# Improving Coverage and Utilization of Maternal and Child Health Services in Lao PDR:

## Impact Evaluation of the Community Nutrition Project

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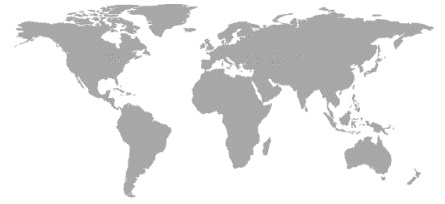World Bank • IFC • MIGA

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The Independent Evaluation Group is an
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Jeffery Tanner, Ryotaro Hayashi, Yunsun Li

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# Contents

# Boxes

# Figures

# Tables

# Abbreviations

CCT          conditional cash transfer
CNP          Community Nutrition Project
DD          difference-in-differences
DPT          diphtheria, pertussis, and tetanus
EU          European Union
GDP          gross domestic product
HAZ          height-for-age z-score
IEG          Independent Evaluation Group
KN          Lao kip (currency)
LPM          linear probability model
MCH          maternal and child health
ORS          oral rehydration solution
PDO          project development objective
PCA          principal component analysis
SES          socioeconomic status
WAZ          weight-for-age z-score
WHO          World Health Organization
WHZ          weight-for-height z-score

# Acknowledgments

IEG maintains a small work program in impact evaluation. By going deep very selectively into some impact evaluations, IEG can demonstrate how rigorous evaluations can have policy impacts. IEG is independent and is not tied to project timelines and does not have an overriding interest in academic publication; instead, it aims to engage in projects relevant to development that may lie in challenging contexts with difficult data and idiosyncratic implementation.

# Summary

The World Bank supported the Community Nutrition Project (CNP) in the Lao People's Democratic Republic (Lao PDR) from 2009 to 2013, ostensibly to respond to the global food crisis of 2007–08. The project was administered through a conditional cash transfer (CCT) with a community-based nutrition education program. Implementation challenges limited activities to 2012–13. This pilot, the first World Bank project executed entirely by a Lao line ministry, was carried out in the central and southern provinces with the intention of scaling up if it demonstrated results.

The Independent Evaluation Group (IEG) conducted an unmatched and matched difference-in-differences impact evaluation to investigate whether the CNP expanded coverage and changed health-seeking behaviors among mothers who were pregnant or had a child under two years old. This is the first impact evaluation on a World Bank–financed project in Lao PDR. It is also the first impact evaluation of a project to estimate the effects of skilled birth attendance in Lao PDR.

Overall, *CNP had little effect on indicators prespecified by the project*. The evaluation finds an attributable effect on only one of the six formal project indicators: Children in intervention areas were more likely to receive full diphtheria, pertussis, and tetanus (DPT) vaccines.

Still*, the project was effective in three important areas not specified as project indicators*. First, the project improved child-caring practices, such as having a vaccination card, breastfeeding within three hours of birth, and seeking treatment for diarrhea. Second, the CNP worked as a social protection measure against price increase shocks. The CNP was justified because it aimed to curb households' propensity to forego health care in the face of the global food crisis; those who experienced price increase shocks were more likely to receive care if they lived in an area where the CNP was available. Third, *project effectiveness in maternal and child health (MCH) coverage and behavior for the bottom 40 percent has implications for improving shared prosperity*. Because of the program, those in the bottom 40 percent of the wealth distribution were more likely to improve health-seeking behaviors and health service use, including institutional delivery. This project could be a policy instrument for closing the gap of the highly inequitable MCH intervention coverage in Lao PDR.

The project underscores three implications for evaluations. First, *single pre-post comparisons and investigations confined to official project indicators can be misleading*. Project areas met or exceeded most formal targets, but that improvement was not due to the project. More accurately, the project caused some (comparatively small) improvements in important outcomes related to the primary indicators. Second, *impact evaluations can be powerful tools, but they take careful planning*. Impact evaluations can demonstrate causality and show which recipient characteristics (such as ethnicity) or contextual factors (such as transportation) are highly influential on project uptake and outcomes. Considerations of statistical power, survey instruments, and identification strategies influence the credibility of results. Finally, *selection criteria of the target population should be explicit and transparent.*

### Promoting Knowledge Sharing and Learning

Impact evaluations are powerful instruments to determine whether achievement of outcomes can be attributed to a project. IEG selected this project for an impact evaluation because of its potential to influence learning: It was a pilot with the promise of scale-up and the first Bank project implemented by a Lao line ministry. It is also the first impact evaluation to estimate the effects of these types of interventions on skilled birth attendance in Lao—the primary indicator for Millennium Development Goal 5. The evidence gap of the CCT on skilled birth attendance was identified by the systematic review of MCH conducted by IEG (2013), which is also supported by the Japan Policy and Human Resources Development Fund, and this impact evaluation strives to close the gap.

### Nutritional Deficiency and Health Inequities in Lao PDR

Although extreme poverty was reduced in Lao PDR, absolute nutritional deficiency has remained prevalent. Recent strong economic growth led the country out of the low-income country classification and reduced the national poverty headcount rate from 27.56 percent in 2007–08 to 23.24 percent in 2012–13. The government set priorities for the health sector to further reduce poverty, and those efforts are starting to bear fruit. However, anthropometric nutritional outcomes, such as stunting and underweight, have been stagnant for a decade. Geographical challenges, sociocultural traditions like food taboos, and linguistic barriers have been shown to inhibit health-seeking behaviors.

Lao PDR is also considered to be one of the most inequitable countries for MCH coverage and outcomes between its rich and poor populations. Inequality also exists in the out-of-pocket expenditure for using MCH services. When the 2007–08 global food price crisis began, the country and the World Bank were concerned that use of MCH services might decline as households saw their budgets shrink. With the support of the World Bank ($2 million) and EU (€1.44 million), the CNP was instituted in 2009 as a pilot social protection measure with the following objective:

"…to improve the coverage of essential maternal and child health services and improve mother and child caring practices among pregnant and lactating women and children younger than 2 years old in Project Provinces."

This impact evaluation aims to determine whether the project caused attributable progress.

### Two Demand-Side Interventions

The CNP bundled two demand-side interventions to achieve its objectives: the CCT, and a community-based nutrition education program—the first attempt to fully administer a project in-house by a Lao line ministry, without a project implementation unit led by external consultants. The CCT delivered a transfer on completion of each encouraged action taken by the beneficiary at an incentivized health center (for example, enrollment, antenatal care visits, delivery, and growth checkups). The amount of cash varied based on the distance from each household to the health center (KN 50,000 to KN 70,000), but was comparable to one week's average consumption per capita. The exception was delivery, where a stronger cash incentive equivalent to the median per capita monthly consumption (KN 260,000 to KN

300,000) was distributed. Beneficiaries were also exempted from user fees for MCH services at public health centers, and the public health center was given incentives to provide more services.

The community-based nutrition education program aimed to encourage behavioral change and strengthen mutual support in improving nutrition. The cascading training of local female facilitators was provided first, and regular village meetings were organized by an international nongovernmental organization that was eventually succeeded by the Lao Women's Union. The regular village meetings provided knowledge about health and nutritional practices to address food taboos and promote better health behaviors, such as breastfeeding and hygiene practices. The CCT program was publicized through the community-based nutrition program.

The CNP was conducted in 62 health center service areas in seven out of eight central and southern provinces in Lao PDR, where health centers had minimum capacities to handle the project. Importantly, the assignment to the intervention was not random. The government used specific criteria to select the health centers. Although the CNP beneficiaries were not means-tested, they were indirectly targeted geographically through the selection of the health centers, which were mostly located in rural parts of the country.

Although the minimum capacity of the health center was examined and some project administration supports were also included to assist in detailed design and supervision, the CNP faced challenges when implementing the project. First, substantiating the project took time because of the weak design at project approval. Second, the CCT distribution was not timely even after the rollout, and not enough cash was transferred for delivery. Comparison areas that were not part of the CNP project still benefitted from user fee exemptions.

**Project Outcome Measurements and Data**

Six main indicators were used to assess the efficacy of the CNP project:

- Antenatal care provided by skilled health personnel
- Child delivery at a health facility
- DPT3 immunization before the first birthday
- Attending a monthly growth checkup
- Breastfeeding within one hour after birth
- Receipt of oral rehydration solutions during diarrhea.

This impact evaluation also examines anthropometric outcomes of stunting, underweight, and wasting as nutrition outcomes, given that they were used to frame the initial project preparation and justify its approval. Constructs related to these project development objective (PDO) indicators; but more were also assessed that were in line with the project's design. For instance, given that the CCT was delivered at public health centers, this impact evaluation examined antenatal care and child delivery at public health centers specifically, and at health facilities generally, which also includes district and provincial hospitals and private clinics.

**Advantage of Impact Evaluation Methods for Attribution**

Although before/after comparisons are easy to implement, it is difficult to attribute them to project outcomes. For instance, the CNP substantively exceeded the target value for all stated outcomes by comparing the pre- and post-outcome data in the treatment area. However, whether or not this improvement was due to the CNP was unclear because other factors might be responsible for observed improvements, such as secular trends in gross domestic product growth.

This impact evaluation uses a difference-in-differences method to improve causal inference. While controlling for (and matching on) important household, village, and health center factors that may influence MCH, the before/after difference in the treatment area is subtracted from the before/after difference in the comparison area to attribute the project achievement to the CNP interventions. If factors beyond the project's control (for example, secular income growth) equally affect both the treatment and comparison areas on average, the double differences will strip down the effect to the CNP intervention.

**Little Overall Effect but Important Nuances**

The impact evaluation analysis suggests that there is little overall effect that can be attributed to the project, except for DPT3 vaccination. But the full story is more complex. First, careful examination of the associated indicators implies that the CNP improves child caring practices. For instance, the proportion of mothers or guardians who could show a vaccination card to the enumerator (a proxy for parental conscientiousness of child health) improved by more than 36 percentage points. Breastfeeding within three hours of birth improved by 6 to 14 percentage points. An increasing proportion of beneficiaries also sought diarrhea treatment.

Second, there were positive effects for those who experienced a price increase shock in their consumption goods. This finding is important, given that the CNP was instituted after the global food crisis began and worked as a social protection measure for those who experienced the price increase shock (self-reported).

Third, the project has shared prosperity implications: The poorest 40 percent of the population benefited from the CNP. Similarly, the Saravan province (the poorest province in Lao PDR) made some progress in achieving the project objectives related to antenatal care and growth checkup. Although there were some positive effects on the matched sample, the project did not improve institutional delivery or health center delivery in the full sample—a surprising finding given how consistently previous CCT interventions improved skilled birth attendance (IEG 2013). However, the project improved skilled birth attendance and institutional delivery among the poor and those who had no education.

**Evaluation Implications**

Three evaluation truths are underscored in this impact evaluation. First, single before/after comparisons can be misleading in understanding the efficacy of the project. Simple pre-post evaluation gives a view of program effectiveness that is often inaccurate. A credible comparator (counterfactual) is needed to understand the welfare outcomes attributable to the

project. Otherwise the evaluation cannot rule out that observed changes are a result of improvements in other factors—such as economic growth—instead of the intervention. The CNP substantively exceeds the original targets for all six indicators through before/after comparison in treatment areas, but the impact evaluation indicates that very little of that improvement in those selected indicators was due to the project.

Second, impact evaluations can be powerful tools, but they take careful planning. Impact evaluations can show causality and show which recipient characteristics (such as ethnicity) or contextual factors (such as transportation) highly influence project uptake and outcomes. Considerations of statistical power, survey instruments, and identification strategies influence the credibility of results. Sample sizes should be well beyond what power calculations may imply to be robust to challenges that arise in implementation and data collections (and they almost always do). Sampling stratification should be done to allow policy-relevant comparisons. For example, if policymakers want to know how the program fared from province to province, a province-representative sample of intervention and nonintervention area comparators is required. Data collection instruments and firms should be consistent across waves.

Several challenges arose, and interesting data could not be analyzed because they were not consistently measured across waves of the sample. Changes were made to either shorten the survey time or to make it consistent with other data collection instruments in the region. Such comparability and time considerations should be made at the baseline, and project design and project measurement must be aligned. The CNP had too many indicators not exactly aligned with the design, and some design elements (such as incentivizing actions taken at health centers) were not aligned with the correct objectives (improving health coverage, regardless of the institution).

Finally, the selection criteria of the target population should be explicit and transparent. This reduces opportunities for graft and favoritism and increases evaluability. To improve evaluation, program assignment should be randomized if at all possible. The causal claims of impact evaluations are only as strong as their counterfactual and identifying assumptions. IEG's analysis of the data uses the best tools available, but there are some hints that the intervention and comparator areas may be somewhat dissimilar. Random assignment can be made after policy makers have defined their target intervention group. If the desired target population is larger than the budget can serve, randomization is a fair way to determine eligibility. Random assignment also significantly strengthens evaluability. Despite IEG's best efforts, the results of this evaluation may still be undermined by confounding explanations.

Less empirically, protocols must be established and held. Implementation problems can create a crisis of credibility and may be responsible for the general lack of overall effects. Late-developing protocols that were not followed led to late and uneven implementation. Transfers were significantly delayed by three months, undermining the credibility of the program to deliver future benefits and eroding the project's effectiveness.

**Limitations and Future Area of Research**

There were other factors and hypotheses worth evaluating, but they were beyond the reach of this impact evaluation. First, other donor and local government activities in the comparison area may cause this evaluation to underestimate the project's effectiveness. Second, the project duration of a year and a half may have been too short to generate favorable impacts, particularly on the anthropometric nutrition outcomes. Third, the quantity and quality of the supply-side factors need to be examined more carefully, which is the future area of research.

# 1. Introduction

1.1     The fourth and fifth Millennium Development Goals on maternal and child health (MCH) have proved to be the most difficult to attain. Even as Lao PDR successfully transitioned out of the low-income tier of the World Bank's classification in 2011, the country is focusing on the health sector to further reduce poverty. The National Growth and Poverty Eradication Strategy prepared by the Lao government in 2004 identified health as one of four sectors of focus for eradicating poverty in the medium term. Numerous efforts in line with this strategy have generated favorable results. Maternal mortality fell from 796 to 357 per 10,000 live births from 1995 to 2009, and infant mortality fell from 123 to 68 per 1,000 live births from 1995 to 2010–2011. Comparing the Multiple Indicator Cluster Survey 2006 with the Lao Social Indicator Survey 2011–12, the percentage of deliveries at health facilities increased from 17 percent to 38 percent, while home delivery dropped from 85 percent to 59 percent during the same period. Overall, the proportion of births attended by skilled health personnel doubled from 20 percent to 42 percent in five years. Despite this progress, mothers in the neighboring countries of Vietnam and Cambodia are attended at birth at far higher rates (92.9 percent and 71.7 percent), yielding better progress on that indicator for Millennium Development Goal 5.

1.2     Geographical, sociocultural, and economic challenges have inhibited the progress. Lao PDR is a landlocked, largely rural country with a variety of food taboos and languages creating barriers among its diverse ethnic groups. These social characteristics may influence MCH service-seeking behaviors. For instance, many mothers believe that food restrictions in pregnancy lead to smaller babies and easier (and more survivable) delivery (Phimmasone et al. 1996). Food taboos can affect breastfeeding and pregnancy (Holmes et al. 2007). The rural poor commonly face a lack of adequate quality and quantity of food (Kounnavong et al. 2011). The Lao Social Indicator Survey shows that 26.6 percent of children are underweight, and 44.2 percent are moderately or severely stunted. Alarmingly, these rates have not changed for a decade, and the stunting and underweight prevalence among children under age five are the worst in the Indochina region (Kamiya 2011). The incidence of low birth weight is also likely to be an explanatory factor in the region's high neonatal mortality rates (Viengsakhone, Yoshida, and Sakamoto 2010).

1.3     To remedy observed shortages in the number of trained health staff (Yamada, Sawada, and Luo 2013) and low use rates of health services (WHO 2012), the Lao government—in consultation with other multilateral and bilateral donors— set out a new policy in 2008 to address nutrition, skilled birth attendance, immunization, and other MCH services. Although some progress has been made, some MCH programs, such as the one for national immunization, did not achieve the intended goals. According to the Lao Social Indicator Survey, only 34 percent of children age 12–23 months received all recommended immunizations before their first birthday, despite remarkable improvement over the 2006 Multiple Indicator Cluster Survey. Regarding preventable diseases, 1 of 10 children in Lao PDR suffered from diarrhea in the two weeks before the survey.

1.4     Globally the bottom 40 percent of the population in income distribution made better progress than the other 60 percent on several MCH outcome indicators (Wagstaff,

Bredenkamp, and Buisman 2014), but Lao PDR is still considered one of the most inequitable countries for the maternal, newborn, and child health interventions (Barros et al. 2012). Of 54 developing countries, Lao PDR is ranked fifth lowest in the composite coverage index, which is the weighted mean of the coverage of eight essential interventions in family planning, maternity care, child immunization, and case management. Skilled birth attendant coverage in particular is the second lowest among countries included in the study, besting only Ethiopia. Furthermore, skilled birth attendance ranges from 5 percent in the lowest wealth quintile to 80 percent in the highest (Barros et al. 2012).

1.5     The distribution of out-of-pocket expenditures for MCH services is also inequitable. Although out-of-pocket health expenditures for the richest quintile in Lao PDR were 26 percent of monthly household expenses, out-of-pocket expenditure for the poorest quintile of households accounts for 43 percent of monthly expenses (World Bank 2013). This high level of out-of-pocket expenditures can be at least partially attributed to health financing by the government, which accounts for only 41 percent of total health expenditures. Still, higher levels of government support of health service use need not be long term. Short-term, demand-side subsidies—through conditional cash transfers (CCTs), for example—can induce positive learning of the health services, which can lead to better take-up in the long run (Dupas 2014a, 2014b, 2014c).

1.6     In this challenging context, potentially made more difficult by the 2007–2008 global food crisis, the Lao PDR government and the World Bank instituted the Community Nutrition Project (CNP) in 2009 as a pilot program that consists of two demand-side interventions. CCTs were complemented by community-based nutrition programs to directly give people incentives to use the public health center's MCH services and to educate them on proper nutrition, hygiene, and child health care.

## Policy Questions

1.7     This impact evaluation seeks to address whether the CNP can make causal claims to improving indicators related to its six stated project development objective measurements for mothers and children under two years old: antenatal care visits, institutional delivery, well-child checkups, breastfeeding, immunization, and diarrhea/oral rehydration solutions. The results of quasi-experimental impact evaluation methods indicate that although general effects for these outcomes are mixed, the project shows improvements for the poorest 40 percent of the population.

1.8     Importantly, although this project was implemented as a pilot, it was done at a relatively large (though subnational) scale and employs a real-world counterfactual. Instead of being tested against no intervention at all, these results are relevant for what nonbeneficiaries did in the absence of the program, including receiving benefits from a mixed range of competing interventions in comparison areas (treatment areas were still relatively clean from contamination, according to the World Bank implementing team). This impact evaluation tries to address such practical issues independently.

1.9     This evaluation further fills the evidence gap for MCH impact evaluation through demand-side interventions. Although the body of impact evaluation evidence improved for

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demand-side interventions such as CCTs, a recent systematic review finds only eight impact evaluations (six studies in Latin America and two studies in South Asia), but no such impact evaluation evidence from Southeast Asia on the causal relationship between CCT and MCH outcomes (Glassman et al. 2014). CCTs have the potential to reduce intergenerational transmission of poverty and improve the uptake of health services, but there is limited and mixed evidence on health and education outcomes (Bastagli 2011). Finally, although skilled birth attendance is included as one of the Millennium Development Goal indicators for monitoring progress, the impact evaluation evidence base for skilled birth attendance as an outcome is still slim (IEG 2013). This is the first impact evaluation in Lao PDR on any CCT intervention or on skilled birth attendance as an outcome.

# 2. Project

2.1     In the wake of the global food crisis, the World Bank provided $2 million ($1.63 million was spent) to "improve the coverage of essential maternal and child health (MCH) services and improve mother and child caring practices among pregnant and lactating women and children less than 2 years old" in the seven southern and central provinces of Lao PDR (Trust Fund Grant Agreement covering the period of 2009–2013). The EU provided an additional €1.44 million of support between 2010 and 2012.

2.2     The Community Nutrition Project (CNP) marked the first World Bank project executed by a Lao line ministry instead of a project implementation unit led by external consultants. The project was administrated by the Department of Hygiene and Health Promotion in the Ministry of Health. Implementing agents included public health centers and their staff for the CCT components, and local nongovernmental organizations—or international nongovernmental organizations with a local presence—and the Lao Women's Union for the community-based nutrition component. To avoid duplicating the project in the northern part of Lao PDR supported by the Asian Development Bank, 62 health centers in seven out of eight central and southern provinces were selected to participate. Although the World Bank team originally proposed that the Ministry of Health randomize the treatment assignment at the health center level, the ministry eventually determined assignment purposively through somewhat opaque criteria. The ministry considered characteristics such as the district financial management capacity, health center staffing, and service capacities in its selection decision.[1] These considerations make finding comparable nonbeneficiary areas more difficult and allow for an increased possibility of manipulating the selection of treatment areas.

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[1] It is not clear what these characteristics are in reality, but according to the project appraisal document, "In selecting the health centers, both health center characteristics (staffing, service capacity, etc.) and district financial management capacity will be considered. A complete set of districts and health centers that meet minimum criteria will be identified, and pilot areas will be selected from this group in ways that permit rigorous evaluation" (World Bank 2009, 5), The World Bank country team indicated that there may have been slightly different criteria: i) health centers needed at least three health center staff, ii) health centers should not be located "too close" or "too far" from the district hospital, and iii) health centers should have quality and readiness to take up this program.

## Two Demand-Side Interventions

2.3      The CNP bundles two demand-side interventions: CCT and a community-based nutrition education program. The CCT program under CNP differed from many other CCT programs in two ways. First, the CNP was not specifically means-tested or targeted beyond the geopolitical targeting or the intervention areas. All pregnant mothers within a selected health center's service area were eligible. Although the CNP is implemented in public health centers in rural areas, the CCT was not means-tested or otherwise targeted, but was paid to all people—regardless of income—who directly satisfied the transfer conditions (for example, birth at a health center). Second, the project was not designed to deliver cash in regular time intervals. Instead, payments were intended to be made at the time beneficiaries complied with the conditions after enrollment. Even so, there was a significant backlog of beneficiaries receiving payments. Information about conditions and entitlements were explained to beneficiaries during enrollment and community-based nutrition activities. A full description of the conditions and transfers is in table 2.1.

### Table 2.1. Conditional Cash Transfer Incentive Structure

| Conditionality | Payment frequency | Total benefit | |
|---|---|---|---|
| Enrollment at health center | One payment | Less than 3 km | = KN 50,000 |
| | | 3 km–6 km | = KN 60,000 |
| | | More than 6 km | = KN 70,000 |
| Antenatal visit | Four times | Same benefit as enrollment | |
| Delivery | One payment for delivery; up to two transport coverages | Less than 3 km | = KN 260,000 |
| | | 3 km–6 km | = KN 280,000 |
| | | More than 6 km | = KN 300,000 |
| Postnatal visit | One payment after birth | Same benefit as enrollment | |
| Child 0–12 months checkup | Monthly, up to 12 payments | Same benefit as enrollment | |
| Child 13–24 months checkup | Monthly, up to six payments | Same benefit as enrollment | |

*Source*: IEG.

*Note*: Two transportation charges are allowed for false labor. The scheme covers the cost of transportation (in addition to the appropriate conditionality payment) in cases in which a patient is transferred to a superior medical facility. Round-trip transportation is covered for institutional delivery for the patient and her escort. For a Caesarian section, KN 160,000 is paid above the normal delivery benefit. 1 U.S. dollar is equivalent to approximately KN 8,100 (as of December 2014).

2.4      By design, the transfer amount varied depending on the distance from the public health center, ranging in increments of KN 10,000 from KN 50,000[2] to KN 70,000 for each 3 kilometers from the health center to the beneficiary's home.[3] The transfer level is intended to cover six to 10 days of average consumption per capita for the bottom 40 percent of people in

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[2] The KN 50,000 is equivalent to 2.4 days' earnings, based on the minimum daily wage for a 19-year-old worker or apprentice in Lao PDR per the 2014 Doing Business report published by the World Bank. It is equivalent to about $6.50 in 2014.

[3] With appropriate data, this characteristic could have been used to estimate the effect of incentive size using a regression discontinuity identification strategy. Unfortunately, neither administrative nor global positioning system data are available for analysis to determine distance from household to health center.

Lao PDR.[4] All conditioned actions received the same size of transfer except for institutional delivery, which merited four to five times the standard payment in addition to the transportation cost because of other costs involved in delivery. The cash incentive for delivery ranges from KN 260,000 to KN 300,000, comparable to the median nominal monthly consumption per capita in a rural area (KN 270,966), and more than the estimated KN 203,600 comprising the 2012–2013 national poverty line for monthly consumption (Pimhidzai et al. 2014). Furthermore, given the high level of out-of-pocket expenditures for MCH services (World Bank 2013), beginning in 2012, beneficiaries were exempt from fees for MCH-related services rendered at the public health centers except for those living in the Bolikhamxay province. Public health centers received a fixed fee per quantity of services provided to compensate for the cost. This user fee exemption was gradually phased out from the end of March 2013 to the end of July 2013. The public health center is also incentivized to provide more MCH services. Importantly, the CCT program benefits accrue only for services rendered at the local government health centers (except in the case of formal referrals to higher-level public facilities). Activities performed in other health facilities, such as private clinics or government district, provincial, or central hospitals, do not trigger cash transfer payments.

2.5    The second demand-side intervention is the community-based nutrition activities, which aimed to encourage behavioral change and strengthen mutual support in improving nutrition for children and pregnant and lactating mothers. Community-based nutrition provided cascading training for local female facilitators before the rollout. Beginning in 2012, regular village meetings were organized by trained residents to teach and discuss nutrition and MCH-related issues, including pregnancy and delivery. This educational activity addressed food taboos and other health behaviors, such as breastfeeding, hygiene, and administering an oral rehydration solution for diarrhea, or even seeking qualified care for other communicable diseases. The community-based nutrition activities also included sensitization about the CCT initiatives to encourage uptake of antenatal care, institutional delivery, and postnatal and routine growth checkups. Health Poverty Action, an international nongovernmental organization for MCH services, facilitated the initial community-based nutrition activities for about six months. In August 2012 the Lao Women's Union took over the monitoring and evaluation for the rest of the project. In practice, the community-based nutrition activities are taken from United Nations Children's Fund and World Health Organization (WHO) training on maternal and child nutrition through a participatory approach.

2.6    Some supply-side elements to the program were provided through a third CNP component used mainly for project administration. Sizable administrative support for overall project supervision at the public health center–level was provided to backstop the Ministry of Health during the project. Regarding support to health providers, the third component funded the five module training developed by the Ministry of Health and implemented by the

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[4] According to the Poverty Profile in Lao PDR (Poverty Report for the Lao Consumption and Expenditure Survey, 2012–2013) prepared by Lao Statistical Bureau and the World Bank (Pimhidzai and others 2014), the average nominal monthly consumption per capita in 2012–13 is KN 227,105.

Department of Education and Research.[5] Community-based distributors were trained by the end of 2011 to include essential MCH micronutrients. The facilities (though not the staff) received a fixed fee for each user fee-exempted service rendered. This was intended primarily to compensate for lost user fees instead of giving an incentive to provide health care. More training and supervision was provided, along with health supplies and commodities. In total, however, actual supply-side support for health care provision was relatively small.

2.7     As a heavily demand-side project, the CCT and community-based nutrition components of the CNP relied on a demand-side program logic. The first component of the project's objectives, expanding MCH service use, is largely accomplished through the CCT program. In the treatment area, the CCT program is operated in the public health centers with health center staff that is assumed to be both capable of providing quality MCH services (after taking the multimodule training) and managing CCT funds. Mothers learn about the program through CCT enrollment campaigns and community-based nutrition educational activities, and they enroll and receive cash incentives at the public health center subject to participating in antenatal care, delivery, and/or children's growth checkups. Mothers are assumed to know about and attend the village meetings, and they understand and change their nutritional and health-seeking behavior for the better. It is also assumed that MCH practices could be improved through talks with health center staff at the expanded MCH visits (and vice-versa). Most important, this program logic essentially assumes that demand-side constraints can be overcome by the financial incentives of the CCT and expanded knowledge of MCH practices through the community-based nutrition component.

2.8     On the supply side, regarding improving MCH-caring practices, unpaid local village facilitators are assumed to be available and trainable, and then reliably hold regular village meetings through community-based nutrition activities to raise awareness of nutrition and available MCH services among mothers. The project design also assumes that supply-side health services and resources at the public health center, including the administrative capacity to implement the project, are sufficient to meet the anticipated uptick in demand.

## Implementation Challenges

2.9     The CNP faced many implementation challenges through supply-side shortcomings that began in the design stage. Despite some project administration support, the CNP faced implementation delays because critical design details were left for the implementation phase. For instance, the cash incentive structure and the mechanism for distribution of the CCT payments were not initially determined. Foreseeable challenges, such as the lack of easily accessible local banking services in the project area, were not addressed until well into implementation; the eventual solution—requiring CCT payments to be disbursed through public health centers—placed significant added burdens on an already under-resourced staff and required more protocols and training for transparency and accountability.

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[5] The five-module training covers areas such as basic emergency obstetric and newborn lifesaving skills; essential newborn care; antenatal care and postnatal care; family planning; and integrated management of neonatal and childhood illness. All treatment health centers have at least one staff member who completed the training.

2.10    As a result, the CCT payments were not delivered in a timely manner. Two large CCT backlog clearing campaigns were held in November 2012 and February 2013. Those enrolled in the program reported an average delay of three months for the enrollment payment. The average delay for payment for delivery was only slightly shorter. This backlog may have cost the project credibility with subsequent and potential beneficiaries.

2.11    Beyond payment delays, compliance with protocols was a significant challenge. In the treatment areas, approximately 30 percent of households that were eligible for the CCT program did not enroll in it, even though 94 percent of mothers or guardians were aware of the program's existence in their villages. Even among the enrolled, 25 percent of beneficiaries did not receive the enrollment transfer. Payment for institutional delivery was not made for 28 percent of beneficiaries who fulfilled the conditions of the transfer. The amount of the transfer seemed to be consistent with the protocol on enrollment, but more than 65 percent of beneficiaries did not receive even the minimum transfer amount for delivery (KN 260,000). Furthermore, although the program's exemption of user fees reduced out-of-pocket costs in the treatment area, there were still many people who reported paying for fee-exempted services.

2.12    Implementation of the community-based nutrition and CCT was uneven. The two demand-side interventions started at about the same time in January 2012, but the CCT was implemented in earnest for only 11 months from July 2012 to June 2013. For most beneficiaries, the community-based nutrition activities started earlier than the CCT. Still, there may have been some anticipation effects where potential beneficiaries changed their health-seeking behavior after finding out about the possibility of the CCT through the community-based nutrition meetings.[6] All the project activities described above are summarized in figure 2.1.

2.13    Imperfect project rollout requires reporting the effects for multiple age groups. Project-targeted beneficiaries did not receive any tangible treatment for the first few years after the World Bank approved the project in 2009. This implies that children age 0–11 months and their mothers received the full set of CNP bundled interventions, and mothers of 12–17-month-olds received a bundle of services weighted more toward community-based nutrition activities and user fee exemptions during their pregnancy and delivery. It follows that children age 18–23 months at the time of the survey were least likely to have benefited from the CNP at or before birth. Because of this, most estimation results in this impact evaluation are reported for both the overall age group (0–23 months) and the age group most likely to receive the full benefit of the program (0–11 months).

2.14    Intervention spillover—households in comparison areas benefitting from the intervention when they should not have—is possible. The only real barrier to such spillover for comparator households is the cost of additional travel required to a (more distant) intervention health center. This would result in a downward bias, likely making the effect estimates reported here a lower bound on the true effect.

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[6] The possibility of these anticipation effects disallows a clean disaggregation of the CNP versus community-based nutrition effects.

2.15    Similarly, nonproject "contamination" of comparison areas may have occurred if other organizations provided some benefits similar to the CNP. The data did not support the view that treatment areas were contaminated. However, some comparison areas were the subject of similar interventions from outside organizations, notably the Luxembourg Agency for Development Cooperation and the Lao Red Cross. Although these organizations may have intervened in comparison areas anyway, the delay in CNP implementation prolonged the period of the vacuum of services in the comparisons areas. This delay, combined with the lack of a sectorwide approach in health, increased the likelihood of contamination.[7] As a result, a similar proportion of eligible mothers in both the treatment and comparison areas seem to have benefited from a user fee exemption. Furthermore, in the Bolikhamxay and Champasak provinces, about one-fifth of mothers in the comparison areas who have a child under age two reported enrolling in some type of CCT scheme; many of these mothers reported that they received a transfer.[8]

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[7] It must be noted that this contamination of the comparison areas receiving benefits is almost certainly a positive outcome for those living in the affected areas.

[8] Without a sectorwide approach or broader donor coordination, it is difficult to verify whether these reports accurately indicate the existence and disbursal of CCTs in comparison areas or a social desirability bias. But anecdotal evidence from the World Bank indicates that other aid agencies with intervention designs somewhat similar to the CNP were likely operating in these areas.

**Figure 2.1**. **Active Project Components by Time**
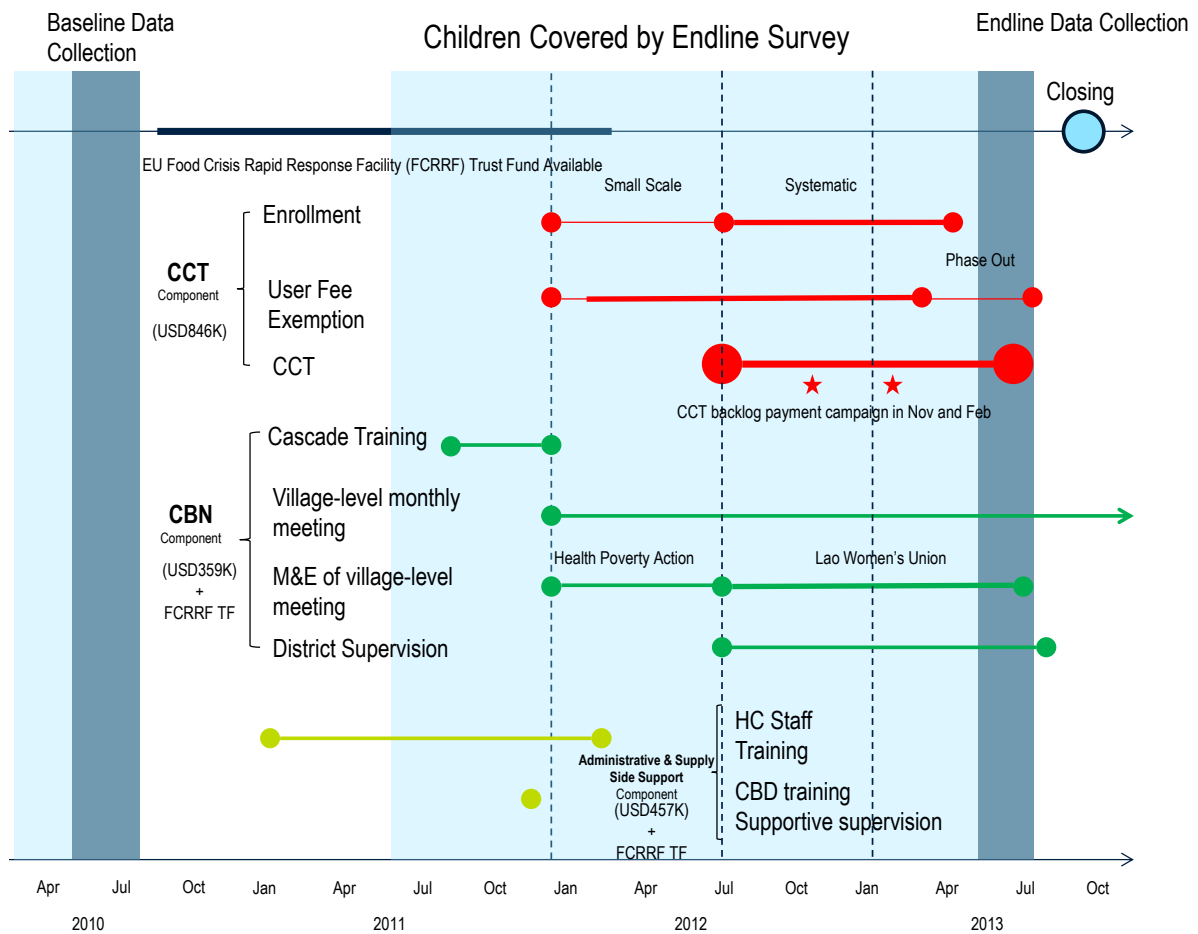


*Source:* IEG.
*Note:* CBD = community based distributor; CBN = community-based nutrition; CCT = conditional cash transfer; FCRRF = Food Crisis Rapid Response Facility; HC = health care; M&E = monitoring and evaluation.

2.16     Even so, the contamination of comparison and potential spillover of health services in the data imply that the program effects estimated in this paper are lower bound estimates for the program compared with a scenario in which no additional benefits are available to the comparison group. Alternatively, the results may be interpreted in relation to the de facto counterfactual, or what *did* happen in the absence of the program, rather than the theoretical (and unrealistic) counterfactual of nothing happening if not for the CNP. Some alternative interventions, proxied by those observed in the comparison areas, almost certainly would have taken place in the CNP intervention areas even if the CNP had never been implemented.

2.17     With respect to community-based nutrition, 56 percent of people in the comparison area responded that there are regular village meetings to discuss nutrition and health issues. This proportion is less than the 83 percent figure in the treatment area, but it could underestimate the effect of community-based nutrition. Yet community-based nutrition in the treatment area had slightly more active participation with better understood messages than in the comparison area. Implementation variations also existed by province.

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2.18    The World Bank country team reports that no activities similar to the CNP were undertaken in CNP target areas by the World Bank or other donors. The World Bank did provide supply-side assistance for five of the seven intervention provinces through the Health Sector Improvement Project, though it did not provide CCT or community-based nutrition–type services during the implementation of CNP. The national expanded program on immunization was also implemented in both the treatment and comparison areas. These activities could have interacted with CNP activities in uneven ways.

2.19    Despite these spillover and contamination issues, results in the effect estimates reported in this study are not significantly undermined. Instead, the counterfactual becomes what did happen in reality in the absence of (and in place of) the program. Therefore, the evaluation is measuring the effectiveness of a real-world project against what happened in observably identical areas. Estimates reported here are thus lower bounds of program effectiveness against a counterfactual of no interventions whatsoever in comparison areas.

# 3. Data and Outcome

## Data

3.1    According to CNP 2010 baseline survey documentation, the baseline survey design included calculations of minimum detectable effect across proxies of the 11 original project development objective (PDO) indicators using the 2005 Lao Reproductive Health Survey or the 2006 Multiple Indicator Cluster Survey. These calculations took into account the multilevel survey design and intracluster correlation. Calculations based on power of 0.80 and two-sided significance at the 5 percent level yielded an optimal sample size of 3,000 households.

3.2    The health center sample was generated through the following steps. First, the Ministry of Health purposively selected 62 health centers (based on loosely defined criteria) to receive the intervention from all health centers in the seven central and south provinces designated as eligible for the intervention. Second, the World Bank country team randomly selected 20 health centers for data collection from among those 62. Third, after this sampling of the treatment health centers, the World Bank country team identified 20 nontreatment health centers as comparators using Stata's "nnmatch" algorithm to select treatment and comparison health center pairs (see appendix A for the list of paired health centers). This process used characteristics of the health center,[9] demographics,[10] and the geographic

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[9] Health center characteristics used include total number of health center staff, whether the health center had a laboratory for examination, whether it used a computer, and whether the health center used electricity.

[10] Demographic characteristics include total population size, poverty rate, and dominant ethnic group. The dominant ethnic group was defined as more than 70 percent of the population belonging to one of six ethnic groups (Lao, Khmuic, Katuic, Bahnaric Khmer, Vietic, and Hmong). In cases where there was no dominant ethnic group in a given health center service area, a best match of the individual ethnic groups was used.

characteristics[11] of its service area to generate the best single matching comparator for each health center.[12]

3.3     The random sample of 20 health centers was not stratified by province. As a result, health centers from only six of the seven intervention provinces entered the random sample. In searching for comparators, the World Bank country team identified health centers in five of the seven intervention provinces (Xekong province was completely excluded from the sample, and Attapeu province was included only for the treatment sample).[13]

3.4     The sampling protocol then called for a random sample of five villages per health center and 15 households with a child under two years old per village (see appendix B for a further description of the conceptual sampling framework). In cases with fewer villages and/or households than prescribed in the protocol, data were collected from additional households in neighboring villages within the same health center's service area. If no such villages were available, additional households were collected from the existing villages. Instances of supplementing the sample by more than three households from other villages were rare.[14] The mapped locations of the public health centers and their corresponding sampled villages are seen in figure 3.1.

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[11] The geographic characteristics are difficulty of terrain measured through range of elevation, and mean travel time to the district town.

[12] In cases where two of the selected health centers happened to have the same match, the better of the two fits was selected, and the second was taken for the rest of the health centers.

[13] See appendix A for a list of the treatment and matched comparator health centers and corresponding districts.

[14] In only 4 percent of villages were there both i) a fewer number of both baseline and endline households than the protocol required, and ii) the number of household in the endline was smaller than the baseline by more than three households.

**Figure 3.1. Location of Public Health Centers and Associated Villages**
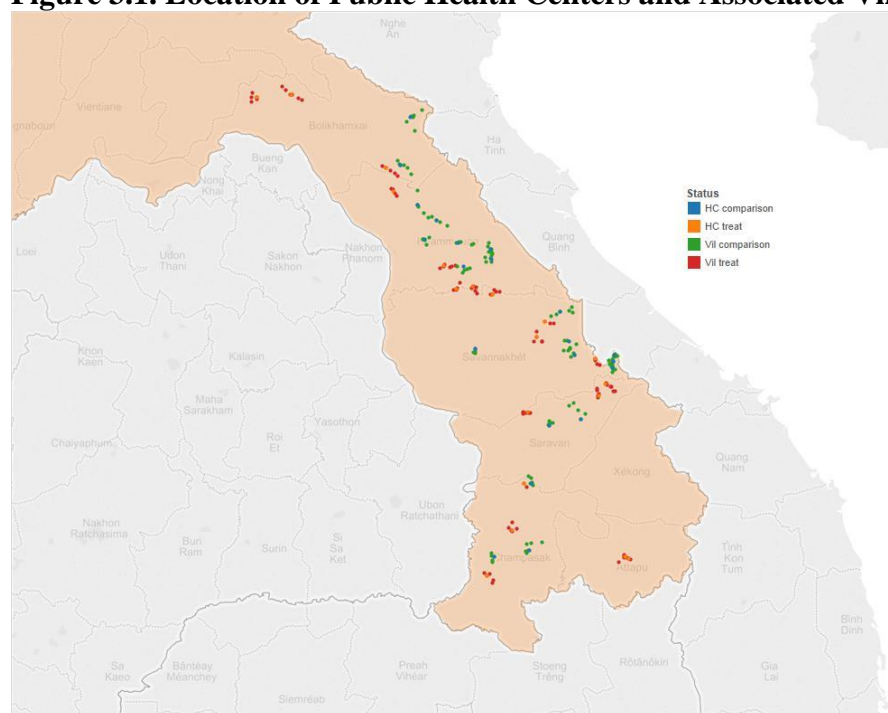
3.5     For both the treatment and comparison areas, the survey instrument collected data at three levels: health centers, villages, and households. The baseline was collected from April to June 2010, before project implementation. The follow-up was collected from May to July 2013, just before the project closing in September 2013. Because the intervention was designed to help children under age 24 months and mothers, and because the households with a child of that age would be different in the endline versus the baseline, the two rounds of data collection constituted a health center–level and village-level panel, and a repeated cross section at the household level. Consistency between baseline and endline surveys was maintained by hiring the same local survey firm for data collection in both rounds.

3.6     As a result, the baseline has 41 health centers, 207 villages, and 2,979 households. Compared with the national average in the Lao Social Indicator Survey 2011–12 and other surveys in a similar period, the baseline survey population has a lower education level of the household head, more non-Lao Tai ethnicity, a lower utilization rate for antenatal care and delivery services, less health insurance coverage, improved sanitation facilities, and fewer monthly household expenditures (World Bank 2013). Because of the Nam Theun 2 Hydropower Project, one health center in the comparison area (Sabkam health center) was flooded during the CNP project implementation, and so was dropped from the analysis. The endline dataset, therefore, covers 40 health centers, 201 villages, and 3,269 households.

3.7     Along with the Sabkam health center, two more health centers (Nongboua from the treatment group and Sob One from the comparison group) were dropped because the villages in the service areas of those two public health centers received relocation benefits and

compensation from the hydropower project during CNP implementation.[15] As a result, data from 19 treatment and 19 comparison health centers and their respective villages (191 total panel villages) and households (2,864 children in households at baseline and 3,101 children in households at endline) are used in the analysis.

3.8     Analysis of the household dataset uses probability weights to make it representative at the health center level. The probability weight is calculated based on the inverse of the product of the proportion of villages selected for the survey sample from the health center service area and the proportion of households selected from a sampled village.[16] The proportion of selected households is calculated using the roster of program-eligible households in each specific village rather than the village population or all households in the village.

## Outcome Measures

3.9     The CNP originally defined 11 PDOs. After a formal restructuring by the World Bank, six main indicators were retained: (i) antenatal care provided by skilled health personnel, (ii) child delivery at a health facility, (iii) DPT3 immunization before the first birthday, (iv) attending a monthly growth checkup, (v) breastfeeding within one hour after birth, and (vi) receipt of oral rehydration solutions during diarrhea. Exact definitions are given in table 3.1.[17]

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[15] Two small villages (Mamonluek and La Vern) were also dropped because they were not sampled at endline (though they were at baseline). The Mamonluek village (in Kimae Health Center) and La Vern village (in Phabang Health Center) have six and seven households; dropping them does not materially affect results. Also, two households do not have health center information; because the intervention indicator cannot be generated for these households, they are also dropped.

[16] $Sampling\ Weight = \left[\frac{Number\ of\ Villages\ Selected\ from\ HC_j}{Number\ of\ Villages\ in\ HC_j} \times \frac{Number\ of\ Households\ Selected\ from\ Village_i\ in\ HC_j}{Number\ of\ Households\ in\ Village_i\ from\ HC_j}\right] - 1$

[17] The ages of the mother and child are adjusted depending on the outcome being estimated for consistency with the Lao Social Indicator Survey 2011–12 (the analysis uses mother's age between 15 and 49). As described in table 3.1, children age 0–11 months are used for all analyses of the formal project development objectives (PDOs), unless otherwise stated, with the exception of PDO indicator 3 (which uses children age 12–23 months) and PDO indicator 6 (which uses children age 0–23 months).

**Table 3.1. Project Development Outcome Indicators**

| PDO | PDO indicators description |
|---|---|
| PDO1 | Percent of women age 15–49 years who were attended at least once during pregnancy in the past 12 months by a skilled health personnel |
| PDO2 | Percent of women age 15–49 with a birth in the last 12 months delivered at a health facility |
| PDO3 | Percent of children age 12–23 months receiving DPT3 before their first birthday |
| PDO4 | Percent of women age 15–49 with a child age 0–11 months who attended at least one routine monthly checkup in the past 12 months |
| PDO5 | Percent of women age 15–49 with a live birth in the past 12 months who breastfed within one hour of birth |
| PDO6 | Percent of children age 0–23 months with diarrhea in previous two weeks who received ORS |

*Source:* World Bank 2015.
*Note:* DPT = diphtheria, pertussis and tetanus; ORS = oral rehydration solutions; PDO = project development objective.

3.10     Because important anthropometric outcomes have not improved much during the last decade in Lao PDR, this report also examines stunting, underweight, and wasting for eligible children. Effect estimates on additional outcomes related to each PDO are also calculated to better understand behavioral changes; these outcomes are defined as follows:

3.11     **Antenatal Care**: PDO indicator 1 (PDO1) refers to antenatal care from skilled health personnel. This outcome (listed in the regression table in chapter 5 as Attended by health staff versus Attended by other persons + None) takes the value 1 if antenatal care is supported by skilled health staff (either doctor, nurse/midwife, or auxiliary nurse) and 0 otherwise (antenatal care seen by either traditional birth attendant, village health volunteer, family, friend, traditional healer, or nobody). PDO1 implicitly assumes that the proportion of biological mothers who see someone for antenatal care during pregnancy will be increased, and given that pregnant mothers have limited access to health facilities other than the nearest public health center, this is the overall extramarginal effect of the CNP. Yet given the cash incentive is conditional on visits to the public health center and not for visits to other facilities, health center visits are examined against all other options (other institutional visit + noninstitutional visit + no antenatal care visit). These alternative options against which health center visits are compared is then broken down to inspect whether there is a substitution effect (or inframarginal effect) of visits moving from other institutions[18] to health centers (health center versus other institutional) and/or an extramarginal effect of individuals coming to the health center who would not have had an institutional visit otherwise (health center versus noninstitutional + no antenatal care visit).

3.12     **Delivery:** PDO2 is one of the most important outcomes—the proportion of women who delivered their baby at the health facility. Although the incentive size for delivery is far

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[18] The "other institutions" category includes government district hospitals, provincial hospitals or central hospitals; private hospitals, clinics, or maternity homes; and hospitals or other institutions in Thailand or Viet Nam.

larger, it is intended to cover higher anticipated expenditures incurred for deliveries (for example, food and potentially higher transport costs due to the uncertainty of time of travel), and be a stronger incentive to encourage more deliveries at health centers. With the far stronger incentive for this action, and the robust evidence that vouchers and CCTs are the interventions most likely to improve skilled birth attendance (IEG 2013), the treatment effect on this outcome is expected to be pronounced. The health facility is defined here as the public health center, but also government hospitals (district, provincial, and central) and private hospitals, clinics, and maternity homes. The PDO2 outcome (institutional delivery versus noninstitutional delivery) takes the value 1 if the delivery took place at any of these health facilities, and 0 if delivery occurred at home, in the forest or outdoors, or in a birth structure. The inframarginal effect (health center delivery versus other institutional delivery) and other comparators (health center delivery versus noninstitutional delivery, health center delivery versus other institutional delivery + noninstitutional delivery) are also examined to better understand the health-seeking behavioral change regarding delivery.

3.13    **DPT3:** Lao PDR is one of few countries where the prevalence of tetanus has yet to be eliminated (Masuno et al. 2009), even though the disease is fully preventable through three doses of the diphtheria, pertussis, and tetanus (DPT) vaccination. Accordingly, the project set the PDO3 indicator to increase the share of children age 12–23 months who receive the three doses of combined DPT-hepatitis B-Haemophilus influenza type B vaccine before their first birthday. This indicator implicitly assumes that all the vaccination and date of birth records are observable; unfortunately, this is not the case, even when requesting records from the child's immunization card.

3.14    Although some respondents could show full immunization records on their vaccination card, many responses on the number of DPT3 vaccinations were based on recall, and no dates are recorded for those who gave this information from recall. The PDO3 outcome takes the value 1 if children age 12–23 months received DPT-hepatitis B-Haemophilus influenza type B at least three times according to recall or vaccine card information provided by their biological mother or guardian, and 0 otherwise.

3.15    The proportion of children with an immunization card actually observed by interviewers was also examined as a proxy for the household's awareness about vaccination and conscientiousness about their child's health—that is, it was assumed that the household is more health conscious of their child if they could show a vaccination card for the child.

3.16    **Checkup**: PDO4 examines the effect on monthly routine checkups for children, which is incentivized through CCT for up to 12 visits in the first year of life and up to six visits in the child's second year (table 2.1). The outcome takes the value 1 if caregivers indicate that the child went to at least one well-child checkup and 0 otherwise (any growth checkup versus none). The report also examines whether the program caused an increase in repeat visits with an indicator for two or more visits.[19] Unlike the cases of antenatal care and delivery, the location of the routine growth checkup and the personnel who assisted with the

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[19] Unfortunately, the structure of the baseline questionnaire does not allow treating the number of visits as a continuous variable.

checkup are not available in the questionnaire, so those health-seeking behaviors cannot be examined for checkups.

3.17    **Breastfeeding:** WHO (2010) recommend that newborns begin breastfeeding within one hour of birth, and this is the basis for PDO5. This outcome takes the value 1 if children get breastfed within one hour of birth, and 0 otherwise. The candidate responses to the question "How long after delivery did you start breastfeeding him/her?" differ slightly for baseline and endline. Instead of using the explicit term "within one hour," the endline questionnaire lists "immediately" as one of the candidate responses to this question, which eventually takes the value 1 under the PDO definition. Even though supplementary guidance is provided to code "immediately" if the breastfeeding was provided within one hour of birth, the possibility that the endline interviewer might code "one hour" instead of "immediately" cannot be ruled out if the biological mother breastfed her child within one hour of birth. Consequently, a variable for breastfeeding within three hours of birth was also developed.

3.18    **Receive Oral Rehydration Solutions with Diarrhea:** The final indicator, PDO6, examines the proportion of children receiving oral rehydration solutions among those who reported having diarrhea in the last two weeks. The rehydration solution is usually defined as either fluid from an Oralyte packet or prepackaged Oralyte fluid, but the Ministry of Health also promotes the use of a government-recommended homemade solution as another anti-diarrheic option. Two outcome variables were developed: one coded for typical oral rehydration solution usage, and another that includes the Ministry of Health option. The outcome takes the value 1 if children are treated through these measures during diarrhea, and 0 otherwise. Because the community-based nutrition component could have direct effects on reducing the incidence of diarrhea itself through teaching techniques for improved sanitation and hand washing education, the incidence of diarrhea within the last two weeks was also examined. Similarly, for those who contracted diarrhea in the last two weeks, the report assesses the likelihood of seeking advice and/or treatment from skilled health personnel for the diarrhea or not (not limited to taking oral rehydration solution measures).

3.19    **Anthropometry:** Anthropometric measures, such as low birth weight, stunting, underweight, and wasting, are not explicitly included in the PDOs, though they are used to frame the project in the appraisal document. These measures could be improved if CNP had positive influences on maternal and child health–seeking behaviors. Mother and child weights were measured to the nearest 0.1 kilogram. The child's height was measured to the nearest 0.1 centimeter. The height-for-age (HAZ), weight-for-age (WAZ), weight-for-height (WHZ) standardized z-scores were normalized by month and computed in accordance with the WHO 2006 child growth standards (WHO 2006).[20] Stunting is dichotomous and defined as HAZ less than two standard deviations below the median. Similarly, underweight is

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[20] Biologically implausible outliers are recoded to missing. These include WAZ < −6, WAZ > 5, HAZ < −6, HAZ > 6, WHZ < −5 and WHZ > 5 as described in the Readme file of the Stata igrowup package prepared by WHO.

defined as WAZ scores two standard deviations or more below the median, and wasting is defined as WHZ scores two standard deviations or more below median.[21]

# 4. Methods

## Difference-in-Differences

4.1      This report uses a difference-in-differences evaluation method to identify the plausible causal relationships between project outcomes and the combined demand-side intervention of conditional cash transfer and community-based nutrition. For child $i$ between 0–23 months of age, the basic difference-in-differences estimating strategy is described as a linear specification for outcome $Y_j$ through

$$Y_{ij} = \beta_0 + \beta_1 D_i + \beta_2 T_i + \gamma(D_i \times T_i) + X_i'\beta + \varepsilon_{ij}$$

where $\beta_0$ is a constant term, a binary variable $D$ captures the fixed effects summarizing the time invariant unobserved effects between the Community Nutrition Project (CNP) intervention and comparison areas (unity for treatment and zero for comparison), $T$ is a binary variable representing baseline and endline time variations (unity for endline and zero for baseline), $(D \times T)$ is an interaction of the CNP intervention area and time, $X$ is a set of observed characteristics for child $i$ that is used consistently across all outcomes $j$, and $\varepsilon$ is an individual specific error term. The evaluation team was interested in estimating $\gamma$, the coefficient of $(D \times T)$ demonstrating the overall effect of the CNP intervention under the ordinary least squares. The resulting program estimates are the intention to treat effects at the household level.

4.2      $Y$ represents PDO indicators, which are predefined by the project documents. Most of the project outcomes are measured by a binary indicator that takes the value 1 if maternal and child health (MCH) service–seeking behavior is made for the child $i$ under the study, and 0 otherwise (for example, whether or not a woman had an antenatal visit attended by skilled health personnel). This report presents linear probability models (LPM) of the difference-in-differences—and matched difference-in-differences discussed later in this chapter—specifications including province-level fixed effects and robust standard errors clustered at the village level.

4.3      As a robustness check, the report also estimates a nonlinear logit model. With a difference-in-differences estimation strategy on binary outcomes, the interpretation of $\gamma$ under a logit (or probit) specification does not directly indicate a causal relationship because all expected outcomes are bounded by 0 and 1, which could violate the parallel trend assumption (Puhani 2012). To this end, the report calculates the marginal effect of the dummy variable of interaction term for causal interpretation as the difference in cross-

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[21] Low birth weight, defined as less than 2,500 grams, could also be a potentially important outcome, but birth weight information is not credible because most of the available responses (n=1,079) for final impact evaluation analysis are based on recall. Half of the samples are placed disproportionately to either exactly 2 kg, 2.5 kg, 3 kg, 3.5 kg or 4 kg.

differences between the conditional expectations of the observed outcome and the counterfactual outcome.[22] Continuous outcomes, including most anthropometrics, such as standardized height-for-age, are estimated through a linear difference-in-differences model; results with and without covariates are reported. The impact evaluation estimates for $\gamma$ are reported both with and without controlling for covariates (under the heading DD) for the linear and nonlinear models for dichotomous outcomes. Regression tables report only the coefficients for the outcomes of interest, and report the baseline and endline mean values of the comparison and treatment groups. The analysis of dichotomous dependent variables also includes nonlinear logit estimates.

4.4    The set of covariates in vector $X$ are chosen to explicitly control for variables that could potentially confound the relationship between treatment and the MCH outcomes of interest. The set of covariates consists of four different levels of individual, household, village, and health center attributes. Individual, child-level characteristics include the child's age, gender, and whether the child is the mother's firstborn. The household-level characteristics captured in the regression are the mother's age, weight, education, presence of the child's grandmother and grandfather, whether any children passed away in the household, ethnicity, the number of children in the household, the total number of births the mother delivered, socioeconomic status, and external shocks. The socioeconomic status variable is produced by a principal component analysis of the household asset indexes (Filmer and Pritchett 2001)—through the examination of scree plots of eigenvalues and factor loadings— and the socioeconomic status is divided into one short-term (consumables) asset index[23] and two long-term (durables) asset indexes,[24] which use consumption-type assets and durable assets (further details are found in appendix C). A shock index was also created using principal component analysis.[25] Considering that the intervention was born out of the global food crisis and because it does not fit well in psychometric analysis of the other shock items, price shock dummies—increases in consumption food prices and decreases in agricultural selling prices—enter into the regressions separately. The presence of the child's grandmother and grandfather is included to control for the intrahousehold decision-making dynamics in determining MCH practices as conveying intergenerational knowledge on delivery options and experience at the time when the mother was born. Village-level variables include travel time to the closest health center from the village; urban or rural status; and ethnic congruence between the village majority and the household head. Finally, since gender and ethnicity of the health staff at the public health center could be important considerations for access to delivery in the context of Lao PDR (Sychareun et al. 2012), those variables were explicitly controlled. For the consistency of the analysis, the same set of covariates $X$ is applied in

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[22] This is done through the "margin" postestimation command in Stata 12.1 (Stata Corporation, College Station, Texas, USA). Since the difference-in-differences estimates the average treatment effect on the treated at the endline, to run the "margin" command, this report sets both the time and intervention variables as 1, and the rest of the variables are set to the sample mean.

[23] The short-term asset index includes ownership of a motorcycle, bicycle, refrigerator, electric rice cooker, electric fan, two-wheel tractor (tuk-tuk), boat, fishing net, radio, telephone, mobile phone, and satellite dish.

[24] The long-term asset index includes availability of a toilet, main source of electricity (electricity or fuel), main source of floor (high class, wood, low class), and main source of wall (wood or bamboo).

[25] The shock index includes dichotomous variables for whether the household experienced drought, fire, floods, crop disease, illness or death of the household head, illness or death of other household members, resettlement, and robbery.

analyzing the various outcomes of interest unless otherwise stated. Further details on the definitions of each covariate are provided in appendix D.

4.5     The identifying assumption of the difference-in-differences method is parallel trending after controlling for covariates—that is, the observed change in the outcome for the comparison group would be the same as the counterfactual change in the treatment group had there been no intervention. This is a strong assumption which cannot be directly tested. To bolster this assumption in practice, many empirical studies use multiple rounds of pretreatment data for both the treatment and comparison group. If the parallel trend can be observed between periods before intervention, it can be argued that this is also likely to be true for the later period between baseline and endline. In this report, despite having only one pretreatment dataset, time trajectories of outcomes can be constructed for both the treatment and comparison group, taking advantage of the child's age in months (as shown in appendix E). This would mimic an actual "pre-trend" for antenatal and delivery outcomes that occurred at or just around child birth. The similarities of such trajectories between the treatment and comparison area provide useful—if still somewhat indirect—evidence on the plausibility of the identifying assumption. As shown in appendix E, the parallel trend for the pretreatment period (different age in month under the baseline dataset) seems to hold for PDO1, PDO2, PDO4 (12–23 months) and the tested anthropometric outcomes. However, PDO3, PDO5, and PDO6 follow a similar but less exact trend during the pretreatment period, meriting caution in the interpretation of the outcomes for these measures.

## Matched Difference-in-Differences

4.6     As noted above, the comparators for the 20 treatment health centers were selected before initiation of the baseline survey. This pre-matching exercise identified treatment and comparison health center pairs (appendix A) using administrative data and followed the selection criteria ostensibly employed by the Ministry of Health. As a robustness check, this report also uses a propensity score matching in conjunction with the difference-in-differences estimation procedure (a matched difference-in-differences model).

4.7     This second round of matching was done using the survey data collected for this evaluation through calculating the propensity score by again following the treatment assignment decision of the Ministry of Health using characteristics of the health centers and their service areas. Given the relatively small number of health centers (n = 38), $k$ nearest neighbors with $k = 4$ propensity score matching was specified. The health center level pretreatment baseline variables for matching included the number of proper staff at the public health center, mean and range of elevation of the public health center, mean access time to the health center from the villages in the service area, population of the health center service area, ethnicity, and basic infrastructure of the health center. The proxy of the pretreatment outcomes, such as the number of deliveries at the public health center and the postnatal visit at baseline, was also included in the matching algorithm to account for unobserved characteristics that might be correlated with the intended outcome. As a result, the risk of bias was reduced for all of the above pretreatment baseline variables except electrification of the public health center (appendix F). In particular, the bias reduction in the proxies of the pretreatment outcomes is 99.5 percent for public health center delivery and 13.6 percent for postnatal visits. However, because 11 of the 38 health centers were off-support after applying

this second matching algorithm, the analyzed sample is reduced to 27 health centers (12 intervention and 15 comparison). Most of the off-support health centers are located in northern regions of the project area. Furthermore, though the baseline balance was improved for delivery outcome, mother's educational background, and socioeconomic status of the household, the balance of other variables, such as ethnicity, became somewhat worse (appendix G). These imbalances were, however, explicitly controlled for in the set of covariates, and time invariant unobservable confounders on the village- and health center–level are ameliorated through the difference-in-differences specification. After this matching exercise, the difference-in-differences analysis was repeated on the weighted on-support sample, as previously described.

# 5. Results

## Main Results

5.1     The only statistically significant effects of the project on improving the PDO indicators is for PDO3: children between one and two years old having received the full complement of diphtheria, pertussis, and tetanus (DPT) vaccinations (at least three shots).[26] In the unconditioned linear models, there is also a marginally significant result on the probability of attending at least one routine growth checkup for the full sample (10.7 percentage points, PDO4), though the effect washes out when controlling for covariates and even have a marginally significant negative effect in matched sample with logit. Contrary to original expectations and a recent comprehensive systematic review of the literature (IEG 2013), although the CNP has marginally significant effect on matched sample with logit, the CCT in Lao PDR does not lead to a statistically significant difference in the rate of institutional deliveries (PDO2). Neither did the project demonstrate an effect on the PDO-defined outcomes of antenatal visits (PDO1), breastfeeding within one hour of birth (PDO5), or of administering oral rehydration solutions to a diarrheic child (PDO6).

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[26] The official project development objective designates that the DPT3 series should have been completed before the child turns one year old, but since vaccine administration dates are unavailable for a large share of the children, this requirement (vaccination before the first birthday) is not included in the definition used for this evaluation.

**Table 5.1. Difference-in-Differences Results on the Full and Matched Sample for PDOs**

| PDO | Indicator | Full Sample | | | | | | | Matched Sample | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Baseline | | Endline | | DD[a] | With covariates | | | With covariates | | |
| | | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| PDO1 | Attended by health staff vs. Attended by other persons + None (all births, 0-11 months) | 0.28 | 0.43 | 0.55 | 0.73 | **0.035** (0.060) | 3,274 | **0.015** (0.051) | **0.008** (0.057) | 2,320 | **0.087** (0.053) | **0.079** (0.065) |
| PDO2 | Institutional delivery vs. Non-institutional delivery (all births, 0-11 months) | 0.13 | 0.21 | 0.25 | 0.35 | **0.014** (0.045) | 3,274 | **-0.018** (0.040) | **-0.018** (0.053) | 2,320 | **0.047** (0.048) | **0.070** * (0.039) |
| PDO3 | DPT is at least three times (last birth, 12-23 months) | 0.41 | 0.45 | 0.46 | 0.66 | **0.164** *** (0.058) | 2,277 | **0.149** *** (0.050) | **0.193** *** (0.064) | 1,643 | **0.119** * (0.062) | **0.162** ** (0.089) |
| PDO4 | Any growth checkup vs. None (last birth, 0-11 months) | 0.04 | 0.10 | 0.42 | 0.59 | **0.107** * (0.056) | 3,257 | **0.046** (0.054) | **-0.104** (0.100) | 2,307 | **0.004** (0.082) | **-0.222** * (0.121) |
| PDO5 | Breastfeeding within one hour of birth vs. None (last birth, 0-11 months) | 0.41 | 0.40 | 0.36 | 0.42 | **0.074** (0.062) | 3,257 | **0.070** (0.060) | **0.076** (0.062) | 2,308 | **0.042** (0.074) | **0.070** (0.072) |
| PDO6 | Received ORS (1, 2 or 3) during diarrhea vs. Not received (last two births, 0-23 months) | 0.64 | 0.64 | 0.62 | 0.70 | **0.077** (0.082) | 933 | **0.042** (0.085) | **0.048** (0.088) | 731 | **0.010** (0.114) | **0.020** (0.113) |

*Source:* IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

5.2       There is a stark difference in conclusions on effectiveness of the project depending on the metrics and methods used. Simple pre-post comparisons and whether targets for PDO indicators are met give very different conclusions than the impact evaluation estimates. Table 5.2 shows the single pre-post difference for the treatment area. All six PDO indicators could be reasonably considered to meet the project target. PDO5 (breastfeeding within one hour of birth) did not meet the target value, but 81 percent of the endline survey population breastfed within three hours, far exceeding the target value of 60 percent. The modest results through impact evaluation are explained by the similar increase in the comparison areas. For instance, there is a remarkable increase in the share of the target population that had at least one growth checkup, improving from 10 percent to 59 percent in treatment areas; but the comparison area experienced a similar increase, from 4 percent to 42 percent. Potential non-CNP drivers of this secular trend are discussed in box 5.1.

**Table 5.2. Single Pre-Post Difference for the Treatment Area on PDOs**

| PDO | Indicator | Baseline | Endline | Target |
|-----|-----------|----------|---------|--------|
|     |           | (percent) | | |
| PDO1 | Percent of women age 15–49 years who were attended at least once during pregnancy in the past 12 months by a skilled health personnel | 43 | 73 | 60 |
| PDO2 | Percent of women age 15–49 years with a birth in the last 12 months that delivered at a health facility | 21 | 35 | 30 |
| PDO3 | Percent of children age 12–23 months receiving DPT3 before their first birthday | 45 | 66 | 50 |
| PDO4 | Percent of women age 15–49 with a child age 0–11 months who attended at least one routine monthly checkup in the past 12 months | 10 | 59 | 50 |
| PDO5 | Percent of women age 15–49 with a live birth in the past 12 months who breastfed within one hour of birth | 40 | 42 | 60 |
|      | Percent of women age 15–49 with a live birth in the past 12 months who breastfed within three hours of birth | 51 | 81 | — |
| PDO6 | Percent of children age 0–23 months with diarrhea in the previous two weeks who received ORS | 64 | 70 | 70 |

*Source:* World Bank 2015.
*Notes:* The definition of PDO5 is breastfeeding within one hour, but because of the comparability issue of the question, breastfeeding within three hours of birth is also included in this table. DPT = diphtheria, pertussis and tetanus; ORS = oral rehydration solutions; PDO = project development objective.

**Box 5.1. Drivers of Changes in Comparison Area**

The before/after comparison (table 5.2) shows that the targets of the CNP were successfully met for all six project development outcome measurements, but the impact evaluation findings (table 5.1) suggest that there is little overall effect except for full DPT vaccination. It seems that the program effects are swamped in magnitude by the other contemporary factors in the Lao context. This stark difference in results may be partly explained by the secular trends in economic growth and improved transportation.

Between 2010 and 2013, when the baseline and endline data were collected, Lao PDR achieved steady real gross domestic product (GDP) growth of more than 8 percent per year. According to the fifth round of the Lao PDR Expenditure Consumption Survey, the national poverty headcount rate ebbed from 27.56 percent in 2007–08 to 23.24 percent in 2012–13. The World Bank estimates that every 1 percentage point in GDP growth is associated with a 0.47 percentage point reduction of poverty headcount rate. A similar inverse relationship is likely for growth and maternal and child health. Still, although most of the provinces in Lao PDR reduced the poverty rate during this period, poverty worsened in CNP intervention provinces such as Saravan and Champasak, among others.

Another possible explanation for the improvement in both treatment and control areas is improved transport. The cost of transportation generally increased and the distance from village to health center remained unchanged, but the time to health center was reduced by one-third, from an average of about 45 minutes to 30 minutes. The household variation in travel time as measured by the standard deviation was also reduced over time. This reduction in travel time suggests that improved infrastructure or mode of transportation may have induced greater overall use of health services.

*Source:* Poverty Report for the Lao Consumption and Expenditure Survey, 2012–13.

## Associated Results

5.3     Even though the project can claim attribution for improvements in only one of the six PDOs, that is largely because of a mismatch between the definition of the PDOs and what the project was designed to do or what the data can reliably answer. By examining the domains of these objectives in more detail, this evaluation shows that the project had a significant effect on a meaningful aspect of nearly all of the objectives.

5.4     The results in the tables in this section come in pairs by age group. Most PDOs are defined as being relevant for children under one year in age (births within the last year). Each of the alternative views of the PDOs gives estimated results for this age group and the complete age group that was sampled: children under two years. The exception is PDO3, which can only be defined for children between one and two years of age, and PDO6 which is defined for children under 2 (the table presents alternative outcomes for children under 1). The relevant PDO from table 5.1 is reproduced at the top of tables 5.3–5.8.

5.5     **Antenatal Care:** Do the PDO1 results mean that CNP has no impact at all on antenatal care? Some important nuances are found when other associated antenatal care behavioral outcomes are examined in table 5.3.

5.6     The impact evaluation results show that the project cannot claim attribution for the improvements seen in PDO 1 (table 5.1, row 1; table 5.3, row 1a), defined as antenatal care visits by skilled health personnel for mothers giving birth in the last year. Despite the significant results for the more complete sample of beneficiaries (births within the last two

years) in the better identified specifications (the matched sample), results are empirically similar for the representative (full) sample, measuring by whether care was given by personnel or by institutions (table 5.1, rows 1a and 1b, 2a and 2b). This is not surprising, given the 0.97 correlation rate between antenatal care visits measured by staff and institutional attendance. The matched difference-in-differences results do suggest that a small share of mothers in the intervention area who had visits by health staff outside of the health center (often at home or in the village center) instead have those visits at health centers where they would be paid for their time;[27] however, these results cannot be generalized to the whole intervention population because the matching process drops health centers from the representative sample.

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[27] The margins inducing this change are quite small: The share of mothers that had a health staff visit outside of a health institution dropped from 3.6 percent in the baseline to 0.75 percent in the endline for the treatment area, but increased from 2.6 percent to 2.9 percent in the comparison service area.

# Table 5.3. Antenatal Care Behavioral Outcomes

| | | Full Sample | | | | | | | | Matched Sample | | |
| | | Baseline | | Endline | | DD[a] | With covariates | | | With covariates | | |
| PDO Indicator 1: Antenatal Care | | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1a | PDO1: Attended by health staff vs. Attended by other persons + None (all births, 0-11 months) | 0.28 | 0.43 | 0.55 | 0.73 | **0.035** (0.060) | 3,274 | **0.015** (0.051) | **0.008** (0.057) | 2,320 | **0.087** (0.053) | **0.079** (0.065) |
| 1b | PDO1 - older: Attended by health staff vs. Attended by other persons + None (all births, 0-23 months) | 0.26 | 0.43 | 0.51 | 0.74 | **0.055** (0.049) | 5,759 | **0.057** (0.045) | **0.048** (0.050) | 4,129 | **0.126** ** (0.052) | **0.111** * (0.065) |
| 2a | Institutional ANC visit vs. Non-institutional + No ANC visit (last birth, 0-11 months) | 0.27 | 0.41 | 0.54 | 0.72 | **0.048** (0.059) | 3,258 | **0.031** (0.050) | **0.021** (0.058) | 2,308 | **0.097** * (0.053) | **0.090** (0.066) |
| 2b | Institutional ANC visit vs. Non-institutional + No ANC visit (last birth, 0-23 months) | 0.25 | 0.41 | 0.50 | 0.74 | **0.075** (0.051) | 5,565 | **0.076** * (0.045) | **0.064** (0.051) | 3,983 | **0.143** *** (0.050) | **0.131** ** (0.064) |
| 3a | Health center visit vs. Other institutional + Non-institutional + No ANC visit (last birth, 0-11 months) | 0.13 | 0.24 | 0.42 | 0.64 | **0.117** ** (0.056) | 3,258 | **0.105** * (0.054) | **0.053** (0.082) | 2,308 | **0.197** *** (0.065) | **0.166** * (0.092) |
| 3b | Health center visit vs. Other institutional + Non-institutional + No ANC visit (last birth, 0-23 months) | 0.12 | 0.24 | 0.38 | 0.64 | **0.132** *** (0.049) | 5,565 | **0.142** *** (0.049) | **0.094** (0.074) | 3,983 | **0.215** *** (0.062) | **0.173** ** (0.087) |
| 4a | Health center visit vs. Other institutional visit (last birth, 0-11 months) | 0.48 | 0.58 | 0.77 | 0.88 | **0.012** (0.102) | 1,558 | **0.017** (0.074) | **0.039** (0.038) | 1,034 | **0.028** (0.089) | **0.053** (0.035) |
| 4b | Health center visit vs. Other institutional visit (last birth, 0-23 months) | 0.48 | 0.58 | 0.77 | 0.86 | **-0.008** (0.084) | 2,607 | **0.035** (0.068) | **0.053** (0.046) | 1,713 | **0.030** (0.086) | **0.050** (0.044) |
| 5a | Health center visit vs. Non-institutional + No ANC visit (last birth, 0-11 months) | 0.15 | 0.29 | 0.47 | 0.70 | **0.090** (0.061) | 2,897 | **0.057** (0.057) | **0.008** (0.076) | 2,061 | **0.123** * (0.067) | **0.094** (0.087) |
| 5b | Health center visit vs. Non-institutional + No ANC visit (last birth, 0-23 months) | 0.14 | 0.29 | 0.44 | 0.71 | **0.123** ** (0.053) | 4,950 | **0.110** ** (0.052) | **0.059** (0.071) | 3,555 | **0.173** *** (0.063) | **0.134** (0.084) |

*Source*: IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

5.7      Lines 3–5 of table 5.3 examine the effects of the program according to what actually the CCT incentivized. Only antenatal care visits at health centers were eligible for the transfer, as opposed to a broader category of health institutions that can include province and district hospitals and private clinics.[28] Across both the 0- to11-month age group consistent with the PDO accounting and the 0- to 2-year age group included in the survey work and eligible for benefits from the project, there is an increase of between 10.5 percentage points and 21.5 percentage points in the share of pregnant mothers who sought care specifically at public health centers versus all other options in linear probability specifications. This result holds for nearly all specifications in lines 3a and 3b (the logit specifications in the full sample being the only exception). So although it cannot be said that the PDO was met for increasing the rate of antenatal care overall, the project did increase antenatal care visits to these centers.

5.8      To understand why there would be an effect on health center–specific antenatal care, but not on general antenatal care, the analysis investigated whether the increase in health center antenatal care came from a substitution effect (an inframarginal effect) that simply induced women who likely would have sought antenatal care anyway to switch from other institutional care into health center care, or whether there was an extramarginal effect, in which mothers who would not have sought institutional care came to the health center. Although no evidence was found for a substitution effect, there is more robust evidence of an extramarginal effect for the complete age group in the linear models.[29] This highlights a mismatch between the PDO indicators and the project design: the extramarginal effect for health center–specific visits among the complete age group is swamped by the nondifferentiated null results, which include visits for institutions not incentivized by the project.

5.9      **Delivery:** The primary indicator for assessing the progress of the fifth Millennium Development Goal—a reduction in maternal mortality of 75 percent—is measured by the improvements in the proportion of births attended by skilled health personnel. The Independent Evaluation Group recently completed a systematic review of all interventions with an impact evaluation that estimates improvements in skilled birth attendance (IEG 2013). That review concluded that vouchers and CCTs were the interventions most likely to result in improvements in skilled birth attendance. By contrast, this CNP intervention in Lao PDR found positive effects for the matched sample, but not for the representative full sample (rows 5a and 5b of table 5.4).

5.10     In the face of the implementation challenges previously described, the instruments in this intervention were insufficient to improve health-seeking behavior in most specifications, particularly in the full sample estimates. As seen in rows 1a and 1b of table 5.4, the World Bank was likely not responsible for the PDO reaching its target. Most frequently in the specifications in table 5.4, the point estimate for program effectiveness is near zero, with relatively small standard errors.

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[28] Visits to those other institutions would be eligible to receive CCT payment only in cases of referral from the health center.

[29] As will be seen in the rest of the report, the full age group models often report larger point estimates that are more likely to be statistically significant. This may be because the backlog in CCT payments to early participants (those 12–23 months) undermined participation of later beneficiaries (those age 0–11 months).

## Table 5.4. Delivery Behavioral Outcomes

| | | Full Sample | | | | | | | | Matched Sample | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Baseline | | Endline | | DD^a | | With covariates | | | With covariates | |
| **PDO Indicator 2: Delivery** | | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| 1a | PDO2: Institutional delivery vs. Non-institutional delivery (all births, 0-11 months) | 0.13 | 0.21 | 0.25 | 0.35 | **0.014** (0.047) | 3,274 | **-0.018** (0.040) | **-0.018** (0.053) | 2,320 | **0.047** (0.048) | **0.070** * (0.039) |
| 1b | PDO2 - older: Institutional delivery vs. Non-institutional delivery (all births, 0-23 months) | 0.11 | 0.20 | 0.23 | 0.33 | **0.013** (0.039) | 5,759 | **-0.010** (0.035) | **-0.020** (0.045) | 4,129 | **0.056** * (0.034) | **0.071** *** (0.027) |
| 2a | Health center delivery vs. Other institutional delivery + Non-institutional (all births, 0-11 months) | 0.04 | 0.08 | 0.14 | 0.17 | **-0.013** (0.043) | 3,274 | **-0.022** (0.037) | **-0.043** (0.042) | 2,320 | **0.050** (0.041) | **0.029** ** (0.012) |
| 2b | Health center delivery vs. Other institutional delivery + Non-institutional (all births, 0-23 months) | 0.03 | 0.08 | 0.12 | 0.16 | **-0.004** (0.035) | 5,759 | **-0.009** (0.032) | **-0.038** (0.032) | 4,129 | **0.054** (0.034) | **0.028** *** (0.012) |
| 3a | Health center delivery vs. Other institutional delivery (all births, 0-11 months) | 0.29 | 0.39 | 0.54 | 0.49 | **-0.161** (0.121) | 713 | **-0.219** *** (0.083) | **-0.287** ** (0.117) | 381 | **0.016** (0.095) | **-0.033** (0.142) |
| 3b | Health center delivery vs. Other institutional delivery (all births, 0-23 months) | 0.28 | 0.40 | 0.50 | 0.49 | **-0.140** (0.105) | 1,156 | **-0.132** * (0.078) | **-0.158** (0.117) | 621 | **0.046** (0.082) | **0.039** (0.113) |
| 4a | Health center delivery vs. Non-institutional delivery (all births, 0-11 months) | 0.04 | 0.10 | 0.15 | 0.21 | **-0.002** (0.048) | 2,927 | **-0.025** (0.041) | **-0.056** (0.055) | 2,102 | **0.044** (0.041) | **0.029** ** (0.014) |
| 4b | Health center delivery vs. Non-institutional delivery (all births, 0-23 months) | 0.03 | 0.09 | 0.13 | 0.19 | **0.005** (0.040) | 5,183 | **-0.011** (0.035) | **-0.051** (0.042) | 3,769 | **0.053** (0.034) | **0.030** ** (0.013) |
| 5a | Delivery assisted by skilled health staff (all births, 0-11 months) | 0.17 | 0.23 | 0.29 | 0.38 | **0.034** (0.048) | 3,274 | **0.008** (0.040) | **0.031** (0.047) | 2,320 | **0.085** * (0.047) | **0.121** *** (0.039) |
| 5b | Delivery assisted by skilled health staff (all births, 0-23 months) | 0.15 | 0.22 | 0.27 | 0.36 | **0.025** (0.040) | 5,759 | **0.010** (0.035) | **0.022** (0.040) | 4,129 | **0.077** ** (0.034) | **0.104** *** (0.028) |

*Source:* IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

5.11    As with the PDO1 outcomes, rows 3a through 5b examine whether there was an effect on the incentivized behavior of delivering specifically at a health center. Surprisingly, evidence indicates the opposite may be true. The full sample regression estimates show that among those who recently delivered at a health institution, the institution was some 20 percentage points less likely to be a health center for mothers living in intervention catchment areas compared with mothers living in comparison areas. In other words, the proportion of pregnant mothers delivering at public health centers versus other health facilities increased for both the treatment and comparison groups, but the rate of increase is faster in comparison areas than in the treatment areas—despite the potential of a sizeable cash incentive for mothers in intervention areas.

5.12    One potential explanation for this counterintuitive result of impaired growth may be the possible reputational damage caused by the bumpy implementation of the project. More than 65 percent of women who delivered at the public health center did not receive the minimum amount of cash transfer in the treatment area. Such a breach of trust among the early adopters may have been shared with other expectant mothers, which caused later cohorts to be disaffected by the CCT scheme and induced fewer to deliver at health centers than would have if the project had not happened.

5.13    **DPT3:** There is a statistically strong positive effect on DPT3 vaccination. Even if the definition of DPT3 was changed slightly to be "exactly three vaccinations" instead of "three or more DPT vaccinations," the results still hold with the same magnitude (table 5.5).[30]

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[30] Payments for well-child visits were available every month for the first 12 months and six times for children between the ages of 13 and 24 months.

## Table 5.5. DPT3 Behavioral Outcomes

| PDO Indicator 3: DPT3 | Baseline | | Endline | | DD[a] | With covariates | | | With covariates | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | **Full Sample** | | | **Matched Sample** | | |
| | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| 1 PDO3: DPT is at least three times (last birth, 12-23 months) | 0.41 | 0.45 | 0.46 | 0.66 | **0.164** *** (0.058) | 2,277 | **0.149** *** (0.050) | **0.193** *** (0.064) | 1,643 | **0.119** * (0.062) | **0.162** * (0.089) |
| 2 DPT is at least three times (last birth, 12-23 months) within vaccination card holder | 0.81 | 0.65 | 0.69 | 0.77 | **0.236** *** (0.081) | 924 | **0.256** *** (0.073) | **0.326** *** (0.109) | 612 | **0.155** (0.102) | **0.170** (0.134) |
| 3 DPT equals three times (last birth, 12-23 months) | 0.41 | 0.43 | 0.45 | 0.65 | **0.188** *** (0.059) | 2,277 | **0.167** *** (0.050) | **0.217** *** (0.063) | 1,643 | **0.140** ** (0.062) | **0.190** ** (0.088) |
| 4 Proportion of mothers/guardians who could show vaccination card (last birth, 12-23 months) | 0.28 | 0.24 | 0.38 | 0.75 | **0.413** *** (0.057) | 2,307 | **0.362** *** (0.055) | **0.467** *** (0.063) | 1,675 | **0.475** *** (0.065) | **0.623** *** (0.073) |

*Source*: IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

5.14    In a somewhat related outcome, the rate of improvement of biological mothers or guardians being able to show the immunization card to the enumerator was much larger for the intervention group—increasing by 51 percentage points from the baseline proportion of 24 percent. Although the comparison groups also increased, the gains in the intervention group were still 41 percentage points higher. All these results strongly suggest that the CNP induced positive behavioral changes regarding immunization. The latter also implies that parents in intervention areas may be taking their children's health more seriously, since they are more fastidious about caring for the child's card. Or perhaps the clinics are better at providing cards and keeping immunization records. Regardless of whether the improvements come from changes in provision or utilization, being able to produce a card is potentially a proxy for more careful attention to child health, which may well result in better care and child health in many small ways not observable by the relatively blunt instrument of this survey.

5.15    **Checkup**: The rate of checkups saw phenomenal growth between the baseline and endline, and the simple difference-in-differences estimates indicate that the simple growth rate in treatment areas was 10–14 percentage points higher than in baseline areas. Even so, the more robust multivariate models in rows 1a and 1b of table 5.6 do not support such claims (a marginally significant result in one specification of the 0- to 23-month-old group notwithstanding).

5.16    It is notable that the intensive margin improved for both the treatment and comparison groups. For both groups, the share visiting at all or multiple times moved from less than 10 percent to the 30–50 percent range. Notwithstanding the fact that parents could have received a payment nearly every month for taking their child to a monthly well-child visit,[31] the robust models do not support the assertion that the intervention induced parents to bring their children for checkups at a higher rate than parents who did not have the incentive (table 5.6, rows 2a and 2b). This suggests that the cash transfer amount may not be enough to induce mothers to bring in a child who appears healthy more than once. Short-term public financial support might not induce long-term adoption of health services if there is no learning effect or a negative learning effect in using those services (Dupas 2014c); this dynamic may be occurring in the case of well-child checkups in this context.

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[31] Payments for well-child visits were available every month for the first 12 months and six times for children between the age of 13 and 24 months.

# Table 5.6. Routine Checkup Behavioral Outcomes

| | | Full Sample | | | | | | | | Matched Sample | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Baseline | | Endline | | DD$^a$ | | With covariates | | | With covariates | |
| **PDO Indicator 4: Checkup** | | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| 1a | PDO4: Any growth checkup vs. None (last birth, 0-11 months) | 0.04 | 0.10 | 0.42 | 0.58 | **0.107** * | 3,257 | **0.046** | **-0.104** | 2,307 | **0.004** | **-0.222** * |
| | | | | | | (0.056) | | (0.054) | (0.100) | | (0.082) | (0.121) |
| 1b | PDO4 - older: Any growth checkup vs. None (last birth, 0-23 months) | 0.05 | 0.12 | 0.45 | 0.66 | **0.144** *** | 5,562 | **0.097** * | **-0.026** | 3,980 | **0.075** | **-0.122** |
| | | | | | | (0.051) | | (0.049) | (0.075) | | (0.073) | (0.093) |
| 2a | 2+ checkups vs. 1 or 0 checkup (last birth, 0-11 months) | 0.03 | 0.07 | 0.30 | 0.41 | **0.067** | 3,257 | **0.010** | **-0.138** | 2,307 | **-0.009** | **-0.274** |
| | | | | | | (0.050) | | (0.047) | (0.115) | | (0.078) | (0.168) |
| 2b | 2+ checkups vs. 1 or 0 checkup (last birth, 0-23 months) | 0.04 | 0.10 | 0.35 | 0.50 | **0.095** ** | 5,562 | **0.052** | **-0.079** | 3,980 | **0.042** | **-0.190** |
| | | | | | | (0.047) | | (0.044) | (0.085) | | (0.072) | (0.119) |

*Source:* IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

5.17    **Breastfeeding**: Although the project did not cause a statistically significant effect on breastfeeding within one hour of birth (PDO5), another variant of this outcome does demonstrate a robust effect (table 5.7). As previously described, there is potentially an issue of comparability in the construction of the questionnaire between the baseline and the endline for breastfeeding within one hour. However, that challenge is mitigated when defining the initiation of breastfeeding within three hours of birth (the minimum time explicitly comparable between the baseline and endline instruments). Using this definition of breastfeeding, the project successfully caused a 6–14 percentage point increase in mothers nursing their babies; these results are robust across most specifications.

5.18    **Receive Oral Rehydration Solutions with Diarrhea:** There was no detectable difference in trends for PDO6 for children age 0–23 months (table 5.8, rows 1a and 2b) or 0–11 months (table 5.8, rows 1b and 2a), regardless of whether or not its definition including the government-approved homemade recipe. The CNP did not reduce the incidence of diarrhea (table 5.8, rows 3a and 3b). However, the project demonstrated a positive impact on seeking treatment and/or advice for children who had diarrhea in the last two weeks for children under age one and under age two (table 5.8, rows 4a and 4b). The results imply that mothers from intervention areas *sought* treatment at a higher rate, even if they were no more likely to *receive* it than mothers in comparison areas.

# Table 5.7. Breastfeeding Behavioral Outcomes

| | Full Sample | | | | | | | | Matched Sample | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Baseline | | Endline | | DD[a] | | With covariates | | | With covariates | |
| **PDO Indicator 5: Breastfeeding** | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| **1a** PDO5: Breastfeeding within 1 hour of birth vs. None (last birth, 0-11 months) | 0.41 | 0.40 | 0.35 | 0.42 | **0.074** (0.062) | 3,257 | **0.070** (0.060) | **0.076** (0.062) | 2,308 | **0.036** (0.067) | **0.042** (0.074) |
| **1b** PDO5 - older: Breastfeeding within 1 hour of birth vs. None (last birth, 0-23 months) | 0.42 | 0.41 | 0.37 | 0.46 | **0.099** (0.063) | 5,564 | **0.085** (0.062) | **0.091** (0.065) | 3,983 | **0.057** (0.069) | **0.063** (0.075) |
| **2a** Breastfeeding within 3 hours of birth vs. None (last birth, 0-11 months) | 0.51 | 0.51 | 0.74 | 0.81 | **0.075** (0.045) | 3,257 | **0.062** (0.044) | **0.064** * (0.036) | 2,308 | **0.129** ** (0.055) | **0.113** ** (0.047) |
| **2b** Breastfeeding within 3 hours of birth vs. None (last birth, 0-23 months) | 0.52 | 0.51 | 0.73 | 0.82 | **0.091** ** (0.043) | 5,564 | **0.069** (0.043) | **0.071** ** (0.035) | 3,983 | **0.137** *** (0.049) | **0.119** *** (0.043) |

*Source*: IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

# Table 5.8. Oral Rehydration Solutions with Diarrhea Behavioral Outcomes

| | Full Sample | | | | | | | | Matched Sample | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Baseline | | Endline | | DD[a] | With covariates | | | With covariates | | |
| PDO Indicator 6: Diarrhea with ORS | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| 1a **PDO6**: Received ORSplus (1, 2 or 3) during diarrhea vs. Not received (last two births, **0-23 months**) | 0.64 | 0.64 | 0.62 | 0.70 | **0.077** | 933 | **0.042** | **0.048** | 731 | **0.01** | **0.02** |
| | | | | | (0.082) | | (0.085) | (0.088) | | (0.114) | (0.113) |
| 1b PDO6 - **younger**: Received ORSplus (1, 2 or 3) during diarrhea vs. Not received (last two births, **0-11 months**) | 0.51 | 0.59 | 0.56 | 0.63 | **-0.006** | 465 | **-0.011** | **-0.006** | 354 | **-0.025** | **-0.020** |
| | | | | | (0.110) | | (0.117) | (0.118) | | (0.146) | (0.156) |
| 2a Received ORS (1 or 2) during diarrhea vs. Not received (last two births, 0-11 months) | 0.43 | 0.47 | 0.45 | 0.60 | **0.112** | 465 | **0.101** | **0.118** | 354 | **0.079** | **0.126** |
| | | | | | (0.101) | | (0.104) | (0.114) | | (0.126) | (0.159) |
| 2b Received ORS (1 or 2) during diarrhea vs. Not received (last two births, 0-23 months) | 0.55 | 0.53 | 0.52 | 0.68 | **0.173** ** | 933 | **0.115** | **0.131** | 731 | **0.121** | **0.140** |
| | | | | | (0.076) | | (0.079) | (0.084) | | (0.111) | (0.119) |
| 3a Had diarrhea in last two weeks or not (last two births, 0-11 months) | 0.13 | 0.14 | 0.15 | 0.13 | **-0.036** | 3,271 | **-0.027** | **-0.024** | 2,313 | **-0.028** | **-0.020** |
| | | | | | (0.038) | | (0.033) | (0.031) | | (0.038) | (0.035) |
| 3b Had diarrhea in last two weeks or not (last two births, 0-23 months) | 0.16 | 0.16 | 0.16 | 0.14 | **-0.019** | 5,753 | **-0.008** | **-0.005** | 4,122 | **0.006** | **0.004** |
| | | | | | (0.032) | | (0.029) | (0.027) | | (0.038) | (0.032) |
| 4a Treated for diarrhea conditional on having diarrhea (last two births, 0-11 months) | 0.58 | 0.53 | 0.60 | 0.76 | **0.206** * | 465 | **0.239** * | **0.257** * | 354 | **0.318** ** | **0.358** ** |
| | | | | | (0.117) | | (0.122) | (0.135) | | (0.141) | (0.151) |
| 4b Treated for diarrhea conditional on having diarrhea (last two births, 0-23 months) | 0.62 | 0.56 | 0.62 | 0.77 | **0.212** ** | 933 | **0.196** ** | **0.206** ** | 731 | **0.191** * | **0.202** * |
| | | | | | (0.084) | | (0.086) | (0.090) | | (0.110) | (0.115) |

*Source:* IEG.

*Notes*: The robust standard error is reported in parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. comp = comparison area; DD = difference-in-differences; LPM = linear probability model; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

5.19     Because the incidence of diarrhea may be nonrandom even after controlling for observable characteristics, the evaluation team members applied a Heckman selection model to the diarrhea treatment indicator as a robustness check, using village-level incidence of diarrhea as the selecting variable (see appendix H for details). This covariate is highly significant in the first stage on incidence of diarrhea for the child. Wald test results do not reject the null hypothesis of no correlation between the error term and unobserved determinants of diarrhea incidence, which implies that the incidence of diarrhea after controlling for covariates is indeed random. The magnitude of the coefficient and standard error are remarkably stable between the selected and nonselected models. The slight reduction in significance levels is likely due to the decrease in power in the selection models. Consequently, the non-selected results in table 5.8 are maintained as the main findings for this PDO.

5.20     **Anthropometry:** The anthropometric outcomes are not included in the final PDO indicators, even though this project was clearly framed by the prevalence of poor nutritional status at the time of the initial grant agreement funding the project (World Bank 2009). Furthermore, the CNP ultimately aims to improve the nutritional status of the beneficiary population and would ideally contribute to reductions in the prevalence of stunting, underweight, and wasting that have persisted in Lao PDR for decades. Clearly, issues contributing to these anthropometric outcomes are complex, and the project was probably right to exclude these as project objectives because they were too ambitious for a relatively small project. Still, because the CNP influenced some positive behavioral changes observed under CNP, it is worth investigating any potential effects on these measures. As shown in table 5.9, the two bundled demand-side interventions under CNP are not strong enough to improve the anthropometric outcomes. None of them—height-for-age z-score, weight-for-age z-score, or weight-for-height z-score (rows 1a–3b), or stunting, underweight, or wasting (rows 4a–6b)—shows a statistically significant improvement.[32]

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[32] As a robustness check, this null result holds even when including outliers (results are not shown).

# Table 5.9. Anthropometric Outcomes

| | Full Sample | | | | | | | Matched Sample | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Baseline | | Endline | | DD[a] | | With covariates | | With covariates | |
| Indicator: Anthropometry | Comp | Treat | Comp | Treat | (Et-Bt)-(Ec-Bc) | n | LPM | | n | LPM | |
| 1a Height-for-age z-score (HAZ, 0-11 months) | -1.13 | -1.05 | -1.04 | -0.84 | **0.115** (0.152) | 3,214 | **0.161** (0.150) | - - | 2,268 | **0.071** (0.188) | - - |
| 1b Height-for-age z-score (HAZ, 0-23 months) | -1.55 | -1.38 | -1.41 | -1.28 | **-0.035** (0.119) | 5,640 | **0.000** (0.119) | - - | 4,029 | **-0.043** (0.146) | **-** - |
| 2a Weight-for-age z-score (WAZ, 0-11 months) | -1.27 | -1.15 | -1.14 | -0.91 | **0.110** (0.109) | 3,255 | **0.103** (0.103) | - - | 2,302 | **-0.058** (0.153) | - - |
| 2b Weight-for-age z-score (WAZ, 0-23 months) | -1.52 | -1.40 | -1.32 | -1.18 | **0.019** (0.086) | 5,720 | **0.011** (0.079) | - - | 4,096 | **-0.071** (0.108) | **-** - |
| 3a Weight-for-height z-score (WHZ, 0-11 months) | -0.65 | -0.65 | -0.53 | -0.46 | **0.070** (0.132) | 3,176 | **0.027** (0.124) | - - | 2,241 | **-0.068** (0.151) | - - |
| 3b Weight-for-height z-score (WHZ, 0-23 months) | -0.87 | -0.84 | -0.66 | -0.64 | **-0.022** (0.098) | 5,580 | **-0.070** (0.089) | - - | 3,987 | **-0.132** (0.096) | - - |

| | | | | | DD[a] | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 4a Stunting (HAZ < -2, 0-11 months) | 0.29 | 0.24 | 0.25 | 0.20 | **0.003** (0.035) | 3,214 | **-0.002** (0.034) | **-0.009** (0.030) | 2,268 | **-0.002** (0.041) | **-0.013** (0.038) |
| 4b Stunting (HAZ < -2, 0-23 months) | 0.40 | 0.33 | 0.34 | 0.31 | **0.045** (0.029) | 5,640 | **0.037** (0.029) | **0.035** (0.028) | 4,029 | **0.028** (0.038) | **0.024** (0.041) |
| 5a Underweight (WAZ < -2, 0-11 months) | 0.28 | 0.23 | 0.23 | 0.21 | **0.031** (0.035) | 3,255 | **0.025** (0.033) | **0.016** (0.029) | 2,302 | **0.048** (0.047) | **0.029** (0.044) |
| 5b Underweight (WAZ < -2, 0-23 months) | 0.36 | 0.31 | 0.28 | 0.26 | **0.027** (0.028) | 5,720 | **0.027** (0.027) | **0.021** (0.025) | 4,096 | **0.029** (0.036) | **0.018** (0.036) |
| 6a Wasting (WHZ < -2, 0-11 months) | 0.14 | 0.12 | 0.11 | 0.10 | **0.004** (0.030) | 3,176 | **0.005** (0.027) | **0.001** (0.022) | 2,241 | **0.036** (0.033) | **0.020** (0.022) |
| 6b Wasting (WHZ < -2, 0-23 months) | 0.16 | 0.16 | 0.12 | 0.12 | **0.004** (0.024) | 5,580 | **0.009** (0.024) | **0.006** (0.018) | 3,987 | **0.024** (0.025) | **0.016** (0.020) |

*Source*: IEG.

*Notes*: The robust standard error is reported in the parentheses, which is clustered at the village level. The coefficient and standard error of the time and intervention interaction term are reported through linear probability model (LPM) and marginal effect of logit regression. Sample size for LPM and Logit is not always the same, but sample size for LPM is only reported here given the minor difference between the two specifications. HAZ, WAZ and WHZ are continuous variable, and logit regression is not applied. comp = comparison area; DD = difference-in-differences; HAZ = height-for-weight z-score; LPM = linear probability model; treat = treatment area; WAZ = weight-for-age z-score; WHZ = weight-for-height z-score.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent

# 6. Heterogeneity

6.1 Lao PDR has a vast geographical, sociocultural, and economic diversity, and care-seeking behavior varies by these factors. This chapter explores potential heterogeneous effects of the CNP through subgroup analyses on the established PDO indicators along these dimensions: gender, ethnicity, whether or not the child's mother had a previous child die, the mother's educational background, being the first-born child or not, the household being in the bottom 40 percent of the short-term and long-term asset indexes or not, experience price increase or decrease shocks (self-reported), and geographic aspects, including province, rural-urban classification of the village, and distance from the village to the health center. The bottom 40 percent of the asset indexes are examined because this measurement is used to measure the promotion of shared prosperity, one of the twin goals of the World Bank. All subgroup analyses compare those within same subgroup across the intervention versus comparison areas as opposed to comparing treatment effects across subgroups within the same treatment status (intervention or comparison).

6.2 All the covariates used for the main results in table 5.1 are controlled for in these analyses except for the variable being investigated through subgroup analysis. The effect estimates for the program on PDO indicators for each subgroup are summarized in appendix I; each of the tables in appendix I corresponds with a PDO, and the top row of each of those tables reproduces the overall estimates for each PDO indicator from table 5.1. Although estimates do not explicitly correct for multiple hypothesis testing, the only results discussed below are those that are fairly consistent across specifications and with at least one result significant at the 1 percent level.

6.3 A substantial challenge for subgroup analysis is the reduction in sample size. This results in a decrease in statistical power, often increasing the minimum detectable effect size beyond what the survey was originally designed to detect. Consequently, this section focuses on those dimensions showing significant differences instead of null results. Furthermore, some subsamples are too small to produce reliable estimates—these are reported in the appendix I tables with a dash ( - ). All estimates are for children (or their mothers) born within the year preceding the survey, except for those in PDO3, which estimated effects for children between one and two years old, and PDO6, which pooled children under two years old.

6.4 **PDO1:** Although there was no significant effect for the general results for PDO1 (table 5.1), the project seems to have significantly increased the likelihood of seeing a professional health worker for an antenatal visit in the Khammaun province by 19.3–23.3 percentage points. A large, significant, and fairly robust positive effect is observed for those who experienced a price increase shock in purchased staples. There is weaker evidence that mothers from intervention areas were more likely to seek antenatal care than observably identical mothers in matched comparison areas.

6.5 **PDO2**: Delivery at the health facility for births within the preceding year varies by province. As with PDO1, there is evidence that CNP-area mothers in Khammaun (and more

weakly, the Saravan province) were more likely to give birth at a health institution. The overall null result for this PDO indicator, however, may be explained by the result of these positive effects being offset by a large, highly significant negative result in Bolikhamxay.

6.6     Socioeconomic status effects are somewhat mixed. Although wealthy intervention residents were more likely to deliver in health institutions than wealthy residents of comparison areas,[33] the intervention was effective for the Mon-Khmer ethnic minority and the uneducated. The intervention caused an increase in institutional delivery among those who lived near a health center.

6.7     **PDO3**: The CNP caused a significant increase in PDO3 for the general population estimates. Therefore, many of the subgroups examined also show positive effects on children between one and two years old receiving at least three diphtheria, pertussis, and tetanus immunizations—in particular, mothers with some primary but no secondary schooling, those living between 3 kilometers and 6 kilometers from the nearest public health center, children without a deceased sibling, and households at the bottom 40 percent of the second principal component of the short-term asset (consumables) index. Also, families immunizes their children as a result of the project, both among those who did and did not report a price increase in their consumables, and from both the top and bottom of the wealth distribution.[34]

6.8     **PDO4**: Subgroup analysis reveals an intriguing mix of positive and negative effects for different segments of the beneficiary population for PDO4, giving insight into the null overall effect that alternates between small positive point estimates for the linear probability models and negative point estimates for the nonlinear models (table 5.1). Unfortunately, motivations for this mix of effects by motherhood experience, wealth, ethnic background, and locality are not always easily understood or explained.

6.9     Experience in motherhood seems to have led to mixed interactions with the project. First-born children were less likely to have a growth checkup if they lived in a project area than if they did not—perhaps, again, because the negative reputational effects of the project's delay in disbursing the transfer was especially influential for new parents who had weaker priors. By contrast, there was weaker evidence that mothers who had previously lost a child were more likely to take advantage of the project and bring their baby in for a growth checkup.

6.10    Wealth was also associated with variation in the likelihood of seeking a growth checkup because of the intervention. Better-off families (those in the top two quintiles for the long- or short-term asset indexes) were less likely to seek a checkup if living in a CNP area than similar families living in a control area. Conversely, there are mixed results for those in the bottom two quintiles: Estimates indicate a positive interaction for the poor as measured

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[33] As measured by the second component of the long-term asset index, which loads more heavily on high-quality building materials in floors and walls.

[34] As measured by the second component of the long-term (durable) asset index, which loads more heavily on high-quality building materials in floors and walls.

by the first principal component of the long-term (durables) asset index, but a negative interaction effect for those in the bottom of the second principal component.

6.11    Ethnic background had a polarizing influence on the project's ability to induce demand for growth checkups. Those in the Lao Tai majority ethnic group were less likely to bring their children for visits, but the project was utilized among the Mon-Khmer.

6.12    Finally, location seems to have played a role in project effectiveness. Like the institutional delivery result of PDO2, the Saravan province saw large benefits from the program with regard to child growth checkups—perhaps because almost all sampled families in the province were ethnic Mon-Khmer. However, the project may have made parents who lived more than 6 kilometers away from a health center less likely to bring their child to a checkup than similar parents from comparison areas.

6.13    **PDO5**: The project was particularly effective among the poor for inducing an increase in the likelihood that mothers breastfed their babies within the first hour of birth. Mothers from households from the bottom two quintiles of the durables index and those who experienced a price increase shock in consumed goods were significantly more likely to immediately breastfeed their children if they lived in an intervention area. But so, too, did mothers from the top two quintiles of the consumables index.

6.14    Province and ethnicity again played a moderating role. Households from Khammaun were negatively influenced by the project, but residents of Savanhnakhet and Champasak, along with ethnic Mon-Khmer, were favorably affected.

6.15    **PDO6**: As shown in table 5.1, the project did not affect the likelihood of receiving an oral rehydration solution for children with diarrhea. Compared with the other PDOs, none of the subgroups demonstrated strong, consistent effects. Positive, consistent but marginal effects were observed among more educated mothers. A significant interaction is observed for first children in the logit models, and a single highly significant result appears for those who experienced a decrease in the price of products sold by the household. Beyond that single result, and in contrast to the other PDOs, there is no evidence that the poor benefitted from this project with regard to receiving oral rehydration solutions for diarrheic children. However, this could be partially due to the smaller sample size of these estimates imposed by the conditionality of having diarrhea.

## The CNP as an Effective Social Protection Mechanism

6.16    The CNP caused overall improvements in MCH in only one of its six primary indicators, but it is worth noting that the project was fairly consistent in having an effect among the most vulnerable across nearly all of the PDO indicators. Mothers and children from households in the bottom 40 percent of the distribution benefited from the CNP. The MCH service utilization dimensions of institutional delivery and antenatal and postnatal checkups increased for the poor. This group was underutilizing services, and CNP narrowed the outcomes gap by providing them greater opportunities to access essential MCH services.

6.17    Among those who self-reported price increase shocks in their consumption basket, the project improved outcomes against those shocks. There is evidence that the CNP worked as a social protection measure against price volatility.

6.18    There is still some variation by province and ethnicity across the PDOs. Those who are in Saravan province—the poorest province in Lao PDR and composed predominantly of the minority Mon-Khmer ethnic group—were more likely to contribute to the positive results on delivery and checkup. Still, such regional effects are not always straightforward: the Khammaun province generated positive effects on antenatal and delivery outcomes, but postnatal health-seeking behavior outcomes were negative.[35]

6.19    This positive impact on the poor and vulnerable might be due to lower opportunity costs. Implementation challenges may have increased the cost of engaging with the project. Delayed CCT payments, for example, may have reduced the appeal of the project for those who are relatively better off. By extension, the project likely could have done more for potential beneficiaries and even for the poor, some of whom were surely dissuaded from participation by the implementation challenges. This implies that if the project were better implemented in the planned scale-up, benefits could be larger on both the intensive and extensive margins.

6.20    These findings may appear to have implications for targeting in future programming of similar policy instruments. However, explicit targeting may not be necessary. There was evidently some self-selection in use. Explicit targeting may not necessarily be more efficient if the desired beneficiary group self-selects, if targeting is difficult to implement and monitor, if it undermines the social compact, or if it creates a negative stigma that erodes the inclination of targeted beneficiaries to use the service. Targeting considerations should be undertaken carefully.

6.21    Differential targeting by distance to the health center, for example, proved difficult to implement. The differential CCT incentive structure was tiered depending on distance from the household to the nearest public health center (less than 3 kilometers, 3 to 6 kilometers, and more than 6 kilometers). As constituted, the differential CCT added bureaucratic complexity without demonstrating differential effects. There is no significant effect within distance subgroups on antenatal care (PDO1). Those who live either less than 3 kilometers or more than 6 kilometers away from the nearest health center had positive effect on institutional delivery among matched samples, but no significant effect was witnessed in the representative (full) sample for households in the intermediate band of the transfer amount–conditioned distance to the health center (3 to 6 kilometers). Perhaps the distance-differentiated incentive was simply not strong enough. Postnatal visits are also tied to a tiered transfer. Those who lived within 3 kilometers of a public health center had more routine

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[35] Caution should be taken when interpreting the regional effects found in appendix I on heterogeneous effects. As indicated previously, the sample was not stratified by province. Therefore, although the sample is balanced overall, it may not be balanced in sample size between intervention and comparison households by region. Some provinces are solely represented in the data by intervention households; others solely by comparison households.

growth checkups, but the project seems to have had a negative effect for those who lived more than 6 kilometers away.[36]

# 7. Discussion

7.1      The CNP had desirable effects on several outcomes, but evidence of sizable positive program effects were not observed for every PDO indicator or its associated variants. What factors are driving these mixed results? In particular, why is it difficult to increase MCH service utilization despite the direct cash incentive to use the public health center services?

7.2      This report estimates the combined impact of the two components of the CNP: the CCT scheme and the community-based nutrition meetings. Still, it may be useful to examine the project design of the CCT element against implicit assumptions that must hold for such a project to yield results. Nine assumptions underlying health CCTs are summarized in table 7.1 (Gaarder, Glassman, and Todd 2010). This chapter discusses these assumptions in the context of the CNP in Lao PDR.

**Table 7.1. Nine Underlying Assumptions of CCT Intervention to Yield Health Outcomes**

| No | Underlying assumptions |
|---|---|
| 1 | CCT interventions lead to an increase in the use of preventive health services among the poor, who are currently underutilizing these services. |
| 2 | An increase in utilization of health care services will improve health status; in particular, an increase in public health services will have this effect. |
| 3 | Cash affects health primarily by ensuring service utilization and improved food consumption. |
| 4 | Poor women lack sufficient health knowledge and that a transfer of information to them will induce behavior changes. |
| 5 | Imposing conditions and monitoring compliance are necessary to increase utilization of services to the desired level. |
| 6 | Some program assumed that the cash transfer and the conditions are not sufficient to ensure optimal child nutritional investment, and therefore have added a food supplement. |
| 7 | The existing supply of services is sufficient or will increase following increases in demand. |
| 8 | Program beneficiaries and program staff are correctly informed about the program. |
| 9 | The evaluation methods chosen implicitly assume that the outcomes impacted by the program are those that are measured. |

*Source:* Gaarder et al. 2010, 12–13.

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[36] This evaluation also aimed to understand the differential comparative effect of the size of the incentive through regression discontinuity using these geographic cutoffs, but the distance data are only available at the village level, and incentive tiers are apparently assigned by distance to the dwelling. There was not enough statistical power with regard to variation and sample size to credibly investigate this effect.

7.3     Explicit testing of whether these assumptions are met is beyond the scope of this evaluation, but may be picked up by the efforts of the country team that is doing a descriptive analysis of the project and preparing for a nationally scaled project to be informed by the experience of the CNP. Here, it is simply noted where the results from this evaluation may illuminate the validity of the implicit assumptions in design. The discussion of what was learned from this impact evaluation is framed in relation to the underlying assumptions of the logic model of a health CCT scheme.

7.4     First, as noted by the low baseline rates of all of the PDO indicators, it seems clear that health services are underutilized. Conversely, the impact evaluation results also clearly indicate that the CCT was effective at incentivizing women—especially poor women—to a small, attributable increase in utilization rates of two of the three services that were conditioned by design: antenatal and well-child checkups. Still, take-up rates often remain somewhat low with significant room to improve, as seen in the endline treatment column for nearly every one of tables 5.2–5.7. The actual project implementation period of two years might be too short to have sufficiently large take-up rates without delay.

7.5     Evidence supporting assumption two, that increased use will improve health, exists at a level largely appropriate to the scale of the program. Although there were increases attributable to immunization rates and breastfeeding, and reductions in the incidence of diarrhea, there were no detectable effects on standardized height or weight anthropometrics. Still, a relatively modest pilot intervention of less than $4 million ($2 million and €1.44 million) with a short actual intervention horizon of just two years is probably insufficient to expect changes in anthropometrics, even without the implementation challenges the project experienced.

7.6     The third assumption contends that cash is used to improve utilization. This assumption was incorporated into the design since women were paid each time (and only if) they came to the health center. However, even if the stronger cash incentive is intended for giving birth at clinics, there are many cases where the minimum amount of CCT for delivery (KN 260,000) was not provided for beneficiaries in accordance with the CCT design protocol. The incentives are not necessarily small for antenatal care and checkups in light of median per capita consumption in rural areas, but they still might not be sufficient to overcome barriers to utilization.

7.7     One such challenge could be transportation. Travel can be particularly onerous for women who are pregnant, in labor, or transporting small children. "Time to health center" was a consistently negative and statistically significant covariate for all three utilization outcomes (PDOs 1, 2, and 4) and across the various econometric specifications (results are not shown). This may explain the somewhat small effect sizes when they do exist, and the lack of an effect altogether for repeated well-child visits.

7.8     Regarding assumption four, preexisting knowledge of MCH practices is likely to be weak. As previously discussed, food taboos are still prevalent in this society. The low baseline values for breastfeeding within one hour of birth (table 5.6) and using oral rehydration solutions during diarrhea (table 5.7) support the view that there is insufficient knowledge of MCH practices. The project sought to find that that increased health center

visits would result in increased exposure to health knowledge transfers from health staff; more directly, the community-based nutrition activities through regular village meetings collectively sought to improve health knowledge among beneficiaries. Although the community-based nutrition village meetings were expected to be held every month in the treatment area, they occurred far less frequently—an average of only 1.4 times in the 6 months before the survey. Aside from their infrequent occurrence, the Aide Memoire in the last supervision mission indicates that meetings frequently covered multiple modules in a single sitting, and the average number of participants per village meeting was 45 people— more than originally envisioned.

7.9     The oral rehydration solutions outcomes perhaps best exemplify that the fourth assumption held. Mothers in the intervention area sought treatment at a higher rate, even though diarrhea incidence or treatment area was no better than in comparison areas. This shows that before the intervention, they lacked knowledge on what to do for diarrheic children, but they sought treatment because of the intervention. Unfortunately, there is no evidence that they received the treatment at a higher rate than the comparison areas. This again shows that changing behavior is insufficient to accrue health gains if the capacity of the health system cannot handle the additional demand induced by the CCT. Still, this illustrates a clear example of health behaviors improving through seeking and obtaining useful health knowledge.

7.10    Because the intervention did not have an unconditional arm, it is difficult to say with certainty whether the conditions were required, as indicated by assumption five. However, given that the size of the transfers seemed barely sufficient to induce small increases in the rate of visits, it seems unlikely that giving these amounts of cash outside of the context of a health visit would have induced those visits to happen with the same level of efficacy.

7.11    Similarly, there was no food supplementation in the program (assumption six). Still, as shown by the lack of anthropometric results, such supplementation may be a useful addition if sustained throughout the first 1,000 days of life from conception to age two.

7.12    Although the seventh assumption is not directly testable by the impact evaluation methods with the available data, Aide Memoires and conversations with the World Bank team indicate that health center staff may not have been sufficiently trained or had sufficient numbers to adequately take on the increased demands the CNP made on their time. Recent work in Lao PDR (Yamada et al. 2013) suggests that health centers do not have a sufficient number or sufficiently qualified health staff. Although the absenteeism rate is lower than five comparator developing countries,[37] health center staff absenteeism (currently at 17 percent) is aggravated by disruptions to payment streams. This could potentially increase the waiting and consultation time for treatment at the clinic. A similar dynamic could have taken place in the CNP, where project administration became an unfunded mandate for health clinic staff.

7.13    Most of the beneficiaries and health staff at the public health center were likely informed about the program (assumption eight) through outreach or sensitization occurring in

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[37] Absenteeism rates above 25 percent among public health workers were found in Bangladesh, India, Indonesia, Peru, and Uganda (Chaudhury et al. 2006).

community-based nutrition meetings or in interactions with health staff. Ninety-four percent of beneficiaries in endline treatment villages responded that they were aware of an ongoing CCT program in their village. Unfortunately, Aide Memoires and World Bank team members indicate that the intervention also suffered from reputational risks when promised transfers were not made until well after women had met conditions for them. This could be the central explanation for the consistent findings that health outcomes were effective when they included children exposed to the early stages of the program (before the payment challenges occurred), but when they included younger children who would have been eligible for participation at later stages.

7.14    Although the timing of the evaluation was appropriate, as previously discussed, there was a mismatch between the outcomes chosen for evaluation and the outcomes that were affected by the program and/or that could be reliably measured. There was no observed effect on antenatal care by health staff, but there was an effect on antenatal care at institutions, particularly at health centers. Although generally there was no observed effect on delivery, there was some sparse evidence that delivery may have increased at health centers. By contrast, there were measurement challenges with diphtheria, pertussis, and tetanus (DPT) being received three times (no date on recalled immunizations), breastfeeding (baseline/endline comparability of feeding within one hour), and oral rehydration solutions (the PDO included the government-approved home remedy, even though it appears that health centers were more likely to push standard treatments). In light of this, outcome measures could be better constructed in a way that more tightly follows the causal chain. This disconnect is likely an artifact of the project objectives being decided before the project was fully designed.

7.15    The importance of the size of incentives—which need not all be financial—was hinted at in the nine assumptions of Gaarder et al. (2010) but not explicitly mentioned. It is assumed that incentives are sufficiently large to be able to overcome whatever barriers there may be to improved outcomes. This is particularly important to consider for institutional delivery, which was not improved by the project, according to the general specifications of this evaluation.

7.16    Further analysis of a survey question asks women the reasons they did not deliver at a health facility (table 7.2). It indicates a plurality of views that "convenience" was the most-cited reason women do not attend, though this reason dropped by a large margin in treatment areas, especially relative to comparison areas. The next biggest issues, which grew over time in the treatment areas, were concerns about not being able to reach the clinic in time, and local customs or traditions. Finances were a much lower priority, and lack of trust toward the health center staff was not a major concern. Together, these two factors of time and tradition, and perhaps some elements of convenience, point to a need to inspect the first two delays of the well-known "Three Delays" model of maternal mortality:[38] the decision to seek care (which is influenced by socioeconomic and cultural factors), and the ability to locate and arrive at a care facility (which is likely reflected by the mothers' responses that they could

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[38] See Thaddeus and Maine (1994).

not get there in time.[39] It may be that sufficiently large financial incentives can overcome tradition and help women plan and start going to the health center earlier, but these obstacles may be more efficiently dealt with as, for example, improvements in the mode of transportation to the health centers.

**Table 7.2. Reasons for Not Delivering Baby in the Health Facility**

| Reasons | Baseline | | | | Endline | | | | DD[a] |
|---|---|---|---|---|---|---|---|---|---|
| | Comp | % | Treat | % | Comp | % | Treat | % | |
| Convenient | 548 | 43.1 | 462 | 39.5 | 524 | 40.7 | 327 | 33.0 | −4.2 |
| Not in time | 193 | 15.2 | 184 | 15.8 | 271 | 21.1 | 331 | 33.3 | 11.7 |
| Tradition | 276 | 21.7 | 380 | 32.5 | 201 | 15.6 | 207 | 20.9 | −5.5 |
| No money | 147 | 11.6 | 92 | 7.8 | 140 | 10.9 | 68 | 6.8 | −0.3 |
| Distance to HC | 97 | 7.7 | 43 | 3.7 | 134 | 10.4 | 36 | 3.6 | −2.8 |
| Do not trust HS | 5 | 0.4 | 0 | 0.0 | 6 | 0.4 | 4 | 0.4 | 0.3 |
| Other | 6 | 0.5 | 8 | 0.6 | 10 | 0.8 | 20 | 2.0 | 1.0 |
| Total | 1,272 | 100 | 1,169 | 100 | 1,286 | 100 | 992 | 100 | |

*Source*: IEG.

*Notes*: comp = comparison area; DD = difference-in-differences; HC = health center; HS = health staff; treat = treatment area.

a. DD means simple difference-in-differences without covariates, and DD is equal to [(Endline Treat) − (Baseline Treat)] − [(Endline Comparison) − (Baseline Comparison)].

## Limitations

7.17     There are three primary limitations to this study. First, the parallel trend assumption incumbent in double-difference methods could not be directly tested because only two waves of cross sectional (at the household) data were available for analysis. This is proxied by tracking outcomes over time, exploiting the range of ages of children in the dataset. Most of the PDO outcomes appear to track with similar trends. Those few that do not may exhibit somewhat erratic behavior because of the small sample sizes for very young and older children in the sample. For instance, PDO3 (DPT3 vaccination for 12–23 months) experienced a sharp decline in the baseline comparison group at around 20 months of age (appendix E).[40]

7.18     Second, the possibility that intensity of activity by the Lao Women's Union—one of the most important players for implementing the CNP—might have been factored into the selection of the treatment areas could not be ruled out. Although the assignment decisions made by the ministry were somewhat needs-based, there may have been intentional or unintentional selection on factors correlated with a better-prepared Lao Women's Union, in which case the intervention would not have been as effective if implemented in the comparison areas. This seems unlikely. When the project was designed and recipient health centers were selected, the project did not plan on using the Lao Women's Union as an implementing partner; that decision was made about one year after selection. Furthermore,

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[39] It is important to note that this response of "Not in time" is different from another (nonexclusive) response option "distance."

[40] Note, however, that children 18–23 months are underrepresented in the sample for both baseline and endline, and the large variations might be an artifact of the relatively small sample size. Similarly, because the survey was not conducted on the first day of the month, children in the age zero category (within the first month of birth) are also underrepresented compared with age-in-months for the rest of the sample.

there is no statistically significant difference on the active Lao Women's Union at the village level before the start of the intervention.[41]

7.19    Finally, this evaluation does not control for the activities of other donors that may have been active in the comparison area, such as the Luxembourg Agency for Development Cooperation and the Lao Red Cross. The village- and health center–level questionnaire documents some of these activities, but the World Bank country team showed concerns about the reliability of the responses to those items. Still, the inability to control for other donor activities would tend to bias the estimates against finding significant effects. The evaluation question, however, assumes a real-world counterfactual scenario in which comparison groups could receive benefits from other program interventions.[42] In this respect, any effect identified in this report should be regarded as an effect of the World Bank program above the menu of programs available to the comparator areas.

# 8. Conclusion

8.1    This impact evaluation evaluated the efficacy of the first World Bank program administered by the government of Lao PDR and the first project incorporating a CCT scheme. The transfer program, in conjunction with community-based nutrition education, induced health-seeking behavior and awareness of MCH and nutrition outcomes for pregnant and lactating women and children younger than two years old. Through quasi-experimental methods, this evaluation finds that the CNP cannot claim to have affected most of the PDO indicators, but it did influence closely related indicators that are more appropriately aligned with the project's design. It also benefitted the poor and vulnerable.

8.2    More specifically, the CNP had a positive influence on child caring practices, such as DPT vaccination receiving three times, and breastfeeding after birth. The effects on increasing MCH utilization, however, were subtle and nuanced. Although there is some evidence pointing to improvements in the rate of antenatal and well-child visits, specifically to the health centers incentivized through the CCT for children 0–23 months, the evidence is not convincing that the project inspired higher rates of institutional delivery generally, despite the cash incentive subject to antenatal care, delivery, and routine growth checkups. In light of the global literature showing the potential for success through CCTs in improving skilled birth attendance (IEG 2013), there were some positive effects on the matched sample. But somewhat surprisingly, robust positive effects were not seen in the full sample.

8.3    The subgroup analysis shows that there are positive influences on the use of public health center services, particularly for those in the bottom 40 percent of the durables asset

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[41] Slightly more villages in the treatment area than in the comparison area had held nutrition and health meetings by the time the intervention started in January 2012, but the difference is not statistically significant.

[42] Villages in the service areas of the comparator health centers of Khammuane (in the Bolikhamxay province) and That (in the Champasak province) had a large number of caretakers who indicated they were aware of the cash payment program for pregnant women or mothers of young children to access MCH services. This effect might be led by other government or donor activities. But respondents in comparison villages that belong to That health center in particular even acknowledged the receipt of enrollment cash transfers. Furthermore, although more than 50 percent of respondents in the endline treatment area received cash from KN 50,000 to KN 70,000 (per protocol), there are large variations in the amount of cash received.

index and those who experienced price increase shock. Furthermore, there are heterogeneous program effects across different socioeconomic statuses and provinces. Even though the overall effect is modest, the project caused an improvement for the bottom 40 percent of the wealth distribution for nearly every outcome, including institutional delivery. This suggests that the project contributed to the second of the World Bank's twin goals—project effectiveness in MCH coverage and behavior for the bottom 40 percent has implications for improving shared prosperity, and could reduce the inequality of MCH interventions within Lao PDR. These findings could also be informative for better targeting in case scaling up of the program is considered as a policy option under the limited fiscal space. CNP also worked as a social protection measure against shocks from rising prices.

8.4    These achievements are encouraging, but the modest effects are almost certainly lower than they could be. Because essential design elements were not sufficiently completed at the time of appraisal in 2009, the design had to be fully realized during the implementation period. Also, this was the first World Bank project fully executed by the in-house capacity of a line ministry in Lao PDR, and not surprisingly there were growing pains that led to additional implementation delays. These delays allowed other donors to enter into the comparison areas with potentially similar interventions, possibly contributing to lower effect sizes than other CCT programs in the literature, which generally have counterfactuals that do not include competing programs. The null effect on anthropometric measures might be due to the short duration of the project as much as these implementation limitations. Also, the relatively low uptake rates imply that significant improvements can still be made to the program's design.

8.5    As the government of Lao PDR and the World Bank work toward scaling up the project, observations from this pilot are salient.

- Simple before/after comparisons can be misleading and induce misattribution of project effectiveness. Understanding true project effectiveness is especially useful for a pilot.
- Lack of planning and attention to the underlying assumptions specific to health CCTs can handicap program effectiveness.
- Effective and efficient management of project implementation under local capacity is a real challenge that will likely be particularly stressed as the project is scaled up.
- Program protocols for transfer amounts and timing need to be followed with much greater fidelity.
- Incentives (financial and nonfinancial) may need to be increased to encourage higher rates of pre- and postnatal care, and to overcome challenges presented by the first two of the standard Three Delays model of maternal and neonatal mortality (the decision to deliver at a facility, transportation to the facility, and the decision to refer up). Cultural norms and traditions of child birth can work against institutional delivery, and the cost of transportation can be highly variable.
- Designs should be fully developed in line with the project objectives, and the match between the actual incentives of the project and the project design should be closely inspected. As a specific example, an extramarginal effect is required to expand the uptake of health services such as antenatal care, instead of inducing only

47

inframarginal behavioral changes in those who would have sought professional health services by merely switching from non-incentivized public health sites to those that are incentivized.

8.6    As an area of future research, the examination of the quantity and quality of the supply-side factors is left for the forthcoming analysis from the World Bank country team.

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# Appendix A. List of Paired Health Centers

The following matched pair of health centers between the treatment and comparison areas is the result of the matching by the country team at project design for survey.

**Table A.1. Initial Matched Pairs of Health Centers**

| No | Intervention | | | Comparison | | |
|---|---|---|---|---|---|---|
| | Health Center | District | Province | Health Center | District | Province |
| 1 | Denvilai | Nong | Savannakhet | Amine | Samouay | Salavane |
| 2 | Xe Keu | Thapangthong | Savannakhet | Asok | Samouay | Salavane |
| 3 | Ban Bo | Bolikhanh | Boulikhamxay | Kengkia | Bachieng | Champassak |
| 4 | Kengchone | Xaibouathong | Khammouane | Sock | Boulapha | Khammouane |
| 5 | Kengmakkheua | Saysettha | Attapeu | Nakong | Nong | Savanakhet |
| 6 | Kuangsy | Bachieng | Champassak | Nasai | Phalanxai | Savanakhet |
| 7 | Ladhor | Xepon | Savannakhet | Kimea | Samouay | Salavane |
| 8 | Manh chi | Xepon | Savannakhet | Dongsavanh | Xepon | Savanakhet |
| 9 | Nakoun | Bolikhanh | Boulikhamxay | Nadou | Toumlan | Salavane |
| 10 | Phameuang | Khamkeud | Boulikhamxay | Nam One | Xayabouathong | Boulikhamxay |
| 11 | Namphao | Xaibouathong | Khammouane | Hai | Nyommalad | Khammouane |
| 12 | Naseuark | Phouvong | Attapeu | Phabang | Xepon | Savanakhet |
| 13 | Natane | Nakai | Khammouane | Nanoi thong | Xaibouathong | Khammouane |
| 14 | Nayom | Vilabouly | Savannakhet | Snod | Pathumphone | Champassak |
| 15 | Nongdeng | Soukhouma | Champassak | Ban That | Soukhouma | Champassak |
| 16 | Panam | Mahaxai | Khammouane | Sobpeng | Boulapha | Khammouane |
| 17 | Phortang | Taoi | Salavane | Phid | Nyommalad | Khammouane |
| 18 | Lak 24 | Pathumphone | Champassak | Khammuane | Khamkeud | Boulikhamxay |
| 19 | Tahouark | Taoi | Salavane | Kokbok | Taoi | Salavane |

*Source*: IEG.
*Note:* The catchment area of health centers (Nongboua health center and Sob One health center) which received relocation benefit because of the Nam Theun 2 Hydropower Project are excluded from the list.

# Appendix B. Data Collection Sampling Framework

**Figure B.1. Sampling Procedure**
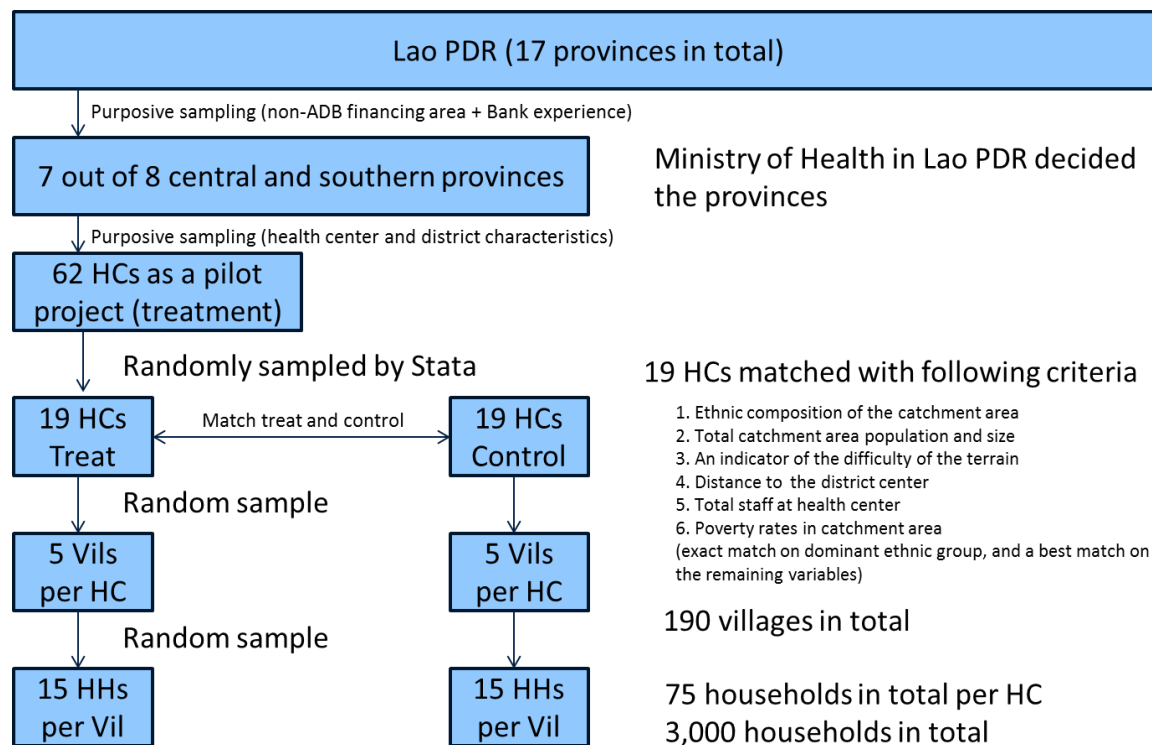


*Source:* IEG.
*Notes:* The baseline survey covers 2,979 households, 207 villages, and 41 health centers after the baseline survey, including those health centers that were eventually flooded or received relocation benefit as a result of the Nam Theun 2 Hydropower Project. Health centers include the health center that was flooded or received relocation benefit of hydropower project. ADB = Asian Development Bank; HC = health center; Vil = village.

The same local survey company collected both baseline and endline datasets. The enumerators were trained through a field pilot test before the full survey and were blind to the treatment allocation information. The enumerators visited each household with eligible children and obtained verbal consent from the parents or guardians at the beginning of the interview. Data cleaning was executed by the survey firm, the Independent Evaluation Group, and the World Bank country office staff. The village household roster used to select the 15 households was compiled with information provided by the health center and the interview with the village head and the Lao Women's Union. Despite these efforts to construct a complete sampling frame, children between 18 and 23 months of age are somewhat undersampled in both waves.
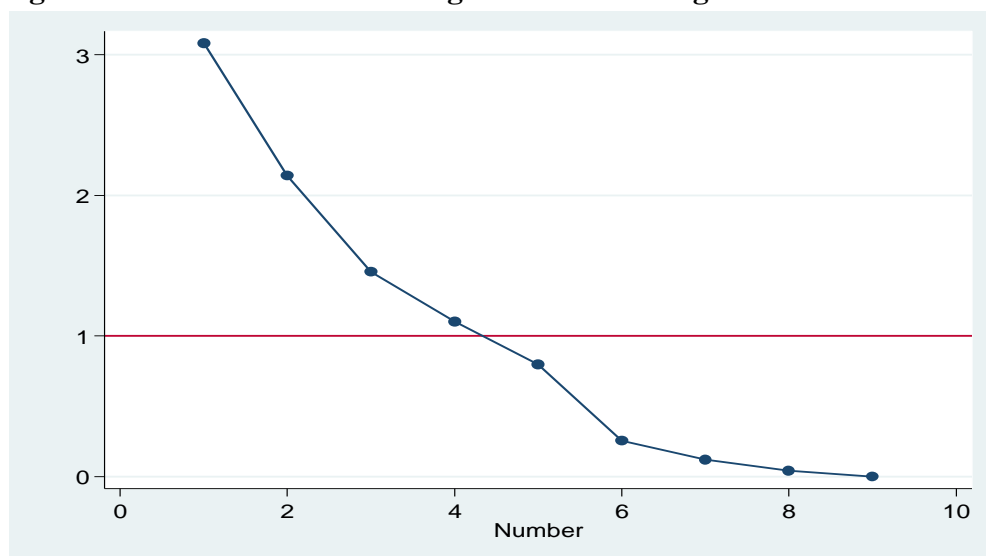
# Appendix C. Principal Component Analysis

Durable asset variables are used to develop two long-term asset indexes. More specifically, nine different durable asset variables—availability of toilet, main source of electricity (electricity or fuel), main source of floor (high class, wood, low class) and main source of wall (wood, bamboo)—are applied for principal component analysis (PCA). The first two principal components, which are more than two eigenvalues, are selected as long-term asset indexes. The first principal component largely consists of (i) access to electricity (rather than fuel), (ii) main source of floor is wood, and (iii) main source of wall is wood. The first principal component also has large, negative eigenvectors on electricity source from fuel, and low class source of floor and wall. The second principal component represents high-class material of floor and wall.

**Table C.1. Principal Components (Eigenvectors) for Long-Term Asset Index**

| Variable | Component 1 | Component 2 | Component 3 | Unexplained |
|---|---|---|---|---|
| Toilet | 0.256 | 0.182 | −0.047 | 0.723 |
| Main source of energy: electricity | 0.331 | 0.285 | −0.528 | 0.082 |
| Main source of energy: fuel | −0.326 | −0.274 | 0.536 | 0.092 |
| Main source of floor: high class | 0.063 | 0.562 | 0.345 | 0.138 |
| Main source of floor: wood | 0.391 | −0.369 | 0.025 | 0.237 |
| Main source of floor: lower class | −0.437 | 0.144 | −0.176 | 0.323 |
| Main source of wall: high class | 0.049 | 0.551 | 0.347 | 0.169 |
| Main source of wall: wood | 0.426 | −0.184 | 0.211 | 0.303 |
| Main source of wall: bamboo | −0.432 | 0.017 | -0.340 | 0.256 |

*Source*: IEG.

**Figure C.1. Scree Plot of PCA Eigenvalues for Long-Term Asset Index**



*Source:* IEG calculation.
*Note:* PCA = principal component analysis.

## Appendix C

### Short-Term (Consumables) Asset Index

Consumption asset variables are used to develop one short-term asset index. There are 13 variables, which are: motorcycle, bicycle, refrigerator, electric rice cooker, electric fan, two-wheel tractor, boat, fishing net, radio, telephone, mobile phone, and satellite dish. The first principal component of these nine items loads predominantly on short-term assets and luxury items, such as owning a TV, satellite dish, electric fan, refrigerator, radio, and mobile phone.

**Table C.2. Principal Components (Eigenvectors) for Short-term Asset Index**

| Variable | Component 1 | Component 2 | Unexplained |
|---|---|---|---|
| Motorcycle | 0.277 | 0.212 | 0.608 |
| Bicycle | 0.157 | 0.291 | 0.780 |
| Refrigerator | 0.341 | −0.232 | 0.423 |
| Electric rice cooker | 0.252 | −0.385 | 0.528 |
| Electric fan | 0.369 | −0.120 | 0.392 |
| Two-wheel tractor | 0.154 | 0.480 | 0.588 |
| Boat | 0.152 | −0.373 | 0.713 |
| Fishing net | 0.103 | 0.411 | 0.728 |
| TV | 0.409 | 0.025 | 0.274 |
| Radio | 0.313 | 0.129 | 0.555 |
| Telephone | 0.048 | −0.305 | 0.865 |
| Mobile phone | 0.305 | 0.067 | 0.592 |
| Satellite dish | 0.407 | 0.000 | 0.282 |

*Source*: IEG.

**Figure C.2. Scree Plot of PCA Eigenvalues for Short-Term (Consumables) Asset Index**
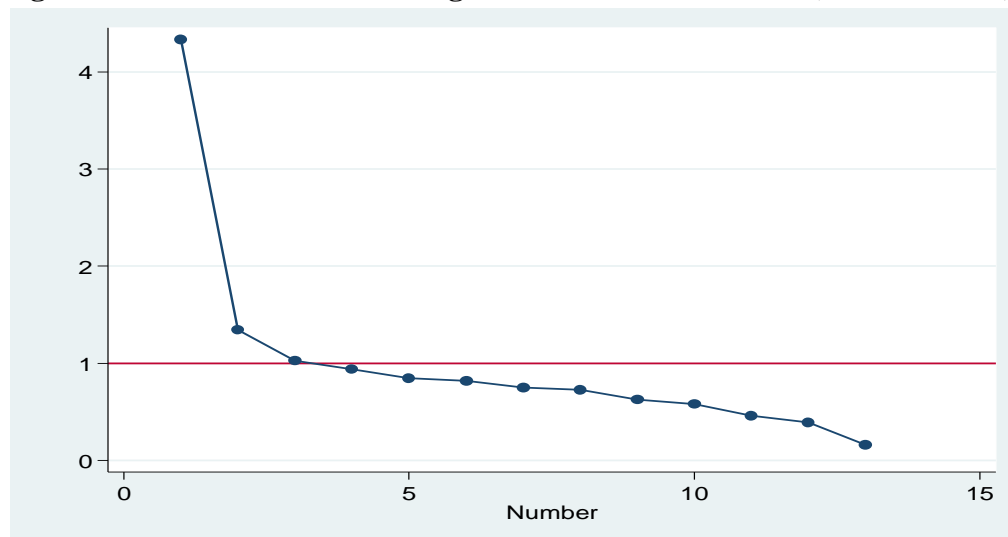


*Source:* IEG calculation.

**Appendix C**

### Household Welfare Shock Index

Similarly, PCA is applied for eight items representing different external shocks (drought, fire, floods, crop disease, sickness or death of household head, sickness or death of other household member, resettlement, and robbery) to develop one shock index. Since the Community Nutrition Project is prepared to respond to the global food crisis, food price increase and decreases are controlled for explicitly as independent covariates in the regression analysis and are not included in the PCA. The first principal component of the shock index loads most prominently on items of drought, floods, crop disease, and sickness or death of household head.

**Table C.3. Scree Plot of PCA Eigenvalues for Shock Index**

| Variable | Component 1 | Unexplained |
|---|---|---|
| Drought | 0.458 | 0.666 |
| Fire | 0.267 | 0.887 |
| Floods | 0.422 | 0.718 |
| Crop disease | 0.475 | 0.642 |
| Sickness or death of household head | 0.412 | 0.731 |
| Sickness or death of other household members | 0.352 | 0.803 |
| Resettlement | 0.093 | 0.986 |
| Robbery | 0.118 | 0.978 |

*Source*: IEG.

**Figure C.3. Scree Plot of PCA Eigenvalues for Welfare Shocks**
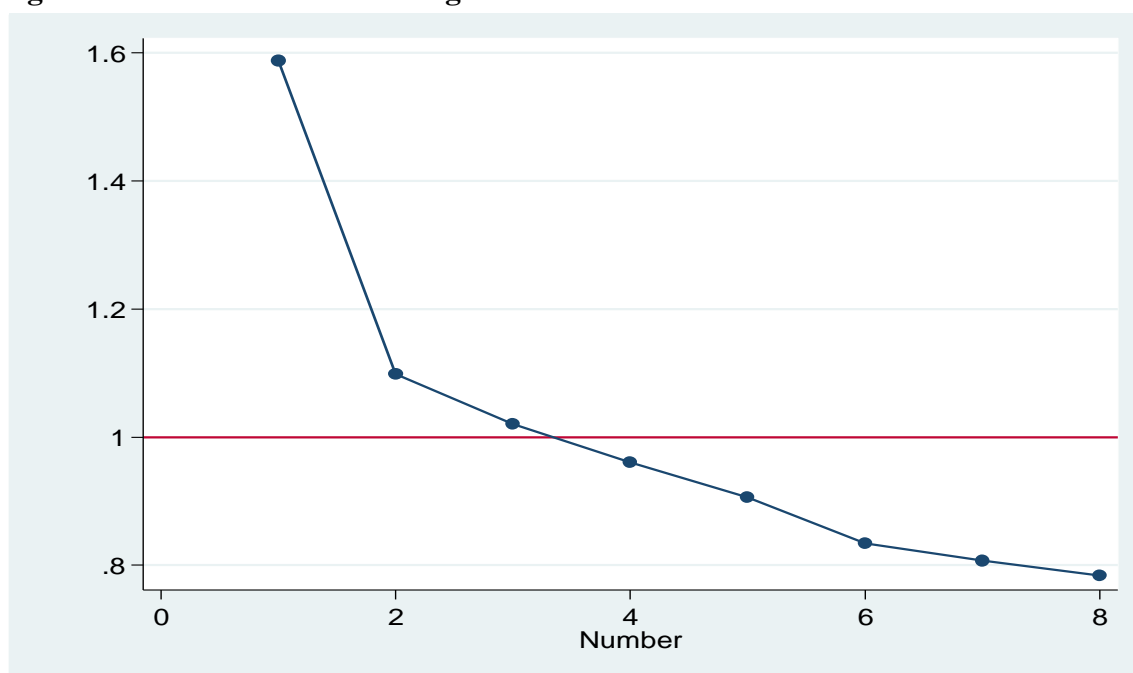


*Source:* IEG calculation.

# Appendix D. Definition of Covariates

There are 41 covariates. Each covariate is defined below.

| No. | Variable | Definition |
|---|---|---|
| 1 | Constant | Constant term |
| 2 | Time | Binary variable (1 for endline, 0 for baseline) |
| 3 | Intervention | Binary variable (1 for treatment area, 0 for comparison area) |
| 4 | Interaction | Time x Intervention |
| 5 | Mother age 1 | Mother's age spline 1 (mother age <= 20) |
| 6 | Mother age 2 | Mother's age spline 2 (20 < mother age <= 40) |
| 7 | Mother age 3 | Mother's age spline 3 (40 < mother age) |
| 8 | Mother weight | Mother's weight (kg) |
| 9 | Mother weight sq | Mother's weight square |
| 10–13 | Mother educ | Mother's educational background (four dummy variables)[a] |
| 14 | Grandfather | Grandfather living within the household |
| 15 | Grandmother | Grandmother living within the household |
| 16 | Dead child | Mother had a child who passed away |
| 17 | Child age in months | Child age in months |
| 18 | Child girl | Gender (binary variable: 1 for girl, 0 for boy) |
| 19 | Short asset index | Short-term asset index: created through PCA[b] |
| 20 | Long asset index 1 | Long-term asset index 1: created through PCA[c] |
| 21 | Long asset index 2 | Long-term asset index 2: created through PCA[d] |
| 22–24 | Ethnicity | Ethnicity gender: three dummy variables consistent with LSIS[e] |
| 25 | Time to HC | Time to health center from village: logged form |
| 26 | First child | First child (binary variable: 1 for first child, 0 for others) |
| 27 | HH size under 5 | Number of children under age 5 within the same household |
| 28 | Total birth | Number of total births from the same natural mother |
| 29 | Shock index | Shock index created through PCA |
| 30 | Price decrease shock | Price decrease shock |
| 31 | Price increase shock | Price increase shock |
| 32 | Urban/rural | Village is in urban or rural area |
| 33 | Ethnic congruence 1 | Ethnic congruence between village and household head |
| 34 | Ethnic congruence 2 | Ethnic congruence between health center staff and household head |
| 35 | HC male propor | Proportion of males among nearest health center staff |
| 36–41 | Province | Five dummy variables for provinces[f] |

*Source:* IEG.

*Note*: HC = health center; HH = household; kg = kilograms; LSIS = Lao Social Indicator Survey 2011–12; PCA = principal component analysis.

a. No education, primary, lower secondary, upper secondary, postsecondary and higher.

b. Consumption type assets: motorcycle, bicycle, refrigerator, electric rice cooker, electric fan, two wheel tractor, boat, fishing net, radio, telephone, mobile phone, satellite dish

c. Durable assets: toilet, main source of electricity (electricity or fuel), main source of floor (high class, wood, low class), main source of wall (wood, bamboo)

d. External shocks: drought, fire, floods, crop disease, ill or death of household head, ill or death of other household member, resettlement, robbery

e. Lao-Tai, Mon-Khmer, Hmong-Mien, Others (Tibetan included in others due to negligible sample size)

f. Bolikhamxay, Khammaun, Savanhnakhet, Saravan, Champasak, Attapue

**Appendix D**

If the father's educational background is included, it does not change the main results much because this variable also includes many more missing values than does the mother's educational background. Therefore, this paper does not include the father's educational background.

# Appendix E. Parallel Trending at Pretreatment Period

**Figure E.1. Utilization of Health Services by Age (0–23 months)**

Panel a. PDO1: Antenatal care assisted by health staff
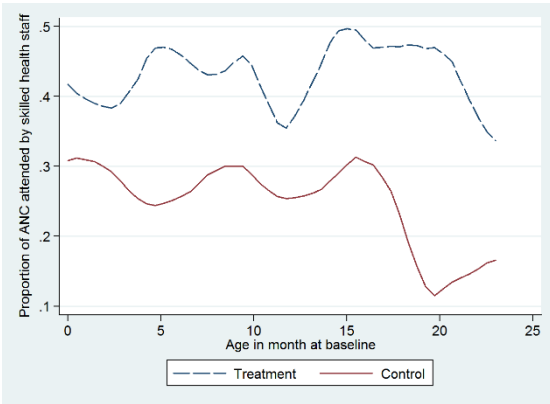


Source: IEG.
Note: ANC = antenatal care.

Panel b. PDO2: Delivery at health facility
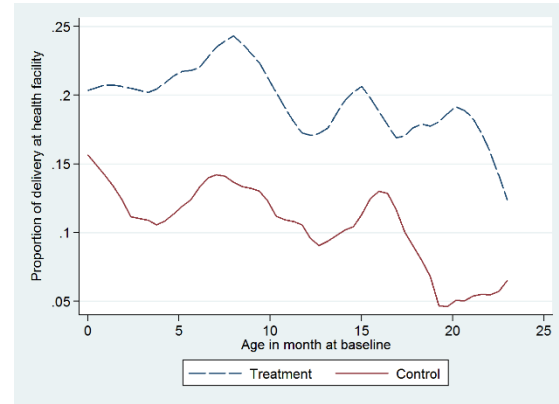


Source: IEG.

Panel c. PDO3: Receive DPT at least three times
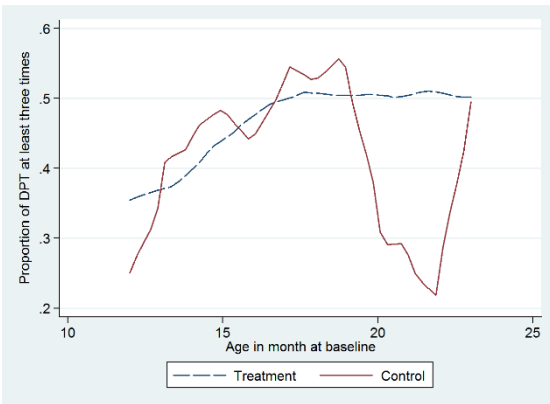


Source: IEG.
Note: DPT = diphtheria, pertussis, and tetanus.

Panel d. PDO4: At least one routine checkup
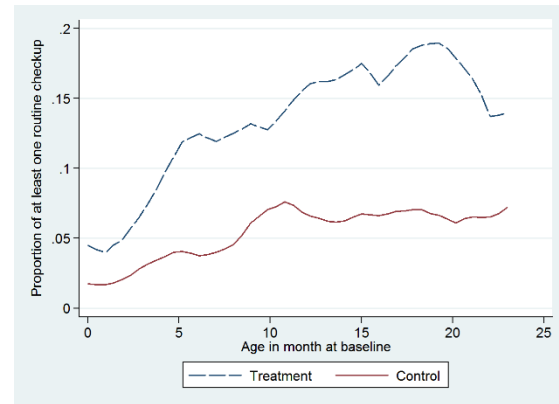


Source: IEG.

# Appendix E

Panel e. PDO5: Breastfeeding within one hour of birth
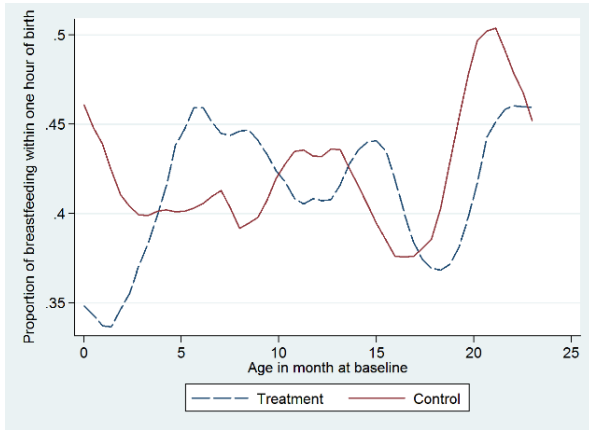
Panel f. PDO6: Oral rehydration solutions with diarrhea
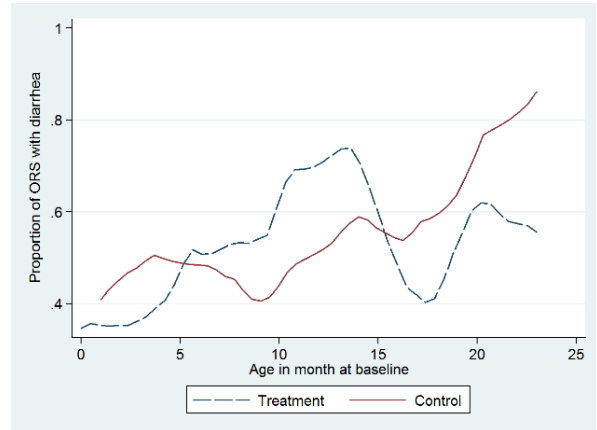


*Source:* IEG.



*Source:* IEG.
*Note:* ORS = oral rehydration solutions.

Panel g. Height-for-age z-score
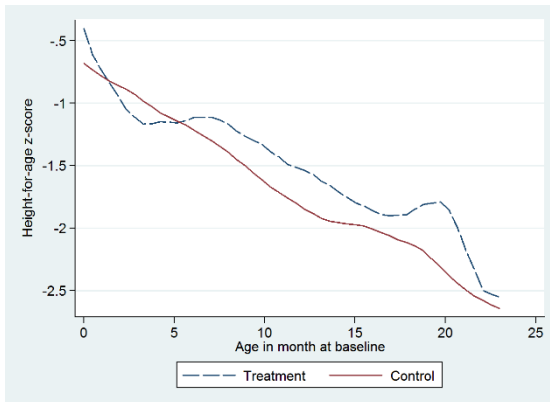
Panel h. Stunting



*Source:* IEG.



*Source:* IEG.

# Appendix E

Panel i. Weight-for-age z-score
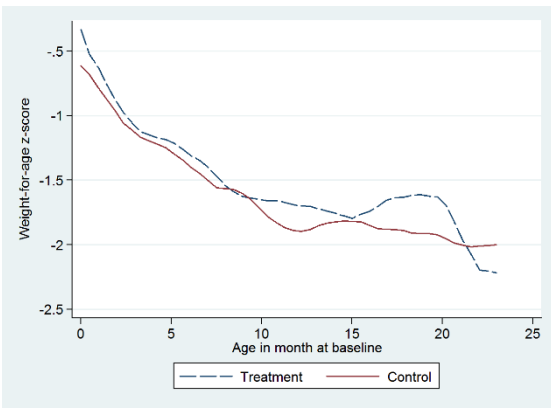


*Source:* IEG.

Panel j. Underweight
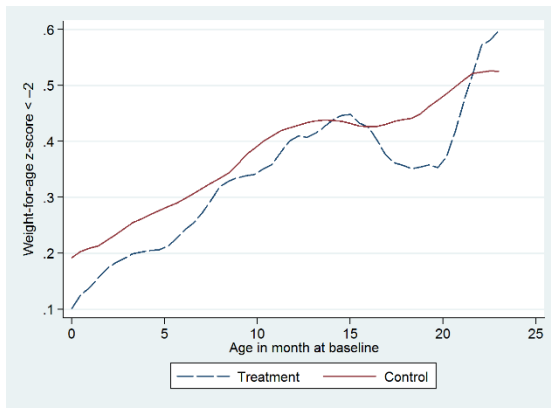


*Source:* IEG.

Panel k. Weight-for-height z-score
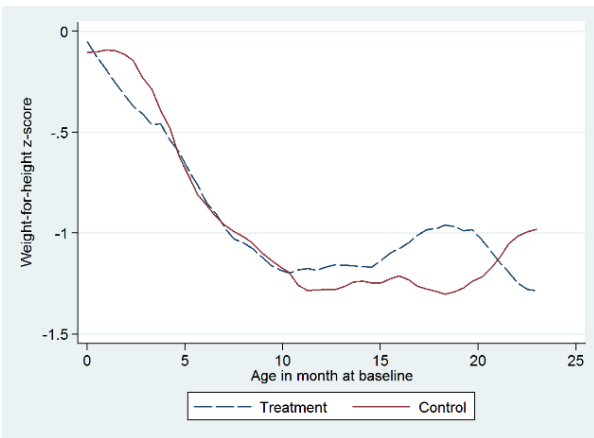


*Source:* IEG.

Panel l. Wasting
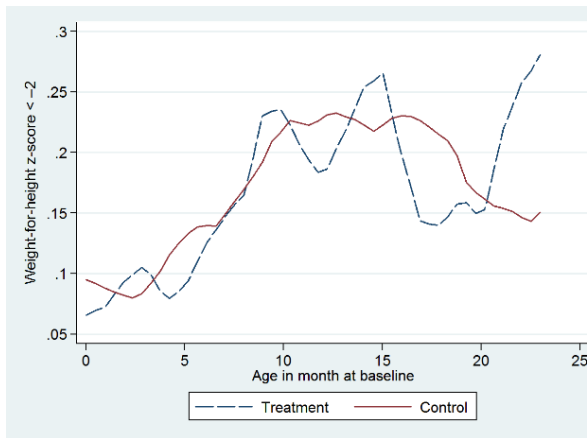


*Source:* IEG.

# Appendix F. Propensity Score Matching

**Figure F.1. Map of Treatment and Comparison Villages and Health Centers**
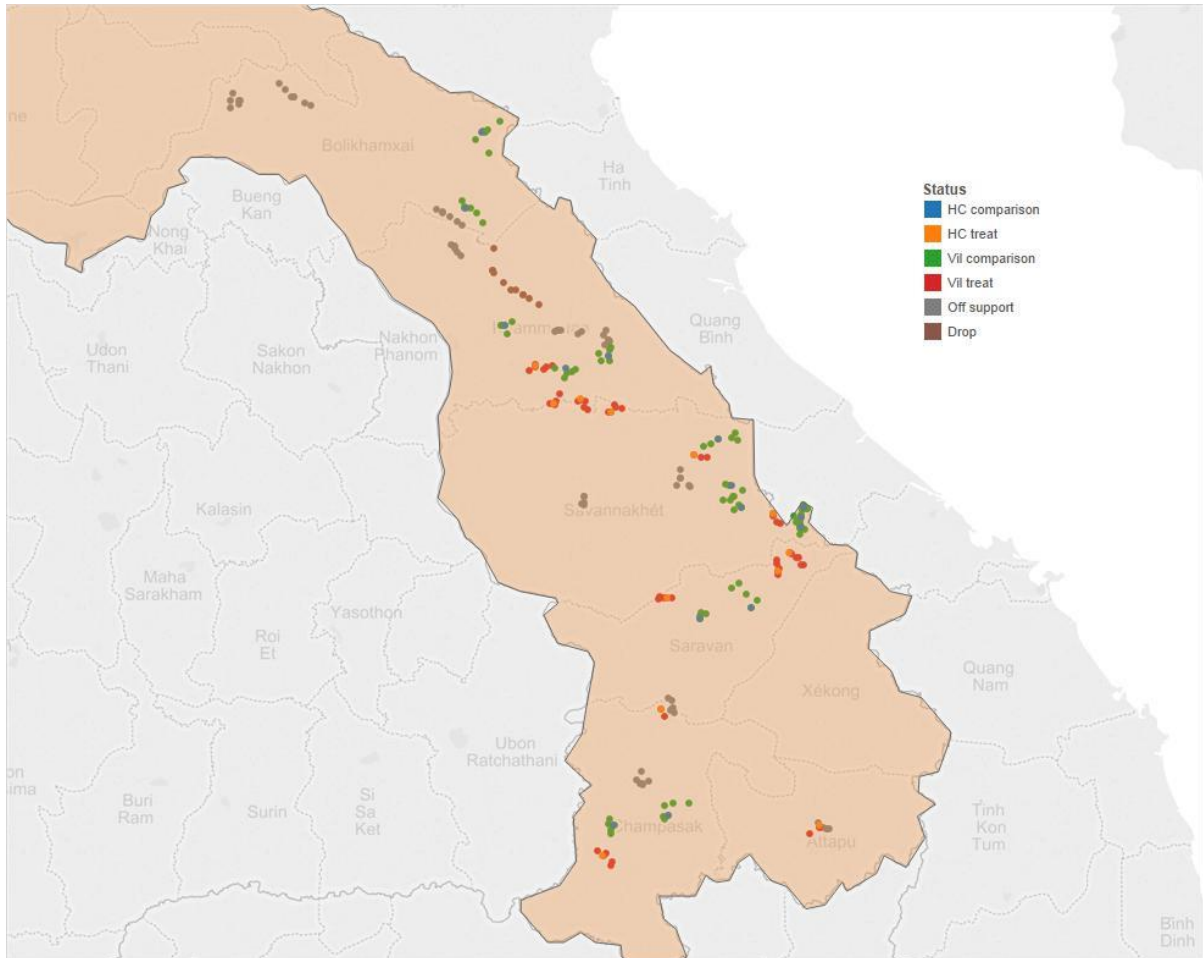


*Source:* IEG.
*Note:* comparison = comparison area; HC = health center; treat = treatment area; vil = village.

**Appendix F**

**Figure F.2. Common Support**
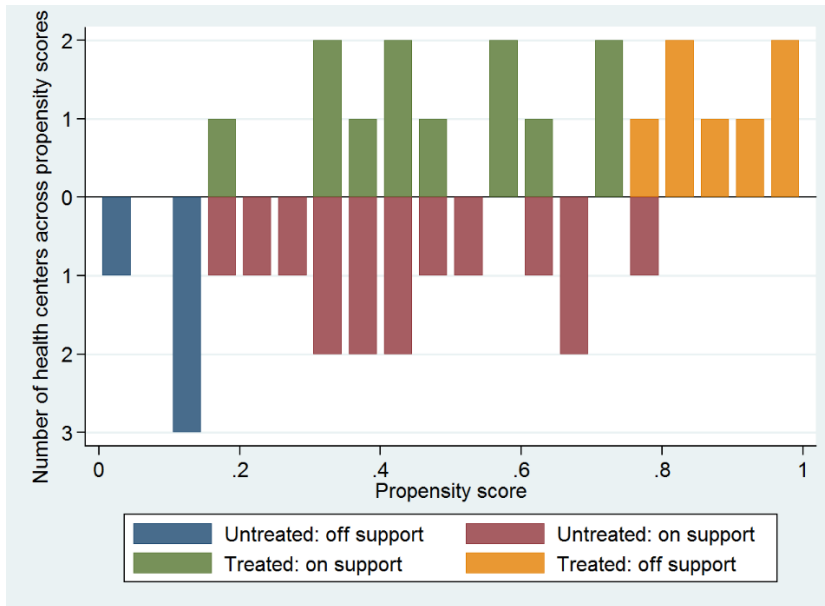
**Figure F.3. Bias Reduction**
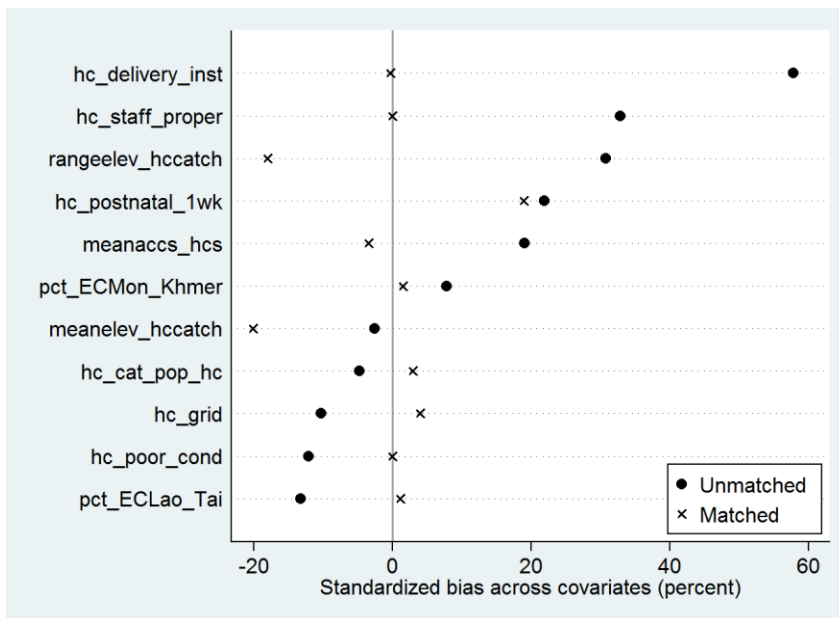
*Note:* hc_cat_pop_hc = population in health center catchment area; hc_delivery_inst = proportion of institutional deliveries in health center catchment area; hc_grid = health center having access to electrical grid; hc_poor_cond = health center building in poor condition; hc_postnatal_1wk = proportion of receiving postnatal visits with 1 week in health center catchment area; hc_staff_proper = number of health center staff excluding volunteers; pct_ECLao_Tai = percentage of Lao/Tai population in health center catchment area; pct_ECMon_Khmer = percentage of Mon/Khmer population in health center catchment area; meanaccs_hcs = mean travel time to nearest health center; meanelev_hccatch = mean elevation in health center catchment area; rangeelev_hccatch = range of elevation in health center catchment area.

## Appendix F

### Table F.1. Bias Reduction

| Variable | | **Mean** | | % bias | Bias reduction | t-stat | p > |t| |
| --- | --- | --- | --- | --- | --- | --- | --- |
| | | Treat | Comp | | | | |
| Ethnicity (Lao Thai) | Unmatched | 38.7 | 43.6 | −13.2 | | −0.41 | 0.69 |
| | Matched | 32.6 | 32.1 | 1.2 | 90.8 | 0.03 | 0.98 |
| Ethnicity (Mon Khmer) | Unmatched | 55.9 | 52.8 | 7.9 | | 0.24 | 0.81 |
| | Matched | 67.4 | 66.8 | 1.5 | 80.8 | 0.04 | 0.97 |
| Health center catchment population | Unmatched | 3,625 | 3,710 | −4.7 | | −0.14 | 0.89 |
| | Matched | 3,399 | 3,345 | 3.0 | 36.6 | 0.07 | 0.94 |
| Range of elevation in health center catchment area | Unmatched | 799.1 | 673.7 | 30.8 | | 0.95 | 0.35 |
| | Matched | 668.7 | 741.7 | −17.9 | 41.8 | −0.43 | 0.68 |
| Mean elevation of health center | Unmatched | 400.3 | 405.4 | −2.5 | | −0.08 | 0.94 |
| | Matched | 369.7 | 410.7 | −20.1 | −699.6 | −0.50 | 0.62 |
| Mean access time to health center | Unmatched | 111.7 | 98.7 | 19.1 | | 0.59 | 0.56 |
| | Matched | 100.8 | 103.2 | −3.4 | 82 | −0.08 | 0.94 |
| Health center connected to grid | Unmatched | 0.47 | 0.53 | −10.3 | | −0.32 | 0.75 |
| | Matched | 0.33 | 0.31 | 4.1 | 60.4 | 0.10 | 0.92 |
| Number of proper health staff at health center | Unmatched | 2.3 | 2.1 | 32.9 | | 1.01 | 0.32 |
| | Matched | 2.0 | 2.0 | 0.0 | 100 | 0.00 | 1.00 |
| Proportion of delivery at health facility (health center level) | Unmatched | 0.19 | 0.11 | 57.9 | | 1.78 | 0.08 |
| | Matched | 0.10 | 0.10 | −0.3 | 99.5 | −0.01 | 0.99 |
| Proportion of postnatal visit after one week of birth | Unmatched | 0.04 | 0.03 | 22.0 | | 0.68 | 0.50 |
| | Matched | 0.05 | 0.04 | 19.0 | 13.6 | 0.40 | 0.69 |
| Proportion of health center in poor condition | Unmatched | 0.21 | 0.26 | −12.1 | | −0.37 | 0.71 |
| | Matched | 0.25 | 0.25 | 0.0 | 100 | 0.00 | 1.00 |
| Mean bias | Unmatched | - | - | 19.4 | - | - | - |
| | Matched | - | - | 6.4 | - | - | - |
| Median bias | Unmatched | - | - | 13.2 | - | - | - |
| | Matched | - | - | 3.0 | - | - | - |

Source: IEG.

*Note:* comp = comparison area; treat = treatment area. The t-statistics and p-values are derived from balancing t-test for unmatched and matched samples.

# Appendix G. Baseline Balance Check

## Table G.1. Baseline Balance on Pretreatment Variables

| | Full sample | | | | | Matched sample | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Treatment (n=1,428) | Comparison (n=1,417) | Difference | p-value | | Treatment (n=890) | Comparison (n=1,107) | Difference | p-value | |
| Child | | | | | | | | | | |
| Age in months | 9.80 | 10.00 | -0.20 | 0.53 | | 10.03 | 10.29 | -0.26 | 0.53 | |
| Gender (girl = 1) | 0.49 | 0.50 | -0.01 | 0.70 | | 0.49 | 0.51 | -0.02 | 0.57 | |
| First child | 0.26 | 0.27 | -0.01 | 0.79 | | 0.22 | 0.28 | -0.06 | 0.04 | ** |
| Mother's education | | | | | | | | | | |
| No education | 0.48 | 0.54 | -0.06 | 0.02 | ** | 0.55 | 0.60 | -0.05 | 0.11 | |
| Primary | 0.30 | 0.34 | -0.04 | 0.06 | * | 0.29 | 0.31 | -0.01 | 0.69 | |
| Lower secondary | 0.10 | 0.04 | 0.06 | 0.00 | *** | 0.05 | 0.03 | 0.02 | 0.09 | * |
| Upper secondary | 0.07 | 0.04 | 0.03 | 0.01 | *** | 0.05 | 0.04 | 0.01 | 0.64 | |
| Postsecondary or higher | 0.04 | 0.03 | 0.01 | 0.28 | | 0.05 | 0.02 | 0.03 | 0.01 | *** |
| Mother's age | 26.5 | 26.0 | 0.50 | 0.08 | * | 26.8 | 26.2 | 0.62 | 0.09 | * |
| Household | | | | | | | | | | |
| Long asset index 1 | 0.02 | 0.01 | 0.00 | 0.98 | | -0.16 | -0.19 | 0.04 | 0.72 | |
| Long asset index 2 | 0.26 | -0.02 | 0.28 | 0.00 | *** | -0.25 | -0.17 | -0.08 | 0.36 | |
| Short asset index | -0.14 | 0.12 | -0.26 | 0.02 | ** | 0.46 | 0.52 | -0.06 | 0.58 | |
| Shock index | -0.20 | 0.05 | -0.25 | 0.00 | *** | -0.24 | -0.12 | -0.12 | 0.17 | |
| Price increase | 0.01 | 0.01 | 0.00 | 0.71 | | 0.02 | 0.01 | 0.00 | 0.84 | |
| Price decrease | 0.01 | 0.04 | -0.03 | 0.00 | *** | 0.02 | 0.03 | -0.01 | 0.13 | |
| Grandfather | 0.30 | 0.27 | 0.02 | 0.25 | | 0.23 | 0.28 | -0.05 | 0.07 | * |
| Grandmother | 0.37 | 0.36 | 0.01 | 0.70 | | 0.31 | 0.36 | -0.06 | 0.04 | ** |
| Household size under 5 years old | 1.62 | 1.57 | 0.05 | 0.11 | | 1.58 | 1.55 | 0.03 | 0.45 | |
| Total births | 3.42 | 3.24 | 0.19 | 0.09 | * | 3.52 | 3.27 | 0.26 | 0.06 | * |
| Ethnicity | | | | | | | | | | |
| Lao Tai | 0.51 | 0.43 | 0.08 | 0.00 | *** | 0.58 | 0.36 | 0.23 | 0.00 | *** |
| Mon Khmer | 0.46 | 0.50 | -0.04 | 0.10 | * | 0.41 | 0.61 | -0.20 | 0.00 | *** |
| Hmong Mien | 0.03 | 0.02 | 0.00 | 0.78 | | 0.00 | 0.01 | -0.01 | 0.00 | *** |
| Other | 0.01 | 0.04 | -0.04 | 0.00 | *** | 0.00 | 0.01 | -0.01 | 0.00 | *** |
| Village | | | | | | | | | | |
| Urban/rural (urban = 1) | 0.18 | 0.17 | 0.01 | 0.49 | | 0.12 | 0.12 | 0.01 | 0.75 | |
| Time to health center (log) | -0.56 | -0.49 | -0.07 | 0.21 | | -0.49 | -0.30 | -0.19 | 0.01 | ** |
| PDO | | | | | | | | | | |
| ANC attended by health staff | 0.43 | 0.26 | 0.17 | 0.00 | *** | 0.38 | 0.25 | 0.14 | 0.00 | ** |
| Institutional delivery | 0.20 | 0.11 | 0.09 | 0.00 | *** | 0.11 | 0.10 | 0.01 | 0.44 | |
| DPT at least three times | 0.45 | 0.41 | 0.03 | 0.36 | | 0.50 | 0.38 | 0.12 | 0.01 | ** |
| Any growth checkup | 0.12 | 0.05 | 0.07 | 0.00 | *** | 0.15 | 0.05 | 0.11 | 0.00 | *** |
| Breastfeeding within one hour | 0.41 | 0.42 | -0.01 | 0.66 | | 0.38 | 0.46 | -0.08 | 0.01 | *** |
| Received ORS during diarrhea | 0.64 | 0.64 | 0.00 | 1.00 | | 0.65 | 0.65 | 0.00 | 1.00 | |

Source: IEG.

*Notes:* All datasets restrict the sample to the newborns less than 24 months who are children of mothers aged between 15 and 49 years old. The sample sizes are for child age in months; depending on the missing values of the other variables, sample sizes could change. ANC = antenatal care; DPT = diphtheria, pertussis, and tetanus; ORS = oral rehydration solutions.
Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

# Appendix H. Heckman Selection Model

**Table H.1. Heckman and Non-Heckman Selection Model Results on ORS**

| | Full Sample | | | | | | Matched Sample | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0-11 mo (N=465) | | | 0-23 mo (N=933) | | | 0-11 mo (N=354) | | | 0-23 mo (N=731) | | |
| | First Stage | Heckman | LPM | First Stage | Heckman | LPM | First Stage | Heckman | LPM | First Stage | Heckman | LPM |
| Coefficient | 4.123 | 0.092 | 0.101 | 4.014 | 0.113 | 0.115 | 3.916 | 0.077 | 0.079 | 3.975 | 0.118 | 0.121 |
| Standard Error | (0.264) *** | (0.101) | (0.104) | (0.128) *** | (0.077) | (0.079) | (0.315) *** | (0.118) | (0.126) | (0.177) *** | (0.106) | (0.111) |
| Wald Test | - | 0.186 | - | - | 0.130 | - | - | 0.848 | - | - | 0.381 | - |
| | First Stage | Heckman logit ME | Logit ME | First Stage | Heckman logit ME | Logit ME | First Stage | Heckman logit ME | Logit ME | First Stage | Heckman logit ME | Logit ME |
| Coefficient | 4.128 | 0.105 | 0.118 | 4.015 | 0.127 | 0.131 | 3.921 | 0.122 | 0.126 | 3.977 | 0.138 | 0.140 |
| Standard Error | (0.265) *** | (0.115) | (0.114) | (0.129) *** | (0.085) | (0.084) | (0.316) *** | (0.159) | (0.159) | (0.177) *** | (0.119) | (0.119) |
| | First Stage | Heckprob | Probit | First Stage | Heckprob | Probit | First Stage | Heckprob | Probit | First Stage | Heckprob | Probit |
| Coefficient | 4.123 | 0.258 | 0.300 | 4.013 | 0.318 | 0.335 | 3.916 | 0.273 | 0.283 | 3.975 | 0.370 | 0.380 |
| Standard Error | (0.264) *** | (0.272) | (0.276) | (0.129) *** | (0.213) | (0.215) | (0.315) *** | (0.362) | (0.361) | (0.177) *** | (0.301) | (0.307) |
| Wald Test | - | 0.200 | - | - | 0.161 | - | - | 0.860 | - | - | 0.479 | - |

Source: IEG.

Notes: The column "First stage" reports the coefficient of the village level incidence of diarrhea on the individual incidence of diarrhea. The Heckman/LPM/Logit marginal effect (ME)/Probit column reports the results of the interaction term between time and intervention. The Wald test row reports the probability greater than chi square value under the null hypothesis that correlation is equal to zero. Heckman logit with marginal effect is estimated separately in first stage with probit, and second stage with logit with marginal effect. Heckman = Heckman's sample selection model; Heckprob = Heckman model using probit in the second step; LPM = linear probability model; ME = marginal effect; ORS = oral rehydration solutions.

Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

**Table H.2. Heckman and Non-Heckman Selection Model Results on ORS (Including Government-Recommended Fluid)**

| | Full Sample | | | | | | Matched Sample | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0-11 mo (N=465) | | | 0-23 mo (N=933) | | | 0-11 mo (N=354) | | | 0-23 mo (N=731) | | |
| | First Stage | Heckman | LPM | First Stage | Heckman | LPM | First Stage | Heckman | LPM | First Stage | Heckman | LPM |
| Coefficient | 4.120 | -0.020 | -0.011 | 4.013 | 0.040 | 0.042 | 3.915 | -0.027 | -0.025 | 3.975 | 0.008 | 0.010 |
| Standard Error | (0.264) *** | (0.114) | (0.117) | (0.128) *** | (0.083) | (0.085) | (0.316) *** | (0.138) | (0.146) | (0.177) *** | (0.110) | (0.114) |
| Wald Test | - | 0.176 | - | - | 0.165 | - | - | 0.874 | - | - | 0.460 | - |
| | First Stage | Heckman logit ME | Logit ME | First Stage | Heckman logit ME | Logit ME | First Stage | Heckman logit ME | Logit ME | First Stage | Heckman logit ME | Logit ME |
| Coefficient | 4.128 | -0.019 | -0.006 | 4.015 | 0.044 | 0.048 | 3.921 | -0.023 | -0.020 | 3.977 | 0.019 | 0.020 |
| Standard Error | (0.265) *** | (0.119) | (0.118) | (0.129) *** | (0.089) | (0.088) | (0.316) *** | (0.158) | (0.156) | (0.177) *** | (0.113) | (0.113) |
| | First Stage | Heckprob | Probit | First Stage | Heckprob | Probit | First Stage | Heckprob | Probit | First Stage | Heckprob | Probit |
| Coefficient | 4.119 | -0.047 | -0.011 | 4.012 | 0.118 | 0.131 | 3.915 | -0.053 | -0.045 | 3.975 | 0.057 | 0.063 |
| Standard Error | (0.265) *** | (0.306) | (0.308) | (0.129) *** | (0.243) | (0.245) | (0.316) *** | (0.412) | (0.409) | (0.177) *** | (0.329) | (0.332) |
| Wald Test | - | 0.188 | - | - | 0.189 | - | - | 0.870 | - | - | 0.631 | - |

Source: IEG.
Notes: The column "First stage" reports the coefficient of the village level incidence of diarrhea on the individual incidence of diarrhea. The Heckman/LPM/Logit marginal effect (ME)/Probit column reports the results of the interaction term between time and intervention. The Wald test row reports the probability greater than chi square value under the null hypothesis that correlation is equal to zero. Heckman logit with marginal effect is estimated separately in first stage with probit, and second stage with logit with marginal effect. Heckman = Heckman's sample selection model; Heckprob = Heckman model using probit in the second step; LPM = linear probability model; ME = marginal effect; ORS = oral rehydration solutions.
Significance level: * = 10 percent, ** = 5 percent, *** = 1 percent.

# Appendix I. Heterogeneity Analysis (Subgroup Analysis)

## Table I.1. PDO Indicator 1 (Antenatal Care Attended by Skilled Health Professional During Pregnancy, 0–11 months)

| | Subgroup | | Full sample | | | Matched sample | |
|---|---|---|---|---|---|---|---|
| | | | | With covariates | | | With covariates |
| | | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| PDO | Attended by health staff vs. Attended by other persons + None (all births, 0–11 months) | 3,274 | 0.015 (0.051) | 0.008 (0.057) | 2,320 | 0.087 (0.053) | 0.079 (0.065) |
| Dead child | No dead child | 2,378 | -0.003 (0.052) | -0.001 (0.053) | 1,637 | 0.037 (0.053) | 0.026 (0.057) |
| | Dead child | 896 | 0.057 (0.092) | 0.036 (0.116) | 683 | 0.208 ** (0.101) | 0.251 * (0.147) |
| First child | Not first child | 2,399 | 0.045 (0.056) | 0.032 (0.069) | 1,720 | 0.120 * (0.064) | 0.130 (0.086) |
| | First child | 875 | -0.068 (0.076) | -0.036 (0.049) | 600 | 0.015 (0.074) | 0.009 (0.055) |
| Urban/rural | Rural | 2,710 | 0.061 (0.056) | 0.052 (0.072) | 2,019 | 0.098 * (0.058) | 0.094 (0.077) |
| | Urban | 564 | -0.090 (0.085) | -0.009 (0.041) | 301 | 0.068 (0.108) | 0.018 (0.063) |
| Price shock | No price increase shock | 2,930 | 0.019 (0.051) | 0.014 (0.060) | 2,050 | 0.096 * (0.049) | 0.098 (0.067) |
| | Price increase shock | 344 | 0.467 * (0.259) | 0.196 *** (0.007) | 270 | 0.639 *** (0.215) | 0.199 *** (0.007) |
| | No price decrease shock | 3,037 | -0.002 (0.052) | -0.011 (0.058) | 2,154 | 0.058 (0.053) | 0.049 (0.066) |
| | Price decrease shock | 237 | 0.170 (0.241) | 0.290 (0.375) | 166 | 0.316 (0.247) | 0.364 (0.316) |
| SES (asset index) | Long-term asset 1 top 40% | 1,281 | -0.055 (0.072) | -0.030 (0.055) | 830 | 0.037 (0.083) | 0.040 (0.063) |
| | Long-term asset 1 bottom 40% | 1,343 | 0.105 * (0.062) | 0.087 (0.095) | 1,027 | 0.128 * (0.074) | 0.118 (0.112) |
| | Long-term asset 2 top 40% | 1,294 | 0.067 (0.073) | 0.072 (0.092) | 847 | 0.143 * (0.075) | 0.175 (0.119) |
| | Long-term asset 2 bottom 40% | 1,300 | 0.072 (0.071) | 0.039 (0.091) | 1,006 | 0.146 * (0.079) | 0.130 (0.120) |
| | Short-term asset top 40% | 1,284 | -0.043 (0.053) | -0.048 (0.059) | 893 | -0.015 (0.065) | 0.016 (0.093) |
| | Short-term asset bottom 40% | 1,324 | 0.059 (0.090) | 0.053 (0.098) | 944 | 0.047 (0.095) | 0.021 (0.085) |
| Ethnicity | Lao-Tai ethnicity | 1,459 | -0.055 (0.069) | -0.012 (0.041) | 887 | 0.006 (0.074) | 0.027 (0.042) |
| | Mon-Khmer ethnicity | 1,638 | 0.049 (0.073) | 0.005 (0.098) | 1,351 | 0.109 (0.077) | 0.116 (0.110) |
| | Hmong-Mien and other ethnicity | 177 | 0.306 / -0.294 | - / - | 82 | - | - |
| Mother's education | No education | 1,625 | 0.037 (0.062) | 0.004 (0.088) | 1,293 | 0.135 ** (0.063) | 0.151 (0.099) |
| | Primary education | 1,201 | 0.002 (0.073) | 0.000 (0.067) | 801 | 0.009 (0.081) | 0.001 (0.065) |
| | Lower secondary and above education | 448 | 0.044 (0.106) | 0.049 (0.064) | 226 | 0.045 (0.140) | 0.048 (0.083) |
| Province | Bolikhamxay province | 463 | -0.090 (0.090) | -0.098 (0.057) | 93 | - | - |
| | Khammaun province | 767 | 0.100 (0.069) | 0.103 * (0.057) | 523 | 0.238 *** (0.076) | 0.190 ** (0.075) |
| | Savanhnakhet province | 753 | -0.075 (0.087) | -0.082 (0.083) | 510 | -0.069 (0.109) | -0.044 (0.112) |
| | Saravan province | 652 | 0.107 (0.106) | 0.064 (0.157) | 652 | 0.093 (0.105) | 0.047 (0.162) |
| | Champasak province | 514 | 0.030 (0.107) | 0.028 (0.106) | 417 | 0.022 (0.082) | 0.008 (0.036) |
| | Attapue province | 125 | - | - | 125 | - | - |
| Distance | Less than 3 km away from health center | 1,182 | 0.004 (0.076) | 0.021 (0.061) | 847 | 0.028 (0.080) | 0.048 (0.065) |
| | 3–6 km away from health center | 980 | -0.016 (0.076) | -0.013 (0.082) | 696 | 0.091 (0.091) | 0.090 (0.119) |
| | More than 6 km away from health center | 1,100 | 0.100 (0.098) | 0.042 (0.140) | 777 | 0.163 (0.103) | 0.141 (0.169) |

*Source:* IEG.
*Notes:* The robust standard error is reported in the parenthesis, which is clustered at village level. The coefficient and standard error of the time and intervention interaction term is reported through linear probability model (LPM) and marginal effect of logit regression. Some results cannot be generated due to the small sample size or no comparator. In addition to the small sample size, there are some cases where LPM has results but not for logit, and this comes from the complete separation or no convergent results through maximum likelihood function for logit. The subgroup analysis is not conducted on child gender because child gender must be unknown before delivery. km = kilometers; LPM = linear probability model; PDO = project development objective; SES = socioeconomic status.
Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.

## Table I.2. PDO Indicator 2 (Child Delivered at a Health Facility, 0–11 months)

| | | Full sample | | | | Matched sample | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | With covariates | | | | With covariates | | |
| | Subgroup | n | LPM | | Logit (margin) | | n | LPM | | Logit (margin) |
| **PDO** | Institutional delivery vs. Noninstitutional delivery (all births, 0–11 months) | 3,274 | -0.018 | | -0.018 | | 2,320 | 0.047 | | 0.070 * |
| | | | (0.040) | | (0.053) | | | (0.048) | | (0.039) |
| **Dead child** | No dead child | 2,378 | -0.038 | | -0.056 | | 1,637 | 0.027 | | 0.054 |
| | | | (0.046) | | (0.069) | | | (0.057) | | (0.052) |
| | Dead child | 896 | 0.047 | | 0.023 | | 683 | 0.113 * | | 0.053 ** |
| | | | (0.058) | | (0.062) | | | (0.060) | | (0.025) |
| **First child** | Not first child | 2,399 | 0.011 | | 0.013 | | 1,720 | 0.088 ** | | 0.069 *** |
| | | | (0.043) | | (0.049) | | | (0.043) | | (0.024) |
| | First child | 875 | -0.076 | | -0.111 | | 600 | -0.034 | | 0.022 |
| | | | (0.077) | | (0.119) | | | (0.092) | | (0.096) |
| **Urban/rural** | Rural | 2,710 | 0.023 | | 0.014 | | 2,019 | 0.076 | | 0.073 *** |
| | | | (0.044) | | (0.050) | | | (0.049) | | (0.028) |
| | Urban | 564 | -0.097 | | -0.140 | | 301 | 0.049 | | 0.107 |
| | | | (0.097) | | (0.151) | | | (0.156) | | (0.072) |
| **Price shock** | No price increase shock | 2,930 | -0.017 | | -0.018 | | 2,050 | 0.048 | | 0.068 * |
| | | | (0.045) | | (0.059) | | | (0.060) | | (0.041) |
| | Price increase shock | 344 | 0.204 | | 0.000 | | 270 | 0.155 | | 0.000 |
| | | | (0.166) | | (0.000) | | | (0.154) | | (0.000) |
| | No price decrease shock | 3,037 | -0.018 | | -0.016 | | 2,154 | 0.044 | | 0.067 ** |
| | | | (0.040) | | (0.051) | | | (0.050) | | (0.034) |
| | Price decrease shock | 237 | 0.011 | | 0.003 | | 166 | 0.057 | | 0.000 |
| | | | (0.163) | | (0.004) | | | (0.120) | | (0.000184) |
| **SES (asset index)** | Long-term asset 1 top 40% | 1,281 | -0.124 ** | | -0.138 | | 830 | -0.026 | | 0.052 |
| | | | (0.063) | | (0.101) | | | (0.080) | | (0.086) |
| | Long-term asset 1 bottom 40% | 1,343 | 0.091 * | | 0.047 | | 1,027 | 0.140 ** | | 0.064 ** |
| | | | (0.053) | | (0.044) | | | (0.062) | | (0.027) |
| | Long-term asset 2 top 40% | 1,294 | 0.041 | | 0.055 | | 847 | 0.194 *** | | 0.159 *** |
| | | | (0.064) | | (0.095) | | | (0.073) | | (0.027) |
| | Long-term asset 2 bottom 40% | 1,300 | 0.048 | | 0.050 | | 1,006 | 0.060 | | 0.055 * |
| | | | (0.056) | | (0.054) | | | (0.056) | | (0.034) |
| | Short-term asset top 40% | 1,284 | -0.012 | | -0.063 | | 893 | 0.027 | | 0.049 |
| | | | (0.060) | | (0.097) | | | (0.089) | | (0.053) |
| | Short-term asset bottom 40% | 1,324 | -0.010 | | 0.064 | | 944 | 0.067 | | 0.092 ** |
| | | | (0.057) | | (0.062) | | | (0.070) | | (0.045) |
| **Ethnicity** | Lao-Tai ethnicity | 1,459 | -0.057 | | -0.095 | | 887 | 0.027 | | 0.049 |
| | | | (0.052) | | (0.083) | | | (0.061) | | (0.063) |
| | Mon-Khmer ethnicity | 1,638 | 0.064 | | 0.055 * | | 1,351 | 0.074 | | 0.054 *** |
| | | | (0.060) | | (0.028) | | | (0.063) | | (0.018) |
| | Hmong-Mien and other ethnicity | 177 | -0.200 | | - | | 82 | - | | - |
| | | | -0.168 | | - | | | - | | - |
| **Mother's education** | No education | 1,625 | 0.077 | | 0.069 * | | 1,293 | 0.084 * | | 0.082 *** |
| | | | (0.049) | | (0.036) | | | (0.049) | | (0.024) |
| | Primary education | 1,201 | -0.079 | | -0.113 | | 801 | 0.060 | | 0.073 |
| | | | (0.058) | | (0.090) | | | (0.067) | | (0.060) |
| | Lower secondary and above education | 448 | -0.101 | | -0.131 | | 226 | -0.233 | | -0.125 |
| | | | (0.106) | | (0.102) | | | (0.185) | | (0.335) |
| **Province** | Bolikhamxay province | 463 | -0.278 *** | | -0.355 *** | | 93 | - | | - |
| | | | (0.060) | | (0.063) | | | - | | - |
| | Khammaun province | 767 | 0.131 ** | | 0.150 *** | | 523 | 0.141 * | | 0.254 *** |
| | | | (0.056) | | (0.052) | | | (0.069) | | (0.075) |
| | Savanhnakhet province | 753 | 0.019 | | 0.060 | | 510 | -0.002 | | 0.016 |
| | | | (0.084) | | (0.118) | | | (0.100) | | (0.091) |
| | Saravan province | 652 | 0.171 ** | | 0.000 | | 652 | 0.176 ** | | 0.000 |
| | | | (0.082) | | (0.000) | | | (0.079) | | (0.000) |
| | Champasak province | 514 | -0.037 | | -0.065 | | 417 | -0.096 | | -0.195 |
| | | | (0.078) | | (0.108) | | | (0.089) | | (0.160) |
| | Attapue province | 125 | - | | - | | 125 | - | | - |
| | | | - | | - | | | - | | - |
| **Distance** | Less than 3 km away from health center | 1,182 | 0.020 | | 0.024 | | 847 | 0.169 * | | 0.236 *** |
| | | | (0.069) | | (0.092) | | | (0.085) | | (0.075) |
| | 3–6 km away from health center | 980 | -0.028 | | -0.001 | | 696 | -0.106 | | -0.077 |
| | | | (0.077) | | (0.086) | | | (0.082) | | (0.106) |
| | More than 6 km away from health center | 1,100 | 0.015 | | -0.021 | | 777 | 0.125 ** | | 0.013 * |
| | | | (0.050) | | (0.066) | | | (0.057) | | (0.007) |

*Source:* IEG.

*Notes:* The robust standard error is reported in the parenthesis, which is clustered at village level. The coefficient and standard error of the time and intervention interaction term is reported through linear probability model (LPM) and marginal effect of logit regression. Some results cannot be generated due to the small sample size or no comparator. In addition to the small sample size, there are some cases where LPM has results but not for logit, and this comes from the complete separation or no convergent results through maximum likelihood function for logit. The subgroup analysis is not conducted on child gender because child gender must be unknown before delivery. km = kilometers; LPM = linear probability model; PDO = project development objective; SES = socioeconomic status. Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.

# Appendix I

## Table I.3. PDO Indicator 3 (DPT Vaccination at Least Three Times, 12–23 months)

| | | Full sample | | | | | | Matched sample | | | | | |
| | | | | With covariates | | | | | | With covariates | | | |
| Subgroup | | n | LPM | | Logit (margin) | | | n | LPM | | Logit (margin) | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| PDO | DPT is at least 3 times (last birth, 12–23 months) | 2,277 | 0.149 | *** | 0.193 | *** | | 1,643 | 0.119 | * | 0.162 | ** | |
| | | | (0.050) | | (0.064) | | | | (0.062) | | (0.089) | | |
| Dead Child | No dead child | 1,594 | 0.181 | *** | 0.241 | *** | | 1,098 | 0.149 | ** | 0.197 | * | |
| | | | (0.054) | | (0.071) | | | | (0.074) | | (0.108) | | |
| | Dead child | 683 | 0.085 | | 0.106 | | | 545 | 0.097 | | 0.131 | | |
| | | | (0.088) | | (0.118) | | | | (0.102) | | (0.155) | | |
| Gender | Boy | 1,105 | 0.107 | | 0.140 | | | 796 | 0.140 | | 0.207 | | |
| | | | (0.072) | | (0.092) | | | | (0.089) | | (0.150) | | |
| | Girl | 1,172 | 0.179 | *** | 0.257 | *** | | 847 | 0.081 | | 0.109 | | |
| | | | (0.065) | | (0.091) | | | | (0.077) | | (0.105) | | |
| First Child | Not first child | 1,742 | 0.085 | | 0.114 | | | 1,276 | 0.119 | * | 0.164 | * | |
| | | | (0.059) | | (0.078) | | | | (0.068) | | (0.095) | | |
| | First child | 535 | 0.349 | *** | 0.448 | *** | | 367 | 0.162 | | 0.216 | | |
| | | | (0.103) | | (0.097) | | | | (0.112) | | (0.187) | | |
| Urban/Rural | Rural | 1,886 | 0.141 | ** | 0.190 | ** | | 1,398 | 0.106 | | 0.153 | | |
| | | | (0.057) | | (0.075) | | | | (0.065) | | (0.100) | | |
| | Urban | 391 | 0.149 | | 0.211 | | | 245 | 0.185 | | 0.181 | | |
| | | | (0.108) | | (0.172) | | | | (0.239) | | (0.337) | | |
| Price Shock | No price increase shock | 2,001 | 0.146 | *** | 0.190 | *** | | 1,441 | 0.094 | | 0.122 | | |
| | | | (0.054) | | (0.070) | | | | (0.064) | | (0.094) | | |
| | Price increase shock | 276 | 0.584 | | 0.046 | *** | | 202 | 0.973 | *** | - | | |
| | | | (0.373) | | (0.003) | | | | (0.249) | | - | | |
| | No price decrease shock | 2,118 | 0.115 | ** | 0.157 | ** | | 1,526 | 0.083 | | 0.116 | | |
| | | | (0.050) | | (0.066) | | | | (0.066) | | (0.097) | | |
| | Price decrease shock | 159 | 0.025 | | -0.151 | * | | 117 | 0.033 | | 0.008 | | |
| | | | (0.198) | | (0.090) | | | | (0.217) | | (0.076) | | |
| SES (Asset Index) | Long-term asset 1 top 40% | 873 | 0.139 | * | 0.175 | * | | 582 | 0.108 | | 0.153 | | |
| | | | (0.078) | | (0.098) | | | | (0.112) | | (0.153) | | |
| | Long-term asset 1 bottom 40% | 945 | 0.060 | | 0.068 | | | 749 | 0.059 | | 0.076 | | |
| | | | (0.072) | | (0.104) | | | | (0.082) | | (0.129) | | |
| | Long-term asset 2 top 40% | 925 | 0.194 | *** | 0.251 | ** | | 648 | 0.139 | | 0.182 | | |
| | | | (0.074) | | (0.102) | | | | (0.087) | | (0.139) | | |
| | Long-term asset 2 bottom 40% | 925 | 0.175 | ** | 0.240 | *** | | 713 | 0.128 | | 0.189 | * | |
| | | | (0.080) | | (0.091) | | | | (0.083) | | (0.110) | | |
| | Short-term asset top 40% | 911 | 0.096 | | 0.125 | | | 621 | 0.012 | | 0.027 | | |
| | | | (0.075) | | (0.103) | | | | (0.092) | | (0.115) | | |
| | Short-term asset bottom 40% | 921 | 0.178 | ** | 0.235 | *** | | 703 | 0.149 | | 0.250 | | |
| | | | (0.078) | | (0.089) | | | | (0.102) | | (0.167) | | |
| Ethnicity | Lao-Tai ethnicity | 990 | 0.183 | ** | 0.231 | ** | | 592 | 0.077 | | 0.063 | | |
| | | | (0.075) | | (0.103) | | | | (0.095) | | (0.106) | | |
| | Mon-Khmer ethnicity | 1,186 | 0.100 | | 0.136 | | | 992 | 0.104 | | 0.157 | | |
| | | | (0.071) | | (0.090) | | | | (0.092) | | (0.137) | | |
| | Hmong-Mien and other ethnicity | 101 | 0.061 | | - | | | 59 | - | | - | | |
| | | | -0.262 | | - | | | | - | | - | | |
| Mother's Education | No education | 1,155 | 0.043 | | 0.034 | | | 936 | 0.067 | | 0.086 | | |
| | | | (0.076) | | (0.103) | | | | (0.098) | | (0.143) | | |
| | Primary education | 848 | 0.235 | *** | 0.316 | *** | | 558 | 0.149 | * | 0.186 | | |
| | | | (0.074) | | (0.101) | | | | (0.086) | | (0.124) | | |
| | Lower secondary and above education | 274 | 0.184 | | 0.321 | | | 149 | -0.011 | | -0.049 | | |
| | | | (0.127) | | (0.208) | | | | (0.160) | | (0.134) | | |
| Province | Bolikhamxay province | 302 | 0.196 | | 0.273 | | | 65 | - | | - | | |
| | | | (0.143) | | (0.253) | | | | - | | - | | |
| | Khammaun province | 545 | -0.016 | | 0.011 | | | 358 | 0.001 | | -0.002 | | |
| | | | (0.097) | | (0.086) | | | | (0.143) | | (0.098) | | |
| | Savanhnakhet province | 508 | -0.103 | | -0.130 | | | 353 | -0.115 | | -0.094 | | |
| | | | (0.077) | | (0.147) | | | | (0.125) | | (0.188) | | |
| | Saravan province | 455 | 0.141 | | 0.122 | | | 455 | 0.082 | | 0.053 | | |
| | | | (0.164) | | (0.164) | | | | (0.171) | | (0.196) | | |
| | Champasak province | 344 | 0.302 | ** | 0.385 | ** | | 289 | 0.149 | | 0.089 | | |
| | | | (0.119) | | (0.153) | | | | (0.144) | | (0.112) | | |
| | Attapue province | 123 | - | | - | | | 123 | - | | - | | |
| | | | - | | - | | | | - | | - | | |
| Distance | Less than 3 km away from health center | 785 | 0.151 | | 0.186 | | | 576 | 0.020 | | 0.021 | | |
| | | | (0.098) | | (0.128) | | | | (0.126) | | (0.147) | | |
| | 3–6 km away from health center | 699 | 0.147 | | 0.224 | * | | 464 | 0.240 | ** | 0.450 | *** | |
| | | | (0.090) | | (0.121) | | | | (0.100) | | (0.149) | | |
| | More than 6 km away from health center | 789 | 0.098 | | 0.142 | | | 603 | 0.008 | | 0.024 | | |
| | | | (0.082) | | (0.106) | | | | (0.069) | | (0.116) | | |

*Source:* IEG.
*Notes:* The robust standard error is reported in the parenthesis, which is clustered at village level. The coefficient and standard error of the time and intervention interaction term is reported through linear probability model (LPM) and marginal effect of logit regression. Some results cannot be generated due to the small sample size or no comparator. In addition to the small sample size, there are some cases where LPM has results but not for logit, and this comes from the complete separation or no convergent results through maximum likelihood function for logit. km = kilometers; LPM = linear probability model; PDO = project development objective; SES = socioeconomic status.
Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.

## Table I.4. PDO Indicator 4 (Child Checkup at a Health Facility, 0–11 months)

| | Subgroup | Full sample n | LPM | | Logit (margin) | | Matched sample n | LPM | | Logit (margin) | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | With covariates | | | | | With covariates | | | |
| PDO | Any Growth Checkup vs. None (last birth, 0–11 months) | 3,257 | 0.046 (0.054) | | -0.104 (0.100) | | 2,307 | 0.004 0.082 | | -0.222 (0.121) | * |
| Dead child | No dead child | 2,363 | 0.016 (0.058) | | -0.130 (0.093) | | 1,626 | -0.054 (0.088) | | -0.223 (0.078) | *** |
| | Dead child | 894 | 0.161 (0.070) | ** | 0.057 (0.145) | | 681 | 0.196 (0.079) | ** | 0.092 (0.155) | |
| Gender | Boy | 1,621 | 0.014 (0.068) | | -0.122 (0.122) | | 1,137 | -0.056 (0.089) | | -0.276 (0.095) | *** |
| | Girl | 1,636 | 0.085 (0.062) | | -0.045 (0.118) | | 1,170 | 0.053 (0.084) | | -0.135 (0.121) | |
| First child | Not first child | 2,387 | 0.116 (0.059) | * | 0.008 (0.122) | | 1,711 | 0.085 (0.088) | | -0.124 (0.162) | |
| | First child | 870 | -0.130 (0.074) | * | -0.241 (0.047) | *** | 596 | -0.191 (0.094) | ** | -0.260 (0.055) | *** |
| Urban/rural | Rural | 2,698 | 0.056 (0.059) | | -0.143 (0.087) | | 2,009 | 0.017 (0.079) | | -0.215 (0.083) | *** |
| | Urban | 559 | 0.021 (0.117) | | 0.002 (0.264) | | 298 | -0.276 (0.177) | | -0.465 (0.226) | ** |
| Price shock | No price increase shock | 2,914 | 0.031 (0.053) | | -0.119 (0.084) | | 2,038 | -0.034 (0.080) | | -0.222 (0.082) | *** |
| | Price increase shock | 343 | 0.251 (0.222) | | - - | | 269 | 0.463 (0.122) | *** | - - | |
| | No price decrease shock | 3,020 | 0.012 (0.053) | | -0.141 (0.087) | | 2,141 | -0.054 (0.079) | | -0.255 (0.082) | *** |
| | Price decrease shock | 237 | 0.560 (0.137) | *** | 0.001 (0.002) | | 166 | 0.512 (0.195) | ** | - - | |
| SES (asset index) | Long-term asset 1 top 40% | 1,274 | -0.074 (0.075) | | -0.188 (0.088) | ** | 825 | -0.208 (0.122) | * | -0.261 (0.084) | *** |
| | Long-term asset 1 bottom 40% | 1,337 | 0.181 (0.054) | *** | 0.061 (0.128) | | 1,021 | 0.251 (0.057) | *** | 0.062 (0.141) | |
| | Long-term asset 2 top 40% | 1,284 | 0.107 (0.063) | * | -0.008 (0.128) | | 839 | 0.135 (0.089) | | 0.015 (0.152) | |
| | Long-term asset 2 bottom 40% | 1,297 | 0.046 (0.081) | | -0.239 (0.061) | *** | 1,005 | 0.015 (0.101) | | -0.174 (0.056) | *** |
| | Short-term asset top 40% | 1,276 | -0.007 (0.069) | | -0.210 (0.072) | *** | 888 | -0.136 (0.087) | | -0.208 (0.069) | *** |
| | Short-term asset bottom 40% | 1,316 | 0.090 (0.081) | | 0.022 (0.152) | | 937 | -0.045 (0.109) | | -0.173 (0.144) | |
| Ethnicity | Lao-Tai ethnicity | 1,450 | -0.046 (0.074) | | -0.148 (0.055) | *** | 881 | -0.257 (0.104) | ** | -0.187 (0.040) | *** |
| | Mon-Khmer ethnicity | 1,630 | 0.219 (0.057) | *** | 0.169 (0.059) | *** | 1,344 | 0.247 (0.065) | *** | 0.153 (0.064) | ** |
| | Hmong-Mien and other ethnicity | 177 | -0.363 -0.186 | * | - - | | 82 | - - | | - - | |
| Mother's education | No education | 1,617 | 0.143 (0.054) | *** | 0.081 (0.103) | | 1,287 | 0.211 (0.070) | *** | 0.108 (0.158) | |
| | Primary education | 1,195 | -0.058 (0.074) | | -0.199 (0.034) | *** | 797 | -0.180 (0.110) | | -0.210 (0.061) | *** |
| | Lower secondary and above education | 445 | -0.033 (0.087) | | -0.098 (0.095) | | 223 | -0.364 (0.145) | ** | -0.241 (0.106) | ** |
| Province | Bolikhamxay province | 462 | -0.070 (0.065) | | -0.001 (0.000) | *** | 93 | - - | | - - | |
| | Khammaun province | 766 | -0.092 (0.107) | | -0.182 (0.098) | * | 522 | -0.132 (0.127) | | -0.238 (0.133) | * |
| | Savanhnakhet province | 746 | 0.151 (0.067) | ** | 0.073 (0.114) | | 505 | 0.185 (0.074) | ** | 0.046 (0.118) | |
| | Saravan province | 649 | 0.386 (0.096) | *** | 0.073 (0.029) | ** | 649 | 0.366 (0.092) | *** | 0.075 (0.026) | *** |
| | Champasak province | 509 | 0.038 (0.104) | | -0.078 (0.057) | | 413 | -0.013 (0.078) | | -0.005 (0.029) | |
| | Attapue province | 125 | - - | | - - | | 125 | - - | | - - | |
| Distance | Less than 3 km away from health center | 1,178 | 0.169 (0.089) | * | 0.067 (0.167) | | 844 | 0.302 (0.131) | ** | 0.239 (0.243) | |
| | 3–6 km away from health center | 971 | -0.023 (0.084) | | -0.161 (0.130) | | 690 | 0.001 (0.113) | | -0.238 (0.146) | |
| | More than 6 km away from health center | 1,096 | 0.006 (0.092) | | -0.314 (0.060) | *** | 773 | -0.175 (0.116) | | -0.220 (0.099) | ** |

*Source:* IEG.

*Notes:* The robust standard error is reported in the parenthesis, which is clustered at village level. The coefficient and standard error of the time and intervention interaction term is reported through linear probability model (LPM) and marginal effect of logit regression. Some results cannot be generated due to the small sample size or no comparator. In addition to the small sample size, there are some cases where LPM has results but not for logit, and this comes from the complete separation or no convergent results through maximum likelihood function for logit. km = kilometers; LPM = linear probability model; PDO = project development objective; SES = socioeconomic status.
Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.

# Appendix I

## Table I.5. PDO Indicator 5 (Breastfeeding within One Hour of Birth, 0–11 months)

| Subgroup | | Full sample | | | Matched sample | | |
|---|---|---|---|---|---|---|---|
| | | | With covariates | | | With covariates | |
| | | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| PDO | Breastfeeding within 1h of birth vs. None (last birth, 0–11 months) | 3,257 | 0.070 (0.060) | 0.076 (0.062) | 2,308 | 0.042 (0.074) | 0.070 (0.072) |
| Dead child | No dead child | 2,363 | 0.082 (0.063) | 0.087 (0.061) | 1,626 | 0.078 (0.081) | 0.086 (0.084) |
| | Dead child | 894 | 0.027 (0.094) | 0.031 (0.103) | 682 | -0.014 (0.113) | -0.025 (0.129) |
| Gender | Boy | 1,622 | 0.118 (0.075) | 0.123 * (0.071) | 1,138 | 0.039 (0.089) | 0.046 (0.097) |
| | Girl | 1,635 | 0.005 (0.066) | 0.008 (0.072) | 1,170 | 0.017 (0.074) | 0.021 (0.086) |
| First child | Not first child | 2,387 | 0.062 (0.071) | 0.065 (0.072) | 1,712 | 0.011 (0.081) | 0.014 (0.090) |
| | First child | 870 | 0.057 (0.082) | 0.068 (0.082) | 596 | 0.079 (0.106) | 0.078 (0.101) |
| Urban/rural | Rural | 2,698 | 0.065 (0.068) | 0.066 (0.068) | 2,010 | 0.033 (0.069) | 0.037 (0.076) |
| | Urban | 559 | 0.115 (0.117) | 0.150 (0.119) | 298 | 0.058 (0.139) | -0.013 (0.205) |
| Price shock | No price increase shock | 2,914 | 0.070 (0.063) | 0.076 (0.063) | 2,039 | 0.024 (0.072) | 0.030 (0.080) |
| | Price increase shock | 343 | 1.012 *** (0.220) | 0.001 (0.001) | 269 | 0.941 *** (0.222) | 0.000 (0.001) |
| | No price decrease shock | 3,020 | 0.077 (0.062) | 0.081 (0.061) | 2,142 | 0.033 (0.069) | 0.037 (0.076) |
| | Price decrease shock | 237 | -0.044 (0.185) | 0.004 (0.003) | 166 | 0.138 (0.248) | 0.003 (0.003) |
| SES (asset index) | Long-term asset 1 top 40% | 1,273 | -0.089 (0.068) | -0.096 (0.079) | 825 | -0.108 (0.095) | -0.122 (0.114) |
| | Long-term asset 1 bottom 40% | 1,337 | 0.214 ** (0.086) | 0.209 *** (0.068) | 1,021 | 0.187 ** (0.082) | 0.200 ** (0.080) |
| | Long-term asset 2 top 40% | 1,284 | 0.090 (0.096) | 0.103 (0.100) | 840 | 0.131 (0.119) | 0.148 (0.136) |
| | Long-term asset 2 bottom 40% | 1,297 | 0.126 * (0.072) | 0.122 * (0.063) | 1,005 | 0.072 (0.076) | 0.084 (0.080) |
| | Short-term asset top 40% | 1,276 | 0.182 ** (0.076) | 0.195 *** (0.067) | 888 | 0.120 (0.089) | 0.170 (0.105) |
| | Short-term asset bottom 40% | 1,316 | -0.067 (0.075) | -0.073 (0.082) | 938 | -0.037 (0.093) | -0.039 (0.100) |
| Ethnicity | Lao-Tai ethnicity | 1,449 | -0.041 (0.078) | -0.044 (0.092) | 881 | -0.136 (0.084) | -0.162 (0.113) |
| | Mon-Khmer ethnicity | 1,631 | 0.173 ** (0.088) | 0.169 ** (0.077) | 1,345 | 0.209 ** (0.094) | 0.216 ** (0.099) |
| | Hmong-Mien and other ethnicity | 177 | -0.061 -0.154 | - - | 82 | - - | - - |
| Mother's education | No education | 1,616 | 0.080 (0.076) | 0.078 (0.069) | 1,287 | 0.066 (0.083) | 0.066 (0.084) |
| | Primary education | 1,195 | 0.124 (0.080) | 0.133 * (0.078) | 797 | 0.023 (0.104) | 0.040 (0.127) |
| | Lower secondary and above education | 446 | -0.099 (0.111) | -0.123 (0.132) | 224 | -0.290 * (0.173) | -0.436 * (0.232) |
| Province | Bolikhamxay province | 462 | 0.130 (0.086) | 0.146 (0.097) | 93 | - - | - - |
| | Khammaun province | 766 | -0.110 (0.089) | -0.110 (0.124) | 522 | -0.378 *** (0.110) | -0.544 *** (0.182) |
| | Savanhnakhet province | 745 | 0.233 ** (0.113) | 0.193 *** (0.062) | 505 | 0.444 *** (0.108) | 0.265 *** (0.059) |
| | Saravan province | 649 | -0.053 (0.114) | -0.063 (0.119) | 649 | -0.027 (0.107) | -0.038 (0.113) |
| | Champasak province | 510 | 0.058 (0.097) | 0.096 (0.108) | 414 | 0.227 * (0.116) | 0.336 *** (0.093) |
| | Attapue province | 125 | - - | - - | 125 | - - | - - |
| Distance | Less than 3 km away from health center | 1,178 | 0.142 (0.095) | 0.150 * (0.087) | 844 | 0.106 (0.101) | 0.128 (0.109) |
| | 3–6 km away from health center | 971 | 0.132 (0.087) | 0.130 * (0.077) | 690 | 0.038 (0.110) | 0.037 (0.115) |
| | More than 6 km away from health center | 1,096 | -0.057 (0.102) | -0.067 (0.113) | 774 | -0.036 (0.123) | -0.045 (0.140) |

*Source:* IEG.

*Notes:* The robust standard error is reported in the parenthesis, which is clustered at village level. The coefficient and standard error of the time and intervention interaction term is reported through linear probability model (LPM) and marginal effect of logit regression. Some results cannot be generated due to the small sample size or no comparator. In addition to the small sample size, there are some cases where LPM has results but not for logit, and this comes from the complete separation or no convergent results through maximum likelihood function for logit. km = kilometers; LPM = linear probability model; PDO = project development objective; SES = socioeconomic status.
Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.

# Appendix I

## Table I.6. PDO Indicator 6 (Receive ORS with Diarrhea in Previous Two Weeks, 0–23 months)

| | Subgroup | | Full sample | | | Matched sample | |
|---|---|---|---|---|---|---|---|
| | | | | With covariates | | | With covariates |
| | | n | LPM | Logit (margin) | n | LPM | Logit (margin) |
| PDO | Received ORS (1, 2, or 3) during diarrhea vs. Not received (last two births, 0–23 months) | 933 | 0.042 (0.085) | 0.048 (0.088) | 731 | 0.010 (0.114) | 0.020 (0.113) |
| Dead child | No dead child | 626 | 0.012 (0.087) | 0.014 (0.088) | 466 | 0.022 (0.118) | 0.029 (0.117) |
| Dead child | Dead child | 307 | 0.087 (0.186) | 0.103 (0.222) | 265 | -0.081 (0.206) | -0.061 (0.181) |
| Gender | Boy | 426 | -0.063 (0.119) | -0.066 (0.099) | 336 | -0.066 (0.143) | -0.069 (0.129) |
| Gender | Girl | 507 | 0.112 (0.106) | 0.156 (0.136) | 395 | 0.069 (0.132) | 0.105 (0.164) |
| First child | Not first child | 725 | -0.018 (0.106) | -0.012 (0.102) | 578 | -0.056 (0.132) | -0.029 (0.115) |
| First child | First child | 208 | 0.276 (0.196) | 0.404 ** (0.172) | 153 | 0.246 (0.225) | 0.396 ** (0.160) |
| Urban/rural | Rural | 807 | 0.050 (0.094) | 0.056 (0.104) | 662 | 0.003 (0.121) | 0.014 (0.120) |
| Urban/rural | Urban | 126 | -0.069 (0.278) | -0.010 (0.368) | 69 | -0.203 (0.393) | - - |
| Price shock | No price increase shock | 762 | 0.071 (0.094) | 0.084 (0.106) | 596 | 0.072 (0.123) | 0.083 (0.132) |
| Price shock | Price increase shock | 171 | -0.803 (0.843) | - - | 135 | - - | - - |
| Price shock | No price decrease shock | 849 | 0.080 (0.094) | 0.087 (0.105) | 674 | 0.061 (0.125) | 0.069 (0.131) |
| Price shock | Price decrease shock | 84 | 0.950 (0.701) | 0.623 *** (0.029) | 57 | 0.848 (1.733) | - - |
| SES (asset index) | Long-term asset 1 top 40% | 292 | 0.062 (0.120) | 0.072 (0.151) | 192 | 0.121 (0.147) | 0.141 (0.213) |
| SES (asset index) | Long-term asset 1 bottom 40% | 469 | 0.064 (0.143) | 0.080 (0.157) | 406 | -0.022 (0.158) | -0.003 (0.129) |
| SES (asset index) | Long-term asset 2 top 40% | 372 | 0.018 (0.114) | 0.043 (0.139) | 281 | -0.027 (0.163) | 0.003 (0.197) |
| SES (asset index) | Long-term asset 2 bottom 40% | 415 | -0.062 (0.137) | -0.053 (0.132) | 357 | -0.131 (0.176) | -0.116 (0.135) |
| SES (asset index) | Short-term asset top 40% | 344 | -0.083 (0.126) | -0.028 (0.034) | 261 | 0.039 (0.165) | 0.026 (0.053) |
| SES (asset index) | Short-term asset bottom 40% | 420 | 0.144 (0.139) | 0.206 (0.171) | 349 | 0.114 (0.181) | 0.127 (0.227) |
| Ethnicity | Lao-Tai ethnicity | 282 | 0.117 (0.111) | 0.164 (0.139) | 168 | -0.006 (0.172) | 0.018 (0.161) |
| Ethnicity | Mon-Khmer ethnicity | 599 | 0.154 (0.118) | 0.207 (0.154) | 538 | 0.121 (0.132) | 0.161 (0.173) |
| Ethnicity | Hmong-Mien and other ethnicity | 52 | - - | - - | 25 | - - | - - |
| Mother's education | No education | 548 | -0.022 (0.125) | -0.019 (0.126) | 484 | -0.046 (0.162) | -0.040 (0.148) |
| Mother's education | Primary education | 289 | 0.029 (0.131) | 0.056 (0.139) | 198 | 0.064 (0.172) | 0.118 (0.210) |
| Mother's education | Lower secondary and above education | 96 | 0.448 * (0.267) | 0.884 * (0.515) | 49 | 0.907 * (0.533) | - - |
| Province | Bolikhamxay province | 117 | -0.056 (0.258) | -0.216 (0.467) | 26 | - - | - - |
| Province | Khammaun province | 108 | -0.006 (0.222) | -0.122 (0.192) | 68 | -0.197 (0.279) | 1.539 (1.081) |
| Province | Savanhnakhet province | 176 | 0.308 * (0.177) | 0.395 (0.283) | 121 | -0.080 (0.318) | -0.004 (0.005) |
| Province | Saravan province | 375 | 0.150 (0.167) | 0.192 (0.228) | 375 | 0.141 (0.177) | 0.186 (0.246) |
| Province | Champasak province | 121 | 0.232 (0.202) | 0.387 (0.321) | 105 | -0.238 (0.200) | -0.004 (0.005) |
| Province | Attapue province | 36 | - - | - - | 36 | - - | - - |
| Distance | Less than 3 km away from health center | 323 | 0.141 (0.119) | 0.156 (0.173) | 262 | 0.122 (0.102) | 0.217 (0.190) |
| Distance | 3–6 km away from health center | 284 | 0.099 (0.132) | 0.085 (0.152) | 210 | 0.181 (0.157) | 0.212 (0.174) |
| Distance | More than 6 km away from health center | 325 | 0.025 (0.185) | 0.005 (0.213) | 259 | -0.045 (0.227) | -0.076 (0.240) |

*Source:* IEG.

*Notes:* The robust standard error is reported in the parenthesis, which is clustered at village level. The coefficient and standard error of the time and intervention interaction term is reported through linear probability model (LPM) and marginal effect of logit regression. Some results cannot be generated due to the small sample size or no comparator. In addition to the small sample size, there are some cases where LPM has results but not for logit, and this comes from the complete separation or no convergent results through maximum likelihood function for logit. km = kilometers; LPM = linear probability model; PDO = project development objective; SES = socioeconomic status.
Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.