

What's up without WhatsApp? Experimental evidence on social capital and well-being in remote areas

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April 13, 2026

Abstract

How social ties are formed, maintained, and mobilized has changed with the adoption of digital communication technologies like WhatsApp. This paper estimates the value individuals place on the communication infrastructure that sustains their social networks and examines the short-run causal effects of losing access to it on social capital and well-being. We conduct a field experiment in three remote island communities in Indonesia, recruiting approximately 1,200 household heads as participants. To elicit individuals' willingness to accept compensation for a one-week restriction on WhatsApp usage, we implement the double-bounded dichotomous choice (DBDC) method. Participants are then randomly assigned to a treatment group, in which WhatsApp access is blocked for one week, or to a control group with no restriction. We compare outcomes across groups along three domains: social capital, subjective well-being, and time use. We further explore the mechanisms underlying these effects by examining changes in time use and daily behavioral patterns during the restriction period.

Keywords: WhatsApp, Social capital, Well-being

JEL Codes: D90, I31, O33, Z13

1 Introduction

Information and communication technologies (ICTs) have fundamentally transformed how individuals connect, coordinate, and access resources. In developing countries, where physical infrastructure is limited and the costs of maintaining relationships across distance are high, this transformation has been especially consequential. Studies have documented how ICTs reduce information frictions in economic markets, enabling more efficient allocation of goods and raising incomes (Jensen, 2007; Röller and Waverman, 2001). However, the welfare-enhancing potential of ICTs extends beyond commodity markets and price information. The rapid diffusion of ICTs has fundamentally altered how social networks are formed and how social ties within them are maintained. Digital platforms have enabled new forms of social capital in online space, expanding individuals' ability to exchange support, build trust, and coordinate daily life beyond geographic and physical constraints (Ali et al., 2023; Apuke and Omar, 2021; Aharony, 2015).

Social capital plays a central role in economic development by facilitating cooperation, trust, and access to economic opportunities (Woolcock, 1998; Francois and Zabojnik, 2005). These benefits arise because social capital is embedded in networks of relationships through which individuals exchange information, coordinate economic activities, and provide mutual support. However, unlike commodity prices or market transactions, access to social capital is not directly observable. The value that individuals place on the communication infrastructure sustaining their social networks, and the welfare consequences of losing access to it, cannot be read off from market data. Quantifying these, therefore, requires a different empirical approach.

WhatsApp offers a unique setting for quantifying both the value individuals place on the communication infrastructure that sustains their social networks and the causal consequences of losing access to it for social capital and well-being. Unlike Facebook, Instagram, or TikTok, which center on public feeds and algorithmic content curation, WhatsApp functions as an intimate, immediate, and private communication infrastructure. It is widely used for one-to-

one and group messaging, voice calls, and coordination within families, neighborhoods, and economic networks. In this sense, WhatsApp represents a modern evolution of earlier communication technologies such as the telephone and SMS, preserving their interpersonal and transactional character while dramatically lowering costs and expanding reach. The rapid adoption of WhatsApp has increasingly displaced these traditional means of communication, and in many developing-country contexts it has become a critical tool for informal business transactions, labor coordination, community organization, and the provision of emotional and social support (Matassi et al., 2019; Sampath et al., 2017).

A central difficulty is that digital communication platforms like WhatsApp are usually provided to users at no monetary cost, so the value users derive from them is unobserved in market prices. In addition, their absence or adoption is rarely observed in a way that allows causal identification. To this end, this paper runs a field experiment pursuing two main objectives. First, it estimates the value individuals place on the digital communication infrastructure through which social capital is formed and maintained, by eliciting their willingness to accept compensation for temporary access restriction to WhatsApp. Second, it provides causal evidence on how a disruption to that infrastructure affects social capital and individual well-being.

We conduct a field experiment in Indonesia, where WhatsApp is the most dominant communication application ¹. We focus particularly on three remote island communities: Karimunjawa in Central Java, Sumba in East Nusa Tenggara, and Banda in Maluku, spanning the western, central, and eastern parts of the archipelago. In such settings, ICT platforms may play a disproportionately important role in sustaining social capital by compensating for weak physical infrastructure and limited institutional reach (Salemink et al., 2017).

In the experiment, approximately 1,200 household heads are surveyed at baseline, where

¹According to Meltwater’s report (Kemp, 2025), 90.8% of adult internet users in Indonesia used WhatsApp at least once within the past month, higher than the global average at 54.0%. As of 2025, WhatsApp remained the most popular social networking application in Indonesia, followed by Instagram, Facebook, and TikTok.

we elicit their willingness to accept (WTA) compensation for temporarily deactivating WhatsApp for one week, using the double-bounded dichotomous choice (DBDC) method (Hannemann et al., 1991). Eligible participants are then randomly assigned to a treatment group, in which WhatsApp access is blocked for seven days via a third-party application, or to a control group with no restriction. We compare outcomes across groups along three domains: social capital, subjective well-being, and time use.

This study contributes to three strands of literature. First, it adds to the literature on social capital and ICT by examining how digital communication technologies shape trust, reciprocity, and network support—dimensions that remain relatively underexplored in online settings (Spottswood and Wohn, 2020; Antoci et al., 2019). Second, it contributes to the literature on digital platforms and individual well-being (Hunt et al., 2018; Verduyn et al., 2015, 2017). We advance both strands by providing, to our knowledge, the first controlled field experiment that compares individuals with restricted access to WhatsApp to those without restrictions, focusing explicitly on social capital and well-being outcomes. Our experimental design addresses key limitations of prior correlational studies. Moreover, by eliciting individuals’ valuations of WhatsApp, we are able to assess potential selection effects, thereby strengthening the robustness of our findings.

Finally, this study complements existing experimental work that examines the effects of social media platforms such as Facebook and Instagram (Allcott et al., 2024; Brailovskaia et al., 2020; Lambert et al., 2022; Mosquera et al., 2020; Tromholt, 2016). These studies predominantly focus on content-driven social media platforms and are largely conducted in developed-country contexts, often using student or online-panel samples. In contrast, we examine a communication-based platform and emphasize outcomes related to social capital rather than information exposure or content consumption.

Our study is most closely related to Mosquera et al. (2020), who assess the impact of a one-week Facebook deactivation on news exposure and awareness. They found that Facebook restrictions lead individuals to reduce news consumption, become less aware of politically

skewed sources, and experience less depression. Beyond differences in platform, outcomes, and context, we address methodological limitations associated with the Becker–DeGroot–Marschak (BDM) (Becker et al., 1964) mechanism by employing a DBDC approach (Hanemann et al., 1991), which mitigates attrition concerns. Moreover, by focusing on adults rather than student samples, our study captures the behavior of individuals for whom social networks carry real economic and social stakes. A communication disruption for this population is therefore far more consequential than for students, whose social networks are more homogeneous and whose communication needs are less economically significant, strengthening the external validity of our findings for developing-country contexts.

The subsequent section presents the experimental design, including the WTA elicitation and the experiment implementation. Section III presents the results, consisting of the baseline, value of WhatsApp, and impact evaluation. Section IV discusses effect heterogeneity and robustness. Section V concludes.

2 Experimental Design

In our experimental design, each participant will be visited twice in person. During the first visit, we collected baseline information on individual and household characteristics, outcome variables of interest, and participants’ eligibility on the experiment. The eligibility includes the condition of whether they used WhatsApp in the past week and their WhatsApp value elicitation. Eligible participants were randomly assigned to either a treatment or a control group. Participants in the treatment group were asked to voluntarily restrict their access to WhatsApp for one week. With their agreement, we install an app in their smartphone to block access to WhatsApp for a week.

One week later, we conducted a second visit to collect endline outcomes. Participants were informed in advance that there would be two survey visits, but were not told the specific content of the second visit. In addition, we collect their opinion on WhatsApp and their

opinion on the study. Upon completion of the experiment, participants received monetary compensation, with the amount determined by their stated valuation of WhatsApp elicited at baseline. This amount was informed during the first visit when they agree to participate.

2.1 WhatsApp Value Elicitation

In the baseline survey, we first check whether respondents are active users of WhatsApp, defined as respondents who report use of WhatsApp in the past week. They are then asked to state the minimum amount of money they would require to give up access to WhatsApp for one week. To elicit this willingness-to-accept (WTA) value, we employ the DBDC method (Carson et al., 1986; Hanemann et al., 1991), a standard contingent valuation technique in empirical economic research (Whittington et al. (1990); Donfouet et al. (2014)). In practice, participants are first asked whether they would accept a monetary offer X , where X is randomly drawn from the set IDR 100,000, 125,000, 150,000, 175,000, and 200,000, corresponding approximately to USD \$6, \$8.5, \$10, \$11.5, and \$13 respectively.² If the respondent answers “Yes,” they are subsequently asked whether they would accept a lower amount ($X/2$). If the respondent answers “No,” they are instead asked whether they would accept a higher amount ($2X$). Participants were told that their choice may have real monetary consequences, as we will randomly implement the experiment.

The highest offer accepted by the participant during the elicitation stage determines the payment amount for both groups. Treated participants receive this amount with certainty as compensation for the restriction, while control participants are offered a 50 percent probability of receiving half this amount as a study participation incentive, with the outcome disclosed at endline. Participants are informed of their group assignment and the corresponding payment arrangement immediately after the elicitation is completed and before the WhatsApp restriction is imposed. This sequencing therefore preserves the incentive-

²The average daily minimum wage across the three study sites is approximately USD \$7.5. The range of monetary offers is therefore calibrated to be salient and sufficient to compensate participants for the inconvenience of the restriction, while remaining modest enough to avoid income effects that could arise from windfall expectations distorting participants’ stated valuations.

compatibility properties of the DBDC mechanism for both groups.

Because respondents directly indicate whether specific offers are acceptable, the DBDC method reveals how individuals value WhatsApp usage. Each respondent’s DBDC response pattern, defined by their answers to the first and second bids, places their true WTA within a known interval. The four possible response patterns and their implied WTA intervals are as follows. A respondent who accepts both the initial offer X and the lower follow-up $X/2$ (YY) reveals $WTA \leq X/2$. A respondent who accepts X but rejects $X/2$ (YN) reveals $X/2 < WTA \leq X$. A respondent who rejects X but accepts the higher follow-up $2X$ (NY) reveals $X < WTA \leq 2X$. Finally, a respondent who rejects both offers (NN) reveals $WTA > 2X$. Respondents who answer “Yes” to at least one of the two offers are inferred to have a finite WTA and are eligible to participate in the experiment. Respondents who either answer “No” to both offers or do not meet the active-user criterion are not eligible for the experimental intervention and complete only the baseline survey.

The DBDC approach offers several advantages in our study context, particularly when compared to the BDM mechanism (Becker et al., 1964), a procedure in which participants state a precise monetary valuation that is then compared against a randomly drawn price to determine whether a transaction takes place, as used in previous studies. First, it is straightforward to implement, as respondents are only required to provide binary (Yes/No) answers. This simplicity is especially important in rural settings, where participants may find it difficult to state a precise monetary valuation or may respond strategically under more cognitively demanding mechanisms such as BDM. More complex valuation methods (e.g. multiple price list or discrete choice experiment) are therefore less suitable for our context. Second, DBDC is asymptotically more statistically efficient than single-bounded approaches, yielding substantially tighter confidence intervals for WTP estimates from the same sample size (Hanemann et al., 1991; Calia and Strazzera, 2000). Third, because eligibility for the experiment depends on the elicitation outcome, the adaptive structure of DBDC, which adjusts the second offer upward following an initial rejection, mechanically increases the pool

of eligible participants relative to a fixed single-offer design. Consistent with this concern, Mosquera et al. (2020) report that only 562 out of 1,769 respondents who completed a BDM survey were eligible for participation, a number that further declined to 167 actual participants who showed up.

A limitation of the DBDC method is that it does not directly observe a point estimate of WTA for each individual, as would be possible under BDM. Instead, each respondent’s WTA is bounded within an interval defined by their pattern of yes and no responses. However, exploiting the full structure of the double-bounded responses, we can recover individual-level WTA estimates through interval regression. This allows us to estimate the distribution of WhatsApp valuations across respondents and to construct individual-level WTA measures that we use both for population-level inference and as categorical measures of valuation in robustness checks to assess potential selection concerns in the main analysis.

Another is the potential for anchoring bias, whereby respondents’ answers to the second bid question are influenced by the initial bid rather than reflecting their true WTA (Herriges and Shogren, 1996). When anchoring is present, the efficiency gains of DBDC over single-bounded approaches can be diminished (Herriges and Shogren, 1996; Veronesi et al., 2011). However, anchoring bias is most severe when respondents have poorly formed preferences for the good being valued, as they may rely on the initial bid as an informational cue. In our context, WhatsApp is a familiar, daily-use platform for which participants have extensive direct experience, making it likely that preferences are well-formed prior to the elicitation. Moreover, Veronesi et al. (2011) show that a well-balanced and symmetric bid design, as employed in our study, substantially reduces starting point bias even when some anchoring is present. As a robustness check, we will test for internal consistency by examining whether the WTA distributions implied by the first and second bid responses differ significantly, following the procedure of Herriges and Shogren (1996).

2.2 Experiment Implementation

Upon completion of the value elicitation, eligible participants are randomly assigned to either the treatment or control group with equal (1:1) probability.³ Participants assigned to the treatment group are informed that the WhatsApp restriction will begin after midnight, allowing sufficient time to prepare and ensuring that participation does not cause immediate disruption. They are then asked to reconfirm their willingness to proceed with the restriction, conditional on knowing their treatment status. If they consent, access to WhatsApp is restricted for one week. Compensation is set equal to the highest monetary offer that the participant accepted during the elicitation stage. If a participant declines after assignment, no restriction is imposed; however, they remain in the study and are included in the follow-up surveys.

Participants assigned to the control group face no restriction on their WhatsApp usage and continue their daily communication activities as normal. Control participants are informed that they have a 50 percent chance of receiving half of their highest accepted DBDC offer, with the lottery result revealed and payment disbursed at the endline visit one week later. This arrangement ensures that control participants, like treatment participants, hold a monetary expectation throughout the seven-day period, mitigating asymmetric anticipation effects in endline responses. For both groups, payment is disbursed only after the endline survey is completed, so no participant receives money during the treatment period.

Both treatment and control participants are interviewed at baseline and again at endline one week later, with the endline survey administered to both groups simultaneously to ensure that any changes in outcomes are attributable to the restriction rather than to differences in the timing of data collection. For both groups, payment is disbursed only after the endline survey is completed, so no participant receives money during the treatment period

³We do not pre-randomize participants prior to the baseline survey, as eligibility depends on information collected during the value elicitation stage. Randomization is implemented within the survey application immediately after eligibility is determined, using a fixed random seed and equal assignment probabilities across all eligible participants.

To ensure proper implementation and compliance with the treatment, we use a third-party application, *Freedom*, which allows access to specific applications to be blocked for a predetermined period. The application is installed on treated participants’ smartphones, and WhatsApp access is disabled for seven consecutive days. During installation, enumerators verify that the restriction is active and provide instructions to participants on how the app functions. Compliance is further monitored during the follow-up visit by checking the app’s restriction log and confirming that WhatsApp remained inaccessible throughout the treatment period.

For ethical reasons and to ensure that participation is fully voluntary, all treated participants are explicitly informed that they may withdraw from the experiment at any time during the restriction period without penalty. Participants are provided with a dedicated contact phone number through which they can reach the research team. Upon request, the research team immediately removes the WhatsApp restriction remotely via the administrator settings of the blocking application. Participants who choose to discontinue the restriction before the end of the treatment period receive pro-rated compensation, calculated based on the number of days during which WhatsApp access was restricted.

2.3 Key Outcomes and Measurements

In the baseline survey, we collect individual characteristics including age, gender, education, physical conditions, and other demographic variables, as well as household characteristics such as expenditure, house conditions, and migration status. We also elicit detailed information on WhatsApp usage, including time spent on the application, frequency of use, and primary purposes of usage. In addition, we collect information on the use of other social media platforms as detailed in Appendix A.1.

For the impact evaluation, we record three outcome families in both the baseline and follow-up surveys as described below: social capital, subjective well-being, and time use. For each dimension within a family, we compute the mean of the relevant items to obtain

dimension-specific scores. The overall family index is then constructed by averaging these dimension scores, thereby assigning equal weight to each dimension. For robustness, we construct an alternative summary index for each family using inverse-covariance weighting following ?. Specifically, we first standardize each dimension score to have mean zero and standard deviation one in the control group at endline, after re-signing so that higher values reflect a uniformly positive direction. For the social capital index, all items are oriented so that higher values reflect greater social capital. For the IWS index, all items are oriented so that higher values reflect greater well-being. We then weight each standardized dimension score by the inverse of the endline control-group covariance matrix across dimension scores within the family. The resulting index is re-normalized to have standard deviation one in the control group.

We do not construct a single omnibus time-use index, because the direction of welfare interpretation is not uniform across time-use categories. Instead, time-use outcomes are analyzed separately, and, where appropriate, grouped into narrower conceptually coherent sub-families.

2.3.1 Social capital

The concept of social capital in this study follows Putnam (2000), who distinguishes social capital in two forms: bridging and bonding. Bridging social capital involves weak ties within broader networks. These relationships lack strong emotional depth but are characterized by mutual recognition and sufficient trust to enable cooperation, information exchange, and instrumental support. Bonding social capital, by contrast, consists of close and intimate relationships within small social circles. Such ties provide emotional support, affection, and essential benefits related to family life, often without strict expectations of reciprocity.

To measure bridging, we adopt 5 items from a scale developed and validated by Williams (2006), measuring emotional support, access to resources, ability to mobilize, and solidarity. We also adopt Williams’s scale to measure bonding with 5 items measuring outward looking,

broader contact, and larger community. These scales are widely used in the social capital literature that involves networking in the online context (Williams, 2019; Liu et al., 2016).

Furthermore, we adopt the concept of maintained social capital from Ellison et al. (2007), capturing an individual’s capacity to mobilize support or collective action, but they are tailored specifically to assess access to assistance from communities to which individuals previously belonged. In our context, we use old friends. The scale consists of 5 items. Each item is measured in a 5-point Likert scale, similar to other two social capital scales. The complete questions are detailed in Appendix A.2.

2.3.2 Subjective well-being

Subjective well-being is measured using an instrument developed and validated for the Indonesian context (Maulana et al., 2019), in which the population is characterized as a collectivist society with a hierarchical social structure, solid group norms, and strong group conformity (Jetten et al., 2002). The society also embraces the importance of adhering to religious practice and other expressions of spirituality (Jaafar et al., 2012). Therefore, the instrument, called the Indonesian Well-being Scale (IWS), incorporates family relations and spiritual dimensions in addition to standard well-being components like self-acceptance and basic needs. Participants completed the 20-item IWS instrument using a 5-point Likert scale. The complete questions are detailed in Appendix A.3.

For the secondary outcome, we employ two conventional measures of life satisfaction and happiness, which are the partial items of the self-acceptance dimension. Specifically, we seek the level of agreement with the statements ”In general, I’m satisfied with my life” and ”I am happy with my life”. These scales are widely adopted in the literature (Mosquera et al., 2020; Allcott et al., 2020) and have also been applied in the Indonesian context (Anna et al., 2019). In addition, we add an original dimension, harmony with the environment, given that our study context is located in the national marine protected areas, which are also an eco-tourism destination.

2.3.3 Time use

We measure time use by eliciting respondents’ estimates of average time spent on a range of daily activities, combining the approach from Mosquera et al. (2020) and Allcott et al. (2020). The activities include working, volunteering, exercising, and socializing. Daily behavioral changes are further captured using a battery of self-reported statements (e.g., “I save more money than I usually do”), with respondents indicating their level of agreement on a five-point Likert scale. The complete questions are detailed in Appendix A.4. We will mainly use this outcome family to explore the channels of the previous outcomes.

2.4 Theory of Change and Hypotheses

WhatsApp can be understood as a form of low-cost communication infrastructure that helps individuals maintain relationships, coordinate daily activities, and access emotional support. In remote and rural settings, where physical mobility is costly and offline communication alternatives are limited, such infrastructure may play a particularly important role in sustaining social ties and everyday coordination. In this context, temporary restriction of WhatsApp increases the effective cost of communication and may disrupt the maintenance and mobilization of social networks.

The intervention may affect outcomes through three main channels. First, participants may reduce communication overall, especially with contacts for whom WhatsApp is the main or lowest-cost mode of interaction. Second, participants may substitute toward alternative channels such as phone calls, SMS, in-person interaction, or other social media platforms, although such substitution may be incomplete because these alternatives are often more expensive, less convenient, or less commonly used for group coordination. Third, the restriction may alter daily routines and time allocation, with consequences for social interaction, emotional well-being, and other activities.

These channels imply potentially different effects across dimensions of social capital. Bonding social capital reflects support from close and intimate ties, which may be more

resilient if participants can substitute toward offline contact or other direct communication methods. Bridging social capital depends more on regular contact across broader and weaker networks, and may therefore be more sensitive to a temporary loss of WhatsApp. Maintained social capital, which captures the ability to stay connected to old friends and previous communities, may also decline if WhatsApp is an important tool for sustaining distant ties over time.

The effect on subjective well-being is theoretically ambiguous *ex ante*. Restricting WhatsApp may reduce well-being by weakening communication, support, and coordination. At the same time, reduced digital communication may lower distraction and induce greater offline interaction or more purposeful time use. Similarly, time-use responses may reflect substitution away from online communication toward family interaction, socializing, work, rest, or other activities, but may also reflect the inconvenience created by the restriction. The net effect is therefore an empirical question.

Based on this framework, we pre-specify the following hypotheses:

- H1.** *Bridging social capital decreases under WhatsApp restriction.*
- H2.** *Maintained social capital decreases under WhatsApp restriction.*
- H3.** *Bonding social capital changes under WhatsApp restriction.*
- H4.** *Subjective well-being changes under WhatsApp restriction.*
- H5.** *Time use and daily behavior change under WhatsApp restriction.*
- H6.** *Treatment effects are heterogenous with respect to reliance on WhatsApp and difference user characteristics.*

3 Data

3.1 Sampling

We conduct a field experiment across three remote island communities in Indonesia: Karimunjawa, Sumba, and Banda, located in the western, central, and eastern parts of the archipelago respectively. These sites share a set of structural characteristics that make them particularly well-suited to studying the role of communication infrastructure in sustaining social capital. Each island is geographically isolated, with limited transportation infrastructure and high costs of physical mobility, making digital communication not merely convenient but functionally central to maintaining social ties beyond the immediate neighborhood. In such settings, the welfare consequences of losing access to a dominant communication platform are likely to be especially pronounced. At the same time, the three sites differ markedly in culture, local language, and economic structure, and are separated from each other by more than 1,000 kilometers, ensuring that no spillover occurs across sites and strengthening the generalizability of our findings across diverse remote communities.

In each site, we obtained a complete list of households from village administrative records. From this sampling frame, we randomly selected 400 households per site as the target sample. Study participants are household heads aged between 18 and 64 years. As the primary economic and social decision-makers in their households, this group relies on WhatsApp not only for personal interaction but also for economic coordination, resource management, and access to support networks, making a communication disruption particularly consequential for them. This stands in contrast to studies relying on student or online-panel samples, and strengthens the external validity of our findings for developing-country contexts.

3.2 Power Calculations

Our target sample is 1,200 respondents, with individual-level random assignment into treatment and control in equal proportion. Power calculations are based on two-sided tests with a

5 percent significance level and 80 percent power. For comparability across outcome domains, we express minimum detectable effects (MDEs) in standard deviation units.

For *subjective well-being*, prior experimental studies on social media deactivation report a wide range of effect sizes. At the lower end, Allcott et al. (2020) report an improvement in their overall subjective well-being index of 0.09 standard deviations following a four-week Facebook deactivation. Mosquera et al. (2020), whose one-week Facebook restriction design most closely resembles ours, report a reduction in feelings of depression but do not find a significant effect on overall life satisfaction, a null result likely attributable to the small experimental sample of 167 participants rather than the absence of a true effect. At the upper end, ? report large effects of $d = 0.58$ on well-being, $d = 0.45$ on depression, and $d = 0.35$ on anxiety, though their sample was self-selected and likely more socially media-dependent than our household head population, which inflates these estimates relative to our context. The most relevant benchmark for our study is ?, who implements a one-week social media break with a large community sample and reports more modest effects of $d = 0.17$ on life satisfaction and $d = 0.21$ on positive emotion.

For *social capital*, we use prior evidence on the relationship between social networking use and social capital as a benchmark for substantive relevance. In a meta-analysis, Liu et al. (2016) report an association of $r = 0.32$ between SNS use and bridging social capital and $r = 0.26$ between SNS use and bonding social capital. They further show that relationship-maintenance activities on social networking sites are especially strongly associated with both bridging and bonding capital. Complementing this, Williams (2019) documents that most studies in the literature find positive links between social networking sites and bonding social capital, while Ali et al. (2023) reports positive associations between WhatsApp use and both bridging and bonding social capital. Although these studies are not experimental and therefore do not provide direct causal effect sizes for a one-week WhatsApp restriction, they indicate that the underlying relationship is substantively meaningful.

The effective experimental sample depends on two sequential adjustments to the 1,200

sampled respondents. First, not all respondents will be eligible for the experiment. Eligibility requires both active WhatsApp use in the past week and acceptance of at least one offer in the DBDC elicitation. We assume an eligibility rate of 80 percent, yielding 960 eligible participants. For comparison, Allcott et al. (2020) report a 61 percent eligibility rate with US Facebook users under BDM. The adaptive upward adjustment in DBDC is expected to recover a substantially larger share of participants. Second, we assume a completion rate of 90 percent among eligible participants, reflecting a 10 percent rate of attrition between baseline and endline. This yields a conservative effective experimental sample of 864 participants.

Table 1 reports the MDE under alternative eligibility rate assumptions, holding the completion rate fixed at 90 percent. Because the main confirmatory outcomes are standardized indices and the design is balanced across treatment and control groups, the MDE is the same for the social capital and subjective well-being indices. These calculations are conservative, as the main analysis exploits baseline and endline observations with individual fixed effects, which improves precision relative to a simple post-treatment comparison (McKenzie, 2012).

Table 1: Minimum Detectable Effects Under Alternative Eligibility Rate Assumptions

Eligibility rate	Eligible sample	Effective sample	MDE
100%	1,200	1,080	0.17 SD
90%	1,080	972	0.18 SD
80%	960	864	0.19 SD
70%	840	756	0.20 SD

Notes: MDEs are calculated for a balanced treatment-control design with 80 percent power and a 5 percent two-sided significance level. Effects are expressed in standard deviation units.

Under our planning assumption of 80 percent eligibility and 90 percent completion, the effective experimental sample is 864 participants and the cross-sectional MDE is approximately 0.19 standard deviations. The main analysis improves on this through the panel structure of the data. Following McKenzie (2012), the panel-adjusted MDE is given by $MDE_{panel} = MDE \times \sqrt{1 - \rho^2}$, where ρ is the autocorrelation of outcomes between baseline and endline. Assuming $\rho \in [0.3, 0.5]$ for social capital and well-being outcomes, the panel-adjusted MDE falls to approximately 0.17–0.18 standard deviations, lower than the

cross-sectional benchmark.

These magnitudes exceed the well-being effects reported in Allcott et al. (2020) of 0.09 standard deviations, indicating that the study is not powered to detect effects of that precise magnitude. However, this places our study at the threshold of detecting effects of the magnitude documented by Tromholt (2016) at $d = 0.17$. Given that our context involves a communication-based platform in remote communities where offline substitutes are scarce, disruptions to social capital and well-being are likely to be larger than those generated by content-based platform deactivations in developed-country settings. We therefore consider effects in the range of 0.17–0.20 SD to be substantively plausible and within the detectable range of this design.

3.3 Data Collection and Processing

The study relies on primary survey. Survey interviews are conducted by trained enumerators recruited by our local partner universities in each study site. Data collection is carried out through two rounds of in-person interviews with each participant, as explained the Section 2. All interviews are administered using Computer-Assisted Personal Interviewing (CAPI) software. The use of CAPI allows responses to be entered directly into digital questionnaires in the field, reducing manual data-entry errors and enabling the implementation of built-in skip patterns, consistency checks, and validation rules during the interview process.

Fieldwork is conducted sequentially across the three study sites according to the following schedule:

- **Karimunjawa:** 28 April–13 June 2026
- **Sumba:** 18 May–29 June 2026
- **Banda:** 18 August–29 September 2026

Because the surveys are administered in person and each respondent is visited twice, the data collection protocol also enables direct verification of treatment implementation,

participant follow-up, and completion of the endline survey. After fieldwork, survey data from all sites will be merged into a harmonized dataset for analysis. Standard cleaning procedures will include checks for duplicate observations, missing identifiers, inconsistent survey responses, and out-of-range values recorded despite CAPI validation rules.

4 Empirical strategy

To evaluate the impact of WhatsApp restriction on individuals, we estimate the average treatment effect by comparing endline outcomes between treatment and control participants, controlling for the baseline value of each outcome. Specifically, our primary specification is an analysis of covariance (ANCOVA) estimated by OLS:

$$Y_{i,1} = \alpha + \beta T_i + \gamma Y_{i,0} + \delta \widehat{\text{WTA}}_i + X_i' \phi + \mu_c + \varepsilon_i \quad (1)$$

where $Y_{i,1}$ is the outcome for individual i measured at endline; T_i is a binary indicator equal to one if individual i was randomly assigned to the WhatsApp restriction group; and $Y_{i,0}$ is the same outcome measured at baseline. $\widehat{\text{WTA}}_i$ is the individual fitted WTA value recovered from the interval regression, included as a pre-treatment covariate capturing individual-level valuation of WhatsApp. X_i is a vector of baseline covariates comprising age, gender, education level, WhatsApp usage intensity, primary purpose of WhatsApp use, per capita household expenditure, and migration status. μ_c denotes site fixed effects for Karimunjawa, Sumba, and Banda, included to account for stratified randomisation across sites. Robust standard errors are reported throughout. The coefficient of interest is $\hat{\beta}$, the intent-to-treat (ITT) effect of WhatsApp restriction on the outcome.

Imposing this restriction is never costless unless $\rho = 1$, which is implausible for psychosocial outcomes measured over a one-week treatment window. Including $Y_{i,0}$ on the right-hand side absorbs baseline variance in outcomes, reducing the residual variance against which $\hat{\beta}$ is estimated. The inclusion of $\widehat{\text{WTA}}_i$ further reduces residual variance, as individuals with

higher WhatsApp valuation likely differ systematically in their baseline levels of WhatsApp-mediated social capital and well-being.

Equation (1) estimates the ITT effect, defined as the average effect of being assigned to the WhatsApp restriction group, regardless of whether the participant fully complied with the restriction throughout the treatment period. The ITT estimator is our primary specification, as it reflects the policy-relevant effect of imposing a communication restriction on a population and is robust to selective non-compliance.

As a secondary analysis, we estimate the local average treatment effect (LATE) among compliers, defined as participants who maintained the WhatsApp restriction for the full seven-day period as verified by the Freedom application log. We instrument actual compliance with treatment assignment using two-stage least squares (2SLS), where the first stage regresses an indicator of full compliance on treatment assignment and the second stage estimates Equation (1) using predicted compliance. The LATE identifies the effect of the restriction among those who would comply when assigned to treatment and is expected to exceed the ITT in magnitude to the extent that non-compliers attenuate the average effect.

As a robustness check, we re-estimate the treatment effect using a Difference in Difference specification:⁴

$$Y_{it} = \alpha + \beta \text{Post}_t + \gamma (\text{Post}_t \times T_i) + \theta_i + \varepsilon_{it} \quad (2)$$

where Post_t is a dummy equal to one for the endline period, θ_i are individual fixed effects, and γ is the DiD estimate of the ITT effect. This specification identifies treatment effects using within-individual variation, effectively differencing out time-invariant individual characteristics and absorbing any baseline imbalance in covariates between the treatment and control groups. Under this framework, identification relies solely on random assignment to treatment status, ensuring that observed differences in outcomes reflect causal effects of the

⁴The ANCOVA specification is preferred over endline-only OLS and DiD on efficiency grounds. McKenzie (2012) shows that ANCOVA yields a smaller minimum detectable effect than DiD for any outcome autocorrelation $\rho < 1$, because ANCOVA estimates the coefficient γ on the baseline outcome freely, whereas DiD imposes the restriction $\gamma = 1$.

intervention, conditional on the eligibility on the experiment.

Participants who decline to proceed with the restriction after assignment, or who withdraw during the treatment period and receive pro-rated compensation, are retained in the ITT sample but excluded from the complier sample used in the LATE estimation. All participants, including non-compliers and early withdrawers, are re-interviewed at endline and included in the ITT analysis.

Our analysis tests treatment effects across multiple outcomes within each family. To address concerns about false discoveries arising from multiple inference, we report sharpened False Discovery Rate (FDR)-adjusted p-values following the procedure of Benjamini et al. (2006). The FDR adjustment is applied separately within each outcome family. For robustness, we additionally report p-values adjusted to control the Family-Wise Error Rate (FWER) using the free step-down resampling method of Anderson (2008). Unadjusted p-values, sharpened FDR-adjusted p-values, and FWER-adjusted p-values are reported together in the Appendix. In the main tables, we report unadjusted p-values via conventional significance indicators.

4.1 Potential Limitation

4.1.1 Spillover

A potential concern in community-based experiments is the possibility of spillover effects between treatment and control participants within the same island. In our setting, this could arise if control group members have treated individuals in their social network, and consequently find it harder to communicate via WhatsApp simply because those contacts are unavailable on the platform. In effect, control participants may experience a partial and indirect communication disruption without having been assigned to the restriction themselves, causing them to behave more similarly to treated participants than they otherwise would. This would compress the estimated treatment effect toward zero, understating the true impact of the restriction.

Several features of our design mitigate this concern. First, randomization is conducted at the individual level, meaning that treated and control participants are interspersed across the same villages rather than being geographically concentrated, reducing the likelihood that treated individuals systematically redirect their communication toward an identifiable cluster of control participants. Second, participants are not informed of the treatment status of other study participants, limiting their ability to deliberately substitute toward control group members as a communication channel. Third, the short duration of the restriction, one week, reduces the window over which such behavioral adjustments could accumulate and propagate through the community.

Nonetheless, we acknowledge that within-island spillover cannot be entirely ruled out in a community environment where social networks are dense and treatment status may become apparent through participants’ observable behavior. To assess the potential magnitude of this concern, we include in the survey administered to control group participants the question: “In the past 7 days, did you have difficulty contacting your friends or family through WhatsApp?” A systematic pattern of affirmative responses among control participants would provide direct evidence of spillover exposure, as it would indicate that their WhatsApp communication was disrupted not by their own restriction but by the restriction imposed on treated individuals in their network. We will use responses to this question to identify potentially contaminated control observations and assess the robustness of our main estimates to their exclusion.

4.1.2 Location and period

Our sample is drawn from three remote island communities in Indonesia, which limits the generalizability of our findings in two respects. First, our sites are not representative of WhatsApp users in Indonesia more broadly, nor of developing countries in general. Rural and remote communities may exhibit different patterns of WhatsApp usage compared to urban populations, both in terms of the intensity of use and the purposes for which the

platform is employed. Urban users, for instance, may rely more heavily on WhatsApp for professional coordination and access to formal economic networks, while rural users may depend on it more for maintaining ties with family members or with the community. Thus, the welfare consequences of a temporary restriction may differ across settings in ways that are difficult to predict a priori. Second, the three sites were selected to represent geographically and culturally diverse remote communities, but they share the common characteristic of island isolation, which may not generalize to landlocked rural areas where transportation infrastructure, while limited, differs in character from maritime isolation.

Furthermore, our treatment is limited to a one-week restriction period, which raises two distinct concerns. First, the short duration may not capture the full range of behavioral and social adjustments that individuals would make in response to a more prolonged disruption. Over longer periods, participants may develop alternative communication routines, substitute more fully toward other platforms or in-person interaction, or experience habituation effects that attenuate the initial welfare costs. The direction of these dynamic effects is theoretically ambiguous, and we cannot verify that the effects we estimate are linear in duration. Allcott et al. (2020), who implement a four-week deactivation, find persistent well-being effects, suggesting that effects do not simply dissipate over time, but the effect may be different in our context.

Second, a one-week window may be too short to detect meaningful changes in social capital dimensions which may depend on the gradual erosion of weaker ties over time rather than immediate disruption. Our choice of one week is motivated by ethical and logistical considerations, as well as comparability with Tromholt (2016), Lambert et al. (2022), and Mosquera et al. (2020), all of whom implement one-week interventions.

4.1.3 Self-report

Lastly, our outcome variables are obtained from self-reported survey responses, which introduces potential sources of bias. The first is classical measurement error, which attenuates

estimated treatment effects toward zero and reduces statistical power. Although we administer outcomes using validated instruments, including the Indonesian Well-being Scale (Maulana et al., 2019) and the social capital scales of Williams (2006), self-reported measures of well-being and social capital are inherently subject to recall bias, social desirability bias, and day-to-day mood fluctuations that may not reflect stable underlying constructs.

The second and more specific concern is experimenter demand effects, whereby participants in the treatment group, aware of the study’s purpose, may systematically adjust their endline responses in the direction they believe the researchers expect, either by reporting lower social capital or well-being to confirm the presumed hypothesis, or conversely by overstating their resilience to signal that the restriction was not burdensome.

To assess the potential magnitude of experimenter demand effects, we follow the approach of Allcott et al. (2020). As shown in the Appendix A.5, the endline survey will ask participants: “Do you think the researchers in this study had an agenda?” with possible responses including: (i) I do not think they had a particular agenda; (ii) yes, they wanted to show that WhatsApp is good for people; (iii) yes, they wanted to show that WhatsApp is bad for people; and (iv) I am not sure. For demand effects to bias our treatment effect estimates, either the Treatment and Control groups must hold systematically different beliefs about what the researchers want, or participants must sense their treatment status and adjust their responses accordingly. We will first test whether perceived researcher agenda is balanced between treatment and control groups.

4.1.4 Income expectation

A potential concern in payment-based experiments is that treated participants, knowing they will receive compensation, may alter their behavior or endline responses in anticipation of the payment rather than in response to the communication disruption itself. In standard experimental designs of this type, including Allcott et al. (2020) and Hall et al. (2021), compensation is paid exclusively to treated participants, creating an asymmetric monetary

expectation that could confound treatment effect estimates with anticipation effects. We address this by extending a payment opportunity to control participants: upon assignment, control participants are informed that they have a 50 percent chance of receiving half of their highest accepted DBDC offer at endline. Both groups therefore hold a monetary expectation throughout the seven-day treatment period.

The remaining residual asymmetry is that the treatment group may expect larger payment (their full accepted bid, with certainty) than the control group (one quarter of that bid in expectation). If a higher expected payment generates stronger salience, treatment participants may still think about the money more frequently than control participants, and this differential salience could affect endline reports. To assess the magnitude of this residual concern, we administer two compensation salience questions to all participants at endline (see Appendix A.5). Specifically, we ask how frequently they thought about the potential payment during the week, and whether they had made plans for its use. If salience is balanced across groups, the residual anticipation concern is unlikely to bias the estimated treatment effects. If salience is higher among treated participants, we will assess robustness by examining whether treatment effects differ between participants with high versus low compensation salience.

Note that a direct income effect during the treatment period is ruled out by design: both groups receive payment only after completing the endline survey, so no participant holds additional cash during the week of the experiment.

5 Main analysis

5.1 Value of WhatsApp

This subsection presents estimates of the value that individuals place on WhatsApp as a communication infrastructure, elicited through the DBDC method described in Section 2.1. We report the distribution of WTA across respondents and the mean WTA for the

experimental sample.

The primary WTA estimates are obtained by maximum likelihood estimation (MLE) of the double-bounded model (Hanemann et al., 1991; Lopez-Feldman, 2012). We first estimate the model without covariates to obtain the unconditional mean WTA, then re-estimate including covariates evaluated at their sample means. As a secondary and complementary approach, we estimate individual-level WTA using interval regression, which treats each respondent’s WTA as interval-censored within the bounds implied by their response pattern. This approach exploits the same interval structure as the MLE but yields individual-level fitted WTA estimates, which we use in robustness checks and heterogeneity analyses. Specifically, individual fitted WTA values are used as a continuous measure of WhatsApp valuation to assess whether WTA predicts treatment effect magnitudes, providing a test of potential selection concerns in the main experimental analysis.

Both the MLE and interval regression specifications are estimated with and without covariates. The covariate vector includes individual characteristics collected at baseline: age, gender, and education level. We additionally include WhatsApp usage intensity and an indicator for the primary purpose of use (personal communication, economic activity, or other). Household-level controls include per capita expenditure and migration status. Including these covariates serves two purposes: it improves the precision of the WTA estimates by reducing residual variance, and it allows us to characterize the distribution of WhatsApp valuations across subgroups relevant to the heterogeneity analysis in Section 5.3.

We report the estimated mean WTA for the full eligible sample and separately by site. We will present the unconditional mean WTA alongside covariate-adjusted estimates evaluated at sample means. To characterize the distribution of WhatsApp valuations across respondents, we will plot the histogram of individual fitted WTA values recovered from the regression with the full covariate vector. We also will report the share of respondents in each DBDC response category (YY, YN, NY, NN) as a descriptive check on the internal consistency of the elicitation.

5.2 Impacts of WhatsApp Restriction

This subsection presents the estimates of the impact of WhatsApp restriction experiment on three main outcomes: social capital, subjective well-being, and time use. The estimate will follow the structure of equation 1, with the ITT estimate as the primary coefficient of interest. The LATE estimate will be presented as a robustness check. The tables presented in this subsection report results separately for the overall family index and for each dimension within the family. Coefficient cells are left empty pending data collection.

For example, Tables 2 and Table 3 present the impact of WhatsApp restriction on social capital and subjective well-being, respectively. We will also provide alternative estimates using DiD (equation 2) in Appendix A.6

Table 2: Effects on Social Capital

	(1)	(2)	(3)	(4)
	Overall index	Bridging	Bonding	Maintained
WhatsApp Restriction				
Baseline outcome				
Fitted WTA				
Covariates	Yes	Yes	Yes	Yes
Site FE	Yes	Yes	Yes	Yes
Observations				
R^2				

Notes: Estimates from OLS (ANCOVA) following Equation (1). The dependent variable in column (1) is the social capital index aggregating all three dimensions. Columns (2)–(4) report dimension-level scores for bridging, bonding, and maintained social capital, respectively, each measured using a five-item Likert scale. Baseline covariates include age, gender, education level, WhatsApp usage intensity, primary purpose of WhatsApp use, per capita household expenditure, and migration status. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5.3 Heterogeneity

This subsections report effect heterogeneity across different individual characteristics like gender, age, and education. We also report heterogeneity across different WhatsApp user types. We will present some figures showing the point of estimates and their corresponding

Table 3: Effects on Subjective Well-being

	(1)	(2)	(3)	(4)	(5)
	Overall	Spirituality	Social &	Basic	Self-
	index		family	needs	acceptance
WhatsApp restriction					
Baseline outcome					
Fitted WTA					
Covariates	Yes	Yes	Yes	Yes	Yes
Site FE	Yes	Yes	Yes	Yes	Yes
Observations					
R^2					

Notes: Estimates from OLS (ANCOVA) following Equation (1). The dependent variable in column (1) is the subjective well-being index aggregating all five dimensions of the Indonesian Well-being Scale (IWS). Columns (2)–(5) report dimension-level scores for spirituality, social and family relations, basic needs, and self-acceptance, respectively, each measured using the five-item IWS instrument on a four-point Likert scale. Covariates include age, gender, education level, WhatsApp usage intensity, primary purpose of WhatsApp use, per capita household expenditure, and migration status. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

confidence interval. The reported estimates are obtained from the interaction between individual characteristics and parameter of interest using the previous regression specification.

6 Administrative information

- **Funding:**

This research is funded by KONEKSI (Collaborative Initiative in the Knowledge and Innovation Sector), BRIN (National Research and Innovation Agency, Indonesia), and the Crawford School’s Higher Degree Research Fund.

- **Institutional Review Board (ethics approval):**

This research was reviewed by the Human Ethics Office, Australian National University, and has received ethical clearance with approval number Protocol H/2025/0492. Approval was also obtained from the Ethical Committee on Social Studies and Humanities - Indonesian National Research and Innovation Agency, with approval number No.769/KE.01/SK/08/2025.

- **Declaration of interest:**

The authors have no conflicts of interest to declare.

- **Acknowledgment:**

We thank local research partners for the implementation of the experiment: Dian Wijayanto (Universitas Diponegoro), Eny Sulistyaningrum (Universitas Gadjah Mada), Umbu Reku Raya (CREDOS), and Wardis Girsang (Universitas Pattimura). We thank Firman Kartaadipoetra, Ryan Edward, and Satoshi Yamazaki for comments and feedback.

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Appendices

A Measurement Details

A.1 WhatsApp and Social Media Use

WhatsApp Use	
<i>In the last 7 days, how much time per day on average did you use WhatsApp for ... ?</i>	
Personal communication	1–6
Group/community communication	1–6
Economic activities (work or business)	1–6
Entertainment	1–6
Education	1–6

WhatsApp Engagement	
<i>In the last 7 days, ...</i>	
How many of your WhatsApp groups were active?	groups
How many WhatsApp groups did you actively participate in?	groups
How many WhatsApp stories did you post?	stories
How many WhatsApp stories did you respond to?	stories
Did you have difficulty contacting your friends or family through WhatsApp?	1–5

Other Social Media	
<i>In the last 7 days, for each platform below ...</i>	
Have you ever used [...]?	yes/no
How much time per day on average did you use [...]?	0–6
How often did you actively post/share/upload content on [...]?	1–5
How often did you actively respond (like, comment, forward) to content from someone else on [...]?	1–5

Notes: For WhatsApp engagement, “active” refers to at least one chat, sticker, or other interaction within the group during the last 7 days. Other platforms include: Facebook, Instagram, TikTok, Telegram, Threads, X (formerly Twitter), LinkedIn, Snack Video, and others. For posting and response frequency: (1) Never, (2) Rarely, (3) Sometimes, (4) Often, (5) Very frequently. Time-use responses follow a seven-point ordinal scale: (1) 0 minutes, (2) 1–30 minutes, (3) 31 minutes – 1 hour, (4) 1–2 hours, (5) 2–3 hours, (6) More than 3 hours.

A.2 Social Capital

<i>Bonding Social Capital</i>	
There are several people I trust to help solve my problems.	1–5
There is someone I can turn to for advice about making very important decisions.	1–5
When I feel lonely, there are several people I can talk to.	1–5
If I needed an emergency loan of \$500, I know someone I can turn to.	1–5
The people I interact with would help me fight an injustice.	1–5
<i>Bridging Social Capital</i>	
Interacting with people makes me want to try new things.	1–5
Interacting with people makes me interested in what people unlike me are thinking.	1–5
Interacting with people makes me feel like part of a larger community.	1–5
I am willing to spend time to support general community activities.	1–5
Interacting with people gives me new people to talk to.	1–5
<i>Maintained Social Capital</i>	
I'd be able to find out about events in another town from my old friends.	1–5
If I needed to, I could ask old friends to do a small favor for me.	1–5
I'd be able to stay with my old friends if traveling to a different city.	1–5
I would be able to find information about job opportunities from old friends.	1–5
It would be easy to find my old friends to invite to my personal event.	1–5

Note: All items are measured using a five-point Likert scale: (1) Strongly agree, (2) Agree, (3) Neither agree nor disagree, (4) Disagree, (5) Strongly disagree.

A.3 Subjective Well-being

<i>Spirituality</i>	
Praying helps me to stay positive.	1–4
God helps me to overcome my problems.	1–4
God has decided what is best for me.	1–4
Praying helps me to stay calm.	1–4
Even in a difficult time, God is with me.	1–4
<i>Social Family Relation</i>	
I spend enough time with the family I live with (e.g., parents, children, grandparents, or relatives).	1–4
My family always supports me (e.g., gives motivation, spirit, and courage).	1–4
I always support my family.	1–4
My parents/family are always there for me when I need them.	1–4
I am always there for my parents/family when they need me.	1–4
I personally know most of the people in the neighbourhood.	1–4
<i>Basic Needs</i>	
I can afford all my family's transportation costs (e.g., bus fare, fuel for own vehicle).	1–4
I can pay for all my children's education (e.g., school/university fees, uniforms, and books).	1–4
I have some money for emergencies or difficult times.	1–4
I can spare money for secondary needs (e.g., recreation).	1–4
<i>Self-Acceptance</i>	
In general, I am satisfied with my life.	1–4
I avoid comparing my life to others.	1–4
Life is not a burden for me.	1–4
I am happy with my life.	1–4
I am not upset if I do not get what I want.	1–4
<i>Harmony with Environment</i>	
I usually engage in leisure activities outside in nature (e.g., hiking, swimming, outdoor activities).	1–4 ^a
I enjoy being outside in nature.	1–4
How close do you think you are to nature? (showcard)	1–4

Note: Unless otherwise specified, items use a four-point Likert scale: (1) Strongly disagree, (2) Disagree, (3) Agree, (4) Strongly agree. ^aFor the leisure-activities item, response options are: (1) Never, (2) Sometimes, (3) Often, (4) Very often. For the nature-closeness item, responses options are: (1) Very close, (2) Close, (3) Far, (4) Vert far.

A.4 Time Use

Time Use

In the past 7 days, how much time per day did you spend on ...

Working for pay	_____ hrs/day
Working voluntarily	_____ hrs/day
Studying	_____ hrs/day
Exercising	_____ hrs/day
Gathering with family	_____ hrs/day
Hanging out with friends or attending social events	_____ hrs/day
Praying	_____ hrs/day
Sleeping	_____ hrs/day

Phone Use

In the last 7 days, ...

How much time did you spend on phone calls?	1–6
How much time did you spend on reading and sending SMS?	1–6
How much time did you spend online using your phone?	1–6

Behavioral Changes

Please think about what you did over the last 7 days as you answer the following questions.

I went out more often than I normally do.	1–5
I met more people than I normally do.	1–5
I saved more money than I usually do.	1–5
I ate out less than I normally do.	1–5
I was less online than I normally do.	1–5
I wasted less time than I normally do.	1–5
I had more time to relax and be with friends.	1–5

Note: Behavioral change items are measured using a five-point Likert scale: (1) Strongly agree, (2) Agree, (3) Neither agree nor disagree, (4) Disagree, (5) Strongly disagree. Phone-use responses follow a seven-point ordinal scale: (1) 0 minutes, (2) 1–30 minutes, (3) 31 minutes – 1 hour, (4) 1–2 hours, (5) 2–3 hours, (6) More than 3 hours.

A.5 Additional endline questions

Question	Response options
Do you think this study had a specific agenda regarding WhatsApp or social media use?	(1) No, I do not think there was a particular agenda (2) Yes, they wanted to show that WhatsApp has a positive effect (3) Yes, they wanted to show that WhatsApp has a negative effect (4) I am not sure
<i>[Treatment only]</i> In the past 7 days, how often did you think about the compensation you would receive for restricting WhatsApp?	(1) Never (2) Rarely (3) Sometimes (4) Often (5) Very frequently
<i>[Treatment only]</i> In the past 7 days, did you already have a plan for how you would use the compensation from this study?	(1) Yes (2) No

Notes: The first question is administered to all endline respondents. The remaining two questions are administered only to participants assigned to the treatment group.

A.6 Alternative estimates

Table A1: Effects on Social Capital (DiD)

	(1)	(2)	(3)	(4)
	Overall index	Bridging	Bonding	Maintained
Post $\times$ Treat				
Post				
Individual FE	Yes	Yes	Yes	Yes
Observations				
R^2				

Note: The dependent variable in column (1) is the social capital index aggregating all three dimensions. Columns (2)–(4) report dimension-level scores for bridging, bonding, and maintained social capital, respectively each measured using the five-item Likert scale. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A2: Effects on Subjective Well-being (DiD)

	(1)	(2)	(3)	(4)	(5)
	Overall index	Spirituality	Social family	Basic needs	Self-acceptance
Post $\times$ Treat					
Post					
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations					
R^2					

Note: The dependent variable in column (1) is the subjective well-being index aggregating all five dimensions of the Indonesian Well-being Scale (IWS). Columns (2)–(5) report dimension-level scores for spirituality, social family relation, basic needs, and self-acceptance, respectively, each measured using the five-item IWS instrument on a four-point Likert scale. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.