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 stillbirths. Specifically, it exploits the geographical deployment of trained and licensed midwives in Sweden during the period 1830-1894 as a source of exogenous variation in the availability of skilled birth attendants. The estimated midwifery policy effect—the intent-to-treat effect—is between 15-30 percent, i.e., a doubling of trained midwives lead to a 15-30 percent reduction in maternal mortality. The intent-to-treat effect can also be re-scaled by the take-up rate of the midwifery policy in order to estimate the risk of dying in childbirth. The risk of dying in childbirth is estimated to be 80-90 percent lower if a trained midwife assisted the birth, which is a substantial effect given that the midwife training was only 6-12 months at that time. The results from this study may therefore inform the current debate of the most effective strategy for reducing maternal mortality in the developing world.

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

1. Introduction

It has been estimated that each year more than half a million women die as a result of pregnancy or childbirth complications (UNICEF 2009). The very high maternal mortality in many developing countries is therefore considered to be a key policy issue. As a result, one of the United Nations Millennium Development Goals (MDGs) is to reduce maternal mortality by 75 percent until 2015.

However, given this importance in reducing maternal mortality, we know surprisingly little about what type of health intervention actually works (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 other health care professional). This is perhaps not surprising since a credible program evaluation faces several great obstacles. To begin with, the absolute numbers of maternal deaths are generally small, and very large populations are needed to investigate the determinants of maternal mortality (e.g., Ronsman et al. 2008). As a result, randomized control trials (RCT), the gold standard in evaluation studies, are not feasible.[1] In addition, there is also a shortage of reliable information on maternal mortality and whether a skilled birth attendant assisted the birth in many countries (e.g., Graham 2002, Ronsman and Graham 2006).[2] Although, observational studies can overcome some of these problems,[3] they still face the difficulty of establishing a causal relationship since they typically do not make use of a credible research design (e.g., Gramham et al. 2001 and Scott and Ronsman 2009).[4] In fact, some

[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 control group, this RCT had very low power to detect any effects on maternal mortality to the small number of maternal deaths in both the treatment (27 deaths) and control group (34 deaths). Thus, this RCT 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 therefore concludes: “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 better 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), which analyze a maternity, care program in Matlab, Bangladesh. They find evidence that MMR is lower in the intervention area compared to a control area. Their result is however questioned by Ronsman et al. (1997) since they argue that the control area is not comparable to the treatment area. Moreover, this type of quasi-experimental design is also problematic since the health intervention included many components and it is therefore difficult to evaluate the role of midwives in reducing MMR (e.g., Maine et al. (2006)).

of the non-experimental work even shows that giving birth with a health professional may *increase* the risk of dying in childbirth. This 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 maternal mortality in various countries from 1800 to 1950.[5] 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 evaluate 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 birth attendant and maternal mortality, I will make use of a unique midwifery policy experiment in Sweden in the 19th Century. With this new data set,[6] I can overcome most, if not all, of the 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.[7] As a result, the statistical analysis can be based on an extremely large sample size since there were roughly 120,000 births and 600 maternal deaths on a yearly basis. Thus, 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 the new data, it is also possible to exploit exogenous sources of variations in one particular type of health intervention since 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 of trained midwives while the other design make use of the opening of the new midwifery school which greatly increased the supply

[5] He 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.

[7] See Högberg and Wall (1986) for a discussion of the Swedish historical vital statistics and the definition of a maternal death being used.

of trained midwives in those areas closest to the midwife school. In other words, this paper uses quasi-experimental designs to estimate the *causal* effect of an intervention on maternal mortality. In the Swedish context, it is also possible to estimate the risk of dying in childbirth if the birth was assisted by a trained and licensed midwife since it was recorded whether a birth was attended by a trained midwife or not. Here it is important to stress that midwives assisted home births are 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 (almost 100% of all births) including any complications associated with the delivery. Finally, it is possible to argue that the finding of this paper is likely to have external validity since Sweden in 19th century was a very poor agrarian society and in many respects similar to many developing countries today.[8]

The result of this paper indicates that a 100 percent increase in the number of trained midwives lead to a 15-30 percent reduction in the MMR. The midwife policy effect is almost twice as large for the period after 1860, which is consistent with the fact that the midwife education was twice as long in this time period. I also estimate the take-up rate of the policy for the period after 1860.[9] The take-up rate is about 20 percent. As a result, the risk of dying in childbirth is estimated to be about 80-90 percent lower if a trained and licensed midwife assisted the birth. Moreover, I also find that the take-up rate the midwifery policy differs depending on the type of supply shock (the take-up rate is larger for positive than negative midwife shocks) and whether the harvest was good or bad (the take-up rate is higher when there is a bad harvest). Nonetheless, the estimate of the risk of dying in childbirth does not vary across these circumstances even though the take-up rate differs significantly. Taken together, these results strongly suggest there is a causal relationship between birth with a trained midwife and maternal mortality. Moreover, a number of additional specification checks also lend strong support for a causal interpretation. Most importantly, there is no relationship between the supply shocks of midwives and other important confounding factors, such as fertility and female mortality other than maternal mortality. In other words, controlling for important confounders has no impact on the estimated effect. Also controlling for lags of MMR do

[8] See also Graham (2001), Högberg (1985, 2004) and Loudon (2000) for related and other arguments for the usefulness of historical data in order to learn how to reduce maternal mortality in the developing world.
[9] The data on the take-up of the midwifery policy only exists after 1860.

not affect the results suggesting that midwives were not placed in regions with high maternal mortality. Finally, I follow the suggestion of Solon et al. (2013) of comparing un-weighted estimation with weighted as a as a useful joint test against model misspecification and/or misunderstanding of the sampling process. Importantly, there is no difference between the un-weighted or weighted specifications.

In this paper, I also test the clam that perinatal mortality or infant mortality can be a used as a proxy indicator for maternal mortality and maternal health care status thereby circumventing the problem of measuring maternal mortality (e.g., Campbell et al. 2005)).[10] The results from this study shows that this claim is likely to be wrong since I find no relationship between midwives and stillbirths or infant mortality but substantial impacts on maternal mortality. Moreover, Loudon (1992) also find little or no relationship between maternal mortality and infant mortality (both neonatal and post-neonatal) in the historical data and therefore he concludes, “it is clear that measures designed to reduce maternal and infant mortality required quite different approaches”. Thus, reducing maternal mortality today in the developing world is also likely to require other type of health interventions than those required for reducing infant mortality.

This paper is related to several literatures and work. Specifically, 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. The paper 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 some work in economics analyzing questions related to maternal mortality. For example, Jayachandran and Lleras-Muney (2009) analyse the impact of the decline in maternal mortality on women's human capital while Jayachandran et al. 2010 evaluate the impact of the sulfa drug on maternal mortality.[11] There is also a large literature in medicine analyzing the impact of various health interventions, such as deployment of midwives, on MMR (e.g., Fauveau et al. (1993), Jokhio et al. (2005), Ronsman et al. (2007). Moreover, She There is also a literature using historical data (e.g., Loudoun

[10] The claim has ben questioned by Alkalin et al. (1997).
[11] See also Albanesi (2011).

(1992, 2000), Högberg et al (1986, 1988), Högberg (2004)).[12] This paper is also a related to the current policy debate of the best way of preventing maternal mortality (e.g., The Lancet maternal survival series).

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 midwives and maternal mortality. Section 4 provides evidence for the relationship between midwives and still births while Section 5 concludes.

2. Background and Data

In this section, I provide information about maternal mortality, the Swedish midwifery policy and data. However, before this it is perhaps useful to briefly describe the general economic and social setting in the 19th 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 agriculture sector was about 80% and the share of rural population 90%. 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. Maternal mortality was about 600 deaths per 100,000 births while the infant mortality was higher than 150 deaths per 1,000 life births. This short description makes it very clear that Sweden in 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

[12] Högberg et al. (1986) and Högberg (2004) also analyzes 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 confounders. This type of epidemiological approach therefore cannot identify any causal relationships (e.g., there estimate is only half the size of my estimate). In addition, these studies have also been criticized on other grounds. One issue concerns 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 credible identification strategies and includes all maternal deaths.

obstetric causes within 42 days after birth.[13] Today, the vast majority of maternal deaths (80%) are due to direct obstetric complications due to (i) hemorrhage (uncontrolled bleeding), infections (sepsis or puerperal fever), (iii) hypertensive disorders (eclampsia), (iv) obstructed labor and (v) complications of abortion. These birth complications occur even in well-nourished, well-educated women receiving adequate prenatal and delivery care and generally cannot be predicted (e.g., Gabrysch and Campbell 2009 and Paxton et al. 2005).

In the Swedish historical data, a maternal death was defined as a death of a woman caused by complications of pregnancy, labor or puerperium. Thus, basically only direct obstetrics maternal deaths should be recorded.[14] Moreover, the diagnosis puerperal sepsis was likely not confounded by septic abortions during the 19th century. When the national statistics recorded puerperal sepsis separately after 1860, it showed that 42 percent of all maternal deaths were caused by puerperal sepsis.

2.2 Sweden's midwifery policy[15]

Sweden has 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 the places with a lack of midwives, i.e., no midwives. The capacity to train and certify midwives were however very severely limited since only one midwifery school in Stockholm, the capital city, should supply midwives to all other regions except two.[16] It was the number of women giving birth at the Lying-in-Hospital of Stockholm that was the key determinant of how many midwives that could be trained each year. For example, during the period 1821-1840, only, on average, 230 women gave birth annually. Until 1822 only 26 midwives were graduated annually from the Stockholm midwifery school. During the period 1823-1842, on average, about 37 midwives graduated. In 1856, a new midwifery school was put into place in the city of

[13] 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)

[14] It has been estimated that in the 19th century two thirds of maternal deaths had direct obstetrical causes, such as difficult labor, eclampsia, hemorrhage, and sepsis, while one third were indirect obstetric deaths due to diseases such as pneumonia, tuberculosis, dysentery, heart disease, and malnutrition (Högberg and Broström 1985).

[15] This section is based on Högberg (2004), Romlid (1996, 1998) and Lundqvist (1940).

[16] The regions of Malmöhus och Kristianstad had their own-midwifery school in Lund.

Gothenburg, increasing the total supply of trained midwives with 80. In addition, to further increase the supply of midwives in rural areas with a shortage of midwives, the Swedish health authorities paid the allowances for a 18 midwife students conditional that she would be deployed in a locality with no 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 and which increased to 2,585 in 1894. The number of midwives can be compared to the total number of doctors which was 379 in 1820 and 964 in 1984. However, the numbers of doctors available for the general public (i.e., "provinsialläkare") was fare less since there were only 94 in 1820 and 138 in 1894. These doctors were employed by the Swedish central government while the midwives were employed by one of the existing 2,500 local governments.

In Sweden, midwives were basically in charge of all deliveries since home births were close to 100 percent. For example only 2.8 percent of all births in 1894 were delivered in hospitals in 1894. The requirement to qualify for the midwife-training program was that women should have basic knowledge in reading and writing.[17] 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. From 1819, qualified midwives could receive 3 months of additional training of how to use obstetrical instruments (delivery forceps, sharp and blunt hooks, perforators). In practice, midwives were responsible for all delivers since many of them were trained and certified to do obstetrical operations. In other words, there were no referrals of women with obstetric complications to hospitals or doctors. Delivery forceps were used between 200-600 times per year (very few interventions since less than 0.5% of all births). Sharp hooks and perforators were used 5-32 times per year. The mean annual number of deliveries per midwife in the rural areas was 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 thus explains why a midwife could only attend a

[17] 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.

limited number of births each year. The share of midwives assisted births constituted 36 percent in 1861 while it constituted 78 percent in 1894.

2.3 Data

The data set includes information on the universe of total number of births (both still and live) during the period 1830 to 1894. There are altogether 7,770,239 live births and 241,841 stillbirths. There were 37,519 maternal deaths implying an average of 482 MMR over the whole period 1830-1894. The number of female deaths from other causes was 2,408,397. The number of infants dying before the age of one was 1,062,413, i.e., the infant mortality ratio, IMR, was 133. The age distribution of mothers was as following: 2.1% under the age 21, 14,4% for the ages 21-25, 25.7% for the ages 26-30, 26.2% for the ages 31-35, 20,5% for the ages 36-40, 10,6% for the ages 41-45, and 1.5% for the ages 46-50.

Importantly, for the empirical analysis, data on 25 geographical areas will be used (24 regions ("län") and the city of Stockholm). Figure 2 shows a map over these areas. There is considerably variation in both the cross-section and the time-series in these areas for both MMR and the number of midwives. For example, in the first year of our sample 1830, the mean of MMR is 622 with a maximum of MMR is 1274 and a minimum of 296. In the end of the sample, 1894, the mean MMR is 288, where the highest MMR is 458 and the smallest is 144. For midwives, the average number of midwife is 40 in 1830 with a minimum of 5 and a maximum of 202. In 1894, the average number of midwife is 103 where the minimum is 43 and the maximum is 346. Table 1 displays the summary statistics of the regional data.

3. The empirical designs and results

Absent a randomized experiment, estimating the impact of an intervention, such as the availability of midwives, on maternal mortality, one would ideally estimate an equation of the following form

(1) $\log(MMR)_{gt} = \alpha + \beta(\text{midwife availability})_{gt} + v_{gt}$

where the dependent variable, $\log(MMR)_{gt}$, is the natural log of maternal mortality ratio (number of maternal deaths per 100,000 births) in geographical area g in year t.[18] The independent variable would be a measure of midwife availability, i.e., a supply shock, that is uncorrelated with the demand for midwives. The hypothesis is that when midwives becomes available; MMR falls (β<0).

It is also possible to estimate the risk of dying in childbirth if a midwife did assist the birth given that we can measure the "take-up"—the number of midwife-assisted births—of the policy.[19] In such a case, the estimated reduced form effect β from equation (1) would be re-scaled by the estimated take up effect from the following regression

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

where the parameter π is the take-up effect of the health intervention. Thus, the estimate of the risk of dying in childbirth if a midwife assisted the birth is equal to ratio of the reduced form effect and the first-stage effect, which corresponds to the following population regression:

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

where the parameter b is the risk of dying in childbirth. [20]

The general idea is that we can estimate the causal effect of skilled assisted births on maternal mortality by using institutional features of the supply side of the health system, i.e., 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

[18] Here I follow the empirical framework laid out by Jayachandran et al (2010). They analyse how the availability of the sulfa drug affected MMR.
[19] Estimating the causal effect of the health intervention (availability of midwives) on maternal mortality 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 health intervention only affects maternal mortality via midwives, which seems quite plausible since there was no referral system and a midwife was basically not allowed to do any important medical interventions other than deliveries.
[20] This estimate of the risk of dying is also equal to the preventive fraction, i.e., the percentage of cases that can be prevented if a population is exposed to an intervention, compared to an unexposed population, if the estimate is negative; otherwise it is equal to the attributable risk percent.

exogenous variation in the availability of midwives. The first design is based on supply shocks or sharp "discontinuities" in the availability of midwives in a geographical area g at time t, and the second design make use of the opening of the new midwifery school in the city of Gothenburg in 1856. Below we describe the two empirical designs in more detail.

3.1 Design 1: Supply shocks in the availability of midwives

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

(4) $log(MMR)_{gt}=\beta log(the\ ratio\ of\ midwives\ per\ birth)_{gt} + \alpha_g + \lambda_t + \pi_g t + v_{gt}$,
g=1,2..,25., and t=1830,1831,..,1894.,

where MMR is the maternal mortality ratio, α_g is an area specific effect, λ_t is a time fixed effects, and $\pi_g t$ is an area-specific time trend.[21] The parameter of interest is β and it measures the effect of the health intervention on the MMR, i.e., an intent-to-treat effect. The impact is measured as elasticity since both the outcome and the explanatory variable is expressed in logarithmic forms. Since we are only interested of getting an unbiased and consistent estimate of β, equation (4) could also be equivalently expressed as

(5) $log(MMR)_{gt}=\beta log(midwives)_{gt}+ \theta log(births)+ \alpha_g+ \lambda_t + \pi_g t + v_{gt}$,

[21] This is the same type of identification strategy as used by Miller (2008). The only difference is that his treatment is binary (female suffrage) while the treatment here is multi-valued (number of midwives).

where *log*(*MMR*) is the number of maternal deaths in logarithmic form,[22] *log(midwives*) is the number of midwives in logarithmic form and *log*(*births*) is the number of births in logarithmic form.[23]

It is important to note that equations (4) and (5) are pseudo panel data regressions, i.e., aggregated data from repeated cross sections with area and time fixed-effects. Pseudo panels typically raise a number of econometric issues such as a small sample bias problem (e.g., Deaton 1985). However, since the data covers the universe of births and that the number of births within the areas is large, 4,782, implies that there are little or no measurement errors in these averages.[24] There is also the question whether one should estimate the pseudo panel regressions by weighted least squares (WLS) and use the number of births as weights in order to get back to the micro data relationship, i.e., the underlying micro data sample with nearly 8 million births during the period 1830-1894. This is however an open question since an argument can be made that an unweighted analysis of aggregates is to be preferred (Angrist and Pischke 2009). Nonetheless, Solon et al. (2013) recommend 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.[25] There is also a benefit of having pseudo panels rather than true panel data since it now possible to control for a lagged dependent variable without introducing bias in fixed effects models (e.g., Wooldridge 2009).[26] 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 area-fixed effects, allow one to deal with the potential problem

[22] The outcome, log(MMR), in equation (5) could also be expressed as log (number of maternal deaths) without affecting the estimate of β.

[23] Specifications (3) and (4) raise the important issue whether one should control for the number of births since this control variable may be endogenous (e.g., a risk avert 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 β if births are 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 births and the number of midwives.

[24] Even if one has data from the entire population, if one takes the perspective that the regression function is intended to capture causal effects, the standard errors can be justified using a generalization of randomization inference (Abadie et al. 2014).

[25] Under exogenous sampling and correct specification of the conditional mean both OLS and WLS are consistent estimators for the regression coefficients.

[26] 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).

of midwives being placed in areas with already high maternal death rates. Finally, similar to other regression discontinuity designs, it is also possible to test whether the treatment—the midwifery policy—is as good as randomly assigned by testing whether there is a discontinuity in any other of the potential confounders.[27]

We start by presenting the results whether the treatment is as good as randomly assigned conditional on area-fixed effects, time-specific effects and area-specific time trends. To perform this test we estimate regressions of the following form

(6) $w_{gt}=\lambda log(midwives)_{gt}+ \alpha_g+ \lambda_t + \pi_g t + v_{gt}$,

where w_{gt} is the candidate confounder. We expect the estimate of λ to be zero if the deployment of midwives are as good as random. The confounders we use is the number births, total female deaths except for maternal deaths, infant deaths, number of doctors, emigration, the age distribution of mothers and seven indicators for harvest yield where 0 corresponds to complete harvest failure. Finding that these set of important confounders are not associated with the placement of midwives would greatly bolster the credibility of the research design.

Table 2 shows that there is only one out of 19 estimate that is statistically significant at the 5% level. However, that is to be expected since if 20 specifications are tested it is likely that one will 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 that the deployment of midwives can be considered as good as random. Here it is noteworthy that infant mortality is not related to supply shocks of midwives.

Table 3 displays the results for the reduced form relationship between MMR and midwives, i.e., specifications (4) and (5). All specifications except one (Columns 8) are un-weighted 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

[27] See also the discussion in Pischke and Schwandt (2013) of different ways of testing the identifying assumption, namely "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.

dependent and the independent variables are expressed in logarithmic forms, the interpretation of estimated coefficient is that doubling of the number of midwives would lead to a 17 percent reduction in MMR.[28] Adding the confounders has little or no impact on the estimated effect as can be seen in Columns 2-6. This is also what one should expect from the previous finding that these set of confounders are not related to the midwifery policy conditional on area-fixed effects, time-specific effects and area-specific time trends. The estimated effect is also little affected if a quadratic area-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. Taken together, all specification checks lend support to a causal interpretation of the estimated relationship between MMR and the midwife policy.

An additional specification check is to control for a lagged dependent variable since it is possible to argue that the midwife policy could be based on such considerations, e.g., midwives are placed in areas with a previous 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 consistent with the findings in the medical literature that most obstetric complications occur around the time of delivery and cannot be predicted.[29]

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

Having estimated the intent-to-treat effect, we next turn to an estimate of the risk of dying in childbirth if a midwife assisted the birth. This estimate requires that we can measure the "take-up" of the midwifery policy, i.e., the number of midwife-assisted births. These were only recorded for part of the investigated period 1830-1894, namely

[28] To obtain the correct percentage interpretation of the estimated effect (when the estimate is large) it is necessary to use the transformation 100*[exp(estimated effect)-1].
[29] See Gabrysch and Campbell (2009) and Paxton et al. (2005).

the years 1861 to 1894. Thus, we can therefore estimate the risk of dying in childbirth for this shorter period.

Table 5 displays the results: Panel A shows the estimates of the take up-rate of the midwifery policy, Panel B the estimates of the intent-to-treat effect (or reduced-form policy effect) and Panel C the treatment effect, i.e., the instrumental variable estimate of the risk of dying in childbirth where the midwifery policy is the instrumental variable. We use the same specification as in Table 5 (i.e., area-fixed effects, time-specific effects and area-specific time trends) with the important extension that we can now also control for two additional confounders: number of female emigrants and the number of doctors. We also control for the lagged MMR.

Panel A shows that a doubling of midwives lead to a 19 percent increase in the take-up of midwife assisted births.[30] This estimate is strikingly robust since it is completely insensitive to adding 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 estimate of the lagged MMR is close to zero,[31] which again suggests that there 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).[32]

Panel B displays that the intent-to-treat effect or the reduced form policy effect is in the range -0.34 to -0.40, meaning that a doubling of midwives leads to 29-33 percent reduction in MMR.[33] Again, the estimates are quite 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 effect is larger in the later period is however not surprising since these midwives had more extensive training as noted previously.

[30] To obtain the correct percentage interpretation of the estimated treatment effect (when the estimate is large) it is necessary to use the transformation 100*[exp(estimated effect)-1].
[31] For example, the estimate in Column 10 is -0.04 with a standard error of 0.04.
[32] Olea and Pflueger (2013) argue that one should adjust the critical value in the case of heteroscedasticity. serial correlation and/or clustering. This would lead to a much more conservative approach.
[33] The exact percentage change is 100(exp(estimated effect)-1).

Turning to the estimate of the treatment effect, i.e., using the midwife policy as an instrumental variable for the number of midwife-assisted births, panel C shows that estimate of the risk of dying in childbirth is 80-86 percent lower if the birth was assisted by a trained and licensed midwife.[34]

Next I investigate whether the take-up of the policy and the estimate of the risk of dying in childbirth vary depending on the type of supply shock. Column 1 in Table 6 shows the results from positive supply shock (an increase in the number of midwives from one year to the next) while Column 2 display the results for a negative supply shock (a reduction the number of midwives from one year to the next). The estimate of the take-up rate is nearly twice as large for the positive supply than for the negative supply: 0.21 versus 0.13 percent. However, the estimate of the risk of dying is almost the same -86 versus -83 percent.

Finally, I analyze whether the take-up of the policy and the estimate of the risk of dying in childbirth vary depending on a bad harvest (harvest<=3) versus a good harvest (harvest>3). Column 3 shows that the take-up rate is almost two times higher during bad harvest than good harvest: 0.26 versus 0.14. Nonetheless, the estimate of the risk of dying is nearly the same: -91 versus -89 percent.

3.2 Design 2: The opening of the new midwifery school

In this design we 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 β in equation (1)—and not the risk of dying in childbirth if a midwife assisted the birth. This has to do with 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 variation in the availability of midwives after 1860. In other words, one would expect that theses two designs would produce similar results unless one of them is flawed.

In this design, it is possible to define treatment and control groups based on the geographical closeness to Gothenburg city. Thus, the treatment group is therefore the

[34] The exact percentage change is 100(exp(estimated effect)-1).

geographical areas closest to Gothenburg city, i.e., the sex regions made up of Göteborgs, 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 can be estimated using the following regression

(6) $\log(\text{MMR}) = \rho 1[\text{treatment group and year}>1855] + \alpha_g + \lambda_t + v_{gt}$,

where *MMR* is the maternal mortality ratio, α_g is an area specific effect, λ_t is a time fixed effects, 1[.] is an indicator variable taking the value 1 after 1855 in the treatment group. The parameter ρ measures the reduced form effect. However, to estimate the policy effect—the parameter β 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

(7) $\log(\textit{midwives}) = \pi 1[\text{treatment group and year}>1855] + \alpha_g + \lambda_t + v_{gt}$.

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[treatment group and year>1855].

Table 7 reports the results from this design. In panel A, we report estimates of first-stage effect, i.e., the parameter π in equation (7), in Panel B we report the estimates of the reduced form effect, i.e., parameter ρ in equation (6) and in Panel C we report the treatment or 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 used in the previous approach. Thus, we control for the number of births in Column 2, Column 3 includes the infant deaths, Column 4 control for the female deaths other than maternal deaths, Column 5 includes the age distribution of mothers while Column 6 control for indicators of harvest yield.

Panel A shows that the first-stage estimate is in the range 0.44-0.48, which imply that the treatment group—regions closet to Gothenburg city—has increased their midwives with 55-63 percent 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 midwife school in 1856 which is followed by a sharp increase in the availability of midwives such that they 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 midwife school lowered the MMR with 12-14 percent in the treatment areas as compared to the control areas. Figure 4 provides additional support for this finding since that the treatment areas has consistently higher MMR than the control areas before the opening of the midwife school in 1856 and that this difference in MMR between the treatment and control areas largely disappears after 1856.

Panel C shows the estimate of the midwife policy effect using a 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 26 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 get similar results from two different research designs.

To further probe the identifying assumption of 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 midwife school in 1856. To conduct such a test, I create the following eight indicator variables 1[Treatment group*Year=1855], 1[Treatment group*Year=1853],…, 1[Treatment group*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 report 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 true impact effect which is 0.40. This estimate thus differs little from the corresponding estimates in Panel A of Table 7. In addition, all the 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 is noticeable. While the true effect is -0.10 and negative, all the other eight placebo estimates expect for one is positive. Although that I fail to reject that the two groups have parallel trends since the standard errors are very large, these clear pattern 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. 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.

4. Midwives and stillbirths

In this section, I will analyze the relationship between midwives and stillbirths. It has often 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 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 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 midwives are related to stillbirths since no one of the three estimates are significantly different from zero. In addition, even the signs also differ across the specifications.

5. Discussion and conclusions

The evidence in this paper may inform the current debate current debate of the best strategy for reducing maternal mortality and thereby achieving of the UN Millennium Development Goal. For example, in 2006, the prominent medical journal 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 centre intrapartum care, (ii) skilled attendance at home, (iii) community health workers at home and (iv) relatives or traditional births attendance at home. The recommendation was to use the health centre intrapartum care strategy since skilled attendance at home was not considered a viable option. However, the result from this paper clearly shows that having a skilled attendance at home may make all the difference. Specifically, my results indicate that even a rather short training in midwifery skills (6-12 months) can have considerably effects on MMR. Consequently, having a skilled attendance at home may be a useful strategy in reducing maternal mortality in low-income countries. Moreover, having home-birth midwives may increase the take-up of the health policy since this strategy better respond to women's demand for home-based care.

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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 are 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.

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Table 1 Regional averages for the period 1830-1894

	Mean	St. Dev	Min	Max	Obs.
Number of maternal deaths	23	15	0	116	1,625
Number of midwives	68	59	5	377	1,610
Number of live births	4,782	2,087	827	10,827	1,625
Number of still births	149	72	12	361	1,625
Number of female deaths excluding maternal deaths	1,505	654	259	4,171	1,625
Number of infant deaths	654	286	60	1576	1,625
Harvest yield	4.0	1.2	0	6	1,625
Percentage of mothers aged below 21	1.5	0.6	0.4	6.7	1,625
Percentage of mothers aged 21 to 25	14.5	2.4	8.7	20.3	1,625
Percentage of mothers aged 26 to 30	25.0	1.9	19.7	32.8	1,625
Percentage of mothers aged 31 to 35	25.5	1.3	21.3	30.0	1,625
Percentage of mothers aged 36 to 40	20.6	1.9	15.0	25.5	1,625
Percentage of mothers aged 41 to 45	11.1	1.9	5.5	15.9	1,625
Percentage of mothers aged 46 to 50	1.5	0.5	0.3	3.0	1,625
Number of midwife assisted births	3,043	2,063	236	10,820	850
Number of female emigrants	395	441	0	2,185	850
Number of community based doctors (“Provinsialläkare”)	5	2	0	9	850
Number of total doctors	25	25	4	199	850

Table 2: Test of conditional randomization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Time varying confounding factors						
	log (births)	log(female deaths)	log(infant deaths)	log(doctors)	log (emigrants)		
log(midwives)	-0.034	0.010	-0.074	0.035	-0.068		
	(0.038)	(0.044)	(0.052)	(0.063)	(0.302)		
	Panel B: Age distribution of mothers						
	Age<=20	Age 21-25	Age 26-30	Age 31-35	Age 36-40	Age 41-45	Age 45-50
log(midwives)	0.002	-0.004	0.000	-0.005**	0.002	0.004	0.002
	(0.009)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)
	Panel C: Indicators of harvest yield: 0-6						
	Harvest=0	Harvest=1	Harvest=2	Harvest=3	Harvest=4	Harvest=5	Harvest=6
log(midwives)	0.003	0.026	0.015	0.042	-0.022	0.005	-0.069
	(0.034)	(0.024)	(0.029)	(0.076)	(0.077)	(0.056)	(0.067)

Notes: Each entry is separate regression. All specifications include a full set of area and time-fixed effects together with unit-specific time trends. Standard errors, clustered at the area level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 3. The relationship between MMR and the number of midwives

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(midwives)	-0.19**	-0.21**	-0.20**	-0.19**	-0.18**	-0.18**	-0.20**	-0.21***
(the-intent-to treat effect)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Births		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Infant mortality			Yes	Yes	Yes	Yes	Yes	Yes
Female deaths				Yes	Yes	Yes	Yes	Yes
Age distribution					Yes	Yes	Yes	Yes
Harvest indicators						Yes	Yes	Yes
Quadratic time trend							Yes	Yes
Weighted least squares								Yes
Number of observations	1,608	1,608	1,608	1,608	1,608	1,608	1,608	1,608

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

Table 4. Controlling for lagged MMR

	OLS (unweighted) estimates			WLS estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
log(midwives)	-0.20**	-0.18**	-0.18**	-0.21**	-0.19**	-0.19**
(the-intent-to treat effect)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)
MMR_{t-1}		0.09***	0.09***		0.08***	0.08***
		(0.03)	(0.03)		(0.03)	(0.03)
MMR_{t-2}			0.01			0.00
			(0.03)			(0.03)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1608	1581	1554	1608	1581	1554

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

Table 5. Estimates of the take up rate of the midwifery policy, the intent-to-treat effect and the risk of dying in childbirth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: The relationship between midwife-assisted births and the midwifery policy											
The take-up effect (first-stage)	0.19***	0.19***	0.19***	0.19***	0.19***	0.19***	0.19***	0.19***	0.19***	0.18***	0.19***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Panel B: The relationship between MMR and the midwifery policy											
The intent-to-treat effect (reduced form)	-0.38***	-0.38***	-0.39***	-0.40***	-0.39***	-0.36***	-0.37***	-0.37***	-0.34***	-0.38***	-0.35***
	(0.13)	(0.13)	(0.13)	(0.13)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.11)	(0.11)
Panel C: The relationship between MMR and midwife-assisted births											
Treatment effect (IV estimate)	-1.99**	-2.04**	-2.10**	-2.11**	-2.09**	-1.94**	-1.93**	-1.99**	-1.77**	-2.09**	-1.81**
	(0.90)	(0.91)	(0.93)	(0.94)	(0.88)	(0.86)	(0.86)	(0.88)	(0.77)	(0.88)	(0.78)
Births		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Infant mortality			Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female deaths				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age distribution					Yes	Yes	Yes	Yes	Yes	Yes	Yes
Harvest indicators						Yes	Yes	Yes	Yes	Yes	Yes
Emigration							Yes	Yes	Yes	Yes	Yes
Doctors								Yes	Yes	Yes	Yes
Lagged MMR										Yes	Yes
Weighted least squares									Yes		Yes
First-stage F-statistic	15	15	15	14	15	15	15	14	14	14	13
Observations	848	848	848	848	848	848	842	842	842	840	840

Notes: All specifications include a full set of area and time-fixed effects together with unit-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 area level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 6. Heterogeneous effects

	(1)	(2)	(3)	(4)
	Positive supply shocks	Negative Supply shocks	Bad Harvest (Harvest<=3)	Good harvest (Harvest>3)
	Panel A: The relationship between midwife-assisted births and the midwifery policy			
The take-up effect (first-stage)	0.21***	0.13**	0.26**	0.14**
	(0.08)	(0.05)	(0.11)	(0.05)
	Panel B: The relationship between MMR and the midwifery policy			
The intent-to-treat effect (reduced form)	-0.42**	-0.24	-0.63**	-0.30*
	(0.21)	(0.17)	(0.29)	(0.18)
	Panel C: The relationship between MMR and midwife-assisted births			
Treatment effect (IV estimate)	-1.96	-1.81	-2.39*	-2.20*
	(1.24)	(1.22)	(1.40)	(1.25)
First-stage *F*-statistic	8.0	7.0	6.1	6.3
Observations	458	384	267	575

Notes: All specifications include a full set of area and time-fixed effects together with unit-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 area level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 7. Results from the opening of a new midwifery school in 1856 on midwife availability and MMR

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: The effect of opening a new midwifery school on the availability of midwives						
First-stage effect	0.44***	0.47***	0.47***	0.48***	0.44***	0.44***
	(0.13)	(0.13)	(0.13)	(0.14)	(0.14)	(0.14)
Panel B: The effect of opening a new midwifery school on MMR						
Reduced form effect	-0.13**	-0.15**	-0.14**	-0.14**	-0.14**	-0.13**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Panel C: Instrumental variable estimates of the effect of midwives on MMR						
Treatment effect	-0.31*	-0.33*	-0.31**	-0.30**	-0.31*	-0.30*
	(0.18)	(0.17)	(0.16)	(0.15)	(0.18)	(0.18)
Births		Yes	Yes	Yes	Yes	Yes
Infant mortality			Yes	Yes	Yes	Yes
Female deaths				Yes	Yes	Yes
Age distribution					Yes	Yes
Harvest indicators						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 area 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 area level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 8. Test of parallel trends

	First-stage	Reduced form
1[Treatment group=1 & year>1855]	0.40**	-0.10*
	(0.15)	(0.06)
1[Treatment group=1 & year=1855]	-0.10	-0.05
	(0.11)	(0.16)
1[Treatment group=1 & year=1854]	-0.10	0.09
	(0.11)	(0.14)
1[Treatment group=1 & year=1853]	-0.06	0.11
	(0.13)	(0.15)
1[Treatment group=1 & year=1852]	-0.11	0.26
	(0.13)	(0.16)
1[Treatment group=1 & year=1851]	-0.15	0.15
	(0.10)	(0.15)
1[Treatment group=1 & year=1850]	-0.15	0.05
	(0.09)	(0.12)
1[Treatment group=1 & year=1849]	-0.15*	0.09
	(0.08)	(0.20)
1[Treatment group=1 & year=1848]	-0.10*	0.06
	(0.05)	(0.18)
Observations	1610	1608

All specifications include a full set of area 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%, **5%, and ***1%.

Table 9. Estimates of the reduced form relationship between the logarithm of the stillbirth rate and midwives

	Design1: 1830-1894 (1)	Design 1: 1861-1894 (2)	Design 2 1830-1894 (3)
Reduced-form effect	-0.02	-0.04	0.04
	(0.02)	(0.05)	(0.03)
Controls	Yes	Yes	Yes
Observations	1,608	842	1,608

Figure1. Total number of midwives in Sweden 1830-1894.

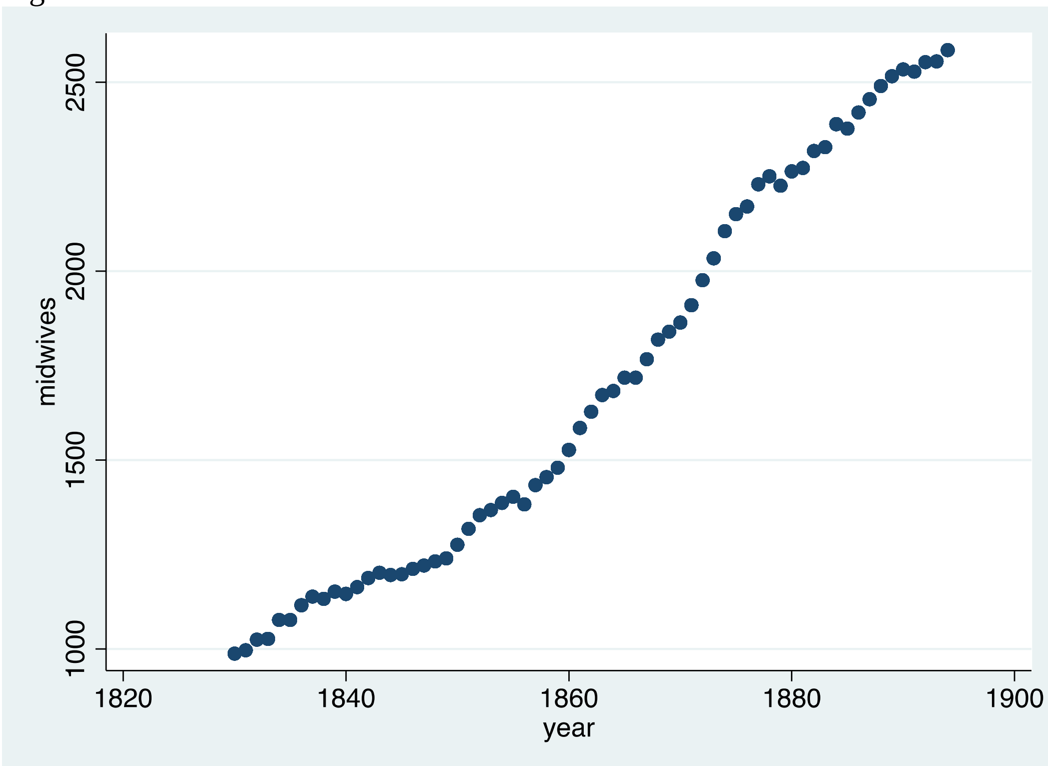


Figure 2. Regions (Län) of Sweden

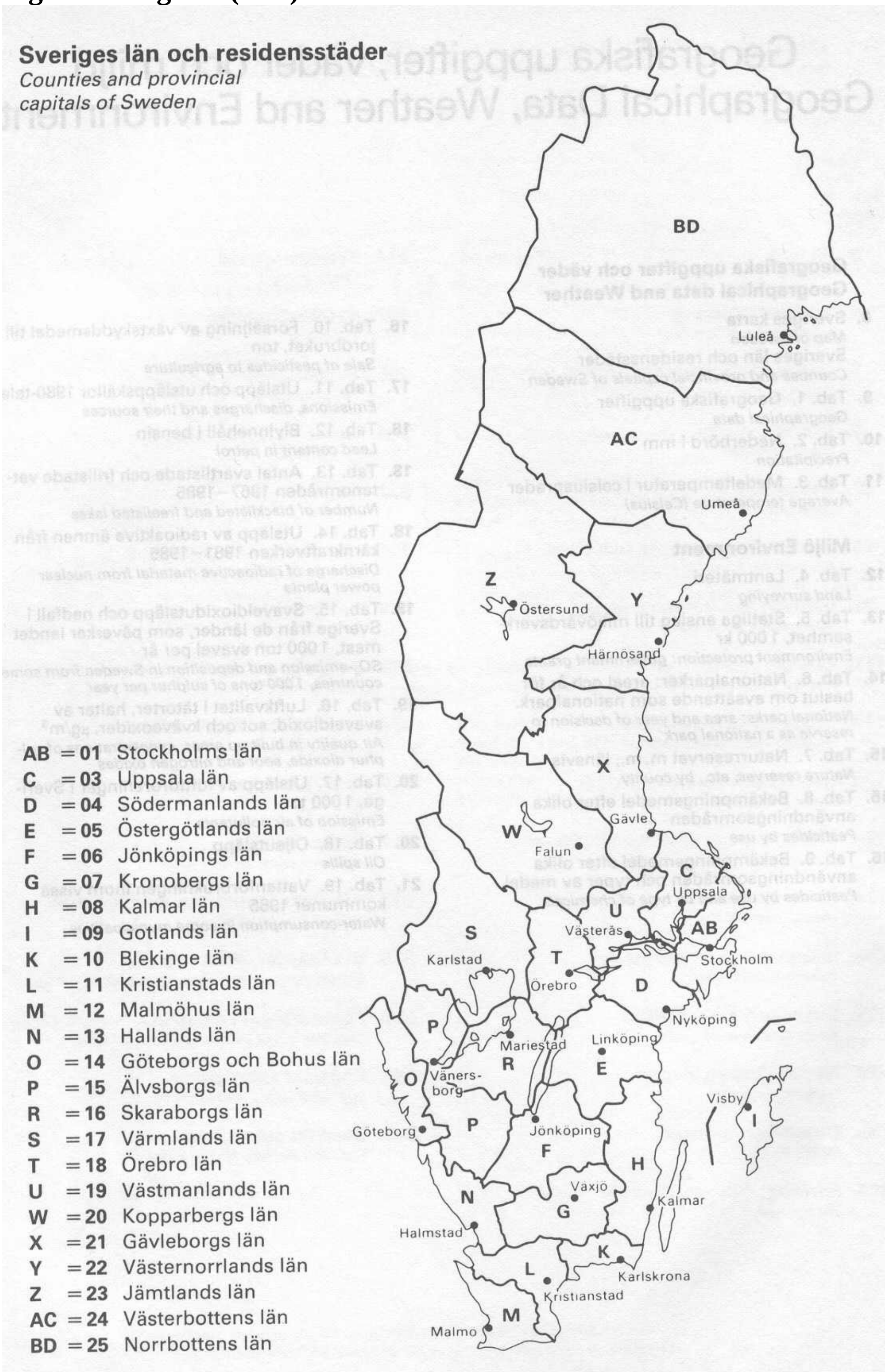
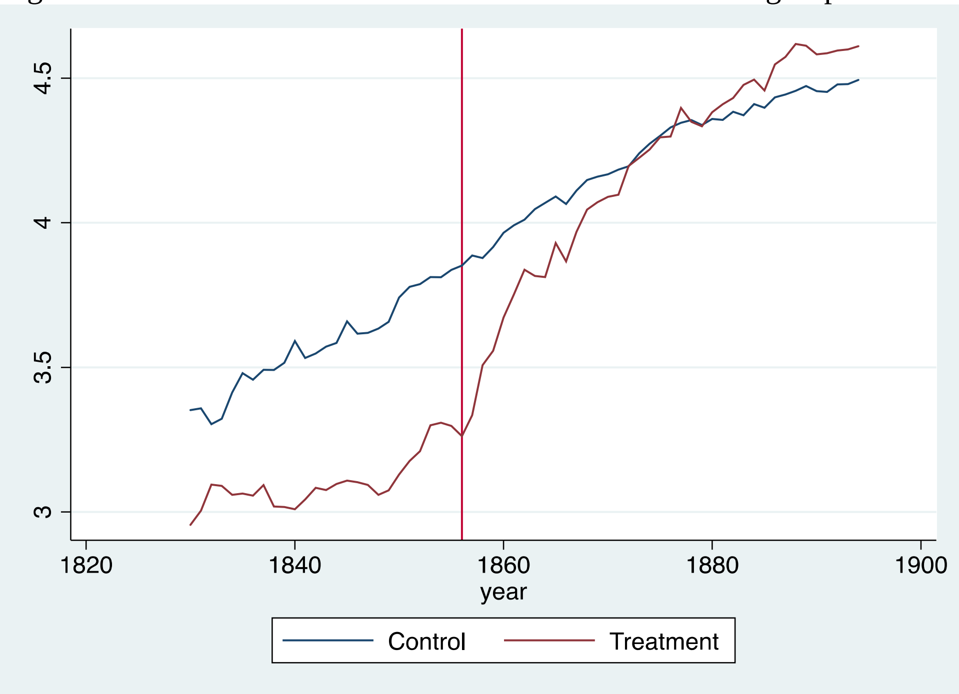
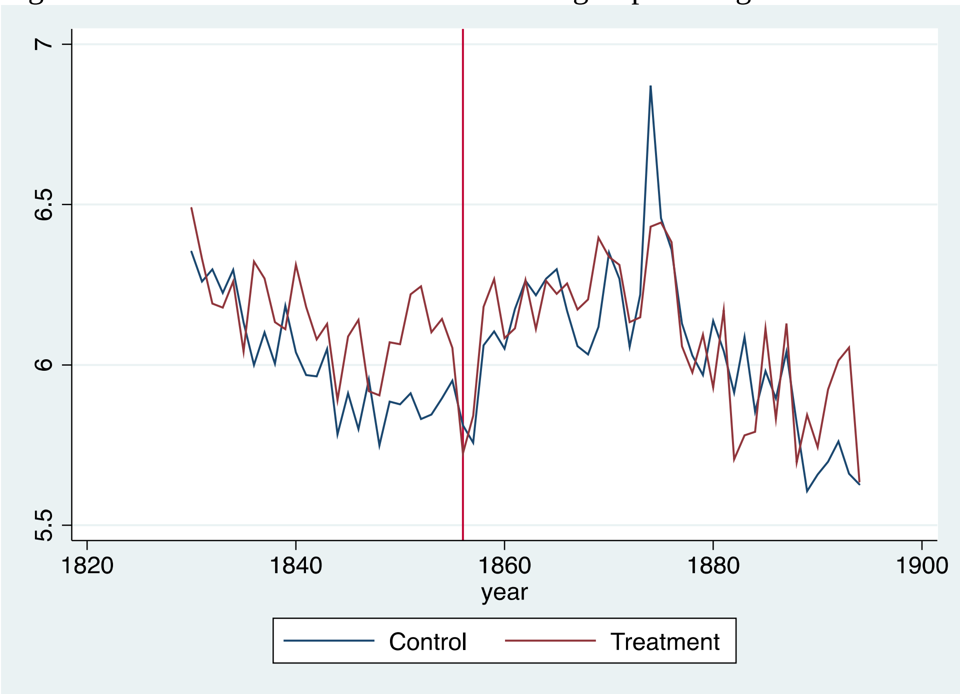


Figure 3. Number of midwives in the treatment and control groups 1830-1894



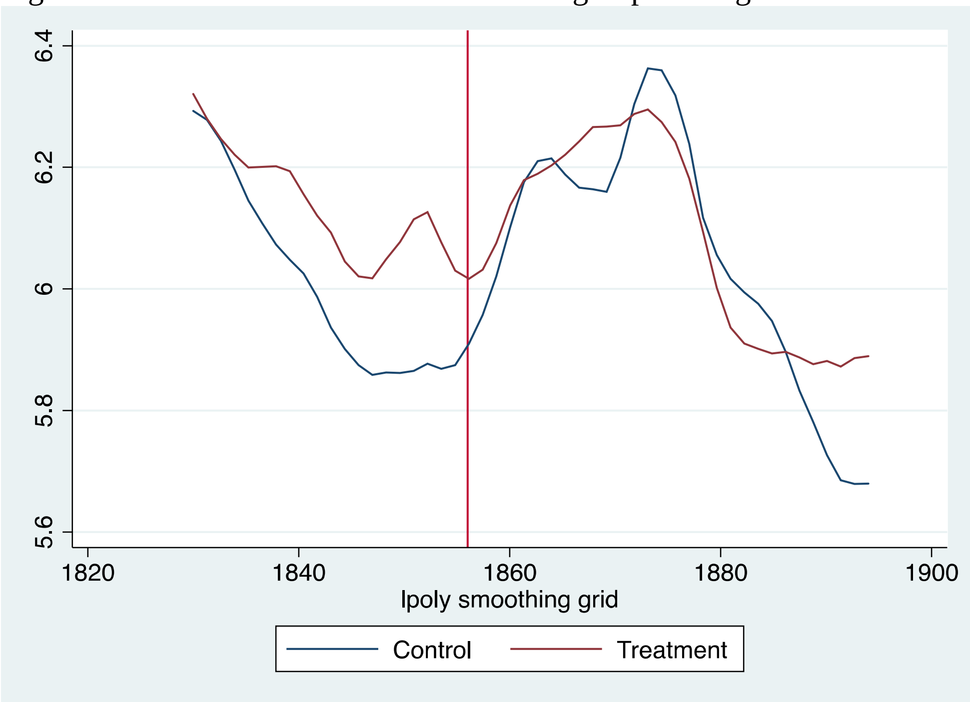
Notes. Number of midwives is expressed in logartimic form

Figure 4. MMR in the treatment and control groups during 1830-1894



Notes. MMR is expressed in logartimic form

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



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