Fighting Silent Killers:

The Equilibrium Effects of India's Primary Healthcare Expansion

Patrick Agte (Yale)   Jitendra Kumar Soni (IAS)*

October 12, 2024

Most recent version here

Abstract

Life expectancy at age 60 in lower-middle-income countries remains lower than expected given their income levels. In this paper, we show that one reason is a fragmented healthcare system that provides low quality of care. We exploit quasi-experimental variation induced by program rules to study the impact of providing a new mid-level provider (non-physician practitioner) to rural public primary healthcare facilities in India. Two years after the large-scale reform, monthly patient loads at treated public primary healthcare facilities increased by 58% and all-age mortality fell by 10%. Eighty percent of the mortality decline is attributable to a decrease in deaths of individuals aged 56+ years, leading to an increase in elderly life expectancy by at least three months. Evidence from patient exit surveys, audit visits, and medical vignettes show that the reform improved healthcare quality and service availability in the public sector. Private providers responded to the increased competition by improving their quality as well. We estimate a structural model of patient demand to decompose mechanisms and evaluate optimal staffing policies. We find that only increasing public healthcare quality would achieve half of the observed decline in mortality. Ten percent of the decrease in mortality can be attributed to improvements in the private sector. Reallocating mid-level providers based on local market conditions would achieve an 18% greater reduction in mortality outcomes.

*We are especially grateful to Janet Currie, Thomas Fujiwara, and Rohini Pande for their encouragement, advice, and patience. We are particularly thankful to Christopher Neilson for detailed comments. We express our sincere gratitude for the continued support from the Government of Rajasthan, especially to Lokesh Chaturvedi, Mahesh Sachdeva, and Sourabh Vijay. We want to thank our team for their excellent assistance throughout this project, especially Ashi Sharma, Sandhya Seetharaman, and Gulshan Banas. We are also thankful for valuable help from Khushi Baby and the WISH Foundation. We thank Abhay Bang, Arielle Bernhardt, Jishnu Das, Mateus Ferraz Dias, Anne Karing, Justine Knebelmann, Seema Jayachandran, Pavitra Mohan, Ruchit Nagar, Nachiket Mor, Natalia Rigol, Sagar Saxena, Christiane Szerman, and seminar participants at Princeton, Yale, IEG, NHSRC, and NEUDC for helpful advice and comments. This project was made possible with funding from the Princeton Institute for International and Regional Studies, the Princeton Center for Health and Wellbeing, the Yale Economic Growth Center, the National Science Foundation, the Weiss Family Fund, the J-PAL Gender and Economic Agency Initiative, the J-PAL Cash Transfers for Child Health Initiative, and SurveyCTO. As the previous Mission Director of Rajasthan's National Health Mission, Jitendra Kumar Soni was involved in the implementation of this reform. However, all views and errors are solely ours, and this paper does not necessarily represent the view of the National Health Mission or any part of the Government of Rajasthan.

1 Introduction

Almost 40% of deceased adults in rural India did not receive any medical attention before their death (NSS 2017–2018). Even when patients seek healthcare services, unreliable access to public facilities makes many patients prefer informal private providers with limited medical qualifications (Das et al., 2016).[1] These factors contribute to an increasing gap in life expectancy at age 60 between poor and rich countries.[2] Looking ahead, the discrepancy could even widen as the health systems in India and other low- and middle-income countries struggle to adapt to an aging population and a rise in chronic diseases.[3] Yet, there is limited evidence on how governments can improve health systems when state capacity is low and private sector alternatives exist.

This paper helps to fill this knowledge gap by studying the rollout of one of the world's largest public primary healthcare reforms. The reform involved posting a mid-level provider, also known as Community Health Officers (CHOs), to every subcenter (rural health outpost) across India. As of the 2023-24 fiscal year, 138,257 CHOs were posted (MHFW, 2024), impacting the healthcare provision for over 750 million people.[4] Prior to the reform, subcenters were only staffed by an Auxiliary Nurse Midwife (ANM), who provided a mix of maternal and newborn health and outpatient care services. The new CHOs, who are posted alongside the existing ANMs, are more highly qualified non-physician practitioners with the mandate to strengthen healthcare services for adults.

The objective of the CHO postings was to improve access to treatments and prevent premature adult mortality by directly improving public healthcare provision. It further

[1] Das et al. (2016) find that 60 percent of primary healthcare visits in Madhya Pradesh are made to private providers without formal qualifications. In our sample in Rajasthan, around 20% of patients report visiting an informal private provider when seeking healthcare.

[2] While life expectancy at age 60 increased from 16.6 to 22.9 years between 1960 and 2019 in high-income countries, it only increased from 14.4 to 18.2 in low- and middle-income countries. When flexibly controlling for log GDP per capita (in PPP) and year fixed effects, life expectancy at age 60 is 0.65 years (p-value = 0.030) lower in low- and middle-income countries than in high-income ones.

[3] Chronic diseases are now the leading cause of death in most low- and middle-income countries. Relative to high-income countries, chronic diseases occur at earlier ages (Rashmi and Mohanty, 2023) and patients die younger in low- and middle-income countries (Stuckler, 2008). Estimates suggest that the economic impact of the five main chronic diseases in India between 2012 and 2030 will be USD 12.7 trillion (Bloom et al., 2014), adjusted to 2022 dollars.

[4] The average population covered by a subcenter is 5,624 (MHFW, 2022). In Rajasthan, catchment areas are smaller and cover, on average, 3,000 people.

had the potential to crowd in private provider quality by creating competitive pressure, an approach that is sometimes known as 'regulation by competition' (McPake and Hanson, 2016). However, labor inputs may not be sufficient to overcome demand-side constraints (Dupas, 2011) or high rates of absenteeism in the public sector (Chaudhury et al., 2006; Banerjee et al., 2008). In addition, improving lower-level public facilities could even worsen health outcomes, if patients substitute away from higher-quality facilities or private providers close down in response to the increase in competition. Taken together, the magnitude and even direction of impacts on adult health outcomes from such a reform is thus ambiguous, making it important to study its impact empirically.

We combine quasi-experimental variation in the rollout of the reform in Rajasthan, one of India's largest and poorest states, with a structural model of patient demand to show that the CHO postings reduced mortality rates by improving public healthcare provision and reducing the market power of private providers. Our analysis relies on large-scale administrative health data for all 46,560 villages in Rajasthan for up to two years after the CHO postings. Our novel administrative data covers monthly patient loads and service utilization for the universe of public primary healthcare facilities as well as mortality outcomes for all villages in a subcenter's catchment area. We supplement the administrative data with two rounds of original survey data on public providers, private providers, and households across 193 villages in the state. The survey data further allows us to measure how the reform affected hospitalization rates, availability of services, and healthcare quality in both the public and private sectors. Additionally, in collaboration with a local NGO, we collect data on households' provider choices as part of a healthcare household census that is being administered in all villages within the state.

We have three main findings. First, we observe large increases in the utilization of primary healthcare services: administrative data shows that the average number of patient visits rose by 58% at subcenters where a CHO was posted. Using state-wide data from a healthcare census of households, we find that the increase in patient visits is driven entirely by patients who would not otherwise seek healthcare services. A higher number of patients diagnosed with acute heart diseases (e.g. heart attacks) and epilepsy as well as with hypertension and diabetes at treated subcenters suggest that the posting of CHOs improved both acute and preventive care. Reassuringly, we find that the posting of CHOs did not divert attention from existing maternal and child health services.

Second, we find a dramatic impact on the health of elderly individuals. Using our statewide data on mortality outcomes, we show that the posting of a CHO reduced all-age deaths in treated areas by 10% within the first 24 months. Eighty percent of this decline can be attributed to a reduction in elderly (age 56+) deaths, implying an increase in elderly life expectancy by at least three months. The mortality outcomes for other age groups are not significantly impacted.[5] Results from our household survey further show a decline in hospitalizations, also concentrated among elderly individuals.

Our third finding relates to mechanisms through which the CHO postings affected health outcomes. We use hypothetical medical vignettes and patient exit surveys to show that the CHOs improved the quality of public healthcare provision. We further observe that the CHOs led to a large increase in access to healthcare services, with treated subcenters being 64% more likely to be open during unannounced audit visits. Finally, we find that private providers in treated areas respond to the arrival of CHOs by investing in quality upgrades, including through increased enrollment in medical degrees and training workshops. This effect is especially pronounced among providers that are located closer to treatment group subcenters or that are the only private providers in the area at baseline, consistent with a decrease in market power driving the results. We observe no change in the number of private providers or in private providers' patient load, prices, or antibiotic dispensing rates.

Our identification strategy exploits the staggered rollout of the new mid-level providers, using a matched difference-in-differences design that is informed by the rules of the provider assignment process. Due to budget constraints, the government of Rajasthan could fill only two-thirds of eligible provider vacancies in the first wave of program implementation (March 2022). The decision of which subcenter within a district would receive a CHO as part of the first wave was ad-hoc: local government officials had one day to decide on assignments and the only information that was given to them was the providers' place of residence and subcenter locations. This resulted in quasi-random variation in assignments to subcenters

[5] Our administrative data provides aggregate information on deaths for five age groups: infants (< 1 year), children (1-4 years), adolescents ($5 - 14$ years), adults ($15 - 55$ years), and the elderly (56+ years).

conditional on a subcenter's location. Consistent with our knowledge of the assignment rule, we find that matching treatment and control samples based on a propensity score function that uses only district fixed effects and the subcenter's distance to the district and subdistrict headquarters is sufficient to create balance along other observable subcenter characteristics.

Our results are robust to various sensitivity tests, including different matching strategies, sample restrictions, and difference-in-differences estimators. We also continue to find effects when we instead use a difference-in-differences design that compares treatment group subcenters with the closest control group subcenter in the area. Similar levels and trends in the pre-period provide further evidence against concerns that differential trends between treatment and control units could be driving our results. We also find declines in mortality outcomes using a separately maintained dataset on deaths from the civil registration system, addressing concerns that changes in reporting bias can explain our results.

To decompose the importance of the different mechanisms and evaluate optimal staffing policies, we estimate a structural model of patient demand. We combine aggregate market shares with individual-level choice data to allow for observed and unobserved heterogeneity in patient preferences over spatially differentiated providers. To separate the increase in healthcare quality from an increase in healthcare access, we exploit variation in the availability of person-hours at subcenters, driven by whether existing healthcare workers live in the village of the subcenter and whether they are assigned to multiple facilities at once. We argue that this variation is related to personal circumstances and external government decisions and support this claim by showing that catchment area characteristics do not predict subcenter person-hours at baseline. Differences in the locations of medicine suppliers of private providers further generate exogenous variation in prices. We also use the covariance in provider prices between a patient's first- and second-choice to identify unobserved preference heterogeneity. In the final step, we combine the demand model estimates with a health production function to translate provider choices into mortality outcomes.

Counterfactual simulations show that only increasing subcenter quality would achieve 47% of the observed decline in mortality. By contrast, only increasing person-hours would almost have no effect on mortality, partly because infra-marginal patients do not benefit in this case. Simultaneously increasing public sector quality and person-hours achieves 90%

of the observed decline in mortality, while changes to private provider quality explain the remaining 10%. We also evaluate another commonly discussed healthcare reform, the closure of private providers, and find that such a policy would decrease average health outcomes, even after the posting of CHOs. Finally, we show that reallocating the existing batch of CHOs within the same market could achieve an 18% greater reduction in mortality outcomes.

Following the Marginal Value of Public Funds framework proposed by Hendren and Sprung-Keyser (2020), we estimate that the CHO reform is highly cost-effective, generating 45 dollars in private benefits for every government dollar spent.[6] We show that the reform remains cost-effective under conservative assumptions and could even pay for itself if the decline in hospitalizations continues to persist.

This paper contributes to three bodies of work. First, we contribute to the literature on improving public healthcare provision and the determinants of elderly mortality in low- and middle-income settings. Previous research has focused on constraints to healthcare utilization from a demand-side perspective.[7] There is scant evidence, however, on the impact of supply-side interventions. While past work finds that the construction of new facilities decreases mortality outcomes (Bailey and Goodman-Bacon, 2015; Mora-García et al., 2024), evidence on labor inputs and other interventions that aim to improve existing facilities is mixed (Carrillo and Feres, 2019; Okeke, 2023; Björkman and Svensson, 2009; Dhaliwal and Hanna, 2017). Most of the previous work also focuses on the impact of policies on infant mortality. As mortality profiles are rapidly changing due to aging populations in low- and middle-income countries, it becomes increasingly important to study how to reduce adult mortality outcomes, a topic about which we still know relatively little.[8] Our findings show how posting mid-level providers to rural outposts, a policy that is already used in 37 countries

[6]These calculations do not only account for the staffing costs and the decrease in mortality rates, but also for the reduction in public spending due to a decline in hospitalizations.

[7]Studies have shown that patients underinvest in healthcare services because they face financial constraints (Tarozzi et al., 2014), are optimistic about their own health (Kim and Niederdeppe, 2013), want to avoid stigma (Birje et al., 2022), lack information (Carrieri and Wuebker, 2016), are present-biased (Bai et al., 2021), want to avoid information (Oster et al., 2013; Li et al., 2021), and have limited access to treatment options if diagnosed with a health condition (Okeke et al., 2013).

[8]Existing studies have shown reduction in adult mortality outcomes in low- and middle-income countries due to the construction of new public primary healthcare facilities and cash transfers (Barham and Rowberry, 2013). Evidence on the impact of health insurance expansions is mixed (Chen et al., 2007; Sood et al., 2014; Gruber et al., 2023; Malani et al., 2024).

across Africa and Asia (Desai et al., 2020), can be highly effective in improving public service provision and reducing elderly mortality rates.

Second, our results speak to a growing literature that studies market structures and interactions between the public and private sectors in low- and middle-income countries. Existing work focuses primarily on the education sector (Dinerstein et al., 2023; Andrabi et al., 2024; Allende, 2021; Neilson, 2021). In healthcare, limited data availability often makes it difficult to study private sector behavior.[9] We add to this literature by combing a large-scale public sector reform with original survey data to show that private healthcare providers improve their quality in response to increased competition from the public sector.

Finally, we add to the existing body of work on optimal staffing policies in healthcare markets.[10] Our evaluation of counterfactual policies highlights the importance of simultaneously improving healthcare access and healthcare quality to reduce mortality rates, mirroring findings by Okeke (2023).[11] We further highlight the potential of achieving even greater improvements in health outcomes by reallocating healthcare workers based on local market conditions, relating to the broader literature on the misallocation of inputs (Hsieh and Klenow, 2009; Walter, 2020; Hsiao, 2022; Diop et al., 2019; Chandra et al., 2023).

The rest of this paper proceeds as follows. Section 2 describes the institutional context and conceptual framework. Section 3 discusses our empirical strategy. Section 4 presents our main results. Section 5 examines mechanisms and counterfactual policies using a structural model of patient demand. Section 6 discusses the findings and the cost-effectiveness of the reform. Section 6 concludes

2 Background

We begin by providing details about the reform to the healthcare sector. We then present a conceptual framework to analyze the expected treatment effects on health outcomes.

[9] Previous studies provide descriptive evidence on the quality of public and private providers in India and other countries (Das et al., 2008, 2016; Banerjee et al., 2023) and show how training and regulation interventions can improve private sector quality (Das et al., 2016; Bedoya et al., 2023).

[10] In education, Dinerstein et al. (2023) study the optimal level of public provision and Walter (2020) examines misallocation of teachers across schools. Ganimian et al. (2021) show how doubling the labor force improved the performance of India's public early-childhood program

[11] Okeke (2023) randomly varies whether health facilities receive a more highly-qualified provider or another provider with the same medical qualifications as the existing ones and finds that health outcomes only improve if a more qualified provider is posted.

2.1 Healthcare in Rural Rajasthan

Our analysis focuses on Rajasthan, the seventh most populous state in India. Rajasthan is one of the poorest states in the country, but it has heavily invested in strengthening its healthcare system in the past years.[12] Patients in rural areas usually have access to three types of primary healthcare facilities: subcenters, *primary health centers* (PHCs), and private providers. Before the Health and Wellness Center reform, a typical subcenter was staffed by one *Auxiliary Nurse Midwife* (ANMs) with a two-year diploma and three community health workers, also known as *Accredited Social Health Activists* (ASHAs). ANMs primarily focus on maternal and child healthcare services but have evolved into multipurpose healthcare workers over time and are now also supposed to provide a wide range of other healthcare services, including basic outpatient care and screening for chronic diseases. The expanded range of expected services leave many of them overburdened.[13] Since the ANMs perform most of their activities in the field, the physical subcenter building is rarely staffed.[14] Patients that the ANMs cannot treat are referred to physician-staffed PHCs, the next level up in the public primary healthcare system. Healthcare services and medicines at subcenters and PHCs are free for all patients. In addition to the public sector, private healthcare providers, who prescribe and dispense medicine with a markup, also play an essential role.[15] Most of these providers have limited medical qualifications, with 69% of our sample providers having less than a bachelor's degree. Survey evidence further suggests that private providers might have local market power, since 43% of them are the only private providers in the catchment area.

To strengthen the provision of public primary healthcare, the Government of India announced the Health and Wellness Center reform in September 2018 as part of the Ayushman Bharat initiative.[16] The reform aims to address the changing burden of disease by pro-

[12]While the Health and Wellness Center reform was launched by the central government, the implementation was done at the state level. Rajasthan ranked 27th out of 33 states in 2019–20 in terms of GDP per capita. In 2020–21, the state allocated 7.1% of the government budget to healthcare, whereas other states spent, on average, 5.3%.

[13]In our baseline survey, 69% of ANMs say that too much work is allocated to them and 57% say that they do not have sufficient time to complete their work.

[14]Only 42% of facilities in the control group were open during unannounced visits in our sample area.

[15]Among the subcenters in our survey sample, 58% have at least one private provider in the catchment area.

[16]The other component of the initiative was the expansion of public health insurance through the Pradhan

viding comprehensive primary healthcare in rural areas and converting 150,000 subcenters and PHCs into Health and Wellness Centers. The operational guidelines for the reform mentioned that the changes were motivated by global evidence that comprehensive primary healthcare "reduces morbidity and mortality at much lower costs and significantly reduces the need for secondary and tertiary care" (MHFW, 2018).

The key component of the Health and Wellness Center Reform is the creation of a new cadre of mid-level health providers, known as *Community Health Officers* (CHOs).[17] CHOs are required to have a three- or four-year degree in nursing and are posted alongside the existing ANMs. The main mandate of the CHOs is the provision of basic adult outpatient care and screening for chronic diseases at the subcenter level. Their payments consist of a fixed component as well as performance-based incentives for 15 indicators.[18]

2.2 Conceptual Framework

To understand how posting CHOs to subcenters could affect health outcomes, we present a model of patient demand. We start with a stylized version in this section to emphasize the main forces at play. In Section 4, we extend the model and take it to the data.

Let J^m be the number of healthcare providers available in market m. Whenever patient i gets sick, the patient needs to choose which of these healthcare providers to visit or whether not to seek healthcare at all. We characterize patients by their poverty status and their location.[19] Locations consist of the village or town in which the PHC is located as well as the subcenter villages that are connected to the corresponding PHC in market m. The main provider characteristics are their location, person-hours (h_j), quality (q_j) and price (p_j). Additional provider characteristics are captured by x_j. We assume that patient preferences over distance, quality, and price differ by poverty status. Patients also have random preference shocks for providers (ϵ_{ij}) that follow an i.i.d. Type 1 extreme-value distribution.

Mantri Jan Arogya Yojana (PMJAY) scheme. In Rajasthan, the government agreed to implement the PMJAY scheme in June 2019. A separate health insurance scheme under the name of Chiranjeevi Yojana was launched in May 2021.

[17] Appendix D describes additional details of the reform.

[18] The performance-based incentives cover a list of 15 service-based indicators at the subcenter level. ANMs and community health workers also receive smaller incentive payments. See Appendix Table A1 for details.

[19] Neilson (2021) uses a similar approach to model education markets in Chile.

A patient i's utility from seeking healthcare at provider j is

$$u_{ij} = \beta_i^q q_j + \beta^h h_j - \alpha_i p_j - \lambda_i d_{ij} + \beta x_{jt} + \epsilon_{ij}, \tag{1}$$

with $\beta_i^q = \bar{\beta}^q + \beta_1^q poor_i$, $\alpha_i = \bar{\alpha} + \alpha_1 poor_i + \nu_i$, and $\lambda_i = \bar{\lambda} + \lambda_1 poor_i$, where $poor_i$ is an indicator variable for whether the patient comes from a poor household and d_{ijt} is the distance between patient i's and provider j's locations. $u_{i0t} = \epsilon_{i0t}$ represents the utility from not seeking any healthcare.

Patients choose the provider j that maximizes their utility. We can write the share of non-poor patients who live in location l and select provider j as a function of provider quality, prices, person-hours,and parameters $(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta)$:

$$s_{j,nonpoor}^l(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) = \frac{exp(\bar{\beta}^q q_j + \bar{\beta}^h h_j - \bar{\alpha} p_j - \bar{\lambda} d_{lj} + \beta x_j)}{\sum_{n \in J^m} exp(\bar{\beta}^q q_n + \bar{\beta}^h h_n - \bar{\alpha} p_n - \bar{\lambda} d_{ln} + \beta x_n) + 1} \tag{2}$$

We can similarly write the share of poor patients who live in location l and select provider j and call it $s_{j,poor}^l(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta)$. To get total market shares, we then sum over the different locations in each market and over each patient type. The population shares of each location for poor and non-poor patients in market m is given by w_{poor}^l and $w_{nonpoor}^l$, such that their total sums are equal to one ($\sum_l^{L_m} w_{poor}^l = 1$ and $\sum_l^{L_m} w_{nonpoor}^l = 1$), where L_m is the total number of locations in market m. Similarly, the poverty share in market m is given by pov_m. Taken together, the total market share of provider j is thus given by

$$s_j(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) = pov_m \sum_l^{L_m} w_{poor}^l s_{j,poor}^l(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) + (1 - pov_m) \sum_l^{L_m} w_{nonpoor}^l s_{j,nonpoor}^l(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) \tag{3}$$

Subcenters and PHCs have fixed characteristics that are determined by the government. By contrast, private providers strategically choose prices, quality, and person-hours and whether to exit the market to maximize profits. We further assume that there is a direct relationship between health outcomes and the average healthcare quality $\bar{q}$ chosen by patients.

$$\bar{q}(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) = \sum_j s_j(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) q_j, \tag{4}$$

with $q_j = 0$ if patients choose not to seek healthcare at all.

To understand how posting CHOs to subcenters can affect health outcomes, we start by considering a market that only has two options: a subcenter or no healthcare at all. We first assume that posting a CHO is equivalent to improving subcenter quality. The intervention would then have the following effect on average healthcare quality:

$$\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} = \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} q_{shc} + s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) \tag{5}$$

The first term, $(ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/dq_{shc})q_{shc}$ captures the change in quality for patients who switch to subcenters once subcenter quality increases, while the second term, $s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)$ captures the increase in quality for inframarginal patients who would choose the subcenter even in the absence of the increase in subcenter quality. Assuming that patients like quality (and are able to observe it), higher subcenter quality results in an increase in the subcenter market share. Since both terms would then be positive, an increase in subcenter quality would result in better average healthcare quality and improved health outcomes.

We next consider a scenario in which patients also have the option to travel to a nearby town to visit a PHC that provides higher healthcare quality than the subcenter ($q_{phc} > q_{shc}$):

$$\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} = \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} q_{shc} + s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} q_{phc} \tag{6}$$

The third term, $(ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/dq_{shc})q_{phc}$, captures patients that switch away from PHCs. Improving subcenter quality would increase the market share of the subcenter and decrease the market share of the PHC. Whether the net effect on average healthcare quality is positive or negative depends on the quality difference between the subcenter and the PHC, the distance between the subcenter village and the PHC town, and patient preferences.

Finally, we also consider a scenario in which a private provider is available as well:

$$\begin{aligned}\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} = & \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} q_{shc} + s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} q_{phc} + \\ & \frac{ds_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} q_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{dq_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} s_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta),\end{aligned} \tag{7}$$

where the fourth and fifth term captures changes in average quality through changes in private sector market shares $[(ds_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/dq_{shc})q_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)]$ and through changes in private sector quality, $[(dq_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/\ dq_{shc})s_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)]$. The inclusion of the private sector further complicates the effect of an increase in subcenter quality on average healthcare quality. Increasing subcenter quality could increase private provider quality by creating competitive pressure and reducing market power (McPake and Hanson, 2016). However, the change in subcenter quality could also lead to market segmentation, in which private providers focus on providing lower-quality care to patients who are insensitive to quality and value other provider characteristics. The equilibrium depends on the sensitivity of the market shares with respect to provider quality, which in turn depends on the distribution of patient preferences. Private provider exit could also either improve average quality by forcing more patients to choose subcenters and PHCs or worsen average quality if most of the patients who would have preferred to continue visiting the private provider choose not to seek healthcare at all in the absence of the private provider.

In addition to improving subcenter quality, the CHO postings also increased the person-hours available at the subcenter. We assume that the increase in subcenter person-hours improves access to healthcare services but has no direct effect on health outcomes by itself. Only increasing subcenter person-hours would have the following effect on average healthcare quality:

$$\begin{aligned}\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dh_{shc}} = \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dh_{shc}}q_{shc} + \frac{ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dh_{shc}}q_{phc} + \\ \frac{ds_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dh_{shc}}q_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{dq_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dh_{shc}}s_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta),\end{aligned} \quad (8)$$

Whether an increase in subcenter quality or person-hours has a larger effect on average healthcare quality depends on patient preferences for these two attributes. We further note that, when we only consider an increase in person-hours, infra-marginal patients do not benefit from the reform since the second term in equation (7) disappears.

Taken together, the conceptual framework shows that not just the magnitude but even the direction of posting CHOs to subcenters on health outcomes is ambiguous ex ante. It is also unclear whether poor and non-poor patients benefited more from the reform since this depends on their relative preferences for provider characteristics and differences in where poor and non-poor patients are located. These different forces make it important to study the effects of the CHO postings empirically. To do this, we first examine average treatment effects using reduced-form variation. In the second part, we then take the framework to the data and estimate a model of patient demand that allows us to separately shut down the different forces and also examine differential effects for poor and non-poor patients.

3 Data and Empirical Strategy

In this section, we start by describing our data sources. We then present the empirical strategy and first-stage results.

3.1 Data

We combine large-scale administrative data on the universe of public primary healthcare facilities in Rajasthan with primary survey data on 193 subcenters in Udaipur district.

3.1.1 Administrative Data

Our primary outcomes come from the Pregnancy, Child Tracking and Health Services Management System (PCTS) portal. We use the portal to obtain aggregate information on healthcare services and deaths at the facility-month level from April 2019 until March 2024. The portal contains information on patient visits and the number of deaths across five age categories: infant deaths (<1 year), child deaths (1–4 years), adolescent deaths (5–14 years), adult deaths (15–55 years), and elderly deaths (56+ years). The reporting covers all deaths of residents in the catchment area, even if the death occurred somewhere else (e.g., at a district hospital). We further use the PCTS portal to get data on five maternal and child healthcare indicators.[20] Importantly, PCTS reporting is always done by the ANM, even after a CHO is posted at the subcenter, ruling out that any differences in indicators due the CHOs could be attributed to a change in the reporting person.

We also obtained access to data from the Community Health Integrated Platform (CHIP),

[20] We use the five indicators to generate a maternal and child health services index. The five service indicators are the number of pregnant women with at least 4 prenatal care visits, the number of pregnant women who received 360 calcium tablets, the number of pregnant women who received their first tetanus shots, the number of women getting a postpartum check-up seven days after delivery, and the number of fully vaccinated children (aged 9–11 months)

a healthcare-focused household census of Rajasthan that Khushi Baby, a local NGO, developed, and that is collected through the ASHA workers at subcenters. As the census was implemented, we added questions on the healthcare provider choices for all household members who had at least one symptom in the past 30 days.[21] We obtain information on CHO postings through the Health and Wellness Center Portal and information on 2011 catchment area characteristics through the Socioeconomic High-Resolution Rural-Urban Geographic Platform for India (SHRUG) (Asher et al., 2021). Finally, we received separate information on elderly deaths at the gram panchayat level from April 2021 until March 2023 through the Rajasthan Civil Registration System (Pehchan). These death records are managed by gram panchayats (village councils) and maintained independently from the ANM's PCTS records.

3.1.2 Survey Data

We supplement our analysis of administrative data with two rounds of primary survey data on ANMs, CHOs, private providers, and households that we collected across 193 subcenters in four subdistricts in Udaipur district. We conducted the endline surveys 9-12 months after the CHOs were posted in treated areas.[22] In our provider surveys, we obtained information on facility characteristics, their medical knowledge through two vignettes (child dysentery and adult asthma) and, for private providers, prices.[23] Among subcenters that had at least one private provider in the catchment area at baseline, we conducted a phone survey with 513 households to collect information on health outcomes and healthcare utilization for all household members. Appendix Table A2 shows that we obtained similar baseline and endline completion rates for all surveys across the treatment and control groups.

As part of our endline activities, we further visited each sample subcenter without prior announcement for a full day to measure facility opening rates. During these visits, we also conducted exit surveys with all patients who visited the subcenter on that day to collect information on patient satisfaction and other measures of provider quality. Finally, we implemented endline surveys with physicians at 49 PHCs that are linked to our sample subcen-

[21] This data is only available for the post-periods since we added the provider choice questions in August 2023.

[22] Appendix Figure A1 shows the timing of the different surveys.

[23] Among the private providers we surveyed at endline, we conducted a second follow-up survey over the phone to get repeat measures of the main outcomes.

ters to benchmark the knowledge of CHOs and obtain information on PHC infrastructure. Additional details on each survey component can be found in Appendix C.

3.2 Empirical Strategy

Our empirical strategy exploits where the first batch of CHOs in Rajasthan was posted. We next describe how the assignment decisions were made.

3.2.1 Reform Rollout in Rajasthan

The first batch of CHOs was assigned to subcenters at the end of March 2022.[24] At this point, 6,419 CHOs had been hired and passed the final exam.[25] The government then had to decide to which of the 10,016 eligible subcenters the new providers would be posted.[26] The posting decisions were implemented in two stages. In the first stage, CHOs were asked to rank districts according to their preferences. Assignments were then made based on their exam scores. In the second step, *Chief Medical and Health Officers*, the leading health officials at the district level, were asked to do the assignments within districts. All of the 33 officials in Rajasthan were requested to visit the state headquarters in Jaipur for a day to do the assignments. On that day, the average district official had to allocate 195 CHOs across 304 subcenters, leaving 109 subcenters vacant. The only information that the district officials received was (i) a list with the names and residential addresses of the CHOs and (ii) a list with the subdistrict and village names of the subcenters in the district that had been converted to Health and Wellness Centers. The only instructions given to the officials were to place the CHOs close to their homes and to finish the task by the end of that day.

We conducted qualitative interviews with officials who were involved in the process to understand how the assignments were made in practice. We were told that the district officials had tried to place CHOs within the area of their residence but had not accounted

[24] A second batch of 500 CHOs was posted in late December 2022. We include them in the control group throughout our analysis. This affects 14% of subcenters of our final control group sample.

[25] As a reference point, around 9,015 nurses with the relevant degree graduate from Rajasthan every year. In our survey data, we find that most of the hired CHOs say that they would have otherwise worked in the public or private sector in urban areas. We should thus think of the reform as the creation of new public sector jobs in rural areas.

[26] To be eligible to receive a CHO, a subcenter first needed to be converted to a Health and Wellness Center. Such conversions mostly involved minor improvements to infrastructure. Subdistrict officials were responsible for nominating subcenters for conversion based on fixed set of criteria. See Appendix D for more details on the conversion process.

for the exact distance between each subcenter and a CHO's home or for other subcenter characteristics. Whenever CHOs resided in the district headquarters or came from outside of the district, they were assigned across the entire district with the aim of achieving balance across subdistricts.[27]

Data from an extended survey that we conducted with 243 Community Health Officers in Udaipur corroborate this process. Ninety-eight percent of the CHOs said that they were not involved in assignment decisions within the district. Figure A2 visualizes the assignment process through a map of subdistricts in Udaipur district. The bubbles in Panel A correspond to the previous residence of the CHOs. Forty-three percent of the CHOs came from the district headquarters and 12% came from outside the district. Panel B shows the locations of converted subcenters. To shed light on how the assignments were made, we present three examples in Panels C, D, and E. In Panel C, we observe that the five CHOs who previously resided in Kherwara subdistrict were all assigned to a subcenter near their previous residence in the southwestern part of Udaipur district. However, when comparing the assignments with the list of available subcenters in Panel B, we also see that the CHOs were not necessarily assigned to the subcenter that was located closest to their home. Instead, district officials relied on rules of thumb to make the assignments. Panels D and E further show that CHOs from the district headquarters or from other districts were assigned across all subdistricts. Finally, Panel F shows the final assignment outcomes in all of Udaipur district. We highlight that many subcenters in close vicinity differ in their treatment status, consistent with the idea that many assignments were based on ad-hoc decisions.

3.2.2 Matching and Estimation

Since assignments were based on CHO's preferences for districts and the location of their previous residence, subcenters that received a CHO are more likely to be located in less remote areas, making the parallel trend assumption less likely to hold in the unconditional sample. We address this concern by computing weights for the control group to match subcenters with and without a CHO based on our knowledge of the assignment rule. In particular, we use district fixed effects interacted with linear and squared terms of a facility's

[27] District headquarters are located in urban centers and so were not served by the Health and Wellness reform.

distance to the district and subdistrict headquarters to estimate propensity scores.[28] We then follow Abadie (2005) and use inverse probability weighting to adjust the control group. The intuition is that control group subcenters that were less (more) likely to have been assigned a CHO receive less (more) weight, making the control group more similar to the treatment group.

In our preferred specification, we exclude districts in which more than 90% of subcenters received a CHO to ensure sufficient variation within districts and common support in propensity scores between treatment and control group units. We also exclude the subdistrict nearest to the district headquarters in each district since they were systematically more likely to receive treatment as most CHOs previously lived in the district headquarters.[29] Following the matching literature, we further implement common support restrictions by excluding observations within the top or bottom 2.5 percent of either the control or treatment group propensity score distribution in each district (Appendix Figure A3). In practice, this excludes the most and least remote subcenters from the sample. We show that our results are robust to alternative sample restrictions and matching strategies, including entropy balancing (Hainmueller, 2012) or estimating propensity scores based on LASSO regressions.

Table 1 compares baseline covariates across treatment and control group subcenters in the unconditional and matched samples. Columns 1–4 show that treatment group subcenters are less rural, more literate, and cover a larger population than control group subcenters. However, once we reweight the control group based on the geographical information, we also achieve balance among most non-targeted subcenter characteristics (columns 5–8). The difference in the Scheduled Caste share remains significant but is small in magnitude.

We also note that any systematic variation in *levels* does not undermine the validity of the empirical design. Our primary identifying assumption is that in the absence of the new CHOs, control and treatment group subcenters would have followed the same *trends* in the outcomes of interest. This assumption would only be violated if treatment and control areas had differential trends in time-varying determinants of outcomes. For example, richer and

[28] We do not use the distance to the nearest PHC since the name of the associated PHC was not included in the list of converted subcenters that was given to the district officials during the assignment process. The results are similar if we also use the distance to the PHC in the estimation of the propensity score.

[29] Within a district, subcenters in the subdistrict nearest to the district headquarters are 15% (p-value = 0.000) more likely to receive a CHO than subcenters in other subdistricts.

less remote areas might have experienced a stronger decline in mortality outcomes during our sample period, even in the absence of the treatment. We thus focus our analysis on the reweighted sample that looks more similar on observables in the pre-period. We also use our empirical specification to check for differential pre-trends. The argument is that any changes between treatment and control group subcenters in the post-period are likely to be caused by the treatment if both types of subcenters followed similar trends before the CHOs were posted. As we discuss below, we do not find evidence for pre-trends that would undermine our results.

We aggregate outcomes at the quarterly level since monthly data on deaths is very noisy. For subcenter i in quarter t, we estimate:

$$y_{it} = \alpha + \sum_{k=-8}^{k=-2} \beta_{pre}^{k} 1[D_{bt} = k] \times Treat_i + \sum_{k=0}^{k=7} \beta_{post}^{k} 1[D_{bt} = k] \times Treat_i + \delta_i + \eta_t + \epsilon_{it} \tag{9}$$

where $1[D_{bt} = k]$ is an indicator for k quarters between quarter t and the second quarter in 2022, the quarter the CHOs were posted at the subcenters.[30] δ_i are subcenter fixed effects, which absorb any time-invariant factors like persistent facility characteristics such as infrastructure and local risks of diseases. η_i are quarter fixed effects that absorb common time trends such as seasonal variation in diseases. We cluster our standard errors at the subcenter level to account for serial correlation. To test for pre-trends, we report p-values for the null hypothesis that all pre-period coefficients are statistically equal to zero.[31]

We also run the standard difference-in-differences regression to analyze pooled treatment effects:

$$y_{it} = \alpha + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \delta_i + \eta_t + \epsilon_{it} \tag{10}$$

As a benchmark for the magnitude of the effects, we report the counterfactual treatment group means in the post-periods by subtracting the treatment coefficient from the observed treatment group mean in the post-periods (Basri et al., 2021). We top-code all continuous

[30] The assignment decisions were made at the end of March 2022, and the CHO started to work in the facilities in April 2022.

[31] We also note that, since our main analysis focuses on the first batch of CHO assignments, all our treatment group subcenters were treated at the same time, so recent advances in staggered difference-in-difference methods are not applicable to our research design (Roth et al., 2023).

outcomes at the 99% level to reduce the influence of outliers.[32]

We replicate a similar empirical strategy for the analysis of our survey data. To maximize sample size, we include 71 subcenters that had a government-owned building but had not been converted to a Health and Wellness Center by March 2022. These subcenters were not eligible to receive a CHO when the posting decisions were made, but they could have been eligible had the subdistrict officials chosen these subcenters for conversion first. We adjust the estimation of the propensity scores to be consistent with the criteria that were used to select facilities for conversion.[33] Appendix Table A3 shows that the reweighted survey sample is balanced.

3.3 Fist-Stage Results

We show in Table 2 how the CHO postings affected subcenter inputs. The posting of the CHOs not only doubled the number of skilled workers (Column (1)) but also increased the highest medical qualification available at the subcenter level (Column (2)). Importantly, we observe no differences in other characteristics, including the number of community health workers and the availability of equipment and medicines (Columns (3)–(6)).[34]

We next use data from time-use modules, unannounced visits, and patient exit surveys to provide suggestive evidence on how the CHOs affect subcenter performance. Since we only collected these survey components at endline, these results are not based on difference-in-differences regressions but instead exploit cross-sectional variation based on propensity score matching.[35] Figure A4 presents the results from the time-use module and documents that the CHOs primarily increase the time spent providing outpatient care and screening for chronic diseases. The posting of the CHOs also made the opening hours of the subcenters

[32]We show robustness to alternative top-coding strategies in Appendix Table A12.

[33]Subdistrict officials had to propose a fixed number of subcenters for conversion annually between 2018 and 2022. The minimum criterion for conversion was that the government must own the subcenter building. In some years, priority was further given to subcenters with electricity, running water, and good physical condition. To account for these criteria, we include baseline survey information on the condition of the subcenter building and the availability of electricity and running water in the estimation of the propensity scores for the survey sample.

[34]These surveys were collected before part of the control group also received a CHO. Column (7) pools pre- and post-period means in the administrative data to show that using first batch assignments increases the likelihood of a CHO in a given post-period quarter by 85 percentage points.

[35]We use the same weights for the control group that we use for the matched difference-in-differences regressions. Since matched treatment and control group subcenter look very similar at baseline, substantial differences in subcenter performance at endline are likely related to the posting of CHOs.

more reliable. Using data from the unannounced audit visits, we find that having a CHO increases the likelihood that a subcenters is open at all on a given day from 42% to 69% (Figure 1). Treatment group subcenters are, on average, open for 2.8 more hours and see 51% more patients at the subcenter per day.

The CHOs further increase the quality of healthcare services. Figure 2 plots the checklist completion rate for the two medical vignettes for different providers. Consistent with their differences in medical qualifications, we find that the new CHOs perform better than ANMs and private providers but worse than PHC physicians.[36] Using results from patient exit surveys, we also find that patients at CHO-staffed subcenters report higher levels of satisfaction. In addition, patients at subcenters with CHOs were asked more questions and were more likely to have their blood pressure measured and be referred to a PHC.[37]

4 Results

We next use the administrative data to study the effect of CHOs on patient visits and mortality outcomes.

4.1 Effects on Patient Visits

The top left panel in Figure 3 shows the effects of the CHOs on patient visits over time.[38] While treatment and control group subcenters followed similar trends prior to the reform, we observe a substantial increase in the number of patient visits once the CHOs were posted to treatment group subcenters. In Column (1) of Table 3, we show that the number of patient visits in a quarter increases on average by 216 visits (p-value < 0.001). Relative to a counterfactual treatment group mean of 371 patients per quarter, this represents an increase of 58%.

To examine substitution patterns and understand what patients would do in the absence

[36] We can reject that the knowledge distributions of ANMs and CHOs are the same (p-value $= 0.016$). We find no difference in the knowledge between treatment and control group ANMs.

[37] These differences remain if we try to adjust for patient selection by controlling for symptom fixed effects or restricting the sample to patients who report having visited the subcenter before.

[38] Appendix Figures A6–A9 plot the trends in our main outcomes separately for treatment and control group subcenters and show that the treatment effects are driven by a trend break in treatment group units. Modest improvements in control group outcomes over time can be attributed to control group subcenters that received a CHO in subsequent quarters.

of the reform, we use data from the CHIP household census on provider choices for 26,097 household members who suffered from at least one symptom in the last 30 days across Rajasthan. We find that the share of households who visited the subcenter increased from 34% to 47% in treated areas (Figure 4). We further observe an equivalent decline in the share of respondents who report not seeking any healthcare when sick. These patterns indicate that patients do not substitute away from other existing healthcare facilities but that the CHOs manage to reach patients who were outside of the healthcare system before the reform.

Using data from the PCTS portal, we also examine the types of patients that visit the subcenter. The increase in patient visits does not only include the treatment of minor symptoms like mild coughs and fever, but also the diagnosis and treatment of potentially life-threatening conditions. Column (4) in Table 3 shows that the number of patients with acute heart diseases (like heart attacks) increases by 67%. While the average subcenter only treats 0.036 acute heart disease patients per quarter, providing medical support to such patients could lead to immediate effects on mortality outcomes. CHOs would not be able to perform surgeries, but could, for example, provide aspirin to thin the blood and improve blood flow and then refer patients to higher-level facilities. We also observe increases in the number of epilepsy but not stroke patients (Columns (5)-(6)). From April 2023 onwards, the administrative data further provide additional details on the types of patients that visit subcenters. When comparing post-period means between treatment and control group subcenters in Appendix Table A5, we do not only see increases in patients with general eye and oral diseases, but also in patients with serious conditions, including chronic obstructive pulmonary disease (COPD) and asthma.

In addition to affecting acute healthcare services, the CHOs also improved the provision of preventive healthcare services by screening patients for chronic diseases. The number of hypertension patient visits increases by 72% (p-value < 0.001), and the number of diabetes patients increases by 62% (p-value < 0.001). As mentioned in Section 3.1, these outcomes include newly and previously diagnosed patients. While the increase in patients diagnosed with hypertension and diabetes could theoretically also be driven by an increase in the prevalence of chronic diseases, low awareness rates in the population make it much more likely that the increase in chronic disease patients is due to higher screening rates.[39] We use data from the Health and Wellness Center portal that is only available for treatment group subcenters to get a better understanding of how CHOs affect chronic patients. Appendix Figure A10 shows a clear increase in the number of hypertension and diabetes patients who are screened, newly diagnosed, or treated after the posting of the CHOs.

We also examine the effect of CHOs on an index of five maternal and child health services. The direction of the treatment effect is ambiguous ex ante. Even if CHOs only had limited involvement in maternal and child health services, their presence could still have freed up additional capacity for the ANMs. However, an increased focus on chronic diseases and basic outpatient care could have also led to a neglect of existing services. Overall, we do not observe substantial changes in the provision of maternal and child health services. Appendix Table A6 shows that we also find no improvements when we analyze each index component individually.

4.2 Effects on Mortality Outcomes

We study the effects of CHOs on mortality outcomes by using statewide PCTS portal data on deaths by age group in each subcenter catchment area. In our preferred outcome specification, we examine a binary version of whether a subcenter reports any death in a particular quarter.[40] Panel A in Figure 5 shows the event-study graph for whether any death was reported in the subcenter area. Except for an outlier in the third pre-quarter, we find that the coefficients for the seven pretreatment quarters are neither individually nor jointly significant (p-value = 0.141). However, once the CHOs are posted, we observe a significant decline in the likelihood that any death occurred in the catchment area in a particular quarter for seven out of the eight observed quarters. When examining the effect by age group, we observe that the effect is largely driven by a decline in whether there was an elderly death in the catchment area (Panel B).[41]

While the reform did not explicitly target the elderly, it is not surprising that the effect

[39] Only 37% of hypertensive patients in India are aware of their condition (Amarchand et al., 2022).

[40] The benefit of the binary outcome is that it is less noisy than examining mortality rates. On average, 36% of subcenters report at least one death in a given quarter. Conditional on reporting any death, 34% report one death, 22% report two deaths, and 15% report three deaths.

[41] Appendix Figure A11 shows that we find no effects for other age groups.

is concentrated among this age group since their health outcomes are most likely to be affected by improved access to public primary healthcare services. Data from the 2017–2018 National Sample Survey (NSS) data show that 56% of total deaths occur among the elderly, making it more difficult to be able to observe changes in mortality for other age groups (see Appendix Figure A12 for a distribution of age at death). Appendix Figure A13 further shows the distribution of common causes of death by age group. Younger adults mainly die from injuries that will likely require trauma care services that are only provided by higher-level facilities. By contrast, most of the elderly die due to cardiovascular diseases which could be prevented by earlier diagnosis and treatment options that are available at subcenters.

Table 4 reports aggregate effects by pooling all pre- and post-periods. In addition to the binary outcome, we also report effects on the total number of deaths, mortality rates in levels, and the inverse hyperbolic sine of the mortality rates.[42] We observe significant declines for all of these outcomes, including a 10% decline in all-age mortality rates (p-value = 0.035) in Column (3) of Panel A. Eighty percent of the decline in all-age deaths is attributable to the reduction in elderly deaths (Panel B). We observe no significant effects in the mortality outcomes of other age groups (Panel C).[43] Appendix Table A8 further breaks down the elderly deaths into different causes of death. We observe that the aforementioned declines in deaths can be completely attributed to a decline in deaths from unknown causes, a category which covers 59% of all reported deaths. While the high rate of unknown-cause deaths is a limitation of our data, the decline in this category is also consistent with earlier diagnosis rates for acute and chronic diseases contributing to the observed effects.

A potential concern is that differences in reporting could explain our results. The implied annualized elderly mortality rate of 10.6 deaths per 1,000 elderly individuals in the administrative data is only around 45% of the elderly mortality rate of 23.5 deaths per 1,000 elderly individuals observed in the NSS 2017–2018 survey, suggesting that many deaths remain unreported. However, as mentioned in Section 3.1, the ANM remains the person who fills out the information forms for the PCTS portal. In our time-use module, we also do

[42]The mortality rate is defined as the number of deaths per 1,000 individuals. When reporting results separately by age group, we multiply the total population in the catchment area by the average population share of this age group in the Socio Economic Caste Census in 2011.

[43]Appendix Table A7 splits Panel C into four age groups. We also find no effects when we analyze infant, child, adolescent, and adult mortality, separately.

not find that ANMs at treatment group subcenters spent more days on administrative and reporting tasks. Better reporting would likely also go against us finding declines in mortality since an increase in the quality of reporting should increase the reported number of deaths in treatment areas. A general increase in the quality of reporting should further show up in other outcomes as well, including maternal and child health indicators.

Qualitative surveys with ANMs suggest that one reason why deaths are underreported in the administrative data is that some of them think that they only need to fill in the maternal and child health indicators and leave the adult and elderly death forms blank. In Appendix Table A9, we thus restrict the sample to subcenters that reported at least one elderly death in the pre-periods, a sample for which the control group means are more similar to the elderly mortality rates in the NSS 2017–2018 survey (21.9 relative to 23.5 deaths per 1,000 elderly individuals). Reassuringly we still find significant declines in the mortality outcomes for this subgroup. We also observe no decline in the likelihood of observing any elderly death in the post-period at all, addressing concerns that the CHOs might have encouraged ANMs to stop reporting such deaths completely.

As an additional check, we obtained access to another administrative dataset, the Civil Registration System, that separately maintains death records at the gram panchayat level. Since some gram panchayats have more than one subcenter in their area, we redefine the treatment assignment as having posted a CHO to at least half of the subcenters in the gram panchayat area. While this leads to noisier results, we still observe a significant decline in elderly deaths in this dataset (Column (5) in Appendix Table A9).

Finally, we also examine effects on patient outcomes using data from the two rounds of household surveys we conducted. Since our household survey sample is too small to detect changes in mortality outcomes, we instead focus on the incidence of health symptoms and medical spending in the past 30 days as well as hospitalizations in the past six months. While we observe no effects on the incidence of symptoms (Column (1)) and overall medical spending (Column (2)), we find a decline in hospitalizations by 1.7 percentage points (p-value = 0.030, Column (3)). Consistent with the previous results, this effect is also driven by a decline in hospitalizations among the elderly (Appendix Table A10).

4.3 Robustness

We implement various robustness checks for our results. We find similar treatment effects if we use alternative common support restrictions (Appendix Table A11) and top-coding strategies (Appendix Table A12). A potential concern is that the district officials also used additional information besides the geographical location of the subcenter to assign CHOs. Panel A in Appendix Table A13 estimates propensity scores using a LASSO regression based on all variables listed in Table 1 instead. Even if district officials only used geographical information, it is also possible that the functional form of the propensity score function is incorrectly specified. We address this concern by replicating our analysis with entropy balancing weights (Hainmueller, 2012). This method chooses the set of control group weights that minimally deviate from uniform weights while matching a specific set of moments between the treatment and control groups.[44] The decline in all-age mortality outcomes becomes insignificant with entropy balancing, but we find similar effects on elderly mortality outcomes in both instances. As shown in Appendix Table A14, our results on elderly mortality are also robust to including subcenter-specific linear time trends in the regression and to using the double-robust difference-in-differences estimator proposed by Sant'Anna and Zhao (2020). Our results are furthermore robust to removing the Covid-affected quarters from our sample period or accounting for the 14% of control group subcenters that also received a CHO at a later date.

Another concern relates to spillover effects. The average control group subcenter is 6.6 kilometers away from the nearest treatment group subcenter. In practice, we do not observe that any household members in our survey data ever visit a different subcenter besides the one in their catchment area. The likely reason is that patients prefer to go directly to the nearest physician-staffed public clinic (PHC), which is, on average, 7.3 kilometers away. We further note that the existence of spillover effects would likely lead us to underestimate the treatment effects since control group households would also experience the benefits of improved access to health care.

We also conduct an alternative empirical strategy in which we match each treatment

[44] Following our information on the assignment rules, we use the average distance to the district and subdistrict headquarters and the average share of subcenters in each district as our matching moments.

group subcenter to the closest control group subcenter.[45] The intuition is that subcenters in the same geographical vicinity should follow the same trends in health outcomes in the absence of the CHO postings. Appendix Table A15 shows that find similar treatment effects if we use this approach instead.

5 Mechanisms & Counterfactual Policies

In this section, we first examine which mechanisms can explain the decline in mortality rates. We start by showing reduced-form evidence based on heterogeneity analysis and private provider surveys. We then combine the administrative and survey data to estimate a discrete choice model of patient demand to quantify the impact of each channel and evaluate counterfactual policies.

5.1 Reduced-Form Evidence

Our conceptual framework suggests that three channels might have contributed to the decline in mortality outcomes: (i) better access to subcenter services, (ii) higher provider quality at subcenters, and (iii) changes to private sector behavior.

5.1.1 Improvements in Access vs. Quality

How important are the first two channels? While our policy variation does not allow us to directly distinguish between them using reduced-form evidence, we take a step in that direction by analyzing whether the treatment effects vary based on the quality difference between the ANM and the CHO. For that, we generate a dummy variable for whether the new CHO has a higher average checklist completion rate in the medical vignettes than the ANM at the same subcenter at baseline. Consistent with improving quality being an important channel, we only observe a decline in mortality outcomes in our survey sample when the CHO has higher levels of medical knowledge than ANMs (Appendix Table A16). In a similar exercise, we also split the sample based on the increase in subcenter person-hours in Appendix Table A17.[46] Besides the posting of CHOs, subcenter person-hours vary based on whether an ANM lives in a village and whether an ANM has to take care of two

[45] We implement matching with replacement, allowing each treatment group subcenter to be matched to more than one control group subcenter.

[46] Appendix Figure A14 shows the distribution of subcenter person-hours at baseline.

subcenters at the same time due to vacancies. In this heterogeneity analysis, we find that a larger increase in subcenter person-hours also leads to a larger increase in patient visits but not to more improvements in health outcomes. Taken together, these findings suggest that the increase in healthcare quality and not the increase in healthcare access is the main driver of our results.

5.1.2 Effects on Private Provider Behavior

We next investigate how the private sector responded to the reform. A recent set of studies has documented the existence of multiplier effects in education, where private schools react to increased competition from public schools by increasing their quality (Andrabi et al., 2024; Dinerstein et al., 2023).[47] However, other work has also shown that increased competition in healthcare markets could increase the adoption of potentially harmful practices that are demanded by patients, including the overuse of antibiotics and opioids (Bennett et al., 2015; Currie et al., 2023). More broadly, increased competition could also hurt some patients by leading to private provider exit (Dinerstein and Smith, 2021) or higher private sector prices through market segmentation (Atal et al., 2024).

We find no treatment effects on the total number of providers in the market (Column (1), Table 6). Instead, we thus focus on analyzing treatment effects on provider attributes in Columns (2)–(4). While we do not observe any differences in the number of patients or prices (Columns (1)–(2)), we find that the posting of the CHOs increases the quality index of private providers by 0.26 standard deviations (p-value = 0.016, Column (3)).[48] Consistent with a decline in local market power, these improvements are concentrated among providers that are located closest to the subcenter (Panel B) and that are the only private provider in the subcenter catchment area at baseline (Appendix Table A19). We observe no differences in the share of patients who received antibiotics or injections (Columns (4)–(5), Appendix

[47] In healthcare, Bennett and Yin (2019) further show the introduction of chain pharmacies in India improved drug quality among incumbents.

[48] The quality index components include the length of the medical degree in years, the number of training workshops attended, and the average checklist completion rate across both vignettes. Treatment effects on the length of the medical degree are driven by a 16 percentage point increase in the share of private providers that are currently enrolled in a degree program (p-value = 0.019). Whenever a provider is enrolled a degree program at the point of the survey, we use the expected length of the medical degree based on the assumption that the provider will finish the current program. Effects on the separate quality index components are reported in Appendix Table A18.

Table A18), providing evidence against concerns that potentially harmful behavior could increase with competition (Bennett et al., 2015).

Overall, these results suggest that the CHO postings also improved private sector quality, potentially multiplying the effect on patient outcomes. More broadly, our findings also contribute to the debate on the role of patient demand and provider financial incentives (Currie et al., 2014; Lopez et al., 2022) by providing direct evidence that local market power leads private providers to underinvest in quality.

5.2 Demand Model

To quantify the effect of each channel in more detail, including the importance of the private sector responses, we require additional structure. In the remaining part of this section, we thus estimate a structural model of patient demand that will allow us to use counterfactual simulations to shut down each of the three channels separately. The results will not only decompose the effects but also directly inform optimal staffing policies, including, for example, whether it would have been better to post a second ANM instead of a more highly qualified CHO, a variation of the policy that has been implemented in the state of Tamil Nadu (Muraleedharan et al., 2018). The model further allows us to predict how the CHO postings have differential effects for poor and non-poor patients. We also evaluate the effect of a ban on private providers to document the value of the private sector before and after the reform.[49] Finally, we examine how the marginal effect of CHOs differs across locations and how much health outcomes could have improved if assignments had taken into account local market conditions.

5.2.1 Mapping the Model to the Data

We expand the conceptual framework described in Section 2.4 to take the model to the data. We use the 46 PHC catchment areas in our survey sample to define markets.[50] Each

[49] Carneiro et al. (2024) evaluate a similar counterfactual to estimate the value of private schools in Pakistan. While a ban on private providers would mechanically reduce patient welfare in the demand model, the predicted effects on average quality and all-age mortality are ambiguous since the direction of the effect depends on two countervailing forces. Some patients would benefit since they would start to go to better public providers, but other patients would be worse off since they would not seek healthcare at all (Godlonton and Okeke, 2016).

[50] Patients rarely visit a provider outside of the PHC catchment area for outpatient care. In the household census data, only 4% of patients in our sample area report visiting another public provider besides the

market is observed at baseline and endline. The sizes of locations in a market are based on population shares in the 2011 population census and imputed poverty shares for each location are retrieved from the SHRUG data. We assume that 20% of the population is suffering from at least one symptom in a given month.[51] We further assume that patients can only choose one provider, that there are no referrals, and that providers do not face capacity constraints.[52]

We combine our administrative and survey data on patient visits for public and private providers to create market shares. Provider characteristics are obtained from survey data. The main provider characteristics are their location, person-hours (h_{jt}), quality (q_{jt}), and price (p_{jt}). We define quality (q_{jt}) as the length of the highest medical degree of all healthcare workers working for a particular facility and person-hours (h_{jt}) as the sum of total hours worked by all healthcare workers in a facility in a typical week. Prices (p_{jt}) for private providers are obtained by asking them about their typical fee, including medicine and consultation fees.

Additional provider characteristics (x_{jt}) include electricity availability and dummy variables for provider type (subcenter, PHC, or private). We also include a provider-specific term that affects a patient's utility but is unobservable to the econometrician (ξ_{jt}). This term includes provider attributes like the provider's attitude towards patients. We allow patient preferences over distance, quality, and price to differ by poverty status. We further include the vector ν_i to allow preferences for prices to be heterogeneous across an unobserved patient characteristic. Patients also have random preference shocks for providers (ϵ_{ij}).

The revised utility function is

$$u_{ijt} = \beta_i^q q_{jt} + \beta^h h_{jt} - \alpha_i p_{jt} - \lambda_i d_{ijt} + \beta x_{jt} + \xi_{jt} + \epsilon_{ijt}, \tag{11}$$

subcenter and PHC or visiting a private provider in another town.

[51] This assumption is informed by our own data and nationally representative household surveys. For example, 20% of rural households in the Indian Human Development Survey 2012 report being sick for at least one day in the past month. We also assume that the likelihood of getting sick is the same for poor and non-poor patients. This is supported by our survey data, where we find that, within the same village, the share of household members who had any symptoms in the past 30 days does not differ by poverty status.

[52] Abstracting from capacity constraints is reasonable in our context since the average provider only treats 2.3 patients per person-hour, indicating that most providers have excess capacity. Less than one percent of providers treat more than 20 patients per person-hour.

with $\beta_i^q = \bar{\beta}^q + \beta_1^q poor_i$, $\alpha_i = \bar{\alpha} + \alpha_1 poor_i + \nu_i$, $\nu_i \sim^{iid} \mathcal{N}(0, \sigma^2)$, and $\lambda_i = \bar{\lambda} + \lambda_1 poor_i$, where $poor_i$ is an indicator variable for whether the patient comes from a poor household.[53] Based on these changes, the share of non-poor patients who live in location l in period t and select provider j becomes

$$s^l_{j,t,nonpoor}(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) = \int_\nu \left(\frac{exp(\bar{\beta}^q q_{jt} + \bar{\beta}^h h_{jt} - \bar{\alpha} p_{jt} - \nu p_{jt} - \bar{\lambda} d_{ijt} + \beta x_{jt} + \xi_{jt})}{\sum_n^{J_t^m} exp(\bar{\beta}^q q_{nt} + \bar{\beta}^h h_{nt} - \bar{\alpha} p_{nt} - \nu p_{nt} - \bar{\lambda} d_{nl} + \beta x_{nt} + \xi_{nt}) + 1} \right) d\nu. \quad (12)$$

For modeling supply-side responses, we take a reduced-form approach by using our estimated treatment effects on private provider quality. We focus on the treatment effects on medical degree length to have a consistent quality measure for public and private providers. We use separate treatment effects based on the number of private providers in a specific location to proxy for differences in the baseline market power of private providers based on estimates from Column (4) in Appendix Table A19.[54]

To translate predicted choice outcomes into mortality effects, we assume that there is a structural relationship π between the elderly mortality rate and the unconditional mean quality in each location, where we define unconditional mean quality as the average weighted quality of care received in the location, taking $q = 0$ if the patient chooses the outside option. We then recover π in two steps. First, we use the CHIP data to calculate the difference in unconditional mean quality between treatment and control areas across the entire state. Second, we use our difference-in-differences estimates for the treatment effects on all-age mortality. The combined estimates imply that an increase in unconditional mean quality by 0.381 points results in a 10% decline in all-age mortality.

[53] We focus on modeling heterogeneity in patient preferences across three provider characteristics since we use three micro-moments from the household census data. We found little differences in patient preferences for person-hours when estimating an extended model with additional interaction terms.

[54] An alternative approach would be to use the first-order conditions of private providers to back out marginal costs and simulate equilibrium responses. While this would allow for a richer model or private sector behavior based on changes in local market power, it would require strong assumptions on the profit maximization behavior of private providers who might also have an altruistic motive. Since one of the primary objective of our model is to decompose mechanisms, we consider the reduced-form approach to be sufficient for our setting.

5.2.2 Estimation and Identification

We follow Berry et al. (2004) and estimate the demand parameters using simulated methods of moments. The aggregate moments match the model's market share prediction for each provider ($s_{j,t}(\theta)$) to those in the data ($s_{j,t}$):

$$s_{j,t} - s_{j,t}(\theta) = 0 \tag{13}$$

Since provider prices likely depend on the unobserved provider term ξ_{jt}, e.g., because private providers with better attitudes towards patients can charge more, we need an instrument for the price. To construct instruments, we use survey data on the source of medicines for each private provider based on the idea that variations in medicine costs across supplier locations are not correlated with the unobserved provider term ξ_{jt}. Appendix Table A20 shows that supplier location fixed effects jointly predict prices (F = 95.23). Another concern is that private providers might strategically choose their quality and person-hours as well which thus might also depend on ξ_{jt}. For this reason, we define instruments that only use subcenter quality and person-hours based on the intuition that these provider characteristics are determined by higher-level authorities in the public sector and are thus not correlated with ξ_{jt}. For each instrument (z_{jt}), we define a second set of moments based on the set of orthogonality conditions:

$$\mathbb{E}[\xi_{jt}(\theta) z_{jt}] = 0 \tag{14}$$

For the micro-moments, we use information on individual-level choices for 771 household members in the post-period from the CHIP household census data. We construct an asset index to classify households in the CHIP data as poor or non-poor.[55] We ask the model to match the poverty share among patients who visit the subcenter, PHC, and private providers in the CHIP data as well as the share of patients who live in the PHC location among patients who visit the PHC:

$$\mathbb{E}[poor_i | \{i \text{ chooses a subcenter}\}] = 0 \tag{15}$$

[55] The resulting village-level poverty shares in the CHIP data are closely correlated with the imputed poverty shares in the SHRUG data (Appendix Figure A15).

$$\mathbb{E}[poor_i|\{i \text{ chooses a PHC}\}] = 0 \tag{16}$$

$$\mathbb{E}[poor_i|\{i \text{ chooses a private provider}\}] = 0 \tag{17}$$

$$\mathbb{E}[lives\_in\_PHC\_location_i|\{i \text{ chooses a PHC}\}] = 0 \tag{18}$$

Finally, we use the covariance in provider prices between the first- and second-order choices in our household survey data:[56]

$$\mathbb{C}(x_j, x_{k(-j)}|j, k \neq 0) \tag{19}$$

The intuition behind the estimation strategy is that we will find a vector of parameters that match the observed and predicted aggregate market shares for each provider, while also trying to meet the orthogonality conditions, the average patient type for each provider category, and the covariance in prices between first- and second-order choices. Using the nested fixed algorithm, we recover mean utilities for each guess of the non-linear parameters. We can then recover the linear preference parameters by running an IV regression of the recovered mean utilities against the provider characteristics. Estimates are obtained using the optimal two-step weighting matrix.

Identification is based on multiple sources of variation. We assume that cost differences across supplier locations affect the pricing decision of private providers and that a private provider's choice of supplier location is not correlated with unobserved provider attributes. We also assume that variation in person-hours and quality at subcenters is unrelated to other unobserved provider attributes that are relevant for patients. We already showed in our previous analysis that the posting of CHOs generated variation in person-hours and quality at subcenters. In addition, person-hours at subcenters vary based on whether the ANMs or CHOs live in a village, a decision which is often related to personal circumstances, including whether they have children of school-going age (Mohan et al., 2003). [57] Some ANMs are also assigned to multiple subcenters at the same time due to unfilled vacancies,

[56] We asked respondents to which provider they would go first if they were to suffer from a mild or moderate symptom (like a cough or mild fever) and to which provider they would go if their first choice was closed.

[57] In support of this argument, we do not find that catchment area characteristics can significantly predict subcenter person-hours at baseline.

generating additional variation in person-hours across facilities. We further take the number of public and private providers across locations as exogeneous, which allows us to compare differences in choices depending on provider availability.

Our micro-moments help to identify how preferences differ by poverty status and location. Information on second choices further pin down the unobserved preference heterogeneity since, given mean choice probabilities, a higher variance of ν_i should lead to a higher covariance in prices across first and second choices (Berry et al., 2004).

5.2.3 Estimates

We report the estimated demand parameters in Table 7. As we would expect, patients like quality and person-hours and dislike prices and distance. We also find that poor patients are more sensitive to prices and distance than non-poor patients. The implied magnitude of the estimate appears reasonable, with non-poor households being indifferent between paying INR 100 or traveling 2.51 kilometers more.

Appendix Table A21 shows that the model fits the data well, as the poverty shares among patients who visit subcenters, PHCs, and private providers in the model are very similar to what we observe in the data. We further assess the performance of the model by plotting changes in subcenter market shares between our baseline and endline surveys in the treatment group against the treatment effects predicted by our model. While the data is noisy, we find that the predicted and observed values tend to be strongly correlated, suggesting that the model is doing well in capturing patient choices. As an out-of-sample test, we also plot our estimates of the unobserved provider term ξ_{jt} against a subjective assessment of providers by our surveyors that is not included in the model. Appendix Table A17 shows that we find a significantly positive relationship between both variables, further increasing our confidence in the model results.

5.2.4 Counterfactuals

We start by considering four counterfactuals in which we separately change subcenter person-hours, subcenter quality, and private provider quality to decompose the effects. In these counterfactuals, we treat all subcenters in our sample area and compare changes to outcomes relative to the baseline scenario in which only one ANM is working at each subcenter The first counterfactual models the full treatment effects of the CHO postings, in which CHOs are posted alongside the existing ANMs, leading to an increase in subcenter quality and subcenter person-hours as well as to an increase in private provider quality. In the second counterfactual, we evaluate what would happen if we only increased subcenter quality, akin to a policy in which we would replace the existing ANMs with the new CHOs instead of posting CHOs alongside the ANMs. In the third counterfactual, we only increase subcenter person-hours, which would be equivalent to a reform that would post a second ANM instead of a more qualified CHO. The fourth counterfactual accounts for the increases in subcenter quality and subcenter person-hours but assumes that private providers did not change in response to the reform.

Table 8 starts by showing the baseline scenario without CHOs being posted to any subcenter (Row (0)). In this case, the average subcenter market share is 9.6% (Column (1)). This goes up to 27.6% if we evaluate the full treatment effect in which CHOs are posted to all subcenters and private providers respond by increasing their quality (Row (2)). Only improving subcenter quality or person-hours would increase their market shares to 16.4% and 18.2%, respectively. Had private providers kept the initial quality, subcenter market shares would have gone up to 28.4% with the posting of CHOs.

Column (4) reports changes in average quality and Column (5) reports predicted changes in all-age mortality rates. We find that the full treatment increases the average medical degree length of the chosen healthcare providers from 1.96 years to 2.38 years, leading to a decline in all-age mortality by 10.9%. Improving subcenter quality alone would achieve approximately half of this effect (5.1%), while solely improving subcenter person-hours would only all-age mortality by 1.2%. The small effect size for the last counterfactual has two reasons. First, based on our specification of the health production function, infra-marginal patients who anyway would have visited the subcenter do not benefit from increased person-hours. Second, we find that the substitution effects offset each other. While some of the marginal patients would not otherwise see any healthcare providers, other patients would have instead visited higher-quality PHCs. Row (4) shows that, if subcenter quality and person-hours are improved simultaneously but private providers do not change their quality, all-age mortality would decline by 9.7%, suggesting that ten percent of the decline in all-age mortality rates can be attributed to private sector responses.

Across the different counterfactuals, we estimate similar changes in mortality rates for poor and non-poor patients (Appendix Table A22). An important difference is that poor patients mostly benefit from the direct improvements in public healthcare provision, while the indirect effects on the private sector play a more important role for non-poor patients.

We evaluate the ban on private providers in Rows (5) and (6). We find that average mortality would slightly increase in these cases as worse health outcomes for patients who would stop seeking healthcare at all dominate improvements in health outcomes for patients who would instead visit a subcenter or PHC. The negative effect of the ban on private providers holds even after the posting the CHOs, suggesting that private providers continue to play a relevant role in the healthcare sector.

Posting CHOs to subcenters has a large effect on health outcomes on average, but there is substantial heterogeneity in the marginal effect of posting CHOs to individual subcenters. In the top plot in Figure 6, we rank subcenters according to how CHO postings would change mortality rates based on counterfactual simulations. For four percent of subcenters, we even find that posting CHOs would lead to worse health outcomes since the substitution effect from patients switching away from higher quality providers would dominate. These findings already suggest that the current policy in which all subcenters are supposed to receive a CHO at some point is not optimal.

Determining the optimal number of CHOs without further information is difficult since it depends on government budget constraints and the value the government assigns to the number of live years saved. However, we can instead investigate by how much a optimal reallocation of providers could improve health outcomes given a fixed number of CHOs. To do that, we consider the first batch of CHOs that the government had posted to our sample area in March 2022.[58] Another complication is that a complete exercise would also account for differences in CHO preferences since reallocating CHOs to remote areas might lead to an increase in requested transfers and staff turnover. Since we do not have data on CHO preferences, we instead try to reallocate CHOs within the same market based on the assumption that CHOs are mostly indifferent between being assigned to different locations within the

[58] 51% of subcenters in our sample area received a CHO in March 2022.

same geographic area.[59] Rows (8), (9), and (10) report the predicted changes in market-level outcomes based on the observed government assignment, the random assignment, and the optimal assignment, respectively.[60] By comparing the predicted mortality outcomes in Column (5), we find the government allocation is very similar to random assignment, whereas the optimal assignment rule, which accounts for local market conditions, would achieve an 18% larger reduction in mortality outcomes relative to the existing government allocation.

6 Discussion

In the final part of the paper, we discuss the plausibility of the size of the treatment effects, alternative mechanisms, and the cost-effectiveness of the reform.

6.1 Plausibility of Treatment Effects

An important question is whether the size of the effects is reasonable. The relevance of better access to basic healthcare services for improving mortality outcomes is supported by NSS 2017–2018 survey data: 38% of deceased household members did not receive any medical attention before their death, suggesting that even small changes to healthcare services could lead to substantial improvements in health outcomes. Another way to address the concern is to look at previous studies that examined the effect of improved public healthcare on short-run health outcomes. Past research has shown that adding a physician to a public facility or improving community monitoring can reduce infant and child mortality by 20%–33% within one year of an intervention (Björkman and Svensson, 2009; Okeke, 2023).

Studies on infant and child mortality, however, might be less informative since the health outcomes of new infants are likely to be more sensitive to targeted improvements to public healthcare than the health outcomes of the elderly. While there is less evidence on the short-run effects of public health interventions on adult mortality, existing work also documents

[59] The exercise provides a lower bound of the benefits of reallocating providers since reallocating providers across markets could lead to an even larger reduction in mortality outcomes. Our assumption that CHOs are indifferent between subcenters in the same area is supported by the observation that 71% of CHOs in our sample do not move to the village to which they are assigned and instead live in larger towns and commute daily to the subcenter.

[60] The random and optimal allocation takes the number of CHOs assigned to a given market as fixed. The optimal assignment rule maximizes the reduction in the mortality rate. For the random assignment, we take 100 random draws and then report average outcomes across them.

the potential for declines in adult mortality outcomes within a short time frame.[61] Bailey and Goodman-Bacon (2015) focus on the long-run effects of community health centers in the US, but their event study graphs suggest that adult mortality already declined sharply within the first year of treatment. Besides improvements to primary care, previous work has shown how increased access to healthcare insurance can lead to a decline in adult mortality. Gruber et al. (2023) report that a healthcare insurance expansion in China decreased adult mortality by 12%, with some of these effects already occurring within the first year. Sood et al. (2014) further find that healthcare insurance in India reduced mortality from conditions covered by the scheme by 64% within two years.[62]

The size of the treatment effects also appears large due to the definition of the mortality outcome. To illustrate this, consider an intervention that would increase life expectancy for everyone in the treatment group by two years. In that case, the number of observed deaths within two years after the intervention would decline by 100% since nobody would die in the treatment group during the observed time frame. However, in the following years, the individuals who initially survived would start to die and the number of deaths in each period between the treatment and control groups would be the same (assuming that all cohort sizes are the same). Following a similar intuition, an CHO-induced increase in life expectancy by 2.4 months would be sufficient to generate the decline in all-age mortality rates that we observe in the administrative data.

The effects of CHOs on mortality outcomes could either be attributed to higher screening rates for chronic diseases or better management of outpatient care patients. In practice, both of these services are interlinked, since many patients who visit subcenters for outpatient care are automatically checked for common chronic diseases like hypertension, a practice

[61] Previous studies also study the effect of non-medical interventions like pension payments and cash transfers on mortality outcomes and find mixed results (Barham and Rowberry, 2013; Huang and Zhang, 2021; Malavasi and Ye, 2024; Jensen and Richter, 2004; Snyder and Evans, 2006). Among those who find positive effects, Barham and Rowberry (2013) show that a conditional cash transfer in Mexico reduced municipal-level elderly (65+ years) mortality rates by 4% and Huang and Zhang (2021) show that a pension program in China reduced mortality rates by 12%.

[62] Existing research on the effect of chronic disease screenings on mortality outcomes also mostly focuses on longer-run outcomes. Among studies that find short-run effects, Lin et al. (2004) show that a hypertension mass campaign led to a substantial decline in stroke mortality within a year in Taiwan, and Hickey et al. (2021) find that patient-centered hypertension care reduced all-cause mortality among adults with uncontrolled hypertension by 21% within three years in rural Kenya and Uganda.

known as 'opportunistic screening'. While our data does not allow us to directly identify which services prevented specific deaths, we can use our estimates to get a general sense of their relative importance. A back-of-the-envelope calculation that combines the treatment effects on hypertension and diabetes patients with estimates from the medical literature and nationally representative household surveys suggests that around 13% of the reduction in all-age mortality can be associated with better screening for chronic diseases (see Appendix E for details). The increase in acute heart disease and epilepsy patients could together explain 35% of the observed decline in mortality rates, while the remaining 52% can be attributed to the earlier diagnosis and treatment of other medical conditions.[63]

6.2 Alternative Mechanisms

Our analysis so far highlighted changes in subcenter quality and person-hours as well as in private provider quality as the main channels through which the CHO postings improved health outcomes. Another potential mechanism is that the CHOs increased the presence of male primary healthcare workers at subcenters. While all of the existing ANMs are female, 64% of the new CHOs are male. This could especially benefit male patients who might feel uncomfortable visiting the ANM. Male providers might also be seen as more competent which could encourage an increase in overall take-up of healthcare services. We explore the importance of this channel in Appendix Table A23 by studying heterogeneity in treatment effects by CHO gender on average subcenter outcomes. While the coefficients tend to be larger for CHOs, we cannot reject equality for six of the eight outcomes.[64] Unfortunately, the PCTS data does not allow us to analyze gender-specific mortality rates and our household survey data are too noisy to split by CHO and patient gender. We do, however, have gender-disaggregated data on the number of patients for treatment group subcenters from the Health and Wellness Center Portal. When we use this data, we find that, while male CHOs tend to increase the share of male patients visiting the subcenters, the relative differences are small in magnitude. Overall, this suggests that an increased presence of male primary healthcare workers at subcenters is not the main explanation for our results.

[63]The calculated contribution of the increase in acute heart disease and epilepsy patients assumes that these patients would die otherwise and should thus be seen as an upper bound.

[64]The only exceptions for which the differences in treatment effects are marginally significant are the effects on acute heart disease patients and the inverse hyperbolic sine of the elderly mortality rate.

It could also have been possible that the CHOs could have improved health outcomes by encouraging patients to enroll in government health insurance. However, we observe no significant increase in the likelihood that the household is covered by healthcare insurance in our household survey data. An increase in health insurance take-up should have also led to an increase in hospitalizations, whereas we observe the opposite.

6.3 Cost-Effectiveness Analysis

In the final part of the paper, we assess the cost-effectiveness of the CHO postings. We first use our main reduced-form estimates to examine how the cost-effectiveness of the reform varies across different assumptions and then use the estimates from the structural model to investigate the cost-effectiveness of the alternative policies.

For government costs, we consider increased salary and drug expenses. CHOs are paid USD 480 per month (including performance-based incentives). Since medicine is provided for free at public facilities, we also account for higher public spending on medicines. We assume that the average medicine cost per patient visit is USD 0.24. When assessing government benefits, we account for future reductions in government spending due to decreased hospitalizations. We use estimates from Garg et al. (2022) who calculate that average public spending per hospitalization episode is equal to USD 91.17. For private benefits, we consider the decline in all-age mortality as well as decreased out-of-pocket spending for hospitalizations. We follow Hendren and Sprung-Keyser (2020) and use USD 100,000 as the value of a statistical life year. For hospitalizations, we again use estimates from Garg et al. (2022) who calculate that average private spending per hospitalization episode is equal to USD 185.10.[65]

In our preferred calculation, we estimate a marginal value of public funds of 45. In other words, the posting of the CHOs generates 45 dollars in private benefits for every government dollar spent. We estimate that the cost per life-year saved is equal to USD 2,782. This corresponds to 1.2 times the GDP per capita of India and is below the benchmark of the World Health Organization, which classifies interventions as cost-effective if the costs per life-year saved are less than three times the GDP per capita of the country (Patenaude

[65] We assume that the treatment effect on hospitalizations reflects a persistent decline and not just a delay. In Appendix F, we also report alternative estimates in which we assume that the CHOs only postpone when the hospitalizations occur.

et al., 2019).

We also report cost-effectiveness results based on alternative assumptions in Appendix Table A24. We calculate a marginal value of public funds of 9 if we ignore the effects on mortality and use zero as the value of a statistical life year. Without accounting for the decrease in hospitalizations, the marginal value of public funds is 7 and costs per life-year saved are equal to USD 14,643.[66] If we assume that the decline in hospitalizations continues until the second year, we would even predict that the reform would pay for itself as the savings from reduced public spending on hospitalizations would be larger than the government's total costs.

In Appendix Table A25, we further use the estimates from the demand model to assess how the cost-effectiveness of the alternative policies in which we either post a second ANM or replace the existing ANM with the new CHO. While we have already shown that posting a CHO alongside the existing ANM leads to the largest declines in mortality rates, the other policies would be cheaper and could thus still be more cost effective. For this exercise, we ignore the effects on hospitalizations and drug expenses and use the estimated changes in mortality rates in Column (5) in Table 8 to examine differences in costs per life-year saved. We find that posting the CHO alongside the existing ANM is also the most cost-effective policy, followed by replacing the ANM with a CHO.[67] Ignoring the private sector response would increase the costs per life year saved from USD $10,590$ to USD $11,720$ per life-year saved.

As discussed in section 5.6, however, these average estimates hide substantial heterogeneity in the marginal effect of posting CHOs. The bottom plot in Figure 6 shows that, among subcenters for which we predict that posting a CHO would lead to a decline in mortality rates, estimates for the costs per life-year saved range from USD 3,212 to 397,030 (ignoring changes in hospitalizations and drug costs). The model estimates also suggest that the reallocation of the existing CHOs would decrease the costs per life-year saved from USD $11,407$

[66] By comparison, Bailey and Goodman-Bacon (2015) estimate that Community Health Centers in the US cost USD 68,580 per life-year saved (after deflating their estimates to 2022 dollars). Medicaid costs between USD 204,470 and 582,930 per life-year saved (Chay et al., 2012). Other work also studies the effects of pension payments and other cash transfers on mortality outcomes and finds mixed results

[67] We do not account for potential private sector responses to only improving subcenter quality or only improving subcenter person-hours. However, it is likely that the quality improvements in the private sector in response to such policies would be weaker to the ones that we observed for the full treatment.

to USD $9,648$ while keeping the budget fixed.

7 Conclusion

In this paper, we examine how strengthening public healthcare provision through mid-level providers affects service provision, private provider behavior, and health outcomes. By exploiting variation in the rollout of the reform, we show that the labor inputs have substantial effects on patient visits and mortality outcomes. Private providers react to the reduction in market power by investing in quality upgrades. Using a structural model of patient demand, we decompose the different mechanisms and highlight how a reallocation of new mid-level providers could have achieved an even greater reduction in mortality outcomes.

Our findings imply that policies that use mid-level providers to strengthen public healthcare provision can be low-hanging fruits to improve health outcomes, especially for governments in low- and middle-income countries that face tight health budgets. Similar healthcare cadres of mid-level providers are already used in 37 countries in Africa and Asia (Desai et al., 2020), making the results also relevant outside of India. A potential barrier to replicating the success of the reform in other settings is that initial investments in the healthcare systems might be necessary before labor inputs can be effective. We find suggestive evidence for the importance of complementary investments in physical infrastructure in our survey sample, where we only observe a decline in mortality outcomes if the subcenter has electricity. Similar increases in patient visits suggest that patients do not avoid facilities with bad infrastructure. Instead, our patient exit surveys indicate that ANMs and CHOs at subcenters without electricity exert less effort when examining patients. Future research should examine such complementarities in more detail to inform optimal policy design.

References

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies 72*(1), 1–19.

Allende, C. (2021). Competition under social interactions and the design of education policies. Working Paper.

Amarchand, R., V. Kulothungan, A. Krishnan, and P. Mathur (2022). Hypertension treatment cascade in India: results from National Noncommunicable Disease Monitoring Survey. *Journal of Human Hypertension*, 1–11.

Andrabi, T., N. Bau, J. Das, N. Karachiwalla, and A. Ijaz Khwaja (2024). Crowding in private quality: The equilibrium effects of public spending in education. *The Quarterly Journal of Economics*, qjae014.

Asher, S., T. Lunt, R. Matsuura, and P. Novosad (2021). Development research at high geographic resolution: an analysis of night-lights, firms, and poverty in india using the shrug open data platform. *The World Bank Economic Review 35*(4), 845–871.

Atal, J. P., J. I. Cuesta, F. González, and C. Otero (2024, March). The economics of the public option: Evidence from local pharmaceutical markets. *American Economic Review 114*(3), 615–44.

Bai, L., B. Handel, E. Miguel, and G. Rao (2021). Self-control and demand for preventive health: Evidence from hypertension in India. *Review of Economics and Statistics 103*(5), 835–856.

Bailey, M. J. and A. Goodman-Bacon (2015). The War on Poverty's experiment in public medicine: Community health centers and the mortality of older Americans. *American Economic Review 105*(3), 1067–1104.

Banerjee, A., J. Das, J. Hammer, R. Hussam, and A. Mohpal (2023). The Market for Healthcare in Low Income Countries. Working Paper.

Banerjee, A. V., E. Duflo, and R. Glennerster (2008). Putting a band-aid on a corpse: incentives for nurses in the Indian public health care system. *Journal of the European Economic Association 6*(2-3), 487–500.

Barham, T. and J. Rowberry (2013). Living longer: The effect of the mexican conditional cash transfer program on elderly mortality. *Journal of Development Economics 105*, 226–236.

Basri, M. C., M. Felix, R. Hanna, and B. A. Olken (2021). Tax administration versus tax rates: evidence from corporate taxation in Indonesia. *American Economic Review 111*(12), 3827–3871.

Bedoya, G., J. Das, and A. Dolinger (2023). Randomized regulation: The impact of minimum quality standards on health markets. Working Paper 31203, National Bureau of Economic Research.

Bennett, D., C.-L. Hung, and T.-L. Lauderdale (2015). Health Care Competition and Antibiotic Use in Taiwan. *The Journal of Industrial Economics 63*(2), 371–393.

Bennett, D. and W. Yin (2019). The market for high-quality medicine: Retail chain entry and drug quality in India. *Review of Economics and Statistics 101*(1), 76–90.

Berry, S., J. Levinsohn, and A. Pakes (2004). Differentiated products demand systems from a combination of micro and macro data: The new car market. *Journal of Political Economy 112*(1), 68–105.

Birje, S., A. D. Patil, K. R. Munne, V. Chavan, B. N. Joshi, A. Akula, N. Salvi, S. Nair, S. P. Valawalkar, D. Tandon, et al. (2022). Enablers & challenges of tribal women & health system for implementation of screening of non-communicable diseases & common cancers: A mixed-methods study in Palghar district of Maharashtra, India. *The Indian Journal of Medical Research 156*(2), 319.

Björkman, M. and J. Svensson (2009). Power to the people: evidence from a randomized field experiment on community-based monitoring in Uganda. *The Quarterly Journal of Economics 124*(2), 735–769.

Bloom, D. E., E. T. Cafiero-Fonseca, M. E. McGovern, K. Prettner, A. Stanciole, J. Weiss, S. Bakkila, and L. Rosenberg (2014). The macroeconomic impact of non-communicable diseases in China and India: Estimates, projections, and comparisons. *The Journal of the Economics of Ageing 4*, 100–111.

Carneiro, P., J. Das, and H. Reis (2024). The value of private schools: Evidence from Pakistan. *Review of Economics and Statistics*, 1–18.

Carrieri, V. and A. Wuebker (2016). Quasi-Experimental Evidence on the Effects of Health Information on Preventive Behaviour in Europe. *Oxford Bulletin of Economics and Statistics 78*(6), 765–791.

Carrillo, B. and J. Feres (2019). Provider supply, utilization, and infant health: evidence from a physician distribution policy. *American Economic Journal: Economic Policy 11*(3), 156–96.

Chandra, A., C. H. Colla, and J. S. Skinner (2023, August). Productivity variation and input misallocation: Evidence from hospitals. Working Paper 31569, National Bureau of Economic Research.

Chaudhury, N., J. Hammer, M. Kremer, K. Muralidharan, and F. H. Rogers (2006). Missing in action: teacher and health worker absence in developing countries. *Journal of Economic perspectives 20*(1), 91–116.

Chay, K., D. Kim, and S. Swaminathan (2012). Health insurance, hospital utilization and mortality: Evidence from Medicare's origins. Working Paper.

Chen, L., W. Yip, M.-C. Chang, H.-S. Lin, S.-D. Lee, Y.-L. Chiu, and Y.-H. Lin (2007). The effects of taiwan's national health insurance on access and health status of the elderly. *Health economics 16*(3), 223–242.

Currie, J., A. Li, and M. Schnell (2023). The effects of competition on physician prescribing. Working Paper 30889, National Bureau of Economic Research.

Currie, J., W. Lin, and J. Meng (2014). Addressing antibiotic abuse in China: An experimental audit study. *Journal of Development Economics 110*, 39–51.

Das, J., A. Chowdhury, R. Hussam, and A. V. Banerjee (2016). The impact of training informal health care providers in India: A randomized controlled trial. *Science 354*(6308), aaf7384.

Das, J., J. Hammer, and K. Leonard (2008). The quality of medical advice in low-income countries. *Journal of Economic Perspectives 22*(2), 93–114.

Das, J., A. Holla, A. Mohpal, and K. Muralidharan (2016). Quality and Accountability in Health Care delivery: audit-study evidence from primary care in India. *American Economic Review 106*(12), 3765–99.

Desai, S., R. K. Bishnoi, and P. Punjot (2020). Community health officer: the concept of mid-level health care providers. *International Journal of Community Medicine and Public Health 7*(4), 1610.

Dhaliwal, I. and R. Hanna (2017). The devil is in the details: The successes and limitations of bureaucratic reform in india. *Journal of Development Economics 124*, 1–21.

Dinerstein, M., C. Neilson, and S. Otero (2023). The equilibrium effects of public provision in education markets: Evidence from a public school expansion policy. Working Paper.

Dinerstein, M. and T. D. Smith (2021). Quantifying the supply response of private schools to public policies. *American Economic Review 111*(10), 3376–3417.

Diop, B. Z., K. Awoonor-Williams, A. Ofosu, and M. J. Williams (2019). Using administrative data to measure the productivity and allocation of health workers and funds. IGC Policy Brief.

Dupas, P. (2011). Health behavior in developing countries. *Annual Review of Economics. 3*(1), 425–449.

Ganimian, A., K. Muralidharan, and C. Walters (2021). Augmenting State Capacity for Child Development in India. *NBER Working Paper w28780, National Bureau of Economic Research*.

Garg, S., N. Tripathi, A. Ranjan, and K. K. Bebarta (2022). How much do government and households spend on an episode of hospitalisation in India? A comparison for public and private hospitals in Chhattisgarh state. *Health Economics Review 12*(1), 1–15.

Godlonton, S. and E. N. Okeke (2016). Does a ban on informal health providers save lives? evidence from malawi. *Journal of Development Economics 118*, 112–132.

Gruber, J., M. Lin, and J. Yi (2023). The largest insurance program in history: Saving one million lives per year in china. *Journal of Public Economics 226*, 104999.

Hainmueller, J. (2012). Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political analysis 20*(1), 25–46.

Hendren, N. and B. Sprung-Keyser (2020). A unified welfare analysis of government policies. *The Quarterly Journal of Economics 135*(3), 1209–1318.

Hickey, M. D., J. Ayieko, A. Owaraganise, N. Sim, L. B. Balzer, J. Kabami, M. Atukunda, F. J. Opel, E. Wafula, M. Nyabuti, et al. (2021). Effect of a patient-centered hypertension delivery strategy on all-cause mortality: Secondary analysis of SEARCH, a community-randomized trial in rural Kenya and Uganda. *PLoS medicine 18*(9), e1003803.

Hsiao, A. (2022). Working Paper.

Hsieh, C.-T. and P. J. Klenow (2009). Misallocation and manufacturing tfp in china and india. *The Quarterly Journal of Economics 124*(4), 1403–1448.

Huang, W. and C. Zhang (2021). The power of social pensions: evidence from china's new rural pension scheme. *American Economic Journal: Applied Economics 13*(2), 179–205.

Jensen, R. T. and K. Richter (2004). The health implications of social security failure: evidence from the Russian pension crisis. *Journal of Public Economics 88*(1-2), 209–236.

Kim, H. K. and J. Niederdeppe (2013). Exploring optimistic bias and the integrative model of behavioral prediction in the context of a campus influenza outbreak. *Journal of Health Communication 18*(2), 206–222.

Li, Y., J. Meng, C. Song, and K. Zheng (2021). Information avoidance and medical screening: A field experiment in China. *Management Science 67*(7), 4252–4272.

Lin, T., C. Chen, and P. Chou (2004). Impact of the high-risk and mass strategies on hypertension control and stroke mortality in primary health care. *Journal of Human Hypertension 18*(2), 97–105.

Lopez, C., A. Sautmann, and S. Schaner (2022). Does patient demand contribute to the overuse of prescription drugs? *American Economic Journal: Applied Economics 14*(1), 225–260.

Malani, A., C. Kinnan, G. Conti, K. Imai, M. Miller, S. Swaminathan, A. Voena, and B. Woda (2024, March). Evaluating and pricing health insurance in lower-income countries: A field experiment in india. Working Paper 32239, National Bureau of Economic Research.

Malavasi, C. and H. Ye (2024). Live longer and healthier: Impact of pension income for low-income retirees.

McPake, B. and K. Hanson (2016). Managing the public–private mix to achieve universal health coverage. *The Lancet 388*(10044), 622–630.

MHFW (2018). Ayushman Bharat - Operational Guidelines. Ministry of Health & Family Welfare, Government of India.

MHFW (2022). Rural Health Statistics 2021-22. Ministry of Health & Family Welfare, Government of India.

MHFW (2024). 2023-24 Annual Report. Ministry of Health & Family Welfare, Government of India.

Mohan, P., S. D. Iyengar, S. Mohan, and K. Sen (2003). Auxiliary nurse midwife: What determines her place of residence. *Journal of Health and Population in Developing Countries, http://www. jhpdc. unc/edu*.

Monami, M., R. Candido, B. Pintaudi, G. Targher, E. Mannucci, L. Delle Monache, M. Gallo, A. Giaccari, M. L. Masini, F. Mazzone, et al. (2021). Effect of metformin on all-cause mortality and major adverse cardiovascular events: An updated meta-analysis of randomized controlled trials. *Nutrition, Metabolism and Cardiovascular Diseases 31*(3), 699–704.

Mora-García, C. A., M. Pesec, and A. M. Prado (2024). The effect of primary healthcare on mortality: Evidence from Costa Rica. *Journal of Health Economics 93*, 102833.

Muraleedharan, V. R., U. Dash, S. Vaishnavi, and R. Gopinath (2018). Universal health coverage-pilot in tamil nadu: Has it delivered what was expected? Technical report, Centre for Technology and Policy, Department of Humanities and Social Sciences, IIT Madras, Chennai, Tamil Nadu.

Neilson, C. (2021). Targeted vouchers, competition among schools, and the academic achievement of poor students. Working Paper.

Okeke, E. N. (2023). When a doctor falls from the sky: The impact of easing doctor supply constraints on mortality. *American Economic Review 113*(3), 585–627.

Okeke, E. N., C. A. Adepiti, and K. O. Ajenifuja (2013). What is the price of prevention? New evidence from a field experiment. *Journal of Health Economics 32*(1), 207–218.

Oster, E., I. Shoulson, and E. R. Dorsey (2013). Optimal expectations and limited medical testing: Evidence from Huntington disease. *American Economic Review 103*(2), 804–830.

Patenaude, B. N., I. Semali, J. Killewo, and T. Bärnighausen (2019). The value of a statistical life-year in Sub-Saharan Africa: evidence from a large population-based survey in Tanzania. *Value in Health Regional Issues 19*, 151–156.

Rashmi, R. and S. K. Mohanty (2023). Examining chronic disease onset across varying age groups of indian adults using competing risk analysis. *Scientific Reports 13*(1), 5848.

Roth, J., P. H. Sant'Anna, A. Bilinski, and J. Poe (2023). What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics 235*(2), 2218–2244.

Sant'Anna, P. H. and J. Zhao (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics 219*(1), 101–122.

Snyder, S. E. and W. N. Evans (2006). The effect of income on mortality: evidence from the social security notch. *The Review of Economics and Statistics 88*(3), 482–495.

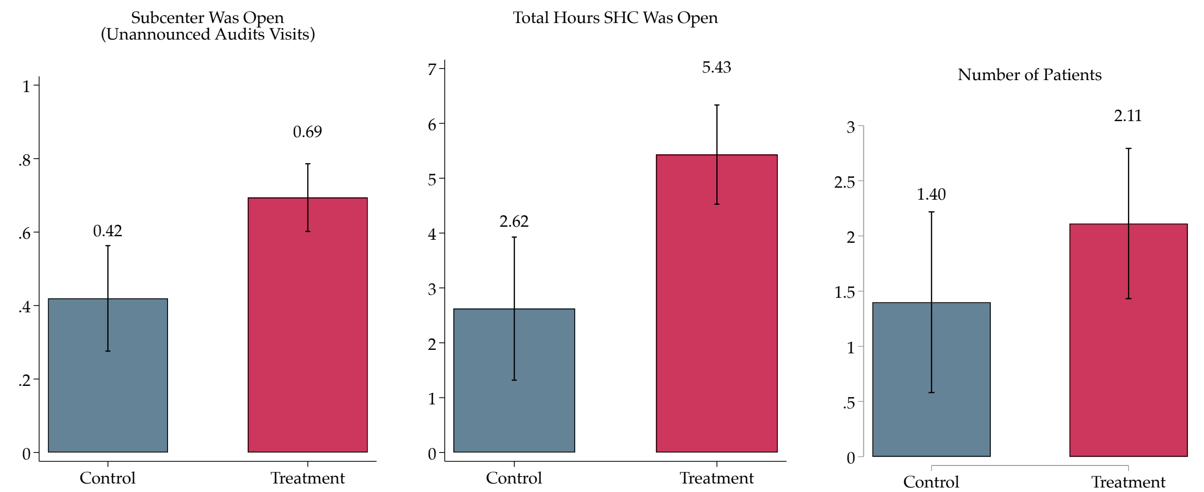
Sood, N., E. Bendavid, A. Mukherji, Z. Wagner, S. Nagpal, and P. Mullen (2014). Government health insurance for people below poverty line in India: quasi-experimental evaluation of insurance and health outcomes. *BMJ 349*.

Stuckler, D. (2008). Population causes and consequences of leading chronic diseases: a comparative analysis of prevailing explanations. *The Milbank Quarterly 86*(2), 273–326.

Tarozzi, A., A. Mahajan, B. Blackburn, D. Kopf, L. Krishnan, and J. Yoong (2014). Microloans, insecticide-treated bednets, and malaria: evidence from a randomized controlled trial in Orissa, India. *American Economic Review 104*(7), 1909–1941.

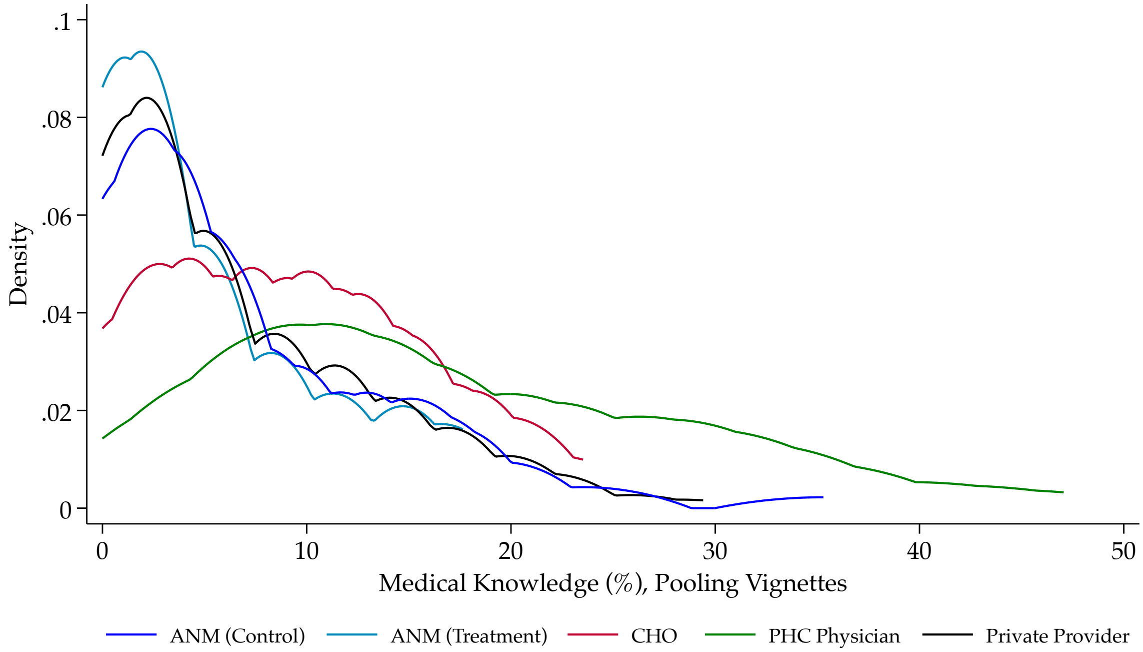
Walter, T. F. (2020). Misallocation in the Public Sector? Cross-Country Evidence from Two Million Primary Schools. Working Paper.

Figure 1: Results from Unannounced Audit Visits



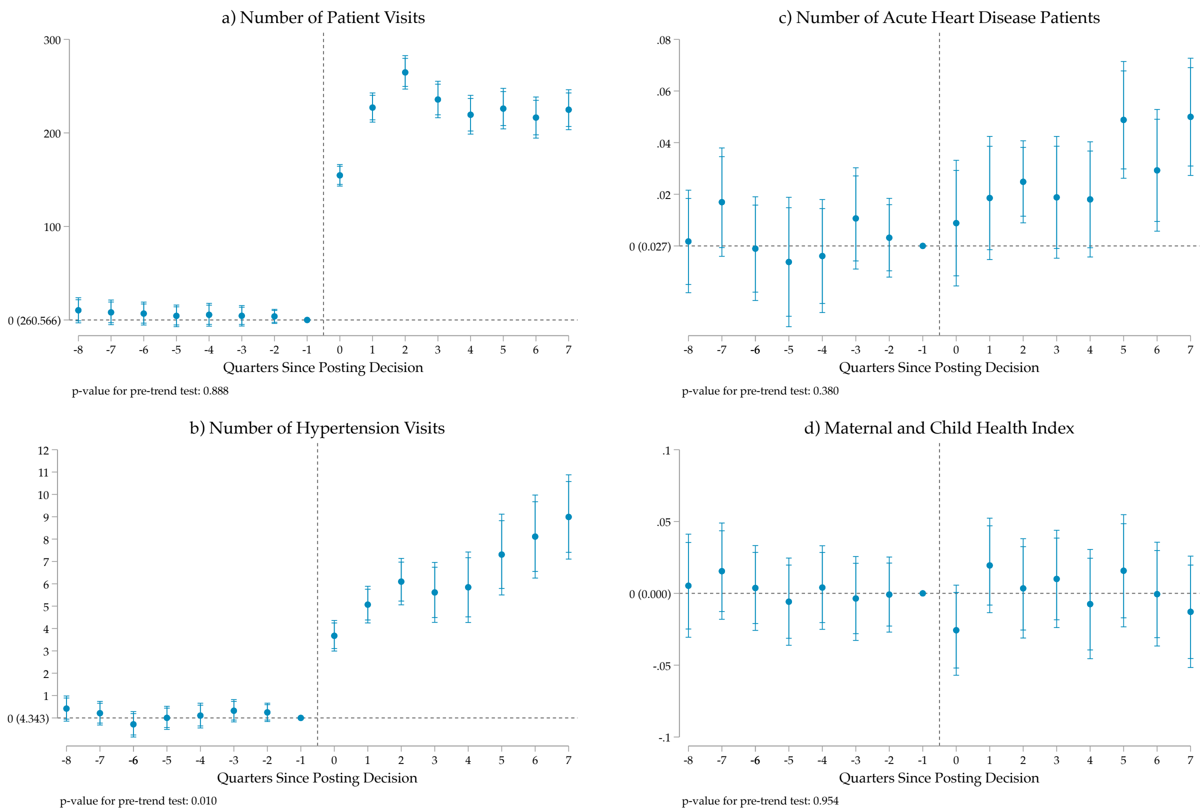
Notes: The figure shows the results from unannounced visits conducted between March and June 2023. The left figure shows the share of subcenters that were open at least at some point during the day of the unannounced visit. The middle shows the average number of hours for which the subcenters were open. The right figure shows the number of patients observed to have visited the subcenter. The whiskers correspond to 95% confidence intervals. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample consists of 94 control group subcenters and 98 treatment group subcenters.

Figure 2: Checklist Completion Rates Across Providers



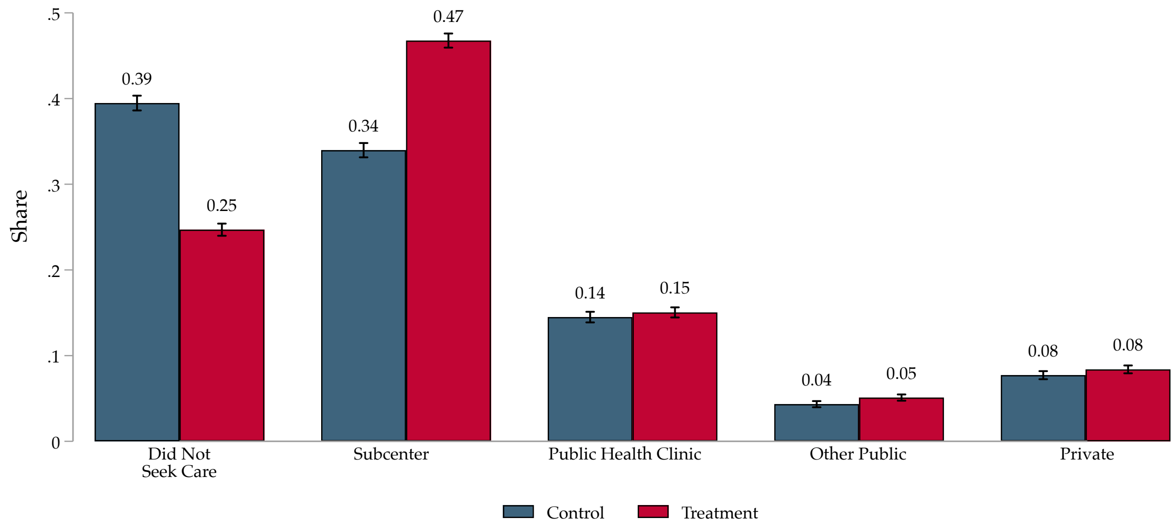
The figure shows the distribution of medical knowledge for different healthcare providers during the endline survey. Medical knowledge is measured as the average checklist completion rates across the child dysentery and adult asthma vignette. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample consists of 97 control group ANMs, 96 treatment group ANMs, 96 CHOs, 49 public clinic physicians, and 207 private providers.

Figure 3: Effects of Community Health Officers on Healthcare Services



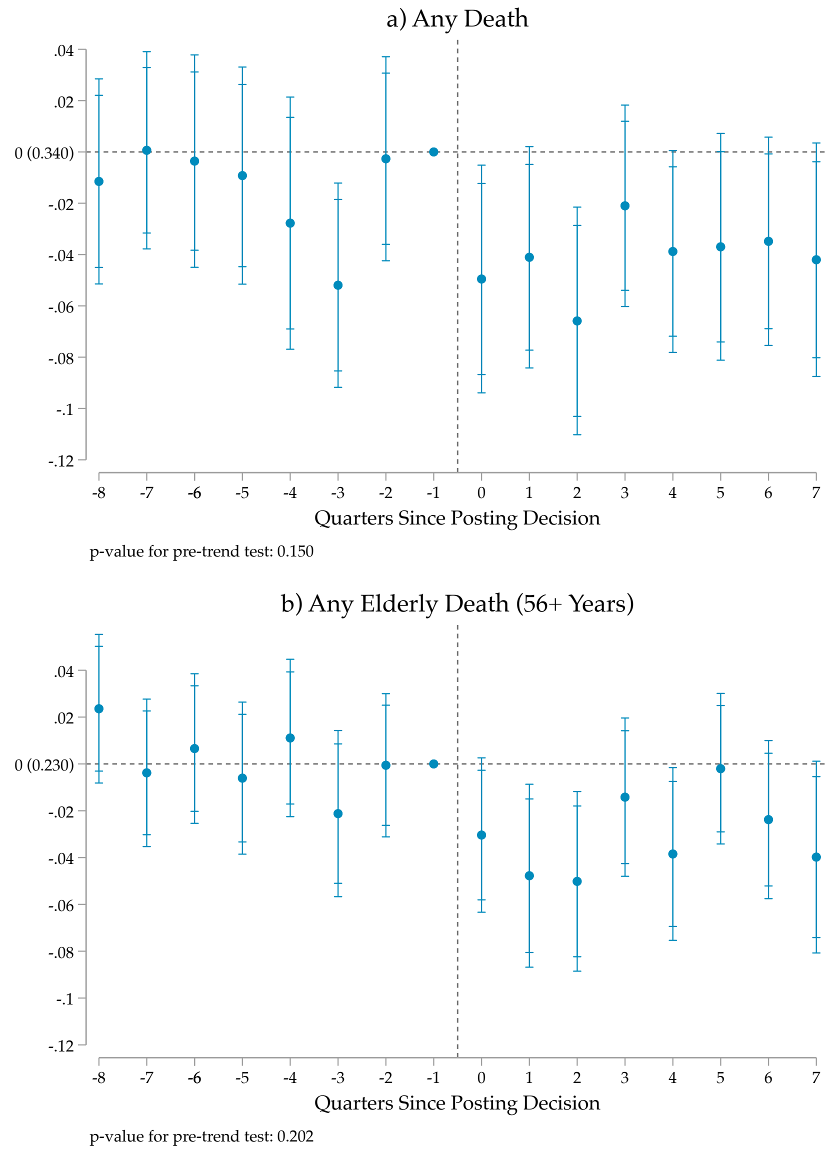
Notes: The figure shows weighted regression estimates of the effect of CHOs on healthcare services. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and quarter dummies, while controlling for year and subcenter fixed effects. The base quarter (Q1 2022) is omitted. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The figures show 90 and 95 percent confidence intervals based on standard errors clustered at the subcenter level. The number in parentheses on the y-axis shows the treatment group mean of the outcome in the base quarter. The p-value at the bottom of each figure corresponds to a chi-squared test of the null hypothesis that all interactions between treatment and pre-period dummies are statistically equal to zero. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure 4: Healthcare Utilization and Provider Choices by Treatment



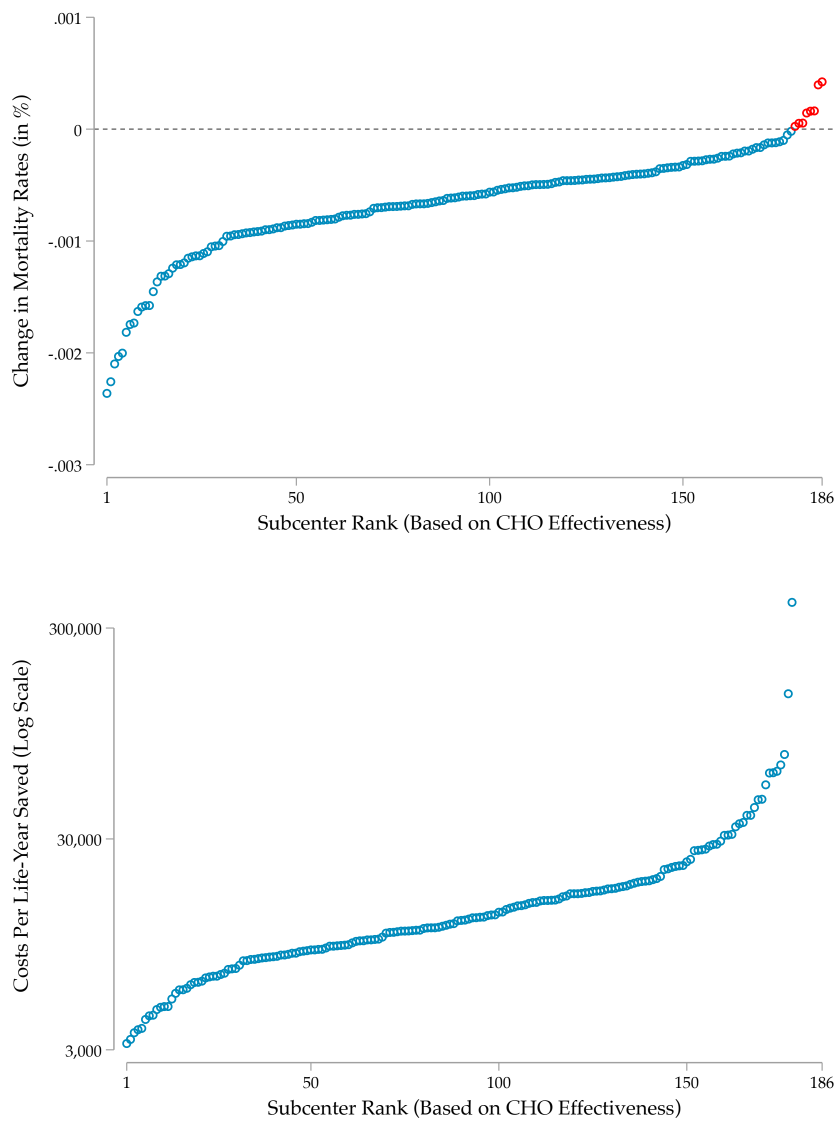
Notes: The figure shows healthcare provider choices in treatment and control group areas. The whiskers correspond to 95% confidence intervals. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 26,496 respondents (across 1,603 subcenters) who reported having at least one symptom in the past 30 days. Outcomes are obtained from the CHIP data. The figure consists of households surveyed between August 2023 and June 2024.

Figure 5: Effects of Community Health Officers on Health Outcomes



Notes: The figure shows weighted regression estimates of the effect of CHOs on health outcomes. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and quarter dummies, while controlling for year and subcenter fixed effects. The base quarter (Q1 2022) is omitted. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The figures show 90 and 95 percent confidence intervals based on standard errors clustered at the subcenter level. The number in parentheses on the y-axis shows the treatment group mean of the outcome in the base quarter. The p-value at the bottom of each figure corresponds to a chi-squared test of the null hypothesis that all interactions between treatment and pre-period dummies are statistically equal to zero. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. Outcomes for other age groups are shown in Appendix Figure A11.

Figure 6: Differences in Marginal Effects of CHOs Based on Counterfactual Simulations



Notes: The figure shows the marginal effect of posting a CHO on predicted changes in mortality rates across all sample markets. Estimates are based on posting a CHO to a particular subcenter relative to the baseline scenario where no CHOs are posted at all. Red dots indicate subcenters for which the posting of a CHO leads to an increase in predicted mortality rates. The second plot is restricted to subcenters for which mortality rates decline. The cost calculations in this exercise only account for CHO salaries.

Table 1: Comparison of Treatment and Control Areas

	Original Sample				Reweighted Sample				
	Control Mean (1)	Control St. D. (2)	Treatment Coeff. (3)	Treatment St. E. (4)	Control Mean (5)	Control St. D. (6)	Treatment Coeff. (7)	Treatment St. E. (8)	N (9)
Panel A: Targeted Characteristics									
Distance to District HQ (in km)	71.90	[37.68]	-3.51***	(1.06)	68.13	[36.72]	0.25	(1.52)	4,909
Distance to Subdistrict HQ (in km)	25.63	[17.59]	-2.73***	(0.45)	23.07	[13.93]	-0.17	(0.55)	4,909
Panel B: Catchment Area Characteristics									
Distance to Public Health Clinic (in km)	8.18	[6.67]	-1.22***	(0.18)	7.23	[5.92]	-0.27	(0.24)	4,909
Total Population	2965.46	[1514.49]	150.15***	(44.47)	3028.62	[1595.32]	86.99	(70.90)	4,909
Elderly Population Share	0.09	[0.04]	0.00	(0.00)	0.09	[0.04]	0.00	(0.00)	4,858
Scheduled Caste Share	0.19	[0.15]	-0.02***	(0.00)	0.16	[0.12]	0.01**	(0.00)	4,909
Scheduled Tribe Share	0.17	[0.28]	0.02***	(0.01)	0.20	[0.31]	-0.00	(0.01)	4,909
Female Share	0.49	[0.02]	0.00*	(0.00)	0.49	[0.02]	-0.00	(0.00)	4,909
Literacy Rate	0.47	[0.10]	0.03***	(0.00)	0.50	[0.11]	0.01	(0.00)	4,909
Land Ownership Rate	0.69	[0.19]	0.04***	(0.01)	0.72	[0.18]	0.00	(0.01)	4,859
Employment Rate	0.50	[0.08]	-0.01***	(0.00)	0.49	[0.09]	0.00	(0.00)	4,909
(Imputed) Consumption per Capita (in INR)	16525.32	[3706.49]	651.17***	(109.91)	16954.37	[3985.32]	222.12	(163.57)	4,859
Panel C: Average Facility Indicators in Q1 2022									
Number of Patients	260.57	[216.24]	-22.55***	(5.61)	241.70	[205.98]	-3.69	(8.60)	4,909
Number of Acute Heart Disease Patients	0.03	[0.21]	-0.00	(0.01)	0.03	[0.21]	-0.00	(0.01)	4,909
Number of Hypertension Patients	4.34	[10.49]	-0.39	(0.27)	4.34	[9.56]	-0.39	(0.33)	4,909
Maternal and Child Health Services Index	-0.00	[0.82]	0.03	(0.02)	-0.01	[0.81]	0.04	(0.04)	4,909
All-Age Mortality Rate	0.38	[0.73]	-0.01	(0.02)	0.38	[0.74]	-0.01	(0.03)	4,909
Elderly Mortality Rate	2.39	[5.54]	0.06	(0.16)	2.60	[6.12]	-0.16	(0.27)	4,909

Notes: This table shows the means of selected covariates for the original and reweighted sample. Panel A reports on the covariates used to estimate the propensity score for the inverse probability weighting. Panel B reports on covariates at the catchment area level. Catchment-level covariates are calculated based on population-weighted averages across all villages in the catchment area of the respective subcenter. Panel C reports on the main outcomes in the pre-treatment reference period. Columns (1)-(4) present the original sample and columns (5)-(8) present the reweighted sample. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Differences in sample sizes across variables reflect missing data. Columns (1) and (5) report the control mean of the dependent variable for each relevant sample. Columns (3), and (7) report the difference in the dependent variable from OLS regressions of each outcome on an indicator variable for CHO assignment.

Table 2: Effects of Community Health Officers on Subcenter Characteristics

	Survey Sample						Admin Data Sample
	Number of Providers	Highest Degree is 3+ Years	Number of Community Health Workers	Electricity	Equipment Index	Medicine Index	CHO Posted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	0.995***	0.954***	-0.070	0.022	-0.172	-0.223	0.845***
	(0.025)	(0.024)	(0.122)	(0.057)	(0.417)	(0.346)	(0.011)
Control Group Mean (Baseline)	1.000	0.038	2.615	0.588	7.417	3.848	0.000
Treatment Group Mean (Baseline)	1.011	0.053	2.947	0.628	7.484	3.661	0.000
Counterfactual Treatment Group Mean (Endline)	1.035	0.046	2.998	0.665	6.860	4.078	0.155
Observations	378	378	376	374	282	282	9,818

Notes: This table shows the effects of CHOs on subcenter staffing and infrastructure. We regress the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. In Columns (1)-(7), the sample consists of 193 subcenters and outcomes are obtained from ANM and CHO surveys. In Column (8), the sample consists of 4,909 subcenters and information on CHO postings are obtained from the Health and Wellness Center Portal.

Table 3: Effects of Community Health Officers on Patient Visits

	Total Number of Patient Visits	Type of Visits					Maternal & Child Health Services Index
		Acute Heart Disease	Stroke	Epilepsy	Hypertension	Diabetes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATET							
Treatment × Post	215.622***	0.024***	-0.002	0.012**	6.211***	4.739***	-0.002
	(7.584)	(0.007)	(0.005)	(0.006)	(0.565)	(0.470)	(0.011)
Control Group Mean (Q1 2022)	237.503	0.036	0.022	0.034	3.731	3.114	-0.012
Treatment Group Mean (Q1 2022)	239.272	0.038	0.026	0.033	3.470	2.947	0.033
Counterfactual Treatment Group Mean (Post-Periods)	370.741	0.036	0.046	0.031	8.646	7.659	0.146
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the aggregate effects of CHOs on healthcare services. The regression coefficients are estimated by pooling the pre- and post-periods and regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Appendix Table A6 provides regression estimates for each index component in the maternal and child health index in Column (7). See Data Appendix for details on variable definitions.

Table 4: Effects of Community Health Officers on Mortality Outcomes

	Any Death (1)	Number of Deaths (2)	Mortality Rate (3)	Mortality Rate (IHS) (4)
Panel A: All-Age Deaths				
Treatment × Post	-0.028**	-0.102**	-0.039**	-0.031**
	(0.011)	(0.041)	(0.019)	(0.013)
Control Group Mean (Pre-Periods)	0.362	1.071	0.409	0.311
Treatment Group Mean (Pre-Periods)	0.372	1.123	0.409	0.314
Counterfactual Treatment Group Mean (Post-Periods)	0.361	1.057	0.392	0.307
Panel B: Elderly Deaths (56+ Years)				
Treatment × Post	-0.032***	-0.081***	-0.339**	-0.095***
	(0.010)	(0.031)	(0.150)	(0.030)
Control Group Mean (Pre-Periods)	0.228	0.596	2.650	0.660
Treatment Group Mean (Pre-Periods)	0.246	0.627	2.641	0.695
Counterfactual Treatment Group Mean (Post-Periods)	0.257	0.635	2.707	0.727
Panel C: Other Age Groups				
Treatment × Post	-0.008	-0.018	-0.009	-0.007
	(0.010)	(0.020)	(0.008)	(0.007)
Control Group Mean (Pre-Periods)	0.262	0.466	0.180	0.158
Treatment Group Mean (Pre-Periods)	0.267	0.482	0.180	0.159
Counterfactual Treatment Group Mean (Post-Periods)	0.229	0.406	0.156	0.137
Observations	9,818	9,818	9,818	9,818

Notes: This table shows the aggregate effects of CHOs on mortality outcomes. The regression coefficients are estimated by pooling the pre- and post-periods and regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table 5: Effects of Community Health Officers on Health Outcomes in Household Surveys

	All Household Members			
	Past 30 Days		Past 6 Months	
	Any Symptoms (1)	Medical Expenses (2)	Any Hospitalization (3)	Hospital Days (4)
Treatment × Post	0.013	-1.572	-0.017**	-0.052**
	(0.031)	(48.503)	(0.008)	(0.021)
Control Group Mean (Baseline)	0.098	118.515	0.024	0.057
Treatment Group Mean (Baseline)	0.092	148.571	0.036	0.090
Counterfactual Treatment Group Mean (Endline)	0.171	234.342	0.033	0.089
Observations	5,846	5,792	5,847	5,846

Notes: This table shows the effects of CHOs on household survey outcomes at the household member level. The regression coefficients are estimated by regressing the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. See Data Appendix for details on variable definitions.

Table 6: Effects of Community Health Officers on Private Provider Behavior

	Number of Providers (1)	Number of Patients (2)	Typical Fee (3)	Quality Index (4)
Panel A: Pooled				
Treatment × Post	0.155	-3.488	4.416	0.262**
	(0.099)	(21.740)	(15.570)	(0.107)
Control Group Mean (Baseline)	1.446	94.810	97.806	0.022
Treatment Group Mean (Baseline)	1.102	114.184	101.429	-0.018
Counterfactual Treatment Group Mean (Endline)	0.978	126.819	110.876	-0.235
Observations	384	452	455	476
Panel B: Heterogeneity by Distance				
Treatment × Post × Close Dist. Providers		-42.365	-19.430	0.525**
		(31.528)	(19.334)	(0.218)
Treatment × Post × Medium Dist. Providers		51.796	-13.162	0.212
		(43.255)	(24.058)	(0.245)
Treatment × Post × Far Dist. Providers		-43.873	33.769	0.018
		(30.810)	(27.825)	(0.212)
p-value: Coef 1 = Coef 2		0.079	0.838	0.379
p-value: Coef 1 = Coef 3		0.971	0.108	0.109
p-value: Coef 2 = Coef 3		0.079	0.203	0.560
Close Dist. Providers:				
Control Group Mean (Baseline)		95.490	87.175	0.059
Treatment Group Mean (Baseline)		124.739	84.400	-0.000
Counterfactual Treatment Group Mean (Endline)		155.526	122.109	-0.441
Observations		149	155	159
Medium Dist. Providers:				
Control Group Mean (Baseline)		89.253	78.368	-0.060
Treatment Group Mean (Baseline)		115.083	114.167	-0.027
Counterfactual Treatment Group Mean (Endline)		98.262	123.258	-0.120
Observations		148	150	159
Far Dist. Providers:				
Control Group Mean (Baseline)		97.757	120.541	0.063
Treatment Group Mean (Baseline)		105.069	105.714	-0.024
Counterfactual Treatment Group Mean (Endline)		149.373	102.753	-0.130
Observations		155	150	158

Notes: This table shows the effects of CHOs on private provider outcomes. In Panel A, we regress the outcome on the treatment dummy, survey round dummies, and an interaction between the treatment dummy and the post-period dummy. In Panel B, we regress the outcome on the treatment dummy, the post-survey dummies, and an interaction between the treatment dummy and the post-period dummy, separately for each distance tercile between the private provider and the subcenter. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample pools the baseline survey round and two endline survey rounds. Appendix Table A18 provides regression estimates for each index component in the quality index in Column (3). See Data Appendix for details on variable definitions.

Table 7: Demand Estimates

Parameters	Estimate	SE
Quality (Highest Degree Length in Years)	0.540	(0.248)
Quality $\times$ Non-Poor	0.226	(0.582)
Man-Hours (in 10 Man-Hours)	0.223	(0.059)
Price (in INR 100)	-2.710	(1.020)
Price $\times$ Non-Poor	1.152	(1.418)
Distance (in km)	-0.522	(0.273)
Distance $\times$ Non-Poor	-0.097	(0.492)
Electricity	0.144	(0.187)
Private	1.194	(1.166)
PHC	-0.378	(0.573)
σ	0.838	(0.626)
Constant	-3.880	(0.379)

Notes: This table reports the results from the estimation of the demand model.

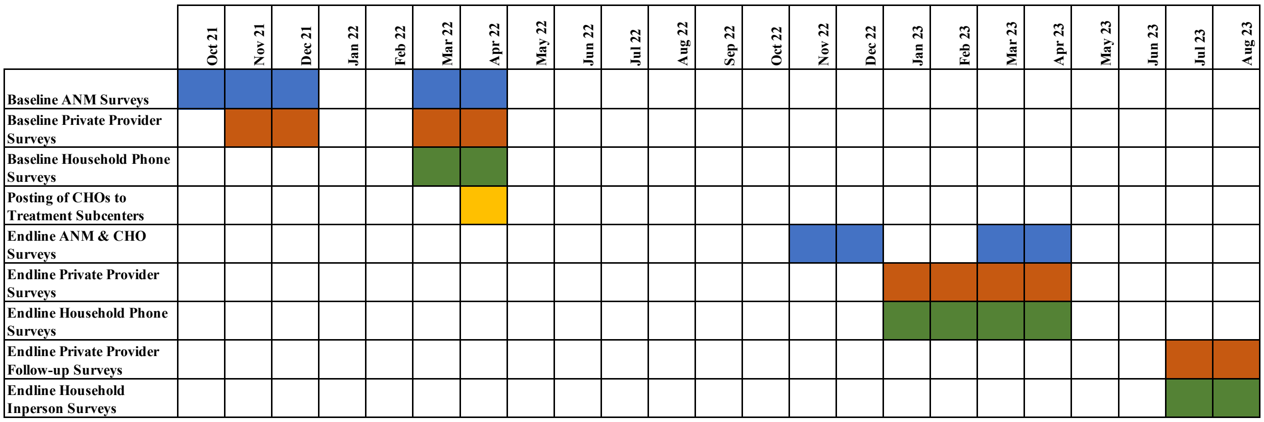
Table 8: Counterfactual Analysis

Counterfactuals	Market Shares: Subcenter (1)	PHC (2)	Private (3)	Average Quality (4)	Δ All-Age Mortality Rate (in %) (5)
Baseline					
0) Baseline	0.096	0.316	0.100	1.960	
Decomposition (All SHCs Get Treated)					
1) Full Treatment Effect ($\uparrow q_{shc}$,$\uparrow h_{shc}$,$\uparrow q_{priv}$)	0.276	0.263	0.099	2.376	-0.109
2) Only Increase in Subcenter Quality ($\uparrow q_{shc}$)	0.164	0.299	0.092	2.154	-0.051
3) Only Increase in Subcenter Person-Hours ($\uparrow h_{shc}$)	0.182	0.294	0.090	2.004	-0.012
4) No Effects on Private Providers ($\uparrow q_{shc}$,$\uparrow h_{shc}$)	0.284	0.267	0.077	2.331	-0.097
Private Sector Ban					
5) Ban Without CHOs	0.109	0.337	0.000	1.906	0.014
6) Ban With CHOs	0.287	0.287	0.000	2.295	-0.088
Within-Market Reallocation of CHOs (51% of SHCs Get Treated)					
7) Observed CHO Allocation	0.200	0.286	0.099	2.200	-0.063
8) Random CHO Allocation	0.196	0.287	0.100	2.192	-0.061
9) Optimal CHO Allocation	0.205	0.290	0.099	2.244	-0.075

Notes: This table presents the results of the the counterfactual analysis. The different scenarios are as follows. Row (0): the baseline model. Row (1): full treatment effect in which subcenter quality and person-hours increase and private providers improve their quality in all subcenter locations. Row (2): only increase in subcenter quality, no change in subcenter person-hours or private provider quality. Row (3): only increase in subcenter person-hours, no change in subcenter or private provider quality. Row (4): increase in subcenter quality and person-hours, but no change in private provider quality. Row (5): ban on private providers, no change in subcenter quality and person-hours. Row (6): ban on private providers and increase in subcenter quality and person-hours in all subcenter locations. Row (7): 95 out of the 186 sample SHCs receive a CHO as per the observed government assignment. Row (8): average outcomes across 100 random allocations of the 95 CHOs within the same markets. Row (9): Reallocation of the 95 CHOs within the same markets with the objective to maximize the decline in the all-age mortality rates. Columns (1)-(3) show the average market shares for subcenters, PHCs, and private providers. The market share of the outside option is omitted. Column (4) reports the average healthcare quality of the chosen provider, with quality defined as 0 if the outside option is chosen. Column (5) reports the predicted relative decline in all-age mortality rates based on the changes in average quality.

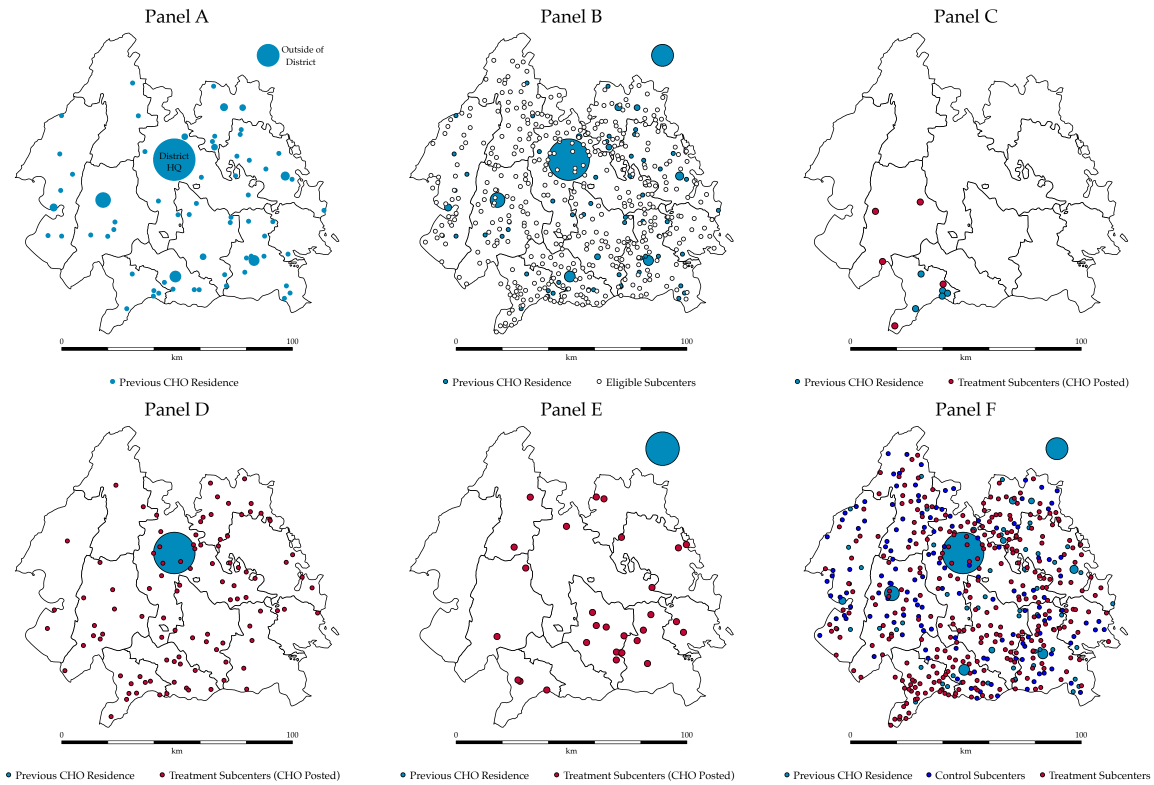
A. Appendix Tables and Figures

Figure A1: Timeline



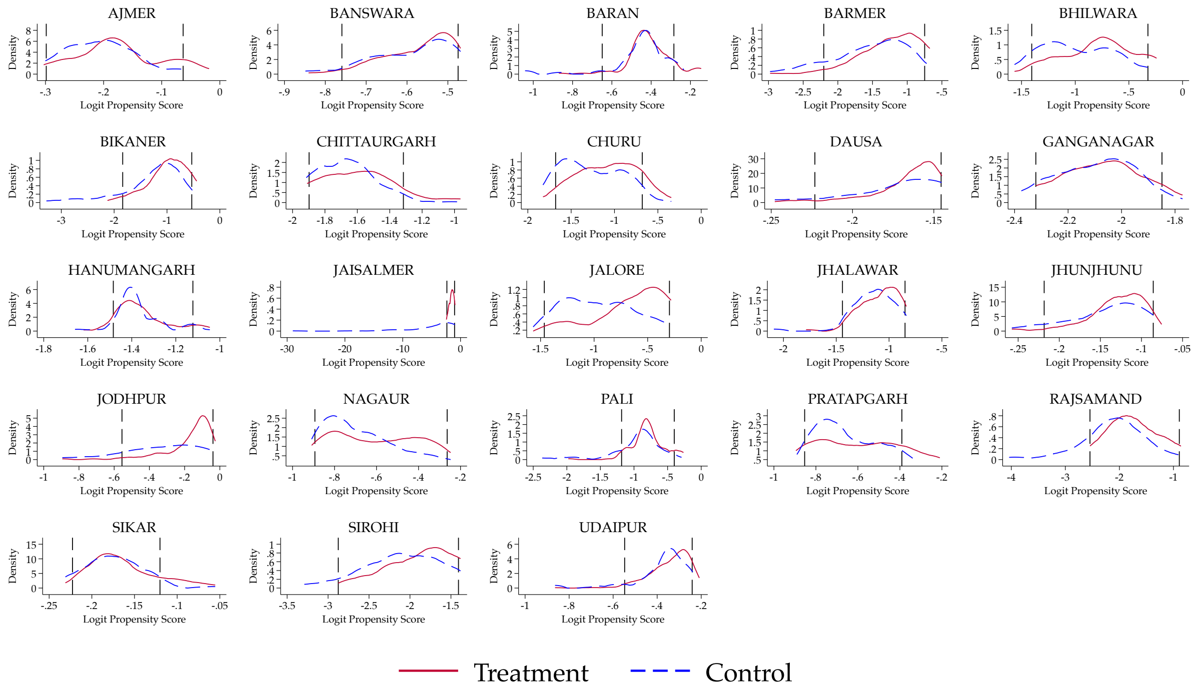
Notes: This figure shows the timeline for the primary data collection. The assignments of CHOs to subcenters were announced on March 27, 2022. Most CHOs started to work in the field by the end of April. Household inperson surveys at endline were done with households that were surveyed over the phone at baseline but could not be reached over the phone at endline. See Appendix Table A2 for survey completion rates.

Figure A2: Assignments of CHOs Across Udaipur District



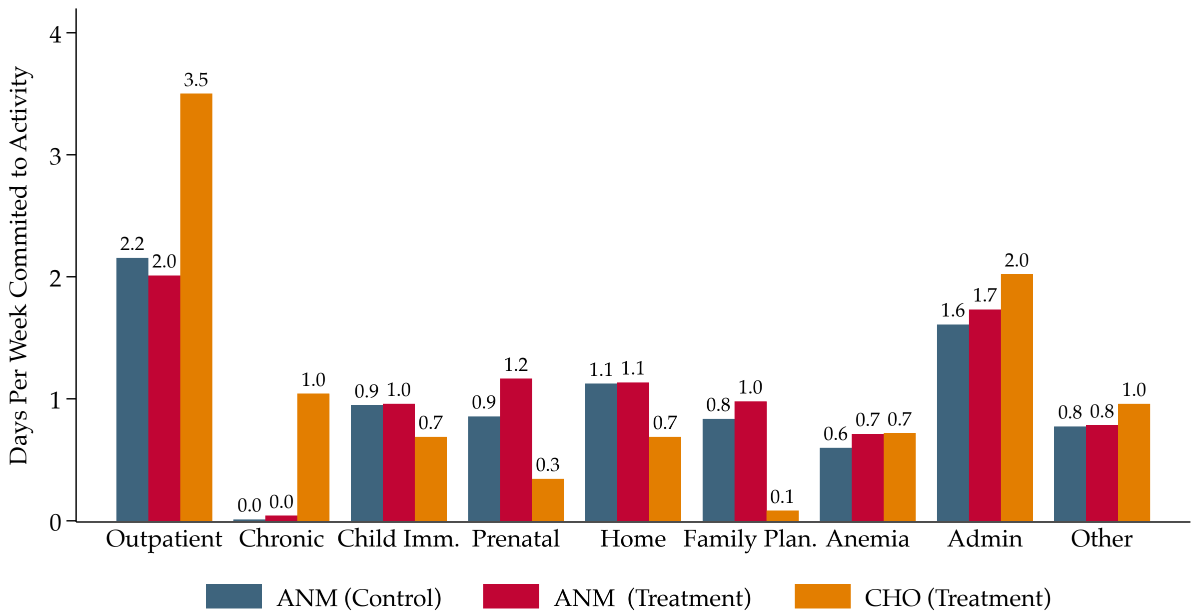
Notes: The figures show where CHOs in Udaipur district previously resided as well as the location of treatment and control group subcenters in the district. The boundaries represent subdistricts. The top-left figure shows where CHOs resided before their postings. The size of each bubble represents the number of CHOs in each location. The top-middle figure shows the number of subcenters that were eligible to receive a CHO. The top-right figure indicates the assignment locations for the CHOs that previously resided in Kherwara subdistrict. The bottom-left figure indicates the assignment locations for the CHOs that previously resided in Udaipur city, the district headquarter. The bottom-middle figure indicates the assignment locations for the CHOs that previously resided outside of Udaipur district. The bottom-right figure shows the location of all treatment and control group subcenters in the district. Information on previous residence locations are obtained from surveys with 243 CHOs.

Figure A3: Common Support Restrictions



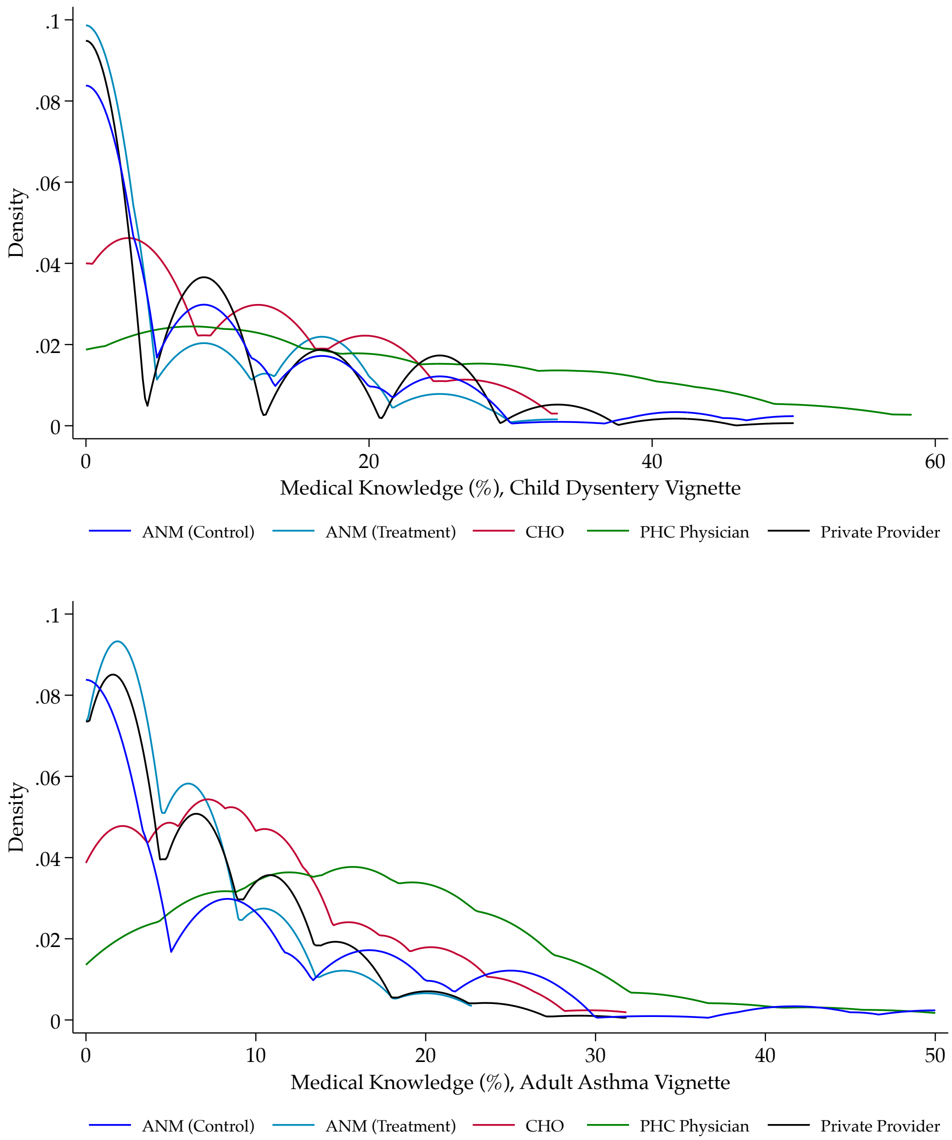
Notes: This figure shows the distribution of the logit propensity score in each district within our sample for treatment and control group facilities. Districts in which more than 90% of converted subcenters received a CHO are omitted. Propensity scores are estimates by a logistic regression that regresses the treatment dummy on linear and squared terms of the subcenter's distance to the district and block headquarters. In our preferred specification, we implement common support restrictions by excluding observations within the top or bottom 2.5 percent of either the control or treatment propensity score distribution in each district (vertical lines). See Appendix Table A11 for robustness regarding alternative common support restrictions.

Figure A4: Time Use of ANMs and CHOs



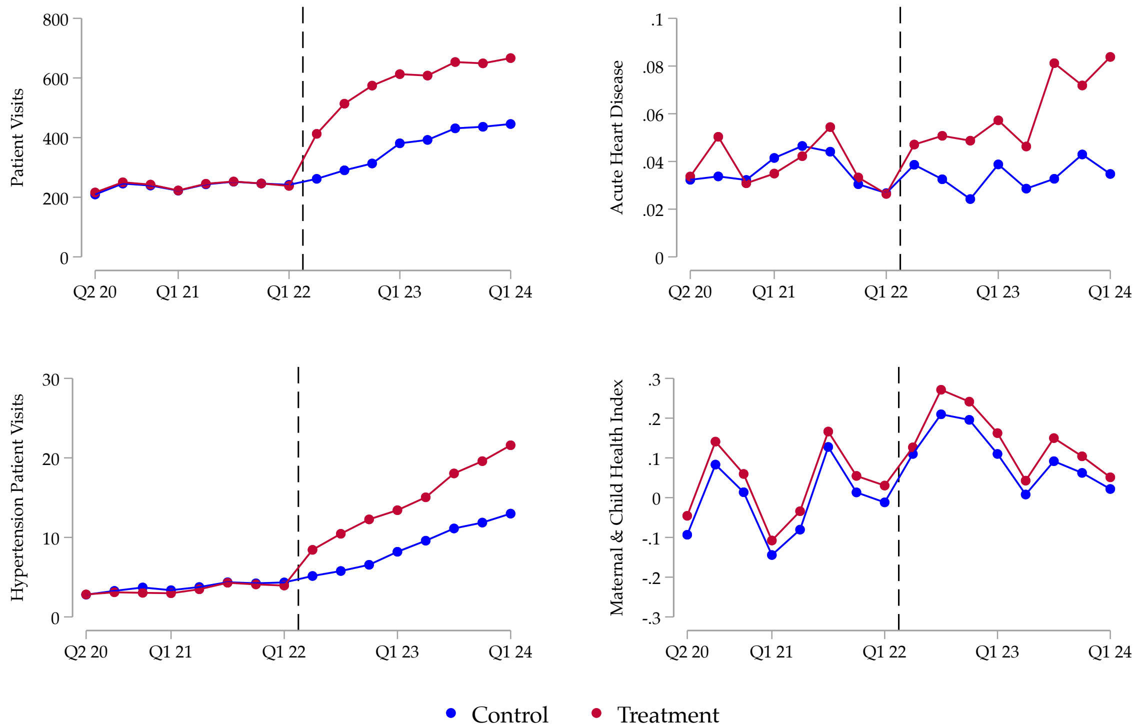
Notes: The figure shows the weighted average number of days per week treatment and control group ANMs and CHOs spent on different activities according to the time-use module in the endline survey. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Respondents could select more than one activity per day, so the aggregate number of days can sum up to more than 7 days. The sample consists of 97 control group ANMs, 96 treatment group ANMs, and 96 CHOs.

Figure A5: Checklist Completion Rates Across Providers by Vignette



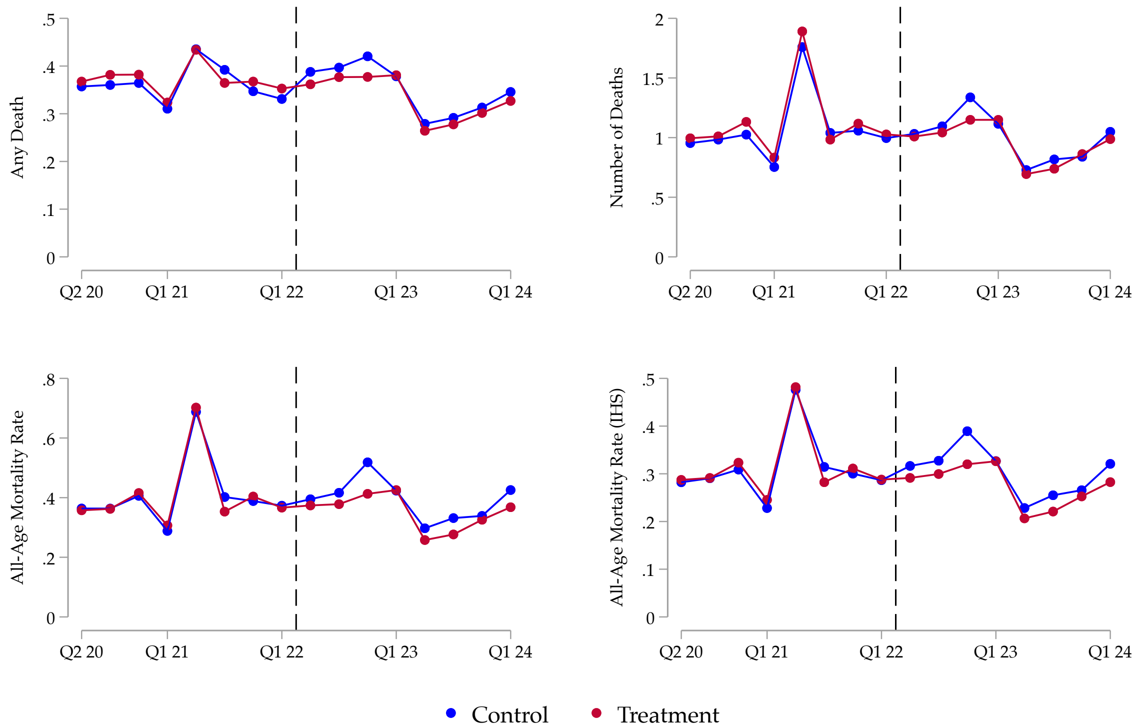
Notes: The figure shows the distribution of medical knowledge for different healthcare providers during the endline survey, separately for the child dysentery and the adult asthma vignette. Medical knowledge is measured as the average checklist completion rates across the child dysentery and adult asthma vignette. The sample consists of 97 control group ANMs, 96 treatment group ANMs, 96 CHOs, 49 public clinic physicians, and 207 private providers.

Figure A6: Trends in Patient Visits by Treatment Group



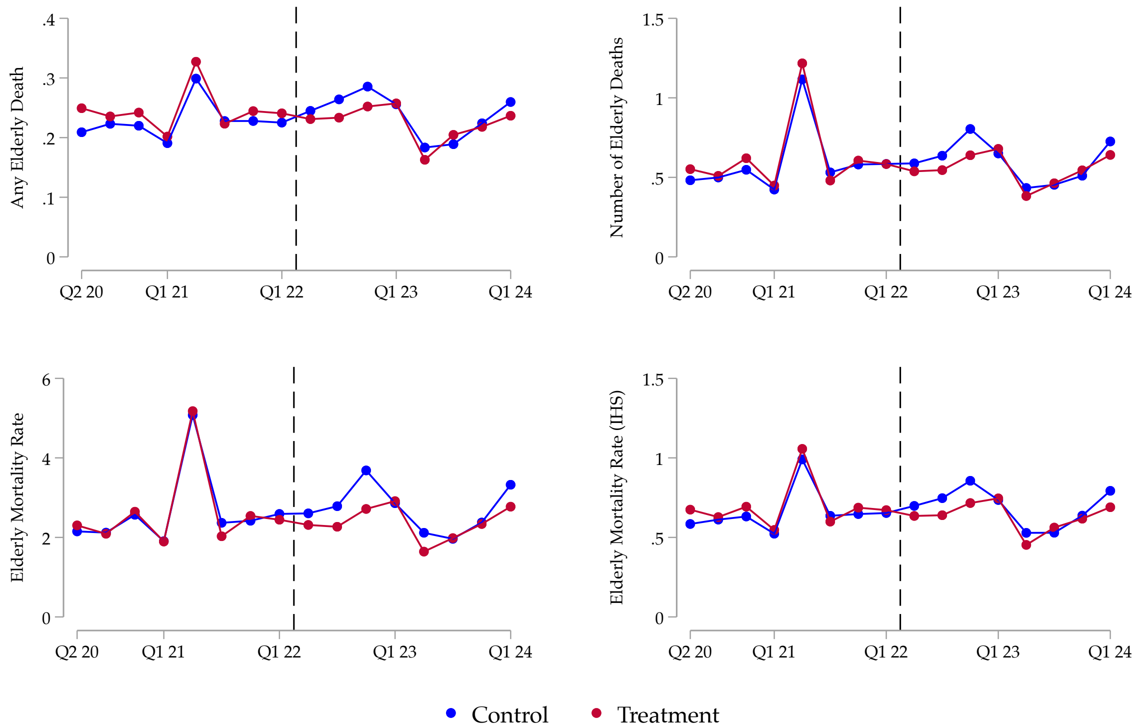
Notes: The figure shows weighted means for our patient visit outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A7: Trends in All-Age Mortality Outcomes by Treatment Group



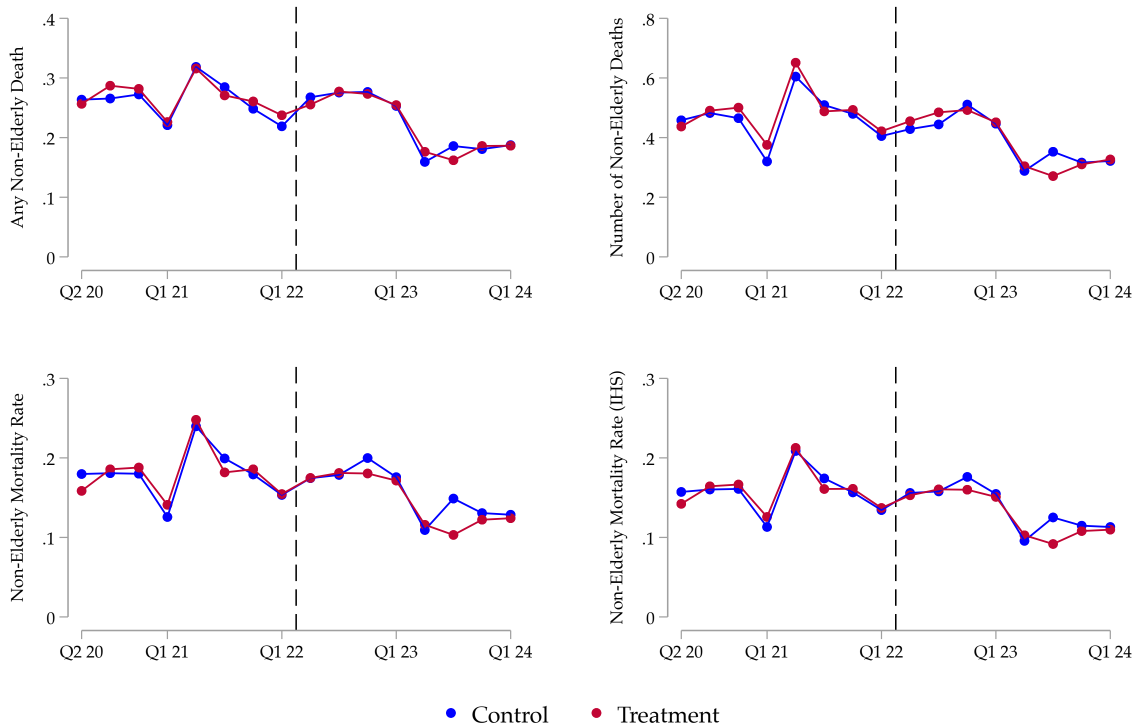
Notes: The figure shows weighted means for our all-age mortality outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A8: Trends in Elderly Mortality Outcomes by Treatment Group



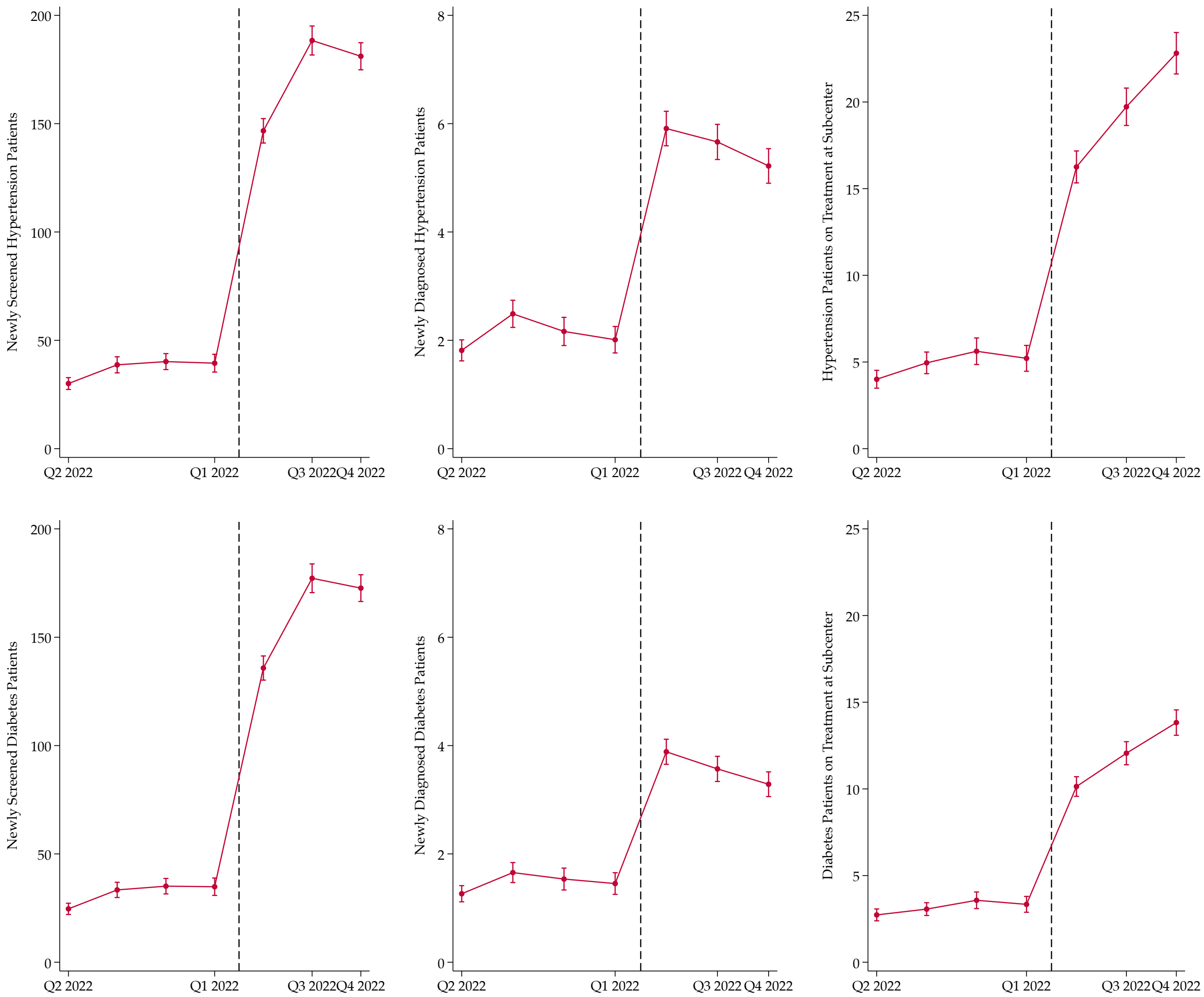
Notes: The figure shows weighted means for our elderly mortality outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A9: Trends in Non-Elderly Mortality Outcomes by Treatment Group



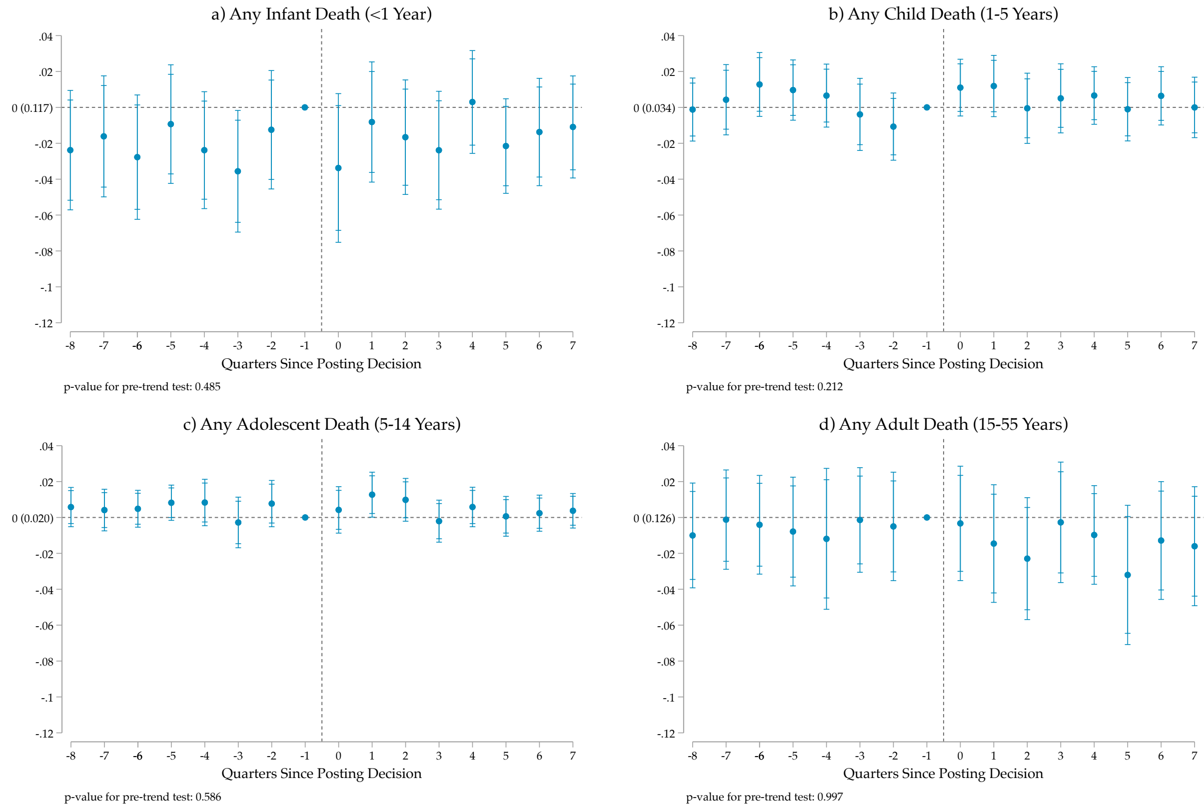
Notes: The figure shows weighted means for our non-elderly mortality outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A10: Hypertension and Diabetes Patients at Treatment group subcenters Over Time



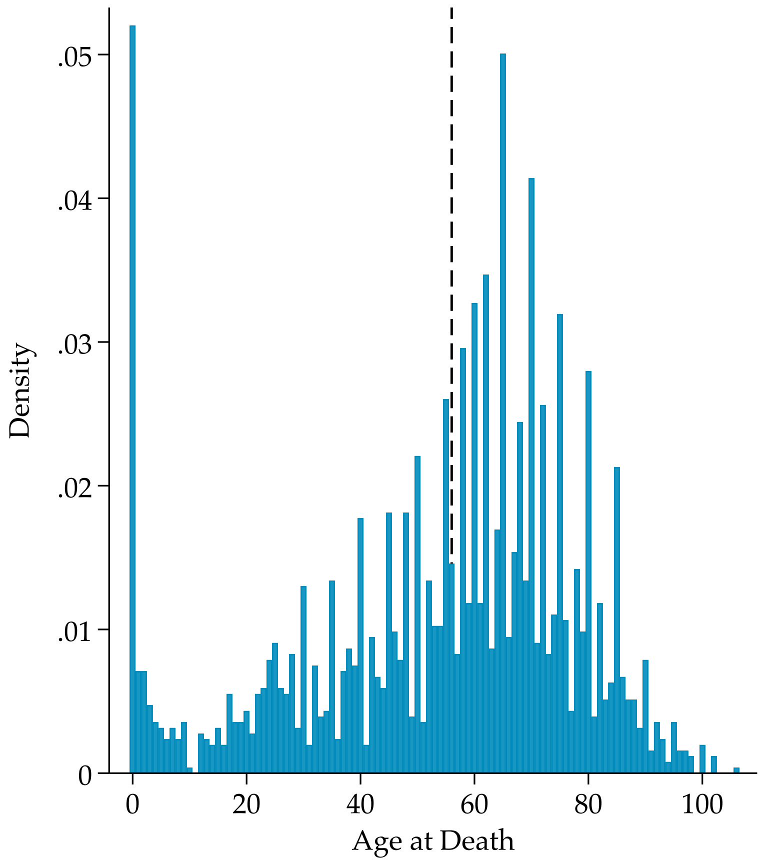
Notes: The figure shows the provision of chronic diseases services for treatment group subcenters over time. Information for control group subcenters areas not shown since they stopped reporting after the first batch of CHOs was posted. The sample consists of 2,545 treatment group subcenters and the sample period covers Q2 2021 until Q4 2022. Outcomes are obtained from the Health and Wellness Center Portal.

Figure A11: Effects of Community Health Officers on Health Outcomes for Other Age Groups



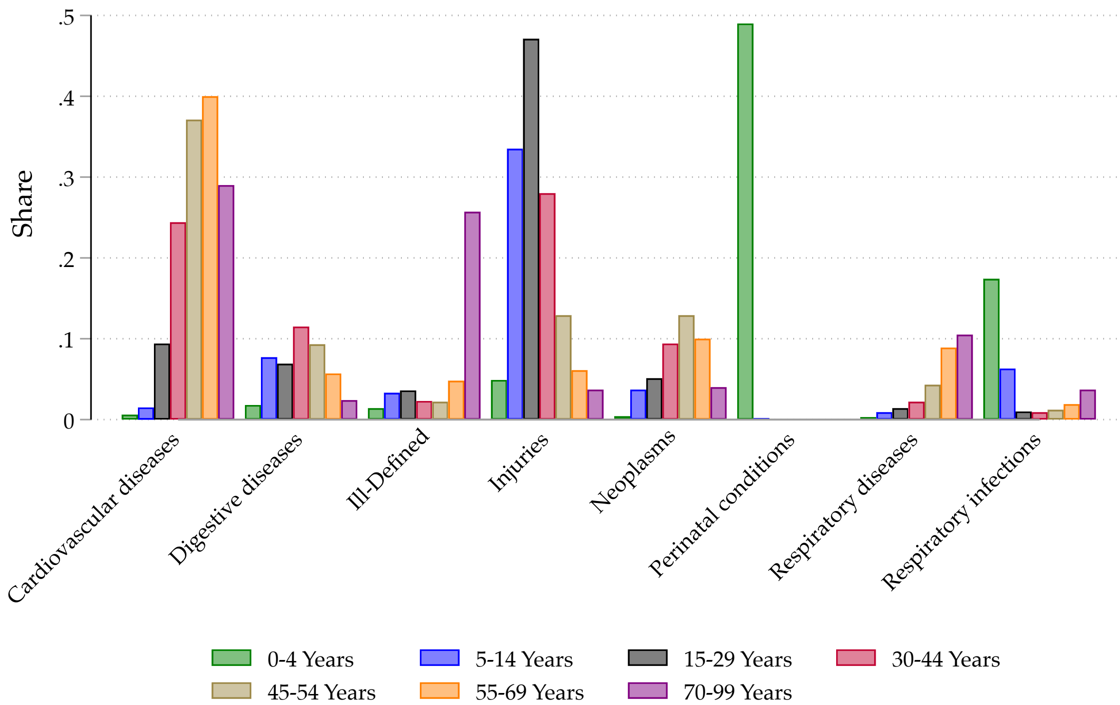
Notes: The figure shows weighted regression estimates of the effect of CHOs on health outcomes. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and quarter dummies, while controlling for year and subcenter fixed effects. The base quarter (Q1 2022) is omitted. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The figures show 95 percent confidence intervals based on standard errors clustered at the subcenter level. The number in parentheses on the y-axis shows the treatment group mean of the outcome in the base quarter. The p-value at the bottom of each figure corresponds to a chi-squared test of the null hypothesis that all interactions between treatment and pre-period dummies are statistically equal to zero. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A12: Distribution of Age at Death



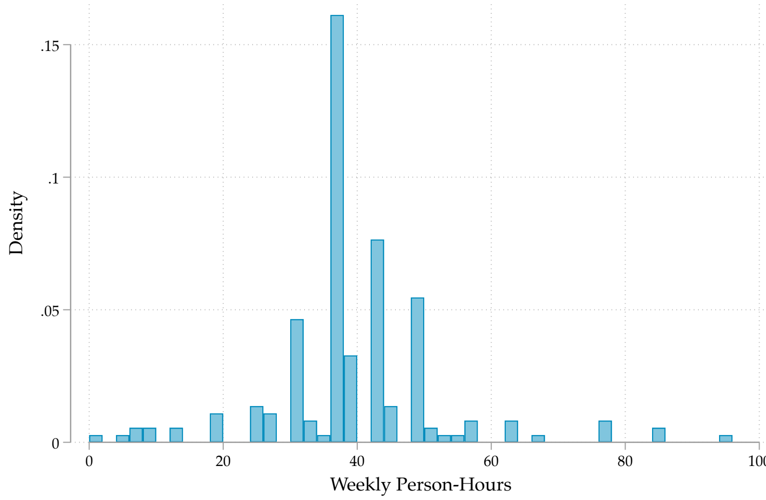
Notes: This sample reports the distribution of age at death using data from the 2017–2018 National Sample Survey.

Figure A13: Causes of Death by Age Group



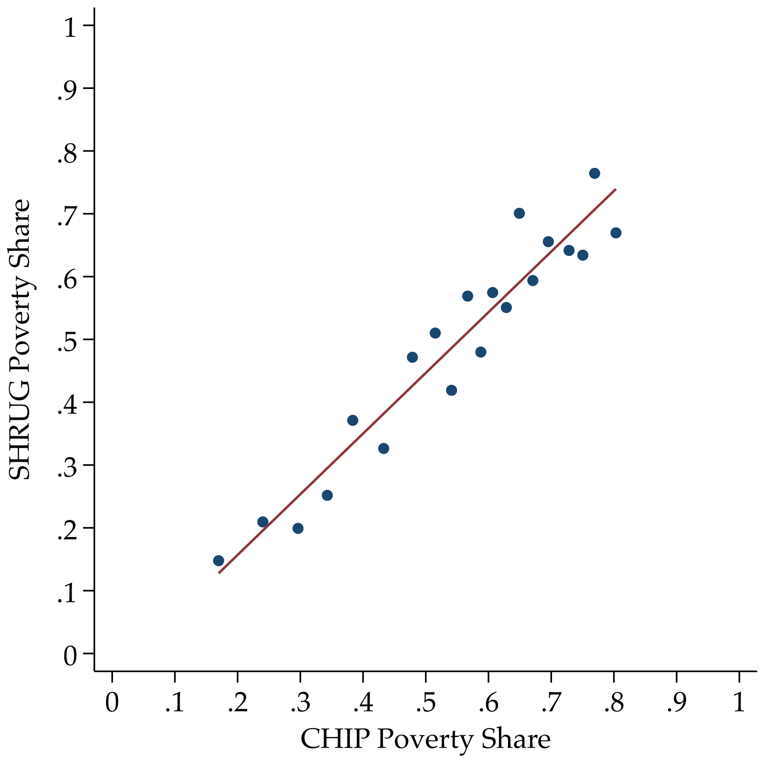
Notes: This sample reports the distribution of causes of death by age group using data from the official 'Causes of Death in India: 2017- 19' report based on data of the Sample Registration System.

Figure A14: Distribution of Subcenter Person-Hours at Baseline



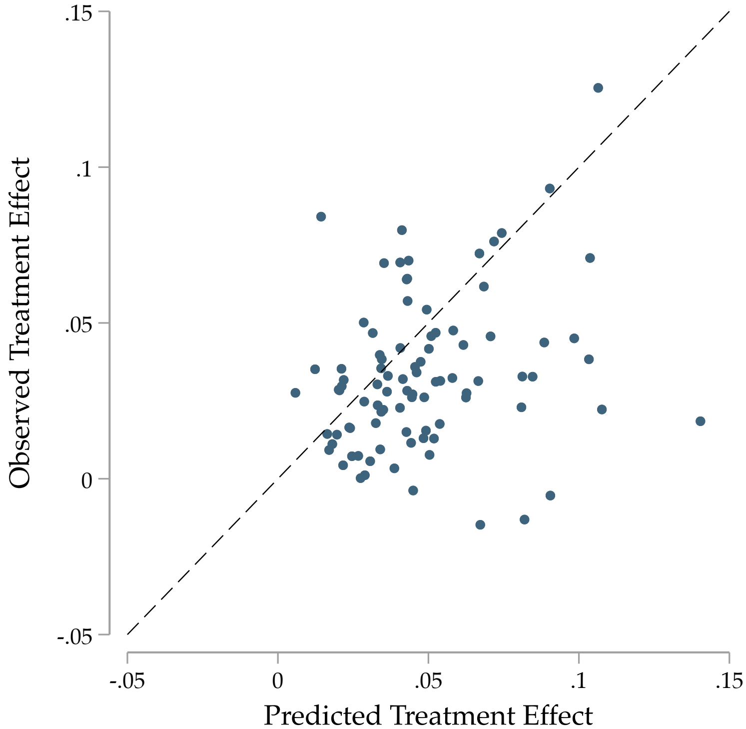
Notes: The figure shows the distribution of ANM working hours at baseline in our survey sample.

Figure A15: Village-Level Poverty Shares in SHRUG and CHIP data



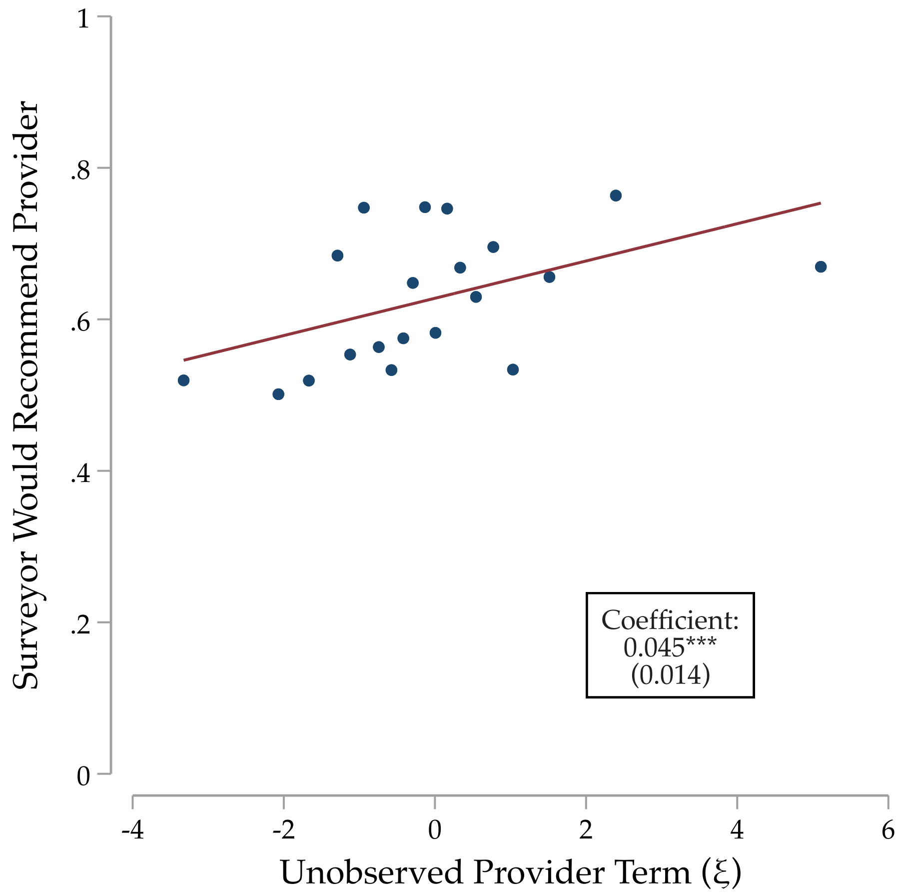
Notes: The figure is a binscatter plot of the imputed poverty shares in the SHRUG data against the imputed poverty shares in the CHIP data. SHRUG data generate imputed poverty shares by first regressing total household consumption on a set of asset and earnings information in the 2011-2012 Indian Human Development Survey data. They then combine these estimates with the asset and earnings information in the 2011 Socioeconomic and Caste Census microdata to predict household-level consumption (Asher et al., 2021). In the CHIP data, we define a household as poor if it meets fewer than three of the following conditions: (i) primary cooking fuel is kerosene or LPG, (ii) primary toilet has running water, (iii) primary drinking water comes from reverse osmosis system, tap water, or hand pump/tube well inside the house, (iv) household has electricity, (v) primary housing material is brick and concrete or wood, and (vi) primary transport is a motorcycle, car, tractor, or animal cart. The red line represents the fitted line from a linear regression.

Figure A16: Observed and Predicted Treatment Effects on Subcenter Market Shares



Notes: This figure plots the observed treatment effects on subcenter market shares against the predicted treatment effects by the demand model. The observed treatment effect come from the difference in patient visits between the pre- and post-period. The dashed line represents the 45-degree line.

Figure A17: Relationship Between Predicted Unobserved Provider Term and Surveyor Assessments



Notes: This figure is a binscatter plot of surveyor recommendation against the unobserved provider term (ξ) that we estimate in the demand model. Surveyor recommendations are based on a binary question in our provider surveyors that asked surveyors whether they would recommend the provider to a friend after the survey was completed. The regression includes controls for provider quality, price, person-hours, electricity, and for whether the observation is a PHC or a private provider. The red line represents the fitted line from a linear regression.

Table A1: Incentive Payments

S. No.	Indicators	CHO	ASHA	ANM
1	Proportion of Pregnant Women registered who received ANC as per scheduled due date	1000	67	100
2	Proportion of new-borns who received Home Based Newborn Care services	1000	67	100
3a	Proportion of Children up to 1 years of age who received immunization as per the due date	500	33	50
3b	Proportion of Children up to 2 years of age who received immunization as per the due date	500	33	50
4	Proportion of cases referred for TB screening	1000	67	100
5	Number of footfalls in the month	1000	67	100
6	Proportion of individuals 30 years and above whose CBAC form was filled	1000	67	100
7a	Proportion of individual 30 years or above screened for Hypertension	500	33	50
7b	Proportion of Hypertension patients on treatment	500	33	50
8a	Proportion of individual 30 years or above screened for Diabetes	500	33	50
8b	Proportion of Diabetes patients on treatment	500	33	50
9	Teleconsultation Services	1000	67	100
10	Wellness sessions Organised at HWCs	1000	67	100
11	Wellness Activities held as per annual Health calendar	1000	67	100
12	Monthly JAS meeting held with minimum 60% of the members	1000	67	100
13	Village Meetings	1000	67	100
14	MCHN held against planned	1000	67	100
15a	Monitoring of Referral cases – Upward	500	33	50
15b	Monitoring Of Referral cases - Downward/ Follow up	500	33	50
		15000	**1000**	**1500**

Notes: The table shows the monthly incentive payments CHOs, ASHAs, and ANMS receive for completing their targets. The payments are denoted in INR.

Table A2: Survey Completion Rates

	Public Providers		Private Providers			Households	
	Baseline Completion Rate	Endline Completion Rate	Baseline Completion Rate	Endline Round 1 Follow-Up Rate	Endline Round 2 Follow-Up Rate	Baseline Completion Rate	Endline Follow-Up Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.019	-0.020	-0.013	0.001	-0.008	-0.012	-0.018
	(0.029)	(0.014)	(0.064)	(0.050)	(0.073)	(0.022)	(0.031)
Control Group Mean	0.968	1.000	0.711	0.882	0.663	0.267	0.910
Observations	193	193	280	169	169	1,971	513

Notes: This table shows differences in survey completion rates by treatment status. In each column, we regress a dummy for whether the survey was completed on a treatment dummy. The sample in columns 1-2 consists of 193 ANMs. The sample in column 3 consists of 280 private healthcare providers that we mapped across the 193 catchment areas. The sample in columns 4 and 5 consists of private providers that were surveyed at baseline and still operational at endline. The second follow-up survey was done over the phone. The sample in column 6 consists of households that have registered pregnancy in the past five years. The sample in column 7 consists of all households that were surveyed at baseline. The endline survey was partly done in person.

Table A3: Comparison of Treatment and Control Areas in Survey Sample

	Original Sample				Reweighted Sample				
	Control Mean (1)	Control St. D. (2)	Treatment Coeff. (3)	Treatment St. E. (4)	Control Mean (5)	Control St. D. (6)	Treatment Coeff. (7)	Treatment St. E. (8)	N (9)
Panel A: Targeted Characteristics									
Distance to District HQ (in km)	67.09	[21.07]	-8.69***	(2.95)	57.21	[19.44]	1.18	(3.30)	193
Distance to Subdistrict HQ (in km)	23.03	[12.61]	-3.27*	(1.77)	18.72	[10.57]	1.05	(1.66)	193
Subcenter Has Electricity	0.69	[0.46]	-0.07	(0.07)	0.60	[0.49]	0.03	(0.09)	193
Subcenter Has Running Water	0.31	[0.46]	0.12*	(0.07)	0.39	[0.49]	0.04	(0.09)	193
Subcenter Building in Good Condition	0.31	[0.46]	0.16**	(0.07)	0.49	[0.50]	-0.03	(0.09)	193
Panel B: Catchment Area Characteristics									
Distance to Public Health Clinic (in km)	6.90	[5.31]	-0.36	(0.87)	7.66	[6.93]	-1.12	(1.28)	193
Total Population	3082.30	[1254.20]	266.88	(189.28)	3190.21	[1364.64]	158.97	(257.48)	193
Scheduled Caste Share	0.06	[0.05]	0.01*	(0.01)	0.07	[0.05]	0.00	(0.01)	185
Scheduled Tribe Share	0.46	[0.32]	-0.07	(0.05)	0.36	[0.29]	0.04	(0.05)	185
Female Share	0.49	[0.01]	-0.00	(0.00)	0.49	[0.01]	-0.00	(0.00)	185
Literacy Rate	0.47	[0.08]	0.01	(0.01)	0.49	[0.07]	-0.01	(0.01)	185
Land Ownership Rate	0.68	[0.14]	-0.00	(0.02)	0.70	[0.12]	-0.02	(0.02)	184
Employment Rate	0.50	[0.09]	-0.01	(0.01)	0.50	[0.09]	-0.02	(0.02)	185
(Imputed) Consumption per Capita (in INR)	15264.34	[3352.43]	766.40	(482.08)	16417.50	[3236.80]	-386.75	(559.93)	184
Panel C: Average Facility Indicators in Q1 2022									
Number of Patients	208.82	[145.82]	19.65	(19.62)	256.54	[195.29]	-28.06	(34.89)	188
Number of Acute Heart Disease Patients	0.04	[0.25]	-0.04*	(0.03)	0.01	[0.13]	-0.01*	(0.01)	188
Number of Hypertension Patients	3.43	[6.87]	-0.18	(1.03)	3.35	[6.35]	-0.10	(1.16)	188
Maternal and Child Health Services Index	0.00	[0.78]	0.15	(0.12)	0.01	[0.81]	0.14	(0.16)	188
All-Age Mortality Rate	0.24	[0.49]	0.03	(0.07)	0.32	[0.66]	-0.06	(0.14)	188
Elderly Mortality Rate	0.84	[2.57]	0.52	(0.40)	0.88	[2.42]	0.48	(0.47)	188

Notes: This table shows the means of selected covariates for the original and reweighted survey sample. Panel A reports on the covariates used to estimate the propensity score for the inverse probability weighting. Panel B reports on covariates at the catchment area level. Catchment-level covariates are calculated based on population-weighted averages across all villages in the catchment area of the respective subcenter. Panel C reports on the main outcomes in the pre-treatment reference period. Columns (1)-(4) present the original sample and columns (5)-(8) present the reweighted sample. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Differences in sample sizes across variables reflect missing data. Columns (1) and (5) report the control mean of the dependent variable for each relevant sample. Columns (3), and (7) report the difference in the dependent variable from OLS regressions of each outcome on an indicator variable for CHO assignment.

Table A4: Results from Patient Exit Surveys at Endline

	Overall Satisfaction	Number of Questions Asked	Measured Blood Pressure	Any Antibiotics	Referred
	(1)	(2)	(3)	(4)	(5)
Treatment	0.376**	0.545**	0.231***	0.051	0.085***
	(0.180)	(0.209)	(0.070)	(0.077)	(0.031)
Mean of Outcome	4.108	1.725	0.066	0.105	0.010
Observations	172	173	177	177	174

Notes: This table shows the effects of CHOs on healthcare quality according to patient exit survey. In each column, we regress the outcome on an indicator variable for whether a CHO was assigned to the subcenter in March 2022. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample consists of 177 patients who visited the subcenter for outpatient care services. The satisfaction outcome ranges from 1 (lowest) to 5 (highest). See Data Appendix for details on variable definitions.

Table A5: Differences in Additional Patient Visits Between Treatment and Control Group Subcenters

	Number of Patient Visits					
	Eye (1)	Oral (2)	Mental (3)	Pallative (4)	COPD (5)	Asthma (6)
Treatment	2.752***	1.604***	0.061**	0.162***	0.149***	0.366***
	(0.434)	(0.310)	(0.026)	(0.026)	(0.033)	(0.079)
Control Group Mean	9.561	6.179	0.180	0.101	0.156	0.599
Observations	19,493	19,493	19,493	19,493	19,493	19,493

Notes: This table shows differences in patient visits for six patient types between treatment and control group subcenter in the post-period. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2023 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A6: Effects of Community Health Officers on Maternal and Child Health Services Index Components

		Maternal & Child Health Services Index Components				
		N Pregnant Women				
	N Pregnant Women Registered	4+ Prenatal Care Visits	360 Calcium Tablets	1st Tetanus Shot	N Children Fully Immunized	N Postnatal Care Visits
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.038	0.067	-0.364*	0.005	0.052	0.131
	(0.133)	(0.142)	(0.214)	(0.100)	(0.111)	(0.133)
Control Group Mean (Pre-Periods)	16.640	10.319	10.908	9.942	14.490	8.923
Treatment Group Mean (Pre-Periods)	16.869	10.630	11.371	10.178	14.744	9.284
Counterfactual Treatment Group Mean (Post-Periods)	16.406	12.058	13.462	10.308	14.544	10.364
Observations	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on the components of the maternal and child health services index. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A7: Effects of Community Health Officers on Mortality Outcomes for Other Age Groups

	Deaths by Age Group			
	Infant (1 < Year) (1)	Child (1-4 Years) (2)	Adolescent (5-14 Years) (3)	Adult (15-55 Years) (4)
Panel A: Any Death				
Treatment × Post	0.003	0.003	0.000	-0.009
	(0.007)	(0.003)	(0.002)	(0.009)
Control Group Mean (Pre-Periods)	0.141	0.035	0.016	0.136
Treatment Group Mean (Pre-Periods)	0.138	0.034	0.017	0.144
Counterfactual Treatment Group Mean (Post-Periods)	0.107	0.026	0.013	0.127
Observations	9,812	9,812	9,812	9,812
Panel B: Mortality Rate (IHS)				
Treatment × Post	0.008	0.010	0.000	-0.012
	(0.173)	(0.010)	(0.002)	(0.009)
Control Group Mean (Pre-Periods)	2.902	0.115	0.015	0.133
Treatment Group Mean (Pre-Periods)	2.814	0.109	0.016	0.139
Counterfactual Treatment Group Mean (Post-Periods)	2.232	0.086	0.012	0.123
Observations	9,812	9,812	9,812	9,812

Notes: This table shows the effects of CHOs on mortality outcomes for different age groups. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A8: Effects of Community Health Officers on Elderly Mortality by Causes of Death

	Elderly (56+ Years) Deaths			
	Chronic	Acute	Accident	Unknown Cause
	(1)	(2)	(3)	(4)
Panel A: Any Death				
Treatment × Post	-0.009	0.001	0.002	-0.038***
	(0.007)	(0.004)	(0.002)	(0.009)
Control Group Mean (Pre-Periods)	0.097	0.043	0.009	0.153
Treatment Group Mean (Pre-Periods)	0.103	0.045	0.011	0.167
Counterfactual Treatment Group Mean (Post-Periods)	0.090	0.041	0.011	0.196
Observations	9,818	9,818	9,818	9,818
Panel B: Mortality Rate (IHS)				
Treatment × Post	-0.015	0.001	0.003	-0.104***
	(0.017)	(0.010)	(0.003)	(0.027)
Control Group Mean (Pre-Periods)	0.243	0.097	0.014	0.422
Treatment Group Mean (Pre-Periods)	0.251	0.104	0.018	0.449
Counterfactual Treatment Group Mean (Post-Periods)	0.212	0.095	0.018	0.528
Observations	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on elderly mortality for different causes of death. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A9: Robustness Checks Related to Reporting Bias

	At Least One Pre-Period Elderly Death (PCTS)				Civil Registration System
	Any Elderly Death	Number of Elderly Deaths	Elderly Mortality Rate	Elderly Mortality Rate (IHS)	Number of Elderly Deaths
	(1)	(2)	(3)	(4)	(5)
ATET					
Treatment × Post	-0.057***	-0.150**	-0.582**	-0.167***	-0.123*
	(0.019)	(0.064)	(0.284)	(0.057)	(0.074)
Control Group Mean (Pre-Periods)	0.469	1.225	5.485	1.360	6.214
Treatment Group Mean (Pre-Periods)	0.490	1.252	5.272	1.387	6.629
Counterfactual Treatment Group Mean (Post-Periods)	0.465	1.180	4.942	1.322	4.676
Observations	5,018	5,018	5,018	5,018	7,670

Notes: This table shows the effects of CHOs on healthcare services. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. In Columns (1)-(4), the sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. In Column (5), the sample consists of 3,835 gram panchayats and the sample period covers Q2 2021 until Q1 2023. The outcome is obtained from the Civil Registration System and the treatment dummy is equal to one if at least half of the subcenters in a gram panchayat received a CHO in March 2022. See Data Appendix for details on variable definitions.

Table A10: Effects of Community Health Officers on Hospitalizations by Age Group

	Any Hospitalization		
	Child (1)	Adult (2)	Elderly (3)
Treatment × Post	0.005 (0.013)	-0.019 (0.014)	-0.042* (0.022)
Control Group Mean (Baseline)	0.011	0.046	0.013
Treatment Group Mean (Baseline)	0.008	0.065	0.040
Counterfactual Treatment Group Mean (Endline)	0.003	0.043	0.070
Observations	2,083	2,230	1,264

Notes: This table shows the effects of CHOs on hospitalizations at the household member level for different age groups. The regression coefficients are estimated by regressing the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Outcomes are obtained from our household surveys. See Data Appendix for details on variable definitions.

Table A11: Robustness Regarding Common Support Restrictions

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Districts with 10-90% Assignment Rate, Minimal Common Support Restriction								
Treatment × Post	220.379***	6.602***	0.027***	-0.002	-0.021**	-0.024**	-0.026***	-0.077***
	(7.657)	(0.556)	(0.006)	(0.011)	(0.010)	(0.011)	(0.009)	(0.028)
Observations	10,592	10,592	10,592	10,592	10,592	10,592	10,592	10,592
Panel B: Districts with 10-90% Assignment Rate , 1% Common Support Restriction								
Treatment × Post	220.996***	6.574***	0.026***	-0.000	-0.021**	-0.024**	-0.026***	-0.077***
	(7.678)	(0.564)	(0.006)	(0.011)	(0.010)	(0.011)	(0.009)	(0.028)
Observations	10,388	10,388	10,388	10,388	10,388	10,388	10,388	10,388
Panel C: Districts with 10-90% Assignment Rate, 5% Common Support Restriction								
Treatment × Post	213.428***	5.950***	0.026***	-0.001	-0.024**	-0.027**	-0.031***	-0.091***
	(7.957)	(0.603)	(0.006)	(0.011)	(0.011)	(0.012)	(0.010)	(0.030)
Observations	9,186	9,186	9,186	9,186	9,186	9,186	9,186	9,186
Panel D: Districts with 15-85% Assignment Rate, Minimal Common Support Restriction								
Treatment × Post	225.371***	6.729***	0.026***	-0.002	-0.019*	-0.024**	-0.026***	-0.080***
	(8.145)	(0.617)	(0.006)	(0.011)	(0.011)	(0.012)	(0.009)	(0.028)
Observations	9,138	9,138	9,138	9,138	9,138	9,138	9,138	9,138
Panel E: Districts with 15-85% Assignment Rate, 1% Common Support Restriction								
Treatment × Post	226.487***	6.670***	0.025***	-0.001	-0.019*	-0.023**	-0.026***	-0.079***
	(8.174)	(0.625)	(0.007)	(0.011)	(0.011)	(0.011)	(0.009)	(0.028)
Observations	8,936	8,936	8,936	8,936	8,936	8,936	8,936	8,936
Panel F: Districts with 15-85% Assignment Rate, 2.5% Common Support Restriction								
Treatment × Post	226.487***	6.670***	0.025***	-0.001	-0.019*	-0.023**	-0.026***	-0.079***
	(8.174)	(0.625)	(0.007)	(0.011)	(0.011)	(0.011)	(0.009)	(0.028)
Observations	8,936	8,936	8,936	8,936	8,936	8,936	8,936	8,936
Panel G: Districts with 15-85% Assignment Rate, 5% Common Support Restriction								
Treatment × Post	217.625***	5.946***	0.026***	-0.000	-0.021*	-0.024**	-0.033***	-0.095***
	(8.593)	(0.668)	(0.007)	(0.012)	(0.012)	(0.012)	(0.010)	(0.031)
Observations	7,850	7,850	7,850	7,850	7,850	7,850	7,850	7,850

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for different sample restrictions. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A12: Effects of Community Health Officers on Alternative Top-Coding Strategies

	All-Age Mortality			Elderly (56+) Mortality		
	Number of Deaths (1)	Mortality Rate (2)	Mortality Rate (IHS) (3)	Number of Deaths (4)	Mortality Rate (5)	Mortality Rate (IHS) (6)
Panel A: No Top-Coding						
Treatment × Post	-0.148**	-0.314	-0.030*	-0.131***	-4.427	-0.096***
	(0.059)	(0.677)	(0.017)	(0.049)	(7.425)	(0.033)
Control Group Mean (Pre-Periods)	1.122	1.655	0.330	0.638	13.924	0.678
Treatment Group Mean (Pre-Periods)	1.232	0.938	0.325	0.723	6.951	0.705
Counterfactual Treatment Group Mean (Post-Periods)	1.131	1.481	0.318	0.702	13.388	0.739
Observations	9,818	9,818	9,818	9,818	9,818	9,818
Panel C: Top-Coding 95%						
Treatment × Post	-0.107***	-0.037**	-0.030***	-0.081***	-0.300***	-0.093***
	(0.033)	(0.015)	(0.011)	(0.026)	(0.108)	(0.028)
Control Group Mean (Pre-Periods)	0.921	0.357	0.292	0.517	2.178	0.637
Treatment Group Mean (Pre-Periods)	0.976	0.360	0.297	0.557	2.236	0.675
Counterfactual Treatment Group Mean (Post-Periods)	0.958	0.354	0.293	0.582	2.341	0.709
Observations	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for alternative top-coding strategies. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A13: Alternative Weighting

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Propensity Scores using LASSO								
Treatment × Post	220.335***	6.499***	0.029***	-0.001	-0.019**	-0.018*	-0.022**	-0.060**
	(8.851)	(0.552)	(0.008)	(0.013)	(0.010)	(0.010)	(0.008)	(0.026)
Control Group Mean (Pre-Periods)	245.560	4.075	0.045	0.044	0.366	0.314	0.235	0.681
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	366.028	8.358	0.031	0.145	0.352	0.293	0.246	0.692
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818
Panel B: Entropy Balancing								
Treatment × Post	223.433***	6.496***	0.027***	0.000	-0.016	-0.017	-0.020**	-0.057**
	(8.270)	(0.550)	(0.007)	(0.012)	(0.010)	(0.011)	(0.009)	(0.026)
Control Group Mean (Pre-Periods)	238.536	3.896	0.041	0.000	0.361	0.314	0.230	0.670
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	362.930	8.361	0.034	0.143	0.349	0.292	0.245	0.689
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for alternative top-coding strategies. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. In Panel A, subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores estimated based on a lasso regression that uses all the covariates listed in Table 1. In Panel B, subcenter-level weights for the control group are constructed by entropy balancing using the first-order moments of the following variables: distance to the district HQ, distance to the subdistrict HQ, and district fixed effects. The sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A14: Alternative Difference-in-Differences Estimators

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Subcenter-Specific Linear Trends								
Treatment × Post	207.418***	3.590***	0.008	0.006	-0.032*	-0.029	-0.030**	-0.091*
	(7.719)	(0.422)	(0.011)	(0.016)	(0.019)	(0.020)	(0.015)	(0.047)
Control Group Mean (Pre-Periods)	236.287	3.746	0.036	-0.007	0.362	0.311	0.228	0.660
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	378.946	11.268	0.053	0.138	0.365	0.304	0.255	0.723
Observations	78,544	78,544	78,401	78,544	78,544	78,544	78,544	78,544
Panel B: Double-Robust Estimator								
Treatment × Post	214.849***	6.176***	0.026***	-0.003	-0.027***	-0.029***	-0.031***	-0.088***
	(7.777)	(0.571)	(0.007)	(0.012)	(0.010)	(0.011)	(0.009)	(0.027)
Control Group Mean (Pre-Periods)	236.287	3.746	0.036	-0.007	0.362	0.311	0.228	0.660
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	371.514	8.681	0.035	0.147	0.360	0.304	0.255	0.721
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for alternative difference-in-differences estimators. In Panel A, the regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy, year and subcenter fixed effects, and an interaction between linear time trends and subcenter fixed effects. In Panel B, the regression coefficients are estimated using the double robust estimator proposed by Sant'Anna and Zhao (2020). The sample period covers Q2 2020 until Q1 2024. While the sample in Panel A consists of subcenter-quarter observations, the sample in Panel is aggregated at the subcenters-pre/post level. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A15: Alternative Empirical Strategy Based on Closest Subcenter

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post	225.546***	5.980***	0.040***	0.005	-0.034**	-0.036**	-0.032**	-0.094**
	(9.851)	(0.863)	(0.008)	(0.016)	(0.016)	(0.018)	(0.014)	(0.043)
Control Group Mean (Pre-Periods)	249.683	3.703	0.042	-0.067	0.370	0.328	0.243	0.710
Treatment Group Mean (Pre-Periods)	239.207	3.433	0.040	0.038	0.372	0.315	0.246	0.701
Counterfactual Treatment Group Mean (Post-Periods)	362.791	9.372	0.022	0.146	0.371	0.315	0.259	0.738
Observations	11,020	11,020	11,020	11,020	11,020	11,020	11,020	11,020

Notes: This table shows the effects of CHOs on healthcare services and health outcomes. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Each treatment group subcenter is matched to the closest control group subcenters (with replacement). The sample consists of 5,510 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A16: Heterogeneity by CHO Quality

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post × CHO Vign. > ANM Vign.	236.618***	3.221**	-0.007	-0.105	-0.147***	-0.153***	-0.062	-0.231**
	(30.478)	(1.340)	(0.039)	(0.069)	(0.045)	(0.042)	(0.039)	(0.109)
Treatment × Post × CHO Vign. ≤ ANM Vign.	186.027***	3.103**	0.002	0.002	-0.038	-0.044	-0.002	-0.030
	(29.756)	(1.256)	(0.037)	(0.062)	(0.049)	(0.039)	(0.038)	(0.105)
p-value: Coef 1 = Coef 2	0.174	0.948	0.887	0.253	0.047	0.013	0.170	0.076
CHO Vign. > ANM Vign.:								
Treatment Group Mean (Pre-Periods)	236.545	5.062	0.068	0.217	0.366	0.263	0.161	0.436
Counterfactual Treatment Group Mean (Post-Periods)	332.153	8.219	0.102	0.184	0.338	0.275	0.151	0.453
Observations	84	84	84	84	84	84	84	84
CHO Vign. ≤ ANM Vign.:								
Treatment Group Mean (Pre-Periods)	255.564	2.795	0.021	0.156	0.351	0.227	0.160	0.398
Counterfactual Treatment Group Mean (Post-Periods)	348.481	6.147	0.046	0.131	0.315	0.234	0.132	0.370
Observations	94	94	94	94	94	94	94	94
Control Group:								
Control Group Mean (Pre-Periods)	240.003	3.525	0.036	0.136	0.349	0.211	0.157	0.360
Observations	182	182	182	182	182	182	182	182

Notes: This table shows the effects of CHOs on healthcare services and health outcomes by changes in subcenter quality. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and a dummy for whether the checklist completion rate in the medical vignettes of the new CHOs is higher than the checklist completion rate of the ANM at baseline, while controlling for year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System and changes in person-hours are based on surveys with ANMs and CHOs. See Data Appendix for details on variable definitions.

Table A17: Heterogeneity by Change in Subcenter Person-Hours

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post × Increase in Person-Hours > Median	296.041***	3.071***	0.039	-0.091	-0.066	-0.088**	-0.011	-0.111
	(29.400)	(1.146)	(0.046)	(0.088)	(0.055)	(0.043)	(0.044)	(0.116)
Treatment × Post × Increase in Person-Hours ≤ Median	233.038***	5.695***	-0.032	-0.046	-0.122**	-0.129***	-0.065	-0.254**
	(26.869)	(1.421)	(0.035)	(0.055)	(0.049)	(0.043)	(0.043)	(0.115)
p-value: Coef 1 = Coef 2	0.067	0.105	0.185	0.610	0.300	0.343	0.211	0.200
Increase in Person-Hours > Median:								
Treatment Group Mean (Pre-Periods)	238.289	4.989	0.022	0.217	0.339	0.225	0.139	0.350
Counterfactual Treatment Group Mean (Post-Periods)	281.631	6.779	0.042	0.214	0.300	0.239	0.122	0.387
Observations	90	90	90	90	90	90	90	90
Increase in Person-Hours ≤ Median:								
Treatment Group Mean (Pre-Periods)	249.231	2.779	0.062	0.201	0.394	0.267	0.185	0.483
Counterfactual Treatment Group Mean (Post-Periods)	292.573	4.569	0.082	0.198	0.355	0.280	0.168	0.519
Observations	104	104	104	104	104	104	104	104
Control Group:								
Control Group Mean (Pre-Periods)	240.003	3.525	0.036	0.136	0.349	0.211	0.157	0.360
Observations	182	182	182	182	182	182	182	182

Notes: This table shows the effects of CHOs on healthcare services and health outcomes by changes in subcenter person-hours. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and a dummy for whether the change in person-hours was above the median, while controlling for year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System and changes in person-hours are based on surveys with ANMs and CHOs. See Data Appendix for details on variable definitions.

Table A18: Additional Effects of Community Health Officers on Private Providers

	Quality Index Components				
	Length of Medical Degree	Number of Training Workshops Attended (Past 12 Months)	Checklist Completion Rate (Vignettes)	Injection Rate	Antibiotic Dispensing Rate
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled					
Treatment × Post	0.408	0.203	0.004	-0.024	0.031
	(0.262)	(0.200)	(0.015)	(0.072)	(0.077)
Control Group Mean (Baseline)	2.765	0.280	0.083	0.189	0.366
Treatment Group Mean (Baseline)	2.459	0.529	0.091	0.204	0.339
Counterfactual Treatment Group Mean (Endline)	2.130	0.499	0.069	0.298	0.341
Observations	476	427	342	364	359
Panel B: Heterogeneity by Distance					
Treatment × Post × Close Dist. Providers	0.479	0.791*	0.034	-0.099	0.083
	(0.415)	(0.435)	(0.022)	(0.094)	(0.165)
Treatment × Post × Medium Dist. Providers	0.316	0.111	-0.035	-0.046	-0.205*
	(0.442)	(0.311)	(0.023)	(0.095)	(0.120)
Treatment × Post × Far Dist. Providers	0.277	-0.291	0.014	-0.000	0.085
	(0.511)	(0.392)	(0.027)	(0.104)	(0.100)
p-value: Coef 1 = Coef 2	0.794	0.203	0.030	0.693	0.173
p-value: Coef 1 = Coef 3	0.760	0.085	0.582	0.480	0.994
p-value: Coef 2 = Coef 3	0.955	0.420	0.196	0.736	0.064
Close Dist. Providers:					
Control Group Mean (Baseline)	2.224	0.590	0.121	0.240	0.454
Treatment Group Mean (Baseline)	2.423	0.409	0.102	0.205	0.286
Counterfactual Treatment Group Mean (Endline)	2.200	-0.081	0.041	0.349	0.308
Observations	159	143	117	120	120
Medium Dist. Providers:					
Control Group Mean (Baseline)	2.808	0.087	0.043	0.081	0.144
Treatment Group Mean (Baseline)	2.310	0.720	0.083	0.177	0.447
Counterfactual Treatment Group Mean (Endline)	2.049	0.832	0.112	0.313	0.572
Observations	159	147	111	121	120
Far Dist. Providers:					
Control Group Mean (Baseline)	3.124	0.215	0.082	0.201	0.420
Treatment Group Mean (Baseline)	2.633	0.429	0.088	0.228	0.285
Counterfactual Treatment Group Mean (Endline)	2.286	0.716	0.051	0.309	0.269
Observations	158	137	114	123	119

Notes: This table shows the effects of CHOs on the components of the private provider quality index. In Panel A, we regress the outcome on the treatment dummy, survey round dummies, and an interaction between the treatment dummy and the post-period dummy. In Panel B, we regress the outcome on the treatment dummy, the post-survey dummies, and an interaction between the treatment dummy and the post-period dummy, separately for each distance tercile between the private provider and the subcenter. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample pools the baseline survey round and two endline survey rounds. See Data Appendix for details on variable definitions.

Table A19: Heterogeneity by Number of Private Providers at Baseline

				Quality Index Components				
	Number of Patients	Typical Fee	Quality Index	Length of Medical Degree	Number of Training Workshops Attended (Past 12 Months)	Checklist Completion Rate (Vignettes)	Injection Rate	Antibiotic Dispensing Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Heterogeneity by Distance								
Treatment × Post × One Private Provider at Baseline	-12.021	-12.595	0.380**	0.952**	0.052	0.013	-0.041	0.071
	(24.879)	(21.044)	(0.175)	(0.475)	(0.296)	(0.023)	(0.077)	(0.101)
Treatment × Post × At Least Two Private Providers at Baseline	-4.052	12.947	0.204	0.082	0.332	-0.001	0.011	-0.011
	(33.321)	(21.003)	(0.143)	(0.290)	(0.292)	(0.019)	(0.091)	(0.103)
p-value: Coef 1 = Coef 2	0.848	0.392	0.438	0.121	0.502	0.644	0.664	0.573
Only One Private Provider at Baseline :								
Control Group Mean (Baseline)	95.490	87.175	0.059	2.224	0.590	0.121	0.240	0.454
Treatment Group Mean (Baseline)	124.739	84.400	-0.000	2.423	0.409	0.102	0.205	0.286
Counterfactual Treatment Group Mean (Endline)	125.182	115.273	-0.295	1.727	0.657	0.061	0.292	0.321
Observations	137	143	147	147	132	110	108	108
At Least Two Private Provider at Baseline:								
Control Group Mean (Baseline)	89.253	78.368	-0.060	2.808	0.087	0.043	0.081	0.144
Treatment Group Mean (Baseline)	115.083	114.167	-0.027	2.310	0.720	0.083	0.177	0.447
Counterfactual Treatment Group Mean (Endline)	154.109	97.149	-0.112	2.283	0.610	0.078	0.257	0.378
Observations	138	140	149	149	137	105	111	110

Notes: This table shows the effects of CHOs on private providers by the number of private providers in the subcenter catchment area at baseline. We regress the outcome on the treatment dummy, the post-survey dummies, and an interaction between the treatment dummy and the post-period dummy, separately for catchment areas with only one or at least two private providers at baseline. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample pools the baseline survey round and two endline survey rounds. See Data Appendix for details on variable definitions.

Table A20: Cost Shifters for Private Providers

	Price (1)
Supplier Location = Chavand	-84.605***
	(6.945)
Supplier Location = Fateh Nagar	-20.438
	(18.488)
Supplier Location = Salumbar	38.908**
	(15.684)
Supplier Location = Udaipur	10.511
	(10.594)
Mean of Outcome	111.465
F-Stat	95.231
Observations	258

Notes: This table shows the effects of supplier locations on the prices of private providers. The regression is restricted to private providers and controls for whether the provider has electricity. Robust standard errors are reported in parantheses.

Table A21: Model Fit

Micro-Moments	Data	Model
$\mathbb{E}[poor_i \mid \{i \text{ chooses a subcenter}\}]$	0.427	0.434
$\mathbb{E}[poor_i \mid \{i \text{ chooses a PHC}\}]$	0.330	0.345
$\mathbb{E}[poor_i \mid \{i \text{ chooses a private provider}\}]$	0.250	0.263
$\mathbb{E}[lives\_in\_PHC\_location_i \mid \{i \text{ chooses a PHC}\}]$	0.674	0.674
$\mathbb{C}(x_j, x_{k(-j)} \mid j, k \neq 0)$	0.051	0.035

Notes: This table reports the observed and predicted micro moments in the data and demand model. The first four micro moments come from the CHIP household census data. The last micro moment comes from our household survey.

Table A22: Counterfactual Analysis by Poverty Status

Counterfactuals	Market Shares			Average Quality	Δ All-Age Mortality Rate (in %)
	Subcenter	PHC	Private		
	(1)	(2)	(3)	(4)	(5)
Panel A: Poor Households					
0) Baseline	0.098	0.265	0.058	1.608	
1) Full Treatment Effect	0.266	0.222	0.057	2.024	-0.109
2) Only Increase in Subcenter Quality	0.151	0.252	0.054	1.795	-0.049
3) Only Increase in Subcenter Person-Hours	0.186	0.244	0.051	1.670	-0.016
4) No Effects on Private Providers	0.269	0.224	0.045	1.996	-0.102
5) Private Sector Ban	0.104	0.274	0.000	1.578	0.008
6) Private Sector Ban (+ CHOs)	0.282	0.229	0.000	1.992	-0.101
7) Observed CHO Allocation	0.195	0.240	0.057	1.852	-0.064
8) Random CHO Allocation	0.189	0.242	0.057	1.838	-0.060
9) Optimal CHO Allocation	0.198	0.243	0.057	1.882	-0.072
Panel B: Non-Poor Households					
0) Baseline	0.096	0.351	0.128	2.194	
1) Full Treatment Effect	0.285	0.292	0.127	2.615	-0.111
2) Only Increase in Subcenter Quality	0.174	0.330	0.116	2.396	-0.053
3) Only Increase in Subcenter Person-Hours	0.180	0.329	0.115	2.227	-0.009
4) No Effects on Private Providers	0.295	0.297	0.098	2.559	-0.096
5) Private Sector Ban	0.112	0.380	0.000	2.127	0.018
6) Private Sector Ban (+ CHOs)	0.291	0.325	0.000	2.501	-0.081
7) Observed CHO Allocation	0.203	0.317	0.126	2.433	-0.063
8) Random CHO Allocation	0.201	0.318	0.127	2.431	-0.062
9) Optimal CHO Allocation	0.210	0.322	0.126	2.486	-0.077

Notes: This table presents the results of the the counterfactual analysis, separately for poor and non-poor households. The different scenarios are as follows. Row (0): the baseline model. Row (1): full treatment effect in which subcenter quality and person-hours increase and private providers improve their quality. Row (2): only increase in subcenter quality, no change in subcenter person-hours or private provider quality. Row (3): only increase in subcenter person-hours, no change in subcenter or private provider quality. Row (4): increase in subcenter quality and person-hours, but no change in private provider quality. Row (5): ban on private providers, no change in subcenter quality and person-hours. Row (6): ban on private providers and increase in subcenter quality and person-hours in all subcenter locations. Row (7): 95 out of the 186 sample SHCs receive a CHO as per the observed government assignment. Row (8): average outcomes across 100 random allocations of the 95 CHOs within the same markets. Row (9): Reallocation of the 95 CHOs within the same markets with the objective to maximize the decline in the all-age mortality rates for the pooled sample. Columns (1)-(3) show the average market shares for subcenters, PHCs, and private providers. The market share of the outside option is omitted. Column (4) reports the average healthcare quality of the chosen provider, with quality defined as 0 if the outside option is chosen. Column (5) reports the predicted relative decline in all-age mortality rates based on the changes in average quality.

Table A23: Heterogeneity by CHO Gender

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post × Female CHOs	208.569***	6.414***	0.013	0.002	-0.026*	-0.023	-0.024*	-0.062
	(10.304)	(0.759)	(0.009)	(0.015)	(0.015)	(0.017)	(0.013)	(0.039)
Treatment × Post × Male CHOs	222.890***	6.237***	0.029***	-0.004	-0.030**	-0.037**	-0.037***	-0.113***
	(9.230)	(0.635)	(0.008)	(0.013)	(0.014)	(0.015)	(0.012)	(0.036)
p-value: Coef 1 = Coef 2	0.131	0.811	0.055	0.634	0.684	0.173	0.156	0.063
Female CHOs:								
Treatment Group Mean (Pre-Periods)	239.142	3.288	0.043	0.032	0.379	0.314	0.242	0.685
Counterfactual Treatment Group Mean (Post-Periods)	368.814	8.392	0.041	0.144	0.368	0.307	0.253	0.716
Observations	1,644	1,644	1,644	1,644	1,644	1,644	1,644	1,644
Male CHOs:								
Treatment Group Mean (Pre-Periods)	240.348	3.543	0.036	0.039	0.370	0.316	0.249	0.704
Counterfactual Treatment Group Mean (Post-Periods)	370.021	8.648	0.035	0.152	0.360	0.309	0.259	0.735
Observations	3,250	3,250	3,250	3,250	3,250	3,250	3,250	3,250
Control Group:								
Control Group Mean (Pre-Periods)	238.112	3.602	0.037	0.015	0.368	0.313	0.237	0.679
Observations	4,892	4,892	4,892	4,892	4,892	4,892	4,892	4,892

Notes: This table shows the effects of CHOs on healthcare services and health outcomes by CHO gender. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and CHO gender, while controlling for year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. In columns 1-8, the sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. In these columns, the outcomes are at the subcenter level and are obtained from the Pregnancy, Child Tracking and Health Services Management System. The outcome in column 9 is at the household member level and is obtained from our household surveys. See Data Appendix for details on variable definitions.

B. Data Appendix

Outcome Variables - Administrative Data

- *Number of Patient Visits:* the total number of patient visits received by the ANM and the CHO.
- *Number of Acute Heart Disease Patient Visits:* the total number of visits from patients diagnosed with acute heart disease.
- *Number of Stroke Patient Visits:* the total number of visits from patients diagnosed with stroke.
- *Number of Epilepsy Patient Visits:* the total number of visits from patients diagnosed with epilepsy.
- *Number of COPD Patient Visits:* the total number of visits from patients diagnosed with chronic obstructive pulmonary disease.
- *Number of Hypertension Patient Visits:* the total number of visits from patients diagnosed with hypertension received by the ANM and CHO. This includes newly and previously diagnosed patients.
- *Number of Diabetes Patient Visits:* the total number of visits from patients diagnosed with diabetes received by the ANM and CHO. This includes newly and previously diagnosed patients.
- *Maternal & Child Health Services Index:* standardized index that consists of: the number of pregnant women with at least four prenatal care visits, the number of pregnant women who were given 360 calcium tables, the number of pregnant women who received their first tetanus shot, the number of children aged 9-11 months who have been fully immunized, and the number of women getting a post partum checkup in the first 7 days.
- *Any Death:* An indicator variable that is equal to one if at least one death occurred in the corresponding quarter in the catchment area of the subcenter. Similar definitions are used for any elderly deaths and any other-age-group deaths.
- *Number of Deaths:* the total number of deaths that occurred in the corresponding quarter in the catchment area of the subcenter. Similar definitions are used for number of elderly other-age-group deaths.
- *Mortality Rate:* the total number of deaths that occurred in the corresponding quarter in the catchment area of the subcenter per 1,000 individuals in the catchment area. For the elderly mortality rate, we divide the number of elderly deaths by the number of elderly individuals in the catchment area. The number of elderly individuals is calculated by multiplying the catchment area population by 8.8%, the average elderly

population share in our sample areas in the SHRUG data. Data on the catchment population are obtained from the Health and Wellness Center Portal. A similar definition is used to define the mortality rates for other age groups.

- *Mortality Rate (IHS):* the inverse hyperbolic sine of the mortality rate.
- *Chronic Elderly Deaths:* aggregates the following cause-of-death categories: heart disease, HIV/AIDS, cancer, tuberculosis, neurological disease, and 'other-chronic' deaths.
- *Acute Elderly Deaths:* aggregates the following cause-of-death categories: diarrhea, respiratory infections, fever, dengue, encephalitis, malaria-plasmodium vivax, malaria-plasmodium falciparum, kala azar, and 'other-acute' deaths.
- *Accident-Related Elderly Deaths:* aggregates the following cause-of-death categories: trauma/accident/burn cases, suicide, and animal bites.
- *Unknown-Cause Elderly Deaths:* the number of elderly deaths from unknown causes.

Outcome Variables - Household Survey Data

- *Any Symptoms:* an indicator variable that is equal to one if the household member had at least one health symptom in the past 30 days.
- *Medical Expenses:* the total amount the household member spent on healthcare visits, medicines, tests, and transport to healthcare providers in the past 30 days.
- *Any Hospitalization:* an indicator variable that is equal to one if the household member has been hospitalized (spent at least one night in a hospital or clinic) in the past 6 months.
- *Hospital Days:* the total number of days the household member has been hospitalized in the past 6 months.

Patient Exit Surveys

- *Overall Satisfaction:* obtained from the following survey question: "On a scale of 1 to 5, how satisfied are you/is the patient with the provider? (1 is least satisfied and 5 is most satisfied)".
- *Number of Questions Asked:* the number of questions the provider asked to the patients based on patient self-reports.
- *Measured Blood Pressure:* an indicator variable that is equal to one if the blood pressure of the patient was measured during the healthcare visit.

- *Any Antibiotics:* an indicator variable that is equal to one if the patient was given antibiotics. Medicines were classified by the research team. Medicines names were collected by asking patients to show the enumerators all the medicines that were given to them by the healthcare provider.

- *Referred:* an indicator variable that is equal to one if the patient was referred to another healthcare facility.

Private Provider Surveys

- *Number of Providers:* the total number of private healthcare providers in the catchment area.

- *Number of Patients:* the total number of patients seen by the private healthcare facility in the past 30 days.

- *Typical Fee:* obtained from the following survey question: "what are your normal fees for primary care, including medicine and consultation fees?"

- *Quality Index:* standardized index that consists of: the number of training workshops attended, length of medical degree, and the checklist completion rate.

- *Length of Medical Degree:* the length of the highest medical degree of the provider in years. If the provider was enrolled in a degree program at the point of the survey, we assume that the provider will complete the degree.

- *Number of Training Workshops Attended:* the number of training workshops that provide information on new diseases and/or medicines the provider attended in the past 12 months.

- *Checklist Completion Rate:* the average checklist completion rate of the provider across a child dysentery and an adult asthma vignette.

- *Injection Rate:* the share of patients who received an injection from the provider in the past 30 days.

- *Antibiotic Dispensing Rate:* the share of patients who received antibiotics from the provider in the past 30 days.

C. Data Collection

We conducted in-person surveys with ANMs, CHOs, and private providers. For private provider surveys, we mapped all private providers in the catchment area of the subcenter at baseline and endline by surveying ANMs and two local shopkeepers. Once the mapping was complete, we attempted to survey each of the private providers. The survey collected information on the personal details of each provider, their medical knowledge through two vignettes (child dysentery and adult asthma), the number of patients in the last 30 days, the share of patients who received antibiotics and injections, average fees, and participation in training workshops. For private providers we surveyed at endline, we conducted a second follow-up survey over the phone to get repeat measures of the main outcomes, with the exception that we could not implement the vignettes over the phone.

Among subcenters that had at least one private provider in the catchment area at baseline, we further conducted a phone survey with 513 households. We obtained contact details through the list of registered pregnancies in the PCTS portal. To be included in our sample, the household needed to have at least one registered pregnancy in the last five years. Since 94% of pregnancies are registered in India according to data from the National Family Health Survey 2019–2021, this covers most households who had a pregnancy in the past five years. The household survey collected information on health outcomes and healthcare utilization for all household members.

We surveyed 95% of ANMs at baseline and 99% of ANMs at endline. ANMs who were not surveyed were on temporary leave. We managed to conduct a baseline survey with 71% of the private providers we mapped in the catchment area. The main reasons for noncompletion were temporary closures and refusals. Out of the private providers still operational at the point of the endline survey, we managed to resurvey 88%. For the household phone survey, we managed to reach 26% at baseline. Noncompletion was primarily due to incorrect phone numbers. Households that we surveyed at baseline and could not reach over the phone at endline we also attempted to survey in person. This helped us to increase the follow-up completion rate at endline from 54% to 90%. The phone and in-person completion rates do not differ by treatment.

D. Rollout of Health and Wellness Center Reform

The first part of the Health and Wellness Center reform was to convert subcenters and PHCs to Health and Wellness Centers. Subdistrict officials had to propose a fixed number of subcenters for conversion annually between 2018 and 2022. The minimum criterion for conversion was that the government must own the subcenter building. In some years, priority was further given to subcenters with electricity, running water, and good physical condition. The physical conversion of subcenters mostly consisted of the construction of another examination room and the painting of the walls in a bright yellow color for branding purposes. In our survey, 74% of ANMs said that a single additional room was built as part of the reform. Changes in electricity (2%), running water (3%), or equipment (2%) were rare. The reform was also supposed to increase the set of medicines available at the subcenters, but these changes had not yet been implemented when we conducted our endline surveys.

The reform officially increased the set of available services at subcenters from six to twelve services. The newly added services include screening and control for chronic diseases as well as basic oral, palliative, and mental healthcare. However, many of these services were not provided frequently during our study period due to insufficient awareness and limited availability of necessary equipment and medicines. We consider these additional services to be part of the strengthening of basic outpatient care.

The main element of the Health and Wellness Center reform was the posting of CHOs to subcenters. While ANMs have a two-year diploma, CHOs are required to have either a three-year degree in general nursing and midwifery or a four-year bachelor's degree in nursing. They further need to complete a six-month bridge course upon being hired. The primary role of CHOs is to provide basic adult outpatient care and screening for chronic diseases at the subcenter level. The screening for chronic diseases is done through outreach camps and by screening patients who visit the subcenters during a routine medical consultation. Once posted, a CHO is the designated team leader of the health worker team at the subcenter. In our sample, the average age of newly hired CHOs is 28 years, and 64% are male. They are paid a fixed monthly salary of INR 25,000 ($\approx$ USD 300) plus INR 15,000 ($\approx$ USD 180) in performance-based incentives. They further need to complete a six-month bridge course upon being hired. The screening for chronic diseases is done through outreach camps and by screening patients who visit the subcenters during a routine medical consultation.

E. Screening Rates and Mortality Declines

We conduct back-of-the-envelope calculations to estimate how much the increase in screening rates contributes to the observed decline in all-age mortality. We use data from the Global Burden of Disease Study to examine how many deaths can be attributed to hypertension and diabetes. For 2021, the study estimates that 15% of deaths in India be attributed to hypertension and 9% can be attributed to diabetes. We then use our estimated treatment effects to predict how many of these deaths could have been averted due to the CHO postings. As shown in Table 3, we estimate that the CHOs increase the number of hypertension patient visits by 6.2 and the number of diabetes patient visits by 4.7. NFHS data further shows that 7.3% of the rural population in Rajasthan have undiagnosed hypertension and 4.5% have undiagnosed diabetes. Combining this with our treatment effects and an average population of 3,115 people, we calculate that the share of undiagnosed hypertension and diabetes patients decreased by 22% and 27%, respectively. Using data from CHO surveys, we further assume that the share of newly diagnosed hypertension and diabetes patients who regularly take their medicine is 91%. Finally, use estimates on the effectiveness of hypertension and diabetes medicines from the medical literature to assess how much mortality rates would decline conditional on medicine adherence. For hypertension, Hickey et al. (2021) find that a patient-centered hypertension care model reduced all-cause mortality among hypertensive patients by 21% within three years in rural Kenya and Uganda. For diabetes, we use estimates from a meta-analysis that finds that metformin, the most commonly used diabetes medicine in our setting, leads to a reduction in all-cause deaths by 29% among diabetes patients (Monami et al., 2021). Taken all of this together, we estimate that all-age mortality could have declined by 1.27% due to the increase in screening for chronic diseases [$0.15 * 0.22 * 0.91 * 0.21 + 0.09 * 0.27 * 0.91 * 0.29$].

F. Cost-Effectiveness Analysis

We use the Marginal Value of Public Funds (MVPF) to calculate the cost-effectiveness of the CHO postings (Hendren and Sprung-Keyser, 2020). For government costs, we account for government spending on CHO salaries and increased spending on medicines. Our analysis consists of 2,487 treatment group subcenters, covering a total of 7,752,343 people. Each of these subcenters received a CHO who gets a monthly salary of USD 480, including incentive-based payments.[68] We further assume that the government spends USD 0.24 on medicines per outpatient visit. Combining these estimates with our treatment effects on quarterly patient visits (Table 3, Column (1)), we find that total government costs in the two years are equal to USD 29,676,874 [2,487 subcenters * USD 480 * 24 months + 2,487 subcenters * 215 patients * 8 quarters * USD 0.24].[69]

On the government benefits side, we account for decreased public spending on hospitalizations. Garg et al. (2022) estimate that average spending per hospitalization episode is equal to USD 276.28 across all facilities in the state of Chhattisgarh.[70] They further estimate that 33% of these costs are, on average, paid by the government and that the remaining 67% are paid out-of-pocket by patients. Combing these estimates with our treatment effects in Column (3) in Table 5, we find that total government benefits in the first year are equal to USD 24,030,842 [7,752,343 people * 0.017 decrease in any hospitalizations in the past 6 months * 2 * USD 91.17].

For private benefits, we follow Hendren and Sprung-Keyser (2020) and use USD 100,000 as the value of a statistical life year. We use the estimates from Garg et al. (2022) for private out-of-pocket spending per hospitalization visit. Combing these estimates with our treatment effects (Column (3) in Table 4 and Column (3) in Table 5), we find that total private benefits in the first year are equal to USD 251,729,091 [2,487 subcenters * 8 quarters * 0.102 decrease in deaths per quarter * 100,000 value of statistical life year + 7,752,343 people * 0.017 decrease in any hospitalizations in the past 6 months * 2 * USD 185.1].

Taken together, these results in a Marginal Value of Public Funds of 44.6 [USD 251,729,091 / (USD 29,676,874 - USD 24,030,842)]. Costs per life-year saved are equal to USD 2782 [(USD 29,676,874 - USD 24,030,842)/(2,487 subcenters * 8 quarters * 0.102 decrease in deaths per quarter)].

Table A24: Sensitivity of Cost-Effectiveness Analysis

All-Age Mortality Decline:	2 Years	2 Years	No Change	2 Years	2 Years	2 Years
Hospitalizations Decline:	No change	1 Year (Delay)	1-Year	6 Months	1 Year	2 Years
Marginal Value of Public Funds	6.84	7.17	8.64	12.87	44.59	∞
Costs per Life-Year Saved (in USD)	14,623	14,031		8,702	2,782	0
Costs per Life-Year Saved / GDP per Cap.	6.18	5.93		3.68	1.18	0

Appendix Table A24 shows how the cost-effectiveness estimates vary with different assumptions regarding the decline in mortality and hospitalization rates. Column (1) shows our estimates if we ignore the change in hospitalizations. Column (2) assumes that hospitalizations did not permanently decline but just got delayed by one year. Column (3) shows estimates if we ignore the decline in mortality rates. Column (4) assumes that hospitalizations only declined by 6 months (there reference period of our household survey). Column (5) shows our preferred specification that assumed a decline in hospitalizations by one year. Finally, Column (6) shows that the reform would pay for itself if we assume that the decline in hospitalizations also remains in the second year.

[68]We use the current exchange rate of 0.13 INR to 1 USD.

[69]Minor differences in our calculations below are due to rounding errors in the converted dollar values.

[70]Chhattisgarh is a central state in India that is slightly poorer than Rajasthan.

In Appendix Table A25, we further report cost-effectiveness estimates based on the counterfactual simulations in the demand model. We ignore changes in hospitalizations and drug costs and focus on estimating costs per life-year saved. An ANM's base salary plus incentive payments is equal to USD 170 per month.

Table A25: Cost-Effectiveness of Alternative Policies

Policy	CHO Instead of ANM (Without Private Sector Responses)	Additional ANM	New CHO Alongside Existing ANM (Without Private Sector Responses)	New CHO Alongside Existing ANM (With Private Sector Responses)
Costs per Life-Year Saved (in USD)	15,482	32,160	11,720	10,590