“Crisis communication and internet penetration: Cross-country evidence with illustrations from Moldova and Armenia”

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CRISIS COMMUNICATION AND INTERNET PENETRATION: CROSS-COUNTRY EVIDENCE WITH ILLUSTRATIONS FROM MOLDOVA AND ARMENIA

Abstract

Coordinated public information campaigns became a near-universal instrument of pandemic crisis communication, while the internet penetration of their audiences ranged from under 3 percent to full coverage. The study examines whether the association between campaign intensity and the pace of vaccination uptake varies with pre-pandemic internet penetration, a contextual marker of the digital environment rather than campaign exposure. Using a global country-month panel of 168 economies over 2021–2022 (N = 3,410), it estimates two-way fixed-effects models with country-clustered standard errors, drawing campaign intensity from the Oxford COVID-19 Government Response Tracker and fixing connectivity at its 2019 level. The interaction between campaigns and internet penetration is positive and robust (0.912, $p < 0.001$): a standard deviation of baseline penetration, 27.9 percentage points, raises the campaign–uptake slope by about 0.9 points per month. The marginal association is significantly negative at low connectivity (−1.955 at the tenth percentile, $p < 0.001$), indistinguishable from zero at the median (−0.289) and the ninetieth percentile (+0.501), with the confidence band excluding zero only below roughly 62 percent penetration; the firm result is the negative association under low connectivity. The pattern survives winsorizing, one-month lagging, separate estimation for 2021 and 2022, and vaccination-policy controls, yet baseline connectivity correlates with income and governance at $r = 0.77–0.90$, so the moderation indexes a development bundle of which connectivity is one legible marker. The illustrative cases of Moldova and Armenia, comparably connected yet facing contrasting overlapping crises, show connectivity alone does not assure uptake where parallel emergencies compete for attention.

Keywords crisis communication, internet penetration, information campaigns, vaccination uptake, digital divide, Moldova, Armenia

JEL Classification I18, L86, M38

INTRODUCTION

Crisis communication – reaching the public when pandemics, conflicts, or economic shocks strike – now unfolds largely within a digital information environment. By 2023 about 67% of the world, some 5.4 billion people, were online and 2.6 billion offline (ITU, 2023); yet connectivity is deeply uneven – 93% in high-income against only 27% in low-income countries, a 66-percentage-point divide (ITU, 2023). For marketing and communication practice this raises a structural question of audience reach: whether the same public campaign is associated with different behavioral outcomes in societies whose connectivity differs so widely.

Delivering an official message is harder within an overloaded information environment – the WHO's "infodemic" of accurate and false content that erodes trust in official guidance (WHO, 2020). Public infor-

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mation campaigns are the main instrument for turning guidance into protective behavior. COVID-19, linked to an estimated 14.9 million excess deaths in 2020–2021 (WHO, 2022), made these campaigns a near-simultaneous global test, with vaccination uptake a rare comparable measure of whether the message worked. According to Ferranna (2023), low-income countries reached only about 27% full vaccination – far below the WHO target of 70% by mid-2022 (WHO, 2021) – against more than 70% in high-income economies. Supply explains much of this gap; whether the association between campaign communication and uptake also varies with the connectivity of the audience remains an open empirical question.

Yet evidence on effective crisis communication is uneven: mostly single-country, qualitative, or focused on advanced economies, with cross-country evidence on whether the campaign–uptake association varies with audience connectivity still scarce and transition economies under-represented. This study addresses that gap. On a global country-month panel for 2021–2022, it examines how pre-pandemic internet penetration – treated as a contextual marker of the digital environment rather than as a measure of campaign exposure – moderates the association between public information campaigns, recorded as coordinated government messaging through any medium, and the pace of vaccination uptake, complemented by illustrative case evidence from two transition economies, Moldova and Armenia. Both combine comparably high digital capacity with contrasting crises overlapping the pandemic – an economic and energy shock in Moldova, armed conflict and its aftermath in Armenia – showing how crisis communication performs where other emergencies compete for public attention. The design is associational throughout, and the cases serve as illustrations rather than as tests of the estimated pattern.

1. LITERATURE REVIEW

Crisis communication increasingly operates through digital channels, and a first strand of research documents how thoroughly digital transformation has reshaped the environment in which organizations communicate. Digital platforms, e-marketing, and ICT investment have become central to socio-economic continuity, competitive resilience, and consumer engagement (Aboalganam & Alzghoul, 2024; Chahdi et al., 2025; Nafei et al., 2025), while digital service quality and telecommunication investment increasingly mediate firm performance (Sahnouni & Kadri, 2025; H. Saifi & F. Saifi, 2025). At the systemic level, ICT-enabled innovation environments have likewise been tied to socio-economic development at the national and local scale (Omelyanenko et al., 2020, 2021). Against this backdrop, foundational crisis-communication theory holds that public messaging must be timely, transparent, and audience-centered to protect reputation and manage acute uncertainty (Coombs, 2007; Reynolds & Seeger, 2005, Vdovichen et al., 2023). The COVID-19 pandemic accelerated this shift, compelling institutions to rely on information and communication technologies to disseminate health guidance and to sustain organizational and entrepreneurial activity (Kornyliuk et al., 2022; Malaquias et al., 2023; Wołowiec et al., 2022); yet practitioners often struggled to map best-practice theory onto a rapid, chaotic digital environment (Okuhara et al., 2025). At the firm level, the same pressures drove enterprises to adopt digital tools for adaptation and anti-crisis management (Galoyan et al., 2023; Mykhalchenko et al., 2023). The open question is therefore not whether crisis messages are digital, but whether they reach and move their audiences.

A second strand specifies the mechanisms through which digital channels carry crisis messages, and the conditions on which their efficacy depends. Mobile-government applications and digital public services enable interactive, real-time communication that clarifies complex directives and raises public satisfaction and institutional trust (Abou-Moghli & Shatem, 2024; Kuzior, Pakhnenko et al., 2023; Wang et al., 2024). On social platforms, the strategic use of empathy, clarity, and a conversational tone increases audience engagement and information-seeking relative to passive broadcasting (Austin et al., 2012; Hima et al., 2025; MacKay et al., 2022), and destination and service-sector organizations leaned on these channels to sustain communication across successive pandemic waves (Keller et al., 2023). Traditional mass media, in turn, retain a structuring role in shaping public orientation alongside these digital channels (Krupskyy et al., 2024). Digitalization has further been linked to integrity, transparency, and corruption reduction (Yefimenko, Malaniuk et al., 2025; Yefimenko, Boronos et al., 2025). Crucially, however, the delivery and impact of digital messages are structurally dependent on robust telecommunication infrastructure and internet penetration, which act as necessary conditions for converting digital capability into public response (Hrytsenko et al., 2026; Sahnouni & Kadri, 2025). These benefits, the next strand shows, are neither universal nor linear.

A third strand documents pronounced heterogeneity, and at points outright contradiction, in how digital reach translates into communication outcomes. Persistent digital divides leave the benefits of connectivity unevenly distributed and expose vulnerable users to privacy risks, algorithmic manipulation, and cyberpsychological harm (Burrell, 2026; Kovac et al., 2024; Wieczorek & Postrzednik-Lotko, 2025), while market disruption stratifies smaller firms along digital lines (Sartamorn et al., 2025). Narrowing these divides is increasingly framed as a matter of regulation, data protection, and the ethics of technology as much as of infrastructure (Mkrtchyan et al., 2024). These dynamics turn most consequential in public-health emergencies, where unmoderated channels accelerate infodemics that breed vaccine hesitancy, undermine pandemic preparedness, and amplify community conflict (Bappayo, Abubakar et al., 2026; Penka et al., 2024; Zahorodnia et al., 2026), and top-down fact-checking frequently fails where localized, peer-to-peer correction succeeds (Penka et al., 2024). A direct contradiction concerns campaign effectiveness: meta-analytic evidence finds that social-media interventions reliably shift vaccine knowledge and attitudes yet exert a weak or statistically non-significant effect on actual uptake without offline support (Nazari et al., 2026), paralleling evidence that the gains from e-governance are non-linear, weakening during a transitional adjustment phase before maturity (Hrytsenko et al., 2026). Trust trajectories are equally paradoxical, as citizens reporting the highest initial satisfaction with crisis communication can exhibit the sharpest subsequent decline in institutional trust (Abdelzadeh & Sedelius, 2024), and leniency-based measures intended to raise compliance may instead crowd out moral obligation (Erina et al., 2026). Beneath many of these contradictions lies a single recurring factor – trust.

A fourth strand situates these dynamics within institutional trust and regional disruption, the terrain on which communication either does or does not translate into compliance. Trust operates as the foundational mechanism behind voluntary compliance, tax morale, and organizational resilience, mediated by service quality, ethical leadership, transparent communication, and early policy responses (Kostenko et al., 2022; Kyzenko et al., 2026; Rodoplu Şahin et al., 2023), and it is further shaped by health awareness and lifestyle among younger cohorts and by remote-work and well-being dynamics (Agustina et al., 2024; Sarwar et al., 2026). Pandemic conditions reshaped sectors from branding and insurance to commercial real estate (Jerobon, 2026; Macovei et al., 2024; Saher et al., 2021), brought digital tools into emergency response (Burrell et al., 2025; Mercer-Bey, 2025), and exposed the ethical strain on responsible reporting and on the oversight of automated decision-making (Bappayo, Auwal et al., 2026; Hayes & Burrell, 2026). Through a Black Swan lens, digitalization has further been framed as a change-management lever that helped firms absorb the same shock across diverse national contexts (Popkova et al., 2024). The capacity to withstand the pandemic has itself been tied to the organization of public-health systems (Kuzior, Vasyliev et al., 2023). These pressures are sharpest in transitional and conflict-affected states, where advanced digital frameworks must be implemented amid infrastructural deficits, corruption, and war (Grigoryan et al., 2025; Hakobyan & Margaryan, 2025; Zakharkina et al., 2025), leaving digitalization pathways highly unequal and locally constrained. Yet the evidence behind these patterns is largely local and fragmented.

Across these strands, one structural question remains under-tested: whether the association between public information campaigns and health behavior varies with the internet penetration of the populations they address. Cross-country panel evidence on this conditioning role is scarce, as most research on COVID-19 vaccination communication relies on localized randomized trials,

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cross-sectional surveys, or single-platform analyses (Kovac et al., 2024; Nazari et al., 2026), and even global assessments tend to examine digital development and health-spending efficiency rather than campaign effectiveness on physical uptake (Hrytsenko et al., 2026). To this end, it estimates a global country-month two-way fixed-effects panel of 168 economies over 2021–2022, interacting campaign intensity from the Oxford COVID-19 Government Response Tracker (Hale et al., 2021) with 2019 internet penetration from the World Bank (World Bank, 2024) to predict the monthly pace of vaccination uptake recorded by Our World in Data (Mathieu et al., 2021). Because connectivity, income and governance are tightly correlated across countries, the study tests, rather than presumes, whether the moderation can be attributed to connectivity specifically, and it complements the panel with the contrasting overlapping-crisis cases of Moldova and Armenia. In doing so, it locates the campaign–uptake association along the digital divide and converts a widely asserted but rarely tested intuition – that campaign communication and audience connectivity are jointly consequential – into macro-level evidence, showing that the statistically established part of the pattern lies precisely where connectivity is thinnest.

This study aims to quantify how pre-pandemic internet penetration – a contextual marker of the digital environment in which campaigns operate – conditions the association between public information campaigns and the pace of COVID-19 vaccination uptake.

2. DATA AND METHODOLOGY

2.1. Data and sources

The study draws on a country-month panel covering the period from January 2021 to December 2022, the interval over which COVID-19 vaccination outcomes and public-information-campaign intensity are jointly observed across countries. Crisis-communication intensity is measured by the public information campaigns indicator of the Oxford COVID-19 Government Response Tracker (OxCGRT), coded H1 in the tracker's indicator scheme (the label is the OxCGRT indicator identifier), a zero-to-two ordinal index of coordinated government messaging through any medium; the index records the presence and scope of a campaign, not its channel or its subject, and is not specific to vaccination. OxCGRT also supplies the composite containment Stringency Index and the vaccination-policy indicators used in robustness checks: vaccine eligibility (OxCGRT code H7, ordinal 0–5), vaccine availability (V2A, ordinal 0–3) and mandatory vaccination (V4, binary). Daily OxCGRT indicators are aggregated to country-months as the within-month mean of daily values; a modal-category alternative for the campaign index is examined in robustness. Vaccination coverage and the epidemiological controls, namely new and cumulative COVID-19 cases and deaths per million, are taken from Our World in Data. The moderating variable is pre-pandemic internet penetration: the share of individuals using the internet in 2019, from the World Bank's World Development Indicators. Fixing the moderator at its pre-pandemic value makes it time-invariant by construction, so that its main effect is absorbed exactly by the country fixed effects (Section 2.3) and its measurement predates the outcome window. For four economies without a 2019 observation – Sudan, Syria, Turkmenistan and Vanuatu – the latest value available up to 2020 is used (2017, 2018, 2016 and 2015, respectively); a robustness check excludes them. The annual internet series for 2021 and 2022 is retained for a further robustness check in which the contemporaneous main effect is estimated explicitly. Log GDP per capita and the Worldwide Governance Indicators of government effectiveness and political stability are obtained from the World Bank; these macroeconomic and institutional covariates are annual and are matched to every month of the corresponding year, so they vary across but not within years. All sources are public; the series are merged on ISO country codes, and the estimation sample retains every country-month with complete information on the dependent variable and the regressors, yielding 3,410 observations for 168 economies (listed in Table A1) over twenty-four months.

2.2. Variables

The dependent variable, the monthly change in full vaccination, is the month-on-month change, in percentage points, in the share of the population that is fully vaccinated; it captures the pace of uptake rather than the accumulated level. The change rather than the level is used because the level is a cumulative, non-decreasing stock that saturates mechanically as coverage rises and largely reflects roll-out timing, whereas the monthly change responds to contemporaneous conditions, including communication, and is comparable across countries at different stages of roll-out; the level is retained as an alternative outcome in the robustness checks and, lagged, as a control for saturation in column 4 of Table 3. The focal explanatory variable is the campaign index H1. The moderating variable, pre-pandemic (2019) internet penetration, is entered in standardized form so that the interaction coefficient is read per standard deviation of baseline connectivity, about 27.9 percentage points; government effectiveness and log GDP per capita are standardized likewise where they interact with campaigns. Table 1 reports each variable, its definition and its source.

2.3. Empirical specification

The empirical model is a two-way fixed-effects specification. Equation (1) relates the monthly change in full vaccination to campaign intensity and to its interaction with standardized pre-pandemic internet penetration, conditioning on a vector of time-varying controls and on country and month fixed effects. The country effects absorb all stable national characteristics; because the moderator is fixed at its 2019 value, it is time-invariant by construction, its main effect is absorbed exactly by the country fixed effects, and the hierarchy-of-interactions principle is respected by design rather than by an approximation argument. The month effects remove shocks common to all countries, such as global vaccine-supply conditions. Identification of the campaign coefficients is correspondingly demanding: the campaign index does not move within the window in 78 of the 168 economies, so the coefficients on campaigns and on their interaction are estimated from the 90 economies with within-country campaign variation, while the remaining economies contribute to the estimation of the controls and the fixed effects; this is stated here because it bounds what the coefficients can claim to describe. The additive baseline reported in the first column of the results is the restriction of Equation (1) in which the interaction coefficient is set to zero. A dynamic augmentation of Equation (1), reported as the fourth column of the results, adds the lagged vaccination level to absorb saturation and roll-out timing; because a lagged outcome level combined with country fixed effects induces Nickell bias of order 1/T, with T = 24 here, that column is read as a bounding exercise rather than as the preferred estimate:

$$\Delta Vax_{it} = \alpha_i + \delta_t + \beta_1 H1_{it} + \beta_2 H1_{it} \cdot Internet_i^z + \gamma' X_{it} + \varepsilon_{it}, \tag{1}$$

Table 1. Variables, definitions and sources

Variable	Definition	Source
ΔVax (dependent variable)	Month-on-month change in the share fully vaccinated (percentage points)	Our World in Data
H1 – public information campaigns	Coordinated government information campaigns, ordinal 0–2; monthly mean of daily values	OxCGRT
Internet penetration, 2019 (moderator)	Individuals using the internet (% of population) at the pre-pandemic 2019 level; standardised; time-invariant	World Bank (WDI)
Lagged vaccination level	Share fully vaccinated in the previous month (percentage points); column 4 only	Our World in Data
Stringency index	Composite COVID-19 containment stringency, 0–100	OxCGRT
Epidemiological controls	New cases, new deaths and cumulative deaths (per million)	Our World in Data
Government effectiveness	Worldwide Governance Indicators estimate; standardized	World Bank (WGI)
Log GDP per capita	Natural log of GDP per capita, constant prices; standardized	World Bank (WDI)
Political stability	Worldwide Governance Indicators estimate	World Bank (WGI)
Vaccination-policy controls (robustness)	Vaccine eligibility (H7, 0–5); vaccine availability (V2A, 0–3); vaccine mandates (V4, 0/1)	OxCGRT
Case-study indicators	E-government sub-indices; political-violence events; conflict fatalities	UN E-Gov Survey; ACLED; UCDP

Note: Standardized variables have mean zero and standard deviation one over the estimation sample. Daily policy indicators are aggregated to country-months as the mean of daily values. Baseline connectivity is the 2019 value (latest available up to 2020 for four economies). Case-study indicators are used only in the country sections, not in the panel regressions.

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where the subscripts i and t index country and month, respectively; ΔVax_{it} is the dependent variable, the month-on-month change (in percentage points) in the share of the population fully vaccinated; α_i and δ_t are the country and month fixed effects; $H1_{it}$ is the public-information-campaign index; $Internet_i^z$ is the standardized pre-pandemic (2019) internet penetration of country i, the superscript z denoting rescaling to mean zero and unit standard deviation over the estimation sample; being time-invariant, it carries no t subscript and its main effect is collinear with α_i, which is the intended design rather than an omission; β_1 is the coefficient on campaign intensity and β_2 the coefficient on its interaction with connectivity; X_{it} is the vector of time-varying controls – the Stringency Index, new and cumulative cases and deaths per million, standardized government effectiveness, standardized log GDP per capita and political stability – with conformable coefficient vector γ (γ' denoting its transpose); and ε_{it} is the idiosyncratic error, against which standard errors are clustered by country.

Equation (2) asks whether the connectivity interaction can be separated from broader administrative capacity or income by augmenting the model with interactions of campaigns with standardized government effectiveness and with standardized log GDP per capita. Because the three moderators are strongly correlated in the sample (r = 0.77–0.90; Table B1), this is a deliberately demanding specification: it can reveal whether the data can apportion the moderation among the three dimensions, but an insignificant coefficient on any single interaction in this horse race is not evidence that the corresponding dimension is irrelevant, and the results are read with that limitation in view:

$$\begin{aligned}\Delta Vax_{it} &= \alpha_i + \delta_t + \beta_1 H1_{it} \\ &+ \beta_2 H1_{it} \cdot Internet_i^z + \beta_3 H1_{it} \cdot Gov_{it}^z \\ &+ \beta_4 H1_{it} \cdot \ln GDP_{it}^z + \gamma' X_{it} + \varepsilon_{it},\end{aligned} \tag{2}$$

where β_3 and β_4 are the coefficients on the interactions of campaign intensity with standardized government effectiveness (Gov_{it}^z) and with standardized log GDP per capita ($\ln GDP_{it}^z$); all remaining terms are defined as in Equation (1).

Because the relationship between campaigns and vaccination pace is conditional, it is summarized by the marginal effect in Equation (3), the derivative of expected vaccination pace with respect to campaign intensity, which varies linearly with connectivity. This quantity is evaluated at the tenth, fiftieth and ninetieth percentiles of the 2019 internet-penetration distribution, with standard errors obtained by the delta method:

$$\frac{\partial \mathrm{E}\left[\Delta Vax_{it}\right]}{\partial H1_{it}} = \beta_1 + \beta_2 Internet_i^z, \tag{3}$$

where E[·] denotes the conditional expectation, so that the left-hand side is the marginal effect of campaign intensity on vaccination pace – the partial derivative of expected pace with respect to $H1_{it}$. This effect is linear in connectivity and equals β_1 at the sample mean of baseline internet penetration (where $Internet_i^z = 0$).

2.4. Estimation and inference

All models are estimated by ordinary least squares with the within (fixed-effects) transformation and standard errors clustered by country to accommodate heteroskedasticity and serial correlation; inference on the interaction is additionally verified under two-way clustering by country and month and under Driscoll–Kraay standard errors robust to cross-sectional dependence, reported with the robustness results. The preference for fixed over random effects is confirmed by a Hausman test, and collinearity among regressors is monitored through variance-inflation factors, both reported with the results. The dependent variable is audited before estimation: because the underlying cumulative vaccination series is non-decreasing, negative monthly increments do not occur in the sample, so retrospective data revisions cannot enter as spurious negative pace; extreme positive increments – 1.1 percent of country-months exceed twenty points, reflecting catch-up reporting – are addressed by winsorization. The robustness battery comprises: winsorizing the dependent variable at the first and ninety-ninth percentiles; replacing campaigns with their one-month lag to attenuate contemporaneous simultaneity; estimating the model separately for 2021 and 2022; substituting alternative outcomes, namely the cumulative vaccination level and COVID-19 deaths per million; re-estimating with the annual 2021–2022 internet series and its explicit contemporaneous main effect; replacing the campaign index with a binary indicator of its maximum category and with modal-category dummies; adding the vaccination-policy controls (eligibility, availability and mandates); and excluding the four economies whose baseline connectivity relies on a pre-2019 fallback. All analyses are conducted in Python with a fixed random seed to ensure reproducibility.

2.5. Limitations

Four limitations bound the interpretation. First, the design is associational, not causal: campaigns are policy-responsive and were intensified where uptake was faltering, and although lagging the measure eases immediate simultaneity, absent an exogenous instrument the estimates characterize how the campaign–uptake relationship varies with connectivity, not the causal effect of campaigns. Vaccine eligibility and mandates are controlled directly in robustness, but vaccine supply, delivery capacity and public trust are not observed at a global monthly scale and remain omitted; the lagged-level specification indicates that part of the unconditional negative campaign coefficient reflects saturation dynamics rather than communication itself, and the remainder cannot be apportioned among the omitted factors. Second, both central variables are imperfect: the campaign index is ordinal, records the presence of coordinated public messaging through any medium – it is neither channel-specific nor vaccine-specific – sits exactly at its ceiling in 86 percent of country-months, and its monthly value is the mean of daily codings; internet penetration, fixed at its 2019 level, is a coarse marker of the digital environment – not message exposure, platform use or digital literacy – and is so strongly correlated with income and governance ($r = 0.77–0.90$) that the estimated moderation cannot be attributed to connectivity as distinct from the broader capacity bundle it indexes; the macroeconomic and institutional indicators are annual and repeat within years. Third, identification is narrower than the sample: the campaign coefficients are estimated from the 90 economies whose campaign index moves within the window; the baseline of four economies relies on a pre-2019 fallback; and the dynamic column is subject to Nickell bias at $T = 24$. Fourth, coverage is non-random, confined to country-months in OxCGRT and Our World in Data; Armenia, absent from the campaign data, enters only as a qualitative case, so the findings generalize to reporting economies during the vaccination roll-out rather than universally.

3. RESULTS

3.1. Descriptive statistics and identification

The empirical analysis draws on an unbalanced monthly panel of 3,410 country-month observations spanning 168 economies from January 2021 to December 2022, the window over which vaccination outcomes and public-information-campaign intensity are jointly observed. Table 2 summarizes the variables entering the estimation. The dependent variable, defined as the month-on-month change in the share of the population that is fully vaccinated, averages 2.51 percentage points but exhibits pronounced right-skewness: its standard deviation (4.42) exceeds its mean, and values range from complete stagnation (0.00) to a maximum monthly gain of 52.52 points. This shape is not a nuisance to be smoothed away but is substantively informative, because vaccination diffusion during the period proceeded in short, concentrated surges separated by long plateaus rather than as a smooth trend. Two modelling choices follow directly. First, the analysis models vaccination pace, the monthly increment, rather than the cumulative level, since it is the speed of uptake during the roll-out, not the slowly accumulating stock, that a contemporaneous communication campaign can plausibly influence. Second, all inference relies on standard errors clustered by country, which accommodates both the skewness and the serial dependence inherent in country trajectories.

The principal explanatory variable, the OxCGRT indicator of public information campaigns (H1), averages 1.87 on its zero-to-two ordinal scale (standard deviation 0.37). The distribution is not merely top-heavy but sits at a ceiling: 86 percent of country-months record exactly the maximum value of two, and in 78 of the 168 economies the index does not move at all during the window. This

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carries an important implication for interpretation: within-country variation in H1 is limited, and the campaign coefficients reported below are identified from the 90 economies in which the index does move, while the remaining economies inform the controls and the fixed effects. Identification of the relationships reported below therefore does not rest on campaign intensity in isolation, for which there is too little movement, but on how the effect of the available variation in campaigns differs across countries with systematically different communication infrastructures. Consistent with this, the raw bivariate correlation between campaign intensity and vaccination pace is negligible ($r \approx 0.08$): any systematic relationship emerges only once the moderating role of connectivity is considered. That infrastructure is proxied by pre-pandemic (2019) internet penetration which, in contrast to H1, varies widely across the sample: it averages 60.9 percent but spans 2.7 to 99.7 percent (standard deviation 27.9 points), reflecting the pronounced cross-country digital divide that this study places at the center of its argument. Containment stringency, included as a control for the broader intensity of the policy response, averages 36.8 on its zero-to-one-hundred scale.

The moderating role of pre-pandemic connectivity Table 3 presents the fixed-effects estimates, which absorb all time-invariant country characteristics and all common monthly shocks. Column 1, in which campaigns enter additively, returns a negative and significant coefficient on H1 ($\beta = -0.932$, $p < 0.001$). Read naively, this might suggest that more intensive public information campaigns retard vaccination, an implausible conclusion that instead signals the policy-responsive nature of the variable. Campaigns were not assigned at random; governments intensified them precisely where uptake was faltering, so the unconditional within-country association between campaigns and pace is contaminated by this targeting. The negative sign should therefore be read as a feature of how campaigns are deployed rather than as evidence of their effect, and it is reported here mainly to motivate the conditional specification that follows; column 4 returns to it directly.

Column 2 adds the interaction between campaigns and standardized pre-pandemic internet penetration. The interaction term is positive and precisely estimated ($\beta = 0.912$, $SE = 0.183$, $p < 0.001$), the stringency control enters with a small positive coefficient ($\beta = 0.076$, $p < 0.001$), and the R^2 reaches 0.323. Because the moderator is fixed at its 2019 value, it is time-invariant by construction: its level effect is absorbed exactly by the country fixed effects, and the coefficient of interest is a difference in slopes rather than a level effect. It indicates that the association between campaign intensity and vaccination pace becomes more favorable as baseline digital reach rises: each additional standard deviation of 2019 penetration, approximately 27.9 percentage points, is associated with an increase of about 0.91 points per month in the marginal relationship between a one-unit change in H1 and uptake. We deliberately frame this as an association rather than a causal effect, since the design identifies how the campaign–uptake relationship varies with connectivity, not the counterfactual response of uptake to an exogenous change in

Table 2. Descriptive statistics (analysis sample, 2021–2022)

Source: Authors' calculations based on OxCGRT, Our World in Data, and World Bank World Development Indicators. The 2019 baseline is the moderating variable in the main specification; the annual series is used in robustness. Baseline values for four economies are the latest available up to 2020.

Variable	N	Mean	SD	Min	Max
Δ fully vaccinated (pp/month)	3,410	2.51	4.42	0.00	52.52
H1 public information campaigns (0–2)	3,410	1.87	0.37	0.00	2.00
Internet penetration, 2019 baseline (%)	3,410	60.92	27.88	2.73	99.70
Internet penetration, annual 2021–2022 (%)	3,410	68.51	26.70	6.67	100.00
Stringency index (0–100)	3,410	36.78	21.05	0.00	93.52
New cases per million	3,410	6,730.99	15,911.44	0.00	178,643.95
New deaths per million	3,410	38.98	76.33	0.00	727.66
Cumulative deaths per million	3,410	1,036.67	1,175.91	0.00	6,512.45
Government effectiveness (WGI)	3,410	0.01	1.01	–2.40	2.25
Log GDP per capita	3,410	8.81	1.45	5.57	12.28
Political stability (WGI)	3,410	–0.12	0.97	–2.78	1.59

campaigns. A robustness check that instead uses the annual 2021–2022 series and estimates the contemporaneous main effect of connectivity explicitly, as hierarchy would otherwise require, returns an almost identical interaction (0.849, $p < 0.001$) alongside an imprecise main effect (2.38, SE 1.52, $p = 0.118$), reflecting how little identifying variation the within-country annual movement of connectivity provides (Table 5); the two designs thus agree, and the baseline specification is preferred because its exclusion of the main effect is exact rather than approximate.

Column 4 augments the model with the lagged vaccination level. The lag enters strongly negatively (−0.137, $p < 0.001$), consistent with saturation: uptake decelerates mechanically as accumulated coverage rises and the pool of unvaccinated adopters shrinks. Its inclusion attenuates the level coefficient on campaigns to statistical insignificance (−0.147, $p = 0.482$), while the interaction remains positive and significant (0.757, $p < 0.001$). The reading is twofold. Much of the unconditional negative association between campaigns and pace in columns 1–3 reflects the timing of campaigns relative to roll-out saturation – messaging persisted, and often intensified, in months when coverage was already high and pace was slowing for mechanical reasons – rather than any property of the communication itself. And the connectivity gradient in the campaign–uptake relationship is not an artefact of that timing, since it survives the saturation control essentially intact. Because a lagged outcome level combined with country fixed effects induces Nickell bias of order $1/T$ ($T = 24$), column 4 is treated as a bounding exercise rather than as the preferred estimate.

The substantive meaning of the interaction is clearest when the marginal effect of campaigns is traced across the 2019 connectivity distribution, reported at representative percentiles in Table 4 and as a continuous function in Figure 1, with the effect implied for each individual economy reported in Table C1 and visualized for a representative spread of economies, Moldova included, in Figure C1. At low connectivity, the tenth percentile corresponding to 18 percent penetration, the marginal effect of H1 is negative and strongly significant (−1.955, $p < 0.001$). At the median (69 percent) it is small and statistically indistinguishable from zero (−0.289, $p = 0.272$). At high connectivity, the ninetieth percentile at 93 percent, the point estimate is positive (+0.501) but does not attain conventional significance ($p = 0.153$). Two distinct thresholds organize Figure 1, and keeping them apart matters. The 95 percent confidence band ceases to exclude zero at about 62 percent

Table 3. Fixed-effects estimates of the campaign–vaccination relationship

Variable	(1)	(2)	(3)	(4)
H1 (public information campaigns)	−0.932*** (0.264)	−0.539* (0.251)	−0.501* (0.228)	−0.147 (0.209)
H1 × Internet penetration, 2019 (z)	–	0.912*** (0.183)	0.552 (0.386)	0.757*** (0.174)
H1 × Government effectiveness (z)	–	–	0.695 (0.407)	–
H1 × Log GDP per capita (z)	–	–	−0.091 (0.460)	–
Lagged vaccination level	–	–	–	−0.137*** (0.010)
Stringency	0.077*** (0.011)	0.076*** (0.011)	0.076*** (0.011)	0.051*** (0.011)
Government effectiveness (z)	1.902 (1.825)	1.915 (1.766)	0.556 (2.068)	−0.297 (1.519)
Log GDP per capita (z)	−6.177 (4.695)	−6.150 (4.706)	−5.229 (4.545)	−4.386 (3.840)
Political stability	1.631 (1.521)	1.419 (1.536)	1.415 (1.535)	2.444 (1.298)
Epidemiological controls (per million)	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Observations	3,410	3,410	3,410	3,410
R^2	0.320	0.323	0.324	0.400

Note: Dependent variable: monthly change in the fully vaccinated share (percentage points). Cluster-robust standard errors (by country) in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Epidemiological controls comprise new cases, new deaths and cumulative deaths per million; new-case incidence is negative and significant in all four columns ($p < 0.001$). The moderator is pre-pandemic (2019) internet penetration, time-invariant by construction and absorbed by the country fixed effects, entering only through the interaction; a specification with the annual series and its explicit main effect is reported in Table 5. Column 4 is a dynamic specification subject to Nickell bias of order $1/T$ ($T = 24$) and is read as a bounding exercise. In column 3 the three moderators are strongly correlated ($r = 0.77–0.90$) and their interactions are not separately identified; see Section 3.3.

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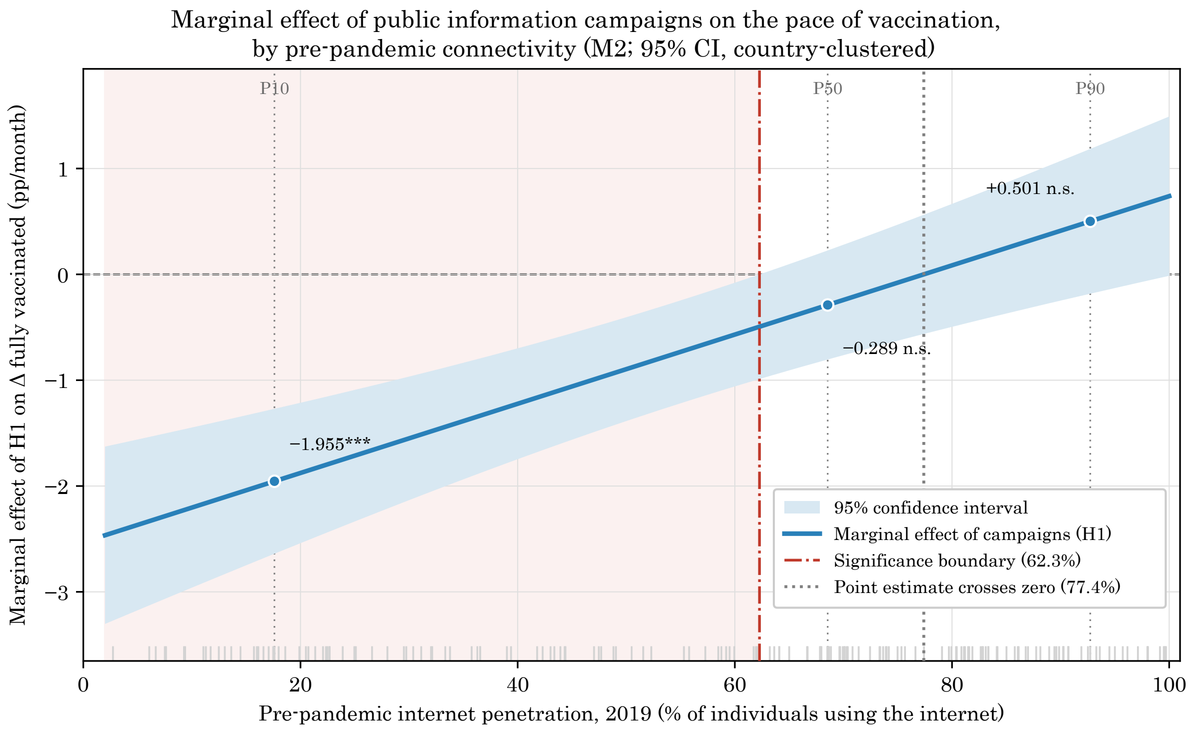
Table 4. Marginal effect of public information campaigns (H1) on vaccination pace across the 2019 internet-penetration distribution

Internet penetration, 2019	Marginal effect of H1	p-value
Low (10th percentile, 17.6%)	–1.955	< 0.001
Median (68.6%)	–0.289	0.272
High (90th percentile, 92.7%)	+0.501	0.153

Note: Marginal effects derived from column 2 of Table 3; delta-method standard errors clustered by country. Percentiles refer to the pre-pandemic (2019) baseline distribution.

penetration: below that level the negative association is statistically established; above it, the association is indistinguishable from zero. The point estimate itself crosses zero later, at about 77 percent, and the band never excludes zero from below anywhere in the observed range, so a positive return to campaigns is not statistically demonstrated at any connectivity level. The asymmetry of this pattern disciplines what can and cannot be claimed. The well-identified result is at the lower end: where connectivity is sparse, more intensive public messaging is not associated with faster vaccination and is, if anything, associated with slower uptake. Column 4 indicates that part of this negative association reflects saturation dynamics rather than the messaging itself, and the design cannot apportion the remainder among the policy targeting, supply, eligibility and trust factors discussed in Sections 2.5 and 4; we therefore read the low-connectivity association as a well-established descriptive regularity about where campaign intensity coincided with slow uptake, not as a communication penalty. The upper end is directionally consistent with a positive return to campaigns in highly connected populations, but the estimate is too imprecise to support a confident claim, and we refrain from asserting one. The contribution of the interaction is thus to establish that the as-

Source: Authors' estimation.



Note: The dash-dot line marks the connectivity level (62.3%) above which the confidence band no longer excludes zero; the dotted grey line marks the point at which the point estimate crosses zero (77.4%). Dotted vertical lines mark the 10th, 50th and 90th percentiles of the 2019 internet distribution; the rug indicates the country distribution.

Figure 1. Marginal effect of public information campaigns (H1) on vaccination pace across pre-pandemic (2019) internet penetration, with 95% confidence interval (column 2 of Table 3)

sociation between public information campaigns and vaccination pace is systematically ordered by pre-pandemic connectivity, with the statistically established part of the pattern concentrated below the 62-percent threshold.

3.2. Connectivity, state capacity and income as correlated moderators

A natural concern is that internet penetration merely proxies for broader administrative capacity or national income, both of which could independently shape communication effectiveness. These three dimensions are strongly correlated in the sample (baseline internet penetration with log GDP per capita, $r \approx 0.90$; with government effectiveness, $r \approx 0.77$; and log GDP with government effectiveness, $r \approx 0.86$); the full correlation matrix is reported in Table B1, and the strength of these correlations means that a horse race among the three interactions is a demanding test with limited power to discriminate between them. Column 3 reports that test. With all three interactions included, none is individually significant at the five percent level: the connectivity interaction falls to 0.552 (SE 0.386, $p = 0.155$), the governance interaction is 0.695 (SE 0.407, $p = 0.089$) and the income interaction is −0.091 (SE 0.460). The data therefore cannot apportion the moderation among connectivity, governance quality and income. What columns 2 and 3 jointly establish is that the campaign–uptake gradient runs along a tightly correlated bundle of development characteristics, of which pre-pandemic connectivity is one legible and policy-relevant marker – not that digital reach is the operative channel to the exclusion of administrative capacity or income. We accordingly do not claim moderator specificity: the article's framing of internet penetration as a contextual moderator, rather than as a measure of digital campaign exposure or a uniquely digital mechanism, reflects that restraint. The insignificance of the governance and income interactions in column 3 is likewise not evidence of their irrelevance, for the same collinearity reason.

3.3. Robustness and scope

The conditioning role of baseline connectivity survives a battery of alternative specifications (Table 5), as well as the exclusion of any single country (Figure D1). The interaction is essentially unchanged when the outcome is winsorized at the first and ninety-ninth percentiles to limit the influence of extreme monthly gains (0.750, $p < 0.001$), addressing the concern that the skewed dependent variable might be driven by outliers; an audit of the dependent variable shows no negative monthly increments anywhere in the sample – the underlying cumulative series is non-decreasing, so retrospective data revisions cannot masquerade as negative pace – and the winsorization therefore acts solely on the 1.1 percent of country-months with catch-up surges above twenty points. It is also robust to lagging campaign intensity by one month (0.631, $p = 0.024$); while this lag attenuates the most immediate form of simultaneity between messaging and uptake, we do not claim that it removes endogeneity entirely, since no exogenous source of variation in campaigns is available, and the estimates retain an associational interpretation throughout. The interaction is positive in both annual sub-samples – 1.615 ($p = 0.013$) in the 2021 roll-out year and 0.364 ($p = 0.059$) in 2022 – significant when vaccination was actively scaling and marginal once the roll-out matured, consistent with communication mattering most during active diffusion. Three further specification checks address the measurement of the two central variables. Re-estimating with the annual 2021–2022 connectivity series and its explicit contemporaneous main effect leaves the interaction essentially unchanged (0.849, $p < 0.001$; main effect 2.38, SE 1.52, n.s.). Replacing the campaign index with a binary indicator of its maximum category yields a similar interaction (0.862, $p < 0.001$), and replacing it with modal-category dummies shows the connectivity slope rising almost linearly with the category – the increment from category 0 to 1 (+0.77) is close to that from 1 to 2 (+0.81) – which supports the continuous coding of the ordinal index as an adequate approximation. Adding the vaccination-policy controls confirms that the pattern is not an artefact of eligibility or mandate timing: vaccine eligibility itself is a strong predictor of pace (1.96, $p < 0.001$), availability and mandates are insignificant, and the interaction is stable whether mandates are entered with missing values set to zero under an explicit no-mandate assumption (0.867, $p < 0.001$) or listwise, which

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drops 21.6 percent of country-months concentrated in early 2021 because OxCGRT coded the mandate indicator progressively (0.817, $p < 0.01$). Excluding the four economies whose baseline connectivity relies on a pre-2019 fallback likewise leaves the result intact (0.825, $p < 0.001$). Two further checks are reported not because they confirm the hypothesis but because they delimit it. When the cumulative vaccination level replaces its monthly change, the interaction is insignificant (–0.220, SE 1.126), as expected once the outcome is dominated by an accumulating, trend-like stock under fixed effects, which corroborates the decision to model pace. When COVID-19 mortality per million replaces vaccination as the outcome, the interaction is again insignificant (–0.102, SE 3.577), indicating that the connectivity-conditioned relationship is specific to an adoption behavior and does not extend mechanically to mortality, which is governed by a different and longer causal chain. Finally, a Hausman test rejects the random-effects specification in favor of fixed effects ($\chi^2(9) = 17.15$, $p = 0.046$), and the variance-inflation factors remain below the conventional threshold of ten (5.13 for government effectiveness, 7.97 for log GDP per capita); the interaction term itself, at 4.76, indicates that standardizing the moderator successfully contains the mechanical collinearity of interaction components.

3.4. Country-level illustrations: Moldova and Armenia

The two focal economies are presented as illustrations of the estimated pattern and of its limits, not as additional tests of it; the asymmetry between them – Moldova inside the panel, Armenia outside it – is a feature of the data and is stated so that the material is weighed accordingly. Moldova is represented in the panel but contributes essentially no within-country campaign variation: as Figure 2 shows, the country sustained the maximum recorded campaign intensity (H1 = 2) through most of 2020–2022 even as containment stringency rose and fell, so a Moldova-specific campaign slope cannot be estimated and the country informs the model mainly through its controls and fixed effects. Its fully vaccinated coverage plateaued at roughly 33 percent by early 2022, with nearly all of the cumulative gain concentrated in a single mid-2021 surge that peaked at about 7.8 points per month. Moldova's pre-pandemic (2019) internet penetration, at approximately 73 percent, places it well within the connected segment of the sample, and the model itself implies a near-zero campaign association at that connectivity level (–0.13, not significant; Table C1), so its stalled coverage is not what the connectivity gradient alone would predict. The stalling of cover-

Table 5. Robustness and scope checks (coefficient on the H1 × internet interaction)

Specification	H1 × Internet, 2019 (z)	Comment
Winsorized Δvax (1st/99th pct)	0.750*** (0.184)	Limits outlier influence
Lagged campaigns, H1(t–1)	0.631* (0.277)	Attenuates simultaneity
2021 sub-sample	1.615* (0.643)	Vaccine roll-out year
2022 sub-sample	0.364 (0.191)	Post-roll-out year; p = 0.059
Annual internet series + explicit main effect	0.849*** (0.174)	Main effect 2.38 (1.52), n.s.; hierarchy check
Binary campaign indicator, 1[H1 = 2]	0.862*** (0.196)	Ordinal-coding check
+ eligibility (H7) and availability (V2A) controls	0.869*** (0.189)	Vaccination-policy timing
+ mandates (V4), missing set to zero	0.867*** (0.189)	Explicit no-mandate assumption
+ mandates (V4), listwise (N = 2,675)	0.817** (0.247)	Structural missingness; –21.6% of sample
Excluding fallback-baseline economies (4)	0.825*** (0.180)	Pre-2019 baseline check
Outcome: cumulative vaccination level	–0.220 (1.126) n.s.	Effect operates on pace, not stock
Outcome: COVID-19 deaths per million	–0.102 (3.577) n.s.	Channel specific to adoption

Note: Each row reports the coefficient on the campaign × internet interaction (cluster-robust standard error by country in parentheses) under the stated specification. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; n.s. = not significant. Under two-way clustering by country and month the column-2 interaction of Table 3 has standard error 0.288 ($p = 0.004$); under Driscoll–Kraay standard errors robust to cross-sectional dependence, 0.302 ($p = 0.003$). Modal-category dummies are reported in the text. A Hausman test favors fixed over random effects ($\chi^2(9) = 17.15$, $p = 0.046$); variance-inflation factors are below ten throughout (government effectiveness 5.13; log GDP 7.97; interaction term 4.76). In the annual sub-samples the annual macro-institutional covariates are absorbed by the country fixed effects.

Source: Authors' elaboration based on OxCGRT and Our World in Data.

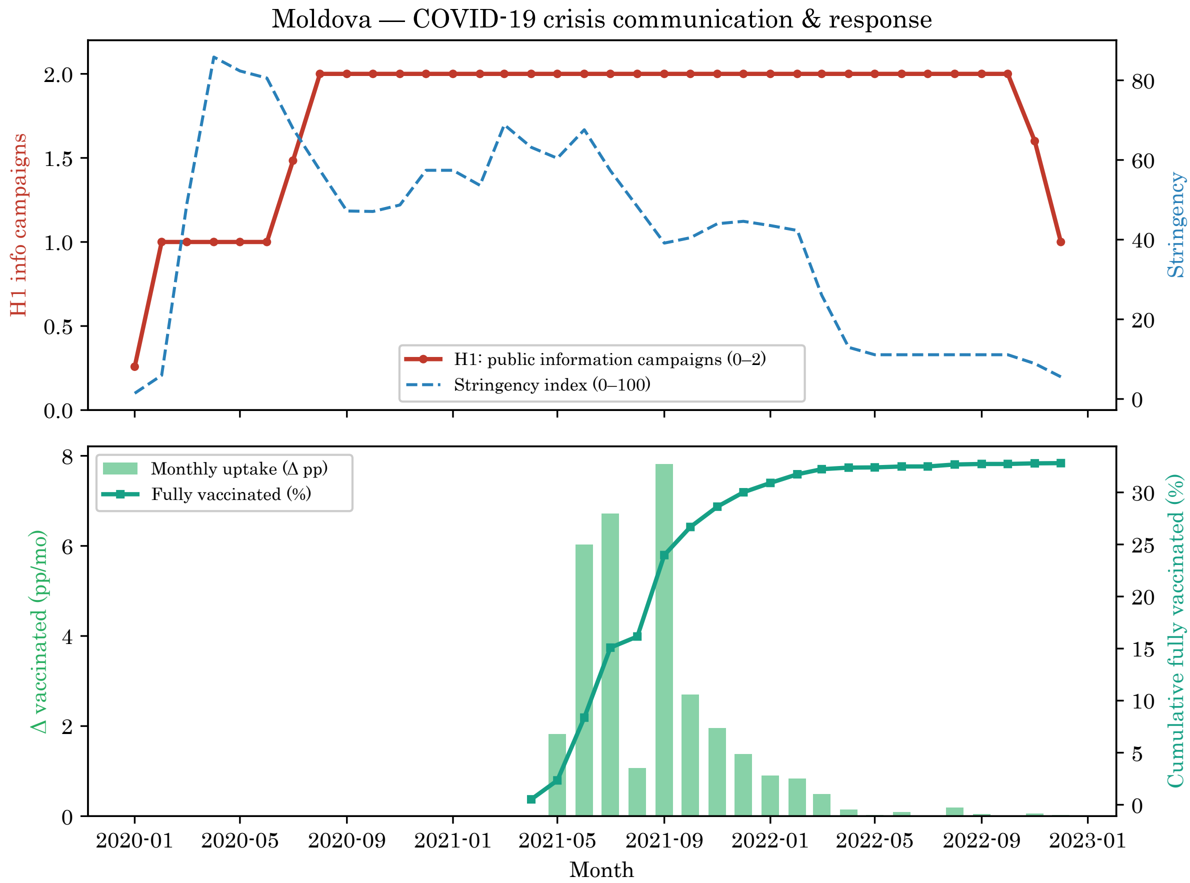

Figure 2. Moldova: COVID-19 crisis communication intensity (public information campaigns and containment stringency) and vaccination dynamics, 2020–2022

age despite both sustained messaging and adequate connectivity is therefore consistent with demand-side constraints such as hesitancy, trust deficits, or competing information, rather than with a shortage of channel reach. We present this as an illustrative reading rather than a formal test: a single national trajectory cannot identify the source of the plateau, but it usefully demonstrates that digital connectivity, while necessary for digitally delivered campaigns to reach audiences, is not sufficient to guarantee uptake.

Armenia enters the study only as a qualitative case because it is absent from the OxCGRT campaign data and therefore contributes no regression evidence; it cannot test the campaign-by-connectivity interaction, and no such test is claimed for it. Its value lies in contextualizing the conditions under which crisis communication operates. On the dimension central to this paper Armenia is, if anything, better equipped than Moldova, with e-government sub-indices of 0.79 to 0.88 and internet penetration of 81 percent (Figure 3); the digital-capacity snapshot in Figure 3 is drawn from the 2024 E-Government Survey and thus post-dates the estimation window, so it is read as a structural marker of the two countries' digital environments rather than as a contemporaneous measurement. Yet its pandemic communication did not unfold in isolation. As Figure 4 documents, the country experienced sustained civil unrest following the 2018 political transition (ACLED event data; Raleigh et al., 2010) and, more consequentially, armed conflict: the 2020 Nagorno-Karabakh war produced on the order of 6,000 recorded conflict deaths in the autumn of that year according to UCDP event data, with a further escalation in 2023. Public-health messaging in Armenia thus competed for institutional attention and public salience with the communication demands of war.

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Source: Authors' elaboration based on the UN E-Government Survey (EGDI) and World Bank World Development Indicators.

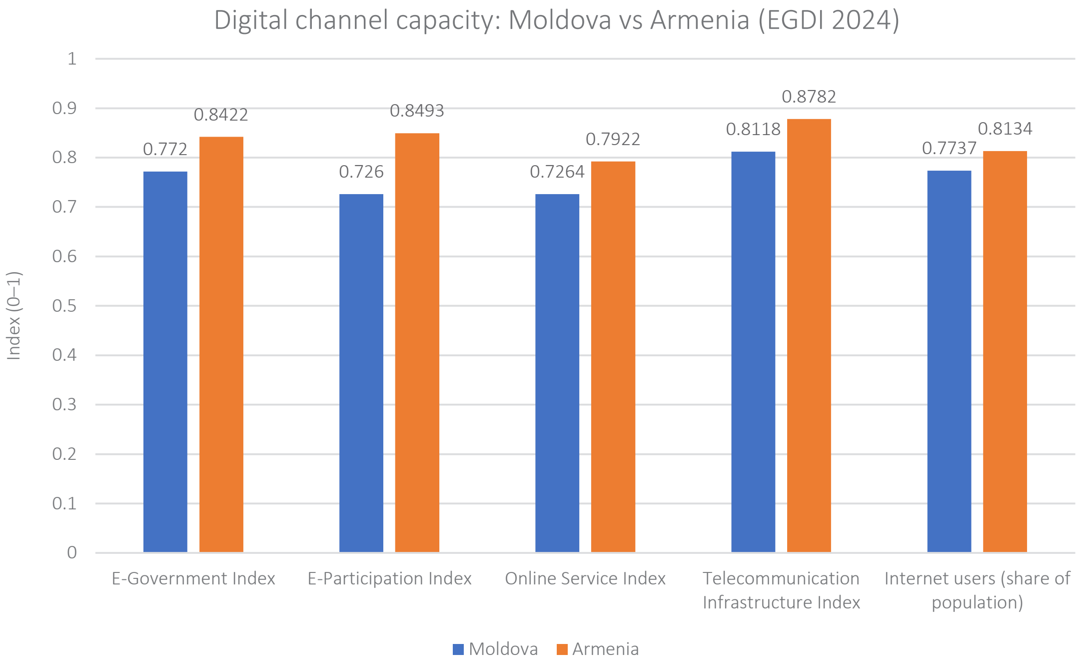

Figure 3. Digital channel capacity, Moldova versus Armenia: e-government sub-indices and internet penetration, 2024

Source: Authors' elaboration based on ACLED and the UCDP Georeferenced Event Dataset.

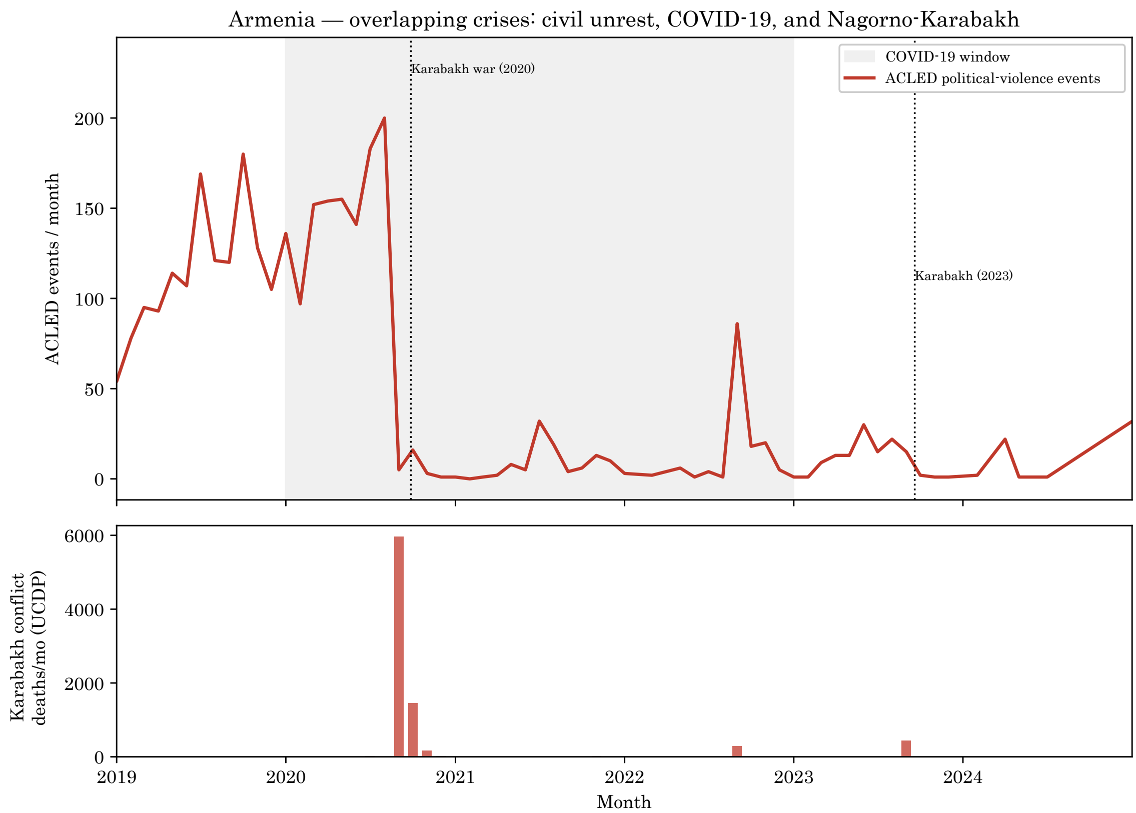

Figure 4. Armenia, overlapping crises: monthly political-violence events (upper panel) and Nagorno-Karabakh conflict fatalities (lower panel), 2019–2024, with the COVID-19 window shaded

Moldova, by contrast, confronted a severe but non-violent shock in 2022: as Figure 5 shows, consumer-price inflation surged to 28.7 percent and real GDP contracted by 4.6 percent amid an energy crisis and a large inflow of refugees, whereas Armenia's economy expanded by 12.6 percent over the same year. The juxtaposition is instructive precisely because the two countries possess comparably strong digital infrastructure: it suggests that the effectiveness of crisis communication is shaped not only by the digital environment available to it but by the number and nature of concurrent crises competing for public attention, a dimension that the cross-country panel, anchored on a single common shock, cannot capture but that the case evidence brings into view. Taken together, the quantitative and qualitative evidence supports a narrow but well-grounded claim: across the global sample, the association between public information campaigns and the pace of vaccination is systematically ordered by pre-pandemic connectivity, with the statistically established part of the pattern being the negative association where connectivity is low, while the country cases show that even ample connectivity does not assure uptake and that overlapping crises further condition how communication performs. The evidence does not establish that campaigns cause faster vaccination, nor that high connectivity guarantees a positive return, and these boundaries are stated explicitly so that the findings are not read beyond what the design can bear.

Source: Authors' elaboration based on World Bank World Development Indicators.

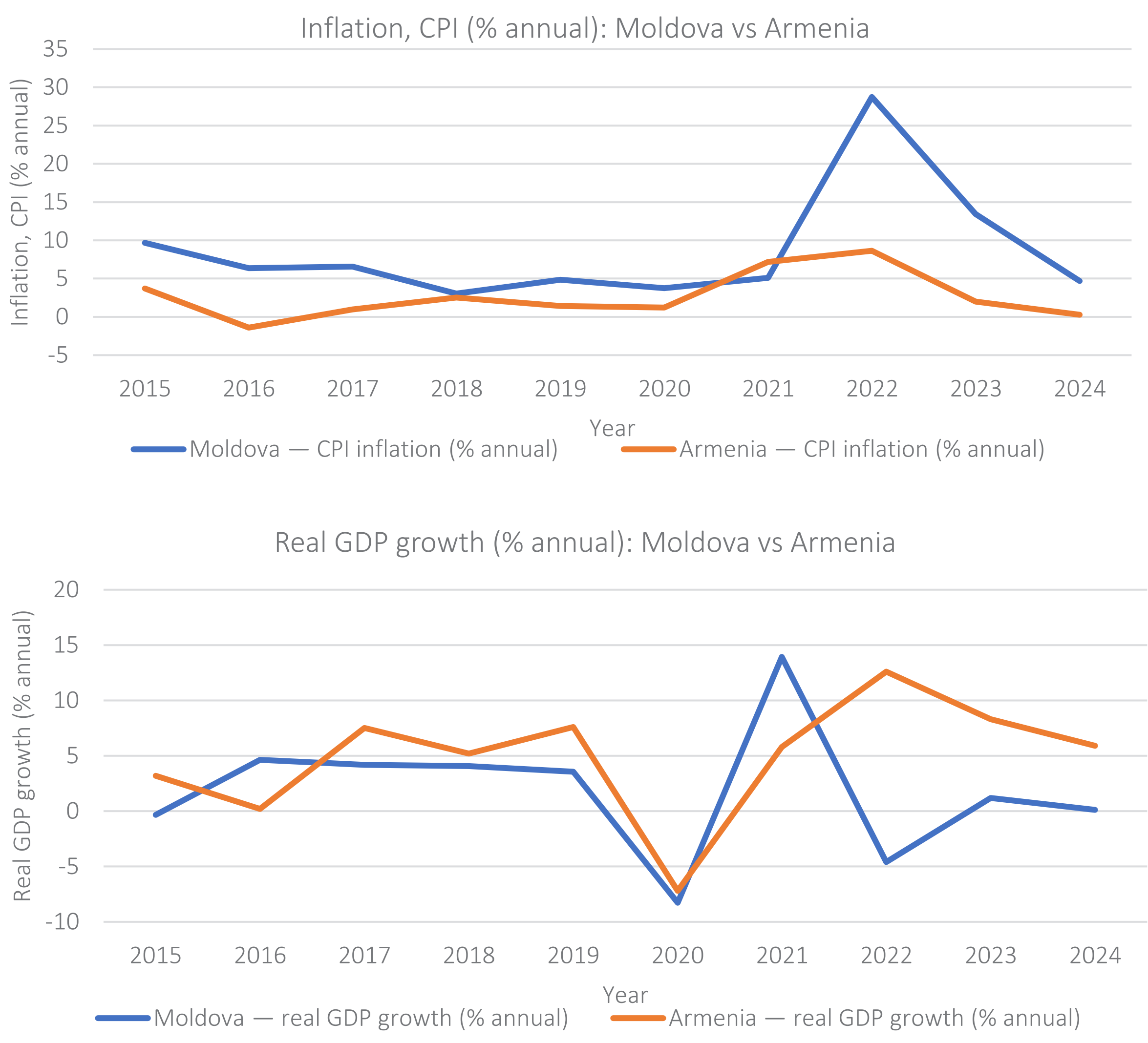

Figure 5. Moldova's 2022 economic shock: consumer-price inflation (upper panel) and real GDP growth (lower panel), Moldova versus Armenia, 2015–2024

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4. DISCUSSION

Broadly, the association between public information campaigns and the pace of uptake is ordered by the connectivity of the societies addressed, but the evidence does not license attributing that ordering to connectivity as distinct from the development bundle it indexes. The finding extends to crisis communication evidence that digital infrastructure non-linearly shapes public-spending efficiency (Hrytsenko et al., 2026), and it is complementary rather than contrary to accounts locating pandemic resilience in health-system funding and organization (Kuzior, Vasylieva et al., 2023): once campaigns are interacted simultaneously with connectivity, governance quality and income, none of the three moderators is separately significant, which is what their tight correlation would produce even if all three mattered. The results therefore identify a robust conditional pattern along a bundle of development characteristics, of which pre-pandemic connectivity is the most legible and policy-relevant marker, rather than isolating a uniquely digital bottleneck; structural and communicative capacity remain distinct, non-substitutable conditions for effective response.

The best-identified part of the pattern, the negative association between campaign intensity and uptake where connectivity is low, admits several readings that the design cannot separate. One is compositional: the dynamic specification indicates that part of the unconditional negative coefficient reflects campaigns persisting into months when uptake was already decelerating for saturation reasons. Another is informational, and it connects the result to the infodemic literature: where official messaging penetrates weakly, the information environment fills with localized misinformation eroding uptake (Penka et al., 2024), weakly governed digital spaces suffer disproportionately (Bappayo, Abubakar et al., 2026), and infodemics travel as non-linear waves under-resourced campaigns cannot suppress (Zahorodnia et al., 2026). The absence of a statistically significant positive association even at the highest connectivity levels echoes meta-analytic evidence that digital campaigns shift knowledge and attitudes but rarely behavior without offline support (Nazari et al., 2026); reach alone does not manufacture behavioral change. On this reading the low-connectivity association marks not harm from communication but the failure of official messaging to displace a rival information environment thriving where reach is thin, yet it remains one candidate mechanism among the policy-targeting, supply and trust channels listed with the limitations.

Time-sensitive checks – lagging campaigns and estimating each year separately – accord with work showing that responses to crisis communication evolve dynamically and resist cross-sectional treatment (Abdelzadeh & Sedelius, 2024); the interaction is strong during the 2021 roll-out and only marginal once uptake had saturated in 2022, consistent with the conditional association mattering most while there is behavior left to move. The Moldova–Armenia contrast – comparably connected societies facing different concurrent emergencies – illustrates how overlapping crises compete with public messaging for attention and bandwidth, regional conflict especially straining adaptive capacity (Hakobyan & Margaryan, 2025). Read together, the panel and the cases reconcile the literature's mixed verdict into a connectivity-contingent reading: the negative association is confined to poorly connected settings and fades as connectivity rises, while nothing in the data demonstrates a positive return to campaigns even where connectivity is nearly universal.

CONCLUSION

The study asked whether the association between public information campaigns and the pace of COVID-19 vaccination varies with the internet penetration of the societies addressed, examining 168 economies (2021–2022, N = 3,410) and two transition-economy cases. Two-way fixed-effects models interacted campaign intensity (OxCGRT) with pre-pandemic internet penetration (World Bank) on monthly uptake (Our World in Data), clustering by country. The interaction is positive and robust (0.912, $p < 0.001$), and the marginal association of campaigns is significantly negative at low connectivity (–1.955 at the tenth percentile, $p < 0.001$), indistinguishable from zero at median and high, and never significantly positive;

the confidence band excludes zero only below about 62 percent penetration, so the firm result is a negative association under low connectivity, not a demonstrated benefit under high. The pattern survives winsorizing, lagging, and separate yearly estimation, and vaccination-policy controls, but it cannot be attributed to connectivity as distinct from income and governance, with which baseline penetration is correlated at 0.77–0.90; the estimates are associational throughout, and the comparably connected cases of Moldova and Armenia illustrate crisis communication competing with parallel emergencies.

For practice, the descriptive regularity is directionally clear even where its mechanism is not: where connectivity is thin, intensified messaging was not accompanied by faster uptake and often coincided with slower uptake, so online-centered campaigns cannot be presumed to change behaviour there; authorities should pair digital campaigns with offline and community channels, target low-connectivity populations explicitly, and treat closing the connectivity gap as part of crisis-communication preparedness, not a separate development goal.

For international organizations and transition economies, audience connectivity should be planned as a precondition of digitally delivered crisis communication, not assumed. Investment in connectivity ahead of crises, with coordination that prevents health messaging from being crowded out where emergencies overlap – as Moldova and Armenia illustrate – would strengthen campaigns' capacity to convert guidance into protective behavior. Yet connectivity is necessary, not sufficient: Moldova paired ample connectivity and sustained campaigns with stalled uptake, and the model itself implies a near-zero campaign association at Moldova's connectivity (–0.13, not significant), so closing the connectivity gap removes a documented negative association without ensuring behavioral change where hesitancy or distrust persists. Future research could add direct measures of message exposure and digital literacy, and exploit exogenous variation in campaign timing, moving from association toward mechanism.

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DATA AVAILABILITY

All data used in this study are publicly available. Public information campaign intensity and the stringency index are from the Oxford COVID-19 Government Response Tracker (Hale et al., 2021); the vaccination and epidemiological series are from Our World in Data (Mathieu et al., 2021); internet penetration and GDP per capita are from the World Bank World Development Indicators; and government effectiveness and political stability are from the World Bank Worldwide Governance Indicators.

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APPENDIX A. Countries in the estimation sample

The estimation sample comprises 168 economies observed monthly over 2021–2022. Connectivity group denotes terciles of pre-pandemic (2019) internet penetration, the dimension along which the moderation operates; it is a descriptive grouping only, since the models use internet penetration as a continuous moderator. Income group follows the World Bank income classification. Armenia is absent from the public-information-campaign data and is examined as a qualitative case only; it is therefore not listed here.

Table A1. Economies in the estimation sample, with income group, pre-pandemic (2019) internet penetration, and connectivity group

Country	Income group	Internet users, 2019, %	Connectivity group
Afghanistan	Low	17.6	Low
Albania	Upper-middle	68.6	Medium
Algeria	Upper-middle	55.8	Medium
Andorra	High	90.7	High
Angola	Lower-middle	32.1	Low
Argentina	Upper-middle	79.9	High
Australia	High	93.6	High
Austria	High	87.8	High
Azerbaijan	Upper-middle	81.1	High
Bahamas	High	90.7	High
Bahrain	High	99.7	High
Bangladesh	Lower-middle	30.4	Low
Barbados	High	70.3	Medium
Belarus	Upper-middle	82.8	High
Belgium	High	90.3	High
Belize	Upper-middle	58.8	Medium
Benin	Lower-middle	20.5	Low
Bhutan	Lower-middle	74.2	Medium
Bolivia	Lower-middle	47.5	Medium
Bosnia and Herzegovina	Upper-middle	69.9	Medium
Botswana	Upper-middle	48.8	Medium
Brazil	Upper-middle	73.9	Medium
Brunei Darussalam	High	95.0	High
Bulgaria	Upper-middle	67.9	Medium
Burkina Faso	Low	9.3	Low
Burundi	Low	6.1	Low
Cabo Verde	Lower-middle	61.9	Medium
Cambodia	Lower-middle	52.3	Medium
Cameroon	Lower-middle	36.5	Low
Canada	High	91.9	High
Central African Republic	Low	7.5	Low
Chad	Low	7.6	Low
Chile	High	85.0	High
China	Upper-middle	64.1	Medium
Colombia	Upper-middle	65.0	Medium
Congo, Rep.	Lower-middle	29.7	Low
Costa Rica	Upper-middle	81.2	High
Croatia	High	79.1	High
Cuba	Upper-middle	68.0	Medium
Cyprus	High	86.1	High
Czechia	High	80.9	High
Côte d'Ivoire	Lower-middle	36.3	Low
DR Congo	Low	13.0	Low

Table A1 (cont.). Economies in the estimation sample, with income group, pre-pandemic (2019) internet penetration, and connectivity group

Country	Income group	Internet users, 2019, %	Connectivity group
Denmark	High	98.0	High
Djibouti	Lower-middle	58.5	Medium
Dominica	Upper-middle	73.4	Medium
Dominican Republic	Upper-middle	79.7	High
Ecuador	Upper-middle	59.2	Medium
Egypt	Lower-middle	57.3	Medium
El Salvador	Lower-middle	50.5	Medium
Estonia	High	90.2	High
Eswatini	Lower-middle	43.4	Low
Ethiopia	Low	16.0	Low
Fiji	Upper-middle	63.2	Medium
Finland	High	89.6	High
France	High	83.3	High
Gabon	Upper-middle	59.5	Medium
Gambia	Low	31.1	Low
Georgia	Upper-middle	68.8	Medium
Germany	High	88.1	High
Ghana	Lower-middle	22.1	Low
Greece	High	75.7	Medium
Guatemala	Lower-middle	29.5	Low
Guinea	Low	23.9	Low
Guyana	High	55.3	Medium
Haiti	Lower-middle	35.8	Low
Honduras	Lower-middle	39.4	Low
Hungary	High	80.4	High
Iceland	High	99.5	High
India	Lower-middle	44.4	Medium
Indonesia	Lower-middle	47.7	Medium
Iran	Upper-middle	72.5	Medium
Iraq	Lower-middle	44.3	Medium
Ireland	High	87.0	High
Israel	High	86.8	High
Italy	High	67.9	Medium
Jamaica	Upper-middle	70.8	Medium
Japan	High	92.7	High
Jordan	Lower-middle	70.1	Medium
Kazakhstan	Upper-middle	81.9	High
Kenya	Lower-middle	22.7	Low
Kiribati	Lower-middle	41.8	Low
Korea, Rep.	High	96.2	High
Kuwait	High	99.5	High
Kyrgyz Republic	Lower-middle	64.1	Medium
Lao PDR	Lower-middle	47.0	Medium
Latvia	High	86.1	High
Lebanon	Upper-middle	81.5	High
Lesotho	Low	42.3	Low
Liberia	Low	19.9	Low
Libya	Upper-middle	71.4	Medium
Lithuania	High	81.6	High
Luxembourg	High	97.1	High
Madagascar	Low	9.4	Low
Malawi	Low	11.1	Low

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Table A1 (cont.). Economies in the estimation sample, with income group, pre-pandemic (2019) internet penetration, and connectivity group

Country	Income group	Internet users, 2019, %	Connectivity group
Malaysia	Upper-middle	84.2	High
Mali	Low	25.0	Low
Malta	High	85.8	High
Mauritania	Lower-middle	25.1	Low
Mauritius	Upper-middle	61.7	Medium
Mexico	Upper-middle	69.6	Medium
Moldova	Lower-middle	73.3	Medium
Monaco	High	98.0	High
Morocco	Lower-middle	84.1	High
Mozambique	Low	12.5	Low
Myanmar	Lower-middle	36.5	Low
Nepal	Low	33.3	Low
Netherlands	High	93.3	High
New Zealand	High	89.9	High
Nicaragua	Lower-middle	39.0	Low
Niger	Low	11.3	Low
Nigeria	Lower-middle	16.1	Low
Norway	High	98.0	High
Oman	High	90.3	High
Pakistan	Lower-middle	17.1	Low
Panama	High	63.6	Medium
Papua New Guinea	Lower-middle	13.6	Low
Paraguay	Upper-middle	68.5	Medium
Peru	Upper-middle	60.0	Medium
Philippines	Lower-middle	43.0	Low
Poland	High	80.4	High
Portugal	High	75.3	Medium
Qatar	High	99.7	High
Romania	Upper-middle	73.7	Medium
Russian Federation	Upper-middle	82.6	High
Rwanda	Low	21.4	Low
San Marino	High	67.9	Medium
Saudi Arabia	High	95.7	High
Senegal	Lower-middle	43.9	Low
Serbia	Upper-middle	77.4	High
Seychelles	High	76.6	Medium
Sierra Leone	Low	15.7	Low
Singapore	High	88.9	High
Slovak Republic	High	82.9	High
Slovenia	High	83.1	High
Solomon Islands	Lower-middle	22.5	Low
Somalia	Low	2.7	Low
South Africa	Upper-middle	69.7	Medium
South Sudan	Low	6.7	Low
Spain	High	90.7	High
Sri Lanka	Lower-middle	32.0	Low
Sudan	Low	18.6	Low
Suriname	Upper-middle	62.0	Medium
Sweden	High	94.5	High
Syrian Arab Republic	Low	33.8	Low
Tajikistan	Lower-middle	22.7	Low
Tanzania	Low	16.6	Low
Thailand	Upper-middle	66.7	Medium

Table A1 (cont.). Economies in the estimation sample, with income group, pre-pandemic (2019) internet penetration, and connectivity group

Country	Income group	Internet users, 2019, %	Connectivity group
Timor-Leste	Lower-middle	28.0	Low
Togo	Low	20.7	Low
Tonga	Upper-middle	51.6	Medium
Trinidad and Tobago	High	75.0	Medium
Tunisia	Lower-middle	66.7	Medium
Turkmenistan	Upper-middle	18.0	Low
Türkiye	High	74.0	Medium
Uganda	Low	11.7	Low
Ukraine	Lower-middle	70.1	Medium
United Arab Emirates	High	99.1	High
United Kingdom	High	92.5	High
United States	High	89.4	High
Uruguay	High	83.4	High
Uzbekistan	Lower-middle	70.4	Medium
Vanuatu	Lower-middle	22.4	Low
Venezuela	Lower-middle	49.1	Medium
Vietnam	Lower-middle	68.7	Medium
Yemen	Low	17.5	Low
Zambia	Lower-middle	14.5	Low
Zimbabwe	Lower-middle	26.6	Low

Note: N = 168 economies. Income group is the World Bank income classification. Internet users, 2019, % is the pre-pandemic share of individuals using the internet, the moderating variable of the main specification; for Sudan, the Syrian Arab Republic, Turkmenistan and Vanuatu it is the latest value available up to 2020 (2017, 2018, 2016 and 2015, respectively). Connectivity group divides the economies into terciles of this distribution.

APPENDIX B. Correlation matrix

Pearson correlations are computed on the estimation sample (N = 3,410; 168 economies, 2021–2022). Variables are defined in Table 1; internet penetration is the pre-pandemic (2019) baseline used as the moderator in the main specification. The strong correlations of baseline internet penetration with log GDP per capita (0.90), government effectiveness (0.77) and political stability (0.61) motivate the standardization of these regressors, the confound-augmented specification reported in the results, and the bundle interpretation adopted in Section 3.3.

Table B1. Pearson correlations among the model variables (estimation sample, N = 3,410)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. ΔVax (dependent)	1.00									
2. H1 (campaigns)	0.08	1.00								
3. Internet penetration, 2019	0.12	0.22	1.00							
4. Stringency	0.38	0.30	0.05	1.00						
5. New cases p.m.	−0.05	0.11	0.32	0.04	1.00					
6. New deaths p.m.	0.13	0.14	0.27	0.31	0.36	1.00				
7. Cumulative deaths p.m.	−0.06	0.13	0.44	−0.16	0.20	0.36	1.00			
8. Government effectiveness	0.13	0.21	0.77	0.02	0.36	0.18	0.27	1.00		
9. Log GDP per capita	0.12	0.23	0.90	0.05	0.37	0.24	0.42	0.86	1.00	
10. Political stability	0.12	0.20	0.61	0.03	0.28	0.18	0.25	0.78	0.70	1.00

Note: Internet penetration is the 2019 baseline (time-invariant; latest available up to 2020 for four economies). Correlations involving time-invariant or annual variables mix cross-sectional and limited temporal variation and are reported for descriptive purposes.

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APPENDIX C. Country-implied marginal effects of campaigns

Appendix C reports the marginal effect of public information campaigns implied by the main model (M2) when its campaign-by-connectivity interaction is evaluated at each country's pre-pandemic (2019) level of internet penetration. These are model-implied effects, ME = $\beta_1 + \beta_2 \times$ (standardised 2019 internet penetration), with 95% confidence intervals from the country-clustered covariance matrix; they are not separately estimated country coefficients, which the data – twenty-four months, with the campaign index near its ceiling in most country-months – cannot identify. The implied effect is significantly negative in 77 of the 168 economies, those with the lowest baseline connectivity, and is statistically indistinguishable from zero in the remaining 91; it is significantly positive in none. Moldova, the focal in-panel country, lies in the non-significant range (2019 internet penetration ≈ 73 percent; implied effect −0.13), consistent with its low vaccination uptake reflecting factors other than the reach of digital channels. Figure C1 shows a representative spread of economies, and Table C1 reports the implied effect for all 168.

Source: authors' estimation

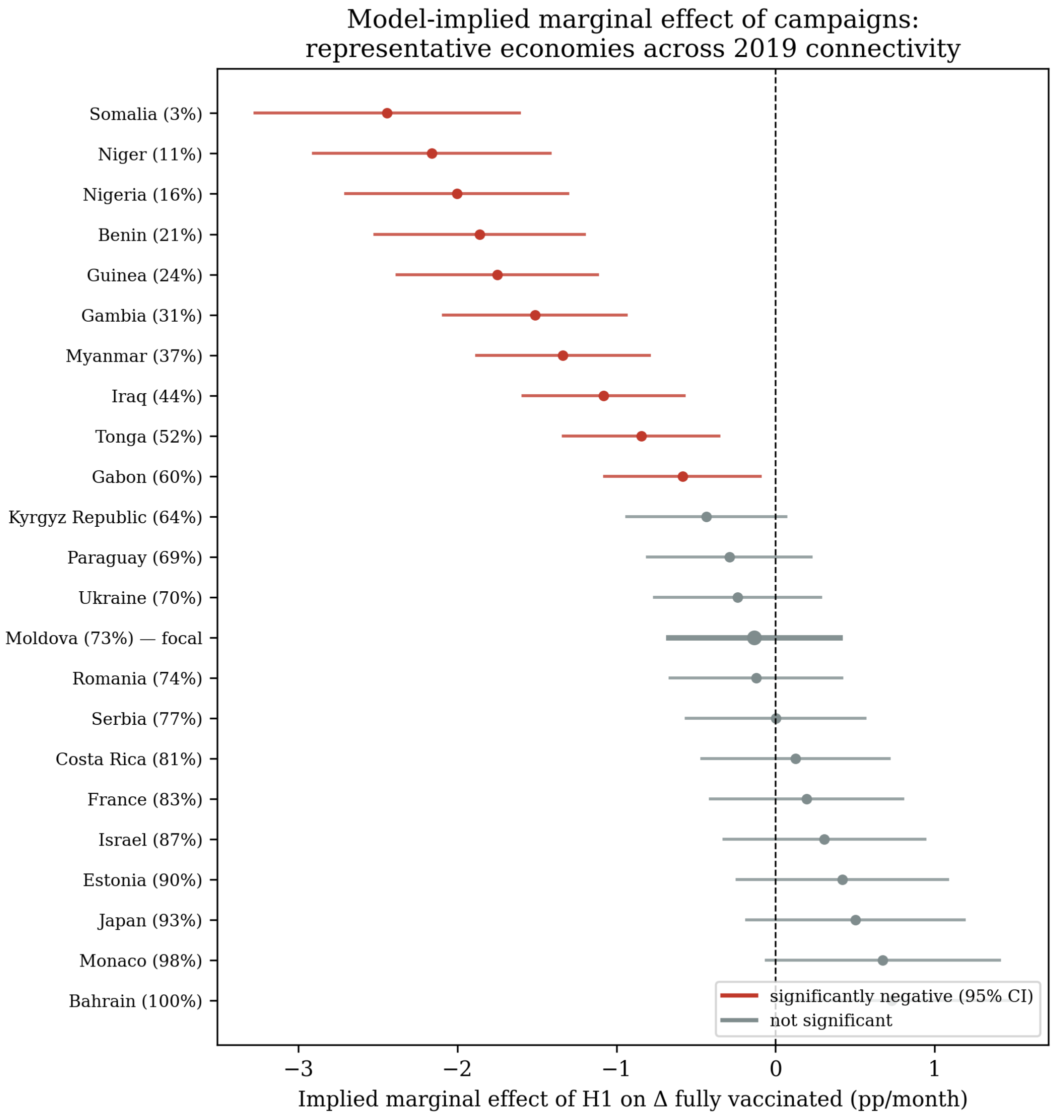

Figure C1. Model-implied marginal effect of campaigns for a representative spread of economies across the 2019 connectivity distribution (23 economies, every eighth by connectivity rank; Moldova highlighted). Point estimates with 95% confidence intervals from the country-clustered covariance matrix; red = significantly negative, grey = not significant

Table C1. Model-implied marginal effect of campaigns, all economies

Country	Internet 2019, %	Implied ME (pp)	95% CI	Significance
Somalia	2.7	–2.44	[–3.27, –1.61]	significant (–)
Burundi	6.1	–2.33	[–3.13, –1.54]	significant (–)
South Sudan	6.7	–2.31	[–3.10, –1.52]	significant (–)
Central African Republic	7.5	–2.29	[–3.07, –1.50]	significant (–)
Chad	7.6	–2.28	[–3.06, –1.50]	significant (–)
Burkina Faso	9.3	–2.23	[–2.99, –1.46]	significant (–)
Madagascar	9.4	–2.22	[–2.99, –1.46]	significant (–)
Malawi	11.1	–2.17	[–2.92, –1.42]	significant (–)
Niger	11.3	–2.16	[–2.91, –1.42]	significant (–)
Uganda	11.7	–2.15	[–2.89, –1.41]	significant (–)
Mozambique	12.5	–2.12	[–2.85, –1.39]	significant (–)
DR Congo	13.0	–2.10	[–2.83, –1.38]	significant (–)
Papua New Guinea	13.6	–2.09	[–2.81, –1.36]	significant (–)
Zambia	14.5	–2.06	[–2.77, –1.34]	significant (–)
Sierra Leone	15.7	–2.02	[–2.72, –1.32]	significant (–)
Ethiopia	16.0	–2.01	[–2.71, –1.31]	significant (–)
Nigeria	16.1	–2.00	[–2.70, –1.31]	significant (–)
Tanzania	16.6	–1.99	[–2.68, –1.29]	significant (–)
Pakistan	17.1	–1.97	[–2.66, –1.28]	significant (–)
Yemen	17.5	–1.96	[–2.64, –1.27]	significant (–)
Afghanistan	17.6	–1.96	[–2.64, –1.27]	significant (–)
Turkmenistan	18.0	–1.94	[–2.62, –1.26]	significant (–)
Sudan	18.6	–1.92	[–2.60, –1.25]	significant (–)
Liberia	19.9	–1.88	[–2.54, –1.22]	significant (–)
Benin	20.5	–1.86	[–2.52, –1.20]	significant (–)
Togo	20.7	–1.85	[–2.51, –1.20]	significant (–)
Rwanda	21.4	–1.83	[–2.48, –1.18]	significant (–)
Ghana	22.1	–1.81	[–2.45, –1.16]	significant (–)
Vanuatu	22.4	–1.80	[–2.44, –1.16]	significant (–)
Solomon Islands	22.5	–1.80	[–2.44, –1.15]	significant (–)
Kenya	22.7	–1.79	[–2.43, –1.15]	significant (–)
Tajikistan	22.7	–1.79	[–2.43, –1.15]	significant (–)
Guinea	23.9	–1.75	[–2.38, –1.12]	significant (–)
Mali	25.0	–1.71	[–2.34, –1.09]	significant (–)
Mauritania	25.1	–1.71	[–2.33, –1.09]	significant (–)
Zimbabwe	26.6	–1.66	[–2.27, –1.05]	significant (–)
Timor-Leste	28.0	–1.61	[–2.21, –1.02]	significant (–)
Guatemala	29.5	–1.57	[–2.15, –0.98]	significant (–)
Congo, Rep.	29.7	–1.56	[–2.14, –0.97]	significant (–)
Bangladesh	30.4	–1.54	[–2.12, –0.96]	significant (–)
Gambia	31.1	–1.51	[–2.09, –0.94]	significant (–)
Sri Lanka	32.0	–1.48	[–2.05, –0.91]	significant (–)
Angola	32.1	–1.48	[–2.05, –0.91]	significant (–)
Nepal	33.3	–1.44	[–2.00, –0.88]	significant (–)
Syrian Arab Republic	33.8	–1.43	[–1.98, –0.87]	significant (–)
Haiti	35.8	–1.36	[–1.91, –0.82]	significant (–)
Côte d'Ivoire	36.3	–1.34	[–1.89, –0.80]	significant (–)
Cameroon	36.5	–1.34	[–1.88, –0.79]	significant (–)
Myanmar	36.5	–1.34	[–1.88, –0.79]	significant (–)
Nicaragua	39.0	–1.26	[–1.78, –0.73]	significant (–)
Honduras	39.4	–1.24	[–1.77, –0.72]	significant (–)
Kiribati	41.8	–1.16	[–1.68, –0.65]	significant (–)
Lesotho	42.3	–1.15	[–1.66, –0.63]	significant (–)
Philippines	43.0	–1.12	[–1.63, –0.61]	significant (–)
Eswatini	43.4	–1.11	[–1.62, –0.60]	significant (–)
Senegal	43.9	–1.09	[–1.60, –0.59]	significant (–)

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Table C1 (cont.). Model-implied marginal effect of campaigns, all economies

Country	Internet 2019, %	Implied ME (pp)	95% CI	Significance
Iraq	44.3	–1.08	[–1.59, –0.58]	significant (–)
India	44.4	–1.08	[–1.58, –0.57]	significant (–)
Lao PDR	47.0	–0.99	[–1.49, –0.50]	significant (–)
Bolivia	47.5	–0.98	[–1.47, –0.48]	significant (–)
Indonesia	47.7	–0.97	[–1.47, –0.48]	significant (–)
Botswana	48.8	–0.93	[–1.43, –0.44]	significant (–)
Venezuela	49.1	–0.93	[–1.42, –0.43]	significant (–)
El Salvador	50.5	–0.88	[–1.37, –0.39]	significant (–)
Tonga	51.6	–0.84	[–1.33, –0.36]	significant (–)
Cambodia	52.3	–0.82	[–1.31, –0.33]	significant (–)
Guyana	55.3	–0.72	[–1.21, –0.24]	significant (–)
Algeria	55.8	–0.71	[–1.19, –0.22]	significant (–)
Egypt	57.3	–0.66	[–1.14, –0.17]	significant (–)
Djibouti	58.5	–0.62	[–1.11, –0.13]	significant (–)
Belize	58.8	–0.61	[–1.10, –0.12]	significant (–)
Ecuador	59.2	–0.60	[–1.08, –0.11]	significant (–)
Gabon	59.5	–0.58	[–1.07, –0.10]	significant (–)
Peru	60.0	–0.57	[–1.06, –0.08]	significant (–)
Mauritius	61.7	–0.51	[–1.01, –0.02]	significant (–)
Cabo Verde	61.9	–0.51	[–1.00, –0.01]	significant (–)
Suriname	62.0	–0.50	[–1.00, –0.01]	significant (–)
Fiji	63.2	–0.46	[–0.96, 0.03]	not significant
Panama	63.6	–0.45	[–0.95, 0.05]	not significant
China	64.1	–0.44	[–0.93, 0.06]	not significant
Kyrgyz Republic	64.1	–0.43	[–0.93, 0.06]	not significant
Colombia	65.0	–0.41	[–0.91, 0.10]	not significant
Thailand	66.7	–0.35	[–0.86, 0.16]	not significant
Tunisia	66.7	–0.35	[–0.86, 0.16]	not significant
San Marino	67.9	–0.31	[–0.82, 0.20]	not significant
Italy	67.9	–0.31	[–0.82, 0.20]	not significant
Bulgaria	67.9	–0.31	[–0.82, 0.20]	not significant
Cuba	68.0	–0.31	[–0.82, 0.20]	not significant
Paraguay	68.5	–0.29	[–0.81, 0.22]	not significant
Albania	68.6	–0.29	[–0.80, 0.23]	not significant
Vietnam	68.7	–0.29	[–0.80, 0.23]	not significant
Georgia	68.8	–0.28	[–0.80, 0.24]	not significant
Mexico	69.6	–0.25	[–0.77, 0.27]	not significant
South Africa	69.7	–0.25	[–0.77, 0.27]	not significant
Bosnia and Herzegovina	69.9	–0.24	[–0.77, 0.28]	not significant
Jordan	70.1	–0.24	[–0.76, 0.28]	not significant
Ukraine	70.1	–0.24	[–0.76, 0.28]	not significant
Barbados	70.3	–0.23	[–0.76, 0.29]	not significant
Uzbekistan	70.4	–0.23	[–0.75, 0.29]	not significant
Jamaica	70.8	–0.21	[–0.74, 0.31]	not significant
Libya	71.4	–0.20	[–0.72, 0.33]	not significant
Iran	72.5	–0.16	[–0.70, 0.37]	not significant
Moldova (focal case)	73.3	–0.13	[–0.67, 0.41]	not significant
Dominica	73.4	–0.13	[–0.67, 0.41]	not significant
Romania	73.7	–0.12	[–0.66, 0.42]	not significant
Brazil	73.9	–0.11	[–0.66, 0.43]	not significant
Türkiye	74.0	–0.11	[–0.65, 0.43]	not significant
Bhutan	74.2	–0.11	[–0.65, 0.44]	not significant
Trinidad and Tobago	75.0	–0.08	[–0.63, 0.47]	not significant
Portugal	75.3	–0.07	[–0.62, 0.48]	not significant
Greece	75.7	–0.06	[–0.61, 0.50]	not significant
Seychelles	76.6	–0.03	[–0.58, 0.53]	not significant

Table C1 (cont.). Model-implied marginal effect of campaigns, all economies

Country	Internet 2019, %	Implied ME (pp)	95% CI	Significance
Serbia	77.4	0.00	[–0.56, 0.56]	not significant
Croatia	79.1	0.05	[–0.52, 0.63]	not significant
Dominican Republic	79.7	0.08	[–0.50, 0.65]	not significant
Argentina	79.9	0.08	[–0.50, 0.66]	not significant
Hungary	80.4	0.10	[–0.49, 0.68]	not significant
Poland	80.4	0.10	[–0.48, 0.68]	not significant
Czechia	80.9	0.11	[–0.47, 0.70]	not significant
Azerbaijan	81.1	0.12	[–0.47, 0.71]	not significant
Costa Rica	81.2	0.12	[–0.47, 0.71]	not significant
Lebanon	81.5	0.13	[–0.46, 0.72]	not significant
Lithuania	81.6	0.14	[–0.46, 0.73]	not significant
Kazakhstan	81.9	0.15	[–0.45, 0.74]	not significant
Russian Federation	82.6	0.17	[–0.43, 0.77]	not significant
Belarus	82.8	0.18	[–0.42, 0.78]	not significant
Slovak Republic	82.9	0.18	[–0.42, 0.78]	not significant
Slovenia	83.1	0.19	[–0.42, 0.79]	not significant
France	83.3	0.19	[–0.41, 0.80]	not significant
Uruguay	83.4	0.19	[–0.41, 0.80]	not significant
Morocco	84.1	0.22	[–0.39, 0.83]	not significant
Malaysia	84.2	0.22	[–0.39, 0.83]	not significant
Chile	85.0	0.25	[–0.37, 0.87]	not significant
Malta	85.8	0.27	[–0.35, 0.90]	not significant
Cyprus	86.1	0.28	[–0.34, 0.91]	not significant
Latvia	86.1	0.29	[–0.34, 0.91]	not significant
Israel	86.8	0.31	[–0.33, 0.94]	not significant
Ireland	87.0	0.31	[–0.32, 0.95]	not significant
Austria	87.8	0.34	[–0.30, 0.98]	not significant
Germany	88.1	0.35	[–0.29, 0.99]	not significant
Singapore	88.9	0.38	[–0.27, 1.03]	not significant
United States	89.4	0.39	[–0.26, 1.05]	not significant
Finland	89.6	0.40	[–0.26, 1.06]	not significant
New Zealand	89.9	0.41	[–0.25, 1.07]	not significant
Estonia	90.2	0.42	[–0.24, 1.08]	not significant
Oman	90.3	0.42	[–0.24, 1.08]	not significant
Belgium	90.3	0.42	[–0.24, 1.08]	not significant
Bahamas	90.7	0.43	[–0.23, 1.10]	not significant
Spain	90.7	0.44	[–0.23, 1.10]	not significant
Andorra	90.7	0.44	[–0.23, 1.10]	not significant
Canada	91.9	0.47	[–0.20, 1.15]	not significant
United Kingdom	92.5	0.49	[–0.19, 1.18]	not significant
Japan	92.7	0.50	[–0.18, 1.19]	not significant
Netherlands	93.3	0.52	[–0.17, 1.21]	not significant
Australia	93.6	0.53	[–0.16, 1.22]	not significant
Sweden	94.5	0.56	[–0.14, 1.26]	not significant
Brunei Darussalam	95.0	0.58	[–0.13, 1.28]	not significant
Saudi Arabia	95.7	0.60	[–0.11, 1.31]	not significant
Korea, Rep.	96.2	0.61	[–0.10, 1.33]	not significant
Luxembourg	97.1	0.64	[–0.08, 1.37]	not significant
Monaco	98.0	0.67	[–0.06, 1.41]	not significant
Norway	98.0	0.67	[–0.06, 1.41]	not significant
Denmark	98.0	0.67	[–0.06, 1.41]	not significant
United Arab Emirates	99.1	0.71	[–0.03, 1.46]	not significant
Iceland	99.5	0.72	[–0.03, 1.47]	not significant
Kuwait	99.5	0.72	[–0.02, 1.47]	not significant
Qatar	99.7	0.73	[–0.02, 1.48]	not significant
Bahrain	99.7	0.73	[–0.02, 1.48]	not significant

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APPENDIX D. Leave-one-country-out robustness

Appendix D probes whether the campaign-by-connectivity interaction is driven by any single country. Model M2 is re-estimated 168 times, each time omitting one country, and the interaction coefficient is recorded. It is highly stable: across all 168 re-estimations it ranges from 0.861 (omitting Sudan) to 0.990 (omitting the Democratic Republic of the Congo), against a full-sample value of 0.912, and it remains positive and significant at the 5% level in every case, with the 95% confidence interval excluding zero throughout. No individual country materially changes the result. Figure D1 plots the 168 estimates with their confidence intervals against the full-sample band, and Table D1 lists every country.

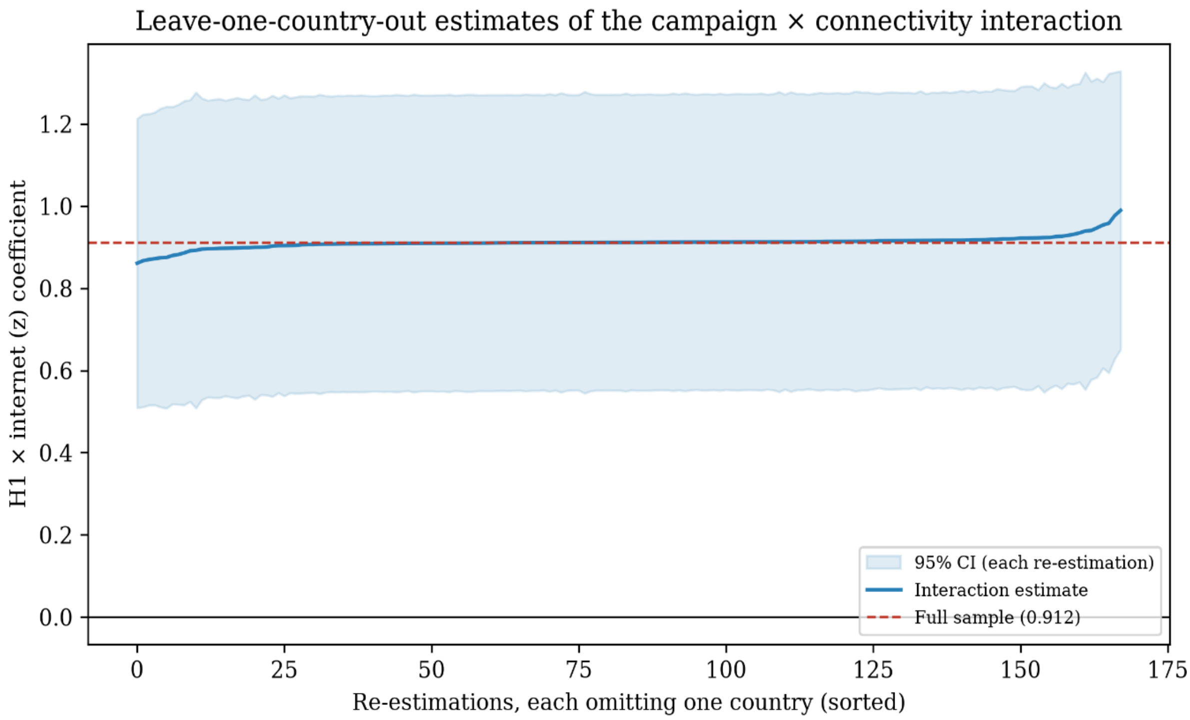

Figure D1. Leave-one-country-out estimates of the campaign × connectivity interaction (sorted; 95% CI; M2)

Table D1. Leave-one-country-out interaction coefficient, all economies (alphabetical)

Country left out	Interaction coefficient	95% CI
Afghanistan	0.872	[0.516, 1.228]
Albania	0.918	[0.557, 1.278]
Algeria	0.912	[0.552, 1.272]
Andorra	0.891	[0.524, 1.258]
Angola	0.909	[0.548, 1.271]
Argentina	0.909	[0.550, 1.269]
Australia	0.913	[0.553, 1.273]
Austria	0.912	[0.553, 1.272]
Azerbaijan	0.911	[0.552, 1.271]
Bahamas	0.910	[0.549, 1.270]
Bahrain	0.916	[0.555, 1.277]
Bangladesh	0.911	[0.551, 1.271]
Barbados	0.908	[0.547, 1.268]
Belarus	0.875	[0.508, 1.242]
Belgium	0.911	[0.552, 1.271]

Table D1 (cont.). Leave-one-country-out interaction coefficient, all economies (alphabetical)

Country left out	Interaction coefficient	95% CI
Belize	0.910	[0.550, 1.269]
Benin	0.911	[0.545, 1.278]
Bhutan	0.923	[0.562, 1.283]
Bolivia	0.912	[0.552, 1.271]
Bosnia and Herzegovina	0.909	[0.549, 1.270]
Botswana	0.913	[0.548, 1.278]
Brazil	0.911	[0.552, 1.271]
Brunei Darussalam	0.916	[0.555, 1.276]
Bulgaria	0.900	[0.540, 1.261]
Burkina Faso	0.927	[0.555, 1.298]
Burundi	0.947	[0.583, 1.311]
Cabo Verde	0.913	[0.553, 1.273]
Cambodia	0.912	[0.552, 1.272]
Cameroon	0.917	[0.553, 1.281]
Canada	0.919	[0.553, 1.285]
Central African Republic	0.958	[0.594, 1.322]
Chad	0.912	[0.550, 1.274]
Chile	0.920	[0.559, 1.281]
China	0.911	[0.551, 1.270]
Colombia	0.909	[0.549, 1.269]
Congo, Rep.	0.920	[0.558, 1.283]
Costa Rica	0.912	[0.552, 1.272]
Croatia	0.916	[0.555, 1.276]
Cuba	0.912	[0.552, 1.273]
Cyprus	0.911	[0.550, 1.271]
Czechia	0.910	[0.550, 1.270]
Côte d’Ivoire	0.914	[0.552, 1.277]
DR Congo	0.990	[0.651, 1.328]
Denmark	0.917	[0.558, 1.276]
Djibouti	0.954	[0.606, 1.302]
Dominica	0.910	[0.549, 1.270]
Dominican Republic	0.880	[0.518, 1.241]
Ecuador	0.911	[0.552, 1.270]
Egypt	0.914	[0.553, 1.275]
El Salvador	0.916	[0.560, 1.273]
Estonia	0.918	[0.559, 1.277]
Eswatini	0.912	[0.551, 1.274]
Ethiopia	0.913	[0.553, 1.273]
Fiji	0.912	[0.553, 1.272]
Finland	0.912	[0.553, 1.271]
France	0.911	[0.551, 1.270]
Gabon	0.913	[0.552, 1.274]
Gambia	0.912	[0.551, 1.273]
Georgia	0.917	[0.557, 1.276]
Germany	0.912	[0.551, 1.272]
Ghana	0.911	[0.549, 1.273]
Greece	0.907	[0.546, 1.267]
Guatemala	0.914	[0.554, 1.274]
Guinea	0.923	[0.547, 1.299]
Guyana	0.911	[0.552, 1.270]
Haiti	0.906	[0.544, 1.268]
Honduras	0.911	[0.551, 1.271]
Hungary	0.914	[0.552, 1.276]
Iceland	0.896	[0.534, 1.259]
India	0.913	[0.552, 1.273]

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Table D1 (cont.). Leave-one-country-out interaction coefficient, all economies (alphabetical)

Country left out	Interaction coefficient	95% CI
Indonesia	0.912	[0.552, 1.273]
Iran	0.913	[0.554, 1.272]
Iraq	0.910	[0.549, 1.270]
Ireland	0.898	[0.535, 1.260]
Israel	0.919	[0.559, 1.279]
Italy	0.911	[0.552, 1.271]
Jamaica	0.870	[0.516, 1.224]
Japan	0.867	[0.511, 1.223]
Jordan	0.914	[0.549, 1.279]
Kazakhstan	0.935	[0.572, 1.298]
Kenya	0.911	[0.550, 1.272]
Kiribati	0.912	[0.550, 1.275]
Korea, Rep.	0.913	[0.555, 1.272]
Kuwait	0.892	[0.508, 1.276]
Kyrgyz Republic	0.908	[0.547, 1.269]
Lao PDR	0.911	[0.552, 1.271]
Latvia	0.922	[0.552, 1.292]
Lebanon	0.911	[0.551, 1.271]
Lesotho	0.914	[0.553, 1.275]
Liberia	0.915	[0.552, 1.277]
Libya	0.910	[0.550, 1.270]
Lithuania	0.915	[0.555, 1.275]
Luxembourg	0.910	[0.548, 1.271]
Madagascar	0.939	[0.553, 1.325]
Malawi	0.911	[0.550, 1.273]
Malaysia	0.900	[0.541, 1.259]
Mali	0.918	[0.555, 1.281]
Malta	0.912	[0.551, 1.272]
Mauritania	0.911	[0.547, 1.275]
Mauritius	0.912	[0.552, 1.273]
Mexico	0.912	[0.552, 1.271]
Moldova	0.910	[0.549, 1.271]
Monaco	0.913	[0.552, 1.273]
Morocco	0.918	[0.557, 1.280]
Mozambique	0.909	[0.547, 1.272]
Myanmar	0.897	[0.538, 1.257]
Nepal	0.909	[0.548, 1.269]
Netherlands	0.909	[0.549, 1.268]
New Zealand	0.916	[0.556, 1.276]
Nicaragua	0.899	[0.539, 1.259]
Niger	0.874	[0.511, 1.238]
Nigeria	0.977	[0.628, 1.325]
Norway	0.915	[0.557, 1.274]
Oman	0.941	[0.578, 1.304]
Pakistan	0.913	[0.553, 1.274]
Panama	0.910	[0.550, 1.270]
Papua New Guinea	0.903	[0.537, 1.269]
Paraguay	0.910	[0.550, 1.269]
Peru	0.913	[0.552, 1.273]
Philippines	0.913	[0.553, 1.273]
Poland	0.913	[0.548, 1.278]
Portugal	0.914	[0.554, 1.274]
Qatar	0.908	[0.548, 1.268]
Romania	0.906	[0.546, 1.267]
Russian Federation	0.909	[0.547, 1.270]

Table D1 (cont.). Leave-one-country-out interaction coefficient, all economies (alphabetical)

Country left out	Interaction coefficient	95% CI
Rwanda	0.917	[0.556, 1.278]
San Marino	0.907	[0.543, 1.271]
Saudi Arabia	0.904	[0.545, 1.263]
Senegal	0.911	[0.551, 1.271]
Serbia	0.916	[0.554, 1.277]
Seychelles	0.908	[0.547, 1.269]
Sierra Leone	0.912	[0.549, 1.274]
Singapore	0.909	[0.549, 1.268]
Slovak Republic	0.896	[0.535, 1.257]
Slovenia	0.905	[0.544, 1.266]
Solomon Islands	0.897	[0.533, 1.261]
Somalia	0.922	[0.554, 1.290]
South Africa	0.910	[0.550, 1.271]
South Sudan	0.922	[0.553, 1.291]
Spain	0.904	[0.538, 1.270]
Sri Lanka	0.916	[0.557, 1.275]
Sudan	0.861	[0.509, 1.212]
Suriname	0.910	[0.550, 1.270]
Sweden	0.913	[0.554, 1.272]
Syrian Arab Republic	0.910	[0.548, 1.271]
Tajikistan	0.931	[0.566, 1.296]
Tanzania	0.886	[0.516, 1.256]
Thailand	0.899	[0.537, 1.260]
Timor-Leste	0.916	[0.553, 1.280]
Togo	0.924	[0.557, 1.290]
Tonga	0.908	[0.548, 1.269]
Trinidad and Tobago	0.909	[0.549, 1.270]
Tunisia	0.908	[0.548, 1.267]
Turkmenistan	0.882	[0.517, 1.247]
Türkiye	0.910	[0.550, 1.269]
Uganda	0.898	[0.533, 1.263]
Ukraine	0.926	[0.564, 1.288]
United Arab Emirates	0.904	[0.545, 1.262]
United Kingdom	0.900	[0.530, 1.270]
United States	0.916	[0.556, 1.277]
Uruguay	0.921	[0.561, 1.281]
Uzbekistan	0.929	[0.568, 1.289]
Vanuatu	0.911	[0.551, 1.272]
Venezuela	0.911	[0.551, 1.270]
Vietnam	0.917	[0.556, 1.278]
Yemen	0.895	[0.528, 1.262]
Zambia	0.914	[0.551, 1.276]
Zimbabwe	0.913	[0.552, 1.273]

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