The Effect of Expanding a Neonatal Intensive Care System on Infant Mortality and Long-Term Health Impairments

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We study the effects of the geographic expansion of a Neonatal Intensive Care Units (NICU) system and a Newborn Emergency Transportation System (NETS) on neonatal and infant mortality and long-term impairments. We utilize gradual expansion in Hungary, we use administrative and census data, and we identify the effect from longitudinal variation in access, using changing distance as an instrument. Improving access to give birth in a city with a NICU decreases 0-6-day mortality by 153/1000 (<1500g) and 24/1000 (<2500g). NETS effects are positive but smaller. Improved access saves lives in the long run, with zero overall effects on long-term impairments. (JEL I1, H51)

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The death of a child is a tragedy, to be prevented if resources allow for it. Thus, cutting infant mortality rate is an important policy goal even if its level is low. Large reductions of high-level infant mortality are possible by promoting relatively inexpensive practices, such as free antenatal care, or the use of antibiotics or aseptic techniques (Martines et al. 2005). However, some infant mortality remain difficult to prevent after such measures are exhausted. In particular, reducing early neonatal mortality (deaths within 6 days of birth) may require highly specialized intensive care among very risky births. Such care is provided by Neonatal Intensive Care Units (NICUs).

NICUs are specialized units located next to obstetrics units in the same hospitals, to care for newborn babies with high risk of mortality, right after they are born. Newborns at high risk include the majority of very low birth weight (VLBW) children (<1500g), and also many of the substantially larger pool of children with birth weight between 1500g and 2499g (the two groups together are called low birth weight, or LBW, children). In this study, as in most of the literature, we focus on level-3 neonatal intensive care units and call them simply NICUs (excluding level-2 units).

NICUs were first established in the 1960s in the U.S.A. and some other rich countries. Virtually all other rich and medium-income countries followed later (e.g., India in the 2000s or Hungary in the 1970s). Typically, such systems are built up gradually, starting with lower capacity and limited geographic coverage. NICU systems are often complemented with a Newborn Emergency Transportation System (NETS), which provides specializes transport to newborn babies from obstetrics units at other hospitals to NICUs.

Both NICUs and NETS are expensive to establish, operate, maintain, and expand. It is therefore important to learn how effective they are in saving lives not only the short run but also the long run. In addition, it is important to know whether they have additional effects on the prevalence of chronic illnesses or significant impairment in the longer run, either by reducing such risks for infants who would survive anyway, or increasing such risks by saving infants who would later develop such conditions.

In this paper we estimate the effect of expanding a NICU system and the corresponding NETS system on three outcomes: neonatal mortality (within 0-6 days of delivery), infant mortality (within 0-364 days), and significant impairment that is diagnosed any time during childhood. Our goal is to get quantitative estimates for the effects that may guide policy decisions of expanding a NICU system in a middle-income country in the 21st century.

We jointly estimate the effect of improved access to NICU hospitals and the NETS that connects non-NICU hospitals to NICU hospitals. We estimate the effects on long-term impairment on a smaller subsample using the same empirical strategy. To be more precise, we estimate the effects of giving birth in a city with a NICU hospital or a NETS-connected hospital, instead of the effects of giving birth in such hospitals. Our empirical strategy identifies these effects from improved access due to decreasing distances in a country where geographic distance tends to be an important determinant of access to public services (Elek, Váradi, and Varga 2015). We argue that these effects are relevant from a policy point of view. They include the choice of hospital of delivery if there are more hospitals in a city, which choice is part of how the system works. And they measure the effect of improved access due to better geographic coverage. In any case, these are the quantities that we can estimate with our empirical strategy and data. Nevertheless, we show that the effects of being born in a hospital with NICU or connected to NETS are likely close to, or somewhat stronger than, our estimates of being born in a city with such a hospital or hospitals.

All papers on the effects being born in a hospital with a NICU on early neonatal mortality found by a comprehensive survey (Lasswell et al. 2010) rely on cross-sectional comparisons. However, identifying the effect of NICUs is difficult due to various selection mechanisms, which make cross-sectional studies vulnerable to bias even if they condition on many covariates or use an instrumental variable such as distance to hospitals. Specific care practices of neonatal intensive care have been examined in a longitudinal framework (e.g. Grytten et al. 2017), but those results are not about expanding the entire system. We don't know of any study that would estimate the effects of expanding the NICU system or the effects of neonatal transportation system (NETS) from non-NICU hospitals to NICU hospitals.

The available evidence is also incomplete in terms of the outcome variables. Typical analyses focus on neonatal mortality, within 0-6 days of delivery. However, when evaluating the social benefits of a NICU/NETS system, it is necessary to uncover the longer-run effects on mortality, or the likelihood of developing significant impairments during childhood. Our paper estimates such effects together with neonatal mortality, in a unified empirical framework.

To gain credible estimates of the effect of establishing a NICU system, including NETS, this paper uses an empirical strategy that allows for identification from longitudinal variation in geographic coverage. We combine a difference-in-differences analysis with an instrumental variables strategy to handle selection, using the distance of residence of the mother to the nearest city with a NICU hospital, and the nearest city with a NETS-connected hospital, as instruments. While the residential distribution of mothers is not random, hindering cross-sectional comparisons, our strategy relies on longitudinal variation in distance, due to opening new NICUs in hospitals in new cities, and due to connecting existing non-NICU hospitals to the NETS in new cities. This longitudinal variation in distance is more likely to be random than its cross-sectional variation would be, which is supported by additional evidence that we'll present. It's also a strong instrument, because distance is an important determinant of access in the context of our analysis. In cross-sectional settings, distance to health facilities has been used in the literature as an instrument (Cutler 2007; Mújica-Moca et al. 2019; McClellan, McNeil, and Newhouse 1994). To our knowledge, ours is the first paper to utilize longitudinal variation in distance to analyze the effect of access to health care services.

We make use of the experience of Hungary. Hungary started to establish its NICU system in the 1970s in a few cities, and it gradually expanded it through 2015 by establishing new NICUs, often in new cities. Starting in 1990, it introduced and then expanded a newborn emergency transportation system (NETS) from hospitals without a NICU to hospitals with a NICU. We collected the information on the expansion of the NICU and NETS systems by a survey with the management of the appropriate organizations. To estimate effects on neonatal and infant mortality, we use individual-level administrative data on all births and all infant mortality events in Hungary from 1990 through 2015. To estimate effects on long-run impairment, we use data from the national census of 2011, which includes questions on impairments, linked to birth registry data. While we have data for earlier time periods, we focus on the effects after 1990, because that's when NICUs started to use highly improved medical technology, making earlier estimates less relevant for today's policy decisions.

To summarize our results, we estimate substantial effects of improved access to NICUs on neonatal mortality (0-6 days), and we find very similar estimates on total infant mortality (0-364 days). The magnitudes are larger for newborns with very low birth weight (<1500g), but they are also significant for the much larger group of newborns with 1500g to 2499g birth weight. When comparing to baseline mortality rates, the effect estimates are similar in magnitude in these two groups. We estimate smaller, but non-negligible, effects of the NETS. Finally, our estimates of the effects on impairments are all very close to zero and statistically not significant. Taken together, these results provide strong evidence that the NICUs/NETS system leads to a substantial decrease of early neonatal mortality, most of the lives it saves are lives saved for the long run, and the NICU/NETS system doesn't increase long-term impairment, either because the children it saves don't develop such impairment, or because it helps prevent impairment of children who are not at the margin of mortality, which compensates for the former.

In more detail, we estimate that giving birth in a city with a NICU decreases 0-6-day mortality by 153 per 1000 live births for infants with birth weight1500g or less, by 10 per 1000 live births for infants with birth weight 1500 to 2499, and by 24 per 1000 live births for infants with birth weight less than 2500g; the corresponding 95% confidence intervals are [77, 229], [4, 16], and [10, 38]. These corresponds to a 35% to 50% reduction in relative to baseline rates at the beginning of the time period (350/1000, 20/1000, and 66/1000). The point estimates for 0-364-day mortality are 144/000, 21/1000, and 31/1000 (baseline rates 460/1000, 40/1000, 100/000). Giving birth outside a city with a NICU but connected in a NETS is estimated to decrease 0-6-day mortality by 57/1000 for <1500g births (not significant), 9/1000 for 1500g-2499g births and 9/1000 for <2500g births; effects on one-year mortality are 20/1000 (not significant), 11/1000, and 8/1000. Our point estimates on the effect of NICUs on the incidence of impairment are 23/1000 for <1500g births, 0/1000 for 1500g-2499g births, and 4/1000 for <2500g births; neither these estimates, nor the estimated NETS effects, are statistically different from zero at any conventional level.

Our analysis contributes to the existing literature in at least four ways. First, to our knowledge, ours is the first study to directly measure the effect of expanding a NICU system as opposed to the effect of delivery in individual hospitals of the effect of specific interventions. Second, it estimates the effect of establishing and expanding neonatal transporting (NETS) jointly with the expansion of NICUs. Third, it estimates longer-run mortality effects and effects and long-run impairment to quantify the effects on saving risky newborns past the first few days of delivery and its potential trade-offs. Fourth, our study uses an identification strategy based on changing distance, which improves upon existing identification strategies and circumvents selection bias.

We believe that the Hungarian experience is especially relevant for middle-income countries that consider establishing or expanding their NICU and NETS systems to improve access to previously underserved regions. Our estimates quantify the potential benefits, which we find to be substantial. Perhaps as importantly, we find that a NICU system can save lives for the long run without substantial effects on developing significant impairment later in life, or compensating such effects by helping other infants.

The remainder of this paper is organized as follows. The next section summarizes the results from the previous literature. We then introduce the sources of our data and the data linkages we carried out. We continue with showing trends in births and infant mortality and discuss the details of the health system of Hungary, with a focus on the establishment and expansion of NICUs and NETS. We then outline our empirical strategy and present evidence in support of it. The subsequent two sections show our main results and summarize the results of the robustness checks. The last part concludes.

I. Literature

Our paper estimates the effect of the geographic expansion of a NICU/NETS system, and we use longitudinal variation in the distance of residence to facility as a source of identifying variation. We are not aware of papers in the literature that attempt to answer the same question or use the same identification strategy. At the same time, there is a rich literature on the effects of various aspects of neonatal intensive care from a wide range of countries.

A meta-analysis of earlier studies finds strong associations of giving birth in NICUS and mortality, but all papers rely on observational cross-sectional data (Lasswell et al. 2010). Sosnaud (2019) uses cross sectional estimates and finds a significant negative relationship between the number of NICUs and infant mortality. The results are based on a large set of data, using almost 23 million infant birth records across 50 states of the US from 1997 to 2002, allowing to control for a rich set of individual characteristics. Grytten et al. (2017) provides an analysis of the effects of various medical interventions, many of which are offered in NICUs. It uses data for more than 40 years in Norway and establishes a negative causal relationship between the introduction of some new medical interventions and mortality among the newborn. As the overlap is incomplete between medical services studied by Grytten et al. (2017) and those offered by the NICUs, their results cannot be interpreted as the effect of NICUs on infant mortality. (Mújica-Moca et al. 2019) uses distance to facility as an instrument in a cross-sectional analysis of the effect of various levels of neonatal care (NICU hospitals versus level 2 and level 2 hospitals) on mortality. They find small effects. Almond et al. (2010) applies a regression-discontinuity framework on U.S. data to estimate the effect of access to more specialized care on infant mortality; Bharadwaj et al. (2013) uses a similar approach to assess the effects on school outcomes. The regression-discontinuity approach makes use of discontinuity in access to additional treatment at 1,500 grams of birth weight; both of these studies find strong effects.

Several papers address the risks of the transportation of newborns to intensive care units. Most of this strand of the literature finds that transportation comes with undoubted benefits as well as higher risks. Most related studies find significant health gains in terms of child outcomes for in utero versus ex utero transfer to NICUs (Bowman et al. 1988; Chung et al. 2009; Harris et al. 1981; Hohlagschwandtner et al. 2001; Kaneko et al. 2015; Kollée et al. 1992; Lamont et al. 1983; Marlow et al. 2014; Messner 2011; Mori et al. 2007; Moro et al. 2007; Resnick et al. 1998; Shlossman et al. 1997). These papers mostly use relatively small samples and cross-sectional data, and none of these studies focus on the gains of newborn transportation as opposed to no access to a NICU at all.

The literature on the long-run health of infants treated in NICUs focus on the health risks related to preterm births, including visual impairments, hearing problems, learning disabilities and many more (Behrman 2007; Wilson-Costello

2007; Lindström et al. 2007; Lindström, Lindblad, and Hjern 2011; M. C. McCormick 1989; Marie C. McCormick and Litt 2017; Blencowe et al. 2013). To our knowledge, there is no documented attempt yet in the literature to estimate the causal effect of having access to a NICU on these long-term outcomes.

Our identification strategy uses longitudinal variation in the distance of residence to cities with NICU/NETS hospitals. We are not aware of studies that use the longitudinal variation of distance. In contrast, cross-sectional variation of distance to health services is used by many papers to identify various effects (McClellan, McNeil, and Newhouse 1994; Cutler 2007; Ambardekar et al. 2010; Abrams et al. 2011; Khan et al. 2011; Lorch et al. 2012; Mújica-Moca et al. 2019). However, as emphasized by (Garabedian et al. 2014), the cross-sectional spatial distribution of patients is likely correlated with health outcomes on top of the potential effects of access to health services. In contrast, our strategy of using longitudinal variation in distance is likely free from that endogeneity.

II. Data

We combine data from three sources for the analysis in this study: vital statistics, the national census, and our own survey on the expansion of NICUs and NETS. Births and mortality data are from the national vital statistics of all births and any subsequent deaths up to 364 days. The birth data include information on birth weight, gestational age, other birth-related variables, municipality of delivery, municipality of residence of the mother, whether the father is known, and education and labor market status of mother and father (if known). For future reference, each city, town and village is a separate municipality in Hungary. In line with the literature, we classified live births of very low birth weight (VLBW) if weight was <1500g and low birth weight (LBW) for <2500g. We present results for the two birth weight groups as well as the non-overlapping group of 1500g to 2499g. The administrative database covers years 1970-2015 and includes 5,331,207 live birth events and 93,398 infant mortality events.

We focus on results by birth weight. An alternative indicator of risk, also contained in our data, is whether the birth is pre-term (<37 weeks) or very pre-term (<32 weeks). Our main results are for birth weight categories as those are more precisely measured; we show that the results are similar for pre-term categories among the robustness checks. These indicators are ex-post to delivery; our data has no ex-ante risk indicators. For reasons similar to ours, much of the related literature has focused on low birth weight infants (Lasswell et al. 2010; Grytten et al. 2017; Koller-Smith et al. 2017).

Long-term impairment data come from the 2011 census that covered the entire population of Hungary. Among other things, the census contains self-reported information on long-term impairment, and its various types. Information on legal minors was provided by their parents. Participating in the census was mandatory but answering these specific questions was voluntary; the response rate to them was around 80%. Some long-term impairments take time to discover (see Figures A1 and A2 on the prevalence rates by birth year in the Appendix), thus we restricted our analysis to people who were born between 1990 and 2008 (they were 3 to 20 years old in the census).

To analyze the incidence of impairment by birth weight, we linked the census records to the records in the national vital statistics using exact date of birth, gender, municipality of residence of the mother when the person was born, and the exact date of birth of the parents if they lived together with the person in 2011. We successfully linked around 75% of LBW and VLBW births from the vital statistics (see Table A.1 in the Appendix). The rate of successful linkages is slightly increasing in the year of birth, because the information on parents helps with linking the records, and older children (of the 3-20-year-old target population) are less likely to reside with their parents. We focus on two indicators of long-term impairment: any impairment and impairment due to issues at birth (presumably due to congenital disorder). The prevalence of the first (any impairment) is only slightly higher than the prevalence of the second: a little over 15% for individuals over age 3 born with birth weight <1500g, and around 5% if birth weight <2500g (Figures A1 and A2 in the Appendix).

Our third data source is a simple survey that we designed and implemented to uncover the history of opening of NICUs and connecting non-NICU hospitals to NETS across the country. The data was collected by the Hungarian Society of Perinatology and Obstetric Anesthesiology and the Hungarian Academy of Sciences. The directors of each Level 3 NICU operating in 2015 were asked to fill in a questionnaire, which asked for the date when their unit was established and a few questions on circumstances. To be more precise, they indicated the first calendar year in which their unit was operating year-long at its planned capacity. A similar data collection was carried out among NETS organizations. This survey collected data on the starting year of their service and their territorial coverage in their start year and in two to other points in time.

III. Trends and institutional background

Fertility decreased substantially in Hungary between 1990 and 1995 and remained relatively stable afterwards. In parallel with this trend, the number of LBW and VLBW births dropped substantially in the first half of the 1990s, followed by relative stability and small further decrease in the 2010s. Figure A3 in the Appendix shows the time series.

During the same time, mortality both among LBW and VLBW births declined steadily, at comparable rates. 0-6-day mortality among VLBW births decreased from around 400/1000 in 1990 to below 100/1000 after 2010; the corresponding figures for 0-364-day mortality are from 500/1000 to below 200/1000. For LBW births 0-6-day mortality decreased from 80/1000 to below 20/1000 while 0-364-day mortality decreased from 120/1000 to 20/1000. Figure A4 shows the time series.

The Hungarian health-care system has been characterized by single-payer health insurance and universal coverage since the 1960's, In Hungary, the majority of the individuals is insured, inpatient and outpatient services are financed through compulsory health insurance, and opting out from the system is forbidden. In 2013, Hungary spent 7.4% of its GDP on healthcare, of which nearly 70% was public expenditure (OECD 2015). The public expenditure part is financed through payroll taxes and transfers from the government budget.

There are no out-of-pocket payments at the points of service, except for drugs. At the same time informal gratuity payments are widespread. About 50% of respondents who used hospital care reported to have paid informal gratuity, and its prevalence was 85% for deliveries, according to a nationally representative survey (Baji et al. 2012). There is territorial supply obligation, where primary care is the responsibility of the municipalities, and county governments are responsible for specialist health care provision. According to the main rule, patients must receive health care at the lowest adequate level (Gaál et al. 2011; Bíró and Elek 2018). At the same time, patients have a choice of where to seek more advanced care, including where to give births.

Cutting the infant mortality rate (IMR) became a leading goal in health policy in the 1970s in Hungary, with focused attention on very low birth weight and preterm births (Gecser, Ifkó, and Kiszel 1977). As a response, Hungary established the first 10 NICUs in 1977 in some of the largest cities, with a gradual expansion of the system opening new NICUs and increasing the capacity of existing NICUs in the following decades. Since the introduction of the NICU system, Hungary underwent major political and economic changes, including the transition from a socialist regime to democracy and capitalism starting with 1989 and joining the European Union in 2004.

In parallel with the major social and economic changes the intensive care of high-risk newborn infants improved considerably as well. One of the major improvements was the introduction of Surfactant that facilitates oxygenation of very low birth weight infants with respiratory distress syndrome caused by developmental insufficiency of pulmonary surfactant production and structural immaturity in the lungs. Meanwhile, the first newborn emergency transportation system (NETS) organizations were established in 1990 to ensure safe transportation of infants to NICUs from hospitals without a NICU. By 2015, 21 NICUs were functioning in 15 cities. The NETS gradually expanded to reach full geographic coverage by 2005. Since 2005, nearly all infants at risk in the country have been born either in a city where either a NICU operated or in a municipality that was covered by NETS.

By 2015, the Hungarian NICU system became similar in its coverage to most rich countries. Conditional on the size of the country and the number of live births, including the number of LBW births and VLBW births, the number of units in the U.S. and Hungary are very similar (see table A2 in the Appendix). That is true not only relative to all live births but also to VLBW births at highest risk. Thus, analyzing the effects of expanding a NICU system to its current level in Hungary is informative of the expected effects of reaching the coverage in a range countries that include both Hungary and the U.S.

In order to inform current policy decisions, our analysis starts with 1990. It ends with 2015 for analyzing mortality and 2008 for analyzing long-term impairments due to data availability. By focusing on this time period, we can estimate effects for neonatal care with medical technology that is closer to what's available now; we can estimate the effects for a health system that is similar to many rich and middle-income countries; and we can jointly estimate the effects for NICUs and NETS.

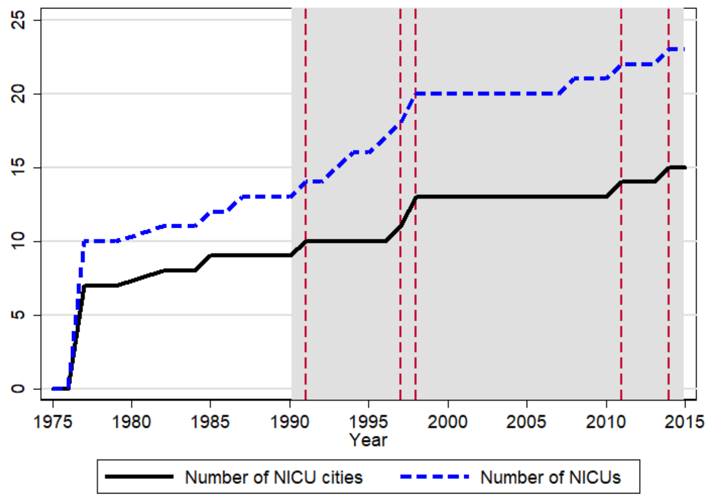

FIGURE 1. NUMBER OF HOSPITALS WITH A NICU AND NUMBER OF CITIES WITH A NICU HOSPITAL

Source: Author calculations, based on the authors' survey on NICU establishments.

Figure 1 shows the expansion of the NICU system from its beginnings in 1977 to 2015. The shaded grey area shows the time period of our analysis, 1990 through 2015. The solid line shows the number of cities with a NICU; the dashed line shows the number of NICUs themselves. The dashed vertical lines show the years when NICUs were established in new cities after 1990. Those changes are the source of identification for the effects of the NICUs.

Another way of describing the expansion of NICUs and NETS is considering the proportion of births in cities they cover. Figure 2 shows the gradual buildup of complete geographic coverage of low birth weight births (<2500g) and very low birth weight births (<1500g) by NICUs and NETS. The number of VLBW births in cities with NICU were 60% in 1990 and increased to over 90% by 2015. The corresponding figures for LBW births are 50% to 70%. The first emergency transport services started in 1990 by adding another 20 percentage points of coverage to both VLBW births and LBW births. Together, NICU and NETS reached full coverage by 2005 so that all births take place in cities with either a NICU hospital or a hospital connected to NETS.

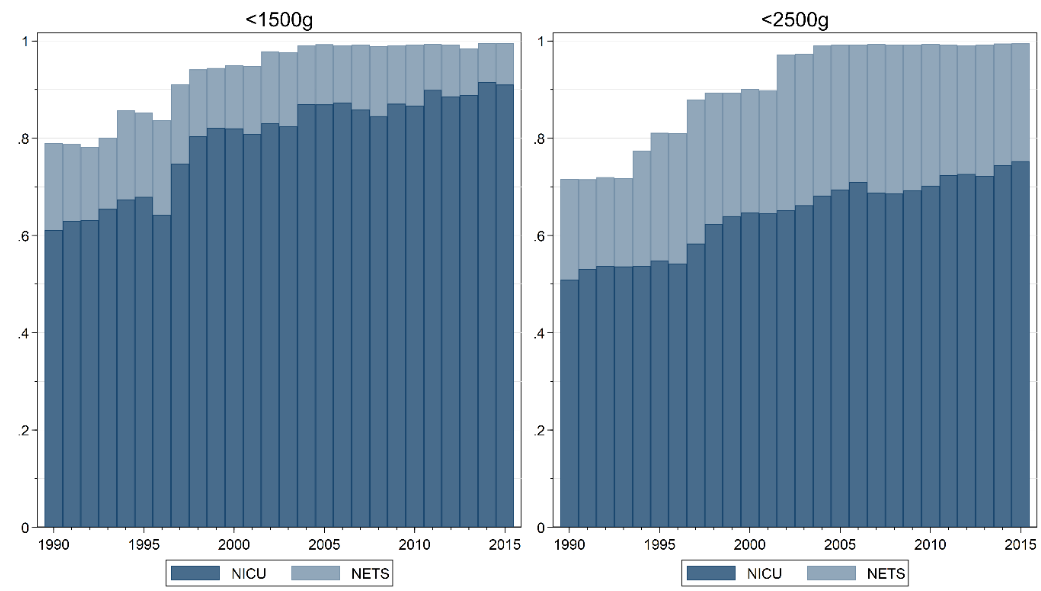

FIGURE 2. PROPORTION OF BIRTHS IN CITIES WITH A NICU AND MUNICIPALITIES WITHOUT NICU BUT COVERED BY NETS

Source: Author calculations. National vital statistics from Hungary, 1990-2015, linked to the authors' survey on NICU and NETS establishments

IV. Empirical strategy

The question of our study is the effect of the geographic expansion of the NICU and NETS systems on neonatal and infant mortality and long-term impairments. We operationalize this question by examining the effects of giving birth in a city with a NICU hospital, and giving birth in a city without a NICU hospital but connected to such a hospital by NETS.

Some cities with a NICU hospital have other hospitals that process deliveries. One way to understand the effect we estimate is as an average intent-to-treat effect where the treatment itself would be giving birth in a NICU hospital. However, we argue that the effect of giving birth in a city with a NICU is the more policy relevant question when investigating the consequences of the geographic expansion of the system. This effect includes the effect of choice of hospital of delivery if there are more hospitals in a city, which choice is part of how the system works. In any case, this is the quantity we can estimate with our data and our empirical strategy that makes use of distance between municipalities (more on that later).

Almost all cities with a hospital but without a NICU have a single hospital that performs deliveries. Thus, infants born in a city with a hospital connected to the NETS but without a NICU hospital, are born in that connected hospital. At the same time, in cities with multiple hospitals, NETS connects non-NICU hospitals to NICUs. By focusing on the effect of being born in a city connected by NETS but without a NICU, we can estimate the effect of NETS for transfers between cities but not within cities. As mortality risk is larger at longer distances, our NETS estimates are likely weaker than the effect that includes saving lives by transferring infants within a city.

In the remainder of this section, we outline our identification strategy in detail. We use the same strategy for estimating the effect of giving birth in a city with a NICU and the effect of giving birth in a city with NETS. For simplicity, we discuss our strategy with respect to cities with NICU here. Everything is analogous to our strategy of estimating the effects of NETS.

Our question is the effect of the geographic expansion of the system. A controlled experiment would chose the location of new NICUs randomly in previously underserved areas and would compare subsequent mortality to the not selected locations. Random assignment would ensure that the location of new NICUs would not depend on the level, or trends, of infant mortality. However, endogenous selection of births into NICU hospitals may occur even in this experiment. First, after the opening of a new NICU, riskier pregnancies could be transferred to them.

Second, from among pregnancies with similar risk, more informed mothers may be more likely to give birth in hospitals with NICU. Third, mothers might move into towns with newly established NICU hospitals. In principle, randomly assigning births to hospitals could circumvent these selection mechanisms.

Our empirical strategy simulates these two experiments at once. First, we address selection of the location of new NICU openings by a difference-in-differences strategy that exploits the variation in the timing of the establishment of new NICUs. Second, we use the distance of the mother's residence to the nearest NICU city as an instrumental variable to address selection of births into NICU hospitals. Within the difference-in-differences framework this instrumental variable is based on the longitudinal variation in that distance. This instrumental variable strategy circumvents the effect of NICU availability on the selection of births into hospitals, as well as cities with such hospitals, as long as mothers at higher risk don't move closer to NICUs. We find no evidence for this: Figure A8 in the Appendix shows the time series of the proportion of potential mothers moving into each of the cities that had a NICU established during our time period. The figures show no evidence of more potential mothers moving into those cities after establishing a NICU.

Using individual birth-level data, we specify the following regression for the effect of giving birth in a city with a NICU/NETS hospital:

$$Y_{ijt} = \beta \cdot BNICU_{ijt} + \gamma \cdot BNETS_{ijt} + \delta' \cdot X_{ijt} + \eta_j + \theta_t + u_{ijt} \quad (1)$$

Index i denotes the newborn child, j is municipality of residence of the mother, and t is the year of birth. Y is the outcome variable: whether died within 6 days, whether died within 364 days, whether developed an impairment by the time we observe them in the census (age 3 to 20). All outcomes are binary; our regressions are linear probability models.

BNICU is a binary variable denoting whether the infant was born in a city with a NICU hospital, and BNETS is a binary variable denoting whether the infant was born in a city with a non-NICU hospital that is connected to the NETS. Note that BNICU and BNEST are disjoint alternatives by definition. The η and θ are municipality of residence and birth year fixed effects. There are around 3000 municipalities of residence in the data; each village, town and city is a municipality. Vector X includes individual covariates, such as gender, parity, month of birth, whether the mother is married, twin birth, highest level of education of the mother and father, labor market status of the mother and father, age of mother and father in 5-year categories, and indicators for previous abortions and miscarriages of the mother.

The coefficients of interest are β and γ. β aims at measuring the effect of giving birth in a city with a NICU hospital. γ aims at measuring the effect of giving birth in a municipality that has no NICU hospital but is connected to a NICU hospital via NETS.

To address selection into NICU hospitals or hospitals connected to NETS, and thus into cities with such hospitals, we instrument BNICU and BNETS with the distance of the mothers' residence to each. The first-stage regressions are the following:

$$BNICU_{ijt} = \pi_1 \cdot DNICU_{ijt} + \phi_1 \cdot DNETS_{ijt} + \delta_1 \cdot X_{ijt} + \eta_{1j} + \theta_{1t} + u_{1ijt} \qquad (2)$$

$$BNETS_{ijt} = \pi_2 \cdot DNICU_{ijt} + \phi_2 \cdot DNETS_{ijt} + \delta_2 \cdot X_{ijt} + \eta_{2j} + \theta_{2t} + u_{2ijt} \qquad (3)$$

We use subscripts to denote parameters in the two first-stage equations. As in the main regression, η and θ are municipality of residence and birth year fixed effects, and vector X includes individual covariates. The instruments are DNICU and DNETS, these are variables indicating the distance between the mother's municipality of residence to the nearest municipality with a NICU and a NETS hospital, respectively. Parameters π show the effect of the distance of mothers' residence to a NICU hospital on giving birth in a municipality with a NICU or NETS hospital. Similarly, parameters φ show the effect of the distance of the mothers' residence to the nearest municipality with a NETS-connected hospital on giving birth in a municipality with a NICU or NETS hospital. As we shall see, our instruments are quite strong.

To assess the identifying assumptions behind our strategy let's consider the reduced form where we use subscript R, for reduced form, to distinguish parameters from the previous equations:

$$Y_{ijt} = \pi_R \cdot DNICU_{ijt} + \phi_R \cdot DNETS_{ijt} + \delta_R' \cdot X_{ijt} + \eta_{Rj} + \psi_{Rt} + \omega_{Rijt} \quad (4)$$

In this reduced form regression π_R show the effect of the distance of mothers' residence from the nearest NICU city on the outcome variable while parameter ϕ_R shows the effect of the distance from the nearest non-NICU NETS city.

Due to the presence of residence fixed effects, this is a generalized difference-in-differences setup. The source of identification is changes in the distance to NICU and NETS cities due to the opening of new NICUs and expanding the coverage of NETS. Recall Figures A5, A6, and A7 in the Appendix that show aggregate trends in the number of municipalities in discrete bins of distance, to illustrate the source of variation in our distance variable.

The reduced form effects, and thus the instrumental variables estimates of the effects, are identified if the parallel trends assumption holds. This assumption stipulates that, without the expansion of NICU or NETS, the trends in the outcomes would have been the same in municipalities that saw their distance change because of a new NICU or NETS hospital as it was in municipalities that didn't experience such a change. This assumption is untestable as it compares actual trends to counterfactual trends, but examining pre-treatment trends can be informative. However, defining and examining pre-treatment trends in a direct way is not straightforward in our setup with a gradual expansion of NICUs and NETS. Thus, we'll examine them among the robustness checks of our estimates by including lead terms of the treatment variables.

Finally, recall that our strategy estimates the effect of giving birth in a city with a NICU and the effect of giving birth in a city without a NICU but connected to NETS. While we argue that these effects are more interesting from a policy point of view, they are, at the same time, likely to be close to the corresponding effects of giving birth in a NICU hospital. The overwhelming majority of risky births in cities with a NICU hospital took place in the NICU hospitals themselves (Over 90% of 0-1500g births and over 60% of 1500-2499g births were treated in NICUs in 2012 (Valek and Szabó 2014); the corresponding figure fir 0-1500g births a few years earlier was 85% (Páll, Valek, and Szabó 2011)). Similarly, the overwhelming majority of newborn emergency transportation took place between cities as opposed to within cities (around 80% of transportations of infants with birth weight less than 2500g in 2012 (Valek and Szabó 2014)). In line with these considerations, when we restrict our analysis to cities with single hospitals we get estimates that are similar to our main results (see the robustness checks later).

V. Main results

Our main results are estimates of regressions (1) to (3) on three subsamples: births with very low birth weight (<1500g), births with low but not very low birth weight (1500g ≤ weight < 2500g), and births with low weight (<2500g). We consider two outcomes in this section: mortality within 0 to 6 days after birth (early neonatal mortality) and mortality within 0 to 364 days after birth (infant mortality). The descriptive statistics of the variables are summarized in Table A3 in the Appendix.

Table 2 shows the second stage (IV) results. The tables show the point estimates of the most important variables, with clustered standard errors. They also include the F-statistics on the excluded instruments from the first-stage regressions. The corresponding first-stage and reduced-form results are included in the Appendix, Table A4 and A5.

TABLE 1—THE EFFECT OF BEING BORN IN A CITY WITH A NICU OR A NETS ON MORTALITY. 2SLS ESTIMATES

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a NICU city	-0.153 (0.038)	-0.010 (0.003)	-0.024 (0.007)	-0.144 (0.042)	-0.021 (0.005)	-0.031 (0.009)
Born in a NETS city	-0.057 (0.040)	-0.009 (0.002)	-0.009 (0.005)	-0.020 (0.043)	-0.011 (0.004)	-0.008 (0.006)
Municipality of residence FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
IV F-stat NICU	78.4	57.3	63.7	78.4	57.3	63.7
IV F-stat NETS	106.5	235.2	231.3	106.5	235.2	231.3
Number of municipalities	2029	2929	2964	2029	2929	2964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Notes: Robust standard errors with municipality clustering are in parentheses. The individual covariates include the infant's gender, parity, twin birth, indicators for previous abortions and miscarriages of the mother indicators for whether the mother is married, and the highest level of education, labor market status, and age of the mother and father(in 5-year categories).

Source: Author calculations. National vital statistics from Hungary, 1990-2015, linked to the authors' survey on NICU and NETS establishments.

According to the point estimates, giving birth in a city with NICU decreased 0-6-day mortality by 153/1000 live births among infants with birth weight <1500g (95% CI [77,229]), by 10/1000 live births among infants with a birth weight between 1500g and 2499g (95% CI [4,16]), and by 24/1000 live births among infants with <2500g (95% CI [10,38]). These are large effects. We can compare them to the corresponding mortality rates at the beginning of the time period, 350/1000, 20/1000, and 66/1000, respectively.

The estimated effects on 0-6-day mortality of being born in a city without a NICU but connected to a NICU hospital by NETS are 57/1000 live births for infants with birth weight <1500g (not statistically significant), 9/1000 between 1500g and 2499g, and 9/1000 for <2500g. These effects are substantially weaker than giving birth in a city with a NICU itself. This result is consistent with the high risks of transporting newborn babies and the more time that it takes to rescue newborn infants from distant hospitals.

The effect estimates on 0-364-day mortality are very similar to the estimates on 0-6-day mortality. These results are important. They imply that the large majority of lives saved in NICUs and by NETS are saved for a long term.

The first stage results are strong, and they are consistent with the causal interpretation of the instrument. Recall that we have two first stage regressions, one for being born in a city with a NICU hospital, and one for being born in a city without a NICU hospital but connected to NETS, and both regressions include both of our instruments. The results show that decreasing distance to a NICU city makes giving birth in a NICU city substantially more likely, and it makes giving birth in a non-NICU but NETS city somewhat less likely. At the same time, decreasing distance to non-NICU but NETS city doesn't change the likelihood of giving birth in a NICU city, or it makes it marginally less likely, while it makes giving birth in a non-NICU but NETS city more likely. The reduced-form estimates are in line with the two stages of the 2SLS, and they have similar t-statistics (coefficient estimates over standard errors). These results strengthen the credibility of our main estimates.

After estimating the effects of NICU/NETS on mortality we turn to its potential effects on long-term impairment. Recall that that most impairments manifest by age 3 but not earlier, therefore our focus on impairments reported for children age 3 or above (Figures A1 and A2 show the age-impairment profiles). The impairment data is from the census of 2011; the response rate in the census were 80%, its records were linked to birth records with a 75% success rate on average. The age restriction leads to focusing on a shorter time period, 1990 through 2008. These factors combined lead to substantially smaller numbers of observations than what we could use for the mortality estimates.

There are two reasons to expect an effect, with opposing signs. First, lives saved by NICU/NETS are from very risky pregnancies and births that may be more likely to result in severe impairments of the children. Thus, the system may save lives but increase the number of individuals with long-term impairments. Second, the high-quality medical interventions in NICUs may directly reduce the risk of developing such impairments even of those that were not at the margin of infant mortality. Our estimates show the net effect of the two. Table 2 shows the results, in the same structure as Table 1 above. The corresponding summary statistics, first-stage and reduced-form results are in tables A6-A8 in the Appendix.

The point estimates are all very close to zero, and none of them are significant at conventional levels. Being born in a NICU city is estimated to increase the incidence of long-term impairment by 20/1000 for birth weight less than 1500g, by 0/1000 for birth weight between 1500 and 2499, and by 4/1000 for birth weight less than 2500. These should be compared to the point estimates of 144/1000, 21/1000, and 31/1000 lives saved by being born in a NICU (the 0-364-day mortality results in Table 1; note that child mortality is low after age 1 so most lives saved to age 1 are saved for a longer time). The estimated effects of NETS are of similar magnitude. While our confidence intervals are wide, it is remarkable that all point estimates are very close to zero. Thus, we think that the evidence here suggests that the effects are most likely close to zero indeed. Recall that these effects are the combination of negative selection (risky lives saved) and a direct effect of treatment on the likelihood of developing impairments. These two effects appear to add up to zero.

TABLE 2—THE EFFECT OF BEING BORN IN A CITY WITH A NICU OR A NETS ON THE PROBABILITY OF LONG-TERM IMPAIRMENT. 2SLS ESTIMATES

	Any impairment			Impairment due to issues at birth		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a NICU city	0.023	0.000	0.004	-0.001	0.008	0.010
	(0.048)	(0.009)	(0.009)	(0.050)	(0.007)	(0.007)
Born in a NETS city	-0.023	-0.004	-0.007	-0.011	0.000	-0.003
	(0.066)	(0.006)	(0.007)	(0.067)	(0.005)	(0.006)
Municipality of residence FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
IV F-stat NICU	50.38	42.70	47.54	50.39	42.29	47.09
IV F-stat NETS	40.13	230.5	225.2	39.07	230.6	225.2
Number of municipalities	1173	2719	2763	1168	2719	2762
Number of observations	9,992	94,106	104,758	9,891	93,726	104,273

Notes: Robust standard errors with municipality clustering are in parentheses. Individual covariates: see notes to Table 1.

Source: Author calculations. National vital statistics of Hungary, 1990-2015, linked to the 2011 Census of Hungary and the authors' survey on NICU and NETS establishments.

VI. Additional Results and Robustness Checks

For comparison, Tables A9 and A10 (Appendix) show the results of the non-instrumented ("OLS") estimates of Eq. 1. They do include the municipality and year fixed-effects and thus estimate the effects from longitudinal variation in giving birth in NICU or NETS cities, but they do not address the endogenous change of the composition of births due to the new NICU hospitals and NETS connections. Recall that we expect selection to be strong for new NICU hospitals but not necessarily new NETS connections, and the direction of that selection is ambiguous in principle: riskier births are likely directed to new NICU hospitals, but conditional on risk, better informed mothers choose the new NICU hospital. We expect the first effect to dominate. Comparing the OLS and 2SLS results is in line with that expectation, especially for non-VLBW births. The coefficient estimates for mortality are less negative or even positive, and the coefficient estimates for impairment stay zero or become positive. These results support the need for our instrumental variables strategy, and they are also consistent with how our instrumental variables strategy should reduce the bias.

Our instruments are the distance of the mother's residence to the nearest city with NICU or NETS. In the baseline specification of Eq. 2 and 3 we entered the distance measures linearly. Although this is the simplest functional form nothing guaranties that it's the right one. Thus we re-estimated our models using different functional forms, including a quartic specification and one with 10-km bins. Tables A11 and A12 show the results for mortality.

To address potential non-parallel trends we re-estimated our models including municipality-specific time trends. Note that we estimated linear probability models while the trends in mortality are more likely convex because mortality rate is bounded from below (Figure A4 in the Appendix shows the national trends). Thus, including linear trends is an imperfect solution to capture pre-trends. In particular, linear trends would predict weaker decline in the earlier time periods and a stronger decline in the later time periods, leading to an upward bias in the effect estimates (making them less negative). Table A13 shows the results for mortality; they are qualitatively similar although somewhat weaker. Given that we expect weaker results by construction, these results provide strong support to the causal interpretation of the main results.

To examine pre-trends more directly, we re-estimated our models with lead terms. These pre-trends are best examined in the reduced-form results, which include the leads of the distance of the mother's residence to NICU and NETS cities. Table A14 shows the results of a specification with the contemporaneous term, the first lead, the second and third leads combined, and the fourth and fifth leads combined. These are lead terms in an FE model showing average differences in mortality from before to after the time period indicated, in successively additive ways. The results should be compared to the positive reduced-form effects we presented in table A5 that show after – before differences corresponding to the assigned start years of NICUs and increasing coverage of NETS. The NICU results show that the significant change in mortality occurs one year prior to the start year, but the coefficients on the further leads do not show pre-trends. Recall that the NICU start date denotes the first full year of the unit; unit itself, or most elements of it, were likely in place the year before already. The NETS results show a more spread out change in the years before. Here the effects are estimated from the timing of increased coverage, which is even less well captured by our data, which only captures snapshots in several years. Taken together, these results are consistent with noise in measuring the precise timing of the expansion. Most importantly, especially in the case of the expansion of NICUs, they do not indicate strong pre-trends.

We also addressed the fact that our estimates show the effect of giving birth in a city with a hospital with a NICU or in the NETS and not of giving birth in a NICU or NETS hospital. The two kinds of effects are not the same because some of the largest cities have multiple hospitals with only some of them having a NICU, and because in such cities neonatal transportation may take place within the city. We argued that the effects we estimate are more policy-relevant, and they are analogous to an intent-to-treat effect. At the same time we also argued earlier that the estimates are likely close to what the effects of giving birth in a NICU or NETS hospital would be, especially among VLBW infants. To provide further evidence for the latter we re-estimated our main model for only cities with a single hospital by excluding from the data all births to mothers who lived in or within 50-km of cities with multiple hospitals. The samples are smaller by more than two thirds, and they are a selected sample, exclude the larger cities including Budapest, the capital. The results, in table A15 the Appendix, are very similar to the main results.

Finally, we estimated our models for preterm births, instead of birth weight groups, in two categories: 0-31 weeks of estimated gestation week and 32-26 weeks. Results to be included.

VII. Conclusions

Thus study estimated the effect of improved access to neonatal intensive care due to the geographic expansion of the system into previously underserved areas. In particular, it estimated the effect of giving birth in a city with a neonatal intensive care unit (NICU), and in a city connected to a NICU hospital by a neonatal transportation system (NETS), on early neonatal mortality (0-6 days) and infant mortality (0-364 days) as well as long-term impairment of the children that survived. We made use of the gradual geographic expansion of this system in Hungary, a middle income country where geographic distance is an important determinant of access to public services, between 1990 and 2015. Our empirical strategy was difference-in-differences identified from longitudinal variation in geographic coverage. We used the distance of the mother's residence to the city of the hospital as an instrument in this diff-in-diffs setup, which helped overcome selection into giving births in hospitals. Our results showed that being born in a city with NICU has a substantial effect on early neonatal mortality, and the effects are very similar for overall infant mortality. Being born in a city without a NICU hospital but connected to such a hospital by NETS also reduces mortality, but its effects are substantially weaker. Our estimates on the effects on long-term impairment are all very close to zero. These are the first results in the literature that estimate the effect of the geographic expansion of a NICU system on 0-6 day mortality, longer-term mortality and long-term limitations in the same framework, jointly with the effects of NETS. The effects are identified using a transparent and credible empirical strategy that assesses multiple kinds of selection, and our estimates are robust to a number of potential issues that may arise with our strategy and our data.

Several conclusions emerge from our results. First, our effect estimates suggest a substantial benefit to geographic expansion of access even though the newly established units may be of lower efficiency and quality due to less experience and, typically, lower number of cases treated. Second, the results suggest that the effects on neonatal mortality are long-term effects: lives saved in the first week tend to be saved for the remainder of the first year, too. That's a remarkable result as it suggests that most lives are saved for a very long time as mortality after the first year is very low. Third, our results suggest that the system helps avoiding with long-term impairments, too. It either helps infants to survive without substantially increasing their risk of developing long-term impairments, or, to the extent that some of them do develop such impairments it balances that by reducing the risk for other infants. Fourth, the estimated effects of the transport system (NETS) are positive, too, in reducing mortality, but they are substantially weaker than the effects of NICUs. Given the substantial risks of transporting newborns in critical condition these results are not surprising. They highlight that giving birth in a hospital with a NICU offers substantially better chances for survival for newborns at risk. Yet our results show that the NETS saves lives, too.

Our estimates can help assessing the benefits of expanding a NICU/NETS system to previously underserved regions using current medical technology in middle income countries where geographic distance matters for access. Giving birth in a city with a NICU hospital is expected to save around 140 of 1000 very low birth weight infants and around 20 of 1000 infants between 1500 and 2500 g of birth weight in the long run. Giving birth in hospitals without a NICU but connected to a NICU by neonatal transportation is expected to save around 20, and possibly zero, of 1000 very low birth weight infants and around 10 of 1000 infants between 1500 and 2500 g of birth weight. There appear to be no long-run impacts on impairment. The high costs of the expansion and subsequent maintenance of the NICU/NETS system should be weighed against these benefits.

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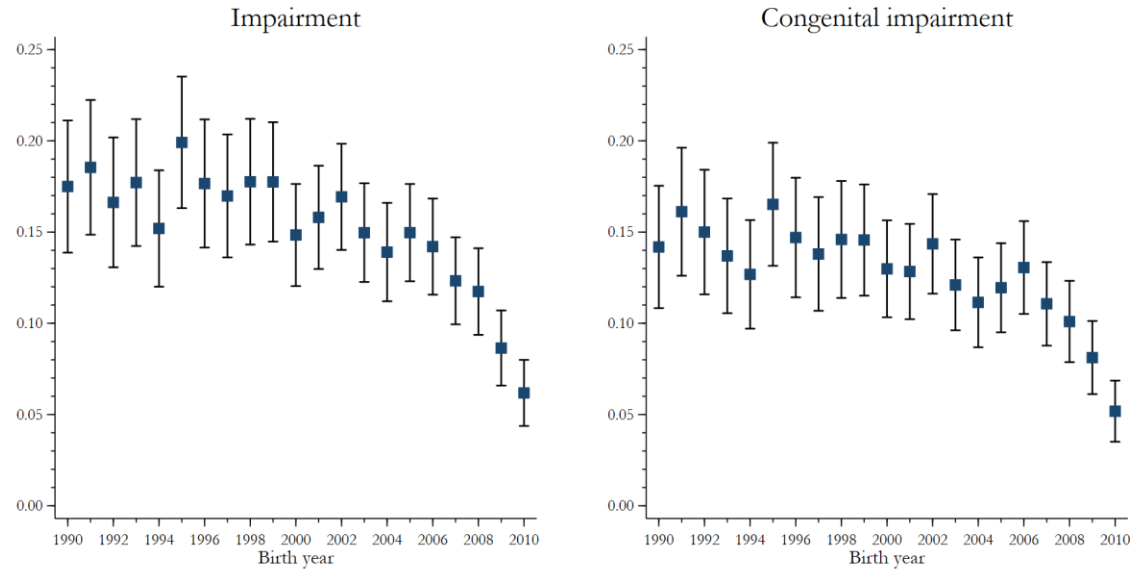
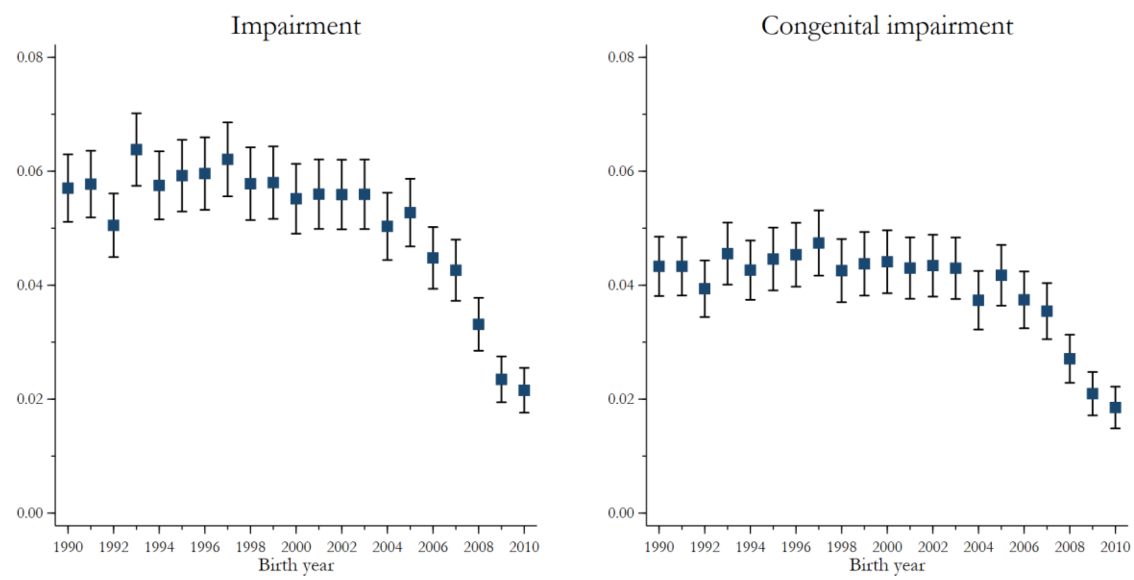
Figure A1 Impairment ratio by birth year in the Hungarian Census 2011, <1500g



Notes: Point estimates and their 95% CIs. Non-respondents (ca. 15-20%) are excluded.

Figure A2 Impairment ratio by age in the Hungarian Census 2011, <2500g



Notes: Point estimates and their 95% CIs. Non-respondents (ca. 15-20%) are excluded.

Figure A3 Number of all births, LBW births (<2500g), and VLBW births (<1500g).

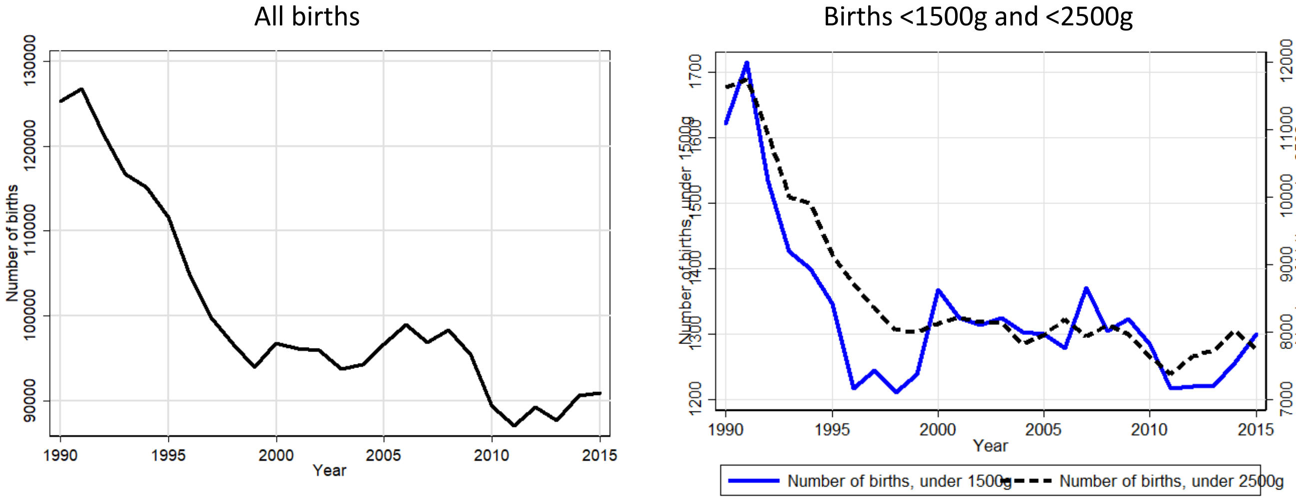


Figure A4 Mortality among LBW births (<2500g) and VLBW births (<1500g): within 0-6 days and within 0-364 days

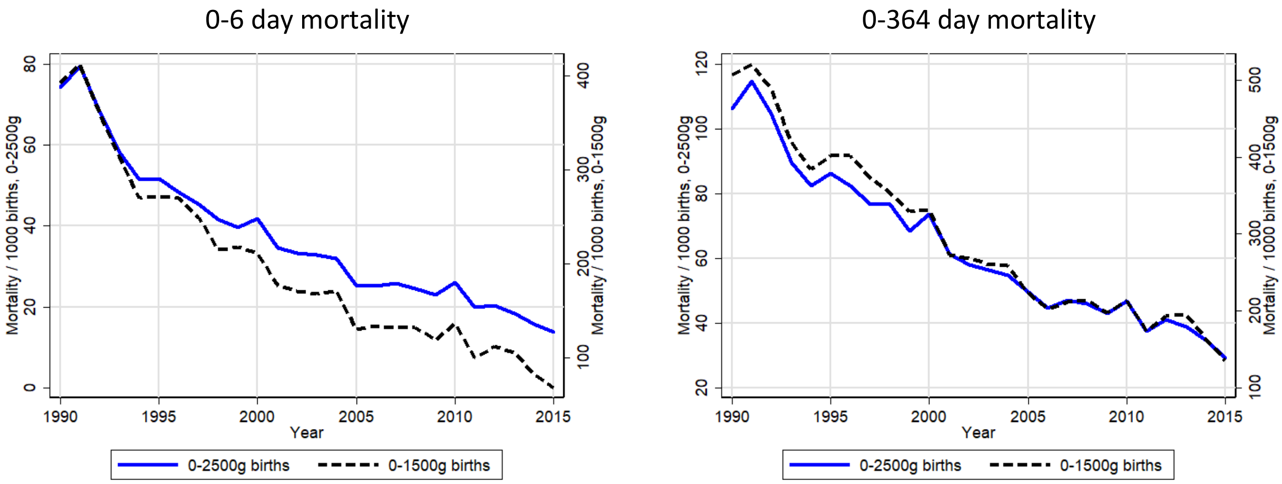


Figure A5 Snapshots of the geographic distribution of NICUs and hospitals connected to NICUs via NETS

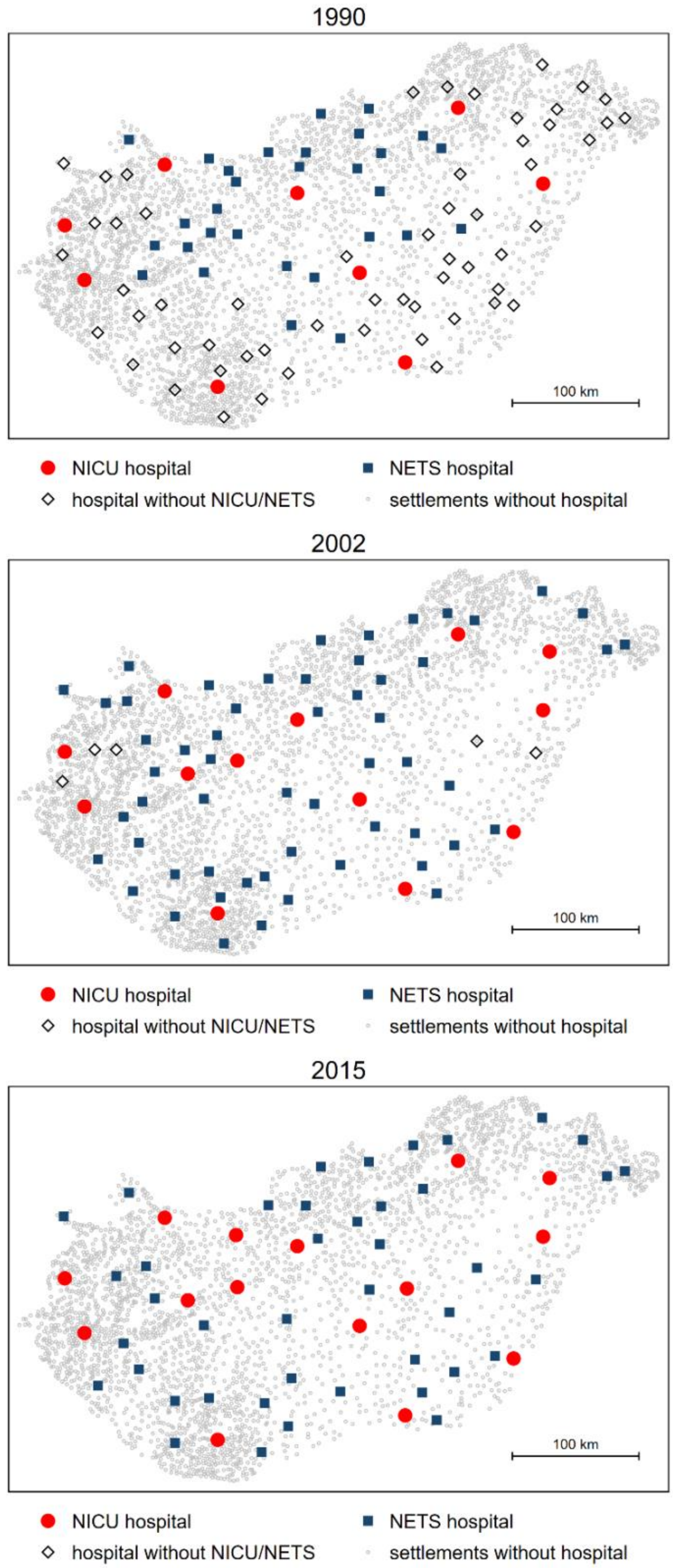


Figure A6 Time series of the number of municipalities by distance to the nearest NICU city. (Number of NICU cities: left scale; Number of municipalities in distance categories: right scale)

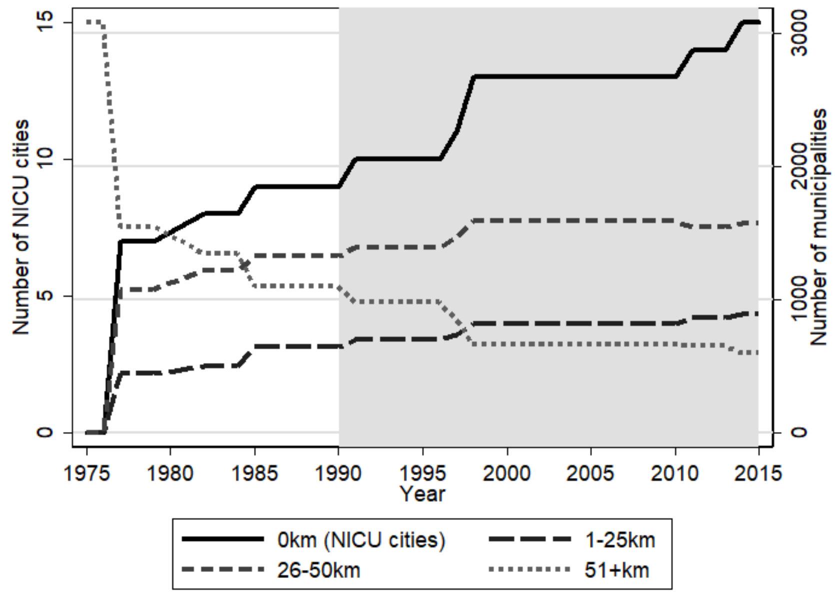


Figure A7 Time series of the number of municipalities by distance to the nearest NICU or NETS city. (Number of NICU/NETS cities: left scale; Number of municipalities in distance categories: right scale)

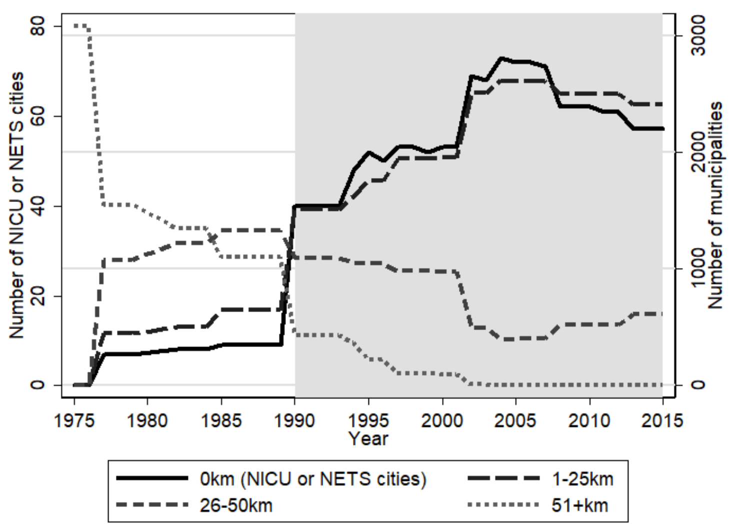


Figure A8. Proportion of women (age 20-34) moved into cities with a newly established NICU as the percentage of all change of residential location (women, age 20-34)

Notes: Vertical lines indicate the first full year of the newly established NICU.

Table A.1 Rate of successful linkages

Linked data	Successful links, %	Notes
Live births to Infant mortality (cohorts of 1990-2015)		
<1500 g	99.8	as the % of infant deaths
1500g-2499 g	99.8	as the % of infant deaths
Live births to Census of 2011 (cohorts of 1990-2008)		
<1500 g	74.7	as the % of newborns are live at age 1
1500g-2499 g	79.9	as the % of newborns are live at age 1

Table A2. The coverage of NICUs in the United States (2008) and in Hungary (2015)

	Number of NICUs	Number of NICUs per 1000 live births		
		All births	LBW infants	VLBW infants
United States, 2008	850	0.2	2.5	13.7
Hungary, 2015	21	0.2	2.7	16.4

Source:

Table A3. Descriptive statistics

	<1500 g			1500-2499 g			<2500 g		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Mortality: 0-6 days	34,683	0,203	0,402	188,697	0,009	0,097	223,380	0,040	0,195
Mortality: 0-364 days	34,683	0,302	0,459	188,697	0,023	0,150	223,380	0,066	0,249
Impairment	10,618	0,157	0,364	94,297	0,043	0,202	104915	0,054	0,226
Congenital impairment	10,514	0,132	0,338	93,915	0,031	0,175	104429	0,042	0,200
Born in NICU city (mortality sample)	34,683	0,791	0,406	188,697	0,605	0,489	223,380	0,634	0,482
Born in NETS city (mortality sample)	34,683	0,137	0,344	188,697	0,282	0,450	223,380	0,260	0,439
Distance from the closest NICU city (in 10km) (mortality sample)	34,683	2,788	2,354	188,697	2,936	2,373	223,380	2,913	2,371
Distance from the closest NETS city (in 10km) (mortality sample)	34,683	2,515	2,209	188,697	2,645	2,349	223,380	2,624	2,328
Born in NICU city (impairment sample)	10,618	0,806	0,395	94,297	0,575	0,494	104915	0,599	0,490
Born in NETS city (impairment sample)	10,618	0,131	0,337	94,297	0,288	0,453	104915	0,272	0,445
Distance from the closest NICU city (in 10km) (impairment sample)	10,618	2,829	2,320	94,297	3,070	2,395	104915	3,046	2,388
Distance from the closest NETS city (in 10km) (impairment sample)	10,618	2,530	2,242	94297	2,763	2,501	104915	2,740	2,477
Twin birth	34,683	0,252	0,434	188,697	0,187	0,390	223,380	0,197	0,398
Boy	34,683	0,504	0,500	188,697	0,459	0,498	223,380	0,466	0,499
Married mother	34,683	0,593	0,491	188,697	0,592	0,491	223,380	0,593	0,491

Mother's education: less than primary	34,683	0,076	0,265	188,697	0,098	0,298	223,380	0,095	0,293
Mother's education: primary	34,683	0,326	0,469	188,697	0,349	0,477	223,380	0,346	0,476
Mother's education: vocational	34,683	0,188	0,391	188,697	0,185	0,388	223,380	0,185	0,389
Mother's education: secondary	34,683	0,251	0,434	188,697	0,228	0,419	223,380	0,231	0,422
Mother's education: college/university	34,683	0,143	0,351	188,697	0,133	0,340	223,380	0,135	0,342
Mother's education: missing	34,683	0,015	0,121	188,697	0,006	0,079	223,380	0,008	0,087
Father's education: less than primary	34,683	0,022	0,146	188,697	0,030	0,169	223,380	0,028	0,166
Father's education: primary	34,683	0,167	0,373	188,697	0,202	0,401	223,380	0,196	0,397
Father's education: vocational	34,683	0,241	0,428	188,697	0,264	0,441	223,380	0,260	0,439
Father's education: secondary	34,683	0,169	0,375	188,697	0,162	0,368	223,380	0,163	0,369
Father's education: college/university	34,683	0,110	0,313	188,697	0,107	0,309	223,380	0,108	0,310
Father's education: missing	34,683	0,292	0,455	188,697	0,236	0,425	223,380	0,245	0,430
Mother's labor force status: active	34,683	0,570	0,495	188,697	0,537	0,499	223,380	0,542	0,498
Mother's labor force status: maternity leave	34,683	0,120	0,325	188,697	0,134	0,340	223,380	0,131	0,338
Mother's labor force status: unemployed	34,683	0,077	0,267	188,697	0,075	0,263	223,380	0,075	0,264
Mother's labor force status: other	34,683	0,213	0,409	188,697	0,243	0,429	223,380	0,238	0,426
Mother's labor force status: missing	34,683	0,020	0,140	188,697	0,012	0,108	223,380	0,013	0,113
Father's labor force status: active	34,683	0,591	0,492	188,697	0,620	0,485	223,380	0,616	0,486
Father's labor force status: unemployed	34,683	0,063	0,243	188,697	0,078	0,268	223,380	0,075	0,264
Father's labor force status: other	34,683	0,049	0,215	188,697	0,060	0,238	223,380	0,058	0,234
Father's labor force status: missing	34,683	0,298	0,457	188,697	0,242	0,428	223,380	0,251	0,433
Mother's age: x-19	34,683	0,086	0,281	188,697	0,122	0,327	223,380	0,116	0,320
Mother's age: 20-24	34,683	0,195	0,396	188,697	0,242	0,428	223,380	0,234	0,424
Mother's age: 25-29	34,683	0,266	0,442	188,697	0,265	0,441	223,380	0,265	0,441
Mother's age: 30-34	34,683	0,255	0,436	188,697	0,223	0,416	223,380	0,228	0,420
Mother's age: 35-39	34,683	0,156	0,363	188,697	0,120	0,326	223,380	0,126	0,332
Mother's age: 40-x	34,683	0,041	0,197	188,697	0,028	0,166	223,380	0,030	0,172
Father's age: x-19	34,683	0,009	0,093	188,697	0,016	0,124	223,380	0,015	0,120
Father's age: 20-24	34,683	0,077	0,267	188,697	0,111	0,314	223,380	0,106	0,308
Father's age: 25-29	34,683	0,174	0,379	188,697	0,203	0,402	223,380	0,198	0,399
Father's age: 30-34	34,683	0,207	0,405	188,697	0,209	0,406	223,380	0,209	0,406
Father's age: 35-39	34,683	0,150	0,357	188,697	0,140	0,347	223,380	0,142	0,349
Father's age: 40-x	34,683	0,106	0,308	188,697	0,092	0,289	223,380	0,094	0,292
Father's age: missing	34,683	0,278	0,448	188,697	0,230	0,421	223,380	0,237	0,425

N of previous live births: 0	34,683	0,376	0,484	188,697	0,406	0,491	223,380	0,402	0,490
N of previous live births: 1	34,683	0,278	0,448	188,697	0,273	0,445	223,380	0,274	0,446
N of previous live births: 2	34,683	0,163	0,369	188,697	0,156	0,363	223,380	0,157	0,364
N of previous live births: 3	34,683	0,085	0,279	188,697	0,078	0,268	223,380	0,079	0,269
N of previous live births: 4+	34,683	0,098	0,298	188,697	0,087	0,282	223,380	0,089	0,284
N of abortions: 0	34,683	0,763	0,425	188,697	0,811	0,391	223,380	0,804	0,397
N of abortions: 1	34,683	0,142	0,349	188,697	0,127	0,333	223,380	0,129	0,335
N of abortions: 2	34,683	0,057	0,232	188,697	0,040	0,196	223,380	0,043	0,202
N of abortions: 3+	34,683	0,038	0,191	188,697	0,022	0,147	223,380	0,025	0,155
N of abortions: missing	34,683	0,000	0,000	188,697	0,000	0,000	223,380	0,000	0,000
N of miscarriages: 0	34,683	0,761	0,427	188,697	0,821	0,383	223,380	0,812	0,391
N of miscarriages: 1	34,683	0,150	0,357	188,697	0,122	0,327	223,380	0,126	0,332
N of miscarriages: 2	34,683	0,056	0,229	188,697	0,038	0,191	223,380	0,041	0,197
N of miscarriages: 3+	34,683	0,034	0,181	188,697	0,019	0,137	223,380	0,021	0,145
N of miscarriages: missing	34,683	0,000	0,005	188,697	0,000	0,000	223,380	0,000	0,002

Table A4: First-stage results of the 2SLS regressions for the effect of being born in a city with a NICU or a NETS on mortality

	<1500g		1500-2499g		<2500g	
	BNICU	BNETS	BNICU	BNETS	BNICU	BNETS
Distance to NICU (10km)	-0.117***	0.058***	-0.119***	0.068***	-0.119***	0.067***
	(0.009)	(0.008)	(0.011)	(0.008)	(0.011)	(0.008)
Distance to NETS (10km)	-0.006	-0.045***	0.007***	-0.080***	0.006**	-0.075***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	2029	2029	2929	2929	2964	2964
Number of observations	34,213	34,213	188,611	188,611	223,319	223,319

Table A5: Reduced-form estimates of the 2SLS regressions for the effect of being born in a city with a NICU or a NETS on mortality

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Distance to NICU (10km)	0.015***	0.001	0.002***	0.016***	0.002***	0.003***
	(0.004)	(0.000)	(0.001)	(0.004)	(0.001)	(0.001)
Distance to NETS (10km)	0.003*	0.001***	0.001*	0.002	0.001**	0.000
	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	2029	2929	2964	2029	2929	2964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Table A7: First-stage results for the 2SLS regressions for the effect of being born in a city with a NICU or a NETS on impairment

	<1500g		1500-2499g		<2500g	
	BNICU	BNETS	BNICU	BNETS	BNICU	BNETS
Distance to NICU (10km)	-0.115***	0.046***	-0.111***	0.058***	-0.112***	0.058***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.012)	(0.008)
Distance to NETS (10km)	-0.009*	-0.037***	0.003	-0.079***	0.002	-0.075***
	(0.005)	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	1173	1173	2719	2719	2763	2763
Number of observations	9,992	9,992	94,106	94,106	104,758	104,758

Table A8: Reduced-form results for the 2SLS regressions for the effect of being born in a city with a NICU or a NETS on impairment

	Impairment: any			Impairment: due to issues at birth		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Distance to NICU (10km)	-0.004	-0.000	-0.001	-0.000	-0.001	-0.001
	(0.006)	(0.001)	(0.001)	(0.006)	(0.001)	(0.001)
Distance to NETS (10km)	0.001	0.000	0.001	0.000	-0.000	0.000
	(0.003)	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	1173	2719	2763	1168	2719	2762
Number of observations	9,992	94,106	104,758	9,891	93,726	104,273

Table A9: OLS (non-instrumented FE) regression results for the effect of being born in a city with a NICU or a NETS on mortality

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a city with NICU	-0.143***	0.002	0.009***	-0.117***	0.005**	0.026***
	(0.012)	(0.001)	(0.003)	(0.013)	(0.002)	(0.003)
Born in a city with NETS	-0.030**	-0.004***	-0.010***	-0.011	-0.006***	-0.013***
	(0.013)	(0.001)	(0.002)	(0.014)	(0.002)	(0.003)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	2029	2929	2964	2029	2929	2964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Table A10: OLS (non-instrumented FE) regression results for the effect of being born in a city with a NICU or a NETS on impairment

	Impairment: any			Impairment: due to issues at birth		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a city with NICU	-0.005	0.008***	0.020***	-0.021	0.009***	0.019***
	(0.021)	(0.003)	(0.003)	(0.019)	(0.003)	(0.003)
Born in a city with NETS	0.014	-0.004	-0.007**	0.002	-0.001	-0.005*
	(0.024)	(0.003)	(0.003)	(0.022)	(0.002)	(0.003)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	1173	2719	2763	1168	2719	2762
Number of observations	9,992	94,106	104,758	9,891	93,726	104,273

Table A11: 2SLS estimates for the effect of being born in a city with a NICU or a NETS on mortality. Distance quartic

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a city with NICU	-0.144***	-0.010***	-0.022***	-0.136***	-0.019***	-0.027***
	(0.036)	(0.003)	(0.006)	(0.041)	(0.004)	(0.007)
Born in a city with NETS	-0.060	-0.008***	-0.010**	-0.031	-0.009***	-0.008*
	(0.038)	(0.002)	(0.004)	(0.040)	(0.003)	(0.005)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
IV F-stat NICU	89.55	224.1	247.6	89.55	224.1	247.6
IV F-stat NETS	64.31	272.9	270.4	64.31	272.9	270.4
Number of municipalities	2029	2929	2964	2029	2929	2964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Table A13: 2SLS estimates for the effect of being born in a city with a NICU or a NETS on mortality. Municipality of residence linear trends included

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a city with NICU	-0.121**	-0.003	-0.015*	-0.158**	-0.006	-0.021**
	(0.054)	(0.004)	(0.008)	(0.063)	(0.006)	(0.010)
Born in a city with NETS	-0.015	-0.007**	-0.010	0.011	-0.005	-0.007
	(0.066)	(0.003)	(0.007)	(0.075)	(0.005)	(0.009)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Municipality of resid. trend	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
IV F-stat NICU	76.42	74.53	81.35	76.42	74.53	81.35
IV F-stat NETS	65.17	230.6	221	65.17	230.6	221
Number of municipalities	2029	2929	2964	2029	2929	2964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Table A14: Reduced-form estimates for the effect of the distance of the mother's residence to the closest city with a NICU or a NETS on mortality.
Lead terms included to test pre-trends

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Distance to NICU (10km)						
contemporaneous	0.004	-0.000	-0.000	0.008	-0.002***	-0.001
	(0.006)	(0.000)	(0.001)	(0.006)	(0.001)	(0.001)
lead 1	0.018**	0.001	0.003**	0.015*	0.005***	0.006***
	(0.008)	(0.001)	(0.001)	(0.009)	(0.001)	(0.002)
leads 2-3	0.001	0.000	0.002	0.000	-0.000	0.002
	(0.006)	(0.001)	(0.001)	(0.008)	(0.001)	(0.002)
leads 4-5	-0.005	0.001	-0.002	-0.005	0.000	-0.003
	(0.008)	(0.001)	(0.001)	(0.008)	(0.001)	(0.002)
Distance to NETS (10km)						
contemporaneous	0.003	0.000	0.000	-0.000	-0.001	-0.001
	(0.004)	(0.000)	(0.001)	(0.004)	(0.001)	(0.001)
lead 1	-0.005	0.000	-0.001	-0.004	0.001	-0.000
	(0.005)	(0.000)	(0.001)	(0.005)	(0.001)	(0.001)
leads 2-3	0.005	0.001	0.001	0.005	0.001	0.001
	(0.005)	(0.001)	(0.001)	(0.005)	(0.001)	(0.001)
leads 4-5	0.004	-0.000	0.001	0.007	0.000	0.001
	(0.005)	(0.000)	(0.001)	(0.005)	(0.001)	(0.001)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of municipalities	2029	2929	2964	2029	2929	2964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Table A15: 2SLS estimates for the effect of being born in a city with a NICU or a NETS on mortality. Only cities with single hospitals (sample with mother's residence within 50km to such cities).

	Mortality 0-6 days			Mortality 0-364 days		
	<1500g	1500-2499g	<2500g	<1500g	1500-2499g	<2500g
Born in a city with NICU	-0.177***	-0.010***	-0.026***	-0.150***	-0.021***	-0.031***
	(0.041)	(0.003)	(0.007)	(0.049)	(0.005)	(0.009)
Born in a city with NETS	-0.074**	-0.005**	-0.011**	-0.014	-0.009***	-0.009*
	(0.036)	(0.002)	(0.005)	(0.037)	(0.003)	(0.005)
Municipality of resid. FE	Y	Y	Y	Y	Y	Y
Municipality of resid. trend	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y
Birth month FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
IV F-stat NICU	79.02	51.63	56.22	79.02	51.63	56.22
IV F-stat NETS	103.3	353.8	325.8	103.3	353.8	325.8
Number of municipalities	1327	2496	2530	1327	2496	2530
Number of observations	13,012	99,665	113,210	13,012	99,665	113,210