Midwives and Maternal Mortality: Evidence from a Midwifery Policy Experiment in Sweden in the 19th Century

Per Pettersson-Lidbom

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Abstract
This paper estimates the causal effect of a historical midwifery policy experiment on maternal mortality, infant mortality and stillbirth in the 19th century in Sweden. Specifically, as a source of exogenous variation, it exploits sharp changes or “discontinuities” across time and place in the availability of trained and licensed midwives during the period 1830 to 1894. I find that a doubling of trained midwives leads to a 20-40 percent reduction in maternal mortality and to a 20 percent increase in the uptake of midwife-assisted homebirths. My results therefore suggest that a one percent increase in the share of midwife-assisted homebirths could potentially decrease maternal mortality by as much as two percent, which is a remarkable finding given that the midwife training was only 6-12 months at that time. The results from this study may therefore provide the current debate with information about the most effective strategy for reducing the unacceptably high maternal mortality in many developing countries, especially in very low-resource settings.

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# Department of Economics, Stockholm University, E-mail: pp@ne.su.se
1. Introduction

It has been estimated that every day, about 800 women die as a result of pregnancy or childbirth complications around the world and almost all maternal deaths occur in developing countries (WHO 2014). The unacceptably high maternal mortality ratio, often more than 500 maternal deaths per 100,000 live births, in many countries is therefore considered to be a key policy issue. Consequently, one of the United Nations Millennium Development Goals is to reduce maternal mortality by 75 percent until 2015.

However, given the important task of reducing maternal mortality in developing countries, we know surprisingly little about what type of health intervention actually works in these low-resource settings (e.g., Campbell and Graham (2006)). In fact, it has even proven to be extremely difficult to establish a causal relationship between maternal mortality and birth with a skilled birth attendant (e.g., midwife, physician, obstetrician, nurse, or any other health care professional) in any type of setting. This is perhaps not surprising since a credible impact evaluation faces a number of severe challenges. To begin with, the absolute numbers of maternal deaths are generally small, and extremely large samples are therefore needed to investigate the determinants of maternal mortality (e.g., Ronsmans et al. (2008)). As a result, randomized control trials (RCT), the gold standard in impact evaluation studies, are generally not feasible. In addition, there is also a shortage of reliable information on maternal mortality and what type of birth attendant that assisted the birth (e.g., Graham (2002) and Ronsmans and Graham (2006)).

Although non-experimental studies can overcome some of these problems, they still face the difficulty of establishing a causal relationship since they typically do not make use of credible research designs (e.g., Graham et al. (2001) and Scott and Ronsmans (2009)).

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1 Jokhio et al. (2005) conduct a clustered RCT consisting of the training of traditional birth attendants in Pakistan. However, despite the fact that there were about 10,000 births in both the treatment and the control group, this RCT had very low power to detect any effects on maternal mortality due to the small number of maternal deaths in both the treatment (27 deaths) and the control group (34 deaths). Thus, this study clearly illustrates the problem of sample size.

2 Attaran (2005) also argues: “that many of the most important MDGs, including those to reduce…maternal mortality…suffer from a worrying lack of scientifically valid data.” Thus, he concludes that: “one cannot know if true progress towards these very important goals is occurring.”

3 See Sanson-Fischer et al. (2007) for a discussion of why it may be more attractive to use an observational study design rather than an experimental design when evaluating a population-based health intervention.

4 An exception is Fauveau et al. (1993) who analyze a maternity care program in Matlab, Bangladesh. They find evidence that MMR is lower in the intervention area as compared to a control area. However, this
fact, many observational studies show that giving birth with a health professional actually
*increases* the risk of dying in childbirth. This counterintuitive finding strongly suggests
that these studies are plagued by severe selection bias, i.e., women with delivery
complications seek professional help. Studies based on historical data are also
inconclusive as noted by Loudon (1992) in his study of the determinants of maternal
mortality in various countries in the 19th century. Yet, another problem in establishing a
causal relationship between birth with a health professional and maternal mortality is that
health interventions aimed at reducing maternal mortality usually consist of many
components (e.g., maternity clinic staffed by female physicians, system for referral and
transport of women with complications) and it is therefore difficult to disentangle the role
of the birth attendants in reducing maternal mortality from these other components (e.g.,
Maine et al. (1996)).

To make progress on the important problem of establishing a causal relationship
between birth with a skilled health professional and maternal mortality, I will make use of
a unique midwifery policy experiment in Sweden in the 19th century. With this new
data, I can overcome most, if not all, of the impact evaluation problems discussed above.
To start with, Sweden is one of the very few countries that have high quality vital
statistics at the local level covering the universe of the Swedish population from the 18th
century on an annual basis. In fact, the statistical analysis can thus be based on extremely
large sample sizes since there were roughly 120,000 births and 600 maternal deaths on a
yearly basis. Consequently, my analysis will be based on a total of 8,012,080 (live and
still) births and 37,519 maternal deaths since the data covers the period 1830-1894. With
this new data, it is also possible to exploit exogenous sources of variations in one
particular type of health intervention. At that time, Sweden had a midwifery policy
consisting of home-based intrapartum care by trained and licensed midwives.
Specifically, two distinct empirical research designs can be implemented. One design
exploits time-varying geographical supply shocks or “discontinuities” in the availability

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5 Loudon writes “it is extremely difficult to find statistical evidence that trained midwives lowered the
MMR of any country or any region in the nineteenth century” (p. 414)
6 I have collected this data myself from the Swedish National Archives and other sources. See the web
appendix for further information.
of trained midwives while the other design makes use of the opening of the new midwifery school which greatly increased the supply of trained midwives in those areas closest to the midwifery school. In other words, this paper uses two types of quasi-experimental designs to estimate the *causal* effect of midwives on maternal mortality. Here it is important to stress that midwife-assisted homebirth was not confounded by the availability of doctors or any other type of health referral system. Put differently, Swedish midwives were in charge of all homebirths including any complications associated with the deliveries. Another great advantage of the Swedish data is that it is possible to estimate the relationship between midwife-assisted homebirths and maternal mortality. This is related to the fact that it was recorded whether a birth was attended by a trained midwife or not for the universe of births since 1860. On average the share of midwife-assisted births was 57 percent but the geographical variation was extremely large, i.e., from 5 percent to almost 100 percent.

The result of this paper indicates that a doubling in the number of trained midwives led to a 20-40 percent reduction in the MMR during the period 1830-1894. However, the effect is nearly twice as large for the period after 1860, which is consistent with the fact that midwives in the later period had more midwifery education. I also estimate the uptake of the midwifery policy for the period after 1860. I find that a doubling of the number of midwives led to a 20 percent increase in the take-up of the midwifery policy. As a result, a one percent increase in the share of midwife-assisted homebirths decreased maternal mortality by about two percent.

I also find that the relationship between midwife-assisted homebirth and MMR is little affected by the context (e.g., harvest failures, negative supply shocks to availability of midwives) even though the take-up rate of the midwifery policy differed significantly across contexts. Thus, that this relationship is very robust bolsters the claim to both internal and external validity of my results.

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7 Worldwide, about one third of births take place without the assistance of skilled health personnel. However, only about one in two births in sub-Saharan Africa and South Asia are attended by a skilled provider.

8 Interestingly, my estimated effect is of similar magnitude as those produced from a modelling approach by Homer et al. (2014). They show that scaling up midwifery could help reduce maternal mortality, even in resource constrained environments. For example, a recurrent 5-year increase of 10 percent coverage of the interventions delivered by midwives would lead to a 27 percent drop in maternal mortality.
In addition, a number of other specification checks also lends further support to a causal interpretation of my findings. Most importantly, I test whether my source of identifying variation—the sharp changes in the availability of midwives across time and place—is “as good as random”. Fortunately, I find no relationship between the discontinuities in midwife availability and other potentially important confounding factors such as fertility, female mortality (from other causes than maternal mortality) and various proxies for economic development and other types of economic shocks (e.g., crop yield). In my empirical design, it is also possible to control simultaneously for time-invariant omitted factors as well as a lagged dependent variable—the lagged MMR—without introducing any Hurwitz-bias (Nickell (1981)). Importantly, controlling for the lagged MMR has no impact on the estimated effects. Finally, I follow the suggestion of Solon et al. (2013) of comparing un-weighted estimation with weighted estimation as a useful test against model misspecification, i.e., the presence of unmodeled heterogeneity of effects. Importantly, there is no difference between the unweighted and weighted specifications which therefore, once again, suggests that there is little population heterogeneity in the estimated effect.

I argue that my finding, i.e. that home-based intrapartum care by midwives with only 6-12 months of formal training had a large effect on reducing MMR in 19th century Sweden, has potentially important implications for thinking about the most effective health intervention for reducing the currently very high MMR in low resource settings. This reasoning is based on the fact that in the 19th century, Sweden was a very poor agrarian society and, in many respects, similar to many developing countries today, i.e. a very high MMR, a very high infant mortality rate, a very low life expectancy and a high fertility rate. Moreover, the fact that many of the major causes of maternal mortality, such as hemorrhage, are similar across these two settings also bolsters my claim of external validity. As a result, it is possible to argue that having a skilled attendance at home may be a preferable strategy in very low-resource settings since home births may increase the coverage of skilled attendance in rural areas and respond to women’s demand for home-based care. Moreover, training, deployment and retention of midwives

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9 See also Graham (2001), Högborg (1985, 2004) and Loudon (2000) for related and other arguments for the benefits of using historical data in order to learn how to reduce maternal mortality in the developing world today.
are crucial tools for breaking through supply barriers. Consequently, a home-based care strategy with a short midwifery training program may be an attractive strategy since it is easier to recruit these midwives because such a strategy requires fewer educational criteria. It is also easier to retain midwives with a short midwifery course since they would then have fewer outside options. Finally, this type of birth attendants may be more acceptable to women than other health professionals, such as doctors, because of the smaller cultural distance from the women whom they serve.

In this paper, I also analyze whether infant mortality and stillbirth were affected by the availability of midwives using the exact same empirical design as previously discussed. Perhaps somewhat surprisingly, I find that midwives had no effects on these two outcomes. However, regarding the absence of the effect on infant mortality, it is important to note that during the 19th century, most infant deaths occurred after the first month of birth, i.e., at a time when all midwives had already left their newly delivered women. Thus, this finding raises the important question if different types of health interventions are required in the developing world today if both infant and maternal mortality are to be simultaneously reduced. When it comes to stillbirths, it is also important to realize that there is a current debate in the medical literature of whether perinatal mortality can be used as a proxy for maternal mortality and maternal health care status (e.g., Campbell et al. (1995) and Alkalin et al. (1997)). The result from this study shows that that stillbirth cannot be used as a proxy for maternal mortality, at least not for the 19th century.

This paper is related to several literatures in different fields. In economics, Miller (2006) measures the impact of midwifery-promoting public policies on maternity care in the United States for the years 1989-1999. She uses state reimbursements laws as an exogenous source of variation. She does not find any effects on maternal mortality. However, the treatment comparison is between births assisted by midwives or physicians and not, as in this paper, between trained midwives and traditional birth attendants. There is also some other related work in economics. For example, Jayachandran et al. (2010) evaluate the impact of the sulfa drug on maternal mortality in the U.S. while

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10 Loudon (1992) also finds little or no relationship between maternal mortality and infant mortality in the historical data and therefore, he concludes: “it is clear that measures designed to reduce maternal and infant mortality required quite different approaches”.

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Jayachandran and Lleras-Muney (2009) analyze the impact of the decline in maternal mortality (due to various health interventions) on women’s human capital investments in Sri Lanka.\textsuperscript{11} Another related study is Miller (2008) which uses the same type of identifying strategy to evaluate the impact of health interventions (induced by changes in woman suffrage laws) on cause- and age-specific mortality in the U.S. There is also a large literature in the medical sciences analyzing the impact of various health interventions, such as deployment of midwives, on maternal mortality (e.g., Fauveau et al. (1993), Jokhio et al. (2005) and Ronsmans et al. (2007)). Moreover, there is also a literature using historical data to investigate this question (e.g., Loudoun (1992, 2000), Högberg et al. (1986, 1988) and Högberg (2004)).\textsuperscript{12} This paper is also related to the current policy debate on the best way of preventing maternal mortality (e.g., The Lancet maternal survival series).\textsuperscript{13}

The rest of the paper is structured as follows. Section 2 provides background and discusses the data. Section 3 presents the empirical designs and results for the relationship between midwife availability and maternal mortality. Section 4 provides evidence for the relationship between midwives and stillbirths while Section 5 concludes the paper.

2. Background and Data
In this section, I provide information about the causes of maternal mortality, the Swedish midwifery policy and the data used in the empirical analysis. However, before this discussion, it is perhaps useful to briefly describe the general economic and social setting in the 19\textsuperscript{th} century in Sweden. In the mid-19th century, Sweden’s GDP per capita was more than 20 times smaller than today. The share of people working in the agricultural

\textsuperscript{11} See also Albanesi (2011).
\textsuperscript{12} Högberg et al. (1986) and Högberg (2004) also analyze the relationship between MMR and midwife-assisted births using historical Swedish data. However, they only compute the preventive fractions of maternal deaths without controlling for any confounders. This type of epidemiological approach can therefore not identify any causal relationships. In addition, these studies have also been criticized on other grounds. One issue concerns the fact that they exclude maternal deaths due to puerperal fever from the measure of MMR, which is a serious problem according to Loudon (1992). In sharp contrast, this study uses a credible identification strategy and includes all maternal deaths.
\textsuperscript{13} In 2006, The Lancet had a series of papers on the best way of reducing the burden of maternal mortality in developing countries. Four types of health strategies were discussed: (i) health center intrapartum care, (ii) skilled attendance at home, (iii) community health workers at home and (iv) relatives or traditional birth attendance at home. The recommendation was to use the health center intrapartum care strategy since skilled attendance at home was not considered a viable option.
sector was about 80 percent and the share of the rural population 90 percent. During the period 1800-1850, the crude birth rate was 30-36 per thousand while the crude death rate was 25-30 per thousand. The average life expectancy was about 40 years and the fertility rate was 4.5 children per woman. The maternal mortality ratio was about 600 deaths per 100,000 births while the infant mortality was higher than 150 deaths per 1,000 live births. This short description makes it very clear that Sweden in the 19th century was a very poor agrarian society and, in many respects, similar to many developing countries today.

2.1 Maternal mortality
The maternal mortality ratio is defined as the number of maternal deaths per 100,000 live births. The current definition of a maternal death includes both direct and indirect obstetric causes within 42 days after birth. Today, the vast majority of maternal deaths (75 percent) are due to direct obstetric complications due to (i) hemorrhage (uncontrolled bleeding): 27 percent, infections (sepsis or puerperal fever): 11 percent, (iii) hypertensive disorders (eclampsia): 14 percent, (iv) obstructed labor: 9 percent, and (v) complications from abortion: 8 percent (Say et al. (2014)). It is important to note that these birth complications occur even in well-nourished, well-educated women receiving adequate prenatal and delivery care and can generally not be predicted (e.g., Gabrysch and Campbell (2009) and Paxton et al. (2005)).

In Swedish historical data, a maternal death was defined as a death of a woman caused by complications of pregnancy, labor or puerperium. Thus, this definition basically means that only direct obstetrics maternal deaths should be recorded. Högb erg and Broström (1986) investigate the causes of maternal mortality in the 19th century in Sweden using individual data from 7 of about 2,500 parishes. In their sample, they find that 69 percent of all known maternal death causes were due to direct obstetric complications. Of these cases, only 11 percent were due to infections while the others were due to difficult labor, eclampsia and hemorrhage.15

14 Maternal death is the death of a woman while pregnant or within 42 days of termination of pregnancy, irrespective of the duration and site of the pregnancy, from any cause related to or aggravated by the pregnancy or its management but not from accidental or incidental causes (WHO).
15 Moreover, they argue that the diagnosis puerperal fever was likely not confounded by septic abortions during the 19th century.
To summarize, the comparison of death causes of maternal mortality between the developing countries today and Sweden in the 19\textsuperscript{th} century suggests a high degree of comparability between the two settings.

\subsection*{2.2 Sweden's midwifery policy\textsuperscript{16}}

Sweden has had a long tradition of thorough training and close regulation of midwives since the 18th century. During the early 19th century, the Swedish health authorities started to deploy trained midwives in places with a severe shortage of midwives, i.e., in parishes with no midwives. The capacity to train and certify midwives was, however, very severely limited, since it had been decided that only one single midwifery school, which was placed in Stockholm (the capital), should supply midwives to all 24 Swedish regions except two.\textsuperscript{17} The key determinant of how many midwives that could be trained annually was the number of women giving birth at the Lying-in-Hospital of Stockholm. For example, during the period 1821-1840, only, on average, 230 women gave birth annually at that hospital. For this reason, only 26 midwives graduated annually from the Stockholm midwifery school until 1822. During the period 1823-1842, there was an increase in the number of graduates to about 37 per year. In 1856, a new midwifery school was put into place in the city of Gothenburg, increasing the total supply of trained midwives to about 80. In addition, to further boost the supply of midwives in rural areas with a shortage of midwives, the Swedish health authorities paid the allowances for 18-24 midwife students conditional on their being deployed in areas with a shortage of midwives.

Figure 1 shows the increase in the total number of midwives in Sweden during the period 1830-1894. In 1830, the number of midwives was 988, which had increased to 2,585 in 1894. The level and the trend in the total number of midwives can also be compared to the total number of doctors: in 1820, there were 379 doctors, which had increased to 964 in 1894. However, it is important to note that the numbers of doctors available to the general public (i.e., “provinsialläkare”) were much fewer. There were only 94 such doctors in 1820 and 138 in 1894. These doctors were employed by the

\textsuperscript{16} This section is based on Högberg (2004), Romlid (1996, 1998) and Lundqvist (1940).

\textsuperscript{17} The regions of Malmöhus län och Kristianstad län had their own-midwifery school in Lund. On average, less than 10 midwives graduated annually from Lund during the 19\textsuperscript{th} century.
Swedish central government while the midwives were employed by one of the about 2,500 parishes.

The requirement to qualify for the midwife-training program was that women should have a basic knowledge in reading and writing. From 1819, the formal training period was 6 months and from 1840, the training period was 9 months. The basic training included: manual removal of placenta, extraction in breech presentation, internal, external and combined versions. Midwives were also trained to reduce postpartum bleeding with the practice of aortic compression and compression of the uterus. From 1819, qualified midwives could receive 3 months of additional training on how to use obstetrical instruments (delivery forceps, sharp and blunt hooks, perforators).

In Sweden, midwives were basically in charge of all deliveries since home births constituted close to 100 percent of all births. For example, only 2.8 percent of all births were delivered in hospitals as late as 1894. Moreover, there were no referrals of women with obstetric complications to hospitals or doctors since many of the midwives were trained and certified to do obstetrical operations. However, delivery forceps were only used 200-600 times per year. Thus, there were very few interventions since they constitute less than 0.5 percent of all deliveries. Moreover, sharp hooks and perforators were only used 5-32 times per year. The average number of deliveries per midwife and year in the rural areas was about 37 during the second half of the 19th century. This number may seem low, but it is important to stress that midwives were required by law to care for the mother and the newborn as long as it was required and this explains why a midwife could only attend a limited number of births each year. The share of midwife-assisted births constituted 36 percent in 1861 while the share had increased to 78 percent in 1894.

2.3 Data
My data set includes information on the universe of total number of births (both still and live) during the period 1830 to 1894. There were altogether 7,770,239 live births and 241,841 stillbirths during this period. There were also 37,519 maternal deaths which

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18 It was not until 1842 that Sweden introduced compulsory basic education but it took a long time to implement. Moreover, there were no requirements on the minimum formal years of schooling which implied that many children still received little or no education even after 1842.
implies an average of 482 MMR over the whole period. The number of female deaths from other causes than maternal mortality was 2,408,397. The number of infants dying before the age of one was 1,062,413, i.e., implying an infant mortality ratio, IMR, of 133. The age distribution of mothers was as follows: 2.1 percent under the age of 21, 14.4 percent for ages 21-25, 25.7 percent for ages 26-30, 26.2 percent for ages 31-35, 20.5 percent for ages 36-40, 10.6 percent for ages 41-45, and 1.5 percent for ages 46-50.

In the empirical analysis, data on 25 geographical areas will be used (24 regions (“län”) and the city of Stockholm). Figure 2 shows a map of these geographical areas. There is considerable variation in both the cross-section and the time-series in these areas for MMR, the number of midwives and the share of midwife-assisted births. For example, in the first year of our sample, 1830, the mean of MMR was 622 with a maximum of 1274 and a minimum of 296. At the end of the sample, 1894, the mean MMR was 288, where the highest MMR was 458 while the lowest was 144. For midwives, the average number of midwives was 40 in 1830 with a minimum of 5 and a maximum of 202. In 1894, the average number of midwives had increased to 103 where the minimum was 43 and the maximum was 346. The average share of midwife-assisted births was 0.57 but this share could be as low as 0.05 and has high 0.99. Table 1 displays the summary statistics of the regional data.

3. The empirical designs and results
Absent a randomized controlled trial (RCT), estimating the impact of a health intervention, such as increasing the availability of midwives, on maternal mortality, one would ideally estimate an equation of the following form

\[
\log(MMR_{gt}) = \alpha + \beta(midwife\ availability_{gt}) + \nu_{gt},
\]

where the dependent variable, \( \log(MMR_{gt}) \), is the natural log of the maternal mortality ratio (number of maternal deaths per 100,000 births) in a geographical area \( g \) in year \( t \).\(^{19}\)

The independent variable would preferably be a measure of midwife availability, i.e., a supply shock, that is uncorrelated with the demand for midwives, i.e., unrelated to the

\(^{19}\)Here, I follow the empirical framework laid out by Jayachandran et al. (2010) for estimating the relationship between the availability of the sulfa drug and MMR.
unobserved factors in the error term $v_{gt}$. In this case, the parameter $\beta$ would be the causal effect of midwife availability on MMR and it can be considered as an intention-to-treat effect, which is one parameter of interest in an experimental design with partial compliance with the treatment protocol. The hypothesis is that when the supply of midwives increases; MMR falls, i.e., $\beta<0$.

It is also important to estimate the causal effect of midwife-assisted births on maternal mortality. In this case, we would estimate a regression of the following form

$$\log(MMR_{gt}) = a + b(\text{share of midwife-assisted births}_{gt}) + n_{gt},$$

where $b$ is the coefficient measuring the causal effect of midwife-assisted births on MMR. In order to estimate this parameter, we need to measure the take-up of the policy, i.e., the number or share of midwife-assisted births. Thus, we need to estimate a regression of the form

$$\log(\text{share of midwife-assisted births}_{gt}) = \alpha + \pi(\text{midwife availability}_{gt}) + n_{gt},$$

where the parameter $\pi$ is the effect of the take-up of midwifery policy. The estimate of the causal effect of midwife-assisted births on MMR, i.e., the coefficient $b$, will therefore be equal to the ratio of the reduced form effect, $\beta$, and the first-stage effect, $\pi$.

Here, it is important to stress that estimating the causal effect of the health intervention—the availability of midwives—on maternal mortality and the take-up of the midwifery policy only requires that the intervention is as good as random (e.g., Duflo et al. (2008)). In contrast, estimating the causal effect of midwife-assisted births on maternal mortality also requires an exclusion restriction, namely that the midwifery policy only affected maternal mortality via midwife deliveries. In my context, the exclusion restriction seems plausible since there was no referral system in case of complications during delivery and a trained and licensed midwife was basically not allowed to perform any important medical treatments other than deliveries.

The general idea of my empirical approach is that we can estimate the relationship between midwife availability and maternal mortality by using institutional features of the
supply side of the Swedish health system, i.e., the use of supply-side variables to help resolve identification problems on the demand side of the health market. In this paper, we make use of two sources of exogenous variation in the availability of midwives. The first design is based on supply shocks or sharp “discontinuities” in the availability of midwives across time and place and the second design makes use of the opening of the new midwifery school in the city of Gothenburg in 1856 which dramatically increased the supply of midwives in that part of Sweden. Below we describe the two empirical designs in detail.

3.1 Design 1: Supply shocks in the availability of midwives

The idea of this design is to isolate a supply shock in the availability of midwives that is arguably uncorrelated with the demand for midwives. To implement this design, I make use of the fact that a differences-in-differences design with unit-specific time trends is essentially a type of regression discontinuity design with time as the forcing variable as discussed by Lee and Solon (2011). Similar to other regression discontinuity designs, the identification in a differences-in-differences approach with group-specific time trends is based on the appearance and size of a “jump” in the dependent variable (MMR) at the point of discontinuity, i.e., the date for the supply shock. Thus, in this approach, I will estimate regressions of the form:

\[
\log(MMR_{gt}) = \beta \log(number \ of \ midwives_{gt}) + \alpha_g + \lambda_t + \pi_{gt} + \nu_{gt},
\]

\[g=1,2,..25., \ and \ t=1830,1831,..,1894,\]

where \( MMR \) is the maternal mortality ratio, \( \alpha_g \) is a region-specific effect, \( \lambda_t \) is a time-fixed effect and \( \pi_{gt} \) is a region-specific time trend. The parameter of interest is \( \beta \) and it measures the effect of midwife availability on MMR, i.e., the intent-to-treat effect. The impact is measured in elasticity form since both the outcome and the treatment variable—the number of midwives—is expressed in logarithmic forms.

The log-log specification is useful for a number of reasons. First, it encompasses a number of other reasonable ways of expressing the relationship between midwife availability and MMR.

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\[20\] This is the same type of identification strategy as that used by Miller (2008). The only difference is that his treatment is binary (female suffrage laws) while the treatment here is multi-valued (number of midwives).
availability and maternal mortality, at least as long as fertility, i.e., \( \log(\text{number of births}) \), is included as a covariate in the specification.\(^{21}\) Then, it is possible to express the dependent variables as MMR or as the number of maternal deaths and the independent variable as the number of midwives or as a ratio of midwives to births without changing the estimator of the parameter \( \beta \). Second, a log-log specification narrows the range in the variables which makes the estimates less sensitive to extreme observations. This is of importance here since both MMR and the number of midwives vary considerably. For example, Table 1 shows that MMR varies between 0 and 4,048 while the number of midwives varies between 5 and 377. Third, it seems reasonable to use a log-log specification given that the identification strategy is based on “matching” discontinues or non-linearities in the treatment variable with potential discontinuities in the outcome variable. Thus, in this case, we match large percentage changes in the number of midwives with large percentage changes in maternal mortality.

It is important to note that equation (4) is a pseudo panel data regression, i.e., aggregated data from repeated cross sections with region-fixed and time-fixed effects. Pseudo panels typically raise a number of econometric issues such as measurement errors (e.g., Deaton 1985). However, there are little or no measurement errors in these averages since the data covers the universe of births and the average number of births within a region is rather large, i.e., 4,782 (see Table 1).\(^{22}\)

There is also the question of whether one should estimate the pseudo panel regression by weighted least squares (WLS) and use the number of births as weights in order to return to the micro data relationship, i.e., the underlying microdata set with nearly 8 million births during the period 1830-1894. However, this is an open question since the argument can be made that an unweighted analysis of aggregates is to be preferred (Angrist and Pischke 2009). Nonetheless, Solon et al. (2013) recommend

\(^{21}\) Controlling for fertility raises the important issue of whether one should control for such a variable since it may be endogenous (e.g., a risk averse woman may decide to give birth depending on the availability of midwives) and therefore considered to be a bad control (Angrist and Pischke 2009). However, the inclusion of this variable will only cause a bias in the estimate of \( \beta \) if fertility is related to the availability of midwives. Below we empirically test for such a relationship and the result strongly suggests that there is no relationship between the number of births and the number of midwives.

\(^{22}\) Even if one has data from the entire population, the standard errors can be justified using a generalization of randomization inference (Abadie et al. 2014) if taking the perspective that the regression function is intended to capture causal effects.
reporting both the weighted and unweighted estimates because the contrasts between OLS and WLS estimates can be used as a diagnostic for model specification or endogenous sampling.\textsuperscript{23}

There is also an advantage of having a pseudo panel rather than true panel data since it is possible to control for a lagged dependent variable without introducing Hurwitz-bias in a fixed-effect model (e.g., Wooldridge 2009).\textsuperscript{24} In other words, pseudo panel data makes it possible to work with a model that includes both lagged dependent variables and unobserved group fixed effects. Controlling for lagged outcomes, in addition to region-fixed effects, makes it possible to deal with the potential problem of midwives being placed in regions with already high maternal mortality.

It is also possible to test whether the treatment—the midwifery policy—is as good as randomly assigned. Similar to other regression discontinuity designs, we conduct such an analysis by testing whether there is a discontinuity in any other of the potential confounders conditional on the “forcing” variables, i.e., region-fixed effects, time-fixed effects and region-specific time trends.\textsuperscript{25} Thus, to perform this test, we estimate regressions of the following form

\begin{equation}
    w_{gt} = \lambda \log(\text{number of midwives}_g) + \alpha_g + \lambda_t + \pi_{gt} + v_{gt},
\end{equation}

where $w_{gt}$ is a candidate confounder. We expect the estimate of $\lambda$ to be zero if the sharp changes in the availability of midwives, i.e. the supply shocks, are as good as random. The confounders we use are the log(number births), log(total female deaths except for maternal deaths), log(infant deaths), log(number of doctors), log(female emigration), the age distribution of mothers and seven indicators for harvest yield where 0 corresponds to

\textsuperscript{23} Under exogenous sampling and correct specification of the conditional mean, both OLS and WLS are consistent estimators for the regression coefficients.

\textsuperscript{24} However, even if the data were true panel data, the bias of including a lagged dependent variable would be negligible since the number of time periods (65) is large enough for the bias to be negligible (Nickell 1980).

\textsuperscript{25} See also the discussion in Pischke and Schwandt (2013) about the different ways of testing the identifying assumption. They write “The confounder can be added as a control variable on the right hand side of the regression. The identifying assumption is confirmed if the estimated causal effect of interest is insensitive to this variable addition. Alternatively, the candidate confounder can be placed on the left hand side of the regression instead of the outcome variable. A zero coefficient on the causal variable of interest confirms the identifying assumption. This is analogous to the balancing test typically carried out using baseline characteristics or pre-treatment outcomes in a randomized trial.”
complete harvest failure. Finding that these sets of important confounders are not associated with the placement of midwives would greatly bolster the credibility of the research design. It is also important to note that this set of confounders has a strong predictive power for MMR since the $R^2$ is 0.28 from a regression of MMR on this set of covariates (without controlling for any other variables).

Table 2 shows the results from this test. It shows that there is only one out of 19 estimates that is statistically significant at the 5 percent level. However, this is to be expected since if 20 specifications were to be tested, it is likely that one would be statistically significant by chance. Moreover, most of the estimates in Table 2 are also small and of different signs. Thus, these results provide strong support for the fact that the sharp changes in the availability of midwives can be considered as good as random. Here, it is particularly noteworthy that infant mortality is not related to the availability of midwives.

Table 3 displays the results for the reduced form relationship between MMR and midwives, i.e., specification (4). All specifications except one (Columns 8) are unweighted OLS regressions. The estimate in column 1, without any controls for confounders, is 0.192 and statistically significant at the 5 percent level. Since both the dependent and the independent variables are expressed in logarithmic forms, the interpretation of the estimated coefficient is that a doubling of the number of midwives would lead to a 19 percent reduction in MMR. Adding the confounders has little or no impact on the estimated effect, as can be seen in Columns 2-6. This is also what should be expected from the previous finding, i.e. that these sets of confounders are not related to the midwifery policy conditional on region-fixed effects, time-specific effects and region-specific time trends. The estimated effect is also little affected if a quadratic region-specific time trend is added in Column 7. Finally, there is little difference between the OLS estimate in Column 7 and the WLS estimate in Column 8. Altogether, all specification checks lend support to a causal interpretation of the estimated relationship between MMR and the midwifery policy.

An additional specification check is to control for a lagged dependent variable since it is possible to argue that the midwifery policy could be based on such considerations, e.g., midwives were placed in regions with a previously high MMR. Table 4 present these
results for both the OLS (Columns 1-3) and WLS specifications (Columns 4-6). For ease of comparison, Columns 1 and 4 restate the results from the specifications without lagged MMR as displayed in Table 3. The estimated effect is little affected by adding lagged outcomes. The estimate of the first order lag is also quite small (0.08-0.09) while the second is very close to zero and not significantly different from zero. These small estimates imply that the lagged MMR has little predictive content for future MMR, which is also consistent with the findings in the medical literature that most obstetric complications occur around the time of delivery and cannot be predicted.\footnote{See Gabrysch and Campbell (2009) and Paxton et al. (2005).}

To sum up, all specification tests suggest that the design, i.e., a differences-in-differences design with unit-specific time trends, is compelling.

Having estimated the intent-to-treat effect, we next turn to an estimate of the relationship between MMR and midwife-assisted births. This estimate requires that we can measure the take-up of the midwifery policy, i.e., the share of midwife-assisted births. These were only recorded for part of the investigated period, namely the years 1861 to 1894. Thus, we can only estimate this parameter for this shorter period.

Table 5 displays the results: Panel A shows the estimates of the take-up of the midwifery policy, Panel B the estimates of the intent-to-treat effect (or the reduced-form policy effect) and Panel C the treatment effect, i.e., the relationship between MMR and the share of midwife-assisted births where the midwifery policy is used as the instrumental variable. We use the same specification as in Table 5 (i.e., region-fixed effects, time-specific effects and region-specific time trends) with the important extension that we can now also control for two additional confounders: the number of female emigrants and the number of doctors where both variables are expressed in logarithmic form. We are also going to control for the lagged MMR.

Panel A shows that a doubling of midwives leads to a 19 percent increase in the take-up of midwife-assisted births. This estimate is strikingly robust since it is completely insensitive to adding the confounding factors (Columns 2-8), weighting by the number of births (Columns 9 and 11) or controlling for the lagged MMR (Columns 10 and 11). It is also noteworthy that the estimate of the lagged MMR is close to zero,\footnote{For example, the estimate in Column 10 is –0.04 with a standard error of 0.04.} which again...
suggests that it is very hard to predict future MMR based on previous MMR. Moreover, the cluster robust first-stage $F$-statistic is in the range 13-15 in all specifications suggesting that this instrument is not weak since the first-stage $F$-statistic is larger than 10 (Staiger and Stock 1997).\footnote{Olea and Pflueger (2013) argue that one should adjust the critical value in the case of heteroscedasticity, serial correlation and/or clustering. However, this would lead to a much more conservative approach in testing for a problem with weak instruments since the critical value is 23.}

Panel B displays that the estimates of the intent-to-treat effect (or the reduced form policy effect) are in the range $-0.34$ to $-0.40$, meaning that a doubling of midwives leads to a 34-40 percent reduction in MMR. Once more, the estimated effect is insensitive to adding confounding factors, weighting and controlling for the lagged MMR. It is also noteworthy that the estimated effect is larger than the corresponding estimates in Table 3 for the whole period 1830-1894. That the estimated policy effect is larger in the later period is not surprising, however, since these midwives had a more extensive training as previously noted.

Next, we turn to the results from estimating the causal effect of midwife-assisted births on MMR. Here we use the midwifery policy as an instrumental variable for the share of midwife-assisted births as previously discussed. Panel C shows that the effect, i.e., the elasticity, is about $-2$, i.e., a 1 percent increase in the share of midwife-assisted homebirths would decrease maternal mortality by about 2 percent.

Next, I investigate whether the effect differs depending on the type of shock to the availability of midwives. Column 1 in Table 6 shows the results from a positive supply shock (i.e., the sample only consists of observations with an increase in the number of midwives from one year to the next) while Column 2 displays the results for a negative supply shock (i.e., the sample only consists of observations where there has been a reduction in the number of midwives from one year to the next). The estimate of the take-up rate is nearly twice as large for positive supply shocks as compared to negative shocks: 0.21 versus 0.13. However, the estimated effect of midwife-assisted births on MMR is almost the same since the elasticity is $-1.96$ and $-1.81$, respectively.

Finally, I analyze whether the treatment effect is different if there is a harvest failure (i.e., harvest$\leq3$). Column 3 shows that the estimated take-up is almost two times higher
during harvest failure than otherwise: 0.26 versus 0.14. Nonetheless, the estimate of the treatment effect is nearly the same: –2.39 versus –2.20.

3.2 Design 2: The opening of the new midwifery school

In this design, I explicitly exploit the opening of a new midwifery school in the city of Gothenburg in the southwest of Sweden in 1856 (see Figure 2). With this design we can only estimate the policy effect—the parameter \( \beta \) in equation (1)—and not the causal effect of midwife-assisted births on MMR. This is related to the fact that the take-up of the midwife policy is only recorded from 1861, i.e., after the opening of the midwifery school. Nonetheless, this design nicely complements the previous design based on data after 1860 since both these designs make use of a variation in the availability of midwives after 1860. In other words, one would expect these two designs to produce similar results about the policy effect unless one of them is compromised.

In this design, it is possible to define treatment and control groups based on the geographical closeness to the midwifery school in Gothenburg. Thus, the treatment group is defined based on the areas closest to the midwifery school, i.e., the six regions of Göteborg, Älvsborg, Halland, Jönköping, Skaraborg and Värmland, while the control group consists of all other Swedish regions. This type of design is therefore a conventional difference-in-difference set-up. Thus, the reduced form effect of the opening of the midwifery school on maternal mortality can be estimated using the following regression

\[
\log(\text{MMR}) = \rho [\text{treatment group and year > 1855}] + \alpha_g + \lambda_t + v_{gt},
\]

where MMR is the maternal mortality ratio, \( \alpha_g \) is a region-fixed effect, \( \lambda_t \) is a time-fixed effect and \( 1[.] \) is an indicator variable taking the value of 1 after 1855 in the treatment group. The parameter \( \rho \) measures the reduced form effect of the newly opened midwifery school on MMR. However, to estimate the midwife policy effect—the parameter \( \beta \) in equation 1—we need to re-scale the reduced form effect with the first-stage effect. We can estimate the first-stage effect using the following specification

\[
\log(\text{midwives}) = \pi [\text{treatment group and year > 1855}] + \alpha_g + \lambda_t + v_{gt},
\]
where the parameter $\pi$ is the first-stage effect. The effect of the midwifery policy is then the ratio of the estimated reduced form with the estimated first-stage effect which can be estimated using a standard two-stage least squares or an instrumental variable approach where the instrument is the indicator variable: $1[\text{treatment group and year}>1855]$.

Table 7 reports the results from this design. In panel A, we report estimates of the first-stage effect, i.e., parameter $\pi$ in equation (7). In Panel B, we report the estimates of the reduced form effect, i.e., parameter $\rho$ in equation (6) and in Panel C we report the policy effect, the ratio of the first-stage effect and the reduced form, using an instrumental variable approach. We control for the same set of confounders as those used in the previous approach. Thus, we control for the number of births in Column 2, Column 3 includes infant deaths, Column 4 controls for all other female deaths except maternal deaths, Column 5 includes the age distribution of mothers while Column 6 controls for a full set of indicators of harvest yield.

Panel A shows that the first-stage estimate is in the range 0.44-0.48, which implies that the treatment group—the regions closest to the midwifery school in Gothenburg—has increased its midwives by 55-62 per cent as compared to the control group. Figure 3 clearly illustrates this result since it shows that the treatment group has much fewer midwives than the control group before the opening of the midwifery school in 1856 which is followed by a sharp increase in the availability of midwives such that the two groups have the same number of midwives in 1872. The estimated first-stage effect is also highly statistically significant since the cluster robust $F$-statistic is between 10 and 12.

Panel B displays that the reduced form estimate is between $-0.13$ and $-0.15$ and it is statistically significant at the 5 percent level in all specifications. Thus, this suggests that the opening of the midwifery school reduced the MMR by 12-14 per cent in the treatment areas as compared to the control areas. Figure 4 provides additional support for this finding since the treatment areas have consistently higher MMR than the control areas before the opening of the midwifery school in 1856 and that this difference in MMR between the treatment and control areas largely disappears shortly after 1856.

The exact estimate is computed as $[\exp(\text{parameter estimate}) - 1]$. 

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29 The exact estimate is computed as $[\exp(\text{parameter estimate}) - 1]$. 

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Panel C shows the estimate of the midwife policy effect using an instrumental variable approach where the policy effect is the ratio of the reduced form effect and the first-stage effect. The policy effect is about \(-0.30\), i.e., a doubling of midwives leads to a 30 percent reduction in MMR. The size of this estimate is in the same ballpark as those in Panel B of Table 5. In other words, this bolsters both internal and external validity since we get similar results from two different research designs.

To further probe the identifying assumption in the difference-in-difference design, we test whether the treatment and control groups have parallel trends in their outcomes before the intervention, i.e., the opening of the midwifery school in 1856. To conduct such a test, I create the following eight indicator variables: \(1[\text{Treatment group} \times \text{Year}=1855]\), \(1[\text{Treatment group} \times \text{Year}=1853]\), ..., \(1[\text{Treatment group} \times \text{Year}=1848]\). With these eight indicator variables, I can test whether there is an effect of the treatment up to 8 years before the actual treatment in 1856. Table 8 reports the results from adding all these indicators to the difference-in-difference specifications reported in the last column of Table 7. Column 1 in Table 8 shows the estimate from the first-stage specification. The estimate in the first row is the impact effect which is 0.40. This estimate thus differs little from the corresponding estimates in Panel A of Table 7. In addition, all other eight “placebo” estimates are much smaller and all of them are also negative. Thus, this clear change in the sign of the estimated effects strongly suggests that there is a “structural break” in the first-stage relationship after 1855. Turning to the reduced form relationship in Column 2, the same type of switch in the sign of the estimated effect can be noticed. While the actual effect is \(-0.10\), all the other eight placebo effects except one is positive. These clear patterns in the signs of the placebo effects suggest that the treatment and control group had diverging trends in the number of midwives and MMR before 1856 and converging trends thereafter.\(^{30}\) This finding is also consistent with graphical evidence from Figures 3, 4 and 5. Figure 1 shows the first-stage relationships for the treatment and control groups while Figures 4 and 5 display the reduced form relationship. Figure 5 shows a smoothed version of the reduced form since the yearly data displayed in Figure 4 is extremely noisy.

\(^{30}\) I fail to reject that the two groups have parallel trends because the standard errors are so large.
4. Midwives and stillbirths
In this short section, I will analyze the relationship between midwives and stillbirths. As noted above, it has been argued that perinatal mortality may be used as a proxy for maternal mortality (e.g., Campbell et al. (2005)). Since perinatal mortality is defined as stillbirths plus early neonatal deaths of less than seven days, we can basically use stillbirths as a measure for perinatal mortality. Table 11 shows the reduced form relationship using the same empirical strategies as before, but where we use the logarithm of the stillbirth rate as the dependent variable. Columns 1 and 2 show the results from the first design while Column 3 displays the results from the second design. There is no evidence that the availability of midwives is related to stillbirths since none of the three estimates are significantly different from zero. In addition, even the signs differ across the specifications. Thus, the conclusion must be that stillbirths cannot be used as a proxy for maternal mortality.

5. Discussion and conclusions
In this paper, I have estimated the causal effect of a health policy experiment—home-based intrapartum care by midwives—in the 19th century on MMR, infant mortality and stillbirth. I find a large effect on MMR but no effects on infant mortality and stillbirths. I argue that my finding that midwives with only 6-12 months of formal training had a large impact on reducing MMR in the 19th century in Sweden has potentially important implications for the most effective health strategy of reducing the currently very high MMR in low resource settings.
References


Lundqvist, Birger (1940), *Det svenska barmmorskeväsendets historia,* ("The history of Swedish midwifery policy") Stockholm.


Pischke, J.S and H., Schwandt (2013), "Poorly Measured Confounders are Useful on the Left But Not on the Right," working paper, LSE.


Data Appendix

I have constructed the data set myself from several sources:

- The regional data for the period 1830-1859 for maternal deaths, infant deaths, the age distribution of mothers, and female deaths comes from Tabellverket, the predecessor of Statistics Sweden.
- The regional data for the period 1860-1894 for maternal deaths, infant deaths, the age distribution of mothers, and female deaths is taken from Statistics Sweden’s publication BISOS A.
- The regional data on midwives is collected from various sources. For the period 1850-1894, I have collected data from the publications BISOS K and Sundhets-Collegii underdåniga berättelser om medicinal-verket i riket. For the period 1830-1849, I have also collected data from the National Archives. Christina Romlid has also generously shared her data on midwives which come from other sources than mine. There are only minor discrepancies between her and my data on midwives. However, Romlid recommends that I should use her data in a personnel communication.
- The regional harvest data for the period 1830-1870 is taken from Hellstenius (1871). The data for the period 1871-1894 is taken from BISOS N and converted to the same scale (0-6) as the Hellstenius index.

Hellstenius, J., (1871), Skördarna i Sverige och deras verkningar, Statistisk Tidskrift 29:e häftet, 77-127.
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Table 2: Test of conditional randomization

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</tr>
<tr>
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</tr>
<tr>
<td>log (midwives)</td>
<td>0.003</td>
<td>0.026</td>
<td>0.015</td>
<td>0.042</td>
<td>–0.022</td>
<td>0.005</td>
<td>–0.069</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.024)</td>
<td>(0.029)</td>
<td>(0.076)</td>
<td>(0.077)</td>
<td>(0.056)</td>
<td>(0.067)</td>
</tr>
</tbody>
</table>

Notes: Each entry is a separate regression. All specifications include a full set of region and time-fixed effects together with region-specific time trends. Standard errors, clustered at the regional level, are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.
<table>
<thead>
<tr>
<th></th>
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<th>(3)</th>
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<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td>log(midwives)</td>
<td>-0.19**</td>
<td>-0.21**</td>
<td>-0.20**</td>
<td>-0.19**</td>
<td>-0.18**</td>
<td>-0.18**</td>
<td>-0.20**</td>
<td>-0.21***</td>
</tr>
<tr>
<td>(the-intent-to treat effect)</td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
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</tr>
<tr>
<td>Births</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Infant mortality</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Female deaths</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Age distribution</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Harvest indicators</td>
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<td>Yes</td>
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<td>Yes</td>
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<td>Quadratic time trend</td>
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<td>Yes</td>
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<td>Yes</td>
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<td>Yes</td>
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</tr>
<tr>
<td>Weighted least squares</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
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<td>1,608</td>
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<td>1,608</td>
</tr>
</tbody>
</table>

Notes: All specifications include a full set of region and time-fixed effects together with region-specific time trends. The dependent variable is log(MMR) where MMR is the maternal mortality ratio. Standard errors, clustered at the regional level, are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.
Table 4. Controlling for the lagged MMR

<table>
<thead>
<tr>
<th></th>
<th>OLS (unweighted estimates)</th>
<th></th>
<th>WLS estimates</th>
<th></th>
</tr>
</thead>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
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<tr>
<td>log(midwives)</td>
<td>–0.20**</td>
<td>–0.18**</td>
<td>–0.18**</td>
<td>–0.21**</td>
</tr>
<tr>
<td>(the-intent-to treat effect)</td>
<td>(0.07)</td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>log(MMR_{t-1})</td>
<td>0.09***</td>
<td>0.09***</td>
<td>0.08***</td>
<td>0.08***</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>log(MMR_{t-2})</td>
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<td></td>
<td>0.00</td>
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<tr>
<td></td>
<td>(0.03)</td>
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<td>(0.03)</td>
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</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Observations</td>
<td>1608</td>
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<td>1554</td>
<td>1608</td>
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</table>

Notes: All specifications include a full set of region and time-fixed effects together with region-specific linear and quadratic time trends. The dependent variable is log(MMR) where MMR is the maternal mortality ratio. Standard errors, clustered at the regional level, are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10percent, **5percent, and ***1percent.
Table 5. Estimates of the take up rate of the midwifery policy, the intent-to-treat effect and the risk of dying in childbirth

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
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<th>(8)</th>
<th>(9)</th>
<th>(10)</th>
<th>(11)</th>
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</thead>
<tbody>
<tr>
<td><strong>Panel A: The relationship between midwife-assisted births and the midwifery policy</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>The take-up effect (first-stage)</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.19***</td>
<td>0.18***</td>
<td>0.19***</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
</tr>
<tr>
<td><strong>Panel B: The relationship between MMR and the midwifery policy</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>The intent-to-treat effect (reduced form)</td>
<td>-0.38***</td>
<td>-0.38***</td>
<td>-0.39***</td>
<td>-0.40***</td>
<td>-0.39***</td>
<td>-0.36***</td>
<td>-0.37***</td>
<td>-0.34***</td>
<td>-0.38***</td>
<td>-0.35***</td>
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<td>(0.13)</td>
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<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
</tr>
<tr>
<td><strong>Panel C: The relationship between MMR and midwife-assisted births</strong></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Treatment effect (IV estimate)</td>
<td>-1.99**</td>
<td>-2.04**</td>
<td>-2.10**</td>
<td>-2.11**</td>
<td>-2.09**</td>
<td>-1.94**</td>
<td>-1.93**</td>
<td>-1.99**</td>
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<td>(0.78)</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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</tr>
<tr>
<td>Infant mortality</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Age distribution</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td>Doctors</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Lagged MMR</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Weighted least squares</td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td>First-stage F-statistic</td>
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<td>15</td>
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<td>848</td>
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<td>848</td>
<td>842</td>
<td>842</td>
<td>842</td>
<td>840</td>
<td>840</td>
</tr>
</tbody>
</table>

Notes: All specifications include a full set of region and time-fixed effects together with region-specific time trends. The dependent variable in Panel A is the share of midwife-assisted births in logarithmic form. The dependent variable in Panels B and C is the maternal mortality ratio (MMR) in logarithmic form. Panel C is the IV or the Wald estimator, the ratio between the reduced form effect and the first-stage estimate. Standard errors, clustered at the regional level, are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.
Table 6. Heterogeneous effects

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<tbody>
<tr>
<td>Positive supply shocks</td>
<td>0.21***</td>
<td>0.13**</td>
<td>0.26**</td>
<td>0.14**</td>
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<tr>
<td>Negative supply shocks</td>
<td>0.13**</td>
<td>0.05</td>
<td>0.11</td>
<td>0.05</td>
</tr>
<tr>
<td>Bad harvest (Harvest&lt;=3)</td>
<td>-0.42**</td>
<td>-0.24</td>
<td>-0.63**</td>
<td>-0.30*</td>
</tr>
<tr>
<td>Good harvest (Harvest&gt;3)</td>
<td>-0.24</td>
<td>-0.17</td>
<td>-0.29</td>
<td>-0.18</td>
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</tbody>
</table>

Panel A: The relationship between midwife-assisted births and the midwifery policy

Panel B: The relationship between MMR and the midwifery policy

Panel C: The relationship between MMR and midwife-assisted births

Notes: All specifications include a full set of region and time-fixed effects together with region-specific time trends. The dependent variable in Panel A is the number of midwife-assisted births in logarithmic form. The dependent variable in Panels B and C is the maternal mortality ratio (MMR) in logarithmic form. Panel C is the IV or the Wald estimator, the ratio between the reduced form effect and the first-stage estimate. Standard errors, clustered at the regional level, are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10percent, **5percent, and ***1percent.
Table 7. Results from the opening of a new midwifery school in 1856 on midwife availability and MMR

<table>
<thead>
<tr>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: The effect of opening a new midwifery school on the availability of midwives</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>First-stage effect</td>
<td>0.44***</td>
<td>0.47***</td>
<td>0.47***</td>
<td>0.48***</td>
<td>0.44***</td>
<td>0.44***</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.13)</td>
<td>(0.13)</td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
</tr>
<tr>
<td><strong>Panel B: The effect of opening a new midwifery school on MMR</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reduced form effect</td>
<td>–0.13**</td>
<td>–0.15**</td>
<td>–0.14**</td>
<td>–0.14**</td>
<td>–0.14**</td>
<td>–0.13**</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>Panel C: Instrumental variable estimates of the effect of midwives on MMR</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>–0.31*</td>
<td>–0.33*</td>
<td>–0.31**</td>
<td>–0.30**</td>
<td>–0.31*</td>
<td>–0.30*</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(0.17)</td>
<td>(0.16)</td>
<td>(0.15)</td>
<td>(0.18)</td>
<td>(0.18)</td>
</tr>
</tbody>
</table>

Births          | Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |
Infant mortality| Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |
Female deaths   | Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |
Age distribution| Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |
Harvest indicators| Yes | Yes   | Yes   | Yes   | Yes   | Yes   |
First-stage $F$-statistics | 11 | 12  | 12  | 12  | 10  | 10  |
Number of observations | 1,608 | 1,608 | 1,608 | 1,608 | 1,608 | 1,608 |

Notes: All specifications include a full set of region and time-fixed effects. The dependent variable in Panel A is the number of midwives in logarithmic form. The dependent variable in Panels B and C is the maternal mortality ratio (MMR) in logarithmic form. Panel C is the Wald estimator, the ratio between the reduced form effect and the first-stage estimate. Standard errors, clustered at the regional level, are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.
Table 8. Test of parallel trends

<table>
<thead>
<tr>
<th>First-stage</th>
<th>Reduced form</th>
</tr>
</thead>
<tbody>
<tr>
<td>1[Treatment group=1 &amp; year&gt;1855]</td>
<td>0.40**</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1855]</td>
<td>−0.10</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1854]</td>
<td>−0.10</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1853]</td>
<td>−0.06</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1852]</td>
<td>−0.11</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1851]</td>
<td>−0.15</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1850]</td>
<td>−0.15</td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1849]</td>
<td>−0.15*</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
</tr>
<tr>
<td>1[Treatment group=1 &amp; year=1848]</td>
<td>−0.10*</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
</tr>
</tbody>
</table>

Observations 1610 1608

All specifications include a full set of region and time-fixed effects and the same control variables as in Table 7. Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.
Table 9. Estimates of the reduced form relationship between the logarithm of the stillbirth rate and midwives

<table>
<thead>
<tr>
<th>Design 1:</th>
<th>Design 1:</th>
<th>Design 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>1830-1894</td>
<td>1861-1894</td>
<td>1830-1894</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Reduced-form effect</td>
<td>–0.02 (0.02)</td>
<td>–0.04 (0.05)</td>
</tr>
<tr>
<td>Controls</td>
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<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>1,608</td>
<td>842</td>
</tr>
</tbody>
</table>

Notes: Se notes from previous tables.
Figure 1. Total number of midwives in Sweden 1830-1894.
Figure 2. Regions (Län) of Sweden
Figure 3. Number of midwives in the treatment and control groups 1830-1894

Notes. The number of midwives is expressed in logarithmic form.
Figure 4. MMR in the treatment and control groups during 1830-1894

Notes. MMR is expressed in logarithmic form.

Figure 5. MMR in the treatment and control groups during 1830-1894

Notes. MMR is expressed in logarithmic form and the data is smoothed.