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Four-year follow-up of CHARM2, an effective family planning intervention, on number and sex of births: Findings from an RCT in rural India

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ABSTRACT

Background: Effective family planning interventions may have inadvertent effects on births of girls given son preference in India. We conducted 36 and 48-month follow-ups to our CHARM2 family planning study to determine long-term intervention effects on births and sex of children.

Methods: Our non-blinded two-armed cluster RCT randomized young married couples ($N = 1201$ couples) from 20 geographic clusters (60–61 couples per cluster) into either the CHARM2 intervention or control (referral to local care) condition. CHARM2 offers 5-session gender-synchronized family planning and gender equity counseling delivered by trained local medical providers. Data were collected at baseline in September 2018–June 2019 and then follow-ups at 9, 18, 36 and 48 months, up to September 2023. We retained 88 %–91 % of women across follow-ups with no difference in retention by treatment group. We used adjusted mixed-effects logistic regression models examining sex composition of births at each follow-up and over the total 48-month follow-up to assess differences in all births of boys and girls by treatment group. We adjusted for treatment condition, cluster, and relevant demographics in adjusted models.

Results: We saw no treatment effects on total births or boy births, but lower likelihood of a girl birth was seen at 9-month follow-up and for the total 48-month follow-up period. We found at 9-month follow-up a girl birth was less likely for intervention compared with control participants (7.1 % vs. 10.3 %, respectively, $p = 0.06$), and the male to female sex ratio of births born between baseline and 9-month follow-up was also significantly higher for intervention vs. comparison participants (1.50 [95 % CI 1.00–2.26] vs. 0.83 [95 % CI 0.56–1.21], $p = 0.04$). We conducted a sensitivity analysis to determine treatment effects on boy births and girl births over the 48-month follow-up and again found no effects on boy births, but a significantly lower likelihood of a girl birth for the intervention group (22 % vs 29 %, $p = 0.03$).

Conclusion: The CHARM2 family planning intervention, previously demonstrating significant effects on contraceptive use and women's reproductive agency in rural India, resulted in lower likelihood of girl births over time, suggesting that family planning programs can contribute to sex ratio imbalances if broader social changes eliminating son preference and improving value of a girl child do not occur.

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1. Introduction

Family planning and women's reproductive agency are central to women's socio-economic development as well as maternal and child health [1], but modern contraception use in India primarily occurs in the form of female sterilization [2]. This impedes women's economic agency until she has completed childbearing and increases maternal and infant health risks related to low birth spacing. Interventions that integrate

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gender transformative programing into family planning counseling - addressing unequal gender role norms and practices, intimate partner violence, male engagement in family planning, and son preference - improve contraceptive use across a range of settings [3–5].

In 2018, we initiated a two-armed randomized control trial to evaluate CHARM2 (Counseling Husbands and wives to Achieve Reproductive health and Marital equity) with young married couples in rural India using this approach [6,7]. Our 18-month evaluation trial showed significant impacts on uptake of modern reversible contraceptives in the short term (9 months), on women's reproductive agency, indicated by contraceptive self-efficacy, communication, and satisfaction, at 9- and 18-month follow-ups, and men's gender equity ideologies and marital satisfaction at both 9- and 18-month follow-ups [7–10]. These findings document that a gender equity-focused family planning intervention that addresses gender norms, centralizes women's reproductive agency, and engages men as respectful and supportive partners can not only support family planning for couples but can also support contraceptive and marital satisfaction.

The promise of an effective family planning intervention, particularly one that supports women's agency and reversible contraceptive use, is important for India. At the same time, one must recognize that contraceptive practices can contribute to ongoing sex ratio imbalances. Research from India has found that son preference affects contraceptive use, with couples without a son less likely to use contraceptives, especially among mothers reporting a preference for more sons than daughters [11,12]. Concerns related to son preference have long been documented in India, but primarily with a focus on abortion. In 1971, India legalized abortion and soon after prenatal testing for sex determination of the fetus; within a decade, less expensive ultrasounds also became available [13]. By the 1990s, prenatal testing for sex of the fetus was outlawed, but it continues to be used. Advancements in abortion access and ultrasound continue to be important for maternal and infant health and women's reproductive choice and rights. However, they also inadvertently increased imbalance in India's sex ratio at birth, which was a normal 105 boys per 100 girls in 1970 and increased to 111 boys per 100 girls at its height in 2010 [13]. Sex ratio imbalance concerns lead to laws restricting abortion access rather than efforts to alter the value of girls for families [13]. With greater policy focus recently on girl child value, sex ratio imbalances are declining but remain high, at 108 [13]. Given the role of son preference on contraceptive decision-making in India [11,12], family planning interventions should discuss son preference in this context.

Our CHARM2 intervention, which as noted above focused on gender equity issues, included discussion of son preference and value for girl children, while simultaneously centralizing women's reproductive agency and male partner engagement to support women's family planning choices and behaviors [7,8]. However, the intervention did not reach broader families or communities on the issue of family planning and son preference norms, nor did we see a shift in son preference norms as part of our intervention, perhaps because disclosure of son preference was fairly low from the start. In this study, we build upon our original evaluation of CHARM2, which relied on an 18-month follow-up, to conduct a 48-month follow-up to allow for a longer-term evaluation of CHARM2 effects on births and births by sex. Findings can offer important insight into longer-term effects of family planning interventions and their impact on sex ratios in India. At the same time, because our original 18-month follow-up fell during the period of the COVID-19 pandemic in India, which was quite severe in terms of both deaths and impact on health system infrastructure [14], observed effects at that time may have been hindered. This study additionally allows for evaluation of the longer term effects of the intervention subsequent to the exogenous shock of the pandemic, which resulted in a reduction in desire for births in some contexts [15].

2. Methods

We conducted a two-arm cluster randomized controlled trial to evaluate the impact of a gender equity focused family planning intervention for couples connected to the local health system, our CHARM2 intervention, relative to standard of care, using survey research with young married couples and pregnancy tests with women. The CHARM2 intervention involved gender equity and family planning counseling by a trained health provider for men and women separately for two sessions, respectively, and then a final session with the couple together to discuss family planning communication and goals. See the study protocol and our primary outcome manuscript for more details on the intervention and study design [7,8]. This study enrolled married couples from the rural Pune district of Maharashtra, India. We collected data at baseline in September 2018–June 2019, 9-month follow-up July 2019–March 2020, 18-month follow-up June–December 2020, 36-month follow-up March–August 2022, and 48-month follow-up in April–September 2023. We focus on births across all four years of follow-up.

Our cluster RCT randomized 20 geographic clusters to either the intervention or control condition. We enrolled 60–61 eligible couples per cluster, with $N = 1201$ total couples. CHARM2 was powered to test for study effects on contraceptive use (primary outcome), not for this study outcome, births by sex [7]. Eligible couples lived together for at least 6 months, wife was aged 18–29 years, and neither spouse was sterilized or infertile. Both spouses agreed to participate and provided baseline survey responses. Gender-matched trained research staff recruited participants in their households, screened them for eligibility, obtained written informed consent, and conducted 30–60-min surveys using electronic tablets in a private location. Staff interviewed husbands and wives separately. Interviewers provided all participants with referrals to local public family planning services; women were given referral to domestic violence services, regardless of survey responses. This procedure was used at baseline and each follow-up. We connected intervention participants with providers following the baseline survey so they could receive the intervention within one month of baseline.

The institutional review boards of the University of California San Diego (UCSD), Population Council, and the National Institute for Research in Reproductive Health in India approved the protocol for the original study, involving data collection from baseline to 18-month follow-up. The 36- and 48-month follow-up surveys were approved by Sigma's Institutional Review Board in India and UCSD.

2.1. Measures

The primary outcome of interest was sex of live births occurring during CHARM2 follow-up. Births were defined based on reported number and sex of live births in the interval prior to each survey (9 months before survey for 9-month and 18-month follow-up; 12 months before survey for 36-month and 48-month follow-up). We examined births by specific time point and over the entire 48-month follow-up.

As the preceding interval was assessed as a fixed number of months rather than time since last survey, some births may not have been included (e.g., only births occurring in the previous 12 months were assessed after an 18-month interval). To address this gap, we included a full birth roster at 48-month follow-up and calculated the number of children born during the study as the difference between the number of births reported at 48-month versus baseline (overall and by child sex).

We classified the distribution of living children by sex at baseline and 48-month follow-up, based on child roster information indicating whether the child was living at time of survey, categorized as: no children; 1 boy only; 1 girl only; 2 children, both boys; 2 children, both girls; 2 children, 1 boy and 1 girl; 3+ children, with a boy; 3+ children, no boy.

2.2. Analyses

We conducted several analyses to disentangle the sex composition of children born subsequent to implementation of the CHARM2 intervention.

First, we summarized the number of births during each follow-up period and for the full 48-month follow-up, in total and by child sex, overall and by mothers' study treatment. The ratio of male to female births was computed at each time point and for entire 48-month follow-up.

Second, we conducted a sensitivity analysis replicating these birth rates and sex ratios, utilizing a measure of births based on full child rosters at baseline and 48-month survey, rather than births reported in the preceding interval at each survey.

Third, we examined the sex of births in preceding time interval at each follow-up survey and for the total follow-up period and conducted bivariate chi-squared tests to assess whether there was a difference in the likelihood of female vs male child sex by treatment status in each of these time periods.

Fourth, we conducted minimally and fully adjusted mixed-effects logistic regression models examining child sex composition of births, for each time period and for the 48-month follow-up period as a whole. For single time point analyses, women with multiple live births (including twins) were excluded as few ($n = 0$ to $n = 2$) women at each period had multiple live births. For the total 48-month follow-up period, the live birth was the unit of analysis, accounting for multiple births to individual mothers via mother-level random intercepts. All models controlled for treatment condition and included random intercepts for subcenters, the intervention's geographic randomization units. Fully adjusted models additionally controlled for factors associated with treatment condition and/or loss-to-follow-up as defined in primary outcome analyses: women's age (continuous), husband's age (continuous), age at marriage (continuous), religion (Hindu versus Muslim, Buddhist, Jain, Christian, or Other), Scheduled Caste, Scheduled Tribe, or Other Backwards Class designation (yes/no), household Below Poverty Line card ownership (a proxy of low income, yes/no), and an indicator for whether the woman's mother-in-law lived on the same property (yes/no) [8].

We then conducted four post-hoc exploratory analyses to better contextualize and understand our findings:

1) The first post-hoc analysis examined whether differences in child sex composition were due to reported abortion rates. We assessed this via bivariate comparisons of self-reported abortion rates by treatment status at each follow-up time point.
2) The second examined whether there were any differences in son preference by treatment status, as measured by a self-reported desire for more sons than daughters. We assessed this via bivariate comparisons of son preference by treatment status at each follow-up time point.
3) The third examined intervention factors (location of intervention receipt and type of intervention provider) via bivariate comparisons and addition to minimally adjusted models defined above, to examine whether specific CHARM2 intervention factors were associated with child sex differentials.
4) The final post-hoc exploratory analysis examined living child composition categorically at baseline and at 48-month follow-up, and assessed whether child sex differentials by treatment status depended on the number and sex of living children at baseline.

3. Results

3.1. Analytic sample and retention rates

The CHARM2 intervention enrolled 1201 married couples. As this study relied on women's survey response, we only focus on women's retention rates: 91 % at 9-month follow-up ($n = 1089$), 91 % at 18-months ($n = 1088$), 88 % at 36-months ($n = 1062$), and 89 % at 48-months ($n = 1065$). A total of $n = 1172$ women (98 %) responded to at least one follow-up survey, and $n = 941$ women (78 %) responded to all four. We found no difference in study retention by treatment group for any follow-up or for all follow-ups combined.

At baseline, women were 24 years old on average (SD 3, range 18–29 years), while their husbands averaged 29 years old (SD 4, range 18–43). Most (86 %) had secondary or higher education, most (92 %) were Hindu, one third (32 %) belonged to a scheduled tribe, scheduled caste, or Other Backwards Caste, one quarter (25 %) held a BPL card (indicating low income), and most lived with their mother-in-law (80 %). About half (54 %) had given birth once, 26 % twice, 4 % 3 or more times, while 16 % were nulliparous; 19 % were pregnant at the time of baseline survey. Age, husband's age, education, caste, BPL card ownership, parity, and current pregnancy did not differ by treatment condition. Of all examined sociodemographic characteristics, only two factors differed by treatment condition at $p < 0.20$: Hindu religion (97 % intervention vs 88 % control, $p < 0.001$) and whether the woman's mother-in-law lived on the same property (82 % intervention vs 78 % control, $p = 0.09$).

3.2. Births and sex ratio at follow-up

Over the entire 48 months, 476 women (39.6 %) reported 523 total live births, 292 boys and 231 girls. The largest number of births were reported between baseline and 9-month follow-up (201), followed by those between 9- and 18-month follow-up (121), 18- and 36-month follow-up (113), and 36- and 48-month follow-up (88). There was no difference by treatment group in terms of having a birth over the total 48-month follow-up period: 39.3 % of intervention and 39.9 % of control reported a birth ($p = 0.83$) (Fig. 1).

There was no difference by treatment group in terms of having a boy over the total follow-up period (25.0 % intervention vs 21.8 % control, $p = 0.19$) (Fig. 2A). Women in the intervention condition were marginally less likely those in the control group to have had a girl over the total follow-up period (16.3 % intervention vs 20.1 % control, $p = 0.09$) (Fig. 2B).

In bivariate single-time-point tests, intervention and control women were equally likely to report a birth. At 9-month follow-up, there was no difference by treatment group in having a boy birth (10.6 % intervention vs 8.7 % control, $p = 0.28$), but intervention women were marginally less likely than women in the control condition to have a girl birth (7.1 % vs 10.3 %, $p = 0.06$). No differences in births by child's sex were seen at any other follow-up.

The sex ratio of all births during the 48-month follow-up period, regardless of treatment group, was 1.26 (95 % CI 1.06–1.50) boys for every girl; this ratio increased over time: 1.09 (95 % CI 0.83–1.44) at 9-month follow-up, 1.37 (95 % CI 0.96–1.97) at 18-months, 1.22 (95 % CI 0.84–1.76) at 36-months, and 1.67 (95 % CI 1.08–2.57) at 48-months (Table 1, Fig. 3). The sex ratio was marginally higher for intervention compared with control group women for the entire 48-month follow-up (1.50 [95 % CI 1.17–1.92] vs. 1.08 [95 % CI 0.85–1.37], $p = 0.06$). The sex ratio was statistically significantly higher at 9-month follow up (1.50 [95 % CI 1.00–2.26] vs. 0.83 [95 % CI 0.56–1.21], $p = 0.04$), but not any other timepoint. (See Fig. 4.)

In minimally adjusted logistic regression models, births to women in the intervention arm were marginally more likely to be boys compared to the control arm (AOR 1.39, 95 % CI 0.97–2.00); this was attenuated in fully adjusted models (AOR 1.33, 95 % CI 0.92–1.91) (TABLE 2). In models specific to follow-up time point, treatment status was significantly associated with likelihood of boy birth only at 9-month follow-up (minimally adjusted AOR 1.77, 95 % CI 1.002–3.11; fully adjusted AOR 1.80, 95 % CI 0.997–3.26).

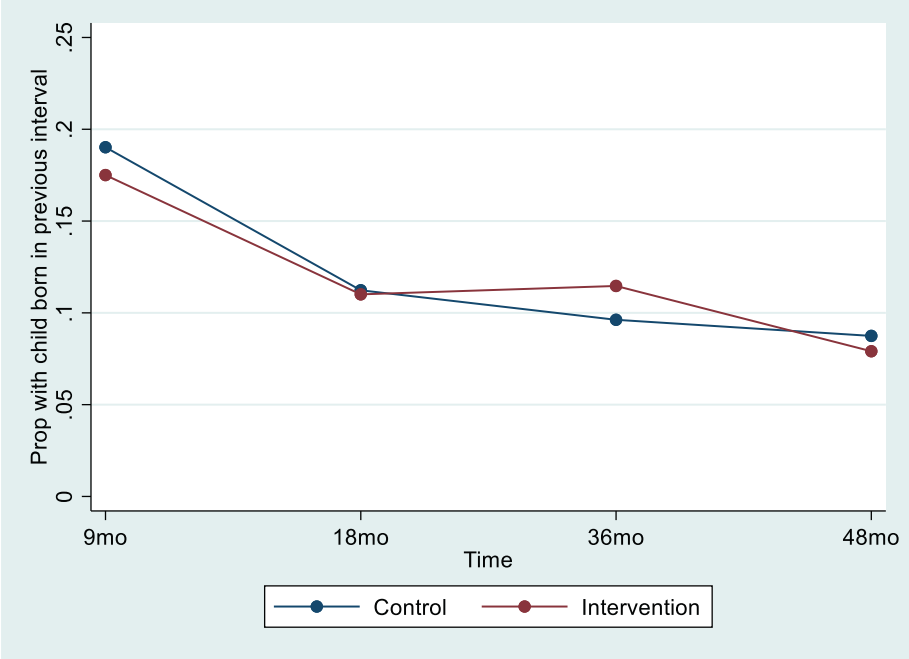

Fig. 1. Proportion of women who gave birth (either sex) in preceding time interval, by CHARM2 treatment condition.

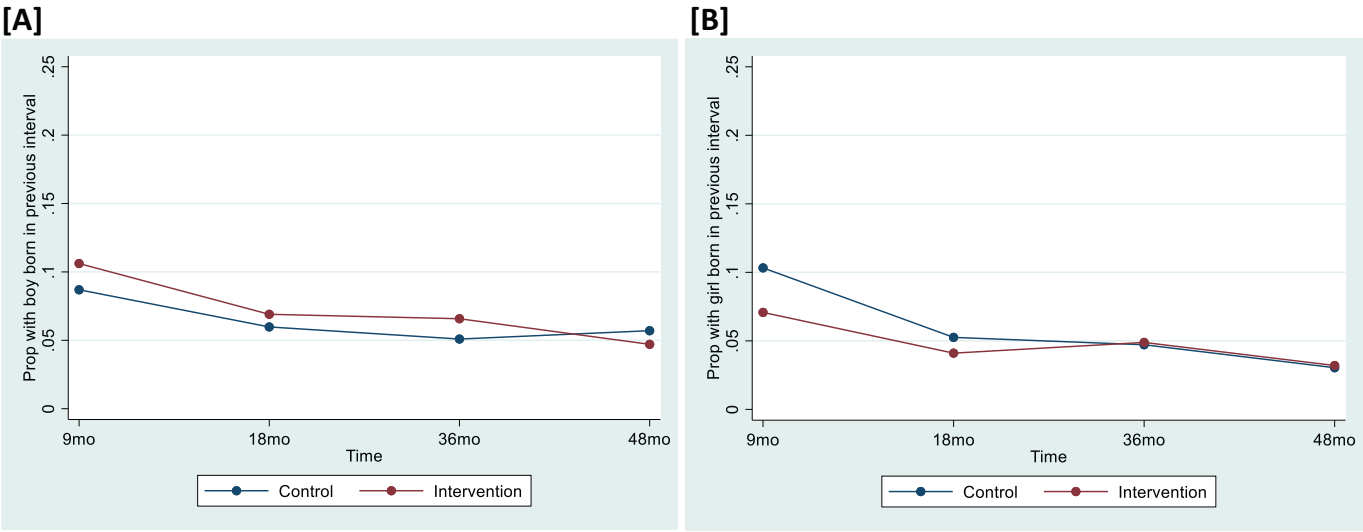

Fig. 2. Proportion of women who gave birth to a boy [Panel A] or a girl [Panel B] in preceding time interval, by CHARM2 treatment condition.

Table 1
Sex ratio of births (boys/girls) in preceding time interval, overall and by treatment condition.

	Overall		Intervention		Control		Intervention vs control *p*-value
	Ratio	95 % CI	Ratio	95 % CI	Ratio	95 % CI	
Total follow-up period	*1.26*	*1.06–1.50*	*1.50*	*1.17–1.92*	*1.08*	*0.85–1.37*	*0.06*
9-month	1.09	0.83–1.44	1.50	1.00–2.26	0.83	0.56–1.21	0.04
18-month	1.37	0.96–1.97	1.68	0.99–2.85	1.14	0.69–1.87	0.29
36-month	1.22	0.84–1.76	1.35	0.81–2.24	1.08	0.63–1.86	0.56
48-month	1.67	1.08–2.57	1.47	0.79–2.72	1.88	1.02–3.44	0.58

3.3. Sensitivity analysis: births in follow-up using full birth history at 48-month follow-up

Results were similar when utilizing differences in full birth histories reported at the 48-month follow-up survey compared to the baseline survey. This sample was necessarily limited to women who provided 48-month follow-up survey data ($n = 1057$, 88 % of full baseline sample).

At 48-month follow-up, 574 women (54 %) reported at least one

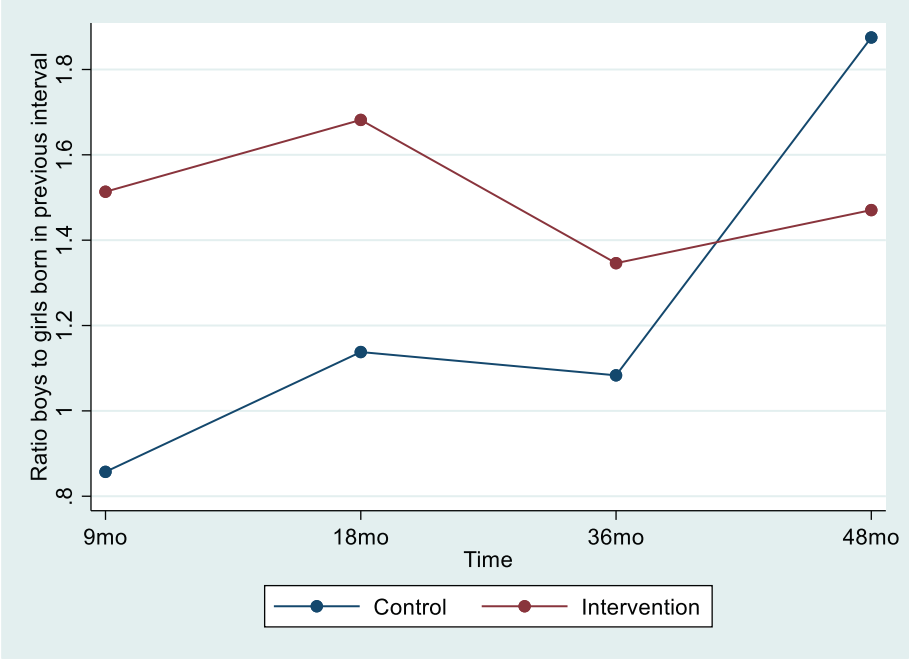

Fig. 3. Sex ratio of births (boys/girls) in preceding time interval, by CHARM2 treatment condition.

additional child born in the 48-month follow-up period, including 634 total births. The percent of women reporting an additional birth did not differ by treatment group (53 % of intervention women reported an additional birth, compared to 56 % of control women, $p = 0.25$). One third of women ($n = 330$, 31 %) reported that they gave birth to at least one boy in the 4 years after baseline survey; this also did not differ by treatment status (32 % intervention, 30 % control, $p = 0.41$). One quarter of women ($n = 273$, 26 %) reported that they gave birth to at least one girl in the 4 years after baseline survey, and this was significantly lower for intervention compared with control group women (22 % vs 29 %, $p = 0.03$). The sex ratio of children born in the 48-month follow-up period for all women was 1.21 (95 % CI 1.03–1.41); this sex ratio was higher for women in the intervention compared with the control group, though not statistically significantly so (1.38 [95 % CI 1.10–1.73] vs. 1.07 [95 % CI 0.86–1.33], $p = 0.11$).

3.4. Post-hoc analysis: reported abortion rates

The difference in child sex composition could be explained by a difference in abortion rates. We directly assessed self-reported abortion history in the survey, but we found no significant difference by treatment condition in reported abortions in the preceding time interval at 9-month ($p = 0.65$), 18-month ($p = 0.54$), 36-month ($p = 0.20$), or 48-month follow-up ($p = 0.53$). There was also no significant difference in abortion reporting at any follow-up survey by treatment status (7.2 % intervention vs 7.8 % control, $p = 0.67$).

3.5. Post-hoc analysis: reported son preference

Differences in the sex of births may have been driven by differences in son preference. Reported son preference (a report of a higher total number of sons desired than total number of daughters desired) was relatively low in the population overall at baseline, with 3.6 % of participants reporting a preference for more sons than daughters. This did not differ by treatment status at baseline (4 % intervention vs 3 % control, $p = 0.16$). Reported son preference also did not differ by treatment status at 9-month follow-up (2 % intervention vs 3 % control, $p = 0.15$), 18-month follow-up (2 % intervention vs 2 % control, $p = 0.30$), or 36-month follow-up (2 % intervention vs 2 % control, $p = 0.27$); this item was not assessed at 48-month follow-up.

3.6. Post-hoc analysis: intervention factors

We next assessed whether specific intervention factors (location of intervention & provider type) explained the observed differences in child sex composition by treatment condition.

First, we limited analyses to births between baseline and 48-month follow-up born to women in the intervention arm. Location of intervention receipt (at their own home, at a subcenter facility, or at a VHP clinic) was not associated with likelihood of girl birth (results not shown). The type of provider who implemented the intervention (auxiliary nurse midwives [ANM], Ayurvedic or Homeopathic physician, allopathic physician, or non-medical research staff) was significantly associated with likelihood of girl birth, however. Relative to women who received the intervention from ANMs, births to women who received the intervention from allopathic physicians were significantly less likely to be girls (AOR 0.47, 95 % CI 0.23–0.98, $p = 0.04$); there was no association with likelihood of girl birth for births to women who had Ayurvedic or Homeopathic physicians or non-medical research staff as their intervention provider. Of births occurring to women in the treatment arm during follow-up, approximately one quarter (26 %) were born to women who received the intervention from an allopathic physician.

To examine whether this difference in likelihood of girl births by intervention provider explained the observed difference in likelihood of girl births by treatment condition, we replicated the fully adjusted models of likelihood of girl birth during follow-up for the full sample, modifying the treatment variable to indicate whether the mother had received the intervention from an allopathic doctor or from another type of provider. Using the 3-level treatment variable, treatment women who saw an allopathic doctor had significantly lower odds of a girl birth over the total 48-month follow-up period relative to control women (AOR 0.47, 95 % CI 0.26–0.87, $p = 0.02$), while treatment women who saw a different type of provider had equivalent odds of girl birth relative to control (AOR 0.89, 95 % CI 0.59–1.32, $p = 0.6$). In individual time point tests, this association was only present at 9-month follow-up, where treatment women who saw an allopathic doctor had lower odds of girl birth relative to control (AOR 0.16, 95 % CI 0.05–0.51, $p = 0.002$) and

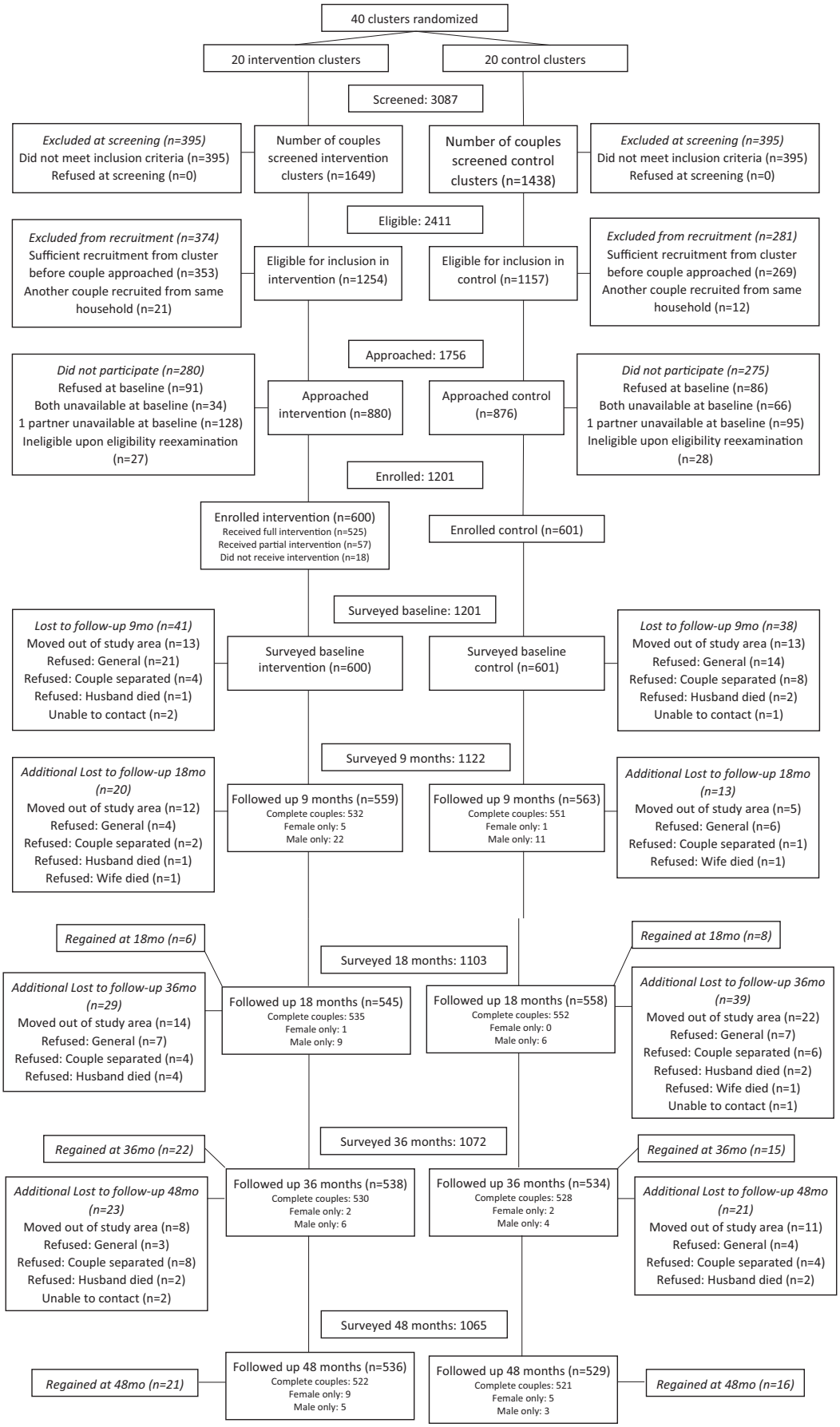

Fig. 4. CHARM2 recruitment and retention CONSORT flow diagram.

Table 2
Percent of boy births by treatment status and likelihood of boy birth in minimally and fully adjusted mixed-effects logistic regression.

	Minimally adjusted likelihood of boy birth		Full adjusted likelihood of boy birth	
	AOR	95 % CI	AOR	95 % CI
Total follow-up period	*1.39*	*0.97–2.00*	*1.33*	*0.92–1.91*
9-month	1.77*	1.002–3.11	1.80	0.997–3.26
18-month	1.50	0.63–3.57	1.56	0.69–4.06
36-month	1.24	0.59–2.64	1.24	0.54–2.86
48-month	0.78	0.33–1.86	0.61	0.23–1.63

treatment women who saw a different type of provider had equivalent odds of a girl birth relative to control (AOR 0.78, 95 % CI 0.41–1.48, $p = 0.4$). No other specific follow-up time periods had significant differences in odds of girl birth across this 3-level treatment measure.

3.7. Post-hoc analysis: birth order and sex of children born in follow-up

To further understand the nature of sex imbalances among children born during follow-up, we examined living children compositions at baseline and 48-month follow-up (TABLE 3). We find that the living child composition of women at baseline did not differ by treatment status for any category of children. At 48-month follow-up, the only living child composition category with significant difference between treatment arms was having three or more children, all girls. Intervention women were significantly less likely to have this grouping of living children compared to control women (2.1 % of intervention women vs 4.8 % of control women, $p = 0.02$).

Finally, we assessed whether the observed treatment effect on lower likelihood of girl birth differed by the number and sex of living children at baseline. To examine this, we stratified by baseline living child composition category and tested whether the proportion reporting a girl birth during follow-up or a boy birth during follow-up differed by treatment status. We found that there was no significant difference by treatment status in likelihood of a girl birth in the follow-up time-period for most baseline living children composition categories, with the sole exception of women with two children, both girls at baseline (9 % of intervention women in this category had a girl birth in follow-up, compared to 30 % of control women, $p = 0.005$). Similarly, the only baseline child composition category which significantly differed by treatment status in likelihood of subsequent boy birth was women with two children, both girls (42 % of intervention women in this category had a boy birth in follow-up, compared to 21 % of control women, $p = 0.01$).

4. Discussion

We evaluated the longer-term impacts of CHARM2, a gender equity and family planning intervention for young married couples in rural India, on births and sex of children born over the 48-month follow-up period using a rigorous two-armed cluster randomized controlled trial, with strong (>80 %) retention rates. We found no significant difference by treatment group in likelihood of births at any follow-up point nor across the total 48-month follow-up period. However, we do find that participants in our intervention group were less likely to have had a girl child over the 48-month follow-up period, with this difference emerging in the interval between baseline and 9-month follow-up, corresponding with receipt of the intervention from a health provider.

This effect was most evident among those with two girls at baseline, with a girl birth less likely (9 % vs 30 %, $p = 0.005$) and a boy birth more likely (42 % vs 21 %, $p = 0.01$) for intervention relative to control participants. This finding corresponds with prior research from nationally representative cross-sectional research from India documenting the role of birth order and sex composition of families in predicting contraceptive use. These studies show that couples without a son are less likely than those with a son to use contraceptives, regardless of whether they have daughters, and they are more likely to obtain sterilization at lower parity only if they have had a son [11,12]. National data on higher order births also shows that boy births are more likely in families with no son and in large landowning families [16], suggesting a link between son preference and land inheritance which may be meaningful for our sample, which was recruited from an agrarian, largely landowning community. Shifting norms on gender and land inheritance may help alter these son preferences.

Within the intervention group, the reduced likelihood of having a girl child was greatest among those who received care from a physician rather than a nurse or nurse-midwife. Because the sole mechanism by which fewer girl children could be produced is sex-selective abortion, and seeing a physician provides greater access to a range of medical services, greater access to abortion services was likely for this group. Although we found no difference in self-reported abortion or son-preference between groups, because sex-selective abortion is illegal in India, stigma and fear may have led to under-reporting. If abortion is, in fact, behind the finding of fewer girl births, the positive effects of CHARM2 on self-efficacy and control over reproductive decisions suggest that an increased likelihood of abortion may also reflect increased female reproductive control for this sample. Given that norms of son preference were not affected by the intervention, increased access to reproductive care, including abortion, in the context of such norms may explain the observed decrease in girl births. These results should be considered cautiously, as allopathic medicine could be a marker for access to abortion, but it also could be a marker for differential intervention delivery. Also, as this is a post-hoc analysis, it is vulnerable to Type I error.

Ideally, all providers, not just allopathic physicians, should be able to support women to receive the full range of reproductive services, but evidence suggests this not to be the case in India. Further, we interpret these findings as indicative of greater reproductive choice for the intervention group aligning with being less likely to have a girl, based on the cluster of demonstrated outcomes including contraceptive use, satisfaction, and perceptions of greater quality of reproductive health care from providers [7–10]. However, it must be noted that neither

Table 3
Composition of living children at CHARM2 baseline and 48-month follow-up.

	Baseline				48-month follow-up			
	Total	Control	Intervention	*p-value*	Total	Control	Intervention	*p-value*
N	*1057*	*526*	*531*		*1057*	*526*	*531*	
No children	*16.2 %*	15.2 %	17.1 %	*0.39*	*1.8 %*	1.9 %	1.7 %	*0.80*
1 boy only	*32.4 %*	33.3 %	31.5 %	*0.53*	*22.0 %*	19.8 %	24.1 %	*0.09*
1 girl only	*23.4 %*	22.2 %	24.5 %	*0.39*	*12.5 %*	12.0 %	13.0 %	*0.62*
2 children, both boys	*3.6 %*	3.2 %	4.0 %	*0.53*	*12.1 %*	12.6 %	11.7 %	*0.66*
2 children, both girls	*11.1 %*	12.0 %	10.2 %	*0.35*	*12.5 %*	12.7 %	12.2 %	*0.81*
2 children, 1 boy/1 girl	*10.6 %*	10.5 %	10.7 %	*0.88*	*29.4 %*	30.2 %	28.6 %	*0.57*
3+ children, with a boy	*1.0 %*	1.3 %	0.6 %	*0.20*	*6.3 %*	6.1 %	6.6 %	*0.73*
3+ children, no boy	*1.9 %*	2.3 %	1.5 %	*0.36*	*3.4 %*	4.8 %	2.1 %	*0.02*

abortion nor contraceptive use are necessarily markers of greater reproductive agency for women; it is only in combination with measures of family planning preferences and choice and satisfaction that we can ascertain reproductive agency in contraceptive use. Abortion and contraceptive use can be forced or coerced acts by partners, family members, and providers; but our measures suggest that this is not the case in this study.

Historically, skewed sex ratios have led to laws restricting access to abortion services, compromising women's health and agency [13,17]. Current findings from this study cannot and should not be used as fodder to restrict women's access to contraceptives or any necessary reproductive healthcare. This approach violates women's rights and has not been shown to impact underlying son preference norms or practices.

Abortion is a form of reproductive health care, but anti-abortion movements vilify both women who seek or obtain these services as well as the physicians who provide them. Shifts toward social acceptability of abortion as a healthy option for women of reproductive age can facilitate a better understanding of its role in the effects of son preference on women's reproductive practices. As discussed above, current findings may suggest that allopathic physicians are better able to provide women access to a broader array of reproductive health services for their care. These findings also highlight the importance of longer-term follow-up of family planning intervention trials, and the likely limitations of clinical interventions for couples that do not support social norm and policy shifts that can improve the value and positioning of girl children. The solution cannot be to limit family planning to those who make the choices we want for them; family planning should support women's reproductive agency, including their choice not to have or proceed with a pregnancy for any reason. At the same time, efforts must be made to improve the value of the girl child and eliminate son preference.

Certain study limitations must be considered. We rely on self-report, which can be subject to recall bias and social desirability. However, as our outcomes focus on births and sex of child at birth, we presume these biases are limited; reports of abortion and son preference are likely more vulnerable to social desirability biases and under-reporting. Post hoc exploratory findings are vulnerable to Type I error and should be interpreted with caution. As this study is longitudinal, we may have some bias in loss to follow-up related to our key areas of interest in this paper. High follow-up rates in this study somewhat reduce this risk. Findings may not be generalizable outside of our study area. However, findings are likely meaningful for much of rural India and particularly for agrarian areas with a large middle-class population.

5. Conclusion

The current study offers insight into the longer-term effects of a gender equity focused family planning intervention, CHARM2, using a two-armed RCT that demonstrated significant treatment effects on contraceptive use, communication, agency, and satisfaction by 18-month follow-up [8,9], by showing that by 48-month follow-up, we see lower likelihood of girl births for intervention compared with control participants. These findings have important implications for India, which is now world's most populous nation [18] but is on a path to population decline, with a fertility rate now lower than replacement [19] and a growing access to and support for modern reversible contraceptives [20]. Certainly, family planning is important for women's economic and reproductive agency as well as for maternal and infant health [21]. However, persistent son preference norms [2,22] especially pervasive in rural India [1,2], mean that rational reproductive choices for women can include family planning practices that increase odds of male births and reduce odds of female births.

These findings highlight the need for broader social change to advance the status and value of women and girls in India. Certainly, we want to see a more balanced sex ratio in the country, but policy restrictions on reproductive services, including those related to abortion, only impede women's reproductive agency and blame women for their rational decision-making to prioritize boy births for social and economic security for themselves and their offspring. Improvements in sex ratio imbalances in India are often presumed to be a demonstration of increasing the value of the girl child, but these may instead be indicative of growing restrictions in reproductive access. Current findings offer important policy implications. First, gender equity focused family planning interventions can increase reproductive agency as well as contraceptive use, but these may benefit from social norm change and policy-focused interventions that increase the value, status, and opportunity of women and girls. Tracking of sex ratios as a metric of success in terms of value for girls can only be useful if abortion is unrestricted; otherwise, it may simply be an indicator of restricted access. We should track women's reproductive choice and agency, girl child survival and aspiration achievement, and women's economic capacities as true indicators of gender equity.

Data sharing statement

Data used for presented analyses and analytic code are available upon reasonable request from the corresponding author.

Evidence before this study

We systematically reviewed clinical trials evaluating gender-equity focused and gender-transformative family planning interventions, as well as systematic reviews of family planning intervention evaluations, to identify those interventions that demonstrate significant impact on births over time and sex ratios of births following treatment exposures. In December 2023 and January 2024,we searched PubMed using the following MeSH search terms: (“family planning” OR “contracept”) AND (“gender-transformative”or“genderequity”) OR (“male engagement”) with the following filters “clinical trial” (Phases I-IV and Pragmatic, excluded veterinary), “randomized controlled trial”, “evaluation study”, “metaanalysis”, “observational study” (excluded veterinary), “review”, and “systematic review”. We also limited our search to articles published in the past 10 years. We additionally obtained papers and reports known to and recommended by our co-authors on this paper and expert colleagues. We identified 63 peer-reviewed publications with rigorous RCT designs and demonstrated effects on contraceptive use. We could identify no single paper or follow-up study from the identified papers that included longer term follow-up data on births nor births by sex in the follow-up period.

Added value of this study

This study involves evaluation of longer-term effects of an effective family planning intervention with young married couples in rural India, CHARM2, on births and sex of births over a 48-month follow-up period.

This work builds of the original CHARM2 evaluation trial, which involved a two-arm cluster evaluation trial testing the effect of CHARM2, a gender synchronized, gender equity focused family planning intervention, on contraceptive practices among young married couples in rural India. CHARM2 is a 5-session intervention embedded into the local health system, offering two gender-synchronized sessions conducted by a gender-matched health provider for couples and a final couple-focused session to support integrated learnings and couple communication on family planning. The originally published CHARM2 trial demonstrated significant effects of the program on modern reversible contraceptive use and women's contraceptive agency at 9-month follow-up and women's contraceptive agency, spousal contraceptive communication, and pregnancy planning at 9 and 18-month follow-ups.

Current findings further show that intervention participants were more likely to have a boy relative to a girl child and these effects were most pronounced at 9-month follow-up (sex ratio of births 1.50 [95 % CI

1.00–2.26] intervention vs. 0.83 [95 % CI 0.56–1.21] control, $p = 0.04$). Post hoc analysis further indicates that these 9-month effects are greater when the CHARM2 intervention was delivered to women by an allopathic doctor rather than an auxiliary nurse midwife (ANM) or other provider (AOR 0.16, 95 % 0.05–0.51, $p = 0.002$). We also see that birth roster data at 48-month follow-up shows fewer girls born over the full follow-up period for intervention versus control participants (22 % vs 29 %, $p = 0.03$). Post-hoc analysis of these data shows strongest effects for women with two girls at baseline, among whom intervention relative to control participants were less likely to have had a girl (9 % intervention vs. 30 % control, $p = 0.005$) and more likely to have had a boy (42 % intervention vs. 21 % control, $p = 0.01$) during the follow-up period.

Implications of All available evidence

Effective family planning interventions that support women's reproductive agency, perhaps especially those provided by allopathic physicians who may support patient connection to a broader array of medical services, may increase sex ratio imbalances in contexts where social norms of son preference and devaluation of girls persist. Family planning must occur in tandem with improved social and economic value, positioning, and opportunity for women and girls.

CRediT authorship contribution statement

Anita Raj: Writing – review & editing, Writing – original draft, Supervision, Resources, Methodology, Investigation, Funding acquisition, Conceptualization. **Nicole Johns:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Formal analysis, Data curation. **Florin Vaida:** Writing – review & editing, Supervision, Methodology, Formal analysis. **Mohan Ghule:** Writing – review & editing, Validation, Supervision, Project administration, Methodology, Data curation. **Namratha Rao:** Writing – review & editing, Validation, Project administration, Data curation. **Jay G. Silverman:** Writing – review & editing, Supervision, Methodology, Investigation, Conceptualization.

Declaration of competing interest

The authors have no conflicts of interest to disclose.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.dialog.2025.100218.

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