THE EFFECTS OF NEONATAL INTENSIVE CARE ON INFANT MORTALITY AND LONG-TERM HEALTH IMPAIRMENTS

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ABSTRACT
We study the effects of being born in a city with a neonatal intensive care unit (NICU) and in a city integrated into the newborn emergency transportation system (NETS) on neonatal and infant mortality and long-term impairments. We use administrative and census data covering the gradual expansion of the NICU and NETS systems between 1990 and 2015, and we identify the effects using the distance of residence to the nearest NICU/NETS hospital as an instrumental variable. Residence fixed effects control for all unobserved municipality-specific time-invariant determinants of newborns' death and impairment. Being born in a city with a NICU decreases 0- to 6-day mortality by 153/1,000 (birth weight less than 1,500 grams) and 24/1,000 (birth weight less than 2,500 grams). NETS effects are positive, too, but they are substantially smaller (57/1,000 and 9/1,000, respectively). The effect estimates on long-term impairment are small in magnitude and are all statistically indistinguishable from zero. Access to NICUs and the NETS saves lives in the long run, without substantial overall effects on long-term impairments.

KEYWORDS: neonatal intensive care, newborn transportation, instrumental variable, fixed effects, infant mortality, impairment
JEL CLASSIFICATION: H51, I1

I. Introduction

The death of a child is a tragedy that should be prevented if resources allow for it. Thus, reducing the infant mortality rate is an important policy goal, even if its level is already low. Large reductions in high-level infant mortality are possible by promoting relatively

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inexpensive practices, such as free antenatal care or the use of antibiotics or aseptic techniques (Martines et al. 2005). However, some infant mortality remains challenging to prevent after such measures are exhausted. In particular, reducing early neonatal mortality (death within 6 days of birth) may require highly specialized intensive care for very risky births. Neonatal intensive care units (NICUs) provide such care (Barfield et al. 2012; Valek and Szabó 2018). An important argument for NICUs is that these interventions are highly cost-effective especially compared with medical interventions at older ages (Cutler and Meara 2000; Profit et al. 2010). However, the marginal cost could be too high once a lower level of infant mortality is reached.

NICUs are specialized units located next to obstetrics units in the same hospitals that care for newborn babies with a high risk of mortality right after they are born. Newborns at high risk include the majority of very low birth weight (VLBW) children (less than 1,500 grams) and many of the substantially larger pool of children with birth weights between 1,500 grams and 2,499 grams (the two groups together are called low birth weight, or LBW, children). As in most of the literature, in this study, we focus on level 3 neonatal intensive care units and call them simply NICUs. Level 2 NICUs are considered to be non-NICU hospitals as they do not provide treatment for extremely low birthweight newborns and those born earlier than the 34th gestation week.[1]

NICU systems are typically built up gradually, starting with lower capacity and limited geographic coverage. They are often complemented with a newborn emergency transportation system (NETS), which provides specialized transport for newborn babies at obstetrics units of other hospitals to NICUs. There are many hospitals without a NICU, and in case of a problematic birth, a specially equipped ambulance car transports the newborn baby to the nearest NICU hospital. These transports often take place across cities. The network of the hospitals without a NICU where such special transportation is available is the NETS. Such neonatal transport systems are available in the United States (Diehl 2018; Perry 2021), United Kingdom (Fenton and Leslie 2012), and many other (mostly developed) countries (Roy et al. 1999; Millán García del Real et al. 2021; de la Mata et al. 2017).

Both NICUs and NETS are expensive to establish, operate, maintain, and expand (Behrman 2007, 403–15; Hallsworth et al. 2008; Phibbs et al. 2019; Russell et al. 2007; Watson, Arulampalam, and Petrou 2017). Therefore, it is important to learn how effective they are in saving lives, not only in the short run but also in 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

1 Level 2 NICU units in Hungary operate next to obstetrics units with more than 1,000 deliveries per year. They provide primary care for premature babies with at least 34 completed weeks of gestation. The main duty of these level 2 units is to stabilize and provide care for neonates born 34 to 36 gestation weeks with no respiratory, cardiovascular, or surgical complications. But they can provide temporary ventilation until transfer to a level 3 NICU. On the other hand, level 3 NICU units play a regional role in neonatal care and provide care to those born earlier than the 34th gestation week, or with very low birth weight (less than 1,500 grams). These level 3 units provide sustained life support, perform special diagnostic procedures, and provide access to a wide range of pediatric medical subspecialists (Health Ministry 2003).

survive anyway or by increasing such risks by saving infants at the margin of survival who would later develop such conditions.

This paper aims to explore the differences in mortality in the first year of life and impairment outcomes between births in NICU hospitals (or in NETS-connected hospitals) and births in hospitals without a NICU (or without NETS connection). As a preliminary analysis, we provide an event study–like graphical illustration of the change in early neonatal and infant mortality due to the NICU openings. In addition, we show the results of a simple difference-in-differences estimation where aggregated data at the level of catchment areas of cities with an obstetrics department are used. In both cases, we find that the NICUs lead to a decrease in mortality of the risky newborns. NETS also reduces mortality, but its effects are substantially weaker. However, these empirical designs are not free from, among others, the bias of endogenous selection of births into NICU/NETS cities and hospitals.

Therefore, in our main approach, we apply an instrumental variable (IV) strategy that handles the endogenous selection of births into a NICU/NETS hospital and provides causal estimates. We utilize the mother's residence distance to the nearest city with a NICU hospital and the closest city with a NETS-connected hospital as instruments. It is a strong instrument because distance is a prominent determinant of access in the context of our analysis (Elek, Váradi, and Varga 2015). However, as the residential distribution of mothers is not random, several factors that influence the mortality and impairment outcomes of the newborns might be related to the distance to the nearest NICU/NETS city.

Consequently, the validity of such an IV analysis might be questioned when using simple cross-sectional data where geographic differences cannot be adequately controlled for. To handle the concern regarding the exogeneity of our instrument, we use a large data set covering the years between 1990 and 2015. In this period, a geographic expansion of the neonatal intensive care system took place in Hungary: new NICUs were established, and the territorial coverage of the NETS was increased. These changes created variation in the distances to the nearest NICU and NETS city measured at the municipality level. It means that we can include in our empirical design not only a rich set of individual covariates but also residence fixed effects. The latter deal with the issue that various characteristics of the geographic areas are correlated with the distance to the nearest NICU/NETS hospital and, at the same time, might be related to the mortality and impairment outcomes of the newborns. In other words, by applying a fixed-effects approach, we can control for all unobserved municipality-specific time-invariant determinants of newborns' death and impairment. As a result, our IV strategy is free from the influence of many potential confounders that can compromise the validity of cross-sectional estimations.

We estimate the effect of being born in NICU hospitals and being born in hospitals integrated into the NETS that connects non-NICU hospitals to NICU hospitals in a single equation. To be more precise, instead of the effects of giving birth in such hospitals, we estimate the impact of giving birth in a city with a NICU hospital or a NETS-connected hospital. 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.

We consider three outcomes: early neonatal mortality (within 0–6 days of delivery), infant mortality (within 0–364 days), and significant impairment that is diagnosed any time during childhood. We focus on newborns with a high risk of mortality. Specifically, we estimate the impacts for newborns with birth weight of less than 1,500 grams, 1,500–2,499 grams, and less than 2,500 grams.

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 from hospitals without a NICU to hospitals with a NICU. We collected information on the expansion of the NICU and NETS systems by a survey with the management of relevant organizations.[2] To estimate the effects on early neonatal and infant mortality, we use individual-level administrative data on all births and infant mortality events in Hungary from 1990 through 2015. To estimate the effects on long-run impairment, we use birth registry data linked to data from the national census of 2011, which includes questions on impairments. While we have data for earlier time periods, we focus on the effects after 1990, 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 being born in NICU cities on early neonatal mortality (0–6 days), and we find very similar estimates on total infant mortality (0–364 days). The magnitudes are larger (153/1,000) for newborns with very low birth weight (less than 1,500 grams), but they are also significant for the much larger group of newborns with birth weight of 1,500–2,499 grams (10/1,000). When comparing with baseline mortality rates, the effect estimates are similar in magnitude in these two groups. We estimate smaller but non-negligible effects of being born in cities integrated into the NETS (57/1,000 and 9/1,000). Compared with the NICU effect, the NETS seems to be especially important for newborns with birth weight of 1,500–2,499 grams. Finally, our estimates of the effects on impairments are all very close to zero and statistically not significant. These results provide strong evidence that the NICU/NETS system led to a substantial decrease in early neonatal mortality of those newborns who have prompt access to a NICU hospital. Importantly, most of the lives that have been saved are lives saved for the long run. In addition, we can rule out that the NICU/NETS system has significantly increased long-term impairment on average. The reason is that either the children on the margin of mortality do not develop such impairment or, if they do, it is compensated by a reduced impairment rate of the inframarginal newborns by the NICU/NETS system.

To our knowledge, all papers on the effects of being born in a hospital with a NICU on early neonatal mortality rely on cross-sectional comparisons (Lasswell et al. 2010; J. H. Chung et al. 2010; Lorch et al. 2012; Jensen and Lorch 2015; Mújica-Mota et al. 2020). However, identifying the effect of NICUs is difficult because of various selection mechanisms, which make cross-sectional studies vulnerable to bias even if they condition on many covariates or use an IV 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). We do not know of any study that has estimated the effects in an IV framework for newborns

2 The response rate of the survey questions regarding the establishment dates of the NICU and NETS entities was 100 percent. To avoid biases from remembering the dates wrongly, we verified the answers using exogenous sources of information.

who gained prompt access to neonatal intensive care owing to an expansion of the NICU system or the neonatal transportation system from non-NICU hospitals to NICU hospitals.

The available evidence is also incomplete in terms of the outcome variables. Typical analyses focus on early neonatal mortality within 0–6 days of delivery or in-hospital mortality. 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 and neonatal mortality in a unified empirical framework.

II. Literature

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

A meta-analysis of earlier studies finds strong associations of giving birth in hospitals with NICUs and mortality, but all these papers rely on observational cross-sectional data (Lasswell et al. 2010). Similarly, strong effects are found by most of the later articles based on observational cross-sectional data (J. H. Chung et al. 2010; Lorch et al. 2012; Jensen and Lorch 2015; Freedman 2010; Mújica-Mota et al. 2020). 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, controlling for a rich set of individual characteristics.

Shah et al. (2020) find that neonatal mortality is significantly lower for infants born in a level 3 hospital than for those born in a non–level 3 hospital. They do not find a significant negative effect for antenatal transfer to level 3 hospitals. Grytten et al. (2017) analyze 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 newborns. 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.[3]

Lorch et al. (2012) and Mújica-Mota et al. (2020) use distance to facility as an instrument in cross-sectional analyses of various levels of neonatal care on mortality. Mújica-Mota et al. (2020) examine the United Kingdom and find small effects; Lorch et al. (2012) examine

3 The four treatments considered by Grytten et al. (2017) are ventilators, antenatal steroids, surfactant, and insure. Of these, antenatal steroids are not a NICU treatment, and of course there are a lot more treatments (e.g., phototherapy, therapeutic hypothermia), personnel (a wide range of pediatric medical subspecialists), and sets of equipment (e.g., laryngoscope, pulse oximeter) available in a NICU that are not covered by this list.

several US states and find effects that vary substantially across states. Watson et al. (2017) use short panel data of NICUs and longitudinal variation in the cost of care at the nearest NICU hospital as an instrument to estimate the effect of higher costs of intensive care on mortality; their source of variation is not changes in distance but changes in costs. They find that increased spending significantly decreases mortality. Almond et al. (2010) apply a regression discontinuity framework on US data to estimate the effect of access to more specialized care on infant mortality. Bharadwaj, Løken, and Neilson (2013) use a similar approach to assess the effects on school outcomes in Chile and Norway. The regression discontinuity approach makes use of discontinuity in access to additional treatment at birth weight of 1,500 grams. This additional treatment includes, among other things, more likely referral to a NICU in Chile and Norway but not in the United States, and it includes additional treatments in non-NICU hospitals in all three countries. Daysal et al. (2022) also utilizes the VLBW cutoff to estimate the effect of early-life medical treatments using data from Denmark. All of these three studies find strong effects on all outcomes, but these effect estimates include the effects of many other treatments besides the effect of treatment in NICUs.

Several papers address the risks of the transportation of newborns to intensive care units. Most of this part of the literature finds that transportation comes with undoubted benefits and 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; M.-Y. Chung et al. 2009; Hohlagschwandtner et al. 2001; Kaneko et al. 2015; Kollée et al. 1992; Lamont et al. 1983; Marlow et al. 2014; Mori et al. 2007; Russell et al. 2007; 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 focuses on the health risks related to preterm births, including visual impairments, hearing problems, learning disabilities, and many more (Behrman 2007; Blencowe et al. 2013; Lindström, Lindblad, and Hjern 2011; Lindström et al. 2007; McCormick 1989; McCormick and Litt 2017; Wilson-Costello 2007). Daysal et al. (2022) investigate the effect of early-life medical treatments on childhood disabilities. We add to this part of the literature by estimating 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 within the NICU-related literature that utilize the change in distance for identification. Our choice of IV is influenced by Garabedian et al. (2014), who emphasize that the simple cross-sectional spatial distribution of patients is likely correlated with health outcomes independently of the potential effects of access to health services.

Our analysis contributes to the existing NICU literature in at least three ways. First, to our knowledge, this is the first study to directly measure the effect of providing better access to the NICU system for the marginal newborns as opposed to the effect of delivery in individual hospitals or the effect of specific interventions. Second, it is the first to estimate the effect of the NETS compared with no access to a NICU at all. Third, using a unique linked database, it estimates one year run mortality and long-run (up to 21 years of age) impairment effects to quantify the impact on saving at-risk newborns past the first few days of delivery and its potential trade-offs.

III. 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. Birth and infant mortality data are from the national vital statistics of all births and any subsequent deaths up to 364 days. Birth and mortality data are linked at the individual level. 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 less than 1,500 grams and low birth weight (LBW) for less than 2,500 grams. We present results for the two birth weight groups as well as the nonoverlapping group of 1,500–2,499 grams. The administrative database covers cohorts born in 1990–2015 and includes 2,610,468 live birth events and 22,136 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 preterm (less than 37 weeks) or very preterm (less than 32 weeks). Our main results are for birth weight categories, as those are more precisely measured; we show among the robustness checks that the results are similar for preterm categories. These indicators are ex post to delivery; our data have no ex ante risk indicators. For reasons similar to ours, much of the related literature has focused on low birth weight infants.

Long-term impairment data come from the 2011 census, which covered the entire population of Hungary. Among other things, the census contains self-reported information on long-term impairment and its various types.[4] 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 approximately 80 percent. Some long-term impairments take time to discover (see Online Appendix Figures A1 and A2 on the prevalence rates by birth year); 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 approximately 75 percent of LBW and VLBW births from the vital statistics (see Table A1 in the Online 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- to 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 present at birth (congenital disorder). The prevalence of the first (any impairment) is only slightly higher than the prevalence of the second: a little over 15 percent for individuals over age 3 born

4 The respondents were asked in the questionnaire whether they have an impairment (disabled, autistic, mentally disabled, visually impaired, deaf-mute, etc.) and since when they have had the impairment. In our analysis, we define congenital impairment as having an impairment since birth.

with birth weight less than 1,500 grams, and approximately 5 percent if birth weight was less than 2,500 grams (see Figures A1 and A2 in the Online Appendix). Birth and infant mortality records and census data are administered by the Hungarian Central Statistical Office (HCSO). We accessed and linked the data sets in the secure data environment of the HCSO.

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 were collected by the Institute of Economics, Centre for Economic and Regional Studies, of the Hungarian Academy of Sciences. The directors of each level 3 NICU operating in 2015 were asked to complete 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.[5] 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 other points in time.

IV. Trends and Institutional Background

Mortality both among LBW and VLBW births has declined steadily in Hungary since the 1990s, at comparable rates. The 0- to 6-day mortality among VLBW births decreased from approximately 350/1,000 in the first five years of the 1990s to below 100/1,000 after 2010; the corresponding figures for 0- to 364-day mortality decreased from 460/1,000 to below 200/1,000. For LBW births the 0- to 6-day mortality decreased from 65/1,000 to below 20/1,000, while the 0- to 364-day mortality decreased from 100/1,000 to below 40/1,000. Figure A3 in the Online Appendix shows the time series.

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, the available therapies of high-risk pregnancies and newborn infants improved considerably (e.g., antenatal steroids, surfactant, and ventilators). Meanwhile, the first 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 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 United States and Hungary is very similar (see Table A2 in the Online Appendix), relative not only to all live births but also to VLBW births at highest risk.

5 Finally, each director answered our questions. We double checked and validated the establishment dates from external information sources, such as contemporary journal articles about the establishment.

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

Figure 1 shows the expansion of the NICU system from its beginnings in 1977 to 2015. The shaded gray 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 (less than 2,500 grams) births and very low birth weight (less than 1,500 grams) births by NICUs and NETS.

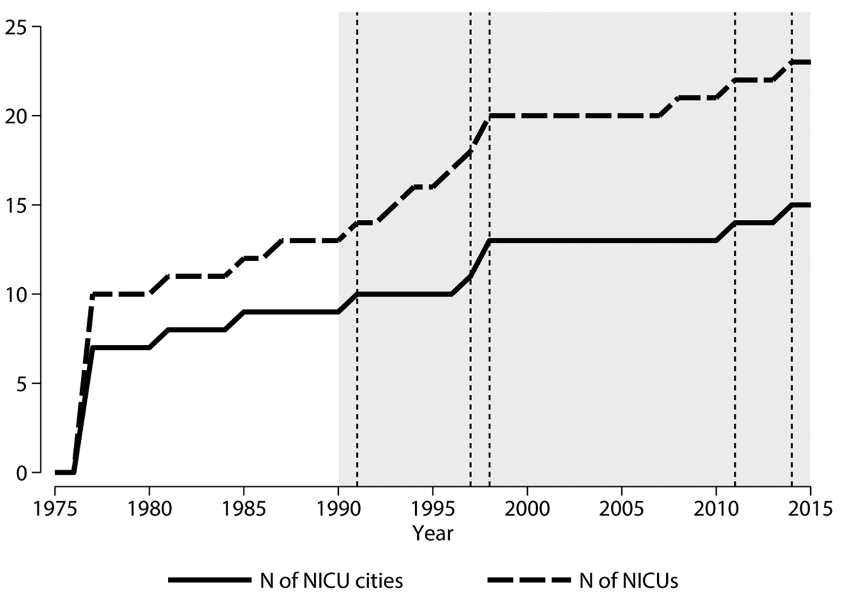

FIGURE 1. Number of hospitals with a NICU and number of cities with a NICU hospital. The solid line shows the number of cities with a NICU; the dashed line shows the number of NICUs themselves. The dashed vertical lines show the years when NICUs were established in new cities after 1990. The shaded gray area shows the time period of our analysis, 1990 through 2015. Source: Author calculations, based on the authors' survey on NICU establishments.

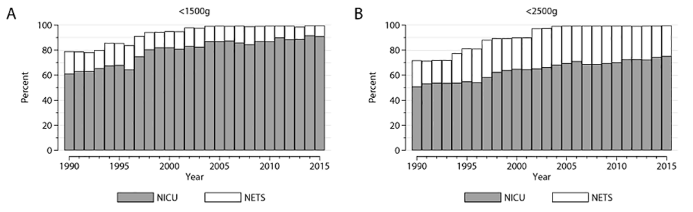

FIGURE 2. Proportions of births in cities with a NICU and municipalities without a NICU but covered by NETS. Source: Author calculations. National vital statistics from Hungary, 1990–2015, linked to the authors' survey on NICU and NETS establishments.

V. Method

Our study seeks to evaluate 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. In an IV analysis, we use as instruments distances of the mother's residence to (1) the nearest city with a NICU hospital and (2) the closest city with a NETS-connected hospital. To circumvent the issue that the newborns' health outcomes might be correlated with the residence, we utilize the geographic expansion of the NICU and NETS systems, by which we are able to use residence fixed effects. These fixed effects control for any municipality-specific time-invariant characteristics. 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 NICUs here. Everything is analogous to our strategy of estimating the effects of NETS.

Our question is the effect of being born in a city with a NICU hospital. A controlled experiment would choose the location of births randomly. Some of these births would take place in NICU hospitals, while others would take place in non-NICU hospitals. However, such an experiment is not feasible. In reality, the location of NICUs is not random and endogenous selection of births into NICU hospitals occurs regularly. Riskier pregnancies often do not take place in local hospitals but are transferred to NICU hospitals. In addition, among pregnancies with similar risks, more-informed mothers may be more likely to give birth in hospitals with NICUs. Finally, mothers might move into towns with NICU hospitals.

In our empirical strategy, we apply an IV approach with fixed effects to estimate the causal effect of being born in a city with a NICU hospital. We use the distance of the mother's residence to the nearest NICU city as an IV to address the selection of births into NICU hospitals. In addition, we control for all unobserved municipality-specific time-invariant determinants of newborns' death and impairment. We also control for year-specific changes in outcomes that are common across municipalities. Within the fixed-effects framework, our instrumental variable is based on the longitudinal variation in that municipality-specific distance.[6] This IV 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 do not move closer to NICUs. We find no evidence for this. Figure A4 in the Online Appendix shows the time series of the proportion of potential mothers moving into each of the cities with a NICU established during our time period. The figures show no evidence of more potential mothers moving into those cities after establishing a NICU. The total number of births and LBW births of women who live in a city with a newly established NICU is also unaffected (Figures A5 and A6, Online Appendix). In addition, the NICU openings do not influence the share of college graduates among women giving birth and living in a given city (Figure A7, Online Appendix). These results suggest that NICU-induced migration and change in incentives to give birth are unlikely to exist.

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 the municipality of residence of the mother, and t is the year of birth. Y is the outcome variable: whether the newborn died within 6 days or within 364 days, and whether the child developed an impairment by the time we observed 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 *BNETS* are disjoint alternatives by definition. The η and θ terms are municipality of residence and birth year fixed effects. There are approximately 3,000 municipalities of residence in the data; each village, town, and city is a municipality. Vector X includes individual covariates, such as gender, parity, the month of birth, mother's marital status, twin birth, the highest level of education of the mother and father, labor market status of the mother and father, age of mother and father in five-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 estimating the impact 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 mother's residence to each. The first-stage regressions are the following:

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

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

6 Note that this fixed-effect approach does not control for the changes of unobserved confounders within locations. Our identification is based on the assumption that even if there are some, these changes are uncorrelated with the timing of the establishing of a NICU/NETS.

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 variables indicate the distances between the mother's municipality of residence and the nearest municipality with a NICU and a NETS hospital, respectively. The π parameters show the effect of the distance of the mother's residence to a NICU hospital on giving birth in a municipality with a NICU or NETS hospital. Similarly, the ϕ parameters show the effect of the distance of the mother's 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 strong.

We note that in our IV approach, β and γ are the effects of giving birth in a city with a NICU/NETS hospital among compliers. They are those women who give birth in a municipality with a NICU or NETS city if they live close to that municipality but give birth in another city if they live farther away.[7] The effects are likely to be different for the population of noncompliers. Always-takers are more likely to need NICU treatment as they are referred (or choose) to give birth in a NICU/NETS city even if they live far away. Consequently, the effects are likely to be stronger among them. In this sense, our estimations are lower bounds of the impact of being born in a NICU/NETS city for an average newborn with high mortality risk. However, from a policy point of view, our estimates of β and γ seem to be more important as they capture the impact of the inclusion of the marginal newborns. They could benefit from prompt access to a NICU department.

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

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

In this reduced-form regression, π_R shows the effect of the distance of the mother's 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.

Residence fixed effects control for time-invariant differences across municipalities. The source of identification is the variation in the municipality-specific distance to NICU and NETS cities due to the opening of new NICUs and expanding the coverage of NETS.

7 To give an insight into the size and characteristics of the complier group, we dichotomized the main instrument (distance to the nearest municipality with a NICU) and created two groups: (1) living in a NICU city ($DNICU = 0$) and (2) living in other municipalities ($DNICU > 0$). Based on Angrist and Pischke (2009), the proportion of compliers among infants born in NICU cities is around 10 percent and 20 percent for the VLBW and LBW children, respectively. Because the number of never-takers is extremely small, the probability of compliance is almost 1 among infants born in non-NICU cities. We also examined some characteristics of the compliers. We found that complier mothers are more likely to be younger and less educated, but the marriage rate in the complier group is not different form the average of the VLBW/LBW sample. These results are not surprising; the always-takers are pregnancies detected to be high risk at an early stage and thus are directed to a NICU hospital irrespective of the distance of residence to the nearest NICU hospital, whereas the compliers are pregnancies that are not detected to be high risk in advance. Always-taker mothers are more likely to be older (higher risk of preterm birth) and highly educated (more informed and cautious).

Figures A8, A9, and A10 in the Online Appendix show territorial coverage of NICU/NETS cities at different years and aggregate trends in the number of municipalities in discrete bins of distance to illustrate the source of variation in our distance variable. In addition, Figure 3 explicitly shows changes in NICU and NETS status of cities that had an obstetrics department in 1990 between the beginning and the end of our sample period.

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. One can argue that the effect of giving birth in a hospital with a NICU might be the more policy-relevant question, as even within-city transfers can increase the mortality rate of premature newborns (Páll, Valek, and Szabó 2011). Accordingly, an effective policy should aim to provide prompt access to neonatal intensive care, which could only be achieved if the risky delivery occurs in a hospital with a NICU. Unfortunately, it cannot be estimated with our data. As most cities in Hungary have a single hospital, they are likely to be close to the corresponding effects of giving birth in a NICU hospital. Indeed, 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).

This measurement error likely causes a downward bias in our estimates. 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

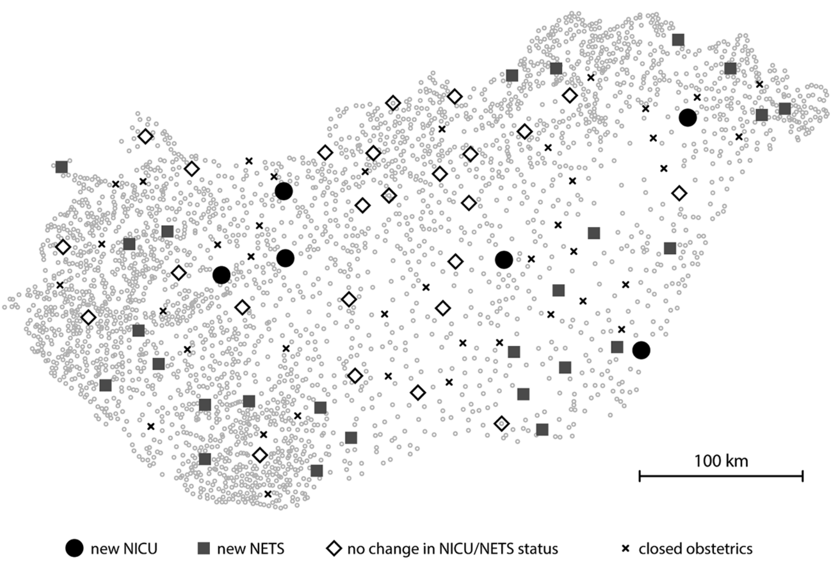

FIGURE 3. Changes in NICU and NETS status of cities that had an obstetrics department in 1990. Changes between 1990 and 2015. Small gray dots are municipalities without an obstetrics department in 1990.

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. Similarly, the estimated effect of being born in a city with a NICU incorporates infants born in a non-NICU hospital and transferred to the NICU hospital; thus, we underestimate the positive impact of a NICU.

VI. Impacts on Mortality

A. PRELIMINARY RESULTS

Before showing the results of the IV models, we provide an event study–like graphical illustration of the change in infant mortality due to the NICU openings. Specifically, we display time trends for two groups of births: (1) births in cities with a newly established NICU and (2) births in other cities. Because the new NICUs were established at different times, we scale the x-axis (time) relative to the year in which the NICU was opened. For births in other cities, the year 1998 is set to be zero, as half of the new NICUs were established in 1997 or 1998. In Figure 4, we show time trends in early neonatal mortality and infant mortality for birth weights less than 1,500 grams, but similar graphs can be found in the Online Appendix for birth weights of 1,500–2,499 grams and less than 2,500 grams (Figures A11 and A12).

As Figure 4 shows, there is a considerable jump in both early neonatal and infant mortality for births in cities with a newly established NICU. In contrast, the time trend is relatively uninterrupted for deliveries in other cities. The change in early neonatal mortality trend in cities with a new NICU is −8.6 percentage points. The corresponding change in infant mortality trend is −7 percentage points. The sharp differences between the trends suggests that new NICUs decreased the mortality of risky births. However, it is essential to note that these figures are not free from, among others, the bias of endogenous selection of births into cities and hospitals; therefore, they provide only suggestive evidence.

In the next step, we present the results of a simple difference-in-differences estimation. In this exercise, based on the mother's residence, each birth is assigned to the nearest city

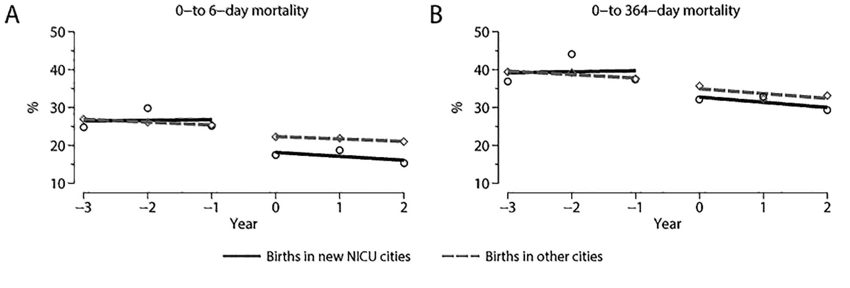

FIGURE 4. Trends in 0- to 6-day and 0- to 364-day mortality of births in cities with a newly established NICU and other cities for newborns <1,500 grams. Year is relative to the opening of the NICU. Year = 0 is set for other cities to be 1998. Source: National vital statistics from Hungary, 1990–2015, linked to the authors' survey on NICU and NETS establishments.

where obstetrics is available in the birth year. In other words, catchment areas of cities with an obstetrics department are defined. After the assignment, an aggregated data set is created by averaging the characteristics of the births (including mortality) at the catchment area-by-year level. The cities in the center of the catchment areas in a given year can be (1) a NICU city, (2) a NETS city, or (3) a city without any of these services. The outcome variables are early neonatal mortality and infant mortality. We estimate two regressions, one that includes only birth year and catchment area fixed effects, and another one including the characteristic of the newborns and their parents. The results are summarized in Table 1. For all weight categories, beneficial effects of NICU and NETS are observed. However, these coefficients are very likely to be biased. As mothers are not obliged to give birth in the nearest hospitals, a higher-risk birth could have access to a NICU hospital even if their local hospital cannot provide such a service. The movements of high-risk births between catchment areas cause a bias, by which these estimations are likely to be a lower bound of the effect of NICU (and NETS). To get more credible estimates, we turn to the IV regressions.

B. MAIN RESULTS

Our main results are estimates of regressions 1 to 3 on three subsamples: (1) deliveries with very low birth weight (less than 1,500 grams); (2) deliveries with low but not very low birth weight (1,500 grams $\leq$ weight $<$ 2,500 grams); and (3) deliveries with low weight (less than 2,500 grams). We consider two outcomes in this section: (1) mortality within 0 to 6 days after birth (early neonatal mortality) and (2) mortality within 0 to 364 days after birth (infant mortality). The descriptive statistics of the variables are summarized in Table A3 in the Online 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.

According to the point estimates, giving birth in a city with a NICU decreased the 0- to 6-day mortality by 153/1,000 live births among infants with birth weight less than 1,500 grams (95 percent CI [77, 229]); by 10/1,000 live births among infants with a birth weight between 1,500 grams and 2,499 grams (95 percent CI [4, 16]); and by 24/1,000 live births among infants with birth weight less than 2,500 grams (95 percent CI [10, 38]). These are large effects. We can compare them with the corresponding mortality rates at the beginning of the time period, 350/1,000, 20/1,000, and 65/1,000, respectively.

The estimated effects on 0- to 6-day mortality of being born in a city without a NICU but connected to a NICU hospital by NETS are 57/1,000 live births for infants with birth weight less than 1,500 grams (not statistically significant), 9/1,000 between 1,500 grams and 2,499 grams, and 9/1,000 for less than 2,500 grams. The effect is substantially weaker than giving birth in a city with a NICU itself for infants with birth weight less than 1,500 grams. This result is consistent with the high risks of transporting newborn babies and the greater amount of time it takes to rescue newborn infants from distant hospitals. However, for newborns between 1,500 grams and 2,499 grams the estimated effect of being born in a NETS city is non-negligible, even when compared with the effect of being born in a NICU city.

TABLE 1. Effect of the NICU and NETS on mortality: Difference-in-differences estimates at the hospital catchment area–year level

	Mortality 0–6 days						Mortality 0–364 days					
	<1,500 g (1)	<1,500 g (2)	1,500–2,499 g (3)	1,500–2,499 g (4)	<2,500 g (5)	<2,500 g (6)	<1,500 g (7)	<1,500 g (8)	1,500–2,499 g (9)	1,500–2,499 g (10)	<2,500 g (11)	<2,500 g (12)
Nearest obstetrics is in a NICU city	−0.067[b]	−0.070[a]	−0.006[a]	−0.005[a]	−0.012[b]	−0.013[a]	−0.050	−0.06[c]	−0.008[a]	−0.007[a]	−0.011[c]	−0.012[a]
	(0.031)	(0.026)	(0.002)	(0.002)	(0.005)	(0.004)	(0.038)	(0.033)	(0.003)	(0.002)	(0.006)	(0.005)
Nearest obstetrics is in a NETS city	−0.022[c]	−0.026[b]	−0.004[a]	−0.003[b]	−0.004[c]	−0.006[b]	−0.003	−0.011	−0.003[c]	−0.002	−0.001	−0.004
	(0.012)	(0.012)	(0.001)	(0.001)	(0.002)	(0.002)	(0.012)	(0.012)	(0.002)	(0.002)	(0.003)	(0.003)
Catchment area FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Number of observations	1,945	1,945	1,947	1,947	1,947	1,947	1,947	1,947	1,947	1,947	1,947	1,947

Source: Based on national vital statistics from Hungary, 1990–2015, linked to the authors' survey on NICU and NETS establishments.

Note: Robust standard errors with catchment area clustering are in parentheses. The controls include averages for the infant's gender, birth months, 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 five-year categories). The regressions are weighted with the number of births in the catchment area–year cells.

[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.10$.

TABLE 2. Effect of being born in a city with a NICU or in a city connected to NETS on mortality: 2SLS estimates

	Mortality 0–6 days			Mortality 0–364 days		
	<1,500 g (1)	1,500–2,499 g (2)	<2,500 g (3)	<1,500 g (4)	1,500–2,499 g (5)	<2,500 g (6)
Born in a NICU city	−0.153[a]	−0.010[a]	−0.024[a]	−0.144[a]	−0.021[a]	−0.031[a]
	(0.038)	(0.003)	(0.007)	(0.042)	(0.005)	(0.009)
Born in a NETS city	−0.057	−0.009[a]	−0.009[b]	−0.020	−0.011[a]	−0.008
	(0.040)	(0.002)	(0.005)	(0.043)	(0.004)	(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	2,029	2,929	2,964	2,029	2,929	2,964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319
Mean of the dep. var. (full sample)	0.203	0.009	0.040	0.303	0.023	0.066
Mean of the dep. var. (1990–94)	0.353	0.020	0.067	0.467	0.040	0.100

Source: Based on national vital statistics from Hungary, 1990–2015, linked to the authors' survey on NICU and NETS establishments.
Note: 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 five-year categories).
[a]$p < 0.01$, [b]$p < 0.05$.

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

The first-stage results (Table 3) 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 a non-NICU but NETS city does not 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. These results

TABLE 3. First-stage results of the 2SLS regressions for the effect of being born in a city with a NICU or in a city connected to NETS on mortality

	<1,500 g		1,500–2,499 g		<2,500 g	
	BNICU (1)	*BNETS* (2)	*BNICU* (3)	*BNETS* (4)	*BNICU* (5)	*BNETS* (6)
Distance to NICU (10 km)	−0.117[a]	0.058[a]	−0.119[a]	0.068[a]	−0.119[a]	0.067[a]
	(0.009)	(0.008)	(0.011)	(0.008)	(0.011)	(0.008)
Distance to NETS (10 km)	−0.006	−0.045[a]	0.007[a]	−0.080[a]	0.006[b]	−0.075[a]
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
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
Number of municipalities	2,029	2,029	2,929	2,929	2,964	2,964
Number of observations	34,213	34,213	188,611	188,611	223,319	223,319

Source: Based on national vital statistics from Hungary, 1990–2015, linked to the authors' survey on NICU and NETS establishments.

Note: 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 five-year categories).

[a]$p < 0.01$, [b]$p < 0.05$.

strengthen the credibility of our main estimates. The reduced-form estimates (Table 4) are in line with the two stages of the 2SLS, and they have similar t-statistics (coefficient estimates over standard errors).

C. ADDITIONAL RESULTS AND ROBUSTNESS CHECKS

First, we examine whether IV strategy is necessary for our estimations. For comparison, Table A4 in the Online Appendix shows the results of the non-instrumented (ordinary least squares) estimates of equation 1. They 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 ordinary least squares and 2SLS results is in line with that expectation, especially for non-VLBW births. The coefficient estimates are negative

TABLE 4. Reduced-form estimates of the 2SLS regressions for the effect of being born in a city with a NICU or in a city connected to NETS on mortality

	Mortality 0–6 days			Mortality 0–364 days		
	<1,500 g (1)	1,500–2,499 g (2)	<2,500 g (3)	<1,500 g (4)	1,500–2,499 g (5)	<2,500 g (6)
Distance to NICU (10 km)	0.015[a]	0.001	0.002[a]	0.016[a]	0.002[a]	0.003[a]
	(0.004)	(0.000)	(0.001)	(0.004)	(0.001)	(0.001)
Distance to NETS (10 km)	0.003[c]	0.001[a]	0.001[c]	0.002	0.001[a]	0.000
	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)
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
Number of municipalities	2,029	2,929	2,964	2,029	2,929	2,964
Number of observations	34,213	188,611	223,319	34,213	188,611	223,319

Source: Based on national vital statistics from Hungary, 1990–2015, linked to the authors' survey on NICU and NETS establishments.
Note: 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 five-year categories).
[a]$p < 0.01$, [c]$p < 0.10$.

but closer to zero or even positive. These results support the need for our IV strategy, and they are also consistent with how our IV strategy should reduce the bias.

Second, one might be concerned that establishing a new NICU could be accompanied by other interventions (such as higher-quality prenatal care) that affect the health of newborns in general. In this case, our estimates might be biased. We test for this issue by applying exactly the same empirical approach as our main model with an indicator for VLBW/LBW status as a dependent variable. After running this model on a full sample of newborns between 1990 and 2015, we find that giving birth in a NICU city (or NETS city) does not influence the probability of being a VLBW/LBW infant (Online Appendix Table A5). We also checked how excluding individual covariates or adding information on birth weight influences the results. Table A6 in the Online Appendix shows the estimates from a regression where only fixed effects are included, whereas Table A7 shows the results where the birth weight of the newborn is controlled for. Despite the changes, the results remain the same and suggest that the main results are not affected by such bias.

Third, we did robustness tests for functional form assumptions. Our instruments are the distance of the mother's residence to the nearest city with a NICU or NETS. In the baseline specification of equations 2 and 3, we entered the distance measures linearly. We reestimate our models using different functional forms, including a quartic specification and one with 10-km bins. Online AppendixTables A8 and A9 show the results for mortality.

Fourth, we further experimented with the instruments. We created new instruments that consider not only the distance to the nearest city with a NICU (or NETS) hospital but also the distance of the mother's residence to the closest hospital. Specifically, we calculated the difference between these two distances. These new instruments might more accurately capture the extra travel "cost" of giving birth in a NICU/NETS city. The results are shown in Online Appendix Table A10. The estimated coefficients are very similar to the baseline ones.

Fifth, the nonrandom locations of NICU hospitals are addressed by including municipality of birth fixed effects, beyond the municipality of residence fixed effects (Table A11, Online Appendix). Again, the results remained practically unchanged.

Sixth, to address potential nonparallel trends, we reestimated our models, including municipality-specific linear time trends. Table A12 in the Online Appendix shows the results; they are qualitatively similar, although somewhat weaker (especially some NETS coefficients). Nevertheless, the conclusions remain the same.

Seventh, we reestimated the models including only those municipalities that experienced a change in the distance to the nearest NICU city between 1990 and 2015 (Table A13, Online Appendix). In this case, the point estimates suggest slightly stronger effects than the baseline coefficients.

To examine pre-trends more directly, we reestimated our models with lead terms (Békés and Kézdi 2021). 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 in the Online Appendix 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 a fixed-effects model showing average differences in mortality from before to after the time period indicated, in successively additive ways. The results should be compared with the positive reduced-form effects we presented in Table 4 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; the unit itself, or most elements of it, were likely already in place the year before. 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 capture 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 earlier that the estimates are likely to be 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 reestimated 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, excluding the larger cities, including Budapest, the capital. The results, presented in Online Appendix Table A15, are very similar to the main results.

Finally, we estimated our models for preterm births, instead of birth weight groups, in three categories: 0–31 weeks of estimated gestation weeks, 32–36 weeks, and 0–36 weeks (Table A16, Online Appendix). Again, these results are very similar to the main results.

VII. Impacts on Impairment

After estimating the effects of NICU/NETS on mortality, we turn to its potential effects on long-term impairment. Recall that most impairments manifest by age 3 but not earlier; therefore, we focus on impairments reported for children aged 3 or above (Figures A1 and A2 in the Online Appendix Appendix show the age-impairment profiles). The impairment data are from the census of 2011; the response rate in the census was 80 percent, and its records were linked to birth records with a 75 percent success rate on average. The age restriction leads to a focus on those born in 1990 through 2008 and observed in 2011 at the age of 3 to 21. These factors result in substantially smaller numbers of observations than what we used for the mortality estimates. However, we note that the mortality results do not change even if the sample is restricted to births between 1990 and 2008.

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 for those who were not at the margin of infant mortality. Our estimates show the net effects of the two. Table 5 shows the results, in the same structure as Table 2. The corresponding summary statistics, first-stage results, and reduced-form results are in Table A3, Table A17, and Table A18 in the Online Appendix. (Table A19 shows the results of a simple ordinary least squares estimation.)

The point estimates are all very close to zero (especially compared with the mortality coefficients), and none of them are significant at conventional levels. While our confidence intervals are wide, it is remarkable that most of the 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. Although the standard errors are too large to completely rule out that NICU/NETS has small or medium-sized effects on impairments, large effects

TABLE 5. Effect of being born in a city with a NICU or in a city connected to NETS on the probability of long-term impairment: 2SLS estimates

	Any impairment			Impairment present at birth		
	<1,500 g (1)	1,500–2,499 g (2)	<2,500 g (3)	<1,500 g (4)	1,500–2,499 g (5)	<2,500 g (6)
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	50.4	42.7	47.5	50.4	42.3	47.1
IV *F*-stat. NETS	40.1	230.5	225.2	39.1	230.6	225.2
Number of municipalities	1,173	2,719	2,763	1,168	2,719	2,762
Number of observations	9,992	94,106	104,758	9,891	93,726	104,273
Mean of the dep. var.	0.157	0.043	0.054	0.132	0.031	0.042

Source: Based on national vital statistics from Hungary, 1990–2008, linked to the authors' survey on NICU and NETS establishments, and population census of 2011.

Note: Robust standard errors with municipality clustering are in parentheses. Individual covariates: see notes to Table 2.

can be explicitly ruled out. In other words, NICU treatment might cause future health consequences, but the extent is unlikely to be very large.[8]

These results are robust for some important sensitivity tests: including birth weight controls (Table A20, Online Appendix), using alternative instruments (Table A21, Online Appendix), using a more flexible functional form of the original instruments (Table A22, Online Appendix), including the municipality of birth fixed effects (Table A23, Online Appendix), and excluding birth years where the linkage of birth and census records were the least successful (Table A24, Online Appendix). Importantly, using groups based on gestation length (instead of the birth weight groups) results in coefficient estimates of practically zero (Table A25, Online Appendix).

8 We note that mortality after age 1 cannot be considered in this analysis. Note that considerable heterogeneity across children with health impairments born in NICU and non-NICU cities might influence the results. If children saved by NICU/NETS and born with health impairments tend to die after the age of 1, in their early years, this may bias our impairment estimation towards zero.

VIII. Conclusions

This study estimated the effect of giving birth in a city with a neonatal intensive care unit and in a city connected to a NICU hospital by a neonatal transportation system 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 between 1990 and 2015; a middle-income country where geographic distance is an important determinant of access to public services. We applied an IV strategy that handled the endogenous selection of births into a NICU/NETS hospital. We used the distance of the mother's residence to the city of the hospital as an instrument. At the same time, residence fixed effects dealt with the issue that various characteristics of the geographic areas are correlated with the distance to the nearest NICU/NETS hospital and might be related to the mortality and impairment outcomes of the newborns.

Our results showed that being born in a city with a 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 in the lowest weight category. 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 access to neonatal intensive care on 0- to 6-day mortality, longer-term mortality, and long-term impairments in the same framework, in a single equation 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 for newborns who have direct and quick access to a NICU hospital. Second, the results suggest that the effects on early neonatal mortality are long-term effects: lives saved in the first week also tend to be saved for the remainder of the first year. This result is remarkable, as it suggests that most lives are saved for a very long time, as mortality after the first year is low. Third, our results suggest that the system also helps to avoid long-term impairments. 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 the deficit by reducing the risk for other infants. Fourth, the estimated effects of the transport system (NETS) are also positive in reducing mortality, but they are substantially weaker than the effects of NICUs for very low birth weight infants. Given the substantial risks of transporting newborns in critical conditions, 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. However, our results show that the NETS saves lives, too. Its impact seems to be especially important for newborns with low but not very low birth weight (1,500–2,499 grams). This result is especially important, because the number of NICUs cannot be increased indefinitely, as higher patient volume of the NICUs is associated with higher efficiency and higher survival rates (Phibbs et al. 1996, 2007; Hentschel et al. 2019; Jensen and Lorch 2015). The NETS provides an opportunity for survival for infants for whom antenatal transfer was not possible.

In our sample period (1990–2015), early neonatal and infant mortality declined by roughly 30–35 percentage points among VLBW newborns and by 6–7 percentage points among LBW newborns. A simple back-of-the-envelope calculation (based on Figure 2 and Table 2) suggests that improved access to NICU explains 10–15 percent of these declines, while the change in the proportion of births in NETS cities adds little. Several important factors contributed to the better mortality figures. The introduction and diffusion of new medical interventions and technologies (e.g., surfactant) probably contributed to the largest extent to the decline in infant mortality (Grytten et al. 2017). Improved antenatal care and change in parental behavior and awareness during pregnancy may also have played a role.

The previous studies differ from our measurement in one or more dimensions; thus, it is hard to compare. First, our article measures the effect of being born in cities with a NICU, taking together all interventions available in a NICU. In contrast, some previous literature measures the impact of particular interventions.[9] Second, this article estimates the effect on compliers, those children who would not be born in a NICU city if none were available at a reasonable distance. It is probably because these pregnancies are not identified as risky by the gynecologist until delivery. Contrarily, some compare outcomes of those born in and out of a NICU hospital while controlling for pregnancy risk.[10] These measurements include always-takers as well. Third, our outcome variables are 0- to 6-day mortality, 0- to 364-day mortality, and impairment probability, whereas some studies measure different outcomes.[11] Nevertheless, the meta-analysis of Lasswell et al. (2010) found that the unadjusted difference in neonatal/predischarge mortality between VLBW infants born in non-NICU and NICU hospitals[12] is 15 percentage points, which corresponds to a 60 percent increase in adjusted odds. Although we examine different outcomes, these figures are comparable to our findings.

Our estimates can help to assess the benefits of providing quick and direct access to the NICU/NETS system for additional newborns in previously underserved regions using current medical technology in middle-income countries where geographic distance matters for access. Moreover, our results may be useful for policy discussions in countries with extended NICU systems about the costs and benefits of NICUs. Giving birth in a city with a NICU hospital is expected in the long run to save approximately 140 of 1,000 very low birth weight infants and approximately 20 of 1,000 infants with birth weight between 1,500 and

9 Examples are Grytten et al. (2017) (ventilators, antenatal steroids, surfactant, and insure), Almond et al. (2010) (increased health spending), Bharadwaj, Løken, and Neilson (2013) (receiving extra medical care), Watson, Arulampalam, and Petrou (2017) (cost per life saved in NICU), or Goodman et al. (2002) (neonatologist per delivery ratio).

10 The studies included in Lasswell et al. (2010), Phibbs et al. (2007), J. H. Chung et al. (2010), and Jensen and Lorch (2015).

11 For instance, Lorch et al. (2012) (in-hospital deaths), Sosnaud (2019) (inequality of infant mortality between mothers with low and high education), and Mújica-Mota et al. (2020) (in-hospital mortality, length of hospital stay, estimated on the sample of births at less than 32 weeks' gestation).

12 NICU hospitals are defined as hospitals with a level 3 unit.

2,500 grams. Giving birth in hospitals without a NICU but connected to a NICU by neonatal transportation is expected to save approximately 20 of 1,000 very low birth weight infants and approximately 10 of 1,000 infants with birth weight between 1,500 and 2,500 grams. There appear to be no sizable long-term impacts on impairment. The high costs of the expansion and subsequent maintenance of the NICU/NETS system should be weighed against these benefits. However, we note that our results do not mean that the number of NICU departments should be increased without any limit. As previous studies have concluded, there is a trade-off between patient volume/efficiency and availability (Phibbs et al. 1996, 2007; Hentschel et al. 2019; Jensen and Lorch 2015). Providing direct access to the neonatal intensive care system can also be achieved by improved accuracy of prediction of the risk of (very) preterm birth (Suff, Story, and Shennan 2019).

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