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The Long-term Effects of Hospital Deliveries

Imprint

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ISSN 2199-8744 (online)

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The Long-Term Effects of Hospital Deliveries

UNIVERSITÄT DUISBURG ESSEN
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Bundesministerium für Bildung und Forschung

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The Long-Term Effects of Hospital Deliveries[*]

Abstract

This paper analyzes the long-term effects on mortality and socio-economic outcomes from institutional delivery. We exploit two Swedish interventions that affected the costs of hospital deliveries and the supply of 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 later-life outcomes such as education and mortality. We argue that a decrease in child morbidity due to better treatment of complications is a likely mechanism. This interpretation is corroborated by evidence from primary school performance, showing a large reduction in the probability of low performance. In contrast to an immediate and large take-up in hospital deliveries as response to an increase in the supply, we find no increase in hospital births from the abolishment of fees – but some degree of displacement of high-SES parents.

Keywords: *Institutional delivery; diffusion of innovations; 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, Daniel Kühnle, Andreea Mitrut, Hanna Mühlrad, Bettina Siflinger and Yuejun Zhao for their comments and suggestions, Lukas Hornig for excellent research assistance and Therese Nilsson for making historical datasets available.*

1 Introduction

The considerable increases in life expectancy during the 20th century represent one of the most striking achievements in recent history [Fogel et al., 2004]. Those health improvements in longevity originated in part from a substantial reduction of neonatal and child mortality. A vivid literature emerged on the origins of that progress and the role that nutrition, public health policies and health technologies had for it [Cutler et al., 2006]. According to one recent study, there is a strong case for medical care being an important driver of improvements in life expectancy since around 1935 [Catillon et al., 2018]. At around the same time, deliveries in hospitals began to replace home deliveries in several countries; and yet the contribution of this specific compoment to general improvements in life expectancy remains largely unknown. The same goes for its long-term effects on other outcomes such as educational attainment and labor market outcomes [cf. Almond et al., 2018].

In this paper, we study the effects of hospital deliveries on mortality and long-term socioeconomic outcomes using administrative data from Sweden. We exploit two historical public health interventions that affected the supply of maternity wards and the demand for hospital delivery during the 1926-46 period. The first intervention was a large nationwide expansion of maternity wards at hospitals and the second a policy related to the establishment of a universal welfare state; it entailed a social insurance reform that made hospital delivery free of charge. By combining a wide range of data sources, we can consider a number of potential mechanisms and also study the individual-level selection into hospital delivery.

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

[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

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].

Consequently, the relative advantages of hospital delivery in comparison to the other available options are still debated within obstetrics as well as in society at large. As a result, government policies vary between countries in their support for births outside hospitals, as do professional guidelines [Scarf et al., 2018]. Moreover, evidence from low resource settings also remain unclear and fail to show a robust association between deliveries in facilities and neonatal mortality reductions [Fink et al., 2015]. Similarly, the proportion of women giving birth at home or in midwife-led birth centres exhibits striking differences between countries [Roome et al., 2016].

In this paper, by exploiting sharp discontinuities in the availability of maternity wards or in the public subsidies for childbirth, we are able to address a number of methodological challenges and estimate the long-term causal effect of being delivered in hospital and not at home. We use linked administrative data for a large and representative sample of individuals tracked from birth, through school, to labour market outcomes and then to retirement and death. It is unusual to have individual longitudinal data from birth to death for a population and, especially unusual to have school test scores linked backwards to quasi-experimental variation in birth conditions, as well as forwards to labour market outcomes. The school performance data we use is available for a representative subset of the population.

Our main identification strategy is based on having the exact date of birth of each child in the sample, and the exact opening or extension date for each institution. Combining these

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].

variables, we develop a difference-in-discontinuities design [cf. Grembi et al., 2016], where openings and extensions happening in surrounding months serve as a control group for each opening. Our estimates are therefore robust to any confounding factor coinciding with the opening, as long as it is not unique to that particular opening. In addition, we exploit having data on place of delivery at the individual level to study selection. Combining our difference-in-discontinuities estimator with a method proposed by Black et al. [2017], we can characterise subpopulations that responded differently to the policies we study. In a supplementary analysis using annual data on home births, we estimate a dynamic two-way fixed effects model [De Chaisemartin and d'Haultfoeuille, 2020] in order to see how the removal of a large number of births affects birth outcomes among the remaining home births.[3]

Thanks to rich administrative data on a number of characteristics and outcomes for the entire population, and a design exploiting two experiments, we can make a number of contributions to the literature. First, we contribute to the large literature on the importance of early life interventions,[4] considering a hitherto under-studied intervention, which appears to be responsible for a large part of the observed improvements in longevity. Our estimates show that the transition to institutional delivery led to a substantial reduction in child mortality: a reduction by 4-5 percentage points appears during the first week of life and remains persistent until at least age 70. The treated children also exhibit large gains in educational attainment and labour market outcomes: the propensity to take secondary schooling increases by 11 percentage points and earnings by 7.6 per cent. Thereby, the effects of hospital deliveries appear to compare favourably to many other early life interventions during the same period, such as e.g. preventive care services for infants [cf. Bhalotra et al., 2017, Hjort et al., 2017, Bütikofer et al., 2019].

In this part, our paper corroborates some findings from a recent paper by Lazuka [2020], which studies the opening of health centres in Sweden, and associates them with improved adult outcomes. With less than 10% of institutionalized births, health centers were of limited importance quantitatively compared to hospitals. Using a different empirical approach, our study relies on arguably more relevant large-scale hospitals and birth clinics which covered

[3]As a further methodological contribution, we propose a method for constructing catchment areas, as a way of working around the problem that the recorded parish of birth corresponds to the parish of delivery (not residence). See Section 3.2 and Appendix A for details.

[4]This literature includes a wide range of policy instruments, such as cash transfers to poor families [Akee et al., 2013, Hoynes et al., 2016], parental leave policies [Carneiro et al., 2015], childcare [Campbell et al., 2014, Carneiro and Ginja, 2014], and access to trained midwives [Lorentzon and Pettersson-Lidbom, 2021, Anderson et al., 2020, Kotsadam et al., 2021, Pettersson-Lidbom, 2014].

the majority of institutionalized deliveries. Thus, our study complements the paper by Lazuka [2020] and also delivers results for a sample which is more representative for the universe of hospital births. In general, our results highlight the importance of access to medical resources around delivery, which is important, given that the previous literature is somewhat inconclusive.[5]

Second, we contribute to the considerably smaller literature on the "missing middle", aiming at tracing improved adult outcomes back into childhood [Almond et al., 2018]. Using data on primary school performance, we find that the improvements in human capital outcomes can be traced back to early life: treated children performed better in school on average, and this improvement was driven in particular by the left tail. Treated children also missed fewer days in school due to sickness absence. These results are striking in comparison with a previous study, which used the same data to evaluate a preventive care programme for infants. That paper also noted significant, albeit smaller, gains in school performance; however, they were to a much greater extent concentrated in higher quantiles and not in the left tail [Bhalotra et al., 2021].

Taken together, these results suggest that beyond the substantial mortality effects, treated children experienced a reduction in morbidity, which also manifested itself in cognitive abilities and human capital. This finding is corroborated by our result that being delivered in hospital reduced the risk of claiming a disability pension later in life. We posit that the crucial mechanism may be that hospitals were better equipped to handle complications that potentially harm babies for life. We are able to show that the opening or extension of a hospital increases the prevalence of medical procedures at childbirth by 3 percentage points in a region, whereas the home births become less complicated and risky on average after a maternity ward is established or expanded.

Moreover, we 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

[5] A seminal paper by [Bharadwaj et al., 2013] evaluates the effects of intensive medical care for infants of very low birth weight. They report significant reductions in infant mortality rates, but also substantial effects on wages. Evidence regarding low-risk newborns is scarce and results are quite mixed – some studies report that more resources do not affect infant health outcomes [Almond and Doyle, 2011, Carrillo and Feres, 2019] whereas promoting midwives has been found to be associated with reductions in neonatal mortality [Miller, 2006]. Of particular relevance are a several papers by Daysal et al., who show that medical interventions for low-risk births are associated with significant reductions in mortality rates [Daysal et al., 2015, 2019].

[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. We also find that the abolition of user charges in 1938 leads to a slight change in these selection patterns: when hospital deliveries became free of charge, there was an increase selection of risky births, and possibly high-SES mothers were crowded out by low-SES mothers. This changed selection pattern was in fact strong enough to leave an impact on infant and child mortality.

By studying the 1938 transfer programme which made hospital delivery affordable to all mothers, we thus also contribute to the literature analysing the effects of health insurance coverage at birth, which typically considers Medicaid expansions [Wherry and Meyer, 2016, Miller and Wherry, 2019, Wherry et al., 2018, Brown et al., 2015]. These studies consistently report positive effects of eligibility on a diverse set of outcomes: e.g. college attendance, earnings, mortality, and obesity. Our contribution in this part is to show that it is not necessary to increase total resources in order to reap some of these gains: by levelling the playing field, the expansion of health insurance allows for a more efficient selection based on medical need.

Our results are robust to a number of robustness checks and alternative specifications. In order to assess the implications of the above-mentioned problem with selective 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.

Our main conclusion is that the transition to hospital delivery appears to have been one key component in the remarkable reduction in infant mortality rates that occurred during 1920-50 period. Infant mortality rates declined from 5.9 to 2.7 per cent between the 1920s and the 1940s [Statistics Sweden, 1939, 1955]. The gains in infant mortality that we estimate are of similar size; however, the numbers are not fully comparable since we also find clear evidence that compliers were negatively selected with regard to survival chances. Moreover, the transition appears to have contributed to large gains in education and labour market outcomes. On the other hand, it remains an open issue whether the transition to institutional delivery also reduced socioeconomic inequalities. The improved survival chances apply uniformly across socioeconomic groups, whereas the improved human capital outcomes are concentrated in better-off families. This heterogeneity highlights the potential importance of parental investments and opportunities as complements to public policies.

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 anlyses the 1938 reform abolishing charges and Section 6 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.

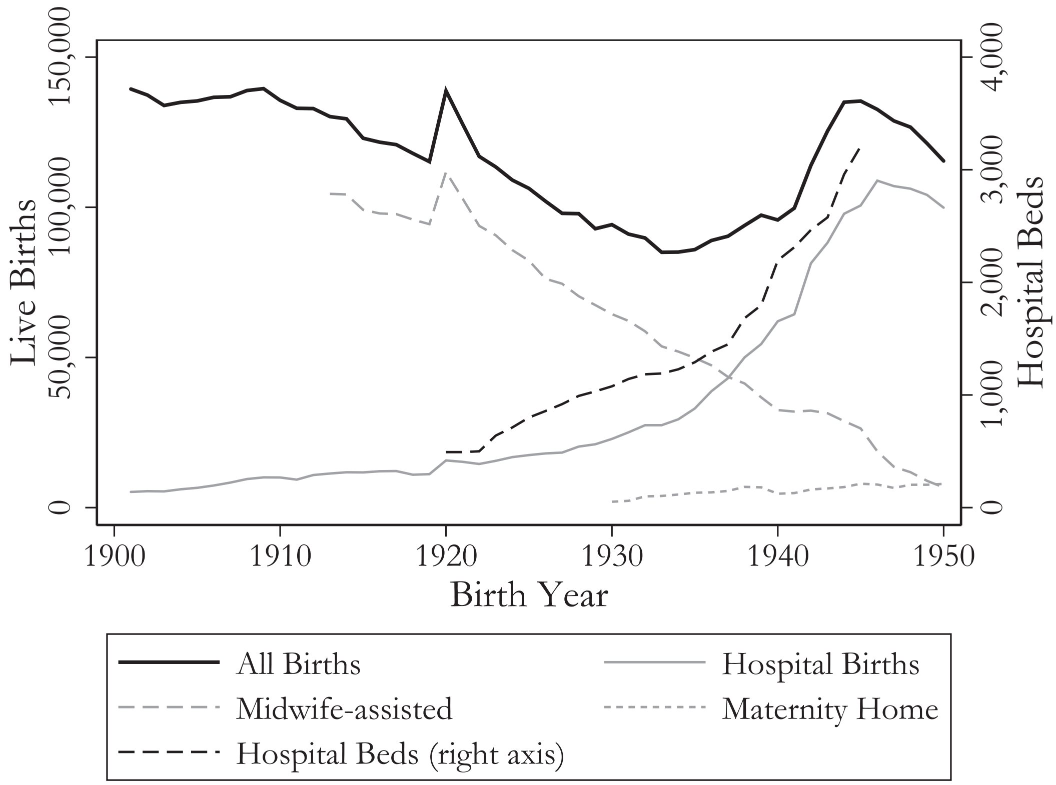


Figure 1: Live Births and Hospital Beds in Sweden, 1901–50.

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.[6] 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].[7] 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 institutions 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 .

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

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

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.

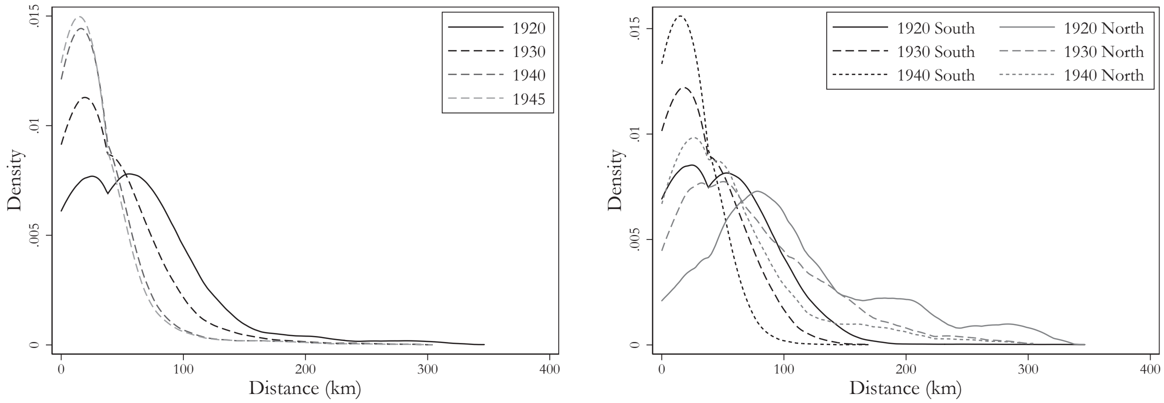
The public debates surrounding the expansion of the hospital sector and the mode of delivery also paid considerable attention to expectant mothers' preferences and views. In the early phase this discussion centered more around the question of why women chose home births instead of hospital deliveries. 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].[8] 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.

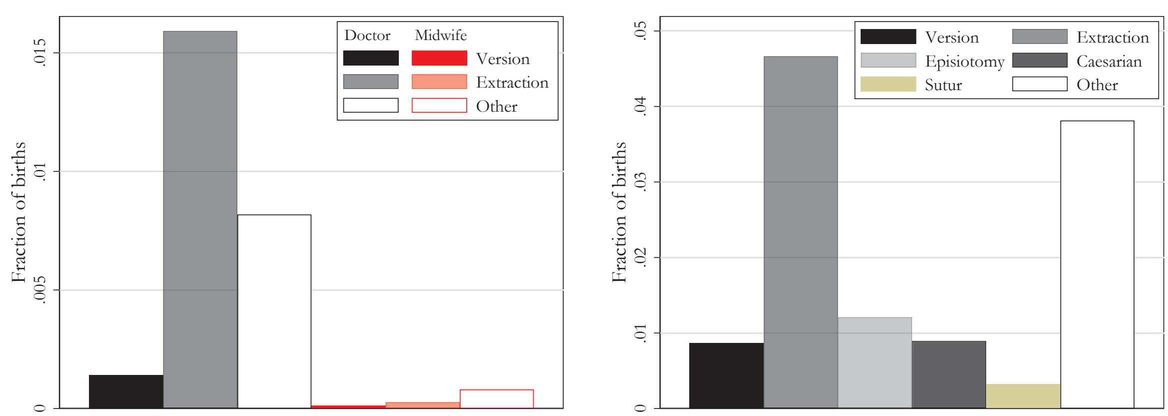
2.3 Quality of Maternal Care

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-

[8] 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.[9] 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).

[9] 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 four administrative sources and consists of individuals born between 1924 and 1946. 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.[10] 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 the Census took place. With respect to labor force participation, persons are classified as economically active if they reported themselves as gainfully employed.[11]

Income statistics stem from official tax returns and are considered as highly accurate.[12] 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.[13]

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

[10] 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].

[11] 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.

[12] 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.

[13] 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.

sample ($N = 184,176$). The source of this information are the birth registers kept at every maternity ward [Swedish Tax Agency, 1989].

Table 1 presents summary statistics for our main 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.

Table 1: 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.

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]. Finally, in order to better assess potential long-term consequences we use information on receiving disability pension from the Swedish Interdisciplinary Panel (SIP)[14] which links

[14]The SIP is administered at the Centre for Economic Demography, Lund University, Sweden, and approved by the *Lund University Regional Ethics Committee, DNR 2013/288*.

multiple Swedish population registers. Information on disability pensions stems from income and tax register and is available for the years 1981-2011. The SIP baseline population consists of the total population born in Sweden 1930 – 1985 and parents if a linkage in the Swedish Multigenerational Register exists.[15] Based on annual receipt of disability pension, we construct for each year a binary indicator if a person receives a disability pension.

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.

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.

[15] By construction, cohorts born before 1930 are only included if they had children and a linkage in the Multigenerational Register is available leading to a selected sample. Furthermore, information on mortality in SIP is available only from 1961 onward. Neonatal and infant mortality is missing for the cohorts of interest. As our intervention data includes openings/extensions prior to 1930 and neonatal mortality is a major outcome, we utilise SIP only for the analysis of disability pensions.

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.[16] All extensions and openings have been 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.[17] 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).

[16]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.

[17]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].

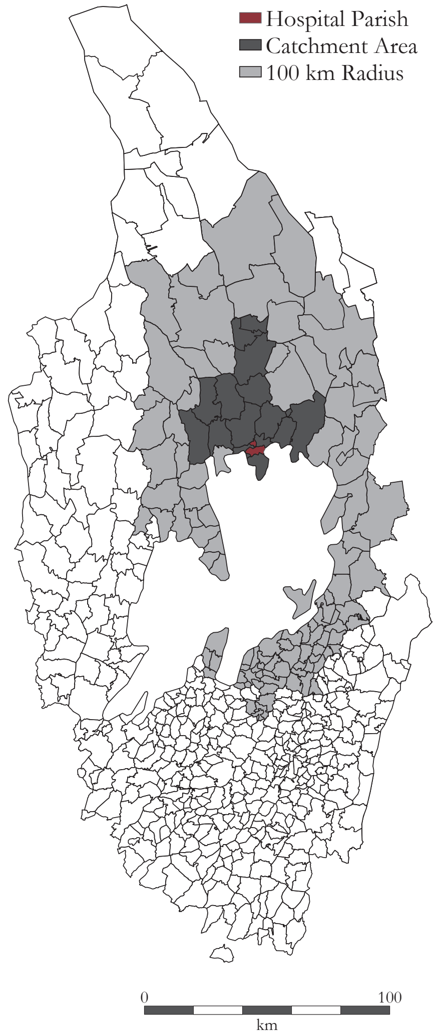


Figure 4: Hospital Catchment area in *Karlstad* 1937.

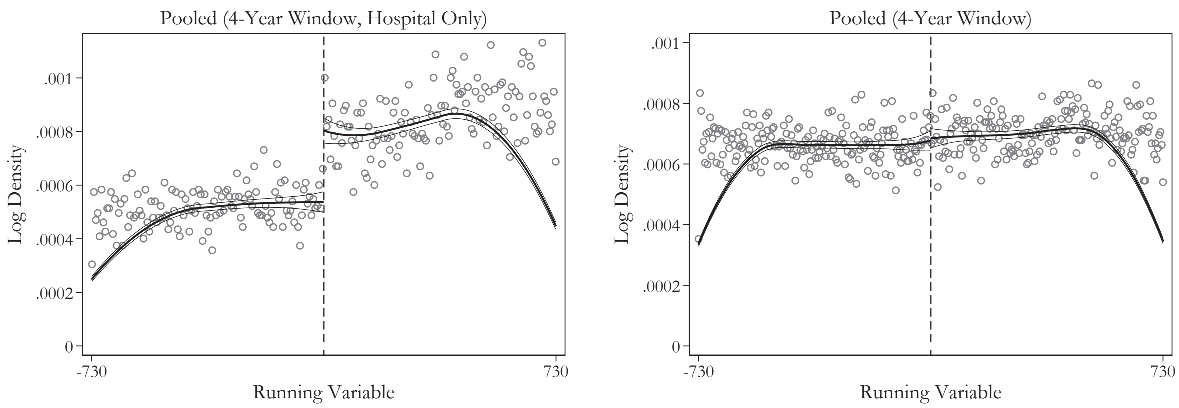
Note: Own calculations based on the 1950 population census [Statistics Sweden, 1952] and the Swedish Death Index [Federation of Swedish Genealogical Societies, 2014]; catchment area definitions provided in Appendix A.

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.[18] In Figure 5 we show the outcome of such a test for the pooled sample of hospital

[18]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.

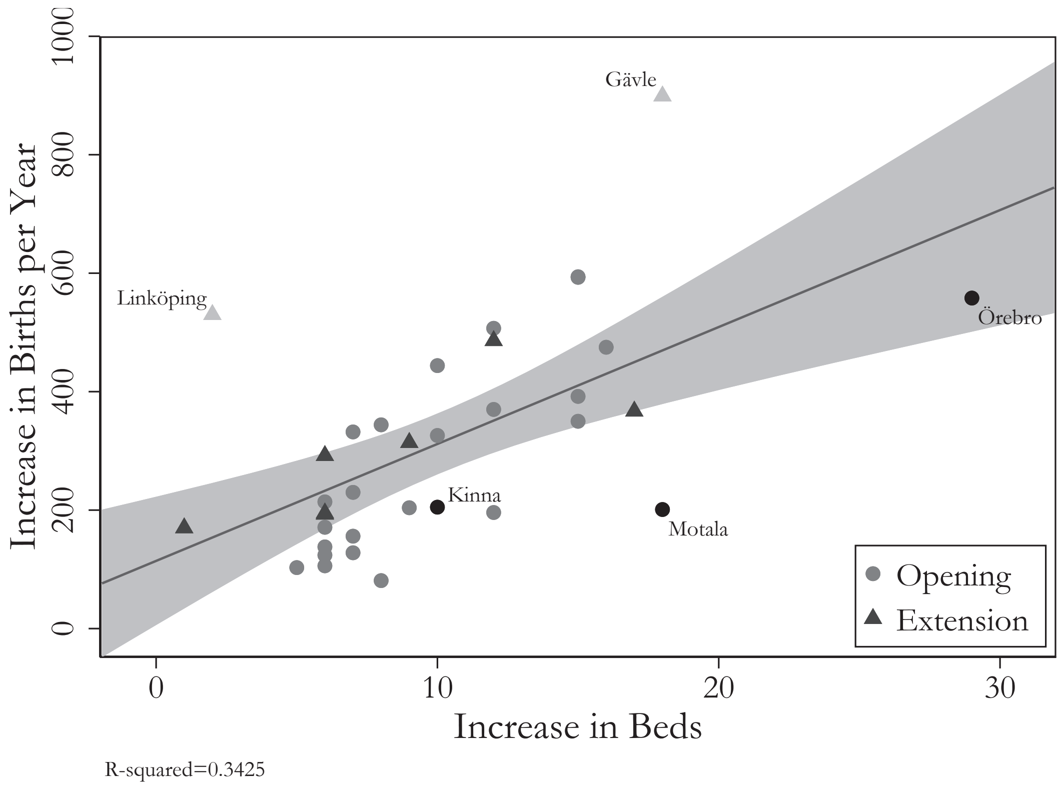


Figure 6: Examples of Discontinuities used in the Analysis.

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

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

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

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

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,[19] one such confounder would be school starting age. However, we control for month of birth in all regressions;

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

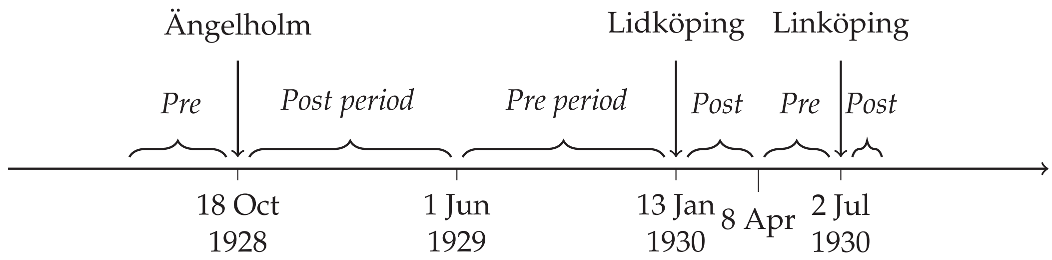


Figure 7: Difference-in-Discontinuities Strategy.

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:

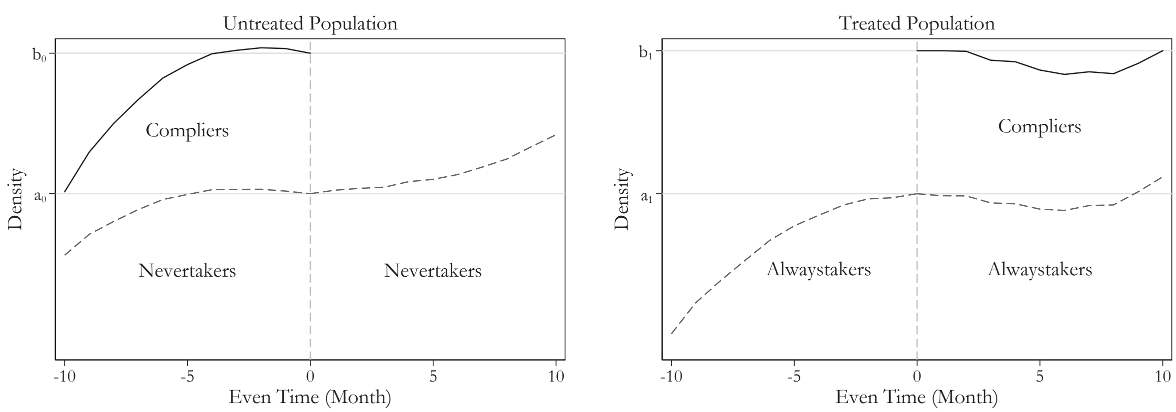
$$\begin{aligned} \tau_j = & \lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \in \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \in \mathcal{I}_j\right] \\ & - \left[\lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \notin \mathcal{I}_j\right]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

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 Analysis of Selection

In order to understand effects of the supply-side shocks we consider, 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]. The simple logic 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.



(a) **(b)**

Figure 8: Selection Analysis.

Consider first the subpopulation of individuals who were *not* born in hospital, displayed in Figure 8a. None of these individuals were treated, and hence, any change in outcomes or in control variables within this group which coincides with the intervention must be a result of selection. In Appendix D we show if the identifying assumptions hold, running the RDD or DIDIsc analysis on any such variable X within the untreated sample, will estimate the difference between compliers and never-takers, scaled by the proportion of nevertakers in the untreated population:

$$\tau = \lim_{d \downarrow 0} \left(\mathbb{E}\left[Y \mid Nevertaker, t_i < R < t_i + d\right] - \mathbb{E}\left[Y \mid Complier, t_i < R < t_i + d\right] \right) \cdot \frac{b_0 - a_0}{b_0}. \quad (4)$$

where a_0 and b_0 represent the densities of births around the cutoff (cf. Figure 8a). Thus, in the sample of home births, the discontinuity estimated by RDD or DIDisc is the difference between *compliers* and *never-takers*, scaled by the proportion of compliers relative to compliers and never-takers ($\frac{b_0-a_0}{b_0}$).[20] In order to get estimates that represent the differences in means between compliers and nevertakers, we rescale the estimate by factor $\frac{b_0}{b_0-a_0}$. Statistical inference is based on the delta method.

We also conduct the same tests within the supbpopulation of treated individuals. However, in this subpopulation only the baseline covariates provide direct information on selection, whereas exposing outcome variables to the test would test the joint hypothesis of selection and treatment effect heterogeneity [cf. Kowalski, 2016]. Therefore, we only report results on background characteristics for this sample.

This proposed test in fact becomes even more useful in the absence of a first-stage effect. Whenever there is no net effect on the propensity to deliver in hospital at the cutoff, but a suspicion that there is selection – which in this case requires defiers – the estimated selection effect will characterise how compliers differ from defiers.

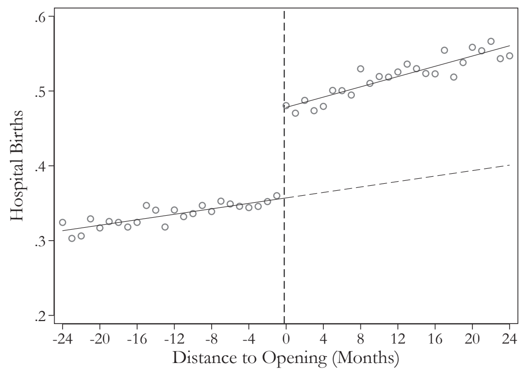
4 Supply-Side Expansions

4.1 Descriptive Evidence

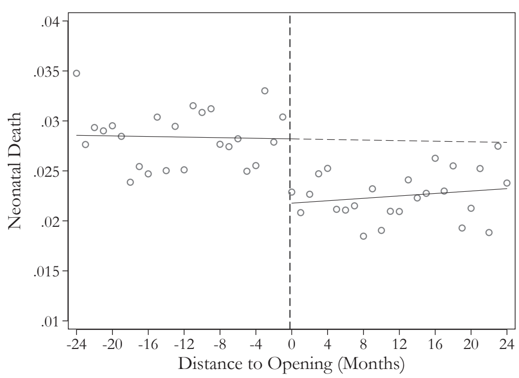
Before turning to regression results, we present a number of binscatter plots for some key outcomes: the treatment variable, neonatal mortality, and secondary schooling completion. The latter represents the most important schooling decision for the cohorts included in the analysis: some 15-20 per cent of each cohort decided at age 10 or 13 to take secondary schooling [cf. Fischer et al., 2020]. Figure 9 shows how these three variables evolve in treated regions around the cutoff, represented by a vertical line. In order to assess the extent to which the intervention had an impact on trends in these variables, we fit trends on each side of the cutoff, and also extrapolate the pre-treatment trends into the post-treatment period. Regarding *hospital births*, depicted in Figure 9a, it becomes clear that the intervention represents a distinct discontinuity in an otherwise approximately linear upward trend. The discontinuity corresponds to an increase in hospital births by around 16 percentage points. For *neonatal mortality* the visual

[20] We thus assume there are no *defiers*, i.e. mothers who reduce their propensity to give birth in hospital after the intervention.

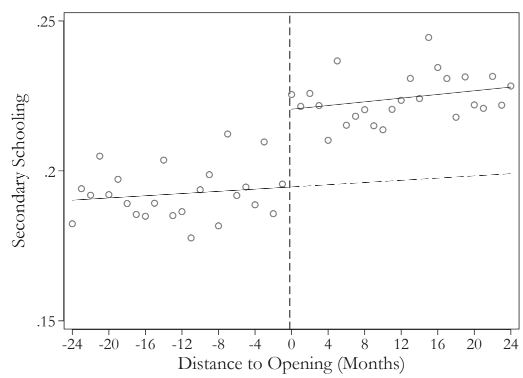
evidence suggests that trends shifts downward in the months following the intervention. For *secondary schooling completion*, there is again a discontinuity at the cutoff, roughly one percentage point in magnitude. Thus, this first evidence suggests that there is a clear first stage and that the intervention may have had a positive effect on education and survival chances.



(a)



(b)



(c)

Figure 9: Binscatter Plot: (**a**) Born in Hospital; (**b**) Neonatal Death ; (**c**) Secondary School

Notes: Figures give binned scatter plots with fixed 30-day bins. The data is re-centered around the extension/opening of a maternity ward. Linear fit added separably on both sides of the cutoff. The dashed line presents a prediction fitted on data before extension/opening. Cohorts 1924–1946. Number of maternity wards included: 51
Source: Census 1950, Census 1970, Swedish Death Register. Own calculations.

4.2 First Stage

Table 3 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 3: 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.

4.3 Main Results

Table 4 presents our main results for child survival and socio-economic outcomes. The first two columns of estimates present RDD results and the following two columns present DIDisc results. We present reduced-form estimates and also IV estimates for each design. The top panel presents the estimated effects on mortality outcomes. According to both designs, being born in hospital is associated with a large and significant reduction in mortality. The DIDisc estimates are somewhat smaller throughout, but with the exception of 'Death before Age 50' they are not significantly different from the RDD estimates. The DIDisc results suggest that the hospital delivery reduces neonatal mortality by 4.5 percentage points. The estimated effect increases slightly as we expand the time window, but it remains quite stable at around 8 percentage points by age 70. This suggests that the marginal life that was saved due to hospital delivery was not negatively selected with regard to health.

The lower panel presents estimates for four socioeconomic outcomes: secondary schooling completion, years of education, 1970 earnings, and disability pensions. For the education measures, the two estimation techniques deliver fairly similar results: the LATE of being born in hospital on secondary schooling completion is around 11 percentage points according to our preferred DIDisc estimate. Considering a baseline level of 19 per cent, this is a very large effect. For these cohorts, secondary schooling would increase educational attainment by 2–3 years – so that our 0.11 estimate would imply 0.2-0.3 additional years of schooling on average. The estimated effects on years of education are in line with this; the somewhat larger point estimates suggest there may have been effects on upper secondary and tertiary education as well, or on vocational training. Our results suggest that the intervention was associated with gains in earnings. The DIDisc estimate corresponds to a 11.6 percent increase in earnings. The effect on the probability of receiving a disability pension is 4.4 percentage points, from a baseline of 20.4 per cent.

All the results presented in Table 4 are robust to adjustments for multiple hypothesis testing: see p values provided in square brackets below each estimate [Romano and Wolf, 2005].

Table 4: Regression Results

	Baseline	RDD (non-parametric) Reduced Form	2SLS	Difference-in-Discontinuities Reduced Form	2SLS
Mortality					
Neonatal Death (Age < 1 Month)	0.028	-0.007**	-0.041**	-0.007***	-0.045***
		(0.003)	(0.017)	(0.002)	(0.009)
		[0.056]	[0.051]	[0.001]	[0.001]
Infant Death (Age < 1 Year)	0.052	-0.021***	-0.135***	-0.009***	-0.057***
		(0.006)	(0.037)	(0.002)	(0.013)
		[0.004]	[0.006]	[0.002]	[0.001]
Child Death (Age < 5 Years)	0.064	-0.016**	-0.107***	-0.009***	-0.055***
		(0.006)	(0.041)	(0.002)	(0.014)
		[0.056]	[0.051]	[0.003]	[0.001]
Death before Age 50	0.116	-0.024***	-0.153***	-0.011***	-0.067***
		(0.007)	(0.049)	(0.003)	(0.018)
		[0.016]	[0.010]	[0.005]	[0.002]
Death before Age 70	0.249	-0.022**	-0.124**	-0.014***	-0.087***
		(0.008)	(0.049)	(0.004)	(0.025)
		[0.056]	[0.051]	[0.005]	[0.002]
Socio-economic Outcomes					
Secondary School	0.192	0.031***	0.175***	0.018***	0.113***
		(0.009)	(0.051)	(0.004)	(0.026)
		[0.005]	[0.008]	[0.002]	[0.001]
Years of Education	8.629	0.183***	1.084***	0.101***	0.620***
		(0.050)	(0.295)	(0.024)	(0.145)
		[0.004]	[0.006]	[0.002]	[0.001]
Earnings 1970	20,591	677.192**	3851.139**	256.518*	1581.901*
		(286.757)	(1639.264)	(142.183)	(874.487)
		[0.056]	[0.051]	[0.082]	[0.091]
Disability Pension†	0.204	-0.007	-0.062	-0.006**	-0.044**
		(0.008)	(0.074)	(0.003)	(0.022)
		[1.000]	[1.000]	[0.312]	[0.336]
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. p values given by multiple testing adjustment [Romano and Wolf, 2005] († Bonferroni for disability pensions) are provided in square brackets. RDD Bandwidth is data-driven. DIDisc sample covers a period of 4 years around the opening (6 years for disability pensions). 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. Probability of receiving a disability pension is based on annual tax records 1981–2011; specifications additionally include tax year fixed effects. Number of extensions/opening maternity wards included: 51.
Source: Census 1950/1970, Swedish Death Register, SIP. Own calculations.

4.4 Mechanisms and Selection

We have found that a hospital delivery is associated with a number of benefits for the affected children: neonatal mortality is reduced by several percentage points, and educational attainment is increased. It is however unclear what the mechanism giving rise to these changes might be. We now try to shed some light on this.

Mortality. The average length of stay was 10 days, and the individual-level variation around this mean was limited: 98 per cent of mothers would stay in hospital for at least 7 days (cf. Appendix C.2). Hence, the estimated effects on mortality may be either related to complications arising at birth, or to the fact that after birth, the child is in a safe environment with direct access to health care staff for several days. In order to discriminate between these two alternative stories, we estimated the effect of hospital delivery on mortality for each individual day after birth. Results are provided in Figure 10. For day 1, 2 and 3, we estimate a reduction in mortality around 0.3 to 1 percentage points. After that, the effect vanishes quickly.

In other words, the protective effect of the hospital environment is mainly visible during the first three days after birth. This in turn suggests that the effect we observe is directly linked to delivery and not to health problems occurring later. It also suggests that the information disseminated to mothers regarding e.g. breastfeeding may have been of secondary importance for the bulk of the mortality effect.

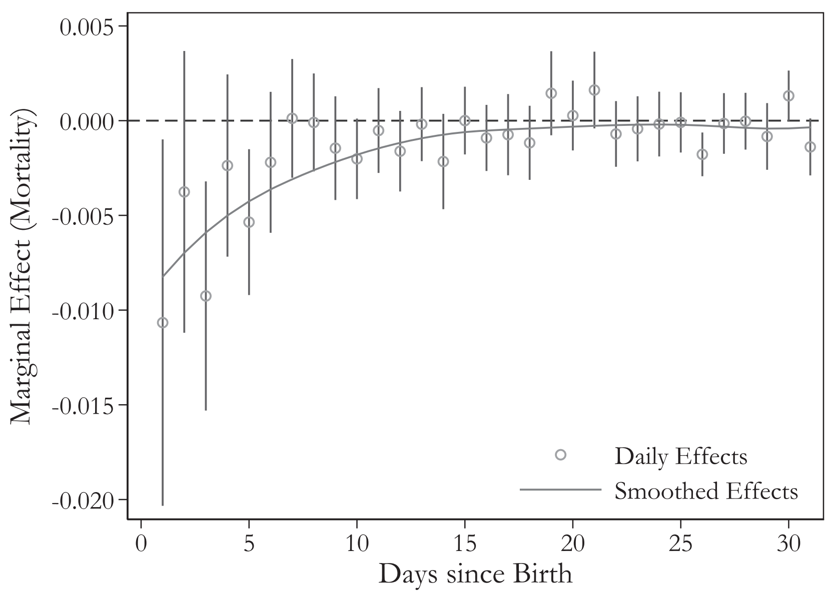


Figure 10: Daily Neonatal Mortality.

Notes: Figure shows effects daily mortality for the first 30 days of life. Results are based on separate difference-in-discontinuities models with a binary indicator for death at day $t \in 1, \ldots, 30$ as dependent variable. Estimates represent the second stage/treatment effect. 95% are CI based on robust standard errors clustered at hospital level. A local polynomial fit is added using the estimated 2SLS point estimates. Number of extensions/opening maternity wards included: 51
Source: Census 1950, Swedish Death Register. Own calculations.

Thus, the estimation of day-specific mortality effect reinforced the previous finding that survival chances improve early in life and then remain relatively flat: we could now narrow down the time frame to the first days of life.

Human Capital. The fact that we see a substantial increase in secondary schooling completion suggests that the hospital expansions improved the cognitive abilities of children. 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 5 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 5: Regression Results: School grades

	Baseline	RDD (non-parametric) Reduced Form	2SLS	Difference-in-Discontinuities 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 socioeconomic outcomes, in particular educational attainment, 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 11 we present those estimated effects together with their 90 and 95 percent confidence intervals. In order to avoid putting too much emphasis on outliers, plot results showing Γ in quantiles instead of absolute values.

The results in Figure 11 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.

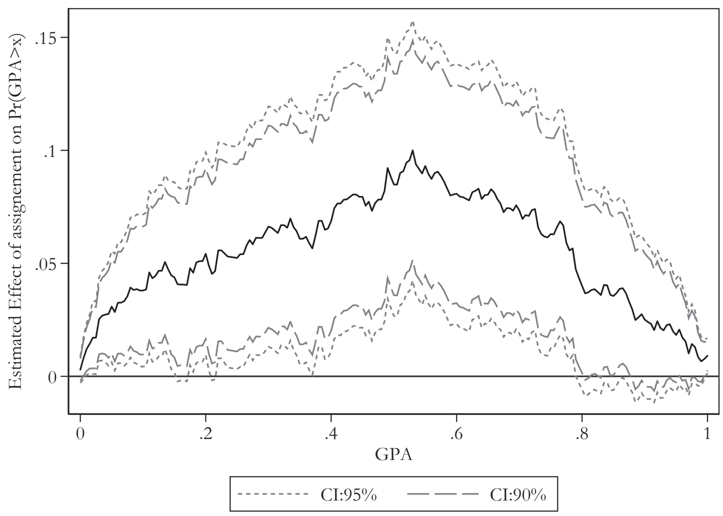


Figure 11: Distribution Regression Plot: Grade Point Average

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.

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.[21] 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.[22]

Results in Table 6 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 6. 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 7. 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 complications were possible to detect early in pregnancy with medical assistance. Event study graphs in Figure 12 suggest there were no anticipation effects and a clear alignment of reduction

[21] 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].

[22] 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 6: Hospital Opening/Extension on Midwife Births

	Outcomes				
		Births		Mother ill /	Twins
	with Midwife	with Procedures	with Complications	diseased	
Hospital Opening	-0.213***	-0.012***	-0.002**	-0.008***	-0.001
	(0.058)	(0.003)	(0.001)	(0.003)	(0.001)
Mean Dep. Var	0.876	0.039	0.005	0.015	0.014
Relative Effect	-0.243	-0.319	-0.509	-0.490	-0.089
N	866,536	866,423	866,423	866,423	866,423
Health Districts	441	441	441	441	441
Robust Effect (Dynamic 2-Way FE)	-0.167	-0.016	-0.003	-0.014	0.000
SE Robust Effect	(0.057)	(0.006)	(0.002)	(0.008)	(0.003)

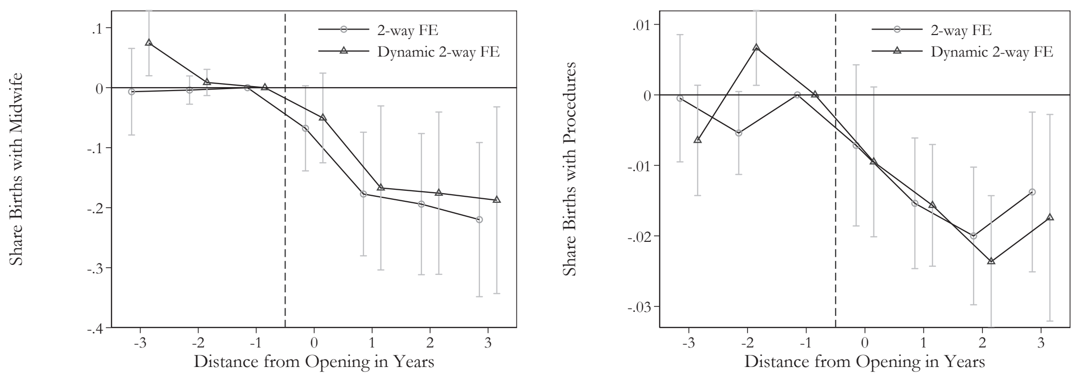
Notes: Table shows effects of the a hospital opening or extension in a given health district. Estimation are based on a standard difference-in-differences regression on aggregated data on health district level controlling for year and health district FE. Results cover all health districts 1928–1938. 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.

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.

Table 7: Hospital Opening/Extension on Complications in Midwife Births

	Complications			
	Preventable			Other
	Eclampsia	Cephalo-pelvic disproportion	Placenta praevia	Uterine rupture
Hospital Opening	-0.0006	-0.0012**	-0.0006**	0.0000
	(0.0004)	(0.0005)	(0.0002)	(0.0000)
Mean Dep. Var	0.0019	0.0015	0.0012	0.0001
Relative Effect	-0.296	-0.834	-0.490	0.353
N	866,423	866,423	866,423	866,423
Health Districts	441	441	441	441
Robust Effect (Dynamic 2-Way FE)	-0.0004	-0.0024	-0.0006	0.0000
SE Robust Effect	(0.0007)	(0.0016)	(0.0004)	(0.0000)

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.



(a) **(b)**

Figure 12: 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.

Selection. The previous analysis highlights the avoidance of harmful complications at birth as one potential mechanism behind the improvements in human capital and labour market 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 3.3.3. 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 8. 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 8: 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)			
Socio-economic Outcomes						
Secondary School	0.166	0.033	0.016			
		(0.023)	(0.021)			
Earnings 1970	20,040	-813.302	-352.538			
		(996.554)	(887.128)			
Years of Education	8.465	0.195	0.117			
		(0.126)	(0.114)			
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)
Family Employment Background						
Farmer	0.342	-0.171***	-0.186***	0.213	0.020	0.019
		(0.038)	(0.036)		(0.026)	(0.022)
Manager	0.006	0.006	0.004	0.009	0.006	0.010*
		(0.006)	(0.006)		(0.007)	(0.006)
White-collar worker	0.083	0.044*	0.037*	0.133	0.007	0.024
		(0.024)	(0.022)		(0.021)	(0.017)
Industrial worker	0.424	0.142***	0.152***	0.491	-0.016	-0.016
		(0.041)	(0.038)		(0.031)	(0.026)
Other	0.146	-0.021	-0.006	0.154	-0.016	-0.036**
		(0.031)	(0.029)		(0.021)	(0.018)
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.

4.5 Effect Heterogeneity

Next, we turn to an analysis of effect heterogeneity. We consider three different dimensions; first, we split the sample based on surname types and contrast patronymics with other surnames. Second, we do the analysis by hospital type, and third, we compare effects of openings and extensions. Since the previous analysis has shown that the mortality effect is largely driven by neonatal mortality, we suppress the outcomes representing mortality over longer time horizons in what follows.

Surnames indicating SES. As previously mentioned, patronymic surnames may be seen as a proxy of low parental SES [cf. Clark, 2012], and is the only indicator of SES that is available for all births, even those that do not survive the neonatal period. We present results split by this indicator in Table 9. The baseline levels of the socioeconomic outcomes clearly support the notion that children with patronymics are from a lower SES background on average, with particularly notable differences in educational attainment.

The first columns in Table 9 show estimates (reduced form and IV) for individuals with patronymics, and the following columns show results for all other individuals (who thus have a higher SES background on average). Apparently, the two groups hardly differ in their propensity to use the services, even though the patronymic group has lower utilisation at baseline.[23]

Turning to the main outcomes, it is clear that there are no relevant differences in the effect on mortality: the two estimates of 5.0 and 4.1 percentage points effects are not significantly different from each other. For the socioeconomic outcomes, on the other hand, there are striking differences between the two groups: the high-SES group has an effect on secondary schooling which is more than twice as large, and this difference is statistically significant. It is reflected in the high-SES group getting almost a full year of additional education on average, whereas the low-SES group increases education by only a third of a year. It is also reflected in the estimated effects on earnings, which increase only in the high-SES group, and do so quite substantially.

[23] This finding is consistent with the result in Table 8 which shows that compliers are less likely than never-takers but more likely than always-takers to have patronymic surnames.

Table 9: Heterogeneity: Surnames (DiDisc)

	Patronymic			Other		
First Stage						
Born in Hospital	0.199	0.153***		0.260	0.170***	
		(0.006)			(0.007)	
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Mortality						
Neonatal Death (Age < 1 Month)	0.029	-0.008***	-0.050***	0.028	-0.007***	-0.041***
		(0.002)	(0.013)		(0.002)	(0.013)
Socio-economic Outcomes						
Secondary School	0.154	0.011**	0.068**	0.242	0.029***	0.172***
		(0.005)	(0.034)		(0.007)	(0.042)
Years of Education	8.380	0.055*	0.355*	8.952	0.161***	0.956***
		(0.029)	(0.185)		(0.040)	(0.239)
Earnings 1970	19,680	-5.522	-35.490	21,771	916.269***	5403.937***
		(222.413)	(1426.637)		(281.349)	(1652.420)
Disability Pension	0.196	-0.006	-0.049	0.205	-0.003	-0.027
		(0.004)	(0.036)		(0.003)	(0.041)
Observations		79,222			61,935	

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. DIDisc sample covers a period of 4 years around the opening. 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. Probability of receiving a disability pension is based on annual tax records 1981–2011. Specifications additionally include tax year fixed effects. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register, SIP. Own calculations.

The estimate in this part thus highlight the critical role of parental background for the realisation of gains in the socioeconomic domain. The striking heterogeneity may be due to either differential parental responses to this large and positive shock, or to different opportunities in life facing individuals from different backgrounds.

Facility type. Next we allow for effect heterogeneity by type of institution. In Appendix C we characterise the equipment and procedures that were available in the hospitals of the time, and show that they compare quite favourably on many indicators to today's hospitals in low-income settings. The other institutions we consider were less well-equipped in general: their physicians would not necessarily be trained obstetricians and they would not have access to the same technology (e.g. surgical care, laboratories). Therefore it is of great interest if there is a dose-response relationship in the sense that the hospitals with their superior equipment would deliver better results. In Table 10 we make that comparison between hospitals and health centers. Interestingly, there are no remarkable differences between the types of institution; both lead to similar reductions in mortality and improvements in educational attainment and earnings. Only for disability pension there is a difference in the effects of hospitals and health centres, suggesting that hospitals more effectively protect against disability.

Table 10: Heterogeneity: Facility Type (DiDisc)

	Hospital			**Health Center**		
First Stage						
Born in Hospital	0.199	0.154***		0.260	0.226***	
		(0.005)			(0.016)	
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Mortality						
Neonatal Death (Age $<$ 1 Month)	0.028	-0.006***	-0.040***	0.033	-0.012*	-0.052*
		(0.002)	(0.011)		(0.006)	(0.027)
Socio-economic Outcomes						
Secondary School	0.194	0.021***	0.138***	0.184	0.038**	0.171**
		(0.005)	(0.030)		(0.016)	(0.069)
Years of Education	8.641	0.131***	0.843***	8.558	0.212**	0.955**
		(0.027)	(0.170)		(0.087)	(0.386)
Earnings 1970	20,659	469.022**	3026.278**	20,196	682.661	3044.988
		(197.466)	(1269.131)		(612.948)	(2713.372)
Disability Pension	0.202	-0.007*	-0.065*	0.190	0.010	0.050
		(0.004)	(0.038)		(0.009)	(0.043)
Observations		119,680			21,477	
Maternity Wards		38			13	

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. DIDisc sample covers a period of 4 years around the opening. 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. The treatment variable represents hospital openings or expansions. Probability of receiving a disability pension is based on annual tax records 1981–2011. Specifications additionally include tax year fixed effects. Number of extensions/opening maternity wards included: 51
Source: Census 1950, Swedish Death Register, SIP. Own calculations.

Openings versus extensions. As a final heterogeneity analysis, we check whether it matters if the shift in supply comes from a hospital opening or from the expansion of an existing maternity ward. We hypothesise that the effect differences may be in either direction: if maternity wards learn over time to perform their tasks better, we would expect extensions to lead to larger gains than openings. If on the other hand extensions lead to less resources per delivery, the differences could be in the other direction. Our estimates are presented in Table 11, with openings in the two leftmost columns and extensions in the two rightmost ones. Again, there are no striking differences in effects between the two types of opening.

Table 11: Heterogeneity: Hospital Openings and Extensions (DiDisc)

	Openings			Extensions		
First Stage						
Born in Hospital	0.189	0.188***		0.280	0.119***	
		(0.006)			(0.009)	
	Baseline	**RF**	**2SLS**	**Baseline**	**RF**	**2SLS**
Mortality						
Neonatal Death (Age < 1 Month)	0.029	-0.008***	-0.042***	0.028	-0.007**	-0.056**
		(0.002)	(0.012)		(0.003)	(0.025)
Socio-economic Outcomes						
Secondary School	0.195	0.022***	0.119***	0.188	0.012	0.096
		(0.006)	(0.032)		(0.008)	(0.061)
Years of Education	8.670	0.122***	0.658***	8.571	0.160***	1.250***
		(0.033)	(0.176)		(0.046)	(0.361)
Earnings 1970	20,530	552.125**	2981.307**	20,675	668.823*	5262.754*
		(249.750)	(1342.198)		(353.299)	(2804.347)
Disability Pension	0.203	-0.007	-0.098	0.198	-0.001	-0.007
		(0.006)	(0.089)		(0.005)	(0.032)
Observations		84,898			56,259	
Maternity Wards		33			18	

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. DIDisc sample covers a period of 4 years around the opening. 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. Probability of receiving a disability pension is based on annual tax records 1981–2011. Specifications additionally include tax year fixed effects. Number of extensions/opening maternity wards included: 51.
Source: Census 1950, Swedish Death Register, SIP. Own calculations.

4.6 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.[24] 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 9 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, effect 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.[25] In order to test the robustness of results to this reform, we ran a series of regressions, where we leave out

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

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

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.

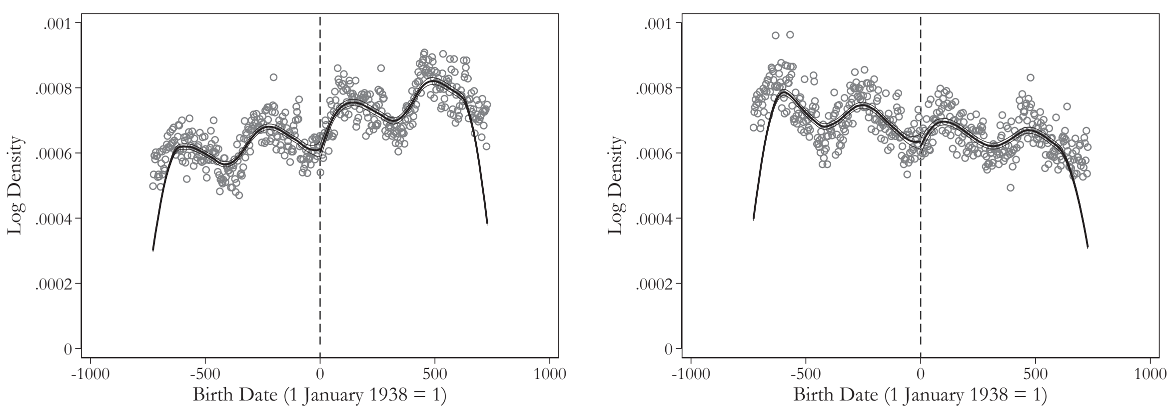
Since the DiDisc specification uses a bandwidth of 730 days we evaluate whether our results are sensitive to this specific choice. In Figure F.1 we report effect estimates for different bandwidths ranging from 90 to 730 days. Those estimates show that our main estimates for the DiDisc design are robust 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.

5 The 1938 Reform

Next, we evaluate the impact of the 1938 reform that abolished charges for hospital delivery. It has been mentioned already that these charges could be substantial; possibly even more than 10 per cent of an ordinary worker's monthly earnings. We posit that the abolishment of fees may have had two effects on utilisation: it may have increased the general propensity to give birth in hospital, and it may have changed the composition of hospital births. Some descriptives provided in section 3.2 suggest that there may be limited scope for the first effect: the fact that each additional hospital bed was associated with 20 more hospital births suggests that hospitals were operating close to full capacity. However, considering the large fees charged before the reform we should at least expect an increased take-up in lower socioeconomic groups.

5.1 First stage

A first test of whether this intervention had any effect on the demand for hospital delivery is provided in Figure 13 where we contrast birth rates in parishes with active hospitals to the rest of the country by means of a McCrary test [McCrary, 2008]. To the extent that the abolition of fees had an effect on the propensity to give birth in hospital, we would expect an upward shift of the number of births in hospital parishes, and a corresponding decline in non-hospital parishes. However, we detect no such effect. As expected, the birth rates in the (predominantly urban) hospital parishes trend upward whereas birth rates in the (predominantly rural) non-hospital parishes exhibit a downward trend; and they are both subject to the same seasonal variation. However, there is no discontinuity in the distribution in either figure.



(a) Hospital Parishes

(b) Rest of Country

Figure 13: McCrary Test of Discontinuity in Birth Rates 1st January 1938.

Note: With the 1st January 1938 costs for giving birth in a maternity ward were abolished. Own calculations based on the 1950 population census [Statistics Sweden, 1952] and the Swedish Death Index [Federation of Swedish Genealogical Societies, 2014].

Here the cut-off is set on 1 January 1938. The running variable is the distance of the birth date to that cutoff. The RD specification includes quadratic trends. The absence of an effect on overall utilisation is confirmed in Table 12 which reports first-stage estimates for the reform. At 0.2 percentage points, the estimated effect is negligible; this holds for the subgroups of patronymics and non-patronymics as well.

Table 12: First Stage: 1938 Reform

	Pooled	Patronymics	Non-Patronymics
Regression Discontinuity Design	0.002	0.002	0.003
	(0.006)	(0.009)	(0.008)
F-Statistic	0.132	0.038	0.117
Observations	305,781	148,746	157,027

Notes: Robust standard errors for the RDD 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 opening. We exclude openings that coincided with the introduction of the policy. The treatment variable represents the abolishment of childbirth fees.
Source: Census 1950, Swedish Death Register. Own calculations.

5.2 Selection

The first-stage estimates in Table 12 do not suggest that the abolishment of fees changed the selection into hospital delivery with regard to the surname types. However, there are other dimensions of selection which are worthy of consideration. We now study these other dimensions, using the method proposed in section 3.3.3. Recall that in the absence of a first stage, the method produces estimates of the differences between compliers and defiers. The results are presented in Table 13. In the two columns to the left, we present estimates for the entire population; and in the following columns, we present estimates for the untreated and treated subsamples. Since the main outcomes are potentially affected by the treatment, we only show estimates for the untreated sample for these variables.

Table 13: Regression Results: Selection at 1938

	Entire Population		Untreated Sample		Treated Sample	
	Mean	Estimate	Mean	Estimate	Mean	Estimate
Mortality						
Neonatal Death (Age $<$ 1 Month)			0.035	0.005*		
				(0.003)		
Socio-economic Outcomes						
Secondary School			0.197	0.001		
				(0.008)		
Years of Education			8.646	-0.004		
				(0.043)		
Earnings 1970			19,735	47.666		
				(300.547)		
Background						
Patronymic Name	0.486	-0.001	0.478	0.006	0.494	-0.007
		(0.006)		(0.009)		(0.008)
High SES Name	0.197	-0.001	0.198	-0.005	0.196	0.003
		(0.005)		(0.007)		(0.006)
Twin	0.011	0.004**	0.016	0.004	0.007	0.004**
		(0.002)		(0.003)		(0.002)
Family Employment Background						
Farmer	0.243	0.001	0.309	0.012	0.192	-0.010
		(0.006)		(0.009)		(0.007)
Manager	0.014	-0.003**	0.007	-0.002	0.019	-0.005**
		(0.001)		(0.002)		(0.002)
White-collar worker	0.129	-0.004	0.089	-0.001	0.161	-0.007
		(0.004)		(0.005)		(0.006)
Industrial worker	0.472	0.009	0.453	-0.000	0.486	0.018**
		(0.006)		(0.009)		(0.009)
Other	0.142	-0.003	0.142	-0.010	0.142	0.003
		(0.004)		(0.007)		(0.006)
Observations:		305,781		139,160		166,621
Parishes:		2.189		2,075		114

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 opening. We exclude openings that coincided with the introduction of the policy. The treatment variable represents the abolishmet of childbirth fees. 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.

Since the reform did not have a first stage, the estimates in Table 13 represent the differences between compliers and defiers, scaled by their size in the total population. We see that the 1938 reform has changed the selection into hospital delivery in some important respects. The compliers have higher neonatal mortality than defiers, and they are more likely to be twin births. Both estimates thus suggest there was an increased selection of risky births. Second, parents in the complier population are significantly less likely to be managers.

Since the estimates are scaled by the size of the complier and defier subpopulations, it is difficult to assess the magnitude of these selection effects. It is however possible to bound the estimates. Around the cutoff date, 55 per cent of children were born in institutions[National

Board of Health, 1939, 1940]. This means that at most 90 per cent of the population can be either compliers or defiers – in the extreme case that there are no never-takers, and all untreated individuals are compliers (before the reform) or defiers (after the reform). In that case, 0.7 per cent of the household heads in the complier population would be managers, compared to $0.7 + 0.3/0.8 \approx 1.1$ per cent in the defier group. Since the combined defier and always-taker population had 1.9 per cent managers among the household heads, the always-takers would have 2.7 per cent managers. This represents a lower bound for the selection: reducing the size of the complier and defier populations would lead to the gap between the compliers and the defiers becoming correspondingly larger.

Doing a similar calculation for the probability of twin births, we find that 1.6 per cent of compliers and 1.1 per cent of defiers would be twin births. Hence, both selection effects are considerable even under this extreme scenario that all home births replace institutional births after the reform.

5.3 Effects of the Reform

Results in Table 13 suggest that despite the absence of a first stage, the reform led to improved access for lower SES groups and for risky births. We now test whether these changed selection patterns had an effect on child outcomes.

In Table 14 we present the reduced-form results. They clearly support a scenario where high-risk births crowd out low-risk births: neonatal mortality drops by 0.3 percentage points from a baseline of 2.6 per cent. This effect is persistent over the life cycle. Conversely, we do not find any effect on edcuation and earnings.

Table 14: Regression Results: 1938 Reform

	Baseline	RDD (non-parametric) Reduced Form
Mortality		
Neonatal Death (Age < 1 Month)	0.024	-0.003*
		(0.002)
		[0.380]
Infant Death (Age < 1 Year)	0.045	-0.005**
		(0.002)
		[0.153]
Child Death (Age < 5 Years)	0.056	-0.006**
		(0.003)
		[0.175]
Death before Age 50	0.106	-0.005
		(0.003)
		[0.470]
Death before Age 70	0.229	-0.004
		(0.005)
		[0.814]
Socio-economic Outcomes		
Secondary School	0.258	-0.004
		(0.006)
		[0.814]
Years of Education	9.014	0.011
		(0.033)
		[0.814]
Earnings 1970	20,931	-183.145
		(214.166)
		[0.814]
Observations:		305,876

Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance l evels: $*$ 0.10 $**$ 0.05 $***$ 0.01. p values given by multiple testing adjustment [Romano and Wolf, 2005] are provided in square brackets. RDD Bandwidth covers a period of 4 years around the opening. We exclude openings that coincided with the introduction of the policy. The treatment variable represents the abolishemt of childbirth fees.
Number of extensions/opening maternity wards included: 163
Source: Census 1950, Swedish Death Register. Own calculations.

Some caution is required when interpreting the results in Table 14: the significant effects do not survive multiple testing adjustment[26] and the estimates are sensitive to any other change coinciding in time with the reform. But taken at face value, they suggest that the reform effectively reduced inequality in survival chances.

As a placebo test we estimate RD regressions using the 1*st* of January of the three previous years and one subsequent year as reform dates. Results are presented in Table F.8. We do not find any evidence of spurious reform effects.

[26] The Romano and Wolf [2005] adjustment is arguably overly conservative in this setting, where five of the outcome variables represent mortality and two represent educational attainment.

6 Conclusion

The general consensus in economics and public health has identified the period before the age of 5 as crucial for the long-term development of human beings in terms of their cognitive and physical development. This paper contributes to the existing literature by examining the relevance of medical treatments during the first days of life on later life outcomes. More specifically, we investigate the potential long term effects of giving birth in a hospital compared to midwife assisted home births.

Our estimated local average treatment effects on long-term socio-economic outcomes of being born in a maternity ward are sizable. For compliers the probability of attending secondary education increases substantially by 11 percentage points from a baseline of 19%. What can explain these massive gains? We can rule out selective survival as a major explanation for the large effects on later-life outcomes. We estimated a mortality reduction by 3-4 percentage points which is less than half of the estimated increase in secondary education; besides, selective survival would normally work in the opposite direction.

We argue that a more likely explanation is a substantial decrease in morbidity of the newborns. Especially in the case of complications, giving birth in a maternity ward with additional trained medical staff than only the midwife could potentially be beneficial. The immediate health effects are insufficiently captured by the worst-case outcome of neonatal and infant mortality. Most of the evidence suggests that avoidance of complications is an important factor: we have shown that the hospital expansions in particular affected the left tail of the distribution of cognitive abilities, and that the remaining home births had fewer easily detectable complications after the expansion of the nearest maternity ward. We also find that hospital deliveries reduced the children's sickness absence rates during primary school, and the probability of receiving a disability pension in adulthood.

Our results on the abolition of charges for giving birth in a maternity ward, suggest that hospitals were operating at full capacity already; therefore, the reduced price did not affect the general propensity to give birth in hospital. However, we find clear evidence of a changed composition of hospital births: the mothers giving birth in hospital after 1 January 1938 had a lower SES on average, and the births were riskier. Our estimates of the effects of the abolition of fees suggest that this changed composition also led to greater chances for risky births to be carried out in hospital: we find that the reform was associated with a substantial reduction in mortality, and with improvement in educational outcomes.

Despite these considerable gains for the treated children, it remains an open issue whether the policy changes also contributed to a reduction in inequality. Whereas we see that reductions in child mortality appear to have applied across the spectrum of socioeconomic background, the positive human capital gains correlate strongly with family background. Future research should try to unpack this heterogeneity, to find out whether they can be attributed to parental behaviours or to later-life opportunities. As a further topic for future research, we suggest looking into the long-term consequences for the treated mothers. If the main mechanism operating is a reduction in harmful complications, we should expect to see important effects also on maternal outcomes in the domains of fertility, labour market participation, and mortality.

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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

$$CA_{jt} := j \cup \left\{ p \in \mathbf{P}_{-J_t} : d\left(p, j\right) \leq D_j \cap h_{ijt} \geq h_{ikt} \forall k \in \mathbf{J_t} \cap h_{ijt} > 0 \right\} \tag{5}$$

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,[27] and h_{ijt} is the proportion of births in hospital j coming from parish i in period t.[28] $\mathbf{J_t}$ 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

$$\hat{B}_{it} = \begin{cases} B_{it} + \sum_{k \neq i} H_{kit} = \sum_k H_{kit} & \text{if } i \in \mathbf{J}_t \\ B_{it} - \sum_{j \in \mathbf{J}_t} H_{ijt} = H_{iit} & \text{if } i \notin \mathbf{J}_t \end{cases} \tag{6}$$

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

[27] 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.

[28] 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

$$\hat{B}_{CA_jt} = B_{CA_jt} + \sum_{k \notin CA_j} H_{kjt} - \sum_{i \in CA_j} \sum_{l \in \mathbf{J}_t \setminus j} H_{ilt} \tag{7}$$

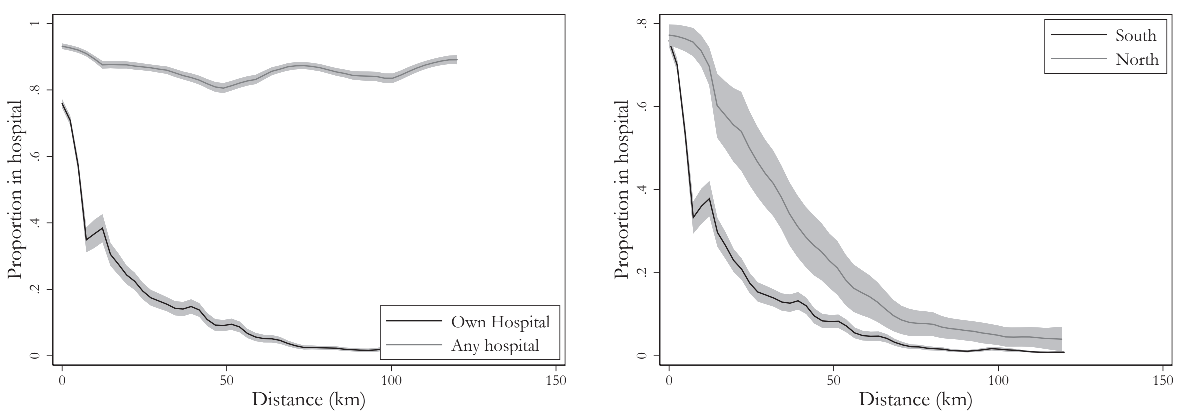
Hence, the mismeasurement of births consists of two components: individuals from parishes outside the catchment area who are born in hospital j, and individuals from parishes inside the catchment area who are born in an outside hospital. Since both terms potentially bias estimates, it is our goal to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

$$h_{ijt} = \frac{H_{ijt}}{\hat{B}_{jt}} > \max_{l \in \mathbf{J}_t \setminus j} h_{ilt} \tag{8}$$

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).[29]

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

[29] 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.

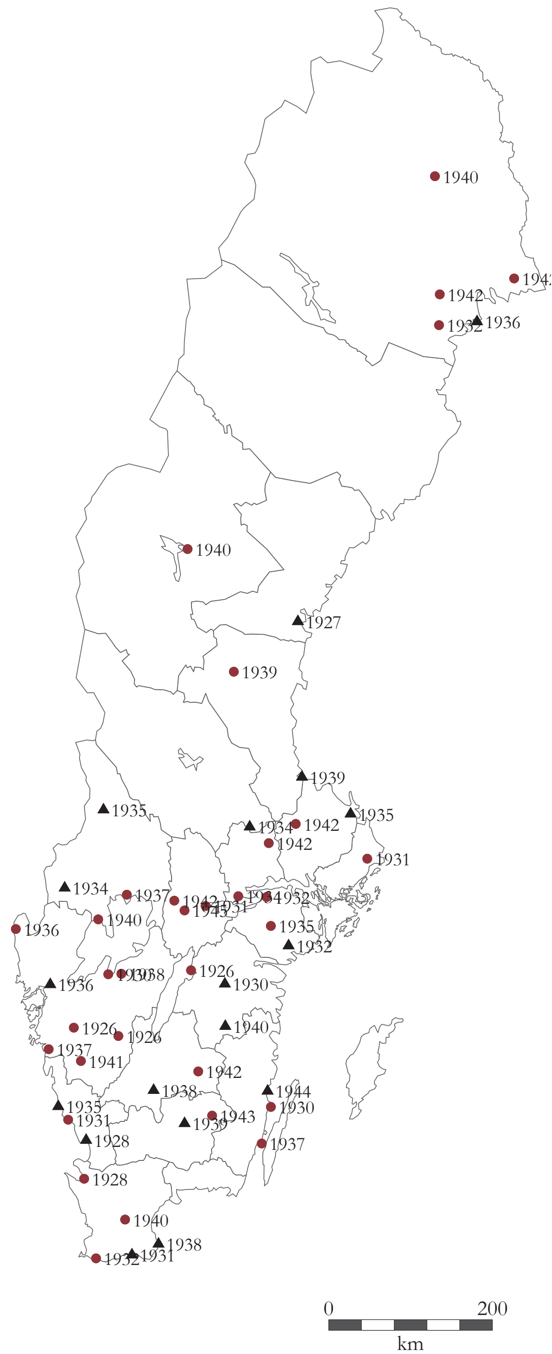
The first two categories required formal approval by two physicians. Abortions in the third category were approved by the National Board of Health and required that the mother was sterilised in connection with the procedure [Committee on International Abortions, 2005].

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:	**Hospitals (N=38)**		
Place			
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:	**Health Centers (N=13)**		
Place			
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 4.4 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.

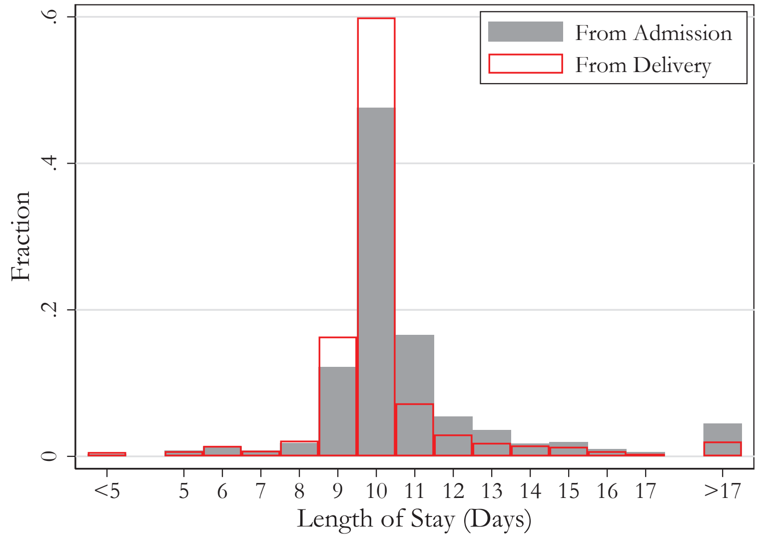


Figure C.1: Distribution of Length of Stay.

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

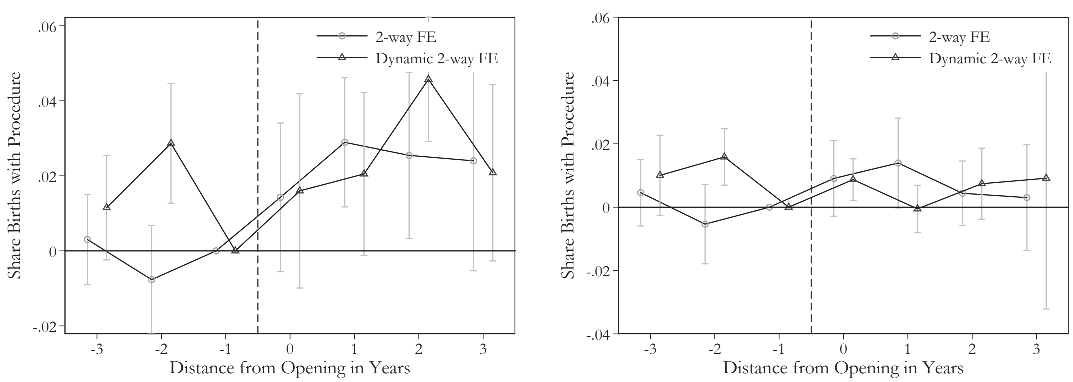
In Table C.6 we estimate the effect of a hospital opening or extension on the procedures applied, using the same dataset as in Table 6 but supplementing it with data on procedures from hospital yearbooks. The main outcome in Table C.6 is the total prevalence of procedures, and estimates are presented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

$$\tau_j = \lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \in \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \in \mathcal{I}_j\right] - \left[\lim_{t_i \downarrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \notin \mathcal{I}_j\right] - \lim_{t_i \uparrow c_j} \mathbb{E}\left[Y_i \mid t_i = c(i), i \notin \mathcal{I}_j\right]\right] \tag{9}$$

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

$$Y_i = \lambda_0 + \sum_{j=1}^{K} \lambda_j^1 D_i^j + \sum_{j=1}^{K-1} \lambda_j^2 D_i^j 1\left(t_i < c_{j-1} + \frac{c_{j-1} - c_j}{2}\right) \tag{10}$$

$$+ \beta_0 \left(t_i - c_1\right) + \sum_{j=1}^{K+1} \beta_j^1 D_i^j 1\left(t_i \geq c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_j\right) \tag{11}$$

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

$$+ \alpha_{J(i)} + \tau D_i^{J(i)} \tag{13}$$

$$+ \beta_0 1\left(i \in \mathcal{I}_1\right)\left(t_i - c_1\right) + \sum_{j=1}^{K+1} \beta_j^1 D_i^j 1\left(i \in \mathcal{I}_j\right) 1\left(t_i \geq c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_j\right) \tag{14}$$

$$+ \sum_{j=2}^{K+1} \beta_j^2 D_i^j 1\left(i \in \mathcal{I}_{j-1}\right) 1\left(t_i < c_{j-1} + \frac{c_{j-1} - c_j}{2}\right)\left(t_i - c_{j-1}\right) \tag{15}$$

$$+ \gamma X_i + \sum_{m=2}^{12} \delta_m month \tag{16}$$

Line 10 includes common period effects for each of the $2K$ periods that we have split the sample into. Lines 11 and 12 represent common trends from below and above to each of the K cutoffs. Line 13 contains K fixed effects and τ which is the main parameter of interest. Rows 14 and 15 allow for differences in trends around the cutoff for treated and untreated hospitals. Line 16 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 Reduced Form	182 Days 2SLS	365 Days Reduced Form	365 Days 2SLS	730 Days Reduced Form	730 Days 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)

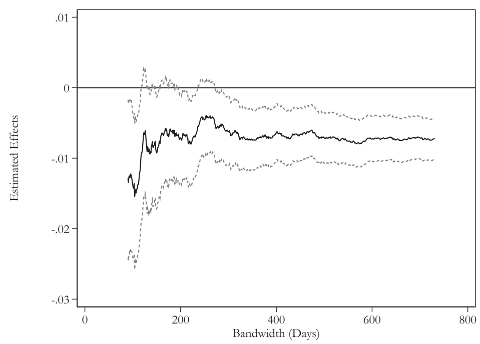
	Openings			Extensions		
	Baseline	RF	2SLS	Baseline	RF	2SLS
Mortality						
Neonatal Death (Age < 1 Month)	0.029	-0.007***	-0.039***	0.027	-0.007**	-0.063**
		(0.002)	(0.011)		(0.003)	(0.025)
Socio-economic Outcomes						
Secondary School	0.193	0.020***	0.105***	0.191	0.019**	0.157**
		(0.006)	(0.031)		(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.

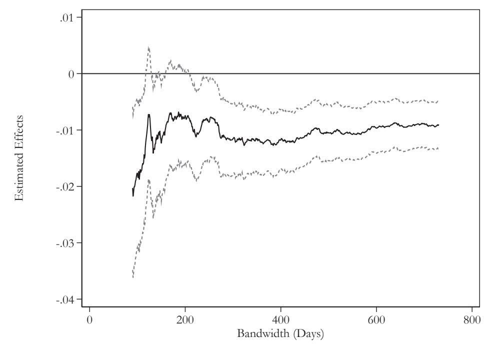
Table F.8: Placebo Regressions: 1938 Reform

	RDD			
Placebo Reform Date:	**1.1.1935**	**1.1.1936**	**1.1.1937**	**1.1.1939**
First-Stage				
Birth in Hospital Parish	0.002	-0.006	-0.004	0.011*
	(0.006)	(0.006)	(0.007)	(0.006)
Outcomes				
Neonatal Death (Age < 1 Month)	0.003	-0.002	-0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Infant Death (Age < 1 Year)	0.002	-0.002	-0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Child Death (Age < 5 Years)	0.001	-0.000	0.002	0.000
	(0.003)	(0.003)	(0.003)	(0.003)
Death before Age 50	0.001	0.000	0.004	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Death before Age 70	0.004	-0.003	0.003	0.000
	(0.005)	(0.005)	(0.005)	(0.005)
Secondary School	-0.006	-0.000	-0.002	0.002
	(0.006)	(0.005)	(0.006)	(0.006)
Years of Education	-0.057*	-0.000	-0.016	0.004
	(0.032)	(0.033)	(0.033)	(0.033)
Earnings 1970	-260.596	8.183	184.291	186.240
	(231.825)	(246.819)	(253.784)	(219.717)
Observations:	280,325	276,841	275,090	292,031

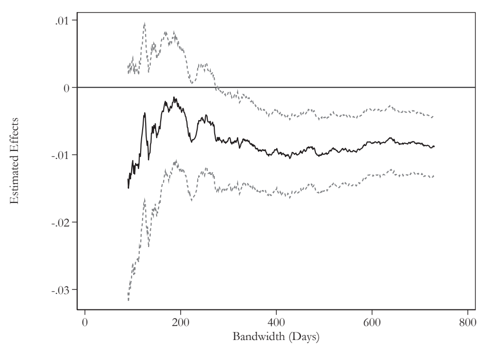
Notes: Robust standard errors are clustered at the level of the running variable. Both are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. RDD Bandwidth covers a period of 4 years around the placebo dates.
Source: Census 1950, Swedish Death Register. Own calculations.



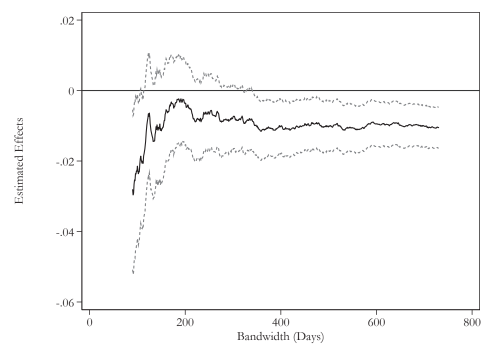
(a) Neonatal Death (Age<1 Month)



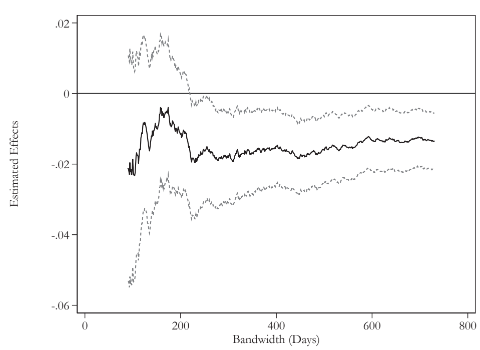
(b) Infant Death (Age< 1 Year)



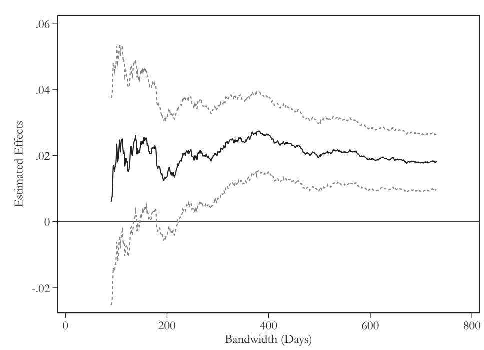
(c) Child Death (Age< 5 Year)



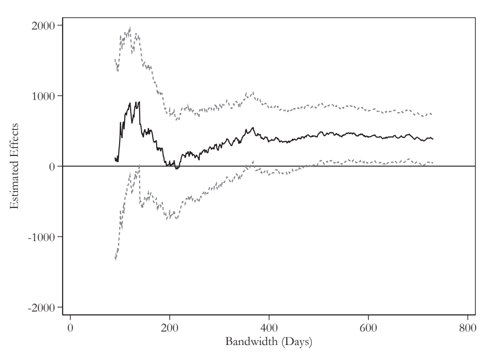
(d) Death before Age 50)



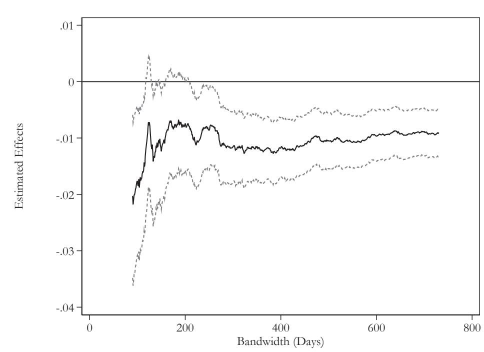
(e) Death before Age 70



(f) Secondary School



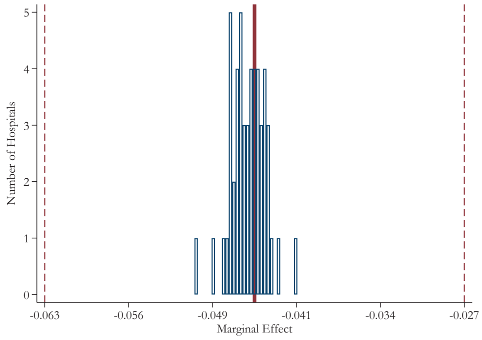
(g) Earnings (1970)



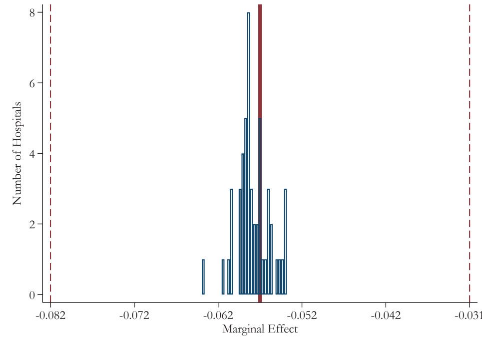
(h) Years of Education

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

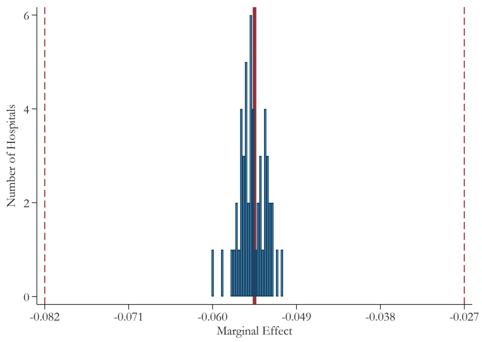
Note: The graph shows the effect estimates for the DiDisc design. Accompanied with the 95 percent CI. Bandwidths ranging from 90 to 730 days-



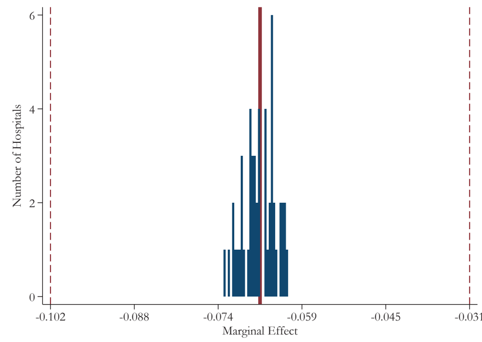
(a) Neonatal Death (Age<1 Month)



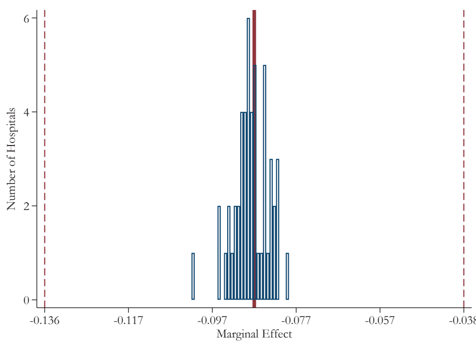
(b) Infant Death (Age< 1 Year)



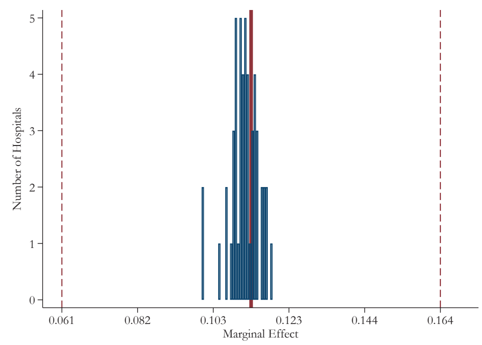
(c) Child Death (Age< 5 Year)



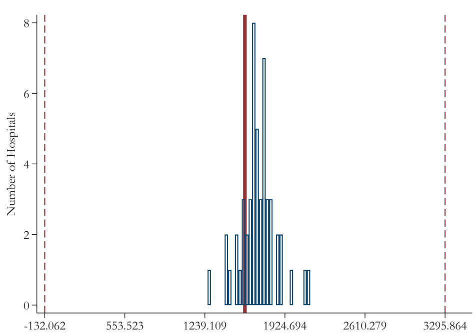
(d) Death before Age 50)



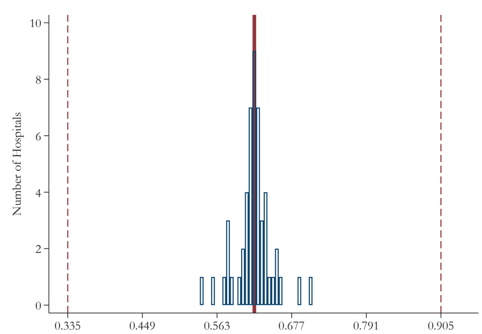
(e) Death before Age 70



(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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