A Community Health Worker–Based Intervention on Anthropometric Outcomes of Children Aged 3 to 21 Months in Urban Pakistan, 2019–2021

Abu S. Shonchoy, PhD, Agha A. Akram, PhD, Mahrukh Khan, BSc, Hina Khalid, PhD, Sidra Mazhar, MSc, Akib Khan, MS, and Takashi Kurosaki, PhD

Objectives. To evaluate the impact of a community health worker–based "in-home growth monitoring with counseling" (IHGMC) intervention on anthropometric outcomes in Pakistan, where 38% of children younger than 5 years are stunted.

Methods. We used an individual, single-blind, step-wedge randomized controlled trial and a pure control group recruited at endline. We based the analysis on an intention-to-treat estimation using the coarsened exact matching (CEM) method for sample selection among treatments and the control. We conducted the baseline in July 2019 and completed endline in September–October 2021. We recruited 1639 households (treated: 1188; control: 451) with children aged 3 to 21 months who were residing in an urban informal settlement area. The CEM sample used for analysis numbered 1046 (treated: 636; control: 410). The intervention continued for 6 months.

Results. Compared with the control group, the height-for-age *z*-score in the IHGMC group increased by 0.58 SD (95% confidence interval [CI] = 0.33, 0.83; $P = .001$) and the weight-for-age *z*-score by 0.43 SD (95% CI = 0.20, 0.67; $P < .01$), measured at endline.

Conclusions. IHGMC substantially improved child anthropometric outcomes in disadvantaged localities, and this impact persisted during the COVID-19 pandemic.

Trial Registration. AER-RCT registry (AEARCTR-0003248). (*Am J Public Health*. 2023;113(1):105–114. https://doi.org/10.2105/AJPH.2022.307111)

Globally, 1 in 4 children younger than 5 years suffers from linear growth faltering,[1] with the highest prevalence in South Asia and sub-Saharan Africa.[2] Stunting (low height-for-age *z*-score [HAZ] < -2) remains a critical public health challenge as it reduces lifetime earnings, hinders cognitive development, and leads to high mortality rates.[3] The COVID-19 pandemic has raised concerns about reversals to improvements in childhood nutrition.[4] These concerns have been met with a renewed emphasis on the importance of mobilizing resources for nutrition[5] and an urgency to increase resilience to malnutrition during times of crises,[6] such as a pandemic.

Research suggests that primary caregivers play a key role in child development.[7] Caregivers are the first point of contact for children, and their engagement is crucial to ensure adequate physical, cognitive, social, and emotional development. Consequently, community health worker (CHW) programs, globally[8] and in Pakistan,[9] leverage regular contact with primary caregivers to improve child health outcomes. Existing CHW-based public health delivery programs, which have shown promise in maternal and child health[10] by encouraging health-care-facility utilization by caregivers, have produced modest gains in child health (typically lower than a 0.25 SD gain in HAZ).[11–13]

Several limitations remain, as these programs predominantly focus on resource and knowledge constraints but provide little attention to behavioral interventions such as engaging caregivers with continuous feedback on the growth measures of their children.[14,15] Programs that use cash transfers to address resource constraints show limited impact.[16] Physical growth promotion programs mostly operate through facility-based growth monitoring[17] and rarely focus on regular home-based growth surveillance by CHWs, with the exception of a handful of small sample studies.[12,18] Programs that simply integrate growth charts into the community-based interventions—without regular growth monitoring—do not see any impact because caregivers often fail to comprehend growth trajectories.[19] The complementarity of regular growth monitoring and counseling for caregivers is essential, as it improves the understanding of child care inputs and physical development, particularly in marginalized communities.

The few studies that explored behavioral interventions have shown limited effect on child growth. One of the first rigorous studies on regular growth monitoring with a growth chart, the South Indian Trial,[18] did not find any additional benefit from growth monitoring. The study setting was small (12 villages in Tamil Nadu), focused on weight measures, and was executed by 1 selected mother in the village. Its impact measures also did not isolate the impact of growth monitoring from that of the growth chart. A related study, conducted in Zambia,[20] focused on home-based growth monitoring (life-sized posters installed in homes to demonstrate children's age-appropriate height) and community-based growth monitoring along with nutritional supplements. This study found modest positive effects on growth among previously malnourished children; however, the study suffered from a lack of professionally measured anthropometrics at regular intervals and did not assess complementarities between monitoring and counseling. Thus far, the existing literature is inconclusive and lacks sufficient evidence in evaluating the impact of regular in-home anthropometric monitoring and counseling executed by trained CHWs.

Motivated by this concern, we tested in-home growth monitoring coupled with nutrition counseling in Pakistan, a lower-middle-income country in South Asia with high levels of childhood stunting: 38% of all children younger than 5 years are stunted, although this figure is lower in urban areas (31%) and for children aged 6 to 8 months (18%).[21] We chose to study the intervention in an informal urban settlement, a setting that hosts marginalized populations but rarely receives health or nutritional aid. Additionally, our study was conducted during a global pandemic, which—as many experts fear—threatens child nutritional development, especially in areas where health facilities are being closed or partially functional.[22]

METHODS

Our main sample for the impact analysis came from a randomized controlled trial, which we conducted in Gulshan-e-Sikandarabad, an urban informal settlement located in Karachi, Pakistan. Households with at least 1 child aged 3 to 21 months were eligible for this trial. An independent survey team listed 4166 households, found 1823 of them to be eligible for our trial, and administered a baseline survey (July 2019) to the biological mother and caregiver of the child, capturing demographics, socioeconomics, and child anthropometrics. If more than 1 eligible child was present in the household, the youngest one was chosen. This process continued until 1188 eligible households completed the baseline survey and were randomly allocated to 1 of 3 treatment arms (1:1:1) entailing 396 households in each group, as follows: T1: monthly in-home growth monitoring with counseling (IHGMC); T2: IHGMC with a poster-sized HAZ-based growth monitoring interactive chart; T3: IHGMC plus growth charts (as in T2) complemented with a monthly unconditional cash transfer (fixed amount of Rs 400 [$11.91 in purchasing power parity]), with a suggestion to use the amount for children's food. This intervention continued for 6 months (September 2019–February 2020) and ended just before the COVID-19 outbreak. The balance table on treatment assignment is reported in Table A10 of Appendix A (available as a supplement to the online version of this article at http://www.ajph.org).

An endline survey was administered 13 months after the start of intervention activities (September–October 2020), with a no-contact period of 7 months. The endline survey was timed this way to allow better understanding of the persistence of gains in child health, especially as measured during the pandemic. At this time, we added a pure control group by surveying an additional 451 households, recruited from the subset of eligible households in the original list of 4166 households generated during our initial community census, utilizing the same eligibility criteria of having a child aged 3 to 21 months and presence of the household in the community at the time of baseline. Adding this pure control group allowed us to compare the treatment impact with a no-intervention scenario, going beyond the ambit of the original randomized controlled trial (a detailed timeline is given in online Appendix B).

Randomization, Matching, and Masking

We initially designed a sample size of 400 households per intervention group to detect an effect size of 0.3 SD in HAZ between any of the 3 treatment arms, with a power of 0.8 and an α level of 0.05, unconditional on covariates. This statistical power remained similar when we used a matched sample.

We used coarsened exact matching (CEM) to select our sample for analysis from the treatment and control groups, since we added the control group to an ongoing randomized controlled trial. We used CEM to match on household size, child's age at baseline, father's education, mother's education, and language.[23] We improved the matching by reducing the L1 distance (an objective measure of how different the raw, unmatched control and treatment samples are from each other) from 0.94 to 0.57. Details of CEM are provided in online Appendix D, and the balance on observables for the matched sample are shown in Table A11 of online Appendix A.

The nature of our intervention did not allow full masking of participants to the CHWs. Although the team of investigators was masked, the data collection team was not strictly blinded to intervention group assignment since the endline survey asked about some of the treatment-related activities, which allowed them to predict individual treatment allocation (online Appendix E). The detailed procedures on team recruitment and training and on intervention operational protocols are given in online Appendixes H, G, and E, respectively.

Outcomes

The primary outcome measures were HAZ, where height was measured using infantometers and stadiometers, and weight-for-age *z*-score (WAZ), where weight was measured using weighing scales (for detailed procedures, see online Appendix G). We calculated HAZ scores using the in-built Stata package "zscore06" (StataCorp LP, College Station, TX) in accordance with the World Health Organization (WHO) Child Growth Standards for children younger than 5 years. Our secondary outcomes were binary indicators for stunted and severely stunted (i.e., 2 SD and 3 SD below the median HAZ score of the reference population, respectively, underlying the WHO Child Growth Standards) as well as binary indicators for underweight and severe underweight (i.e., 2 SD and 3 SD, respectively, below the median WAZ score from the WHO Child Growth Standards).[24] Another secondary outcome was weight-for-height *z*-score (WHZ), which captured the weight of the child compared with their height as well as 2 binary variables: wasting (i.e., WHZ < -2 SD) and severely wasted (i.e., WHZ < -3 SD). We measured height and weight in duplicates, following the WHO Multicenter Growth Reference Study method.[25] Additional variables analyzed were caregiver knowledge, quality of diet, and the home environment (online Appendix F).

Statistical Analysis

All our analyses followed an intention-to-treat (ITT) estimation on the matched sample. CEM yielded a total sample of 1046 households across the control and treatments (198 in T1, 208 in T2, 230 in T3, and 410 in the control), with a matching control:T1:T2:T3 ratio of 1:0.48:0.51:0.56. Our ITT estimation generated causal effects of treatment on outcome variables. ITT estimates minimize bias through selective take-up of the intervention, providing lower bound impact estimates. We employed ITT regression analysis using binary variables to designate treatment status (versions with individual- and household-level covariates are reported in Tables A15 through A23 of online Appendix A) to evaluate the impact of the 3 treatments, using Stata 14 with Huber–White robust standard errors. We used the same strategy for the treatment component–specific analysis (termed "reclassification"), where we estimated the ITT impacts using binary indicators for treatment components: counseling = T1+T2+T3 (n = 636); growth chart = T2+T3 (n = 438); and cash transfer = T3 (n = 230). We present ITT coefficient estimates (means) with 95% confidence intervals (CIs) and *P* values using outcomes measured at endline. We estimated heterogeneous treatment effects by interacting treatment status with child's gender, marginalized ethnicity dummy, and age at baseline (online Appendix C). Additionally, we used propensity score matching as a robustness check for our estimates.

RESULTS

Of the 4166 households assessed for eligibility at baseline, 1823 were found eligible, of which 1188 were employed for the intervention (online Figure A7). Of these, 5 households witnessed a death or injury of the child (< 1%) and 202 households (17%) could not be recontacted for program implementation through a combination of weak address systems (typical of informal urban settlements) and migration out of the neighborhood. Thus, our program implementation sample was 981 households (83% of baseline sample). Of these, we successfully reinterviewed 790 households at endline (81% of implementation sample); 58 households refused to be reinterviewed (5%), 11 exceeded our interview rescheduling threshold of 3 attempts (1%), 9 were located but were absent despite multiple attempts (1%), and 113 had moved out of the community (10%).

Our total available endline sample was 1241 households: 790 households from the original randomized controlled trial sample and 451 recruited to serve as the control. After we matched using CEM, our final analysis sample was 1046 households. The characteristics of these 2 groups were similar: the control arm children were 55% male and 45% female whereas the corresponding numbers in the treatment arms were 52% male and 48% female. Mother's literacy rate in this community was low: 68% of mothers had not attended at least 1 year of schooling across the control and treatment samples. Households in treatment and control were balanced across all 5 neighborhood categories. In terms of ethnicity, the proportion of historically marginalized groups was balanced across the control and treatments at 25% and 24%, respectively. Detailed descriptive statistics for the study sample are given in Table 1.

Our first set of results compared the matched control with any treatment

TABLE 1— Demographic Characteristics of the Matched Sample: Pakistan, September–October 2021

	Control Group, Mean ±SD or No. (%)	Treatment Group, Mean ±SD or No. (%)			
		T1	T2	T3	All Treatments
Household size, no.	7.84 ±4.32	8.73 ±4.44	9.09 ±4.55	8.50 ±4.15	8.77 ±4.38
Child's age, y	25.70 ±6.80	26.11 ±5.73	25.48 ±5.68	25.53 ±5.81	25.69 ±5.74
Father's education					
Not literate[a]	223 (54.52)	91 (46.19)	104 (50.24)	101 (44.10)	296 (46.76)
Literate	186 (45.48)	106 (53.81)	103 (49.76)	128 (55.90)	337 (53.24)
Mother's education					
Not literate[a]	278 (67.80)	135 (68.18)	144 (69.23)	152 (66.09)	431 (67.77)
Literate	132 (32.20)	63 (31.82)	64 (30.77)	78 (33.91)	205 (32.23)
Neighborhood					
Neighborhood 1	158 (38.73)	70 (35.35)	77 (37.02)	73 (31.74)	220 (34.59)
Neighborhood 2	66 (16.18)	31 (15.66)	42 (20.19)	55 (23.91)	128 (20.13)
Neighborhood 3	32 (7.84)	25 (12.63)	19 (9.14)	23 (10.00)	67 (10.53)
Neighborhood 4	77 (18.87)	36 (18.18)	40 (19.23)	43 (18.70)	119 (18.71)
Neighborhood 5	75 (18.38)	36 (18.18)	30 (14.42)	36 (15.65)	102 (16.04)
Language					
Urdu	3 (0.73)	2 (1.01)	2 (0.96)	2 (0.87)	6 (0.94)
Sindhi	6 (1.46)	3 (1.52)	2 (0.96)	1 (0.44)	6 (0.94)
Punjabi	56 (13.66)	16 (8.08)	19 (9.14)	18 (7.83)	53 (8.33)
Pashto	278 (67.80)	146 (73.74)	166 (79.81)	179 (77.83)	491 (77.20)
Saraiki	64 (15.61)	30 (15.15)	19 (9.14)	29 (12.61)	78 (12.26)
Other	3 (0.73)	1 (0.51)	0 (0.0)	1 (0.44)	2 (0.31)
Child's gender					
Female	184 (44.88)	104 (52.53)	87 (41.83)	112 (48.70)	303 (47.64)
Male	226 (55.12)	94 (47.47)	121 (58.17)	118 (51.30)	333 (52.36)
Marginalized ethnicity					
Other	307 (74.88)	146 (73.74)	162 (77.88)	170 (73.91)	478 (75.16)
Marginalized	103 (25.12)	52 (26.26)	46 (22.12)	60 (26.09)	158 (24.84)
Total sample	410	198	208	230	636

Note. T1 = monthly in-home growth monitoring with counseling (IHGMC); T2 = IHGMC with a poster-sized HAZ (height-for-age *z*-score)–based growth monitoring interactive chart; T3 = IHGMC plus growth charts (as in T2) complemented with a monthly unconditional cash transfer.

[a] "Not Literate" is a category for no schooling or incomplete schooling: the parent had not completed at least 1 year of schooling (i.e., was illiterate) or had not completed grade 1 or attended vocational training or madrasa education.

to quantify the impact of treatment at the time of the COVID-19 pandemic (Figure 1). Aggregate treatment estimates showed an increase in HAZ by 0.42 SD (95% CI = 0.23, 0.61; *P* < .001) compared with the control mean of −1.86 SD. The prevalence of stunting was reduced by 10 percentage points (95% CI = −0.17, −0.03; *P* < .001), and the prevalence of severe stunting was reduced by 5 percentage points (95% CI = −0.10, 0.00; *P* = .04) in the treated group compared with the control. We also found improvements in weight-related measures: a 0.25 SD increase in WAZ (95% CI = 0.07, 0.44; *P* = .01), a 6-percentage-point reduction in cases of underweight (95% CI = −0.12, 0.01; *P* = .07), and a 5-percentage-point reduction in cases of severely underweight (95% CI = −0.10, −0.01; *P* = .02). These estimates are robust to alternative matching (i.e., propensity score matching as reported in Table A14 of online Appendix A).

Next, as shown in Figure 2, we observed that T1 showed the largest improvements, with a statistically significant gain in HAZ of 0.58 SD (95% CI = 0.33, 0.83; *P* < .001), and reductions in stunting (−10 percentage points; 95% CI = −0.19, −0.01; *P* = .02) and severe stunting (−7 percentage points; 95% CI = −0.13, −0.01; *P* = .03). We also saw gains in WAZ (0.43 SD; 95% CI = 0.20, 0.67; *P* < .001), and reductions in underweight (−9 percentage points; 95% CI = −0.16, −0.01; *P* = .03) and severely underweight (−7 percentage points; 95% CI = −0.12, −0.02; *P* = .01). Compared with the control, T2 and T3 also largely followed the same direction as T1, although the magnitude of gains in HAZ and WAZ, and the reductions in stunting, severe stunting, underweight, and severe underweight, were not as large as in T1. None of our treatments had a statistically discernible impact at the conventional level on WHZ, wasted, and severely wasted compared with the control. Furthermore, as shown in Figure A1 and Table A3 of online Appendix A, reclassified treatment component–specific estimates demonstrate similar conclusions as estimated before.

Next, we estimated 2-way interactions (Figure 3) to understand if the

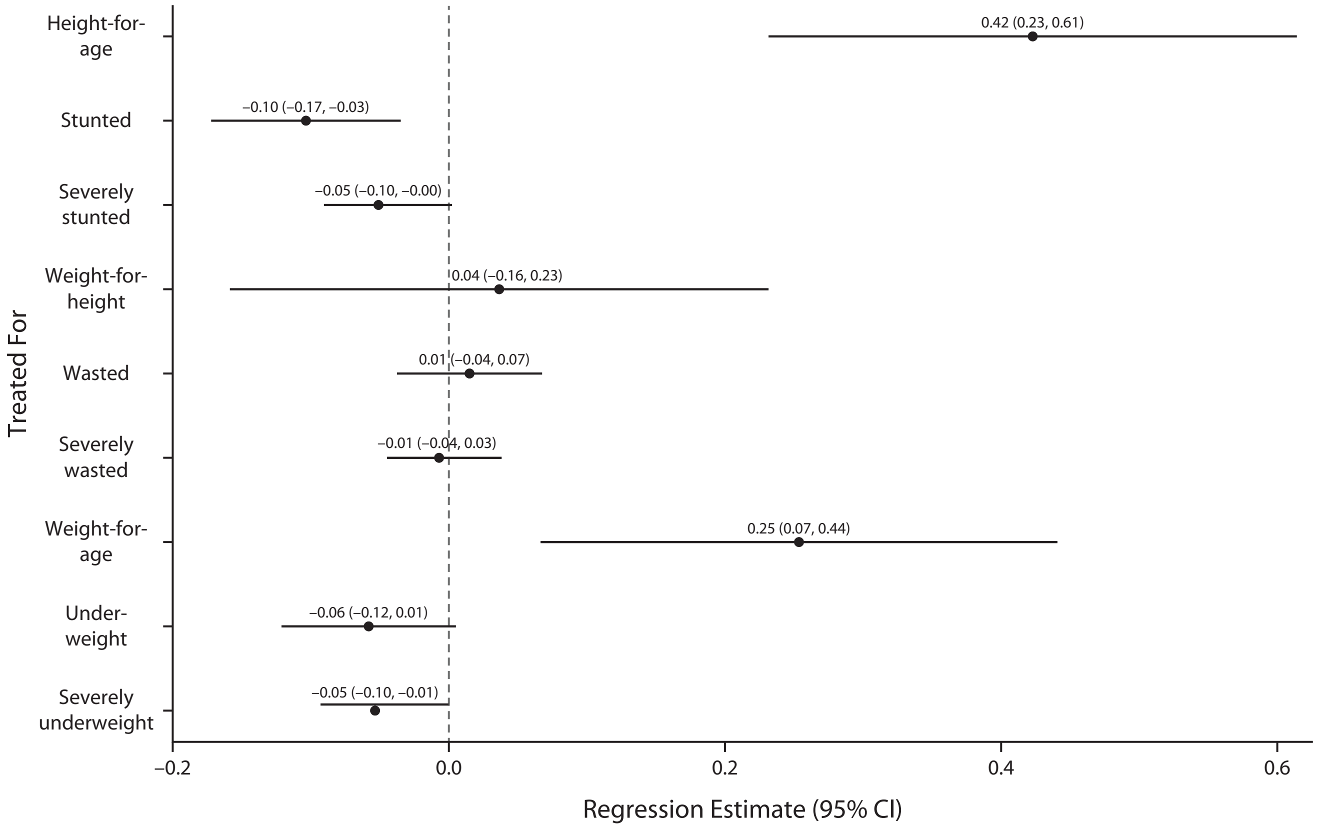

FIGURE 1— **Aggregated Treatment Effect of Intervention on Primary and Secondary Outcomes: Pakistan, September–October 2021**

Note. CI = confidence interval; T1 = monthly in-home growth monitoring with counseling (IHGMC); T2 = IHGMC with a poster-sized HAZ (height-for-age *z*-score)–based growth monitoring interactive chart; T3 = IHGMC plus growth charts (as in T2) complemented with a monthly unconditional cash transfer.

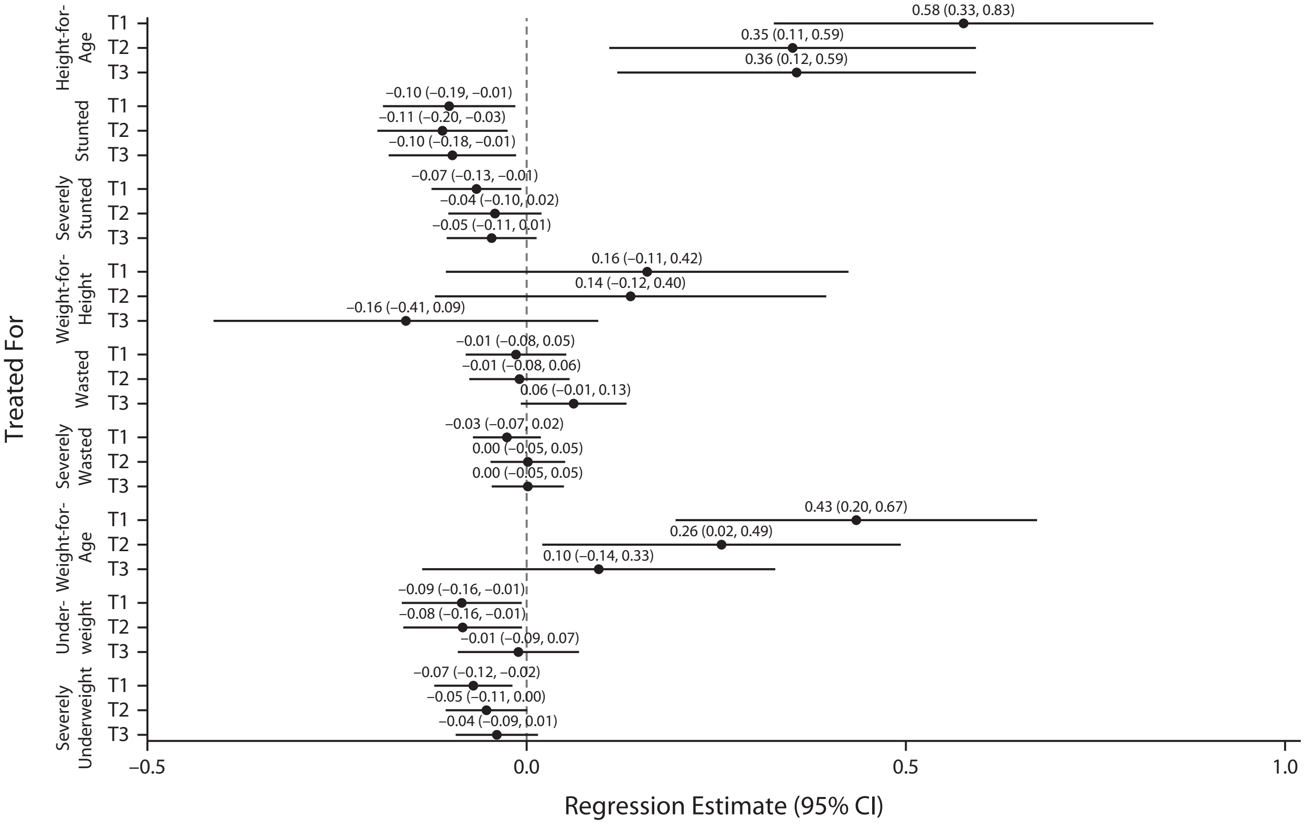

FIGURE 2— **Disaggregated Treatment Effect of Intervention on Primary and Secondary Outcomes: Pakistan, September–October 2021**

Note. CI=confidence interval; T1 = monthly in-home growth monitoring with counseling (IHGMC); T2 = IHGMC with a poster-sized HAZ (height-for-age *z*-score)–based growth monitoring interactive chart; T3 = IHGMC plus growth charts (as in T2) complemented with a monthly unconditional cash transfer.

effect of the treatments was different among girls and boys. The results showed that relative to girls, boys in T3 tended to have a higher HAZ (0.46 SD; 95% CI = −0.01, 0.94; $P = .05$) with an associated reduction in severe stunting (−13 percentage points; 95% CI = −0.25, −0.02; $P = .03$). Additionally, male children in T3 saw increased WAZ (0.63 SD; 95% CI = 0.17, 1.09; $P = .01$), and reduced cases of being underweight (−14 percentage points; 95% CI = −0.30, 0.02; $P = .08$) and severely underweight (−19 percentage points; 95% CI = −0.29, −0.08; $P < .001$). Finally, boys in T3 also saw increased WHZ (0.60 SD; 95% CI = 0.10, 1.11; $P = .02$) and a reduction in cases of severe wasting (−14 percentage points; 95% CI = −0.23, −0.04; $P < .001$). We found broadly similar results in reclassified treatment component estimates (Figure A2 and Table A5 of online Appendix A): male children in households that received a cash transfer had higher WAZ (0.61 SD; 95% CI = 0.12, 1.09; $P = .01$) along with a lower probability of being severely underweight (−16 percentage points; 95% CI = −0.26, −0.05; $P < .001$), and higher WHZ (0.61 SD; 95% CI = 0.04, 1.19; $P = .04$) along with a lower probability of being severely wasted (−13 percentage points; 95% CI = −0.23, −0.03; $P = .01$). We did not see a statistically significant increase in HAZ and related decreases in the probability of stunting. The other program components (IHGMC and growth chart) did not suggest any statistically significant difference by gender. Heterogeneity effects by child age and caste were inconclusive (Figures A3–A6 and Tables A6–A9 of online Appendix A).

Finally, we investigated the measures of caregiver knowledge, quality of diet, and the home environment that might have contributed to our findings (Table B2 of online Appendix B). There were 2 results of note. First, we found that children in T1 were given a 0.09 SD (95% CI = 0.01, 0.18; $P = .04$) larger quantity of dairy products compared with the control, measured as a standardized difference of an index for consuming multiple dairy food (index creation process detailed in online Appendix F). Second, we found that caregivers in T2

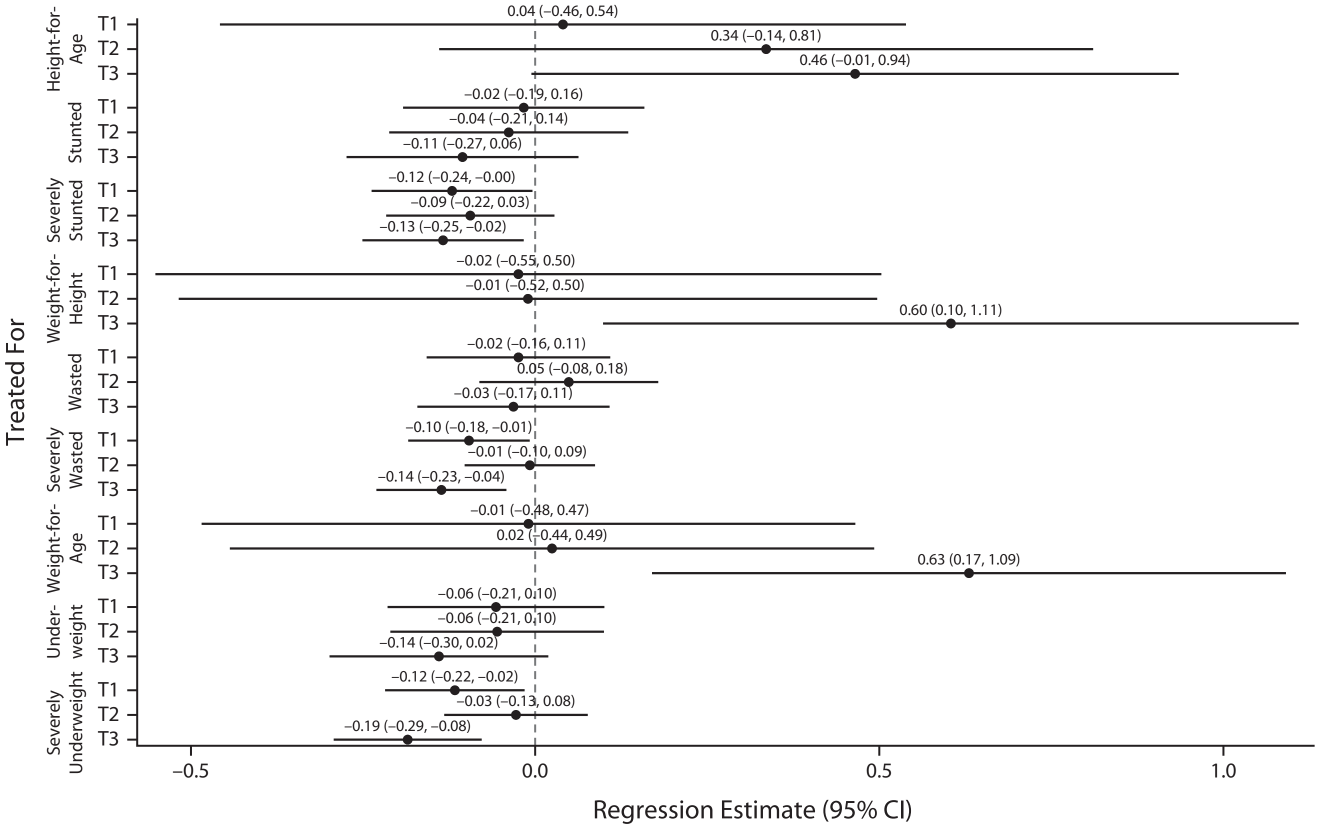

FIGURE 3— **Disaggregated Treatment Heterogeneity Analysis for Male Children (Effects Additional to Those for Female Children): Pakistan, September–October 2021**

Note. CI = confidence interval; T1 = monthly in-home growth monitoring with counseling (IHGMC); T2 = IHGMC with a poster-sized HAZ (height-for-age *z*-score)–based growth monitoring interactive chart; T3 = IHGMC plus growth charts (as in T2) complemented with a monthly unconditional cash transfer.

reported improved gender-related attitudes toward care, an increase of 0.17 SD (95% CI = 0.01, 0.32; $P = .04$) in a standardized index for multiple categories of child care–related questions, a larger quantity of fish and meat (0.10 SD; 95% CI = 0.02, 0.19; $P = .02$), and a greater dietary diversity score (0.35 SD; 95% CI = −0.01, 0.72; $P = .06$). However, we note that caregivers in this arm also demonstrated a 0.22 SD decrease (95% CI = −0.38, −0.05; $P = .01$) in general health care knowledge.

DISCUSSION

Our study has 3 major results. First, ours is one of the first studies demonstrating the impact of regular IHGMC by CHWs on young children, and we found a 0.42 SD gain in HAZ. To put this estimate into perspective, a range of comparable studies found increases in HAZ that did not exceed 0.25 SD, with time horizons ranging from a few months to 2 years.[20,26–28] This is an important finding because gains in height are harder to achieve and represent a relatively permanent positive change in health, unlike weight, which tends to respond quicker to a range of inputs. The gains in height that we documented are especially significant because these were realized during the COVID-19 pandemic, when income shocks resulted in severe nutritional deficiency in poor countries.[4] Our sample suffered aggregate welfare and health shocks; 75% of our sample reported that a member of the household lost work and 76% reported a loss in income due to the pandemic.

Second, we found that the simple IHGMC intervention contributed the most to child anthropometric outcomes. Specifically, we found that having IHGMC alone (T1) resulted in a 0.58 SD gain in HAZ, but layering a growth chart and unconditional cash transfer on top of IHGMC yielded positive albeit lower gains in child health. This suggests that the growth charts added complexity that resulted in these relatively lower gains. In our endline survey—7 months after the intervention—we specifically asked whether the growth chart was still in use and its primary function was understood. Use of the growth chart was not universal in the treatment groups; households reported limited use (14%) of the growth chart in the postintervention period. Moreover, they had questionable understanding of the chart; 60% failed to explain how the chart worked. These facts suggest that more effort may be needed from CHWs in explaining the growth charts to primary caregivers for greater understanding and usability.

Third, we found that the cash arm (T3) had a gendered effect: male children in the cash transfer arm differentially benefited on almost all anthropometric measures. Male children in T3 had higher HAZ, WAZ, and WHZ scores and lower probability of being severely stunted, severely wasted, underweight, or severely underweight. The simple IHGMC and the IHGMC with growth chart, however, did not show a gendered effect. These facts suggest that the simplest intervention of IHGMC tends to work equally well for children, irrespective of gender. Moreover, any program that chooses to add cash transfers must carefully consider gender dynamics in their respective settings. Our study showed that the cash transfer differentially benefited male children; this may be a consequence of local cultural preferences, including son bias.[29] Additional programming and counseling are needed to encourage more gender-equal allocation of resources.

Our results have several implications. First, we have demonstrated the effectiveness of a relatively simple intervention to induce gains in child height and weight by providing monthly nutrition counseling and in-home growth monitoring through direct engagement of caregivers by CHWs. Second, we effectively served households in an informal urban settlement. These communities are typically underserved, having few formal high-quality health facilities—which was exacerbated during the COVID-19 pandemic. Third, our program has the potential for scale in dense urban settings where homes are close to each other and CHWs do not need to carry equipment for long distances. We followed the established CHW model that orients the IHGMC intervention for the possibility of scaling-up with other CHW programs, which abound across the developing world. Our program is cost-effective: the total monthly cost of implementation per child in the IHGMC arm (T1) was $18 (including intervention, implementation, and administrative costs); the cost per case of stunting averted by the intervention was $360 (total implementation cost divided by additional cases of stunting averted in T1 compared with the control), which is on the lower end of the range for similar interventions in Pakistan and globally ($202–$1107).

Our study has 3 limitations. First, we noticed sizable attrition of our sample with unequal survey retention propensity across the treatment groups (Table A13 of online Appendix A). This is a consequence of working in informal settlement areas, which challenged our logistical capability. Working in an urban informal settlement is difficult[28,30] because there are no formal addresses and many dwellers tend to out-migrate (average annual turnovers of 25% have been documented). On the basis of information from key informants, we understood that this high rate of out-migration is reasonable, as the community is predominantly Pashtun immigrants who are highly mobile and frequently change address. Second, this reduced sample likely affected our ability to detect statistically significant differences in subgroup analysis. Finally, our treatment assignment was at the household level. Despite the highly idiosyncratic nature of our program, which delivered household-specific counseling and made child-specific measurements, households in our treatment group may have shared some insights from their experiences with households in their network. This has the potential to produce a positive spillover effect by contaminating the treatment and control groups, making the impact estimates lower bounds.

Taken together, we have demonstrated that a simple, low-cost, scalable intervention—regular home-based growth monitoring and nutrition counseling by CHWs—has a sizable impact on child HAZ and associated reduction in severe stunting, measured during the COVID-19 pandemic. Our findings suggest that regular IHGMC can increase resilience to malnutrition. These are compelling findings, in terms of tackling both the long-term challenge of child stunting and the short-term impact during this global pandemic, and they provide an important policy tool for low- and middle-income countries. AJPH

ABOUT THE AUTHORS

Abu S. Shonchoy is with the Department of Economics, Steven J. Green School of International and Public Affairs, Florida International University, Miami. At the time of this work, Agha A. Akram was with the Department of Economics, Mushtaq Ahmad Gurmani School of Social Science, Lahore University of Management Sciences; Mahrukh Khan was with the Centre for Economic Research in Pakistan; Hina Khalid was with the Department of Economics, School of Humanities and Social Sciences, Information Technology University; and Sidra Mazhar was with the Center for Economic Research in Pakistan, Lahore, Pakistan. Akib Khan is with the Department of Economics, Uppsala University, Uppsala, Sweden. Takashi Kurosaki is with the Institute of Economic Research, Hitotsubashi University, Tokyo, Japan.

CORRESPONDENCE

Correspondence should be sent to Abu S. Shonchoy, Florida International University, 11200 SW 8th St, Miami, FL 33199 (e-mail: shonchoy@fiu.edu). Reprints can be ordered at http://www.ajph.org by clicking the "Reprints" link.

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CONTRIBUTORS

A. S. Shonchoy conceptualized the research idea, conducted the formal analysis, acquired funding, designed the methodology, and guided the analysis; he administered and supervised the implementation of this study, was involved in the writing of the original draft, and led the revision of the article. A. A. Akram conceptualized the research idea, conducted the formal analysis, acquired funding, designed the methodology, and guided the analysis; he administered and supervised the implementation of this study, managed resources, was involved in the writing of the original draft, and led revision of the article. M. Khan curated the data set, conducted the formal analysis, and managed project resources and the software for analysis; she conducted the data validation, created visualizations, and was involved in the writing of the original draft and the revisions of the article. H. Khalid conducted the formal analysis, acquired funding, designed the methodology, and guided the analysis; she administered and supervised the project and was involved in the writing of the original draft and the revisions of the article. S. Mazhar conducted the investigation, administered the project, and participated in data validation; she was also involved in the writing of the original draft of the article. A. Khan conceptualized the research idea, conducted the formal analysis, acquired funding, designed the methodology, and guided the analysis; he administered and supervised the project, managed project resources and the software, led the data validation and analysis, and was involved in the writing of the original draft of the article. T. Kurosaki conducted the formal analysis, acquired funding, designed the methodology, and led the investigation; he also supervised the implementation of this study, and was involved in the writing of the original draft and the revisions of the article.

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CONFLICTS OF INTEREST

We declare that we have no conflicts of interest.

HUMAN PARTICIPANT PROTECTION

Ethical approval to conduct this study was obtained from the institutional review board at Interactive Research and Development (Karachi approval no. IRD_IRB_2018_09_003). Oral consent was obtained from all caregivers before the survey was administered.

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