The Mental Health Benefits of Social Media Deactivation under Collective Participation

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We study the mental health effects of social media deactivation in a pre-registered randomized experiment among a nearly nationally representative sample of Indonesian adults ($N = 1,502$), comparing (1) a status quo control group to (2) individual and (3) household deactivation of five social media platforms. Deactivation improved subjective well-being and mental health, on average, with effects concentrated among those assigned to household deactivation. Additional analyses show effects depend on users' offline household networks: deactivation benefits users with infrequent interaction within households but reverses for those with tighter bonds, helping explain disagreement over social media's harms.

1 Introduction

A growing chorus of policymakers and scholars alike increasingly attribute a global mental health crisis to the rise of social media platforms (*1, 2*). These claims invoke diverse mechanisms: the idea that social media amplifies outrage (*3*), undermines genuine community (*4, 5*), and promotes corrosive social comparison (*6, 7*). Consistent with this, observational evidence has shown that social media usage correlates with declines in mental health (*8–14*). To probe the credibility of these claims, scholars have increasingly turned to randomized experiments that incentivize individuals to temporarily deactivate from social media. While these studies generally find improvements in subjective well-being, the estimated effects are modest in magnitude and often short-lived (*15–22*).

We hypothesize that existing experimental estimates may represent the lower bound on the harms of social media. One reason is that most experiments assign deactivation at the individual level, even though many of social media's effects are likely mediated through social networks. Treated individuals may continue to encounter information, norms, or emotional spillovers originating online through offline interactions with peers who remain active, thereby attenuating estimated treatment effects (*23, 24*). Individual-level deactivation may also impose costs of exclusion or disconnection, partially offsetting any mental health benefits (*25*). A second challenge is substitution: participants who deactivate from one platform often reallocate time to other digital environments with similar psychological affordances, making it difficult to estimate the effect of more comprehensive disengagement (*15–17*). Finally, existing studies often rely on samples of participants willing to temporarily leave social media. If the individuals most adversely affected by social media are also least likely to enroll in deactivation experiments, estimated effects may be biased toward zero (*16*).

We investigate these claims by conducting a three-arm cluster randomized deactivation experiment among a nearly nationally representative population of 1,502 adults in Indonesia. In addition to assigning participants to (1) an incentivized status quo control ($N = 402$) and (2) an incentivized individual-level four-week deactivation ($N = 399$), we also recruited participants to take part in (3) a household *cluster deactivation* in which all consenting adults in the participant's household were eligible to also obtain a large financial incentive if they chose to partake in the four-week deactivation of their social media ($N = 701$). (In the event, 32.5% of households in this condition had at least one additional household member participate.) Our intervention targeted all five major social media platforms in Indonesia (Facebook, Instagram, TikTok, Twitter/X, YouTube) and compliance was externally verified by a tailor-made monitoring application.

2 Experimental Design

We expand on the experimental design in the Supplementary Materials (SM), but several features warrant comment at the outset. First, to maximize external validity, we recruited participants face-to-face via a random-walk procedure. Moreover, we offered unusually large financial incentives (randomly delivered 400,000, 500,000, or 600,000 IDR; $\approx$10-15% of median monthly income), which enabled us to obtain a study population that approximates the broader population of social media users in Indonesia—rather than simply those *ex ante* interested in a social media hiatus. In Table S4, we show the representativeness of our study population favorably benchmarked against the both the broader Indonesian population and social media users.

Second, to manage concerns over time-use substitution, we deactivated five major social media platforms (Instagram, Facebook, TikTok, Twitter/X, and YouTube). To block the selected platforms, enumerators assisted participants in installing the StayFree application on all of their internet-connected devices (tablets and phones). The StayFree app allows users to select certain applications on the device (tablet or phone) for indefinite deactivation, as well as to block the associated websites (i.e., blocking the Facebook application as well as the ability to access www.facebook.com via browser). To verify compliance, participants installed an originally crafted application ("SosMedMonitoring") that automatically monitored the participants' devices' activity. SosMedMonitoring was only available for Android users, who comprise 90% of users in Indonesia and 95.1% of the users in our sample. For iOS users in our sample, deactivation was verified daily by sending screenshots of their "screen time" page to research assistants.

We report both intent-to-treat (ITT) and local average treatment effect (LATE) estimates. The household arm should be interpreted as the effect of *assignment to a household-level deactivation offer*, in which the index participant and other eligible adult household members were invited and incentivized to deactivate. Because only 32.5% of households in this arm had at least one additional member accept the offer, the ITT does not estimate universal household deactivation. Instead, it captures the effect of a household-level offer under imperfect take-up.

3 Results

3.1 Main Outcomes

Both individual- and household-level deactivation were highly effective in reducing participants' time on social media. For the purposes of estimation, the benchmark for compliance was pre-registered and defined as having spent on average less than 5 minutes per day on any platform. Panel (A) of Figure 1 shows that the individual- and household interventions increased the share of respondents satisfying this condition by 43.6pp (95% CI: 0.372, 0.502; $P < 0.001$) and 42.9pp (95% CI: 0.372, 0.486; $P < 0.001$), respectively, above an absolute baseline of 17.5% in the control condition. Panel (B) of Figure 1 shows that compliance occurred immediately on installation of the blocking and tracking applications without decay over the course of the intervention. Uptake returned to pre-intervention levels upon completion of the endline survey. Figure S1 shows the app-wise compliance mirroring the same pattern.

The main effects of the interventions are presented in Figure 2, benchmarked against a status quo control. (In Figure S2 we estimate the effect of the household deactivation treatment arm benchmarked against the individual deactivation treatment.) The survey items comprising each of the indices can be found in the Supplementary Materials. We do not adjust for attrition in the endline survey as it was minimal and not significantly different between experimental conditions (recontact rate: 95.0%, 91.7% and 94.2% for C, T1, T2, respectively). In line with the pre-analysis plan, we estimate both an Intent-to-Treat (ITT) analysis and attempt to recover the Local Average Treatment Effect (LATE) using two stage least squares (2SLS) with treatment assignment instrumenting for the compliance indicator presented above in Panel (A) of Figure 1. All models control for the baseline outcomes and also adjust for a pre-registered vector of covariates. We adjust for multiple hypothesis testing in Table S7.

On average, social media deactivation improved several dimensions of subjective well-being, with effects larger for household-level deactivation than for individual deactivation (See Fig. 2). It is worth underscoring that non-index participants' compliance with deactivation was low (32.5%, see Figure S3), biasing our estimates towards zero. Nonetheless, for our main pre-registered index capturing subjective well-being, household deactivation increased the composite index by 0.072 SDs under the intention-to-treat (ITT) estimand (95% CI: 0.0058 0.138; $P = 0.033$), while the individual deactivation effect was smaller and not statistically distinguishable from zero. Instrumental-variable estimates indicate larger effects among compliers: household deactivation increased well-being by 0.185 SDs (95% CI: 0.032, 0.338; P = 0.018), consistent with incomplete compliance.

Disaggregating the subjective well-being index reveals that these gains are driven by improvements in (C) anxiety (D) depression symptoms as well as (E) sleep quality. The largest effects in absolute terms relate to improvements in subjectively assessed sleep quality. Both individual and household deactivation substantially increased self-reported sleep quality relative to the status quo control group. Under ITT, individual deactivation increased sleep quality by 0.173 SDs (95% CI: 0.042, 0.303; P < 0.01), and household deactivation increased sleep quality by 0.169 SDs (95% CI: 0.056, 0.282; P < 0.01). These effects approximately double in magnitude under the LATE estimand, reaching 0.377 SDs for individual deactivation (95% CI: 0.0673, 0.687; P = 0.017) and 0.367 SDs for household deactivation (95% CI: 0.099, 0.634; P < 0.01). Note that the magnitude of the effects do not vary according to whether the participant was assigned to individual or household deactivation.

We also detect improvements in anxiety and depression, the scales for which we reversed such that higher values denote declines in symptoms, with these effects being more concentrated among participants assigned to the household treatment arm. Household deactivation reduced anxiety symptoms by 0.121 SDs under ITT (95% CI: 0.0099, 0.232; P = 0.033) and by 0.279 SDs among compliers (95% CI: 0.0204, 0.538; P = 0.0345). Effects on depressive symptoms are directionally similar. Although only marginally significant, the household deactivation arm improved participants' depression symptoms by 0.098 SDs (95% CI: -0.00531, 0.202; P = 0.063). The estimate for compliers was larger, yielding a 0.25 SD improvement in the depression index (95% CI: 0.0169, 0.507; P = 0.0362). Although directionally consistent with the estimates from the household arm, we detect no improvement in anxiety or depressive symptoms for participants assigned to the individual deactivation arm. Further, we find no evidence that deactivation affects positive–negative affect on average.

3.2 Heterogeneous Effects by Household Social Density

We also examine whether the mental health effects of social media deactivation vary systematically according to participants' baseline household social environment. Specifically, on the baseline survey, we asked respondents to list the seven individuals with whom they interact with most frequently on a weekly basis and the amount of time they interact with them—and to identify these individuals by their role (e.g., coworker, friend, neighbor, household member). We leverage this information to characterize the extent to which the composition of respondents' offline social networks is dominated by within-household connections. We interpret this variable as an approximation of the density of within-household social ties. We pre-registered this analysis, although it is worth underscoring that our *ex ante* hypothesis anticipated a positive rather than the observed negative slope.

In Figure 3, we present the marginal effects of social media deactivation on our subjective well-being index and composite sub-indices according to the density of respondents' within-household social networks. Except for our self-reported sleep quality measure, the treatment effect of deactivation are largest among individuals with less household interaction and attenuate as household closeness increases. This pattern is most pronounced for household-level deactivation. For overall well-being (Fig. 3, A), household deactivation increases well-being by 0.163 SDs among respondents with no meaningful household interaction (95% CI: 0.0755, 0.250; $P < 0.01$), but the effect declines monotonically with household closeness, crossing zero at approximately fifty-percent interaction being within-household and becoming negative for respondents embedded in socially dense households. In extremely socially dense households in which index respondents report all of their meaningful social interaction occurs within the household the effect of household deactivation is -0.189 SDs (95% CI: -0.381, 0.00203; $P = 0.053$). By contrast, the individual deactivation arm exhibits smaller and statistically indistinguishable effects across the distribution of household ties.

Similar gradients emerge for the sub-indices on which we observe the strongest effects of the intervention, namely, anxiety (C) and depression (D). For these outcomes, household deactivation yields sizable improvements in our outcomes among respondents with few household ties. For example, comparing index participants assigned to the household deactivation against the status quo control, we detect a 0.25 SD reduction in anxiety when the index participant reports zero close household interaction (95% CI: 0.0984, 0.405; P < 0.01). But these benefits reverse as household closeness increases. The same outcome pattern can be observed more starkly for our depression index: positive improvements from household deactivation in socially sparse households (95% CI: 0.0795, 0.355; P < 0.01) and a worsening of symptoms in socially dense households (95% CI: -0.513, 0.0329; P = 0.085). Individual deactivation again shows weaker and less systematic heterogeneity.

Sleep quality presents a notable contrast (E). Improvements in sleep are positive and stable across levels of household closeness for both individual and household deactivation, with little evidence of reversal even among respondents with many close household ties. The mechanisms underpinning the gains in subjective assessments of sleep quality—such as reduced nighttime engagement—operate independently of household social structure.

4 Discussion

This study provides experimental evidence that incentivized social media deactivation can improve some dimensions of subjective well-being among adults, but that these effects are neither uniform nor independent of offline social context. In a nearly nationally representative sample of Indonesian adult social media users, assignment to a household-level deactivation offer improved a pre-registered well-being index, with especially consistent gains in sleep quality and suggestive improvements in anxiety and depressive symptoms. However, these average effects mask substantial heterogeneity. Benefits were concentrated among respondents with relatively sparse within-household social interaction, while effects attenuated and sometimes reversed among respondents whose social interaction was concentrated within the household. These patterns suggest that social media may serve different psychological and social functions depending on users' offline environments.

Several methodological features of our study merit further discussion, particularly in light of the magnitude of the effects we uncovered. First, our study was conducted independently of social media platforms (*26, 27*). This design choice allowed us to block access to all major social media sites, limiting treated participants' ability to substitute time from one platform to another. Second, we recruited a nearly nationally representative sample of users through a random sampling procedure in which participants were approached at their doorstep. We also offered unusually large financial incentives. Together, these design choices were intended to improve the external validity of our estimates. Finally, we recruited a large sample ($N = 1,502$), giving the study sufficient power to detect relatively small effects. For reference, a recent meta-analysis of 32 deactivation studies (*18*) found a median sample size of 124 participants, which is typically powered only to detect treatment effects larger than 0.51 SDs—roughly two to three times larger than the effects we detect here—thus providing a possible explanation for the variability in estimates uncovered in other studies (*21*).

Finally, third, we introduced a third treatment arm in which we incentivized household-level social media deactivation. The household-cluster design moved treatment assignment closer to the social unit at which spillovers may plausibly operate. The larger well-being gains under household-level deactivation are consistent with the idea that social media partly shapes the household information atmosphere. A growing empirical literature emphasizes that social media can affect behavior and emotions via contagion-like processes (*28, 29*). By reducing both direct use and secondhand exposure within the household, clustered disengagement may better capture the total effect of social media "presence" than individual-only interventions (*15, 30, 31*). In this sense, social media resembles other networked environments in which effects propagate through interpersonal pathways and shared contexts, rather than being fully reducible to individual consumption (*28, 29*).

Our additional analyses point towards the idea that social media may operate as a psychological substitute for offline social interaction in socially sparse environments, with online communication further isolating individuals from engaging with their immediate environment in ways that can undermine mental health. But for individuals with already-rich offline social lives, in socially dense households, social media may serve as a coordinating device that enables and complements activities the bring joy and reduce anxiety. We believe broader pattern of results supports this interpretation. Our most behaviorally atomized outcome concerns respondents' reported quality of sleep, which improves substantially in both deactivation arms, and which evinces stable gains across levels of household social density (*32–35*). In contrast, our more socially contingent outcomes—anxiety and depression—are evidently conditional on household structure. The strongest improvements are concentrated among participants with fewer close household ties. This echoes prior work emphasizing that problematic social media use, rather than time per se, is more reliably linked to poor mental health (*2, 10–12, 36, 37*). Our results extend this literature by showing that the social setting of disengagement itself moderates psychological effects: in socially sparse households, social media may operate as a harmful substitute to healthy behavior; in socially dense households, it may work as a complement.

There are at least several limitations that merit discussion. First, the intervention was time-limited; we cannot infer persistence beyond the four-week window, and effects may attenuate as users adapt or substitute toward other digital activities (*15, 30*). Second, while our sample was designed for broad external validity within Indonesia, institutional and cultural differences in household structure and platform use may moderate generalizability. Indeed, one of the key implications of our findings is that the effects of social media deactivation are circumscribed by users' broader offline context. Third, partial uptake in the household arm implies that the ITT understates the effect of full collective disengagement; the IV estimates partially address this but remain local to compliers.

Despite these caveats, the results have direct implications for research and policy. For researchers, they motivate shifting from individual-only interventions to designs that explicitly model social spillovers and ambient exposure in networked media environments (*28,29*). For policymakers and practitioners, the heterogeneous treatment effects we uncover caution against "one-size-fits-all" digital detox guidance. Disengagement may yield the largest mental health gains for individuals whose offline social capital is limited, but may impose costs where social media complements dense in-person networks (*2, 12, 36*). Understanding social media as something embedded in an offline context helps reconcile why prior individual deactivation experiments often find modest average effects while public concern remains high, and it clarifies when collective disengagement is most likely to improve mental health (*15, 17, 30, 31*).

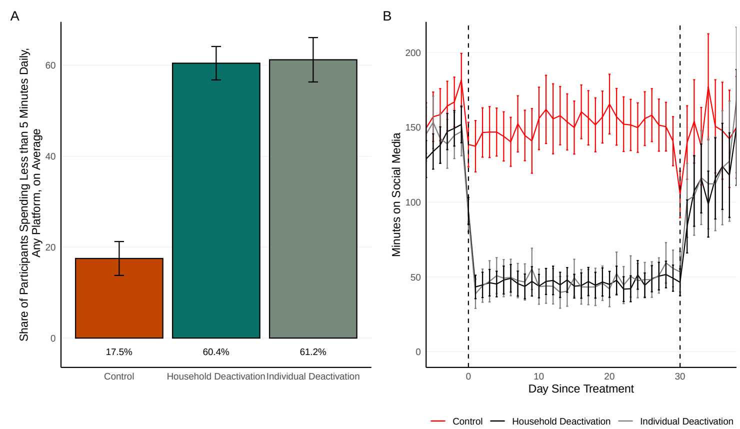

Figure 1: **Social media deactivation substantially reduced time spent on platforms in both treatment arms.** (A) Share of participants who spent less than five minutes per day on any social media platform, averaged over the treatment period, by experimental condition. Both individual and household deactivation sharply increased compliance relative to the control group; bars show means and error bars indicate 95% confidence intervals. (B) Daily minutes spent on social media by days since treatment assignment. Solid lines denote group means and vertical error bars indicate 95% confidence intervals. Dashed vertical lines mark the start and end of the four-week deactivation period. Participants assigned to either deactivation condition exhibit an immediate and sustained reduction in social media use during the treatment window, with usage rebounding after incentives ended.

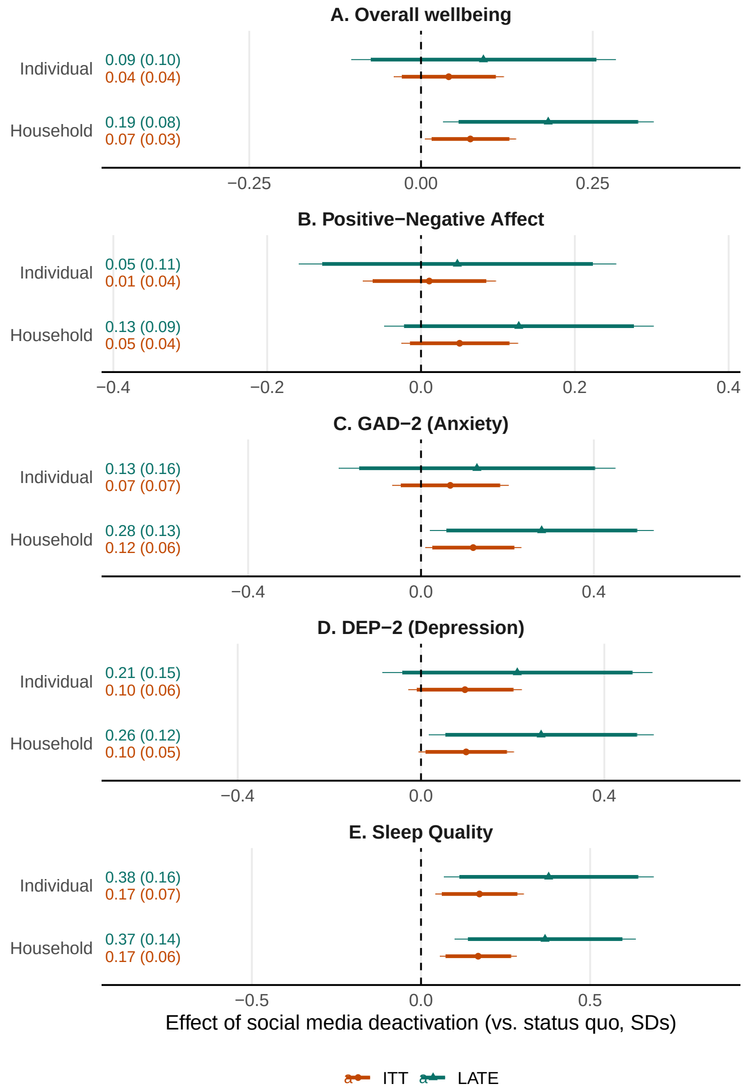

Figure 2: **Average effects of social media deactivation on mental health outcomes.** Points show estimated treatment effects of individual and household social media deactivation relative to the status quo, expressed in standard deviation units. Horizontal lines indicate 95% confidence intervals. Orange markers correspond to intention-to-treat (ITT) estimates, and green markers correspond to local average treatment effects (LATE) among compliers; the dashed vertical line denotes zero effect. Panels report outcomes within the well-being family: (A) overall well-being index, (B) positive–negative affect, (C) GAD-2 anxiety index, (D) DEP-2 depression index, and (E) sleep quality. The (C) GAD-2 anxiety index and (D) DEP-2 depression index have scales reversed such that higher values denotes improvements in symptoms.

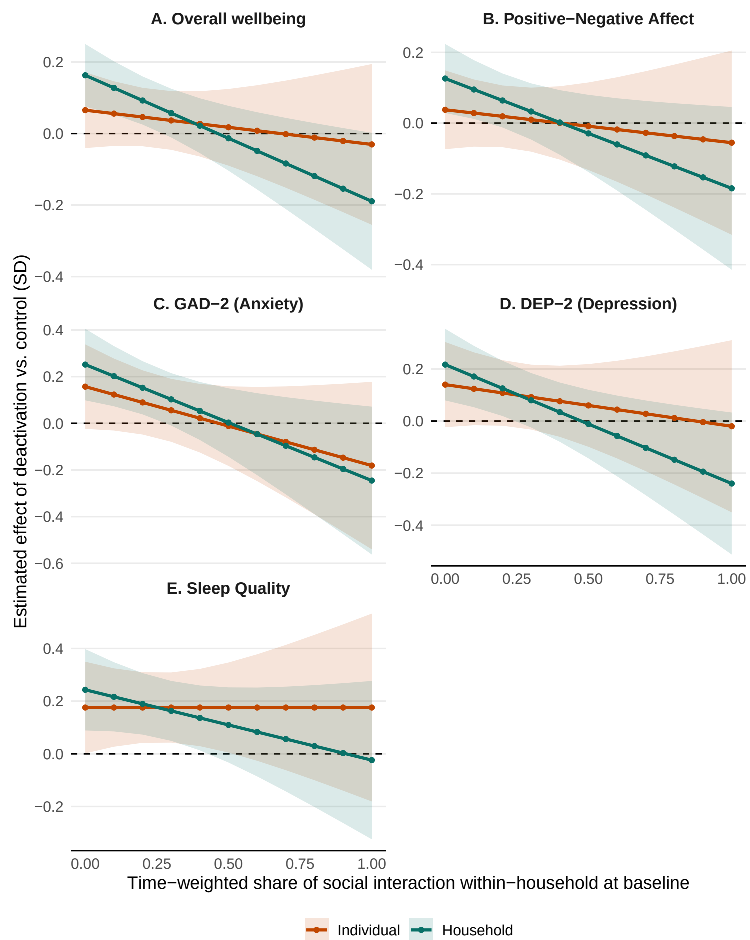

Figure 3: **Household social ties moderate the well-being effects of social media deactivation.** The figure plots estimated marginal effects of individual (T1) and household (T2) deactivation relative to the control condition as a function of respondents' baseline time-weighted share of within-household social interaction. Solid lines show point estimates and shaded bands show 95% confidence intervals; the dashed horizontal line indicates zero effect. Panels report outcomes within the well-being family: (**A**) overall well-being index, (**B**) positive–negative affect index, (**C**) GAD-2 anxiety index, (**D**) DEP-2 depression index, and (**E**) sleep quality index. The (**C**) GAD-2 anxiety index and (**D**) DEP-2 depression index have scales reversed such that higher values denotes improvements in symptoms.

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Competing interests: There are no competing interests to declare.

Data and materials availability: The data and code necessary to replicate all of the analyses contained in the paper can be found on Dryad.

Supplementary materials

Materials and Methods

Figs. S1 to S3

Tables S1 to S7

References *(34-45)*

Supplementary Materials for

The Mental Health Benefits of Social Media Deactivation under Collective Participation

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5 Materials and Methods

5.1 Overview

Testing the claim that social media poses harms to users' mental health is challenging. Social media use is correlated with a range of observable and unobservable characteristics that may independently affect outcomes of interest, complicating causal inference from observational correlations.

To address these challenges, the study employed a three-arm randomized controlled experiment conducted among Indonesian social media users. A total of 1,502 participants were randomly assigned to one of three conditions: (1) a pure control group, (2) an individual-level social media deactivation lasting four weeks, or (3) a household-level social media deactivation lasting four weeks. Assignment followed a 400:400:700 split across the three arms, respectively.[1] All participants received a financial incentive, described below. This section describes the sampling and recruitment procedures, experimental design, piloting and incentive delivery, outcome measurement, estimation strategy, handling of attrition and compliance, and ethical considerations.

5.2 Sampling and Recruitment

The target population consisted of Indonesian adults aged 18 and older who reported daily use of at least one major social media platform (Facebook, Instagram, TikTok, YouTube, or Twitter/X). Inclusion criteria were designed to ensure that participants were plausibly affected by changes in social media exposure. To balance representativeness with logistical feasibility, recruitment was restricted to the thirty most populous provincial capitals in Indonesia, which together account for approximately 32 million residents.

The study employed multistage random sampling stratified by province, with target sample sizes proportional to provincial population shares. Within each selected city, neighborhoods (*rukun tangga*, RT) were randomly sampled. Enumerators then used a kish-grid-assisted random walk procedure to identify households. Upon entering a household, enumerators listed eligible members and randomly selected an "index" participant. The index participant was briefed on the study and screened for eligibility, including confirmation of daily social media use.

[1]The larger allocation to the household-level treatment reflected anticipated one-sided noncompliance due to the higher burden associated with household-wide participation. To account for this, intake was oversampled.

Respondents who used social media primarily for business purposes were excluded.[2] If the selected individual declined participation or failed eligibility screening, enumerators continued the random walk procedure until a replacement was enrolled. Recruitment continued until an index participant was successfully assigned to one of the three experimental conditions.

The final sample comprised 1,500 index participants: 400 assigned to the control group, 400 to the individual deactivation group, and 700 to the household deactivation group. Additional adult household members who participated in the household deactivation arm did not count toward the index sample size. Power calculations indicated that the study was sufficiently powered to detect effect sizes comparable to those reported in prior research, with lower but acceptable power for comparisons between individual- and household-level treatments.

5.3 Experimental Design

Participants were randomly assigned to one of three conditions using randomization without replacement, with blocking at the PSU level to ensure balance across locations. We include the target sample size in each arm, as well as the observed counts accounting for attrition.

- **Control Group ($N = 400$, $N_0 = 402$, $N_1 = 382$):** Participants continued using social media as usual without restrictions. To avoid confounding from incentive receipt, control participants received the same financial incentive structure as treated participants.

- **Individual Deactivation Group ($N = 400$, $N_0 = 399$, $N_1 = 366$):** Participants were offered a fixed financial incentive to deactivate their social media applications (Facebook, Instagram, TikTok, YouTube, and Twitter/X) for a continuous four-week period. Deactivation was implemented using the StayFree application, which restricts access to selected apps while allowing potential access through alternative mechanisms (e.g., web browsers). The intervention therefore reduced, rather than fully blocked, social media access.

  Compliance monitoring differed by operating system. Android users installed an additional monitoring application that automatically recorded usage and transmitted data to a secure

[2] Pilot work indicated that business use was the primary source of noncompliance, motivating its use as an exclusion criterion in the main study.

dashboard. iOS users submitted daily screenshots of their device "screen time" summaries.[3] Implementing partners also conducted daily WhatsApp check-ins to encourage compliance.

- **Household Deactivation Group** (N = 700**,** N_0 = 701**,** N_1 = 660)**:** This condition mirrored the individual deactivation protocol, but index participants were informed that other adult household members could also participate. Each additional household participant received the same financial incentive as the index participant. Compliance was monitored using the same procedures as in the individual deactivation arm. Only index participants were included in the main analytical sample.

5.4 Piloting and Incentive Delivery

Given uncertainty regarding the appropriate size of financial incentives in the Indonesian context, a pilot study was conducted among 40 respondents in the Greater Jakarta area. Participants were randomly assigned to one of three incentive levels: 200,000, 400,000, or 600,000 Indonesian Rupiah (approximately USD 12, 24, and 36, respectively) and distributed across experimental conditions. Pilot results indicated that participation rates were largely inelastic beyond 500,000 IDR. Accordingly, the main study adopted 500,000 IDR as the benchmark incentive.

To study compliance responses, incentive levels in the full experiment were further randomized at 400,000, 500,000, or 600,000 IDR. Incentives were delivered via participants' chosen e-wallet platforms (e.g., OVO, GrabPay, GoPay). To establish trust and reduce attrition, participants received 20% of the incentive upon completion of the baseline survey, with the remaining 80% disbursed following completion of the endline survey.

5.5 Measurement

All participants completed an approximately 50-item baseline survey administered on their personal devices, with enumerators present to assist as needed. The same survey was administered at endline, remotely. Endline incentives were withheld until completion (or released after two weeks regardless of completion).

[3] iOS devices account for approximately 10% of the Indonesian smartphone market.

Survey instruments (in English; see Supplementary Materials) measured mental health and well-being using a variety of validated measures. All outcomes were grouped into an overall index, and the sub-indices were grouped into "families," each summarized using a composite index. Indices were constructed as control-group standardized averages (*38*). For a family with K outcomes, the index was defined as

$$\bar{y}_i = \frac{1}{K} \sum_{k=1}^{K} \left(\frac{y_{ik} - \mu_{0k}}{\sigma_{0k}} \right), \tag{S1}$$

where μ_{0k} and σ_{0k} denote the control-group mean and standard deviation of outcome k.

5.6 Estimation

5.6.1 Intent-to-Treat Analyses

Intent-to-treat (ITT) effects were estimated using ordinary least squares. For pooled hypotheses, the following specification was estimated:

$$Y_i = \gamma_0 + \gamma_1 T_i^1 + \gamma_2 T_i^2 + X_i' \theta + \epsilon_i, \tag{S2}$$

where Y_i denotes the indexed outcome, T_i^1 and T_i^2 indicate assignment to individual and household deactivation, respectively, and X_i includes baseline covariates (age, gender, religion, income, district). We selected these covariates ex ante because they capture major demographic, socioeconomic, and geographic sources of variation in mental health, social media use, and treatment compliance. Their inclusion improves precision and adjusts for chance imbalance while avoiding post-treatment controls.

To assess differential effects between individual- and household-level deactivation, interaction models were estimated incorporating respondents' baseline share of within-household social interaction.

5.6.2 Local Average Treatment Effect Analyses

Local average treatment effects (LATE) were estimated using two-stage least squares, with randomized assignment serving as an instrument for actual deactivation. Deactivation was defined as a binary indicator equal to one if participants averaged less than five minutes per day on any blocked platform over the study period, as measured by monitoring applications.

Separate first-stage equations were estimated for individual and household deactivation, followed by a second-stage regression of outcomes on predicted deactivation. Standard errors were clustered at the assignment unit. Interaction models were estimated to assess whether treatment effects varied with baseline within-household interaction intensity.

$$D_i^1 = \pi_0 + \pi_1 T_i^1 + X_i'\theta + \varepsilon_i, \tag{S3}$$

$$D_i^2 = \delta_0 + \delta_1 T_i^2 + X_i'\theta + \varepsilon_i, \tag{S4}$$

$$Y_i = \beta_0 + \beta_1 \widehat{D_i^1} + \beta_2 \widehat{D_i^2} + X_i'\theta + \varepsilon_i, \tag{S5}$$

where Y_i captures the indexed outcome at endline, X_i' is a vector of baseline demographic controls (age, gender, religion, income, district). As before, I establish a pooled parameter of interest as $\tau^{LATE} = \beta_1 + \beta_2$, which identifies the local average treatment effect (LATE) of social media deactivation among compliers—those who deactivate when assigned to do so.

5.7 Pre-registration

Key elements of the research design and experimental protocol were pre-registered. An anonymized version of the pre-registration can be found on the OSF directory (`https://osf.io/b9mwq/overview?view_only=e4daa4cbb00547d3a24c16f8adedf6aa`).

5.8 Ethical Considerations

All study procedures were approved by Princeton University's Institutional Review Board and by the implementing partner's internal review process. Participation was fully informed and voluntary, with no deception involved. Informed consent was obtained. Participants were permitted to resume social media use at any time for emergency communication without penalty.

5.9 Artificial Intelligence

The authors acknowledge the use of artificial intelligence in the copyediting of the manuscript and in the construction of the R code used to produce the associated analyses (ChatGPT 5.2). All errors are ours.

5.10 Survey items

Owing to space concerns, we fielded an abbreviated survey. We drew on a series of validated measures to capture subjective well-being. Specifically, affect items draw on the positive and negative affect framework and the day reconstruction method (*39, 40*). Life satisfaction follows standard cognitive well-being measurement. Anxiety and depression items correspond to the GAD-2 and PHQ-2, respectively (*41–43*). We relabel the PHQ-2 items to DEP-2 for clarity of exposition for non-technical readers. Sleep quality is measured using subjective self-reported sleep quality from the Pittsburgh Sleep Quality Index (PSQI) (*44*). It is worth flagging that we field abbreviated screening inventories to measure depressive symptoms and generalized anxiety disorder. Specifically, we draw on the first two items in the PHQ-9 and the GAD-7 indices. Analyses have shown that the abbreviated screening questionnaires (i.e., the two-item instruments) have strong psychometric validity to capture the evaluations identified in the longer inventories (*41, 45*).

Table S1: Well-being module items and sub-index mapping (V_44–V_47)

Var.	Question (English translation)	Responses	Sub-index
V_44_A–V_44_I	"Now, take a moment to remember what you did and experienced yesterday. Yesterday, did you feel: **(A)** Frustrated; **(B)** Lonely; **(C)** Satisfied; **(D)** Worried; **(E)** Bored; **(F)** Happy; **(G)** Angry; **(H)** Tired; **(I)** Stressed."	1 = Not at all; 2 = A little; 3 = Somewhat; 4 = Quite; 5 = Very	PNA
V_45	"Overall, on a scale from 1 to 7, how satisfied are you with your life right now? 1 means very dissatisfied, and 7 means very satisfied."	1–7	—
V_46_A–B	"In the last 2 weeks, how often have you been bothered by the following problems?" **(A)** Feeling nervous, anxious, or on edge; **(B)** Not being able to stop or control worrying	1 = Never; 2 = Sometimes; 3 = Often; 4 = Always;	GAD-2
V_46_C–D	"In the last 2 weeks, how often have you been bothered by the following problems?" **(C)** Little interest or pleasure in doing things; **(D)** Feeling down, depressed, or hopeless.	1 = Never; 2 = Sometimes; 3 = Often; 4 = Always	DEP-2
V_47	"In general, how would you rate your sleep quality in the last 2 weeks?"	1 = Very poor; 2 = Poor; 3 = Good; 4 = Very good;	Sleep

Notes: Items are grouped into sub-indices following standard practice. The PNA index aggregates yesterday affect items (V_44) and life satisfaction (V_45). The GAD-2 index aggregates anxiety items V_46_A and V_46_B; the DEP-2 index aggregates depression items V_46_C and V_46_D. Sleep quality (V_47) enters as a standalone sub-index. All items are coded such that higher values correspond to higher levels of the underlying construct prior to standardization.

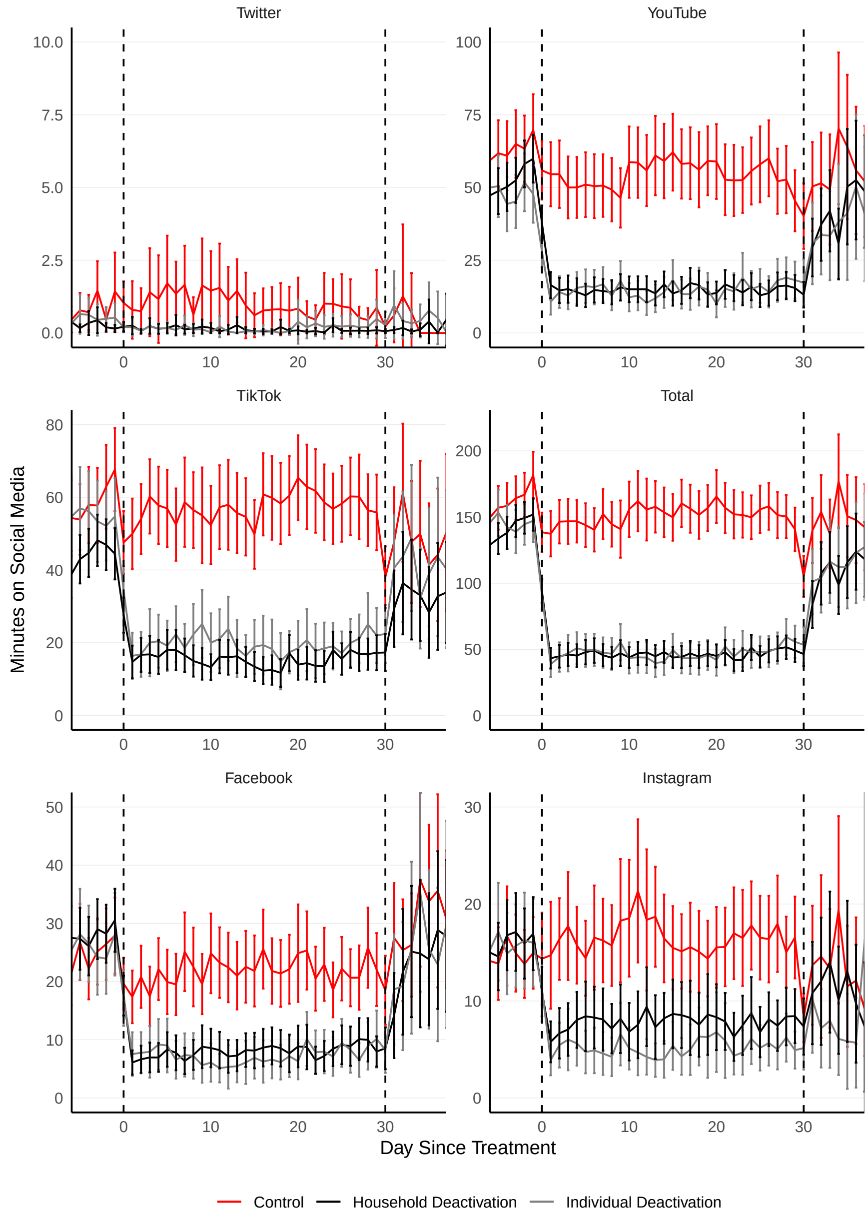

Figure S1: **Social media deactivation substantially reduced time spent on all platforms in both treatment arms.** Daily minutes spent on social media by days since treatment assignment by different platform. Solid lines denote group means and vertical error bars indicate 95% confidence intervals. Dashed vertical lines mark the start and end of the four-week deactivation period. Participants assigned to either deactivation condition exhibit an immediate and sustained reduction in social media use during the treatment window, with usage rebounding after incentives ended.

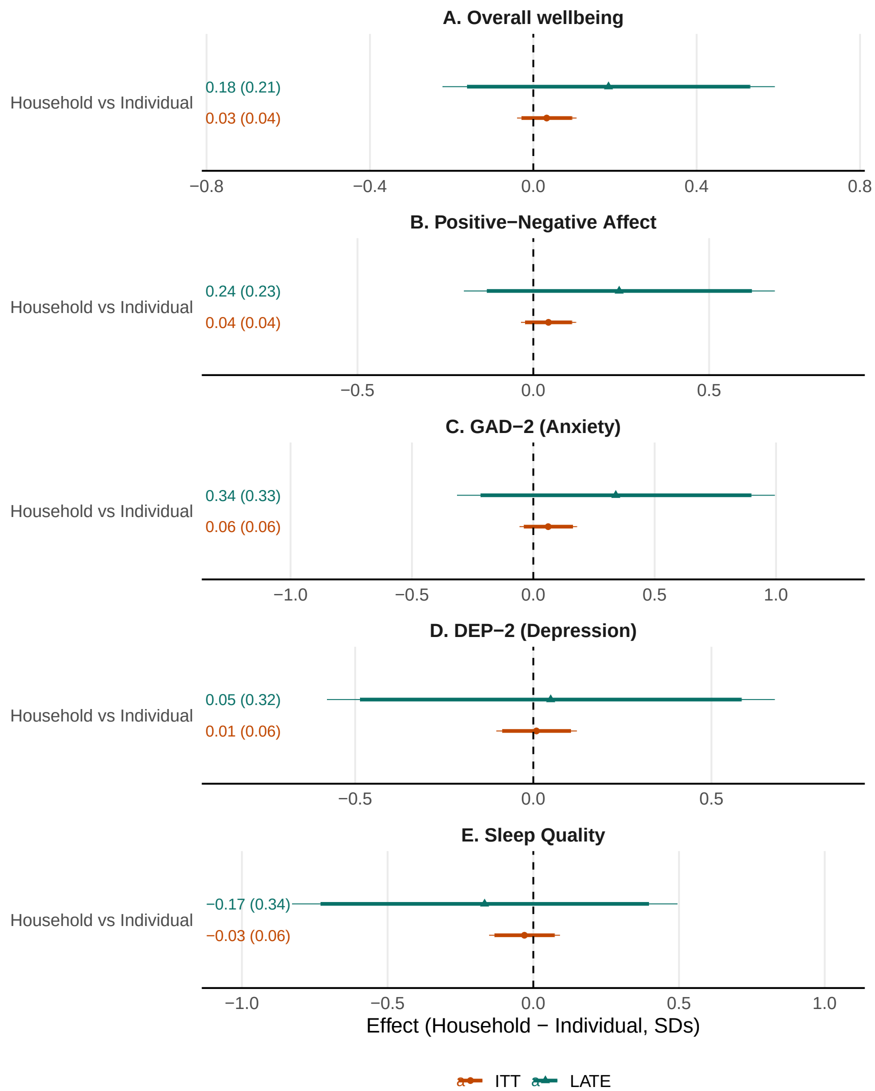

Figure S2: **Household versus Individual Social Media Deactivation: Effects on well-being.** This figure reports intent-to-treat (ITT) and local average treatment effects (LATE) of *household-level* versus *individual-level* social media deactivation on standardized well-being outcomes. Each panel corresponds to a different endline outcome. ITT estimates compare individuals assigned to the Household arm relative to the Individual arm. LATE estimates instrument compliance with assignment using the treatment assignment indicator and report the causal effect of compliance for compliers. All specifications control for baseline outcomes and pre-specified covariates. Point estimates are shown with 95 percent (thin bars) and 90 percent (thick bars) confidence intervals. Outcomes are standardized using the control-group baseline standard deviation. Robust standard errors are used.

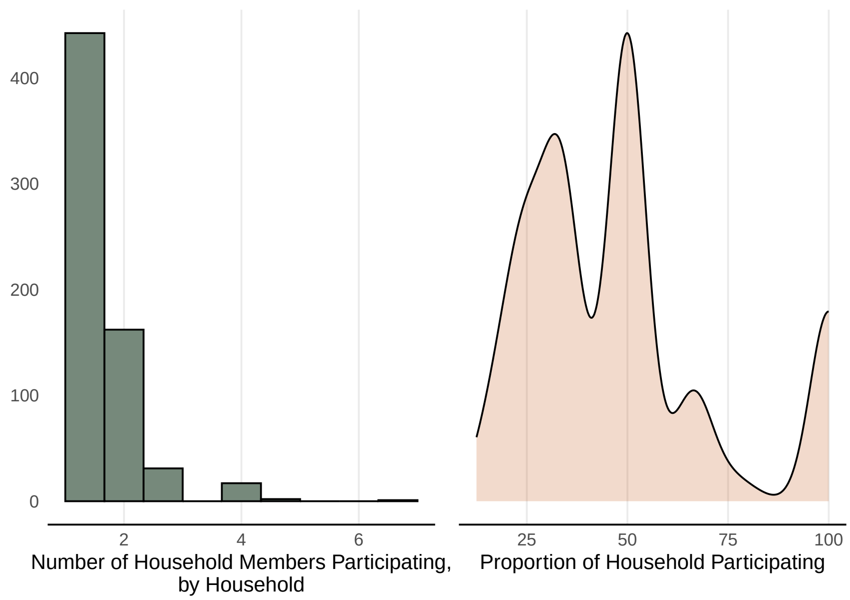

Figure S3: **Participation in Household Deactivation.** This figure depicts each household's participation in the household deactivation treatment arm. The left-hand panel shows the absolute number of participants in each household participating in the deactivation experiment. The right-hand panel shows the proportion of all potential participants who deactivated social media.

Table S2: Summary statistics: demographics (shares) and well-being outcomes.

Measure	N	Mean	St. Dev.	Min.	Median	Max.	source
Age (years)	1502	41.81	13.36	18	43	92	Demographics (Baseline)
Female (=1)	1502	0.50	0.50	0	0	1	Demographics (Baseline)
Male (=1)	1502	0.50	0.50	0	0	1	Demographics (Baseline)
Education: < HS (=1)	1502	0.83	0.38	0	1	1	Demographics (Baseline)
Education: ¿ HS (=1)	1502	0.17	0.38	0	0	1	Demographics (Baseline)
Religion: Islam (=1)	1502	0.91	0.29	0	1	1	Demographics (Baseline)
Religion: Other (=1)	1502	0.09	0.29	0	0	1	Demographics (Baseline)
Ethnicity: Java (=1)	1502	0.33	0.47	0	0	1	Demographics (Baseline)
Ethnicity: Sunda (=1)	1502	0.12	0.32	0	0	1	Demographics (Baseline)
Ethnicity: Other (=1)	1502	0.55	0.50	0	1	1	Demographics (Baseline)
Income: 1-3m IDR (=1)	1502	0.60	0.49	0	1	1	Demographics (Baseline)
Income: 3-6m IDR (=1)	1502	0.34	0.48	0	0	1	Demographics (Baseline)
Income: 6-9m IDR (=1)	1502	0.03	0.18	0	0	1	Demographics (Baseline)
Income: 9m+ IDR (=1)	1502	0.02	0.14	0	0	1	Demographics (Baseline)

Table S3: Baseline balance table: group means and p-values.

Measure	N	Control	T1	T2	p(T1-C)	p(T2-C)
Age (years)	1542	41.40	42.45	41.53	0.255	0.869
Female (=1)	1542	0.49	0.51	0.50	0.555	0.584
Male (=1)	1542	0.51	0.49	0.49	0.465	0.467
Education: < HS (=1)	1542	0.84	0.84	0.82	0.890	0.330
Education: > HS (=1)	1542	0.16	0.16	0.18	0.890	0.330
Religion: Islam (=1)	1542	0.90	0.91	0.91	0.540	0.463
Religion: Other (=1)	1542	0.10	0.09	0.09	0.540	0.463
Ethnicity: Java (=1)	1542	0.32	0.34	0.34	0.606	0.617
Ethnicity: Sunda (=1)	1542	0.12	0.11	0.12	0.657	0.673
Ethnicity: Other (=1)	1542	0.56	0.56	0.54	0.835	0.456
Income: 1-3m IDR (=1)	1542	0.59	0.62	0.60	0.501	0.887
Income: 3-6m IDR (=1)	1542	0.34	0.34	0.35	0.998	0.600
Income: 6-9m IDR (=1)	1542	0.04	0.03	0.04	0.273	0.694
Income: 9m+ IDR (=1)	1542	0.03	0.02	0.01	0.392	0.109

Table S4: Sample Characteristics Compared to Indonesian Population and Social Media Users

Categories	Sample	SM Users	Population
Gender			
Male	49.67%	49.80%	49.80%
Female	49.93%	50.20%	50.20%
Age			
18–24	12.98%	12.71%	17.90%
25–34	18.04%	24.29%	26.30%
35–44	24.57%	32.11%	22.40%
45–54	25.90%	19.48%	16.40%
55+	18.51%	9.94%	17.00%
Region			
Sumatera	22.84%	23.06%	20.40%
Java and Bali	56.52%	54.12%	61.10%
Central and Eastern Provinces	20.64%	22.82%	18.50%
Religion			
Islam	90.68%	89.89%	87.40%
Christian	6.39%	7.50%	9.30%
Others	2.93%	2.61%	2.80%
Education			
Less than High School	34.89%	42.22%	67.40%
High School	47.67%	41.08%	25.30%
Higher than High School	17.44%	16.71%	7.20%

Population shares are taken from the reference table (2010 census, ages 18+). Social media users are characterized by October 2023 SMRC survey with 1,227 social media users. Regions are constructed from BPS kab/kota codes (first two digits).

Table S5: Summary statistics: well-being module items (Baseline)

Measure	N	Mean	St. Dev.	Min.	Median	Max.	Source
Frustrated (1-5)	1466	1.73	1.08	1	1	5	PNA
Lonely (1-5)	1466	1.89	1.17	1	1	5	PNA
Satisfied (1-5)	1469	3.31	1.21	1	4	5	PNA
Worried (1-5)	1470	2.27	1.18	1	2	5	PNA
Bored (1-5)	1472	2.29	1.17	1	2	5	PNA
Happy (1-5)	1477	3.74	1.12	1	4	5	PNA
Angry (1-5)	1468	2.13	1.14	1	2	5	PNA
Tired (1-5)	1470	2.91	1.25	1	3	5	PNA
Stressed (1-5)	1468	1.96	1.13	1	2	5	PNA
Life satis (1-7)	1502	5.64	1.37	1	6	7	Overall
Nervous/anxious (1-4)	1478	1.76	0.65	1	2	4	GAD-2
Can’t control worrying (1-4)	1474	1.71	0.70	1	2	4	GAD-2
Little interest/pleasure (1-4)	1470	1.78	0.70	1	2	4	DEP-2
Down/depressed/hopeless (1-4)	1473	1.60	0.70	1	1	4	DEP-2
Sleep quality (1-4)	1489	2.88	0.60	1	3	4	Sleep

Table S6: Summary statistics: well-being module items (Endline).

Measure	N	Mean	St. Dev.	Min.	Median	Max.	Source
Frustrated (1-5)	1401	1.68	1.08	1	1	5	PNA
Lonely (1-5)	1401	1.90	1.19	1	1	5	PNA
Satisfied (1-5)	1407	3.33	1.19	1	4	5	PNA
Worried (1-5)	1408	2.26	1.18	1	2	5	PNA
Bored (1-5)	1405	2.31	1.21	1	2	5	PNA
Happy (1-5)	1410	3.76	1.10	1	4	5	PNA
Angry (1-5)	1402	2.06	1.08	1	2	5	PNA
Tired (1-5)	1409	2.89	1.28	1	3	5	PNA
Stressed (1-5)	1402	1.94	1.18	1	2	5	PNA
Life satis (1-7)	1438	5.67	1.70	1	6	7	Overall
Nervous/anxious (1-4)	1409	1.72	0.67	1	2	4	GAD-2
Can't control worrying (1-4)	1404	1.68	0.72	1	2	4	GAD-2
Little interest/pleasure (1-4)	1399	1.78	0.73	1	2	4	DEP-2
Down/depressed/hopeless (1-4)	1398	1.59	0.73	1	1	4	DEP-2
Sleep quality (1-4)	1428	2.89	0.63	1	3	4	Sleep

Table S7: ITT and LATE effects on well-being outcomes

		ITT			LATE		
Outcome	Arm	Est (SE)	p	BH p	Est (SE)	p	BH p
A. Overall well-being	T1	0.04 (0.04)	0.323	0.323	0.09 (0.10)	0.355	0.355
A. Overall well-being	T2	0.07 (0.03)	0.033	0.066	0.19 (0.08)	0.018	0.036
B. Positive-Negative Affect	T1	0.01 (0.04)	0.808	0.808	0.05 (0.11)	0.653	0.653
B. Positive-Negative Affect	T2	0.05 (0.04)	0.193	0.386	0.13 (0.09)	0.155	0.310
C. GAD-2 (Anxiety)	T1	0.07 (0.07)	0.322	0.322	0.13 (0.16)	0.428	0.428
C. GAD-2 (Anxiety)	T2	0.12 (0.06)	0.033	0.065	0.28 (0.13)	0.035	0.069
D. DEP-2 (Depression)	T1	0.10 (0.06)	0.128	0.128	0.21 (0.15)	0.162	0.162
D. DEP-2 (Depression)	T2	0.10 (0.05)	0.063	0.126	0.26 (0.12)	0.036	0.072
E. Sleep Quality	T1	0.17 (0.07)	0.009	0.009	0.38 (0.16)	0.017	0.017
E. Sleep Quality	T2	0.17 (0.06)	0.003	0.007	0.37 (0.14)	0.007	0.015