Saving lives with cooking gas? Unintended effects of targeted LPG subsidies in Peru

Thomas Thivillon*

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Abstract. I evaluate the effect of the conversion of households from wood-fuel cooking to liquefied petroleum gas (LPG) cooking on infant mortality using data from sixteen waves of Peru's continuous Demographic and Health Survey. I exploit the sequential introduction of LPG subsidies targeting low-income households and compare early-treated districts to later or never treated districts using a staggered difference-in-difference estimation strategy. I find that infant mortality increased by 15% as a result of the massive fuel switch induced by the intervention, which corresponds to at least 6,600 additional infant deaths between 2010 and 2020. Subsidizing LPG also caused a higher incidence of symptoms of acute respiratory infections in children under five and of moderate or severe anemia among adult women, two conditions which are known to be induced by exposure to air pollution from cooking fuels. I show that these unexpected results are most likely explained by the fact that the switch to LPG led households which were previously cooking outdoors to start mainly cooking indoors, thus radically modifying the ventilation quality of their cooking area. These findings suggest that clean cooking interventions need to pay more attention to choices of cooking location and to cooking area ventilation.

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Keywords: Energy access, Air pollution, Infant mortality, Clean cooking, LPG, Peru.

* Paris Dauphine University, PSL University, LEDa, CNRS, IRD, [DIAL research team]. Mailing address: DIAL, 4 rue d'Enghien, 75010 PARIS, FRANCE. Email: thomas.thivillon[at]dauphine.eu. I am grateful to Philippe De Vreyer, Elise Huillery, Stéphanie Monjon and participants at the DIAL seminar in development economics for comments and suggestions on earlier versions of this work.

1 Introduction

Neonatal complications and acute respiratory infections (ARI) are the two leading causes of under-5 mortality (Perin et al. 2022). They accounted for half of all child deaths in 2019. The Global Burden of Disease study estimates that 20% of deaths from neonatal complications and 45% of under-5 deaths from ARI are due to air pollution (Institute for Health Metrics and Evaluation 2022). These estimates are corroborated by a rich literature which documents the causal effects of air pollution on child health and early-life mortality (Chay and Greenstone 2003; Currie and Neidell 2005; Jayachandran 2009; Greenstone and Hanna 2014; Arceo, Hanna, and Oliva 2016; Knittel, Miller, and Sanders 2016; Rangel and Vogl 2019). Curbing air pollution could thus save many young lives every year, especially in developing countries where most child deaths occur. Among the public policy options which can be considered to achieve this goal, the promotion of less polluting cooking solutions is often seen as a priority. Indeed, half of the world population mainly relies on solid fuels such as wood, charcoal, or dung, to cook its meals (Energy Sector Management Assistance Program 2020) and the combustion of these fuels contributes to a large share of ambient air pollution in sub-Saharan Africa and in South-Asia in particular (Chafe et al. 2014).

After several decades of effort to promote improved biomass stoves which provided marginal combustion efficiency gains but were often seen as failing to deliver on pollution reduction, the focus of the international development community recently shifted to so-called "clean" cooking fuels and technologies (World Health Organization 2014; Morrison 2018). This wording generally refers to gas, ethanol and electricity powered stoves (World Health Organization 2014). Liquefied petroleum gas (LPG, also known as bottled gas) in particular has been receiving renewed attention due to its relative ease of deployment. In 2013, the United Nations' Sustainable Energy for All initiative co-signed a pledge to transition 1 billion people from solid fuels to LPG with the World LP Gas Association, the global industry organization for LPG (Sustainable Energy for All 2013).

One of the key arguments in support of LPG is its capacity to reduce emissions of fine particles ($PM_{2.5}$), one of the most hazardous pollutants emitted by biomass combustion, by a factor of at least ten relative to wood and charcoal stoves in laboratory conditions (Shen et al. 2018). However, past research on biomass stoves has shown that good laboratory performances did not always translate into real-life health benefits (Smith et al. 2011; Hanna, Duflo, and Greenstone 2016; Mortimer et al. 2017; Jeuland et al. 2020). Indeed, exposure to household air pollution (HAP) from cooking activities is a function of many variables beyond the choice of cooking technology: the skills and cooking habits of the cook, the type of food being cooked, the environment in which cooking takes places[1], and whether several cooking fuels are being used simultaneously or not for instance. The extent to which LPG can contribute to reducing the burden of disease from household air pollution is thus an empirical question which deserves rigorous evaluation.

1. Whether cooking takes place indoors or outdoors for instance

In this study, I estimate the impact of the introduction of LPG subsidies targeting low-income households on early-childhood health and mortality in Peru. I exploit a nationwide initiative launched in 2012 which subsidizes half of the cost of a LPG cylinder every month for eligible households. 90% of these households cooked with wood at baseline. The program claimed 1.8 million registered users at the end of 2020 (Fondo de Inclusión Social Energético 2022b). It is one of the largest LPG conversion programs to date, after those implemented in Indonesia and India (Imelda 2020; Afridi, Debnath, and Somanathan 2021). I combine administrative data on the location and year of registration of LPG retailers authorized to exchange the discount vouchers with data on early childhood mortality and respiratory health from Peru's Continuous Demographic and Health Survey (Peru Continuous DHS)[2].

I match the retailer data and the DHS data at the district level and rely on the progressive roll-out of authorized retailers across districts to study the effect of the subsidies on LPG adoption and on child health. The study uses a staggered difference-in-differences (DID) design in which districts are considered as "treated" when they receive their first authorized LPG retailers. I compare observations from treated districts to individuals or households residing in not-yet and never treated districts over the sixteen-year period going from 2005 to 2020. To do so, I apply econometric tools developed as part of the recent DID methods revolution (Chaisemartin and D'Haultfœuille 2022; Roth et al. 2022) which are robust to the presence of heterogenous treatment effects. I conduct the analysis in the sample of households which satisfied the eligibility criteria of the program at the time of the survey. A key feature of the intervention is that its implementation was entirely sub-contracted to several electricity distribution companies (*Empresas de Distribución Eléctrica*, or EDEs). Thus, although the Peruvian government set some criteria for the targeting of districts, the decentralized management of program activities added a degree of randomness to the roll-out. I show that treated and not-yet treated districts followed parallel trends on the outcomes of interest during pre-treatment periods conditional on a set of basic covariates.

My baseline results show that the subsidy program had a large impact on the adoption of LPG as a primary cooking fuel. Eligible households in treated districts are on average 10.8 percentage points more likely to mainly cook with LPG than they would have been absent the treatment. This represents 1.5 times the pre-treatment usage rate. Unfortunately, the effect of this large fuel switch on health indicators is the opposite of what LPG promoters generally anticipate. I find that infant mortality and acute respiratory infections in under-5 children both *increased* as a result of the policy. The increase amounts to 4.3 deaths per 1,000 live births for infant mortality, 15% of the pre-treatment mortality rate, and to 2.6 percentage points for ARI, 14% of the pre-treatment incidence.

I explore several potential channels which could explain these somewhat unexpected results.

2. The Peru Continuous DHS is managed by the *Instituto Nacional de Estadística e Informática* (INEI) and known locally under the name *"Encuesta Demográfica y de Salud Familiar"* (ENDES, see Instituto Nacional de Estadística e Informática (2020))

I rule out the possibility that the program had an impact on infant mortality through changes in breastfeeding and through a decrease in water disinfection by boiling. A falsification test also shows no effect on diarrheal diseases or on the prevalence of wasting in under-5 children, suggesting that the effect of the LPG subsidies on respiratory health is not confounded by environmental or economic shocks. Instead, I show that the intervention led to a large reduction in the proportion of households which report cooking outdoors and that the effects on most air pollution related health outcomes are concentrated in households which live in homes without ventilation. This suggests that air pollution is most likely the main channel linking the fuel switch to negative health outcomes.

I interpret my results as indicating that women and their young children started spending more time indoors in poorly ventilated rooms because LPG stoves tend to be used indoors. The increase in indoor cooking might be an issue for several reasons. First, it can potentially be disastrous if households stack LPG with wood in indoor kitchens or if they spend more time in wood-heated homes without chimneys. Second, in the case of households simultaneously switching to exclusive LPG use and to indoor cooking, it can lead to a decrease in outdoor exposure to particulate matter emissions from wood at the cost of an increase in indoor exposure to nitrogen dioxide emissions from LPG. Indeed, LPG combustion can emit significant amounts of this pollutant which is known to have negative effects on respiratory health at high concentrations (Hasselblad, Eddy, and Kotchmar 1992; Jarvis et al. 2010).

Several other results and robustness checks support the hypothesis that the switch to LPG led to higher levels of exposure to air pollution. Most importantly, the treatment also caused an increase in the prevalence of moderate and severe anemia among adult women, a health condition which is known to be positively associated with exposure to high concentrations of both fine particles and nitrogen dioxide (Honda et al. 2017; Elbarbary et al. 2019; Morales-Ancajima et al. 2019; Mehta et al. 2021). In addition, I verify that the treatment effects on child health are not due to selective migration, that they are only found in the group of eligible households, and that they are robust to controlling for additional covariates, to using a definition of treatment which accounts for the potential contamination of untreated districts by treated units, and to dropping observations from the year 2020 in which a significant share of the data was collected by phone due to the COVID-19 epidemic. Finally, I also show that the results are not sensitive to my choice of estimator.

This article makes several important contributions. First, it fills a gap in the literature on the health impacts of clean cooking solutions, and in particular of transitions from solid fuels to cleaner cooking fuels. While many evaluations of improved biomass cookstove programs have been published (Smith et al. 2011; Bensch and Peters 2015; Hanna, Duflo, and Greenstone 2016; Mortimer et al. 2017; Jeuland et al. 2020; LaFave et al. 2021), the evidence on the impacts of so-called "clean" fuels is scarcer. Regarding the effects of LPG on respiratory health and associated mortality in particular, evidence from large-scale studies is limited to Imelda (2020) who finds

that the conversion of Indonesian households from kerosene to LPG led to a decline in infant mortality. Cesur, Tekin, and Ulker (2017) consider a slightly different fuel and study the effect of the replacement of charcoal by natural gas networks in Turkey. They also conclude to a decrease in infant mortality. Finally, Ye et al. (2022) report that conversion to LPG caused an increase in gestational blood pressure among a large sample of pregnant women in a field experiment conducted simultaneously in Guatemala, India, Peru and Rwanda, a negative health impact which they suggest could be caused by nitrogen dioxide emissions from LPG.

Thus, this paper reports results from the first large-scale evaluation of the impacts of the conversion of domestic users from *wood fuel* cooking to LPG cooking on early-childhood mortality and health. This contribution is important because wood is still the primary cooking fuel for 35% of the world population (Energy Sector Management Assistance Program 2020)[3] and the effects of a fuel switch are likely to vary significantly by type of baseline cooking fuel. Charcoal and kerosene, for instance, are more likely to be used indoors than wood so that the transition from one of these fuels to LPG would not necessarily result in the decrease in outdoor cooking that I observe in Peru. This may be the reason why my results differ so markedly from those published by Imelda (2020) and Cesur, Tekin, and Ulker (2017). Considering that current efforts to promote LPG in developing countries are likely to target a large proportion of wood users, the findings of this study may be of interest for the decision makers in charge of these policies.

My second key contribution is to present new evidence on the role of outdoor cooking and ventilation quality in mitigating exposure to air pollution. This evidence is relevant not only to the clean cooking literature but also more broadly to the rich literature on the health effects of air pollution (Chay and Greenstone 2003; Currie and Neidell 2005; Jayachandran 2009; Chen et al. 2013; Greenstone and Hanna 2014; Ebenstein et al. 2015; Ebenstein et al. 2017; Arceo, Hanna, and Oliva 2016; Knittel, Miller, and Sanders 2016; Sheldon and Sankaran 2017; Beach and Hanlon 2018; Deryugina et al. 2019; Rangel and Vogl 2019; He, Liu, and Zhou 2020; Hanlon 2022). My results provide support for the argument previously made by Langbein, Peters, and Vance (2017) that the effectiveness of clean cooking policies in developing countries could be greatly improved by taking cooking location and ventilation into account. They also suggest that the promotion of simple ventilation systems in countries where the use of solid fuels is still widespread could bring about significant health benefits. This idea is supported by recent epidemiological evidence from China which shows that the use of ventilation is associated with lower mortality in both solid fuel and clean fuel users (Yu et al. 2020).

Third, this paper conducts the first large-scale evaluation of Peru's national LPG conversion program. Although previous contributions have documented the effects of this policy (Calzada and Sanz 2018; Pollard et al. 2018), these studies exploit data from small samples collected in a single department or a single province of Peru. They cover a shorter period of time than I do. They

3. By contrast, charcoal, coal, and kerosene together account for only 10% of the global primary cooking fuel mix.

thus have external validity limitations. Some of them also report associational rather than causal estimates of the impacts of the program (Pollard et al. 2018). Their conclusions point to a lack of effect of the program rather than to the negative impacts that I identify. This contrast might be explained by differences in statistical power, in identification strategies, or in geographic areas and time periods covered.

This paper is organized as follows. Section 2 provides background information on Peru's national LPG conversion program. Section 3 describes the data. Section 4 discusses my empirical strategy. Results and robustness checks are presented in Section 5. Section 6 concludes.

2 Background

Peru's economy experienced three decades of rapid growth between the downfall of the Shining Path terrorist group at the beginning of the 1990s and the start of the COVID-19 epidemic in 2020. More than a third of the population escaped poverty during this period (World Bank 2022b). However, it took time for this improvement in living standards to induce a change in the cooking fuel choices of the population. In 2000, 39% of Peruvian households cooked with firewood, charcoal or animal dung. A decade later, this proportion was nearly unchanged: more than 36% of the population was still primarily cooking with one of these three solid fuels[4]. Importantly, the persistence of solid fuels was associated with a high incidence of lower respiratory infections in children. These were among the top three causes of under-5 deaths, accounting for nearly 20% of mortality in this age group (Hirschhorn et al. 2020).

It is approximately at this time that transitioning households to LPG cooking became a priority for the Peruvian authorities. The National Family Cookstove Program *"Cocina Peru"* was initiated in 2009 in a context of governmental effort to curb cocaine production[5]. The program anticipated on a ban on the sale and use of kerosene which would be enacted in 2010 in order to reduce the access of drug traffickers to one of the key inputs for coca paste production (Peruvian Times 2009). It aimed to offer an alternative cooking fuel to existing kerosene users (only 2% of the population at the time), and to households interested in switching from biomass to a more modern cooking fuel. To apply for a kit, households had to be classified as poor or extremely poor in the national household poverty classification database (SISFOH), a poverty index based on principal component analysis (Osinergmin 2014). The program ended up equipping more than 870,000 low-income households, approximately 10% of the country's population, with free LPG cooking kits between 2009 and 2016 (Ramírez-Candia, Curt, and Domínguez 2022).

In 2012, the Peruvian authorities added a second pillar to their LPG conversion policy with the creation of the *Fondo de Inclusión Social Energético* (FISE), a social fund in charge of increasing the penetration of clean and efficient energy solutions in Peru. The fund's first mission was to implement

4. Author's calculations using Peru DHS 2000 and Peru Continuous DHS 2009.
5. The program was initially known as Project NINA and was renamed "Cocina Peru" in 2010.

a program of targeted subsidies promoting LPG consumption among low-income populations. These subsidies were to be financed by surcharges on the electricity bills of industrial customers and on sales of liquid hydrocarbon products and natural gas to final consumers (Asia-Pacific Economic Cooperation 2015).

FISE's LPG subsidy scheme was piloted between July and November of 2012. Scale-up started early in 2013 (Asia-Pacific Economic Cooperation 2015). The program's main operational rules were refined progressively during the course of 2013 and remained nearly unchanged until the end of 2020[6]. During this period, eligible beneficiaries who had enrolled in the program received a monthly voucher worth 16 soles (approximately 5.5 USD in 2013) which they could use to purchase a 10kg LPG refill from an authorized LPG retailer (Osinergmin 2014). The voucher covered approximately 50% of the cost of the refill, with some variation between regions due to differences in local LPG prices.

Importantly, FISE delegates the implementation of its subsidy scheme to local electricity distribution companies (EDEs) which are in charge of recruiting and training LPG retailers in the program's target districts. EDEs are also responsible for identifying eligible households, distributing the monthly FISE-vouchers to enrolled beneficiaries, and conducting promotional activities for the program such as airing radio spots and video advertisement, holding public meetings, and distributing flyers and pamphlets (Osinergmin 2014). The vouchers are distributed as a code printed on the beneficiary's monthly electricity bill or sent via text message on their mobile phone. Paper vouchers are also printed for the households which do not have access to electricity and cannot receive their vouchers on a mobile phone (Osinergmin 2021).

FISE uses a multidimensional poverty assessment strategy to select its beneficiaries. EDEs have access to a census of eligible households within their concession area (Calzada and Sanz 2018). During the study period covered in this paper, households were deemed eligible if they belonged to the five poorest categories of the SISFOH index and if their annual income did not exceed 18,000 soles (approximately 6,200 USD in 2013). In addition, they had to live in a house made of precarious materials and, if they had access to the electricity grid, their consumption could not exceed 30kWh per month. Households were also excluded from the program if they lived in an area where connection to the natural gas network was possible[7].

At the end of 2020, FISE reported 1.8 million registered beneficiaries, almost 6,000 authorized LPG retailers, and more than 60 million vouchers exchanged since its launch (Osinergmin 2021). This represents between 270 and 330 million USD invested in LPG subsidies for low-income households over a period of 9 years[8], making it one of the largest clean cooking interventions to date.

The program faced some operational challenges during its first years of implementation. First,

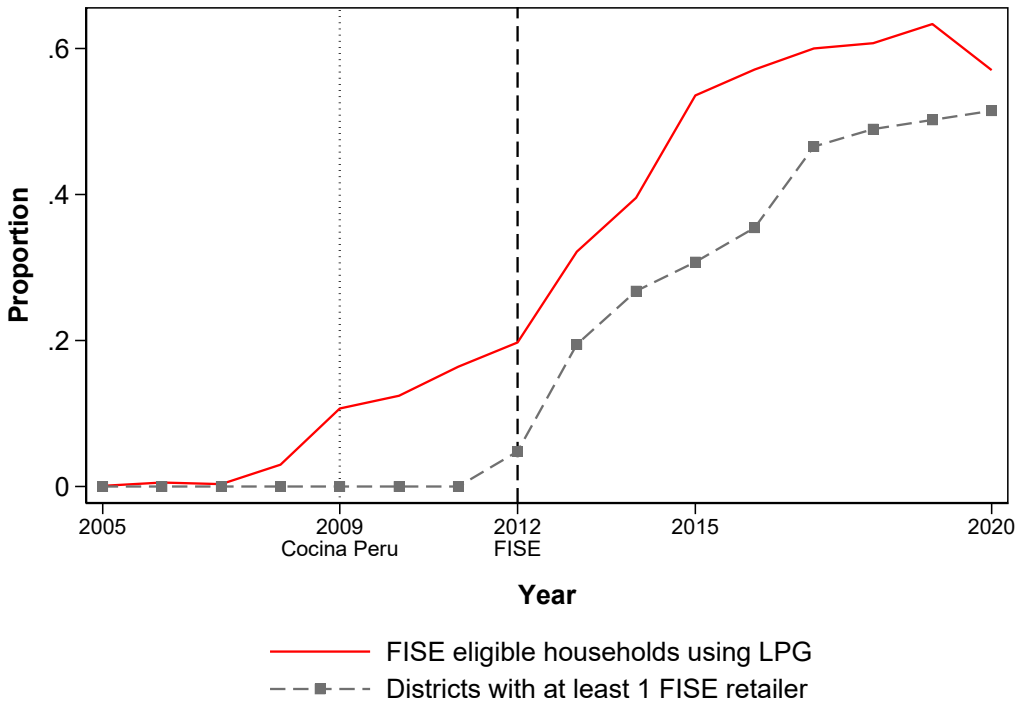
6. The value of the subsidy and some of the eligibility criteria were adjusted in 2021.

7. In this case, they could apply for financial support from FISE's Bonogas program which finances part of the cost of connection to the gas network.

8. Depending on the exact exchange rate used.

the initial reimbursement period for the sums advanced by the retailers frequently exceeded 15 days which led some retailers to charge extra costs for FISE beneficiaries and slowed the recruitment of new retailers (Calzada and Sanz 2018). This motivated the introduction of digital vouchers sent by text message in mid-2013, a solution which enables FISE to pay the retailers instantly. However, beneficiaries without access to a cell phone may still be discriminated in their access to FISE subsidies. Second, it seems that households which did not satisfy all the eligibility criteria nevertheless received the vouchers due to coordination difficulties between local authorities and EDEs (Nuño Martínez, Mäusezahl, and Hartinger 2020). Contamination of the target population by less vulnerable population groups might therefore have occurred in some areas. Finally, there is also anecdotal evidence that the increase in demand for LPG which followed the introduction of the subsidies led to occasional shortages of 10kg cylinder refills (Marticorena 2015). These implementation issues and their potential implications for the identification strategy are discussed further in the next sections of this paper.

Figure 1: Change in FISE coverage and growth of LPG use rate among eligible households (2005-2020)



Source: Author's calculations using Peru Continuous DHS 2005-2020 and the public database of authorized FISE retailers (Fondo de Inclusión Social Energético 2022a).

3 Data and descriptive statistics

The analysis combines sixteen waves of the Peru Continuous DHS and a database of authorized FISE retailers. The full dataset contains repeated cross-sectional data on 390,769 households whose members were interviewed between 2005 and 2020. I exploit mortality data on 469,094 children born to the women who were surveyed in the sample of households. These births occurred over a period of two decades, from 2000 to 2020. Data on childhood respiratory health is available for 189,965 children who were less than 5 years old at the time of the survey.

3.1 Administrative data and assignment to treatment

Data on retailers come from the database of authorized FISE retailers which is publicly available on FISE's website[9]. This database contains the postal address and the unique identifier of the district of operation of all the LPG retailers who accept FISE vouchers as partial payment for LPG refills, along with their date of entry in the program. The boundaries of some districts changed during the course of the study period. A few of these districts were divided into several smaller ones between 2012 and 2020. To address this issue, I use a fixed definition of district boundaries for the whole study period and assign households and retailers to a district of residence or operation based on the 2008 map of Peruvian districts. Using this definition, the database contained 5,900 unique retailer addresses operating in 952 of Peru's 1,834 districts at the end of December 2020[10]. To the best of my knowledge, this is the first time that this data is used to study the impact of FISE's LPG subsidy program.

I use the subscription dates of the retailers to map the progressive roll-out of the subsidy scheme between July 2012 and December 2020. More specifically, I consider that a district starts being affected by the policy when it receives its first authorized FISE retailer. The motivation for this treatment definition is that EDEs managed both the demand-side and the supply-side activities of FISE's LPG promotion program in a given district (Pollard et al. 2018). This suggests that the registration of the first retailers and the introduction of the vouchers were often coordinated at the district level. In many instances, the date of registration of the first retailer in a district is therefore likely to provide a relatively good approximation of the date at which households started receiving vouchers in that district. Even if voucher distribution also occurred in districts which were not served by an authorized retailer, the cost of travelling to a neighboring district to find a retailer who would accept the voucher certainly limited the effect of the subsidy on LPG uptake in these cases.

9. See Fondo de Inclusión Social Energético (2022a).

10. I treat retailers with the same name and the same postal address but a different registration date as duplicates except when one of the records corresponds to a delivery vehicle. Records with different names but a shared postal address are also treated as duplicates. For all duplicates, I keep the earliest record in the final database of 5,900 addresses.

In other words, the identification strategy in this paper exploits the fact that the program's ability to affect its beneficiaries' fuel choices largely depended on the development of its last mile distribution network at the district level. A district is considered treated when this last mile network finally reaches it. While FISE already claimed that more than 90% of Peruvian districts were covered by the subsidy scheme at the end of 2013 (Osinergmin 2014), using the above definition of the program roll-out suggests a much more progressive scale-up. As illustrated in Figure 1, less than 20% of Peruvian districts had an authorized retailer at the end of 2013, approximately 30% in 2015, and 51.5% at the end of 2020. This means that nearly half of Peru's districts are never treated during the study period and serve as a pure comparison group in the analysis[11].

Of course, this treatment definition leaves space for potential contamination of the not-yet treated and never treated districts by the intervention in cases where EDEs went on to distribute vouchers in districts which were not served by an authorized retailer. The extent of this issue is reflected in the large mismatch between the coverage rates claimed by FISE early in the implementation period and the actual coverage rates suggested by the supply-side definition of treatment that I use. I address this concern in the robustness checks section of this paper by replicating my main results using an alternative treatment definition in which districts are also considered treated if a treated district exists within a 15km radius of the centroid of the district of interest in the year of observation. This allows districts to be treated in time periods when they did not host a FISE retailer so long as there is an active retailer within a reasonable distance of the district.

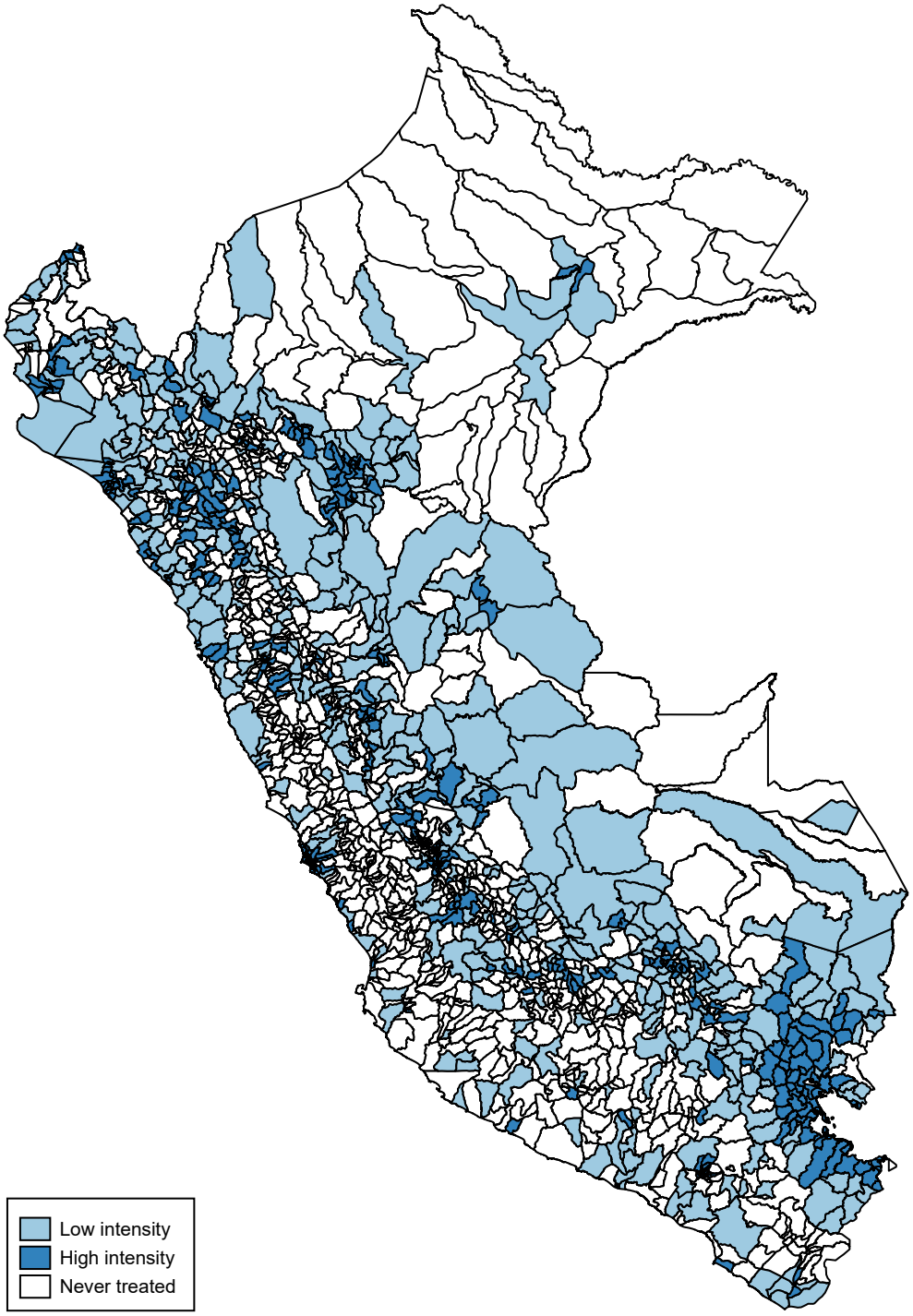
Finally, I differentiate between districts exposed to a high intensity of treatment and those which are treated with a low level of intensity based on the number of authorized retailers per one-hundred square kilometers in the district at the end of the study period. I define districts as high intensity if they hosted more than 1.3 retailers per one-hundred square kilometers in 2020. This threshold corresponds to the median concentration in the treated districts. Districts with a concentration below the median are classified as low intensity districts. Figure 2 shows the geographic location of treated districts along with their intensity of treatment. High intensity districts tend to be concentrated at the extreme south of Peru where the subsidy program was initially piloted in 2012. Low intensity districts dominate in the east of Peru which corresponds to the Amazonian forest region where districts are much larger and less dense than in the rest of the country. I use this categorization to evaluate the effect of the intensive margin of treatment with the DID imputation estimator since the estimator only allows binary treatment variables (Borusyak, Jaravel, and Spiess 2022).

3.2 Demographic data

All demographic data are from the Peru Continuous DHS. While standard DHS surveys collect fertility and health data for a nationally representative sample of households at five-year intervals,

11. For the districts which are covered by the sampling frame.

Figure 2: Outreach of FISE's LPG subsidy program



Source: Author's calculations using the public database of authorized FISE retailers (Fondo de Inclusión Social Energético 2022a).

the Continuous DHS methodology is designed to increase the frequency at which data are available by conducting yearly surveys on a sample representing roughly one-fifth of the size of a standard DHS sample (Rutstein and Way 2014)[12]. In the case of Peru however, governmental health reporting requirements led to a rapid increase in survey size compared to what was originally envisioned. The sample size went from 6,837 households in 2005 to 18,445 in 2008 and to more than 35,000 from 2015 onward. I assemble these large annual cross-sectional surveys over the period 2005 – 2020. Each survey is nationally representative and records basic socio-demographic information as well as a complete birth and child mortality history for all women aged 15-49 in the randomly selected households. Self-reported health data, anthropometric measurements, and results of hemoglobin tests are also reported for children. A list of surveys with information on sampling frames and sample sizes for each year is available in appendix Table A.1.

3.2.1 Eligible households

To take the program's targeting strategy into account, I conduct most of the analysis with the sub-sample of households which satisfy the program eligibility criteria which I can observe in the data. More specifically, I define as eligible the households which belonged to the first three deciles of the DHS wealth index and who lived in a house in which the floor or the walls were made of traditional materials at the time of the survey. I use the first criterion as a proxy to select the households which satisfy FISE's annual income limit of 18,000 soles. Indeed, this income level approximately corresponds to the third decile of Peru's income distribution during the study period (Instituto Nacional de Estadística e Informática 2019). The second criterion is a direct application of FISE's rule regarding housing characteristics. Finally, I also exclude visitors from the analysis because their district of residence is unknown. The resulting analytical sample includes 146,371 households, 197,135 childbirths, 71,674 living children aged less than 5 at the time of the survey, and 80,081 adult women who were tested for anemia as part of the survey.

Importantly, in the case of birth related outcomes such as birthweight or infant mortality, income level and housing characteristics might have changed between the date of birth and the survey date. Unfortunately, each household is only observed once in the dataset and the analysis therefore makes the assumption that the eligibility status of the household remained unchanged over time. This source of measurement error in the eligibility status of respondents is likely to result in the contamination of the sub-sample of interest by observations from households which were ineligible to receive subsidies at the time of birth, and ultimately in attenuation bias affecting the coefficients of interest (Wooldridge 2010). In this situation, point estimates will likely be underestimated. A similar measurement error problem is caused by the fact that electricity consumption levels, one of FISE's key eligibility criteria, are unobserved in the data. Again, this will most likely result in ineligible households contaminating the sub-sample of interest and therefore in downward biased

12. Typically, around 30,000 households.

coefficients.

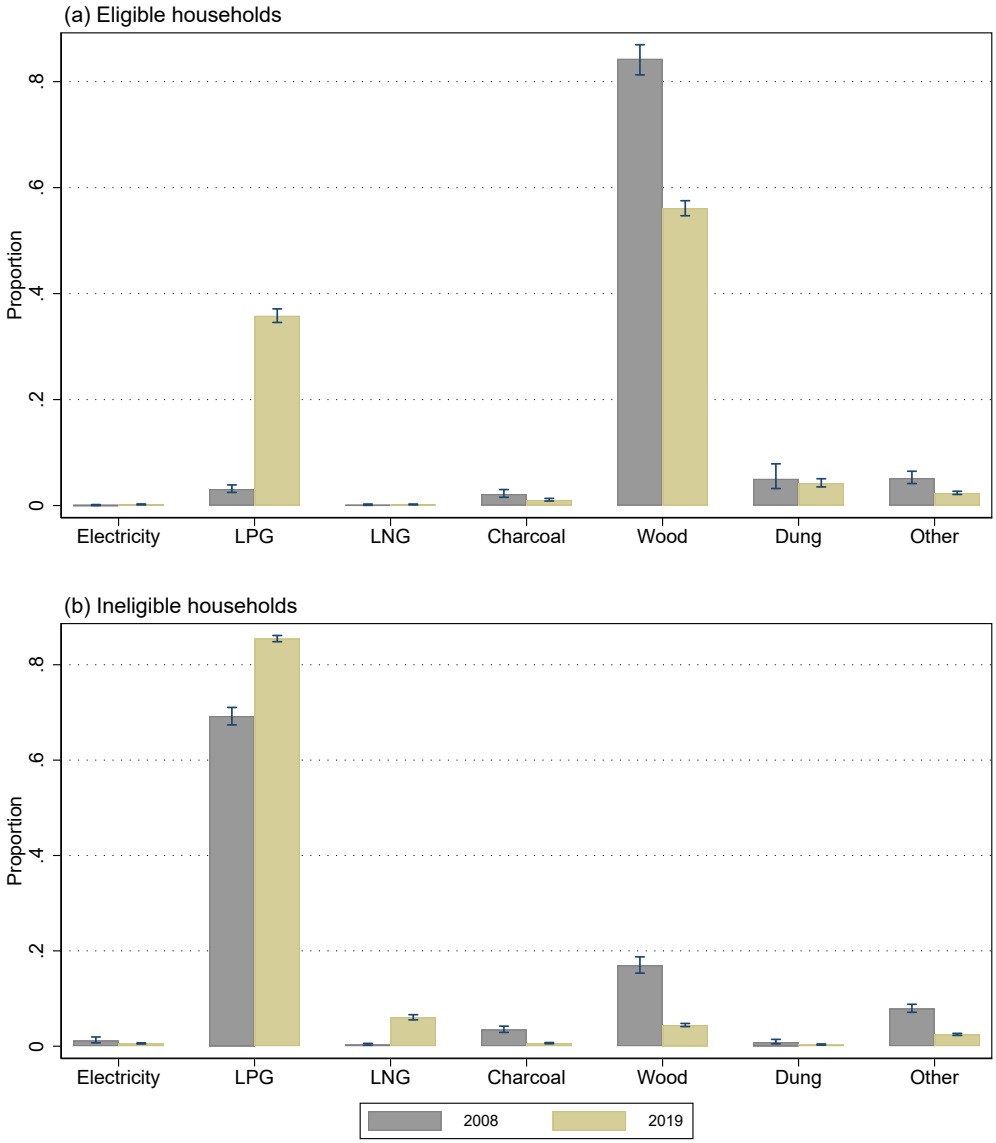
3.2.2 Household-level data, fuel choices and cooking habits

The household questionnaire records the cooking fuel which households report using most frequently (primary cooking fuel), along with a second less frequently used cooking fuel if relevant (secondary cooking fuel). I use this information to evaluate the effect of LPG subsidies on the extensive and intensive margins of LPG use. More specifically, I estimate the impact of the intervention on the probability of using LPG to cook, on the likelihood that LPG is adopted as the primary cooking fuel, and on the probability of cooking with LPG exclusively. I report similar outcomes for wood fuel in order to document the effect of the subsidies on traditional fuel use. Figure 3 reports the distribution of primary cooking fuels for households satisfying FISE's eligibility criteria and for would-be ineligible households in 2008, before the start of Cocina Peru, and in 2019, before the onset of the COVID-19 epidemic. It shows that the uptake of LPG as a primary cooking fuel was rare at the start of the study period in the group of vulnerable households corresponding to FISE's target population. Twelve years later, the situation was radically different with more than a third of eligible households reporting LPG as their primary cooking fuel, a sevenfold increase compared to the baseline situation. Almost all of the increase seems attributable to households abandoning wood for LPG. While, LPG use also increased in the ineligible group, the increase was more modest in absolute terms and compared to the baseline situation. Importantly, the use of electricity as a primary source of energy for cooking remained stable and close to zero in both groups over the study period, so that the analysis in this paper is unlikely to confound the effect of access to LPG with the effect of access to electricity on exposure to air pollution from domestic sources.

3.2.3 Mortality and health data

My main outcome of interest is infant mortality. I also report results for neonatal, post-neonatal and mortality before age three. I follow Jayachandran (2009) in referring to the latter as "early-life mortality" rather than child mortality to avoid any confusion with the standard definition of child mortality as mortality between ages 1 and 4 (Croft, Marshall, and Allen 2018). I focus on infant mortality, defined as mortality during the first 12 months of life, because there is a rich causal literature documenting the large effect of exposure to air pollution on mortality in this age group (Chay and Greenstone 2003; Currie and Neidell 2005; Jayachandran 2009; Greenstone and Hanna 2014; Arceo, Hanna, and Oliva 2016; Knittel, Miller, and Sanders 2016; Rangel and Vogl 2019; Heft-Neal et al. 2020). If pro-LPG public policies had an effect on respiratory health and mortality in the Peruvian population, I therefore expect this effect to be exacerbated and easier to identify among infants. This literature also shows that *in-utero* exposure during late-pregnancy is an important mechanism behind air pollution induced infant mortality. As a result, mortality from smoke exposure tends to be particularly concentrated during the first month of life. I conduct

Figure 3: Primary cooking fuel by FISE eligibility status in 2008 and 2019



Source: Author's calculations using Peru Continuous DHS 2008 and 2019..

separate analyses for neonatal and post-neonatal mortality in order to provide an understanding of the role of prenatal exposure in explaining my findings. Finally, I present results for mortality before age 3 to give a broader picture of the impacts of targeted LPG subsidies on early-life mortality in Peru. It would have been preferable to produce estimates for mortality before age 5. However, this would require that all children who had not reached the age of five at the time of being surveyed be excluded from the analysis because the DID imputation estimator does not handle censoring bias. Such sample restriction would result in a significant loss of power and would limit my ability to estimate the long-term effects of the policy by reducing the number of post-treatment time periods which could be included in the analysis.

I explore the effect of the policy on three dimensions of child health which are known to be affected by exposure to air pollution and especially to fine particulate matter: birthweight, acute respiratory infections (ARI) and anemia. The size of my analytical sample is much smaller for these health variables than in the case of mortality data because the child health module of the Peru Continuous DHS questionnaire only collects information on the health condition of the three last births in the 5 years preceding the survey for each woman aged 15-49. In addition, hemoglobin tests are only conducted among children between 6 and 59 months of age and these tests were not conducted in 2006.

I use the probability of being born with a low birthweight as an indicator of health at birth. I follow the standard DHS definition of low birthweight and classify all children whose weight at birth is reported as less than 2.5 kilograms as cases of low birthweight (Croft, Marshall, and Allen 2018). There is a large body of evidence showing that *in-utero* exposure to household air pollution from cooking fuels increases the risk of low birthweight (see Li et al. 2017, for a review). Several $PM_{2.5}$ constituents are suspected to contribute to this effect including metals, sulfur constituents, and polycyclic aromatic hydrocarbons, a product of wood and charcoal combustion (Berkowitz et al. 2003; Basu et al. 2014; Jo et al. 2022). A limitation of my analysis for this variable is that birthweight data is missing for approximately 14% of the sample of births. However, the probability of missing birthweight is exactly the same among observations from treated and from never-treated districts so that unobserved birthweights are unlikely to bias my estimates.

My second health variable of interest is the probability of having experienced symptoms of acute respiratory infection (ARI) during the two weeks preceding the survey among children under 5. Symptoms of ARI are defined by DHS as "short, rapid breathing which was chest-related and/or [...] difficult breathing which was chest-related" (Croft, Marshall, and Allen 2018). Childhood exposure to pollution from biomass combustion is consistently associated with acute respiratory infections in the public health literature (Dherani et al. 2008; Po, FitzGerald, and Carlsten 2011). It is suspected that air pollution affects the immune response to respiratory viruses (Loaiza-Ceballos et al. 2022) and increases the expression of the primary cellular receptors of some of these viruses (Spannhake et al. 2002; Lee et al. 2015).

Finally, I also assess the effect of the intervention on the probability of moderate or severe anemia among children. I define as moderately or severely anemic children whose hemoglobin level is inferior to 10 grams per deciliter after adjustment for altitude[13]. Exposure to air pollution, and in particular to high concentrations of $PM_{2.5}$, is suspected to affect blood cell production through chronic systemic inflammation (Honda et al. 2017). There is empirical evidence associating particulate matter exposure with anemia and this relationship has recently been documented in the case of under-5 children in Peru (Honda et al. 2017; Elbarbary et al. 2019; Morales-Ancajima et al. 2019; Mehta et al. 2021). In terms of physiological mechanism, the chronic systemic inflammation caused by exposure to air pollution is suspected to reduce the production of red blood cells (erythrocytes) by the bone marrow and to decrease the average lifespan of erythrocytes (Mehta et al. 2021).

It is important to stress that some fuels which are generally referred to as "clean" also emit high levels of pollutants suspected to adversely affect early-life health. LPG combustion in particular is an important source of nitrogen dioxide (NO_2) which is has been shown to increase the probability of respiratory illness in children (Hasselblad, Eddy, and Kotchmar 1992; Jarvis et al. 2010). An effect of NO_2 on anemia is also documented in adults (Honda et al. 2017; Elbarbary et al. 2019). Considering that NO_2 concentrations in homes with gas stoves frequently exceed WHO annual guidelines (Levy 1998; Jarvis et al. 2010; Zhu et al. 2020; Lebel et al. 2022), the magnitude of the improvement in child health to be expected following a massive switch from solid fuel to gas cooking is hard to predict.

3.2.4 Summary statistics

Table 1 provides child-level and household-level descriptive statistics by treatment status of the district of residence before the start of governmental LPG promotion. It shows that FISE-eligible households were significantly different in treated districts and in the districts which would remain unexposed to FISE until 2020. In particular, households from treated districts were more likely to be urban and less likely to use wood for cooking. However, they were 6.7 percentage points less likely to have access to electricity than households in untreated districts (see Panel A of Table 1). This reflects FISE's strategy of targeting households with a low electricity consumption or without access to electricity. Table 1, Panel B reports individual characteristics for all the children born in the 5 years preceding the survey to the women residing in FISE-eligible households. The infant mortality rates and under-3 mortality rates presented in this panel indicate that children from treated districts had a significantly lower risk of early life mortality. These differences will not be a threat to my identification strategy, which is based on the parallel trends assumption.

13. I use the standard DHS adjustment formula as described in Croft, Marshall, and Allen (2018).

Table 1: Extensive summary statistics at baseline

	Never treated (NT)	Treated (T)	Diff. (NT - T)
Panel A: Characteristics of households			
Uses LPG for cooking	0.022	0.019	0.003
Primary cooking fuel is LPG	0.014	0.008	0.006
Cooks with LPG exclusively	0.011	0.006	0.005
Uses wood for cooking	0.933	0.884	0.049*
Primary cooking fuel is wood	0.908	0.870	0.038*
Cooks with wood exclusively	0.864	0.826	0.038*
Main cooking area is outdoor	0.062	0.097	-0.035*
Boils water for disinfection	0.709	0.638	0.071*
Household head is a woman	0.236	0.183	0.053*
Age of household head	52.356	49.776	2.581*
Household size	3.677	4.125	-0.448*
Urban	0.040	0.123	-0.083*
Has access to electricity	0.236	0.170	0.067*
Owns mobile phone	0.026	0.063	-0.037*
Observations	4883		
Panel B: Demographic characteristics and early-life mortality			
Girl	0.495	0.461	0.034
Multiple birth	0.016	0.014	0.001
Mother's age at childbirth	27.431	27.481	-0.049
Mother < 18 y.o at childbirth	0.069	0.071	-0.002
Mother smokes	0.014	0.023	-0.009
Ever breastfed	0.992	0.992	0.001
Neonatal mortality risk	0.013	0.014	-0.001
Infant mortality risk	0.042	0.024	0.018*
Under-3 mortality risk	0.052	0.029	0.023*
Observations	2182		
Panel C: Health at birth			
Birth weight	3108.366	3117.023	-8.656
Low birth weight	0.079	0.105	-0.027
Observations	1507		
Panel D: Children's respiratory health			
Acute respiratory infection in past 2 weeks	0.248	0.234	0.014
Observations	2082		
Panel E: Children's hemoglobin level			
Hemoglobin level adjusted for altitude (g/dL)	109.994	109.845	0.149
Moderate or severe anemia	0.224	0.194	0.031
Observations	1558		

Source: Author's calculations using Peru Continuous DHS 2007-2008. Notes: Unweighted statistics. Column (1) reports statistics for observations from never-treated districts and column (2) for observations from districts treated between 2012 and 2020. Sample: (A) FISE-eligible households, (B) live-births in past 5 years to women residing in a FISE-eligible household, (C) last 3 live-births in past 5 years to women residing in a FISE-eligible household, (D) under-5 children whose mother resides in a FISE-eligible household, (E) children aged 6 to 59 months whose mother resides in a FISE-eligible household. * $p<0.05$ for t-test between never-treated (NT) and treated (T) groups.

4 Empirical strategy

4.1 Choice of estimator

I exploit the progressive deployment of authorized FISE retailers across Peruvian districts and estimate the impact of FISE subsidies using a difference-in-differences design with staggered adoption of treatment. My main specification uses the following event-study model:

$$y_{idt} = \alpha_d + \lambda_t + \sum_l \mu_l \mathbf{1}\{t - E_d = l\} + X'_{idt}\theta + \epsilon_{idt} \tag{1}$$

where y_{idt} is the outcome variable of interest for unit i in year t and in district d. t is the year of birth when the outcome of interest is related to mortality or health at birth[14] and the year of observation otherwise. α_d and λ_t are district and year fixed effects. E_d is the year in which a district receives its first FISE retailer and l, the relative time, corresponds to the distance in years between the year of birth or observation and the year when treatment starts. l is negative for units observed in pre-treatment years. I refer to units belonging to districts which share the same initial treatment year as "cohorts", following Sun and Abraham (2021). Finally, X'_{idt} is a set of exogenous control variables.

Estimation of Equation (1) via Ordinary Least Squares (OLS) will lead to biased results in the presence of heterogenous treatment effects across cohorts. It could potentially return coefficients with the wrong sign (Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021; Borusyak, Jaravel, and Spiess 2022). Several alternative estimators have been proposed to address this issue (Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021; Borusyak, Jaravel, and Spiess 2022). In what follows, I use the "imputation estimator" developed by Borusyak, Jaravel, and Spiess (2022) to estimate the average treatment effect across cohorts for each non-negative relative time-period l ($l \geq 0$). I also compute standard overall treatment-on-the-treated (ATT) coefficients which provide an unbiased estimate of the average treatment effect across all cohorts and all relative time periods, again following Borusyak, Jaravel and Spiess. For all the estimations, I report standard errors clustered at the treatment level (i.e. the districts) for both pre-treatment coefficients and ATTs. This is an intention-to-treat analysis since eligible households in treated districts had to register with their electricity provider or through the local office of the Peruvian Ministry of Energy (MINEM) to benefit from the program. I do not observe who registered and who did not in the data.

My choice of estimator is motivated by the fact that Borusyak, Jaravel, and Spiess (2022) explicitly discuss the application of the imputation estimator to repeated cross-sectional data while most alternative estimators are best suited for panel data analysis (Sun and Abraham 2021; Chaisemartin and D'Haultfoeuille 2022). Callaway and Sant'Anna (2021) also propose a staggered DID

14. i.e. for neonatal, post-neonatal, infant and early-life mortality, for the probability of low birthweight, and for the number of months of breastfeeding.

estimator which allows for cross-sectional data. However, this estimator is challenging to implement in datasets with many time periods and a large number of treated groups as is the case here. A limitation of the imputation estimator is that it drops the observations corresponding to districts which are observed an insufficient number of years in the pre-treatment period because district and year fixed-effects cannot be computed for these districts. This corresponds to approximately 8% of the households in the present case. To address this issue I re-estimate my main model using a standard two-way-fixed-effects (TWFE) estimator in the robustness checks section of this paper and show that the findings are qualitatively similar when observations from less frequently surveyed districts are included in the analysis, although coefficients are generally smaller when estimated via TWFE due to the inherent bias of this estimator in staggered DID designs.

I conduct the analysis at the household level for the outcomes related to cooking fuel choices, to the characteristics of the cooking area, and to the probability of boiling water for disinfection. In the household-level imputations, I include the following list of controls: gender and age of the household head, size of the household, urban residence, access to electricity and mobile phone ownership. The last two control variables account for the fact that the intervention was implemented by electricity distribution companies, which might have led to a different treatment effect for households without access to electricity, and that FISE vouchers are mainly distributed via mobile phone. In the imputations where the outcome variable is related to early-life mortality or to child health, I add controls for female children, for twin births, for the mother's age at birth, for mothers aged less than 18, and for mothers who report smoking at the time of the survey. I also control for the child's age in months where relevant. Analyses in the sample of adult women are restricted to the effects of the treatment on the probability of moderate or severe anemia and control for the woman's age, body mass index (BMI) and smoking status in addition to the household-level control variables listed above. As previously discussed, when the unit of analysis is a birth, some of the control variables might have changed between the year of birth and the year of observation. This is particularly the case if the household moved between the birth and the year of observation which could affect its area of residence or its access to electricity. In Section 5.6, I re-estimate my main specification after the observations which moved to a new residence between the year of treatment and the survey year have been dropped from the sample. This addresses part of the concerns associated with time-varying control variables along with the issue of selection into treatment through migration. I also probe the sensitivity of the results to the inclusion of additional control variables in this section.

4.2 Testing for parallel trends

Similar to the estimation of DID designs via OLS, the DID imputation estimator relies on the assumption that potential outcomes without treatment would follow parallel trends. Borusyak, Jaravel, and Spiess (2022) provide an empirical test for this assumption in which the following model is estimated via OLS for the untreated observations only:

$$y_{idt} = \alpha_d + \lambda_t + \sum_{l=-L}^{-1} \mu_l \mathbf{1}\{t - E_d = l\} + X'_{idt}\theta + \epsilon_{idt} \tag{2}$$

In this model, the observations from districts which will be treated 1 to L years later are compared to all the observations from never treated districts or from districts which will be treated more than L years later. This approach differs from the conventional test of pre-trends used with TWFE models in which placebo coefficients for the "lead" periods are estimated jointly with the "lags" corresponding to the treatment effects. Borusyak, Jaravel, and Spiess (2022) highlight two key benefits of this placebo test. First, and most importantly, the test does not suffer from the bias which results from the practice of conditioning the analysis on the results of a test of pre-trends in which the treatment effect and the pre-treatment estimates are correlated (see Roth 2022, for a detailed exposition). Second, it improves the power of treatment effects estimation because all the untreated observations can be included in the control group for the imputation of the ATT coefficients. However, this second advantage comes at the cost of relatively low power in the placebo test. In practice, the model estimated for the test of pre-trends is almost always a linear probability model because most of the outcome variables used in the analysis are binary.

In the next sections of this article, I use the approach to pre-trends testing recommended by Borusyak, Jaravel, and Spiess (2022). Although the placebo test is performed separately, I present evidence of parallel trends jointly with the ATT coefficients in a single graph, as is usually done for conventional event studies. The fact that the main intervention, the introduction of FISE subsidies for LPG refills, was preceded by the distribution of free LPG cookstoves in some areas through the Cocina Peru program represents a challenge for the analysis, in particular because these two programs shared some eligibility criteria[15]. To take this fact into account, I follow Bismarck-Osten, Borusyak, and Schönberg (2022) and allow for anticipatory effects of treatment up to two years before the registration of a first authorized LPG retailer in a district. Concretely, this means that the indicator variables in Equation (1) are redefined as: $\sum_{l=-2}^{L} \mu_l \mathbf{1}\{t - E_d = l\}$ where L corresponds to the last post-treatment relative period for which there are observations in the sample. The choice of a relatively short anticipation period of 2 years reflects the fact that there was some coordination between the two programs and that Cocina Peru was more likely to operate in districts where EDEs were actively preparing the extension of the FISE retailer network.

To allow for similar anticipatory effects in the placebo test of parallel trends, I rewrite the indicator variables in Equation (2) as: $\sum_{l=-L}^{-3} \mu_l \mathbf{1}\{t - E_d = l\}$. When testing for parallel trends, I set $L = 7$ for household-level outcomes and child health outcomes and $L = 10$ for mortality and birth related outcomes. This choice is motivated by efficiency concerns: setting $L > 7$ for the first group of outcomes results in excessively large confidence intervals because the number of pre-treatment relative time periods, and thus the number of untreated observations, is lower when

15. see Section 2 above.

the time unit is the year of observation than when it is the year of birth.

5 Results

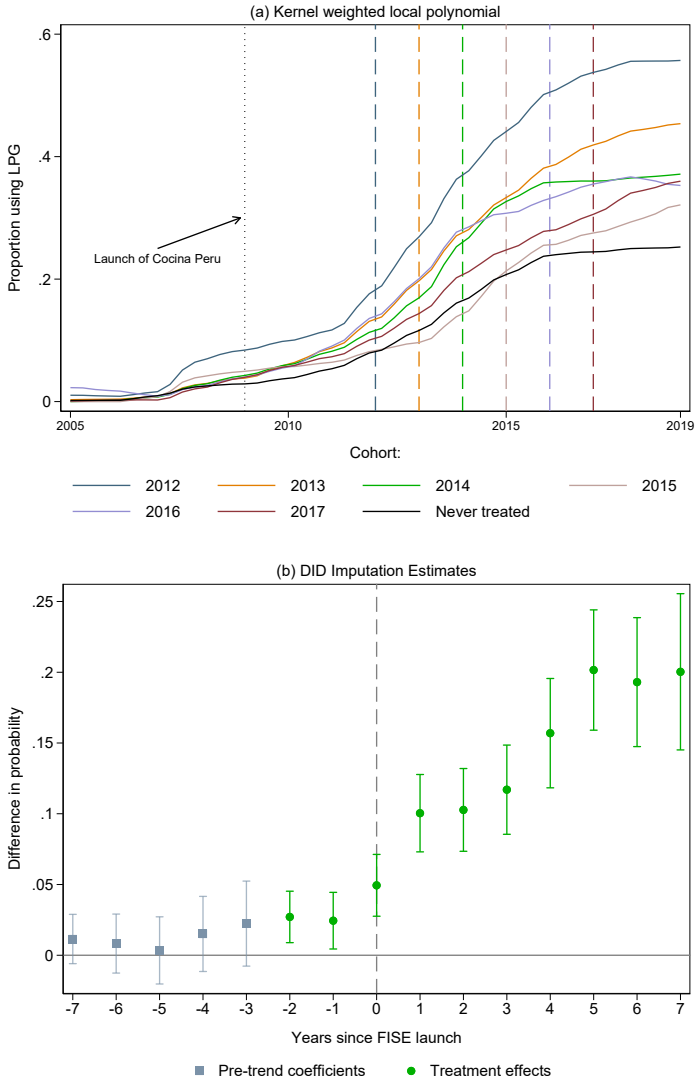
5.1 Effect of FISE subsidies on cooking fuel choices

I start by assessing the success of the subsidy program in converting Peruvian households to LPG, its primary operational objective. In Panel (a) of Figure 4 I display a kernel-weighted local polynomial smooth of the proportion of households using LPG as their primary cooking fuel by treatment cohort. I exclude the year 2020 in this figure because it corresponds to the year of the COVID-19 epidemic. LPG use decreased in most groups in 2020 due to the associated economic crisis and the proportion of LPG users in 2019 is therefore a more accurate indicator of the penetration of LPG in each cohort at the end of the study period. For most cohorts, the trends start diverging in 2009, the year in which Cocina Peru started distributing LPG cookstoves to low-income households. There are some signs of anticipation for the 2012 cohort which suggests that the program was piloted in districts in which the LPG market was already more dynamic. The graph shows that LPG use grew rapidly in all groups after 2012, including in never treated districts. However, growth was faster in treated districts and the difference in LPG use rate relative to never treated areas is much larger in 2019 than in 2012 for all treatment cohorts. In addition, for some cohorts, there is an upward shift in trend in the year corresponding to the introduction of the FISE subsidies or in the two years preceding it. This shift is particularly visible for the 2012, 2014 and 2015 cohorts. Finally, the difference in the proportion of households which report using LPG as their primary cooking fuel at the end of the period of observation is generally larger in the earliest treated cohorts, which suggests that it could be a function of the duration of exposure to the program.

To confirm these initial findings, I plot coefficients for treatment effects estimated by the imputation method of Borusyak, Jaravel, and Spiess (2022) as well as 95% confidence intervals in Panel (b) of Figure 4 (the green dots). Pre-trend coefficients estimated by OLS are also reported (the grey squares). The figure shows that the LPG subsidies had a large impact on the adoption of LPG as a primary cooking fuel. All the pre-trend coefficients are small and the corresponding 95% confidence intervals always include zero. On the other hand, the treatment coefficients are all statistically significant and increase in size over time. In the districts which have been exposed to the FISE subsidies for more than 5 years, eligible households are twenty percentage points more likely to report that LPG is their main cooking fuel than similar households in not-yet-treated treated districts.

Columns (1) to (3) of Table 2 report treatment effects averaged over ten years (from year -2 to year 7). In addition to the average effect on the use of LPG as a primary cooking fuel (column (2)), I also present results for the probability that LPG is one of the household's two main cooking fuels (column (1)) and for the probability of cooking with LPG exclusively (column (3)). The results for

Figure 4: Impact of FISE subisidies on LPG adoption



Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2019 for Panel (a)). Sample: FISE-eligible households. Notes: Panel (a) shows a kernel-weighted local polynomial smooth of the proportion of households using LPG as their primary cooking fuel by treatment cohort. The local mean smoothing uses an Epanechnikov kernel function. Vertical dashed lines correspond to the year in which treatment switches on for each cohort. Panel (b) displays treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy variable equal to one if the household reports that LPG is its primary cooking fuel. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of the survey. In addition to district and year fixed effects, the covariates include the gender and age of the household head, the size of the household, and dummy variables for urban households, for households with access to electricity and for households which own a mobile phone. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Table 2: Average yearly effect of FISE subsidies on fuel switch - DID imputation estimates

	(1) Use LPG	(2) LPG primary	(3) LPG exclusive	(4) Use wood	(5) Wood primary	(6) Wood exclusive
Panel A: All households						
Exposed to FISE	0.066***	0.108***	0.078***	-0.060***	-0.075***	-0.019
	(0.013)	(0.013)	(0.010)	(0.012)	(0.014)	(0.015)
Mean of ouctome	0.394	0.219	0.122	0.795	0.697	0.504
Mean (treatment, pre)	0.161	0.072	0.040	0.855	0.807	0.671
N	137907	137907	137907	137907	137907	137907
Panel B: High treatment intensity vs. never treated						
Exposed to FISE	0.089***	0.124***	0.092***	-0.053***	-0.069***	-0.016
	(0.016)	(0.018)	(0.015)	(0.015)	(0.020)	(0.021)
Mean of ouctome	0.399	0.214	0.119	0.794	0.698	0.496
Mean (treatment, pre)	0.161	0.071	0.039	0.815	0.767	0.633
N	82719	82719	82719	82719	82719	82719
Panel C: Low treatment intensity vs. never treated						
Exposed to FISE	0.045***	0.097***	0.067***	-0.063***	-0.083***	-0.021
	(0.014)	(0.014)	(0.011)	(0.014)	(0.016)	(0.017)
Mean of ouctome	0.368	0.196	0.105	0.834	0.741	0.542
Mean (treatment, pre)	0.162	0.072	0.040	0.877	0.829	0.692
N	98021	98021	98021	98021	98021	98021

Source: Author's calculations using Peru Continuous DHS 2005-2020. Sample: FISE-eligible households. Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. Each column reports the coefficient of interest from a separate imputation. The dependent variable is a dummy variable equal to one if the household reports (1) that LPG is either its primary or secondary cooking fuel, (2) that LPG is its primary cooking fuel, or (3) that it exclusively cooks with LPG. Columns (4) to (6), report results for similar dependent variables in the case of wood fuel. In Panel A, imputation is conducted for the whole sample of FISE-eligible households. In Panel B, the sample is restricted to households residing in high treatment intensity districts and in never treated districts. In Panel C, the sample is restricted to households residing in low treatment intensity districts and in never treated districts. In addition to district and year fixed effects, the covariates include the gender and age of the household head, the size of the household, and dummy variables for urban households, for households with access to electricity and for households which own a mobile phone. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

the whole sample of FISE-eligible households are presented in Panel A and show that the program had a larger effect on the intensive margin than on the extensive margin of LPG use. Indeed, the coefficient in Panel A, column (1), indicates that the overall effect of the program across all cohorts on the adoption of LPG as one of the household's two main cooking fuels is 6.6 percentage points. This corresponds to 40% of the pre-treatment average in treated districts. The overall effect on the use of LPG as a primary cooking fuel is 10.8 percentage points, 150% of the pre-treatment average in the treated group. Interestingly, I find that LPG subsidies also had a large overall effect on the probability of exclusively using LPG for cooking. Households in treated districts are 7.9 percentage points more likely to report doing so than households from the comparison group, almost twice the pre-treatment average. This finding contradicts results from previous studies covering a shorter time period and using a smaller sample size, which generally conclude that most FISE beneficiaries stack LPG with other fuels (Pollard et al. 2018; Calzada and Sanz 2018, see). All coefficients are statistically significant at the 1% level.

In columns (1)-(3) and panels B and C of Table 2, I explore the heterogeneity of the results by treatment intensity. Panel B compares households in districts which were exposed to a high intensity of treatment to households residing in never treated districts. Panel C conducts a similar comparison for households in districts which were exposed to a low intensity of treatment. For each of the three outcomes, the coefficients are larger in high intensity districts. This suggests that the effectiveness of FISE's intervention depended in part on the density of the last-mile LPG distribution network. Once again, all coefficients are statistically significant at the 1% level.

Finally, I repeat the analysis from the first three columns of Table 2 for wood fuel. The results are presented in columns (4) to (6) and show that most of the increase in LPG use is due to changes among wood users. In particular, treated households are on average 6 percentage points less likely to list wood as one of their two main cooking fuels and 7.5 percentage points less likely to report that wood is their primary cooking fuel (Panel A, columns (4) and (5)). There is no effect on the probability of cooking with wood exclusively (Panel A, column (6)). I interpret this result as an indication that the program was most effective among households which were already mixing several types of fuels (wood and charcoal for instance). The effects on wood use are slightly larger in low intensity districts than in high intensity districts (panels B and C, columns (4) and (5)). This might reflect the fact that the baseline fuel mix differed in these two groups. As shown in columns (4) and (5) of panels B and C, the proportion of treated households which used wood as a primary or secondary cooking fuel was 6 percentage points higher in low intensity districts at baseline.

Overall, FISE achieved its main goal of converting low-income Peruvian households to LPG and decreasing the proportion of the population which mainly relies on wood for cooking. The magnitude of its impact is large for both the extensive and the intensive margin of LPG cooking. Using the coefficients from Figure 4 and district populations from the 2017 census of the Peru-

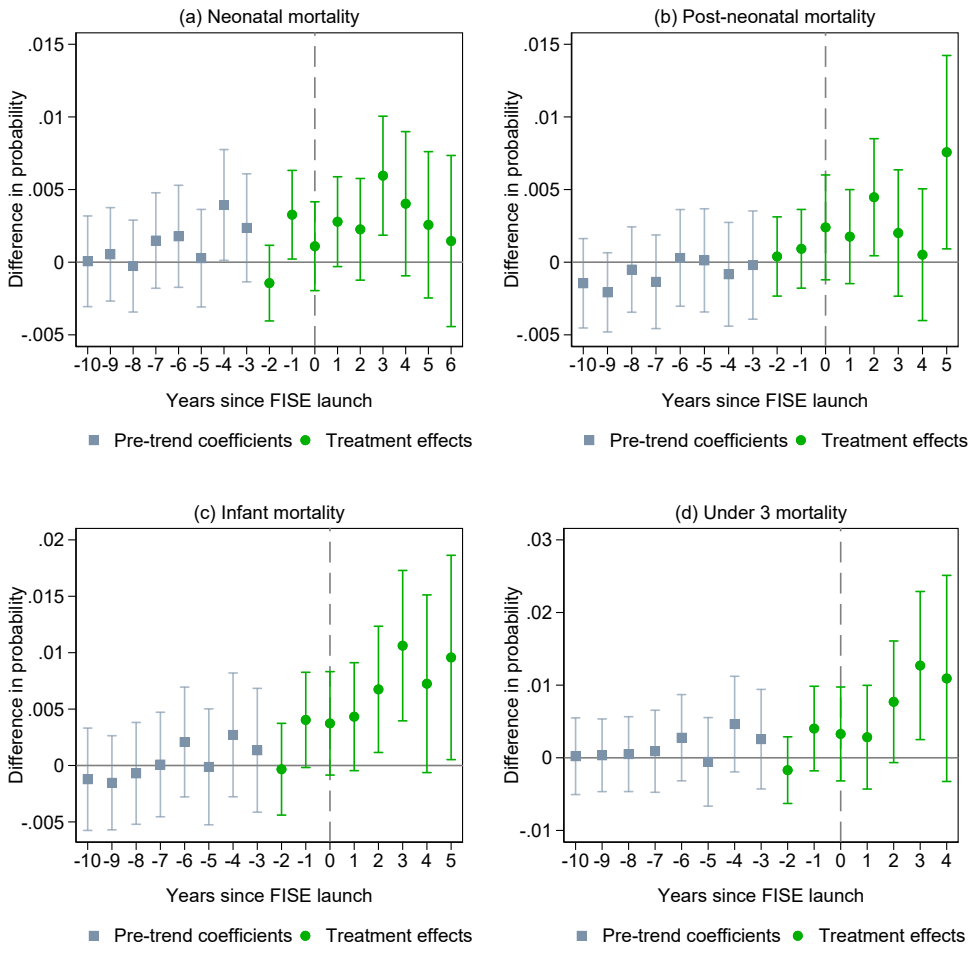
vian population to conduct a back of the envelope calculation, I estimate that 365,000 households adopted LPG as their main cooking fuel as a result of FISE's LPG promotion program. Based on the conservative estimate of 270 million USD disbursed in LPG subsidies between the launch of FISE and the end of 2020, this represents an expenditure of 740 USD per converted household. Was this investment effective in improving child health? I investigate this question in the remainder of this article.

5.2 Effects on early-life mortality

In Figure 5, I report the imputation coefficients and OLS pre-trend coefficients for neonatal mortality, post-neonatal mortality, infant mortality, and early-life mortality. Note that the number of reported lags is reduced for post-neonatal mortality, infant mortality and under-3 mortality because I drop the children who had not reached the corresponding age at the time of the survey. The treatment effects on neonatal mortality in Panel (a) and on post-neonatal mortality in Panel (b) are generally positive but few of the coefficients are statistically significant. Looking at the results for the first twelve months of life in Panel (c) confirms that the intervention had a positive effect on mortality in infancy. Here, the point estimates are large and positive for all treatment periods except the first anticipatory period. A majority of the coefficients are also significant at the 5% level. The pre-trend coefficients are generally much smaller and never statistically different from zero which indicates that the empirical strategy is valid. The results for mortality during the first three years of life in Panel (d) also suggest a positive effect of LPG subsidies on early-life mortality although I only observe them for five time periods after the registration of the first FISE retailer.

Table 3 presents coefficients for the average yearly effect of FISE subsidies on each of the four mortality outcomes. The coefficient in Panel A, column (3), confirms that the intervention had a large positive effect on infant mortality, causing an extra 4.3 infant deaths per 1,000 live births in treated districts relative to not-yet treated districts on average. This represents a 15% of the pre-treatment infant mortality rate in treated areas. This result is significant at the 1% level. The effect on infant mortality is due to increases in neonatal and post-neonatal mortality (Panel A, columns (1) and (2)), which suggests that both *in-utero* exposure and early childhood exposure to air pollution might have increased as a result of the policy. The overall effect on infant mortality is larger in the sub-sample of births registered in districts exposed to a high intensity of treatment (Panel B, column (3)) than in the districts treated with a lower intensity (Panel C, column (3)). This is what I would expect if the observed impacts were caused by the joint effect of the subsidies and of the last-mile network of authorized LPG retailers. This finding is once again reassuring regarding the validity of the identification strategy. Finally, the overall effect on under-3 mortality is not statistically significant in the entire sample of births (Panel A, column (4)) but note that this may in part be due to the fact that I observe a smaller number of treatment lags for this outcome. In addition, there is a large and statistically significant effect on early-life mortality in

Figure 5: Impact of FISE subisidies on mortality in infancy and during early-life



Source: Author's calculations using Peru Continuous DHS 2005-2020. Sample: live births registered between 2000 and 2020 to mothers residing in FISE-eligible households. Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the child died (a) before the age of 1 month, (b) at ages 1 to 11 months, (c) at ages 0 to 11 months, and (d) before reaching the age of 3 years. Children who did not reach the corresponding age by the time of survey are dropped from the sample. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of birth. In addition to district and year fixed effects, the covariates include the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mothers aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Figure 4. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Table 3: Average yearly effect of FISE subsidies on infant mortality and early-life mortality - DID imputation estimates

	(1) Neonatal mortality	(2) Post-neonatal mortality	(3) Infant mortality	(4) Under 3 mortality
Panel A: All births				
Exposed to FISE	0.0021**	0.0019*	0.0043***	0.0031
	(0.0011)	(0.0011)	(0.0016)	(0.0019)
Mean of ouctome	0.014	0.012	0.026	0.033
Mean (treatment, pre)	0.008	0.008	0.028	0.035
N	195591	182600	182600	153084
Panel B: High treatment intensity vs. never treated				
Exposed to FISE	0.0031**	0.0023	0.0056**	0.0059**
	(0.0014)	(0.0015)	(0.0022)	(0.0025)
Mean of ouctome	0.013	0.011	0.025	0.032
Mean (treatment, pre)	0.014	0.014	0.026	0.032
N	109010	101820	101820	85427
Panel C: Low treatment intensity vs. never treated				
Exposed to FISE	0.0018	0.0015	0.0036**	0.00074
	(0.0012)	(0.0013)	(0.0018)	(0.0024)
Mean of ouctome	0.014	0.012	0.027	0.035
Mean (treatment, pre)	0.016	0.016	0.029	0.037
N	139222	130057	130057	109239

Source: Author's calculations using Peru Continuous DHS 2005-2020. Sample: live births registered between 2000 and 2020 to mothers residing in FISE-eligible households. Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. Each column reports the coefficient of interest from a separate imputation. The dependent variable is a dummy equal to one if the child died (1) before the age of 1 month, (2) at ages 1 to 11 months, (3) at ages 0 to 11 months, and (4) before reaching the age of 3 years. Children who did not reach the corresponding age by the time of survey are dropped from the sample. In Panel A, the imputation is conducted for the whole sample of FISE-eligible observations. In Panel B, the sample is restricted to live births in high treatment intensity districts and in never treated districts. In Panel C, the sample is restricted to live births in low treatment intensity districts and in never treated districts. In addition to district and year fixed effects, the covariates include the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mothers aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

high-intensity districts (Panel B, column (4)). In this sub-sample, I estimate that the program caused 6 additional deaths before the age of 3 per 1,000 live births.

Overall, the evidence clearly points in the direction of a large increase in infant mortality as a result of the national effort to convert Peruvian households from wood to LPG. This finding comes as a relative surprise given that the replacement of solid fuels by LPG is generally presented as beneficial for human health and child health by policy makers (see Van Leeuwen, Evans, and Hyseni 2017, for instance). In addition, as previously mentioned, recent impact evaluations of large-scale fuel switches from charcoal or kerosene to LPG concluded to a decrease in infant mortality rates (Cesur, Tekin, and Ulker 2017; Imelda 2020). To find out why a similar policy led to opposite effects in the case of Peru, I start by evaluating the effects of FISE's LPG promotion program on several child health outcomes which are known to be associated with air pollution from cooking fuels.

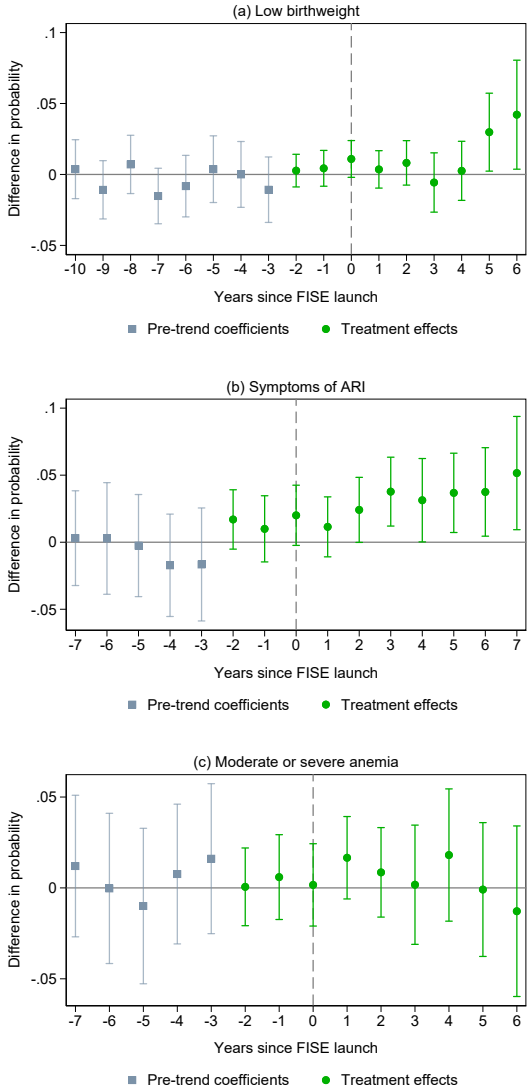
5.3 Effects on health at birth and during childhood

I consider three key health outcomes which are associated with *in-utero* and early childhood exposure to air pollution in the literature: the probability of low birthweight, cases of acute respiratory infections among under-5 children, and the incidence of moderate or severe anemia in children aged 6 to 59 months. If the level of exposure to air pollution among beneficiary households was affected by LPG subsidies, I expect this to be reflected in these air pollution-sensitive outcomes. The results are displayed in Figure 6 and in Table 4. Figure 6, Panel (a), suggests that there is no significant effect of the policy on the probability of low birthweight, except for distant lags. However, the coefficients for the overall effect on the probability of low birthweight reported in column (1) of Table 4 give a slightly different picture. Although the coefficient is statistically insignificant for the whole sample of births (Panel A, column (1)), the results are in fact quite heterogenous and the point estimate for districts exposed to a high treatment intensity shows that the probability of low birthweight is 1.5 percentage points higher on average in these districts in the post-treatment period (Panel B, column (1)). This result is significant at the 5% level.

Turning to health during childhood, the results in Panel (b) of Figure 6 show a large and statistically significant effect of the subsidies on respiratory infections starting three years after the registration of the first authorized FISE retailer. The effect persists over time until the end of the study period. The average yearly effect in the whole sample of under-5 children amounts to 2.6 additional percentage points, a result which is significant at the 1% level (Table 4, Panel A, column (2)). This is 14% of the pre-treatment probability of ARI symptoms in treated districts. The effect is found in both high treatment intensity and low treatment intensity districts. However, the coefficient is twice larger in districts exposed to a high intensity of treatment (Table 4, panels B and C, column (2)).

Finally, there is no evidence of an effect of the treatment on the incidence of moderate or severe

Figure 6: Impact of FISE subisidies on health at birth and during childhood



Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005 and 2007-2019 for Panel (c)). Sample: live births in 5 years preceding the survey to women residing in FISE-eligible households (Panel (a)) or under-5 children whose mother resides in a FISE-eligible household (panels (b) and (c)). Notes: In Panel (c), observations from year 2006 are missing and observations from 2020 are dropped because interviews scheduled after March 16 were conducted by phone. The sample is trimmed at the 1st and 99th percentile of hemoglobin level in this panel. Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the child (a) weighed less than 2.5 kg at birth, (b) experienced symptoms of ARI in the 15 days preceding survey, and (c) has an hemoglobin count lower than 10 grams/deciliter. Hemoglobin level only measured for children aged 6 to 59 months. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of birth (Panel (a)) or at the time of survey (panels (b) and (c)). In addition to district and year fixed effects, the covariates include the child's age in months (except in Panel (a)), the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mothers aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Figure 4. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Table 4: Average yearly effect of FISE subsidies on health at birth and during childhood - DID imputation estimates

	(1) Low birth weight	(2) Symptoms of ARI	(3) Moderate/severe anemia
Panel A: All observations			
Exposed to FISE	0.0059	0.026***	0.0035
	(0.0056)	(0.010)	(0.012)
Unit of observation	Birth	Child	Child
Mean of ouctome	0.086	0.156	0.152
Mean (treatment, pre)	0.092	0.180	0.170
N	63107	66259	51175
Panel B: High treatment intensity vs. never treated			
Exposed to FISE	0.015**	0.036**	-0.00047
	(0.0074)	(0.015)	(0.019)
Unit of observation	Birth	Child	Child
Mean of ouctome	0.086	0.146	0.152
Mean (treatment, pre)	0.089	0.153	0.167
N	35939	36679	28276
Panel C: Low treatment intensity vs. never treated			
Exposed to FISE	0.00064	0.018*	0.011
	(0.0063)	(0.011)	(0.012)
Unit of observation	Birth	Child	Child
Mean of ouctome	0.088	0.161	0.155
Mean (treatment, pre)	0.093	0.191	0.172
N	43733	48300	37644

Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005 and 2007-2019 for Panel (c)). Sample: live births in the 5 years preceding the survey to mothers residing in FISE-eligible households (column (1)) or under-5 children whose mother resides in a FISE-eligible household (columns (2) and (3)). Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. In column (3), observations from year 2006 and observations from 2020 are dropped because interviews scheduled after March 16 were conducted by phone due to the covid crisis. The sample is trimmed at the 1st and 99th percentile of hemoglobin level in this column. Each column reports the coefficient of interest from a separate imputation. The dependent variable is a dummy equal to one if the child (1) weighed less than 2.5 kg at birth, (2) experienced symptoms of ARI in the 15 days preceding the survey, and (3) has an hemoglobin count lower than 10 grams per deciliter. Hemoglobin level is only measured for children aged 6 to 59 months. In Panel A, the imputation is conducted for the whole sample of FISE-eligible observations. In Panel B, the sample is restricted to observations in high treatment intensity districts and in never treated districts. In Panel C, the sample is restricted to observations in low treatment intensity districts and in never treated districts. In addition to district and year fixed effects, the covariates include the child's age in months (except in column (1)), the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

anemia in children (Figure 6, Panel (c), and Table 4, column (3)). However, note that anemia is not measured for children aged less than 6 months, the age group which is most sensitive to air pollution. Despite the absence of effect on anemia, the evidence from the analysis of child health outcomes is consistent with a scenario in which exposure to air pollution increased in treated districts as a result of a greater access to LPG. The large effect on the probability of reporting symptoms of ARI for children under 5 in particular is an indication that the incidence of respiratory infections increased in the treated group, a fact which could explain the impact of the program on infant mortality.

5.4 Effects on anemia in adult women

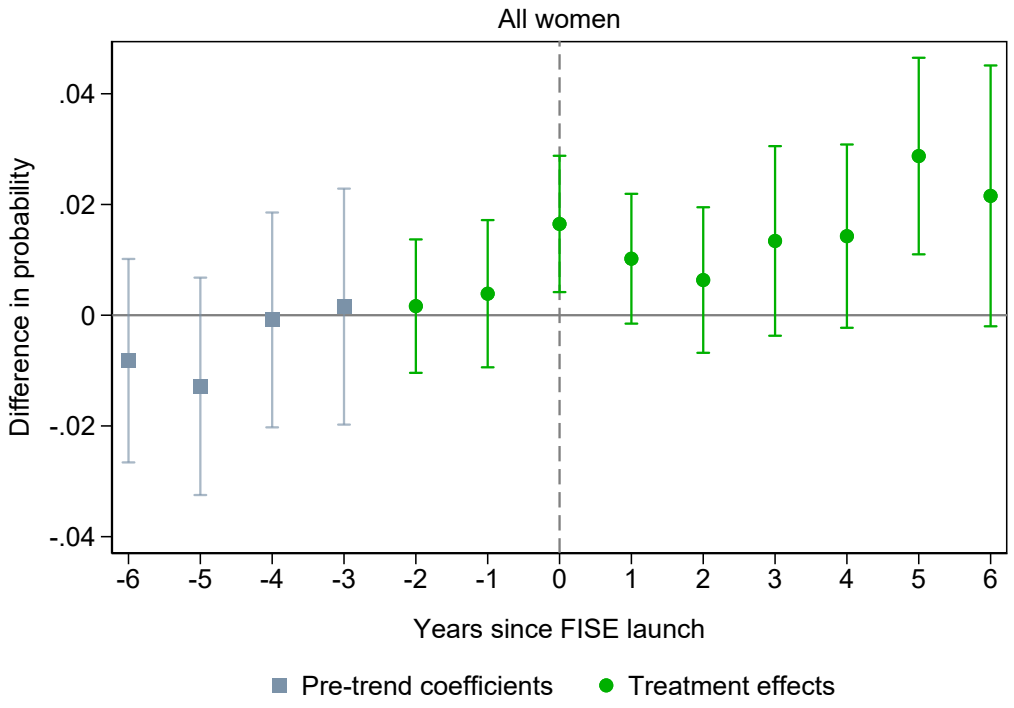
The richness of the Peru Continuous DHS data allows me to also study the impact of FISE subsidies on the health of adult women, the population group in charge of most cooking activities. In particular, the DHS survey measured hemoglobin levels for all the women who answered the individual questionnaire over the period 2005-2019 except in 2006. I am therefore able to estimate treatment effects on anemia in adult women. This is relevant because the risk of anemia increases significantly with age (Patel 2008). The effects of the increase in air pollution which seems associated with the intervention may thus be easier to identify in adults than in children, and especially in the group of adults which is most exposed to this pollution.

Figure 7 and Table 5 confirm this intuition. Women who reside in treated districts have a 1 percentage point higher probability of anemia than they would absent the treatment (Table 5, Panel A, column (1)). This effect is entirely driven by women aged 35 to 49 for whom the treatment effect amounts to 2.2 percentage points, a third of the baseline incidence rate of anemia in this group (Table 5, Panel A, column (3)). The effect is found in both high treatment intensity districts and low treatment intensity districts. However, the magnitude of the effect is once again much larger in high treatment intensity areas (Table 5, panels B and C, column (3)). There is no treatment effect on women aged less than 35 in the whole sample (Table 5, Panel A, column (2)). However, there is one for women aged less than 35 in low treatment intensity districts, which explains that the overall coefficient in low treatment intensity districts is statistically significant (Table 5, Panel C, column (1)). There is thus a causal effect of the treatment on anemia in adult women and more particularly on women aged 35 and more. The sign of this effect is the one we would expect if these women were exposed to more air pollution as a result of the intervention. This is consistent with the findings from previous sub-sections which all point to a negative effect of treatment on air pollution and associated health outcomes.

5.5 Mechanisms

How could a switch from wood fuel cooking to cooking with a cleaner fuel such as LPG result in an increase in exposure to air pollution? In what follows, I argue that this unexpected outcome was caused by three facts. First, as mentioned earlier in this article, the common description of LPG as

Figure 7: Impact of FISE subisidies on moderate or severe anemia among adult women



Source: Author's calculations using Peru Continuous DHS 2005 and 2007-2019. Sample: women aged 18-49 and residing in FISE-eligible households at the time of survey. Notes: Sample trimmed at the 1st and 99th percentile of hemoglobin level. The graph displays treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the woman has an hemoglobin count lower than 11 grams per deciliter (10 grams for pregnant women). Observations from year 2006 are missing. Observations from year 2020 are dropped because interviews scheduled after March 16 were conducted by phone due to the covid crisis and hemoglobin levels are missing for these interviews. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of survey. In addition to district and year fixed effects, the covariates include the woman's age, BMI, and smoking status, as well as the set of household-level control variables defined in Figure 4. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Table 5: Average yearly effect of FISE subsidies on the probability of moderate or severe anemia among adult women - DID imputation estimates

	(1) All	(2) 18-34 y.o	(3) 35-49 y.o
Panel A: All observations			
Exposed to FISE	0.012**	0.00079	0.022***
	(0.0055)	(0.0072)	(0.0084)
Mean of ouctome	0.065	0.061	0.071
Mean (treatment, pre)	0.061	0.057	0.066
N	79470	47368	32102
Panel B: High treatment intensity vs. never treated			
Exposed to FISE	0.0077	-0.017	0.037***
	(0.0084)	(0.011)	(0.011)
Mean of ouctome	0.063	0.058	0.069
Mean (treatment, pre)	0.056	0.052	0.064
N	45797	26882	18915
Panel C: Low treatment intensity vs. never treated			
Exposed to FISE	0.015***	0.012*	0.016*
	(0.0057)	(0.0069)	(0.0091)
Mean of ouctome	0.067	0.064	0.073
Mean (treatment, pre)	0.064	0.062	0.068
N	56594	33648	22946

Sample: women residing in FISE-eligible households at the time of survey. Sample trimmed at the 1st and 99th percentile of hemoglobin level. Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. Each column reports the coefficient of interest from a separate imputation.The dependent variable is a dummy equal to one if the woman has an hemoglobin count lower than 11 grams per deciliter (10 grams for pregnant women). Observations from year 2006 are missing. Observations from year 2020 are dropped because interviews scheduled after March 16 were conducted by phone due to the covid crisis and hemoglobin levels are missing for these interviews. In Panel A, the imputation is conducted for the whole sample of FISE-eligible observations. In Panel B, the sample is restricted to women in high treatment intensity districts and in never treated districts. In Panel C, the sample is restricted to women in low treatment intensity districts and in never treated districts. In addition to district and year fixed effects, the covariates include the woman's age, BMI, and smoking status, as well as the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

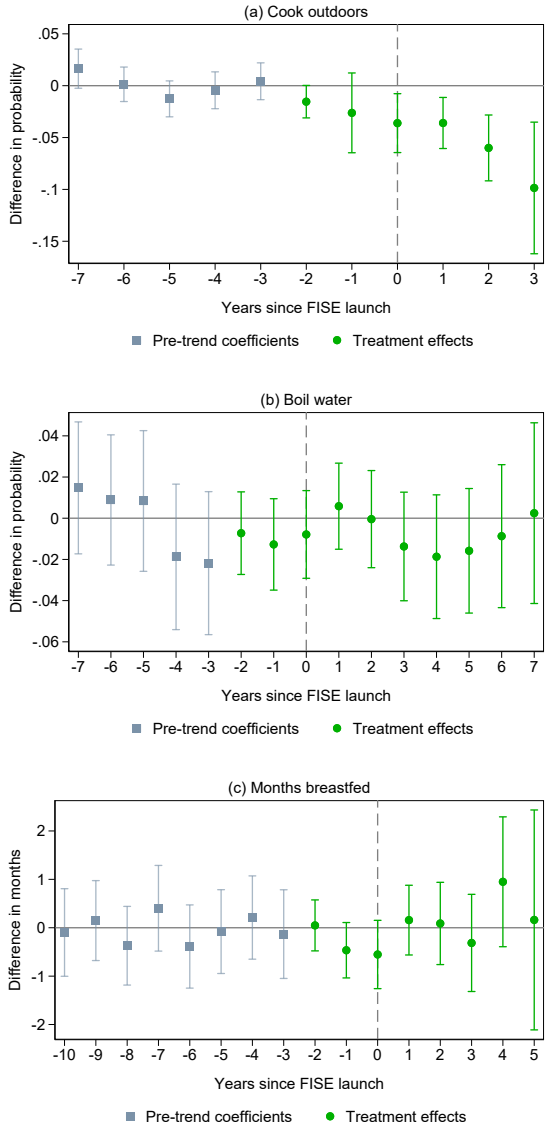
a "clean" fuel is somewhat misleading since its combustion emits nitrogen dioxide, a pollutant which is known to increase the susceptibility of respiratory infections (Jarvis et al. 2010). A recent field experiment conducted in Peru shows that kitchen area concentrations of nitrogen dioxide exceed the WHO hourly guideline 1.3 hours per day on average in households which cook exclusively with LPG (Kephart et al. 2021). Second, LPG stoves tend to be used mainly indoors whereas Peruvian households traditionally cook both indoors and outdoors when they use firewood (Rhodes et al. 2014). It is therefore very likely that the adoption of LPG resulted in a significant increase of the time spent indoors by mothers and their children. Third, in the homes made of traditional materials in which FISE-eligible households live, indoor cooking areas are rarely equipped with a modern ventilation system. Kitchens without windows are not uncommon (Kephart et al. 2021). If a shift towards more indoor cooking occurred, it would therefore have been associated with a dramatic decrease in the ventilation quality of the cooking area.

The combined effect of these three factors is potentially dramatic. For the households which entirely stopped burning wood as a result of FISE subsidies, the change in ventilation quality would partially offset the benefits of the fuel switch. In this case, the reduction in exposure to fine particles caused by the adoption of LPG would come at the cost of an increase in exposure to nitrogen dioxide due to the indoor combustion of LPG. The consequences would be much worse in the case of households stacking LPG with wood. In this case, both the exposure to particulate matter and to nitrogen dioxide could increase as a result of the higher frequency of indoor cooking. In a different context, Bensch and Peters (2015) report that the dissemination of an improved cookstove which facilitated outdoor cooking resulted in a reduction in smoke-related disease symptoms.

To confirm this interpretation, I start by assessing the effect of the intervention on the proportion of households whose primary cooking area is located outdoors. Unfortunately, the Peru Continuous DHS questionnaire does not collect information on secondary cooking areas or on the frequency of indoor and outdoor cooking. Nevertheless, the findings for the main cooking area are likely to reflect a broader trend which would also affect secondary cooking areas and the overall frequency of outdoor cooking. Another limitation of the Peru Continuous DHS data is that information on the location of the main cooking area was only collected until 2015. As a result, I can only report treatment effects for the first four relative time periods after the registration of the first authorized FISE retailer.

I also test two alternative hypotheses which could explain the effect of LPG adoption on infant mortality. First, I evaluate the effect of the fuel switch on the practice of boiling drinking water for disinfection. More than 70% of households from treated districts report that they boil their drinking water in the pre-treatment period. If this practice became less frequent as a result of the intervention, it could have affected infant mortality through diarrheal diseases. Second, I test the hypothesis that LPG adoption led to a reduction in the duration of breastfeeding as a result of the greater ease of preparing meals for children. Changes in breastfeeding practices could have affected

Figure 8: Impact of FISE subisidies on outdoor cooking, water disinfection and breastfeeding



Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (a)). Sample: FISE-eligible households (panels (a) and (b)) or live births in the 5 years preceding the survey to women residing in FISE-eligible households (panel (c)). Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). In panels (a) and (b), the dependent variable is a dummy equal to one if the household (a) reports that its primary cooking area is located outdoors, or (b) declares that it boils water for disinfection. In Panel (c), the dependent variable is the number of months of breastfeeding reported by the mother for children above 40 months of age at the time of survey. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of survey (panels (a) and (b)) or at the time of birth (Panel (c)). In addition to district and year fixed effects, the covariates include the set of household-level control variables defined in Figure 4. In Panel (c), the estimation also controls for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mothers aged less than 18 and for children born to mothers who report smoking. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Table 6: Average yearly effect of FISE subsidies on cooking habits, water disinfection and breastfeeding - DID imputation estimates

	(1) Cook outdoors	(2) Boil water	(3) Months breastfed
Panel A: All observations			
Exposed to FISE	-0.035***	-0.0080	-0.13
	(0.011)	(0.010)	(0.24)
Unit of observation	Household	Household	Birth
Mean of ouctome	0.063	0.711	18.737
Mean (treatment, pre)	0.069	0.651	18.728
N	78526	142476	24099
Panel B: High treatment intensity vs. never treated			
Exposed to FISE	-0.057***	0.0011	-0.040
	(0.020)	(0.015)	(0.35)
Unit of observation	Household	Household	Birth
Mean of ouctome	0.057	0.737	19.133
Mean (treatment, pre)	0.066	0.656	19.361
N	46850	85334	13391
Panel C: Low treatment intensity vs. never treated			
Exposed to FISE	-0.016*	-0.019*	-0.30
	(0.0086)	(0.011)	(0.26)
Unit of observation	Household	Household	Birth
Mean of ouctome	0.062	0.715	18.603
Mean (treatment, pre)	0.071	0.649	18.407
N	56075	101109	17145

Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (a)). Sample: FISE-eligible households (columns (1) and (2)) or live births in the 5 years preceding the survey to women residing in FISE-eligible households (column (3)). Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. Each column reports the coefficient of interest from a separate imputation. In columns (1) and (2), the dependent variable is a dummy equal to one if the household (1) reports that its main cooking area is located outdoors, or (2) declares that it boils water for disinfection. In column (3), the dependent variable is the number of months of breastfeeding reported by the mother for children above 40 months of age. In Panel A, the imputation is conducted for the whole sample of FISE-eligible observations. In Panel B, the sample is restricted to observations in high treatment intensity districts and in never treated districts. In Panel C, the sample is restricted to observations in low treatment intensity districts and in never treated districts. In addition to district and year fixed effects, the covariates include the set of household-level control variables defined in Table 2. In column (3), the estimation also controls for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mothers aged less than 18 and for children born to mothers who report smoking. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

the nutritional status of children which, in turn, could be a cause of increased early-life mortality. I estimate Equation (1) for the duration of the breastfeeding period in months for the sub-sample of births which occurred more than 40 months before the survey date. The 40-month recall period corresponds to the 99th percentile of breastfeeding duration.

The results of this analysis are presented in Figure 8. They provide support for the hypothesis that a change in the frequency of outdoor cooking might explain the negative effects LPG subsidies on child health and on early-life mortality. Panel (a) shows that the proportion of households which report cooking outdoors decreases continuously in the years which follow the registration of the first authorized FISE retailer in a district. The OLS pre-trend coefficients support the parallel trends assumption. On the other hand, there is no evidence of an effect of the intervention on water boiling and breastfeeding practices (Figure 8, panels (b) and (c)). The average yearly coefficients reported in Table 6 show that the proportion of households whose main cooking area is located outdoors decreased by 3.5 percentage points as a result of LPG subsidies. This result is significant at the 1% level. Although the magnitude of this change may seem relatively small compared to the large treatment effects on LPG adoption, notice once again that I only observe the location of the main cooking area and that the frequency of outdoor cooking might have decreased in households which previously used a secondary outdoor cooking area. In addition, the treatment effect would most likely be larger if additional time periods could be included in the analysis. The heterogeneity analysis in panels B and C of Table 6 shows that the treatment effect on outdoor cooking is mainly driven by households from high treatment intensity districts.

To provide additional support for the finding that the treatment effects on child health and infant mortality are due to a lower frequency of outdoor cooking, I study the heterogeneity of the results by ventilation quality of the dwelling. I define houses equipped with a hood or a chimney as houses "with ventilation". Houses or apartments which have no hood and no chimney are classified as houses "without ventilation". I conduct this analysis for infant mortality, for child health outcomes and for anemia in women. The results should be interpreted with caution due to several data limitations. First, the information on ventilation quality is only available for the years 2009 to 2020. Second, this information is only recorded for the households which report using at least one solid fuel (i.e. wood, charcoal, dung or agricultural waste). This means that I do not observe the ventilation quality of the dwelling for the households which cook exclusively with LPG. This is a source of selection bias. The results presented in Table 7 nevertheless provide additional suggestive evidence regarding the role of cooking area ventilation as a key mechanism explaining the negative effect of LPG adoption on infant mortality. The treatment effects on the probability of low birthweight, on symptoms of ARI and on anemia in adult women are concentrated in the households which have no access to ventilation (Panel B, columns (2), (3) and (7)). The coefficient on infant mortality is insignificant in both sub-samples, but it is 1.5 times larger for the households without ventilation (panels A and B, column (1)). There is no effect of treatment on child anemia

overall and in each of the sub-samples.

Taken together, the results from Table 6 and Table 7 draw a picture in which new LPG adopters reduced the frequency at which they cooked outdoors and started spending more time in poorly ventilated dwellings which could be polluted by nitrogen dioxide from LPG combustion, or by wood smoke from indoor cooking and heating. Such a scenario would also explain why the impact of the switch to gas cooking on infant mortality was different in Peru and in the other countries where a similar transition was evaluated. In Indonesia, Imelda (2020) evaluates the effect of a switch to LPG in a population which mainly cooked with kerosene at baseline and finds that the introduction of LPG led to a decrease in infant mortality. Kerosene stoves are much more polluting than LPG stoves but they tend to be adapted to indoor cooking, just like LPG stoves. Imelda does not discuss the effect of the intervention on the choice of cooking area but a switch from kerosene to LPG seems less likely to induce a shift towards more indoor cooking than a transition from wood to LPG. If Indonesian kerosene users simply replaced a polluting fuel by a less polluting one while ventilation quality remained constant, it is not surprising to find that the change had a positive effect on exposure to air pollution and related health outcomes. Similarly, in the case of Turkey, Cesur, Tekin, and Ulker (2017) find that the replacement of charcoal by natural gas was followed by a decrease in infant mortality. Charcoal is also more suited to indoor cooking than wood and the intervention seems less likely to have induced major changes in choices of cooking area than in the case of Peru. This interpretation of the heterogenous impacts of gas cooking adoption across countries is supported by previous work by Langbein, Peters, and Vance (2017) who review the literature on particulate matter concentration in the cooking area and report that for a given biomass cooking technology, the concentration can be 40 to 80% lower when cooking takes place outdoors rather than indoors.

To summarize, when a clean cooking intervention reduces pollutant emissions without affecting the average ventilation quality in cooking areas, it is likely to have beneficial health impacts. On the other hand, the health effects are harder to predict in the case of an intervention which would reduce emissions of particulate matter and other pollutants while also affecting ventilation quality, for instance by inducing users to cook indoors rather than outdoors. The Peruvian case suggests that these effects can be negative.

5.6 Robustness checks and additional results

5.6.1 Falsification test

To provide additional evidence in support of the main results presented in previous sections, I conduct a falsification test in which I evaluate the effect of the treatment on the incidence of diarrheal diseases and of acute malnutrition. These two health conditions are unrelated to air pollution. Consumption of unsafe water and oral contacts with contaminated objects are the main routes of transmission of diarrheal diseases to young children (Zwane and Kremer 2007). A change of cook-

Table 7: Average yearly effect of FISE subsidies on mortality and health outcomes - By kitchen ventilation quality, DID imputation estimates

	(1) Infant mortality	(2) Low birthweight	(3) Symptoms of ARI	(4) Anemia in children	(5) Anemia in women
Panel A: All solid fuel users					
Exposed to FISE	0.0032*	0.0082	0.025**	0.0086	0.0098*
	(0.0017)	(0.0058)	(0.0099)	(0.011)	(0.0059)
Unit of observation	Birth	Birth	Child	Child	Woman
Mean of ouctome	0.027	0.089	0.158	0.153	0.065
Mean (treatment, pre)	0.028	0.093	0.181	0.168	0.071
N	157575	51727	55644	42877	68180
Panel B: Solid fuel users - Home without ventilation					
Exposed to FISE	0.0031	0.017**	0.032***	0.0083	0.016**
	(0.0023)	(0.0077)	(0.012)	(0.012)	(0.0074)
Unit of observation	Birth	Birth	Child	Child	Woman
Mean of ouctome	0.028	0.090	0.168	0.155	0.069
Mean (treatment, pre)	0.029	0.095	0.189	0.162	0.071
N	99376	33260	37204	28569	43480
Panel C: Solid fuel users - Home with ventilation					
Exposed to FISE	0.0021	0.0022	-0.0036	0.0088	-0.0025
	(0.0027)	(0.0098)	(0.018)	(0.016)	(0.0089)
Unit of observation	Birth	Birth	Child	Child	Woman
Mean of ouctome	0.025	0.089	0.137	0.150	0.059
Mean (treatment, pre)	0.027	0.090	0.157	0.185	0.069
N	58176	18030	17088	13264	23667

Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005 and 2007-2019 for columns (4)-(5)). Sample: live births registered between 2000 and 2020 to women residing in FISE-eligible households (column (1)), live births registered in the 5 years preceding the survey to women residing in FISE-eligible households (column (2)), under-5 children whose mother resides in a FISE-eligible household (columns (3)-(4)) or women residing in FISE-eligible households (column (5)). Notes: In columns (4) and (5), observations from year 2006 are missing and observations from 2020 are dropped because interviews scheduled after March 16 were conducted by phone due to the covid crisis. The sample is trimmed at 1st and 99th percentile of hemoglobin level. Each column reports the coefficient of interest from a separate imputation. The dependent variable is a dummy equal to one if the child (1) died at ages 0 to 11 months, (2) weighed less than 2.5 kg at birth, (3) experienced symptoms of ARI in the 15 days preceding the survey, or (4) has an hemoglobin count lower than 10 grams per deciliter. In column (5), the dependent variable is a dummy equal to one if the women has an hemoglobin count lower than 11 grams per deciliter (10 grams for pregnant women). Hemoglobin level is only measured for children aged 6 to 59 months. In Panel A, the imputation is conducted for the whole sample of FISE-eligible observations. In Panel B, the sample is restricted to observations in homes without ventilation. In Panel C, the sample is restricted to observations in homes with ventilation. In addition to district and year fixed effects, columns (1)-(4) control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mothers aged less than 18 and for children born to mothers who report smoking. In columns (3)-(4), the child's age in months is added to the list of individual controls. In column (5), individual controls include the woman's age, BMI, and smoking status. All imputations also control for the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

ing fuel is also unlikely to induce cases of acute malnutrition and obtaining significant coefficients for this outcome would suggest that identification is confounded by unobserved exogenous shocks. In Panel (a) of Appendix Figure B.1, I present imputation estimates of treatment effects on the probability of reporting an event of diarrhea in the two weeks preceding the survey for children aged less than 5. Panel (b) of Figure B.1, shows estimates of treatment effects on the probability of being wasted at the time of survey for the same sub-sample. I use the standard definition of wasting as having a weight-for-height z-score less than minus 2 standard deviations below the mean of the WHO Child Growth Standards (Croft, Marshall, and Allen 2018). The results suggest that there is no effect of the intervention on these two outcomes. This is confirmed by the overall coefficients presented in Appendix Table C.1 which are both statistically insignificant and close to zero. Thus, the intervention seems to only affect health outcomes which are known to be induced by air pollution.

5.6.2 Selection issues

The treatment effects might result from selection effects induced by the intervention. In particular, if vulnerable mothers moved to treated districts in order to benefit from LPG subsidies, the increase in infant mortality and in self-reported symptoms of respiratory infections might be due to a change in the composition of the population of these districts. In Appendix Figure B.2, I re-estimate my main specification for these two outcomes after children whose mothers report having moved to a new address between the district's initial treatment date and the survey date have been dropped from the sample. Note that this restriction indifferently drops children whose mothers moved between districts and children of mothers who moved within the same district. It is therefore excessively conservative. Unfortunately, the information needed to differentiate between these two groups was not collected as part of the Peru Continuous DHS. However, the event study coefficients are generally similar to the initial results for both infant mortality and symptoms of ARI. The coefficients in columns (1) and (2) of Appendix Table C.2 confirm this visual impression. This is reassuring for the identification strategy and suggests that selection into treatment through migration is unlikely to bias the results.

5.6.3 Effects on ineligible households

Although my main analysis was performed in the sub-sample of households which satisfied FISE's eligibility criteria at the time of the survey, it is important to consider the effect of the treatment in the group of ineligible households. This is not a placebo test because ineligible households might have been indirectly affected by the policy through externalities from eligible households or through general equilibrium effects for instance. Greater demand for LPG refills in the group of eligible households might have triggered a price increase on non-subsidized refills, thus decreasing LPG use rates in the ineligible group. On the other hand, informal reselling of subsidized refills by

beneficiary households could also have lowered the price of LPG for some ineligible households. The effect of the policy on the ineligible group is thus *a priori* ambiguous. However, finding similar effects in ineligible and ineligible households would cast doubt on my main results since most treatment effects should be concentrated in the group which directly benefited from the intervention.

Appendix Figure B.3 and Table C.3 show that there are no overall treatment effects in the group of ineligible households. All the coefficients for average yearly effects on the main outcomes of interest are statistically insignificant in Table C.3 and the large sample sizes suggest that this is not due to a lack of power. Looking at the trends in Figure B.3 shows that the program led to a decrease in LPG use rates after several years since the coefficients on the last lags in Panel (a) are statistically significant and negative. It is possible that wealthier households switched from LPG to natural gas after some years since FISE started offering subsidies for connections to the natural gas network shortly after the beginning of the LPG promotion program. Wealthier households are more likely to live in areas which are served by natural gas networks. There are a couple of statistically significant coefficients on infant mortality in Panel (b) of Figure B.3 around the time of treatment start. However, these effects do not persist over time and decrease continuously towards 0 between period 1 and period 6, which corresponds to the period in which ineligible households start abandoning LPG. The coefficients on outdoor cooking and on symptoms of ARI are almost always small and statistically not different from zero (Figure B.3, panels (b) and (d)). Generally speaking, the treatment effects on the main outcomes of interest seem to be specific to the group of eligible households.

5.6.4 Alternative treatment definition

As discussed earlier in this article, the main definition of treatment used in the analysis leaves space for contamination of the comparison group by households which received FISE LPG vouchers while residing in districts which did not host any authorized LPG retailer. This would most likely lead to downward biased treatment coefficients but one cannot exclude worse problems if the extent of the contamination is large. To assess this issue, I re-estimate my coefficients with a different treatment definition for the key variables of interest in Appendix Figure B.4. In these estimates, a district is considered treated in time t if it hosts at least one authorized LPG retailer or if there is another district with a FISE retailer within a 15km radius of its centroid. The results are generally qualitatively very similar. However, as expected, treatment effects tend to be larger than in the initial specification, especially for distant lags. This suggests that the results obtained with the main definition of treatment are most likely conservative estimates of the actual treatment effects.

5.6.5 Additional covariates

Next, I add several additional control variables to my main model to verify that the results are not sensitive to the choice of covariates. I focus on variables which account for the households' income

level and level of education, as well as on potential predictors of child mortality. More specifically, I control for the household head's level of education, and I add indicators for households belonging to the poorest wealth quintile, for households with access to tapwater and for households with access to modern toilet facilities. Appendix Figure B.5 shows that the results are robust to these additional controls.

5.6.6 Excluding the covid crisis period

Most of the estimates presented in this article use data collected during 2020, the year of the covid crisis. Peru is among the countries which were most significantly affected by this epidemic and, as a result, some of the interviews scheduled in 2020 were conducted by phone. This concerns approximately 25% of the sample. Because data quality may be lower in the case of phone interviews, I drop the observations which correspond to interviews conducted in 2020 and re-estimate my main model with the remaining sub-sample. The results are displayed in Appendix Figure B.6 for the adoption of LPG as a primary cooking fuel (Panel (a)), infant mortality (Panel (b)) and symptoms of ARI (Panel (c)). I do not re-estimate coefficients for outdoor cooking because the data for this outcome are only available for the period 2005-2015. The results are robust to the sample restriction for the three outcomes of interest.

5.6.7 Results from conventional event study

Finally, I check the robustness of my main results to using an alternative estimation method. To do so, I estimate the event study model from Equation 1 via OLS. In this case, the test of pre-trends and the estimation of treatment coefficients are conducted in a single stage, by including dummies for pre-treatment periods in the model. The last time-period before treatment switches on is dropped due to collinearity. I am able to report more pre-treatment time periods because the placebo test is better powered with this estimation method. The results displayed in Appendix Figure B.7 are qualitatively similar to those obtained with the imputation estimator of Borusyak, Jaravel, and Spiess (2022) for all outcomes. However, the treatment coefficients are generally smaller in magnitude when estimated with the standard TWFE model suggesting that the arbitrary weights on district-period effects tend to bias the estimates downwards (Borusyak, Jaravel, and Spiess 2022). Nevertheless, this robustness check shows that the main conclusions of the analysis would not be very different with a more conventional estimator.

6 Conclusion

LPG is being actively promoted as a clean cooking fuel in developing countries, yet we have limited understanding of the extent to which its adoption might actually benefit the targeted population groups. In this article, I attempt to fill this knowledge gap with a specific focus on the implications

of LPG adoption for the third of the world population which still cooks with wood. To do so, I study Peru's FISE LPG promotion program, one of the largest LPG conversion programs to date, over a period of ten years (2010-2020). Using data from 16 waves of the Peru Continuous DHS survey, I estimate that the program's average yearly effect on the adoption of LPG as a primary cooking fuel amounts to 10.8 percentage points, 1.5 times the usage rate in the eligible group before treatment. Unfortunately, this success did not translate into health improvements, at list for the youngest beneficiaries. On the contrary, the incidence of respiratory infections among under-5 children and infant mortality rates both increased by roughly 15% compared to pre-treatment levels. A back of the envelope calculation conducted with the treatment effect coefficients from the main estimation suggests that the policy caused approximately 6,600 additional infant deaths during the whole study period, in a country which registers less than 600,000 births per year. Thus, while Peru's infant mortality rate dropped by approximately 33% between 2010 and 2020, from 15 to 10 deaths per 1,000 live births (World Bank 2022a), it would probably have dropped even faster if the intervention had been implemented differently.

These disheartening results are most likely due to behavioral changes induced by the use of LPG. I show that the policy led to a decrease in rates of outdoor cooking in treated districts which suggests that women and their children started spending more time indoors after the introduction of LPG. In households which stack LPG with solid fuels, the increased frequency of indoor cooking is likely to have disastrous effects on exposure to air pollution since concentrations of air pollutants can be 40 to 80% lower when cooking takes place outdoors rather than indoors for a given fuel (Langbein, Peters, and Vance 2017). Even for households which fully switch to LPG the net effect of the adoption of LPG may not be positive because the decrease in exposure to fine particles from wood combustion could be partly offset by indoor emissions of other pollutants from LPG combustion such as nitrogen dioxide. This interpretation is supported by a heterogeneity analysis which shows that the detrimental effects of the intervention on several health outcomes are concentrated in the households which live in homes without ventilation.

An important limitation of the analysis is that I do not directly observe personal levels of exposure to various air pollutants in the sample of eligible households. Thus, I am forced to rely on indirect evidence to establish that LPG adoption impacted infant mortality rates through the channel of increased air pollution. However, several findings support my main results. First, the policy also caused an increase in the incidence of moderate and severe anemia among adult women. This is what I would expect in a situation of increased exposure to air pollution because several air pollutants have an inflammatory effect which affects the production of blood cells and increases the risk of anemia (Honda et al. 2017). Second, I find no effect of treatment on the most obvious alternative channels through which LPG adoption might have induced an increase in infant mortality: changes in breastfeeding habits and in children's nutritional status or modifications of water boiling habits and of the associated risk of diarrheal diseases. Third, ineligible households

residing in treated districts were largely unaffected by the policy. And fourth, I show that the effects are unlikely to be caused by selective migration of vulnerable households from untreated to treated districts. Taken together, these results leave little space for competing explanations of the negative health effects of the LPG promotion program.

Another caveat concerns the fact that the article mainly covers early-childhood health and mortality outcomes and says relatively little about the effects of the intervention on other age groups. While this comment certainly warrants further research on Peru's FISE program, note that several important observations can be made regarding other age groups with the results at hand. On the one hand, the positive effect of the intervention on anemia in adult women suggests that older age groups were also negatively affected by the change in cooking habits associated with LPG uptake. On the other hand, even if positive health effects of the intervention existed for some age groups, they would need to be incredibly large to offset the disability-adjusted life years lost in the youngest population group.

To conclude, the findings of this paper do not necessarily imply that LPG promotion policies are inherently flawed, at least when considered through the lens of public health. Rather, they suggest that greater attention should be paid to the context in which these policies are implemented to ensure that they deliver positive health impacts. In areas where cooking activities frequently take place outdoors, and thus in particular in communities which mainly cook with wood, policy makers will certainly obtain better results if they combine the promotion of LPG or other clean fuels with interventions aimed at optimizing ventilation quality for cooks and their relatives. This could involve informing households of the adverse health effects of smoke (see Abdel Sater et al. 2021; Afridi, Debnath, and Somanathan 2021, for recent examples) and of the benefits of ventilation or outdoor cooking, selecting LPG stoves which can easily be used outdoors, installing ventilation systems in kitchens, or a combination of these.

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Appendix A - Characteristics of Peru Continuous DHS surveys

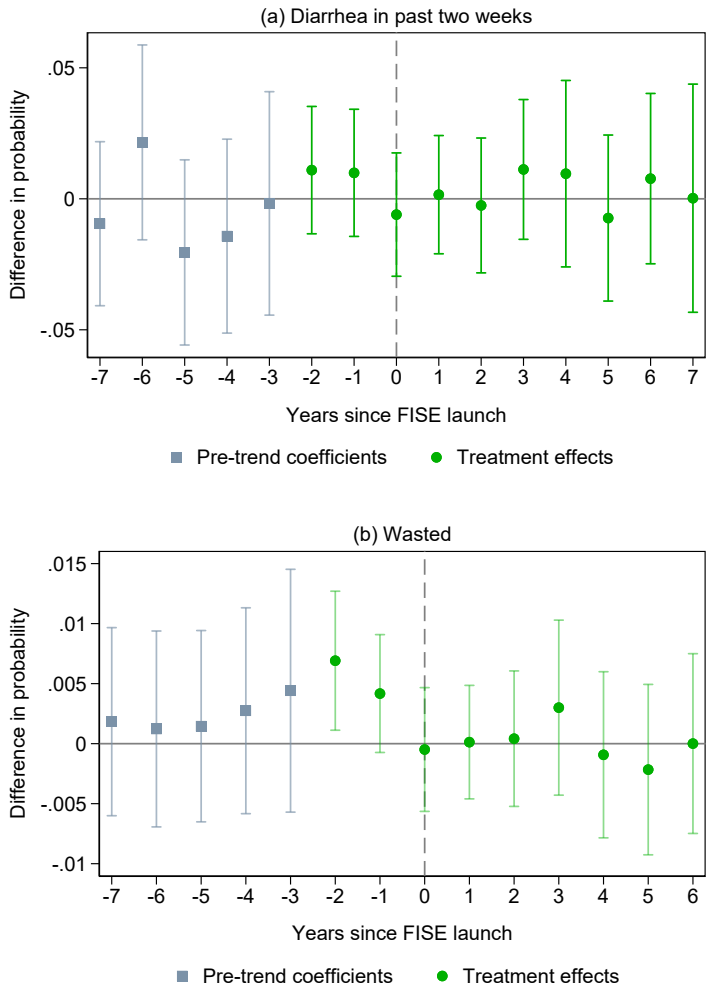
Table A.1: Peru Continuous DHS - Survey characteristics by year

Year	Sample frame	Number of sample clusters	Number of households	Domain
2020	2017 census	3,254	37,390	National, urban/rural, department
2019	2007 census (including 2012-13 update)	3,254	36,760	National, urban/rural, department
2018	2007 census (including 2012-13 update)	3,254	36,760	National, urban/rural, department
2017	2007 census (including 2012-13 update)	3,175	35,910	National, urban/rural, department
2016	2007 census (including 2012-13 update)	3,175	35,910	National, urban/rural, department
2015	2007 census (including 2012-13 update)	3,175	35,900	National, urban/rural, department
2014	2007 census	1,558	29,806	National, urban/rural, department
2013	2007 census	1,426	26,853	National, urban/rural, department
2012	2007 census	1,426	27,718	National, urban/rural, department
2011	2007 census	1,132	26,528	National, urban/rural, department
2010	2007 census	1,132	26,605	National, urban/rural, department
2009	2007 census	1,132	26,834	National, urban/rural, department
2008	1/5 of 2000 DHS clusters + 2005 census	720	18,445	National, urban/rural (department if merged with 2007 Cont. DHS)
2007	1/5 of 2000 DHS clusters	281	7,188	National, urban/rural
2006	1/5 of 2000 DHS clusters	283	7,226	National, urban/rural
2005	1/5 of 2000 DHS clusters	284	6,837	National, urban/rural

Source: Rutstein and Way (2014) and Instituto Nacional de Estadística e Informática (2020).

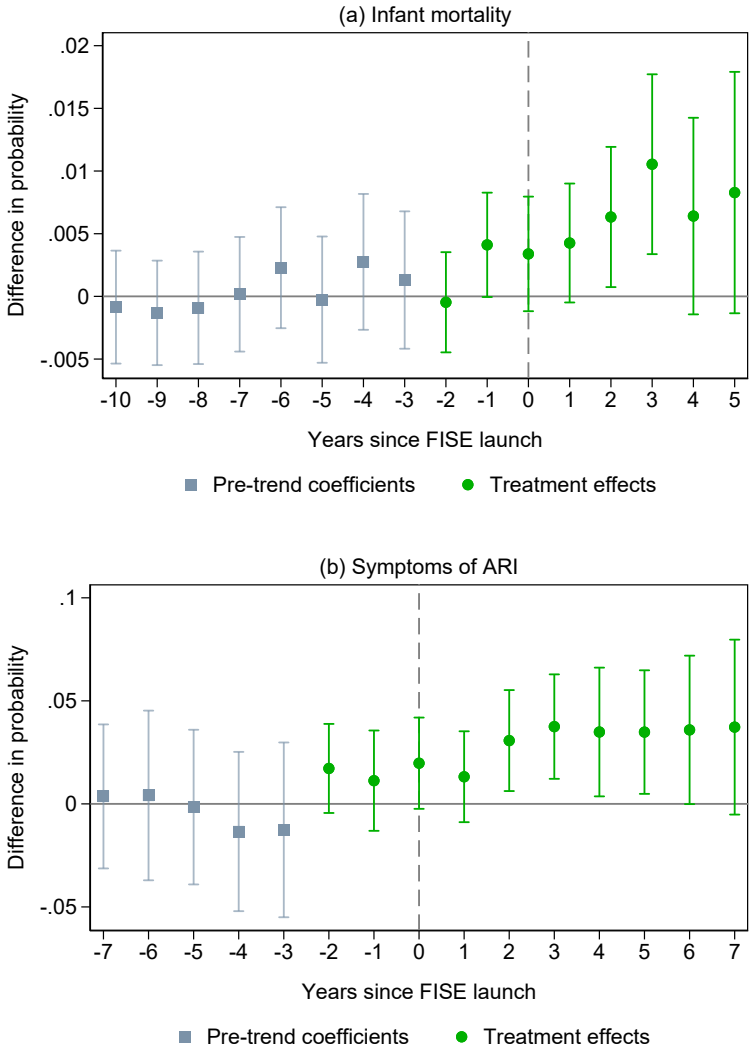
Appendix B - Supplementary figures

Figure B.1: Falsification test - Impact of FISE subisidies on diarrhea and nutritional status



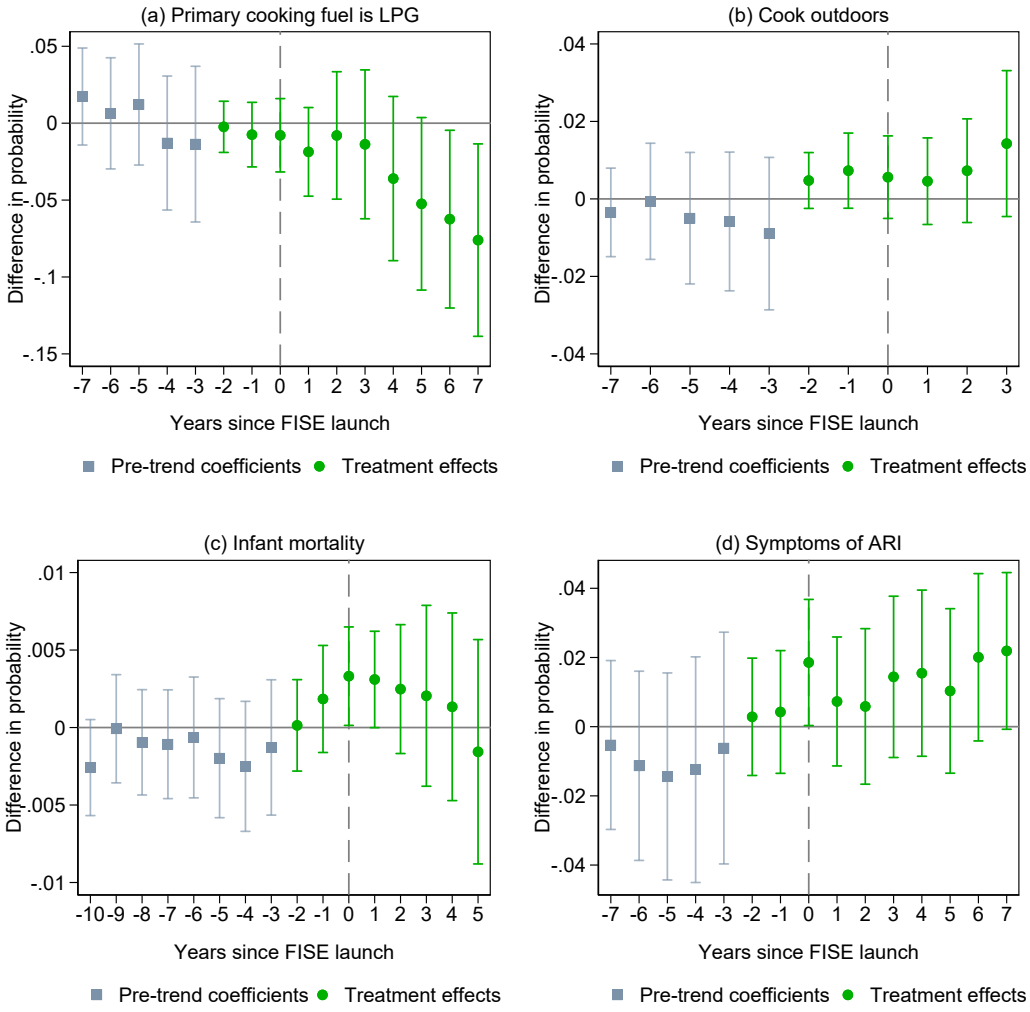
Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005 and 2007-2019 for Panel (b)). Sample: under-5 children whose mother resides in a FISE-eligible household. Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the child (a) experienced symptoms of diarrhea in the 15 days preceding the survey, or (b) is wasted (weight-for-height z-score)<-2). In Panel (b), observations from year 2006 are missing and observations from year 2020 are dropped because interviews scheduled after March 16 were conducted by phone due to the covid crisis. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of survey. In addition to district and year fixed effects, the covariates include the child's age in months, the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Figure B.2: Impact of FISE subisidies on infant mortality and symptoms of ARI - Excluding recent movers



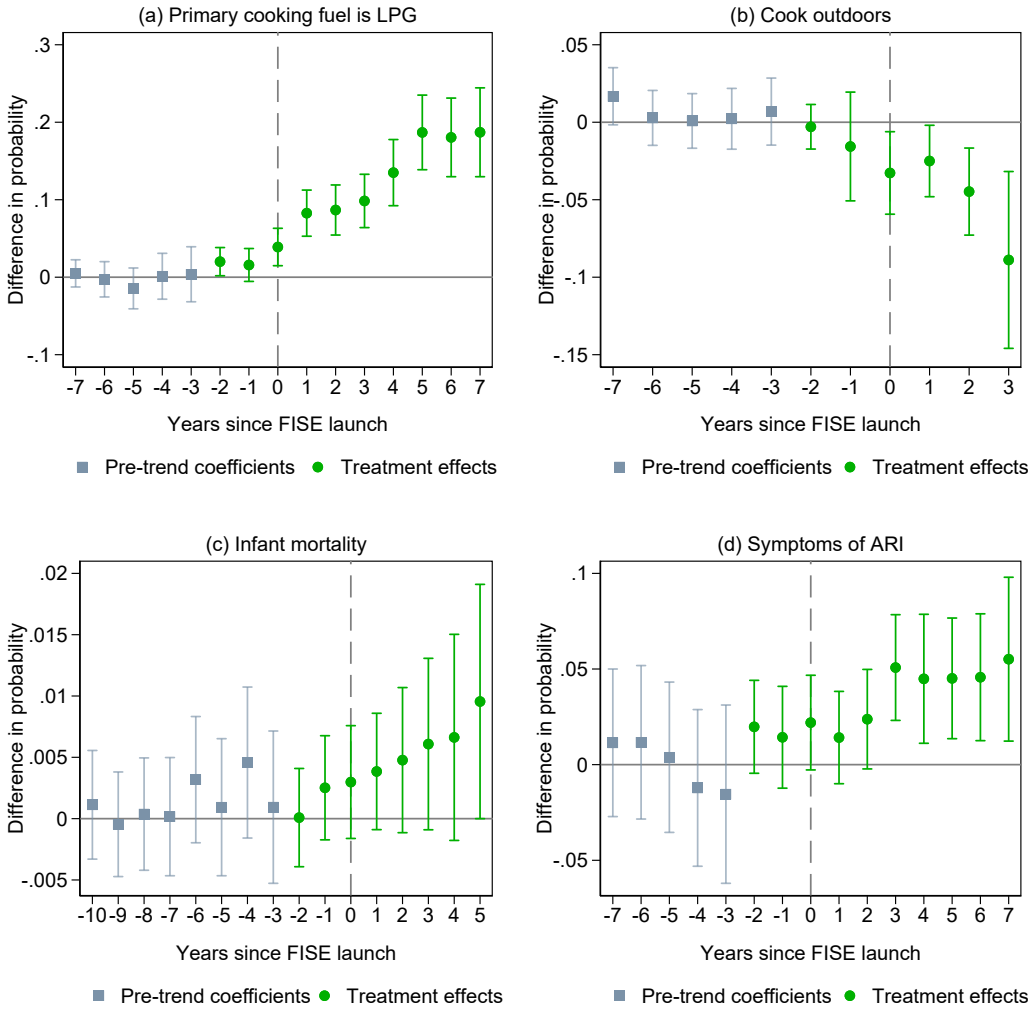
Source: Author's calculations using Peru Continuous DHS 2005-2020. Sample: live births registered between 2000 and 2020 to women residing in FISE-eligible households (Panel (a)), or under-5 children whose mother resides in a FISE-eligible household (Panel (b)). Observations are dropped from the sample if the mother reports having moved to a new home between treatment date and survey. Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the child (a) died at ages 0 to 11 months, or (b) experienced symptoms of ARI in the 15 days preceding the survey. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of birth (panel (a)) or at the time of survey (panel (b)). In addition to district and year fixed effects, the covariates include the child's age in months (only in panel (b)), the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Figure 4. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations.

Figure B.3: Impact of FISE subisidies on ineligible households



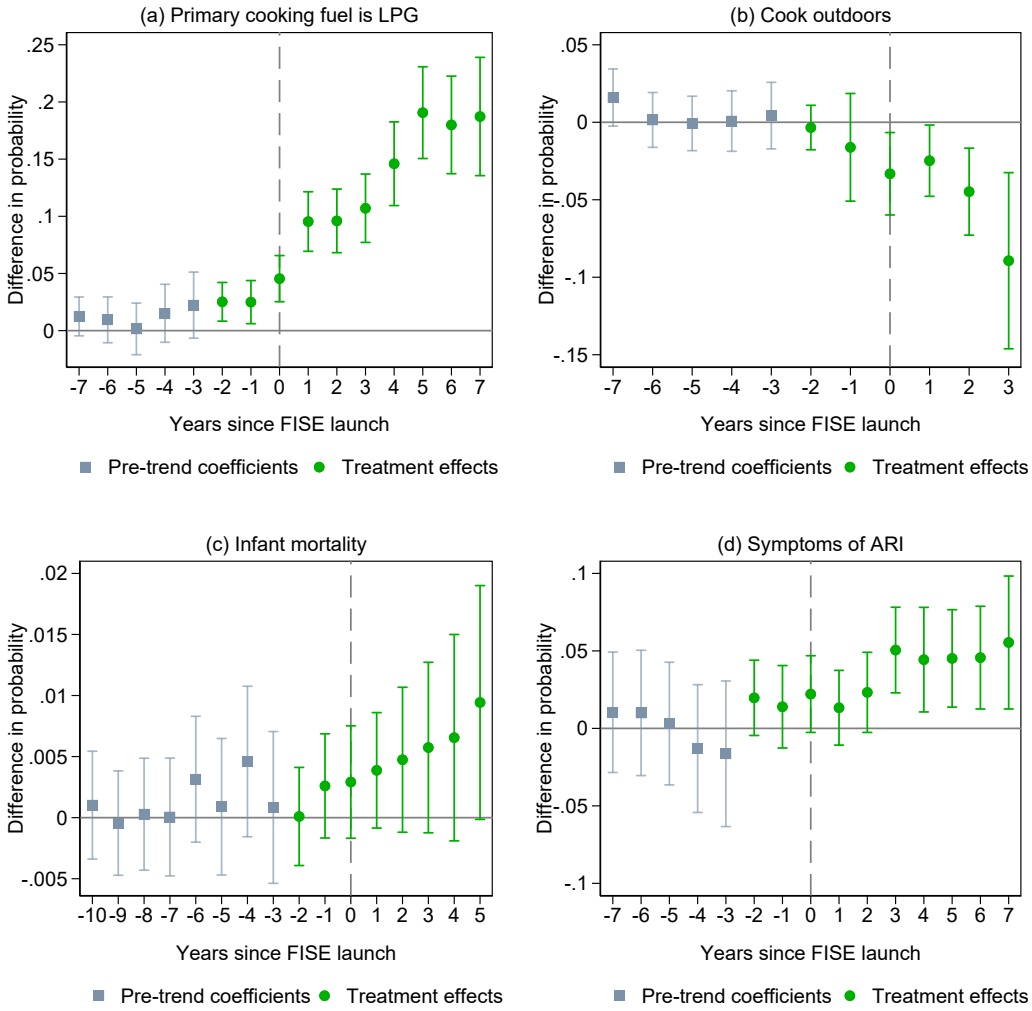
Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (b)). Sample: FISE-ineligible households (panels (a) and (b)), live births registered between 2000 and 2020 to women residing in FISE ineligible households (Panel (c)), and under-5 children whose mother resides in a FISE-ineligible household (Panel (d)). Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the household (a) reports that LPG is its primary cooking fuel, (b) primarily cooks outside, or if the child (c) died at ages 0 to 11 months, or (d) experienced symptoms of ARI in the 15 days preceding the survey. In Panel (b), observations from years 2016-2020 are missing. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district at the time of survey (panels (a), (b) and (d)) or at the time of birth (panel (c)). In addition to district and year fixed effects, all imputations control for the set of household-level control variables defined in Figure 4. Panels (c) and (d) also control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. In Panel (d), the child's age in months is added to the list of individual controls. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations in panels (a), (c) and (d).

Figure B.4: Impacts of FISE subisidies - Estimates with alternative treatment definition



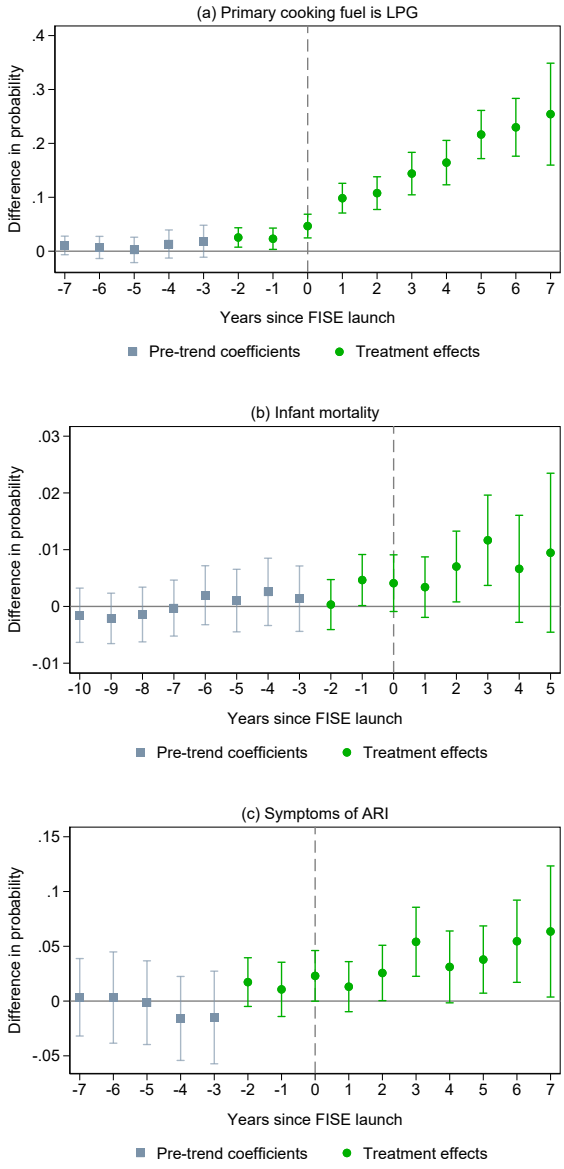
Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (b)). Sample: FISE-eligible households (panels (a) and (b)), live births registered between 2000 and 2020 to women residing in FISE-eligible households (Panel (c)), and under-5 children whose mother resides in a FISE-eligible household (Panel (d)). Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the household (a) reports that LPG is its primary cooking fuel, (b) primarily cooks outside, or if the child (c) died at ages 0 to 11 months, or (d) experienced symptoms of ARI in the 15 days preceding the survey. In Panel (b), observations from year 2016-2020 are missing. Relative time periods are defined as the number of years since the registration of the first FISE retailer in any district located within a 15km radius of the district of residence at the time of survey (panels (a), (b) and (d)) or at the time of birth (panel (c)). In addition to district and year fixed effects, all imputations control for the set of household-level control variables defined in Figure 4. Panels (c) and (d) also control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. In Panel (d), the child's age in months is added to the list of individual controls. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations in panels (a), (c) and (d).

Figure B.5: Impacts of FISE subisidies - Estimates with additional covariates



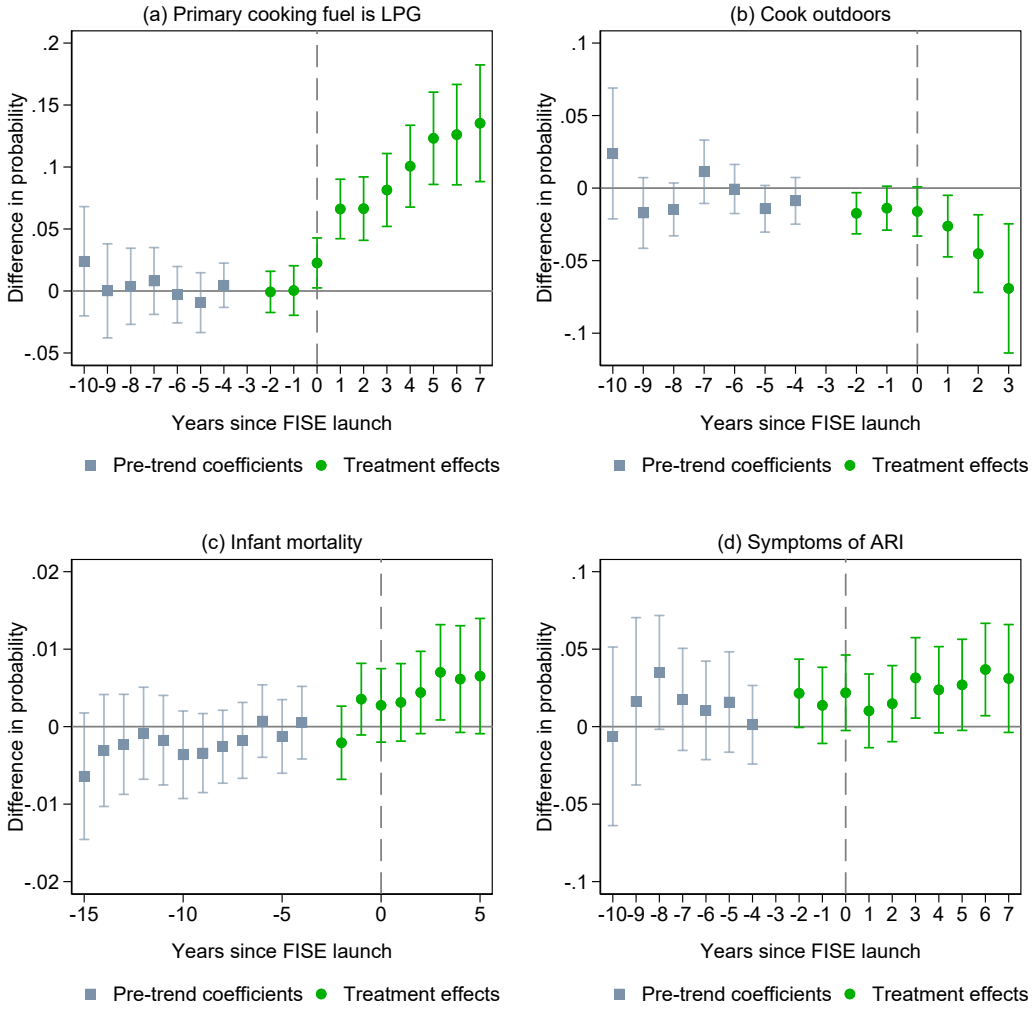
Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (b)). Sample: FISE-eligible households (panels (a) and (b)), live births registered between 2000 and 2020 to women residing in FISE-eligible households (Panel (c)), and under-5 children whose mother resides in a FISE-eligible household (Panel (d)). Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if the household (a) reports that LPG is its primary cooking fuel, (b) primarily cooks outside, or if the child (c) died at ages 0 to 11 months, or (d) experienced symptoms of ARI in the 15 days preceding the survey. In Panel (b), observations from year 2016-2020 are missing. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district of residence at the time of survey (panels (a), (b) and (d)) or at the time of birth (Panel (c)). In addition to district and year fixed effects, all imputations control for the set of household-level control variables defined in Figure 4. Additional controls are included for the household head's educational attainment, for households belonging to the poorest wealth quintile, for households with access to tap water, and for households with access to modern toilet facilities. Panels (b) and (c) also control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. In Panel (c), the child's age in months is added to the list of individual controls. Standard errors are clustered at the district level and 95% confidence intervals are reported.

Figure B.6: Impacts of FISE subisidies - Estimates excluding year 2020



Source: Author's calculations using Peru Continuous DHS 2005-2019. Sample: FISE-eligible households (Panel (a)), live births registered between 2000 and 2020 to women residing in FISE-eligible households (Panel (b)), and under-5 children whose mother resides in a FISE-eligible household (Panel (c)). Notes: Graphs display treatment effects estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator (green dots, following Equation (1)), along with pre-trend coefficients estimated via OLS (grey squares, following Equation (2)). The dependent variable is a dummy equal to one if (a) the household reports that LPG is its primary cooking fuel, or if the child (b) died at ages 0 to 11 months, or (c) experienced symptoms of ARI in the 15 days preceding the survey. Relative time periods are defined as the number of years since the registration of the first FISE retailer in the district of residence at the time of survey (panels (a) and (c)) or at the time of birth (Panel (b)). In addition to district and year fixed effects, all imputations control for the set of household-level control variables defined in Figure 4. Panels (b) and (c) also control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. In Panel (c), the child's age in months is added to the list of individual controls. Standard errors are clustered at the district level and 95% confidence intervals are reported.

Figure B.7: Impacts of FISE subisidies - Estimates from conventional event study



Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (b)). Sample: FISE-eligible households (panels (a) and (b)), live births registered between 2000 and 2020 to women residing in FISE-eligible households (Panel (c)), and under-5 children whose mother resides in a FISE-eligible household (Panel (d)). Notes: Graphs display treatment effects and pre-trend coefficients estimated using a standard dynamic TWFE estimator following Equation (1). The dependent variable is a dummy equal to one if the household (a) reports that LPG is its primary cooking fuel, (b) primarily cooks outside, or if the child (c) died at ages 0 to 11 months, or (d) experienced symptoms of ARI in the 15 days preceding the survey. In Panel (b), observations from year 2016-2020 are missing. Relative time periods are defined as the number of years since the registration of the first FISE retailer the district of residence at the time of survey (panels (a), (b) and (d)) or at the time of birth (panel (c)). In addition to district and year fixed effects, all imputations control for the set of household-level control variables defined in Figure 4. Panels (c) and (d) also control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. In Panel (d), the child's age in months is added to the list of individual controls. Standard errors are clustered at the district level and 95% confidence intervals are reported. Distant lags not shown due to small number of observations in panels (a), (c) and (d).

Appendix C - Supplementary tables

Table C.1: Falsification test: Average yearly effect of FISE subsidies on diarrhea and nutritional status - DID imputation estimates

	(1) Diarrhea	(2) Wasted
Exposed to FISE	0.0051	0.0014
	(0.012)	(0.0023)
Mean of ouctome	0.140	0.009
Mean (treatment, pre)	0.161	0.009
N	66259	58169

Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005 and 2007-2019 for column (2)). Sample: under-5 children whose mother resides in a FISE-eligible household. Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. Each column reports the coefficient of interest from a separate imputation. The dependent variable is a dummy equal to one if the child (1) experienced symptoms of diarrhea in the 15 days preceding the survey, or (2) is wasted (weight-for-height z-score)<-2). In column (2), observations from year 2006 are missing and observations from year 2020 are dropped because interviews scheduled after March 16 were conducted by phone due to the covid crisis. In addition to district and year fixed effects, the covariates include the child's age in months, the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

Table C.2: Average yearly effects of FISE subsidies - DID imputation estimates, excluding recent movers

	(1) Infant mortality	(2) Symptoms of ARI
Exposed to FISE	0.0040***	0.024**
	(0.0016)	(0.0100)
Unit of observation	Birth	Child
Mean of ouctome	0.026	0.156
Mean (treatment, pre)	0.028	0.180
N	181026	61897

Source: Author's calculations using Peru Continuous DHS 2005-2020. Sample: live births registered between 2000 and 2020 to women residing in FISE-eligible households (column (1)) or under-5 children whose mother resides in a FISE-eligible household (column (2)). Children whose mother moved to a new home between the treatment date and the date of survey are excluded from the sample. Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. Each column reports the coefficient of interest from a separate imputation. The dependent variable is a dummy equal to one if the child (1) died at ages 0 to 11 months, or (2) experienced symptoms of ARI in the 15 days preceding the survey. In addition to district and year fixed effects, the covariates include the child's age in months (only in column (2)), the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. The estimation procedure also controls for the set of household-level control variables defined in Table 2. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

Table C.3: Average yearly effects of FISE subsidies on ineligible households - DID imputation estimates

	(1) LPG primary	(2) Cook outdoors	(3) Infant mortality	(4) Symptoms of ARI
Exposed to FISE	-0.027	0.0064	0.0019	0.013
	(0.019)	(0.0046)	(0.0013)	(0.0089)
Unit of analysis	Household	Household	Birth	Child
Mean of ouctome	0.850	0.038	0.015	0.131
Mean (treatment, pre)	0.762	0.049	0.018	0.145
N	240985	139179	248643	115943

Source: Author's calculations using Peru Continuous DHS 2005-2020 (2005-2015 for Panel (b)). Sample: FISE-ineligible households (columns (1) and (2)), live births registered between 2000 and 2020 to women residing in FISE-ineligible households (column (3)), and under-5 children whose mother resides in a FISE-ineligible household (column (4)). Notes: This table reports overall treatment effects across all relative time periods estimated using the Borusyak, Jaravel, and Spiess (2022) imputation estimator. The dependent variable is a dummy equal to one if the household (1) reports that LPG is its primary cooking fuel, (2) primarily cooks outside, or if the child (3) died at ages 0 to 11 months, or (4) experienced symptoms of ARI in the 15 days preceding the survey. In column (2), observations from year 2016-2020 are missing. In addition to district and year fixed effects, all imputations control for the set of household-level control variables defined in Table 2. Columns (3) and (4) also control for the mother's age at birth, as well as dummies for female children, for multiple births, for children born to mother's aged less than 18 and for children born to mothers who report smoking. In column (4), the child's age in months is added to the list of individual controls. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.