital: The nearest midwife might either be so far away that expensive travel fees accrued, or she might already be working in the hospital, thus reducing her availability for home births. The commission further identified the possibility to remain in a safe and peaceful environment after birth, and the possibility to get nitrous oxide, which was not an option in home births, as important factors [Royal Commission on Population Issues, 1945].

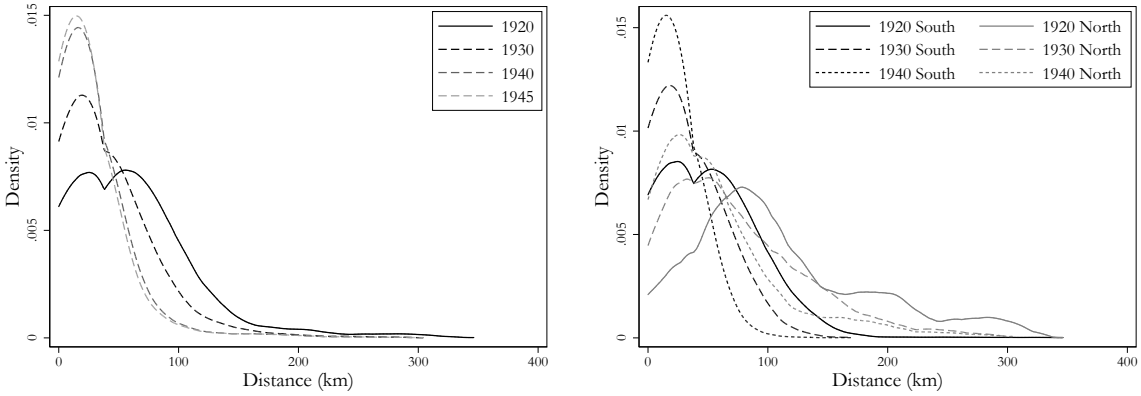
The 1928 Hospital law stipulates that regional authorities and independent cities now *should* run maternity institutions, unless there were independent institutions that already catered to

the demand [Royal Commission on Health Care, 1934]. It represents an important shift compared to the 1901 Hospital law and directly addressed the shortage in supply which was identified as major obstacle. The law stopped short of an obligation, and yet it mainly codified a change that was already under way: a number of county councils had decided to open maternity wards already before the law came into force (cf. Figure C.3). The next milestone in the promotion of institutional delivery was a 1937 law, introducing government grants for the building and operation of several different types of maternity institutions [Royal Commission on Government Grants, 1948].

Implementation. County councils investigated the needs for maternal healthcare and proposed locations to establish maternity wards. Great importance was given to find locations that could serve as many parishes as possible and reduce the inconveniences of long traveling distances. The goal was to achieve a distribution of places that would limit the travelling distances within the range of 40 km from each dwelling. In cases that a maternity ward already existed, but had reached its maximum capacity, expansions were proposed. New hospital openings or expansions of maternity wards were a process that also involved the local communities and municipal authorities [Royal Commission on Population Issues, 1945]. Further details regarding the decision process may be found in Appendix Section B.1.

Parallel to the expansion of maternity wards in hospitals, there was a large expansion in the supply of health centers. Despite the fact that they never became an important childbirth option in quantitative terms they rapidly spread through the country mainly in rural areas. In 1943, there were 84 birth institutions of this type across the country [Royal Commission on Population Issues, 1946].

The 1920s and 1930s brought rapid change throughout the country on the availability of maternal care. In Figure 2, estimates of the distance to the nearest maternity ward illustrate the consequences of the expansion of the maternity ward sector. In the early years, distances of 50–100 kilometres to the nearest maternity ward were completely normal. At the end of the period, such distances were rather exceptional. In Figure 2b, we show that the northern and southern parts of the country were following the same trend but changes were less marked in the North. In the 5 northern counties long distances remained normal for a longer period.



(a) Entire Country **(b)** North vs. South

Figure 2: Distribution of Distances to Nearest Maternity Ward, 1920–45.

Note: Own calculations based on parish population sizes according to the 1930 census [Statistics Sweden, 1935a] and haversine distance between parish centroid and nearest active maternity ward. Estimates are weighted by the 1930 population of each parish. The North is defined as the 5 northmost counties.

Expectant mothers were, however, willing to travel long distances to give birth in hospital. Based on the universe of 1930 births, one Royal Commission found that 55% of mothers giving birth in hospital were resident in the hospital location, 15% were from a neighbouring parish, and 30% came from farther away. Excluding the six maternity hospitals in the bigger cities, which naturally had a very high proportion of mothers from the city itself, the tendency to travel far to give birth becomes more pronounced. In this population giving birth in maternity wards in general hospitals, only 36% came from the same location; 16% from a neighbouring area, and 49% from farther away. This was possible thanks to a fully developed system for transportation of patients, which had been established already in the 1920s – including ambulances, rescue vehicles, and even aeroplanes [Royal Commission on Health Care, 1934].

Monetary benefits. As a parallel development, the national government expanded the financial support to families around childbirth. This trend dates back to at least 1913, when state support to sickness funds providing maternity benefits was introduced. Accordingly, a sickness fund offering such benefits – either in cash or in kind – above a minimum level, would enjoy a state subsidy covering around two-thirds of the costs. These regulations remained in place for the next two decades, and only benefited the small minority of women who were members of a sickness fund [Royal Commission on Social Insurance, 1954].

A reform in 1931 introduced more generous state support within a dual system: the sickness funds were obliged to provide maternity benefits, and these benefits were doubled. For non-members, a means-tested cash benefit was introduced, which was calibrated to correspond to the costs of a delivery in hospital. These new regulations were fully in force by 1934, at which point around 60 per cent of all mothers enjoyed benefits in some form. However, the benefits fell short of the effective charges in most hospitals, by substantial amounts: in the mid-1930s, the typical charge was SEK 3 per diem, and a delivery fee was charged on top. The public transfer amounted to SEK 1 per diem. Hence, the net cost for a typical delivery would be SEK 25. In addition, ambulance fees of SEK 4-5 per 10 kilometres would be charged, if applicable. The resulting sum is a substantial cost compared to e.g. a male industrial worker's monthly earnings, which were SEK 230 on average at the time [Royal Commission on Health Care, 1934, Statistics Sweden, 1935b].

The 1931 reform had a successor in 1938, which made all delivery care essentially free of charge and substantially increased the benefits in both systems, as well as the upper earnings limit in the means tested system [Royal Commission on Social Insurance, 1954].[5] The law stipulated that any maternity institution meeting certain requirements would be eligible for a subsidy of SEK 2 per diem, provided they charged at most SEK 1 per diem from their patients. The increase in maternity benefits were calibrated to cover the remaining fees [Swedish Government, 1937].

Abortion law. Throughout the period we consider, abortion remained illegal in Sweden in all but a few exceptional cases. The abortion law was liberalised in 1939 and in 1946; however, the number of legal abortions increased from 200 per year to 600 during the 1930s, and thus remained negligible in relation to illegal abortions (estimated at 10,000-20,000 per year) and total births, which fluctuated around 100,000. Further details and sources are provided in Appendix B.3.

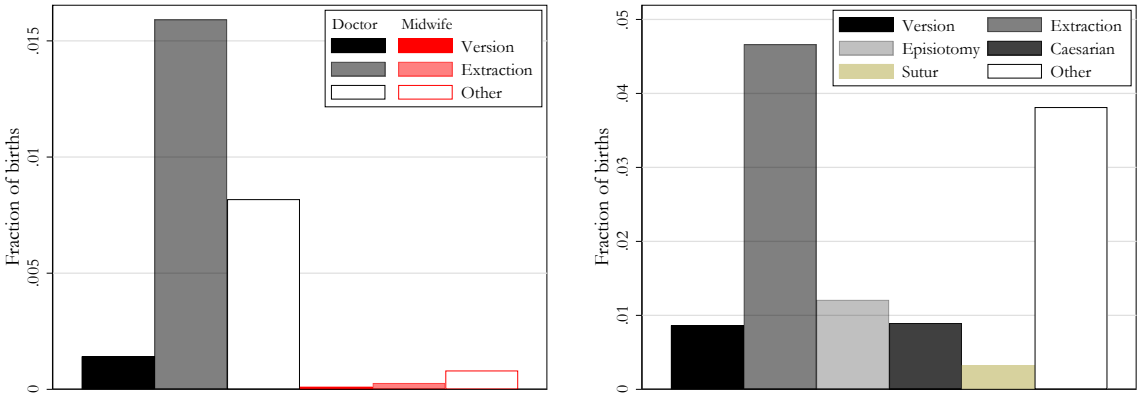
2.3 Quality of Maternal Care

Already by 1900 the majority of childbirths in Sweden were attended by licensed midwifes. Those midwifes were well-trained and experienced healthcare professionals that were evalu-

[5] The implementation of the 1931 reform was staggered and thus did not give rise to any notable discontinuities in eligibility. In contrast, the 1938 reform abolished charges throughout the country effective 1 January 1938 – leading to a sharp discontinuity in the costs associated with childbirth and hospital delivery.

ated and retrained on annual basis by the relevant medical authorities. Midwife-led deliveries have been linked to positive improvements for the maternal and infant health in this context [Högberg, 2004, Lazuka, 2018]. Despite their solid training and the high quality of services they offered for the uncomplicated births, midwives lacked the ability to apply advanced techniques and procedures that were necessary during childbirth complications. Starting from 1919 midwives were also discouraged from using obstetrical instruments (e.g forceps). Their use was intended to be exclusively by physicians [Vallgårda, 1996].

The main alternative to home births assisted by a midwife were births in larger hospital. Following the strong obstetric tradition in Sweden, Swedish hospitals of the era offered a high standard of neonatal care. Those healthcare institutions could be either maternity hospitals (*barnbördshus*) or maternity units (*förlossningsavdelning*) at general hospitals (*lasarett*). All of those institutions were offering specialized maternity services [Royal Commission on Population Issues, 1946]. In Appendix Table C.1 we present a comparison of Swedish hospitals of the era with contemporary hospitals that offer maternity services in low- and middle-income settings. Figure 3 provides a comparison of the prevalence of various procedures in home deliveries and hospital deliveries, based on the universe of births during the 1928–38 period. In home deliveries (Figure 3a), procedures would be applied in less than 5 per cent of cases; in almost all cases, this would entail calling a physician. The by far most common procedure would be a forceps delivery, which happened in 1.5 per cent of cases.



(a) Home Births **(b)** Hospital Births

Figure 3: Proportion of Deliveries Carried out with Procedures.

Note: Own calculations based on 586,317 home births and 118,253 hospital births during the 1928-38 period. Sources: National Board of Health [1937] and various hospital yearbooks.

For hospital births, a wider range of procedures would be available. Naturally, the hospitals would also carry out versions and extractions (manual or with forceps), as in home deliveries. These procedures were also much more common in hospital births, which probably reflects a combination of selection into hospital delivery, and the limited availability of physicians in home births. Additional procedures only available in hospitals include episiotomy and Cæsarians, and a wide range of other procedures captured in the "other" category – including blood transfusions, intentional membrane rupture, venesections, etc.

Hospitals offered advantages beyond the advanced medical interventions in case of complications during labor. The health and the feeding of low-birth weight infants were closely monitored.[6] In case of need, infants were transferred to incubators to regulate the temperature they were exposed to and to protect them from possible infections. The health of the mother was also monitored and she had the opportunity to recover from the delivery and rest away from the possibly crowded conditions of her home. The hospital further gave the opportunity for knowledge diffusion towards the mothers regarding aspects of proper infant care. Given the low breastfeeding rates at that time, the long length of stay was used in order to promote breastfeeding among the mothers [Royal Commission on Population Issues, 1945].

Beside hospitals, small-scale health centers offered facility-based childbirths. These smaller institutions also offered constant supervision after birth by a midwife, a safe environment and timely access to a doctor. However, they lacked the specialized personnel, the technology and the operational capabilities that a hospital had. Quantitatively, health centers were always of secondary importance. Close to their peak in the mid-1940s they accounted for around 12% of deliveries in a year [National Board of Health, 1948]. Recent findings show that those institutions nevertheless were pretty successful in providing quality care and their expansions have been linked with neonatal mortality reductions [Lazuka, 2020]. Historically, health centers were an intermediate step towards hospitalization of close to all births. They were soon considered obsolete and closed (See B.2 at Appendix for more details about health centers).

[6]The monitoring cut-off for LBW in this historical context was set at 2.700 grams.

3 Data and Empirical Strategy

3.1 Individual-Level Data

The base study population for our main analysis is drawn from various administrative sources and consists of individuals born between 1924 and 1946.

3.1.1 Main Outcome

Following prior register-based research on dementia risks Mosing et al. [2018], Seblova et al. [2021], dementia diagnoses are identified in the National Inpatient Register (NPR) and the Cause of Death Register (CDR) using International Classification of Diseases codes.[7] Individuals are followed from 1991 to the end of 2016. An individual is classified as having a dementia diagnosis if main or any secondary diagnosis for hospitalisation or death include a dementia diagnosis. As proxy for onset timing of dementia we use the earliest date of hospitalisation or date of death.[8]

Using health administrative data to identify dementia cases allows the analysis of large samples needed when applying a quasi-experimental approach. The downside is moderate sensitivity and with detecting 70% of all dementia cases for the cohorts in this study Rizzuto et al. [2018], Seblova et al. [2021]. The true timing of dementia onset likely occurs prior to when it is measured by hospitalisation or death. The outcome measure likely captures more severe cases of dementia.

Descriptive statistics for the main outcome variable and some background variables are provided in Table 3.

[7] Vversion 10 codes: F00.0, F00.1, F00.2, F00.9, F01.0, F01.1, F01.2, F01.3, F01.8, F01.9, F02.0, F02.3, F03.9, G30.1, G30.8, G30.9, G31.1, and G31.8A; version 9 codes: 290, 294B, and 331A

[8] Dementia cases that appear before the age of 65 are not considered in the analysis.

Table 1: Descriptive Statistics

	(1) Base Sample	(2) Other	(3) 3 Largest Cities	(4) Δ = (2) -(1)
DEMENTIA OUTCOMES				
Dementia Diagnosis (1985–2016) (%)	5.57	4.29	3.82	1.28
Age First Diagnosis	77.49	76.01	75.56	1.49
BACKGROUND				
Year of Birth	1937.14	1938.82	1939.43	-1.68
Male (%)	49.09	51.04	51.41	-1.95
Backgrond Farmer (%)	24.23	21.36	2.44	2.87
Backgrond High SES (%)	10.81	11.61	16.63	-0.79
Backgrond High Educ. (%)	3.45	4.82	15.86	-1.37
Surname Low SES (%)	55.57	50.58	40.04	4.98
N	86,675	1,277,066	254,948	1,363,741

Notes: Column 1 shows mean values for the analysis sample with max. 2-year window around a ward opening or extension. Column 2 gives the mean for the remaining country, excluding the three largest cities Stockholm, Gothenburg and Malmö, and Column 3 showing the mean for the three largest cities. Δ = shows the difference in mean values between Column 2 and 1.

3.1.2 Early-Life School Performance

In order to analyse potential mechanisms, we also use a sample of school grades during primary school (ages 7–12) for a representative subset of parishes. Descriptives of this dataset are provided in Table 2. A more detailed description of the dataset is provided in Bhalotra et al. [2021].

Table 2: Descriptive Statistics: School Data Sample

	Mean	Std.Dev.	Min	Max	Obs
MAIN OUTCOMES					
GPA (SD)	0.015	0.79	-2.85	3.12	6,359
Top GPA	0.187	0.39	0.00	1.00	6,359
Math score (SD)	0.005	0.91	-3.02	3.26	6,348
Read and speak score (SD)	0.026	0.89	-2.96	3.81	6,351
Writing score (SD)	0.043	0.92	-3.04	3.21	5,484
Sports score (SD)	-0.002	0.80	-4.02	3.38	6,081
Religion score (SD)	0.019	0.88	-3.45	3.27	6,342
Fraction sickness absence days	0.044	0.06	0.00	0.68	6,364
TREATMENT					
Hospital Birth	0.283	0.45	0	1	6,359
BACKGROUND VARIABLES					
Year of Birth	1931.5	2.8	1924	1940	6,359
School Grade	3.0	1.7	1	6	6,359
Academic Year	1934.2	119.1	0	1951	6,359
Length of school year (days)	207.3	10.3	82	214	6,359

Notes: School grades have been standardised within each individual subject and school year using a normal distribution and a sample representative for the entire population. In the population, the mean score in each subject equals 0 and the standard deviation equals 1. A description of the dataset and its sources is provided in Bhalotra et al. [2021].
Source: Own calculations.

3.1.3 Additional Variables

The 1950 census includes information on registered parish of birth, birth date, and sex. Information on mortality is taken from the Swedish Death Index [cf. Bhalotra et al., 2017]. The data set stems from official church books and population registers and covers the near-complete number of deaths in the population occurring between 1901–2017, including information on the date of death. Together these two sources represent an almost complete enumeration of all individuals who were born in Sweden during the relevant time period: only individuals emigrating between birth and 1950 would be excluded.[9] Our third source is the 1970 population and housing census. The 1970 Census data covers information on individual labor market status, occupation, income, and education. Information on living conditions and individual characteristics are based on self-enumeration and refer to the first week of October 1970 when

[9] During the period under consideration (1926–50) emigration was at the lowest level recorded between the 1860s and today: on average, less than 5,000 individuals emigrated each year during this period – which corresponds to less than 0.07% of the total population [Statistics Sweden, 1939, 1944, 1955].

the Census took place. With respect to labor force participation, persons are classified as economically active if they reported themselves as gainfully employed.[10]

Income statistics stem from official tax returns and are considered as highly accurate.[11] With information from 1970 this measure gives earnings for our cohorts born 1925–1946 at ages 25–45. We use the combined income from employment (*inkomst av tjänst*), self-employment (*inkomst av rörelse*) and agriculture (*inkomst av jordbruk*) as a measure of annual labor earnings, and CPI adjust incomes to SEK in 2014.[12]

The "treatment" we consider here is being born in hospital. Therefore, we collected information on whether individuals were born in hospital for all subjects included in the analysis sample ($N = 184,176$). The source of this information are the birth registers kept at every maternity ward [Swedish Tax Agency, 1989].

Table 3 presents summary statistics for some outcome variables, the main treatment variable, and some socio-demographics. Our analysis sample consists of individuals born between 1924–46 in a catchment area of an expanding hospital, within 48 months of the expansion. For the sake of comparison, we also present the corresponding descriptives for the entire population born between 1924–46. Clearly, the analysis sample is representative for the country as a whole.

[10] Workers within the family (paid and unpaid) and persons who were temporarily on leave (including parental leave) were also regarded as economically active in case their absence lasted less than four months.

[11] In general all individuals aged 16 or older are liable of submitting a tax declaration. If individual annual income or aggregated annual income in the case of married falls below 2,350 SEK, individuals were exempted from mandatory tax declaration leading to left censoring of the income distribution. With an annual income of $\sim$ 2,080 US$ (CPI adjusted for 2015) the threshold is however extremely low.

[12] Our choice of the income variable follows Edin and Fredriksson [2000]. The income measure in 1970 is not fully consistent with the current standard labor earnings measure (*arbetsinkomst*) used by Statistics Sweden. We do not have information on sick pay benefits which only became taxable in 1974 and which should be included in income from employment. We also lack information on pensions which should be subtracted. Given that pensions are unlikely a major source of income in 1970 for cohorts born after 1925 and sickness benefits are only a minor part of the income, we conclude that the income measure is a very reasonable approximation of annual labor earnings.

Table 3: Descriptive Statistics

	Analysis Sample			Entire Population		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
MAIN OUTCOMES						
Labor Earnings (1970)	20,381	17,856	126,843	20,540	136,898	2,090,492
Secondary School	0.20	0.40	123,759	0.25	0.43	2,014,132
Years of Education	8.74	2.36	123,749	9.02	2.43	2,013,896
Neonatal Death (Age $<$ 1 Month)	0.03	0.16	141,157	0.02	0.15	2,274,327
Infant Death (Age $<$ 1 Year)	0.05	0.21	141,157	0.04	0.21	2,274,327
Child Death (Age $<$ 5 Years)	0.06	0.23	141,157	0.06	0.23	2,274,327
Death before Age 50	0.11	0.31	141,157	0.11	0.31	2,274,327
Death before Age 70	0.24	0.43	141,157	0.23	0.42	2,274,327
Exit before Census 1970	0.08	0.27	141,157	0.08	0.28	2,274,327
TREATMENT						
Born in Hospital	0.35	0.48	141,157	0.43	0.49	2,274,327
SOCIO-DEMOGRAPHICS						
Patronymic Name	0.56	0.50	141,157	0.49	0.50	2,274,272
High SES Name	0.18	0.38	141,157	0.19	0.39	2,274,272
Male	0.51	0.50	141,157	0.51	0.50	2,274,327
Year of Birth	1934.75	5.36	141,157	1935.28	6.96	2,274,327

Notes: Descriptive statistics for cohorts born 1924–1946. Labor earnings are measured in 2014 SEK.
Source: 1950 population census [Statistics Sweden, 1952], 1970 population and housing census [Statistics Sweden, 1972], Swedish Death Index [Federation of Swedish Genealogical Societies, 2014]. Own calculations.

Finally, we use aggregate annual data on midwife and hospital deliveries generated from midwife diaries and hospital yearbooks. These data are available at the health district level ($N = 446$). A description of this dataset is provided in Appendix Section C.1. The main purpose of compiling the midwife data is to study selection into treatment: since the alternative to hospital delivery was a home delivery, the panel of midwife data will give aggregate statistics on the untreated population.

3.2 Intervention Data

Our intervention data are based on two components: a set of opening and extension dates, and catchment areas applying to each opening or extension. We collected information on supply-side expansions of maternity wards from various sources during the years 1926-45, including openings as well as expansion of established facilities.[13] All extensions and openings have been

[13]Sources include yearbooks from the National Board of Health (National Board of Health, 1913–1950), birth records from the individual hospitals, discontinuities in the parish birth rates in the 1950 census [Statistics Sweden, 1952], and yearbooks from hospitals.

validated using a combination of these sources. In a second step, we identified a catchment areas for each maternity ward which experienced an expansion. We define a catchment area to encompass all parishes that were relevant in the sense that they represented a non-negligible share of the total admissions in the maternity ward.

The identification of catchment areas is needed as individual administrative data sources recorded *place (parish) of delivery* as the place of birth. Thus, an expansion of a maternity ward will lead to more births recorded in the parish of the hospital. Following the expansion of the maternity ward, the original parish population is supplemented by an endogenously selected group of births from surrounding parishes within the catchment area. This selection would automatically confound an analysis carried out at the parish level. Basing the analysis on a wider catchment area before and after an expansion keeps the overall population constant.[14] In Appendix A we provide extensive information of how the catchment areas were defined. Figure 4 provides an example for the hospital in the city of Karlstad, in operation from 1937. It shows that expectant mothers would travel relatively far in order to give birth in hospital. For the hospital in Karlstad the most remote relevant parish is located approximately 50 kilometres from the hospital (Ransäters församling).

[14] Due to this 'mismeasurement' of parish of birth, it is also not possible to use floating catchment areas or other gravity-based approaches to assign access at the parish level [cf. Luo and Qi, 2009].

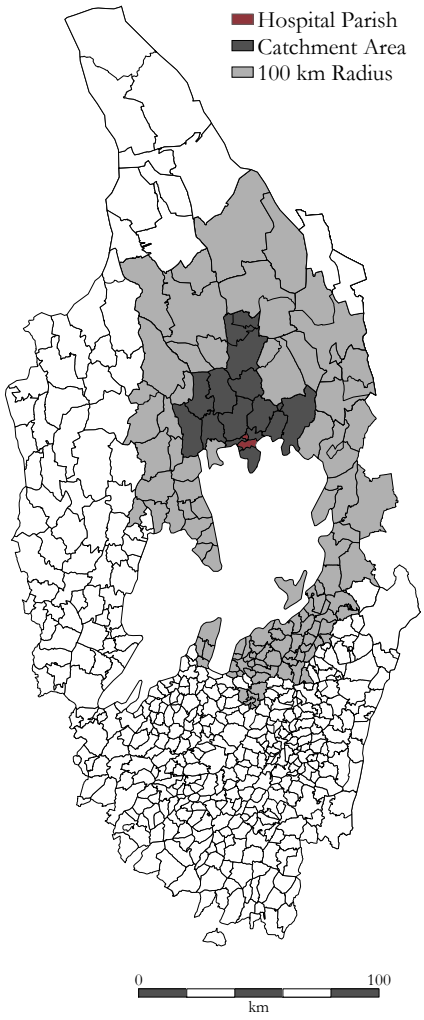


Figure 4: Hospital Catchment area in *Karlstad* 1937.

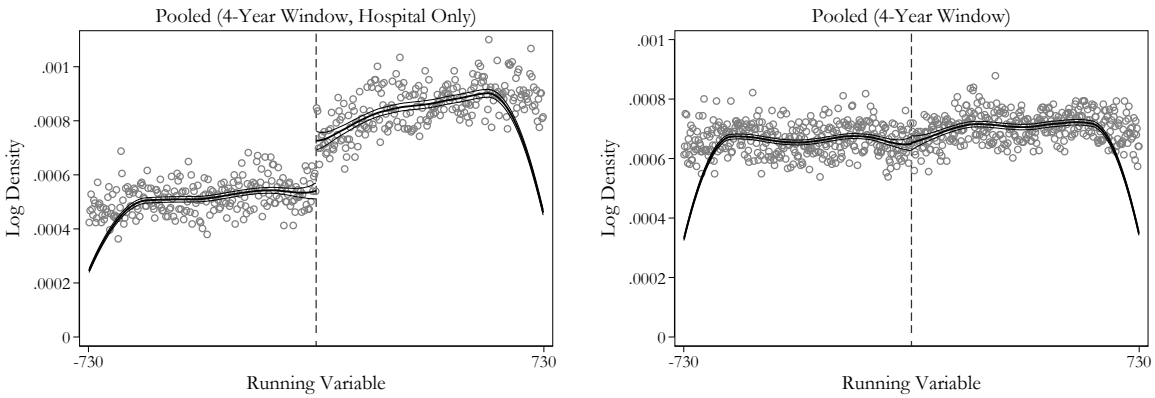
Note: Own calculations based on the 1950 population census [Statistics Sweden, 1952] and the Swedish Death Index [Federation of Swedish Genealogical Societies, 2014]; catchment area definitions provided in Appendix A.

We restrict our sample of openings and expansion to institutions for which a catchment area is well-defined, i.e. the overall population density is smooth around the opening of a maternity ward. Our final sample consists of 51 local interventions. The majority of those are maternity wards at hospitals. We consider 38 hospital openings or expansions which cover the 85% of our sample observations. Additionally, we include 13 openings of health centers. A description of the interventions can be found in Appendix Table C.3.

The validity of the proposed catchment areas is in fact testable. If it is too narrowly defined and not all relevant parishes are captured, it will exhibit a discontinuity in the birth rates at the cutoff.[15] In Figure 5 we show the outcome of such a test for the pooled sample of hospital

[15]The opposite case of a catchment area defined too widely should not cause problems for the empirical analysis other than reducing the precision of the analysis.

expansions: we conduct a McCrary test for births within the maternity ward parish [Figure 5a, cf. McCrary, 2008] and within the entire catchment area (Figure 5b). The running variable is the distance of the individual's birth date to the opening of the relevant maternity ward. There is a sharp increase in recorded birth rates in the parish with the maternity ward. This is due to the assignment of all births from surrounding parishes to this parish of delivery. There is no corresponding discontinuity for births is in the catchment area as a whole.



(a) Maternity Ward Parish **(b)** Catchment Area

Figure 5: McCrary Test of Discontinuity in Birth Rates.
Note: Own calculations based on the 1950 population census [Statistics Sweden, 1952] and the Swedish Death Index [Federation of Swedish Genealogical Societies, 2014]; catchment area definitions provided in Appendix A. Running variable is measured in days surrounding the maternity ward opening or expansion.

Figure 6 gives an overview of the quantitative importance of the interventions in terms of beds and hospital births. The relationship between additional births and additional beds is roughly linear with a slope of around 20, so that each additional bed gives rise to 20 additional births. This number is consistent with the reported average length of stay, which was very stable at 10 days, cf. National Board of Health (1913–1950). We provide some descriptive statistics on the distribution of length of stay in Appendix C.2, showing that there is very little variation around this mean.

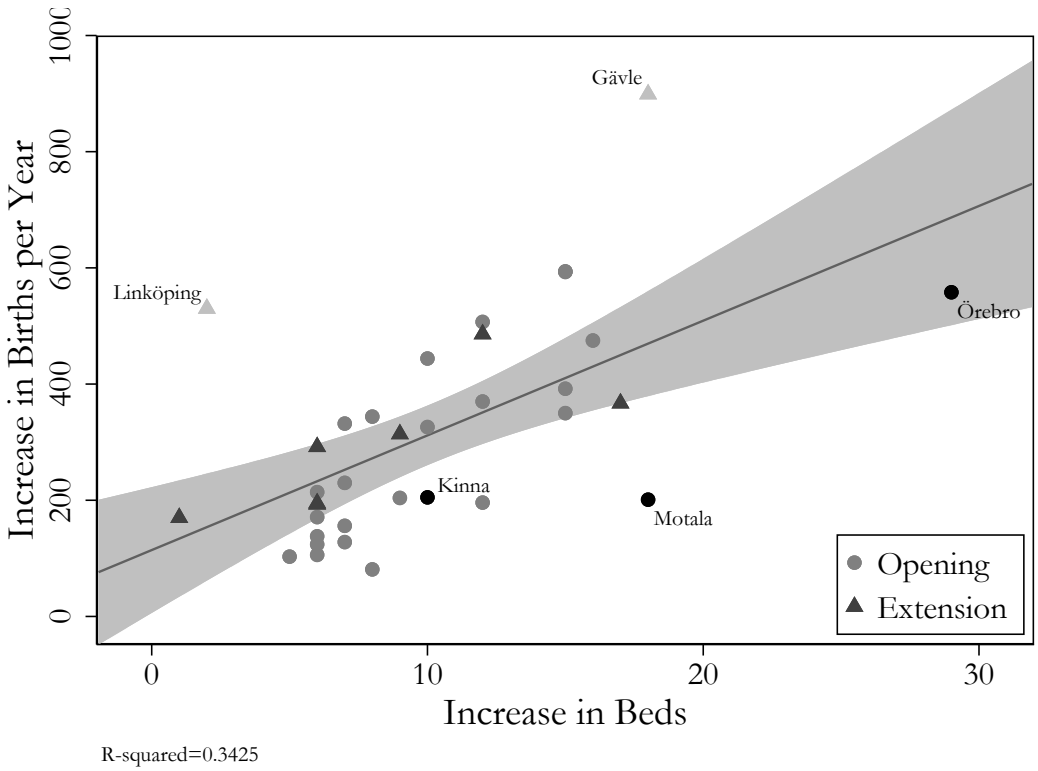


Figure 6: Examples of Discontinuities used in the Analysis.

Note: Source: National Board of Health (1913–1950). Expansions included in the figure are those for which information on the number of hospital beds is available in years surrounding the cutoff.

3.3 Method

Our aim is to estimate the average treatment effect on the treated; i.e., the impact of the two interventions on various short- and long-term outcomes. The identification of causal effects is complicated by the fact that most outcomes we consider, as well as the treatment (institutional delivery) exhibit strong time and cohort trends. For this reason, we rely on two different strategies for identification: Regression Discontinuity Design (*RDD*) and Difference-in-Discontinuities [Grembi et al., 2016, *DIDisc* henceforth]. A comparison of the two is useful not only to assess the robustness of results. If they deviate from each other, it suggests that there are relevant confounders conciding with the opening dates.

3.3.1 Fuzzy Regression Discontinuity

We employ a *fuzzy regression discontinuity design* [Imbens and Lemieux, 2008]. Since the expansion of supply only applied to children born after it had happened, we get a discontinuity in the access to the maternity ward at the opening date. Thus, the day of birth – which we observe

for every individual in the population – gives the running variable R_{ic} for the RDD design. We normalize the day of birth to zero around the exact opening date.

First Stage. Our first stage is thus the effect of the maternity ward opening on the propensity to be born in hospital, and our main specification is given by

$$H_i = \beta_0 + \beta_1 D_i + \gamma X_i + \sum_{m=2}^{12} \delta_m month + f(R_i) + \eta_i, \tag{1}$$

where H_i takes on the value one if individual i was born in hospital, and zero otherwise. $f(R_i)$ is a flexible polynomial in the distance to the opening R_i measured in days, D_i is an indicator such that $D_i = 0$ for individuals born before the opening date and 1 otherwise. X_i is a vector of family background and catchment-area level covariates which are not affected by our treatment and which are included in order to increase the precision of our estimates [Calonico et al., 2019]. We also control for month-of-birth fixed effects. Following the standard recommendation in the literature, we cluster standard errors at the level of the running variable [Lee and Card, 2008]. The coefficient β_1 measures a discontinuity in the the probability of a hospital birth for individuals born on either side of opening.

Second stage. In our analysis, we consider a number of outcomes Y_i which are potentially affected by being born in hospital. Thus, we estimate the structural equation

$$Y_i = \gamma_0 + \gamma_1 \hat{H}_i + \gamma_2' X_i + \sum_{m=2}^{12} \kappa_m month + f(R_i) + \epsilon_i, \tag{2}$$

where $\hat{H}_i$ is the predicted value of hospital birth from the first stage. Thus, γ_1 represents a local average treatment effect for families who are incited by the expansion to give birth in hospital.

3.3.2 Difference-in-Discontinuities

The critical assumption required for the RDD approach is that potential outcomes are continuous around the cutoff [Cattaneo et al., 2019]. The RDD can thus handle a number of confounders that would bias the estimates in a standard DID design (like e.g. diverging trends). However, a remaining threat to identification is other events that coincide with the opening or expansion of a ward. Since some openings happen on the first of January,[16] one such confounder would be school starting age. However, we control for month of birth in all regressions;

[16] Two openings out of 51 occur on January 1st, 6 occur in the month of January and 1 in December.

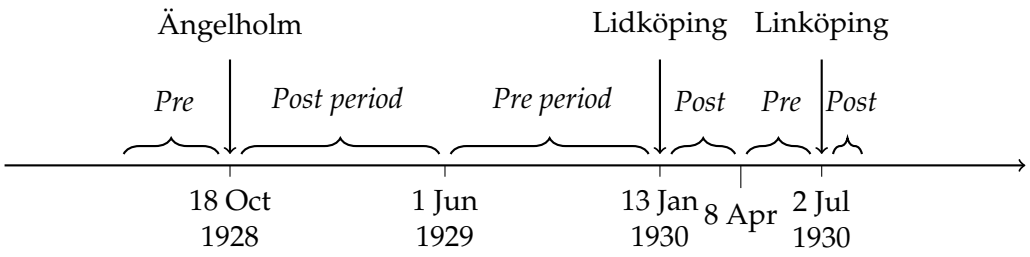


Figure 7: Difference-in-Discontinuities Strategy.

therefore this should not be a concern. But there may be other critical events that affect child outcomes and that happen to coincide with an opening date. In order to safeguard against such potential threats, we also use a DIDisc design, where the effect of an opening is identified from the difference in discontinuity in the treated hospital compared to all other hospitals included in the analysis.

Implementing a DIDisc design comes with some challenges in this context. Since event time is based on calendar time, a certain time period may belong to the post-treatment period for one opening, and to the pre-treatment period for another one, and yet the design requires that the trend in each period is normalised to equal zero at the cutoff. We solve this problem by splitting each intermediate period into two equal parts, and each part is normalised to equal zero at the nearest cutoff. A simple sketch of the idea is provided in Figure 7. It shows for three of the openings how we allocate the time between the openings to the pre-treatment or post-treatment periods of the individual hospitals.

Put more formally, there are K different treatment dates, which leads to $K+2$ "cutoff points" in the data, where c_0 is the earliest birth date in the sample and c_{K+1} is the latest birth date. Denote by the function $J(i) \in \{1, \ldots, K\}$ the cutoff applying for individual i, and denote by $\mathcal{I}_j$ the set of individuals who have a birth date such that $J(i) = j$. Our estimand is:

$$\begin{aligned} \tau_j = &\lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \in \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \in \mathcal{I}_j\right] \\ &- \left[\lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \notin \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \notin \mathcal{I}_j\right]\right] \end{aligned} \tag{3}$$

In Appendix section D.1 we provide the regression equation used to estimate the average treatment effect $\bar{\tau} = \sum w_j \tau_j$ where w_i is a weight representing the number of births contributing to each estimate. It is similar in form to specifications (1) and (2) but includes many additional parameters: in total we need $2K$ period effects for the periods before and after each opening; $2K$

common time trend parameters, $2K$ time trends allowing for diverging trends between treated and untreated hospitals, K opening fixed effects, K opening date dummies and one parameter representing the treatment effects. In additon, we include month-of-birth fixed effects and baseline covariates.

3.3.3 Cox proportional hazards model

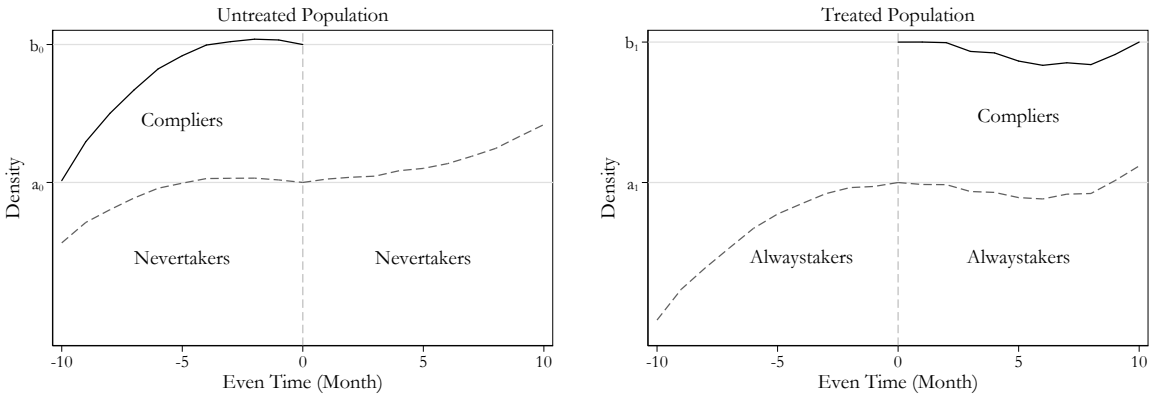
Finally, we apply a Cox proportional hazards model for both an RD and a DiD specification to evaluate the risk of dementia diagnosis within a survival model framework. For the DiD specification we rely on a flexible approach with stratified baseline hazards at the catchment area level g:

$$\lambda_g(t|X) = \lambda_{0,g}(t) \exp\left(\gamma_1 Z_{g,c} + \nu_c\right), \tag{4}$$

with ν_c date of birth fixed effects measured in quarters and $Z_{g,c}$ equal to one if a maternity ward is opened or extended in the specific catchment area.

3.3.4 Analysis of Selection

In order to understand and contextualize effects of the supply-side expansions, it is very helpful to understand who the compliers are. We thus conduct a separate analysis of the selection of individuals into hospital delivery by implementing a test proposed by Black et al. [2015] for heterogeneous selection within the LATE framework. The idea behind this test is that within subsamples defined by treatment status, the estimating equations will pick up selection effects. We provide a sketch of the underlying idea in Figure 8. Kowalski [2021] refers to this as *untreated outcome test*. A more general treatment can be found in Mogstad et al. [2018].



(a) Untreated $H = 0$ **(b)** Treated $H = 1$

Figure 8: Selection Analysis.

Consider first the subpopulation of individuals who were *not* born in hospital, displayed in Figure 8a. The subpopulation consists of compliers (C) before a supply-side expansion and never-takers (N). None of these individuals were treated. Hence, any change in outcomes or in control variables $W^0 \ \epsilon \ \{Y^0, X^0\}$ within the untreated group which coincides with the intervention Z must be a result of selection. Before a supply-side expansion the mean in the non-treated group is the weighted average between compliers and never-takers

$$\mathbb{E}(W^0|Z=0) = \frac{\mathbb{P}(C)}{\mathbb{P}(C)+\mathbb{P}(N))}\mathbb{E}(W^0|C) + \frac{\mathbb{P}(C)}{\mathbb{P}(C)+\mathbb{P}(N))}\mathbb{E}(W^0|N). \tag{5}$$

and after a supply-side expansion simply the mean of nevertakers $\mathbb{E}(W^0|Z=1) = \mathbb{E}(W^0|N)$. To assess how different compliers and never-takers are before treatment, we can quantify selection by

$$\Delta^0 = \mathbb{E}(W^0|C) - \mathbb{E}(W^0|N) = (-1)\frac{\mathbb{P}(C)+\mathbb{P}(N)}{\mathbb{P}(C)}\left[\underbrace{\mathbb{E}(W^0|Z=1) - \mathbb{E}(W^0|Z=0)}_{\theta_0}\right], \tag{6}$$

with θ_0 the reduced form estimand of the supply-side expansion. Δ^0 represents the mean difference between compliers and never-takers prior to treatment. We will use Δ^0 to investigate to which extent compliers are a selected group in terms of health risk and socio-economic status. Statistical inference is based on the delta method.

We also conduct the same analysis within the supbpopulation of treated individuals. However, exposing outcome variables would test the joint hypothesis of selection and treatment effect heterogeneity [cf. Kowalski, 2016]. Therefore, we restrict ourselves to results on background characteristics X^1 for the supbpopulation of treated to calculate the equivalent Δ^1 comparing compliers and always-taker.

4 Main Results

Descriptive evidence

The RD design is visualised by plotting the data before and after the specific cutoff points. As shown in Figure 9 there was approximately 15% increase in births at a ward after an opening/extension in the area. The scatter plot and local linear regression for the dementia diagnosis between 1991-2016 shows a substantial decrease around the ward opening date of about 1 percentage point.

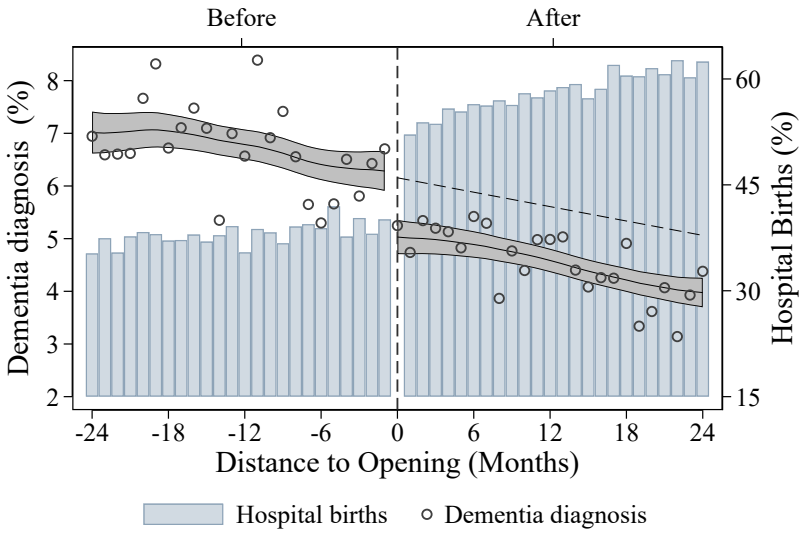


Figure 9: Regression Discontinuity Plot: The figure shows the share of hospital births and cumulative dementia incidence in relation to the distance in months from the opening of a new maternity ward or an extension of an existing facility. The left axis shows the share of individuals with at least one dementia diagnosis in the years 1991–2016. The right axis gives the share of individuals born in a maternity ward. Results are based on cohorts born 1930 – 1946 and include 42 maternity ward openings/extensions. The regression line is fitted using local linear regression with 12 month bandwidth and triangular kernel. *Source:* SIP. Own calculations.

4.1 First Stage

Table 4 presents our first stage results for both estimators. There is strong agreement between the RDD and the DIDisc specifications: estimates show a strong increase in hospital deliveries directly after an extension or opening of a maternity ward of about 16-17 percentage points. The estimates are significant at the 0.001 per cent level and the F statistics suggest we have a very strong instrument.

Table 4: First Stage: Born in Hospital

	Baseline	**RDD**	**DiDisc**
Born in Hospital	0.226	0.167***	0.161***
		(0.008)	(0.004)
F-Statistic		496.476	1336.547
N Hospitals		51	51
Observations		70,400	141,157
Bandwidth (Days)		365	730

Notes: Robust standard errors are clustered at the level of the running variable. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Regression controls for family SES proxied by surnames, month-of-birth fixed effects and hospital catchement-area level socioeconomic indicators and educational reforms. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51
Source: Census 1950, Swedish Death Register. Own calculations.

4.2 Main Results

Regarding the effect of ward openings or extensions on dementia risk, we estimate intent-to-treat effects (ITT) of the interventions on our dementia measures. The estimates refer to the direct effect of policy, and should be interpreted as the effect from increased availability of institutional delivery service on dementia risk. Panel B shows results from a linear probability model for the risk of ever receiving a dementia diagnosis between 1991–2016. The opening or extension of a maternity ward leads to a decrease by 0.89 percentage points (95% CI: [-1.63,-0.16], baseline risk prior intervention 5.87). This result indicates a decrease in the relative risk of 13.24% relative to baseline (0.89/6.72). Results for survival analysis using a proportional hazard model in Panel C corroborate the baseline findings, showing a decrease in the baseline hazard by a similar magnitude of 12.24% (HR=0.876; 95% CI: [0.774,0.990]).

Instrumental variable estimates in Panel B show very large (local) average treatment effect on compliers, those born in a maternity ward only due to the newly available facilities. The decrease in dementia risk for individuals born in a maternity ward as a consequence of an

extension or opening is 7.19 percentage points (95% CI: [-12.65,-1.73]). Column (2) shows our alternative specification in using DiDisc and a window of 24 months, catchment area FE and a date of birth trend. The estimates are quantitatively very similar. For the DiDisc the estimated ITT effect of an opening or extension of a maternity ward shows a reduction of 0.54 percentage points (95% CI: [-1.08,-0.00]) and a LATE of -4.31 (95% CI: [-8.99,0.37]). Intent-to-treat estimates for the survival model again correspond to the results from the LPM relative to the baseline risk, showing a risk reduction of 11.7% as a consequence of the public health intervention.

Table 5: Main Results

	(1) RD	(2) DiDisc
A: LPM (Ever Dementia Diagnosis)		
ITT (p.p.)	-0.89	-0.54
	[-1.63,-0.16]	[-1.08,-0.00]
LATE (p.p.)	-7.19	-4.31
	[-12.65,-1.73]	[-8.99,0.37]
Mean Dep. Var	6.72	6.72
B: Prop. Hazard Model (First Dementia Diagnosis)		
ITT	0.876	0.883
	[0.774,0.990]	[0.798,0.976]
N	43,512	86,675
Cluster	24	42
Cohort FE		✓
CA FE		✓
Window (bandwidth, months)	12	24

Notes: Panel A shows the effect of a maternity ward opening or extension on the share of children born in hospital. Panel B shows estimates from a linear probability model (LPM) on the risk of receiving at least one dementia diagnosis between 1991–2016. Panel C presents estimates from a survival model (proportional hazard) on the risk of dementia onset measured by first diagnosis between 1991–2016. Column 1 shows estimates from a regression discontinuity design (RD) with bandwidth 12 month and a rectangle kernel. Column 2 shows DiDisc estimates, including catchment area (CA) and date of birth fixed effects measured in quarters, and a window of 24 months. ITT refers to the effect of the ward opening/extension (intent-to-treat), LATE refers to the local average treatment effect of being born in a hospital in response to the ward opening as instrumental variable. Robust standard errors are clustered at the running variable for RD (month of birth, 24 cluster) and at the CA level for DiDisc with fixed effects (42 cluster). 95% confidence interval given in parenthesis using *t*-distribution with the number of clusters - estimated parameters as degree of freedom.

4.3 Mediation

4.3.1 School Performance

The fact that we see a substantial reduction in dementia risk raises the question whether the intervention improved the cognitive abilities of treated subjects in general. One potential channel for this is through the reduction of babies in poor cognitive condition after childbirth. The deprivation of oxygen at birth (perinatal asphyxia, PE) and the associated neurological function injuries (neonatal encephalopathy, NE) are diseases that can cause those malfunction at newborns.

The epidemiological literature documents how those types of brain damage have long-lasting effects towards cognitive development, health, and education. They have been associated with intellectual disabilities, developmental disorders and mental illnesses [Morales et al., 2011]. Children diagnosed with NE have been found to perform worse in school. Those effects remain large across a wide range of severity in the NE condition; it is estimated that 4 out of 10 children with moderate NE will score at least 1 S.D less in scholastic topics [Van Handel et al., 2007]. Recent findings link adverse neuro-developmental outcomes even for mild cases that, due to a low perceived risk, did not even qualify for specialized treatments like 'therapeutic hypothermia' [Conway et al., 2018]. The occurrence of those conditions is relatively limited in high-income countries. They are preventable and they can be avoided with the application of medical interventions during labor in case of complications like obstructed labor, eclampsia (seizures) and bleeding [Lawn et al., 2010]. Nevertheless, they still remain one of the leading causes of neonatal mortality. Globally, around 11 percent of deaths before the age of 5 are attributed to them [Liu et al., 2015]. The prevalence of perinatal asphyxia is very high in low-income settings; it is estimated to be around 16 percentage points [Workineh et al., 2020].

As evidence regarding this point, we now report effects on school performance. Table 6 presents results on school performance for a sub-sample of hospital expansions. We report results for GPA and for individual subjects during the first 6 years of schooling. School grades are in general represented as z scores and thus measured in standard deviations. See Bhalotra et al. [2021] for an extensive description of the dataset.

Table 6: Regression Results: School grades

	Baseline	RDD (non-parametric)		Difference-in-Discontinuities	
		Reduced Form	2SLS	Reduced Form	2SLS
GPA					
GPA (SD)	-0.006	0.135**	0.861**	0.122**	0.730**
		(0.059)	(0.403)	(0.052)	(0.333)
Bottom Quintile	0.190	-0.062**	-0.395**	-0.040*	-0.242*
		(0.027)	(0.189)	(0.024)	(0.147)
Subjects					
Math score (SD)	-0.012	0.085	0.538	0.096	0.570
		(0.067)	(0.425)	(0.059)	(0.352)
Read and speak score (SD)	0.005	0.206***	1.297***	0.169***	1.005***
		(0.065)	(0.480)	(0.057)	(0.385)
Writing score (SD)	0.011	0.129*	0.802*	0.130**	0.735**
		(0.069)	(0.444)	(0.062)	(0.367)
Sports score (SD)	-0.017	0.168***	1.036***	0.175***	1.049***
		(0.050)	(0.358)	(0.044)	(0.332)
Religion score (SD)	-0.025	0.131**	0.844**	0.095*	0.571*
		(0.063)	(0.427)	(0.054)	(0.325)
Absence					
Fraction sickness absence days	0.046	-0.008**	-0.048**	-0.008**	-0.047**
		(0.003)	(0.023)	(0.003)	(0.020)
Observations		6,359	6,359	6,359	6,359

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD and DiDisc sample covers a period of 4 years around the opening. The treatment variable represents hospital openings or expansions. All specifications include school year, school and term length fixed effects. Number of extensions/opening maternity wards included: 25.
Source: Census 1950, Swedish Death Register. Own calculations.

Starting with the grade point average, we find that a hospital delivery leads to a significant improvement by around 0.7 standard deviations. In particular, it decreases the probability of being in the bottom quintile by as much as 24 percentage points. These effects are driven by improvements in cognitive subjects like math, reading and speaking – but we also see substantial improvements in Religion and Sports, which are not included in the GPA. Notably, exposure to the treatment also reduced sickness absence quite substantially (4.7 percentage points). This suggests that delivery in hospital also had an effect on the children's health.

As mentioned above, one plausible mechanism which could explain the observed effects on adult outcomes, is that a hospital delivery prevents complications that potentially harms the child for life. If this is the relevant mechanism, we would expect to see hospital deliveries lift children out of the left tail of the distribution of cognitive abilities. In order to test this, we rely on a method proposed by Chernozhukov et al. [2013]. The effects in different parts of the distribution are captured through the implementation of several regressions. We estimate our main specification using as dependent variables dummies for $\Pr(Y_i \leq \gamma) \forall \gamma \in \Gamma$, where Γ

represents all possible realisations of the GPA. In Figure 10 we present those estimated effects together with their 90 and 95 percent confidence intervals. In order to avoid putting too much emphasis on outliers, we plot results showing Γ in quantiles instead of absolute values.

The results in Figure 10 are clearly consistent with hospital deliveries moving treated children out of the left tail: the estimated effect becomes statistically significant already at very low quantiles and from quantile 10 it remains flat for large parts of the distribution.

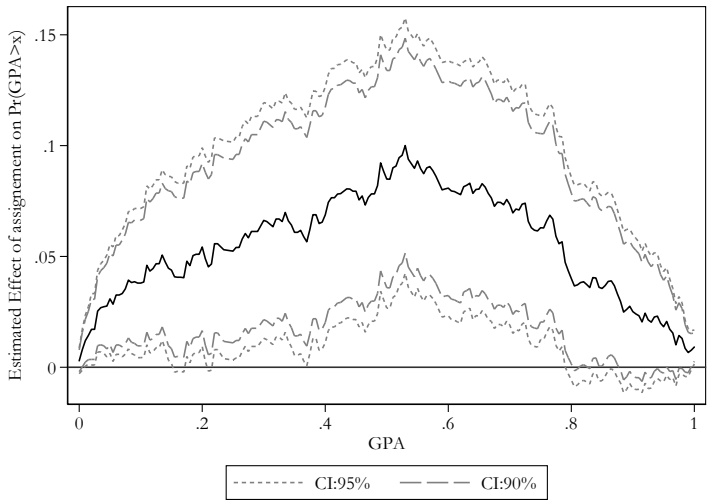


Figure 10: Distribution Regression Plot: Grade Point Average

Notes: Based on DIDisc using a bandwidth of 730 days.

An alternative mechanism could lie in the improvement of nutrition due to breastfeeding. It has been documented that breastfeeding is causally associated with cognitive and educational outcomes [Horta et al., 2015]. Breastfeeding rates were very low in Sweden in this period, however, all maternity wards kept records of breastfeeding and uniformly noted very high compliance. Despite the lack of data to formally test this hypothesis it remains a plausible explanation given the breastfeeding promotion and early initiation the maternity wards of our setting did. On the other hand, previous research has shown that a concurrent intervention that also promoted breastfeeding and nutrition had improvements in school performance concentrated in higher quantiles of the distribution [Bhalotra et al., 2021]; therefore, it is unlikely that it is the main mechanism behind the improvements in the bottom quintile that we observe here.

4.3.2 Occupational Sorting

There is thus clear evidence suggesting that the intervention had a considerable effect on cognitive performance in primary school. However, the estimation sample used in that part is an order of magnitude smaller than the main sample. In order to corroborate this finding, we also study the task content of occupations when the subjects are in early adulthood. This is based on a crosswalk of Swedish occupations from different censuses. The assignment of task content to occupations follows Autor et al. [2003].

Results are provided in Table 7. The IV estimates for "Quantitative reasoning" and "Direction, control and planning" are positive but statistically insignificant. On the other hand, the estimates for "Non-routine manual" and "Routine cognitive" are negative and significant, suggesting that treated children ended up in more demanding occupations than their untreated counterparts.

Table 7: Instrumental Variable Estimates (occupational sorting)

	Occupational task content (standardized)				
	Quantitative reasoning	Direction, control & planning	Non-routine manual	Routine manual	Routine cognitive
Born in Maternity Ward					
RDD	0.12	0.09	-0.17*	-0.12	-0.29***
	(0.12)	(0.10)	(0.09)	(0.11)	(0.09)
DiD	0.14	0.12	-0.21**	-0.02	-0.19*
	(0.10)	(0.11)	(0.09)	(0.08)	(0.10)

Notes: To assign task content to occupations, the Swedish occupations from different censuses are first cross-walked to ISCO-88. The assignment of task content to occupations then follows Autor et al. [2003]. All occupational task content measures are finally standardised with mean 0 and standard deviation 1. The table shows estimates for the second stage (LATE). Regression discontinuity design (RD) with bandwidth 12 month and a rectangle kernel. DiD estimates include CA and date of birth fixed effects measured in quarters, and a window of 24 months. Robust standard errors in parenthesis are clustered at the running variable for RD (month of birth, 24 clusters) and at the ward level for the DiD with fixed effects (42 clusters). Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

5 Selection into Treatment

5.1 Home Births

In order to assess whether more complicated births are shifted to hospitals, we investigate the effects of hospital openings and extensions on the composition of births by midwives in treated health districts. The health districts, of which there were 446 at the time, roughly match the catchment areas of our hospitals. We acquired data from midwife diaries for years 1928–1938.[17] We estimate effects of openings by a difference-in-differences (DiD) regression with openings and extensions as treatment variable. We control for year and health district FE.[18]

Results in Table 8 show that, as expected, the number of midwife-assisted births substantially decline by 24% after an opening. When we estimate effects on the proportion of births which needed medical procedures or had complications, we observe a reduction by 32 and 51 per cent, respectively. Similarly, the probability that the mother was ill or deceased two weeks after delivery dropped by 49 per cent (the vast majority of those were ill; maternal mortality rates were very low in Sweden). On the other hand, there is no change in the proportion of births that are twin births.

The recent literature on difference-in-differences designs has highlighted issues with two-way fixed effects estimators when there is staggered implementation, as in our case. We therefore also implement the dynamic estimator proposed by De Chaisemartin and d'Haultfoeuille [2020]. Results are presented in the bottom panel of Table 8. Results are in general quite similar.

However, the estimated reduction in home birth procedures did of course not reduce the overall intensity of care. In Appendix C.3 we present estimates showing that for the universe of births, the prevalence of procedures increased by 3.2 percentage points (61 per cent) following a hospital opening. This effect is driven by an increase in procedures that could not be provided in the mothers' homes (cf. Figure 3).

We differentiate the specific complications in Table 9. The estimates suggest that especially births with Cephalo-pelvic disproportion and Placenta praevia were shifted to hospitals: the prevalence of these two complications is reduced by 83 and 49 per cent, respectively. Both

[17] The diaries are mostly missing in archives after 1938. A description of the dataset is provided in Appendix Section C.1, cf. Boberg-Fazlic et al. [2021], Bhalotra et al. [2017].

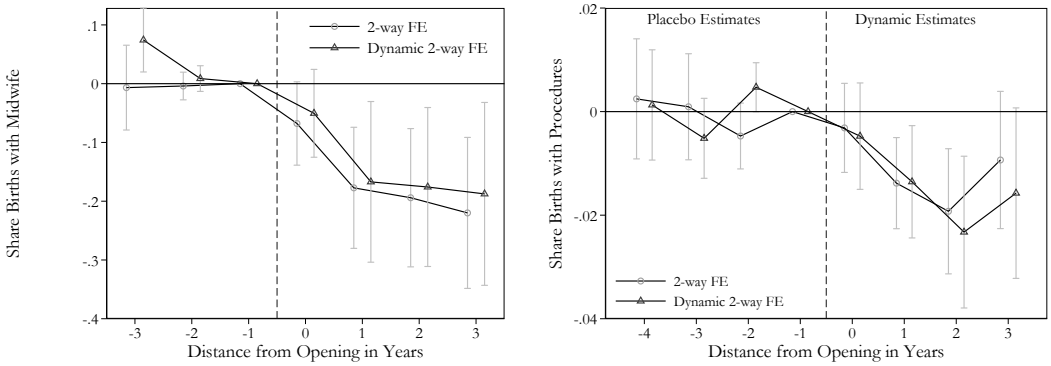
[18] In contrast to our indiviudal level data, the health district data on midwifes is on annual level. The openings and extension are evenly spread across the year. We therefore prefer a doughnut DiD specification, leaving out the year of intervention. Event-study figures and dynamic effect estimations show that this captures the actual treatment effect more accurately.

Table 8: Hospital Opening/Extension on Midwife Births

	Outcomes				
	Births with Midwife	Births with Procedures	Births with Complications	Mother ill / diseased	Twins
Hospital Opening	-0.284***	-0.013***	-0.003**	-0.006***	-0.001
	(0.054)	(0.004)	(0.001)	(0.002)	(0.001)
Mean Dep. Var	0.803	0.042	0.005	0.015	0.015
Relative Effect	-0.354	-0.298	-0.511	-0.420	-0.046
N	791,755	622,930	622,930	622,930	622,930
Health Districts	397	413	413	413	413
	Untreated outcome test (Selection)				
$\mathbb{E}(W^0\|N)$	0.513	0.029	0.003	0.011	0.012
Δ^0		0.055	0.011	0.027	0.003
Relative Risk		2.916	4.730	3.529	1.252
	Robust DiD Estimator (Dynamic 2-Way FE)				
Robust Effect	-0.232	-0.012	-0.002	-0.008	-0.000
SE Robust Effect	(0.061)	(0.004)	(0.001)	(0.007)	(0.002)

Notes: Table shows effects of the a hospital opening or extension in a given health district. Estimation are based on a standard difference-in-differences regression on aggregated data on health district level controlling for year and health district FE. Results cover all health districts 1928–1938. Robust standard errors clustered at health district level. Standard errors for robust dynamic 2-way fixed effect estimator are based on 1,000 bootstrap replications.
Source: Midwife Diaries. Own calculations.

complications were possible to detect early in pregnancy with medical assistance. Event study graphs in Figure 11 suggest there were no anticipation effects and a clear alignment of reduction in births assisted by midwifes and share of births assisted by midwifes with complications supports our interpretation that critical births were shifted to hospitals and maternity clinics.



(a) **(b)**

Figure 11: Event Study: (**a**) Births with Midwife; (**b**) Midwife Birth with Procedure

Notes: Figure shows coefficients from an event-study-type regression with lags and leads of the a hospital opening or extension in a given health district. Estimates are based on a difference-in-differences specification controlling for year and health district FE. Results cover all health districts 1928–1938. 95% CI based on robust standard errors clustered at health district level. Standard errors for robust dynamic 2-way fixed effect estimator are based on 1000 bootstrap replications.
Source: Midwife Diaries. Own calculations.

Table 9: Hospital Opening/Extension on Complications in Midwife Births

	Complications Preventable			Other
	Eclampsia	Cephalo-pelvic disproportion	Placenta praevia	Uterine rupture
Hospital Opening	-0.0008*	-0.0012***	-0.0007**	0.0000
	(0.0004)	(0.0004)	(0.0003)	(0.0000)
Mean Dep. Var	0.0018	0.0020	0.0012	0.0000
Relative Effect	-0.4214	-0.5766	-0.5717	0.6010
N	622,930	622,930	622,930	622,930
Health Districts	413	413	413	413
	Untreated outcome test (Selection)			
$\mathbb{E}(W^0\|N)$	0.0014	0.0010	0.0006	0.0001
Δ^0	0.0029	0.0044	0.0026	-0.0001
Relative Risk	3.081	5.443	5.111	-0.571
	Robust DiD Estimator (Dynamic 2-Way FE)			
Robust Effect (Dynamic 2-Way FE)	-0.0017	-0.0012	0.0002	0.0001
SE Robust Effect	(0.0008)	(0.001)	(0.0003)	(0.0001)

Notes: Table shows effects of the a hospital opening or extension in a given health district. Estimation are based on a standard difference-in-differences regression on aggregated data on health district level controlling for year and health district FE. Results cover all health districts 1928–1938. Robust standard errors clustered at health district level. Standard errors for robust dynamic 2-way fixed effect estimator are based on 1000 bootstrap replications.
Source: Midwife Diaries. Own calculations.

5.2 Individual-Level Analysis

The previous analysis highlights the avoidance of harmful complications at birth as one potential mechanism behind the improvements in later-life outcomes. If this mechanism is operating, it will lead to beneficial effects in particular if risky births are over-represented in the complier population. The analysis of midwife data clearly suggests this to be the case: after a hospital opening, the proportion of complicated home births drops significantly. We now investigate this possibility further using individual-level data and the method outlined in section 3.3.4. This approach is based on estimating the main specification within subsamples defined by the treatment status.

Results for the main analysis sample are presented in Table 10. In the two leftmost columns we estimate how compliers compare to never-takers; in the two rightmost columns we show the analogous comparison between compliers and always-takers. The latter columns include fewer variables since only background characteristics can be studied for the treated subpopulation.

In the first panel we study early life outcomes: neonatal mortality and school performance. All variables included suggest that the compliers are *negatively* selected: they exhibit higher

neonatal mortality (2 percentage points or 77 per cent), and they perform systematically worse in school, with a 0.7 SD lower GPA. They also have 4.6 percentage points higher sickness absence rates. The evidence thus clearly suggest that the compliers weren't a random subset of the untreated subpopulation, but instead that the opportunity to deliver in hospital was disproportionately taken up by those who needed it the most.

On the other hand, there is not much evidence suggesting that the compliers were negatively selected with regard to later-life outcomes such as educational attainment or earnings: the estimated differences are generally small and not statistically significant. And for the family background characteristics, the evidence is a bit mixed. The compliers are less likely than the never-takers to have patronymic surnames. They are also more likely to have a household head who is a white-collar worker or industrial worker, and less likely to have a household head working in agriculture. Hence, in summary, the compliers appear to be mainly negatively selected on child outcomes, and slightly positively selected on background characteristics.

When we instead compare the compliers to always-takers in the two rightmost columns, there is little evidence of systematic selection: the compliers appear to be comparable to the always-takers on most characteristics.

Table 10: Regression Results: Selection

	Mean	**Compliers vs. Never-Takers (NT)**		**Mean**	**Compliers vs. Always-Takers (AT)**	
	NT	RDD	DiDisc	**AT**	RDD	DiDisc
Mortality						
Neonatal Death (Age $<$ 1 Month)	0.026	0.018*	0.020**			
		(0.010)	(0.009)			
School Performance						
GPA (SD)	0.143	-0.728**	-0.717**			
		(0.372)	(0.336)			
Bottom Quintile	0.135	0.399**	0.290**			
		(0.176)	(0.144)			
Fraction sickness absence days	0.035	0.040*	0.046**			
		(0.021)	(0.019)			
Background Characteristics						
Patronymic Name	0.590	-0.023	-0.058**	0.506	0.012	0.014
		(0.031)	(0.028)		(0.023)	(0.019)
High SES Name	0.162	0.025	0.039*	0.202	-0.013	-0.013
		(0.022)	(0.020)		(0.018)	(0.015)
Twin	0.010	-0.010	-0.001	0.007	0.003	0.004
		(0.009)	(0.008)		(0.006)	(0.005)
Observations:		91,912			49,245	

Notes: Robust standard errors clustered at hospital level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. The treatment variable represents hospital openings or expansions.
Number of extensions/opening maternity wards included: 51. The bottom panel (*Family Employment Background*) uses household head employment in the 1950 census and thus requires survival of the child and one parent until 1950.
Source: Census 1950, Swedish Death Register. Own calculations.

6 Sensitivity Analysis

6.1 Robustness Analysis

We test the robustness of our results by comparing our baseline specification to alternative empirical models. For the RDD we first add control variables (gender and non-parametric trend in quarter of birth). Further we test robustness of using the fixed bandwidth of 12 month and automatically select the optimal bandwidth using cross validation. Results in the upper panel of Table 11 show that estimates are not altered for adding control variables. The optimal bandwidth is smaller and delivers slightly larger reductions, albeit not statistically different from our baseline.

For the DiDisc we add catchment area specific linear trends and a pre-post interacted linear trend. We additionally estimate a regression-in-discontinuity estimator which excludes treated individuals from the control group Fischer et al. [2021]. In a staggered difference-in-differences with multiple groups and multiple periods De Chaisemartin and d'Haultfoeuille [2020] and increasing number of units adopting the treatment of interest, treated units are eventually used as control units by construction. This violates the standard underlying assumption of a common trend on the untreated and can lead to biased estimates. Results in Table 11 show that the DiDisc estimates are robust to the inclusion of various trends and control group specification.

Finally, we test the robustness of our data-driven catchment areas by comparing estimates to specification with a larger radius around the maternity wards. We set the radius 60 km for southern parts and 120 km in the northern parts of the country. Our data-driven catchment area is efficient by using only relevant parishes, but if chosen too small, estimates can exhibit a bias. 60/120 kilometres is a conservative choice which exceeds the maximal travel distance found in historical governmental reports. The extended radius obviously reduces intent-to-treat parameters by adding parishes which were identified as irrelevant to the maternity ward and thus adding noise. IV estimates are slightly larger, indicating that the data-driven CAs are if anything conservative.

Table 11: Robustness Analysis

	N	First Stage	LPM (ITT)	LPM (IV)	PROP. HAZ. (ITT)
PANEL A (RDD)					
Baseline	43,512	12.42 [10.69,14.16]	-0.89 [-1.63,-0.16]	-7.19 [-12.65,-1.73]	0.88 [0.77,0.99]
+ control variables	43,512	11.35 [9.01,13.69]	-0.81 [-1.63,0.00]	-7.16 [-14.01,-0.32]	0.87 [0.77,0.98]
Optimal Bandwidth	21,433	12.14 [9.86,14.42]	-1.66 [-2.14,-1.18]	-13.65 [-17.98,-9.33]	0.79 [0.71,0.89]
Radius 60/120 km	137,362	4.27 [3.37,5.16]	-0.50 [-0.88,-0.12]	-11.73 [-19.59,-3.86]	0.91 [0.83,1.00]
PANEL B (DID)					
Baseline	86,675	12.51 [9.06,15.97]	-0.54 [-1.08,-0.00]	-4.31 [-8.99,0.37]	0.88 [0.80,0.98]
Linear Trends	86,675	12.83 [9.33,16.34]	-0.57 [-1.12,-0.01]	-4.41 [-9.12,0.30]	0.87 [0.79,0.96]
Linear Trends Int.	86,675	12.94 [9.46,16.43]	-0.61 [-1.19,-0.02]	-4.68 [-9.50,0.14]	0.87 [0.77,0.98]
Diff. in Disc.	86,675	14.09 [9.31,18.87]	-0.48 [-0.94,-0.03]	-3.44 [-6.95,0.07]	0.91 [0.83,1.00]
Radius 60/120 km	273,482	5.18 [3.82,6.54]	-0.42 [-0.77,-0.07]	-8.11 [-15.52,-0.71]	0.92 [0.85,0.98]

Notes: see table 5. Alternative specification to show robustness. For Panel A (RDD) a specification with controls for date of birth (in quarters) and gender is added and a specification estimates the optimal bandwidth using bandwidth selection according to Calonico et al. [2014] instead of using a fixed 12-month bandwidth. Panel B (DID) includes estimates including linear catchment area trends (with and without interaction with the treatment). The last specification estimates a regression-in-discontinuity design which restricts estimates in the control group to untreated. Both panels include an estimates based on a radius of 60/120 km are used instead of a data-driven CA. Compare appendix figure C.2 for illustration and sufficient radius to capture relevant births for maternity wards.

6.2 Specification Tests

Next we proceed to evaluate the identifying assumption and assessing the robustness of our results. The main assumption underlying the RDD design, is that potential confounders are continuous around the opening dates. The DIDisc specification is identified even if this continuity is not satisfied, provided that any discontinuities are common to all hospitals opening

at nearby dates. In Appendix Tables E.1 and E.2, we provide a balancing tests evaluating the plausibility of this assumption.

In Table E.1 we present balancing tests for aggregate characteristics of the hospital parish, for variables such as poverty rates and taxable incomes, which are measured annually. As the OLS estimates in the second column clearly demonstrate, the presence of a maternity ward is far from random: it correlates with higher incomes and property values, higher poverty rates, and longer compulsory schooling. However, when we estimate the "effect" of an opening or extension using either RDD or DID within a 4-year time window (which would be the closest equivalent to our DIDisc specification that we can get for these variables), all the correlations become small and lose their statistical significance. Table E.2 conducts similar tests for individual-level background characteristics, applying our two main specifications to these variables. Again, the estimates are small and insignificant throughout. In conclusion, our main specifications appear to deal with observable confounders quite successfully.

A related requirement for identification is that parents are unable to manipulate the birth date of their children. In the historical context we consider, the possibilities to influence the delivery date were very limited: Caesarian sections were extremely rare.[19] A systematic change in fertility (conception) rates could in principle be a confounder; however, the exact opening date would typically not be known 9 months in advance, and changes in the timing of conceptions would not have the precision required to induce a discontinuity around the cutoff. On the other hand, labour induction has a long history in obstetrics and could in theory cause manipulation of the birth date [Sanchez-Ramos, 2009]. Therefore, we conduct a test of manipulation proposed by Cattaneo et al. [2018]. Results are presented in Table F.1. For none of the bandwidths considered do we reject the hypothesis of no manipulation.

We have estimated large gains in survival chances and in socioeconomic outcomes for the treated children. This raises the question as to whether the estimates for socioeconomic outcomes are biased by selective mortality. The estimates by SES background in Table **??** do not hint at strongly selective survival, but there may well be unobserved background characteristics that affect survival chances. In Appendix Table F.9 we investigate this issue, applying Lee bounds to our main specification [Lee, 2009]. Under the assumption that deaths are negatively selected, the effects on schooling and earnings are inflated by 19–75 per cent, with the largest change noted for earnings. Under the opposite assumption of *positively* selected mortality, ef-

[19] The Cæsarian rate increased from 0.25% in the late 1920s to 0.87% in the late 1940s [Högberg, 1989]

fect sizes are reduced by 24-65 per cent; they remain statistically significant for the education variables. The results become weaker for earnings in this case, but the point estimate still corresponds to an increase in earnings by 2.8 per cent.

As mentioned in section 2.2 and in Appendix B.3, abortion law changed on 1 January 1939. Since several openings and expansions happened around that date, it is a potential confounder, despite the very small impact of the reform on abortion rates. In case this is a concern, it would affect the estimates for the hospital opening that is closest to the reform date.[20] In order to test the robustness of results to this reform, we ran a series of regressions, where we leave out one of the institutions in each iteration. Results from this exercise are presented in Appendix Figure F.2. Clearly, no single hospital drives the results and in particular, the abortion reform apparently does not affect our estimates. In Appendix Table F.8, we check whether there are any discontinuities around the abortion reform. Apart from a marginally significant first stage, there is no evidence suggesting that the reform had an impact on outcomes.

As a further robustness check, we test how sensitive results are to the exclusion of covariates. Our preferred specification includes controls for family SES (proxied by surnames), month-of-birth fixed effects, socioeconomic outcomes at the hospital catchement-area level and schooling reforms [cf. Fischer et al., 2020]. The month-of-birth fixed effects are desired in the specification given that openings sometimes happen near school starting age cutoffs; the other covariates mainly serve the purpose of increasing precision in the estimates. We report regression results for a specification without these covariates in Table F.2. The results are hardly affected at all. In Table F.4 we present RDD results with a covariate adjusted robust-bias corrected estimator [Calonico et al., 2017]; also these results are similar to our baseline estimates.

Since the DiDisc specification uses a bandwidth of 730 days we evaluate whether our results are sensitive to this specific choice. In Figure F.1 we report effect estimates for different bandwidths ranging from 90 to 730 days. Those estimates show that our main estimates for the DiDisc design are robust to the bandwidth choice. In Table F.3 we conduct a test for the RDD using different bandwidths ranging from six months to two years. Bandwidth choice is also relatively inconsequential for the RDD estimates.

[20] This holds by construction in our DIDisc design, cf. Figure 7.

7 Conclusion

We have presented evidence that increased access to institutional delivery service, through the openings and extensions of maternity wards, reduced the risk of late-onset dementia for the affected children. To establish causality we relied on sharp discontinuities in the availability of maternity wards which substantially increased the probability of giving birth in a hospital as compared to midwife-assisted home births in the geographical area surrounding the maternity ward. Our analysis suggests large and significant long-term effects. The effects are especially striking given the historical context and the arguably lower quality of medical care as compared to today's standards.

We moreover show that the policy interventions shifted births with complications from home-births assisted by a midwife to hospital wards. Complications at birth, such as deprivation of oxygen at birth (perinatal asphyxia) and associated disturbances in neurological function (neonatal encephalopathy), can have long-lasting consequences on cognitive development Van Handel et al. [2007], Morales et al. [2011]. This is further corroborated by our finding that the intervention had a significant effect on school performance and occupational sorting. Access to maternity wards further reduced the likelihood of receiving sickness pension. These factors represent important intermediate midlife outcomes predicting dementia risk Dekhtyar et al. [2016]. Another study with different study-design, focusing mainly on small-scale health centres with limited catchment area (< 5.5km), also found quantitatively similar results on midlife socio-economic outcomes Lazuka [2021].

Our results are consistent with recent findings showing that adverse birth outcomes and complications such as cerebral palsy substantially increase dementia risk Mahmoudi et al. [2022], Smith et al. [2021]. Avoiding severe complications can potentially avoid future dementia cases. The results are also compatible with the large literature showing the importance of very early life health on cognitive test scores, educational attainment, and income Black et al. [2007]. Very early life health markers have been shown to be associated with dementia risk. For Sweden Mosing et al. [2018] find evidence of increased risk of dementia from anthropometric measures for very early health such as very low birth weight and small head circumference (adjusted and unadjusted for gestational age) using dementia diagnosis from registers. We view this evidence as supportive of our interpretation of the present analysis: a reduction in

extreme events early in life and improvements in very early life can substantially alter late-life dementia risk. Public health policies can help foster this positive impact.

This study has several strengths. First, it utilises a unique data set on openings and extensions of maternity wards, which allow for causal inference regarding the effects of access to institutional delivery service. Second, we use individual-level administrative data ensuring a sufficiently large sample for inference in a quasi-experimental design, representative for a well-defined population. The rich administrative data further allows to investigate intermediate outcomes, allowing us to identify potential intermediate outcomes of relevance to the relationship between access to delivery service and dementia risk. Third, the study relies on multiple quasi-experimental designs and sensitivity specifications.

A shortcoming of our study is the use of inpatient and cause of death registers to identify dementia cases. This approach increases power, which facilitates the use of quasi-experimental designs to study dementia risk, and implies high specificity, but only moderate sensitivity. We likely also primarily capture severe dementia cases. This can be seen in line with our argument of preventing severe early-life outcomes which translate into severe dementia cases. But one should be cautious against strong extrapolation as effect sizes might be considerably smaller for less adverse early-life outcomes.

Further caution is warranted given the external validity as the risk reduction occurred in a historical context. Even so, Sweden in the 1930s had an infectious disease environment and infant mortality rate similar to that in many developing countries today Bhalotra et al. [2017], and despite progression in modern medicine considerable scope remains to improve preventive measures, procedures and supplies. Daysal et al. [2015] also show that home births can still constitute a potential risk in the case of adverse events in contemporary Netherlands.

We conclude that public health policies that facilitate access to institutional delivery services can causally reduce late-life dementia risk, probably by reducing extreme negative events at birth. These results provide first evidence of causal effects of early life circumstances on dementia risk, supporting the recommendation to target prevention in early life.

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Online Appendix: Not Intended for Publication

A Catchment Areas

In this section, we provide a more detailed overview of our definition of catchment areas. In our main specifications, we used catchment areas defined as

$$CA_{jt} \coloneqq j \cup \left\{ p \in \mathbf{P}_{-J_t} : d\left(p, j\right) \leq D_j \cap h_{ijt} \geq h_{ikt} \forall k \in \mathbf{J_t} \cap h_{ijt} > 0 \right\} \tag{7}$$

where CA_{jt} is the set of parishes belonging to the catchment area of hospital j in period t; $\mathbf{P}_{-J_t}$ is the set of parishes not containing an active hospital, $d\left(p, j\right)$ is the haversine distance between the centroid of parish p and the centroid of the hospital parish j, D_j is the maximum distance allowed,[21] and h_{ijt} is the proportion of births in hospital j coming from parish i in period t.[22] $\mathbf{J_t}$ is the set of parishes with an active hospital in period t. For each of the 50 expansions included in the analysis, we then fixed the catchment areas according to the situation in the two years following the discontinuity.

Put in words, this definition of a catchment area includes parishes that

1. Enclose the hospital, or,
2. Are important to the hospital in the sense that they
   (a) Are within distance D_j from the hospital ($d\left(p, j\right) \leq D_j$) and
   (b) Represent a larger share in hospital j's births in period t than in any other hospital ($h_{ijt} \geq h_{ikt} \forall j \in \mathbf{J_t}$) and
   (c) Contribute a positive number of births in period t ($h_{ijt} > 0$).

A.1 Measurement Error

For our analysis to deliver unbiased estimates, it is essential that the catchment areas are defined so that they are insensitive to the endogenous selection into hospital birth. Denote by H_{ijt} the number of births in location j in period t whose parents live in parish i. Likewise, denote by B_{it} the number of (hospital and non-hospital) births in period t whose parents reside in parish j. The corresponding birth numbers actually observed in the data are

$$\hat{B}_{it} = \begin{cases} B_{it} + \sum_{k \neq i} H_{kit} = \sum_k H_{kit} & \text{if } i \in \mathbf{J}_t \\ B_{it} - \sum_{j \in \mathbf{J}_t} H_{ijt} = H_{iit} & \text{if } i \notin \mathbf{J}_t \end{cases} \tag{8}$$

where $\mathbf{J}_t$ is the set of active hospital locations in period t. Starting with the first line of the equation which represents the measured number of births in a parish with an active hospital. The measure number of births is composed by the actual births to parents residing in parish i, plus the additional births coming from outside parishes ($\sum_{k \neq i} H_{kit}$). For parishes without a hospital, the measured number of births corresponds to the actual births to parents residing in the parish, less hospital births ($\sum_{j \in \mathbf{J}_t} H_{ijt}$). Now suppose we assign the set CA_j of parishes to

[21] As detailed below, we set $D_j = 60km$ in the 20 southern counties and $D_j = 120km$ in the five northern counties. These numbers correspond closely to the travel distances actually observed in the data.

[22] h_{ijt} is not observed in our data but was estimated according to parish of residence in 1946. Hence, we approximate h_{ijt} with $\hat{h}_{ijt} = \frac{\sum 1(p_{46}=i, p_b=j)}{\sum 1(p_b=j)}$ or in words: the number of individuals with 1946 parish of residence equal to i and parish of birth equal to j divided by the number of individuals with parish of birth equal to j.

hospital j. The recorded number of births in that catchment area will then be

$$\hat{B}_{CA_jt} = B_{CA_jt} + \sum_{k \notin CA_j} H_{kjt} - \sum_{i \in CA_j} \sum_{l \in \mathbf{J}_t \setminus j} H_{ilt} \tag{9}$$

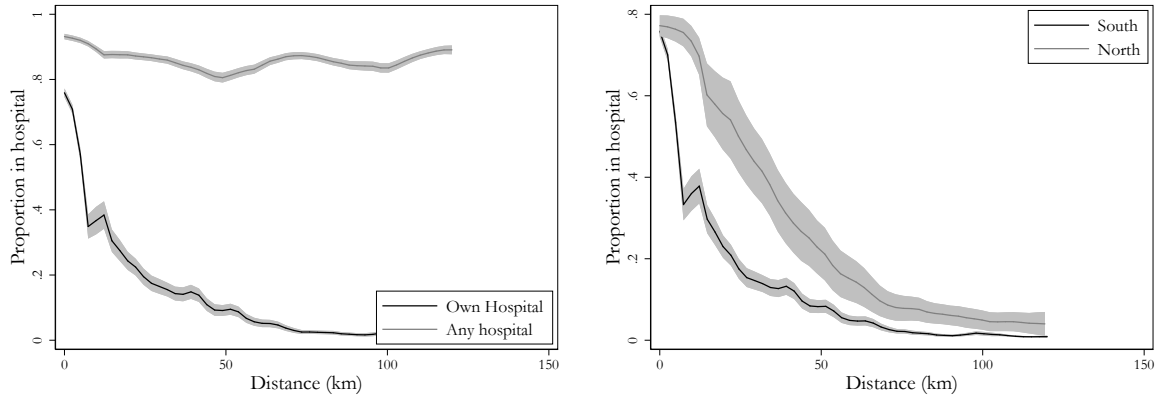
Hence, the mismeasurement of births consists of two components: individuals from parishes outside the catchment area who are born in hospital j, and individuals from parishes inside the catchment area who are born in an outside hospital. Since both terms potentially bias estimates, it is our goal to minimse their relative importance at the hospital level. We therefore use the assignment rule that parish i is assigned hospital j if

$$h_{ijt} = \frac{H_{ijt}}{\hat{B}_{jt}} > \max_{l \in \mathbf{J}_t \setminus j} h_{ilt} \tag{10}$$

This assignment rule thus assures that parish i is added to the hospital where its potential contribution to the measurement error is otherwise the largest.

A.2 Distance Parameter

All assignment rules have in common that they disregard hospital outside a certain range. Since we observe parish of residence and parish of birth for the 1946 cohort, we used this cohort to empirically estimate the radius within which individuals would consider giving birth in a hospital. The results are presented in Figure B.1. Figure B.1a shows, for the entire country, how the probability of being born in a hospital decays with the distance to that hospital. Figure A.1b shows heterogeneity by part of the country. Clearly, individuals in the five northern countries were willing to travel much farther to give birth. Therefore we use the radius $D_j = 60km$ for the 20 southern regions and $D_j = 120km$ for the five northern regions. These numbers correspond to the distance at which the proportion of hospital births drops to 5 per cent in both cases.



(a) Entire Country **(b)** North vs. South

Figure A.1: Proportion Hospital Births by Distance to Hospital.

Note: Own calculations based on the Population Census 1950 and children born in 1946 – for which place of residence of parents and place of birth was available.

B Historical Context

We provide an overview of some important concurrent events regarding childbirth in Sweden.

B.1 Decisions to Open Maternity Wards

In close cooperation with the county councils, a committee that included local politicians and physicians was drafting the funding application that included all the major details regarding the hospital plans (size, costs, location, personnel). Such proposals were based on their knowledge of the local health conditions and the future needs of the population and it was firstly submitted to the National Board of Health, which in turn had to approve that the hospital drawings met certain health care standards. Finally, the project had to approved by all the sides that were contributing financially (national government, county councils, municipality authorities, and banks).[23]

There was a consensus among regional and local politicians regarding the importance of such projects. For the majority of the towns, openings and expansions happened without important disruptions. But there were important disagreements in the regional authorities regarding the financial feasibility of such projects in smaller towns and in the rural areas. Instead, in those cases, they often proposed to build health centers that were cheaper to establish and operate.

B.2 Health Centers

The health centers we mention in the paper cover every childbirth institution that was not a hospital. The sector of health centers was much more heterogeneous than the hospital one in terms of ownership, fees charged, and the quality of provided care. We are pooling them together since they were more comparable with each other than with hospitals. Those institutions could be maternity wards at cottage hospitals (Sjukstuga), maternity homes (Förlossningshem) or maternity rooms (Förlossningsrum).

The cottage hospitals were smaller facilities that offered primary care. They lacked specialist care and could perform only minor and routine operations. These institutions were public and not all of them offered childbirth services. Setting a maternity ward required the approval from the National Board of Health like in the case of hospitals. In 1945, there were on total 80 such establishments in the country, of which 37 offered childbirth services with a total of 197 beds. The available beds for maternity purposes ranged from a minimum of 2 to a maximum of 12 [National Board of Health, 1948]. Only 3 of those were employing a physician trained and specialized in obstetrics or gynecology [Royal Commission on Population Issues, 1946]. Those institutions achieved their peak of importance during late 40s. They remained on place before they started gradually closing down in the subsequent decades. By 1970 only 15 of such institutions continued to offer maternity services with a total of 101 beds [The National Board of Health and Welfare, 1972].

Maternity homes were institutions that were run by midwives and were solely committed to childbirths. Those institutions could be either public, private or non-governmental. At the peak, 21 maternity homes were publicly owned and 62 were independent; of the latter, 27 were owned and run by the Red Cross [Royal Commission on Health Care, 1934]. During the first three decades of the 20th century, those institutions were largely unregulated. For example, it

[23]The decision making processes of hospital constructions we describe are reported in detail in various historical archives. Such as regional authorities' documents like register protocols (e.g Västernorrlands) or reports (e.g Investigation and proposal for a plan for the arrangement of the maternity ward in the Jököping County in 1938) or historical reports written by hospital physicians (e.g. Dr. G. Vidfelt. Report on Värnamo hospital)

was not necessary to have formal qualification to run a birth center. Starting from 1909, those centers were subject to regular controls by public authorities, and these controls led to a decline in the worst practices in terms of e.g hygiene. Still, some of the homes remained controversial and were accused of exploiting poor mothers in dire circumstances. On the other hand, there were also non-profit birth centres with the explicit aim of supporting low-SES single mothers; yet other centres offered single rooms – which were typically not available in public hospitals – at higher fees, which made them accessible to relatively well-off mothers only [Socialdepartementet, 1929]. A new law in 1931 introduced a stricter regime: new birth centres subsequently had to be licensed by the National Board of Health. It became a requirement to have a physician affiliated, and to keep medical records of all patients [Royal Commission on Health Care, 1934]. Lastly, there were also some single room childbirth facilities that could serve prospective mothers. Those institutions were comparable with maternity homes, but they were smaller. Maternity homes were soon considered to be obsolete and a rapid wave of closures occurred during 50s. By 1960 only 15 of such institutions continued to operate[National Board of Health, 1962].

B.3 Abortion Law

As in most other countries, abortion was generally illegal in Sweden in this period, with punishments for abortion dating back to at least the 13th century. During the 18th century a woman completing an abortion would normally be punished with death. Over the course of the century a more humanitarian perspective became predominant, and an 1890 law reduced the punishment to one year of forced labour, whereas the punishments would be increased for the person carrying out the abortion. A new law in 1921 reduced the punishments further – and these regulations were in place at the beginning of our observation period [Committee on International Abortions, 2005].

According to the 1921 law, abortion was illegal from conception until the start of labour. In order to be punishable, the abortion the abortion needed to be using an "internal or external medium" and thus an abortion which was the result of e.g. intentional exhaustion would not be punished. In addition, a legal practice emerging in the 1920s accepted abortions that were necessary to save the mother's life or prevent serious harm to her health. In the early 1930s, around 200 women were granted safe abortion on these grounds – whereas estimates suggest that 10,000-20,000 illegal abortions were carried out each year, leading to 75 deaths on average. Most of these abortions did not result in criminal charges; only 21 women were convicted each year, and convictions typically resulted in suspended sentences [Royal Commission on Abortion Law, 1935].

In 1934 a royal commission was given the task to propose a modernised abortion law. The commission presented its report in 1935, in which it emphasised that the criminalisation of abortions appeared not to have had the intended effect, because most illegal abortions had social causes [Royal Commission on Abortion Law, 1935]. The report resulted in a new abortion law in 1938, which made abortion legal in some circumstances:

1. If the pregnancy represents a serious risk to the mother's health.
2. If the pregnancy was the result of a crime.
3. If the child might inherit a genetic predisposition to mental or life-limiting illness.

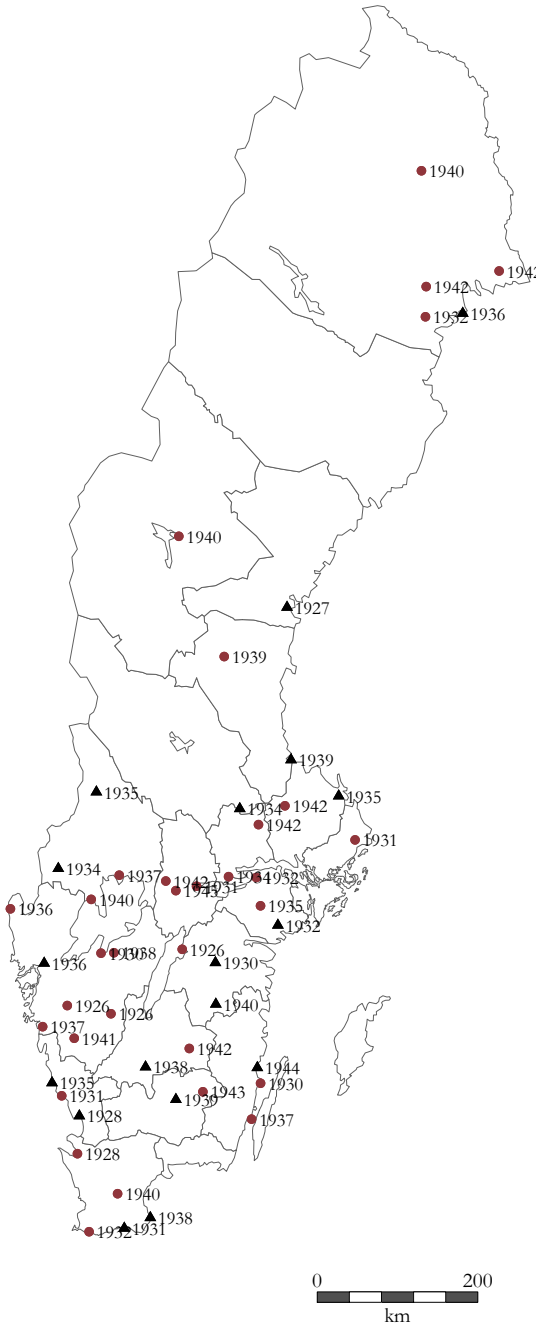
The first two categories required formal approval by two physicians. Abortions in the third category were approved by the National Board of Health and required that the mother was sterilised in connection with the procedure [Committee on International Abortions, 2005].

The 1938 abortion law came into force on 1 January 1939 and remained in force until 1975. It was supplemented with a fourth legal ground for abortion in 1946: a risk of severe social consequences for the mother would also represent a route to legal abortion [Committee on International Abortions, 2005].

Clearly the liberalisation in 1939 is close in time to some of the hospital openings and extensions we consider (it falls within the observation window for 12 of them, and also within the observation window of the 1938 abolition of fees. This raises the question as to whether it might confound our estimates. The terminated pregnancies were apparently very selective (single mothers in dire circumstances were strongly over-represented). However, the 1938 law was interpreted very restrictively and therefore, only very few cases were approved each year: numbers went up from 523 to 990 between 1939 and 1943. Thus, the legal abortions remained a very small part compared to the illegal abortions, which according to experts did not become less prevalent during the 1930s, despite the new law [Royal Commission on Population Issues, 1944].

Hence, it appears unlikely that the 1938 abortion law, or the 1946 liberalisation of it, would confound our estimates. Nevertheless, we test the robustness of our results to controlling for these legal changes below.

B.4 Interventions



(a) Entire Country

Figure B.1: Openings or expansions of maternity wards.

Notes: Pending. A ● refers to hospital opening and a ▲ to an extension of an already established facility.

C Data on Facilities

Table C.1: Comparison: Facility-Based Childbirth

Setting:	**Sweden 1940**	**Linköping**	**Bangladesh**		**Congo (DRC)**	
Information						
Infant Mortality (deaths per 1,000 births)	24	19	38		58	
Deliveries in Health Facility	0.75	0.45	0.49		0.80	
Deliveries by Medically Trained Provider	1	1	0.53		0.80	
Population (million)	6.7		159.7		84.07	
Income Level (World Bank)			Lower Middle		Low	
Type of Facility:		**Hospital**	**All**	**Hospitals**	**All**	**Hospitals**
Facilities						
Ambulance		*Yes*	0.40	0.87	0.18	0.37
Connected to Electricity Grid		*Yes*	0.90	0.93	0.19	0.27
Electricity Generator		*Yes*	0.79	0.97	0.95	0.94
Telephone		*Yes*	0.46	0.87	0.10	0.16
Piped Water		*Yes*	0.67	0.82	0.26	0.40
Medical diagnosis:						
Cardiovascular Diseases		*Yes*	0.84	0.92	0.99	1.00
Diabetes		*Yes*	0.81	1.00	0.81	0.98
Anemia			0.89	0.94	0.90	0.99
Urine Protein Test		*Yes*	0.60	0.91	0.29	0.48
Delivery: Procedures and Supplies						
Caesarean section		*Yes*	0.33	0.87	0.62	0.98
Delivery Bed		*Yes*	0.81	0.89	0.95	1.00
Weigh the newborn after birth		*Yes*	0.84	0.98	0.86	0.96
Infant Scale		*Yes*	0.60	0.76	0.78	0.93
Thermometer		*Yes*	0.92	1.00	0.82	0.87
Stethoscope		*Yes*	0.99	0.99	0.85	0.87
Forceps		*Yes*	0.85	0.93	0.08	0.13
Stadiometer or Height Rod		*Yes*	1.00	1.00	1	1.00
Sterilization Equipment		*Yes*	0.90	1	0.69	0.87
Post-30s Discoveries:						
Antibiotics		.	0.35	0.85	0.42	0.46
Ultrasound		.	0.78	0.87	0.35	0.67
Folic Acid		.	0.67	0.44	0.53	0.63
Number:		1	818	83	1352	616
Year:		1931	2017	2017	2017-18	2017-18

Notes: From the data from other countries we include only facilities that are supposed to regurarly hold births. Hospitals and other health facilities without at least one childbirth specialist (physician, midwife, nurse etc) are dropped. Since those data they represent specialized facilities they should interpreted as upper bounds of childbirth health services.
We choose to gather information and use as baseline for our comparisons a hospital from a regional town (Linköping). Our interest lies on basic amenities, services and supplies that are important for safe childbirth and were routinely applied to childbirths back then and today.
Source: Sweden:Census 1950, Swedish Death Register. Own calculations. Rest: Bangladesh DHS (2017-18), Bangladesh SPA (2017), Congo Democratic Republic SPA (2017-18), Congo Democratic Republic DHS (2013-14)

Table C.2: Hospital Operations

Operation	Complications / Diseases	ICD-11 Codes
Forcep Delivery	birth asphyxia, maternal weakness, eclampsia	KB21, JB0D.Y, JA25
	haemorrhage, Puerperal sepsis, maternal any chronic disease	JA43,JB40.0
	tuberculosis, umbilical cord prolapse	1B10, JB08.0
Cesarean section	placenta previa, narrow pelvis, placental abruption	JA8B, JB05,JA8C.Z
	pre-eclampsia	JA24
Neonatal resuscitation	birth asphyxia	KB21
Blood transfusion (newborn)	Vitamin K deficiency bleeding	KA8F.0

Notes:The ICD 11 codes we report here are following the latest revision of World Health Organization. The table reports the most common operations for the main complications. Other operations were also on place like polydactylia treatments, intentional membrane ruptures, venesctions and widening of the cervix by incision.
Source: Annual Hospital Reports. .

Table C.3: Maternity Wards Included in the Analysis

	Type	Type	Year
Institutions: *Place*	**Hospitals (N=38)**		
Motala	*Maternity Hospital*	Opening	1926
Sundsvall	*Maternity Unit at General Hospital*	Extension	1927
Ängelholm	*Maternity Unit at General Hospital*	Opening	1928
Halmstad	*Maternity Unit at General Hospital*	Extension	1928
Linköping	*Maternity Hospital*	Extension	1930
Lidköping	*Maternity Unit at General Hospital*	Opening	1930
Örebro	*Maternity Unit at General Hospital*	Opening	1931
Ystad	*Maternity Unit at General Hospital*	Extension	1931
Falkenberg	*Maternity Unit at General Hospital*	Opening	1931
Norrtälje	*Maternity Unit at General Hospital*	Opening	1931
Trälleborg	*Maternity Unit at General Hospital*	Opening	1932
Nyköping	*Maternity Unit at General Hospital*	Extension	1932
Eskilstuna	*Maternity Unit at General Hospital*	Opening	1932
Avesta	*Maternity Unit at General Hospital*	Extension	1934
Silbodal	*Maternity Unit at General Hospital*	Extension	1934
Östhammar	*Maternity Unit at General Hospital*	Extension	1935
Varberg	*Maternity Unit at General Hospital*	Extension	1935
Fryksände	*Maternity Unit at General Hospital*	Extension	1935
Flen	*Maternity Hospital*	Opening	1935
Strömstad	*Maternity Unit at General Hospital*	Opening	1936
Uddevalla	*Maternity Unit at General Hospital*	Opening	1936
Luleå	*Maternity Unit at General Hospital*	Extension	1936
Karlstad	*Maternity Unit at General Hospital*	Opening	1937
Kalmar	*Maternity Unit at General Hospital*	Extension	1937
Mölndal	*Maternity Unit at General Hospital*	Opening	1937
Värnamo	*Maternity Unit at General Hospital*	Extension	1938
Simrishamn	*Maternity Unit at General Hospital*	Extension	1938
Gävle	*Maternity Unit at General Hospital*	Extension	1939
Växjö	*Maternity Unit at General Hospital*	Extension	1939
Ljusdal	*Maternity Unit at General Hospital*	Opening	1939
Östersund	*Maternity Unit at General Hospital*	Opening	1940
Hörby	*Maternity Unit at General Hospital*	Opening	1940
Kisa	*Maternity Unit at General Hospital*	Extension	1940
Gällivare	*Maternity Unit at General Hospital*	Opening	1940
Sala	*Maternity Unit at General Hospital*	Opening	1942
Karlskoga	*Maternity Unit at General Hospital*	Opening	1942
Nederkalix	*Maternity Unit at General Hospital*	Opening	1942
Oskarshamn	*Maternity Unit at General Hospital*	Extension	1944
Institutions: *Place*	**Health Centers (N=13)**		
Ulricehamn	*Maternity Home (Förlossningshem)*	Opening	1926
Alingsås	*Maternity Home (Förlossningshem)*	Opening	1926
Mönsterås	*Maternity Home (Förlossningshem)*	Opening	1930
Piteå landskom.	*Maternity Home (Förlossningshem)*	Opening	1932
Arboga	*Maternity Home (Förlossningshem)*	Opening	1934
Skara	*Maternity Home (Förlossningshem)*	Opening	1938
Säffle köp	*Cottage Hospital (Sjukstuga)*	Opening	1940
Kinna	*Maternity Home (Förlossningshem)*	Opening	1941
Vetlanda	*Cottage Hospital (Sjukstuga)*	Opening	1942
Östervåla	*Cottage Hospital (Sjukstuga)*	Opening	1942
Överluleå	*Cottage Hospital (Sjukstuga)*	Opening	1942
Lenhovda	*Cottage Hospital (Sjukstuga)*	Opening	1943
Knista	*Maternity Home (Förlossningshem)*	Opening	1945

Source: Interventions were identified from historical reports: General Healthcare Reports, Tax reports, Local Administrative Reports. The openings/expansions were validated through a number of procedures: presence of a birthbook, discontinuity in births.

C.1 Midwife and Hospital Data

The dataset used to study home births in Section 5 is based on annual reports from the Chief medical officers in each county, and the corresponding publication for the independent cities. Each year, information from the midwives' diaries were aggregated to present statistics for home births within each of the 446 health districts. This data source entails a complete enumeration of all births – live births and still births – including the mother's marital status, parity, the child's sex, multiple births. The source also includes complications arising and procedures applied either by the midwife or by a physician. This dataset is described in more detail in Boberg-Fazlic et al. [2021] and Bhalotra et al. [2017].

In order to evaluate the medicalisation of childbirth, we added data on hospital deliveries to this dataset. Most of the active hospitals had specific yearbooks for the maternity wards, and these would include statistics on things like presentation of the fetus, marital status of the mother, parity, multiple births, and various birth outcomes. The yearbooks also include data on procedures and complications. We defined variables for procedures and added the hospital procedures to the procedures reported by midwives in the corresponding health district. In order to make the health districts match the catchment areas of the hospitals, we added the surrounding rural health district to each city with a hospital.

Table C.4 provide descriptive statistics for both datasets.

Table C.4: Descriptive Statistics: Midwife and Hospital Data

	Mean	Std.Dev.	Min	Max	Obs
MIDWIFE DATASET					
Home birth	0.699	0.29	0.00	1.00	785,280
Any Procedure	0.024	0.02	0.00	0.20	785,167
Complication	0.003	0.01	0.00	0.09	785,167
Mother ill/diseased	0.013	0.01	0.00	0.20	785,167
Twins	0.012	0.01	0.00	0.13	785,167
Eclampsia	0.001	0.00	0.00	0.07	785,167
Cephalo-pelvic Disproportion	0.001	0.00	0.00	0.06	785,167
Placenta Praevia	0.001	0.00	0.00	0.05	785,167
Uterine Rupture	0.000	0.00	0.00	0.02	785,167
COMBINED DATASET					
Any Procedure	0.027	0.02	0.00	0.20	583,770
Procedure Available in Home Birth	0.026	0.02	0.00	0.20	583,770

Notes: A detailed description of the dataset and its sources is provided in Boberg-Fazlic et al. [2021].
Source: Own calculations.

C.2 Length of Stay

In this subsection, we provide some descriptives on length of stay for a subset of patient journals from the 1930s.

Figure C.1 shows the distribution of length of stay in hospital, measured in two alternative was: from admission and from delivery. The distribution is fairly concentrated around the mode of 9: 60 per cent of mothers spend 9 or 10 days in hospital, and 86 per cent spend between 6 and 12 days.

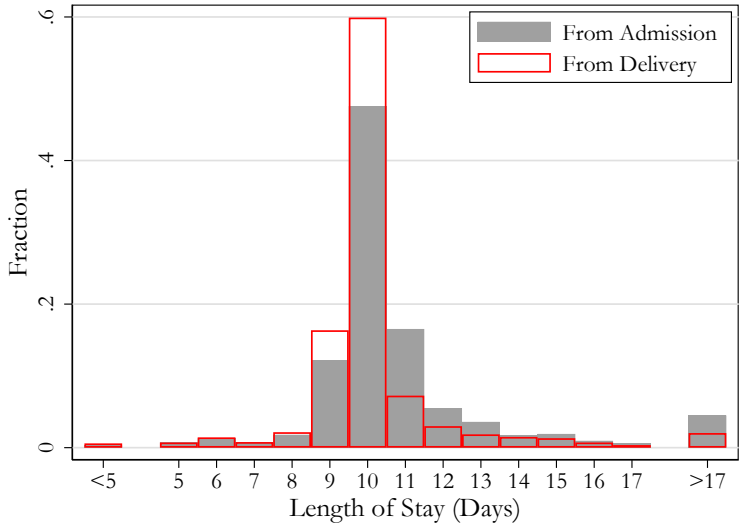


Figure C.1: Distribution of Length of Stay.

Note: Calculation based on 1,669 births at Falun hospital 1932–38.

Table C.5 shows how length of stay relates to observable characteristics. We consider three outcomes: time between admission and discharge (Total), time between admission and delivery (Pre) and time between delivery and discharge (Post). Apparently, pre-term births have significanrtly longer length of stay: the difference is 2.3 days on average, and most of it applies post partum. Male babies also had shorter lenght of stay on average. Finally first-time mothers stayed longer in hospital on average. On the other hand, the mother's age and marital status do not turn out statistically significant.

Table C.5: Length of Stay: Determinants

	Total	**Pre**	**Post**
Age	0.031	0.015	0.016
	(0.028)	(0.013)	(0.024)
First birth	0.511	0.141	0.370
	(0.345)	(0.165)	(0.293)
Married	-0.429	-0.033	-0.396
	(0.395)	(0.189)	(0.336)
Pre-term Birth	1.709***	0.435	1.274**
	(0.631)	(0.302)	(0.536)
Male child	-0.568*	0.147	-0.714***
	(0.297)	(0.142)	(0.252)
N	1,669	1,669	1,669
Adj. R-squared	0.020	0.009	0.023

Notes: Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Each specification includes year and month fixed effects. *Source:* Patient journals from Faluns lasarett 1932–38.

C.3 Medicalisation through Hospital Delivery

In Table C.6 we estimate the effect of a hospital opening or extension on the procedures applied, using the same dataset as in Table 8 but supplementing it with data on procedures from hospital yearbooks. The main outcome in Table C.6 is the total prevalence of procedures, and estimates

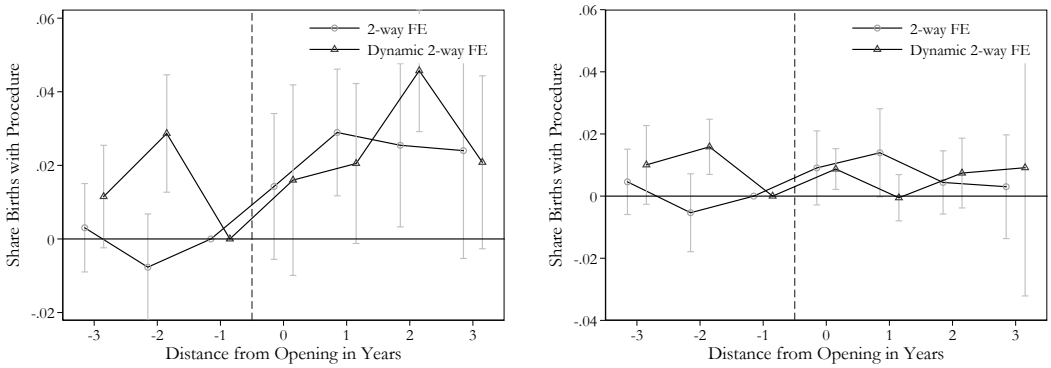
are presented in the first column. Accordingly, hospital openings and extensions increased the prevalence of procedures by 2.8 percentage points, or 59 per cent compared with the baseline. If we consider only procedures that could be offered in home births (rightmost column), the estimated effect is small and insignificant. Hence, the increase appears to be driven by the procedures that only the hospitals could offer.

Table C.6: Effects of Hospital Openings and Extension on Procedures

	PROCEDURES	
	All	Available home
Hospital Opening	0.032***	0.008
	(0.011)	(0.006)
Mean Dep. Var	0.053	0.050
Relative Effect	0.606	0.162
N	583,770	583,770
Health Districts	391	391
Robust Effect (Dynamic 2-Way FE)	0.021	-0.001
SE Robust Effect	(0.01)	(0.004)

Notes: Table shows effects of the a hospital opening or extension in a given health district. Estimation are based on a standard difference-in-differences regression on aggregated data on health district level controlling for year and health district FE. Results cover all health districts 1928–1938. 95% CI based on robust standard errors clustered at health district level.
Source: Midwife Diaries and Hospital Yearbooks. Own calculations.

Figure C.2 present event studies for the two outcomes.



(a) **(b)**

Figure C.2: Event Study: (**a**) All procedures; (**b**) Procedures offered in home births

Notes: Figure shows coefficients from an event-study-type regression with lags and leads of the a hospital opening or extension in a given health district. Estimates are based on a difference-in-differences specification controlling for year and health district FE. Results cover all health districts 1928–1938. 95% CI based on robust standard errors clustered at health district level.
Source: Midwife Diaries and Hospital Yearbooks. Own calculations.

D Methods

D.1 Difference-in-Discontinuities Specification

As outlined in Section 3.3.2, our estimand is

$$\begin{aligned}\tau_j = &\lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c\left(i\right), i \in \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c\left(i\right), i \in \mathcal{I}_j\right] \\ &- \left[\lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c\left(i\right), i \notin \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c\left(i\right), i \notin \mathcal{I}_j\right]\right]\end{aligned} \tag{11}$$

Next define the dummy variable $D_i^j = 1\left(t_i \geq c_j\right)$. Our implementation is

$$Y_i = \lambda_0 + \sum_{j=1}^{K} \lambda_j^1 D_i^j + \sum_{j=1}^{K-1} \lambda_j^2 D_i^j 1\left(t_i < c_{j-1} + \frac{c_{j-1} - c_j}{2}\right) \tag{12}$$

$$+ \beta_0\left(t_i - c_1\right) + \sum_{j=1}^{K+1} \beta_j^1 D_i^j 1\left(t_i \geq c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_j\right) \tag{13}$$

$$+ \sum_{j=2}^{K+1} \beta_j^2 D_i^j 1\left(t_i < c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_{j-1}\right) \tag{14}$$

$$+ \alpha_{J(i)} + \tau D_i^{J(i)} \tag{15}$$

$$+ \beta_0 1\left(i \in \mathcal{I}_1\right)\left(t_i - c_1\right) + \sum_{j=1}^{K+1} \beta_j^1 D_i^j 1\left(i \in \mathcal{I}_j\right) 1\left(t_i \geq c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_j\right) \tag{16}$$

$$+ \sum_{j=2}^{K+1} \beta_j^2 D_i^j 1\left(i \in \mathcal{I}_{j-1}\right) 1\left(t_i < c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_{j-1}\right) \tag{17}$$

$$+ \gamma X_i + \sum_{m=2}^{12} \delta_m month \tag{18}$$

Line 12 includes common period effects for each of the $2K$ periods that we have split the sample into. Lines 13 and 14 represent common trends from below and above to each of the K cutoffs. Line 15 contains K fixed effects and τ which is the main parameter of interest. Rows 16 and 17 allow for differences in trends around the cutoff for treated and untreated hospitals. Line 18 includes month-of-birth fixed effects and baseline covariates.

E Balancing Regression

Table E.1: Balancing Regression: Parish Level Data

	Mean	**OLS**	**DiD**	**RDD**	**DiD Window** *(DiDisc)*
Share poor	0.046	0.021***	-0.005	-0.000	-0.000
		(0.003)	(0.004)	(0.002)	(0.003)
Taxable income per cap.	712	593.891***	3.390	4.129	-13.425
		(35.646)	(15.496)	(18.314)	(13.431)
Property per cap.	2,874	453.960***	-42.068	87.852	76.198
		(94.762)	(60.519)	(86.544)	(85.790)
Debt/Asset ratio per cap.	1.443	-0.788***	-0.485	0.087*	0.079
		(0.208)	(0.468)	(0.048)	(0.067)
7-Year extension	0.625	0.152***	-0.049	0.005	0.018
		(0.018)	(0.040)	(0.035)	(0.032)
Term Length Extension	0.598	0.163***	0.006	0.037	0.017
		(0.015)	(0.032)	(0.049)	(0.021)
Infant Welfare Program	0.544	-0.130***	0.050	0.035	0.000
		(0.021)	(0.036)	(0.047)	(0.047)
$N \times T$		59,239	59,239	†	351
N (Cluster)		2,472	2,472	51	51
Year FE		✓	✓		✓
Parish FE			✓		✓
Linear Parish Year Trends			✓		✓
4-Year Window					✓

Notes: Robust standard errors clustered at the parish level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD Bandwidth is data-driven, the number of observations † depends on the selected bandwidth. To mimic DiDisc estimator, the window sample covers a period of 4 years around the opening. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Parish Panel. Own calculations.

Table E.2: Balancing Regression: Individual Level Data

	Baseline	**OLS**	**RDD**	**DiDisc**
Male	0.514	0.001	-0.007	0.005
		(0.003)	(0.009)	(0.005)
Patronymic Name	0.561	-0.008***	-0.017	0.000
		(0.003)	(0.012)	(0.005)
High SES Name	0.177	0.001	0.000	-0.003
		(0.002)	(0.008)	(0.004)
Twin	0.013	-0.003***	0.004	0.001
		(0.001)	(0.003)	(0.001)
Observations:				141,157

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.Regression controls for month-of-birth fixed effects. RDD Bandwidth is data-driven. DIDisc sample covers a period of 4 years around the opening. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations

F Robustness

Table F.1: Manipulation Density Test

Bandwidth	T	P > |T|
180 Days	-0.855	0.392
365 Days	1.026	0.305
730 Days	0.528	0.597
Data-driven	-0.664	0.506

Notes: The manipulation testing employed here is following the method proposed by Cattaneo et al. [2018]

Table F.2: Regression Results without Covariates

	Baseline	RDD (non-parametric)		Difference-in-Discontinuities	
		Reduced Form	2SLS	Reduced Form	2SLS
Mortality					
Neonatal Death (Age < 1 Month)	0.028	-0.008**	-0.045**	-0.007***	-0.044***
		(0.003)	(0.018)	(0.001)	(0.009)
Infant Death (Age < 1 Year)	0.052	-0.020***	-0.131***	-0.009***	-0.057***
		(0.006)	(0.037)	(0.002)	(0.013)
Child Death (Age < 5 Years)	0.064	-0.015**	-0.101**	-0.009***	-0.056***
		(0.006)	(0.042)	(0.002)	(0.014)
Death before Age 50	0.116	-0.024***	-0.152***	-0.011***	-0.069***
		(0.007)	(0.049)	(0.003)	(0.018)
Death before Age 70	0.249	-0.022**	-0.126**	-0.015***	-0.092***
		(0.009)	(0.052)	(0.004)	(0.025)
Socio-economic Outcomes					
Secondary School	0.192	0.031***	0.173***	0.019***	0.118***
		(0.009)	(0.051)	(0.004)	(0.026)
Years of Education	8.629	0.205***	1.209***	0.101***	0.617***
		(0.054)	(0.318)	(0.024)	(0.145)
Earnings 1970	20,591	160.518	873.480	421.493**	2580.513**
		(337.083)	(1828.716)	(172.295)	(1050.122)
Observations:				141,157	141,157

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.Regression controls for month-of-birth fixed effects. RDD Bandwidth is data-driven. DIDisc sample covers a period of 4 years around the opening. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

Table F.3: Alternative Bandwidths: RDD

	Baseline	182 Days		365 Days		730 Days	
		Reduced Form	2SLS	Reduced Form	2SLS	Reduced Form	2SLS
Mortality							
Neonatal Death (Age $<$ 1 Month)	0.028	-0.009**	-0.057**	-0.006**	-0.039**	-0.007***	-0.041***
		(0.004)	(0.022)	(0.003)	(0.015)	(0.002)	(0.010)
Infant Death (Age $<$ 1 Year)	0.052	-0.015***	-0.092***	-0.010***	-0.060***	-0.008***	-0.046***
		(0.005)	(0.029)	(0.003)	(0.021)	(0.002)	(0.014)
Child Death (Age $<$ 5 Years)	0.064	-0.010*	-0.061*	-0.006	-0.034	-0.007***	-0.042***
		(0.005)	(0.032)	(0.004)	(0.022)	(0.003)	(0.015)
Death before Age 50	0.116	-0.012**	-0.075**	-0.008*	-0.051*	-0.011***	-0.063***
		(0.006)	(0.037)	(0.005)	(0.027)	(0.003)	(0.019)
Death before Age 70	0.249	-0.020**	-0.120**	-0.017**	-0.102**	-0.017***	-0.098***
		(0.009)	(0.057)	(0.007)	(0.040)	(0.005)	(0.026)
Socio-economic Outcomes							
Secondary School	0.192	0.025**	0.147***	0.021***	0.120***	0.021***	0.116***
		(0.010)	(0.057)	(0.007)	(0.039)	(0.005)	(0.026)
Years of Education	8.629	0.179***	1.059***	0.112***	0.646***	0.115***	0.638***
		(0.050)	(0.296)	(0.037)	(0.214)	(0.026)	(0.146)
Earnings 1970	20,591	741.800**	4374.553**	431.994*	2482.135*	314.993*	1754.587*
		(303.073)	(1798.450)	(228.377)	(1308.485)	(161.375)	(896.967)
Observations:		34,773	34,773	70,400	70,400	141,157	141,157

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.Regression controls for family SES proxied by surnames, month-of-birth fixed effects and hospital catchement-area level socioeconomic indicators and educational reforms. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

Table F.4: Alternative Specification (RDD): Robust bias-corrected (Covariate-adjusted) [Calonico et al., 2017]

	Reduced Form	2SLS
Mortality		
Neonatal Death (Age $<$ 1 Month)	-0.009***	-0.065**
	(0.003)	(0.028)
Infant Death (Age $<$ 1 Year)	-0.024***	-0.141***
	(0.006)	(0.034)
Child Death (Age $<$ 5 Years)	-0.019***	-0.110***
	(0.007)	(0.038)
Death before Age 50	-0.025***	-0.146***
	(0.007)	(0.046)
Death before Age 70	-0.020**	-0.136**
	(0.009)	(0.064)
Socio-economic Outcomes		
Secondary School	0.029***	0.188***
	(0.010)	(0.065)
Years of Education	0.231***	1.385***
	(0.052)	(0.307)
Earnings 1970	786.151***	5196.023***
	(286.797)	(1935.393)

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.Regression controls for month-of-birth fixed effects. RDD Bandwidth is data-driven. DIDisc sample covers a period of 4 years around the opening. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

Table F.5: Heterogeneity: Surnames (RDD)

	Patronymic			Other		
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Mortality						
Neonatal Death (Age $<$ 1 Month)	0.029	-0.013***	-0.081***	0.028	-0.000	-0.003
		(0.004)	(0.024)		(0.004)	(0.023)
Socio-economic Outcomes						
Secondary School	0.158	0.030**	0.175**	0.243	0.023	0.127
		(0.012)	(0.073)		(0.015)	(0.086)
Years of Education	8.405	0.145**	0.854**	8.973	0.198**	1.055**
		(0.069)	(0.403)		(0.079)	(0.429)
Earnings 1970	19,503	157.386	886.134	21,521	614.952	3220.569
		(415.808)	(2336.555)		(518.192)	(2707.934)

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD Bandwidth is data-driven, estimated each time for each sub-sample. Regression controls for month-of-birth fixed effects and hospital catchement-area level socioeconomic indicators and educational reforms. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

Table F.6: Heterogeneity: Facility Type (RDD)

	Hospital			Health Center		
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Mortality						
Neonatal Death (Age $<$ 1 Month)	0.028	-0.008**	-0.049**	0.031	-0.005	-0.022
		(0.003)	(0.020)		(0.008)	(0.036)
Socio-economic Outcomes						
Secondary School	0.200	0.031***	0.189***	0.186	0.042**	0.180**
		(0.010)	(0.063)		(0.018)	(0.074)
Years of Education	8.677	0.177***	1.047***	8.529	0.191	0.813
		(0.052)	(0.309)		(0.120)	(0.494)
Earnings 1970	20,503	555.601	3382.785	19,585	-586.643	-2305.243
		(418.664)	(2533.271)		(889.088)	(3513.190)
Maternity Wards		13			38	

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD Bandwidth is data-driven, estimated each time for each sub-sample. Regression controls for month-of-birth fixed effects and hospital catchement-area level socioeconomic indicators and educational reforms. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

Table F.7: Heterogeneity: Hospital Openings and Extensions (RDD)

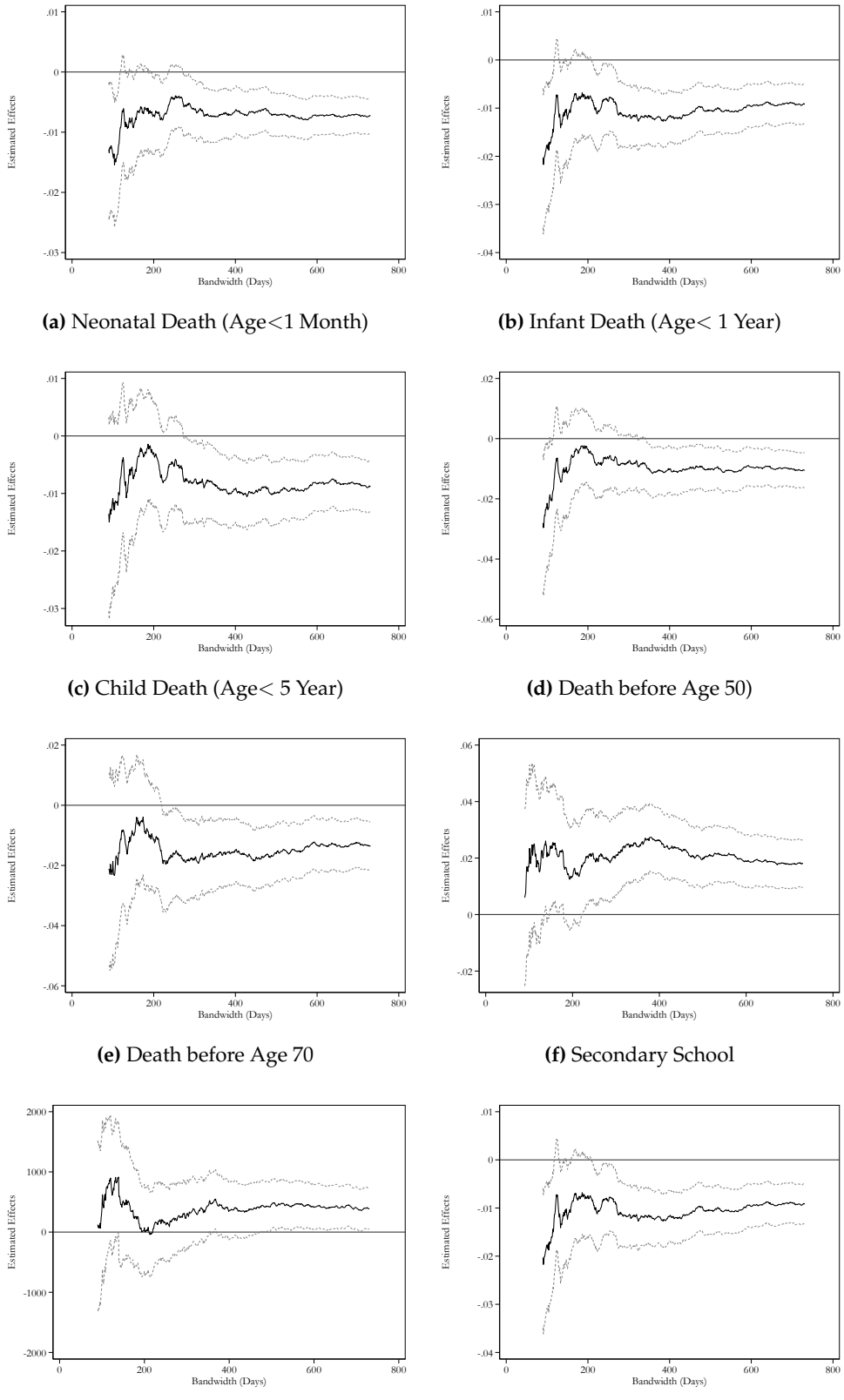
	Openings			Extensions		
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Mortality						
Neonatal Death (Age $<$ 1 Month)	0.029	-0.007***	-0.039***	0.027	-0.007**	-0.063**
		(0.002)	(0.011)		(0.003)	(0.025)
Socio-economic Outcomes						
Secondary School	0.193	0.020***	0.105***	0.191	0.019**	0.157**
		(0.006)	(0.031)		(0.008)	(0.065)
Years of Education	8.661	0.101***	0.530***	8.582	0.197***	1.670***
		(0.033)	(0.171)		(0.045)	(0.381)
Earnings 1970	20,484	402.338*	2119.848*	20,747	595.439*	5005.521*
		(244.101)	(1279.701)		(334.375)	(2819.432)
Maternity Wards		18			33	

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD Bandwidth is data-driven, estimated each time for each sub-sample. Regression controls for month-of-birth fixed effects and hospital catchement-area level socioeconomic indicators and educational reforms. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

Table F.8: Placebo Regressions: 1938 Reform

		RDD			
Placebo Reform Date:	**1.1.1935**	**1.1.1936**	**1.1.1937**	**1.1.1939**	
First-Stage					
Hospitals Parish Birth		0.020	0.001	0.017	0.029
		(0.018)	(0.016)	(0.018)	(0.018)
Neonatal Death (Age $<$ 1 Month)		-0.002	-0.003	0.002	0.001
		(0.006)	(0.005)	(0.006)	(0.006)
Infant Death (Age $<$ 1 Year)		0.001	0.002	-0.009	0.009
		(0.008)	(0.007)	(0.008)	(0.007)
Child Death (Age $<$ 5 Years)		-0.004	0.008	-0.007	0.011
		(0.008)	(0.007)	(0.009)	(0.008)
Death before Age 50		-0.007	-0.008	-0.004	0.019
		(0.011)	(0.010)	(0.012)	(0.012)
Death before Age 70		0.005	-0.011	-0.019	0.016
		(0.015)	(0.015)	(0.016)	(0.015)
Secondary School		-0.012	0.005	0.003	-0.007
		(0.017)	(0.016)	(0.015)	(0.017)
Years of Education		0.013	-0.006	0.035	-0.074
		(0.098)	(0.095)	(0.087)	(0.095)
Earnings 1970		1225.701	56.334	-205.706	-1041.299
		(792.110)	(697.291)	(684.817)	(702.000)
Observations:	280,325	276,841	275,090	292,031	

Notes:Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD Bandwidth covers a period of 4 years around the placebo dates.
Source: Census 1950, Swedish Death Register. Own calculations.



(a) Neonatal Death (Age<1 Month)

(b) Infant Death (Age< 1 Year)

(c) Child Death (Age< 5 Year)

(d) Death before Age 50)

(e) Death before Age 70

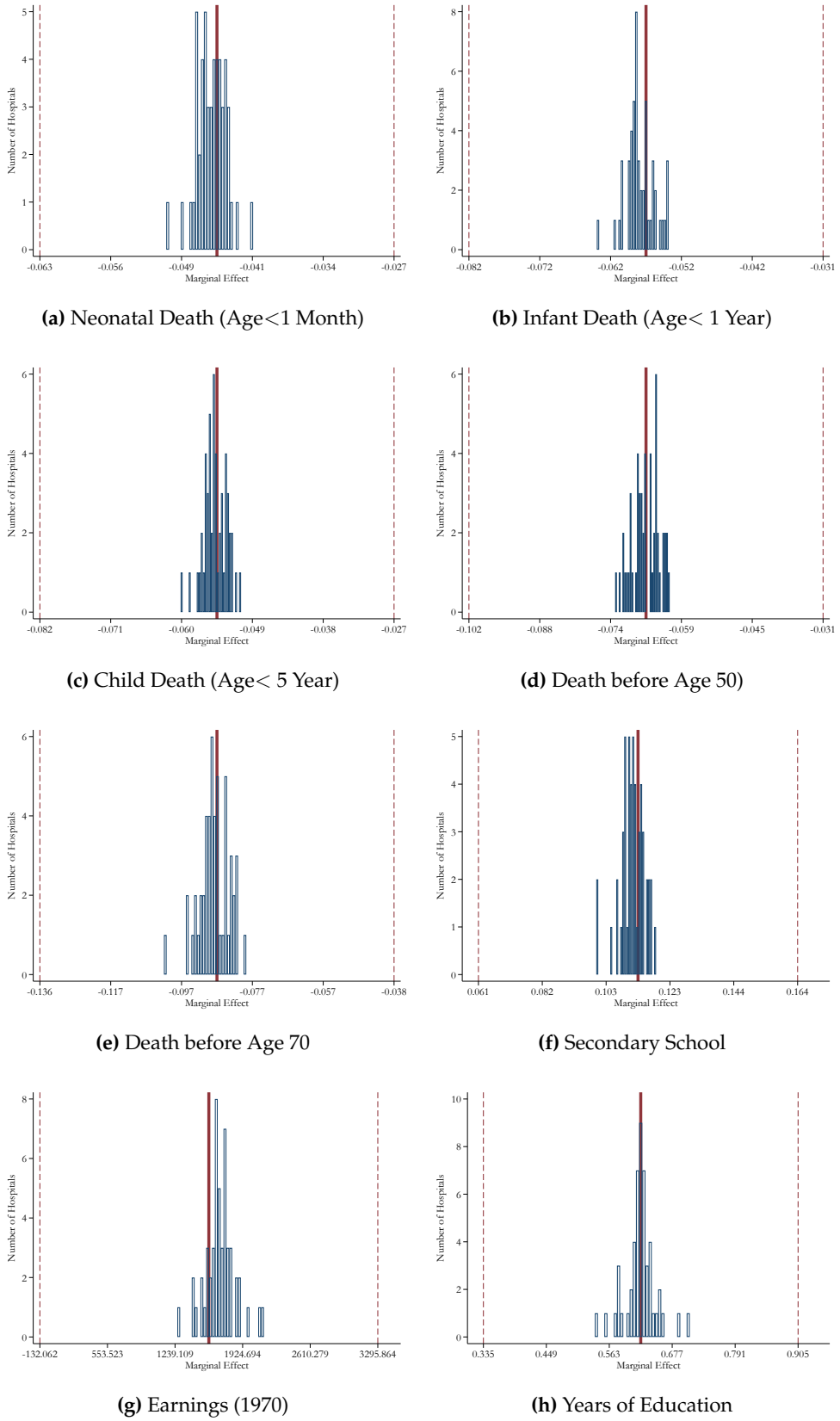
(f) Secondary School

(g) Earnings (1970)

(h) Years of Education

Figure F.1: Effects for different bandwidths (DiDisc).

Note: The graph shows the effect estimates for the DiDisc design. Accompanied with the 95 percent CI. Bandwidths ranging from 90 to 730 days-



(a) Neonatal Death (Age<1 Month)

(b) Infant Death (Age< 1 Year)

(c) Child Death (Age< 5 Year)

(d) Death before Age 50)

(e) Death before Age 70

(f) Secondary School

(g) Earnings (1970)

(h) Years of Education

Figure F.2: Leave-one-out Hospital Exercise

Note: We run all the regressions for the DiDisc design excluding each hospital one time. The histograms plots the 51 obtained estimates along with the main estimate and its confidence interval (dashed line).)

Table F.9: Bounds(Conditioning Variables:CAs, Week of Birth and all the baseline covariates in binary form)

	Censoring					
	from Below Upper Bounds			from Above Lower Bounds		
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Socio-economic Outcomes						
Secondary School	0.204	0.022***	0.134***	0.167	0.013***	0.078***
		(0.005)	(0.028)		(0.004)	(0.026)
Years of Education	8.726	0.129***	0.786***	8.459	0.075***	0.472***
		(0.025)	(0.150)		(0.024)	(0.150)
Earnings 1970	21,249	450.576***	2772.390***	19,631	89.553	559.664
		(148.138)	(909.837)		(137.697)	(858.475)

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Regression controls for family SES proxied by surname, sex, month-of-birth fixed effects and hospital catchement-area level socioeconomic indicators and educational reforms. The treatment variable represents hospital openings or expansions. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register. Own calculations.

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