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Abstract

This paper investigates the effects of alleviating remoteness constraints on access to quality maternal and newborn health care. Using a cluster-randomized controlled trial, we provided transportation vouchers to impoverished pregnant women residing in remote Nicaraguan villages located approximately five hours from the nearest health center, along with accommodations vouchers for maternal waiting homes. These vouchers were provided to the women and to companions of their choosing. The subsidies increased the utilization of quality antenatal care, institutional delivery, and quality postnatal care for mothers and newborns. Additionally, neonatal and infant mortality rates, as well as fertility rates, decreased in treated communities five years after the intervention began.
Keywords: remoteness, subsidies, maternal health, infant mortality. (JEL D10, D04, I15, O12, O18)

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Poverty disproportionately affects rural areas, and, particularly for remote populations, the burden of travel costs significantly hinders access to services and markets located in urban areas (Bryan et al. 2014; Herrera-Almanza and Rosales-Rueda 2020; Meghir et al. 2021; Lagakos et al. 2023). This issue is especially critical for health-care access, as it can pose life-threatening risks. Remarkably, a global estimate indicates that 646 million people cannot reach health-care services within an hour, even with motorized transportation (Weiss et al. 2020). Despite the importance of this issue, there is a notable gap in the literature regarding effective strategies to address the challenges of geographical remoteness in health-care delivery.

In this paper, we investigate the extent to which alleviating remoteness constraints can boost access to quality maternity care. Community-based obstetric care, while safe for normal deliveries, can be risky if life-threatening and hard to predict complications arise (such as postpartum hemorrhage or obstructed labor) (Bancalari et al. 2023). Additionally, medical staff require advanced medical technology and laboratory tests to identify and prevent life-threatening complications during pregnancy and delivery, and for these complications to be successfully treated, women must have timely access to facility-based emergency care (Campbell and Graham 2006; Meltem Daysal et al. 2015; Lazuka 2018). This risk is particularly prevalent in hard-to-reach remote rural areas, which face multiple logistical challenges, including long travel times, the impracticality of constructing obstetric clinics in sparsely populated areas, and difficulties in staffing such clinics with skilled personnel and equipping them with adequate obstetric technology (Acevedo et al. 2020).

We conducted this study in remote Nicaraguan villages that are largely disconnected from the health-care system. The study communities are, on average, five hours away from health and birth centers offering delivery and essential obstetric services (referred to as childbirth facilities in the rest of this study). Transportation costs in these remote communities are prohibitively expensive, amounting to around 40% of the median household's monthly expenditures. Once women reach childbirth facilities through a combination of various means of transportation, they receive free services within the Nicaraguan public health network. Thus, liquidity constraints are primarily related to travel costs.

Through a cluster-randomized controlled trial (RCT), we investigated the effectiveness of complementary subsidies in alleviating interconnected constraints. We provided transportation vouchers to pregnant women and companions of their choosing in treated communities to access quality antenatal care and institutional delivery at the closest childbirth facility. Additionally, we offered vouchers that covered the cost of accommodations for the mother and a companion for 10 days before the delivery date and three days after, with the aim of facilitating timely access to institutional delivery and quality postnatal checkups, as well as providing time for recovery after childbirth.

We randomly allocated 76 community clusters to the subsidies treatment and 76 to control. Women living in control communities were not allocated to receive any voucher. The intervention was implemented between June 2013 and December 2018. Notably, the quality of care at reference childbirth facilities rose over the study period due to the facilities' involvement in the Salud Mesoamerica Initiative (SMI), a multi-country, public-private partnership focused on improving reproductive, maternal, and child health services. Since these supply-side improvements occurred at all reference facilities for both treated and control communities, any effects observed in the treatment group were in excess of the general quality enhancements that benefited all study communities.

We used several sources of data collected before the start of the intervention as baseline data. Additionally, in late 2018, we conducted an endline survey, in which we identified all pregnant women in the study communities. We used their birth histories to investigate the effects of the intervention on access to maternal care, the timeliness and quality of that care, and maternal and infant health.

Nearly all women who reported receiving a voucher utilized it, and the social support provided during the transfer to maternal waiting homes—a challenging time before childbirth—significantly increased. The vouchers were distributed to pregnant women during their initial prenatal visit to the community home or health post. The compliers, in terms of receiving vouchers, were more educated, married, had fewer children, and lived in better-quality dwellings in the least remote communities.

The subsidies improved access to quality maternal health care. Across all outcomes, while there is no effect in the pre-intervention cohort (2011–2012), the effect becomes positive and significant for the intervention cohorts.

The likelihood of receiving antenatal care from skilled staff (a nurse or doctor) increased by 5 percentage points (intent-to-treat or ITT). Reporting receiving at least one voucher, instrumented by the treatment allocation, increased the likelihood by 14 ppts (local average treatment effects, or LATE). Importantly, the treatment led to a significant increase in access to quality antenatal care, with an average increase in the likelihood of receiving all recommended clinical and laboratory checks of 8 percentage points (11.4% higher than the control group mean, with a LATE estimate of 22 percentage points). This effect is driven by improvements in all components of quality antenatal care, with the greatest relative increase estimated for having a blood and urine tests taken (roughly 10% higher than the control group mean).

The treatment also increased the likelihood of institutional delivery by 8 percentage points (12% higher than the control mean). We find no effect on the likelihood of giving birth through c-section, which is reassuring considering the potential negative long-term consequences when c-sections are used in non-complicated pregnancies (Costa-Ramón et al. 2018; Tonei 2019).

Furthermore, the treatment increased the likelihood of receiving postnatal care by 8 percentage points (13.6% higher than the control mean). Notably, high-quality postnatal care offered to mothers increased by 8 percentage points (20.0% higher than the control mean). The effect sizes are 23 percentage points (ppts) when estimating the LATE.

Finally, neonatal and infant mortality dropped in the treatment group five years after the intervention began. It had 10 fewer infant deaths per 1,000 births—almost half of the infant mortality rate of the control group. Half of this effect stemmed from a reduction in neonatal mortality. To put these magnitudes into perspective, global declines in infant and neonatal mortality rates over the last 30 years (1990–2020) stand at 45% and 49% respectively (World Bank 2023).

We rule out ascribing the drop in mortality in the treatment group to selection at birth or differences in the composition of mothers across cohorts. Instead, the decrease can be attributed to supply-side improvements in neonatal clinical care late in the intervention, which likely interacted with the demand-side treatment. Additionally, prevention of unwanted pregnancies may also explain the decline, since we observed a dramatic increase in contraceptive distribution to mothers post-birth late in the intervention, and a 19.6 pregnancy per 1,000 women reduction in fertility in the treatment group compared to the control group five years after the intervention began.

We find that women in more remote communities benefited the most from the subsidies, with significant increases in institutional deliveries and reductions in newborn mortality and fertility. ITT estimates for effect on service use fell short of the reductions in early-life mortality; this can be attributed to the fact that the mothers who gained access were likely those with high-risk pregnancies where the newborn's survival would be under significant threat without the subsidies.

Our study contributes to three key areas of literature. First, while most research on alleviating financial constraints focuses on services that cost a fee, our study examines a context where health care is free at the point of use and travel costs are the primary barrier.[1] Our study builds on research on India's JSY program that provided subsidies for institutional delivery but found only modest effects on service utilization and health outcomes (Powell-Jackson and Hanson 2012; Powell-Jackson et al. 2015; Ghosh and Kochar 2018; Cygan-Rehm and Karbownik 2022), primarily due to congestion externalities (Andrew and Vera-Hernández 2024). We extend this work by showing that addressing demand-side financial constraints while covering maternal waiting home costs to prevent overcrowding and exclusion can significantly improve health-care access and health outcomes.

Second, our study contributes to research on the cost of remoteness, building on the well-established finding that distance is a major obstacle to accessing quality health care.[2] Our study aims to integrate pregnant women from remote areas into the health system, enabling access to life-saving obstetric care. While medical research highlights the importance of ambulance services in delivering emergency care in LMICs, such solutions are often limited to regions with adequate road infrastructure.[3] Moreover, ensuring quality maternal care requires not only emergency services but also routine screenings, including antenatal and postnatal checkups (Campbell et al. 2016). We build on studies in rural India (Anukriti et al. 2023) and rural Kenya (Grépin et al. 2019), which provided transportation vouchers for family planning and institutional deliveries, respectively. By subsidizing not only transportation but also accommodations for pregnant women and their companions, our study addresses multiple interconnected barriers and finds significant improvements in the full continuum of maternal care and infant survival.

Third, we contribute to research on effective solutions to reduce early-life mortality. Our finding that neonatal and infant mortality halved in treated areas after five years

[1] For example, extensive research explores how insurance coverage enhances prenatal care in developed economies (Currie and Grogger 2002; Almond and Doyle 2011; Sonchak 2015; Di Giacomo et al. 2022; Guldi and Hamersma 2023) and postnatal care in LMICs (Miller et al. 2013; Chou et al. 2014; Bernal et al. 2017; Conti and Ginja 2023). Other studies investigate how cash transfers improve access to costly maternity services (Amarante et al. 2016; Triyana 2016; González and Trommlerová 2022; Reader 2023).

[2] In advanced economies, this is often studied in the context of maternal ward closures, which has shown mixed effects (Avdic et al. 2024; Fischer et al. 2024). In LMICs, poor connectivity has been found to hinder the effectiveness of community-based health-care programs (Herrera-Almanza and Rosales-Rueda 2020).

[3] For example, see Razzak and Kellermann (2002); Prinja et al. (2014); Babiarz et al. (2016).

compares favorably with other studies in LMICs, such as the analysis by Bhalotra et al. (2019) of Brazil's health access expansion, which found a 34% reduction after eight years; the evaluation by Conti and Ginja (2023) of Mexico's Seguro Popular, which observed a 10% drop in infant mortality after more than three years; the study by Björkman Nyqvist et al. (2019) of door-to-door health visits in rural Uganda, which estimated a 27% decrease in child mortality three years post-intervention; and experimental evidence from Okeke (2023) that an additional doctor in Nigerian communities decreased newborn mortality by 20%. The substantial decline in mortality observed in our study is partly due to the high baseline mortality rate of 20 deaths per 1,000 live births in these remote areas. Our study shows that a voucher intervention, can be both more logistically feasible than health system reforms and equally effective. Vouchers provide targeted access to specialized obstetric care, including labs and medical technologies, which is often logistically unfeasible for community-based interventions to offer, demonstrating a practical and scalable approach to improving early-life survival.

The paper is organized as follows. Section I describes the context and intervention. Section II presents the experimental design and section III the details about the data collection and measurements. Section IV introduces the empirical strategy, and section V shows the results. Section VII concludes this study.

I. Background and interventions

Background. Nicaragua is a lower-middle-income country that by 2012 had achieved 88% coverage of institutional delivery at the national level but still had substantial disparities within the country. That same year, coverage of institutional delivery was 97% in urban areas but only 79% in rural areas (i.e., communities with one thousand or fewer inhabitants) (ENDESA 2014). About 43% of the population lived in these rural communities (ENDESA 2014), and most of this rural population(about 60%) lived below the national poverty line (FIDEG 2016).

The Ministry of Health (MoH) provides services to around 60% of the population in the country. It mainly serves the population with no social security, and it is the main provider in rural areas (Muiser et al. 2011). The MoH provides multiple health services free of charge, including reproductive, maternal, and child care services. It does so through a network of facilities, which include community homes (*casas base*), health posts, health centers, and hospitals. Community homes, which are run by community health workers, and health posts, staffed by nurses and general practitioners, are the main primary care providers in rural communities. These units provide essential preventive and curative services but have no specialists, medical technology, or laboratory services.

In our study's setting, childbirth facilities provide delivery and obstetric services. These facilities are mostly hospitals, but they also include some large health centers. They are larger than community-based facilities, are located in urban areas, and have the necessary technology, laboratories, and specialists to treat complications that could arise during pregnancy and childbirth.

Maternal waiting homes are a community-managed, publicly-funded strategy (also found in other countries of Latin America and Africa), providing accommodations and board for women living in remote areas before and after they gives birth in nearby childbirth facilities, enabling institutional delivery and postnatal care. Maternal waiting homes and childbirth facilities are usually located close to each other in the town or city that serves as a municipality's administrative center.

In 2012, the Nicaraguan Ministry of Health (MoH) began participating in the Salud Mesoamérica Initiative (SMI), a public-private partnership aiming to enhance the quality of reproductive, maternal, and child health services in the poorest areas of eight Mesoamerican countries. In Nicaragua, the initiative focused on 19 municipalities in the regions of Bilwi, Jinotega, Matagalpa, and Minas. The SMI worked to improve the quality of care by strengthening community-based platforms for delivering contraceptive methods and child health and nutrition programs. It also aimed to bridge gaps in care quality by increasing the availability of essential supplies and equipment for maternal and child health and implementing continuous quality improvement strategies at hospitals. These strategies were designed to enhance adherence to clinical guidelines, particularly in managing common neonatal and obstetric complications.

Despite the supply-side improvement, women living in remote areas still faced significant barriers to accessing these services. Utilization of quality maternal health care drops dramatically with travel time to the closest childbirth facility. In 2011–2012 (pre-intervention), women living two hours away from the closest facility had access to antenatal care provided by skilled staff in roughly 90% of live births, and they had access to institutional delivery and postnatal care in approximately 60% of cases. These figures drop dramatically for women located more than five hours away: roughly half had access to antenatal care, and less than half had an institutional delivery and access to postnatal care (Figure A4). Transportation costs in these remote communities could amount to around 40% of the median household monthly expenditures, and travel times could range from three to 24 hours.[4]

Intervention. To bridge gaps in access to quality maternal health care in remote areas, we designed an intervention in collaboration with the MoH to provide complementary subsidies for pregnant women. In practice, this strategy involved offering a set of three vouchers to pregnant women in randomly selected community clusters (treated). The vouchers consisted of:

1. An antenatal care (ANC) transportation voucher covering round-trip transportation costs to the closest childbirth facility for the pregnant woman and a companion to enable access to quality antenatal care.
2. A delivery transportation voucher for round-trip transportation costs to the closest maternal waiting home and childbirth facility, for the pregnant woman and two companions.
3. A maternal waiting home voucher for accommodations for the mother and a companion in the maternal home closest to the childbirth facility for 10 days prior to expected delivery date and 3 days after the delivery date.

All three vouchers were provided to pregnant women residing in treated community clusters when they attended prenatal care visits at their health post or community home. Women were able to cash in the transportation vouchers upon arriving at the health center or hospital for antenatal care or at the reference childbirth facility for labor. The transportation voucher's value was preestablished based on the estimated transportation cost for each community; it was not a reimbursement. The fixed amount was paid upon presenting the voucher at the health facility.

The maternal waiting home voucher was paid directly to the maternal waiting homes as a fixed amount per voucher, equivalent to a 13-day stay. Its aim was to help cover all costs incurred by the pregnant women and their companions during their stay, including meals.

The ANC transportation voucher was intended to enable pregnant women to travel to the closest health center or hospital where they could get tetanus vaccines, access

[4] Transportation costs were computed in the eligible communities at baseline. The household expenditures data is for the control group at endline. The travel time range comes from communities identified as remote by the MoH in study regions.

obstetric technology, and take lab tests and have them reviewed by skilled personnel. The labs included blood and urine tests commonly done during pregnancy to screen for conditions that could cause complications for the mother or child, such as syphilis, HIV or urinary tract infections. The labs are among those recommended by the WHO for a safe pregnancy (WHO 2016).

The delivery transportation voucher was intended to cover the costs of transporting the mother and two companions to the childbirth facility (or maternal waiting home closest to this facility). In the study's remote areas, the journey to the maternal waiting home can take an average of five hours. Before the intervention, the most common means of transportation to the maternal waiting home was public transportation (63%), followed by ambulance (17%). During this journey, women who could afford to bring companions were most often accompanied by their husbands and/or parents, including parents-in-law (53%, see Table C4).

For delivery, pregnant women were encouraged to travel to the closest maternal waiting home about 10 days prior to their expected delivery date. Approximately 10% of women who used the maternal waiting home before the intervention reported receiving baby items (e.g., bed linen, diapers, clothing, or bed nets), and almost 80% received information about maternal and newborn care and family planning in the maternal waiting home. Satisfaction with maternal waiting homes was quite high: 86% of women who used them before the intervention reported being satisfied with the food, and 92% were satisfied with the cleanliness (see Table C4).

The voucher intervention lasted nearly five years, from June 2013 to December 2018. It was implemented, monitored, and supervised by the Nicaraguan MoH and SMI.

II. Research design: randomization and sampling

The research design is a clustered randomized controlled trial with treatment assignment at the community-cluster level. We determined eligible communities and built community clusters through a multi-step process prior to the start of the intervention.

First, we selected eligible communities from the municipalities identified by the SMI as having high poverty levels and rurality.[5]

[5] In the majority of these municipalities, about 80% or more of their population live in rural areas, with the exception of three municipalities: Rosita, Bonanza and Puerto Cabezas, where 62%, 56%, and

Next, we had a field survey team visit communities that, according to MoH records, were 4 to 10 hours from the nearest childbirth facility. During these visits, they recorded GPS coordinates and interviewed community leaders about travel times, transportation methods, and costs.

Finally, in collaboration with the MoH, we grouped these communities into clusters based on geographical proximity, ensuring that each cluster could be expected to have at least 30 pregnancies in a given two-year period and was served by a common health post or community home. Following these criteria, we grouped a total of 471 communities from 18 municipalities into 282 community clusters (CCs) that were deemed eligible.[6]

Due to cost considerations and based on the ex-ante power calculations, we randomly selected a total of 152 CCs from the list of 282 eligible municipalities. We randomly assigned the subsidy treatment to 76 CCs and gave control status to the other 76 CCs. Table A1 in the appendix contains the final assignment of community clusters and Figure A2 in the appendix has the geographical location of the selected clusters by region and the reference childbirth facilities.

III. Data

To obtain information on eligible communities, treatment fidelity, and utilization of health services by pregnant women over time, we gathered a substantial amount of primary data. Appendix B provides definitions of the main variables used in this study, including the list of preregistered outcomes.

Pre-intervention. As baseline data, we use two main sources of data collected at the community level prior to the start of the intervention. The data was collected for each community within each cluster.

Ministry of Health: To determine eligible communities and build CCs, the MoH provided the most recent community-level data that included population and pregnancies for 2012 and 2013, as well as estimated travel time from each community to its childbirth reference facility. This data was collected by health workers in eligible communities (the previous census in the country, in 2005, was outdated). This

40% of the population, respectively, is rural. These are all located in the northeast part of the country (see Figure A1)

[6]The pre-analysis plan mentions 292 clusters and 487 communities in 19 SMI municipalities, but this reflects the sample before the municipality of Waspan was excluded. The MoH excluded this municipality prior to the random assignment, since the network of health posts and community homes was too limited to make the intervention operational.

dataset used the travel time reported by health workers at health posts or community homes.

Geographical: For the project, an independent field survey team collected data on the subset of eligible communities, including the GPS coordinates of each one, travel time to the reference childbirth health facility according to community leaders, common modes of travel, and estimated travel costs.

Both the MoH and the geographical datasets contained travel time from each community to the nearest childbirth facility. For our analysis, we use the travel time collected in the geographical survey, since this data was independently verified and the most up to date. On average, a CC had a baseline of roughly 400 inhabitants in 2012, approximately 16 pregnancies, and a five-hour travel time to the closest childbirth facility (Table C1).

Figure A5 shows the large variation in travel times to the closest childbirth facility. Half the communities are more than four hours away, and 10% are over ten hours distant. Most study communities lack paved roads and regular public transportation, and some were only accessible by boat. Community leaders indicated that women often had to first reach a public transport stop by walking, horseback, boat, motorcycle, or other means. Additionally, travel times could vary greatly by mode and season; for example, road conditions deteriorate during the rainy season, leading to longer travel times and potential reroutes. This was especially true for the 25% of CCs with travel times of two to three hours, which were deemed eligible due to increased travel time in the rainy season.

Post-intervention. We use data from an endline survey administered in 2018 in one randomly selected community from each of the 152 CCs in the study. Within each sampled community, we use data from a household census and a survey of women. The data was collected by the Nicaraguan National Institute of Information for Development (Spanish acronym: INIDE).[7]

Household census: We conducted a census of all households within the boundaries of the sampled communities. We divided the five sampled communities with more than 1,000 inhabitants into areas of 300 inhabitants (or 65 households) and randomly selected one of these areas for the census. The census contains a roster of all

[7]INIDE is the National Statistics Institute and had previously collected the country's Demographic and Health Surveys, which are very similar to the instruments used to evaluate this project but are nationally representative. The previous survey of this kind (Spanish acronym: ENDESA) was conducted in 2011–2012 in the country and was representative of Nicaragua, but not of the communities in our sample.

household members and their socio-demographic characteristics. The informant for the census was the household head.

Survey of women: We used the household roster from the census to identify all women aged 15 to 56 that had a live birth in the previous seven years. The survey of women included questions on their pregnancy history, use of maternal and newborn health services for each live birth between 2011 and 2018, and reception and use of vouchers and use of maternal waiting homes. The survey also captured detailed data on women's education, employment, dwelling characteristics, asset ownership, and household expenditures.

The survey of women has a design similar to that of surveys collected by the Demographic and Health Surveys Program (DHS), which are the main source of high-quality data on the coverage and quality of reproductive, maternal, and child health services in over 90 countries. This type of survey is the most accurate source of such information when a large share of the population has limited access to health services and when the coverage of medical records and even vital statistics is often incomplete, as is the case in this study's setting.

A key difference between our survey and DHS surveys is that the latter capture use of maternal health services retrospectively for live births in the five years prior to the survey for women age 15 to 49, whereas we focused on live births in the seven years prior to the survey (from 2011 to the date of the survey). We took this approach in order to capture information on the coverage of these services during two years prior to the start of the intervention. As a result, we also expanded the age of respondents to 56 in order to capture data on births among women who were 49 years old as of 2011.

We rely on retrospective data from the women's endline survey to construct our main outcomes. We took this approach, which is common in studies like this, mainly due to cost constraints, since repeated surveys in remote communities were not feasible. Maternal recall has been found to be accurate for key outcomes such as institutional delivery, type of delivery, use of prenatal care services, and even birth weight for periods as long as eight years (Quigley et al. 2007). However, recall accuracy may decline over time for outcomes related to care quality, such as the specific content of visits (Colacce et al. 2020; Ramos et al. 2020; Sou et al. 2006).[8] As long as recall bias is not systematically different between the treated and control groups, our es-

[8] Recall accuracy for specific procedures performed during delivery (e.g., anesthesia, induction) is not reliable, so we do not include these measures (Colacce et al. 2020).

timates remain consistent. We discuss evidence to further alleviate this concern in Section VI.

We use the birth histories to measure the following prespecified primary outcomes for each live birth: had an institutional delivery; had at least one antenatal care (ANC) visit with skilled staff (a doctor or nurse); and had at least one postnatal checkup for newborn and mother. Similarly, for each live birth the prespecified secondary outcomes include: had a first ANC visit during the first trimester of pregnancy with skilled staff, quality of prenatal care, delivery by c-section, quality of postnatal checkup for woman; and received or used a voucher (transportation for ANC, transportation for delivery, and maternal waiting home).

We define quality ANC as care during which women had urine and blood tests collected, blood pressure taken, weight and height checked, and a tetanus vaccine administered during ANC visits. While there is no common international definition of ANC quality, since it can vary depending on the population and disease burden (WHO 2016), our measure of quality includes routine elements considered essential across most settings (Benova et al. 2018).[9]

Quality of maternal postnatal checkup is based on WHO recommendations (Benova et al. 2018),and is defined as whether at least one checkup included counseling on what to do if the mother show danger signs, family planning methods, breastfeeding, and newborn care. Additionally, we use the birth histories to measure infant and neonatal mortality, as well as different indicators of quality of births (miscarriages, stillbirths, birth weight) and fertility.

Analysis sample: For the endline survey, we could not collect data from 11 out of the 152 initially randomly selected communities, mainly due to access limitations, including roads closed by heavy rainfall or security concerns. To minimize sample loss, we visited additional communities that had been randomly chosen as replacements from the sampling frame prior to the start of data collection. After using replacements, we were unable to access only two communities (both from the control group), so the total number of clusters in the endline data was 150. We observe that being a replacement community is orthogonal to treatment allocation (see Table C5). In one treated community, we found no pregnancies in the pre-intervention period (2011–2012), so we only have 149 clusters for our baseline period.

[9] The pre-analysis plan stated that quality ANC will be measured by blood and urine tests, but we included additional criteria in the analysis (blood pressure, weight, height, and tetanus vaccine) for a more comprehensive measure of quality. Since our measure requires that *all* criteria be met for ANC to be considered high quality, this new approach is more restrictive than the prespecified one. We report the effects on each criteria separately in the appendix.

We collected data from 6,674 women, out of which 2,315 had live births before the intervention (2011 and 2012), and 5,726 had live births after the intervention (between 2013 and 2018). On average, women had two live births. There were 2,410 total live births pre-intervention and 7,377 post-intervention. Approximately, 1,370 women (20%) had pregnancies in both the pre-intervention (2011–2012) and intervention (2013–2018) periods.

The fact that such a large share of treated women reported births before and after the intervention alleviates concerns of the vouchers increasing the salience of maternal care and delivery. In Section IV, we discuss how outcomes were balanced during the pre-intervention period across treatment and control, and we show that the effects were only observed for the births that occurred during the intervention period.

In the pre-intervention cohort, on average, women had their first child at age 18 and had a total of four pregnancies. Additionally, 75% of the women were teenage mothers, 7% were indigenous, roughly 75% had no education or incomplete primary, 90% were married or in cohabitation, and 14% lived in a female-headed household. They also had an average of fewer than two of the basic public services (out of six) and assets (out of ten) in their household (see Table C2).

Figure A3 shows the spatial variation in the number of women interviewed across the 18 remote and rural municipalities included in the study, which ranges from 175 to 897. At the community level, we have data on an average of 49 live births that happened after the intervention began (2013–2018) and 16 live births that happened before the intervention (2011–2012).

IV. Specification

We estimate the impact of the treatment on the outcome $Y_{ij,t}$ of pregnancy (resulting in live birth) i in community j from cohort t using the following preregistered specification:

$$Y_{ij,t} = \beta T_j + \delta_t + \epsilon_{ij,t} \tag{1}$$

Here T_j is an indicator variable equal to 1 if the community j was allocated to the *treatment*, and 0 to *control*. δ_t is a cohort indicator variable or cohort fixed effects. The error term $\epsilon_{ij,t}$ is assumed to be clustered by community cluster.

We identify different cohorts using the birth histories collected in the survey of women. The 2011–2012 cohorts provide data on the outcomes of interest before the intervention, and those spanning 2013–2018 provide data during the intervention. Our main estimates pool the multiple intervention cohorts to average out noise and increase power, following McKenzie (2012). To assess the dynamics of the treatment over time, we also present treatment effect estimates for cohorts grouped every two years (2011–2012, 2013–2014, 2015–2016, and 2017–2018). We used this two-year grouping to ensure an adequate sample size for the estimates. The 2013–2014 cohort is partially treated, since the intervention started in June 2013, whereas the 2015–2016 and 2017–2018 cohorts are fully treated.

Additionally, we estimate two-stage least squares (2SLS), where the endogenous uptake of at least one voucher (D_j) is instrumented using the random treatment allocation (T_j), as in the following equation:

$$Y_{ij,t} = \beta^{IV}\,\hat{D_{ij}} + \delta_t + \psi_{ij,t} \tag{2}$$

We can interpret parameter β in equation 1 as the intention-to-treat (ITT) effects, capturing the effect of allocating communities to the subsidies, regardless of whether a pregnant women receives and uses them. Conversely, β^{IV} reflects the magnitude of the effects for full compliance. In light of the likely heterogeneity in the (potential) impacts of the vouchers, we can interpret these estimates as the local average treatment effects (LATE) for participants who comply with the intervention (Imbens and Angrist 1994).

Several pieces of evidence give us confidence that randomization successfully created observationally equivalent groups that support interpreting β as the causal effect of the treatment. First, there is balance in baseline community-cluster-level characteristics, including population, number of pregnancies, travel time to closest childbirth facility, and cluster size in terms of number of communities (Table C1). Second, we find that women- and household-level characteristics from the endline survey (such as women's education, indigenous self-identification, marital status, and household assets) are balanced across treatment and control communities both for the pre-intervention and intervention cohorts (tables C2–C3).[10] Third, the sample composition, based on these woman- and household-level characteristics, is the same across treatment arms in every cohort group (Figure C1). Fourth, there

[10]There is a small imbalance in the intervention cohort on having at least incomplete primary (2 ppts higher in treated group), so we control for this imbalance as a robustness check in our main tables.

is balance in all of our primary and secondary outcomes, including treatment fidelity, for the pre-intervention cohort (Figure 1 and columns (2) and (5) in tables 1 –3). Finally, the sample size does not vary by cohort, which alleviates concerns that differences in statistical power across cohorts could be driving the differences in estimates (Figure C2).

We also calculate estimates using alternative specifications that control for women's characteristics (being indigenous, married, attaining no education or up to incomplete primary, and age at pregnancy) and household characteristics (female-headed household, asset ownership, and dwelling quality) and adding municipality fixed effects (see columns (5)–(7) in tables 1 to 3). We also estimate difference-in-differences (DID) models to account for initial differences between treatment and control in the pre-intervention cohort (see Section VI). The results remain robust to these alternative specifications: they increase slightly in magnitude and become more precisely estimated.

For inference, we supplement standard p-values with those adjusted for multiple hypothesis testing. In each table, we present both p-values for the significance of each individual coefficient and p-values adjusted for multiple hypotheses using the List et al. (2019) bootstrap-based procedure. The latter takes into account all hypotheses tested within a table, separately for different specifications.

In Section VI we discuss heterogeneous effects for all outcome variables. Specifically, we estimate heterogeneous effects by remoteness, defined as the baseline travel time to the closest childbirth facility as captured in interviews with community leaders, which was the most updated data pre-intervention.

V. Results

A. Releasing remoteness constraints

Nearly all women who reported receiving a voucher utilized it. Of those who received any voucher during their pregnancy between 2013 and 2018, 93% reported using any of the vouchers. This level of voucher usage remains high across the different vouchers and pregnancy cohorts (Table D1).

The logistical difficulties of distributing vouchers to every eligible women (anyone pregnant at any time after the start of the intervention in treated communities) in these remote areas led to imperfect compliance. As explained in Section I, the vouchers were distributed to pregnant women that attended their initial prenatal visit at their corresponding community home or health post. The compliers (in terms of receiving vouchers) were more educated, married, had fewer children, and lived in better-quality dwellings in less remote communities (Table D2).

Significant differences in measures of exposure to the intervention across experimental arms reflect the success of the intervention's implementation. Figure 1 shows that before the intervention started, there were no discernible differences in reports of receiving vouchers across treatment and control. After the intervention began, eligible women in the partially treated cohort (2013–2014) were 30 ppts more likely than those in the control group to report receiving any voucher, a difference that increased over cohorts (to close to 40 ppts). We observe these marked differences before and after the intervention, and across treatment groups, for each individual voucher (Figure D1).

There were minimal reports of women in the control group receiving vouchers (at 5% on average during the whole intervention period, see figures 1 and D1), which alleviates concerns of attenuation bias due to contamination.

The treatment significantly boosted the social support available during labor, especially at the critical moment of being transferred to the maternal waiting home. Based on data reported by women who used maternal waiting homes, the treatment increased the likelihood of having company during their transfer by 7 ppts, which is 10.0% higher than the control group's average for the intervention period. This improvement in social support is also observed in the intensive margin. The number of companions increased by 8 ppts (10.3%). Both effects are significant at the 10% level and survive multiple hypothesis testing. The IV effect is larger, with a 12 ppt increase in having had a companion and a 14 ppt increase in the number of companions, both significant at the 5% level (see Table D3).

B. Antenatal care

Table 1 presents estimates of treatment effects on antenatal care provided by skilled staff, quality antenatal care, and antenatal care provided by skilled staff during the first trimester of pregnancy. The last two variables are built based on WHO recommendations (WHO 2016). Columns (2)–(3) present results based on the preestablished specification following Equation 1. Columns (5)–(6) add controls for woman- and household-level characteristics and municipal fixed effects. In columns (3) and (6) we present the ITT estimates and in columns (4) and (7) the TT estimates, including controls and municipal fixed effects in column (7).

We find that the treatment improved antenatal care. On average, the likelihood of receiving antenatal care from a skilled provider was 5 ppts higher in the treatment group, or 5.8% higher than the control mean in the intervention period. The effect's magnitude is greater for those who report receiving at least one voucher (14 ppts). The ITT effect is significant in the fully-treated cohorts (2015–2016 and 2017–2018). It is positive but not significant for the partially treated cohort (2013–2014), since a share of these pregnant women were in more advanced stages of pregnancy when the intervention was introduced (Figure E1, Panel A).

We also find that the treatment led to a significant increase in the quality of antenatal care accessed. We estimate an average increase in the likelihood of receiving all recommended components of antenatal care of 8 ppts (which is 11.4% higher than the control group mean during the intervention period). The LATE estimate for quality antenatal care is 22 ppts. The estimated effects are significant at the 1% level in the specifications with controls and municipal fixed effects. Figure 3), Panel A, shows that the ITT effect is present in the fully treated cohorts.

We observe improved access to each component of quality antenatal care. The likelihood of receiving blood and urine tests had the most significant relative increase (approximately 10% higher than the control group mean, Table E1). It is expected that this component would see the largest improvement, since the ANC transportation vouchers were specifically designed to reduce the cost of accessing prenatal care at facilities equipped with laboratory services, in contrast to community health posts, which are capable of measuring blood pressure and anthropometrics but often lack the capacity to conduct more advanced tests.

The treatment shifts the entire distribution of the number of components of quality antenatal care (five components in total, including blood and urine sample collected and tested, blood pressure and anthropometrics measured, and tetanus vaccine administered). During the intervention period, the shift in the distribution for the treatment group is detectable mostly at higher levels of ANC quality (Panel A of Figure 2). The Kolmogorov-Smirnov test of equality of distributions across treatment and control yields a p-value of 0.002, confirming that the distributions are statistically different from each other.

Since quality measures are reported exclusively by those who attended antenatal care (ANC), and since attendance varies based on treatment assignment, we face a selection issue. Women who attend ANC solely due to the incentive of the subsidies may differ in several aspects from those who did not. These differences could then influence the reported quality measures. For instance, perceptions of quality or willingness to do tests, get vaccinated or allow anthropometric measurements might vary among the marginal women. To tackle this problem, we employ a Heckman selection model, using random assignment to treatment as an instrument for ANC attendance in the first stage (Heckman 1976). The underlying assumption for identification is that the treatment, on an individual level, should not directly impact quality. This assumption is likely because providers had no access to the funds and were unaware of the women's treatment status. Table E3 demonstrates that the results regarding quality ANC remain positive and significant (5 ppts with a p-value of 0.03).

We find a positive but non-significant effect on having the first antenatal care visit provided by skilled staff during the first trimester of pregnancy. This is not a surprising result, since women were deemed eligible for treatment only during their first antenatal care visit at the health posts or community homes.

C. Delivery

Table 2 displays birth-related outcomes. We first show the impact on place of birth, followed by impacts on delivery mode. We find that the treatment increases the likelihood of women giving birth in a medical facility, such as a hospital, clinic, or health-care center, as opposed to giving birth at home or in non-medical environments.

In the treatment group, the treatment led to an 8 ppt increase from the control group's institutional delivery rate of 67%. Using the prespecified estimation, the effect is significant at the 10% level. When we add controls and municipal fixed effects, the coefficient falls slightly to 6 ppts but becomes statistically significant at the 1% level.

As expected, there is no effect in the pre-intervention cohort (2011–2012), but the effect on institutional delivery becomes positive and significant for the intervention cohorts. Figure 3, Panel B, shows that the ITT effect is positive for all cohorts but increases slightly in magnitude and precision for the last study cohort (2017–2018). The LATE estimates show an effect of 21 ppts for women in treated communities who received at least one voucher when pregnant.

While the treatment increases institutional delivery, we do not observe a significant effect on deliveries by C-section, and the effect size is close to zero. The control mean C-section rate is 14%, which is within the 10% to 15% range considered ideal by the WHO. Evidence compiled by the WHO indicates that C-section rates up to 10% are associated with decreases in maternal morbidity and mortality, but there seem to be no additional gains above that rate. In contrast, countries with higher rates might increase health risks with no additional benefits (WHO 2015). Our null results might indicate that the intervention did not lead to overuse of the procedure, which is encouraging.

D. Postnatal care

Because the treatment covers three nights of stay in maternal waiting homes after delivery (in addition to 10 nights before delivery), and these homes are in the proximity of childbirth facilities, we investigate effects in postnatal care. Table 3 presents estimates of treatment effects on postnatal care provided by skilled staff to the mother and newborn and quality postnatal care for the mother.

We find that the treatment increased the likelihood of receiving postnatal care by an average of 8 ppts in the treatment group, which is 13.6% higher than the control mean in the intervention period. The IV effect is greater in magnitude, at 23 ppts. The ITT effect is significant even in the partially treated cohort (2013–2014) and remains positive and significant for the later cohorts (see Figure E1, Panel C). The pregnant women at a later stage of gestation when the treatment was introduced were still able to use the subsidies for delivery and to stay in maternal homes and obtain postnatal care.

In addition, we find that quality postnatal care for mothers increased by 8 ppts in the treatment group, which is 20.0% higher than the control mean in the intervention period. Again, the effect is greater in magnitude for those who report receiving at least one voucher (23 ppts). The effects are present even in the partially treated cohort and remain in the fully treated cohorts, as shown in Figure 3), Panel C. The estimated effects are significant at the 1% level in the specifications with controls and municipal fixed effects.

The treatment also shifts the distribution of the number of components of quality postnatal care for mothers (four components in total, including counseling on danger signs for the mother, family planning methods, breastfeeding, and newborn care). During the intervention period, the shift in the distribution for the treatment group is evident after the first component (Panel B of Figure 2). The Kolmogorov-Smirnov test of equality of distributions across treatment and control yields a p-value of 0.009, confirming that the distributions are statistically different from each other.

As was the case with the results in Section B, here we face an issue of selection in the reported quality of postnatal care. We alleviate this issue by showing that the effect remains similar in magnitude, though with a p-value of 0.11, when using the Heckman correction model (see Table E3).

Finally, we look at effects on continuity of care, which we measure with an indicator that captures whether the women had access to quality antenatal care, institutional delivery, and quality postnatal checkups for a given live birth. Table shows that the subsidies increased the likelihood of receiving high-quality services in the continuum of maternal and newborn health care by 8 ppts (23 ppts for LATE) . The treatment group had 23.5% higher continuity of care compared to the control mean in the intervention period. The effect is positive but not significant for the partially treated cohort (2013–2014), but it becomes significant and increases over time for later cohorts (see Figure E1, Panel D).

E. Maternal and child health

The observed improvements in antenatal and postnatal care, coupled with increased rates of institutional delivery, set a promising stage for improvements in health. We leverage the pregnancy histories captured at endline to then estimate the effects of the treatment on early-life mortality. Early-life mortality rates are direct measures of health outcomes during the most vulnerable stages of a child's life and are sensitive indicators of quality maternal health care (Meltem Daysal et al. 2015; Lazuka 2018; Tekelab et al. 2019).

Our outcomes of interest are the infant mortality rate and the neonatal mortality rate, defined respectively as deaths per 1,000 live births in the first year of life (within the first 12 months) and deaths per 1,000 live births in the first month (within the first 28 days). In the study's setting, the mean infant mortality rate in the control group during the pre-intervention period stood at 20 deaths per 1,000 births, and the neonatal mortality at more than 5 deaths per 1,000 births.

Figure 3, Panels D and E, show that the treatment decreased early-life mortality five years after the start of the subsidy intervention. Notably, this same cohort also experienced the largest improvements in ANC quality and institutional delivery (Panels

A and B), so the drop in infant mortality coincided with these significant gains in maternal health care. Infant deaths dropped by an average of 10 per 1,000 births in the treated group in the 2017–2018 cohort, a result significant at the 5% level. Half of this effect is attributable to reduced neonatal mortality. Among the treated group in the last study cohort, neonatal deaths decreased by an average of 5 per 1,000 births, an effect significant at the 10% level. The magnitude of these effects is substantial, at almost half the mortality rate of the control group in the same cohort. These effects are not significant when all post-intervention cohorts are pooled together (Figure E4).

We explore five mechanisms that could explain the drop in mortality. We first rule out selection at birth. Despite improvements in access to quality antenatal care in the treatment group, we find no significant effects on miscarriages, stillbirths, and birth weight (Figure E2).[11] These findings suggest that the marginal birth during the intervention was not less fragile, and that the gains in neonatal survival cannot be attributed to selective fetal survival.

Next, we observe that increased child survival rates five years after the intervention began are not due to changes in the composition of mothers across cohorts, as mother characteristics are balanced across treatment arms in every cohort (Figure C1).

The drop in mortality could instead be attributed to improvements in the quality of neonatal health care at childbirth facilities in the study's municipalities, which were part of the SMI intervention during the study period. Since these supply-side improvements affected all reference facilities, both treatment and control groups benefited, but the treatment group's outcomes exceeded the general quality enhancements available to all communities. External audits of medical records from SMI childbirth facilities show a notable increase of 5.6 ppts in adherence to clinical standards for managing neonatal complications, as well as a 35.2-ppt increase in immediate neonatal care, between the pre-intervention (2011–2012) and late-intervention (2016–2017) periods (Figure E3). Such improvements included hygiene and prevention of asphyxia and other neonatal and labor complications—all identified as key drivers of neonatal survival (Weiner et al. 2003; Lawn et al. 2005; Seward et al. 2012)— and likely contributed to the drop in neonatal mortality. While we lack detailed data tracking these improvements throughout the intervention,

[11] For these outcomes, we use data on all pregnancies. Because we have the gestational age at which the miscarriages or stillbirths occurred, we can assign these pregnancies to a cohort, assuming they would have had a 9-month gestational period. We assumed a 9-month gestational period for all live births.

the significant gains from the pre- to late-intervention period suggest that the treatment may have positively interacted with these supply-side enhancements in the later stages of the study.

Another potential explanation is prevention of unwanted pregnancies through enhanced access to family planning services. External audit data reveal a significant increase of 37.1 ppts in distribution of contraceptives to mothers post-birth in SMI facilities from the pre-intervention to late-intervention period (Figure E3). Highly effective long-acting reversible contraceptive methods, such as IUDs, are often administered at these childbirth facilities. Figure 3, Panel F is consistent with this explanation, indicating that the treatment reduced fertility five years after the subsidy treatment began. Fertility, measured as the number of pregnant women per 1,000 women, dropped by 19.6 pregnancies per 1,000 women in the treated group of the 2017–2018 cohort, a 10% reduction from the control group's fertility rate. This result is significant at the 1% level. When all post-intervention cohorts are pooled, we see a similar negative effect, but it is not statistically significant (Table E4).

Finally, the drop in infant mortality could also be attributed to improved child-rearing practices. Treated women were 8 ppts more likely to have received quality postnatal care, which not only provided information on family planning—helping to prevent unwanted pregnancies, as discussed earlier—but also offered counseling on caregiving practices, including how to identify danger signs in newborns and take appropriate actions. Additionally, the accommodations vouchers facilitated stays in maternity waiting homes, where baby items and counseling were often distributed (Table C4 shows that during the pre-intervention period, 80% of women who stayed in these homes reported receiving such support).

VI. Additional analysis

In this section we present additional tests that support the robustness of our results, as well as the results from the prespecified heterogeneity analysis.

A. Recall bias

We provide evidence that alleviates concerns that recall bias might be driving balance in the pre-intervention cohort and driving the positive effects in the utilization of maternal health care for the intervention cohorts. The concern hinges on the fact that women's recall of maternal care and place of delivery might be worse for earlier pregnancies.

We estimate effects on the latest birth, for which women were expected to have a better recall. Table E5 shows that the results are robust when focusing on the latest pregnancy. Effects on the pre-intervention cohort are all insignificant, close to zero, and negative for most outcomes. The coefficients turn positive, highly significant, and similar in magnitude to the main results, despite the loss in power due to the reduced sample size.

Furthermore, as discussed in Section III, recall bias is unlikely to impact the recollection of significant life events that women remember with accuracy, such as the death of a child.

Three additional pieces of evidence address recall bias concerns regarding access to maternal healthcare. First, while recall bias is a valid concern when respondents are asked to remember specific details of services provided, certain events are more salient and less prone to memory distortion. In particular, giving birth in an institutional setting and receiving postnatal care —compared to care at home— are distinct, significant events that women are unlikely to forget or misreport.

Second, if recall bias regarding services provided were significant, we would expect it to affect all aspects of ANC quality. However, we find that the effects are predominantly associated with blood and urine tests—services not available at community health posts.

Lastly, the pattern of treatment effects across cohorts suggests that recall bias is not a major factor. If recall bias were influencing the results, we would expect a gradual increase in effect magnitude across cohorts. Instead, the effects for the 2017–2018 cohort are not consistently higher than those for the 2015–2016 cohort, particularly in postnatal care quality (Figure 3, Panel C).

B. Sampling bias

Given that the subsidy treatment could potentially enhance both maternal and child survival, sampling bias is a potential concern. Although we have no data on maternal mortality—an outcome that would have been valuable in its own right—we can assess whether the composition of mothers varies between treatment and control groups and across different cohorts.

Our analysis provides evidence against sampling bias. Figure C1 in the appendix demonstrates that the sample composition remains consistent across treatment arms in each cohort group. This holds true for key women and household characteristics, including maternal age at pregnancy, indigenous status, educational level, marital status, migration during the study period, and household characteristics such as female headship, dwelling quality, and asset ownership.

C. Difference-in-differences

To control for potential imbalances in time-invariant, unobservable characteristics across treatment and control groups at baseline, we estimate a difference-in-differences (DID) model. In this model, we compare treatment and control groups before and after the start of the subsidy intervention.

Table E6 shows that the effects remain statistically significant and similar in magnitude compared to the ITT effects using the prespecified estimation. The effects are more precisely estimated in the DID model, as expected due to the increase in sample and power. The effects on ANC by skilled staff, institutional delivery, and quality postnatal checkup are all statistically significant at the 1% level.

D. Control group

Another concern is that the intervention might have jeopardized access to health care for the control group, perhaps because providers served control women poorly or due to congestion in childbirth facilities and maternal waiting homes.

However, the subsidies should not have directly affected consultation quality, as health-care workers could not claim funds for recipient women and were unaware of each woman's subsidy status. Additionally, the SMI initiative implemented during the same period strengthened maternal and child care services across all childbirth facilities in targeted municipalities (as explained in Section I), regardless of whether they served treatment or control communities, making congestion in health facilities less likely. Furthermore, congestion in maternal waiting homes was mitigated through the accommodations vouchers. In line with this reasoning, Figure E4 shows that the outcomes for both control and treated groups improved over cohorts, but the improvement was greater for the treatment group.

E. Heterogeneity by remoteness

The treatment's effectiveness might depend on travel time. We estimate heterogeneous effects by analyzing variations across communities, using pre-intervention reports from an independent field survey team on the travel time from community centers to the nearest childbirth facility. We focus on locally-reported travel time rather than distance to account for terrain conditions, geographical obstacles, routes commonly used by the population, mixed modes of transport (e.g., walking, horseback riding, motor vehicles, boats), and varying public transport schedules.

We present the results of this exercise in Table 4. We stratify the sample by median travel time (around four hours), with "close" being smaller than or equal the median, and "far" being above the median. We present these results in columns (1)–(6). We also present the p-value of the difference in estimated effects across those "close" and "far" in column (7). To gain more statistical power, in columns (8) to (11) we present the estimates of an OLS regression in the whole sample, including the treatment indicator interacted with the "far" indicator. As a robustness check, we find no evidence of heterogeneous effects in the pre-intervention cohort (Table E7).

The subsidies seem to have been more effective for women living relatively farther away. Although the differences are not significant, the effects on access to quality antenatal and postnatal care are larger for women living in more remote areas. We find clear heterogeneous effects for institutional delivery: the intervention was 7 ppts more effective in more remote communities (column (8)) at the 10% significance level.

Another marked heterogeneity is observed in fertility. Fertility in treated women living further away was markedly lower, by 16.25 pregnancies per 1,000 women (at the 5% significance level), than that of treated women living closer. We also observe that the drop in neonatal and infant mortality, though not statistically significant, is driven by live births from treated women living in more remote villages.

Overall, it seems that there was a greater need for quality care among women located further away. In the absence of the subsidies, these pregnancies would have been the ones at risk of life-threatening complications.

VII. Conclusion

Millions of people globally are unable to reach health-care facilities within an hour. This issue is particularly acute in remote villages, where long travel times and logistical challenges complicate the provision of quality maternity care. Addressing these geographical barriers is crucial, since effective maternity care relies on medical technology and timely access to obstetric emergency services, which are difficult to provide in sparsely populated and isolated regions.

Through a randomized controlled trial (RCT) conducted in remote communities of Nicaragua, we show that providing complementary subsidies, in the form of transportation and accommodations vouchers, enhanced access to quality maternal health care and reduced early-life mortality.

We find that women in more remote communities benefited the most from the subsidies, with significant increases in institutional deliveries and reductions in newborn mortality and fertility among this group. It appears that the mothers who gained access were those with high-risk pregnancies where the survival of their newborn would have been significantly threatened without the subsidies.

Considering only how effective the intervention was in reducing infant mortality by the end of the study period, a back-of-the-envelope calculation indicates that each dollar invested generated a value of 29,737 USD. For further details, see Appendix F.

We draw a number of lessons from this study that are relevant for policy and future research. Importantly, our results highlight the need to simultaneously address interconnected barriers faced by rural women in remote areas. In these areas, people must traverse long distances or bear substantial transportation costs to reach health-care facilities. We demonstrate how subsidizing transportation costs and providing quality accommodations for women and their peers promotes improvements in the continuum of maternal and newborn health care.

Our work demonstrates the feasibility of providing subsidies to vulnerable women in remote rural areas, despite their logistical challenges. The vouchers, which involve minimal administrative effort for registration and documentation, can be efficiently scaled using local health centers. While the primary impact of the vouchers is financial, they may also enhance awareness and emphasize the importance of consultations and facility-based deliveries. Although information alone is unlikely to change behavior, due to prohibitive transportation costs, future research should explore how vouchers might generate social learning and improve uptake of preventive care and institutional deliveries in remote areas.

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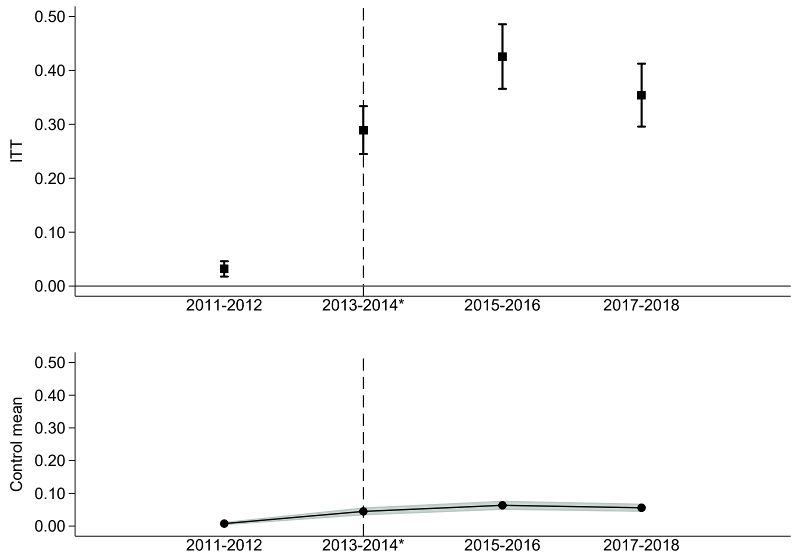
Figure 1: Received any voucher



Notes. ITT estimates based on women-level OLS regressions using equation (1) separately for each two-year cohort. Period 2011–2012 indicates *pre-intervention* pregnancies. The pregnancies within the vertical dashed lines are the *mid-intervention* pregnancies, which were partially affected by the intervention that started in June 2013. All subsequent periods (to the right of the vertical dashed line) are the *intervention* births. Confidence intervals are computed at the 95% level of confidence using standard errors clustered at the community level. All specifications include indicator variables for cohort and municipality fixed effects.

Table 1: Antenatal care

Outcome	Control mean	No Controls: ITT Pre 2011–2012	No Controls: ITT Intervention 2013–2018	No Controls: IV Intervention 2013–2018	With Controls: ITT Pre 2011–2012	With Controls: ITT Intervention 2013–2018	With Controls: IV Intervention 2013–2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ANC by skilled staff	0.86	-0.00	0.05	0.14	-0.01	0.04	0.11
		(0.03)	(0.02)	(0.06)	(0.02)	(0.01)	(0.03)
p-value		[0.89]	[0.05]	[0.02]	[0.65]	[0.01]	[0.00]
Adjusted p-value		[0.89]	[0.07]		[0.65]	[0.02]	
AR p-value				[0.03]			[0.00]
Quality ANC	0.70	0.02	0.08	0.22	0.02	0.07	0.19
		(0.04)	(0.03)	(0.09)	(0.03)	(0.02)	(0.05)
p-value		[0.71]	[0.03]	[0.01]	[0.46]	[0.00]	[0.00]
Adjusted p-value		[0.92]	[0.05]		[0.79]	[0.00]	
AR p-value				[0.02]			[0.00]
ANC by skilled staff in first trimester	0.58	-0.01	0.03	0.07	-0.01	0.01	0.04
		(0.04)	(0.03)	(0.09)	(0.02)	(0.02)	(0.05)
p-value		[0.89]	[0.44]	[0.41]	[0.61]	[0.45]	[0.40]
Adjusted p-value		[0.98]	[0.44]		[0.81]	[0.45]	
AR p-value				[0.43]			[0.40]
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237

Notes. ITT estimates based on OLS regressions using equation (1) in columns (2),(3),(5) and (6) and TT estimates in columns (4) and (7) based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Dependent variables by row: (1) *ANC by skilled staff*, indicator variable equal to 1 if women attended at least one antenatal care (ANC) checkup provided by skilled staff (doctor or nurse) during pregnancy, and 0 otherwise; (2) *Quality ANC*, indicator variable equal to 1 if during pregnancy women attended at least one antenatal care (ANC) checkup in which a blood and urine sample was collected and tested, blood pressure and anthropometrics were measured, and the tetanus vaccine was administered (see Table E1 for effects on individual components), and 0 otherwise; (3) *ANC in first trimester*, indicator variable equal to 1 if during pregnancy women attended their first ANC checkup during the first trimester and had at least one ANC checkup by skilled staff (doctor or nurse), and 0 otherwise. All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also includes municipality fixed effects and controls for the following characteristics of women: being indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. The p-values are presented in brackets. The first row shows values from individual testing, the second adjusts for testing that treatment is jointly different from zero for all outcomes presented in the table, per specification, and the third shows the Anderson-Rubin p-values, robust to weak instruments.

Table 2: Delivery

Outcome	Control mean	No Controls: ITT Pre 2011–2012	No Controls: ITT Intervention 2013–2018	No Controls: IV Intervention 2013–2018	With Controls: ITT Pre 2011–2012	With Controls: ITT Intervention 2013–2018	With Controls: IV Intervention 2013–2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Institutional delivery	0.67	0.01	0.08	0.21	0.01	0.06	0.18
		(0.05)	(0.04)	(0.11)	(0.02)	(0.02)	(0.05)
p-value		[0.86]	[0.10]	[0.06]	[0.83]	[0.00]	[0.00]
Adjusted p-value		[0.98]	[0.16]		[0.83]	[0.00]	
AR p-value				[0.09]			[0.00]
C-section	0.14	-0.00	0.00	0.00	-0.01	-0.00	-0.01
		(0.02)	(0.02)	(0.05)	(0.01)	(0.01)	(0.03)
p-value		[0.90]	[0.92]	[0.92]	[0.61]	[0.72]	[0.69]
Adjusted p-value		[0.90]	[0.92]		[0.85]	[0.72]	
AR p-value				[0.92]			[0.70]
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237

Notes. ITT estimates based on OLS regressions using equation (1) in columns (2),(3),(5) and (6) and TT estimates in columns (4) and (7) based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Dependent variables by row: (1) *Institutional delivery*, indicator variable equal to 1 if a woman gave birth in a health center, assisted by a skilled health provider, as opposed to at home, and 0 otherwise; (2) *C-section*, indicator variable equal to 1 if delivery was through c-section, and 0 otherwise. All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also includes municipality fixed effects and controls for the following characteristics of women: being indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. The p-values are presented in brackets. The first row shows values from individual testing, the second adjusts for testing that treatment is jointly different from zero for all outcomes presented in the table, per specification, and the third shows the Anderson-Rubin p-values, robust to weak instruments.

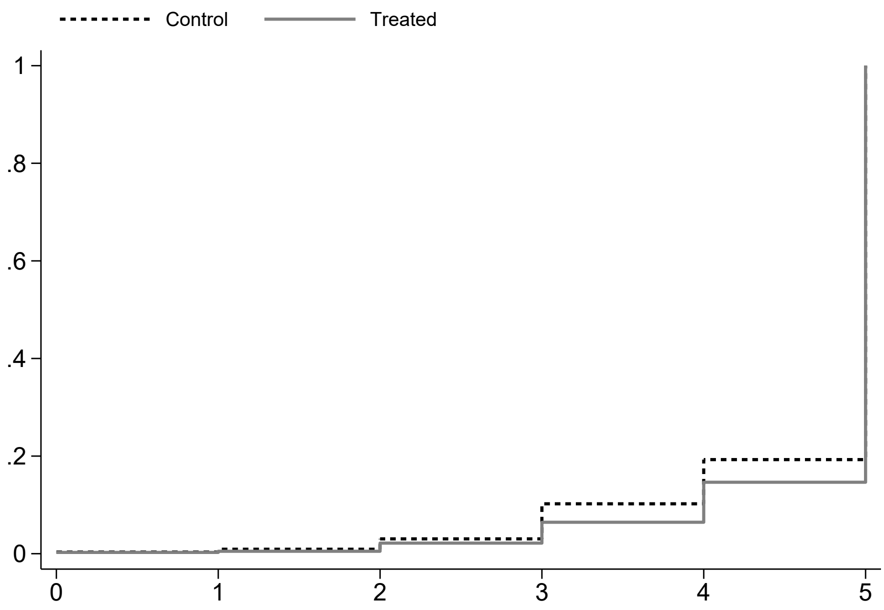
Table 3: Postnatal care

Outcome	Control mean	No Controls: ITT Pre 2011–2012	No Controls: ITT Intervention 2013–2018	No Controls: IV Intervention 2013–2018	With Controls: ITT Pre 2011–2012	With Controls: ITT Intervention 2013–2018	With Controls: IV Intervention 2013–2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Postnatal checkup for mother and newborn	0.59	0.04	0.08	0.23	0.04	0.07	0.21
		(0.05)	(0.04)	(0.12)	(0.02)	(0.02)	(0.05)
p-value		[0.47]	[0.08]	[0.05]	[0.16]	[0.00]	[0.00]
Adjusted p-value		[0.59]	[0.08]		[0.27]	[0.00]	
AR p-value				[0.07]			[0.00]
Quality postnatal checkup for mother	0.40	0.02	0.08	0.23	0.02	0.08	0.22
		(0.04)	(0.04)	(0.10)	(0.02)	(0.02)	(0.05)
p-value		[0.58]	[0.04]	[0.02]	[0.35]	[0.00]	[0.00]
Adjusted p-value		[0.58]	[0.06]		[0.35]	[0.00]	
AR p-value				[0.04]			[0.00]
Observations		2,408	7,365	7,364	2,408	7,365	7,364
Communities		149	150	150	149	150	150
Obs. imputated					67	236	236

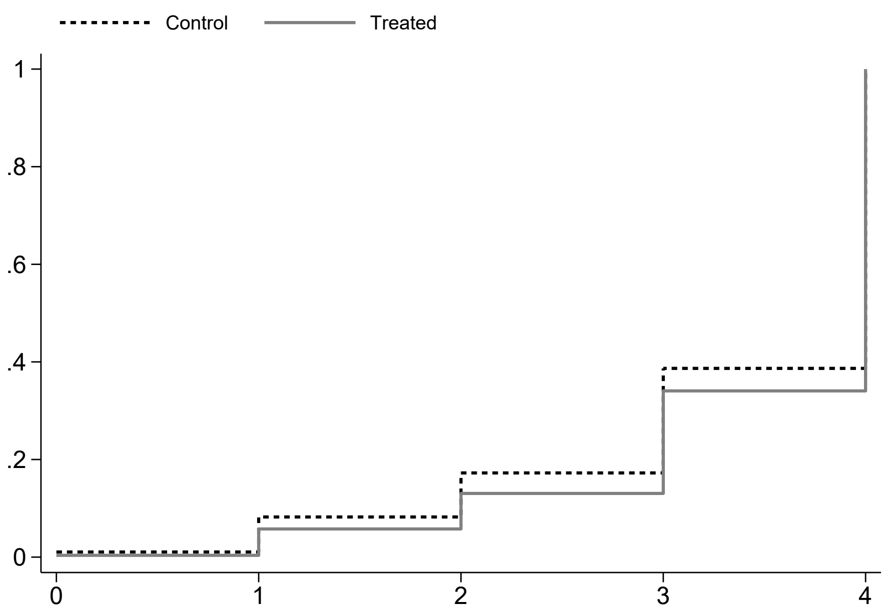
Notes. ITT estimates based on OLS regressions using equation (1) in columns (2),(3),(5) and (6) and TT estimates in columns (4) and (7) based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Dependent variables by row: (1) *Postnatal checkup for mother and newborn*, indicator variable equal to 1 if mother and newborn each had at least one postnatal checkup, and 0 otherwise; (2) *Quality postnatal checkup for mother*, indicator variable equal to 1 if postnatal checkup for mother included counseling on what to do if mother has danger signs (bleeding, fever, etc.), family planning methods, breastfeeding, and newborn care (danger signs, feeding, etc.). All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also include municipality fixed effects and controls for the following characteristics of women: being indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. The p-values are presented in brackets. The first row shows values from individual testing, the second adjusts for testing that treatment is jointly different from zero for all outcomes presented in the table, per specification, and the third shows the Anderson-Rubin p-values, robust to weak instruments.

Figure 2: Components of quality ANC and PNC, by treatment group

Panel A. Quality antenatal care



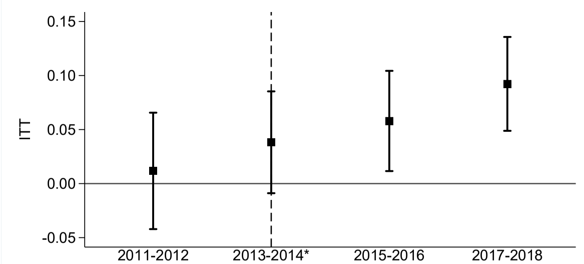
Panel B. Quality postnatal care



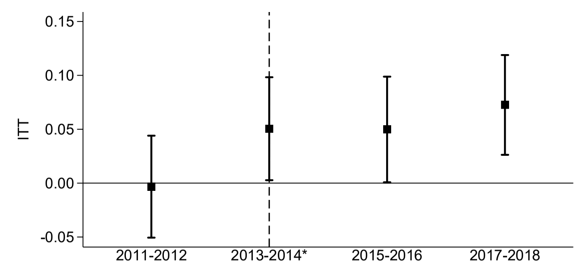
Notes. The figure shows the empirical cumulative distribution functions of the number of components included in quality ANC (Panel A) and PNC (Panel B), distinguishing between control and treatment group. The p-value of a Kolmogorov–Smirnov test of equality of distributions is equal to 0.002 for Panel A, and 0.009 for Panel B. Appendix B presents additional details about the variables.

Figure 3: Estimates by cohort

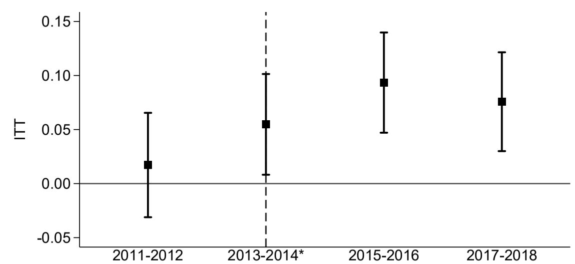
A. Quality ANC



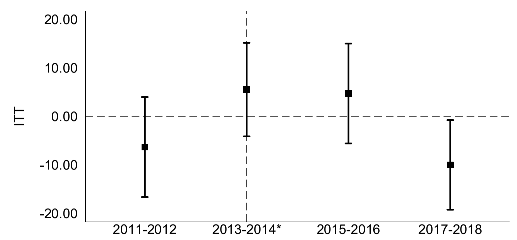
B. Institutional delivery



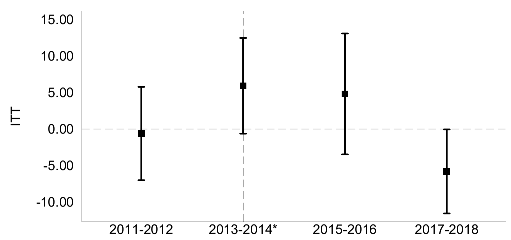
C. Quality postnatal for mother



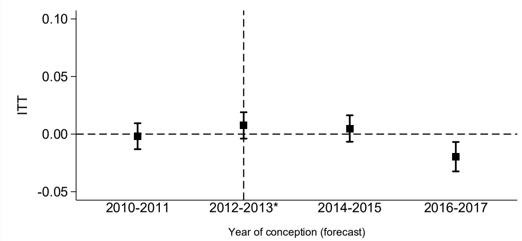
D. Infant mortality



E. Neonatal mortality



F. Fertility (per 1,000 women)



Notes. ITT estimates based on pregnancy-level OLS regressions using equation (1) separately for each two-year cohort. Period 2011–2012 indicates *pre-intervention* pregnancies. * The pregnancies within the vertical dashed lines are the *mid-intervention* pregnancies, which were partially affected by the intervention that started in June 2013. All subsequent periods (to the right of the vertical dashed line) are the *intervention* pregnancies. Mortality and fertility indicators are multiplied by 1,000. Confidence intervals are computed at the 95% level of confidence using standard errors clustered at the community level. All specifications include indicator variables for cohort and municipality fixed effects.

Table 4: Heterogeneous effects by travel time to closest childbirth center, intervention cohorts

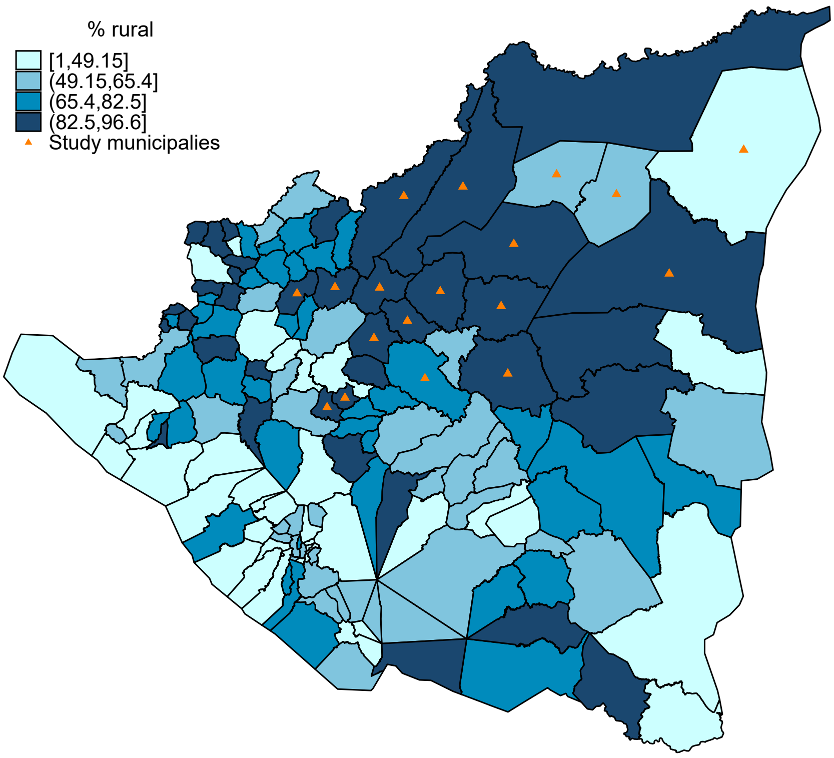
	Close			Far				Interaction			
	β (1)	se (2)	N (3)	β (4)	se (5)	N (6)	p-value (1)–(4) (7)	β (8)	se (9)	N (10)	p-value $t \times h$ (11)
Antenatal Care											
ANC by skilled staff	0.02	0.01	3488	0.04*	0.02	3889	0.59	0.03	0.02	7377	0.23
Quality ANC	0.05**	0.02	3488	0.07**	0.03	3889	0.84	0.02	0.04	7377	0.56
ANC by skilled staff in first trimester	-0.01	0.02	3488	0.01	0.03	3889	0.78	0.02	0.04	7377	0.51
Delivery											
Institutional delivery	0.02	0.02	3488	0.08**	0.04	3889	0.54	0.07*	0.04	7377	0.08
C-section	-0.02	0.02	3488	0.00	0.01	3889	0.60	0.03	0.02	7377	0.16
Postnatal care											
Postnatal checkup for mother and newborn	0.04*	0.02	3484	0.08*	0.04	3881	0.69	0.05	0.05	7365	0.28
Quality postnatal checkup for mother	0.06***	0.02	3484	0.11***	0.03	3881	0.58	0.04	0.04	7365	0.29
Maternal and child heath											
Neonatal mortality (per 1000 live births)	1.83	2.56	3483	-0.86	3.30	3891	0.59	-3.29	4.19	7374	0.43
Infant mortality (per 1000 live births)	1.78	3.62	3483	-3.51	4.55	3891	0.47	-7.65	6.06	7374	0.21
Fertility (per 1,000 women)	3.70	4.33	16555	-13.69**	6.45	17260	0.13	-16.25**	8.17	33815	0.05

Notes. The category for heterogeneity analysis was defined during the pre-intervention data collection, with *close* (*far*) indicating whether the travel time from the community health unit to the delivery health unit is smaller than or equal to (*larger than*) the sample median. The median travel time is four hours. In columns (1)–(6), ITT estimates are based on OLS regressions for each category in the post-treatment period. Column (7) presents the p-value of a t-test of the difference in estimated effects across those who are "close" (presented in column (1)) and "far" (presented in column (4)). Column (8) presents the interaction term between the treatment indicator t and an indicator variable for the far. The p-value in column (11) corresponds to the coefficient on the interaction term $t \times h$. All specifications include indicator variables for cohort and municipality fixed effects. Standard errors are clustered by community. The dependent variables are indicated in the rows. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX

I. Study location

Figure A1: Rural population share by municipality and study municipalities



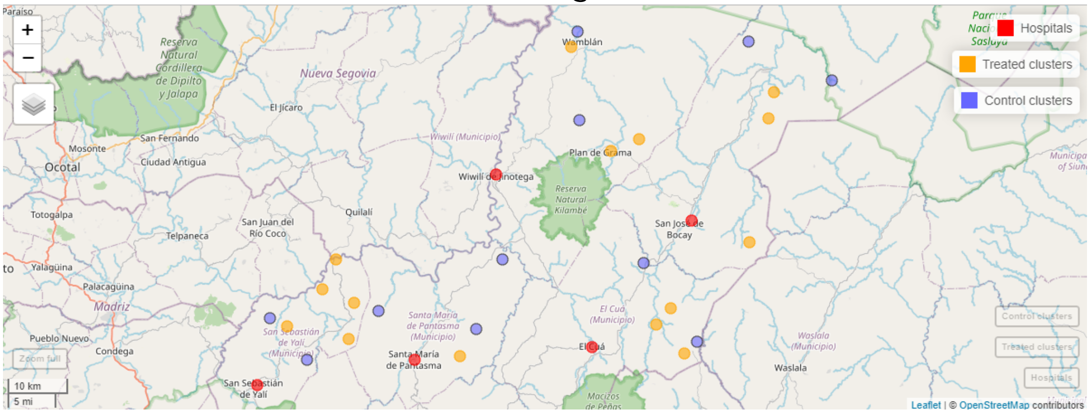
Notes. This map of Nicaragua shows the rural population as a percent of total population in all municipalities.

Table A1: Communities and clusters by municipality and treatment assignment

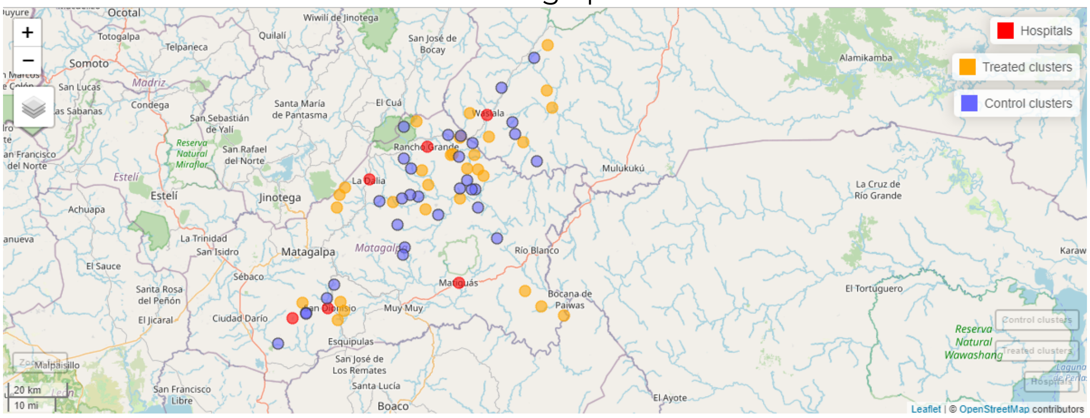
		Control		Treatment		Total	
Region	**Municipality**	**Clusters**	**Communities**	**Clusters**	**Communities**	**Clusters**	**Communities**
Bilwi	Prinzapolka	3	2	4	2	7	4
Bilwi	Puerto Cabezas	5	2	8	3	13	5
Jinotega	El Cua	2	2	5	3	7	5
Jinotega	Pantasma	6	3	7	2	13	5
Jinotega	San José d Bocay	3	2	8	3	11	5
Jinotega	San Sebastian de Yali	4	2	10	5	14	7
Jinotega	Wiwili	2	2	6	3	8	5
Matagalpa	Matiguas	6	5	5	5	11	10
Matagalpa	Rancho Grande	10	9	9	8	19	17
Matagalpa	San Dionisio	3	3	3	3	6	6
Matagalpa	Terrabona	9	3	4	2	13	5
Matagalpa	Tuma la Dalia	15	8	10	5	25	13
Matagalpa	Waslala	6	5	7	7	13	12
Minas	Bonanza	4	3	6	4	10	7
Minas	Mulukuku	9	7	6	3	15	10
Minas	Paiwas	15	6	13	6	28	12
Minas	Rosita	12	5	15	8	27	13
Minas	Siuna	14	7	9	4	23	11
	Total	128	76	135	76	263	152

Figure A2: Geographical location of treatment and control clusters and reference childbirth facilities, by region

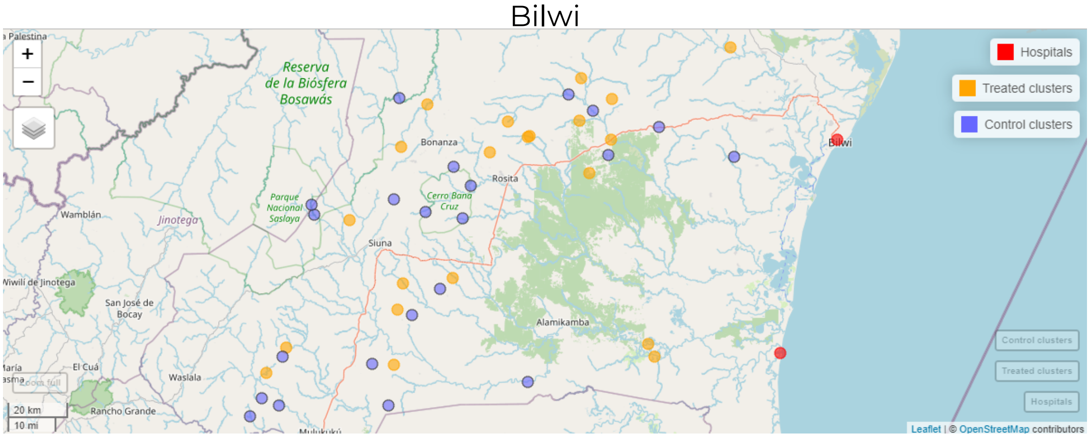
Jinotega



Matagalpa



Bilwi



Las Minas

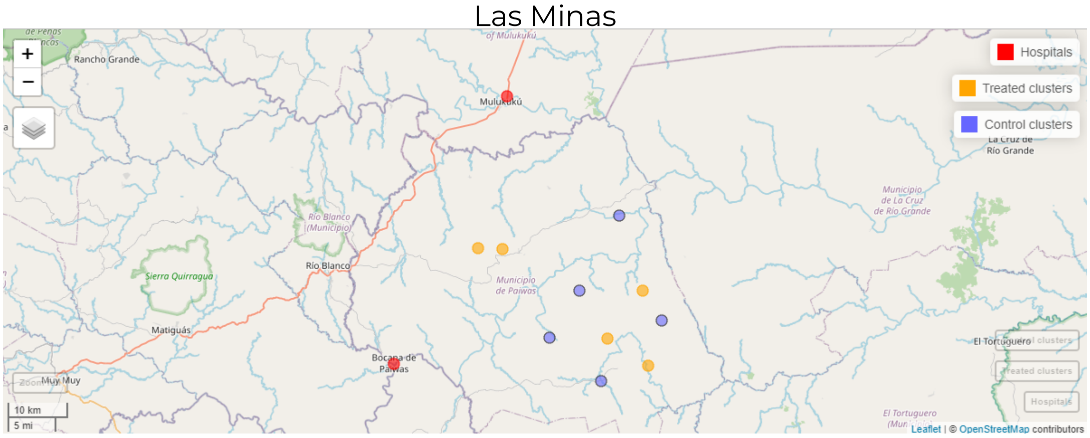
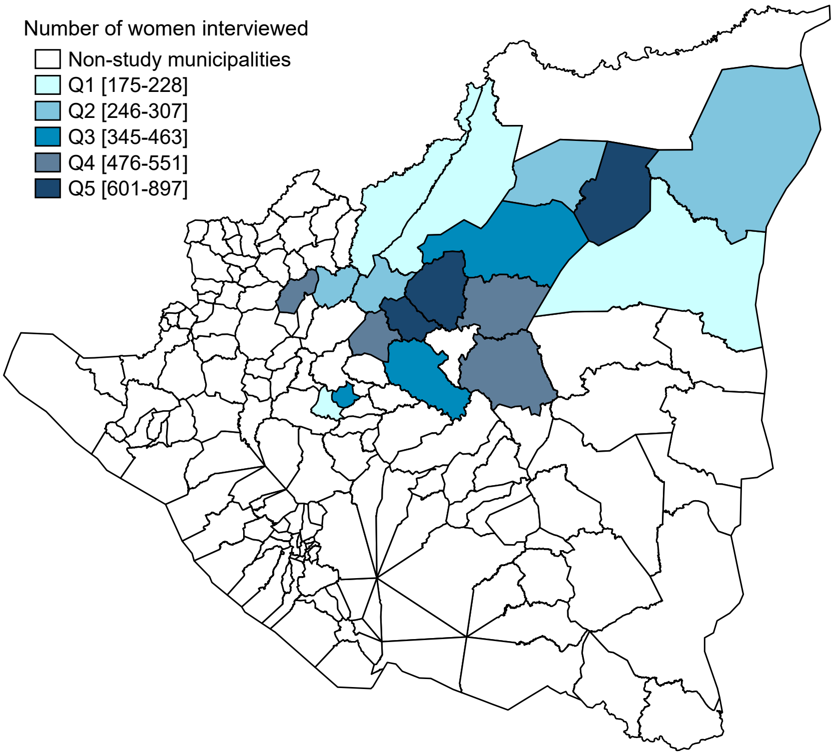
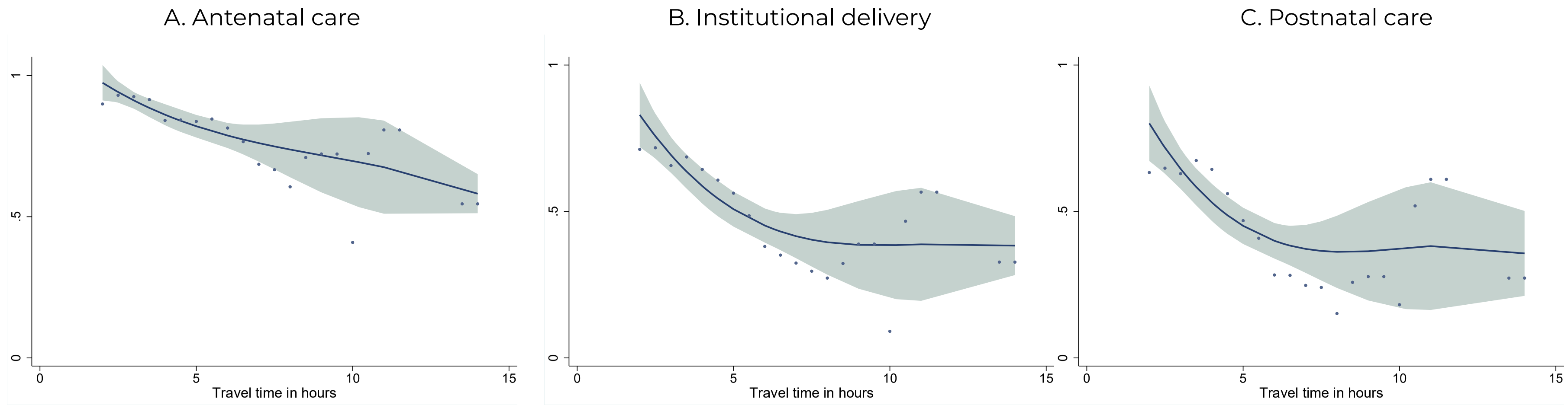


Figure A3: Women interviewed in endline survey, by municipality



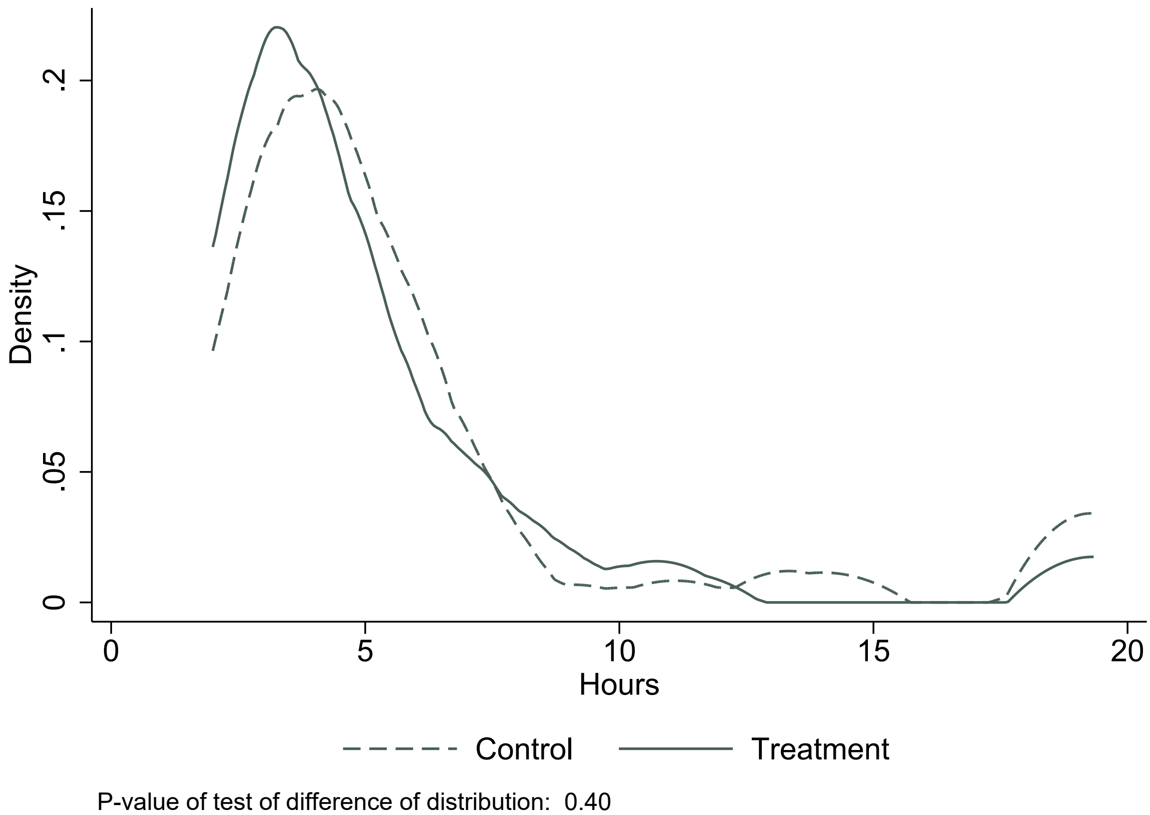
Notes. This map of Nicaragua shows the total number of women interviewed in the 18 municipalities included in the study.

Figure A4: Maternal health care by distance to clinic (control, pre-intervention)



Notes. Sample restricted to CCs allocated to the control group and for pregnancies reported as having occurred during the pre-intervention period (2011–2012).

Figure A5: Travel time in hours from community clusters to nearest childbirth facility, by treatment assignment



Group	N	p10	p25	p50	p75	p90
Treatment	76	2.0	3.0	4.0	5.7	8.1
Control	76	2.5	3.4	4.5	6.2	14.0
Total	152	2.2	3.0	4.3	6.0	10.0

Notes. Survey teams collected travel time in hours in the baseline. Averages by cluster and atypical values were adjusted by winsorizing the 5% of observations. Missing values were replaced with the travel time in hours reported by the Ministry of Health (Minsa). Observations that are still missing were replaced with municipal averages.

II. Definition of variables

Table B2: Definition of outcome variables

Variable	Description
ANC by skilled staff*	Indicator variable equal to 1 if women attended at least one antenatal care (ANC) checkup provided by skilled staff (doctor or nurse) during pregnancy, and 0 otherwise.
Quality ANC*	Indicator variable equal to 1 if during pregnancy women attended at least one antenatal care (ANC) checkup at which a blood and urine sample was collected and tested, blood pressure and anthropometrics were measured, and the tetanus vaccine was administered.
ANC by skilled staff in first trimester*	Indicator variable equal to 1 if during pregnancy women attended their first ANC checkup during the first trimester and had at least one ANC by skilled staff (doctor or nurse), and 0 otherwise.
Institutional delivery*	Indicator variable equal to 1 if women gave birth in a health center, as opposed to at home, and 0 otherwise.
C-section	Indicator variable equal to 1 if delivery was through c-section, and 0 otherwise.
Postnatal checkup for mother and newborn*	Indicator variable equal to 1 if mother and newborn had each at least one postnatal checkup, and 0 otherwise.
Quality postnatal checkup for mother*	Indicator variable equal to 1 if postnatal checkup for mother included counseling on what to do if the mother has danger signs (e.g. bleeding, fever), family planning methods, breastfeeding, and newborn care (e.g. danger signs, feeding).
Continuity of care	Indicator variable equal to 1 if women had quality ANC, institutional delivery, and quality postnatal checkup for mother, and 0 otherwise.
Infant mortality	Indicator variable equal to 1 if the child was born alive and died within the first year of life. Indicator multiplied by 1,000.
Neonatal mortality	Indicator variable equal to 1 if the child was born alive and died within the first 28 days of life. Indicator multiplied by 1,000.
Fertility	Indicator variable equal to 1 if the women in the study was pregnant in the relevant period. Indicator multiplied by 1,000.

III. Balance in observable characteristics and replacements

Table C1: Balance table for characteristics of community clusters

CC characteristic	Control		Treatment		T-C
	Nc	Mean	Nc	Mean	(p-value)
Population 2012	76	407.76	76	398.07	0.85
Population 2013	76	456.54	76	426.48	0.56
Pregnancies 2012	76	16.35	76	15.43	0.61
Pregnancies 2013	76	16.27	76	15.11	0.44
Travel time in hours - reported by MoH	76	5.56	76	5.20	0.34
Travel time in hours-collected by survey teams	76	5.94	76	4.97	0.15
Size of CC	76	1.68	76	1.78	0.47

Notes. The p-value comes from a two-tailed hypothesis test. Nc is the number of community clusters.

Table C2: Balance table for pre-intervention cohort (2011–2012)

	Control			Treatment				
	Ni	Nc	Mean	Ni	Nc	Mean	Coef.	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Characteristics of women								
Age at pregnancy of first child	1117	74	18.01	1201	75	18.19	0.17	0.24
Woman was a teenager mother	1117	74	0.75	1201	75	0.74	-0.01	0.49
Pregnancies per women	1117	74	4.03	1201	75	4.12	0.09	0.39
Indigenous	1095	74	0.07	1183	75	0.06	-0.01	0.24
No education or incomplete primary	1107	74	0.72	1190	75	0.75	0.03	0.17
Married or in cohabitation	1117	74	0.89	1201	75	0.89	-0.00	0.82
HH characteristics								
HH head is female	1095	74	0.14	1183	75	0.13	-0.01	0.63
Dwelling Index (0–6)	1118	74	1.81	1201	75	1.79	-0.02	0.71
Asset Index (0–10)	1117	74	1.63	1201	75	1.65	0.02	0.72

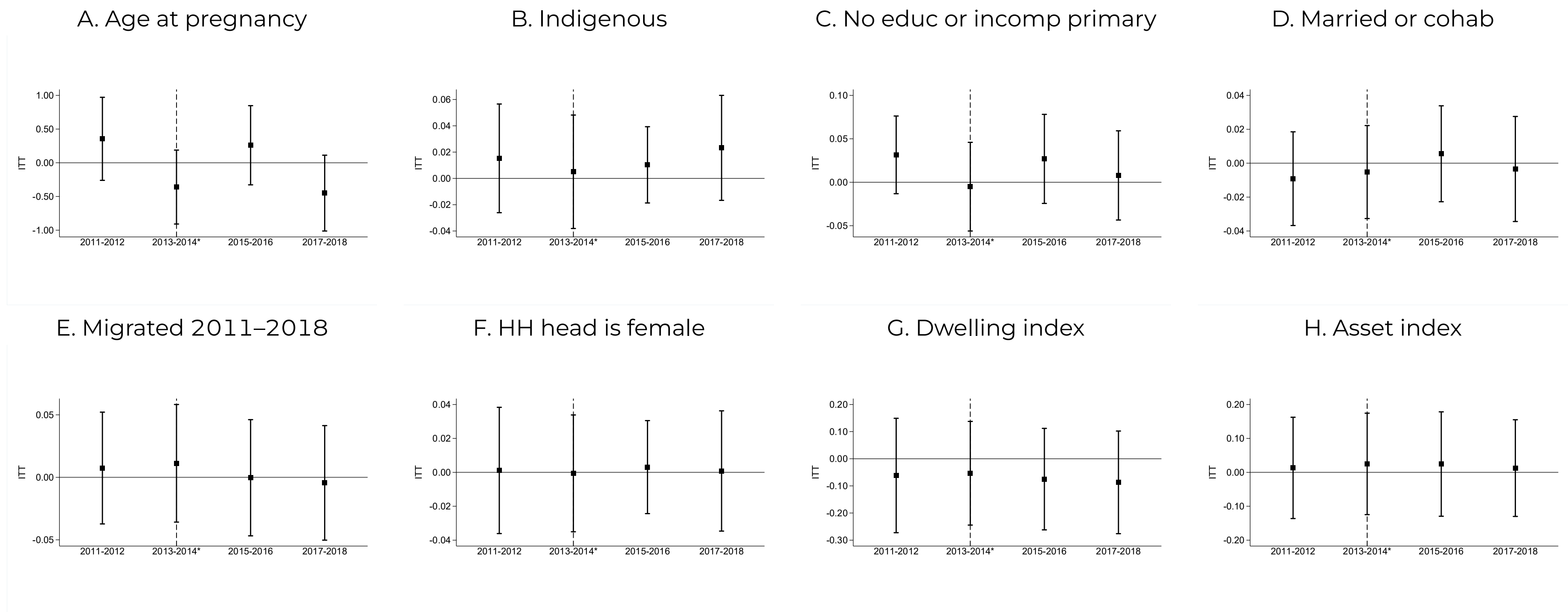
Notes. Columns (1)–(4) present the number of women and (2)–(5) the number of communities in the control and treatment groups, respectively. Columns (3) and (6) report the sample mean for control and treatment groups, respectively. Column (7) reports the treatment group's difference from the control group, estimated using OLS with robust standard errors. Column (8) presents a joint test of significance of the coefficients for each treatment dummy. Statistical significance denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table C3: Balance table for intervention cohort (2013–2018)

	Control			Treatment				
	Ni	**Nc**	**Mean**	**Ni**	**Nc**	**Mean**	**Coef.**	**P-value**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Characteristics of women								
Age at pregnancy of first child	2785	74	18.33	2940	76	18.23	-0.10	0.27
Woman was a teenager mother	2785	74	0.72	2940	76	0.72	0.00	0.82
Pregnancies per women	2787	74	2.99	2941	76	3.01	0.02	0.73
Indigenous	2713	74	0.06	2868	76	0.05	-0.00	0.47
No education or incomplete primary	2771	74	0.63	2927	76	0.65	0.02	0.10
Married or in cohabitation	2787	74	0.86	2941	76	0.87	0.00	0.79
HH characteristics								
HH head is female	2713	74	0.14	2868	76	0.13	-0.00	0.59
Dwelling Index (0–6)	2787	74	1.84	2942	76	1.79	-0.04	0.17
Asset Index (0–10)	2787	74	1.68	2941	76	1.68	0.00	0.93

Notes. Columns (1)–(4) present the number of women and (2)–(5) the number of communities in the control and treatment groups, respectively. Columns (3) and (6) report sample mean for control and treatment groups, respectively. Column (7) reports the treatment group's difference from the control group, estimated using OLS with robust standard errors. Column (8) presents a joint test of significance of the coefficients for each treatment dummy. Statistical significance denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure C1: Balance by cohort



Notes. Same notes as Figure 3.

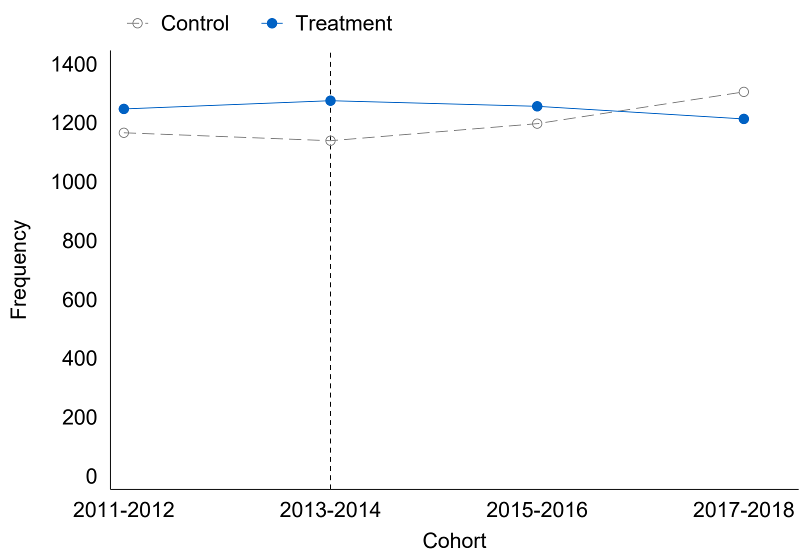
Table C4: Transfer to and stay in maternal waiting homes, pre-intervention

	Total		Treat		Control		T-C
	N	**Mean**	**N**	**Mean**	**N**	**Mean**	**P-value**
Used a maternal waiting home	2410	0.24	1244	0.26	1166	0.22	0.21
Descriptives of maternal waiting home use							
Transportation to maternal waiting home							
On foot	584	0.09	323	0.10	261	0.08	0.65
Private vehicle	584	0.05	323	0.05	261	0.05	0.87
Ambulance	584	0.17	323	0.15	261	0.19	0.39
Public transportation	584	0.63	323	0.64	261	0.62	0.81
Boat or skiff	584	0.02	323	0.02	261	0.02	0.67
Beast	584	0.03	323	0.04	261	0.03	0.56
Other	584	0.01	323	0.01	261	0.01	0.83
Companion during transportation to maternal waiting home							
Husband or parents (inc. in-law)	586	0.53	325	0.55	261	0.50	0.29
Children	586	0.06	325	0.06	261	0.08	0.32
Other relative	586	0.05	325	0.05	261	0.06	0.47
No relative	586	0.02	325	0.02	261	0.02	0.91
Midwife	586	0.00	325	0.00	261	0.01	0.14
Community worker / brigadier	586	0.01	325	0.01	261	0.00	0.68
Other	586	0.02	325	0.02	261	0.02	0.83
Provided at maternal waiting home							
Received baby items	586	0.10	325	0.11	261	0.09	0.54
Received information	586	0.79	325	0.80	261	0.77	0.43
Satisfaction with meals at maternal waiting home							
Very dissatisfied	581	0.02	324	0.02	257	0.02	0.94
Dissatisfied	581	0.05	324	0.05	257	0.05	0.94
Neither dissatisfied nor satisfied	581	0.07	324	0.06	257	0.09	0.26
Satisfied	581	0.69	324	0.71	257	0.68	0.48
Very satisfied	581	0.17	324	0.17	257	0.17	0.98
Satisfaction with cleanliness at maternal waiting home							
Very dissatisfied	585	0.01	325	0.02	260	0.01	0.21
Dissatisfied	585	0.03	325	0.03	260	0.02	0.73
Neither dissatisfied nor satisfied	585	0.04	325	0.03	260	0.05	0.18
Satisfied	585	0.77	325	0.78	260	0.76	0.73
Very satisfied	585	0.15	325	0.15	260	0.16	0.85

Table C5: Replacement communities

	Replacement
Treatment	-0.03
	(0.04)
N	150
Replacement in treatment(%)	5.26%
Replacement in control (%)	8.1%

Figure C2: Sample by cohort



Notes. Number of children born alive in each cohort, for treatment and control groups.

IV. Treatment fidelity

Table D1: Treatment take-up by cohort

	2013–2018		2013–2014		2015–2016		2017–2018	
Treatment	**Received**	**Used**	**Received**	**Used**	**Received**	**Used**	**Received**	**Used**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any voucher	23.6	92.8	20.1	95.0	28.5	93.7	22.3	89.7
Transportation voucher for ANC	20.6	90.6	16.7	93.5	25.4	92.4	19.6	85.8
Transportation voucher for delivery	13.4	92.6	11.7	95.0	17.5	92.8	11.2	90.0
Maternal waiting home voucher	12.2	99.1	9.6	99.1	15.2	99.2	11.6	99.0
N	7376		2407		2453		2516	

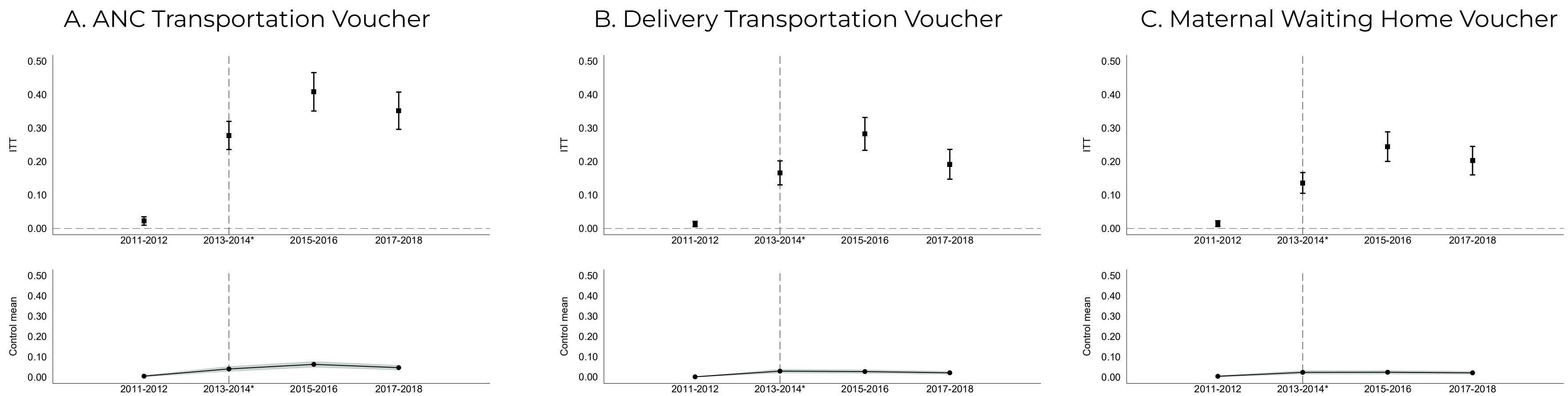
Notes. Percentage of women who received vouchers for their children in columns (1), (3), (5) and (7). Percentage of women who used vouchers for their children over the women who received them in columns (2), (4), (6) and (8).

Table D2: Determinants of voucher distribution (all 2013–2018 cohorts)

	No voucher			Any voucher				
	Ni	**Nc**	**Mean**	**Ni**	**Nc**	**Mean**	**Coef.**	**P-value**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Woman age at pregnancy: <18 years	5633	150	0.15	1743	118	0.16	0.01	0.26
Woman age at pregnancy: 18-40	5633	150	0.83	1743	118	0.82	-0.01	0.60
Woman age at pregnancy: 40+	5633	150	0.02	1743	118	0.02	-0.01	0.13
Number of previous pregnancies before 2013	5633	150	1.20	1743	118	0.99	-0.21	0.00
Any abortion before 2013	5633	150	0.04	1743	118	0.03	-0.01	0.18
Any stillbirth before 2013	5633	150	0.01	1743	118	0.01	-0.00	0.24
No education or incomplete primary	5596	150	0.67	1735	118	0.62	-0.05	0.00
Complete primary	5596	150	0.15	1735	118	0.18	0.03	0.00
Incomplete secondary	5596	150	0.11	1735	118	0.13	0.02	0.04
Complete secondary	5596	150	0.04	1735	118	0.04	0.00	0.86
Tertiary	5596	150	0.02	1735	118	0.02	-0.00	0.27
Married or in cohabitation	5627	150	0.87	1743	118	0.89	0.02	0.03
Indigenous	5478	150	0.05	1702	118	0.06	0.01	0.08
Dwelling Index (0–6)	5633	150	1.72	1743	118	1.91	0.19	0.00
Asset Index (0–10)	5627	150	1.61	1743	118	1.65	0.04	0.14
Travel time in hours to nearest childbirth facility from community clusters	5633	150	5.16	1743	118	4.63	-0.53	0.00

Notes. Columns (1)–(4) present the number of women and (2)–(5) the number of communities in the control and treatment groups, respectively. Columns (3) and (6) report sample mean for control and treatment groups, respectively. In this table, treatment is defined as having received any voucher, and control is not having received any voucher. Column (7) reports the treatment group's difference from the control group, estimated using OLS with robust standard errors. Column (8) presents a joint test of significance of the coefficients for each treatment dummy. Statistical significance denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure D1: Received vouchers



Notes. ITT estimates based on women-level OLS regressions using equation (1) separately for each two-year cohort. Period 2011–2012 indicates *pre-intervention* pregnancies. The pregnancies within the vertical dashed lines are the *mid-intervention* pregnancies, which were partially affected by the intervention that started in June 2013. All subsequent periods (to the right of the vertical dashed line) are the *intervention* births. Confidence intervals are computed at the 95% level of confidence using standard errors clustered at the community level. All specifications include indicator variables for cohort and municipality fixed effects.

Table D3: Social support in transfer to maternal waiting homes

Outcome	Control mean	No Controls: ITT Pre 2011–2012	No Controls: ITT Intervention 2013–2018	No Controls: IV Intervention 2013–2018	With Controls: ITT Pre 2011–2012	With Controls: ITT Intervention 2013–2018	With Controls: IV Intervention 2013–2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Presence of companion	0.70	0.02	0.07	0.12	0.05	0.05	0.09
		(0.06)	(0.03)	(0.06)	(0.05)	(0.02)	(0.04)
p-value		[0.76]	[0.06]	[0.04]	[0.40]	[0.07]	[0.03]
Adjusted p-value		[0.86]	[0.08]		[0.48]	[0.09]	
AR p-value				[0.04]			[0.04]
Observations		586	3,110	3,110	586	3,110	3,110
Communities		135	149	149	135	149	149
Obs. imputated					20	94	94
Number of companions	0.78	-0.02	0.08	0.14	0.01	0.05	0.10
		(0.07)	(0.04)	(0.07)	(0.06)	(0.03)	(0.05)
p-value		[0.84]	[0.07]	[0.05]	[0.83]	[0.08]	[0.05]
Adjusted p-value		[0.84]	[0.07]		[0.83]	[0.08]	
AR p-value				[0.05]			[0.06]
Observations		585	3,105	3,105	585	3,105	3,105
Communities		135	149	149	135	149	149
Obs. imputated					20	94	94

Notes. ITT estimates based on OLS regressions using equation (1) in columns (2),(3),(5) and (6), and TT estimates in columns (4) and (7) based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Panel A shows data on transfers to maternal waiting homes, Dependent variables by rows: (1) *Presence of companion*, indicator variable equal to 1 if woman had anybody accompanying her, and 0 otherwise; (2) *Number of companions*, number of people who accompanied the woman. All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also include municipality fixed effects and controls for the following women characteristics of women: is indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. The p-values are presented in brackets. The first row shows values from individual testing, the second adjusts for testing that treatment is jointly different from zero for all outcomes presented in the table, per specification, and the third shows the Anderson-Rubin p-values, robust to weak instruments. The lower sample is due to non-responses to the item about companions during transfer.

V. Additional analysis

Table E1: Components of quality antenatal care

		No Controls			With Controls		
		ITT		IV	ITT		IV
Outcome	**Control mean**	**Pre 2011–2012**	**Intervention 2013–2018**	**Intervention 2013–2018**	**Pre 2011–2012**	**Intervention 2013–2018**	**Intervention 2013–2018**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Urine test	0.78	0.01	0.08**	0.21***	0.01	0.06***	0.18***
		(0.04)	(0.03)	(0.08)	(0.02)	(0.01)	(0.04)
AR p-value				0.01			0.00
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237
Blood test	0.77	0.02	0.08***	0.23***	0.02	0.07***	0.20***
		(0.04)	(0.03)	(0.08)	(0.02)	(0.02)	(0.04)
AR p-value				0.01			0.00
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237
Blood pressure	0.85	0.00	0.05**	0.14**	-0.00	0.04***	0.10***
		(0.03)	(0.02)	(0.06)	(0.02)	(0.01)	(0.03)
AR p-value				0.05			0.00
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237
Anthropometrics	0.86	-0.00	0.05**	0.14**	-0.01	0.04***	0.11***
		(0.03)	(0.02)	(0.06)	(0.02)	(0.01)	(0.03)
AR p-value				0.04			0.00
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237
Tetanus vaccine	0.79	0.00	0.05*	0.14*	0.00	0.04***	0.11***
		(0.04)	(0.03)	(0.07)	(0.02)	(0.01)	(0.04)
AR p-value				0.07			0.00
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputated					67	237	237

Notes. ITT estimates based on OLS regressions using equation (1) in columns (2), (3), (5) and (6) and TT estimates in columns (4) and (7) based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Dependent variables by row: (1) *Urine test*, indicator variable equal to 1 if women had at least one ANC checkup that included a urine test; (2) *Blood test*, indicator variable equal to 1 if women had at least one ANC checkup that included a blood test; (3) *Blood pressure*, indicator variable equal to 1 if women had at least one ANC checkup with blood pressure measured; (4) *Anthropometrics*, indicator variable equal to 1 if women had at least one ANC checkup with weight and height measured; (5) *Tetanus vaccine*, indicator variable equal to 1 if women had at least one ANC checkup with tetanus vaccine administered. All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also include municipality fixed effects and controls for the following women characteristics of women: is indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. Statistical significance denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E2: Continuity of care

		No Controls			With Controls		
		ITT		IV	ITT		IV
Outcome		**Pre**	**Intervention**	**Intervention**	**Pre**	**Intervention**	**Intervention**
	Control mean	**2011–2012**	**2013–2018**	**2013–2018**	**2011–2012**	**2013–2018**	**2013–2018**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Continuity of care	0.34	0.03	0.08	0.23	0.03	0.08	0.22
		(0.04)	(0.04)	(0.10)	(0.02)	(0.02)	(0.04)
p-value		[0.52]	[0.04]	[0.02]	[0.19]	[0.00]	[0.00]
AR p-value				[0.03]			[0.00]
Observations		2,409	7,377	7,376	2,409	7,377	7,376
Communities		149	150	150	149	150	150
Obs. imputed					67	237	237

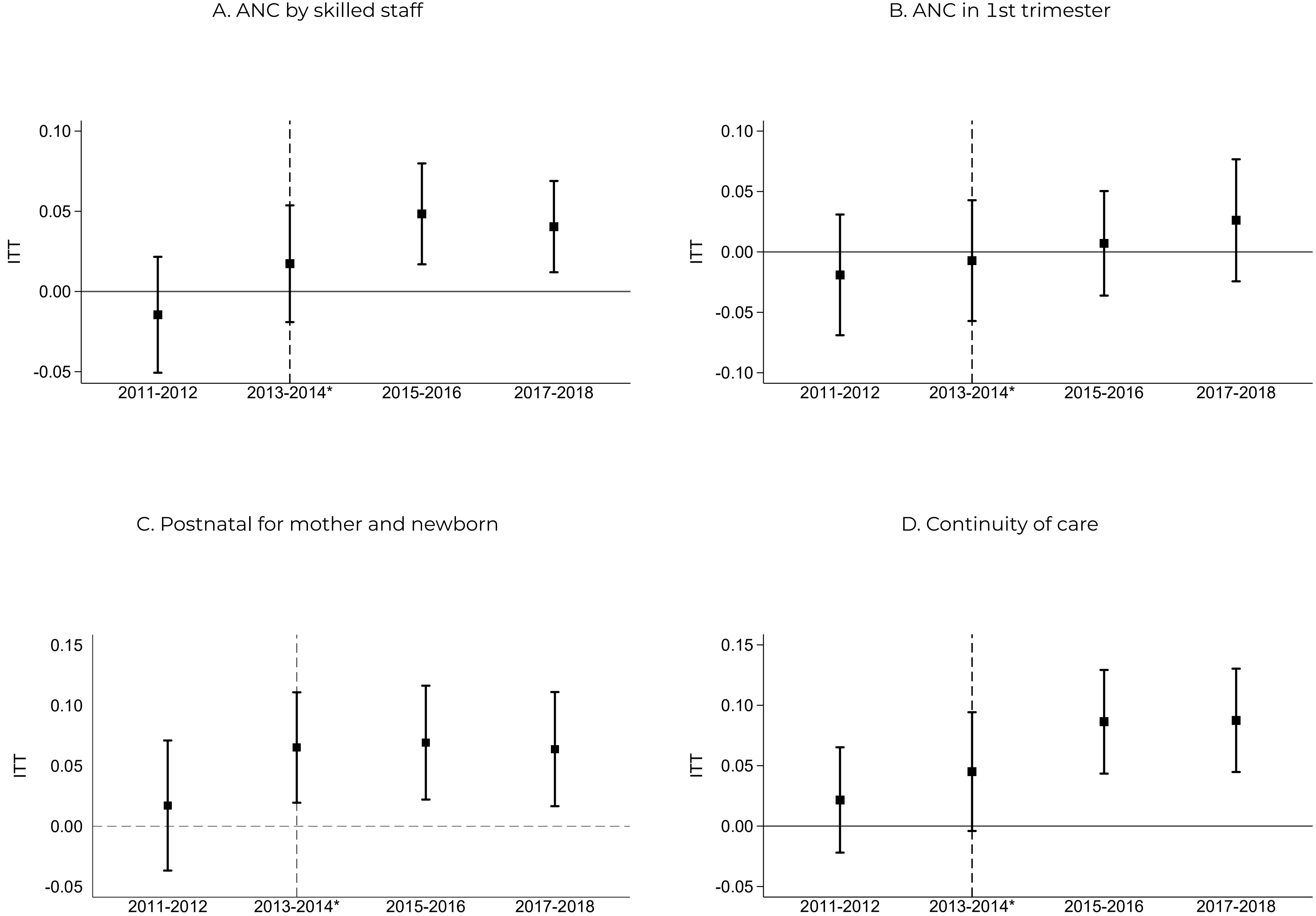
Notes. ITT estimates based on OLS regressions using equation (1) in columns (2), (3), (5) and (6) and TT estimates in columns (4) and (7) based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Dependent variable: *Continuity of care*, indicator variable equal to 1 if women had quality antenatal care, institutional delivery, and quality postnatal care. All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also include municipality fixed effects and controls for the following characteristics of women: is indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. The p-values are presented in brackets. The first row shows values from individual testing, while the second row contains the Anderson-Rubin p-values, robust to weak instruments. The lower sample is due to non-responses to the item about companions during transfer.

Table E3: Heckman selection model for quality variables

Variable	**Coef.**
Panel A	
Quality ANC	0.05**
	(0.02)
N	7,377
Panel B	
Quality postnatal checkup for mother	0.05
	(0.03)
N	7,365

*Notes. Heckman selection estimates of the impact of voucher assignment on the quality of antenatal (Panel A) and postnatal care (Panel B), as reported by women. Analysis restricted to the intervention period. All estimations control for birth cohort fixed effects. Standard errors clustered by community are reported in parentheses. Statistical significance denoted by *** p<0.01, ** p<0.05, * p<0.1.*

Figure E1: Estimates by cohort, additional outcomes



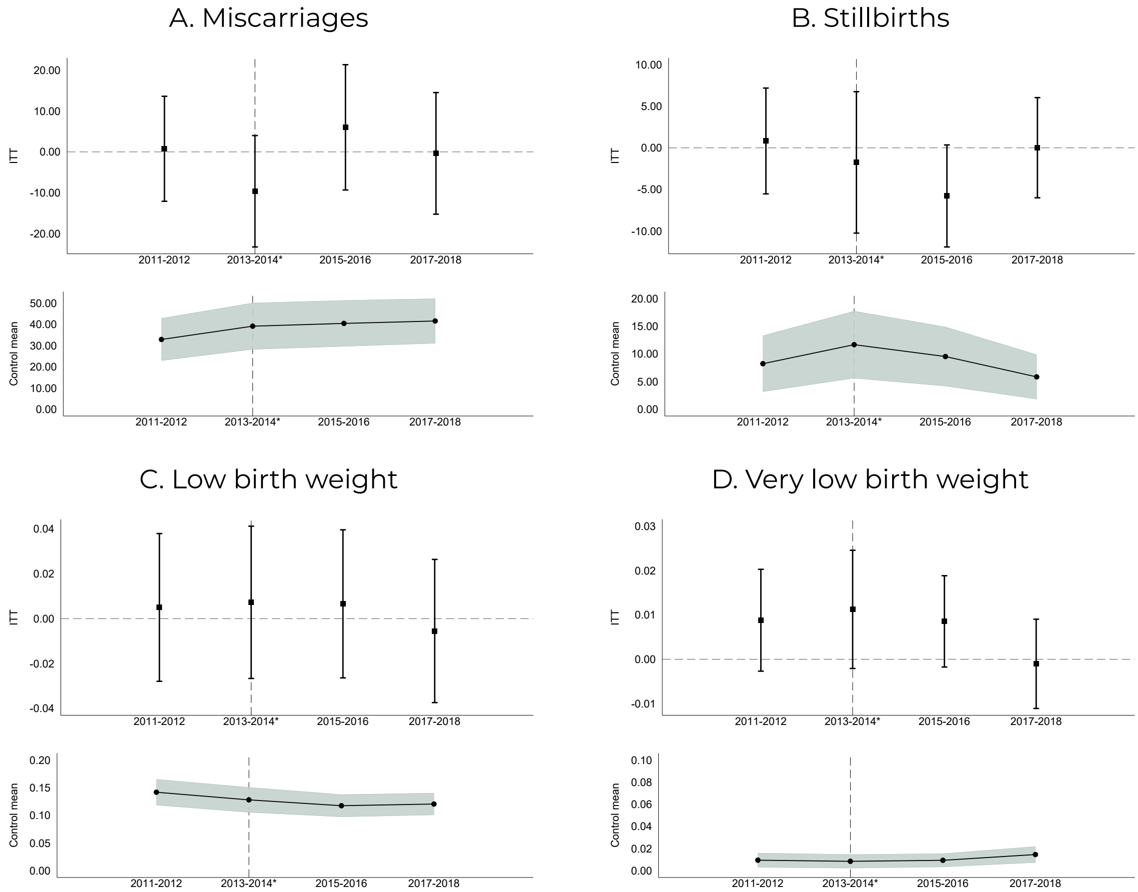
Notes. ITT estimates based on pregnancy-level OLS regressions using equation (1) separately for each two-year cohort. Period 2011–2012 indicates *pre-intervention* pregnancies. The pregnancies in the vertical lines is the *mid-intervention* pregnancies, those that were partially affected by the intervention that started in June 2013. All subsequent periods (to the right of the vertical dashed line) are the *intervention* pregnancies. Confidence intervals are computed at the 95% level of confidence using standard errors clustered at the community level. All specifications include indicator variables for cohort and municipality fixed effects. * The 2013–2014 cohort is partially treated, as the intervention was started in June 2013.

Table E4: Infant mortality and fertility

Outcome	Control mean	No Controls ITT Pre 2011–2012	No Controls ITT Intervention 2013–2018	No Controls IV Intervention 2013–2018	With Controls ITT Pre 2011–2012	With Controls ITT Intervention 2013–2018	With Controls IV Intervention 2013–2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Infant mortality (per 1000 live births)	14.84	-6.15	-0.75	-3.51	-6.63	-0.28	-2.68
		(5.80)	(3.11)	(8.48)	(5.10)	(2.96)	(8.30)
p-value		[0.31]	[0.81]	[0.68]	[0.25]	[0.93]	[0.75]
AR p-value				[0.68]			[0.75]
Observations		2,409	7,374	7,365	2,409	7,374	7,365
Communities		149	150	150	149	150	150
Obs. imputated					67	236	236
Neonatal mortality (per 1000 live births)	6.87	-0.53	1.67	3.97	-0.60	1.56	3.07
		(3.90)	(2.09)	(5.77)	(3.46)	(2.12)	(5.83)
p-value		[0.89]	[0.43]	[0.49]	[0.87]	[0.50]	[0.60]
AR p-value				[0.49]			[0.60]
Observations		2,409	7,374	7,365	2,409	7,374	7,365
Communities		149	150	150	149	150	150
Obs. imputated					67	236	236
Fertility (per 1,000 women)	191.65	2.30	-6.40	-19.65	-1.40	-4.68	-13.57
		(6.32)	(5.15)	(14.78)	(5.05)	(3.76)	(11.04)
p-value		[0.72]	[0.22]	[0.18]	[0.79]	[0.25]	[0.22]
AR p-value				[0.19]			[0.21]
Observations		20,289	33,815	33,365	20,268	33,780	33,365
Communities		150	150	150	150	150	150
Obs. imputated					882	1470	1035

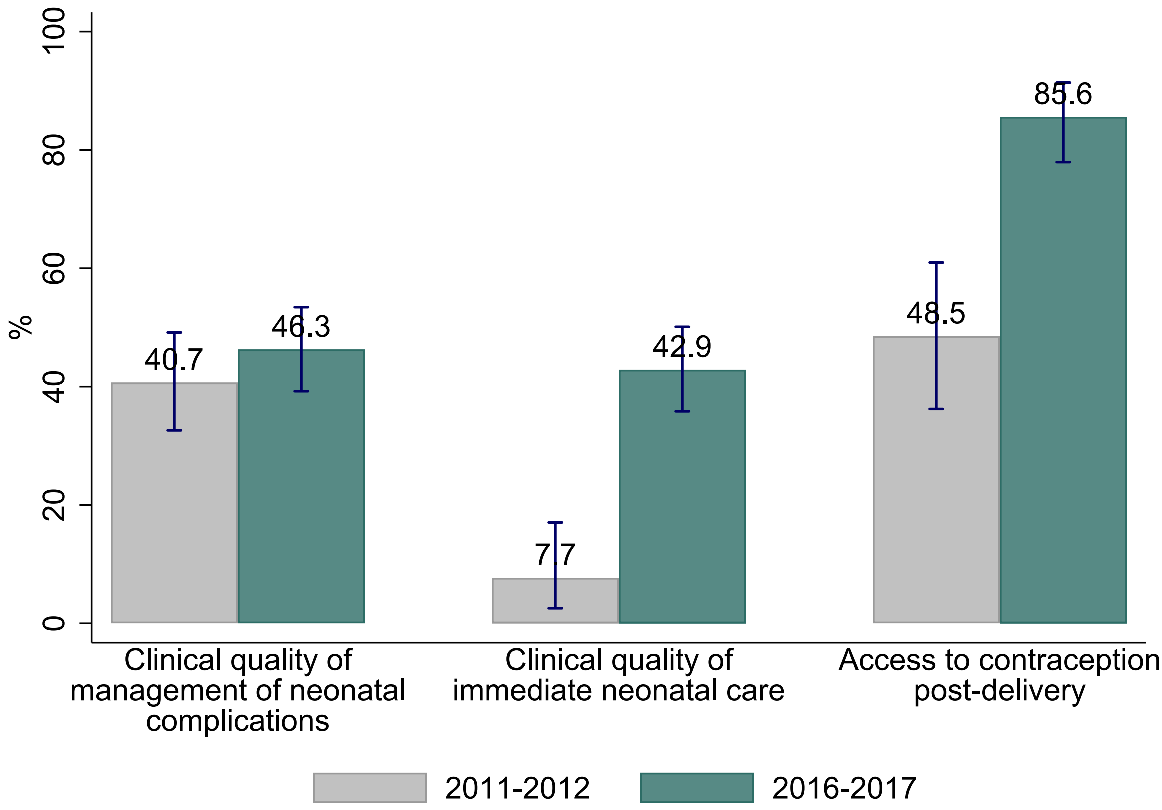
Notes. ITT estimates based on OLS regressions using equation (1) in columns (2), (3), (5) and (6) and TT estimates in columns (4) and (7), based on 2SLS regression of outcomes on "having received at least one voucher," instrumented using the treatment allocation at the community level. Standard errors clustered by community are reported in parentheses. Dependent variable by row: (1) Infant mortality is an indicator variable equal to 1 if the child was born alive and died within the first year of life; (2) neonatal mortality is an indicator variable equal to 1 if the child was born alive and died within the first 28 days of life; (3) Fertility is an indicator variable equal to 1 if the woman in the study was pregnant in those years. Mortality indicators are multiplied by 1,000. All specifications include indicator variables for cohort. Specifications in columns (5) to (7) also include municipality fixed effects and controls for the following characteristics of women: is indigenous, migrated between 2011 and 2018, is married, has no education or attained up to incomplete primary, age at pregnancy, female-headed household, and dwelling quality and access to public services. The p-values are presented in brackets. The first row shows values from individual testing, the second adjusts for testing that treatment is jointly different from zero for all outcomes presented in the table, per specification, and the third shows the Anderson-Rubin p-values, robust to weak instruments.

Figure E2: Effects on stillbirths, miscarriages, and birthweight



Notes. Dependent variables by panel: (A) miscarriages is an indicator equal 1 if a pregnancy ended in a miscarriage; (B) Stillbirths is an indicator equal 1 if a child was born dead; (C) Low birth weight is an indicator equal 1 if the birth weight is below 2.5 kgs; and (D) Very low birth weight is an indicator equal 1 if the birth weight is below 1.5 kgs. The outcomes in panels (A) and (B) are scaled by 1,000 pregnancies. ITT estimates based on pregnancy-level OLS regressions using equation (1) separately for each two-year cohort. Period 2011–2012 indicates *pre-intervention* pregnancies. The pregnancies within the vertical dashed lines are the *mid-intervention* pregnancies, which were partially affected by the intervention. * The 2013–2014 cohort is partially treated, since the intervention began in June 2013. All subsequent periods (to the right of the vertical dashed line) are the *intervention* pregnancies. Confidence intervals are computed at the 95% level of confidence using standard errors clustered at the community level. All specifications include indicator variables for cohort and municipality fixed effects. The lower panel shows the control mean across cohorts.

Figure E3: Improvement in quality of maternity health care



Notes. Mean (bars) and 95 % confidence intervals are presented for each indicator. The data is from external audits to a random sub-set of medical records at hospitals in SMI municipalities. 473 observations for "Clinical quality of management of neonatal complications," 350 for "Clinical quality of immediate neonatal care," and 192 for "Access to contraception post-delivery."

Table E5: Effects on utilization based on latest pregnancy

		No Controls			With Controls		
		ITT		IV	ITT		IV
Outcome	**Control mean**	**Pre 2011–2012**	**Intervention 2013–2018**	**Intervention 2013–2018**	**Pre 2011–2012**	**Intervention 2013–2018**	**Intervention 2013–2018**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Antenatal care							
ANC by skilled staff	0.88	-0.04	0.04**	0.12**	-0.03	0.03***	0.09***
		(0.03)	(0.02)	(0.05)	(0.02)	(0.01)	(0.03)
AR p-value				0.03			0.00
Observations		947	5,726	5,725	947	5,726	5,725
Communities		146	150	150	146	150	150
Quality ANC	0.72	-0.02	0.08**	0.20**	-0.01	0.07***	0.17***
		(0.04)	(0.03)	(0.08)	(0.03)	(0.02)	(0.04)
AR p-value				0.02			0.00
Observations		947	5,726	5,725	947	5,726	5,725
Communities		146	150	150	146	150	150
ANC by skilled staff in first trimester	0.60	-0.04	0.02	0.05	-0.04	0.01	0.03
		(0.04)	(0.03)	(0.08)	(0.03)	(0.02)	(0.05)
AR p-value				0.52			0.48
Observations		947	5,726	5,725	947	5,726	5,725
Communities		146	150	150	146	150	150
Continuity of care	0.36	-0.00	0.08**	0.21**	0.01	0.08***	0.21***
		(0.04)	(0.04)	(0.10)	(0.03)	(0.02)	(0.04)
AR p-value				0.04			0.00
Observations		947	5,726	5,725	947	5,726	5,725
Communities		146	150	150	146	150	150
Delivery							
Institutional delivery	0.71	-0.02	0.06	0.17*	0.00	0.05***	0.15***
		(0.05)	(0.04)	(0.10)	(0.03)	(0.02)	(0.04)
AR p-value				0.11			0.00
Observations		948	5,725	5,724	948	5,725	5,724
Communities		146	150	150	146	150	150
C-section	0.15	0.01	0.00	0.00	0.01	-0.00	-0.00
		(0.03)	(0.02)	(0.05)	(0.02)	(0.01)	(0.03)
AR p-value				0.94			0.89
Observations		948	5,725	5,724	948	5,725	5,724
Communities		146	150	150	146	150	150
Postnatal care							
Postnatal checkup for mother and newborn	0.62	-0.01	0.08*	0.20*	0.01	0.07***	0.19***
		(0.05)	(0.04)	(0.11)	(0.03)	(0.02)	(0.05)
AR p-value				0.08			0.00
Observations		947	5,719	5,718	947	5,719	5,718
Communities		146	150	150	146	150	150
Quality postnatal checkup for mother	0.42	-0.03	0.08**	0.21**	-0.02	0.08***	0.20***
		(0.04)	(0.04)	(0.10)	(0.03)	(0.02)	(0.05)
AR p-value				0.04			0.00
Observations		947	5,719	5,718	947	5,719	5,718
Communities		146	150	150	146	150	150

Notes. Same notes as tables 1–3. Estimation restricted to the latest pregnancy of each surveyed women. Statistical significance denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

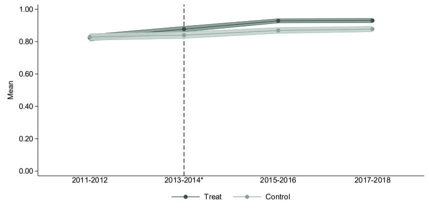
Table E6: Difference-in-differences specification for all outcomes

	Control mean (1)	**N** (2)	**Cluster** (3)	**DID estimate** (4)
Antenatal care				
ANC by skilled staff	0.83	9786	150	0.05***
				(0.02)
Quality ANC	0.66	9786	150	0.06**
				(0.02)
ANC by skilled staff in first trimester	0.54	9786	150	0.03
				(0.02)
Continuity of care	0.30	9786	150	0.06**
				(0.02)
Delivery				
Institutional delivery	0.57	9787	150	0.07***
				(0.02)
C-section	0.10	9786	150	0.00
				(0.01)
Postnatal care				
Postnatal checkup for mother and newborn	0.54	9773	150	0.04*
				(0.03)
Quality postnatal checkup for mother	0.37	9773	150	0.06***
				(0.02)
Maternal and child health				
Infant mortality (per 1000 live births)	16.60	9783	150	5.39
				(6.09)
Neonatal mortality (per 1000 live births)	7.47	9783	150	2.20
				(4.30)
Fertility (per 1,000 women)	176.89	54104	150	-8.70
				(7.56)

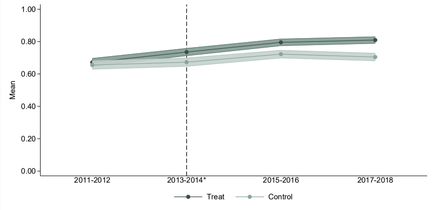
Notes. Estimates based on difference-in-difference estimation. Column (4) shows the coefficient of the interaction between an indicator variable equal to 1 if the community cluster where a women resides was allocated to the treatment group and an indicator variable equal to 1 for the years of intervention (2013–2018). All specifications include indicator variables for treatment allocation, intervention period, and cohort. Standard errors clustered by community are reported in parentheses. Statistical significance denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure E4: Averages by cohort

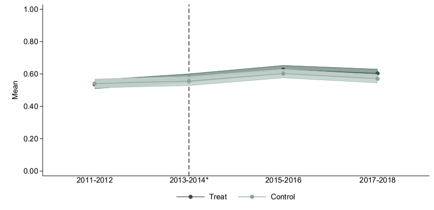
A. ANC by skilled staff



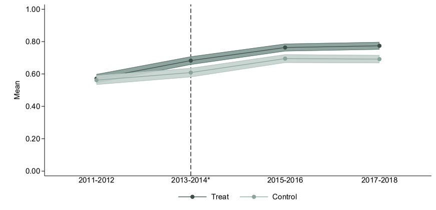
B. Quality ANC



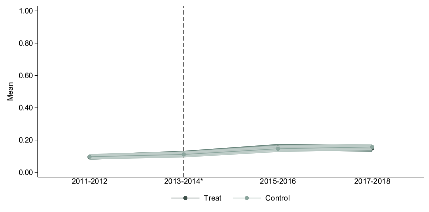
C. ANC by skilled staff in first trimester



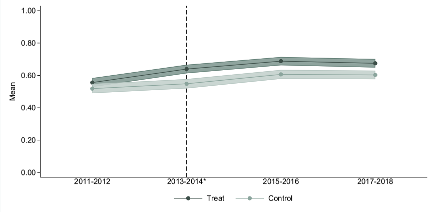
D. Institutional delivery



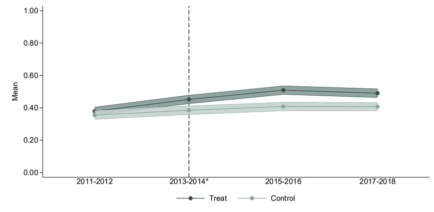
E. C-section



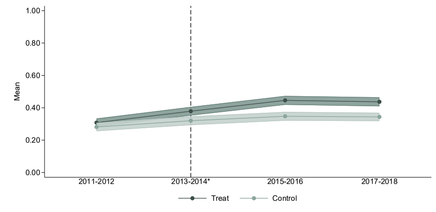
F. Postnatal checkup for mother and newborn



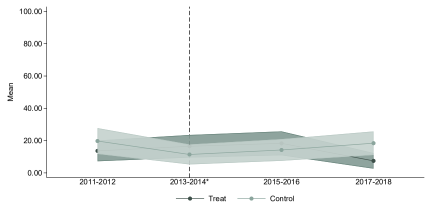
G. Quality postnatal checkup for mother



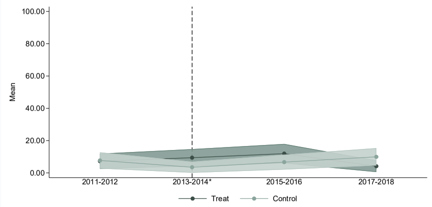
H. Continuity of care



I. Infant mortality (per 1000 live births)



J. Neonatal mortality (per 1000 live births)



K. Fertility (per 1,000 women)

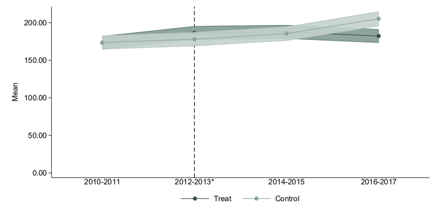


Table E7: Heterogeneous effects by travel time to closest childbirth center, pre-intervention cohorts

	Close			Far			
	β (1)	se (2)	N (3)	β (4)	se (5)	N (6)	p-value (1)–(4) (7)
Antenatal Care							
ANC by skilled staff	-0.02	0.02	1157	-0.01	0.03	1252	0.90
Quality ANC	0.03	0.03	1157	-0.01	0.04	1252	0.64
ANC by skilled staff in first trimester	0.00	0.03	1157	-0.03	0.03	1252	0.67
Delivery							
Institutional delivery	-0.01	0.03	1158	-0.02	0.04	1252	0.99
C-section	-0.01	0.02	1158	-0.01	0.02	1251	1.00
Postnatal care							
Postnatal checkup for mother and newborn	0.02	0.03	1158	0.04	0.04	1250	0.84
Quality postnatal checkup for mother	0.04	0.03	1158	0.00	0.04	1250	0.70
Maternal and child heath							
Neonatal mortality (per 1000 live births)	3.05	4.85	1158	-2.84	5.17	1251	0.53
Infant mortality (per 1000 live births)	-0.10	6.78	1158	-13.78	8.91	1251	0.32
Fertility (per 1,000 women)	5.51	7.12	9933	-1.60	8.52	10356	0.64

Notes. Same notes as 4.

VI. Cost-effectiveness: Back-of-the-envelope calculation

Effectiveness	
Life expectancy	74
Value of a healthy life year (USD)	75,000
Change in IMR in 2018 per live births	10
Social benefit in 2018 (USD)	55,500,000
Cost	
Cost per live birth	253
Number of live births (total 2013–2018)	7,377
Total cost	1,866,381
Cost per 1,000 live births (2013–2018)	1,866
Value per USD invested	29,737

Note. This table is based on the following assumptions: (i) a child who survives as a result of the intervention would live another 74 healthy life years—life expectancy in Nicaragua in 2018 (World Bank 2024); and (ii) the value of a healthy life year is about USD 75,000, a lower bound of estimates in economic studies (Cutler and Meara 2000). The cost per live birth is the cost of the intervention considering the cost of the three vouchers (transportation for ANC, transportation for delivery, and maternal waiting home).

References

Cutler, David, and Ellen Meara. 2000. 'The Technology of Birth: Is It Worth It?' *Frontiers in Health Policy Research* 3 (1): 33–67.

World Bank. 2024. *World Development Indicators*. Washington DC: World Bank.