IT'S A LONG WALK: LASTING EFFECTS OF MATERNITY WARD OPENINGS ON LABOUR MARKET PERFORMANCE

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Abstract

Being born in a hospital versus having a traditional birth attendant at home represents the most common early life policy change worldwide. By applying a difference-in-differences approach to register-based individual-level data on the total population, this paper explores the long-term economic effects of the opening of new maternity wards as an early life quasi-experiment. It first finds that the reform substantially increased the share of hospital births and reduced early neonatal mortality. It then shows sizable long-term effects on labour income, unemployment, health-related disability and schooling. Small-scale local maternity wards yield a larger social rate of return than large-scale hospitals.

JEL codes: I18, I38, J24, N34

Key words: Early life, quasi-experiment, maternity ward, labour market performance, efficiency, Sweden

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I. Introduction

There is a growing body of literature investigating the importance of early childhood health interventions (see Currie and Rossin-Slater, 2015, Almond et al., 2018, for some recent reviews). However, only a handful of studies examines the returns to additional medical care for at-risk newborns, and even fewer examine those for uncomplicated births with the focus on short-term survival and medium-term benefits such as test scores.[1] Despite its importance, there is an even more limited literature on the potential benefits of one of the most common health care interventions undertaken by the majority of developed countries in the past: expanded access to maternity wards (MWs) for childbirth.[2] The evaluation of these benefits is particularly important in order to design and implement efficient childbirth policies given the current discussions on reducing costs in developed countries on the one hand, and the large shifts toward facility deliveries currently occurring in developing countries on the other hand.[3] This paper aims to fill in the gap by examining the short and long-term effects of

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[1] This small universe of studies includes Almond et al. (2010), Almond and Doyle (2011), Bharadwaj et al. (2013), Sievertsen and Wüst (2017), Daysal et al. (2019), and Daysal et al. (2020).

[2] The majority of studies on the benefits of hospital versus home delivery are medical and do not build on credible research designs. One notable exception is Daysal et al. (2015), which studies the impact of giving birth in a hospital versus the home on infant mortality in 2000–2008 in the Netherlands. There are also studies that investigate the impacts of trained birth attendants in a home setting with historical interventions (Lazuka, 2018; Lorentzon and Pettersson-Lidbom, 2021) and with interventions in developing countries (e.g., Frankenberg et al., 2005; Choulagai et al., 2017).

[3] Facility deliveries increased from 39% to 79% on average between 2005 and 2015 across

expanded access to MWs in Sweden.

The purpose of this study has benefited from a unique reform in Sweden that, between 1931 and 1946, increased access to childbirth hospital facilities at a time when home deliveries assisted by midwives were predominant. In 1931, Sweden increased several state subsidies that covered the costs of the opening and running of a MW as well as all the individual costs related to childbirth, including delivery and travel. Although this was a universal policy, the fact that predominantly rural or semi-urban locations gained access to hospital delivery facilities became its key feature. 170 new MWs were established in different parts of the country and, as a result, the share of hospital births tripled to 90% by 1946, when delivery at a hospital became compulsory (Myrdal, 1944).

There are a number of reasons why birth in a MW, rather than at home, might impact early-life health and have lasting consequences. First, MWs provide direct access to hospital resources important for the short-term survival and health of newborns, such as surgical interventions in complicated deliveries, prevention or treatment of infectious diseases, and specialized care for at-risk newborns (e.g., preterm or low birth weight). Both the medical and the economic literature stress that exposure to disease and injury early in life, especially in the neonatal period, can lead to lasting negative outcomes (Finch, 2007; Almond and Currie, 2011). Second, MWs can potentially reduce newborn stress through more specialist and maternal care, such as time spent nursing. Previous studies suggest that early-life exposure to stress hormones can permanently affect brain function, with long-term consequences on mental health and wellbeing (Danese and McEwen, 2012; Persson and Rossin-Slater, 2018). Finally, newborn screening and treatment in hospital can lead to early

countries in Africa and Asia (Montagu et al., 2017), and this shift is similar to that in Sweden in 1931–1946, which is the subject of study in this paper.

disease detection and treatment, potentially stopping the development of disease.

The gradual opening of new MWs across different municipalities has allowed me to implement a difference-in-differences (DD) approach with staggered adoption. Using archival data on reform implementation linked to rich Swedish register data, I find large effects of expanded access to MWs in both the short and long term. First, the reform led to an increase in the share of hospital births of 19 percentage points and to a reduction in neonatal mortality of 20 deaths per 1,000 live births. Such reduction in mortality almost fully explains the reduction in mortality due to preventable causes, such as infection, preterm birth and low birth weight, in the same period. Second, the long-term (reduced-form) effects on economic outcomes are sizable and robust: a 4.3% increase in labor income, a 10% reduction in unemployment and an 11% decline in health-related disability pension receipt as compared to the average in cohorts born before the MW expansion. These effects are roughly equivalent to a return to up to one year of schooling for the cohorts studied. Third, by looking at the channels through which MWs could produce these effects, I find the reform led to a reduction in long-term hospitalizations (9%), an increase in school attainment (0.08 years), and an increase in the probability of employment in a non-manual occupation (3%). Fourth, among MWs of different sizes and resources, the effects are strongest for mid-scale hospital-based MWs in both the short and long term.

There are several potential threats to the empirical design that I attempt to address in this paper. The first is the presence of other early-life interventions, such as the arrival of first antibiotics and the introduction of the infant care program, which may potentially confound and explain the effects above. I have studied carefully the independent and interaction effects of these and all other presumably important interventions, and conclude that both the short and long-term effects of MWs are largely independent. A second potential threat is long-term selective survival due to large neonatal mortality effects, as well as potentially selective migration and fertility. The estimated effects of MWs would be biased if these responses to the reform changed the composition of cohorts to include more children from high-resource families, who presumably achieve higher levels of human capital. Finally, in the case of staggered reform adoption, the two-way fixed effects estimator may suffer from a negative weighting problem that leads to biased estimates in the presence of heterogeneous treatment effects. I performed several robustness analyses to confirm that my estimates are not contaminated by these issues.

This paper contributes to several strands of literature in economics. First, it adds to the literature on the short-term benefits of medical care in early childhood (e.g., Almond and Doyle, 2011; Bharadwaj et al., 2013; Daysal et al., 2015) by showing that early hospital childbirth technologies improved the neonatal survival of complicated and uncomplicated births. Second, this paper contributes to the growing literature on the long-term effects of large-scale public health interventions (Almond et al., 2018) by examining a type of intervention that has not been studied previously. In addition, I provide evidence on the interaction between the expanded access to care at birth and various post neonatal care interventions, in the spirit of a small but growing literature on the dynamic effects of early and later childhood investments (Rossin-Slater and Wüst, 2020). Finally, my paper contributes to the literature on the efficiency of hospital competition in the modern context (Kessler and McClellan, 2000; Schmitt, 2018) by estimating the total (i.e., both short and long-term) efficiency of childbirth institutions of different scales and with different resources.

My findings are also particularly relevant to current policy debate. The costs of a birth in hospital are large given that childbirth is the most common reason for hospitalization in the US and the second most common in Europe (Sakala and Corry, 2008; OECD/EU, 2018). Consequently, many developed countries are trying to reduce costs by closing local MWs and reducing the length of post-childbirth stay (OECD, 2017). In contrast, developing countries are currently experiencing a shift toward hospital delivery facilities, with poorer regions lagging behind when compared between and within countries (Montagu et al., 2017). My paper provides evidence that expanding access to hospital facilities for childbirth is a powerful tool that not only saves lives but also leads to more investment in human capital and to income growth in the long run. It also provides specific recommendations for the design of childbirth policies, such as focusing on quality childbirth technology and personnel, to make hospital delivery affordable and accessible to each and every individual, and to make the shift from rural delivery practices to mid-scale hospital-based facilities.

II. The Reform

The early 1930s marked important changes in how childbirth is organized and financed in Sweden. Prior to the late 1920s, women in labor could be admitted to hospital only if they needed specialist intervention, and all uncomplicated deliveries had to take place at home (Socialdepartamentet, 1942). General hospitals were also obliged to admit poorer and unmarried women for childbirth. The country was divided into small healthcare units comprising one or more municipalities supervised by either a public midwife or a city doctor.[4] Midwife-assisted home births comprised 80% of all births in 1930 (see Appendix A Figure for the development over time).[5]

[4] Since 1862, Sweden has been divided into secular local governments - municipalities (2,498 originally), and into secular regional governments – county councils (25 in total). In 1930, there were 2,103 healthcare districts supervised by midwives and 106 city districts supervised by city doctors. For each year between 1931 and 1946, this paper uses consistent municipality boundaries (2,529 municipalities in total).

[5] Poor women could also give birth in charity maternity homes. The demand for inpatient childbirth care, usually for a bed in a small private MW with a high patient fee, also began to

The new Hospital Act of 1928 established that hospitals should bear the responsibility of assisting with childbirth and that the regional authorities organize the provision of such care. An investigation by the National Health Board found there was an inadequate number of MWs given the potential demand for childbirth care (Socialdepartamentet, 1929). Its guidance was therefore that the number of MWs should be such that an expectant mother would travel at most 5.5 km (a walking distance of one hour) to a MW from the center of her home municipality. From then on, county and municipality authorities and, a few years later, the state launched subsidies covering the costs of new childbirth beds either in new or existing general hospitals or in social care institutions. In 1931, new childbirth benefits were also introduced that made all childbirths free of charge regardless of the place of delivery. The new childbirth benefit was sufficient to fully cover the cost of a stay at a MW in an equipped county hospital for at most 10 days, or cover the cost of an at-home birth attended by a midwife. A maternity support benefit was also provided in order to cover the travel costs of the expectant mother to the MW or of the midwife coming to her to assist with a home delivery.[6]

Between 1931 and 1946, the number of the MWs as well as the share of hospital births saw remarkable growth. In total, the number of MWs doubled, with 170 new MWs established across the country (see Figure 1). Out of all the new childbirth beds, 95% were in institutions, departments and locations either largely rural or semi-urban that had not provided childbirth care before, while the rest were additional beds in already existing

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increase among wealthier women. These MWs began to flourish as early as the 1920s, having increased from 17 to 83 (Skatteförvaltningen, 1989).

[6] Specifically, this childbirth benefit provided 2 SEK per day and maternity benefit amounted to 28 SEK in total, equivalent to 7 and 94 US dollars respectively in 2020.

maternity clinics. Appendix B lists the number of municipalities by proximity (using the 5.5 km threshold) to an existing or a new MW for each year during 1931–1946.

[Figure 1 is about here]

While the increase in hospital births occurred everywhere starting from 1931, it varied in intensity across these different types of municipalities. Since the state subsidies were only able to cover a portion of the construction costs, only a small number of independent maternity clinics were built. Instead, the grants were generally used to build extensions or to renovate and equip available buildings in institutions that had not provided childbirth care previously.[7] Such use can be divided into three types: large-scale maternity departments under the authority of a county council, built as part of the renovations or additions to an existing general hospital (*Type I* MWs); mid-scale locally-governed wards opened in connection to either cottage or Red Cross hospitals (*Type II hospital-based* MWs); or small private homes in connection to retirement, social care and midwifery homes (*Type II private* MWs).[8] The institution of childbirth at home assisted by a public midwife was abolished in 1947, by which time 90% of all births occurred in a hospital.

By the 1930s, delivery by skilled midwives in the home included the use of manual

[7] The typology of the MWs used in this paper closely follows that used by the National Health Board. During 1931–1946, the following number of new MWs were opened by type: 2 *specialized maternity clinics* (with 40 childbirth beds per each), *65 Type I* MWs (with 20 childbirth beds per each), 39 *Type II hospital-based* MWs and 64 *private* MWs (with 6 childbirth beds per each). The number of childbirth beds levelled off after the reform.

[8] These expansive childbirth investments did not push the opening of general healthcare institutions; by way of comparison, the county councils opened only two general hospitals in parallel.

obstetric techniques and forceps, basic preventive measures, and several postnatal check-ups of the mother and newborn (Lazuka, 2018). Maternity hospitals had several advantages. First, MWs reduced overcrowding and the risk of contagion due to the availability of isolation rooms, a relatively long duration of the postpartum stay, strict preventive measures and better hygiene, and the arrival of antibiotics against certain infections after 1937. In contrast, the majority of the homes where expectant mothers lived were one-room apartments without central heating or a bath (Socialdepartamentet, 1935). Second, MWs provided better emergency and supportive neonatal care for both at-term and preterm newborns.[9] Specially trained personnel carried out daily check-ups, procedures and instructions, including instruction on nursing and breastfeeding for all newborns, and specialist knowledge on feeding and care for the premature. Among mothers who delivered at home, a large share did not seek postpartum care so they lacked much of the information on proper care and in far more cases practised early weaning (Socialdepartamentet, 1945). By the late 1920s, MWs were equipped with the basic tools of intensive care units essential for the survival of preterm newborns, such as revival machines, similar to artificial lung respirators, and heating reservoirs used in-house and for transportation.[10] In addition, only hospital physicians were allowed to perform emergency childbirth interventions such as C-sections. Finally, hospitals provided pain relief in the form of nitrous oxide and pain-killing drugs, along with hourly

[9] Guidelines used the term "preterm birth" for low-birthweight newborns, i.e. those below 2,500 grams at birth.

[10] To gain an understanding of which babies could be saved with these tools, I refer to the successful experimental trials in 1928–1930 in Sweden which showed that the use of the lung revival respirator substantially improved the survival of newborns weighing 2,000–2,500 grams who had asphyxia (Wachenfeldt, 1931).

monitoring and the opportunity to rest for at least 10 days. These measures contributed to the mothers' recovery and to their ability to care for their children, and were greatly valued by working-class women (Gröne, 1949).

III. Empirical strategy

This paper aims to investigate the causal impact of the expanded access to MWs on short and long-term outcomes. Comparing municipalities that did or did not have access to hospital childbirth facilities is likely to be subject to omitted variable bias. The source of this bias comes from unobserved time-varying characteristics that correlate with both the presence of MWs and early-life health, as well as other early-life cohort-specific shocks or policies that may affect the outcomes. For example, municipalities which differ in terms of MW availability are likely to differ in many other aspects too, such as wealth and norms, that can vary over time and also influence the outcomes for children born in these areas.

To address this potential bias, my identification strategy relies on plausibly exogenous group and cohort variation in access to new MWs driven by the childbirth reform in Sweden. Municipalities can be classified into three different treatment statuses between 1931 and 1946 depending on their proximity to new or existing MWs. Municipalities close (less than 5.5 km) to the site of a newly opened MW in 1931–1946 are defined as "treated", while "control" municipalities include those that had a MW built prior to the reform ("always-treated") and those that did not have a MW nearby at any point during the period analyzed ("never-treated").[11] Using this classification, I implemented a DD approach and estimated the following specification:

$$y_{(i)mb} = \alpha + \beta \, MW_m \text{ x } post_{mb} + \delta_m + \mu_b + \mathbf{X}_i + \gamma_{r(m)b} + \varepsilon_{(i)mb} \qquad (1)$$

[11] Following this definition, there were 208 "treated" and 2,321 "control" municipalities in the entire country.

where $y_{(i)mb}$ are the outcomes of interest for individual i born in municipality m in year b. Short-term outcomes are aggregated at the municipality level and long-term outcomes at the individual level. MW_m indicates if the municipality of birth is located no further than 5.5 km away from a newly established MW, and $post_{mb}$ is an indicator for birth cohort b being born after the new MW was established. δ_p are municipality-of-birth fixed effects that control for any permanent unobserved differences between municipalities. μ_b are year-of-birth fixed effects that account for the effects of any common time-specific shocks to the outcomes. $\mathbf{X}_i$ are individual characteristics (sex in the baseline model). Finally, $\gamma_{r(m)b}$ denote county-of-birth by urbanization by year-of-birth fixed effects, included to flexibly control for differences in treatment initiation across regions and the location of MWs in mostly urban areas.[12]

The main identification assumptions of the DD approach are that the evolution of the outcomes in the control group provides a valid counterfactual for the evolution of the outcomes in the treated group in the absence of the treatment, and that the only event potentially affecting outcomes in the post-treatment period is the treatment. If these assumptions are satisfied, the parameter of interest in my setup, β, represents an intention-to-treat effect estimated by comparing the difference in outcomes between individuals born before and after the opening of a new MW in the "treated" municipalities to the difference in outcomes between individuals in the same cohorts born in the "control" municipalities, averaged across all birth cohorts between 1931–1946 and across all municipalities in

[12] These are defined as 16 x 49 = 784 interactions between year of birth dummies (16) and separate dummies for urban and rural areas within each county of birth (49 in total: the city of Stockholm, and separately cities and rural municipalities in each of the 24 counties). The estimated effects from a specification without these terms (available upon request) are statistically similar to the baseline results.

Sweden.[13] Due to the inclusion of county by urbanization by year of birth fixed effects, these comparisons are made within each such geographic region, weighted by its share of municipalities and a variance term, and then averaged in the main DD estimate (see Goodman-Bacon, 2021, for details).

The "treatment" received is the possibility of the birth taking place in a hospital setting through access to an MW. As mentioned in the previous section, there were different types of MWs created in response to the reform, and in the analysis I exploited the variation in "treated" municipalities by the type of MW accessed: *Type I*, *Type II* and *private* MWs.[14] The main counterfactual treatment is delivery at home assisted by a midwife. An alternative counterfactual exists in the "always-treated" group due to the particular features of the reform: hospitals in existence prior to 1931 did not accept women for uncomplicated deliveries, and the childbirth reform induced the establishment of new MWs rather than the addition of childbirth beds.

I complemented the DD approach with an event study (ES) specification. This allowed me to check for the existence of mean-reverting shocks or pre-treatment differences in the outcomes, as well as to verify the stability of the treatment effect across the treated cohorts.

[13] As clarified in Goodman-Bacon (2021), in the DD application with staggered adoption, the two-way fixed-effects DD estimator is a weighted average of all possible two-group-two-period DD estimators that compare treatment groups to each other. That said, in addition to pure "control" municipalities, different treatment municipalities acted at times as "treated" and "control".

[14] Only a few *specialized maternity clinics* were opened during the reform in the municipalities where an MW had already been located (they enter the "always-treated" group), hence their effect cannot be estimated with the municipality-level policy variation.

In particular, I estimated the following specification:

$$(2)\qquad y_{(i)mb} = \alpha + \sum_{t=-2}^{\leq -6} \beta_t MW_m 1(b - b_m^* = t) + \sum_{t=1}^{\geq 6} \beta_t MW_m 1(b - b_m^* + 1 = t) + \delta_m + \mu_b + \mathbf{X}_i + \gamma_{(i)mb} + \varepsilon_{(i)mb}$$

where the DD interaction MW_m x $post_{mb}$ has been replaced with the interaction between the treated indicator MW_m and indicators $1(b - b_m^* = t)$ for cohorts born between 6 years before and 6 years after the opening of a MW in the municipality, and all the other terms are defined as before.[15] Cohorts born in the year prior to the reform (t = - 1) have been omitted.

The recent methodological literature shows that the two-way fixed effects regression typically used in a DD design may suffer from a negative weighting problem in the presence of staggered adoption and heterogeneous treatment effects. This may bias both the overall and the event-study estimates of the reform effect, and I address the related concern in several ways. First, the control group that did not change its treatment status in my final estimation sample was constructed to include more than 80% of the relative time, a threshold proposed by Borusyak et al. (2021) to overcome the weighting problem. Second, in this paper, the DD estimates for the full sample are similar across subsamples when controls include only “treated”, “always-treated” or “never-treated” pointing to the absence of a negative weighting bias. Therefore, I provide the results for the full sample in the main body and the results for different subsamples of “control” municipalities in accompanying appendices.[16]

[15] The end-point categories respectively capture cohorts born 6 or more years prior to and after the opening of an MW.

[16] I also measured the robustness of the two-way fixed effects regressions to heterogeneous treatment effects for the main outcomes, following Chaisemartin and D'Haultfœuille (2020), and similarly conclude that the estimates of β from these regressions are likely to be robust.

IV. Data

A. Regional-Level Data

The main source of the data on hospital openings is the registry of MWs throughout Sweden from Skatteförvaltningen (1989).[17] This report provides information on the MWs' dates in operation, their names, types, and municipality locations. I confirmed the dates in operation, types and construction plans of MWs against their annual reports located in the archives of the National Health Board (Riksarkivet, 1931-1950). To assure (geographical) consistency with the municipality-of-birth information of individuals, hospital location was chosen as the centroid of the municipality in which the hospital in question was located. Municipality boundaries have come from the GIS maps prepared by the Swedish National Archives (Riksarkivet, 2016).

Short-term outcome variables are measured at a regional level. I obtained the number of neonatal deaths from the Swedish register of deaths, which provides a complete count for the period under analysis (Släktforskarförbund, 2017), and then aggregated them at municipality-of-birth level. The number of live births in each municipality and for each year was

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The negative weights only amount to -0.02 at minimum, and the minimal standard deviation of the average treatment effects across the treated municipalities x cohorts required to revert the sign of β was at least 20 times larger than the estimate of β across all outcomes, a very large and implausible level of heterogeneity.

[17] The supporting information in the registry indicates that the list of maternity institutions should be regarded as complete, although some small MWs owned and run by midwives might not be included. Less than 1% of women gave birth in the homes of midwives (Socialstyrelsen, 1931-1946), and the registry includes the majority of these births.

constructed by summing up two numbers.[18] The first is the number of survivors up to the age of 37 among cohorts born between 1931 and 1946 in the Swedish Interdisciplinary Panel (SIP), a full-population panel dataset available from 1968 onwards. The second is the number of deaths up to the age of 37 among the same cohorts entered in death records provided by Släktforskarförbund (2017). These two sources of data allowed me to obtain the panel of mortality rates up to the age of 65 for the cohorts under study, rates that apply not only to the neonatal age group. Additionally, I collected data on the participation rates (the number of live births at home and in a MW) and other birth outcomes, including shares of preterm births, stillbirths and sick mothers (with fever 3 weeks after childbirth).[19] These data come from my digitization of the full set of yearly reports of the first rural doctors across Sweden that summarize data from local rural and city doctor districts (National Health Board, 1931-1946). The reports cover 23 counties (for participation rates) and 119 doctor districts (for

[18] The number of births obtained by this summation aligns perfectly with the official counts of annual births (Statistiska Centralbyrån, 1969). The only difference of note is that the number obtained does not include individuals who permanently emigrated from Sweden at a young age, but their number amounts to no more than 0.02% of all births.

[19] Given that compliers cannot be directly identified, the survey conducted by the National Health Board in 1941 likely provides the best suggestive evidence on who would be willing to give birth in a hospital if any was built (Socialdepartamentet, 1945). Among the reasons for choosing hospital facilities for delivery, women mentioned not only better delivery and postpartum care but also social factors such as cramped living conditions at home and difficulties in obtaining the necessary domestic help. These women lived in rural areas and were married to lower-class workers (e.g., artisans, small farmers and civil servants), which correspond to mothers of around 60% of the individuals in my study.

birth outcomes and hospital records). The link between municipalities and doctor districts comes from Medicinalstyrelsen (1939).

B. Individual-Level Data

For the long-term outcomes, I used individual-level data from the SIP, which combines information from various administrative registers at the yearly level for all the individuals born in Sweden between 1930–1980, as well as for their parents and siblings. In Lazuka (2020), I have previously described data, their sources and completion. I selected from these data information about individuals born between 1931 and 1946 over the period 1960–2011, i.e. prior to death or age 65 which is the retirement age. For economic outcomes, the time span of the data allowed me to obtain the log of the average real labor income between ages 47–64 and two indicators of labor market participation between ages 55–64, such as whether the individual was unemployed at any time and whether they received disability insurance on a permanent basis.[20] Finally, I considered a variety of variables: education (years of schooling, the highest education level achieved and fields of education), occupation (sector affiliation and a socio-economic category of the main occupation of the individual aged between 34–49), and health (the total length of stay in hospital aged between 37–64) – variables that act as mediators.[21] The final sample contains information for 799,192

[20] For all variables, the results are almost identical to those using a narrower age range (available upon request), e.g., prior to the age of 60, indicating no differential early retirement across treatment groups. This also suggests that the disability indicator reflects chronic health-related limited capacity to work rather than labor market reasons, because the latter applied only to workers aged no younger than 60.

[21] All outcome variables, except for education, are constructed as unconditional, i.e. they do not exclude individuals with zero observations.

individuals.

Date and municipality of birth, both available in SIP, are needed to construct the treatment status of the individual. Municipality of birth from SIP overwhelmingly indicates municipality of mother's (and child's) residence, although in certain instances it gives the municipality of child delivery; namely the municipality where the MW was located (Skatteförvaltningen, 1989). To avoid a potential measurement error arising due to this misreporting, I identified where and when maternity hospitals registered births instead of the maternal municipalities based on municipality church books that accurately report this information (Riksarkivet, 1931-1946). These municipalities overwhelmingly represent large cities falling into the "always-treated" group and are excluded from the analysis together with municipalities in a 5.5 km radius around them.[22] The final estimation sample includes 2,398 municipalities of births (205 "treated," 124 "always-treated," and 2,069 "never-treated"), consisting of smaller cities and rural municipalities (see Appendix C for a comparison of the initial and estimation samples).[23] Descriptive statistics for the estimation sample is presented in Appendix D.

V. Main Results

A. The Immediate Impacts of the Policy Experiment on Take-up and Birth Outcomes

Table 1 and Figure 2 present DD and ES estimates respectively for the immediate impacts of the MW reform available at the municipality level. The results indicate a strong

[22] In total, 124 "always-treated", 4 "never-treated" and 3 "treated" municipalities are excluded.

[23] The baseline results from the full sample (available upon request) are not statistically different from those from the estimation sample, although the latter are preferred for the sake of internal validity.

positive effect of the reform on the share of hospital births of about 19 percentage points (39% of pre-treatment mean), statistically significant at the 1% level. The ES estimates in Figure 2 suggest that there are no significant differences in the pre-intervention trends, and that the effects emerge in the first year of the opening of a MW and remain strong and relatively constant afterwards. The results in Appendix E show that the effects are similar across specifications with different control groups. Interestingly, the magnitude of the take-up following the MW reform is similar to those previously found in Sweden, either in the earlier period for the employment of higher-quality midwives (Lorentzon and Pettersson-Lidbom, 2021) or in the overlapping period for the well-child program (Bhalotra et al., 2017).

[Table 1 and Figure 2 are about here]

The short-term results also suggest that the MW reform led to a decrease in 28-day mortality of 20 deaths per 1,000 live births (56% of the pre-treatment mean). This decrease can be fully attributed to preventable causes, such as infection, preterm birth, and low birth weight, given that mortality related to these causes declined throughout 1931–1946 while mortality due to congenital malformations remained stagnant. I further estimated the DD and ES effects for both early (7-day) and late neonatal (8 to 28-day) mortality, and found that the period coinciding with treatment in hospital explains the bulk of the mortality effect for neonates. While these effects seem to be large in relative terms, they are similar to those found in other quasi-experimental studies on the effects of access to higher-quality services at childbirth on neonatal mortality. For instance, there is at least a 49% decline in 28-day mortality due to treatment by a licensed midwife as opposed to that by a traditional birth attendant in 1881–1930 in Sweden (Lazuka, 2018), or at least a 46 (49)% decline in 7-day (28-day) mortality due to birth in hospital as opposed to delivery at home between 1980 and 2009 in the Netherlands (Daysal et al., 2015). As shown in Appendix E, there are no effects of the MW reform on other birth outcomes, such as preterm birth rate and stillbirth rate which could be thought as placebo outcomes, or on maternal health.

B. The Long-Term Impacts of the MW Policy Experiment on Economic Outcomes and Mediators

Table 2 presents DD estimates for the long-term economic impact of the MW reform obtained from the individual-level data. The results suggest strong and statistically significant effects of the MW reform on labor market outcomes between ages 47 (or 55) and 64. The MW openings led to a 4.3% increase in average labor income, a decline of 1.3 percentage points (10% of the pre-treatment mean) in the likelihood of being unemployed, and a reduction of 1.4 percentage points (11% of the pre-treatment mean) in the use of disability insurance. These long-term effects are similar between men and women (results available upon request). Figure 3 presents the corresponding ES estimates that point to the emergence of beneficial effects on all outcomes strictly after the opening of MWs. As before, the effects are similar across specifications with different control groups (see Appendix F).

Let us recall that the estimated effects are the intention-to-treat effects. The average treatment effect on the treated can be calculated by dividing the intention-to-treat effects by the estimated take-up rate, 0.19. For example, the average treatment effect on the treated for the average real labor income is $(e^{0.042} - 1) / 0.19 = 22.6\%$.

[Table 2 and Figure 3 are about here]

Turning to mediators, the results from Table 3 show that the MW reform had positive and statistically significant effects on health, education and occupation. First, the expanded access to MWs at birth led to a decrease in the length of hospital stay of 2 days (or 9% of the pre-treatment mean) for those aged between 37 and 64. In the additional analyses reported in Appendix G, I found that this effect is driven by three disease groups: cardiovascular, degenerative (largely arthritis), and mental and nervous conditions. The first two of these groups link adult health to reduced exposure to infection in early life and chronic inflammation (Finch, 2007), and the last group could be associated with the reduced stress response due to better care shortly after birth (Danese and McEwen, 2012). There are no effects on mortality, either overall or cause-specific, after age 1 up to age 64.

Second, due to the MW reform, individuals received 0.08 additional years of schooling in the form of completing secondary school and obtaining a degree in a specific field (3% of the pre-treatment mean for both outcomes). Third, the MW reform encouraged individuals between ages 34–49 to sort into non-manual jobs and work in the service sector (each increase by 1 percentage point, equivalent to 2–3% of the pre-treatment mean). While the labor market entered into by the studied cohorts during adulthood experienced growth in jobs in knowledge-intensive enterprises, services, and unskilled work in serial production (Palme and Wright, 1998), my results suggest that treated individuals obtained complex knowledge and worked in skilled jobs. As shown in Appendix H, the choice of occupation was gender-specific in a pattern that conforms with strong gender segregation in the labor market. Finally, Figure 4 presents the corresponding ES graphs for the medium-term effects and suggests no pre-trends and relatively stable effects after the reform.

[Table 3 and Figure 4 is about here]

To my knowledge, the current paper is the only one to study the long-term effects of expanded access to MWs at birth, so it is worthwhile comparing the magnitude of the estimated effects to those taken from existing studies on the effect of access to better health care in infancy. Several studies conducted using Swedish individual-level data have found similar effect sizes. Bhalotra et al. (2021) found that eligibility to take part in a well-child trial program for a year during infancy increased income by 18% in middle age. Lazuka (2018) found that having access to a qualified midwife during a home birth led to an increase of at least 11 percentage points in the likelihood of the individual being healthy and skilled at conscription, which is equivalent to a 21.6% higher income in adulthood. Other studies also reported comparable intention-to-treat effects. Bharadwaj et al. (2013) found that more intensive medical care for newborns with very low birth weight increased the time spent in education, resulting in 2–3% higher earnings. Bütikofer et al. (2018) showed that an infant well-child program in Norway increased adult earnings by 2% between ages 31–50. The arrival of sulpha antibiotics in infancy increased adult income by 3–7% among men in the US (Bhalotra and Venkataramani, 2013) and among men and women in Sweden (Lazuka, 2020). The long-term economic effects on at-risk children have been found to be even larger, similar to the magnitude of the effects on disability and unemployment in this study (e.g., Brown et al., 2020).

C. Validity of the DD Design

As mentioned in Section III, the main identification assumptions in the DD framework are that the control group provides a valid counterfactual (the "parallel trends" assumption) and that the treatment is the only factor influencing the outcomes in the post-treatment period. Both assumptions are essentially untestable, but in the following I provide suggestive evidence of their plausibility.

The ES estimates presented above suggest that there are no significant differences in either short-term or long-term outcomes between the treated groups and the control groups prior to treatment. This can make us more confident that in the absence of treatment there would have been no differences in outcomes between treatment and control in the post-treatment period either; that is, that the parallel trends assumption holds. In Appendix I, I estimated several other specifications that further address the potential threat to the parallel trends assumption. First, I included interactions between the set of baseline region-of-birth characteristics in 1930 and linear time trends to control for the effects of health, income and other regional factors on the development of later-life outcomes across cohorts. Second, I controlled for the interactions between observable municipality-level characteristics measuring income and provision of health care and other public goods, and linear time trends. The inclusion of quadratic trends instead produced similar results across all checks. Third, I added observable parental characteristics or mother fixed effects. The estimated effects are not statistically different from those presented in the main body of the paper.

There are several potential threats to the second assumption that treatment alone affected the outcomes in the post-treatment period. First, other early-life reforms or events affecting infant health in both the short and long term might have had confounding or interaction effects with the MW reform. One such intervention was the arrival of sulphapyridine, a drug effective against pneumonia, which was the largest threat to infant health at that time. This drug was introduced in Sweden in 1939 and yielded long-run beneficial impacts on the health and labor income of the survivors into adulthood (Lazuka, 2020). Another such intervention, the well-child program, provided postnatal care in the form of information, support, and the monitoring of infant health through home nursery visits and doctor check-ups at local clinics. The program was first initiated in 1931–1933 as a trial covering around 60 municipalities, and it led to a short-term reduction in infant mortality (primarily due to preterm birth and low birth weight) and a long-term increase in survival for both sexes and in earnings among women (Bhalotra et al., 2017; Bhalotra et al., 2021). However, (Knutsson, 2018) found no long-term effects of the nationwide rollout of the program in 1938–1947.[24] To study the influence of these cointerventions on the impact of the MW reform, I estimated the following model separately for the two cointerventions described above:

$$(3) \quad y_{(i)mb} = \alpha + \beta_1 \, MW_m \text{ x } post_{mb} + \beta_2 \, Cointervention_{m(r)b} + \\ + (\beta_3 \, MW_m \text{ x } post_{mb} \text{ x } Cointervention_{m(r)b}) + \delta_m + \mu_b + \mathbf{X}_i + (\gamma_{r(m)b}) + \varepsilon_{(i)mb}$$

[24] Knutsson (2018) suggests that the reform eased access of families to sulpha drugs so the long-term effects were exercised through this cointervention instead.

where $Cointervention_{m(r)b}$ is a DD indicator constructed separately for the two interventions. For sulphapyridine, it is equal to 1 for individuals born after 1938 in regions where the baseline pneumonia rate was above the 80th percentile of the pneumonia rate distribution, and is zero otherwise.[25] For the well-child program, it is equal to 1 for individuals born in the year of or after the opening of a well-child center in their municipality of birth, and is zero otherwise.[26] The county-by-urbanization fixed effects are excluded from the specification for sulphapyridine because pneumonia rates are available at that level.

I conducted this test in two different ways. First, I estimated whether the cointervention confounded the impact of the MW reform by including only the DD treatment indicators in Eq.3, MW_m x $post_{mb}$ and $Cointervention_{m(r)b}$. In this case, β_1 should not be statistically different from the MW reform estimate obtained based on Eq.1. The results are shown for neonatal mortality and log average labor income in Panel A of Table 4. They indicate that the two cointerventions did not bias the effect of the MW reform in either the short or the long term: adding a DD indicator for each of the cointerventions to Eq.1 yields estimates for the MW reform that are similar in statistical terms to those obtained from Eq.1.

Second, I added the DD-by-DD indicator, MW_m x $post_{mb}$ x $Cointervention_{m(r)b}$, which captures the interaction effect between the MW reform and the cointervention. In this case, β_1

[25] As shown in Lazuka (2020), regions with higher pre-1938 pneumonia rates followed sharper decreases in pneumonia mortality after the arrival of sulphapyridine in 1939 that should translate into higher long-term economic outcomes. The binary indicator was chosen for ease of interpretation across interventions.

[26] Information on the opening of the well-child infant centres at the municipality level has been collected from Riksarkivet (1938-1946). Pneumonia mortality rates come from official statistical yearbooks.

measures the impact of the MW reform without the cointervention and β_3 measures the additional impact due to the existence of the cointervention at the same time as the MW reform.[27] The results shown in Panel B of Table 4 suggest that the interventions were likely to have interacted in an additive way. The strong beneficial effects of the MW reform emerge regardless of whether another intervention was in place or not: individuals born in municipalities not affected by the arrival of sulphapyridine (well-child reform) experienced a reduction in early neonatal mortality of 19 (11) deaths per 1,000 live births and an increase in long-term labor income of 4.7 (3.7) %.[28] Having both the MW reform and a cointervention in place produced additional benefits. In the short term, the interaction effect led to an additional reduction of 8 deaths per 1,000 live births for each of the cointerventions. Interestingly, the benefits emerge for each of the cointerventions *only* once the MW reform was in place. This points to the reduction in exposure to infection (from sulphapyridine) and to the importance of the early detection of growth-retarded newborns at a MW who could be referred to doctors

[27] To interpret the interaction effects as causal requires an additional assumption that the timing of the cointervention is uncorrelated with the MW reform rollout. This assumption is likely to hold; conditional on the covariates in Eq.3, the estimates of the correlation between the DD indicator for MW reform and the DD indicator of each cointervention is as follows: for sulphapyridine it is equal to 0.036 (standard error is 0.041), and for the well-child program it is equal to -0.011 (standard error is 0.031). Parallel trends assumption is also required to hold for each of these cointerventions. See the evidence for this in Lazuka (2020) for sulphapyridine and in Knutsson (2018) for the well-child program.

[28] In additional analyses, I found that no other program or shock explained the effect of the MW reform (see Appendix J) and that the timing of the MW openings was uncorrelated with the majority of important determinants of early-life health (Appendix K).

and nurses for postnatal care (from the well-child program) as being important treatment components. In the long term, the estimates for the interaction effects are positive, suggestive of the reinforcement, although not statistically significant.

[Table 4 is about here]

Another potential violation of the assumption that treatment is the only factor affecting the outcomes in the post-treatment period is reform-induced compositional changes. For example, the members of the cohorts under study who survived until they were recorded in the administrative registers may be a potentially selected sample due to the effects of the reform on survival in the neonatal period and plausibly throughout the life cycle. However, previous studies have argued that the marginal survivor of an intervention tends to be negatively selected (e.g., Bhalotra and Venkataramani, 2013), in which case the long-run estimates are likely to be biased downwards. In addition, the reform could induce selective migration or fertility responses among the parents of the cohorts under study. The reform organizers expected that the state subsidies for childbirth might induce mothers to have more children (Socialdepartamentet, 1929). At the same time, there was a large migration from rural to the more densely populated areas where MWs were situated (Gyllenswärd, 1946).

In Appendix L, I checked whether several scenarios that potentially led to compositional changes could bias my findings. First, the results show no effects of the reform on fertility, in either the initial sample or the adult sample. There is some evidence of induced fertility for one subsample, albeit with no differences among high and low-resource families. Second, there are no overall or heterogeneous effects of the reform on migration. Third, the long-term effects are not affected by selective mortality due to the reduction in early neonatal mortality after the reform. This test was performed as a bounding exercise, by dropping individuals in the control group whose labor income distribution was likely to be right-skewed due to a disproportionately smaller share of fragile births. Fourth, a two-stage Heckman selection procedure (Heckman, 1979) and an inverse probability of participation weighting based on individual background characteristics (Bonander et al., 2019) confirm that no selective mortality effects are present in the adult sample due to left-truncation.

Finally, I performed several robustness estimations in order to confirm that the results are not affected by the definition of treatment (see Appendix M) and by the potential measurement error in the registration of the place of birth (see Appendix N).

VI. Additional Analyses

A. Heterogeneity in the Effects by Type of MW

To pinpoint the most effective components of the MW reform, I estimated separately the effects across MWs of different types: *Type I* (large-scale MWs), *Type II* (mid-scale hospital-based MWs), and *private* MWs (the smallest wards in elderly, social care and midwifery homes). These effects were estimated by replacing the DD indicator in Eq.1 with three separate interaction terms, one for each type of MW. We expect *Type I* MWs to have produced the largest benefits because, although a mother could receive basic childbirth and emergency care and the opportunity for rest in any ward, only hospital-based wards had facilities for high-quality neonatal preventive and supportive care (Riksarkivet, 1930-1936). For instance, care of preterm neonates with respirators, heating reservoirs and special feeding could be provided only by hospital-based facilities. However, these benefits could have been partially offset by the fact that the largest hospitals tended to suffer from overcrowding, leading to cross-infection and to reduced postpartum stays (Gyllenswärd, 1946).

Indeed, the results in Table 5 show that although all types of MWs led to reductions in early neonatal mortality there is an obvious gradient in line with earlier expectations. The largest effects are observed for mid-scale hospital-based MWs, followed by large-scale MWs and private MWs. These results point to the benefits of being born in a healthcare facility as opposed to a home for full-term newborns. Similar to the pattern found for early-life health, the long-term effect of the MW reform on labor income is largest for *Type II* wards, followed by *Type I* MWs.

[Table 5 is about here]

B. Distributional Effects

To explore the sources of the average long-term impact of the MW reform and to further analyze the mechanisms, Figure 5 presents unconditional quantile estimates for the log of labor income in adulthood overall and by type of MW (Firpo et al., 2009). The results show that the mean effect on labor income was driven by changes in income below the 40th percentile of the labor income distribution, after which the effects declined and levelled off somewhat at about 1.7%. This pattern is consistent with the expectation that the benefits should have been larger for the unhealthiest individuals, supporting the previous findings on the large mean impact on unemployment and disability. However, as the effects are not limited to the lowest percentiles but rather emerge for individuals across the whole distribution, the marginal newborn who was assured better chances of health by being born in a MW had a normal birth weight.

While the overall pattern combines the effects due to all types of MWs, important differences emerge once each MW type is studied separately. Similar to the analysis in the previous section, the largest effects are typically found for *Type II* MWs, followed by *Type I* MWs and private MWs. Similar to the overall pattern, *Type I* and *Type II* MWs produced the highest benefits for individuals below the 40th percentile of the labor income distribution, and smaller and relatively constant benefits for individuals with a higher income. This points to the importance of the number and quality of the childbirth and neonatal services provided. In contrast, private MWs tended to provide small and relatively constant benefits along the entire distribution. Still, all three types of MWs yielded positive benefits across the entire distribution of labor income, suggesting that childbirth in a specialized facility produces long-term benefits as opposed to that produced by a home birth.

[Figure 5 is about here]

Going back to the mediators, the returns to education and health inputs due to the MW reform account for the full impact of the MW reform on labor income across its distribution. The average rate of return to one year of schooling estimated from a Mincer equation in our sample is around 13%, and the return to secondary schooling is 93%. As for the returns to health, one additional night in hospital in adulthood is associated with 0.5% lower labor income. Hence, a longer period spent in education accounts for 1-1.5% (0.08 x 13% for total years of schooling and 0.016 x 93% for secondary schooling) higher income, and fewer hospitalizations resulted in 1% (2.1 x 0.5%) higher income. Put together, these effects align well with the effect of the reform for individuals in the higher percentiles of the labor income distribution (see Figure 5). For individuals in the lower half of the distribution, let us recall that the effect of the reform on health-related disability receipt amounted to 10% of the pre-mean. These calculations provide suggestive evidence that the MW reform pushed the unhealthiest members of population into the labor force due to improvements in health, and increased labor income through health and education for the middle or higher earners.

C. Social Rate of Return

A "back-of-the-envelope" calculation points to high societal returns from investment in MWs. Among the benefits, I include the discounted increase in labor earnings due to the MW reform (based on the estimates from previous sections) through ages 47–64, summed across the cohorts, and the total value of saved neonatal lives. The discount rate is set to 3.4%, the real return on long-term government bonds in 1931–1946. The value of a statistical life was adjusted for the growth rate of GDP per capita between 1931–2012 (181) and the elasticity of the value of a statistical life to income (2.3). Detailed information on the costs of each MW opened during the reform years, including both government subsidies and patient fees, comes from the official statistical sources (Socialstyrelsen, 1931-1946). Based on these figures, the overall social rate of return to the MW reform (i.e., the ratio of benefits to costs) is 22/1. I also calculated that the internal rate of return, the discount rate that equalizes the present values of long-term benefits and costs, was equal to 11%. Both of these numbers match well with previous studies.[29] Mirroring the differences in benefits between MWs of different types that were not outweighed by the associated costs, the social rate of return is estimated to be a ratio of 24/1 for *Type I* MWs, 45/1 for *Type II* hospital-based MWs, and 3/1 for *private* MWs. In conclusion, the opening of mid-scale hospital-based childbirth facilities yielded the highest efficiency in the long run.

VII. Conclusion

This paper has studied the short and long-term benefits of the reform that led to the opening of MWs throughout Sweden in 1931–1946. It adds to the previous literature on early-life investments by being the first to find sizable long-term economic effects of the opening of new MWs (Almond et al., 2018). It finds that this reform led to an increase in the share of hospital births (by 19 percentage points) and a reduction in neonatal mortality (by 20 deaths per 1,000 live births) in the short term. The long-term (reduced-form) effects on economic outcomes are sizable and robust: 4.3% for labour income, 10% for unemployment and 11% for health-related disability pension receipt. Regarding the mediators, the results suggest that the MW reform included pushing the unhealthiest members of the population into the labour force due to improvements in health, and drove labour income performance through health and education for the middle or higher earner. The average treatment effect on

[29] For instance, Bütikofer et al. (2018) found that the internal rate of return to a home-visiting infant program was 8.4%. Garcia et al. (2020) found the rate of 13.7% for the early childhood program targeting disadvantaged children, with an associated benefit/cost ratio of 7.

the treated is 22.6% for labour income in adulthood. The results appear robust to multiple analyses that operationalize the effects of the overlapping reforms, selective processes, heterogeneous treatment effects, and a plausible measurement error.

Being born in a MW leaves an imprint on individuals in a very narrow age window – at birth and up to a week after birth – making this critical for human capital accumulation. Several sets of results point to the most productive treatment components of the reform. First, results indicate that a marginal newborn treated by a MW had a normal birth weight while a marginal newborn saved by a MW weighed no less than 2,000–2,500 grams. To compare, prior to the study period, trained midwives could save full-term neonates with access to antiseptics and preventive procedures at home, in contrast to the harmful techniques of traditional birth attendants, but were helpless in the case of premature newborns (Lazuka, 2018). A few decades after the study period, the use of lung surfactant could save those babies weighing around 1,500 grams and increase their future prospects (Bharadwaj et al., 2013). Second, there are long-term beneficial effects of the reform not only on cardiovascular morbidities and arthritis, linking long-term health to early-life exposure to, but also on mental and nervous diseases, pointing to a lower early-life stress response due to better care after birth. Third, while the effects of access to MWs were not influenced by the effects from early-life cointerventions, the MW reform determined the appearance of the cointerventions' effects, thereby pointing to the importance of early detection of growth-retarded births and treatment of infectious disease. Finally, this paper establishes the highest short and long-term efficiency of mid-scale hospital-based MWs.

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