

Convergence without connectivity: a difference-in-differences analysis of women's mobile phone ownership and antenatal care coverage across 35 Indian states, 2015–2021

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Research Article

Keywords: Maternal health, Digital health, Antenatal care, Health equity, India, Difference-in-differences, Null results, National Family Health Survey

Posted Date: October 7th, 2026

DOI: <https://doi.org/10.21203/rs.3.rs-10554016/v1>

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Additional Declarations: No competing interests reported.

Abstract

Background

India has built one of the largest national digital health infrastructures of any low- or middle-income country, and much of it presumes women can be reached through a mobile phone. Between 2015 and 2021, women's mobile phone ownership and antenatal care coverage both rose substantially, and the two trends are often discussed as though one produced the other. We tested whether that interpretation survives contact with the data.

Methods

From the published state fact sheets of India's National Family Health Survey we assembled a two-round panel spanning NFHS-4 (2015–16) and NFHS-5 (2019–21) for 35 states and union territories. The exposure was the proportion of women aged 15–49 owning a mobile phone they themselves use; the outcome was coverage of four or more antenatal care visits (ANC4). States were divided at the NFHS-4 baseline median, with group membership fixed at baseline. We estimated difference-in-differences models across five specifications: a continuous-exposure form, five grouping thresholds, and three maternal outcomes. Inference used cluster-robust standard errors, randomization inference (10,000 permutations), and a cluster bootstrap (5,000 resamples), with leave-one-state-out refitting, power calculation, and equivalence testing.

Results

ANC4 coverage rose nationally by 5.9 percentage points (95% CI 1.3 to 10.5; $p = 0.012$) and converged across states. Beta-convergence was strong: each percentage point of baseline coverage predicted 0.273 fewer points of gain (95% CI – 0.412 to – 0.134; $p < 0.001$; $R^2=0.326$). Adding change in mobile ownership raised explained variance by 0.008 ($p = 0.539$). The difference-in-differences interaction was – 2.91 percentage points (95% CI – 9.98 to 4.16; $p = 0.420$) and reversed sign across specifications. Randomization inference agreed ($p = 0.412$), and the estimate was significant in none of 35 leave-one-out refits. Minimum detectable effect was 10.1 percentage points; equivalence testing excluded effects beyond ± 10 points but not ± 5 . Twelve of 35 states recorded declining coverage, including Kerala and Punjab.

Conclusions

Maternal coverage converged in India between 2015 and 2021, explained by baseline coverage rather than digital expansion. This is a well-characterized but imprecise null. Aggregate co-movement of connectivity and coverage should not be read causally.

Background

The Ayushman Bharat Digital Mission, the Health Management Information System, the Reproductive and Child Health portal and the Mother and Child Protection card together constitute a national digital health architecture of considerable ambition [1]. Each of them, in different ways, assumes that a woman can be registered, reminded, tracked and followed up through a device she owns and can operate. Until recently, that assumption described a minority of Indian women.

It describes more of them now. Between the fourth and fifth rounds of the National Family Health Survey, mobile phone ownership among women aged 15–49 rose from a state mean of 55.2% to 64.6%. Over the same interval, the share of mothers receiving four or more antenatal visits rose from 61.0% to 65.5%. The two series climb together, and that shared trajectory is routinely offered as evidence that connectivity is doing the work [2].

We are not persuaded that it can be. The same five years saw India expand conditional cash transfers, recruit frontline health workers, build facilities, extend insurance coverage and raise female educational attainment. All of these rose alongside connectivity, and any of them could account for the coverage gains. The published evidence for a digital effect comes mostly from evaluations of particular applications, which can establish that a well-designed intervention works in the setting where it was tested but say little about whether the underlying spread of handsets moves population coverage [3, 4].

The question we ask here is narrower and, we think, more tractable. Did states whose women were better connected in 2015–16 gain more antenatal coverage over the following five years? And did states where connectivity grew fastest gain the most? We report what we found, including the places where we found nothing.

Methods

Data sources

We used the published state fact sheets of NFHS-4 (2015–16) and NFHS-5 (2019–21), the nationally representative household survey conducted under India's Ministry of Health and Family Welfare.

Indicators were identified by matching the entire text of each column header. This is important because both files include full headers, with one column for “at least 4 antenatal care visits” and another for “mobile phone that they themselves use.” As a result, there is no confusion about the variables. Only rows representing the total population were used, excluding those specific to urban or rural areas, and all-India summary rows were omitted.

Several states and union territories are named differently across the two rounds, and we harmonized them; NCT of Delhi to Delhi, and the various spellings of Andaman and Nicobar Islands to a single form. Dadra and Nagar Haveli and Daman and Diu are reported separately in NFHS-4 and jointly in NFHS-5,

following their 2020 merger, so we averaged the earlier pair. Ladakh appeared only in NFHS-5 and was excluded. The resulting panel covers 35 states and union territories observed twice, giving 70 observations and 35 clusters.

Variables

Our exposure was the proportion of women aged 15–49 who own a mobile phone that they themselves use. The primary outcome was ANC4 coverage: the proportion of mothers who received four or more antenatal care visits for their most recent birth in the five years preceding the survey. We examined two further maternal outcomes, first-trimester antenatal check-up and institutional birth, to determine whether any findings were specific to ANC4. Female literacy and the proportion of women with 10 or more years of schooling were used as covariates. The sex ratio of the total population served as a placebo outcome and was deliberately excluded from every covariate set to remain available for that purpose.

Assigning states to groups

We divided states at the NFHS-4 median of women's mobile ownership, which is 54.2%, giving 17 higher-access and 18 lower-access states. Group membership was fixed using the baseline round and does not vary over time. This matters for the credibility of the design: assigning states on the basis of their later connectivity would condition the treatment on an outcome measured after the exposure period, and would not identify what we claim to identify.

Statistical analysis

Our primary model took the form

$$Y_{st} = \alpha + \beta_1 \text{Group}_s + \beta_2 \text{Post}_t + \beta_3 (\text{Group}_s \times \text{Post}_t) + X_{st}\gamma + \varepsilon_{st}$$

where Y_{st} is ANC4 coverage in state s at survey round t ; Group_s is a time-invariant binary indicator taking the value 1 for states above the NFHS-4 median of women's mobile ownership; Post_t indicates the NFHS-5 round; X_{st} is a vector of time-varying covariates with coefficient vector γ ; and ε_{st} is an error term clustered at the state level, allowing arbitrary correlation between a state's two observations [5].

The coefficient of interest is β_3 , which measures the change in coverage among higher-access states over and above the change common to all states. Because Group_s and Post_t also enter separately, β_3 is identified net of two things it might otherwise be confused with: whatever is permanently different about the two groups, which β_1 absorbs, and whatever moved all states together over the period, which β_2 absorbs. What remains is the differential change.

We estimated the model five ways. The first was unadjusted. The second added female literacy and the third added literacy together with the proportion of women having ten or more years of schooling, educational attainment being the most plausible common cause of both connectivity and service use. The fourth added baseline ANC4, to establish whether any apparent effect could be distinguished from simple convergence toward the mean. The fifth replaced the group indicator with a full set of state fixed effects, which absorbs every time-invariant state characteristic rather than only the binary access grouping.

Dichotomizing a continuous exposure at its median discards information and imposes a cut point that carries no substantive meaning. We therefore also fitted a continuous specification in which baseline mobile ownership, measured in percentage points, interacts with the post indicator. The interaction coefficient in that model estimates the additional change in ANC4 associated with each further percentage point of baseline connectivity, and does not require states to be sorted into groups at all.

Thirty-five clusters are not many, and asymptotic cluster-robust standard errors are known to be anti-conservative in that range. We therefore did not rely on them alone. We computed randomization-inference p-values by permuting group assignments 10,000 times and refitting, and obtained confidence intervals from a cluster bootstrap with 5,000 resamples. We also refitted the model thirty-five times, each time omitting one state, to see whether any single unit was carrying the result.

Convergence was assessed in the two conventional forms. Sigma-convergence asks whether states became more alike, and we tested the change in cross-state variance directly. Beta-convergence asks whether those starting lower gained more, and we regressed the change in coverage on its baseline value.

A null result is only interpretable if one knows what could have been detected, so we computed the minimum detectable effect at conventional power and used two one-sided tests to assess equivalence at bounds of five, ten and fifteen percentage points.

Robustness comprised five alternative thresholds for defining the groups, the three maternal outcomes, propensity-score-matched difference-in-differences, and the placebo outcome.

All analyses were run in Python 3.12.3 with statsmodels 0.14.6, pandas 3.0.2 and scikit-learn, under random seed 20260726. The complete code and the derived panel are provided as supporting information, so that every number reported here can be regenerated from the two source spreadsheets.

Results

The panel

Table 1 sets out the two indicators for all 35 states and union territories at both rounds. Mean ANC4 coverage rose from 61.0% to 65.5% and mean mobile ownership from 55.2% to 64.6%, but the state-level

spread around those means is wide: ANC4 change ranges from a fall of 16.3 percentage points to a rise of 30.9.

Table 1

Women's mobile phone ownership and ANC4 coverage by state and union territory, NFHS-4 and NFHS-5.

State or union territory	Mobile, NFHS-4 (%)	Mobile, NFHS-5 (%)	ANC4, NFHS-4 (%)	ANC4, NFHS-5 (%)	ANC4 change (pp)
Andaman and Nicobar Islands	66.9	80.8	92.1	83.4	-8.7
Andhra Pradesh	36.2	48.9	76.3	67.5	-8.8
Arunachal Pradesh	59.8	76.4	26.8	36.5	+ 9.7
Assam	46.0	57.2	46.5	50.7	+ 4.2
Bihar	40.9	51.4	14.4	25.2	+ 10.8
Chandigarh	74.2	70.0	64.5	78.7	+ 14.2
Chhattisgarh	31.0	40.7	59.1	60.1	+ 1.0
Dadra and Nagar Haveli & Daman and Diu	48.8	60.5	69.2	86.2	+ 17.0
Delhi	66.6	73.8	68.6	77.2	+ 8.6
Goa	80.9	91.2	89.0	93.0	+ 4.0
Gujarat	47.9	48.8	70.6	76.9	+ 6.3
Haryana	50.5	50.4	45.1	60.4	+ 15.3
Himachal Pradesh	73.9	79.5	69.1	70.3	+ 1.2
Jammu & Kashmir	54.2	75.2	81.4	80.9	-0.5
Jharkhand	35.2	49.0	30.3	38.6	+ 8.3
Karnataka	47.1	61.8	70.3	70.9	+ 0.6
Kerala	81.2	86.6	90.2	78.6	-11.6
Lakshadweep	68.0	84.0	82.8	88.3	+ 5.5
Madhya Pradesh	28.7	38.5	35.7	57.5	+ 21.8
Maharashtra	45.6	54.8	72.2	70.3	-1.9
Manipur	63.1	72.2	69.0	79.4	+ 10.4
Meghalaya	64.3	67.5	50.0	52.2	+ 2.2
Mizoram	77.3	82.3	61.7	58.0	-3.7
Nagaland	70.5	82.5	15.0	20.7	+ 5.7
Odisha	39.2	50.1	62.0	78.1	+ 16.1

State or union territory	Mobile, NFHS-4 (%)	Mobile, NFHS-5 (%)	ANC4, NFHS-4 (%)	ANC4, NFHS-5 (%)	ANC4 change (pp)
Puducherry	67.3	82.9	87.7	86.9	-0.8
Punjab	57.2	61.2	68.5	59.3	-9.2
Rajasthan	41.4	50.2	38.5	55.3	+ 16.8
Sikkim	79.8	88.6	74.7	58.4	-16.3
Tamil Nadu	62.0	74.6	81.2	89.9	+ 8.7
Telangana	47.8	60.0	75.0	70.4	-4.6
Tripura	43.9	53.1	64.3	52.7	-11.6
Uttar Pradesh	37.1	46.5	26.4	42.4	+ 16.0
Uttarakhand	55.4	60.9	30.9	61.8	+ 30.9
West Bengal	41.9	50.1	76.5	75.8	-0.7
All states (mean)	55.2	64.6	61.0	65.5	+ 4.5

ANC4, four or more antenatal care visits for the most recent birth in the five years before the survey; NFHS, National Family Health Survey; pp, percentage points. Mobile refers to women aged 15–49 who own a mobile phone that they themselves use. Values are for the total population of each state or union territory. Ladakh is excluded, appearing only in NFHS-5. Dadra and Nagar Haveli and Daman and Diu are combined to match NFHS-5 reporting. Source: NFHS-4 and NFHS-5 state fact sheets.

How the two groups differed to begin with

The higher- and lower-access states were not comparable at baseline, and the extent of the difference is worth dwelling on (Table 2). Higher-access states had 26.3 percentage points more mobile ownership, a standardized mean difference of 3.50. They also had 17.1 points more female literacy and 17.3 points more secondary schooling, with standardized differences of 2.18 and 1.73. All three gaps were significant beyond $p < 0.001$. Baseline ANC4 differed by 9.7 points but not significantly, and institutional birth was almost identical across groups.

These imbalances are the reason no cross-sectional comparison between the two groups could be informative. A contrast between states differing by more than two standard deviations in female literacy is a contrast between different kinds of state, not a measure of what connectivity does. The difference-in-differences design is preferred here precisely because it removes whatever is fixed about a state.

Table 2
Baseline characteristics at NFHS-4, by digital access group.

Variable	Higher access (n = 17)	Lower access (n = 18)	Difference	SMD	p
Mobile phone ownership (%)	68.7	42.4	26.3	3.50	< 0.001
ANC4 (%)	66.0	56.3	9.7	0.44	0.199
Antenatal care, first trimester (%)	67.6	63.5	4.1	0.26	0.443
Institutional birth (%)	81.4	80.6	0.8	0.05	0.886
Female literacy (%)	84.5	67.4	17.1	2.18	< 0.001
Women with 10 + years schooling (%)	49.8	32.5	17.3	1.73	< 0.001
Sex ratio (females per 1,000 males)	994.1	977.2	16.9	0.28	0.422

ANC4, four or more antenatal care visits; SMD, standardized mean difference. All values are state means at NFHS-4. Difference is higher-access minus lower-access. An absolute SMD above 0.10 conventionally indicates imbalance; above 0.80 indicates a large difference. Comparisons by Welch's t-test for unequal variances.

Coverage converged, and baseline coverage explains it

Dispersion in ANC4 across states fell over the period, with the standard deviation decreasing from 21.93 to 18.12 and the coefficient of variation decreasing from 0.359 to 0.277. We report this as suggestive rather than established: the variance ratio of 1.46 does not reach conventional significance ($p = 0.271$), and with 35 units it would need to be larger to do so.

Beta-convergence is on firmer ground. Each additional percentage point of baseline ANC4 was associated with 0.273 fewer percentage points of subsequent gain (95% CI – 0.412 to – 0.134; $p = 0.00034$), and baseline coverage alone accounts for a third of the variance in how much states gained ($R^2=0.326$). Figure 1 shows the relationship. States that began furthest behind moved furthest; several that began near the top moved backward.

The obvious next question is whether connectivity adds anything to that picture. It does not. Entering the change in mobile ownership alongside baseline coverage left the baseline coefficient essentially untouched at – 0.269 ($p = 0.0005$), returned a coefficient for connectivity of – 0.186 that was indistinguishable from zero ($p = 0.539$), and raised explained variance from 0.326 to 0.334 – an increment of 0.008.

Each point is one state or union territory. The fitted line has slope -0.273 (95% CI -0.412 to -0.134), $p < 0.001$, $R^2=0.326$. The dashed horizontal line marks no change.

Difference-in-differences

Table 3 gives the estimates across five specifications. In the primary model the only coefficient distinguishable from zero is the time term: coverage rose by 5.90 percentage points across all states (95% CI 1.30 to 10.49; $p = 0.012$). The interaction, which is the quantity we care about, is -2.91 points with a confidence interval running from -9.98 to 4.16 .

Table 3
Difference-in-differences estimates for ANC4 coverage across model specifications.

Specification	DiD estimate (pp)	SE	p	95% CI
(1) Unadjusted	-2.91	3.61	0.420	-9.98 to 4.16
(2) Adding female literacy	+ 1.15	3.26	0.723	-5.23 to 7.54
(3) Adding literacy and schooling	-1.09	3.75	0.772	-8.43 to 6.26
(4) Adding baseline ANC4	-2.91	3.63	0.423	-10.03 to 4.21
(5) State fixed effects	-2.91	5.10	0.568	-12.90 to 7.09

CI, confidence interval; DiD, difference-in-differences; SE, standard error; pp, percentage points. The DiD estimate is the coefficient on the group-by-period interaction. The reference category is the lower-access group at NFHS-4. Standard errors are clustered at state level. $n = 70$ state-round observations across 35 clusters. Specification (1) is the primary analysis.

The instability across specifications is itself informative. An estimate that changes sign when a single covariate is added — from -2.91 to $+1.15$ — is not a real effect being obscured by confounding. It is noise.

The continuous form points the same way. Each percentage point of baseline mobile ownership was associated with -0.210 points of subsequent ANC4 change (95% CI -0.430 to 0.010 ; $p = 0.061$). The sign is negative, which is consistent with the catch-up pattern rather than with any benefit from connectivity.

Inference that does not depend on large-sample approximations

Since 35 clusters fall within a range where cluster-robust inference can be misleading, we verified the results in three ways, and they all agree. The analytic p-value is 0.420. Randomization inference with ten thousand permutations yields 0.412, with a null distribution centered at -0.05 and a standard deviation of 3.52; the observed value of -2.91 is well within its central range, where the 2.5th and 97.5th

percentiles are – 7.04 and 6.76. A cluster bootstrap with 5,000 resamples produces a 95% confidence interval from – 9.70 to 4.16.

Influence and sensitivity to how the groups were drawn

Refitting the model thirty-five times, each omitting one state, produced estimates between – 4.65 and – 1.70, and the interaction reached $p < 0.05$ in none of the thirty-five fits. No single state is driving anything.

We also varied the threshold used to define the groups (Table 4). Here we report a complication rather than smoothing it over. One of the five cut points, the upper tertile, returns an estimate of – 6.79 at $p = 0.045$.

Table 4
Sensitivity of the difference-in-differences estimate to the grouping threshold.

Cut point in baseline mobile ownership	States in higher-access group	DiD estimate (pp)	p
40th percentile	21	–0.62	0.865
Median (primary specification) ^a	17	–2.91	0.420
60th percentile	14	–5.16	0.130
Upper tertile	12	–6.79	0.045 ^b
Upper quartile	9	–6.31	0.088

DiD, difference-in-differences; pp, percentage points. All estimates from the unadjusted specification with standard errors clustered at state level; 35 clusters throughout. ^a Primary analysis, reported in Table 3, specification (1). ^b The only nominally significant estimate among the five thresholds tested. See the accompanying text: with five thresholds examined, one result at $p < 0.05$ is close to chance expectation, the sign is opposite to the hypothesized direction, and the estimate is not supported by the primary specification, the continuous-exposure model, permutation inference, or any leave-one-out refit.

We do not read that as evidence of an effect, for three reasons. Testing five thresholds and finding one nominally significant result is close to what chance alone delivers. The sign runs counter to the hypothesis that better-connected states benefit, so, if taken at face value, it would imply that connectivity harms coverage. And it is unsupported by the pre-specified median split, by the continuous specification, by permutation inference, or by any of the thirty-five leave-one-out fits. We report it because concealing it would misrepresent what we did.

What could the analysis have detected?

A null is only as informative as the study’s power, so we quantified it. The standard error of the difference-in-differences estimate is 3.61 percentage points, which puts the minimum detectable effect

at 80% power and conventional significance at 10.1 points. Equivalence testing places the estimate within ± 10 points ($p = 0.029$) and within ± 15 points ($p = 0.001$), but not within ± 5 points ($p = 0.283$).

The precise claim the data support is therefore this: effects larger than roughly ten percentage points are excluded; effects of five to ten points are not. We are not in a position to say that connectivity does nothing, only that nothing large is happening at this level of aggregation.

Other maternal outcomes

The null is not peculiar to ANC4 (Table 5). First-trimester antenatal care returns -4.32 points ($p = 0.181$) and institutional birth -3.25 points ($p = 0.112$). All three-point estimates are negative, and none approaches significance.

Table 5 Difference-in-differences estimates across three maternal health outcomes.				
Outcome	DiD estimate (pp)	SE	p	95% CI
ANC4	-2.91	3.61	0.420	-9.98 to 4.16
Antenatal care, first trimester	-4.32	3.23	0.181	-10.65 to 2.01
Institutional birth	-3.25	2.04	0.112	-7.25 to 0.76

ANC4, four or more antenatal care visits; CI, confidence interval; DiD, difference-in-differences; SE, standard error; pp, percentage points. All models unadjusted, with standard errors clustered at state level; $n = 70$, 35 clusters. Outcomes are defined for the most recent birth in the five years preceding each survey, except for institutional births, which cover all births in that period.

Matching, and why it failed?

We attempted propensity-score-matched difference-in-differences, matching states on baseline literacy and schooling. It did not work. The procedure retained only six states and failed to achieve balance, with standardized mean differences of $+0.500$ for literacy and -0.674 for schooling, both far outside the conventional ± 0.1 threshold. We therefore report the matched estimate of -3.58 points ($p = 0.618$) as uninformative rather than presenting it as corroboration. With 35 units and baseline gaps as large as those in Table 2, there is simply not enough common support for matching to work. That is a limitation of the data, not a finding about connectivity.

The placebo outcome behaved as expected under no effect, with the sex ratio returning $+6.37$ ($p = 0.576$; $R^2=0.072$). We note that a placebo passing carries limited weight when the primary estimate is itself null.

Connectivity growth and coverage growth are unrelated

Regressing the change in ANC4 on the change in mobile ownership across states gives a slope of -0.29 points per point (95% CI -1.01 to 0.44 ; $p = 0.427$), with an R^2 of 0.02 . Figure 2 shows the scatter; there is no relationship in it.

The scatter says one specific thing: **across Indian states, how fast connectivity grew tells you nothing about how fast antenatal coverage grew.** Uttarakhand gained 5 points of mobile ownership and 31 of coverage. Jammu and Kashmir gained 21 points of connectivity and lost coverage. The cloud has no tilt worth speaking of, and $R^2=0.02$ means connectivity growth accounts for about 2% of the variation in coverage growth.

That is a claim about *states*, not about women — and the distance between those two things is where most of the interpretive work lies.

What does it not claim?

This does not prove that a phone is useless for an individual woman. Inferring so would be an ecological fallacy. The mechanism everyone considers operates at the individual level: this woman owns her phone, receives her reminder, and attends her appointment. The state's average ownership rate is a very crude summary of this. Two states with 60% ownership could differ greatly in who owns the phones — whether they are held by poorer rural women facing significant access barriers or by urban women already attending antenatal care. State averages do not account for these differences, and it is precisely these distinctions that determine whether connectivity actually improves coverage.

Add the power limits and the honest statement narrows further: no *large* effect is detectable *at this level of aggregation*.

Four implications worth drawing

Aggregate co-movement should stop being cited as evidence. Connectivity and coverage both rose in India, and so did electrification, insurance enrolment, institutional delivery and female literacy. Anything that rises over that period correlates with anything else that rises. The moment you ask connectivity to explain *differences between* states rather than the national trend, the association evaporates. That is diagnostic — a real effect would generally survive the move from levels to changes.

Adoption metrics are not outcome metrics. Digital health programs are commonly reported on in terms of registrations, downloads, and network coverage. Your figure is a direct demonstration that those numbers can move a great deal while the health outcome they are meant to serve moves independently, or backward. That argues for evaluating digital investment against service outcomes rather than against uptake.

Connectivity is plausibly necessary without being sufficient. This is where your original framing partly survives. If a phone only produces care when there is a registry to be enrolled in, a health worker reachable through it, and a record that follows the pregnancy, then handing out connectivity into a system lacking those things should produce exactly what you found: nothing measurable. The null is consistent with the infrastructure argument — it just relocates the burden onto institutional integration rather than device ownership.

The negative slope is not a warning. The sign is negative but the interval spans zero, and the likely reason is the convergence dynamic you documented: states with the highest baseline coverage had the least room to gain and were somewhat more connected to begin with. Read it as a shadow of the catch-up pattern, not as evidence that phones harm anyone.

Where does this point?

The finding makes the individual-level study more valuable, not less. We now have a documented, well-characterized null at the aggregate level — which is precisely the setup that justifies going to the NFHS-5 microdata to ask whether the effect exists at the level where the mechanism actually operates. If it shows up there and not here, that is a genuinely interesting result about aggregation. If it shows up in neither, that is a serious challenge to a widely held assumption.

Each point is one state or union territory. The fitted line has a slope of -0.29 (95% CI: -1.01 to 0.44), $p = 0.427$, $R^2=0.02$. The dashed horizontal line marks no change.

Twelve states went backward

Twenty-three states gained ANC4 coverage between the rounds and twelve lost it (Table 6, Fig. 3). Among the twelve the mean fall was 6.5 percentage points, against a mean rise of 10.2 points among the twenty-three. The declines are not trivial in size: five states lost more than eight points, and Sikkim lost 16.3.

Table 6
States and union territories record declining ANC4 coverage, NFHS-4 to NFHS-5.

State or union territory	ANC4, NFHS-4 (%)	ANC4, NFHS-5 (%)	Change (pp)	Mobile ownership change (pp)
Sikkim	74.7	58.4	-16.3	+ 8.8
Kerala	90.2	78.6	-11.6	+ 5.4
Tripura	64.3	52.7	-11.6	+ 9.2
Punjab	68.5	59.3	-9.2	+ 4.0
Andhra Pradesh	76.3	67.5	-8.8	+ 12.7
Andaman and Nicobar Islands	92.1	83.4	-8.7	+ 13.9
Telangana	75.0	70.4	-4.6	+ 12.2
Mizoram	61.7	58.0	-3.7	+ 5.0
Maharashtra	72.2	70.3	-1.9	+ 9.2
Puducherry	87.7	86.9	-0.8	+ 15.6
West Bengal	76.5	75.8	-0.7	+ 8.2
Jammu and Kashmir	81.4	80.9	-0.5	+ 21.0

ANC4, four or more antenatal care visits; pp, percentage points. Ordered by size of decline. Mobile ownership refers to women aged 15–49 who own a mobile phone that they themselves use.

Part of this is a ceiling effect, but not all of it

The single characteristic separating declining from rising states is where they began. Declining states averaged 76.7% baseline coverage against 52.8% among those that rose, a gap of 23.9 points ($p < 0.001$). Of the ten states entering the period above 75% coverage, seven subsequently declined; of the twenty-five below, five did. The odds of decline were roughly nine times higher among states already near the top.

Some of this is arithmetic. A state at 92% has four points of headroom and considerable room to slip, so a ceiling effect is expected and its presence is not by itself surprising.

But it does not account for the whole pattern. Five of the twelve declining states began below 75% coverage, and three of those were nowhere near a ceiling: Mizoram at 61.7%, Tripura at 64.3% and Punjab at 68.5%. Tripura losing 11.6 points from a base of 64.3% cannot be explained by having run out of room. Neither can Sikkim, which fell 16.3 points from 74.7% — the largest decline in the panel, from a state that was mid-table rather than saturated.

Connectivity does not distinguish them either

Declining and rising states were statistically indistinguishable on both baseline mobile ownership (58.3% versus 53.6%; $p = 0.407$) and growth in mobile ownership over the period (+ 10.4 versus + 8.9 points; $p = 0.406$). If anything, the declining states gained slightly more connectivity.

The individual cases make the point more sharply than the averages. Jammu and Kashmir recorded the largest increase in women's mobile ownership anywhere in the panel, 21.0 points, and lost ground on ANC4. Puducherry gained 15.6 points of connectivity and lost coverage. Andaman and Nicobar Islands gained 13.9 and lost 8.7. This is the study's central finding restated at the level of individual states: connectivity rose almost everywhere, including in the places where maternal service coverage was falling.

Regional concentration

The declines cluster geographically. Four of the six southern states and union territories in the panel declined, with a mean change of -2.7 points; three of eight north-eastern states declined, with a mean change of $+0.1$; among the remaining twenty-one states, the figures are five and $+8.2$. The south is the outlier, and it is the region conventionally regarded as having India's strongest maternal health systems.

Each row is one state or union territory, ordered from highest to lowest NFHS-4 coverage. Grey markers show NFHS-4 coverage and colored markers NFHS-5; the connecting segment spans the change, which is printed at the outer end of each row. Blue denotes an increase (23 states) and red a decrease (12 states). Declines concentrate among states that began with high coverage, but not exclusively: Tripura, Mizoram and Punjab all fell from below 70%. ANC4, four or more antenatal care visits; pp, percentage points.

What are the permissible and impermissible things to say about why?

We can identify this pattern, but we cannot explain it, and it is important to distinguish between the two.

Three explanations seem worth investigating. The first is survey operations. NFHS-5 fieldwork was conducted in two phases, the first largely completed before the COVID-19 pandemic and the second carried out during it. If the declining states are concentrated in the later phase, some of the fall may reflect disrupted fieldwork or disrupted services during data collection rather than a durable change in coverage. We have not tested this, because phase assignment is not recorded in the fact sheets we used; it is the first thing a follow-up should check.

The second is genuine service disruption. Routine antenatal care was interrupted across India during the pandemic, and states differed in the extent to which outreach was suspended and how quickly it resumed.

The third is measurement. Antenatal visit counts on maternal recall, and any change in how the question was administered or understood between rounds would move the indicator without any change in

underlying care.

None of these can be separated with two rounds of aggregate data. What can be said is that a third of India's states recorded lower antenatal coverage at the end of a period in which national coverage rose by 4.5 points, that this includes states normally held up as exemplars, and that the national mean conceals it completely.

Discussion

What we found

Maternal care coverage in India converged between 2015 and 2021, and where a state started explains that convergence far better than how connected its women became. Beta-convergence is strong and holds up; connectivity contributes 0.008 to the explained variance of the same model. Every specification we ran to test for a digital effect returned a null, and the estimates were unstable in sign.

We think the period is best understood as catch-up. States with low baseline coverage had room to gain and were the target of national maternal health programming; states near the ceiling had little room, and a dozen of them lost ground. Connectivity, meanwhile, rose almost everywhere, which makes it a poor candidate for explaining why states differed from one another.

Why does the aggregate correlation mislead?

The simultaneous increase in connectivity and coverage in national data is real but not insightful, as both improved during this period. Similarly, household electrification, insurance coverage, institutional delivery, and female literacy all rose. A two-period comparison at the state level cannot distinguish these trends, and our analysis shows that the correlation evident in national figures vanishes when examining differences between individual states.

It is crucial to clearly state the main point. Analyses estimating the total population impact based on co-movement or cross-sectional links between connectivity and service use likely reflect broader developmental effects rather than connectivity alone [6]. For instance, Table 2 indicates that better-connected states have female literacy rates more than two standard deviations higher than others. Consequently, any cross-sectional comparison between these groups essentially reflects this difference, regardless of the specific aspect they aim to measure.

The declines are the more urgent finding

The fact that twelve Indian states experienced a drop in antenatal coverage during this period seems insufficiently addressed. One reason could be the fieldwork conditions: NFHS-5 data collection coincided with the COVID-19 pandemic, which disrupted regular antenatal services nationwide and posed challenges for survey efforts. However, these declines are not limited to states with weaker healthcare

systems, and Kerala's situation particularly warrants closer examination as it is completely absent from the national statistics.

Limitations

The central limitation is imprecision, and we would rather be explicit about it than let a reader infer more than the data support. The minimum detectable effect is 10.1 percentage points. Equivalence testing rules out effects beyond ± 10 points but not beyond ± 5 . We can therefore say that nothing large is detectable at this level of aggregation. We cannot say that nothing is happening.

Four further limitations bear on how this should be read. State aggregates conceal variation within states, and within-state variation is where any connectivity effect would plausibly operate — the mechanism runs through a woman's own phone, not her state's average. Two time points cannot support a parallel-trends test, so the identifying assumption is untestable here; we have not tested it and do not claim to have. NFHS-5 fieldwork overlapped the pandemic, which affected both service delivery and data collection in ways we cannot separate from the trends we observe. And mobile ownership is a crude stand-in for what actually matters: digital literacy, autonomy in using a device, and access to content in a language a woman reads. Fact sheets measure none of these.

Where should this go next?

The question deserves individual-level data, and that data exists. NFHS-5 microdata records mobile phone ownership and antenatal care for each respondent, along with wealth quintile, education, caste, birth order, and place of residence, across roughly 700,000 women. A study built on it would be adequately powered, could examine equity gradients within states rather than only between them, and could distinguish a woman's own connectivity from her state's average — which is the distinction that matters for the mechanism. We regard the present analysis as showing that the aggregate approach cannot answer the question, not as having answered it.

Conclusions

Antenatal coverage in India converged between 2015 and 2021, driven more by catch-up among states that had started furthest behind than by the expansion of women's digital access that occurred alongside it. We find no association between connectivity growth and coverage growth in any specification, at any threshold, or for any of three maternal outcomes, with analytic, permutation, and bootstrap inference all agreeing. The concurrent decline in twelve states, several of them national exemplars, is a more pressing finding than the null and is entirely hidden by national averages.

Abbreviations

ANC4

four or more antenatal care visits

CI

confidence interval
DiD
difference-in-differences
NFHS
National Family Health Survey
pp
percentage points
SD
standard deviation
SE
standard error
SMD
standardized mean difference
TOST
two one-sided tests
UT
union territory.

Declarations

Ethics approval and consent to participate

Not applicable. This study analysed publicly available aggregate data from published survey fact sheets. No human participants were involved and no individual-level records were accessed, so ethics approval and informed consent were not required.

Not applicable.

Consent for publication

Not applicable.

Competing interests

The authors declare that they have no competing interests.

Funding

The authors received no specific funding for this work. No funding body had any role in the design of the study, in the collection, analysis or interpretation of data, or in writing the manuscript.

Author Contribution

A.D.G. conceived the study, curated the data, performed the statistical analysis, produced the figures and wrote the first draft. Y.Y. contributed to the methodology, validated the analysis and revised the manuscript.1-2. Both authors read and approved the final manuscript.

Acknowledgement

We thank Anusha Das Gupta for assistance with figure preparation.

Data Availability

All data underlying this study are publicly available. The NFHS-4 and NFHS-5 state fact sheets are published by the International Institute for Population Sciences and can be obtained from <http://www.nfhsiips.in/nfhsnew/nfhsuser/publication.php>

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Figures

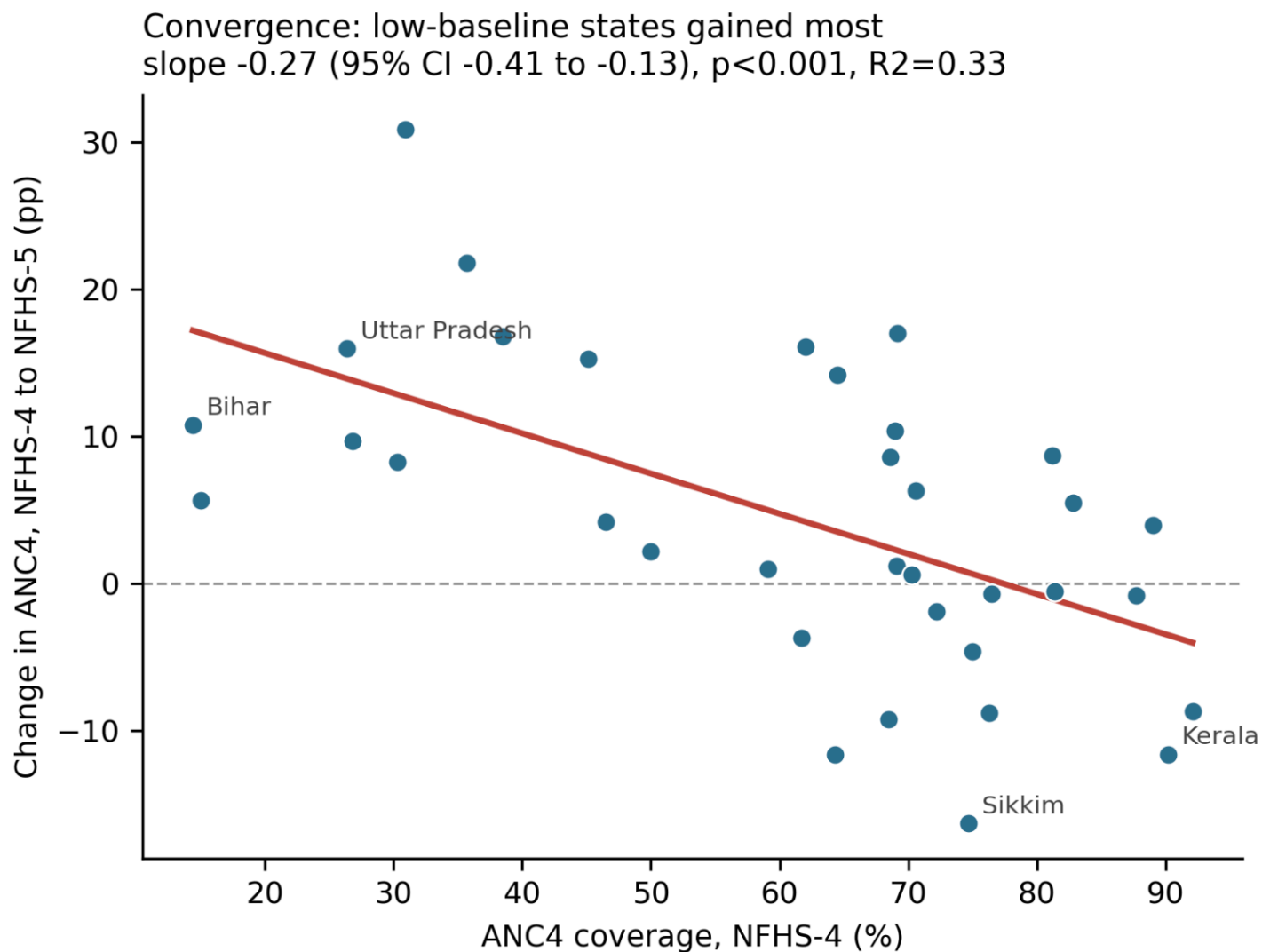


Figure 1

Change in ANC4 coverage against baseline ANC4 coverage across 35 states and union territories.

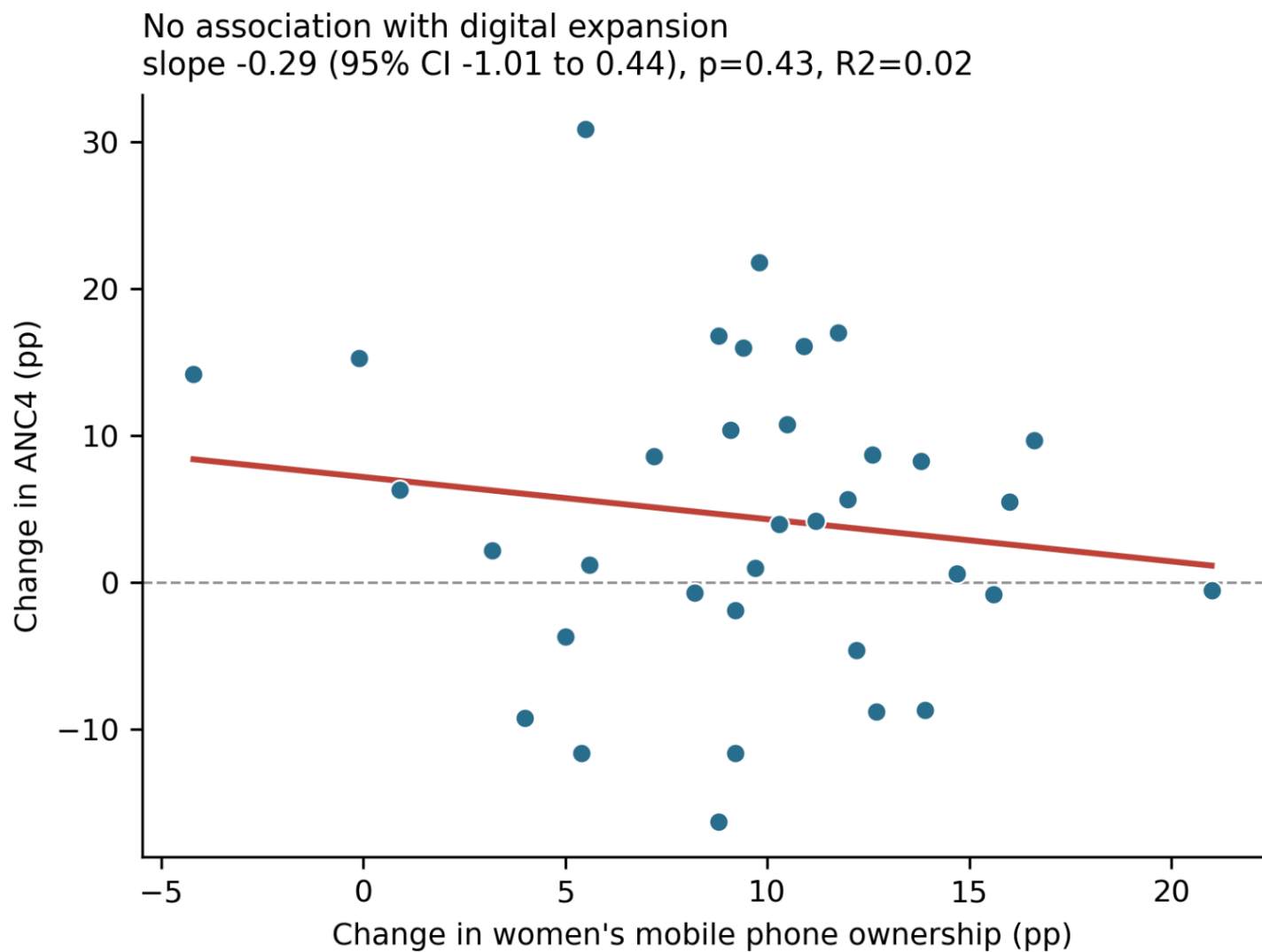


Figure 2

Change in ANC4 coverage against change in women's mobile phone ownership.

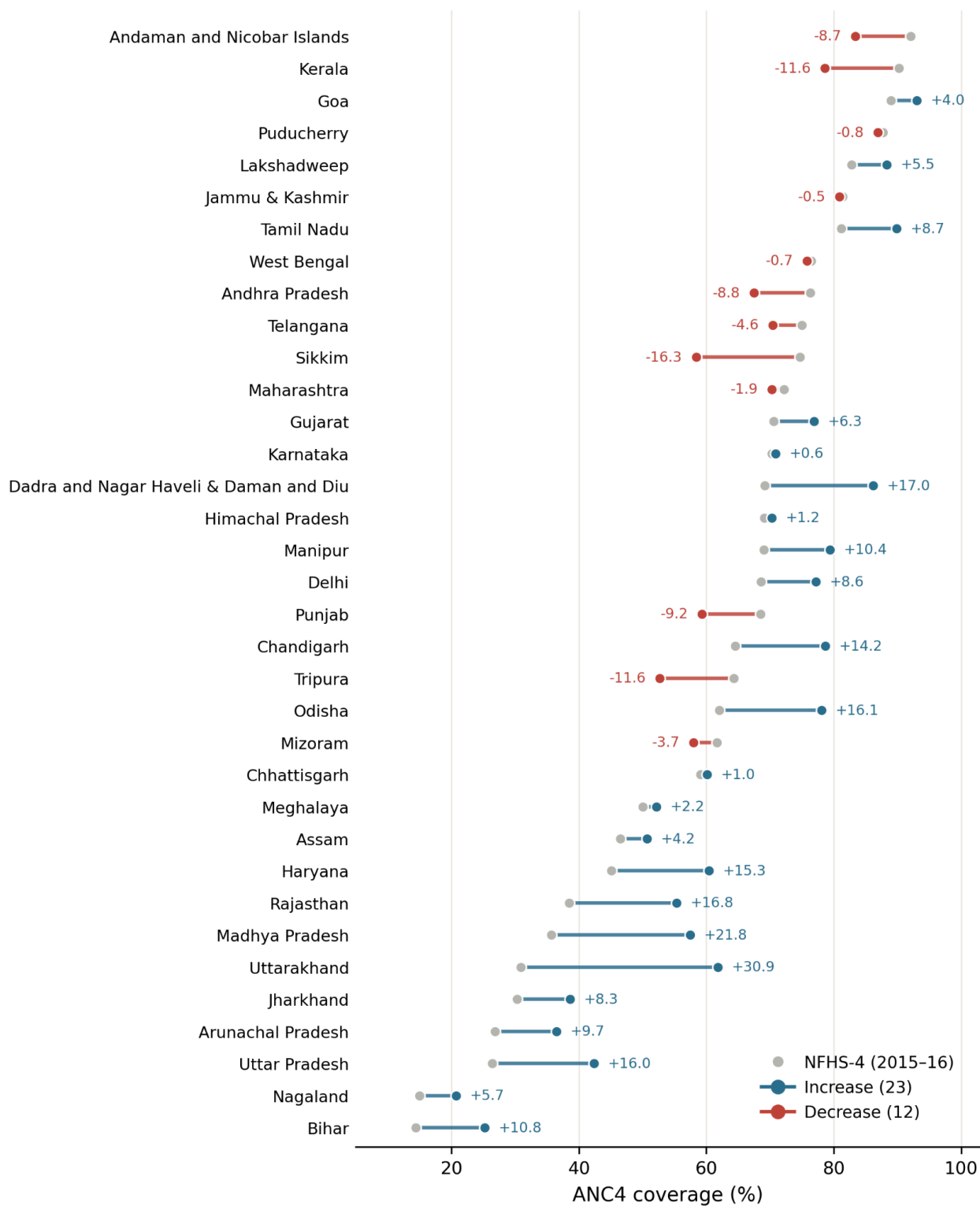


Figure 3

ANC4 coverage by state at NFHS-4 and NFHS-5, ordered by baseline coverage.