Sustaining Exposure to Fact-checks: Misinformation Discernment, Media Consumption, and its Political Implications[*]

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Exposure to misinformation can affect citizens' beliefs, political preferences, and compliance with government policies. However, little is known about how to durably reduce susceptibility to misinformation, particularly in the Global South. We evaluate an intervention in South Africa that encouraged individuals to consume biweekly fact-checks—as text messages or podcasts—via WhatsApp for six months. Sustained exposure to these fact-checks induced substantial internalization of fact-checked content, while increasing participants' ability to discern new political and health misinformation *upon exposure*—especially when fact-check consumption was financially incentivized. Fact-checks that could be quickly consumed via short text messages or via podcasts with empathetic content were most impactful. We find limited effects on news consumption choices or verification behavior, but still observe changes in political attitudes and COVID-19-related behaviors. These results demonstrate that sustained exposure to fact-checks can inoculate citizens against future misinformation, but highlight the difficulty of inducing broader behavioral changes relating to media usage.

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1 Introduction

Misinformation about politics, social issues, and public health is a growing and ubiquitous concern. Such content—defined by its potential to generate misperceptions about the true state of the world—encourages beliefs and behaviors that are potentially harmful for both individuals and societies at large (Kuklinski et al. 2000; Nyhan 2020). Across the globe, the spread of misinformation on social media has been linked with citizens' distrust in politics and unwillingness to comply with government policies (Argote et al. 2021; Berlinski et al. 2023). By fueling ideological divides and increasing political polarization (Tucker et al. 2018), exposure to misinformation may have contributed to events such as the 2020 Capitol Hill riots and Brexit. In the Global South, where citizens are especially reliant on closed platforms like WhatsApp for information (Pereira et al. forthcoming), misinformation has been linked to lynchings and mass electoral mobilization in India and racial violence in South Africa (Allen 2021; Badrinathan 2021).

Interventions to limit the potential impact of misinformation most frequently engage in *debunking* or *prebunking* (Blair et al. 2024). Debunking facilitates learning through retroactively correcting specific pieces of misinformation, often by explaining why it is false and providing an alternative explanation (Nyhan and Reifler 2015). Prebunking, which is closely connected to inoculation theory (Cook, Lewandowsky and Ecker 2017), entails warning individuals about the threat of misinformation through examples and preemptively providing knowledge to help them identify and resist it. Both prebunking (e.g. Guess et al. 2020; Pereira et al. forthcoming; Roozenbeek and Van der Linden 2019) and debunking (e.g. Henry, Zhuravskaya and Guriev 2022; Nyhan et al. 2020; Wood and Porter 2019) have been shown to increase skepticism of misinformation.

Sustained exposure to fact-checks—one popular method of combating misinformation—leverages complementarities between debunking and prebunking. Fact-checking most obviously debunks by informing citizens about particular false (and true) claims. But, more generally, repeated engagement with fact-checks should also prebunk by increasing general awareness of misinformation, explaining the logic behind common forms of misinformation, and demonstrating information verification strategies. As a result, fact-checking potentially limits the harmful consequences of misinformation both by shaping citizens' discernment and verification of misinformation *upon exposure* and also by shaping media consumption choices, which affect the extent of exposure in the first place.

Despite these potential benefits, it is difficult to induce citizens to repeatedly consume fact-checks and internalize the lessons contained within them (Nyhan 2020; Walter et al. 2020). While fact-checked information can be effective when delivered in one-off forced consumption settings (e.g. Porter and Wood 2021), sustained consumption outside of the lab or online surveys competes against attention-grabbing content on traditional media, the internet, and now social media (e.g. Prior 2007). Furthermore, existing studies—which largely consist of testing single-shot efforts to combat misinformation—find that most effects attenuate significantly within a few weeks (Guess et al. 2020; Nyhan 2020; Porter and Wood 2021). The short-lived nature of these effects highlights the challenge of internalization, even conditional on information consumption (Zaller 1992), and points to the need to assess if continued fact-checking exposure is effective at combating misinformation. Moreover, little is yet known about how fact-checking shapes political dispositions and compliance with the state beyond attitudes and behaviors closely connected to debunked misinformation.

To understand the consequences of sustained engagement with fact-checks in the field, we implemented a six-month field experiment via WhatsApp in South Africa, where misinformation about social, political, and health issues is rife (Servick 2015; Wasserman 2020). We partnered with Africa Check, the first fact-checking organization serving sub-Saharan Africa, to expose citizens to professionally produced fact-checks. Once every two weeks for six months, Africa Check delivered three fact-checks via WhatsApp messages to treated participants in our large rolling sample of social media users. These fact-checks dissected mostly false stories pertaining to politics, health, and other high-profile topics that were trending on social media in South Africa in the preceding weeks. To measure baseline demand for—as well as encourage the consumption of—the fact-checks, we cross-randomized whether treated participants received quizzes with financial incentives to correctly answer questions about the fact-checks or placebo quizzes containing questions about unrelated content.[1]

We further examine if, and how, citizens can be induced to engage with and internalize fact-checks by randomly varying how the fact-checks were disseminated to participants. Our four WhatsApp-based treatment conditions varied the appeal and cost of consuming the fact-checks as well as how empathetic the content was likely to be. First, imposing a low cost on consumers with competing time pressures, a simple text-based condition sent a single-sentence summary of each fact-check together with a weblink to additional information assessing each disputed claim. Second, the fact-checks were disseminated as a 6-8 minute podcast hosted by two narrators who fact-checked each claim and explained their verification process in a lively and conversational discussion that intended to generate engagement by making fact-checks entertaining.[2] Third, recognizing limits on time and attention span, we tested an abbreviated 4-6 minutes podcast. Fourth, the full-length podcast was augmented with empathetic language emphasizing the narrators' understanding of how fear and concern for loved ones might lead individuals to be fooled by misinformation. These treatments build on literature relating to the challenges of ensuring citizens' attention to corrective information and news more generally (Baum 2002; Marshall 2023; Prior 2007), the effectiveness of "edutainment" in inducing behavioral change (Banerjee, La Ferrara and Orozco-Olvera 2019; La Ferrara 2016), and the role of empathy in internalizing information (Gesser-Edelsburg et al. 2018; Gottlieb, Adida and Moussa 2022; Kalla and Broockman 2020).

Our panel survey establishes three core findings. First, we find that interest in fact-checks is difficult—but not impossible—to generate. While some participants engaged with the fact-

[1] The quizzes did not provide the correct answers, and thus simply provided incentives to consume fact-checks rather than constituting an additional information source. Moreover, since incentives were constant across conditions, we can isolate the effect of different conditions on information internalization upon consumption.

[2] Africa Check recorded these podcasts in partnership with the podcasting firm Volume. These podcasts were already a part of Africa Check's existing programming.

checks in the absence of incentives, relatively small financial incentives generated substantially greater engagement with fact-checks during the intervention. Furthermore, sustained exposure to fact-checks significantly increased demand for future fact-checks, even absent the provision of incentives. This suggests that the intervention activated latent demand for entertaining fact-checks, as prior work encouraging citizens' access to novel news sources also finds (Chen and Yang 2019). These findings highlight the importance of attracting consumers for fact-checks to be effective at combating misinformation at scale.

Second, we demonstrate that sustained exposure to fact-checks helps to inoculate citizens against misinformation *upon exposure*. Receiving any incentivized form of treatment persistently increased respondents' ability to discern truth from falsehood among a battery of political and public health stories we asked participants to assess, while increasing their skepticism towards prominent conspiracy theories—none of which were covered during the intervention. Importantly, greater discernment primarily reflected skepticism of false content, whereas confidence in true content was not systematically altered. Our results suggest that this may be driven by treated participants' greater attention to online content, increased understanding of what credible content looks like, and reduced trust in social media. Nevertheless, the treatments did not impact the amount of news that participants consumed from social and traditional media—and thus did little to change their risk of being exposed to misinformation—or their verification behavior. These findings suggest that sustained exposure to fact-checks primarily combats misinformation by increasing attention and skepticism upon exposure to false content, rather than by altering the type of content individuals consume in the first place.

Third, comparisons across treatment variants indicate that the mode of dissemination matters. With respect to engagement, we find that less can be more: the quickly-consumable WhatsApp text message consistently produced larger effects on discernment than the more involved long and short podcasts. Furthermore, the text treatment shifted attitudes and reported behaviors relating to COVID-19 and government performance away from positions that could be fueled by misinformation: citizens became more likely to report complying with COVID-19 preventative behaviors recommended by the government and more favorable toward the current South African government. Only the empathetic version of the podcast increased discernment as much as the simple text messages, which suggests that edutainment is more effective when it includes emotive appeals to increase the resonance of corrective information with consumers.

Our study adds to the comparatively limited, but growing, body of work studying interventions to hinder misinformation in the Global South (cf. Ali and Qazi 2023; Badrinathan 2021; Gottlieb, Adida and Moussa 2022; Pereira et al. forthcoming; Porter and Wood 2021). A recent comprehensive review of misinformation studies noted that more than 80% focused on contexts across the Global North, which "highlights the challenges of drawing conclusions about effective strategies for countering misinformation in the Global South" (Blair et al. 2024: 2). Our study's findings thus help to validate the benefits of inoculation—which, through sustained exposure, essentially becomes media literacy—in settings where consumers have variable media literacy levels and face high data costs of independently validating information they find on social media platforms.

The unusually sustained aspect of our intervention in the field, along with the richness of our experimental research design, mean that our findings advance our broader understanding of misinformation, how to combat it, and its political consequences in three key ways. First, we demonstrate that sustained exposure to fact-checks can not only debunk the specific misinformation addressed in the fact-checks but also prebunk new misinformation. The importance of repeated engagement helps to make sense of the mixed evidence that single-shot interventions can effectively prebunk misinformation (Maertens et al. 2021; Pereira et al. forthcoming; Roozenbeek and Van der Linden 2019; cf. Badrinathan 2021; Hameleers 2022). We also contribute to this literature by showing that interventions which encourage sustained exposure in a natural media consumption environment can be effective when citizens are motivated to consume fact-checks. By further measuring a broad array of outcomes, we establish that the enduring effects of our sustained intervention are largely driven by increasing citizens' capacity to discern content upon exposure, rather than by changing their media consumption habits. While the moderate effects we observe offer optimism for demand-side interventions, this finding simultaneously emphasizes the importance of complementary supply-side policies.

Second, our findings illuminate the theoretical mechanisms required for fact-checks to be impactful at scale. In line with inventive studies seeking to "gamify" digital literacy lessons (Iyengar, Gupta and Priya 2023; Maertens et al. 2021; Roozenbeek and Van der Linden 2019), we show that entertaining fact-checking podcasts can durably enhance citizens' discernment, and are most effective when delivered empathetically—as a growing literature suggests (Gesser-Edelsburg et al. 2018; Gottlieb, Adida and Moussa 2022; Kalla and Broockman 2020; Williamson et al. 2021). However, we also show that "edutainment" is not the only pathway for stimulating engagement with, and internalization of, fact-checks. Indeed, short text messages that summarized fact-checks were at least as effective. Given the difficulty of engaging citizens in today's multi-platform media environment, interventions requiring little time commitment from citizens may be critical for conveying specific information and general lessons in the face of limited demand for fact-checks. This finding chimes with the importance of integrating brief accuracy nudges into social media platforms (e.g. Pennycook et al. 2021).

Third, this article addresses the important—but as yet understudied—question of whether misinformation shapes political attitudes and behaviors. While it is natural to believe that false beliefs might translate into such outcomes, misinformed beliefs could instead reflect partisan cheerleading with limited political impact (Jerit and Zhao 2020). By demonstrating that text messages regularly conveying fact-checks both increased faith in the incumbent government and reported compliance with its policies, we show that (combating) misinformation can have durable political consequences. Our results thus corroborate the perception that modern polities should be concerned about misinformation's potentially corrosive effects on state capacity and political accountability.

2 When might fact-checking be effective?

In much of the Global South, there are at least two important challenges to mitigating harmful exposure to misinformation. First, limited levels of digital literacy might amplify citizens' susceptibility to misinformation upon exposure (Badrinathan 2021; Guess et al. 2020; Offer-Westort, Rosenzweig and Athey 2022). Second, high data costs restrict citizens' access to the broader internet and increase reliance on low-cost social media platforms such as WhatsApp (Bowles, Larreguy and Liu 2020; Pereira et al. forthcoming). While platforms such as Facebook and Twitter can fact-check misinformation or warn users about flagged posts (Clayton et al. 2020), governments may lack the capacity or incentive to encourage such interventions by platforms and these options are not possible for encrypted platforms like WhatsApp. Consequently, both citizens' overall exposure to misinformation, and the costs they face to verify it, are potentially high.

Research designed to mitigate the negative consequences of misinformation has predominantly focused on two types of interventions: corrective interventions (debunking) and preemptive interventions (prebunking). Corrective interventions, which debunk specific misconceptions and pieces of misinformation, are especially important for disproving prevalent or consequential claims of particular significance (Nyhan 2020). Conversely, prebunking posits that people can be "inoculated" against specific claims, but also misinformation in general, by warning people about misinformation's existence and pre-emptively providing tools to identify and counteract it (Cook 2013; Martel, Pennycook and Rand 2020).

Fact-checking is commonly associated with debunking, but may—with sustained exposure—combine both debunking and prebunking. While fact-checking interventions provide corrections about specific pieces of misinformation, fact-checkers often also explain the general steps taken to establish their conclusions. These explanations can inform consumers about the broader threat of misinformation and where it is most commonly found, demonstrate how misinformation can be debunked using reliable sources and how fact-checking techniques work, and explain general forms of faulty logic underlying specific false claims. Even without directly undergoing media literacy education, individuals may thus experience observational learning through repeated exposure to such messaging (Bandura 2001; Tewksbury, Weaver and Maddex 2001; Zukin and Snyder 1984). Observational learning may even occur among individuals initially lacking motivation for active learning, where knowledge can still be acquired incidentally through passive consumption of information (Shehata et al. 2015; Stroud, Scacco and Kim 2022). Fact-checking may therefore increase consumers' general awareness about how to avoid or spot misinformation and engage in critical thinking or fact-checking themselves.

Sustained exposure to fact-checks may then combat misinformation in two main ways. First, it could *reduce exposure* to misinformation. When consumers receive fact-checks consistently, they may become aware of the prevalence of misinformation, leading them to become more selective about what they read. As fact-checks also educate people about which types of sources are legitimate information providers, they may start consuming more reputable sources.

Second, sustained exposure to fact-checks can reduce misinformation's impact *upon exposure* by promoting internalization of the critical thinking skills they impart—which may require longer and more frequent exposure (Guess et al. 2020; Tully, Vraga and Bode 2020). Thus, even if overall exposure to misinformation is not affected, internalization of the lessons from fact-checks may nevertheless ensure that individuals become more attentive to, discerning of, or likely to verify misinformation they encounter on social media or elsewhere; ideally, they would also become more trusting of truthful information. Sustained exposure may further enhance users' trust in fact-checking sources (Gentzkow, Wong and Zhang forthcoming), which may in turn increase internalization (Alt, Marshall and Lassen 2016).

Although a number of studies experimentally demonstrate fact-checking's promise (see Blair et al. 2024), these studies also have important limitations (Flynn, Nyhan and Reifler 2017; Walter et al. 2020). First, existing work primarily relies on one-shot interventions, often forcing participants to consume fact-checks in lab or survey environments. Outside these settings, however, citizens allocate their time across a wide array of activities and rarely choose to consume fact-checks. Various studies show that political news may only appeal to unusually-engaged individuals (Prior 2007) or when elections are upcoming (Marshall 2023), while relatively few people who visit untrustworthy websites get exposed to even one fact-check in the US (Guess, Brendan and Reifler 2020)—let alone in the Global South, where mobile data is expensive. Corrective and preemptive interventions that work in the lab may then be of limited use in combating misinformation in the field if they cannot regularly capture the public's attention.

Second, consumption of fact-checks does not necessarily imply enduring internalization. Following Zaller (1992), people may read fact-checks and recall their content, but still fail to accept—and thus internalize—the information they receive or quickly move it to the back of their mind without repeated exposure. Indeed, some studies find evidence of motivated reasoning in response to counter-attitudinal information (Peterson and Iyengar 2021; Taber and Lodge 2006). Furthermore, existing research has tended to find short-term success only in combating the *specific* pieces of misinformation that fact-checks targeted, while failing to affect consumers' broader susceptibility or underlying attitudes or behaviors (Barrera et al. 2020; Carey et al. 2022; Hopkins, Sides and Citrin 2019). Via either mechanism, limited internalization restricts fact-checking's potential benefits for media literacy.

2.1 Improving the efficacy of fact-checks

Drawing from established theoretical frameworks, we consider how individuals might be encouraged to both consume and internalize fact-checks in the field.

2.1.1 Encouraging engagement

Attracting consumers in a competitive media environment is likely to require reducing costs or increasing the benefits of consuming fact-checks. We first consider *reducing the time cost* of consumption. Competing against a flow of potentially more interesting or emotive content on social media and elsewhere, misinformation-correcting interventions that are quicker to digest for users might induce more consumption than interventions that take longer to ingest and understand. Given that internalization depends on initial consumption, easier-to-consume fact-checks may prove to be more effective at increasing audience reach and awareness. However, shorter interventions usually convey less information, so may have weaker effects on those exposed.

Another potential solution is to make fact-checking content *more appealing*. Following Bandura's (2001) application of social learning theory to mass media communication, using entertainment for educational objectives can promote attitudinal and behavioral change by increasing attention to favored behaviors, enhancing retention of modeled behaviors by making them more memorable, imparting the skills to reproduce their behaviors, and providing a strong motivation to carry out these behaviors. Prior research on "edutainment" shows that delivering information in entertaining and varied ways positively affects consumption, information recall, beliefs, and behaviors (e.g. Baum 2002; Baum and Jamison 2006; Kim 2023; La Ferrara 2016). For example, Banerjee, La Ferrara and Orozco-Olvera (2019) find that exposure to television programming helped to increase awareness of HIV and health behaviors in Nigeria. Furthermore, Iyengar, Gupta and Priya (2023), Maertens et al. (2021), and Roozenbeek and Van der Linden (2019) find that "gamified" media literacy training increased participants' likelihood of discerning between true and false tweets. Administering fact-checking interventions in more engaging ways might enhance users' demand for them.

2.1.2 Enhancing internalization

The mode by which fact checks are delivered also has the potential to shape citizens' internalization. Within the literature, there is little consensus on the most effective modes of fact-checking, both when considering the *level of detail* or *tone of delivery* needed to inhibit susceptibility to misinformation. With respect to detail, lengthier fact-checks might appear more credible (Chan et al. 2017) and increase information retention (Lewandowsky et al. 2012); they also allow the fact-checking organization to provide more tips on how to spot, and verify, potential misinformation. Moreover, more detailed fact-checks may increase information retention and thereby boost media literacy (Lewandowsky et al. 2012). On the other hand, shorter messages may be less taxing on readers' attention, leading to greater engagement and, in turn, greater internalization (Pennycook et al. 2021). By reducing nuance, shorter and simpler interventions' concise takeaways might increase consumers' acceptance and recall of the fact-checked information (Walter et al. 2020).

Considering the tone of delivery, prior work points to the potential role of empathy in promoting internalization. An expanding body of work highlights the role of emotions in increasing susceptibility to misinformation (Martel, Pennycook and Rand 2020). Thus, interventions that promote emotional engagement and empathy could induce sustained internalization (Gesser-Edelsburg et al. 2018). More generally, Kalla and Broockman (2020) show that empathetic narratives durably decreased out-group exclusion, while Williamson et al. (2021) finds that shared experiences, which induce empathy, increased support for immigrants.

However, the role of tone remains contested in the context of fact-checking. Bode, Vraga and Tully (2020) find no improvement using either uncivil or affirmational tones in comparison to neutral-toned misinformation corrections. Martel, Mosleh and Rand (2021) similarly find no impacts of polite corrective messages on the likelihood of engagement on social media or internalization of the misinformation correction. Since the inclusion of empathetic narratives is likely to increase the length of the fact-checks, trading detail for tone of delivery could reduce the effectiveness of empathetic fact-checks.

2.2 Hypotheses

Together, we anticipate that sustained exposure to fact-checking ought to combine aspects of both debunking and prebunking for misinformation correction. We next summarize our hypotheses, which were registered in our pre-analysis plan and are enumerated alongside a preview of our findings in Table 1.

Our hypotheses relate to four main groups of outcomes: engagement with fact-checks, discernment of content on social media, engagement with content on social media, and the political consequences of any changes in how individuals engage with the content they encounter. First, hypothesis H1 expected that easing access to fact-checks would increase exposure to, and knowledge of, the facts that were covered by the treatment deliveries. In a media environment with many available options, ensuring engagement with such content is not a given and may require further incentives. Second, we hypothesized that sustained exposure to corrective information would inoculate individuals against misinformation by making them more attentive to the veracity of the content they encounter (H2), more aware of misinformation on social media and less trusting of

Table 1: Preregistered hypotheses and findings

Hypothesis in pre-analysis plan	**Preregistered direction**	**Finding**
Pooled effects		
H1: Exposure to, and knowledge about, information covered by treatment (Figs. 4a, 4b)	+, +	+, +
H2: Attention to veracity of content on social media (Fig. 6b)	+	+
H3: Perceived extent of true information on social media and trust in social media content (Fig. 6c)	-	-
H4: Capacity to identify misinformation based on its characteristics (Fig. 5a, 5b)	+	+
H5: Consumption and sharing of information from social media (Fig. 7a, 7c)	-, -	null, -
H6: Active fact-checking and knowledge about how to fact-check (Figs. 7b, 6a)	+, +	null, +
H7: Willingness to take COVID-19 precautions (Fig. 8a)	+	+
H8: Perceptions of government performance (Fig. 8b)	+	+
Comparisons between treatment arms		
Difference between podcast and text	+	-
Difference between empathetic and long podcast	+	+
Difference between long and short podcast	?	null

Notes: We merged hypotheses H2 and H3 in our pre-analysis plan into a combined H3; Figures E3a and E3b report similar results separately. Due to merging these hypotheses and the order that results are presented, H2, H4, H5, H6, and H7 correspond to H5, H6, H4, H7, and H9 in our pre-analysis plan.

social media content (H3), and ultimately better able to discern false from true information (H4). Third, in addition to altering how citizens respond to content upon exposure, we further anticipated that repeated exposure to fact-checks would cause individuals to consume and share less content from social media (H5) and more actively use verification techniques (H6). Finally, to the extent that misinformation typically focuses on salient false claims about politics or public policy, sustained exposure to fact-checks might then increase compliance with its policies (H7) and improve perceptions of government performance (H8).

Beyond the effects of sustained exposure to fact-checks in general, understanding *how* to effectively increase organic consumption and internalization is more theoretically ambiguous. Indeed, simpler interventions might promote consumption while undermining the broader benefits from internalization, while more engaging modes enhance internalization but require more costly consumption decisions by citizens. Our pre-specified expectations relating to this trade-off was that interventions leveraging "edutainment" or more empathetic content would be more effective at enhancing internalization at the potential cost of lower engagement. As such, we expected delivery through an entertaining podcast would be more effective than a text message with the same information, and an empathetic podcast even more so.

3 Misinformation in South Africa

Misinformation has been a growing concern in South Africa in recent years, particularly in the context of political and social issues (Reuters Institute 2021). In July 2021, for example, national unrest sparked by former president Jacob Zuma's arrest resulted in widespread faked images and posts of destruction and racialized killings appearing on social media, which further exacerbated inter-community tensions, violence, and looting (Allen 2021). During elections, false rumors and conspiracy theories about politicians and political parties have been disseminated to influence voters and to worsen social divisions (International Federation of Journalists 2021). Misinformation has targeted women, particularly journalists and politicians (Agunwa and Alalade 2022; Wasserman 2020), and has also worsened xenophobic violence in the country (News24 2019).

Since the pandemic's onset in 2020, health misinformation has also increased dramatically. Prominent false claims included COVID-19 not affecting Black Africans, 5G technologies being responsible for the virus, vaccines implanting microchips for government surveillance, and the efficacy of various home remedies and miracle cures (Africa Check 2023). Such pandemic-related misinformation capitalized on citizens' distrust of information provided by their government and perceived political elites (Steenberg et al. 2022). Moreover, misinformation widened health inequality and compliance with government policies; vaccine hesitancy was highest among the most segregated and marginalized communities (Steenberg et al. 2022).

The widespread use of mobile phones and social media platforms like Facebook and WhatsApp in South Africa has fueled the proliferation of misinformation. WhatsApp is a popular choice of communication and news consumption for South African internet users due to its affordability in a country with high data usage costs. In 2021, 88% of South Africans used WhatsApp, and 52% of South Africans used WhatsApp to access news (Newman et al. 2021). However, WhatsApp has also become a breeding ground for misinformation, and its negative impacts worsened during the

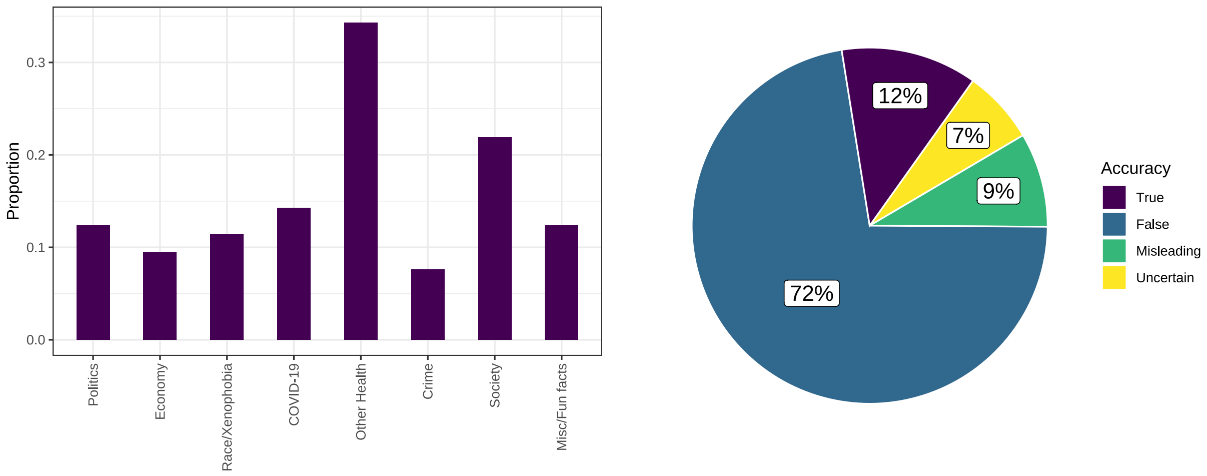
COVID-19 pandemic (Quartz Africa 2020).

To combat misinformation, civil society organizations have developed fact-checking tools and initiatives to verify the accuracy of the information circulating on social media. Africa Check is a prominent example: since its founding in 2012, the South African nonprofit has focused its efforts on verifying claims made by public figures and popular content that appears online or on social media. Since 2019, Africa Check has partnered with the podcasting firm Volume to produce a biweekly podcast—entitled "What's Crap on WhatsApp?"—which debunks three locally viral pieces of misinformation each episode in an engaging investigative style. As podcast consumption in South Africa is fast-growing, Africa Check's misinformation podcast seeks to capture a broader audience through an accessible audio format.

4 Research design

To understand the constraints on *consumption* and *internalization* that potentially limit fact-checking's effectiveness, we implemented a six-month field experiment that varied participants' access to different forms of Africa Check's fact-check programming. During the study period, Figure 1 shows that most fact-checks related to (usually false) claims about politics, health issues, and broader social issues. Political fact-checks tended to debunk incendiary claims relating to government corruption or incompetence; health fact-checks often focused on debunking myths and false cures related to COVID-19 and other health conditions (such as diabetes, high blood pressure, or HIV) as well as the adoption of new technologies purportedly harmful to health.

Each fact-check summarized the claim being examined before then explaining Africa Check's process for verifying the claims (which typically involved investigating its source, cross-checking, and consulting experts) and ultimately concluding whether the claim was true or false. Our study evaluates whether sustained exposure to such fact-checks—and how the fact-checks are conveyed—helps individuals to become more discerning of the content they consume in general, changes what information they consume and what they do with it, and ultimately affects citizens'



(a) Topics fact-checked

(b) Accuracy of facts checked

Figure 1: Biweekly fact-checked content

Notes: Fact check categories in (a) were coded independently by an undergraduate research assistant. Examples of fact-checks within each category are provided in Appendix B.1. Accuracy categories in (b) are provided by Africa Check's fact-checking.

attitudes and behaviors.

4.1 Participant recruitment

Following a brief pilot, the research team recruited participants from social media for the study from across South Africa between October 2020 and September 2021. Facebook advertisements were used to recruit adult Facebook users in 21 "batches" on a rolling basis (typically once every two weeks) for a research study on misinformation in South Africa.[3] Individuals were eligible to participate if they were at least 18 years old, lived in South Africa at the time of recruitment, had a South African phone number, understood English, and used WhatsApp. We restricted our recruitment to social media users due both to their higher anticipated exposure to misinformation

[3] Appendix Figure C1a shows an example recruitment ad. Ads were targeted at individuals who did not follow Africa Check's Facebook page, and were stratified at the province-gender-age level to increase representativeness. Few users above 50 years old were targeted, given their lower use of social media. Appendix A.1 provides additional information on recruitment.

as well as the relative feasibility of collecting survey responses (any in-person enumeration would have been challenging due to the COVID-19 pandemic).

Eligible participants then completed a baseline survey administered by the research team via a WhatsApp chatbot (see Appendix Figure C1b). The baseline survey recorded participants' demographic characteristics, attitudes regarding misinformation, knowledge about misinformation and current affairs, trust and consumption of different information sources, information verification and sharing behavior, and COVID-19 knowledge and preventative behavior. 11,672 individuals completed the baseline survey and 8,947 satisfied the conditions necessary to enroll in the study.[4]

This pool of participants was 28 years old on average, and mostly urban (76%), female (61%), and educated (89% report receiving secondary education). Appendix Figure C2 compares this sample with nationally representative data from 2018 round of the Afrobarometer survey. While this sample is systematically different from the *overall* broader population, it is similar in terms of observables to the relevant Afrobarometer subgroup who report ever using social media, with only modest differences in age, gender, and education observed.

4.2 Treatment assignment and delivery

Participants were randomly assigned to either a control group that received no fact-checks or one of four treatment conditions that varied how fact-checks were conveyed. Africa Check delivered these treatments through separate WhatsApp lists created specifically for this intervention, and all

[4] Participants were required to send a WhatsApp message to an Africa Check-managed phone number and add that number to their phone contacts to receive a small financial incentive for completing the survey; this was necessary for Africa Check to be able to deliver treatment information to participants through its WhatsApp broadcast lists. Further, we added three simple attention checks (see Appendix A.1) to screen out low-quality respondents. Participants had to respond to all attention check questions correctly and not complete the survey in less than eight minutes to enroll in the study.

treated participants received the same three fact-checks via WhatsApp once every two weeks for six months.[5] Figure 2 provides an example of how the text and three versions of the podcast differed in their presentation of a single fact-check. Appendix Table B.1 provides additional examples of specific fact-checks, along with examples of wording for the treatment variants.

4.2.1 Fact-check treatment variants

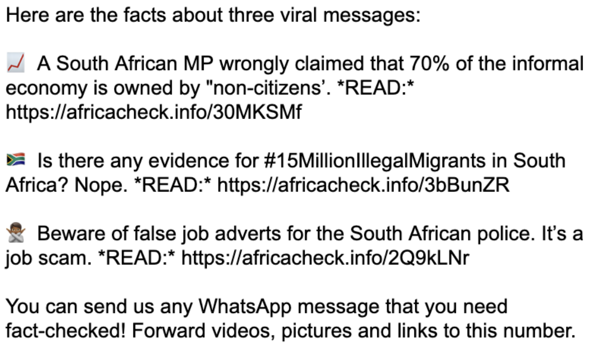
We first varied whether the fact-checks were disseminated through a short text message or a podcast. The *Text* condition simply provided a one-sentence summary of each fact-check, together with a clickable link to an article on Africa Check's website assessing the disputed claim. These messages enabled consumers to quickly learn the veracity of viral online claims without reading the articles, and also to access articles for each of the claims separately.

The three podcast conditions delivered the fact-checks in a more entertaining but longer-form way. In each variant, two narrators explained the veracity of each claim and how they verified the claims in a lively and conversational tone.[6] Among those receiving podcasts, we further varied how costly or empathetic the content was. The default *Long* podcast—which Africa Check disseminates to its regular subscribers—generally lasted 6-8 minutes, while the *Short* podcast cut some discussion of how the claims were verified to reduce the podcast to 4-6 minutes in length. The *Empathetic* podcast augmented the *Long* podcast with empathetic language emphasizing the narrators' understanding of how fear and concern about family and friends might lead individuals to be fooled by misinformation; Appendix B.2 provides examples of empathetic additions.

Once assigned, treated participants were informed about the mode of dissemination for their

[5] Although Africa Check delivered these treatments, both the Africa Check team and researchers finalized the wording of the messages to ensure the integrity of treatment variants.

[6] Although participants that received podcasts also received an initial text message similar to the *Text* condition without the links to the articles, their treatment arm was explained as consuming a podcast. Since this instruction was always the most recent, it is likely that participants perceived this intervention as costlier to engage with relative to just reading text information.



Here are the facts about three viral messages:

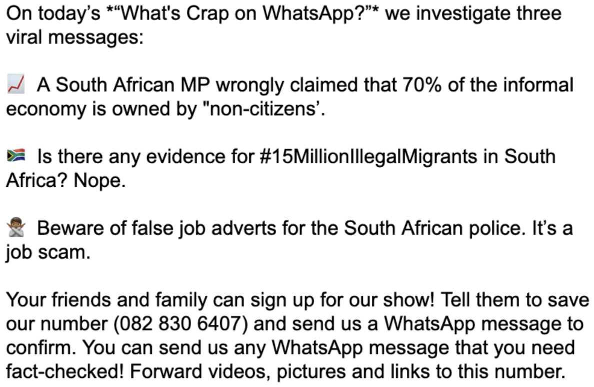
📈 A South African MP wrongly claimed that 70% of the informal economy is owned by "non-citizens'. *READ:* https://africacheck.info/30MKSMf

🇿🇦 Is there any evidence for #15MillionIllegalMigrants in South Africa? Nope. *READ:* https://africacheck.info/3bBunZR

🕵🏾 Beware of false job adverts for the South African police. It's a job scam. *READ:* https://africacheck.info/2Q9kLNr

You can send us any WhatsApp message that you need fact-checked! Forward videos, pictures and links to this number.

(a) WhatsApp message to *text* group



On today's *"What's Crap on WhatsApp?"* we investigate three viral messages:

📈 A South African MP wrongly claimed that 70% of the informal economy is owned by "non-citizens'.

🇿🇦 Is there any evidence for #15MillionIllegalMigrants in South Africa? Nope.

🕵🏾 Beware of false job adverts for the South African police. It's a job scam.

Your friends and family can sign up for our show! Tell them to save our number (082 830 6407) and send us a WhatsApp message to confirm. You can send us any WhatsApp message that you need fact-checked! Forward videos, pictures and links to this number.

(b) WhatsApp message to *podcast* group

Host 1: Let's jump straight into our first fact-check today. *It was made during the South African state of the nation address on 16 February 2021.* It's the type of claim that can get a lot of people worked up and even angry.

Host 2: Vuyolwethu Zungula, president of the African Transformation Movement, focused on the country's informal economy. This is what he had to say: [AUDIO]

Host 1: *Wow! That's a massive claim.* That 70% of the informal economy is being controlled by foreign nationals. When South Africans – especially those that are unemployed and struggling - hear stats like this it gets their blood boiling.

Host 2: That's right. They can feel like resources are being taken away from them when there isn't much to go around!

Host 1: *How accurate is it?*

Host 2: Zungula provided Africa Check with a number of documents in support of his claim. However, only one included a mention of 70%. This was a report on an interview on talk radio station 702.

Host 1: But the article stated that "foreigners...run about 70% of informal stores in South Africa" — not that they own or control 70% of the informal economy.

Host 2: *The origin of the statistic can be traced to a small survey conducted by Minanawe Marketing saying that 70% of informal retailers were owned by foreigners. These informal stores, known as spaza shops, sell essential household items.*

Host 1: *Zaheera Jinnah, a research associate at Wits University's African Centre for Migration and Society in Johannesburg, told Africa Check that migrant workers are increasing in number but they still constitute a small share of the labour force. What do the stats actually show?*

Host 2: An analysis by the [*short*: Wits University's African Centre for Migration and Society in Johannesburg] [*long:* centre] found that around 20% of people working in the informal economy in 2017 were born outside the country. In Gauteng 19.7% of business owners had migrated to South Africa from another country.

Host 1: *Jinnah warned that "data-light and emotion-heavy" comments like these were likely to "stoke fear and intolerance".*

Host 2: So this claim is incorrect?

Host 1: Yep, it's crap!

Notes: regular text indicates *short* podcast; *italicized* indicates *long* addition; underline indicates *empathy* addition.

(c) Podcast script

Figure 2: Example of a single fact-check for all treatment arms

fact-checks. 7,331 participants saw their treatment assignment; the residual 1,616, which was balanced across treatment arms, selected out of continued engagement with the study after completing the baseline survey. Treatment was then delivered via Africa Checks' WhatsApp account every fortnight for six months to treated participants, while control participants received no further information from Africa Check.

4.2.2 Incentives to consume fact-checks

To understand organic demand for fact-checks and stimulate engagement among participants lacking interest, we further varied the provision of financial incentives for treated participants to consume Africa Check's fact-checks. Specifically, a randomly selected 83% of treated participants received short monthly quizzes covering recent fact-checks (*fact-check quizzes*). All control participants and the remaining treated participants received quizzes asking about popular culture (*placebo quizzes*). Regardless of quiz type, participants knew in advance that they would receive greater payment for completing these optional monthly quizzes if they answered a majority of quiz questions correctly; see Appendix A.4 for details. Appendix Figure C3 shows that participants who received their treatment regularly took these interim quizzes, with similar rates of quiz participation across treatment arms.

These quizzes were administered by the research team through a different WhatsApp account from the Africa Check account used for treatment delivery. To minimize the risk that the fact-check quizzes would treat participants directly, we did not provide participants with the correct answers or tell them which questions they answered correctly. In line with prior studies adopting similar designs (e.g. Chen and Yang 2019), the quizzes should therefore be construed as generating variation in participants' instrumental incentives to engage with their treatments without constituting an independent source of information in their own right.

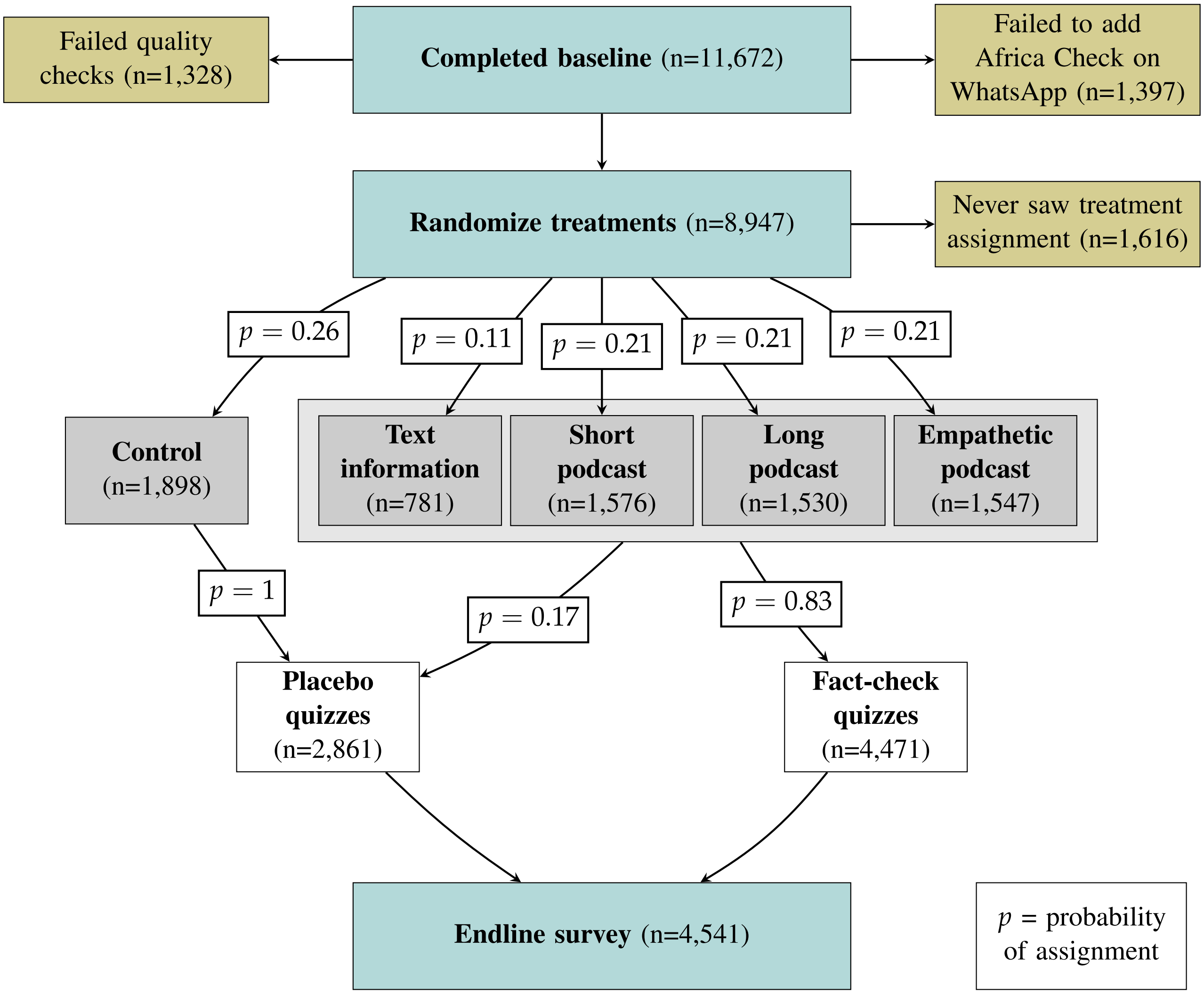

Failed quality checks (n=1,328)
Completed baseline (n=11,672)
Failed to add Africa Check on WhatsApp (n=1,397)
Randomize treatments (n=8,947)
Never saw treatment assignment (n=1,616)
$p = 0.26$
$p = 0.11$
$p = 0.21$
$p = 0.21$
$p = 0.21$
Control (n=1,898)
Text information (n=781)
Short podcast (n=1,576)
Long podcast (n=1,530)
Empathetic podcast (n=1,547)
$p = 1$
$p = 0.17$
$p = 0.83$
Placebo quizzes (n=2,861)
Fact-check quizzes (n=4,471)
Endline survey (n=4,541)
p = probability of assignment

Figure 3: **Overview of treatment assignments**
The main treatment arms include a pure *Control*, a *Text*-only treatment, a *Short* (4-6min) podcast, a *Long* (6-8min) podcast, and an *Empathetic* variant of the long podcast. Participants were additionally incentivized to consume particular content through optional monthly quizzes, relating either to the treatment information (*Fact-check quizzes*) or pop culture (*Placebo quizzes*).

4.2.3 Summary of interventions

Figure 3 summarizes the overall research design, noting the share of participants assigned to control and each treatment arm as well as the share cross-randomized to fact-check versus placebo quizzes. For each recruitment batch, treatment conditions were randomly assigned within blocks of individuals with similar demographics, social media consumption patterns, trust towards different news sources, and misinformation knowledge.[7] Appendix A.5 provides a discussion of ethical considerations and risks of study participation, which we considered to be minimal.

4.3 Outcome measurement

After six months, each participant completed an endline survey administered by the research team. Those participants who reached the endline ($n = 4,541$) were highly engaged, taking an average of 88% of the monthly quizzes.[8] To uniformly measure fact-check consumption and internalization, we embedded a final quiz relating to Africa Check's recent fact-checks in the endline survey, even if participants had been assigned to placebo quiz incentives during the treatment period. Along with other measures of treatment engagement and internalization, the endline survey measured our primary outcomes: trust in media, attention to content, and discernment of content truth; information consumption, verification, and sharing behaviors; and attitudes and behaviors relating to COVID-19 and politics. Our main analyses aggregate indicators within each of these groups into inverse covariance weighted (ICW) indexes to limit the number of outcomes considered and increase statistical power (Anderson 2008). Table 2 describes each index component,

[7] We assigned more of the sample to the podcast treatments relative to the text information treatment to improve our statistical power to detect differences across the more similar podcast treatment conditions. In addition to the four main treatment arms, we cross-randomized whether the WhatsApp messages delivering each treatment variant included text priming the importance of fact-checking for social good. We report the effects of this further encouragement to consume the fact-checks in Appendix B.3, where we show that participants assigned to the social prime consumed fact-checks at indistinguishable rates but experienced greater internalization. Given its assignment was orthogonal to the main treatments, our results pool across participants that were and were not primed.

[8] On average, endline respondents received a total of 155 Rand (9.74 USD) through all components of the study.

provides summary statistics, and notes the figures in which each outcome is presented. Appendix A.6 explains how we deal with missing data and justifies some differences from our pre-specified outcome measures.[9]

4.4 Estimation

We estimate intent-to-treat (ITT) effects of different combinations of treatment arms relative to our control group. Specifically, we estimate the following pre-specified OLS regressions:

$$Y_{ib} = \alpha_b + \beta Y_{ib}^{pre} + \gamma \mathbf{X}_{ib}^{pre} + \boldsymbol{\tau} \mathbf{T}_{ib} + \varepsilon_{ib}, \tag{1}$$

where Y_{ib} is an outcome for respondent i from block b, $\mathbf{T}_{ib}$ is the vector of individual treatment assignments, α_b are randomization block fixed effects, Y_{ib}^{pre} is the baseline analog of the outcome (where feasible), and $\mathbf{X}_{ib}^{pre}$ is a vector of predetermined baseline covariates selected separately for each outcome variable via cross-validated LASSO. The vector $\boldsymbol{\tau}$ captures the ITT effect of each treatment condition. Reflecting the individual-level randomization, robust standard errors are used throughout.

We focus on two pre-specified approaches to combining treatment conditions: (i) a pooled specification, where we pool all text and podcast fact-check conditions; and (ii) a disaggregated specification, where we examine the *Text*, *Short* podcast, *Long* podcast, and *Empathetic* podcast conditions separately. The principal deviation from our preregistered specifications is our decision to pool the treated participants that received placebo quiz incentives into a single group (*Placebo incentives*).[10] For inference, we use one-sided t tests to evaluate hypotheses where we pre-specified

[9] These differences are quite minimal, but include our decision to exclude questions about WhatsApp itself from indexes relating to social media (given WhatsApp's usage in treatment delivery) and our combination of pre-specified indexes relating to perceived misinformation on and trust in these other social media platforms (due to their high conceptual and empirical overlap).

[10] We had pre-specified that such individuals would be pooled with groups receiving the *Text*,

Table 2: Outcome variables

Outcome variable	**Variable definitions**	Mean	SD	Range
Consumption of fact-checks (H1)				
Podcast take-up (Fig. 4a)	How often listen to podcasts (never - all the time)	3.24	1.25	[1,5]
	Included "What's Crap on WhatsApp" in selection of podcasts listened to	0.41	0.49	{0,1}
Treatment knowledge (Fig. 4b)	Number of correct responses from 6 questions on fact-checked content	2.75	1.56	[0,6]
Intended future take-up (Fig. 4c)	Stay subscribed (or start subscribing) to "What's Crap on WhatsApp"	0.83	0.37	{0,1}
	Requested Africa Check's fact checking content	0.85	0.36	{0,1}
	Requested Africa Check reminders to pay attention to misinformation	0.71	0.45	{0,1}
	Requested vaccine info from Africa Check	0.72	0.45	{0,1}
Discerning fact from fiction (H2-H4)				
Discernment between true and fake news stories (Fig. 5a)	Alcohol decreases ability to fight COVID-19 infections (not at all likely - very likely) [true]	3.51	1.27	[1,5]
	Almost 100% of workers in South Africa are foreign (not at all likely - very likely) [false] [-]	2.89	1.31	[1,5]
	COVID-19 spreads by a person's mouth or nose (not at all likely - very likely) [true]	4.45	0.91	[1,5]
	Matriculation scores to be inflated (not at all likely - very likely) [false] [-]	3.11	1.34	[1,5]
Skepticism of conspiracy theories (Fig. 5b)	AIDs intentionally created (not at all likely - very likely)	3.69	1.37	[1,5]
	Nelson Mandela died in 1985 (not at all likely - very likely)	3.82	1.38	[1,5]
	COVID-19 vaccines used to implant chips (not at all likely - very likely)	3.70	1.34	[1,5]
	Vaccines used to reduce world's population (not at all likely - very likely)	3.72	1.34	[1,5]
Knowledge of verification methods (Fig. 6a)	How to avoid being misled: Ask other people [-]	0.13	0.34	{0,1}
	How to avoid being misled: Seek information from reputable organizations	0.36	0.48	{0,1}
	Verification strategies: Ask experts	0.42	0.49	{0,1}
	Verification strategies: Ask themselves [-]	0.88	0.32	{0,1}
	Verification strategies: Check source popularity [-]	0.63	0.48	{0,1}
	Verification strategies: Talk to others [-]	0.82	0.38	{0,1}
	Verification strategies: Use reverse image searches	0.16	0.36	{0,1}
	How to verify info: Ask people I know through WhatsApp [-]	0.82	0.39	{0,1}
	How to verify info: Ask people I know in person [-]	0.71	0.46	{0,1}
	How to verify info: Ask people I don't know well on WhatsApp group [-]	0.91	0.29	{0,1}
	How to verify info: Ask people I know by posting on social media [-]	0.87	0.33	{0,1}
	How to verify info: Submit a fact-checker request	0.21	0.40	{0,1}
	How to verify info: Go to fact-checker	0.49	0.50	{0,1}
	How to verify info: Use the internet to fact-check yourself	0.46	0.50	{0,1}
Attention to content on social media (Fig. 6b)	How often pay close attention to social media information	3.83	1.05	[1,5]
	How to avoid being misled: Pay close attention to source	0.39	0.49	{0,1}
	How important to verify social media information	4.05	1.22	[1,5]
Trust in social media (besides WhatsApp) (Fig. 6c)	Likely to be true: Information from other social media (e.g. Facebook, Twitter) (all fake - all truthful)	2.83	0.75	[1,5]
	Trust the most for information: Other social media (e.g. Facebook, Twitter)	0.16	0.37	{0,1}
	Trust: Information from other social media (e.g. Facebook, Twitter) (strongly distrust - strongly trust)	2.88	1.04	[1,5]
Information consumption, verification, and sharing (H5 and H6)				
Social media consumption (Fig. 7a)	Regularly go to for news: Other social media (Facebook, Twitter)	0.42	0.49	{0,1}
Active verification (Fig. 7b)	How often verify content seen on social media (never - always)	3.83	1.10	[1,5]
Sharing (Fig. 7c)	How often share social media content shared by others (never - always)	2.83	1.11	[1,5]
Attitudes and behaviors relating to COVID-19 and government (H7 and H8)				
COVID-19 beliefs and preventative behaviors (Fig. 8a)	Number of days stayed home in the past week	4.20	2.27	[0,7]
	Number of days visited others indoors in the past week [-]	4.18	2.10	[0,7]
	Number of days wore mask in the past week	5.26	2.36	[0,7]
	View COVID-19 as a fake disease (strongly disagree - strongly agree) [-]	4.36	1.11	[1,5]
	View on COVID-19 lockdown (definitely necessary - definitely unnecessary) [-]	3.21	0.92	[1,4]
	Trust that COVID-19 vaccines in South Africa are safe (strongly distrust - strongly trust)	3.89	1.37	[1,5]
	Would take available COVID-19 vaccine (strongly disagree - strongly agree)	3.49	1.54	[1,5]
Views and attitudes about the government (Fig. 8b)	How well national government is performing in general (very badly - very well)	2.38	1.20	[1,5]
	How well national government is handling the COVID-19 pandemic (very badly - very well)	3.09	1.22	[1,5]
	Likely to be true: Information from politicians and government officials (all fake - all truthful)	3.02	0.95	[1,5]
	Trust the most for information: Government officials	0.30	0.46	{0,1}
	Trust the most for information: Politicians and other public figures	0.13	0.34	{0,1}
	Trust: Information from politicians and government officials (strongly distrust - strongly trust)	2.89	1.20	[1,5]
	Vote for regional incumbent in a parliamentary election held tomorrow	0.23	0.42	{0,1}
	Vote for national incumbent in a parliamentary election held tomorrow	0.21	0.41	{0,1}

Notes: These descriptive statistics underlie all survey variables used in results and figures presented in Section 5. The final column represents the full (integer) range of value options in our survey for each question. Variables followed by [-] indicate that variable has been reversed for use in index before providing summary statistics.

a directional hypothesis in Table 2. Otherwise, or in cases where the pre-specified direction is the opposite of the estimated treatment effect, we use two-sided t tests.

Short, *Long*, or *Empathetic* treatment arm. This ultimately made less sense due to relatively low engagement with fact-checks among participants assigned to placebo quizzes (see Figure 4).

We focus on intent-to-treat effects, rather than the local average treatment effects of consuming fact-checks, for several reasons. First, we consider this to be the quantity of theoretical and policy relevance. Our theoretical framework considers potential trade-offs in how fact-checking interventions might shape participants' consumption of corrective information *and* their impacts conditional on consumption. Because we cannot force consumption outside of the lab, understanding the net effect of such interventions—while parsing potential differences in uptake—is then the relevant quantity for policy as well. Second, our treatment conditions could affect relevant outcomes through causal pathways that extend beyond just consumption of fact-checks, rendering the exclusion restriction difficult to defend in an instrumental variable analysis. Third, we lack a measure of uptake that does not rely on participants' self-reported consumption of Africa Check's fact-checks.[11]

Finally, we validate the research design in several ways.[12] First, we find no evidence of differential attrition across treatment arms. Appendix Table C1 shows balance in the probability of completing the endline survey over treatment conditions.[13] Second, treatment conditions are well balanced across baseline survey covariates in the endline sample. As Appendix Table C2 shows, a joint F-test only fails to reject the null hypothesis that the mean of all characteristics are equal to zero at the 10% significance level. Third, we assess the possible concern that demand effects

[11] While we are able to measure the overall frequency with which relevant URL links were clicked, which is relevant for some treatment conditions, we observe this only at the link rather than the individual level.

[12] Because participants are scattered across the country and make up a tiny fraction of the South African population, the stable unit treatment value assumption is likely to hold.

[13] Overall attrition rates from baseline to endline are nearly 50%. These attrition rates owe to the six-month study duration, our use of relatively small financial incentives to induce continued engagement, and our survey enumeration through a WhatsApp chatbot. Participants who dropped out during the study were broadly similar to those who took the endline, aside from being slightly younger and more likely to be male.

drive our main effects in Appendix A.7. As discussed there, we focus on factual outcomes less susceptible to survey response biases, consider such biases to be unlikely to account for differences *between* treatment groups, and find it improbable that biases would affect only the subset of outcome families where we find consistent treatment effects.

5 Results

We focus on four sets of outcomes. First, we assess how treatment assignment shaped participants' attention to, and consumption of, the fact-checks. Second, we consider whether our sustained intervention improved participants' capacity to discern true and false information *not* covered by the fact-checks. Third, to understand the extent to which individuals reduced their exposure to misinformation, we examine participants' broader media consumption behaviors. Fourth, in line with the fact-checks' topical focus, we evaluate broader impacts on participants' attitudes towards the government and their COVID-19 beliefs and behaviors.

We present results from both the pooled treatment specification and the disaggregated treatment specification. Given our use of ICW indexes to aggregate similar outcome variables, treatment effect estimates reflect standard deviation changes relative to the control group. Our graphical results plot 90% and 95% confidence intervals in each figure; the lower panels provide p-values from tests of differences in effect size between particular treatment arms. Appendix Tables F1-F13 report the regression estimates underlying our figures as well as unstandardized estimates for each index component.

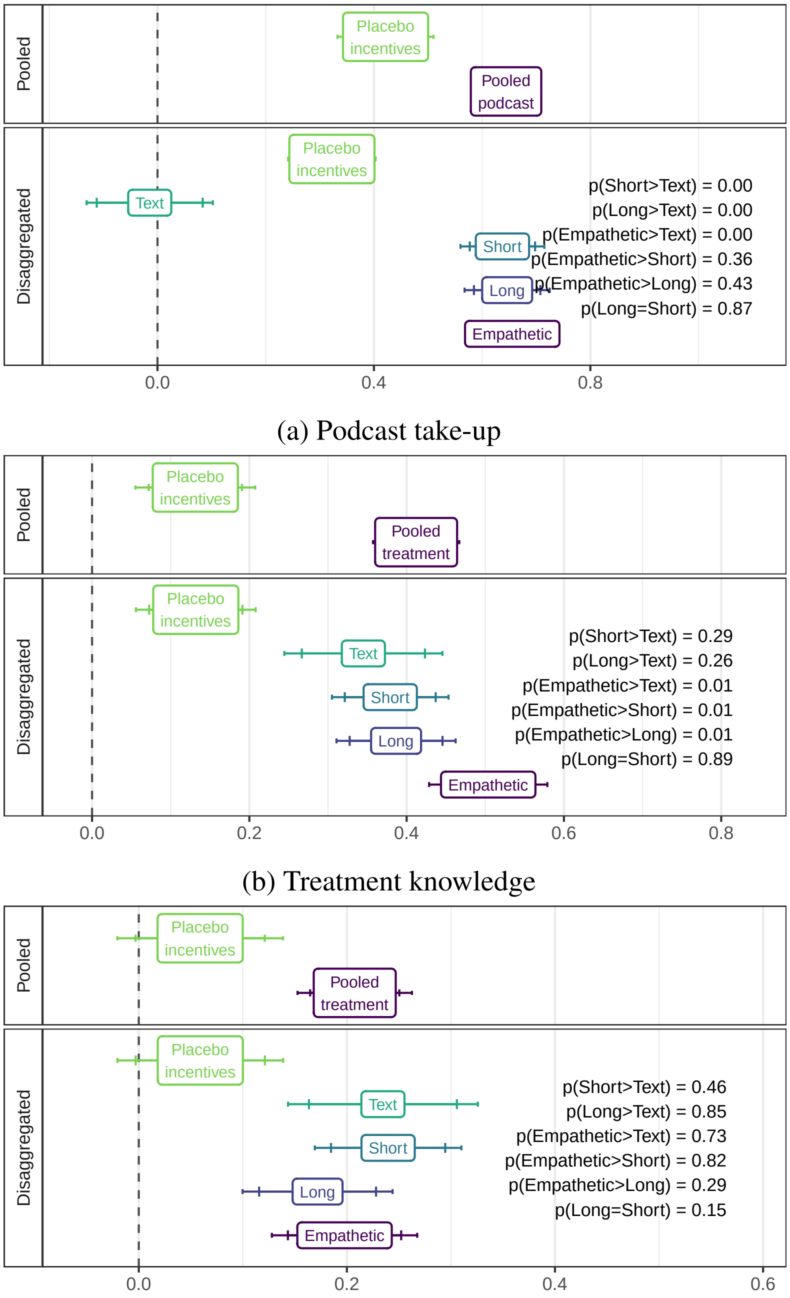
5.1 Consumption of fact-checks

In line with hypothesis H1, we find substantial and sustained levels of fact-check consumption in Figure 4. The upper panel of Figure 4a demonstrates that podcast listenership increased by 0.65 standard deviations across pooled podcast treatment conditions ($p < 0.01$). For our most direct metric of intervention take-up, Appendix Table F1 shows that participants assigned to podcasts became 36 percentage points more likely to report listening to the WCW podcast relative to the control group (or participants assigned to text messages) by endline. With respect to text consumption, only around 11% individual webpage links sent as part of the biweekly text messages were clicked by study participants, although the fact-check's conclusion was always conveyed in the WhatsApp message itself.

To capture the extent to which participants paid attention to their assigned treatments, and address the concern that treated respondents over-reported their consumption of the podcast, we consider two behavioral measures of engagement. First, consistent with the debunking aspect of the intervention, Figure 4b demonstrates that the average treated respondent receiving fact-check quiz incentives increased the number of questions about Africa Check's recent fact-checks that they answered correctly on the endline survey by 0.41 standard deviations ($p < 0.01$). This increased the probability of answering such a question correctly from 0.4 to 0.5.

Second, to measure intent to engage with the fact-checks once the modest incentives were removed, we asked participants whether they wished to receive fact-checks, reminders to pay attention to fake news, or COVID-19 vaccine information from Africa Check after the six months of financial incentives concluded. The results in Figure 4c show that treated respondents with incentives to consume fact-checks became 0.2 standard deviations more likely to subscribe to Africa Check's content ($p < 0.01$). Appendix Table F3 disaggregates the index to show that the probability of treated respondents signing up to receive the WCW podcast after the intervention increased by 14 percentage points from 75%.

However, indicative of the challenges of generating organic demand for corrective information, the treatments that came with placebo quiz incentives resulted in significantly smaller increases in self-reported engagement, knowledge of fact-checks, and intended future take-up. Our results mirror prior findings suggesting that financial incentives can play a key role in activating latent demand for politically salient information (Chen and Yang 2019). An important challenge for fact-checkers is thus to generate appeal at scale. Our finding that financial incentives generated persisting demand suggests that doing so is possible if initial interest can be ignited. Nevertheless,



(a) Podcast take-up

(b) Treatment knowledge

(c) Intended future take-up

Figure 4: Treatment effects on take-up

Notes: All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1). Top panel of Figure 4a excludes *Text* from *Pooled treatment* since they were not sent podcasts; *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals. Appendix Tables F1-F3 report regression results for each index and its components; Appendix Tables F14 and F15 further include LASSO-selected covariates.

the limited effects on treatment take-up among participants assigned to placebo incentives leads us to henceforth focus on those treated respondents assigned to fact-check quiz incentives, who engaged far more intensively with their assigned treatments.

The lower panel within each subfigure indicates that treatment take-up was fairly uniform across different treatment conditions where participants were assigned to fact-check quiz incentives. We detect no differences between the long, short, and empathetic podcast conditions in self-reported podcast listening in Figure 4a or between these conditions and the text condition in intended future take-up in Figure 4c. We do find that participants assigned to the empathetic condition were somewhat more accurate in answering questions about recent fact-checks at endline than the other treatment conditions. Rather than differences in engagement, this could reflect empathetic content increasing users' information internalization. Overall, any differences in subsequent effects across treatment variants, conditional on the assignment of fact-check quiz incentives, are thus unlikely to reflect differential take-up and consumption rates.

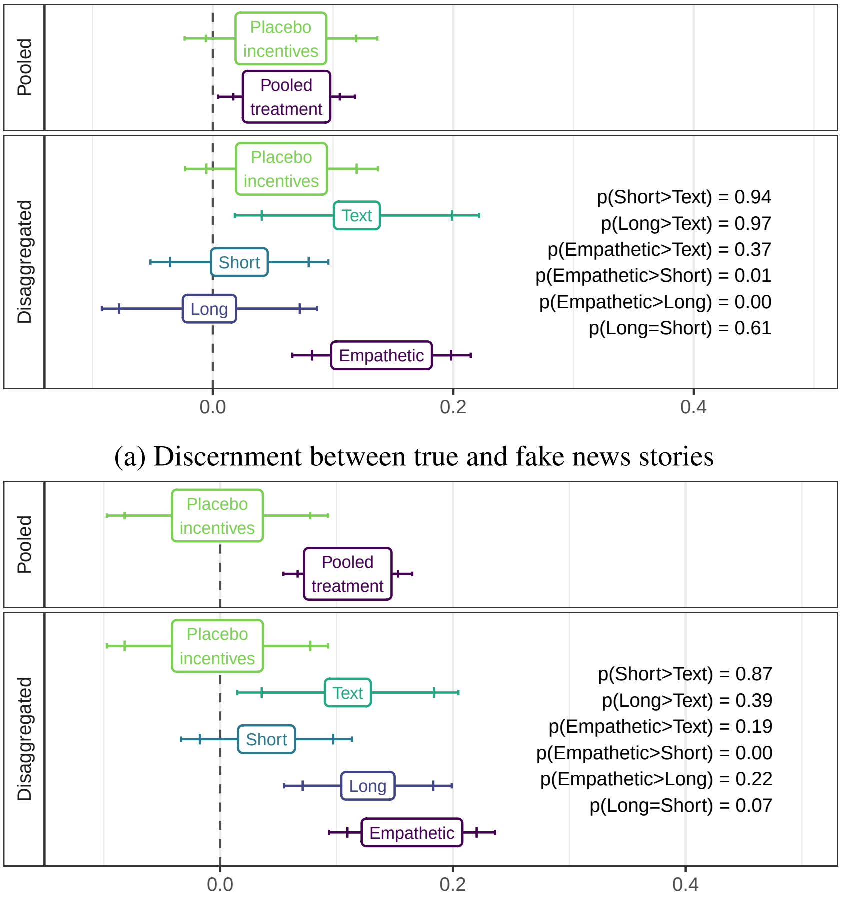
5.2 Discerning fact from fiction

Having demonstrated significant engagement with the fact-checks, we next turn to the broader consequences of treatment. We first show that sustained exposure to fact-checks increased treated respondents' ability to discern between true and false content *upon exposure*. In line with established approaches to measuring discernment (see Guay et al. 2023; Pennycook and Rand 2021), we showed respondents two true and two fake news stories relating to COVID-19 and government policy decisions, which were *not* covered by any Africa Check fact-check during the study period. We asked respondents to indicate how likely they believed each to be true on a five-point scale ranging from not at all likely to very likely. We then reverse-coded false questions and produced an ICW index measuring respondents' discernment between true and false. Figure 5a's upper panel shows that any treatment with fact-check quiz incentives increased respondents' discernment between true and false information at endline relative to the control group by 0.06 standard deviations ($p < 0.05$); consistent with their limited consumption of the fact-checks, respondents who received placebo quizzes showed little improvement in misinformation discernment relative to the control group. Appendix Figures D1a and D1b further show that improved discernment is driven by respondents' greater distrust of false statements rather than their greater trust of true statements, which suggests that treatment did not simply make people more skeptical of everything they see. As the treatment variant tests in the lower panel illustrate, the pooled treatment effect is driven by the text message and empathetic podcast conditions.

Second, we presented participants with four widespread conspiracy theories *not* investigated by Africa Check (listed in Table 2) and similarly asked respondents to indicate how likely each is to be true. In contrast with the discernment measures above, these outcomes—which were not preregistered due to their greater detachment from our treatments—capture skepticism of well-known falsehoods that participants are likely to have encountered in real life (Pennycook and Rand 2020). All responses are reverse-coded such that higher values indicate greater skepticism of the conspiracy theories' likelihood of being true. The upper panel of Figure 5b indicates that pooling treatments with incentives to consume the fact-check quiz increased respondents' skepticism of these conspiracy theories by 0.1 standard deviations, or an average of 0.12 units on our five-point scale ($p < 0.01$). Increased skepticism is driven by the text message and the long and empathetic podcast formats ($p < 0.05$, $p < 0.05$, and $p < 0.01$), which all produced larger effects than the short podcast.

Across participants' ability to discern false from true stories and increased skepticism of conspiracy theories, sustained exposure to fact-checks reduced participants' susceptibility to fake news beyond the fact-checks' narrow content. Supporting hypothesis H4, this suggests that repeated exposure to fact-checks can help to inoculate individuals against misinformation more broadly.

We next consider whether such generalized discernment and skepticism is driven by the broader lessons imparted by Africa Check's fact-checking practices. Suggesting that prebunking is an important component of fact-checks, the upper panel of Figure 6a shows that repeated exposure to fact-checks led respondents to score 0.1 standard deviations higher on our information verification knowledge index ($p < 0.01$), which aggregates 13 items capturing good and bad practices for



(a) Discernment between true and fake news stories

(b) Skepticism of conspiracy theories

Figure 5: Treatment effects on (a) discernment between fake and true news and (b) skepticism of conspiracy theories

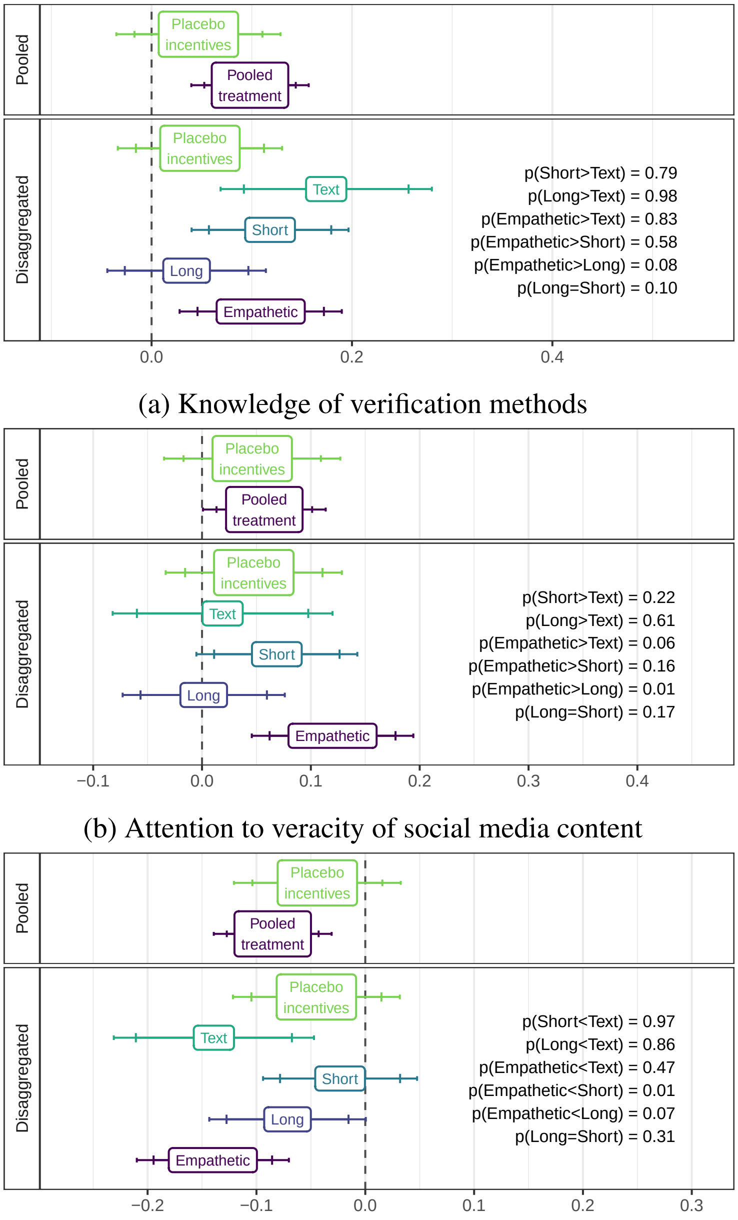
Notes: All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals. Appendix Tables F4 and F5 report regression results for each index and its components; Appendix Tables F14 and F15 further include LASSO-selected covariates.

verifying news. Appendix Table F6 disaggregates the index, showing that this effect principally reflects respondents' greater awareness that they can avoid misinformation by relying on reputable sources or consulting fact-checking institutions and cannot effectively verify information simply by asking others. Similar to our discernment outcomes, the lower panel of Figure 6a shows that the text messages and short and empathetic podcast modes of delivery were notably more effective ($p < 0.01$, $p < 0.01$, and $p < 0.05$, respectively) than the standard long podcast.

In line with hypothesis H2, these lessons appear to translate into increased attention to the veracity of content on social media. Figure 6b examines an index combining three items: listing paying close attention to sources as one of the best ways to avoid being misled by fake news on social media; how important it is to verify information received on social media; and how frequently you pay close attention to content received on social media platforms. The results show a 0.06 standard deviation increase in these measures of perceived importance of paying attention and self-reported attention ($p < 0.05$). This effect is driven, primarily, by respondents becoming 3 percentage points more likely to report that paying close attention to the source of social media content ensures they are not misled by fake news ($p < 0.05$). Comparing across treatment arms, respondents' greater willingness to look twice at content encountered on social media is driven primarily by the empathetic podcast.

Effective inoculation might also reflect greater awareness of platforms that supply a significant share of misinformation, as predicted by hypothesis H3. Aggregating respondents' assessments of the truthfulness of content on social media platforms with the extent of their trust in such content (other than WhatsApp, through which our fact-checks were delivered), the upper panel of Figure 6c shows that the treatments incentivizing participants to consume fact-checks reduced trust in social media platforms by 0.09 standard deviations ($p < 0.01$).[14] The effect is driven by each component of the index; for example, treatment reduced the share of respondents believing that social media

[14] We disaggregate this index into participants' perceptions of the truthfulness of social media content, versus their trust in such content, in Figure E3 and find similar results across each. Figure E4b shows that trust in information from close ties also modestly decreases.



(a) Knowledge of verification methods

(b) Attention to veracity of social media content

(c) Trust in social media (besides WhatsApp)

Figure 6: Treatment effects on news verification knowledge, attention to veracity of social media content, and attitudes towards social media

Notes: All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals. Appendix Tables F6-F8 report regression results for each index and its components; Appendix Tables F14 and F15 further include LASSO-selected covariates.

information sources are credible by 17% ($p < 0.01$) and reduced the extent to which content on social media is believed to be true by 0.08 standard deviations ($p < 0.05$). In line with our previous results, the lower panel shows the largest effects for the text message and empathetic podcast delivery formats ($p < 0.01$ and $p < 0.05$, respectively).

Nevertheless, the intervention did not entirely reshape participants' engagement with their information environment. Despite becoming more knowledgeable about verification methods, Appendix Figure D2 reports no effect on participants' perceptions about the ease of fact-checking. We also find that beliefs about online sources of information did not carry over to traditional media sources. Appendix Figure E4a reports no significant reduction in participants' trust in radio or television news, which tend to be more legitimate sources of information.

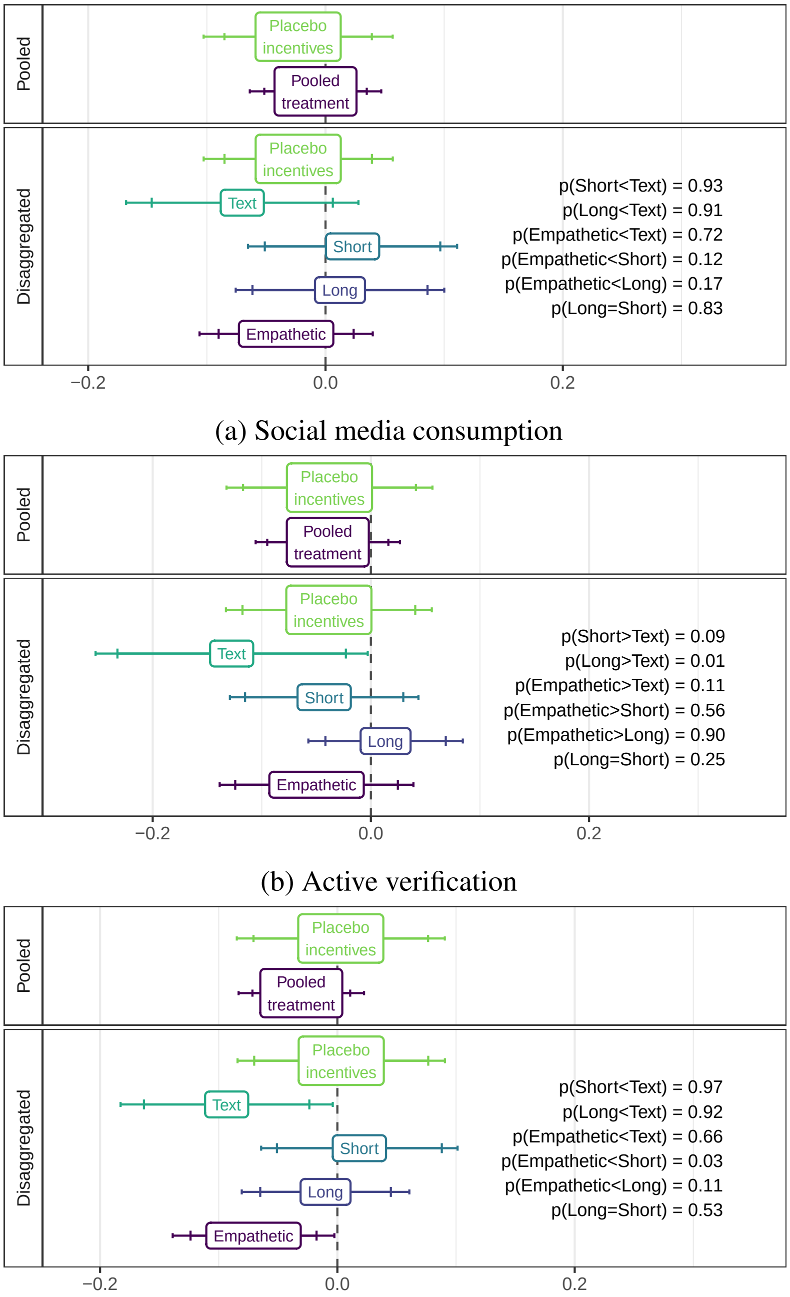
Together, these results indicate that sustained access to fact-checks—especially when expressed in a simple text form or conversationally with empathy—increased respondents' capacity to verify suspicious information, generally doubt content on social media *upon exposure*, and ultimately discern misinformation. Further, the heterogeneity across treatment groups, which all received fact-check quiz incentives and experienced similar effects on fact-check consumption, suggests that *how* fact-checked content was conveyed—rather than differential consumption or the quizzes themselves—was responsible for the generally larger effects of the short text messages and more empathetic podcasts.

5.3 Information consumption, verification, and sharing

Moving beyond efforts to inoculate participants upon exposure to misinformation, hypothesis H5 assesses whether sustained exposure to fact-checks altered the extent of participants' exposure to and engagement with misinformation in the first place. We first examine treatment effects on a self-reported index of the regularity with which respondents use social media to obtain news (again excluding WhatsApp, through which treatments were delivered). Across our pooled and disaggregated estimations, Figure 7a reports substantively small and consistently statistically insignificant treatment effects ($p = 0.45$). Furthermore, Appendix Figure E5 shows that consumption of news from traditional media and close ties were also unaffected. Thus, while individuals learned to scrutinize suspect claims and became less trusting of content on social media, the intervention did not shift *where* individuals got their news in the first place. Given that social media platforms are consumed for many purposes beyond acquiring news, this illustrates the supply-side challenge of limiting misinformation exposure.

We similarly observe limited effects on respondents' active efforts to verify the truth of claims encountered outside the study. Failing to reject the null hypothesis for H6, Figure 7b reports no significant increase in how often respondents reported trying to actively verify information they received through social media on a five-point scale from never to always ($p = 0.27$). Appendix Figure D3 indicates that, while verification efforts through Africa Check did increase, verification through traditional media was crowded out for all treated participants ($p < 0.01$) and verification via online and social media was crowded out for respondents who were sent fact-checks by text ($p < 0.01$). Along with the increase in verification knowledge observed in Figure 6a, these negligible treatment effects on respondents' verification behavior imply that limited *capacity* to verify news stories might not be the only driver of citizens' limited *efforts* to do so.

While sustained exposure to fact-checks did not affect costly decisions to alter media consumption patterns or actively verify information, greater discernment upon exposure to potential misinformation did translate—for participants that received fact-checks via *Text* or the *Empathetic* podcast—into a reduced propensity to share suspected misinformation. Providing some support for hypothesis H5 with respect to sharing, the lower panel of Figure 7c shows that these participants became around 0.1 standard deviations less likely to report sharing content received via social media ($p < 0.05$), or a 0.1 unit reduction on our five-point scale capturing the frequency with which respondents shared news stories they encounter on social media with others. Thus, in addition to becoming more discerning, sustained treatment may limit viral misinformation outbreaks by making individuals more conscientious about the risks of sharing misinformation once exposed.



(a) Social media consumption

(b) Active verification

(c) Sharing

Figure 7: Treatment effects on social media consumption, verification, and sharing

Notes: All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); p-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals. Appendix Tables F9-F11 report regression results for each index and its components; Appendix Tables F14 and F15 further include LASSO-selected covariates.

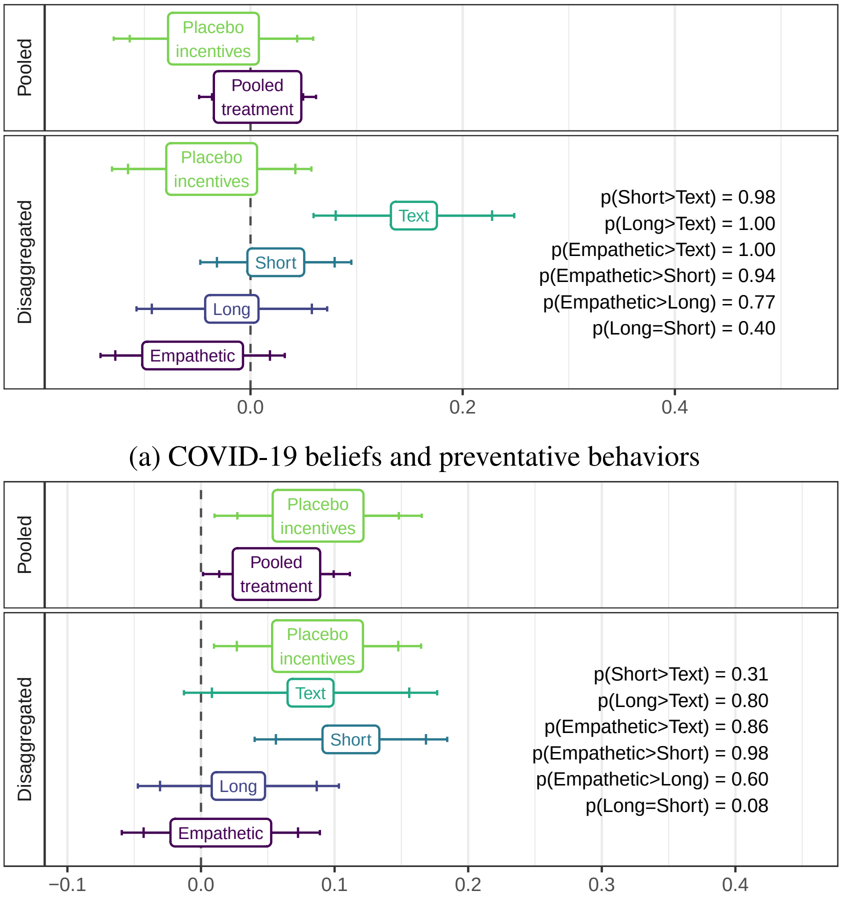
5.4 Attitudes and behaviors relating to COVID-19 and government

We finally turn to hypotheses H7 and H8, which assess the political consequences of sustained exposure to fact-checks. A significant share of viral misinformation during the study period related to the COVID-19 pandemic, government officials and policies, and politically salient social issues. By emphasizing false cures or casting doubt on the severity of the pandemic, health-related misinformation risked reducing citizens' compliance with preventative behaviors; exposure to politics-related misinformation would potentially further reduce citizens' trust in formal political institutions. Corresponding fact-checks generally then corrected false claims about COVID-19 and often portrayed incumbent politicians' performance in a more favorable light by casting doubt on outlandish falsehoods.

For our final set of outcomes, we measure effects on indexes of attitudes and self-reported behaviors relating to COVID-19 and politics to assess whether the treatment mitigated the negative downstream consequences typically associated with exposure to misinformation. Since these outcomes are not connected directly to the fact-checks, this enables us to test whether sustained efforts to combat salient misinformation influenced participants' perspectives on public health and politics more broadly.

Overall, we detect modest but some significant effects after six months of exposure to fact-checks on such beliefs and behaviors. Figure 8a generally reports no treatment effect on COVID-19 beliefs and preventative behavior for the three podcast treatments with fact-check quiz incentives. However, providing some support for hypothesis H7, we find that fact-checks delivered by short and simple text messages increased an index of health-conscious outcomes associated with COVID-19 by 0.14 standard deviations ($p < 0.01$). Examining the components of the index separately, Appendix Table F12 indicates that the effects of the text-only treatment are driven by significant increases in respondents' willingness to comply with government policies by getting vaccinated, wearing a mask, and reducing indoor activity.

In line with hypothesis H8, Figure 8b reports an increase in favorable views toward the government—measured in terms of government performance appraisals, trust in government, and intentions to



(a) COVID-19 beliefs and preventative behaviors

(b) Views and attitudes about the government

Figure 8: Treatment effects on COVID-19 beliefs and preventative behaviors and views and attitudes about the government

Notes: All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals. Appendix Tables F12 and F13 report regression results for each index and its components; Appendix Tables F14 and F15 further include LASSO-selected covariates.

vote for their region's incumbent party—across treatment conditions. The pooled treatment effect of 0.06 standard deviations ($p < 0.05$) is largely driven by the text message format—although the coefficient is not quite statistically significant ($p = 0.11$)—and the short podcast ($p < 0.05$). Appendix Table F13 shows that these effects are primarily driven by significant increases in the extent to which respondents reported trusting information from politicians and the government most, as well as more favorable evaluations of government performance and willingness to vote for incumbent parties.

These results indicate that broader politically-relevant beliefs and behaviors are harder to move than the capacity to discern fact from fiction. Nevertheless, our findings suggest that the greater discernment and verification knowledge inspired by sustained exposure to fact-checks may start to push individuals to make fact-based judgments in their private and political lives as well. In particular, text messages that can be consumed at little cost appear to help combat misinformation-induced perspectives of polarizing issues.

6 Conclusion

Due to its potentially negative consequences for political and health-related behaviors, misinformation on social media is a growing concern around the globe. Recent studies have advanced our understanding of how to mitigate the consumption of, and susceptibility to, misinformation online. But most struggle to explain how sustained changes in beliefs and behaviors can be achieved beyond single-shot treatments with short-term effects.

In addition to estimating effects of sustained exposure to fact-checks, we explored two key challenges in a world where many factors compete for citizens' attention: how to generate *persistent consumption* of corrective information and how to induce *internalization* of the lessons imparted by fact-check content. Our sustained intervention allowed us to examine whether fact-checking can play both debunking and prebunking roles by correcting existing misinformation and warning participants about future misinformation. Partnering with an existing fact-checking organization, Africa Check, also highlights the relatively low cost and scalability of the intervention.

Our study yields several key conclusions. First, it is feasible to stimulate citizens to consume fact-checking content delivered through WhatsApp. Modest financial incentives helped to induce consumption in our South African sample; once the incentives were removed, treated participants expressed their desire to continue receiving Africa Check's content. Consequently, while organic consumption was difficult to generate at the beginning, an initial push towards consumption may subsequently activate latent demand. Once high-quality fact-checking content is available, policymakers and researchers should therefore focus on generating initial exposure by: (i) finding ways to cultivate citizen demand for fact-checking content, whether by increasing their perceived importance or by capturing spillovers by enhancing their entertainment value; or (ii) using tools like media campaigns, trusted community leaders or online influencers, or school curricula to, at least initially, disseminate fact-checking content widely.

Second, sustained exposure to fact-checks helped to inoculate participants upon exposure to misinformation. While treated participants did not report altering behaviors that limit exposure to misinformation in the first place or active verification efforts, the intervention increased participants' attention to veracity as well as capacity to discern fact from fiction and willingness to act on this by not sharing unverified online content. Since the effects we observe are relatively small in magnitude, it is imperative to increase the efficacy of inoculation efforts beyond the effects we document in this study. Such efforts should complement supply-side efforts targeting the production and promotion of misinformation.

Third, not all treatment arms performed equally: the simple text-only treatment and empathetic podcast treatments were the most effective delivery mechanisms for internalizing fact-checking messages. Our results thus suggest that repeated, short, and sharply-presented factual proclamations from a credible source are more likely to train people to approach information more critically than longer-form edutainment—unless such content prioritizes empathizing with consumers.

Finally, our results suggest that combating misinformation can be politically consequential. Although not all types of fact-checks generated significant effects, we find that sustained exposure to fact-checks made citizens somewhat more compliant with government policies and more trusting in incumbent governments. As such, text-based fact-checks that could be consumed almost costlessly helped to reverse two important concerns of the social media age, reduced state capacity and declining faith in government.

While our findings illustrate the potential for sustained fact-checking interventions to make citizens more discerning of the content they consume, several limitations point to avenues for future research. First, although fact-checks were consumed in a natural environment, we still recruited participants for an academic study. Since willing participants are likely to be at least somewhat interested in subscribing to online content, further research should examine whether our findings are larger or smaller in broader populations with lower barriers to opt-in and lower baseline levels of interest in and knowledge of fact-checking. Second, our treatment effects were strongest for participants with financial incentives to consume fact-checking content. As such, we demonstrate that the intervention can be effective, but more organically generating repeated exposure to fact-checks remains a critical outstanding research question. Third, this study largely relies on self-reported survey responses. Although our findings are unlikely to be driven by experimental demand effects (for reasons discussed above), studies conducted in more naturalistic environments would benefit from linking fact-checking interventions to behavioral outcomes. Finally, there is more to be learned about which aspects of fact-checks make citizens more discerning. Our findings point to trust in content's source and a general understanding of when and how to exercise skepticism as key mechanisms, but future research could further break apart the components of a typical fact-check to identify which components to emphasize.

South Africa's salient political issues—pandemic responses, identity politics, and economic concerns—are reflected in politics globally and contribute to misinformation worldwide. Our findings show that similar mechanisms that have helped to combat misinformation relating to polarizing issues in the Global North can generalize to Global South contexts, and thus advance existing scholarship on limits misinformation's potential for adverse effects (Blair et al. 2024; Cook, Lewandowsky and Ecker 2017; Nyhan 2020; Walter et al. 2020). However, insofar as the problems of misinformation are exacerbated by the use of closed platforms such as WhatsApp, and by lower digital literacy in part due to costs associated with data usage, our study's context highlights that the challenges of misinformation—and need for low-cost and scalable solutions—is especially pressing in the Global South. However, our findings are more optimistic than prior media literacy studies in the Global South (Badrinathan 2021; Guess et al. 2020), suggesting that sustained observational learning via social media platforms themselves can help to combat misinformation.

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Online Appendix

Sustaining Exposure to Fact-checks: Misinformation Discernment, Media Consumption, and its Political Implications

Table of Contents

A Methods

A.1 Recruitment and low-quality responses

To target a reasonably representative sample of the adult population of Facebook users in South Africa, recruitment ads on Facebook were stratified at the province-gender-age level, generating a total of 54 different ads that were targeted on the basis of the user's: (i) province (of which there are 9); (ii) gender; and (iii) age bracket (18-29, 30-49, or above 50 years old). Figure C1a provides an example of a recruitment ad, explaining that participants will receive airtime for participating in a social media study in South Africa.

Low-quality respondents were removed during the recruitment process using three attention-checking questions within the baseline survey. Questions were designed to be easy to respond to if respondents read the question somewhat carefully (e.g. "What year is it?"). We further restricted the sample to respondents who completed the baseline in more than eight minutes, which pilots of the baseline survey suggested was the minimum time required for the baseline survey to be comprehended and completed. Respondents who did not pass either check were excluded from randomization; consequently, dropped respondents are not correlated with treatment assignment. Their WhatsApp numbers were also prevented from restarting the baseline survey.

A.2 Randomization

We blocked-randomized individuals approximately once every two weeks by demographics, social media consumption, trust towards different news sources, and knowledge about misinformation. Figure 3 indicates the probabilities that participants were assigned to control and each treatment arm. We assigned more of the sample to the podcast treatments relative to the text information treatment to improve our statistical power to detect differences across the more similar podcast treatment conditions. We used the `R` package `blocktools` to assign blocks, batch by batch, based on a greedy algorithm using Mahalanobis distance over seven predetermined baseline covariates. Our nested blocking strategy involved first creating blocks of size 38 (to ensure whole numbers of respondents were assigned across the various treatment combinations within a block) and then creating smaller sub-blocks of size 19 within each block. Our regression analyses use the blocks of size 38 rather than 19 because attrition often leaves the sub-blocks with missing treatment arms at endline. Whether we use the larger or smaller block fixed effects, results remain substantively unchanged.

A.3 Quiz administration

Participants were randomly assigned to take either *fact-check* quizzes (which served as incentivization to consume fact-checks) or *placebo* quizzes (which were meant to ensure similar levels of study engagement). Both quiz types were administered by the research team, and participants were asked six questions once every four weeks and informed that the quizzes were entirely voluntary. If they decided to take the quiz, they would earn R10 (0.62 USD) and would earn a further R10 if they answered at least four out of the six questions correctly. *Fact-check* quizzes covered information provided as part of the fact-checks over the past month, while *placebo* quizzes covered pop culture questions. Regardless of quiz type, participants were informed *how many* questions they answered correctly, but they were not told *which* questions they answered correctly. We did not provide answers to mitigate the risk of the quiz informing participants directly. Although the intervention did not forcibly inform participants of what is and is not true, it provided easy to use tools for participants to do so if they wanted to.

A.4 Financial compensation

We provided small financial compensation (mobile airtime credits) to induce participation and continued engagement. Respondents who fulfilled all conditions for study enrollment (see above) received R30 (1.90 USD) in airtime. For each quiz, regardless of quiz type, respondents received R10 (0.62 USD) if they completed the quiz and an additional R10 if they answered a majority of the questions correctly. For a short midline survey, the results of which we do not report in the manuscript due to their broad similarity with the endline survey but with a smaller set of outcomes, respondents were provided R30 for completion and an additional R10 if they answered a majority of the quiz questions embedded in the midline survey correctly. For the endline survey, respondents received R40 (2.50 USD) and an additional R10 if they answered a majority of the quiz questions embedded in the endline survey correctly. On average, endline respondents received a total of R155 (9.74 USD) through all components of the study. Figure C3a documents the share of participants completing each of the four quiz interim quizzes during the study (excluding midline and endline survey quizzes) during a given batch's study period, and the share of those completing each quiz who answered a majority of the questions correctly (and hence received high incentive payments).

A.5 Research ethics

The design of our intervention reflected careful attention to the ethics of field experimentation and associated data collection consistent with APSA's *Principles and Guidance for Human Subjects Research* (2020).

First, regarding the intervention itself, our expectation was that each treatment arm would positively affect participants' ability to discern potentially harmful misinformation. This is because the interventions uniformly delivered misinformation-correcting information. While we preregistered theoretical expectations of *differences* between treatment arms in the magnitude of these positive effects, we did not anticipate—and, indeed, do not find—that any treatment arm would have effects consistent with potentially harmful welfare consequences. At the same time, participants assigned to control were not prevented from independently signing up to receive fact-checking programming from Africa Check outside of the confines of the study.

Second, regarding participation and consent, we solicited informed consent from all participants in the study and did not use deception relating to the study's purpose. Participants were free to take, or not take, the optional monthly quizzes as well as the subsequent surveys. While we did use financial incentives in the form of mobile airtime transfers (see Section A.4), these were relatively small overall and served as small incentives to maintain the engagement of participants through a relatively long study period overall. Participants were free to leave the study at any time, all their responses were anonymized, and we anticipated that participants would face no retaliation or repercussions from taking part in the study.

Third, regarding the broader impact of the study, we expected that the limited sample size would render any wider political consequences highly unlikely (beyond informing the programming strategy of the implementing partner). While we collaborated with Africa Check to implement the study, they had no ability to veto or review study conclusions prior to writing the paper and the authors have no conflicts of interest relating to the organization.

A.6 Outcome measurement

All our main outcomes are inverse covariance weighted (ICW) indexes (see Anderson 2008). Each such outcome aggregates families of individual survey items, and is standardized with respect to the control group mean and standard deviation. Each grouping of outcomes contains several ICW outcome indexes capturing different types of outcome within the family. These groupings are provided in Table 2.

Missing responses were imputed as follows. "Don't know" responses to specific questions were coded as "negative" responses relative to the expected treatment effect sign, which were all normalized to positive; e.g. when the respondents were asked about listening to podcasts, "Don't know" is coded as "Never." Similarly for the importance of an issue, "Don't know" is coded as "Not at all important". In turn, when "Don't know" relates to a Likert scale, "Don't know" is coded as the median/neutral option (e.g. as "neither agree not disagree").

The final indexes we settled on largely conform with the indexes specified in the pre-analysis plan. Due to capturing similar concepts, we merged hypotheses H2 and H3 in our pre-analysis plan into a combined H3 in the paper; Figures E3a and E4b report the results separately. Due to this merge and the order that results are presented in the paper, H2, H4, H5, H6, and H7 in the paper correspond to H5, H6, H4, H7, and H9 in our pre-analysis plan. We note below some deviations from our pre-specified measurement strategy; these changes were designed to focus attention on theoretically-relevant outcomes.

First, for exposure to the intervention (H1), we examine podcast take-up and knowledge of the content of the podcast separately to distinguish self-reported attention from internalization; we excluded a pre-specified index item about the frequency with which participants report being alerted to fake news across all social media platforms because we viewed this as a more general test of a distinct mechanism proposed in the literature on accuracy primes (e.g. Pennycook et al. 2021) rather than being a direct measure of exposure to our specific intervention; we find limited support for it (see Figure E1). We further examined *future* take-up as a separate indicator of treatment take-up once the small financial incentives to participate in the study had been removed, but this did not alter our pre-specified approach.

Second, for perceptions of misinformation and trust in social media (H3), the trust in social media component focuses on Facebook, Instagram, and Twitter. Because we merged perceptions of the extent of misinformation on social media, our index also included questions asking what source is trusted most for information and how much of the information received from social media platforms is likely to be true. We exclude all questions relating to WhatsApp because the fact-checking intervention was delivered via WhatsApp and hence results are difficult to interpret. Figure E4b shows that trust in information from close ties, again excluding information sent by these ties from WhatsApp, modestly decreases.

Third, for discernment (H4), our outcomes relating to conspiracy theories were not pre-registered due to their greater detachment from our treatments, but provide a valuable check on citizen evaluations of claims that could be the subject of misinformation. Additionally, we pre-registered the use of a conjoint experiment for the discernment outcomes. In its intended implementation, we sought to measure source credibility as a mechanism for discernment: respondents were meant to be randomly assigned to a slightly different version of each claim which added information relating to sourcing of that claim—whether from the National Institute on Alcohol and Alcoholism (NIAA, for the true claim about alcohol and COVID-19), Facebook (for the false claim about matric marks), the WHO (for the true claim about COVID-19 transmission), or the Ministry of Finance (for the false claim about foreign restaurant workers). We exclude this analysis due to an implementation failure which led nearly half of the batches in our study to be sent only one version of the relevant claim. In addition, interpretation of the individual findings was ambiguous. This was particularly true where the source may have seemed credible to participants at its face (Ministry of Finance) but the claim was false (foreign restaurant workers)—leading to possibly conflicting effects depending on whether discernment was driven by source credibility or increased skepticism. Within two other items, adding credible institutions—the NIAA and WHO—as information sources for true claims weakly reduced discernment in one instance (COVID-19 transmission claim) and had no clear effect on the other (alcohol reduces ability to fight COVID-19 infections). These results could be because credible sources are independent from, or serve as substitutes, for true claims. However, our limited power to conduct this analysis due to the implementation failure, plus the ambiguity of the results, means we do not present results of this analysis in the manuscript.

Fourth, for consumption and sharing of social media content (H5), we again exclude WhatsApp for the same reason as for H3. We also examine the consumption and sharing of information separately to examine effects on both important outcomes.

Fifth, for engaging in fact-checking (H6), we distinguish between active verification efforts and knowledge about the correct way to verify information. For active verification, we solely focus on the frequency with which a respondent reports fact-checking information (see Figure 7b and Table F10). We use the following variables for knowledge on how to verify: the perceived importance of fact-checking, verifying by seeking out dedicated fact-checkers, and levels of knowledge about how and where to check misinformation (see Figure 6a and Table F6). We exclude the pre-specified variable on whether respondents share fact-checks with friends and family, as that does not fall appropriately into either active verification or knowledge of how to verify information (see Figure E2).

Finally, for attitudes toward the government (H7), we deviate from the pre-analysis plan in three ways to focus on trust in and appraisals of government politicians and performance: (i) we add items relating to trust in government and politicians and the information they provide (see Figure 8b); (ii) we exclude two questions eliciting perceptions of government capacity (see Figure E6 for results) and two questions on populism-related beliefs (see Figure E7 for results), on the basis that these questions were worded to capture beliefs about how government *ought* to behave rather than concrete government appraisals; and (iii) we add willingness to vote regional incumbents to the index alongside our pre-specified measure of willingness to vote for the national government since the intervention could shape updating about either level of government.

A.7 Demand effects

Because our outcomes are derived from survey measures, participants who were assigned to treatment arms, in principle, may have responded to questions based on perceptions of what answers were more desirable. We provide evidence against social desirability bias in three ways.

First, social desirability bias is unlikely to account for differences across treatment arms. Consistent differences in treatment effects across the treatment arms suggest that particular components of the intervention did elicit real change in participants' knowledge and beliefs about information from online news media.

Second, results from questions that test participants' capacity to discern true from false news and their ability to identify conspiracy theories require knowledge of correct answers. The information in these two sets of questions were *not* covered by the information Africa Check delivered weekly. These knowledge questions are difficult to falsify, as they require participants to be aware of current events and better adjudicate a piece of news' credibility. Moreover, treated participants were better able to recall treatment content and identify plausible verification methods—other outcomes that are less susceptible to social desirability bias.

Third, demand effects are unlikely to explain our set of results, which show differences between the intervention's success in increasing participants' knowledge and awareness versus actual behavioral change. If participants who were assigned to treatment arms selected socially desirable survey responses, we would expect participants to also report greater behavioral changes with respect to social media consumption and active verification of online content. Our findings indicate that this is not the case: estimated treatment effects suggest that actual behavior with respect to social media interaction is hard to shift despite consistent exposure to the intervention.

Finally, we examine a behavioral outcome that is unlikely to be affected by social desirability bias. Every treatment delivery from Africa Check also included a message that encouraged participants to submit fact-checking requests to discern true participant interest in the fact-checking information. Participants could submit text or forward videos, pictures, or links to the Africa Check phone number for fact-checking. Estimates in Figure E8 show that treated participants were indeed more likely to submit fact-check requests. Moreover, the greater effectiveness of the text message version of treatment, in comparison to the other treatment arms, is consistent with our other survey outcomes and assuages concerns about demand effects across the study.

B Examples of treatment

B.1 Examples of fact-checks

The fact-checks conducted by Africa Check's were deemed true, false, misleading, or uncertain (unsubstantiated). Figure 1 shows that these fact-checks covered (broadly) eight families of issues but often touch upon more than one set of issues. Below are examples of each type of issue:

- **Politics:** "Did a R200m Covid-19 vaccine tender go to the daughter of South African premier? This is incorrect!"

- **Economy:** “Beware of false job adverts for the South African police. It’s a job scam.”
- **Race/Xenophobia:** “Did a recent tweet by Julius Malema encourage attacks on ‘racist farms’? No, it’s fake!”
- **COVID-19:** “No, a World Health Organization head didn’t say Covid vaccines kill kids.”
- **Other Health:** “There is no scientific evidence that a mixture of bitter gourd leaves and snails is a remedy for stroke.”
- **Crime:** “Has the murder rate for the North West nearly doubled from 2020 to 2021? Yes, but the Covid-19 lockdown skewed the comparison.”
- **Society:** “Are there 5.6 billion women in the world to just 2.2 billion men? Nope, not even close!”
- **Miscellaneous fun facts:** “There is no elephant-shaped mountain in Oregon, US – the image that has been circulating was photoshopped by an artist.”

B.2 Examples of empathetic addition to podcast

- “Misinformation about vaccine and vaccine mandates can be scary. Especially when it suggests that we may be forced to do something or the vaccines could have side effects. So it’s really important that we check claims like this before we pass them on.”
- “With the rising number of daily COVID-19 positive cases and of course the new variant, many people may be feeling anxious about an onset of cold or flu symptoms. Even seasonal allergies. And the panic around this may lead you to fall for misinformation on how to mitigate symptoms as well as unverified remedies on how to get better quicker. Which is the case with this claim.”
- “You may have seen pictures or videos shared on social media of gas or paraffin heater incidents that led to serious burn-related injuries. And this first claim may make you feel anxious or fear for the safety of your friends or family members who regularly use these appliances. And you might want to share safety hacks to protect your loved ones and to caution them to take extra care to avoid danger with appliances this winter. But sometimes, these aren’t entirely true...”

B.3 Treatment delivery message primes

All treatment arms included a short message that accompanied the delivery of the treatment. Within each treatment arm, a random half of the participants received a message that simply introduced the fact-check information being delivered (*Factual*), while the other half received a message that primed participants about the information’s importance to encourage consumption of the fact-check material (*Prime*). We expected treatment effects to be particularly concentrated among participants assigned to *Prime* rather than *Factual* messages.

For our main analysis, we focus on the preregistered approach of pooling the *Factual* and *Prime* messages within each form of treatment. We now examine potential complementarities between these treatments and the *Prime* message. We return to examine the outcomes for which *Text* and all podcast treatments produced significant impacts: discernment between fake and true information; identification of conspiracy theories; and verification knowledge. The variation in treatment delivery message does not induce clear differential effects on our other outcomes.

The message priming the social importance of misinformation increased discernment (results omitted due to length constraints and available upon request). Across two treatment arms—*Text* and *Empathetic* podcast paired with *Fact-check* quizzes—we find that messages with the social *Prime* significantly increased the likelihood that participants were able to discern between fake and true information. While the incentivized *Long* podcast also performed better when paired with a *Prime* message, the treatment combination is not statistically distinguishable from the *Control* condition. We similarly find that the *Prime* message amplified the impact of other treatments on the likelihood of doubting conspiracy theories. When primed, participants were more likely to identify conspiracy theories across three incentivized treatment arms: the *Text* treatment, the *Long* podcast, and the *Empathetic podcast*. Moreover, the *Prime* message—when paired with the incentivized *Text*, *Short* podcast, and *Empathetic* podcast—was once again significantly more likely to help participants identify correct strategies for verifying information.

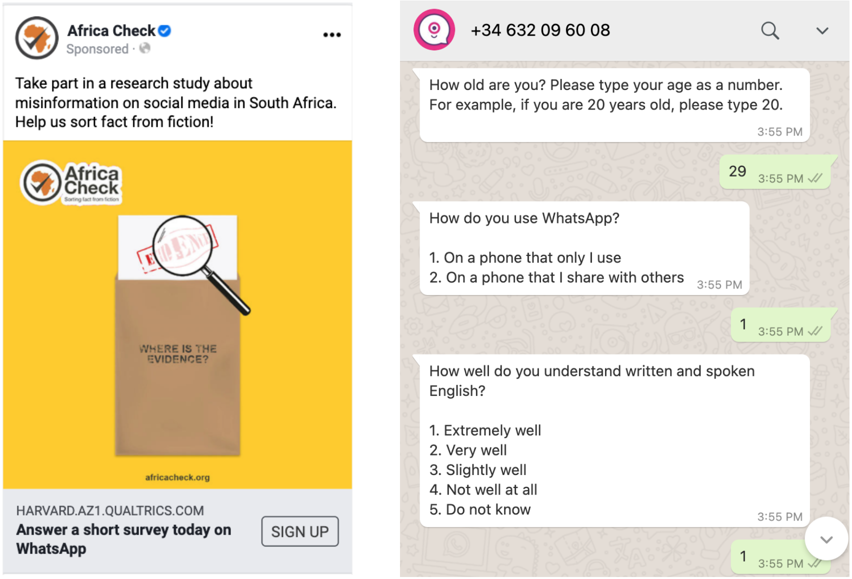
Overall, we find evidence consistent with the inclusion of a *Prime* message when encouraging participants to internalize their assigned treatments—particularly for the incentivized *Text* and *Empathetic* podcasts. These originally identified effects are then amplified by a *Prime* message which repeatedly reminded participants of fact-checking's importance. Because the prime did not increase reported *consumption* but did increase knowledge about its content, the results are primarily driven by participants' *internalization* upon exposure.

B.4 Examples of additional prime in delivery message

- "Myth busters and fake news debunkers play a vital role in checking the facts online! Here are the facts about three viral online messages so you can prevent your friends and family from being fooled by false information."
- "False information can be dangerous. Sometimes it can be deadly. Play your part in sharing accurate information online to help protect your friends and family. Here are the facts about three viral online messages:"
- "False and misleading information can be dangerous. When it comes to health issues, it can be deadly. Verify before you share message online to keep your fiends and family safe. They'll thank you for it! We've fact-checked three viral messages for you:"

C Study design

C.1 Figures



Africa Check
Sponsored ·

Take part in a research study about misinformation on social media in South Africa. Help us sort fact from fiction!

HARVARD.AZ1.QUALTRICS.COM
Answer a short survey today on WhatsApp

+34 632 09 60 08

How old are you? Please type your age as a number. For example, if you are 20 years old, please type 20. 3:55 PM

29 3:55 PM

How do you use WhatsApp?

1. On a phone that only I use
2. On a phone that I share with others 3:55 PM

1 3:55 PM

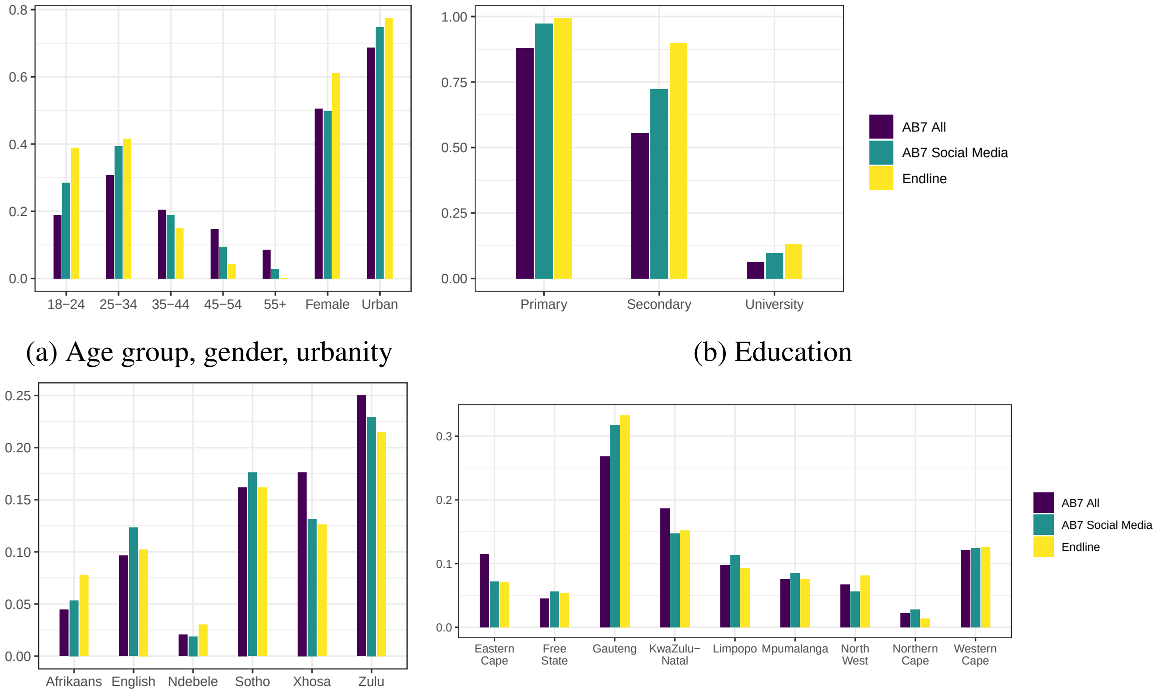
How well do you understand written and spoken English?

1. Extremely well
2. Very well
3. Slightly well
4. Not well at all
5. Do not know 3:55 PM

1 3:55 PM

(a) Recruitment Facebook ad (b) Survey through WhatsApp chatbot

Figure C1: Recruitment and surveying



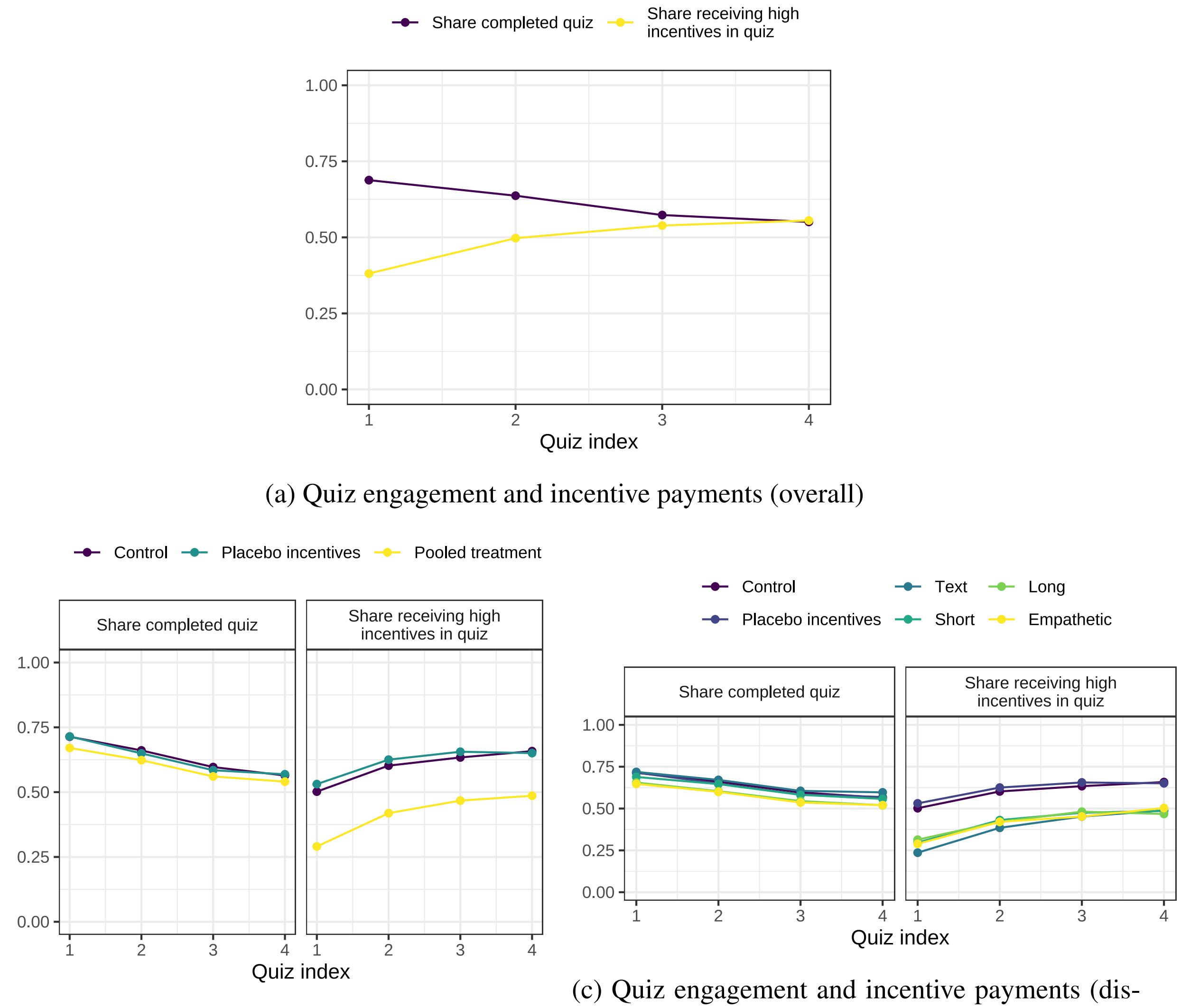
(a) Age group, gender, urbanity

(b) Education

(c) Ethnicity

(d) Province

Figure C2: Comparison of endline sample with Afrobarometer round 7 (2018)



(a) Quiz engagement and incentive payments (overall)

(b) Quiz engagement and incentive payments (pooled treatment)

(c) Quiz engagement and incentive payments (disaggregated treatment)

Figure C3: Quiz engagement over study

Notes: Figure plots average participation, and the average share of participants answering more than 50% of questions correctly (and hence receiving a larger incentive payment for completing the quiz), through each of the four study quizzes (fact-check or placebo) participants were sent between baseline and endline.

C.2 Balance and attrition

Table C1: Attrition

	Attrition	
	(1)	(2)
A. Pooled estimation		
Placebo incentives	0.023	0.021
	(0.017)	(0.016)
	[0.172]	[0.209]
Pooled treatment	-0.014	-0.017
	(0.012)	(0.012)
	[0.220]	[0.137]
B. Disaggregated estimation		
Placebo incentives	0.023	0.021
	(0.017)	(0.017)
	[0.171]	[0.197]
Text information	-0.022	-0.026
	(0.021)	(0.021)
	[0.302]	[0.215]
Short podcast	0.002	-0.003
	(0.016)	(0.015)
	[0.878]	[0.846]
Long podcast	-0.021	-0.022
	(0.015)	(0.015)
	[0.172]	[0.156]
Empathetic podcast	-0.021	-0.022
	(0.016)	(0.015)
	[0.171]	[0.145]
Controls	×	✓
Directional hypothesis	×	×
Control Mean	0.51	0.51
Control SD	0.50	0.50
R^2	0.12	0.16
Observations	8947	8947

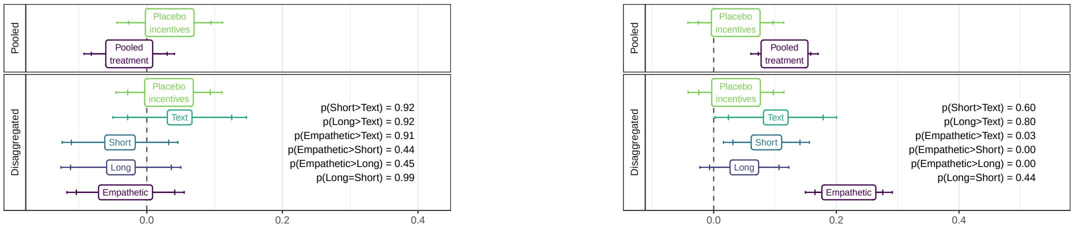
Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets.

Table C2: Balance on pre-treatment outcomes

Variable	$p(\tau_{pooled} = 0)$	$p(\tau_{disagg.} = 0)$
A. Socio-demographic		
Gender: Female	[0.990]	[0.666]
Locality: Urban	[0.573]	[0.297]
Locality: Peri-urban	[0.572]	[0.909]
Locality: Rural	[0.558]	[0.796]
Age: 18-24	[0.791]	[0.620]
Age: 25-34	[0.176]	[0.463]
Age: 35-44	[0.518]	[0.761]
Age: 45-54	[0.147]	[0.095]
Age: 55+	[0.371]	[0.441]
Education: Primary	[0.495]	[0.204]
Education: Secondary	[0.857]	[0.744]
Education: University	[0.790]	[0.707]
Province: Eastern Cape	[0.328]	[0.643]
Province: Free State	[0.629]	[0.898]
Province: Gauteng	[0.870]	[0.994]
Province: KwaZulu-Natal	[0.796]	[0.388]
Province: Limpopo	[0.956]	[0.512]
Province: Mpumalanga	[0.499]	[0.138]
Province: Northern Cape	[0.032]	[0.204]
Province: North West	[0.271]	[0.664]
Province: Western Cape	[0.493]	[0.879]
B. Baseline survey responses		
Verify challenge	[0.430]	[0.783]
Consume close friends	[0.784]	[0.917]
Consume social media	[0.190]	[0.426]
Consume traditional media	[0.257]	[0.345]
Consume WhatsApp	[0.409]	[0.834]
COVID-19 beliefs and behavior	[0.159]	[0.465]
Podcast take-up	[0.877]	[0.905]
First stage placebo	[0.609]	[0.603]
Misinformation harmful	[0.878]	[0.501]
Sharing	[0.962]	[0.715]
Trust close friends	[0.663]	[0.806]
Trust social media	[0.482]	[0.747]
Trust organizations	[0.989]	[0.872]
Trust traditional media	[0.850]	[0.930]
Trust WhatsApp	[0.562]	[0.903]
Active verification	[0.722]	[0.179]
Verification knowledge	[0.161]	[0.271]

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects. $p(\tau_{pooled} = 0)$ provides the p-value from a test of joint significance of coefficients in the pooled estimation (control; placebo incentives; pooled treatment); $p(\tau_{disagg.} = 0)$ provides the p-value from a test of joint significance of coefficients in the disaggregated estimation (control; placebo incentives; text; short; long; empathetic).

D Figures referenced in main text



(a) Correct discernment of true news stories

(b) Correct discernment of false news stories

Figure D1: Treatment effects on discernment between fake and true news

Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): level of confidence in truthful claims about how COVID spreads (true) and if alcohol exacerbates infections (true); (b) lack of confidence in false claims about inflation of matriculation exam scores (false) and most workers being immigrants (false). Estimated using Equation (1); while the interior and exterior bars represent 90% and 95% confidence intervals.

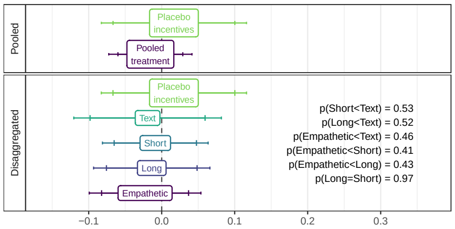
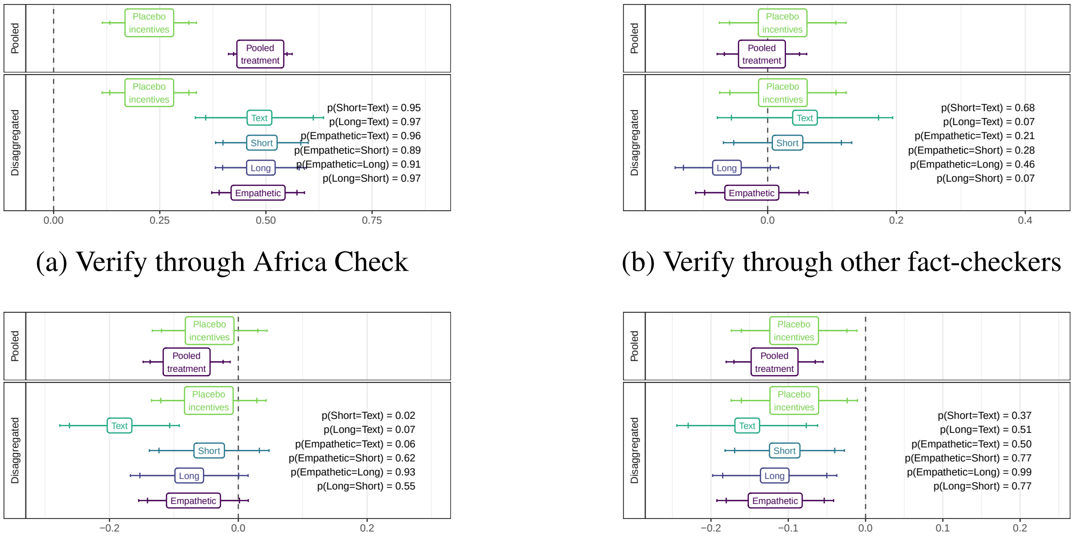


Figure D2: Treatment effects on difficulty of fact-checking

Notes: All outcomes are standardized inverse covariance-weighted indexes: Challenging to verify information due to knowledge, irrelevant fact-checks, distrust fact-checkers, too expensive, overwhelming information, takes too long. Estimated using Equation (1); while the interior and exterior bars represent 90% and 95% confidence intervals.



(a) Verify through Africa Check

(b) Verify through other fact-checkers

(c) Verify through online and social media

(d) Verify through traditional media

Figure D3: Treatment effects on the use of different information sources for verification

Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): lists WCW as a source for fact-checking; (b) lists AFP or Snopes as a source; (c) lists Facebook, Google, Moya, Telegram, Twitter, WhatsApp, or YouTube as a source; (d) lists News24 or SABC as a source. Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); p-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.

E Figures referenced in supplementary materials and PAP

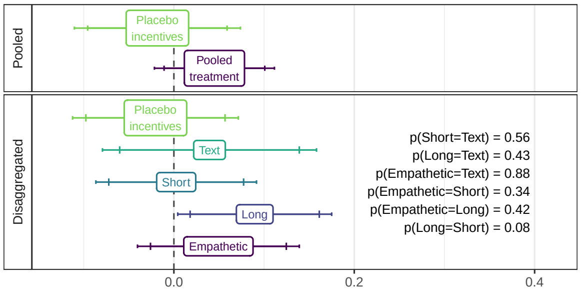


Figure E1: Being alerted about fake news

Notes: Outcome is standardized: How often participant is alerted about fake news. All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.

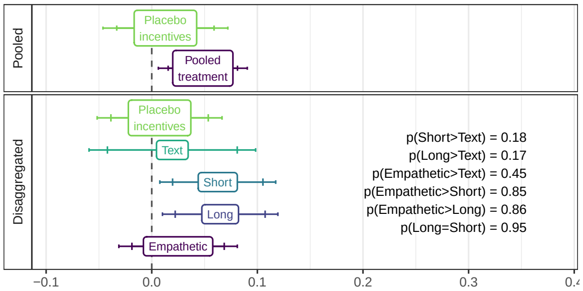
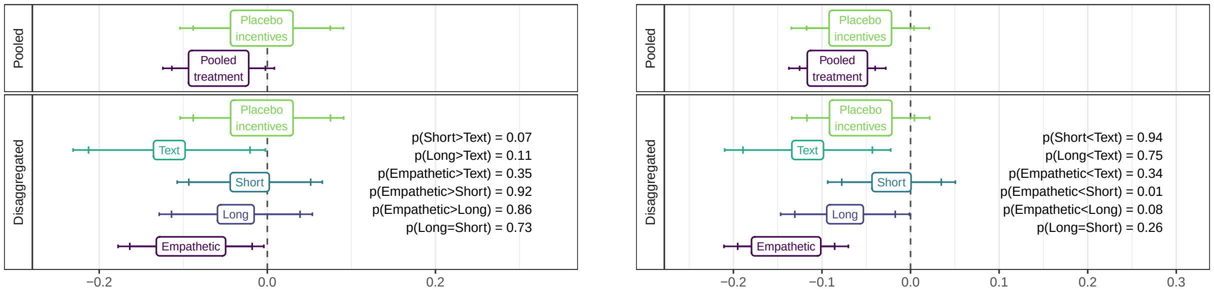


Figure E2: Alerting others about fake news

Notes: Outcome is standardized: How often participant reports alerting others about misinformation. All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.

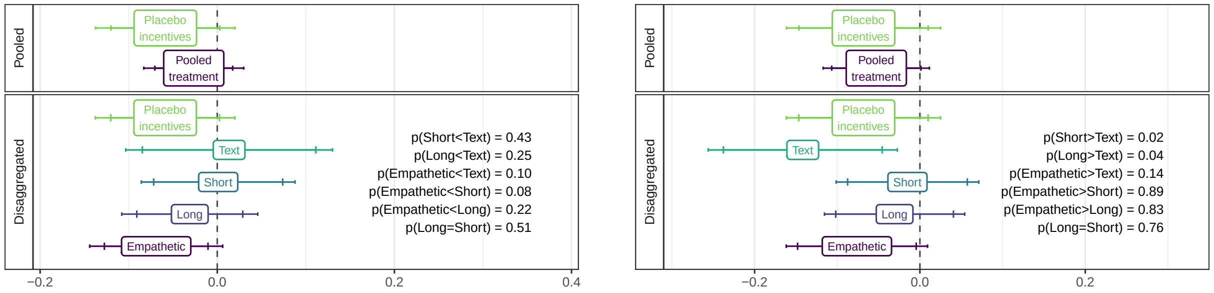


(a) Perceived truthfulness of social media content

(b) Trust in social media content

Figure E3: Disaggregating index on social media trust

Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): believes information from social media likely to be true; (b) trusts information on social media, and thinks information on social media is most trustworthy. All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.

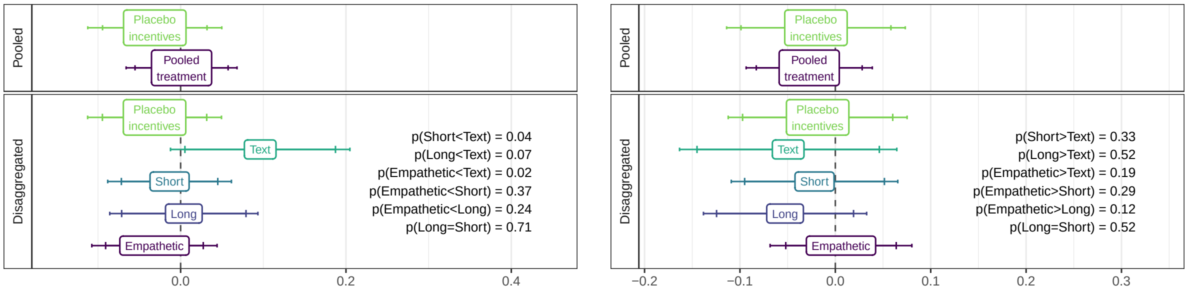


(a) Trust in traditional media

(b) Trust information sent by close ties

Figure E4: Treatment effects on trust in different sources

Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): how true is info on radio/TV, trusts newspapers most for information, trusts information from radio/TV; (b) how true is info from friends and family, trusts info from friends and family, trusts WhatsApp messages from friends and family. All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.



(a) Traditional media consumption

(b) Consumption of news from close ties

Figure E5: Treatment effects on consumption from different sources

Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): how often gets news from radio/TV; (b) how often gets news from friends and family. All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.

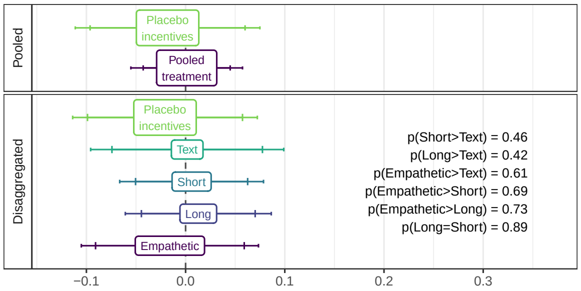


Figure E6: Perceptions of government capacity

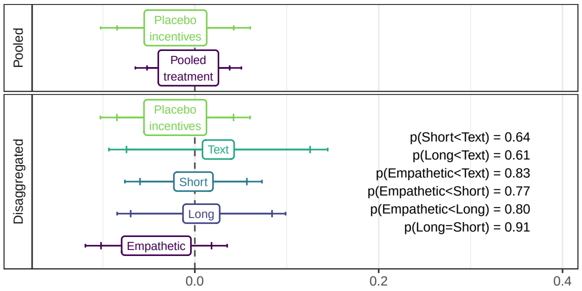


Figure E7: Populist attitudes

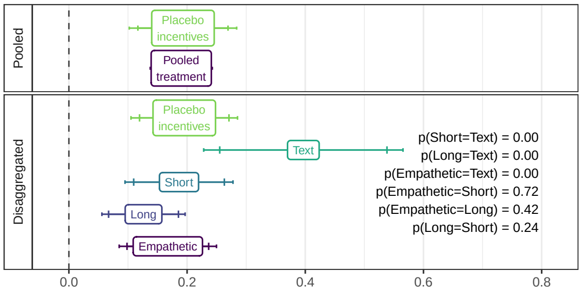


Figure E8: Fact-check requests

Notes: **Fig E6:** Outcome is standardized inverse covariance-weighted index comprising perception of government capacity to provide roads; perception of government capacity to supply electricity. **Fig E7:** Outcome is standardized inverse covariance-weighted index comprising perception of policies benefit elites; perception that ordinary people have no influence over policy. **Fig E8:** Outcome is a standardized indicator for participant submitting a fact-check request to Africa Check. All outcomes are standardized ICW indexes (see items in Table 2). Top panels within each subfigure provide pooled estimates of treatment effects; bottom panels provide estimates with disaggregated treatment variants. Estimated using Equation (1); *p*-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals.

F Tables corresponding to figures in main text

Table F1: Podcast take-up

	ICW: Podcast take-up		How often listens to podcasts		Listens to WCW	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Pooled estimation						
Placebo incentives	0.416	0.424	0.018	0.023	0.247	0.251
	(0.054)	(0.054)	(0.059)	(0.059)	(0.025)	(0.024)
	[0.000]	[0.000]	[0.381]	[0.348]	[0.000]	[0.000]
Pooled podcast	0.651	0.646	0.132	0.123	0.361	0.360
	(0.036)	(0.035)	(0.041)	(0.041)	(0.015)	(0.015)
	[0.000]	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]
B. Disaggregated estimation						
Placebo incentives	0.321	0.323	0.020	0.021	0.188	0.190
	(0.050)	(0.049)	(0.055)	(0.055)	(0.023)	(0.022)
	[0.000]	[0.000]	[0.355]	[0.354]	[0.000]	[0.000]
Text information	-0.020	-0.014	-0.088	-0.084	0.014	0.018
	(0.060)	(0.059)	(0.072)	(0.071)	(0.024)	(0.025)
	[0.744]	[0.818]	[0.224]	[0.232]	[0.282]	[0.232]
Short podcast	0.648	0.638	0.160	0.153	0.349	0.345
	(0.047)	(0.047)	(0.052)	(0.052)	(0.021)	(0.021)
	[0.000]	[0.000]	[0.001]	[0.002]	[0.000]	[0.000]
Long podcast	0.646	0.646	0.120	0.114	0.360	0.361
	(0.048)	(0.048)	(0.054)	(0.054)	(0.021)	(0.021)
	[0.000]	[0.000]	[0.013]	[0.017]	[0.000]	[0.000]
Empathetic podcast	0.665	0.656	0.116	0.099	0.375	0.374
	(0.048)	(0.047)	(0.054)	(0.053)	(0.021)	(0.021)
	[0.000]	[0.000]	[0.015]	[0.030]	[0.000]	[0.000]
Controls	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	3.18	3.18	0.20	0.20
Control SD	1.00	1.00	1.25	1.25	0.40	0.40
R^2	0.22	0.25	0.22	0.26	0.20	0.23
Observations	4541	4541	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 4a.

Table F2: Treatment knowledge

	ICW: Treatment knowledge		Fact-check quiz knowledge	
	(1)	(2)	(3)	(4)
A. Pooled estimation				
Placebo incentives	0.112	0.133	0.159	0.186
	(0.047)	(0.046)	(0.067)	(0.066)
	[0.009]	[0.002]	[0.009]	[0.002]
Pooled treatment	0.411	0.411	0.584	0.584
	(0.034)	(0.033)	(0.048)	(0.047)
	[0.000]	[0.000]	[0.000]	[0.000]
B. Disaggregated estimation				
Placebo incentives	0.113	0.132	0.160	0.187
	(0.047)	(0.046)	(0.067)	(0.066)
	[0.008]	[0.002]	[0.008]	[0.002]
Text information	0.335	0.345	0.476	0.489
	(0.064)	(0.061)	(0.091)	(0.087)
	[0.000]	[0.000]	[0.000]	[0.000]
Short podcast	0.388	0.379	0.551	0.538
	(0.046)	(0.045)	(0.065)	(0.064)
	[0.000]	[0.000]	[0.000]	[0.000]
Long podcast	0.373	0.386	0.529	0.548
	(0.048)	(0.046)	(0.068)	(0.065)
	[0.000]	[0.000]	[0.000]	[0.000]
Empathetic podcast	0.509	0.503	0.722	0.714
	(0.047)	(0.046)	(0.066)	(0.065)
	[0.000]	[0.000]	[0.000]	[0.000]
Controls	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓
Control Mean	0.00	0.00	2.40	2.40
Control SD	1.00	1.00	1.42	1.42
R^2	0.22	0.27	0.22	0.27
Observations	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 4b.

Table F3: Future take-up

	ICW: Future take-up		Stay subscribed to WCW		Want AC fact checks		Want AC reminders		Want AC vaccine info	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Pooled estimation										
Placebo incentives	0.061	0.058	0.013	0.011	-0.003	-0.002	0.030	0.029	0.049	0.047
	(0.050)	(0.048)	(0.021)	(0.021)	(0.019)	(0.019)	(0.023)	(0.023)	(0.023)	(0.023)
	[0.112]	[0.116]	[0.268]	[0.302]	[0.884]	[0.898]	[0.097]	[0.100]	[0.016]	[0.018]
Pooled treatment	0.205	0.207	0.140	0.139	0.052	0.053	0.082	0.083	0.092	0.092
	(0.034)	(0.033)	(0.014)	(0.014)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)	(0.016)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
B. Disaggregated estimation										
Placebo incentives	0.061	0.058	0.013	0.011	-0.003	-0.002	0.030	0.029	0.050	0.049
	(0.050)	(0.048)	(0.021)	(0.021)	(0.019)	(0.019)	(0.023)	(0.023)	(0.023)	(0.023)
	[0.111]	[0.116]	[0.265]	[0.305]	[0.885]	[0.900]	[0.096]	[0.100]	[0.016]	[0.015]
Text information	0.214	0.235	0.019	0.022	0.065	0.072	0.081	0.091	0.084	0.091
	(0.057)	(0.055)	(0.026)	(0.026)	(0.021)	(0.020)	(0.028)	(0.027)	(0.028)	(0.028)
	[0.000]	[0.000]	[0.230]	[0.195]	[0.001]	[0.000]	[0.002]	[0.000]	[0.001]	[0.001]
Short podcast	0.234	0.239	0.150	0.150	0.061	0.063	0.094	0.097	0.103	0.105
	(0.044)	(0.043)	(0.017)	(0.017)	(0.016)	(0.016)	(0.021)	(0.020)	(0.021)	(0.020)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long podcast	0.172	0.171	0.168	0.166	0.039	0.040	0.069	0.068	0.085	0.085
	(0.045)	(0.044)	(0.016)	(0.016)	(0.017)	(0.016)	(0.021)	(0.021)	(0.021)	(0.021)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.009]	[0.008]	[0.001]	[0.001]	[0.000]	[0.000]
Empathetic podcast	0.202	0.196	0.156	0.153	0.049	0.048	0.083	0.080	0.093	0.090
	(0.044)	(0.043)	(0.017)	(0.017)	(0.017)	(0.016)	(0.021)	(0.021)	(0.021)	(0.021)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	0.75	0.75	0.82	0.82	0.66	0.66	0.66	0.66
Control SD	1.00	1.00	0.43	0.43	0.38	0.38	0.47	0.47	0.48	0.48
R^2	0.09	0.14	0.11	0.15	0.08	0.11	0.08	0.13	0.08	0.12
Observations	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 4c.

Table F4: Discernment

	ICW: Discernment		Alcohol and COVID (true)		Foreign restaurant workers (false)		How COVID spreads (true)		Inflated matriculation scores (false)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Pooled estimation										
Placebo incentives	0.045	0.055	-0.020	-0.018	0.049	0.043	0.066	0.075	0.035	0.036
	(0.050)	(0.049)	(0.065)	(0.065)	(0.067)	(0.066)	(0.048)	(0.048)	(0.071)	(0.070)
	[0.180]	[0.130]	[0.758]	[0.776]	[0.234]	[0.255]	[0.085]	[0.060]	[0.311]	[0.303]
Pooled treatment	0.058	0.061	-0.126	-0.121	0.175	0.174	0.050	0.056	0.062	0.062
	(0.035)	(0.034)	(0.046)	(0.046)	(0.048)	(0.047)	(0.034)	(0.034)	(0.049)	(0.048)
	[0.048]	[0.039]	[0.007]	[0.009]	[0.000]	[0.000]	[0.072]	[0.050]	[0.102]	[0.098]
B. Disaggregated estimation										
Placebo incentives	0.046	0.056	-0.020	-0.014	0.049	0.043	0.066	0.076	0.035	0.036
	(0.050)	(0.049)	(0.065)	(0.065)	(0.067)	(0.066)	(0.048)	(0.048)	(0.071)	(0.070)
	[0.178]	[0.127]	[0.755]	[0.827]	[0.233]	[0.255]	[0.085]	[0.057]	[0.310]	[0.304]
Text information	0.120	0.120	-0.002	0.014	0.193	0.175	0.061	0.072	0.044	0.029
	(0.063)	(0.062)	(0.079)	(0.079)	(0.081)	(0.079)	(0.057)	(0.058)	(0.088)	(0.087)
	[0.029]	[0.026]	[0.982]	[0.432]	[0.009]	[0.013]	[0.146]	[0.106]	[0.309]	[0.369]
Short podcast	0.025	0.021	-0.155	-0.147	0.151	0.146	0.052	0.051	0.023	0.022
	(0.046)	(0.045)	(0.061)	(0.061)	(0.062)	(0.060)	(0.043)	(0.043)	(0.062)	(0.061)
	[0.289]	[0.317]	[0.012]	[0.017]	[0.007]	[0.008]	[0.112]	[0.114]	[0.359]	[0.360]
Long podcast	-0.018	-0.003	-0.161	-0.153	0.085	0.092	0.047	0.057	-0.020	-0.012
	(0.046)	(0.046)	(0.063)	(0.063)	(0.064)	(0.063)	(0.046)	(0.046)	(0.066)	(0.065)
	[0.691]	[0.945]	[0.010]	[0.015]	[0.092]	[0.073]	[0.151]	[0.106]	[0.767]	[0.854]
Empathetic podcast	0.141	0.143	-0.119	-0.109	0.280	0.287	0.045	0.053	0.194	0.194
	(0.046)	(0.045)	(0.061)	(0.061)	(0.063)	(0.062)	(0.045)	(0.045)	(0.063)	(0.063)
	[0.001]	[0.001]	[0.053]	[0.074]	[0.000]	[0.000]	[0.158]	[0.120]	[0.001]	[0.001]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	-2.41	-2.41	2.78	2.78	-1.58	-1.58	3.07	3.07
Control SD	1.00	1.00	1.27	1.27	1.32	1.32	0.97	0.97	1.35	1.35
R^2	0.08	0.13	0.08	0.09	0.11	0.16	0.08	0.10	0.10	0.14
Observations	4541	4541	4143	4143	4143	4143	4143	4143	4143	4143

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while *p*-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 5a.

Table F5: Skepticism of conspiracy theories

	ICW: Conspiracy theories		AIDS intentionally created (reversed)		Nelson Mandela died in 1985 (reversed)		Vaccines cause infertility (reversed)		Vaccines have microchips (reversed)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Pooled estimation										
Placebo incentives	-0.024	-0.003	-0.095	-0.078	0.013	0.028	0.015	0.039	-0.012	0.009
	(0.050)	(0.048)	(0.070)	(0.068)	(0.070)	(0.068)	(0.067)	(0.066)	(0.069)	(0.068)
	[0.635]	[0.947]	[0.170]	[0.255]	[0.427]	[0.339]	[0.413]	[0.276]	[0.867]	[0.450]
Pooled treatment	0.104	0.109	0.071	0.079	0.093	0.098	0.177	0.183	0.110	0.110
	(0.035)	(0.034)	(0.048)	(0.048)	(0.048)	(0.047)	(0.048)	(0.047)	(0.047)	(0.047)
	[0.001]	[0.001]	[0.071]	[0.048]	[0.026]	[0.018]	[0.000]	[0.000]	[0.010]	[0.009]
B. Disaggregated estimation										
Placebo incentives	-0.024	-0.003	-0.095	-0.079	0.013	0.029	0.015	0.041	-0.012	0.009
	(0.050)	(0.049)	(0.070)	(0.068)	(0.070)	(0.068)	(0.068)	(0.066)	(0.069)	(0.068)
	[0.637]	[0.954]	[0.170]	[0.248]	[0.426]	[0.336]	[0.411]	[0.269]	[0.868]	[0.448]
Text information	0.106	0.110	0.103	0.108	0.085	0.088	0.132	0.132	0.133	0.134
	(0.058)	(0.058)	(0.085)	(0.084)	(0.080)	(0.079)	(0.082)	(0.082)	(0.078)	(0.079)
	[0.034]	[0.029]	[0.113]	[0.100]	[0.143]	[0.134]	[0.053]	[0.054]	[0.045]	[0.045]
Short podcast	0.039	0.039	0.000	-0.004	0.064	0.066	0.061	0.065	0.052	0.050
	(0.046)	(0.045)	(0.064)	(0.063)	(0.064)	(0.062)	(0.063)	(0.062)	(0.062)	(0.061)
	[0.199]	[0.189]	[1.000]	[0.947]	[0.157]	[0.145]	[0.166]	[0.145]	[0.202]	[0.209]
Long podcast	0.109	0.126	0.082	0.104	0.089	0.111	0.190	0.206	0.108	0.120
	(0.046)	(0.044)	(0.064)	(0.062)	(0.064)	(0.062)	(0.063)	(0.061)	(0.063)	(0.062)
	[0.009]	[0.002]	[0.100]	[0.047]	[0.083]	[0.036]	[0.001]	[0.000]	[0.043]	[0.026]
Empathetic podcast	0.166	0.163	0.119	0.126	0.132	0.124	0.306	0.301	0.163	0.152
	(0.045)	(0.043)	(0.063)	(0.062)	(0.063)	(0.062)	(0.060)	(0.059)	(0.061)	(0.060)
	[0.000]	[0.000]	[0.029]	[0.021]	[0.018]	[0.022]	[0.000]	[0.000]	[0.004]	[0.006]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	-2.34	-2.34	-2.24	-2.24	-2.39	-2.39	-2.36	-2.36
Control SD	1.00	1.00	1.38	1.38	1.36	1.36	1.35	1.35	1.35	1.35
R^2	0.09	0.16	0.08	0.12	0.08	0.15	0.08	0.12	0.07	0.12
Observations	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while *p*-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 5b.

Table F6: Knowledge of verification methods (part 1)

	ICW: Verification knowledge		Avoid misinfo: Ask others (reversed)		Avoid misinfo: Seek reputable orgs		How verify (use sources)		Strategy: Ask experts		Strategy: Ask themselves (reversed)		Strategy: Check popular source (reversed)		Strategy: Talk to others (reversed)		Strategy: Use image search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
A. Pooled estimation																		
Placebo incentives	0.039	0.048	0.012	0.010	0.025	0.028	-0.028	-0.028	0.022	0.025	-0.021	-0.021	-0.011	-0.009	0.002	0.004	0.035	0.034
	(0.050)	(0.050)	(0.018)	(0.018)	(0.024)	(0.023)	(0.051)	(0.049)	(0.025)	(0.024)	(0.017)	(0.017)	(0.025)	(0.024)	(0.019)	(0.019)	(0.017)	(0.017)
	[0.216]	[0.167]	[0.245]	[0.287]	[0.149]	[0.114]	[0.579]	[0.571]	[0.185]	[0.153]	[0.198]	[0.200]	[0.654]	[0.699]	[0.460]	[0.417]	[0.022]	[0.025]
Pooled treatment	0.096	0.099	-0.020	-0.017	0.031	0.034	0.026	0.030	0.049	0.050	-0.013	-0.015	-0.014	-0.016	-0.001	-0.003	0.070	0.070
	(0.036)	(0.036)	(0.012)	(0.012)	(0.017)	(0.017)	(0.036)	(0.035)	(0.017)	(0.017)	(0.011)	(0.011)	(0.017)	(0.017)	(0.014)	(0.013)	(0.012)	(0.011)
	[0.003]	[0.003]	[0.099]	[0.142]	[0.031]	[0.022]	[0.231]	[0.195]	[0.002]	[0.002]	[0.235]	[0.161]	[0.396]	[0.331]	[0.955]	[0.824]	[0.000]	[0.000]
B. Disaggregated estimation																		
Placebo incentives	0.039	0.048	0.012	0.010	0.025	0.027	-0.027	-0.027	0.022	0.024	-0.021	-0.022	-0.011	-0.009	0.002	0.003	0.035	0.034
	(0.050)	(0.050)	(0.018)	(0.018)	(0.024)	(0.023)	(0.051)	(0.049)	(0.025)	(0.024)	(0.017)	(0.017)	(0.025)	(0.024)	(0.019)	(0.019)	(0.017)	(0.017)
	[0.214]	[0.166]	[0.246]	[0.282]	[0.149]	[0.125]	[0.588]	[0.576]	[0.186]	[0.159]	[0.199]	[0.187]	[0.654]	[0.703]	[0.457]	[0.430]	[0.022]	[0.025]
Text information	0.167	0.173	0.011	0.012	0.036	0.038	-0.031	-0.027	0.071	0.071	-0.031	-0.033	-0.009	-0.013	0.011	0.010	0.039	0.038
	(0.064)	(0.064)	(0.022)	(0.022)	(0.030)	(0.030)	(0.064)	(0.060)	(0.031)	(0.030)	(0.020)	(0.020)	(0.030)	(0.030)	(0.024)	(0.023)	(0.021)	(0.021)
	[0.005]	[0.004]	[0.316]	[0.295]	[0.120]	[0.100]	[0.625]	[0.649]	[0.010]	[0.009]	[0.132]	[0.103]	[0.762]	[0.651]	[0.325]	[0.339]	[0.033]	[0.033]
Short podcast	0.124	0.118	-0.003	-0.002	0.010	0.008	0.061	0.054	0.034	0.032	0.001	0.001	-0.006	-0.006	-0.010	-0.010	0.076	0.074
	(0.048)	(0.048)	(0.016)	(0.016)	(0.022)	(0.022)	(0.047)	(0.046)	(0.023)	(0.022)	(0.014)	(0.014)	(0.022)	(0.022)	(0.018)	(0.018)	(0.016)	(0.016)
	[0.005]	[0.007]	[0.872]	[0.893]	[0.320]	[0.351]	[0.098]	[0.117]	[0.069]	[0.075]	[0.469]	[0.916]	[0.804]	[0.790]	[0.597]	[0.594]	[0.000]	[0.000]
Long podcast	0.022	0.034	-0.032	-0.030	0.035	0.040	-0.030	-0.016	0.056	0.061	-0.012	-0.013	-0.018	-0.021	-0.018	-0.022	0.063	0.065
	(0.048)	(0.048)	(0.015)	(0.015)	(0.023)	(0.022)	(0.047)	(0.046)	(0.023)	(0.022)	(0.015)	(0.015)	(0.023)	(0.023)	(0.018)	(0.018)	(0.016)	(0.016)
	[0.324]	[0.240]	[0.034]	[0.049]	[0.062]	[0.035]	[0.533]	[0.728]	[0.007]	[0.003]	[0.415]	[0.363]	[0.438]	[0.353]	[0.325]	[0.226]	[0.000]	[0.000]
Empathetic podcast	0.110	0.109	-0.039	-0.033	0.048	0.050	0.073	0.076	0.046	0.046	-0.021	-0.021	-0.023	-0.024	0.020	0.016	0.087	0.086
	(0.049)	(0.049)	(0.015)	(0.015)	(0.023)	(0.022)	(0.048)	(0.047)	(0.023)	(0.023)	(0.015)	(0.015)	(0.022)	(0.022)	(0.018)	(0.017)	(0.017)	(0.017)
	[0.012]	[0.013]	[0.010]	[0.025]	[0.017]	[0.013]	[0.065]	[0.052]	[0.021]	[0.020]	[0.164]	[0.152]	[0.311]	[0.275]	[0.122]	[0.172]	[0.000]	[0.000]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	0.14	0.14	0.34	0.34	0.00	0.00	0.39	0.39	-0.11	-0.11	-0.36	-0.36	-0.18	-0.18	0.11	0.11
Control SD	1.00	1.00	0.35	0.35	0.48	0.48	1.00	1.00	0.49	0.49	0.31	0.31	0.48	0.48	0.38	0.38	0.31	0.31
R^2	0.09	0.11	0.06	0.09	0.06	0.09	0.07	0.15	0.07	0.12	0.07	0.10	0.06	0.08	0.06	0.09	0.09	0.14
Observations	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while *p*-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 6a.

Table F6: Knowledge of verification methods (part 2)

	ICW: Verification knowledge		To verify: Ask family on WA (reversed)		To verify: Ask in person (reversed)		To verify: Ask others on WA (reversed)		To verify: Post on social media (reversed)		To verify: Submit fact-check request		To verify: Use fact-checker		To verify: Use internet	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
A. Pooled estimation																
Placebo incentives	0.039	0.046	0.006	0.008	0.000	0.003	0.008	0.011	-0.017	-0.013	0.017	0.020	0.026	0.025	-0.023	-0.019
	(0.050)	(0.050)	(0.019)	(0.018)	(0.023)	(0.023)	(0.015)	(0.015)	(0.017)	(0.017)	(0.020)	(0.019)	(0.025)	(0.024)	(0.024)	(0.024)
	[0.216]	[0.177]	[0.370]	[0.334]	[0.991]	[0.452]	[0.283]	[0.222]	[0.321]	[0.431]	[0.190]	[0.150]	[0.144]	[0.150]	[0.338]	[0.417]
Pooled treatment	0.096	0.098	-0.014	-0.013	0.027	0.025	0.005	0.005	0.007	0.006	0.050	0.049	0.053	0.055	-0.012	-0.007
	(0.036)	(0.036)	(0.013)	(0.013)	(0.016)	(0.016)	(0.010)	(0.010)	(0.012)	(0.011)	(0.014)	(0.014)	(0.017)	(0.017)	(0.017)	(0.017)
	[0.003]	[0.003]	[0.315]	[0.316]	[0.045]	[0.055]	[0.311]	[0.317]	[0.275]	[0.300]	[0.000]	[0.000]	[0.001]	[0.001]	[0.495]	[0.681]
B. Disaggregated estimation																
Placebo incentives	0.039	0.046	0.006	0.008	0.000	0.003	0.008	0.011	-0.017	-0.014	0.017	0.020	0.026	0.026	-0.023	-0.020
	(0.050)	(0.050)	(0.019)	(0.018)	(0.023)	(0.023)	(0.015)	(0.015)	(0.017)	(0.017)	(0.020)	(0.019)	(0.025)	(0.024)	(0.024)	(0.024)
	[0.214]	[0.176]	[0.370]	[0.339]	[0.991]	[0.451]	[0.281]	[0.223]	[0.319]	[0.405]	[0.189]	[0.155]	[0.144]	[0.142]	[0.342]	[0.415]
Text information	0.167	0.174	-0.007	-0.007	0.056	0.055	0.010	0.009	0.026	0.028	0.075	0.074	0.087	0.089	-0.016	-0.008
	(0.064)	(0.064)	(0.024)	(0.023)	(0.027)	(0.026)	(0.018)	(0.017)	(0.019)	(0.019)	(0.026)	(0.026)	(0.030)	(0.030)	(0.030)	(0.030)
	[0.005]	[0.003]	[0.765]	[0.748]	[0.020]	[0.019]	[0.294]	[0.308]	[0.085]	[0.071]	[0.002]	[0.002]	[0.002]	[0.001]	[0.592]	[0.780]
Short podcast	0.124	0.119	-0.011	-0.011	0.002	0.001	0.004	0.003	0.010	0.010	0.059	0.056	0.053	0.050	0.001	0.003
	(0.048)	(0.048)	(0.018)	(0.018)	(0.021)	(0.021)	(0.013)	(0.013)	(0.015)	(0.015)	(0.019)	(0.019)	(0.023)	(0.023)	(0.023)	(0.022)
	[0.005]	[0.006]	[0.552]	[0.536]	[0.469]	[0.474]	[0.397]	[0.423]	[0.244]	[0.256]	[0.001]	[0.001]	[0.011]	[0.014]	[0.487]	[0.446]
Long podcast	0.022	0.033	-0.019	-0.020	0.017	0.014	-0.008	-0.006	0.007	0.005	0.027	0.028	0.046	0.052	-0.034	-0.026
	(0.048)	(0.048)	(0.018)	(0.018)	(0.021)	(0.021)	(0.014)	(0.014)	(0.015)	(0.015)	(0.018)	(0.018)	(0.023)	(0.023)	(0.023)	(0.022)
	[0.324]	[0.245]	[0.293]	[0.267]	[0.206]	[0.250]	[0.560]	[0.663]	[0.327]	[0.367]	[0.071]	[0.058]	[0.025]	[0.012]	[0.133]	[0.251]
Empathetic podcast	0.110	0.109	-0.014	-0.013	0.050	0.048	0.018	0.017	-0.005	-0.007	0.052	0.050	0.046	0.050	0.000	0.000
	(0.049)	(0.049)	(0.018)	(0.017)	(0.021)	(0.020)	(0.013)	(0.013)	(0.015)	(0.015)	(0.019)	(0.019)	(0.024)	(0.023)	(0.023)	(0.023)
	[0.012]	[0.014]	[0.432]	[0.467]	[0.007]	[0.009]	[0.087]	[0.095]	[0.724]	[0.635]	[0.003]	[0.004]	[0.024]	[0.016]	[1.000]	[0.496]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	-0.18	-0.18	-0.31	-0.31	-0.1	-0.1	-0.13	-0.13	0.18	0.18	0.46	0.46	0.47	0.47
Control SD	1.00	1.00	0.38	0.38	0.46	0.46	0.30	0.30	0.33	0.33	0.38	0.38	0.50	0.50	0.50	0.50
R^2	0.09	0.10	0.08	0.11	0.09	0.14	0.08	0.10	0.07	0.09	0.07	0.11	0.08	0.11	0.11	0.14
Observations	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while *p*-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 6a.

Table F7: Attention to veracity of social media content

	ICW: Attention to veracity of social media content		Avoid misinfo: Check source		How important to verify		How often think twice	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Pooled estimation								
Placebo incentives	0.039	0.046	0.025	0.027	0.000	0.007	-0.013	-0.007
	(0.050)	(0.049)	(0.024)	(0.024)	(0.061)	(0.060)	(0.053)	(0.052)
	[0.219]	[0.175]	[0.157]	[0.133]	[0.498]	[0.454]	[0.804]	[0.894]
Pooled treatment	0.054	0.057	0.032	0.033	0.035	0.034	-0.036	-0.031
	(0.035)	(0.034)	(0.017)	(0.017)	(0.043)	(0.042)	(0.037)	(0.036)
	[0.061]	[0.048]	[0.032]	[0.028]	[0.209]	[0.206]	[0.342]	[0.395]
B. Disaggregated estimation								
Placebo incentives	0.040	0.048	0.025	0.027	0.000	0.008	-0.013	-0.004
	(0.050)	(0.049)	(0.024)	(0.024)	(0.061)	(0.060)	(0.053)	(0.052)
	[0.215]	[0.167]	[0.154]	[0.133]	[0.497]	[0.447]	[0.806]	[0.935]
Text information	0.007	0.019	0.018	0.017	-0.037	-0.020	-0.062	-0.033
	(0.064)	(0.061)	(0.030)	(0.030)	(0.079)	(0.076)	(0.065)	(0.063)
	[0.455]	[0.379]	[0.276]	[0.286]	[0.638]	[0.793]	[0.336]	[0.601]
Short podcast	0.077	0.069	0.045	0.042	0.013	0.000	-0.008	-0.014
	(0.046)	(0.045)	(0.023)	(0.022)	(0.056)	(0.054)	(0.049)	(0.047)
	[0.046]	[0.063]	[0.024]	[0.031]	[0.410]	[0.498]	[0.863]	[0.772]
Long podcast	-0.010	0.002	-0.008	-0.003	0.062	0.070	-0.053	-0.044
	(0.047)	(0.045)	(0.023)	(0.022)	(0.057)	(0.055)	(0.050)	(0.048)
	[0.829]	[0.486]	[0.739]	[0.899]	[0.140]	[0.104]	[0.143]	[0.364]
Empathetic podcast	0.117	0.120	0.064	0.065	0.065	0.058	-0.034	-0.032
	(0.046)	(0.045)	(0.023)	(0.023)	(0.057)	(0.055)	(0.049)	(0.048)
	[0.006]	[0.004]	[0.003]	[0.002]	[0.127]	[0.147]	[0.485]	[0.506]
Controls	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	0.38	0.38	4.04	4.04	3.86	3.86
Control SD	1.00	1.00	0.49	0.49	1.25	1.25	1.06	1.06
R^2	0.07	0.14	0.07	0.11	0.07	0.14	0.07	0.13
Observations	4543	4543	4543	4543	4543	4543	4543	4543

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 6b.

Table F8: Trust in social media (besides WhatsApp)

	ICW: Trust social media		How true: Info from other social media		Trust most for info: Other social media		Trust: Info from other social media	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Pooled estimation								
Placebo incentives	-0.035	-0.045	0.004	-0.005	-0.023	-0.023	-0.014	-0.027
	(0.047)	(0.047)	(0.038)	(0.036)	(0.019)	(0.018)	(0.050)	(0.049)
	[0.226]	[0.168]	[0.910]	[0.450]	[0.111]	[0.101]	[0.387]	[0.294]
Pooled treatment	-0.088	-0.086	-0.049	-0.043	-0.035	-0.031	-0.049	-0.050
	(0.034)	(0.033)	(0.026)	(0.025)	(0.014)	(0.013)	(0.035)	(0.035)
	[0.004]	[0.004]	[0.028]	[0.043]	[0.005]	[0.009]	[0.083]	[0.073]
B. Disaggregated estimation								
Placebo incentives	-0.036	-0.046	0.004	-0.005	-0.023	-0.023	-0.015	-0.027
	(0.047)	(0.047)	(0.038)	(0.036)	(0.019)	(0.018)	(0.050)	(0.050)
	[0.226]	[0.163]	[0.912]	[0.446]	[0.111]	[0.111]	[0.385]	[0.290]
Text information	-0.153	-0.138	-0.102	-0.085	-0.055	-0.049	-0.066	-0.054
	(0.058)	(0.056)	(0.044)	(0.043)	(0.022)	(0.022)	(0.062)	(0.061)
	[0.004]	[0.007]	[0.011]	[0.023]	[0.007]	[0.012]	[0.144]	[0.185]
Short podcast	-0.023	-0.024	-0.024	-0.015	-0.010	-0.006	-0.007	-0.015
	(0.044)	(0.043)	(0.034)	(0.032)	(0.018)	(0.018)	(0.046)	(0.045)
	[0.303]	[0.289]	[0.234]	[0.318]	[0.278]	[0.369]	[0.439]	[0.367]
Long podcast	-0.067	-0.071	-0.023	-0.027	-0.033	-0.031	-0.030	-0.039
	(0.045)	(0.044)	(0.035)	(0.034)	(0.018)	(0.017)	(0.047)	(0.047)
	[0.065]	[0.052]	[0.253]	[0.212]	[0.032]	[0.038]	[0.262]	[0.199]
Empathetic podcast	-0.148	-0.142	-0.076	-0.068	-0.052	-0.048	-0.103	-0.099
	(0.043)	(0.043)	(0.034)	(0.032)	(0.017)	(0.017)	(0.046)	(0.045)
	[0.000]	[0.000]	[0.012]	[0.018]	[0.001]	[0.002]	[0.013]	[0.014]
Controls	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	2.87	2.87	0.19	0.19	2.91	2.91
Control SD	1.00	1.00	0.73	0.73	0.39	0.39	1.04	1.04
R^2	0.14	0.18	0.10	0.18	0.07	0.10	0.14	0.17
Observations	4541	4541	4541	4541	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while *p*-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 6c.

Table F9: Social media consumption

	ICW: Consume social media		Get news from: Other social media	
	(1)	(2)	(3)	(4)
A. Pooled estimation				
Placebo incentives	-0.015	-0.022	-0.015	-0.015
	(0.049)	(0.048)	(0.024)	(0.024)
	[0.381]	[0.326]	[0.265]	[0.270]
Pooled treatment	-0.004	-0.007	-0.008	-0.007
	(0.034)	(0.034)	(0.017)	(0.017)
	[0.453]	[0.416]	[0.323]	[0.335]
B. Disaggregated estimation				
Placebo incentives	-0.015	-0.022	-0.015	-0.015
	(0.049)	(0.048)	(0.024)	(0.024)
	[0.381]	[0.327]	[0.266]	[0.271]
Text information	-0.071	-0.069	-0.037	-0.036
	(0.060)	(0.060)	(0.030)	(0.030)
	[0.120]	[0.123]	[0.107]	[0.112]
Short podcast	0.022	0.024	0.008	0.010
	(0.045)	(0.045)	(0.023)	(0.022)
	[0.622]	[0.599]	[0.732]	[0.663]
Long podcast	0.023	0.013	0.002	0.000
	(0.045)	(0.045)	(0.023)	(0.022)
	[0.607]	[0.767]	[0.940]	[0.989]
Empathetic podcast	-0.028	-0.031	-0.020	-0.019
	(0.045)	(0.044)	(0.022)	(0.022)
	[0.263]	[0.240]	[0.185]	[0.195]
Controls	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓
Control Mean	0.00	0.00	0.43	0.43
Control SD	1.00	1.00	0.50	0.50
R^2	0.12	0.14	0.10	0.13
Observations	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 7a.

Table F10: Active verification

	ICW: Active verification		How often verify	
	(1)	(2)	(3)	(4)
A. Pooled estimation				
Placebo incentives	-0.039	-0.038	-0.043	-0.042
	(0.048)	(0.048)	(0.054)	(0.053)
	[0.419]	[0.435]	[0.419]	[0.435]
Pooled treatment	-0.038	-0.039	-0.042	-0.043
	(0.034)	(0.034)	(0.038)	(0.037)
	[0.271]	[0.252]	[0.271]	[0.252]
B. Disaggregated estimation				
Placebo incentives	-0.039	-0.040	-0.044	-0.042
	(0.048)	(0.048)	(0.054)	(0.053)
	[0.417]	[0.403]	[0.417]	[0.434]
Text information	-0.127	-0.126	-0.141	-0.141
	(0.065)	(0.064)	(0.072)	(0.071)
	[0.050]	[0.048]	[0.050]	[0.046]
Short podcast	-0.042	-0.043	-0.046	-0.047
	(0.045)	(0.044)	(0.049)	(0.049)
	[0.351]	[0.334]	[0.351]	[0.336]
Long podcast	0.016	0.015	0.018	0.015
	(0.043)	(0.043)	(0.048)	(0.048)
	[0.357]	[0.364]	[0.357]	[0.375]
Empathetic podcast	-0.046	-0.047	-0.051	-0.052
	(0.046)	(0.045)	(0.051)	(0.050)
	[0.312]	[0.303]	[0.312]	[0.300]
Controls	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓
Control Mean	0.00	0.00	3.86	3.86
Control SD	1.00	1.00	1.11	1.11
R^2	0.11	0.14	0.11	0.14
Observations	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 7b.

Table F11: Sharing

	ICW: Sharing		How often share stories	
	(1)	(2)	(3)	(4)
A. Pooled estimation				
Placebo incentives	0.022	0.004	0.023	0.001
	(0.046)	(0.045)	(0.054)	(0.051)
	[0.630]	[0.928]	[0.673]	[0.495]
Pooled treatment	-0.027	-0.029	-0.033	-0.033
	(0.033)	(0.032)	(0.039)	(0.037)
	[0.206]	[0.184]	[0.194]	[0.182]
B. Disaggregated estimation				
Placebo incentives	0.022	0.004	0.023	-0.001
	(0.046)	(0.045)	(0.054)	(0.051)
	[0.630]	[0.932]	[0.675]	[0.991]
Text information	-0.101	-0.093	-0.118	-0.104
	(0.057)	(0.054)	(0.065)	(0.062)
	[0.038]	[0.044]	[0.034]	[0.046]
Short podcast	0.022	0.017	0.025	0.021
	(0.044)	(0.042)	(0.051)	(0.049)
	[0.613]	[0.687]	[0.628]	[0.658]
Long podcast	-0.001	-0.010	0.006	-0.009
	(0.044)	(0.043)	(0.051)	(0.049)
	[0.487]	[0.410]	[0.900]	[0.429]
Empathetic podcast	-0.070	-0.068	-0.095	-0.085
	(0.043)	(0.041)	(0.050)	(0.048)
	[0.050]	[0.050]	[0.029]	[0.037]
Controls	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓
Control Mean	0.00	0.00	2.85	2.85
Control SD	1.00	1.00	1.13	1.13
R^2	0.17	0.24	0.12	0.22
Observations	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 7c.

Table F12: COVID-19 beliefs and preventative behavior

	ICW: COVID-19 beliefs and behavior		Behavior: Stayed home		Behavior: Visited indoors (reversed)		Behavior: Wore mask		COVID is a hoax (reversed)		Lockdowns unnecessary (reversed)		Trust vaccines		Would get vaccinated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
A. Pooled estimation																
Placebo incentives	-0.041	-0.037	-0.068	-0.075	-0.108	-0.099	0.169	0.175	0.068	0.081	-0.041	-0.028	-0.043	-0.029	-0.031	-0.028
	(0.048)	(0.048)	(0.107)	(0.106)	(0.103)	(0.101)	(0.114)	(0.114)	(0.055)	(0.054)	(0.045)	(0.045)	(0.068)	(0.067)	(0.078)	(0.077)
	[0.389]	[0.443]	[0.527]	[0.479]	[0.295]	[0.327]	[0.070]	[0.062]	[0.109]	[0.068]	[0.367]	[0.529]	[0.531]	[0.671]	[0.691]	[0.715]
Pooled treatment	0.003	0.006	-0.030	-0.026	-0.027	-0.023	0.049	0.054	0.084	0.091	-0.017	-0.009	0.025	0.030	0.041	0.045
	(0.034)	(0.033)	(0.076)	(0.076)	(0.071)	(0.070)	(0.080)	(0.080)	(0.040)	(0.039)	(0.032)	(0.032)	(0.049)	(0.048)	(0.055)	(0.054)
	[0.469]	[0.432]	[0.696]	[0.728]	[0.703]	[0.745]	[0.273]	[0.251]	[0.017]	[0.010]	[0.594]	[0.788]	[0.304]	[0.267]	[0.231]	[0.206]
B. Disaggregated estimation																
Placebo incentives	-0.042	-0.035	-0.068	-0.078	-0.108	-0.100	0.167	0.174	0.068	0.080	-0.040	-0.029	-0.043	-0.029	-0.032	-0.029
	(0.048)	(0.048)	(0.107)	(0.107)	(0.103)	(0.101)	(0.114)	(0.114)	(0.055)	(0.054)	(0.045)	(0.045)	(0.068)	(0.067)	(0.078)	(0.077)
	[0.384]	[0.463]	[0.524]	[0.464]	[0.294]	[0.323]	[0.071]	[0.063]	[0.109]	[0.072]	[0.372]	[0.523]	[0.528]	[0.668]	[0.684]	[0.708]
Text information	0.142	0.153	0.054	0.052	0.265	0.275	0.271	0.295	0.093	0.096	-0.063	-0.049	0.048	0.073	0.121	0.142
	(0.057)	(0.057)	(0.131)	(0.130)	(0.124)	(0.122)	(0.129)	(0.128)	(0.067)	(0.067)	(0.057)	(0.056)	(0.084)	(0.082)	(0.093)	(0.092)
	[0.007]	[0.004]	[0.341]	[0.345]	[0.016]	[0.012]	[0.018]	[0.011]	[0.084]	[0.076]	[0.266]	[0.383]	[0.284]	[0.186]	[0.096]	[0.062]
Short podcast	0.019	0.022	-0.003	0.002	-0.033	-0.027	0.090	0.087	0.114	0.116	0.040	0.045	0.054	0.054	0.053	0.054
	(0.044)	(0.043)	(0.101)	(0.101)	(0.094)	(0.093)	(0.105)	(0.104)	(0.051)	(0.050)	(0.042)	(0.041)	(0.064)	(0.063)	(0.072)	(0.072)
	[0.330]	[0.303]	[0.973]	[0.494]	[0.726]	[0.767]	[0.195]	[0.201]	[0.012]	[0.010]	[0.167]	[0.135]	[0.198]	[0.195]	[0.230]	[0.225]
Long podcast	-0.025	-0.018	-0.016	-0.019	-0.126	-0.111	0.067	0.073	0.060	0.074	-0.057	-0.046	0.044	0.050	0.089	0.090
	(0.047)	(0.046)	(0.101)	(0.101)	(0.099)	(0.097)	(0.106)	(0.106)	(0.052)	(0.052)	(0.043)	(0.043)	(0.065)	(0.064)	(0.072)	(0.071)
	[0.599]	[0.694]	[0.875]	[0.848]	[0.201]	[0.253]	[0.264]	[0.245]	[0.125]	[0.076]	[0.186]	[0.284]	[0.252]	[0.219]	[0.109]	[0.103]
Empathetic podcast	-0.051	-0.056	-0.108	-0.102	-0.055	-0.064	-0.116	-0.111	0.072	0.072	-0.015	-0.006	-0.034	-0.036	-0.058	-0.056
	(0.045)	(0.044)	(0.101)	(0.101)	(0.095)	(0.094)	(0.109)	(0.108)	(0.052)	(0.051)	(0.042)	(0.042)	(0.064)	(0.063)	(0.073)	(0.072)
	[0.253]	[0.206]	[0.282]	[0.313]	[0.562]	[0.494]	[0.288]	[0.307]	[0.082]	[0.079]	[0.720]	[0.882]	[0.594]	[0.572]	[0.427]	[0.441]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	4.25	4.25	-1.75	-1.75	5.23	5.23	-1.7	-1.7	-1.77	-1.77	3.37	3.37	3.46	3.46
Control SD	1.00	1.00	2.25	2.25	2.05	2.05	2.41	2.41	1.14	1.14	0.92	0.92	1.39	1.39	1.57	1.57
R^2	0.11	0.15	0.15	0.16	0.10	0.13	0.14	0.15	0.08	0.11	0.09	0.11	0.07	0.11	0.06	0.09
Observations	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541	4541

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 8a.

Table F13: Government attitudes

	ICW: Government attitudes		General gov performance		Gov handled COVID well		How true: Info from politicians		Trust most for info: government		Trust most for info: politicians		Trust: Info from politicians		Vote: Local incumbent		Vote: National incumbent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
A. Pooled estimation																		
Placebo incentives	0.094	0.087	0.079	0.074	0.026	0.024	-0.030	-0.035	0.009	0.009	0.019	0.018	-0.013	-0.023	0.060	0.057	0.033	0.029
	(0.050)	(0.047)	(0.059)	(0.056)	(0.061)	(0.060)	(0.048)	(0.046)	(0.023)	(0.022)	(0.017)	(0.017)	(0.059)	(0.056)	(0.021)	(0.021)	(0.020)	(0.020)
	[0.030]	[0.032]	[0.089]	[0.096]	[0.334]	[0.347]	[0.525]	[0.456]	[0.343]	[0.337]	[0.132]	[0.139]	[0.827]	[0.682]	[0.002]	[0.003]	[0.055]	[0.071]
Pooled treatment	0.060	0.056	0.051	0.042	0.027	0.029	-0.033	-0.032	0.021	0.020	0.020	0.020	-0.035	-0.035	0.020	0.020	0.007	0.004
	(0.035)	(0.033)	(0.042)	(0.040)	(0.043)	(0.042)	(0.033)	(0.032)	(0.016)	(0.016)	(0.012)	(0.011)	(0.041)	(0.040)	(0.014)	(0.014)	(0.014)	(0.014)
	[0.042]	[0.046]	[0.109]	[0.150]	[0.264]	[0.248]	[0.323]	[0.319]	[0.090]	[0.099]	[0.046]	[0.040]	[0.396]	[0.383]	[0.081]	[0.081]	[0.322]	[0.377]
B. Disaggregated estimation																		
Placebo incentives	0.094	0.087	0.079	0.072	0.027	0.023	-0.030	-0.034	0.009	0.010	0.019	0.018	-0.013	-0.024	0.059	0.058	0.032	0.029
	(0.050)	(0.047)	(0.059)	(0.057)	(0.061)	(0.060)	(0.048)	(0.046)	(0.023)	(0.022)	(0.017)	(0.017)	(0.059)	(0.056)	(0.021)	(0.021)	(0.020)	(0.020)
	[0.030]	[0.032]	[0.089]	[0.103]	[0.331]	[0.350]	[0.528]	[0.459]	[0.342]	[0.335]	[0.131]	[0.139]	[0.827]	[0.672]	[0.003]	[0.003]	[0.056]	[0.072]
Text information	0.074	0.081	0.033	0.032	-0.062	-0.046	0.037	0.050	0.047	0.049	0.000	0.004	-0.009	0.005	0.055	0.059	0.041	0.042
	(0.061)	(0.058)	(0.069)	(0.068)	(0.074)	(0.072)	(0.057)	(0.056)	(0.030)	(0.029)	(0.020)	(0.020)	(0.076)	(0.074)	(0.026)	(0.026)	(0.026)	(0.025)
	[0.114]	[0.080]	[0.316]	[0.319]	[0.405]	[0.522]	[0.258]	[0.187]	[0.057]	[0.045]	[0.492]	[0.424]	[0.910]	[0.472]	[0.017]	[0.011]	[0.054]	[0.046]
Short podcast	0.120	0.111	0.095	0.085	0.118	0.111	0.015	0.016	0.032	0.028	0.026	0.027	0.017	0.013	0.032	0.030	0.020	0.016
	(0.046)	(0.044)	(0.055)	(0.053)	(0.056)	(0.055)	(0.043)	(0.042)	(0.021)	(0.021)	(0.015)	(0.015)	(0.054)	(0.052)	(0.019)	(0.019)	(0.019)	(0.019)
	[0.005]	[0.006]	[0.043]	[0.055]	[0.017]	[0.021]	[0.361]	[0.349]	[0.067]	[0.089]	[0.046]	[0.039]	[0.377]	[0.404]	[0.050]	[0.060]	[0.143]	[0.196]
Long podcast	0.038	0.029	0.032	0.014	-0.013	-0.012	-0.098	-0.108	0.000	0.001	0.020	0.019	-0.062	-0.072	0.034	0.032	0.017	0.013
	(0.048)	(0.046)	(0.056)	(0.055)	(0.057)	(0.056)	(0.045)	(0.044)	(0.021)	(0.021)	(0.016)	(0.016)	(0.056)	(0.055)	(0.020)	(0.019)	(0.019)	(0.019)
	[0.212]	[0.264]	[0.283]	[0.397]	[0.822]	[0.832]	[0.028]	[0.014]	[0.993]	[0.978]	[0.103]	[0.117]	[0.270]	[0.188]	[0.041]	[0.049]	[0.193]	[0.249]
Empathetic podcast	0.013	0.013	0.034	0.031	0.013	0.020	-0.049	-0.044	0.020	0.020	0.021	0.022	-0.075	-0.067	-0.023	-0.020	-0.034	-0.033
	(0.047)	(0.045)	(0.055)	(0.053)	(0.056)	(0.055)	(0.045)	(0.044)	(0.021)	(0.021)	(0.016)	(0.015)	(0.055)	(0.053)	(0.018)	(0.018)	(0.018)	(0.018)
	[0.392]	[0.387]	[0.269]	[0.278]	[0.407]	[0.361]	[0.276]	[0.316]	[0.169]	[0.174]	[0.085]	[0.078]	[0.173]	[0.205]	[0.219]	[0.275]	[0.063]	[0.062]
Controls	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓	×	✓
Directional hypothesis	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	2.33	2.33	3.07	3.07	3.04	3.04	0.29	0.29	0.12	0.12	2.91	2.91	0.21	0.21	0.20	0.20
Control SD	1.00	1.00	1.21	1.21	1.24	1.24	0.94	0.94	0.45	0.45	0.32	0.32	1.20	1.20	0.40	0.40	0.40	0.40
R^2	0.10	0.20	0.11	0.17	0.08	0.13	0.08	0.13	0.07	0.11	0.07	0.09	0.09	0.18	0.08	0.12	0.08	0.13
Observations	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543

Notes: See Table 2 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets. ICW estimate plotted in Figure 8b.

Table F14: Pooled estimation ICW outcomes (including LASSO-selected controls)

Figure	4a	4b	4c	5a	5b	6a	6b	6c	7a	7b	7c	8a	8b
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Placebo incentives	0.32 (0.05)	0.13 (0.05)	0.06 (0.05)	0.06 (0.05)	0.00 (0.05)	0.05 (0.05)	0.05 (0.05)	-0.04 (0.05)	-0.02 (0.05)	-0.04 (0.05)	0.00 (0.04)	-0.03 (0.05)	0.09 (0.05)
Pooled treatment	0.56 (0.03)	0.41 (0.03)	0.21 (0.03)	0.06 (0.03)	0.11 (0.03)	0.10 (0.04)	0.06 (0.03)	-0.09 (0.03)	-0.01 (0.03)	-0.04 (0.03)	-0.03 (0.03)	0.01 (0.03)	0.06 (0.03)
Gender: Female	-0.06 (0.04)	-0.03 (0.04)	-0.01 (0.03)	-0.02 (0.04)		-0.01 (0.04)			0.01 (0.03)			0.11 (0.04)	-0.02 (0.04)
Locality: Peri-urban	0.02 (0.03)	0.04 (0.02)		0.14 (0.05)				-0.01 (0.02)	0.01 (0.03)				
Locality: Rural	0.06 (0.03)		0.00 (0.02)	0.09 (0.05)		-0.01 (0.02)			-0.02 (0.03)		-0.01 (0.02)		0.06 (0.02)
Age: 18-24	0.10 (0.04)	0.00 (0.04)	-0.08 (0.02)	-0.03 (0.02)	-0.21 (0.02)	0.03 (0.02)	-0.01 (0.02)		-0.05 (0.02)	-0.05 (0.02)	-0.02 (0.02)	-0.13 (0.02)	0.18 (0.02)
Age: 25-34	0.12 (0.04)	0.01 (0.04)		-0.04 (0.02)	-0.11 (0.02)	0.01 (0.02)						-0.07 (0.02)	0.12 (0.02)
Age: 35-44	0.03 (0.03)	-0.02 (0.03)	0.03 (0.01)								-0.05 (0.02)		
Education: Secondary	0.02 (0.02)	0.05 (0.01)	-0.01 (0.01)	0.04 (0.01)	0.04 (0.01)	0.03 (0.02)	0.02 (0.02)	-0.02 (0.01)			-0.02 (0.01)		-0.02 (0.01)
Education: University	0.03 (0.02)	0.06 (0.02)	-0.06 (0.02)	0.04 (0.02)	0.01 (0.02)	0.04 (0.02)	0.02 (0.02)		0.02 (0.02)	0.02 (0.02)		-0.01 (0.02)	-0.01 (0.02)
Province: Eastern Cape	-0.03 (0.02)		0.01 (0.01)	0.02 (0.02)	0.01 (0.01)			-0.04 (0.01)	-0.01 (0.02)	-0.02 (0.02)	-0.02 (0.01)	-0.04 (0.02)	
Province: Free State	0.01 (0.02)	0.04 (0.02)		0.02 (0.02)			0.03 (0.01)	-0.02 (0.01)		0.02 (0.01)	0.02 (0.01)	-0.01 (0.02)	
Province: KwaZulu-Natal	-0.01 (0.02)	0.04 (0.02)		0.03 (0.02)							0.01 (0.02)	-0.02 (0.02)	-0.02 (0.02)
Province: Limpopo	0.02 (0.02)	0.03 (0.02)	0.01 (0.02)	-0.02 (0.02)	-0.01 (0.02)						0.02 (0.02)	0.01 (0.02)	
Province: Mpumalanga	0.00 (0.02)	0.01 (0.02)	-0.02 (0.02)		-0.04 (0.02)					0.02 (0.02)			0.03 (0.02)
Province: North West	-0.02 (0.02)	0.04 (0.02)		-0.04 (0.02)			-0.04 (0.02)				-0.01 (0.02)	0.02 (0.02)	
Province: Western Cape	-0.03 (0.02)		-0.02 (0.02)		0.02 (0.01)			-0.03 (0.02)	-0.02 (0.02)	-0.03 (0.02)	-0.05 (0.01)	-0.03 (0.02)	0.00 (0.02)
Challenge: DNK where	0.04 (0.02)	0.02 (0.02)		-0.01 (0.01)						-0.01 (0.01)		0.02 (0.01)	0.01 (0.02)
Challenge: Too expensive	0.05 (0.02)	0.00 (0.02)	0.02 (0.01)		0.02 (0.02)	0.02 (0.02)		0.02 (0.01)	0.03 (0.02)				0.02 (0.02)
Challenge: Too time consuming	0.02 (0.02)				-0.02 (0.01)		0.03 (0.02)				0.01 (0.01)	-0.01 (0.02)	0.04 (0.02)
Challenge: Mistrust fact-checkers	0.02 (0.02)	-0.01 (0.02)		0.01 (0.02)	-0.02 (0.01)			-0.02 (0.01)		0.03 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.00 (0.01)
Challenge: Fact-checks are irrelevant	0.04 (0.02)	-0.02 (0.02)	0.03 (0.01)						0.03 (0.02)			0.02 (0.02)	
Get news from: Other social media	0.01 (0.02)	0.02 (0.02)	-0.01 (0.02)	0.03 (0.02)			0.03 (0.02)	0.06 (0.01)	0.04 (0.02)				-0.03 (0.02)
Get news from: Family	0.02 (0.02)	0.07 (0.02)	-0.04 (0.02)	0.03 (0.02)	0.02 (0.02)		0.07 (0.02)		0.06 (0.02)	0.03 (0.01)			-0.04 (0.02)
Get news from: Radio/TV	0.05 (0.02)	0.03 (0.02)	0.05 (0.02)	0.03 (0.02)	0.06 (0.01)	0.02 (0.02)	0.04 (0.02)			0.02 (0.02)	-0.02 (0.01)	0.05 (0.02)	0.05 (0.02)
WA news freq: Family	0.01 (0.02)	-0.03 (0.02)	0.00 (0.02)	-0.06 (0.02)	-0.03 (0.02)	-0.01 (0.02)					0.03 (0.02)	0.01 (0.02)	0.07 (0.02)
WA news freq: Large groups	0.06 (0.02)	0.02 (0.02)	0.03 (0.02)			-0.02 (0.02)	0.02 (0.02)	0.02 (0.02)		0.04 (0.02)	0.08 (0.02)		0.00 (0.02)
WA news freq: Orgs	0.04 (0.02)	-0.03 (0.02)	0.09 (0.02)	0.00 (0.02)	-0.01 (0.02)		0.02 (0.02)	0.04 (0.02)	0.01 (0.02)	0.03 (0.02)	0.04 (0.02)		0.07 (0.02)
Behavior: Stayed home	0.02 (0.02)		0.01 (0.02)		-0.05 (0.02)		-0.02 (0.02)		-0.04 (0.02)	0.02 (0.02)	-0.03 (0.01)	0.02 (0.02)	
Behavior: Visited indoors (reversed)	0.01 (0.02)	-0.01 (0.02)	0.02 (0.01)	0.01 (0.02)	0.03 (0.02)	0.03 (0.02)			-0.01 (0.02)			0.04 (0.02)	0.00 (0.01)
Behavior: Wore mask	-0.03 (0.02)	-0.04 (0.02)		0.00 (0.02)	-0.01 (0.01)			0.02 (0.01)	-0.03 (0.02)		-0.02 (0.01)	0.00 (0.02)	
How often listens to podcasts	0.18 (0.02)	0.01 (0.02)	0.03 (0.02)				0.02 (0.02)	0.02 (0.02)	0.00 (0.02)	0.06 (0.02)	0.04 (0.02)	0.01 (0.02)	0.05 (0.02)
Listens to WCW	0.08 (0.02)	0.02 (0.02)	-0.03 (0.02)		0.03 (0.02)		0.04 (0.02)		0.03 (0.02)	0.02 (0.01)	0.05 (0.02)	-0.05 (0.01)	0.05 (0.02)
Listens to Money Show	-0.01 (0.02)	-0.05 (0.02)	0.01 (0.01)	-0.03 (0.01)	-0.04 (0.02)	-0.04 (0.02)			0.06 (0.02)		0.01 (0.02)	0.01 (0.02)	0.02 (0.02)
Fake news leads to harmful behavior	0.05 (0.02)	0.01 (0.02)	0.03 (0.01)	-0.02 (0.02)					0.01 (0.02)		0.01 (0.01)		0.07 (0.02)
Trust on WA: Family members	-0.01 (0.02)	-0.08 (0.02)	0.01 (0.02)	-0.07 (0.02)	-0.09 (0.02)	-0.03 (0.02)	-0.07 (0.02)	0.08 (0.02)	0.01 (0.02)	0.00 (0.02)	0.08 (0.02)	-0.03 (0.02)	0.03 (0.02)
Trust: Info from other social media	-0.01 (0.02)		0.00 (0.02)	-0.05 (0.02)	-0.04 (0.02)	-0.01 (0.02)	-0.01 (0.02)	0.15 (0.03)	0.04 (0.02)		0.04 (0.02)		0.03 (0.02)
Trust on WA: Orgs	0.03 (0.02)	0.05 (0.02)	0.05 (0.02)	0.03 (0.02)	0.02 (0.02)		0.02 (0.02)	-0.03 (0.02)			-0.03 (0.02)	0.01 (0.02)	0.03 (0.02)
Trust: Info from radio/TV	0.02 (0.02)	0.04 (0.02)		0.04 (0.02)	0.05 (0.02)	0.02 (0.02)	0.04 (0.02)			0.02 (0.02)	0.02 (0.02)	0.03 (0.02)	0.08 (0.02)
Trust: Info from WhatsApp	0.01 (0.02)	-0.06 (0.02)	0.04 (0.02)	-0.02 (0.02)	-0.04 (0.02)	-0.02 (0.02)		-0.01 (0.02)			0.04 (0.02)	0.03 (0.02)	0.06 (0.02)
Trust on WA: Large groups	0.01 (0.02)	0.05 (0.02)	-0.05 (0.02)		0.03 (0.02)		0.05 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.00 (0.02)	-0.02 (0.02)	-0.02 (0.02)
How often verify	0.02 (0.02)	0.03 (0.02)	0.06 (0.01)	0.03 (0.02)	0.02 (0.02)	0.05 (0.02)	0.07 (0.02)			0.17 (0.02)		0.01 (0.02)	0.00 (0.02)
To verify: Ask in person (reversed)	0.04 (0.02)	0.02 (0.02)				0.04 (0.02)			-0.01 (0.02)	0.03 (0.02)		0.02 (0.02)	0.00 (0.02)
To verify: Ask others on WA (reversed)	0.01 (0.02)	0.00 (0.02)	-0.01 (0.01)		0.05 (0.02)	0.02 (0.02)					-0.04 (0.01)	0.01 (0.02)	-0.05 (0.02)
To verify: Use fact-checker	0.01 (0.02)	0.07 (0.02)	-0.01 (0.01)	0.03 (0.02)	0.05 (0.01)	0.07 (0.02)	0.08 (0.02)	-0.05 (0.01)		0.04 (0.01)	-0.05 (0.01)	0.05 (0.01)	0.00 (0.02)
To verify: Post on social media (reversed)	-0.04 (0.02)	0.00 (0.02)	-0.03 (0.01)	0.01 (0.01)	0.01 (0.01)	0.05 (0.02)		-0.08 (0.01)	-0.03 (0.02)		-0.02 (0.01)	0.01 (0.02)	
Locality: Urban		-0.01 (0.04)	-0.02 (0.04)	0.10 (0.07)	0.01 (0.04)			0.06 (0.04)				0.03 (0.04)	
Province: Gauteng		0.02 (0.02)	-0.01 (0.02)	-0.01 (0.02)		0.02 (0.02)	0.00 (0.02)	0.01 (0.02)	0.02 (0.02)			0.00 (0.03)	0.04 (0.02)
Get news from: WhatsApp		0.01 (0.02)	-0.01 (0.02)	0.01 (0.02)		-0.04 (0.02)	0.01 (0.02)		-0.01 (0.02)		0.02 (0.02)	-0.05 (0.02)	0.02 (0.02)
Trust: Info from family		0.02 (0.02)	0.00 (0.02)	0.04 (0.02)	0.02 (0.02)				-0.06 (0.02)			0.03 (0.02)	-0.02 (0.02)
To verify: Ask family on WA (reversed)		0.00 (0.02)	0.03 (0.01)			0.03 (0.02)	-0.04 (0.02)		-0.03 (0.02)		-0.01 (0.01)	-0.03 (0.02)	
To verify: Use internet		0.09 (0.02)	-0.02 (0.01)	0.07 (0.02)	0.07 (0.02)	0.08 (0.02)	0.10 (0.02)		0.02 (0.02)	0.04 (0.02)	-0.03 (0.01)	0.04 (0.02)	-0.03 (0.02)
ICW: Trust social media								0.07 (0.03)					
ICW: Consume social media									0.14 (0.02)				
ICW: Sharing											0.15 (0.02)		
ICW: COVID-19 beliefs and behavior												0.16 (0.02)	
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control SD	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
R^2	0.23	0.27	0.14	0.13	0.16	0.10	0.13	0.18	0.14	0.14	0.23	0.14	0.20
Observations	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543

Notes: Regressions of ICW index outcomes used in main figures in text including all LASSO-selected controls. Column header provides Figure corresponding to relevant ICW outcome in the manuscript. All specifications are estimated using OLS, and adjust for randomization block fixed effects. Heteroskedasticity-robust standard errors in parentheses.

Table F15: Disaggregated estimation ICW outcomes (including LASSO-selected controls)

Figure	4a	4b	4c	5a	5b	6a	6b	6c	7a	7b	7c	8a	8b
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Placebo incentives	0.32 (0.05)	0.13 (0.05)	0.06 (0.05)	0.06 (0.05)	0.00 (0.05)	0.05 (0.05)	0.05 (0.05)	-0.04 (0.05)	-0.02 (0.05)	-0.04 (0.05)	0.00 (0.04)	-0.03 (0.05)	0.09 (0.05)
Text information	-0.01 (0.06)	0.34 (0.06)	0.23 (0.06)	0.12 (0.06)	0.11 (0.06)	0.17 (0.06)	0.02 (0.06)	-0.14 (0.06)	-0.07 (0.06)	-0.13 (0.06)	-0.09 (0.05)	0.16 (0.06)	0.08 (0.06)
Short podcast	0.64 (0.05)	0.38 (0.05)	0.24 (0.04)	0.02 (0.04)	0.04 (0.04)	0.12 (0.05)	0.07 (0.04)	-0.02 (0.04)	0.02 (0.04)	-0.04 (0.04)	0.02 (0.04)	0.03 (0.04)	0.11 (0.04)
Long podcast	0.65 (0.05)	0.39 (0.05)	0.17 (0.04)	0.00 (0.05)	0.13 (0.04)	0.03 (0.05)	0.00 (0.05)	-0.07 (0.04)	0.01 (0.04)	0.01 (0.04)	-0.01 (0.04)	-0.02 (0.05)	0.03 (0.05)
Empathetic podcast	0.66 (0.05)	0.50 (0.05)	0.20 (0.04)	0.14 (0.05)	0.17 (0.04)	0.11 (0.05)	0.12 (0.05)	-0.14 (0.04)	-0.03 (0.04)	-0.05 (0.05)	-0.07 (0.04)	-0.05 (0.04)	0.01 (0.05)
Gender: Female	-0.06 (0.04)	-0.03 (0.04)	-0.01 (0.03)	-0.03 (0.04)		-0.01 (0.04)			0.01 (0.03)			0.11 (0.03)	-0.02 (0.04)
Locality: Peri-urban	0.01 (0.03)	0.04 (0.02)		0.08 (0.03)				-0.01 (0.02)	0.01 (0.03)				
Locality: Rural	0.06 (0.03)		0.00 (0.02)	0.03 (0.03)		0.00 (0.02)			-0.02 (0.03)				0.06 (0.02)
Age: 18-24	0.10 (0.04)	0.00 (0.04)	-0.09 (0.02)	-0.03 (0.02)	-0.21 (0.02)	0.03 (0.02)	-0.01 (0.02)		-0.05 (0.02)	-0.05 (0.02)		-0.17 (0.04)	0.23 (0.03)
Age: 25-34	0.13 (0.04)	0.01 (0.04)		-0.04 (0.02)	-0.11 (0.02)	0.01 (0.02)						-0.10 (0.04)	0.17 (0.03)
Age: 35-44	0.03 (0.03)	-0.02 (0.03)	0.03 (0.01)								-0.04 (0.01)	-0.03 (0.03)	0.05 (0.02)
Education: Secondary	0.02 (0.02)	0.05 (0.01)	-0.01 (0.01)	0.04 (0.01)	0.04 (0.01)	0.03 (0.02)	0.02 (0.02)	-0.02 (0.01)			-0.02 (0.01)		-0.02 (0.01)
Education: University	0.03 (0.02)	0.06 (0.02)	-0.06 (0.02)	0.04 (0.02)	0.01 (0.02)	0.04 (0.02)	0.02 (0.02)		0.02 (0.02)	0.02 (0.02)		-0.01 (0.02)	
Province: Eastern Cape	-0.03 (0.02)		0.01 (0.01)	0.02 (0.02)	0.01 (0.01)	-0.01 (0.02)		-0.04 (0.01)	-0.01 (0.02)	-0.02 (0.02)	-0.02 (0.01)	-0.03 (0.02)	
Province: Free State	0.01 (0.02)	0.04 (0.02)		0.02 (0.02)		0.02 (0.02)	0.03 (0.01)	-0.02 (0.01)		0.02 (0.01)		0.00 (0.02)	
Province: KwaZulu-Natal	0.00 (0.02)	0.04 (0.02)		0.03 (0.02)							0.00 (0.01)		-0.02 (0.02)
Province: Limpopo	0.01 (0.02)	0.03 (0.02)	0.01 (0.02)	-0.02 (0.02)								0.03 (0.02)	
Province: Mpumalanga	-0.01 (0.02)	0.01 (0.02)	-0.02 (0.02)		-0.03 (0.02)					0.01 (0.02)			0.03 (0.02)
Province: North West	-0.03 (0.02)	0.04 (0.02)		-0.04 (0.02)			-0.04 (0.02)				-0.01 (0.02)	0.03 (0.02)	
Province: Western Cape	-0.03 (0.02)		-0.02 (0.02)		0.02 (0.01)			-0.03 (0.02)	-0.03 (0.02)	-0.03 (0.02)	-0.05 (0.01)	-0.01 (0.02)	0.00 (0.02)
Challenge: DNK where	0.04 (0.02)	0.02 (0.02)		-0.01 (0.01)		-0.01 (0.02)			-0.01 (0.02)	0.00 (0.01)		0.02 (0.01)	
Challenge: Too expensive	0.05 (0.02)	0.00 (0.02)	0.02 (0.01)		0.02 (0.02)	0.02 (0.02)	0.01 (0.02)	0.02 (0.01)	0.03 (0.02)				0.02 (0.02)
Challenge: Too time consuming	0.01 (0.02)				-0.02 (0.01)		0.03 (0.02)					-0.01 (0.02)	0.04 (0.02)
Challenge: Mistrust fact-checkers	0.03 (0.02)	-0.01 (0.02)	0.01 (0.01)	0.01 (0.02)	-0.02 (0.01)			-0.01 (0.01)		0.03 (0.01)			0.00 (0.01)
Challenge: Fact-checks are irrelevant	0.04 (0.02)	-0.02 (0.02)	0.03 (0.01)			-0.02 (0.02)			0.03 (0.02)	0.01 (0.01)		0.02 (0.02)	
Get news from: Other social media	0.01 (0.02)	0.02 (0.02)	-0.01 (0.02)	0.03 (0.02)			0.03 (0.02)	0.06 (0.01)	0.04 (0.02)				-0.03 (0.02)
Get news from: Family	0.02 (0.02)	0.07 (0.02)	-0.04 (0.02)	0.03 (0.02)	0.02 (0.02)	0.02 (0.02)	0.07 (0.02)		0.07 (0.02)	0.04 (0.02)			-0.04 (0.02)
Get news from: Radio/TV	0.05 (0.02)	0.03 (0.02)	0.04 (0.02)	0.03 (0.02)	0.06 (0.01)	0.02 (0.02)	0.03 (0.02)		-0.02 (0.02)	0.02 (0.02)	-0.01 (0.01)	0.04 (0.02)	0.05 (0.01)
WA news freq: Family	0.02 (0.02)	-0.03 (0.02)	0.00 (0.02)	-0.06 (0.02)	-0.03 (0.02)	-0.01 (0.02)					0.03 (0.02)		0.07 (0.02)
WA news freq: Large groups	0.05 (0.02)	0.02 (0.02)	0.02 (0.02)			-0.03 (0.02)	0.02 (0.02)	0.02 (0.02)		0.04 (0.02)	0.08 (0.02)		0.00 (0.02)
WA news freq: Orgs	0.04 (0.02)	-0.03 (0.02)	0.08 (0.02)		-0.01 (0.02)	-0.01 (0.02)	0.02 (0.02)	0.03 (0.02)	0.01 (0.02)	0.03 (0.02)	0.04 (0.02)		0.07 (0.02)
Behavior: Stayed home	0.01 (0.02)	0.00 (0.02)	0.01 (0.02)		-0.05 (0.02)		-0.02 (0.02)		-0.04 (0.02)	0.02 (0.02)		0.02 (0.02)	
Behavior: Visited indoors (reversed)	0.01 (0.02)	-0.01 (0.02)	0.03 (0.01)	0.01 (0.02)	0.03 (0.02)	0.03 (0.02)			-0.01 (0.02)			0.04 (0.02)	0.00 (0.01)
Behavior: Wore mask	-0.03 (0.02)	-0.04 (0.02)	0.02 (0.01)					0.02 (0.01)	-0.03 (0.02)		-0.02 (0.01)	0.00 (0.02)	
How often listens to podcasts	0.18 (0.02)	0.01 (0.02)	0.03 (0.02)				0.02 (0.02)	0.02 (0.02)	0.00 (0.02)	0.06 (0.02)	0.04 (0.02)	0.01 (0.02)	0.05 (0.02)
Listens to WCW	0.08 (0.02)	0.02 (0.02)	-0.03 (0.02)		0.03 (0.02)		0.04 (0.02)		0.03 (0.02)	0.02 (0.01)	0.06 (0.02)	-0.05 (0.01)	0.05 (0.02)
Listens to Money Show	-0.01 (0.02)	-0.05 (0.02)	0.01 (0.01)	-0.03 (0.01)	-0.04 (0.02)	-0.03 (0.02)			0.06 (0.02)		0.01 (0.02)		0.02 (0.02)
Fake news leads to harmful behavior	0.04 (0.02)	0.01 (0.02)	0.03 (0.01)	-0.02 (0.02)					0.01 (0.02)		0.01 (0.01)		0.07 (0.02)
Trust: Info from other social media	-0.01 (0.02)		0.00 (0.02)	-0.05 (0.02)	-0.04 (0.02)	-0.01 (0.02)	-0.01 (0.02)	0.15 (0.03)	0.04 (0.02)		0.03 (0.02)		0.03 (0.02)
Trust on WA: Orgs	0.03 (0.02)	0.05 (0.02)	0.05 (0.02)	0.03 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)					0.01 (0.02)	0.03 (0.02)
Trust: Info from radio/TV	0.02 (0.02)	0.04 (0.02)		0.04 (0.02)	0.05 (0.02)	0.01 (0.02)	0.04 (0.02)			0.02 (0.02)	0.01 (0.01)	0.03 (0.02)	0.08 (0.02)
Trust: Info from WhatsApp	0.01 (0.02)	-0.06 (0.02)	0.04 (0.02)		-0.04 (0.02)	-0.02 (0.02)		-0.01 (0.02)			0.03 (0.02)	0.03 (0.02)	0.06 (0.02)
Trust on WA: Large groups	0.01 (0.02)	0.05 (0.02)	-0.05 (0.02)		0.03 (0.02)		0.05 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)			-0.02 (0.02)
How often verify	0.01 (0.02)	0.03 (0.02)	0.06 (0.01)	0.03 (0.02)	0.02 (0.02)	0.05 (0.02)	0.07 (0.02)			0.17 (0.02)		0.01 (0.02)	
To verify: Ask in person (reversed)	0.04 (0.02)	0.02 (0.02)				0.04 (0.02)			-0.02 (0.02)	0.03 (0.02)		0.02 (0.02)	
To verify: Ask others on WA (reversed)	0.01 (0.02)	0.00 (0.02)	-0.01 (0.01)		0.04 (0.02)	0.02 (0.02)					-0.04 (0.01)	0.01 (0.02)	-0.05 (0.02)
To verify: Use fact-checker	0.01 (0.02)	0.07 (0.02)	-0.01 (0.01)	0.03 (0.02)	0.05 (0.01)	0.07 (0.02)	0.08 (0.02)	-0.05 (0.01)		0.04 (0.01)	-0.05 (0.01)	0.05 (0.01)	0.00 (0.02)
To verify: Post on social media (reversed)	-0.04 (0.02)		-0.03 (0.01)	0.01 (0.01)	0.01 (0.01)	0.05 (0.02)		-0.07 (0.01)	-0.03 (0.02)		-0.02 (0.01)		
To verify: Use internet	0.01 (0.02)	0.09 (0.02)	-0.02 (0.01)	0.08 (0.02)	0.08 (0.02)	0.08 (0.02)	0.10 (0.02)	-0.02 (0.01)	0.02 (0.02)	0.04 (0.02)	-0.03 (0.01)	0.03 (0.02)	-0.04 (0.02)
Locality: Urban		-0.01 (0.04)	-0.02 (0.04)		0.00 (0.04)	0.01 (0.04)		0.06 (0.04)				0.03 (0.04)	
Province: Gauteng		0.02 (0.02)	-0.01 (0.02)	-0.01 (0.02)		0.02 (0.02)	0.00 (0.02)	0.01 (0.02)	0.02 (0.02)			0.02 (0.02)	0.04 (0.02)
Get news from: WhatsApp		0.01 (0.02)	-0.01 (0.02)	0.01 (0.02)		-0.04 (0.02)	0.01 (0.02)		-0.01 (0.02)		0.02 (0.02)	-0.04 (0.01)	0.02 (0.02)
Trust: Info from family		0.02 (0.02)	0.00 (0.02)	0.04 (0.02)	0.02 (0.02)	0.01 (0.02)			-0.06 (0.02)			0.02 (0.02)	-0.02 (0.02)
Trust on WA: Family members		-0.08 (0.02)	0.01 (0.02)	-0.07 (0.02)	-0.09 (0.02)	-0.03 (0.02)	-0.06 (0.02)	0.08 (0.02)	0.01 (0.02)	0.00 (0.02)	0.08 (0.02)	-0.03 (0.02)	0.03 (0.02)
To verify: Ask family on WA (reversed)		-0.01 (0.02)	0.03 (0.01)			0.03 (0.02)	-0.04 (0.02)		-0.03 (0.02)	0.02 (0.02)	-0.01 (0.01)	-0.02 (0.02)	
ICW: Trust social media								0.07 (0.03)					
ICW: Consume social media									0.14 (0.02)				
ICW: Sharing											0.15 (0.02)		
ICW: COVID-19 beliefs and behavior												0.16 (0.02)	
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control SD	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
R^2	0.26	0.27	0.14	0.13	0.16	0.11	0.14	0.18	0.14	0.14	0.23	0.14	0.20
Observations	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543

Notes: Regressions of ICW index outcomes used in main figures in text including all LASSO-selected controls. Column header provides Figure corresponding to relevant ICW outcome in the manuscript. All specifications are estimated using OLS, and adjust for randomization block fixed effects. Heteroskedasticity-robust standard errors in parentheses.

G Pre-analysis Plan

Can fact-checking podcasts combat misinformation in South Africa?

Potentially harmful misinformation runs rampant on social media across a wide set of countries. We explore how fact-checking pod- casts can be used to inhibit citizens' exposure to misinformation, increase their knowledge about COVID-19, and ultimately increase their compliance with public health policies. The intervention we study uses WhatsApp-delivered podcasts as an attention-catching method of delivering verified information to individuals who may otherwise have limited access to credible online sources. We partner with the first and largest fact-checking organization in sub-Saharan Africa, Africa Check, and randomize the delivery of variants of their programming to a recruited sample of participants in a panel survey in South Africa. The study has implications both for understanding how citizens' exposure to misinformation can be reduced with low- cost interventions and how the correction of false information can increase citizens' trust in public policies.

1 Introduction

Misinformation about social, political, and public health issues is a growing problem in many sub-Saharan African countries, where the rapid spread of social media technologies has led to the increasingly viral spread of misinformation (Zarocostas 2020). The COVID-19 crisis, for example, has highlighted the need to identify ways to counter social media posts spreading fake cures, false information about vaccines, and other misinformation (Van Bavel et al. 2020). In particular, the spread of misinformation through WhatsApp has become a major challenge, since high data costs for Internet access mean that discounted WhatsApp data bundles are the only affordable source of online information for many people in southern Africa (The Economist 2019). Moreover, since WhatsApp, unlike other social networks like Facebook or Twitter, is protected by end-to-end encryption, misinformation can spread while remaining especially difficult to monitor and regulate. The rise of misinformation is concerning because it may cause individuals to make harmful choices, whether by inducing false beliefs, priming particular modes of thinking, or by crowding out more credible information.

As social media is cost efficient for citizens in developing country settings, our project seeks to counter misinformation through these same popular low-cost channels. We propose to test the effectiveness of a WhatsApp-delivered fact-checking biweekly podcast on knowledge, attitudes, and behavior related to controversial topics which have been the subject of viral misinformation. We are interested in studying the longer-term effects of exposure to misinformation-targeting interventions, with a view toward understanding how to inoculate news consumers from believing potentially harmful, unverified information. To the extent that citizens seek to form accurate beliefs, rather than engage in motivated reasoning or adopt views of which they doubt the credibility, our intervention is expected to alter how citizens process information, what they believe, and potentially how they behave.

1.1 Literature

There is a growing literature on the efficacy of policies that combat fake news and viral misinformation, including (but not limited to) fact-checking interventions (Nyhan 2020; Pennycook et al. 2021). Most commonly, researchers provide corrective information to sample surveys and measure whether such researcher-provider information can shift knowledge and opinions about related topics in surveys. On average, studies in this literature demonstrate that it is possible to increase the accuracy of participants' beliefs through fact-checks, although effects vary depending on participants' prior beliefs and knowledge (Walter et al. 2020).

However, most fact-checking studies to date have important limitations. One challenge is that many survey-based fact-check experiments control the respondent's information environment for a short study period, raising the salience of researcher-provided fact-checks (Brashier et al. 2021; Guess et al. 2020). However in real life individuals can choose from multiple competing sources of information to consume or ignore. These experiments are also limited by the short time between provision of corrective information and survey implementation. By contrast, this study will use a field experiment in which information is provided naturalistically to respondents over a 6 month period; they are modestly incentivized to consume this information but can also choose to ignore it if they prefer.

In addition, the experimental design aims to test several mechanisms, suggested by both theory and the existing literature, which are hypothesized to strengthen the value of the fact-checking treatments. First, we focus on the role of emotion. A large literature demonstrates that belief in fake news (Martel, Pennycook and Rand 2020), as well as updating beliefs based on fact-checks (Gaines et al. 2007), is not a purely rational cognitive process—rather, it is deeply shaped by the emotional and identity commitments of individuals (Jerit and Zhao 2020). To date, the literature on emotions and fact-checking has largely focused on how negative or partisan emotions, either inadvertently or purposefully elicited by fact-checking treatments, reduce the ability of individuals to update and learn (Van Bavel and Pereira 2018). We add to these studies by examining another form of emotion—specifically, an appeal to the broader social good—as a way to elicit greater levels of updating. Another area of uncertainty in the literature relates to the length and complexity of fact-check messages. While meta-analysis of fact-check length on outcomes suggests no impact (Walter et al. 2020), we are not aware of evidence on the length of audio content (such as podcasts) or contrasts of text-based to audio-based interventions.

1.2 Intervention

The intervention we study consists of a set of informational treatments administered through WhatsApp. For each of these, we collaborate with the South Africa-based civil society organization Africa Check—the first and largest fact-checking organization in sub-Saharan Africa. As part of Africa Check's programming, the organization partnered with Volume, an independent South African podcasting firm, to launch "What's Crap on WhatsApp?" (WCW), a short biweekly podcast which debunks locally-relevant misinformation. Episodes generally last 6-8 minutes and cover three specific stories which have circulated on social media in South Africa in the preceding few weeks, with items occasionally suggested by podcast subscribers.

The podcast is disseminated to subscribers directly through WhatsApp, and consumes relatively little data to download. Relative to other misinformation-targeting interventions, the podcast has two potential advantages. First, it is a professionally-produced product, and are therefore likely to be more accessible, entertaining, and engaging than more anodyne modes of information delivery. Second, due to its mode of delivery through WhatsApp, it potentially allows listeners to quickly share content with their contacts, offering a chance for corrective information to spread relatively quickly within users' social networks. Our study experimentally tests the impact of the podcast intervention. Further, as detailed below, we produce three variants of each podcast episode—the normal version that Africa Check already disseminates to its subscribers, a version that seeks to empathize with participants that might have been fooled by the misinformation that the podcast shows to be fake, and a shortened version—and its accompanying messaging in order to understand which aspects of the intervention drive its potential effects. We further compare the podcasts with a simpler text-based intervention that only conveys the results of fact-checks via the basic WhatsApp message received by all participants that also receive the podcast.

Online recruitment for the study commenced in October 2020 and continues at the time of writing. This pre-analysis plan was submitted after the earliest batch of participants took the endline survey (n=126) but prior to any endline data analysis.

2 Research design

This section provides an overview of our study sample recruitment, treatment variants and randomization, data collection, and estimation strategy.

2.1 Sample recruitment and baseline survey

Individuals are eligible for study participation if they are currently living in South Africa, have a South African phone number, are at least 18 years of age, and are WhatsApp users. We recruit our study sample using a set of Facebook ads (see Appendix A for a sample ad). In an effort to ensure reasonably broad geographical coverage, we stratify these ads at the province-gender-age level, generating a total of 36 different ads.[1] The ads invite participation in a research study relating to social media in South Africa for which participants will be provided airtime.

Upon clicking an ad, potential participants are first redirected to a Qualtrics landing page where they read the informed consent information and agree to participate. If the participant agrees to proceed, they are then asked to send a WhatsApp message to the phone number associated with our interactive project WhatsApp chatbot. The chatbot repeats the informed consent process and further determines eligibility based on demographic information that the participant provides at the start of the baseline survey.[2]

Conditional on eligibility, the chatbot then immediately administers the baseline survey instrument. The baseline survey records (1) initial attitudes on different sources of information, both off- and online; (2) attitudes and behaviors regarding misinformation and fact-checking; (3) baseline knowledge about current affairs and COVID-19; (4) podcast listening habits; (5) behaviors relating to social distancing measures that were undertaken at the start of the pandemic in South Africa. As part of the baseline survey, participants are required to send a WhatsApp message to a phone number run by Africa Check and add that number to their phone contacts,[3] which we validate. They are informed that, subsequent to the baseline survey, Africa Check might send them information. Participants are incentivized with R30 (approximately $2) in mobile airtime credit for

[1] Specifically, ads are targeted according to (1) province of the user, of which there are 9 total (2) whether the user is male or female (3) whether they are between 18-29 or 30-49 years old. Our pilot testing suggested that attracting over-50s to participate in the study was extremely expensive.

[2] Potential participants found to be ineligible have their phone numbers banned by the chatbot to avoid falsified eligibility information. See Appendix B for an example of the chatbot interface.

[3] This is required for Africa Check to be able to send them their podcast through a WhatsApp list.

completing the baseline survey and for successfully messaging Africa Check's WhatsApp account.

2.2 Random assignment and experimental treatments

Due to the rolling nature of study recruitment (detailed below), we block randomize batches of participants into treatment conditions once every two weeks. We block on a set of variables including demographic characteristics, social media usage, attitudes towards different media sources, and knowledge regarding pieces of misinformation.[4]

We adopt a "nested" blocking strategy, whereby we construct two levels of concentric randomization blocks. At the lower level, a block contains 19 respondents. To account for the possibility of attrition reducing within-block variation in treatment assignment, we also aggregate these blocks into higher-level blocks containing a greater number of participants—specifically, the larger blocks combine two smaller blocks to contain 38 individuals. As a result, with a choice of blocks defined at different levels of granularity, for estimation purposes we will be able to choose the level which minimizes within-block participant characteristic variation subject to sufficient levels of endline survey completion across the different treatment conditions within a block.

Table 1: Treatment Assignment

		Text only		Short podcast (3-5 min)		Long podcast (6-8 min)		Emotional podcast (6-8 min)	
	Control	F	S	F	S	F	S	F	S
Podcast incentives	0.00	0.04	0.04	0.09	0.09	0.09	0.09	0.09	0.09
Placebo incentives	0.24	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.02

Table presents the sample sizes of the planned design. 'F': factual message; 'S': social message. All podcast treatments also include the text message via WhatsApp.

Study participants are randomly assigned to either *control* or one treatment group. The treatments are distinguished along three dimensions: (1) mode of information delivery; (2) messaging encouraging information consumption; (3) whether participants are incentivized to take up the treatment. Table 1 summarizes the research design with the approximate share of participants assigned to each cell. In total, we are targeting a baseline survey sample of around 5,500 participants, with the expectation of approximately 2,000 completing the full six month study. In Appendix C and D, we provide a sample script of the different messaging and quizzes, respectively.

[4] We use the `R` package `blocktools` to assign blocks, batch by batch, based on a greedy algorithm using Mahalanobis distance.

2.2.1 Mode of information delivery

First, we vary how the information contained in the podcasts is delivered to participants. We administer four treatment variants: (1) a text-only treatment, (2) a short podcast, (3) a longer podcast, and (4) a longer podcast which includes emotional appeals. Each variant contains the same core information regarding the truthfulness of (often viral) news fact-checked by Africa Check; the variation comes from the mode of information delivery.

The text-only treatment contains a true, false, or misleading tag for three pieces of news that Africa Check has identified for the specific week. This information is summarized simply in a single sentence. Each such WhatsApp message also includes a link to a longer article on Africa Check's website for each item. The items that WCW covers are generally sourced from social media, are mostly shown to be false, and frequently cover issues relating to public health, government, and immigration.

The text-only fact-checking content is contrasted with three more engaging, but also more time-consuming, forms of information dissemination via a podcast. Each form of the podcast is sent as part of a WhatsApp message that also contains the text-only information; the podcast thus come in addition to the text-only treatment. The short podcast is a 3-5 minute conversation between the man and woman serving as co-hosts, explaining, discussing, and evaluating the truth of the same three pieces of viral news. The short conversation of each viral news items culminates in concluding whether it is true, false, or misleading, and how Africa Check came to that conclusion. The longer podcast is a 6-8 minute conversation between the co-hosts. In the longer podcast, the co-hosts go into greater depth about the sources that they consulted and the conclusions they are able to draw. In the emotional variant of the longer podcast, which also lasts for around 6-8 minutes, the hosts specifically acknowledge in an empathetic manner the underlying reasons—such as economic insecurity or distrust in the state—which might lead people to be susceptible to a particular piece of misinformation. The rationale is that by acknowledging the emotions behind misinformation, this variant of the treatment may increase engagement with the podcast and information, especially among those fooled by the misinformation who may be more likely to engage in motivated reasoning. It may also increase the salience of fake news and fact-based decision making among listeners. However, since the emotional component is only added to one of the three fact checks in each episode, this treatment is relatively subtle.

2.2.2 Messaging encouraging information engagement

Along the second dimension, we vary the type of messaging used to induce participants to consume their informational treatment. Specifically, we vary whether participants receive a 'factual' WhatsApp message or a 'social' WhatsApp message. Under the 'factual' message condition, participants are sent a message which announces the availability of the podcast variant (or just contains the text variant summarizing the fact checks). Under the 'social' message variant, participants are sent the same message but containing an appeal which highlights the potential harms of misinformation—whether to participants' friends and family or society more broadly—and in some cases further emphasizes potential reputational benefits of being informed within a social network.

2.2.3 Incentivized treatment uptake

To maximize treatment uptake and continued engagement with the project (across mode of delivery, as well as in general), we further administer incentivized monthly quizzes that encourage participants to pay attention to the information provided. However, since the quizzes cover information from the treatment deliveries, incentivized quizzes can only be delivered to participants in treatment groups and not participants in the control group. Yet, not providing the control group with quizzes may introduce differential attrition. We therefore provide all participants with incentivized quizzes, but all control participants and a portion of treated participants are randomly assigned to receive "placebo" quizzes, which contain questions about pop culture or sports topics which are not covered in the treatment messages or podcasts. We specifically avoid political and current affairs topics for the placebo quizzes to minimize potential overlap with the content of the podcasts. We assign some treated participants to receive the placebo quizzes in order to test whether incentives are required for individuals to engage with the treatments.

Each quiz is six questions long and takes roughly two minutes to complete. If the participant answers less than four questions correctly, they receive R10; if they answer four or more questions correctly, they are rewarded with an additional R10 for a total of R20. These incentives are delivered in the form of mobile airtime credits. All participants are informed of which types of quiz questions they will receive at the outset of the study and their assignment is constant across quizzes.

2.3 Treatment delivery and data collection

Treatment delivery and data collection are all conducted through WhatsApp.

2.3.1 Treatment delivery

Once participants subscribe to the Africa Check WhatsApp account during the baseline survey, Africa Check assigns participants to a specific WhatsApp broadcast list associated with their treatment condition (or to no broadcast list for control). Then, Africa Check delivers the corresponding treatment combination to participants through messaging every two weeks.

2.3.2 Data collection

We collect survey data through the WhatsApp chatbot provider Landbot. Data is collected through the baseline survey, monthly quizzes, a midline survey administered three months into the study for a given batch, and finally an endline survey administered six months into the study for a given batch. Participants are enrolled on a rolling basis and are grouped into two-week "batches" to correspond with their biweekly treatment delivery from Africa Check. A sample of the study timeline is reproduced in Appendix E for each batch of participants. Quizzes contain material relevant to the two prior treatment deliveries.[5]

2.4 Estimation

To estimate the effect of treatment assignments on engagement with the fact-checking content and subsequent beliefs and behaviors, we use the midline and endline surveys (as well as the quiz answers) to compare treated individuals across different treatments conditions and with the

[5] For example, a podcast-incentivized quiz will ask participants quiz questions about content sent to participants in the preceding month; while a placebo-incentivized quiz will ask about pop culture events that occurred in the preceding month.

control condition. We start by describing the most general form of regression specification before then detailing how we will collapse treatment conditions to increase statistical power.

We estimate average treatment effects using the following OLS regression:

$$Y_{ib} = \alpha_b + \beta Y_{ib}^{pre} + \boldsymbol{\gamma} \mathbf{X}_{ib}^{pre} + \boldsymbol{\tau} \mathbf{T}_{ib} + \varepsilon_{ib}, \tag{1}$$

where Y_{ib} is an outcome for respondent i from block b in a given survey wave, $\mathbf{T}_{ib}$ is the vector of individual treatment assignments, α_b are randomization block fixed effects,[6] Y_{ib}^{pre} is the baseline analog of the outcome (where feasible) and $\mathbf{X}_{ib}^{pre}$ is a vector of additional baseline covariates selected via LASSO.[7] The vector $\boldsymbol{\tau}$ captures the effect of each treatment condition; the effect of different treatment conditions can be identified by comparing elements within this vector. Robust (HC2) standard errors will be used throughout, except where survey waves are pooled (to examine quiz scores across treatment conditions and for questions repeated in midline and endline) when standard errors will be clustered at the individual level. We can further estimate heterogeneous and conditional treatment effects by pooling across relevant treatments and interacting $\mathbf{T}_{ib}$ in equation (1) with relevant predetermined covariates.

Although we can analyze each treatment condition separately, the study was designed with the intention of pooling across similar treatment conditions to increase statistical power. To examine how access to the fact-checking content by text-only messages and/or podcasts affect outcomes, we will pool across treatment conditions in the following ways:

1. Emotional podcast vs. long podcast vs. short podcast vs. text only vs. control: pool conditions across quiz incentives *and* across 'factual' and 'social' WhatsApp message types.

2. Long podcast vs. short podcast vs. text only vs. control: pool conditions across quiz incentives *and* across 'factual' and 'social' WhatsApp message types *and* across long and emotional podcasts.

3. Any podcast vs. text only vs. control: pool conditions across quiz incentives *and* across

[6] In practice we intend to report both of the potential blocking levels in our analyses.

[7] As potential covariates, we will consider all standardized baseline covariates and their interaction with $\mathbf{T}_{ib}$. For each outcome variable, we will use cross-validated LASSO to select the conditioning variables for inclusion in Equation (1). When examining heterogeneous effects, we will hold fixed the set of conditioning variables between estimating the ATE and the CATE.

'factual' and 'social' WhatsApp message types *and* across longer, shorter, and emotional podcasts.

4. Any fact-checking treatment vs. control: pool conditions across quiz incentives *and* across 'factual' and 'social' WhatsApp message types *and* across text only messages and all podcast types.

5. Differential effects of fact-checking treatments by encouragement message: pool conditions across quiz incentives.

6. Differential effects of fact-checking treatments by incentive: pool conditions across 'factual' and 'social' WhatsApp message types.

The first four of these comparisons constitute the analyses of principal interest. The fifth and sixth are important in conjunction with the engagement results (discussed next) for understanding whether any differences between treatment conditions reflect a greater probability of exposure to treatment across treatment conditions and/or differences in the content itself. For each type of analysis, we will report results that both include these observations in the control group and drop these observations from the analysis in the event that placebo incentives do not affect text only messages or podcast engagement.

To examine the effects of encouragement messages on engagement with the fact-checking content (which we measure in various ways described below), we will pool across treatment conditions in the following ways (excluding control group respondents that did not receive any content to engage with):

1. Factual vs. social encouragement messages crossed with podcast vs. placebo incentives, by fact-checking information type: no pooling.

2. Factual vs. social encouragement messages, by fact-checking information type: pool conditions across quiz incentives.

3. Factual vs. social encouragement messages, by any podcast vs. text only : pool conditions across quiz incentives *and* across all longer, shorter, and emotional podcast conditions.

4. Podcast vs. placebo incentives, by fact-checking information type: pool conditions across ‘factual’ and ‘social’ WhatsApp message types.

5. Podcast vs. placebo incentives, by any podcast vs. text only: pool conditions across ‘factual’ and ‘social’ WhatsApp message types *and* across all longer, shorter, and emotional podcast conditions.

2.4.1 Missing data

We expect to encounter two forms of missing data: attrition from surveys; and “don’t know” responses to particular questions. To assess the extent to which differences in attrition across treatment conditions may introduce biases, we will: (i) use the equation specified above to examine the extent to which attrition varies across treatment groups; and (ii) compare balance tests of predetermined (baseline) covariates at the point of assignment (before attrition can occur) with balance tests among the non-attrited sample in the midline and endline surveys. In the event that we encounter severe attrition, we will seek to condition the sample on predetermined covariates for which there is limited imbalance and conduct analysis using Lee bounds. With regard to “don’t know” responses to specific questions in a survey, such responses will be coded as “negatives”—that is to say, not doing the thing noted in the question (e.g. when asked about listening to podcasts “don’t know” would be coded as “never”, while for the importance of an issue “don’t know” would be coded as “not at all important”); where “don’t know” relates to a Likert scale, don’t know will be coded as the median/neutral option (e.g. as “neither agree not disagree”).

2.4.2 Low-quality responses

Low quality respondents are removed during the recruitment process using three attention-checking questions that randomly appear throughout the baseline survey. These attention-checking questions are designed such that they are easy to respond if respondents read the question (e.g. ”What year is it?”). Respondents who do not pass these these questions are deemed ineligible to proceed with the study and are not included in the randomization process. Their phone numbers are also prevented from restarting the baseline survey.

Though we are able to ascertain a baseline level of response quality across all participants in the study using the aforementioned method, we further restrict the sample to conduct robustness checks in two ways. First, our own pilots of the baseline survey suggest that the entire survey cannot be plausibly comprehended and completed in less than 6 minutes. Therefore, as a conservative estimate, we conduct robustness checks using only the subsample of participants who took more than 8 minutes to complete either the baseline survey or endline surveys. Second, we obtain pre-treatment demographic data on the participant's province and level of education at baseline and midline. While it is possible that the participant may have moved during the study or may have attained additional education, such instances are likely to be rare. For a second set of robustness checks for data quality, we therefore restrict the sample only to individuals whose responses to these two questions match across baseline and midline.

2.4.3 Statistical inference

For hypotheses where we prespecify an expected direction, e.g. a positive effect of treatment on a given outcome, we will use one-sided t tests to evaluate the hypothesis. In the event that the coefficient has the opposite sign, we will use two-sided t tests to evaluate whether the null hypothesis can be rejected. Where no direction for a hypothesis is specified, we will instead conduct two-sided t tests.

3 Hypotheses

We next pre-specify our primary hypotheses by outcome family. For each family of outcomes, we also compute inverse covariance weighted (ICW) indices that are standardized relative to the control group.

The hypotheses below refer to the text only message and podcasts collectively as the treatment. However, across all hypotheses, we expect the effects of fact-checked information to be particularly concentrated among participants assigned to: (1) podcasts rather than text messages; (2) emotional podcasts rather than similarly-long non-emotional podcasts; (3) podcast-incentives rather than those assigned to placebo-incentives; (4) social messages rather than factual messages. For each of these predicted differences in effect magnitude, we conduct one-sided tests. We do not anticipate a particular direction for (5) longer podcasts rather than short podcasts, for which we conduct two-sided tests.

3.1 Exposure to intervention ("first stage")

We first expect that participants assigned to the treatment conditions should exhibit greater knowledge and awareness of the information they have received through the duration of the study at endline:

H1 : Access to fact-checking content increases exposure to, and knowledge about, information covered by the treatment deliveries.

We measure these effects using responses to questions about (1) participants' self-reported listening to podcasts, specifically WCW; (2) participants' correct answers to quizzes embedded in the midline and endline cover factual information from the two prior treatment deliveries; (3) the frequency with which participants report being alerted that particular pieces of information on social media are fake; (4) participants' knowledge about sources which can be used to verify information; (5) participants' knowledge about specific fact-checkers. In addition, we will combine core outcomes (1)-(3) using an ICW index; variables (4) and (5) will be analyzed separately because they are less direct measures of engagement. We can also compare the monthly podcast quiz scores between treatment conditions, but cannot draw comparisons with the group (or other treated groups) that only received the placebo quizzes.

3.2 Perceptions of misinformation and trust in information sources

We hypothesize that participants assigned to treatment should then become more aware of the extent of misinformation. In the context of our study, Africa Check debunks misleading or fake information that are shared on various social media websites through various friend and family networks. We therefore expect that:

H2 : Access to fact-checking content increases participants' perceptions of the extent of misinformation circulated through social media platforms.

We measure participants' perceptions of the extent of misinformation using: (1) participants' beliefs about how much information on platforms like WhatsApp, Facebook, and Twitter is false; and (2) how much information from WhatsApp groups (either consisting of close friends/family or large WhatsApp groups) is false. We will combine these two measures using an ICW index.

In addition to perceptions of the extent of misinformation, we also hypothesize that the treatment will induce a more general decrease in trust in information from the same set of sources:

H3 : Access to fact-checking content reduces participants' trust in information received on social media platforms.

We measure participants' trust in the information they receive from the same set of sources as H2, which we will similarly combine using an ICW index. We expect weaker treatment effects, if any, on beliefs about misinformation (and trust) relating to traditional media sources, such as radio, TV, and newspapers, which are generally more likely to verify the information they cover and are less frequently the targets of fact-checks on WCW.

3.3 Consumption and sharing behavior

We expect that the treatment, by shifting participants' beliefs about the credibility of different information sources, will change participants' behavior regarding consuming and sharing information:

H4 : Access to fact-checking content reduces participants' consumption, and sharing, of information from social media platforms.

H5 : Access to fact-checking content increases participants' attention to the veracity of information they encounter on social media platforms.

Specifically, for H4, we expect that treated participants will (1) consume less information from social media platforms (such as WhatsApp, Facebook, and Twitter) overall, and (2) more specifically from sources on WhatsApp aside from organizations to which they have subscribed. Additionally, due to their increased knowledge of the extent of misinformation, we expect that (3) treated participants in general should share and forward information on social media platforms less frequently. We will again combine these measures using an ICW index. We assess H5 based on responses to a set of questions about how much attention participants pay to the truthfulness of information they are sent on social media platforms.

3.4 Behavior around misinformation

A primary set of outcomes relates to participants' changes in behavior when presented with potential misinformation. We hypothesize that treatment will have the following effects on participants' behavior:

H6 : Access to fact-checking content changes participants' capacity to identify, and express skepticism on the basis of, characteristics of misinformation.

H7 : Access to fact-checking content changes participants' behavior in checking the veracity of information they encounter through social media platforms.

For H6, we primarily measure participants' beliefs about the characteristics of misinformation using a conjoint experiment embedded in the endline survey instrument. Across a set of four questions which hold fixed the truthfulness of a given claim (some of which are true and others are false), we vary whether participants are (1) provided a credible source for the claim; (2) told that the claim has been independently validated; (3) told that the piece of information was from a viral Facebook post; and (4) told that the claim came from a source that is likely to be subject to sensationalized fabrication. The potential importance of each characteristics for identifying fake news could have been learned or primed by the text and podcast treatments. Characteristics (1,2) are intended to positively signal truthfulness of a particular claim, while (3,4) negatively signal truthfulness. We test this by randomizing whether these features are associated with a given claim and then test whether treated respondents are more more likely to believe a claim when characteristics (1) and (2) are present and less likely to believe a claim when characteristics (3) and (4) are present. We combine these four measures using an ICW index. We expect that treated participants are likely to be more responsive to these signals than control, such that the interaction between treatment and the conjoint treatment is larger.

For H7, we measure effects on behavior relating to verifying information using questions asking: (1) how important they think fact-checking is; (2) how often they fact check information; (3) when they fact check, whether they use fact-checkers relative to other less reliable sources; (4)

whether they state that lack of knowledge about how and where to check information inhibits the extent of their fact-checking; and (5) whether they shared misinformation corrections with their friends and family. We combine these five measures using an ICW index.

The effects on these behavioral outcomes in H6 and H7 depend on how participants adjust to increased perceptions of misinformation, altered beliefs about the topics that were fact-checked, and/or empowerment to detect whether a piece of content constitutes misinformation.

3.5 Secondary treatment effects

We also examine potential secondary effects that the treatment may elicit. The posts that are fact-checked in the text messages and podcasts are topically broad. These fact-checks can be roughly divided into the following categories: (1) stoking anti-government or racial/nationalist sentiments from various important figures and politicians; (2) general conspiracy theories or fear-based misinformation; and (3) misinformation pertaining specifically to COVID-19 or vaccine hesitancy. The content of these podcasts could then influence related beliefs in several domains.

First, misinformation stemming from viral posts in categories (1) and (2) may promote political polarization and populist attitudes. We therefore hypothesize secondary treatment effects that temper such polarization:

H8 : Access to fact-checking content improves participants' perceptions of government performance and capacity and reduces support for populism.

We adapt questions on polarization and populism from various sources comprising: (1) perceptions of government performance, overall and with respect to COVID-19; (2) perceptions about government capacity (i.e. government's ability to carry out roads and electricity projects, conditional on its desire to do so); (3) beliefs about whether the government only serves elite interests; (4) whether the respondent intends to vote for the national incumbent party; and (5) whether the respondent feels close to the national incumbent party. We combine these outcomes using an ICW index.

Second, misinformation stemming from category (3) may discourage preventative behaviors while heightening fears around vaccination. We therefore test whether:

H9 : Access to fact-checking content increases participants' knowledge and beliefs in the severity of COVID-19 and their willingness to take preventative measures.

We measure this using questions relating to (1) self-reported preventative behavior in the week prior to enumeration; (2) beliefs in whether COVID-19 is a hoax and whether lockdowns are justified; and (3) trust in, and intentions to receive, a COVID-19 vaccine when available. We again combine these outcomes using an ICW index.

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