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

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Abstract

This paper estimates the causal effect of a historical midwifery policy experiment on maternal mortality, infant mortality, and stillbirth during the period from 1830 to 1894 in Sweden. Exploiting sharp changes or "discontinuities" across time and place in the availability of trained and licensed midwives as an exogenous source of variation, I find that a doubling of trained midwives leads to a 20-40 percent reduction in maternal mortality and to 20 percent increase in the uptake of midwife-assisted homebirths. The results thus suggest that a 1 percent increase in the share of midwife-assisted homebirths decreases maternal mortality by as much as 2 percent, which is a remarkable finding given that the midwife training was only 6-12 months at that time. The results from this study contribute to current debate about the most effective strategy for reducing the unacceptably high maternal mortality in many developing countries, especially in low-resource settings.

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1. Introduction

About 800 women die everyday as a result of pregnancy or childbirth complications around the world and almost all maternal deaths (99 %) occur in developing nations (WHO 2014). In fact, a woman's lifetime risk of maternal death is 1 in 160 in developing countries,[1] as compared to 1 in 3700 in developed countries. The difference between maternal mortality in the developing world and the developed world is greater than that of any other health indicator. Reducing the high maternal mortality in the developing world is therefore considered to be a key policy issue. Consequently, one of the United Nations Millennium Development Goals is thus to significantly reduce maternal mortality.

However, despite 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)). It has 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) 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 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.[2] In addition, there is also a shortage of reliable information on maternal mortality and the type of birth attendant that assisted the birth (e.g., Graham (2002) and Ronsmans and Graham (2006)).[3] Although non-experimental studies can overcome some of these problems,[4] they still face the difficulty

[1] In many sub-Sarahan countries, the lifetime risk of maternal deaths could be as high as 1 in 20.

[2] 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.

[3] 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."

[4] 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.

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)).[5] In 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 a 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.[6] 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 explore a unique midwifery policy experiment in Sweden in the 19th century. With this new data,[7] I overcome most, if not all, of the impact evaluation problems discussed above. To start with, Sweden is one of the 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.[8] The statistical analysis can thus be based on extremely large sample sizes since there were roughly 120,000 births and 500 maternal deaths on a yearly basis. Consequently, the analysis will be based on a total of 8,012,080 (live and still) births and 37,519 maternal deaths as 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

[5] 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 result is questioned by Ronsman et al. (1997) who argue that the control area is not comparable to the treatment area.
[6] 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)
[7] I have collected this data myself from the Swedish National Archives and other sources. See the web appendix for further information.
[8] See Högberg and Wall (1986) and the references therein for a discussion of the Swedish historical vital statistics.

intervention. At the 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 *sharp* time-varying regional changes or "discontinuities" in the availability of trained midwives due to the severely restricted supply of educated and licensed midwives at the national level. 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 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 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 regional variation was extremely large, i.e., from 5 to almost 100 percent.[9]

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 time period had much more midwifery training. 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.[10] While the effect may seem large, it should be kept in mind that the (counterfactual) comparison is with respect to having a traditional

[9] 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.

[10] 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.

birth attendant (TBA) assisting the homebirth. Thus, it is a well-known fact that traditional practices may include harmful health care behavior during pregnancy and childbirths, such as improper use of drugs, pushing on the abdomen to hasten delivery and even the use of certain surgical procedures (McCarthy and Maine 1992).[11] In other words, the impact effect is the difference between a potentially deleterious treatment (TBA) and a much safer treatment (trained and licensed midwives).

A number of specification checks lends 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". I find no relationship between these "discontinuities" in midwife availability and other potentially important confounding factors such as fertility, female mortality (from other causes than maternal mortality) and various proxies of economic development and types of economic shocks (e.g., harvest failure). In the 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 bias since the number of time periods is very large. Importantly, controlling for the lagged MMR has no impact on the estimated effects. I also follow the suggestion of Solon et al. (2013) of comparing unweighted with weighted estimations as a useful test against model misspecification. Reassuringly, there is no difference between the unweighted and the weighted models. Finally, and perhaps the most convincingly, I find that the timing of the sharp changes in the availability of midwives lines up with the sharp changes in MMR and that both negative and positive supply shocks to midwife availability produces identical impact estimates. In other words, the sharp changes or discontinuities in the availability of midwives are arguably unrelated to any other confounding factor, such as the demand for midwives, which are likely to be much more slow-moving.

I argue that my finding, 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 in Sweden, has important implications for thinking about the most effective health intervention for reducing the currently very high MMR in low resource settings. This

[11] McCarthy and Maine (1992) discuss that traditional healers in northern Nigeria make "Gishiri cuts" (incisions in the vagina) on women who are not making progress in labor. In Sweden, both Romlid (1998) and Lundqvist (1940) provide many historical examples of the malpractices of TBA ("hjälpgummor").

follows from the fact that 19th century Sweden was a poor agrarian society and, in many respects, similar to many developing countries today with a high MMR, a high infant mortality rate, a low life expectancy, and a high fertility rate.[12] Moreover, the fact that many of the major causes of maternal mortality, such as hemorrhage, are similar across these two settings also supports external validity. As a result, it is possible to argue that having a skilled attendance at home can be a preferable strategy in 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 midwifes are crucial tools for breaking through supply barriers. Consequently, a home-based care approach with a short midwifery training program may be an attractive strategy as it is easier to recruit these midwives because it requires fewer educational criteria. It is also easier to retain midwives with a short midwifery course since they have fewer attractive alternatives. 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 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, 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.[13] 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

[12] See also Graham (2001), Högberg (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.

[13] 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".

shows that stillbirth cannot be used as a proxy for maternal mortality, at least not in the 19th century context.

This paper is related to several literatures in different fields. In economics, Miller (2008) uses a similar type of identifying strategy to evaluate the impact of an historical public health intervention, as induced by changes in woman suffrage laws in the U.S. in the early 20th century, on cause- and age-specific mortality. Two other examples in economics are Jayachandran et al. (2010), which analyzes the impact of the sulfa drug on maternal mortality in the U.S., and Jayachandran and Lleras-Muney (2009) which estimates the impact of the decline in maternal mortality (due to various public health interventions) on women's human capital investments in Sri Lanka.[14] There is also a large literature in the medical sciences analyzing the impact of various public 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 the relationship between midwives and maternal mortality (e.g., Loudoun (1992, 2000), Högberg et al. (1986, 1988) and Högberg (2004)).[15] 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).[16]

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.

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[14] Two other related studies are Miller (2006) and Daysal et al. (2013). In contrast to this study, these studies compare the outcomes between births assisted by midwives with those assisted by physicians using data from developed countries.

[15] 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 for a small area and without controlling for any confounders. As a result, this type of epidemiological approach can therefore not convincingly 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.

[16] 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.

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 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 agricultural 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 in the beginning of the period 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, especially those in very low-resource settings.

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.[17] 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/raptured uterus: 9 percent, and (v) complications from abortion: 8 percent (e.g., Say et al. (2014), McCarthy and Maine (1992)). It is important to stress that there can be considerably overlap among these direct causes. For example a hemorrhage may result from a ruptured uterus, or a life-threatening infection could be due to a prolonged and obstructed labor. Thus, this makes it is hard to unambiguously classify the direct causes of maternal deaths. It is also important to note that many of these birth complications occur among well-nourished and well-educated

[17] 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).

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. Högberg 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 deaths were due to direct obstetric complications. Of these direct cases, only 11 percent were due to infections while the others were due to difficult labor, eclampsia, and hemorrhage.[18]

To summarize, the comparison of death causes of maternal mortality between the developing countries today and Sweden in the 19th century suggests a high degree of comparability between the two settings.

2.2 Sweden's midwifery policy[19]

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, 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.[20] 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 about 35 midwives graduated annually from the Stockholm midwifery school. 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

[18] For the period after 1860, infections are reported to constitute nearly 50 percent of all maternal deaths. However, it is very difficult to know how many of the cases were only due to infections and not a consequence of any of the other direct causes of maternal deaths such as obstructed labor since there are considerably overlap among the causes as noted above. Högberg and Broström (1986) argue that the diagnosis puerperal fever was likely not confounded by septic abortions during the 19th century.
[19] This section is based on Högberg (2004), Romlid (1996, 1998) and Lundqvist (1940).
[20] 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 19th century.

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 them being deployed in places 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 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.[21] From 1819, the formal training period was 6 months and from 1840, the training period was 9 months. The basic training included the 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, and perforators).

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 for the entire country. 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

[21] 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.

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 the total number of births (both still and live) during the period 1830 to 1894. At the national level, there were altogether 7,770,239 live births and 241,841 stillbirths during this period. There were also 37,519 maternal deaths which 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, 20.5percent for ages 36-40, 10.6 percent for ages 41-45, and 1.5 percent for ages 46-50.

In the empirical analysis, yearly data on 25 geographical regions will be used. Figure 2 shows a map of these regions. It is important to stress that these regions are sufficiently large to get a reasonable estimate of MMR since the average number of yearly births is about 5,000 and with an average of 23 maternal deaths. There is considerable variation in both the cross-section and the time-series in these regions 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 1,274 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. Figure 3 shows the yearly variation in the MMR for all regions over the investigated period 1830-1894. The MMR is expressed in logarithmic form to make it consistent with the empirical specification discussed below. For midwives, the average number of midwives in a region 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. Figure 4 displays the yearly variation in the number of midwives, i.e., the explanatory variable of interest. Again, the variable is expressed in logarithmic form to make it comparable with the empirical specification in next section. The average share of midwife-assisted births was 57 percent but this share could be as low as 5 percent and as high 99 percent. 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 public health intervention, such as the availability of midwives on maternal mortality, one would ideally estimate an equation of the following form

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

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 region g in year t.[22] 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 unobserved factors in the error term v_{gt}. In this case, the parameter β 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., β<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

$$(2) \qquad log(MMR_{gt}) = a + b(\text{ share of midwife-assisted births}_{gt}) + n_{gt},$$

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 share of midwife-assisted births. Thus, we need to estimate an equation of the form

[22] Here, I follow the conceptual framework laid out by Jayachandran et al. (2010) for estimating the relationship between the availability of the sulfa drug and MMR.

$$(3) \qquad log(\text{share of midwife-assisted births}_{gt}) = \alpha + \pi(\text{midwife availability}_{gt}) + n_{gt},$$

where the parameter π 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, β, and the first-stage effect, π.

It is important to stress that estimating the causal effect of the public 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 (i) no referral system in case of complications during delivery; and (ii) a licensed midwife was basically not allowed to perform any important medical treatments other than deliveries. In addition, below I provide empirical support for that the exclusion restriction is likely to hold since both negative and positive supply shocks to the availability of midwives yield similar impact estimates, which is analogous to an overidentifying restriction test.

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 as induced by the supply restriction at the national level. 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 local or regional supply shocks in the availability of midwives that is arguably uncorrelated with the demand for midwives. To implement this design, I will make use of a difference-in-differences study with region-specific time trends. With this particular setup it is possible to relax the common trend assumption in a standard difference-in-differences design.[23] Clearly, the common trend assumption is not likely to hold in this setting because the data covers an extensive long time period, i.e., 65 years (1830-1894), with recurrent regional supply shocks throughout the whole period. In other words, it is unlikely that all regions will have similar trends in MMR in absence of the public health interventions. In sharp contrast, in a model that includes a region-specific time trend, the identification of the causal effect is based on sharp deviations from otherwise smooth trends, even where trends are not common. This particular empirical strategy is essentially a type of a regression discontinuity design (RD) with time as the forcing variable as noted by Lee and Solon (2011). Consequently, the identification relies on is on the appearance and size of a "jump" in the outcome (MMR) at the time of the public health intervention, i.e., the shock to the availability of midwives. In other words, if the intervention does not induce a sharp deviation from trend the identification fails (e.g., if the causal effect emerges only gradually). More formally, I will estimate regressions of the form:

$$(4) \qquad log(MMR_{gt}) = \beta \log(midwives_{gt}) + \alpha_g + \lambda_t + \pi_g f(t) + v_{gt},$$
$$g = 1,2..,25., \text{ and } t = 1830,1831,..,1894,$$

where *MMR* is the maternal mortality ratio, α_g is a region-specific effect, λ_t is a time-fixed effect and $\pi_g f(t)$ is a region-specific time trend, i.e., the "forcing variable" in a RD design.[24] The parameter of interest is β and it measures the effect of midwife availability, the number of midwives, on MMR. Thus, β is an intent-to-treat effect. The impact is measured as an elasticity since both the outcome and the explanatory variable is

[23] In a conventional difference-in difference design there are typically only a few years before and after a policy change making the assumption of common trends much more plausible.
[24] This is similar to the type of identification strategy used by Miller (2008). The only difference is that his treatment is binary (the introduction of female suffrage laws) while the treatment here is multi-valued (number of midwives).

expressed in logarithmic form. In the following, I will discuss a number of important specification issues concerning equation (4) and various specification checks.

Starting with the functional form of the region-specific trend $f(t)$, both a linear and a quadratic specification will be used (see Tables 4 and 5 for the results). Effectively, $f(t)$ will control for smooth changes in the outcome while the explanatory variable— $log(midwives_{gt})$— is going to captures any discontinues effects in the availability of midwives. Consequently, there must be sharp changes or discontinuities in the availability of midwives at the regional level to be able to estimate the parameter β. Naturally, there will be both large positive and negative shocks to the availability of midwives at the regional level on an annual basis due to the fact that the capacity to train and license midwives was limited as discussed above. Clearly, as long as these large regional supply shocks are uncorrelated with the local demand for midwives, both types of variations are permissible for identifying the effect β. Arguably, any change in the local demand for midwife-assisted births is likely to be much smoother than any sharp change in the regional availability of midwives. As a result, the timing of the causal effect must correspond closely with the sharp changes in the availability of midwives if this design is going to be credible. A distributed lag specification, i.e., including lags of $log(midwives_{gt})$, can be used to probe the timing issue (see Table 6 for the result). Ideally, the causal effect should appear immediately with little or no dynamic effects, otherwise the supply shocks may be correlated with slow-moving changes in the demand for midwife-assisted births. Moreover, a causal interpretation would also be strengthened if the results are similar for both sharp *positive* and *negative* changes in the availability of midwives (see Table 7 for the result). The basic idea is that if these sharp changes in the supply of midwives are unrelated to the demand for midwife-assisted births, the estimates of β should be more or less the same.

Turning to the functional form assumption of Equation (4), 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 maternal mortality, at least as long as fertility, i.e., log(number of births), is included as a covariate in the specification.[25] 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 *β*. 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 midwifes 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 explanatory variable with potential discontinuities in the outcome variable. Thus, in this case, we match large *percentage* changes in the number of midwives with potentially large *percentage* changes in maternal mortality.

It is important to note that equation (4) is a grouped data regression or a pseudo panel, i.e., aggregated data from repeated cross sections at the region-year level. 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 grouped data covers the universe of births and the average number of yearly births within a region is large,[26] i.e. almost 5,000 (see Table 1).[27] More importantly, however, there is little or no measurement error in the key regressor of interest, i.e., the number of regional midwives, which does not vary at the individual level but only at the group level,[28] i.e., the region-year level. As result, a bias in the estimated effect due to measurement error problems is likely to be negligible since the measurement error problem is only in

[25] 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 β 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.
[26] 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.
[27] Devereux (2007) argues that group sizes, i.e., number of annual regional births, should be larger than 2,000 to avoid the problem with measurement errors in pseudo panels.
[28] Since the regressor only varies at the group level, then equation (4) weighted by the group size is identical to OLS on the micro data.

the dependent variable and that the measurement errors is most likely of the classical form.

There is also the question of whether one should estimate the grouped data regression (4) by weighted least squares, WLS, (weighted by size of the regional birth cohort) 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 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 (see Tables 3 and 4 for the results).[29]

Another specification issue concerns the fact that midwives were likely to be placed in regions with an already high maternal mortality (e.g., due to the absence of midwives),[30] i.e.,

$$log(midwives_{gt}) = \pi log(MMR_{g,t-1}) + \alpha_g + \lambda_t + \pi_g f(t) + v_{gt}.$$

Thus, the deployment of midwives depends on past MMR. One way of dealing with this issue of a feedback from MMR to future values of midwife availability is to include a lagged dependent variable, $log(MMR_{gt-1})$, into equation (4). However, this introduces a bias in a panel data model with fixed effects (Nickell 1981).[31] Nonetheless, the bias of fixed-effect estimator is of the order of T^{-1}. Thus, since the number of years is very large, T=65, the bias is likely to be minimal (See Table 5 for the result).[32]

As a final comment concerning regression equation (4) is that the standard errors will be clustered at the regional level to address problems with serial correlations in the

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[29] Under exogenous sampling and correct specification of the conditional mean, both OLS and WLS are consistent estimators for the regression coefficients.
[30] The estimate of π is highly statistically significant. Thus, log(MMR is not strictly exogenous but it fulfills sequential exogeneity since there is no lagged responses..
[31] Alternatively, if one interprets equation (4) as representing a pseudo-panel regression rather than a true panel data equation it is possible to control for a lagged dependent variable without introducing bias as discussed by Wooldridge (2009). 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
[32] See Wooldridge (2002, p. 302).s

errors within clusters. Importantly, Hansen (2007) has shown that the clustered standard errors works well even if the number of clusters, N is fairly small as long as the the number of time periods, T, is sufficiently large, which is the case here since T=65 and N=25.[33]

Next we turn to the results of estimating equation (4). However, before we present these results, we start by testing whether the public health intervention—the sharp changes in midwife availability—is as good as randomly assigned. We conduct this "test of balance" by checking whether there any discontinuities in any other of the potential confounders conditional on the key conditioning covariates,[34] i.e., region-fixed effects α_g, time-fixed effects λ_t and region-specific time trends $\pi_g f(t)$. Thus, to perform this test, we therefore estimate regressions of the following form

$$(5) \qquad w_{gt} = \lambda log(midwives_{gt}) + \alpha_g + \lambda_t + \pi_g f(t) + v_{gt},$$

where w_{gt} is a candidate confounder. We expect the estimate of λ to be zero if the sharp changes in the availability of midwives, i.e. the supply shocks, are as good as randomly assigned. The confounders we use are the following: the log(number births), log(total female deaths except for maternal deaths), log(infant deaths), log(number of doctors), log(female emigration), seven variables capturing the age distribution of mothers and seven indicators for harvest yield where 0 corresponds to 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 covariates has a strong predictive power for MMR since the R^2 is 28 percent from a regression of MMR on only this set of variables.

Table 2 displays the results from this test. It shows that there is only 1 out of 19 estimates that is statistically significant at the 5 percent level. However, this is to be

[33] Clustering on both region and time, i.e., two-way clustering, does not affect the results.

[34] See also the discussion in Pischke and Schwandt (2014) 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."

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 and therefore not related the demand for midwives. It is particularly noteworthy that fertility (i.e., a demand-side variable), infant mortality (i.e., a measure of economic development and population health) and female mortality (i.e., a measure of population health) are not associated with the sharp changes in the availability of midwives.

Table 3 displays the results for the reduced form relationship between MMR and midwives, i.e., specification (4). All specifications 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.

Table 4 presents exactly the same specifications as in Table 3, with regressions weighted by the size of the regional birth cohort. The results from WLS are almost identical to the unweighted estimates in Table 3. Thus, reassuringly weighting does not matter for our understanding of the effect of midwifes on MMR.

Another specification check is controls for a lagged dependent variable since one could argue that the midwifery policy followed a strategy of placing midwives in regions with a previously high MMR. Table 5 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 Tables 3 and 4. The estimated effect is little affected by adding lagged outcomes. The estimate of the first order lag is also rather 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 only limited 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.[35]

Yet an additional specification check is to estimate a distributed lag model, i.e., lags of *log*(*midwives*), to investigate timing issues of the causal effect. Table 6 shows the results for a one and two-lag specification for both unweighted and weighted regressions. Importantly, there are no dynamic causal effects since the coefficients on both the first and second lag of *log*(*midwives*) are close to zero and not significantly different from zero. Thus, the causal effect of midwives on MMR comes immediately.

A final specification check is to test whether the results are similar for both *positive* and *negative* changes in the availability of midwives. Table 7 displays these results. Reassuringly, both types of variation yield strikingly similar results.

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

Having estimated the intent-to-treat effect, we next turn to the 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 8 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 previously (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

[35] See Gabrysch and Campbell (2009) and Paxton et al. (2005).

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,[36] 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).[37]

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 and 4 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.

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— parameter β 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 strategy based on data after 1860 since both these designs make use of a variation in the availability of midwives

[36] For example, the estimate in Column 10 is –0.04 with a standard error of 0.04.
[37] 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.

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, since the midwives were placed in these 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

(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 a region-fixed effect, λ_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 ρ measures the reduced form effect of the newly opened midwifery school on MMR. However, to estimate the midwife policy effect—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) $\log(\textit{midwives}) = \pi 1[\text{treatment group and year}>1855] + \alpha_g + \lambda_t + v_{gt}$,

where the parameter π 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[treatment group and year>1855].

Table 9 reports the results from this design. In panel A, we report estimates of the first-stage effect, i.e., 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 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 closet to the midwifery school in Gothenburg—has increased its midwives by 55-62 per cent as compared to the control group.[38] 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.

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[Treatment

[38] The exact estimate is computed as [exp(parameter estimate) –1].

group*Year=1855], 1[Treatment group*Year=1854],.. , 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 10 reports the results from adding all these indicators to the difference-in-difference specifications reported in the last column of Table 9. Column 1 in Table 10 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 9. 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.[39] This finding is also consistent with graphical evidence from Figures 5 and 6. Figure 5 shows the first-stage relationships for the treatment and control groups while Figure 6 displays the reduced form relationship. Importantly, Figure 6 reveals that the reduced form effect comes immediately after the opening of the midwife school in 1856 since there is a large and sharp decrease in the MMR for the treatment group. In other words, the treatment group has on average a much larger MMR than the control group before 1856, but this difference in MMR disappears immediately after 1856 since both groups then has both similar levels and trends in MMR.

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

[39] I fail to reject that the two groups have parallel trends because the standard errors are so large.

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. In other words, to estimate whether a public health intervention had an impact on maternal mortality one must have data on MMR.

5. Discussion and conclusions

In this paper, I have estimated the causal effect of a public health intervention—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 19th century Sweden has potentially important implications for the most effective health strategy of reducing the currently high MMR in low resource settings. Specifically, the results from this study suggests that having a skilled attendance at home can be a preferable strategy in 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.

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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 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.

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
MMR	489	285	0	4,048	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 stillbirths	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
Share of midwife-assisted births	0.58	0.25	0.05	0.99	850
Number of female emigrants	395	441	0	2,185	850
Number of community based doctors (“Provinsialläkare”)	5	2	0	9	850
Total number of 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 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 3. The relationship between MMR and the number of midwives (unweighted estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(midwives)	–0.19**	–0.21**	–0.20**	–0.19**	–0.18**	–0.18**	–0.20**
(the-intent-to treat effect)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Births		Yes	Yes	Yes	Yes	Yes	Yes
Infant mortality			Yes	Yes	Yes	Yes	Yes
Female deaths				Yes	Yes	Yes	Yes
Age distribution					Yes	Yes	Yes
Harvest indicators						Yes	Yes
Region-specific time trends	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic
Number of observations	1,608	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 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. The relationship between MMR and the number of midwives: weighted least squares estimates (WLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(midwives)	–0.19**	–0.20**	–0.19**	–0.19**	–0.17**	–0.17**	–0.21***
(the-intent-to treat effect)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Births		Yes	Yes	Yes	Yes	Yes	Yes
Infant mortality			Yes	Yes	Yes	Yes	Yes
Female deaths				Yes	Yes	Yes	Yes
Age distribution					Yes	Yes	Yes
Harvest indicators						Yes	Yes
Region-specific time trends	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic
Number of observations	1,608	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 together with region-specific time trends. The dependent variable is log(MMR) where MMR is the maternal mortality ratio. The estimates are weighted by the size of the yearly regional birth cohort. 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 5. Controlling for the lagged MMR

	Unweighted estimates			Weighted 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)
log(MMR_{t-1})		0.09***	0.09***		0.08***	0.08***
		(0.03)	(0.03)		(0.03)	(0.03)
log(MMR_{t-2})			0.01			0.00
			(0.03)			(0.03)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,608	1,581	1,554	1,608	1,581	1,554

Notes: All specifications include a full set of region and time-fixed effects together with region-specific quadratic time trend and a full set of control variables, i.e., births, infant mortality, female deaths, age distribution and harvest indicators. 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 6. Distributed lag models

	Unweighted estimates			Weighted estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
log(midwives$_t$)	–0.18**	-0.17*	-0.18*	–0.19**	-0.20**	-0.20**
(the-intent-to treat effect)	(0.08)	(0.09)	(0.08)	(0.07)	(0.08)	(0.08)
log(*midwives$_{t-1}$*)		-0.02	0.02		0.02	0.06
		(0.10)	(0.11)		(0.09)	(0.09)
log(*midwives$_{t-2}$*)			-0.01			-0.04
			(0.08)			(0.06)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1608	1581	1554	1608	1581	1554

Notes: All specifications include a full set of region and time-fixed effects together with region-specific quadratic time trend, a lagged dependent variable and a full set of control variables i.e., births, infant mortality, female deaths, age distribution and harvest indicators. 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 7. Samples with increasing and decreasing availability of midwifes

	Increasing availability of midwives		Decreasing availability of midwives	
	Unweighted estimates	Weighted estimates	Unweighted estimates	Weighted estimates
	(1)	(2)	(3)	(4)
log(midwifes)	-0.22	-0.19	-0.18*	-0.18*
(the-intent-to treat effect)	(0.15)	(0.14)	(0.10)	(0.09)
Control variables	Yes	Yes	Yes	Yes
Observations	769	769	812	812

Notes: All specifications include a full set of region and time-fixed effects together with region-specific quadratic time trend and a lagged dependent variable and a full set of control variables i.e., births, infant mortality, female deaths, age distribution and harvest indicators. The control variables included are number of births. 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 8. 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									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 region and time-fixed effects together with a linear region-specific time trend**.** 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 9. 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 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 10. 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 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 11. Estimates of the reduced form relationship between the logarithm of the stillbirth rate and midwives

	Design 1: 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

Notes: Se notes from previous tables.

Figure 1. Total number of midwives in Sweden 1830-1894.

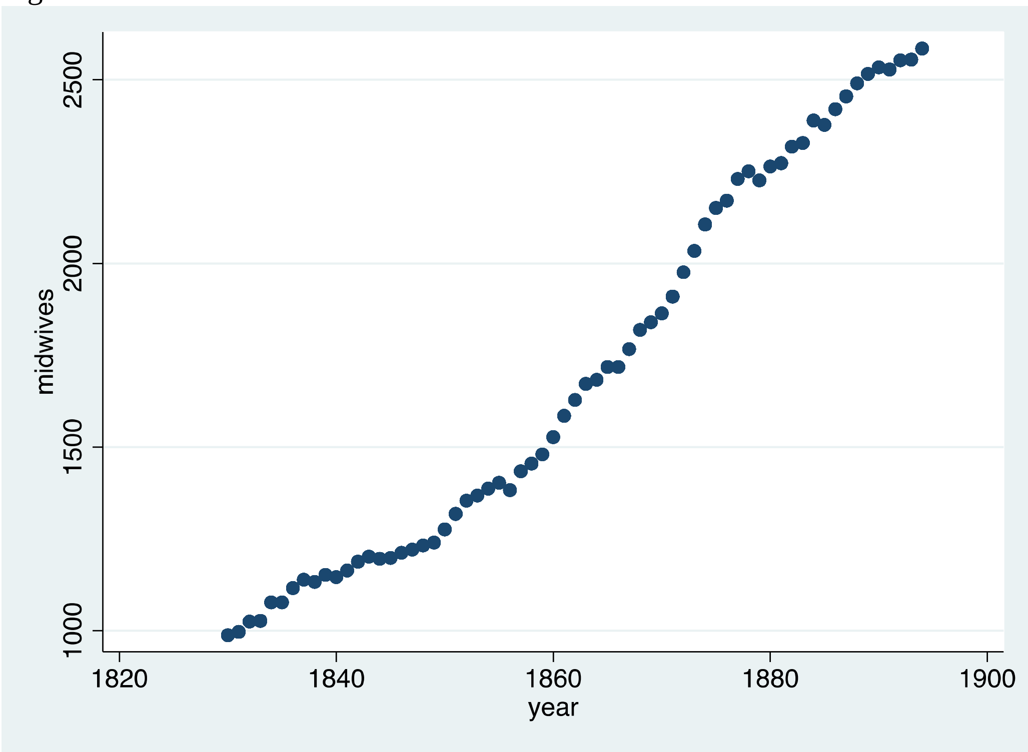


Figure 2. Regions (Län) of Sweden

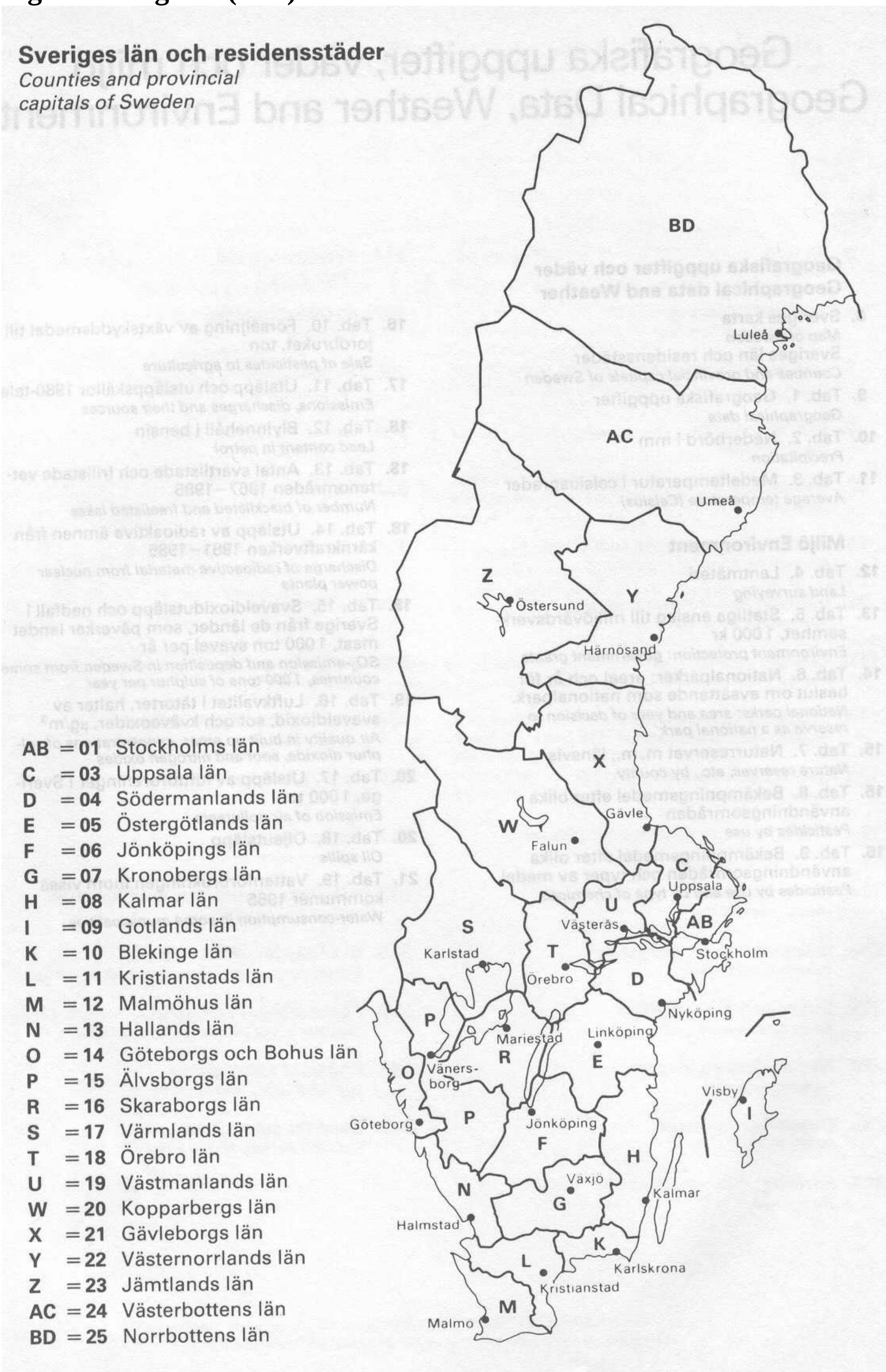
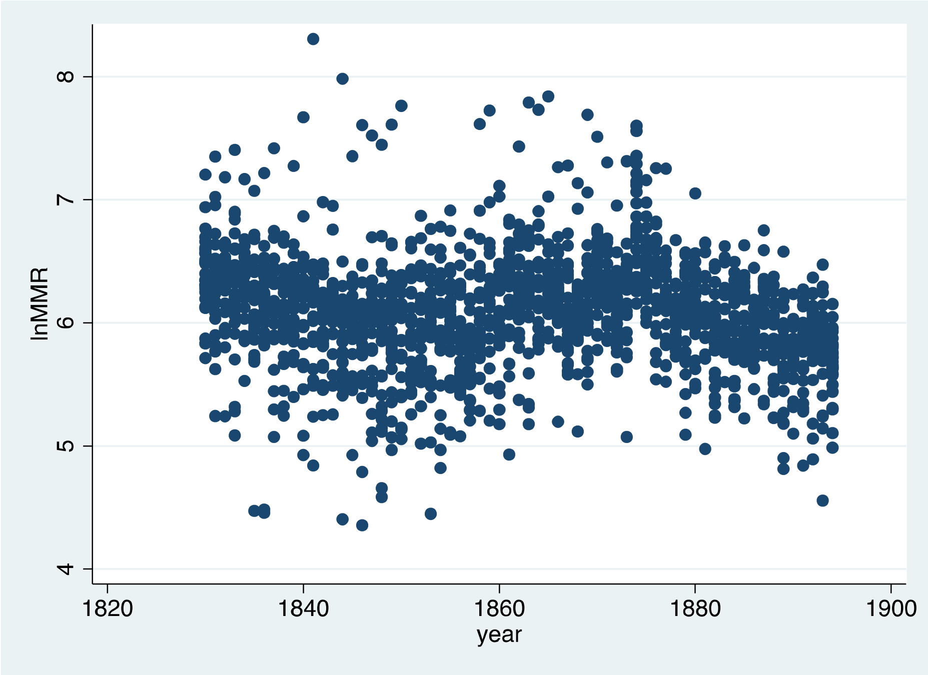
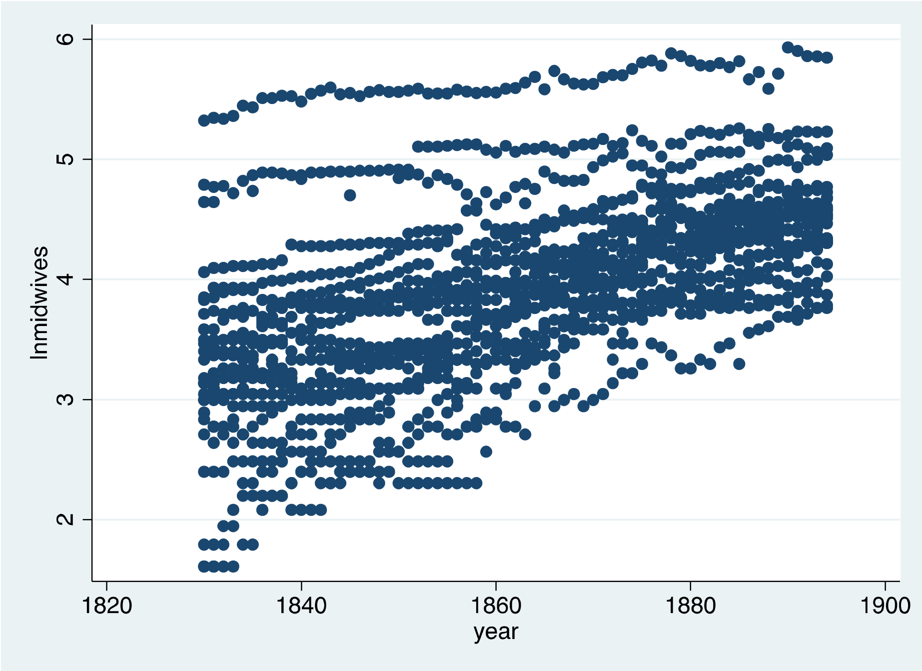


Figure 3. Regional variation in MMR over the period 1830-1894



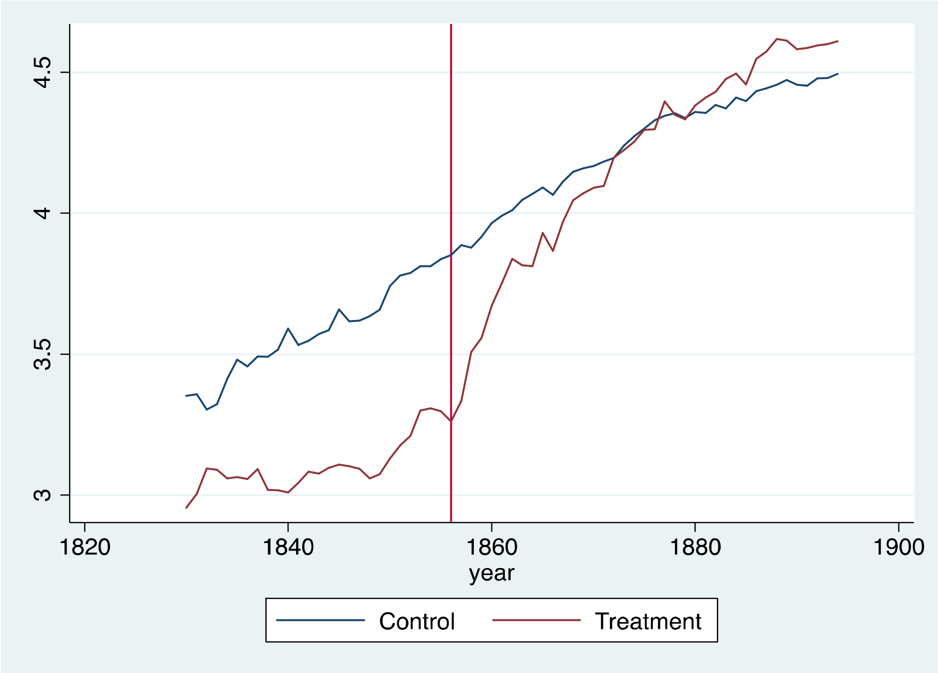
Note: MMR is expressed in logarithmic form

Figure 4. Regional variation in the number of midwifes over the period 1830-1894



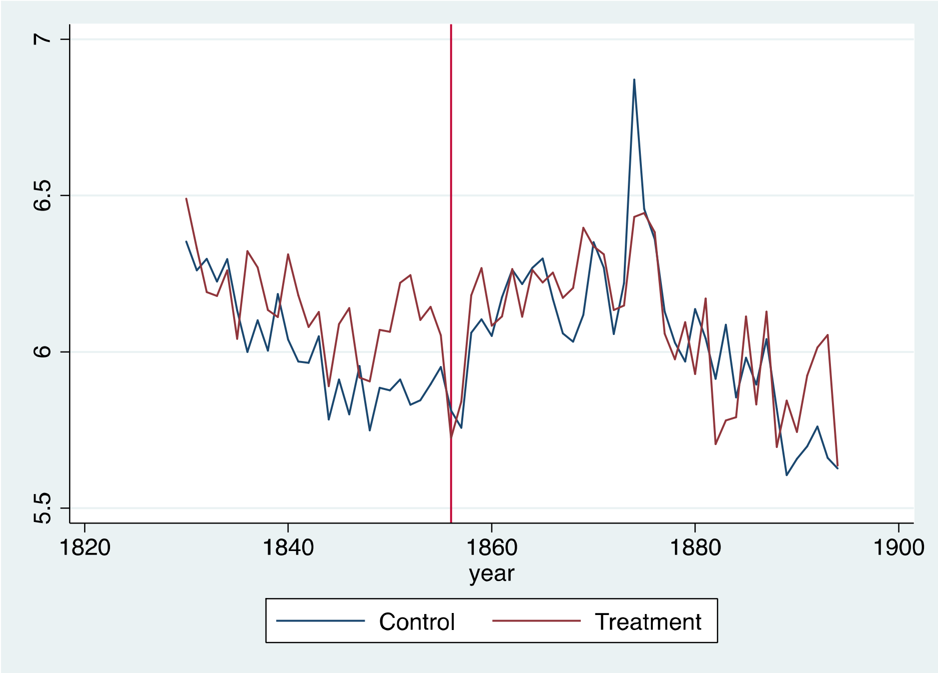
Note: The number of midwives is expressed in logarithmic form

Figure 5. Number of midwives in the treatment and control groups 1830-1894



Notes. The number of midwives is expressed in logartimic form

Figure 6. MMR for the treatment and control groups during 1830-1894



Notes. MMR is expressed in logartimic form