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 reduce susceptibility to misinformation in a sustained manner outside controlled environments, 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. The intervention induced substantial consumption and internalization of fact-checks, while increasing participants' ability to discern political and health misinformation *upon exposure*—especially when consumption was financially incentivized. Fact-checks that could be quickly consumed via short text messages or via podcasts with empathetic content were most impactful; short messages further increased government approval and compliance with COVID-19 policies. Conversely, we find limited effects on news consumption choices. Our results demonstrate the benefits of inducing sustained exposure to fact-checks, but highlight the difficulty of shifting broader media consumption patterns.

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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 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 (Arechar et al. 2022; Argote Tironi et al. 2021; Berlinski et al. 2021). By fueling ideological divides and increasing 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 already been linked to lynchings and mass electoral mobilization in India and racial violence in South Africa (Allen 2021; Badrinathan 2021).

Efforts to limit the potential impact of misinformation typically engage in *debunking* or *prebunking*. 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, derived from inoculation theory (Cook, Lewandowsky and Ullrich 2017; McGuire 1964), 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. Nyhan et al. 2020; Wood and Porter 2019) have been shown to increase skepticism of misinformation.

Fact-checking—one popular method of combating misinformation—highlights the complementarities between debunking and prebunking. Fact-checking most obviously debunks by informing citizens about particular false (and true) claims. However, it also prebunks by increasing general awareness of misinformation, explaining the logic behind common forms of misinformation, and explaining information verification strategies. As a result, fact-checking potentially limits

the harmful consequences of misinformation by shaping citizens' discernment of misinformation *upon exposure* as well as by shaping media consumption choices which affect the extent of exposure in the first place. Correspondingly, fact-checking institutions have been established across the world to combat misinformation. The International Fact-Checking Network now includes more than 100 member organizations, while Facebook, Twitter, Instagram, and TikTok have integrated such fact-checks into their platforms.

However, despite these potential benefits, it is difficult to induce citizens to *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 forced consumption settings (e.g. Porter and Wood 2021), outside of the lab it 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 problem of internalization, even conditional on information consumption (Zaller 1992), and calls into question fact-checking's efficacy at combating misinformation beyond the lab or online surveys. Moreover, little is yet known about how fact-checking shapes political dispositions beyond those narrowly 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. Twice a week for six months, treated participants in our large rolling sample of social media users were sent three fact-checks via WhatsApp messages. These fact-checks dissected largely-false stories that were trending on social media in South Africa in the preceding weeks pertaining to politics, health, and other high-profile topics. 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.

We further examine if, and how, citizens can be induced to engage and internalize fact-checks by randomly varying how the fact-checks were disseminated to participants. These four treatment conditions varied the appeal and cost of consuming the fact-checks, and 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 link to additional information assessing a 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. 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 (Pennycook et al. 2021) 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 empathy plays in driving the internalization of information (Gesser-Edelsburg et al. 2018; Gottlieb, Adida and Moussa 2022; Kalla and Broockman 2020).

Our corresponding 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-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, suggesting that the intervention activated latent demand—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, 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 true from false stories relating to politics and public health issues and increased their skepticism towards prominent conspiracy theories—none of which were covered during the intervention. Our results suggest that this may be driven by treated participants' increased understanding of what credible content looks like, their reduced trust in social media, and their greater capacity to verify content for themselves. Nevertheless, the treatments did not impact the amount of news that participants consumed from social and traditional media, and thus their risk of being exposed to misinformation. These results suggest that sustained exposure to fact-checks primarily combats misinformation by increasing skepticism upon exposure to such 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 can be effective particularly when it includes emotive appeals to increase the resonance of corrective information with consumers.

Our study advances understanding of misinformation, how to combat it, and its political consequences in several key ways. First, we demonstrate that sustained exposure to fact-checks can debunk and prebunk misinformation. The importance of repeated engagement helps to make sense of the mixed evidence that single-shot media literacy interventions can effectively prebunk mis-

information (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 interventions conducted outside controlled research environments can be effective when citizens are motivated to consume fact-checks. By further measuring an unusually broad array of outcomes, we establish that the enduring effects of our prebunking 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 hope for demand-side interventions, this finding simultaneously emphasizes the need for complementary supply-side change.

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 (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 emphatically—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 competitive 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).

Finally, this article address 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 more limited political impact (Jerit and Zhao 2020). By demonstrating that WhatsApp-based text messages regularly conveying fact-checks both increase faith in the incumbent government and reported compliance with its policies, we show that (combating) misinformation can have

durable political consequences. We are not aware of any studies that have previously established this connection outside of lab settings. 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?

Within developing country settings, 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 (Busam 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 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—which is derived from inoculation theory—posits that people can be "inoculated" against misinformation in general when they are consistently warned about misinformation's existence and are equipped with tools to identify it (Cook 2013; Martel, Pennycook and Rand 2020). Common prebunking interventions include warning labels or digital literacy training (e.g. Badrinathan 2021; Cook, Lewandowsky and Ullrich 2017; Offer-Westort, Rosenzweig and Athey 2022; Pereira et al. forthcoming; Tully, Vraga and Bode 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 highlight the broader threat of misinformation, explain how misinformation can be debunked using reliable sources and fact-checking techniques, and simultaneously explain the faulty logic behind certain false claims. Ultimately, fact-checking may not only debunk but also prebunk by increasing consumers' media literacy, thereby generating awareness about how to spot misinformation and engage in fact-checking themselves.

Fact-checks potentially then combat misinformation in two main ways. First, misinformation's impact could be reduced *upon exposure* as people become more discerning of, and also more equipped to verify, what they are consuming. Even if their overall exposure to misinformation is not affected, internalization of the lessons from fact-checks may nevertheless ensure that individuals become more skeptical of the misinformation—and, ideally, more trusting of truthful information—they encounter on social media or elsewhere. Second, they could *reduce exposure* to misinformation by teaching individuals how to recognize—and thus avoid—potential sources of such misinformation. Because fact-checks also educate people about which types of sources are legitimate information providers, they may start consuming more reputable sources.

Although a number of studies experimentally demonstrate fact-checking's promise (see Nyhan 2020), these studies also have important limitations (Flynn, Nyhan and Reifler 2017; Walter et al. 2020). First, existing work primarily relies on one-shot interventions and often forces participants' exposure to fact-checks in lab or survey environments. Outside these settings, however, citizens who allocate their time across a wide array of activities often choose not 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, Nyhan 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 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 backlash (Nyhan and Reifler 2010) or motivated reasoning (e.g. Taber and Lodge 2006) in response to counter-attitudinal information. Furthermore, existing research has tended to find only short-term success in combating the *specific* pieces of misinformation that the fact-checks targeted, while failing to affect consumers' broader susceptibility or underlying attitudes or behaviors (Carey et al. 2022; Hopkins, Sides and Citrin 2019; Nyhan 2021). Via either mechanism, limited internalization has negative implications for fact-checking's potential benefits for media literacy.

2.1 Improving the efficacy of fact-checks

Drawing from established theoretical frameworks, we consider how citizens 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 the 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 emotive content on social media, misinformation-correcting interventions that are quicker to digest for users might induce more consumption than interventions that take longer to parse and understand. Given that internalization depends on initial consumption, easier-to-consume fact-checks may ultimately prove to be more effective in increasing audience reach and awareness.

Another potential solution is to make fact-checking content more *engaging*. Research on "edutainment" demonstrates how delivering information in more interesting and entertaining ways positively affects consumption, information recall, beliefs, and behaviors (e.g. Baum 2002; Baum and

Jamison 2006; Kim 2023; La Ferrara 2016). Notably, Banerjee, La Ferrara and Orozco-Olvera (2019) found that exposure to television programming helped to increase awareness of HIV and health behaviors in Nigeria. Furthermore, Roozenbeek and Van der Linden (2019) and Maertens et al. (2021) 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

Sustained exposure may mitigate some of the shortcomings associated with the internalization of fact-checking interventions. First, by increasing the volume of content consumed, sustained exposure might reduce the likelihood that fact-checking content is crowded out by other content.[1] Second, internalization of media literacy lessons may require longer and more frequent exposure (Guess et al. 2020; Tully, Vraga and Bode 2020). While individual fact-checks may teach viewers about certain warning signs, consistent fact-checking content can help to build up an arsenal of reliable strategies and misinformation logics, which encourage critical thinking skills and equip people to be more discerning media consumers. Third, sustained exposure could enhance users' trust in the fact-checking source (Gentzkow, Wong and Zhang 2021), which may in turn increase internalization (Alt, Marshall and Lassen 2016).

The mode by which fact checks are delivered might also 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. Finally, more detailed fact-checks may increase increase information retention and thereby boost media literacy (Lewandowsky et al. 2012). On the other hand, shorter messages may be less taxing on

[1] In addition, when consumers receive fact-checks consistently, they are more likely to be aware of the prevalence of misinformation, leading them to become more careful about what they read.

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 which 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, the trade-off between the optimal level of detail and tone of delivery may instead *decrease* fact-checks' effectiveness.

2.2 Theoretical expectations

Together, we anticipate that sustained exposure to fact-checking ought to combine aspects of both debunking and prebunking for misinformation correction. By enhancing consumers' consumption and internalization of corrective information, their ability to discern true from false information online and knowledge of verification techniques should increase, while reducing the extent of their trust in, and consumption of, social media content. Citizens exposed to such fact-checking interventions over a sustained period could then either learn to identify and discern misinformation, and also verify it, upon exposure, or otherwise change their behaviors which affect exposure to misinformation in the first place. To the extent that misinformation typically focuses on salient

false claims about politics or public policy, sustained exposure to fact-checks might then induce improved perceptions of government performance and compliance with its policies.

Understanding *how* to effectively increase organic consumption and internalization, however, is 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. Appendix A.4 discusses our pre-specified expectations relating to this trade-off for our study, including that interventions leveraging "edutainment" or more empathetic content would be most effective by enhancing internalization at the potential cost of initial consumption.

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. From rumors that COVID-19 did not affect Black Africans, to vaccines implanting microchips for government surveillance, to home remedies and miracle cures (Africa Check 2023), pandemic-related misinformation capitalized on deep citizen distrust of information provided by their government and perceived global elites (Steenberg et al. 2022). Such misinformation has widened health inequality and compliance with government policies; vaccine hesitancy was highest among

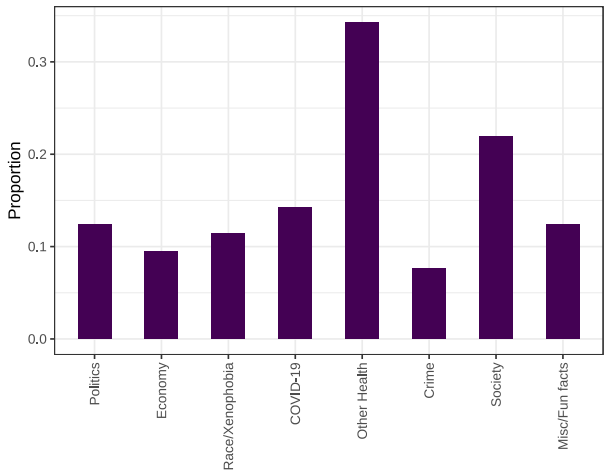
the most segregated and marginalized communities (Steenberg et al. 2022).

The widespread use of mobile phones and social media platforms like Facebook and WhatsApp in South Africa has fueled the proliferation of misinformation. WhatsApp stands out as 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 have only worsened during the COVID-19 pandemic (Quartz Africa 2020).

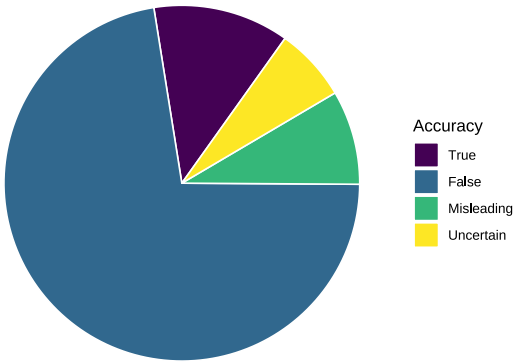
To combat rising quantities of misinformation, civil society organizations have developed fact-checking tools and initiatives to verify the accuracy of the information circulating on social media. Africa Check is a prominent example: since its founding in 2012, the South African nonprofit has focused its efforts on verifying claims made by public figures and popular content that appears online or on social media. Since 2019, Africa Check has also 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 entertaining 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-checking programming. During the study period, Figure 1 shows that most of these fact-checks related to (generally false) claims about politics, health issues, and broader social issues. Political fact-checks tended to debunk incendiary claims relating to government corruption or incompetence, while health fact-checks often focused on debunking myths and false cures related to COVID-19. Appendix B.1 provides specific examples.



(a) Topics fact-checked



(b) Accuracy of facts checked

Figure 1: Biweekly fact-checked content

4.1 Participant recruitment

Following a brief pilot, we recruited participants for the study from across South Africa between October 2020 and September 2021, with participants recruited in 21 "batches" on a rolling basis (typically once every two weeks). Facebook advertisements were used to recruit adult Facebook users for a research study on misinformation in South Africa (see Figure A1a).[2] 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.

Eligible participants then completed a baseline survey administered via a WhatsApp chatbot (see Figure A1b). The baseline survey recorded participants' demographic characteristics, attitudes regarding misinformation, baseline 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.[3]

[2]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. See Appendix A.1 for additional information on recruitment.

[3]Participants were further 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

This pool of participants was 28 years old on average, and mostly urban (76%), female (61%), and educated (89% report receiving secondary education). Figure A2 compares this sample with nationally representative data from 2018 round of the Afrobarometer survey. Our sample is similar in terms of observables to the Afrobarometer subgroup who report ever using social media, with only modest differences in age, gender, and education observed.

4.2 Treatment assignment and delivery

Our sample of participants were randomly assigned to either a control group that received no fact-checks or one of four treatment conditions. All treated participants received the same three fact-checks via WhatsApp once every two weeks for six months; Appendix B.1 provides examples of specific fact-checks. However, the fact-checks were delivered in different ways across treatment conditions.

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.[4] Among those receiving podcasts, we further varied how costly or empathetic the content was. The default *Long* podcast—which Africa Check

necessary for Africa Check to be able to deliver treatment information to participants through its WhatsApp broadcast lists. Further, we added simple attention checks (see Appendix A.1) to screen out low-quality respondents.

[4]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.

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 fact-checks. 7,331 participants saw their treatment assignment; the residual 1,616 selected out of continued engagement with the study after completing the baseline survey. Treatment was then delivered via Africa Checks' WhatsApp account every two weeks 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.3 for details. Participants who received their treatment regularly took these interim quizzes (see Figure A3).

The fact-check quizzes did not provide participants with the correct answers or tell them which questions they answered correctly. Further, these quizzes were administered through a different WhatsApp account from the Africa Check account used for treatment delivery. 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.

4.2.3 Summary of interventions

Figure 2 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.[5]

4.3 Outcome measurement

After six months, each participant completed an endline survey. Conditional on reaching the endline (n=4,541), participants were highly engaged, taking an average of 88% of the monthly quizzes.[6] In addition to a final quiz—which related to the fact-checks, regardless of a participant's quiz assignment during the study—and other measures of treatment engagement and internalization, the endline survey measured our primary outcomes: discernment of content truth, verification knowledge, and trust in media; information consumption, verification, and sharing patterns; and attitudes and self-reported 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). Appendix Table A1 provides definitions and summary statistics for each index component, while Appendix A.5 notes how we deal with missing data and justifies some differences from our pre-specified outcome measures.

[5]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.

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

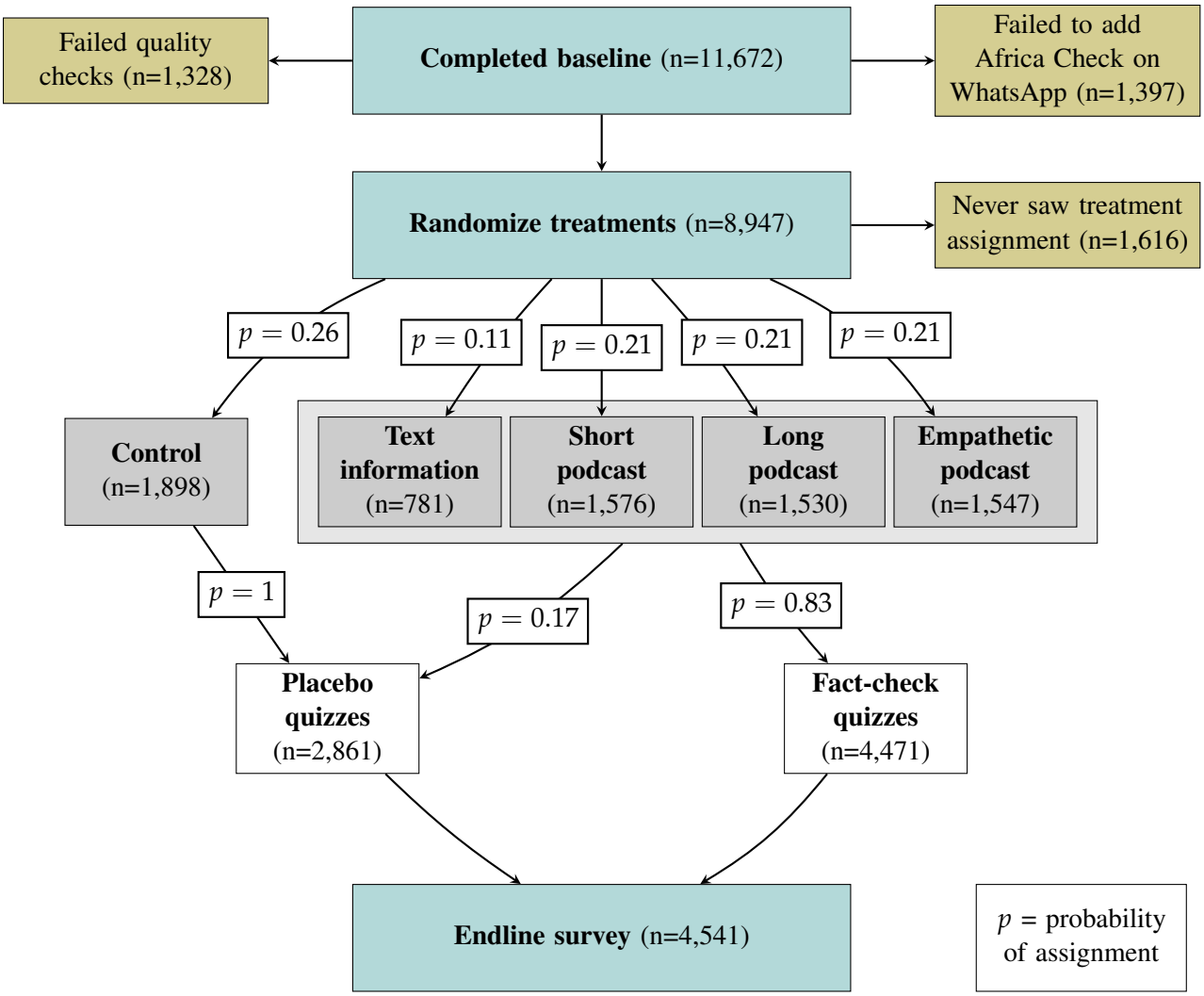


Figure 2: **Overview of treatment assignments**
The main treatment arms include a pure *Control*, a *Text*-only treatment, a *Short* (4-6min) podcast, a *Long* (6-8min) podcast, and an *Empathetic* variant of the long podcast. Participants were additionally incentivized to consume particular content through optional monthly quizzes, relating either to the treatment information (*Fact-check quizzes*) or pop culture (*Placebo quizzes*).

4.4 Estimation

We estimate intent-to-treat effects of different combinations of treatment arms relative to the control group using the following pre-specified 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, $\mathbf{T}_{ib}$ 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 effect of each treatment condition.

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 *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*).[7] Reflecting the individual-level randomization, robust standard errors are used throughout. For inference, we use one-sided t tests to evaluate hypotheses where we pre-specified a directional hypothesis (see Appendix A.4). Otherwise, or in cases where the pre-specified direction is the opposite of the estimated treatment effect, we use two-sided t tests.

We validate the research design in several ways.[8] First, we examine differences in the probability of completing the endline survey by treatment arm. Appendix Table A2 shows balance in attrition across treatment conditions. Second, we conduct balance tests across baseline survey covariates in the endline sample. As Appendix Table A3 shows, a joint F-test only fails to reject the

[7] We had pre-specified that such individuals would be pooled with groups receiving the *Text*, *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 3).

[8] 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.

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 drive our main effects in Appendix A.6. 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. Next, we consider whether our sustained intervention improved participants' capacity to discern true and false information *not* covered by the fact-checks. To understand the extent to which individuals reduced their exposure to misinformation, we then examine participants' broader media consumption behaviors. Finally, in line with the fact-checks' 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 index 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 the effects between particular treatment arms, which test for our directional hypotheses noted above. Appendix Tables A4-A16 report the regression estimates underlying our figures as well as unstandardized estimates for each index component.

5.1 Consumption of fact-checks

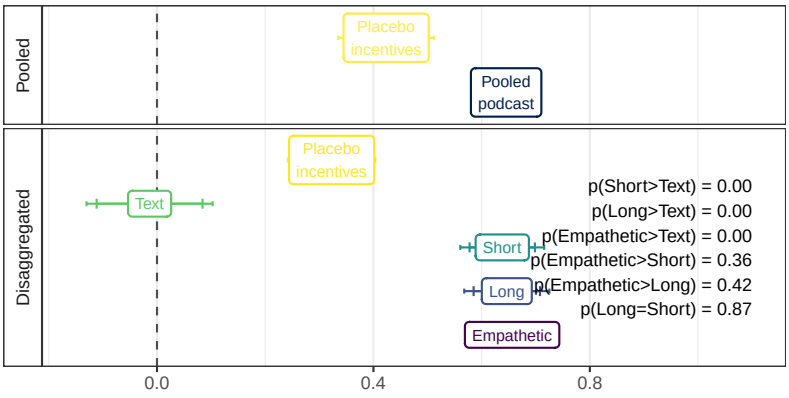
We find substantial and sustained levels of fact-check consumption in Figure 3. The upper panel of Figure 3a demonstrates that podcast listenership increased by 0.65 standard deviations ($p < 0.01$)

across pooled podcast treatment conditions ($p < 0.01$). For our most direct metric of intervention take-up, Table A4 shows that podcast-treated participants became 36 percentage points more likely to report listening to the WCW podcast relative to the control group. Only around 11% of the total number of individual webpage links sent out were clicked by study participants, although the fact-check's conclusion was always conveyed in the WhatsApp message itself.

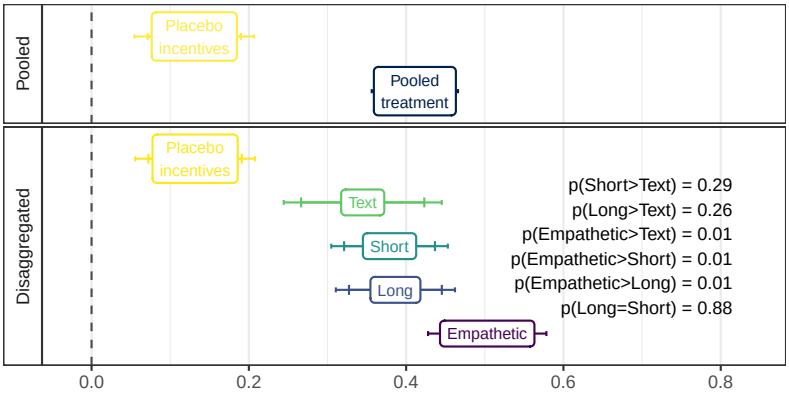
To capture the extent to which participants paid attention to the treatments, and address the concern that treated respondents over-reported their consumption of the podcast, we consider two behavioral measures of engagement. First, consistent with the debunking aspect of the intervention, Figure 3b demonstrates that the average treated respondent who received fact-check quiz incentives increased the number of questions relating to fact-check content 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 continue receiving information from Africa Check after the six months of financial incentives concluded. The results in Figure 3c show that treated respondents with incentives to consume fact-checks became 0.2 standard deviations more likely to subscribe to Africa Check's content ($p < 0.01$). Table A6 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%.

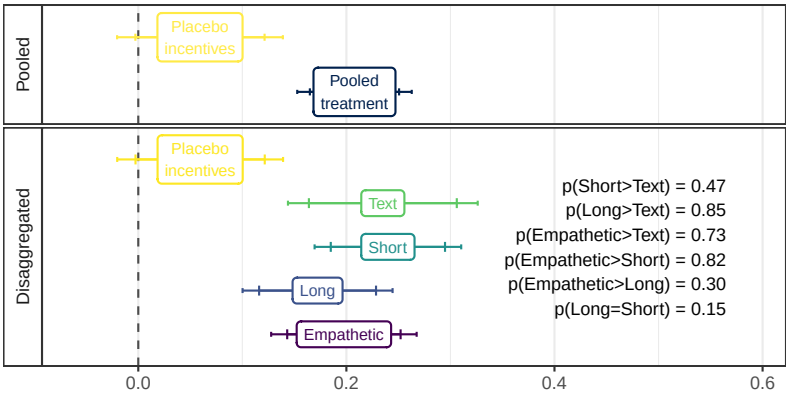
However, indicative of the challenges of generating organic demand for corrective information, the treatments combined with placebo quiz incentives resulted in significantly smaller increases in self-reported engagement, knowledge of fact-checks, and intended future take-up. Our results mirror prior findings suggesting that modest 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, although our findings suggest doing so is possible and could engender enduring engagement. Nevertheless, the limited effects on treatment take-up among participants assigned to placebo incentives leads us to henceforth focus on those treated respondents



(a) Podcast take-up



(b) Treatment knowledge



(c) Intended future take-up

Figure 3: Treatment effects on take-up

Notes: All outcomes are standardized inverse covariance-weighted indexes (see Table A1): (a): how often reports listening to podcasts and reports listening to WCW; (b) number of fact-check quiz questions answered correctly out of 6; (c) indicators for wanting future Africa Check (AC) vaccine info, AC fact-checks, AC reminders, and to subscribe to WCW. Estimated using Equation (1). Top panel of Figure 3a 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. Regression tables provided in Tables A4-A6.

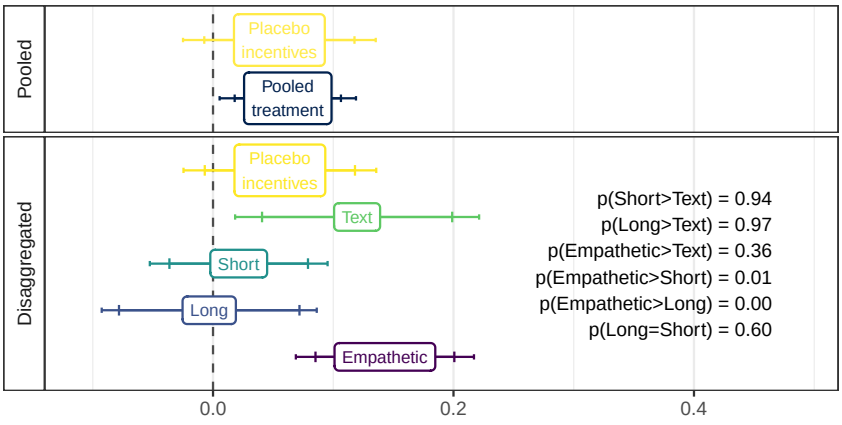
assigned to fact-check quiz incentives, who engaged far more strongly with their assigned treatments.

The lower panel within each subfigure indicates that consumption of the fact-checks was fairly uniform across incentivized fact-check treatment conditions. Any differences in effects across treatment variants are thus unlikely to reflect differential consumption rates. However, correct quiz responses were notably greater for the *Empathetic* podcast ($p < 0.01$), suggesting that empathetic content potentially increased users' attention or recall of the treatment.

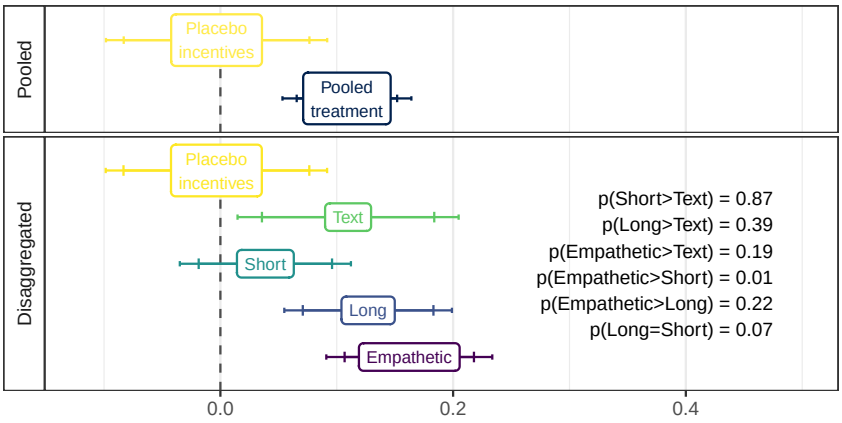
5.2 Discerning fact from fiction

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*. 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, and asked respondents to indicate how likely they believed each to be true. Figure 4a'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 were unmoved. Appendix Figures A4a and A4b further show that improved discernment is driven by respondents' greater distrust of false statements than greater trust of true statements. As the treatment variant tests in the lower panel illustrate, the pooled treatment effect is driven by the *Text* and *Empathetic* podcast conditions.

Second, we presented participants with four widespread conspiracy theories *not* investigated by Africa Check and asked respondents to indicate how likely each is to be true. The upper panel of Figure 4b indicates that any treatment with incentives to consume the fact-check quiz increased respondents' skepticism of conspiracy theories by 0.1 standard deviations, or an average of 0.12 units on a five-point scale ($p < 0.01$). Increased discernment is driven by the *Text* message and the



(a) Discernment between true and fake news stories



(b) Identification of conspiracy theories

Figure 4: Treatment effects on discernment between fake and true news and belief in conspiracy theories

Notes: All outcomes are standardized inverse covariance-weighted indexes (see Table A1): (a): level of confidence in truthful claims and lack of confidence in false claims about how COVID spreads (true), whether matriculation exam scores inflated (false), if alcohol worsens infections (true), and that most workers are immigrants (false); (b) perceived likelihood that AIDS was intentionally created, Mandela died in 1985, COVID-19 vaccines have microchips, and vaccines used to reduce population. 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. Regression tables provided in Tables A7 and A8.

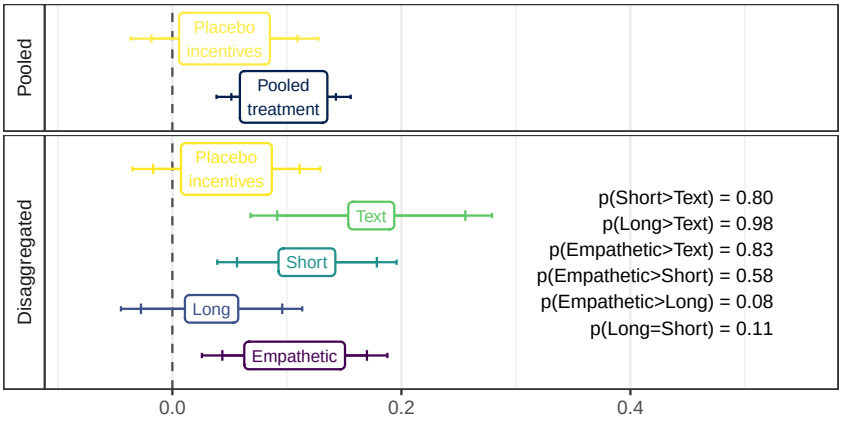
Long and *Empathetic* podcast formats ($p < 0.05$, $p < 0.05$, and $p < 0.01$), which all produced larger effects than the short podcast. Combined with participants' ability to distinguish true from false stories, sustained exposure to fact-checks reduced participants' susceptibility to fake news beyond the narrow content of the fact-checks. This suggests that sustained exposure to fact-checks can inoculate individuals against misinformation more broadly.

We next consider whether such generalized discernment 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 5a 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 verifying news. Table A9 disaggregates the index, showing this effect principally reflects increases in respondents' 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 5a shows that the *Text*, *Short*, and *Empathetic* podcast modes of delivery were notably more effective ($p < 0.01$, $p < 0.01$, and $p < 0.05$, respectively) than the *Long* podcast.

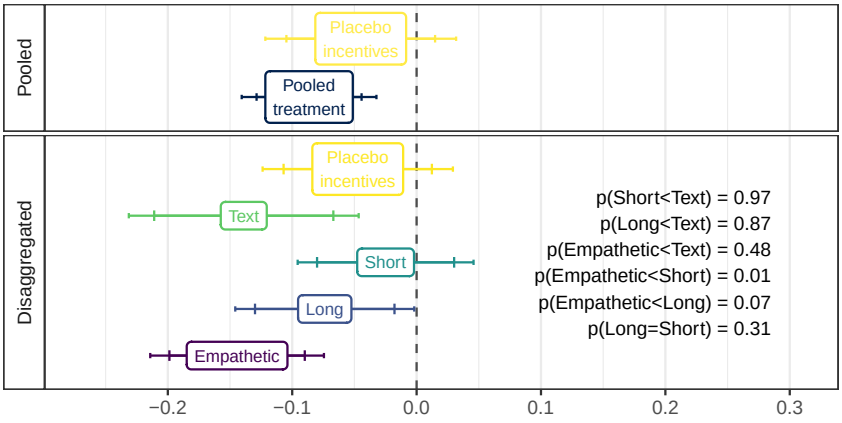
Finally, effective inoculation might also reflect greater skepticism of platforms which supply a significant share of misinformation. Aggregating respondents' assessments of truth content on and trust in social media platforms (other than WhatsApp, through which fact-checks were delivered), the upper panel of Figure 5b shows that the treatments incentivizing participants to consume fact-checks reduced trust in social media platforms by 0.09 standard deviations ($p < 0.01$).[9] The effect is driven by each component of the index; for example, treatment reduced the share of respondents believing that social media information sources are credible by 17% ($p < 0.01$). In line with our previous results, the lower panel shows the largest effects for the *Text* and *Empathetic* podcast delivery formats ($p < 0.01$ and $p < 0.05$, respectively).

Together, these results indicate that sustained access to fact-checks—especially when expressed

[9] shows that trust in information from close ties, including information sent from WhatsApp, modestly decreases.



(a) Knowledge of verification methods



(b) Trust in social media (besides WhatsApp)

Figure 5: Treatment effects on news verification knowledge and attitudes towards social media (besides WhatsApp)

Notes: All outcomes are standardized inverse covariance-weighted indexes (see Table A1): (a): separate indicators for correctly identifying 2 ways to avoid being misled, correctly identifying 7 methods to verify information, and correctly identifying 4 strategies fact-checkers use to verify information; (b) believes information from social media likely to be true, trusts information on social media, and thinks information on social media is most trustworthy. 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. Regression tables provided in Tables A9 and A11.

in a simple text form or conversationally with empathy—increased respondents' capacity to discern misinformation, verify suspicious information, and generally doubt content on social media *upon exposure*. Further, the heterogeneity across treatment groups, which all received fact-check quiz incentives and experienced similar effects on fact-check consumption, suggests that the main treatments—rather than differential consumption or the quizzes themselves—were responsible for the observed pattern of effects. In Appendix Figures A5a and A5b, we show no effects on participants' perception that misinformation is an important problem or that verification is important, nor any changes in their perception about the ease of fact-checking. This suggests that treated individuals became more *capable* of discerning fact from fiction, but not more motivated to do so.

5.3 Information consumption, verification, and sharing

Moving beyond efforts to inoculate participants *upon exposure* to misinformation, we assess 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 social media consumption (besides WhatsApp). Across the pooled and disaggregated estimations, Figure 6a reports substantively small and consistently statistically insignificant treatment effects. Furthermore, Appendix Figure A10 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 overall. Given that social media 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. Specifically, Figure 6b shows that we fail to detect an increase in how often respondents reported trying to actively verify information they received through social media. Appendix Figure A6 indicates that, while verification 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

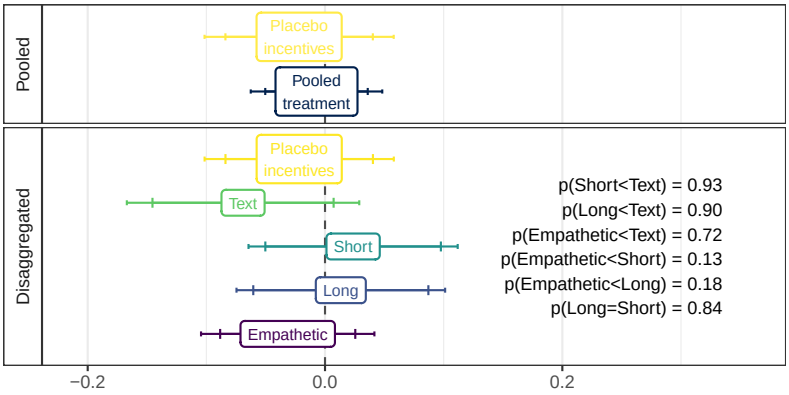
by text ($p < 0.01$). Along with the increase in verification knowledge observed in Figure 5a, these negligible treatment effects on respondents' verification behavior imply that limited *capacity* to verify news stories might not be the only driver of citizens' limited *efforts* to do so.

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 lower propensity to share suspected misinformation. The lower panel of Figure 6c shows that these participants became around 0.1 standard deviations less likely to report sharing information received via social media ($p < 0.05$), or a 0.1 unit reduction on our five-point scale capturing the frequency with which respondents share 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.

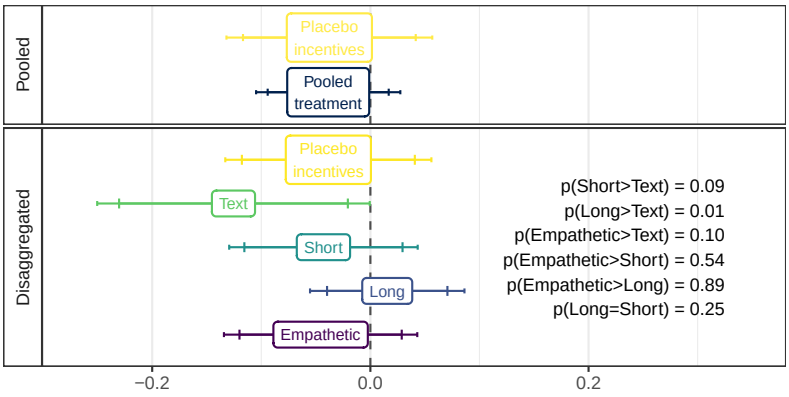
5.4 Attitudes and behaviors relating to COVID-19 and government

We finally turn to 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. Health-related misinformation, by emphasizing false cures or casting doubt on the severity of the pandemic, 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.

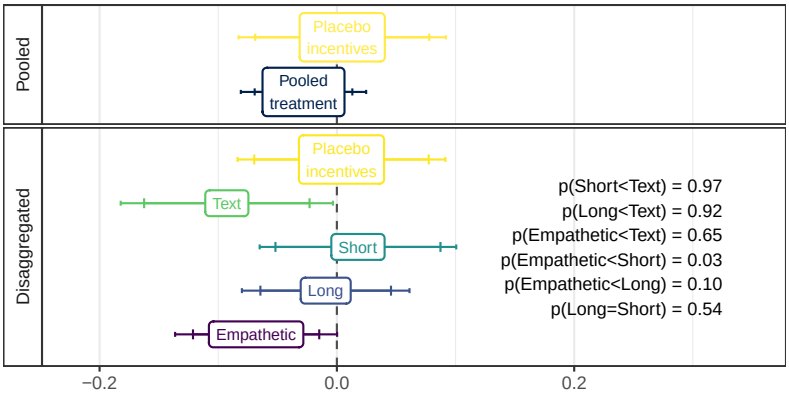
For our final set of outcomes, we therefore evaluate effects on indexes of attitudes and self-reported behaviors relating to COVID-19 and politics to assess whether the treatment mitigated the broader negative downstream consequences typically associated with exposure to misinformation. Since these outcomes are not connected directly to the fact-checks, this enables us to test whether



(a) Social media consumption



(b) Active verification



(c) Sharing

Figure 6: Treatment effects on information consumption, verification and sharing

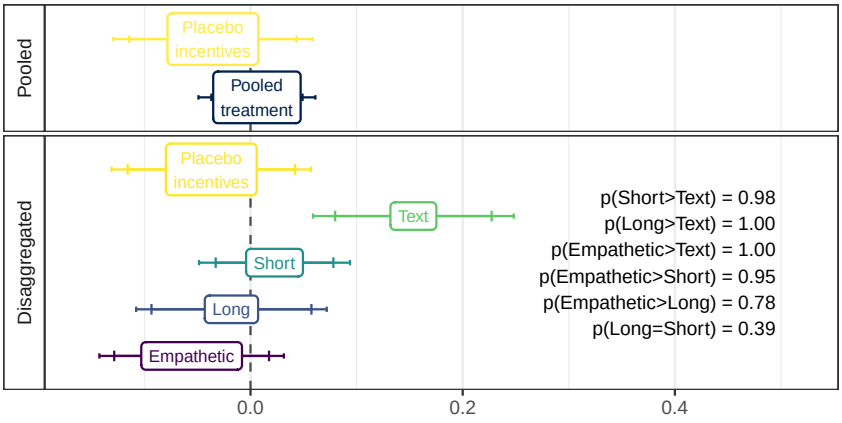
Notes: All outcomes are standardized (see Table A1): (a): how often gets news from non-WhatsApp social media; (b) how often actively verifies information; (c) how often shares stories on social media. Estimated using Equation (1); p-values are from pre-registered tests of differences between treatment variants indicated in bottom panels, while the interior and exterior bars represent 90% and 95% confidence intervals. Regression tables provided in Tables A12-A14.

sustained efforts to combat salient misinformation influenced participants' perspectives on public health and politics more broadly.

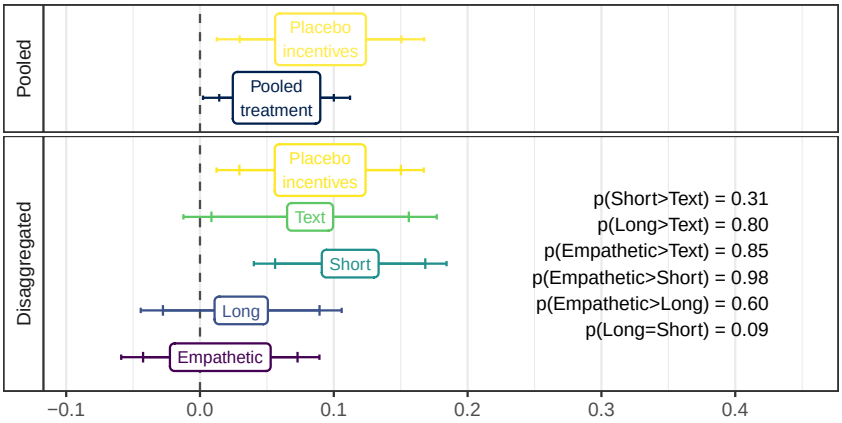
Overall, we detect modest effects after six months of exposure to fact-checks on such beliefs and behaviors. Figure 7a generally reports no treatment effect on COVID-19 beliefs and preventative behavior for the three podcast treatments with fact-check quiz incentives. However, we find that fact-checks delivered by short and simple text messages increased an index of health-conscious outcomes associated with COVID-19 by 0.14 standard deviations ($p < 0.01$). Particularly encouragingly, Appendix Table A15 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.

Figure 7b reports an increase in favorable views toward the government—measured in terms of government performance appraisals, trust in government, and intentions to vote for their region's incumbent party—across treatment conditions. The pooled treatment effect of 0.06 standard deviations ($p < 0.1$) is largely driven by the *Text* format—although the coefficient is not quite statistically significant ($p = 0.11$)—and *Short* podcast format ($p < 0.05$). Appendix Table A16 shows that these effects are primarily driven by significant increases in the extent to which respondents trusted information from politicians and the government.

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 highly polarizing issues.



(a) COVID-19 beliefs and preventative behavior



(b) Views and attitudes about the government

Figure 7: Treatment effects on COVID-19 beliefs and preventative, and views and attitudes about the government

Notes: All outcomes are standardized inverse covariance-weighted indexes (see Table A1): (a): how many days stayed home in the last week, how many days visited other people indoors in the last week (reversed), how many days wore a mask in the last week, believes COVID-19 is a hoax (reversed), thinks lockdowns are necessary, trusts vaccines, and would get vaccinated; (b) central government performance appraisal, believes government handled COVID-19 well, faith in truth of information from politicians, trusts government/politicians most for information, level of trust in information from politicians, and would vote for regional incumbent party. 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. Regression tables provided in Tables A15 and A16.

6 Conclusion

Misinformation on social media, due to its potentially negative consequences for political and health-related behaviors, is a growing concern around the globe. Misinformation has been linked to eroding trust in democratic institutions and political polarization; in South Africa, recent widespread misinformation has exacerbated racial tensions, fueled conflicts by stoking anger and fear, and substantially increased vaccine hesitancy.

While recent studies have advanced our understanding of how to mitigate the consumption of, and susceptibility to, misinformation online, most struggle to explain how sustained changes in beliefs and behaviors can be achieved outside controlled research environments. 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 organic *consumption* of corrective information, and how to induce *internalization* of the lessons imparted by fact-check content. The comparatively naturalistic setting of our intervention, along with its length, allowed us to examine whether fact-checking can play both *debunking* and *prebunking* roles by both correcting existing misinformation and warning participants about future misinformation. Our partnership with an existing fact-checking organization, Africa Check, 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 from the very beginning, an initial push towards consumption may subsequently activate latent demand. Policymakers should therefore target increasing the dissemination of fact-checked information in tandem with effort to increase the public's appetite for such information. Our findings suggest that getting citizens over the initial hump could yield significant improvements in media literacy.

Second, while treated participants did not report altering behaviors that limit exposure to mis-

information or active verification efforts, the robust effects on participants' capacity to discern fact from fiction—and willingness to act on this by not sharing unverified online content—indicate that the intervention contributed to participants' inoculation against misinformation *upon exposure*. 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. Different types of interventions, perhaps addressing access or production incentives in the broader media environment or consumption patterns within social networks, may be required to alter broader social media consumption patterns. In contrast, efforts to reduce exposure to misinformation could be more effectively targeted at its production than its consumption.

Third, not all treatment arms performed equally: the simple text-only treatment and empathetic podcast treatments were consistently the most effective delivery mechanisms for *internalization*. 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 modestly 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 key concerns of the social media age, reduced state capacity and declining faith in government.

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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 A1a 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 that randomly appeared throughout the baseline survey. These 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 the randomization process; consequently, dropped respondents are not correlated with treatment assignment. Their phone 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 2 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 Financial incentives

We administered small financial incentives (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 much smaller set

of outcomes, respondents were provided R30 (1.90 USD) 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 A3a documents the share of participants completing each quiz during a given batch's study period, and the share of those completing each quiz who answered a majority of the questions correctly.

A.4 Pre-specified hypotheses

We preregistered the following hypotheses for pooled treatment effects, which correspond to the outcomes presented in the main text and in the top panel of each subfigure:

- **Treatment take-up:** Access to treatment increases both exposure to, and knowledge about, information covered by the treatment deliveries (H1).
- **Discerning fact from fiction:** Fact-check treatments would increase participants' capacity to identify, and express skepticism on the basis of, characteristics of misinformation (H6); reduce trust in social media information (H3); and increase the perceived extent of misinformation on social media (H2).
- **Information consumption, verification, and sharing:** Fact-check treatments would decrease information consumption and sharing from social media (H4), increase awareness and attention paid to information on social media (H5), and increase active fact-checking behavior (H7).
- **COVID-19 and political attitudes and behavior:** Fact-check treatments would increase participants' knowledge and beliefs in the severity of COVID-19 and their willingness to take preventative measures (H9) and improve participants' perceptions of government performance (H8).

The corresponding hypothesis from our pre-analysis plan is noted in parentheses. Overall, we find evidence consistent with H1, H3, H4 (with regard to sharing), H6, H8, and H9. In addition to the pooled effects, we hypothesized that treatment would be more effective for incentivized ("fact-check quizzes") rather than unincentivized ("placebo quizzes") treatments, which we find strong support for. Between treatment arms, we hypothesized that (1) effects would be greater for podcasts rather than text messages, and (2) *Empathetic* podcasts rather than *Long* podcasts, but (3) we made no directional predictions for differences between the *Long* and *Short* podcasts. We find evidence consistent with (2) but not (1), since the text treatment was ultimately highly effective. Finally, we preregistered an expectation of greater treatment effects for treatments delivered using a social prime that highlighted the importance of fact-checking for social good, which we also found to be the case (see below).

A.5 Outcome measurement

All our main outcomes are inverse covariance weighted (ICW) indexes (see Anderson 2008). Each such outcome aggregates individual survey items in line with the families outlined in our pre-analysis plan, 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 A1.

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. However, we note below some deviations designed to focus attention on theoretically-relevant outcomes.

First, for exposure to the intervention, we examine podcast take-up and knowledge of the content of the podcast separately to distinguish self-reported attention from internalization; we cut an index item about the frequency with which participants report being alerted to fake news on social media because it was originally designed to test a distinct mechanism proposed in the literature (Pennycook et al. 2021), but we found limited support for it (see Figure A7). We further added future take-up as an additional indicator of treatment take-up once the small financial incentives to participate in the study had been removed.

Second, for trust in social media, the index focuses on Facebook, Instagram, and Twitter. We exclude WhatsApp because the fact-checking intervention was delivered via WhatsApp and hence results are difficult to interpret. Figure A9b shows that trust in information from close ties, including information sent by these ties from WhatsApp, modestly decreases. Third, for consumption of social media, we exclude WhatsApp for the same reason. We also examine the consumption and sharing of information separately to examine effects on both important outcomes.

Fourth, our discernment outcomes relating to conspiracy theories were not pre-registered, but provide a valuable check on citizen evaluations of claims that could be the subject of misinformation.

Fifth, 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 6b and Table A13). 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 5a and Table A9). We exclude the variable on whether they 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 A8).

Finally, for attitudes toward the government, 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 7b); (ii) we exclude two questions eliciting perceptions of government capacity (see Figure A11 for results) and two questions on populism-related beliefs (see Figure A12 for results), on the basis that these questions were worded to capture beliefs about how government *ought* to behave rather than concrete government appraisals.

A.6 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. This interpretation of our findings is bolstered by results from questions that test participants' capacity to discern true from false news and their ability to identify conspiracy theories. 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.

Second, 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.

Third, 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 A13 show that treated participants were indeed more likely to submit fact-check requests. Importantly, the incentivized *Text* treatment participants were the most likely to send in fact-checking requests in comparison to all other treatment arms ($p < 0.01$). The particular effectiveness of the *Text* 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.

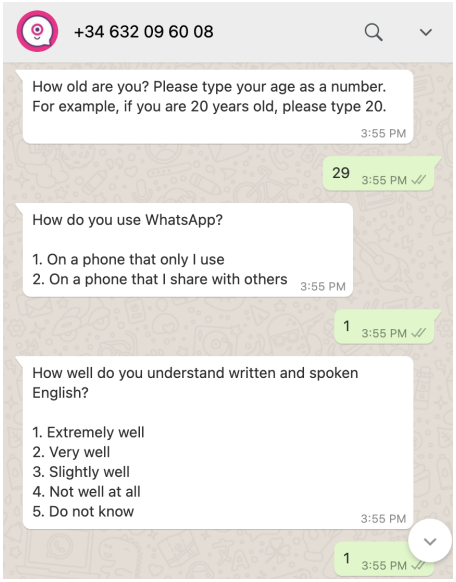
B.4 Examples of additional prime in delivery message

- "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

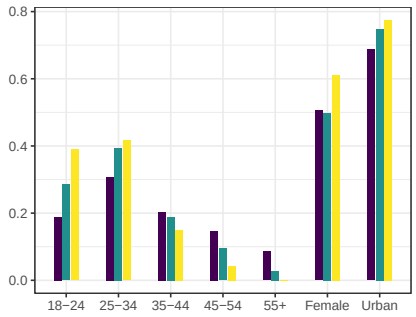
C.1 Figures

(a) Recruitment Facebook ad

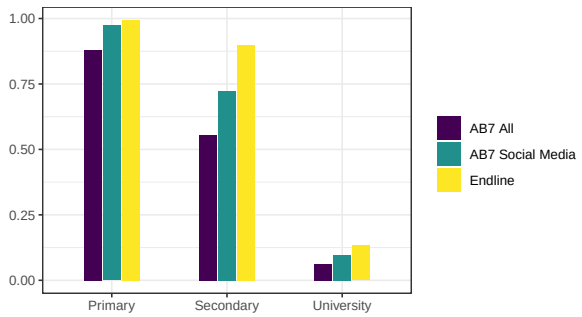


(b) Survey through WhatsApp chatbot

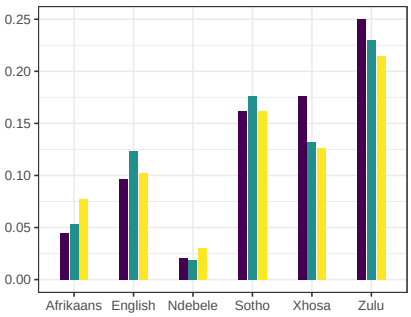
Figure A1: Recruitment and surveying



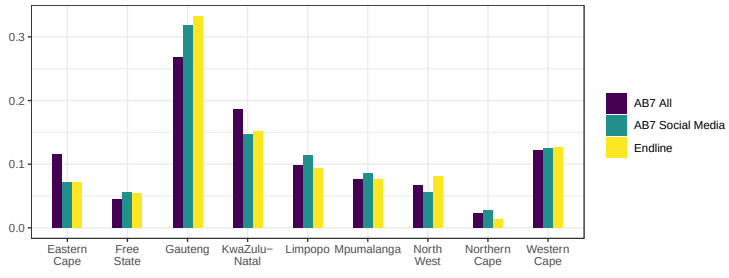
(a) Age group, gender, urbanity



(b) Education

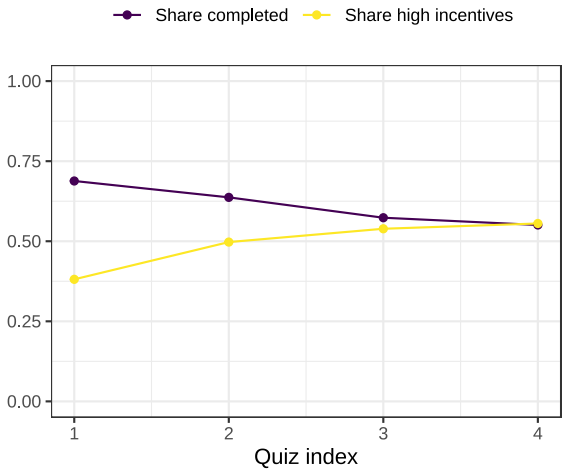


(c) Ethnicity

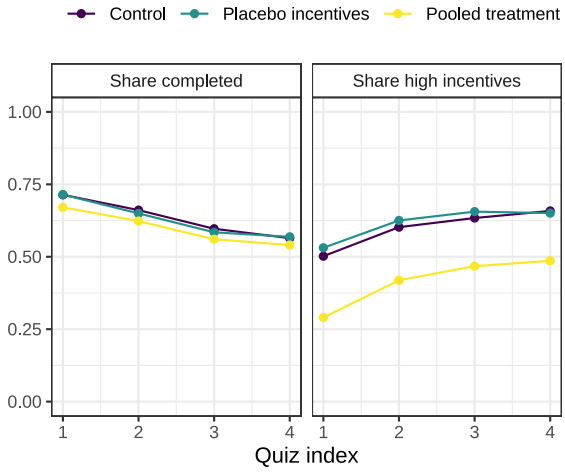


(d) Province

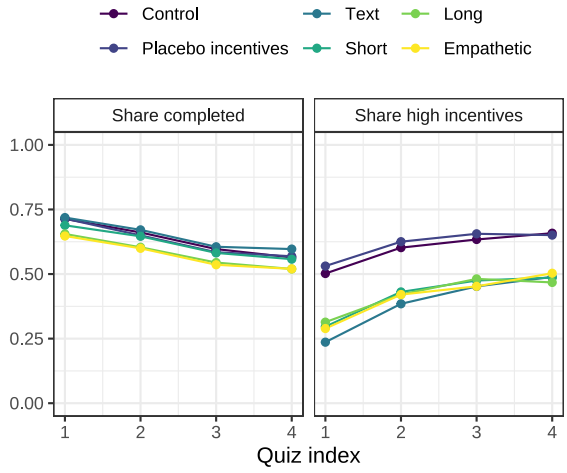
Figure A2: Comparison of endline sample with Afrobarometer round 7 (2018)



(a) Quiz engagement and incentive payments (overall)



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



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

Figure A3: Quiz engagement over study

Notes: Figure plots average participation, and average share of participants answering more than 50% of questions correctly, through study quizzes (fact-check or placebo) between baseline and endline.

C.2 Outcome variables

Table A1: Outcome variables

Outcome variable	Variable definitions	Mean	SD	Range
Treatment take-up				
Podcast take-up (Fig. 3a)	How often listen to podcasts	3.24	1.25	[1,5]
	Included "What's Crap on WhatsApp" in selection of podcasts they listened to	0.41	0.49	[0,1]
Treatment knowledge (Fig. 3b)	Number of correct responses from 6 questions on fact-checked content	2.75	1.56	[0,6]
Future take-up (Fig. 3c)	Want vaccine info from Africa Check	0.72	0.45	[0,1]
	Want Africa Check's fact checking content	0.85	0.36	[0,1]
	Want Africa Check reminders to pay attention to misinformation	0.71	0.45	[0,1]
	Stay subscribed (or start subscribing) to "What's Crap on WhatsApp"	0.83	0.37	[0,1]
Discerning fact from fiction				
Discernment between T/F news (Fig. 4a)	How COVID-19 spreads (true)	4.45	0.91	[1,5]
	Matriculation scores to be inflated (false) (-)	3.11	1.34	[1,5]
	Alcohol decreases ability to fight infections (true)	3.51	1.27	[1,5]
	Almost 100% of workers in SA are foreign (false) (-)	2.89	1.31	[1,5]
Identification of conspiracy theories (Fig. 4b)	Not at all likely to very likely: AIDs intentionally created	3.69	1.37	[1,5]
	Not at all likely to very likely: Nelson Mandela died in 1985	3.82	1.38	[1,5]
	Not at all likely to very likely: COVID-19 vaccines used to implant chips	3.70	1.34	[1,5]
	Not at all likely to very likely: Vaccines used to reduce world's population	3.72	1.34	[1,5]
Verification knowledge and trust				
Knowledge of verification methods (Fig. 5a)	To avoid being misled: Seek info from reputable org	0.36	0.48	[0,1]
	To avoid being misled: Ask other people to avoid being misled (-)	0.13	0.34	[0,1]
	To verify: Ask people I know in person (-)	0.71	0.46	[0,1]
	To verify: Ask people I know through WhatsApp (-)	0.82	0.39	[0,1]
	To verify: Ask people I don't know well on WhatsApp group (-)	0.91	0.29	[0,1]
	To verify: Go to fact-checker	0.49	0.50	[0,1]
	To verify: Submit a fact-checker request	0.21	0.40	[0,1]
	To verify: Ask people I know by posting on social media (-)	0.87	0.33	[0,1]
	To verify: Use the internet to fact-check	0.46	0.50	[0,1]
	Verify strategies: Ask experts	0.42	0.49	[0,1]
	Verify strategies: Check source popularity (-)	0.63	0.48	[0,1]
	Verify strategies: Use reverse image searches	0.16	0.36	[0,1]
	Verify strategies: Talk to others (-)	0.82	0.38	[0,1]
Trust in social media besides WhatsApp (Fig. 5b)	Likely to be true: Information from other social media (FB, Twitter, Instagram)	2.83	0.75	[1,5]
	Trust: Information received from other social media (FB, Twitter, Instagram)	2.88	1.04	[1,5]
	Trust the most for information: Other social media (FB, Twitter, Instagram)	0.16	0.37	[0,1]
Info consumption, verification, and sharing				
Online and social media consumption (Fig. 6a)	Go to source for news: other social media (Facebook, Twitter, Instagram)	0.42	0.49	[0,1]
Verification (Fig. 6b)	How often verify information seen on social media	3.83	1.10	[1,5]
Sharing (Fig. 6c)	How often share social media info shared by others	2.83	1.11	[1,5]
COVID-19 and political attitudes				
COVID-19 beliefs and behaviors (Fig. 7a)	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]
	Strongly disagree to strongly agree: COVID-19 is a fake disease (-)	4.36	1.11	[1,5]
	Definitely to definitely not: COVID-19 lockdown justified (-)	3.21	0.92	[1,4]
	Strongly disagree to strongly agree: Would take available vaccine	3.49	1.54	[1,5]
	Strongly distrust to strongly trust: COVID-19 vaccines in South Africa are safe	3.89	1.37	[1,5]
Views and attitudes about government (Fig. 7b)	Trust information from politicians and gov officials	2.89	1.20	[1,5]
	Most trustworthy sources: Selected "Government officials"	0.30	0.46	[0,1]
	Most trustworthy sources: Selected "Politicians and other public figures"	0.13	0.34	[0,1]
	How likely information from politicians and gov officials are true	3.02	0.95	[1,5]
	Vote for regional incumbent (vote tomorrow in parl elections: ANC, DA, EFF, IFP, VF+)	0.23	0.42	[0,1]
	Very badly to very well: National government's general performance	2.38	1.20	[1,5]
	Very badly to very well: National government handling COVID-19 crisis	3.09	1.22	[1,5]

Descriptive statistics for all variables used in figures in main paper.
Variables indicated with (-) indicate that variable has been reversed for use in index before providing summary statistics.

C.3 Balance and attrition

Table A2: 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

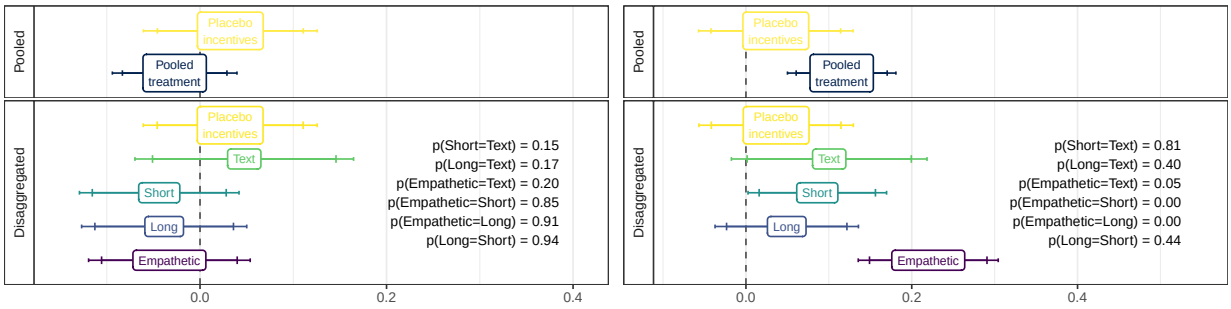
Notes: See Table A1 for variable definitions. All specifications are estimated using OLS, and adjust for randomization block fixed effects; even-indexed columns further include LASSO-selected controls. Heteroskedasticity-robust standard errors in parentheses, while p-values (adjusted for pre-registered direction when relevant) are in square brackets.

Table A3: 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 A1 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).

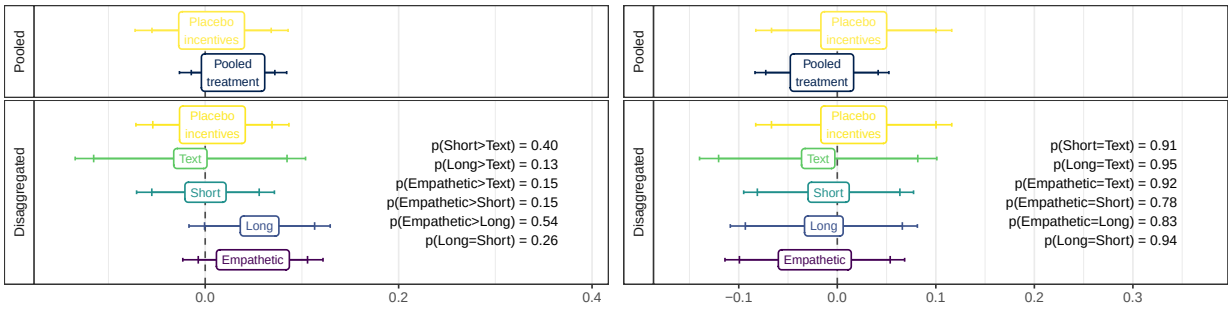
D Figures referenced in main text



(a) Correct discernment of true news stories (b) Correct discernment of false news stories

Figure A4: Treatment effects on discernment between fake and true news

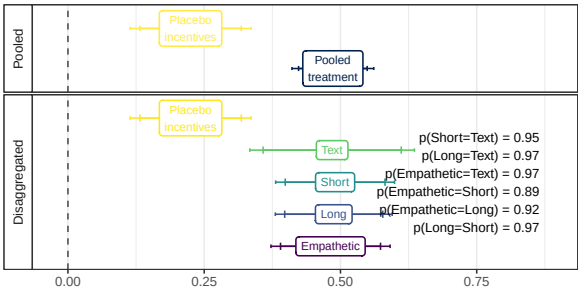
Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): level of confidence in truthful claims about how COVID spreads (true) and if alcohol exacerbates infections (true); (b) lack of confidence in false claims about inflation of matriculation exam scores (false) and most workers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



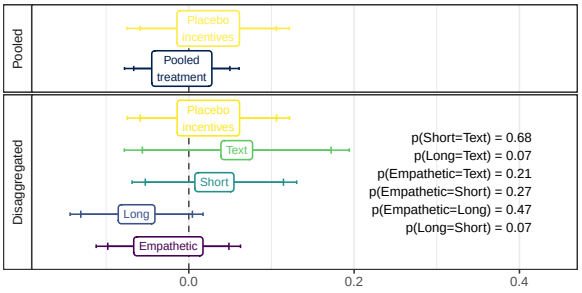
(a) Verification is important (b) Fact-checking is challenging

Figure A5: Treatment effects on verification and ease of fact-checking

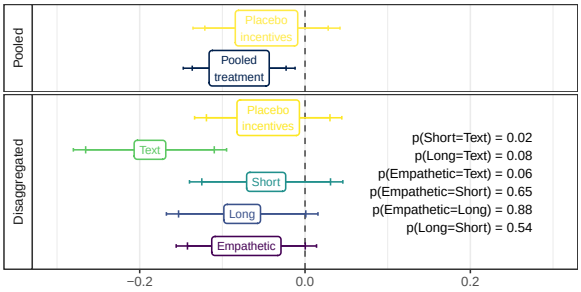
Notes: All outcomes are standardized inverse covariance-weighted indexes: (a): thinks it is important to verify information; (b): challenging to verify information due to knowledge, irrelevant fact-checks, distrust fact-checkers, too expensive, overwhelming information, takes too long. Estimated using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



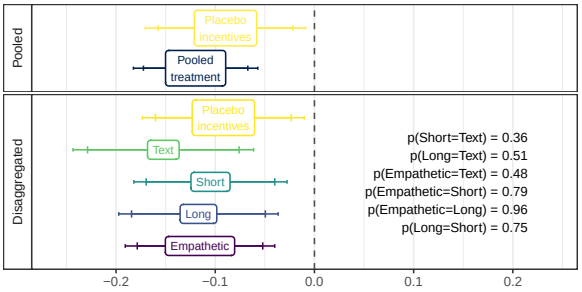
(a) Verify through Africa Check



(b) Verify through other fact-checkers



(c) Verify through online and social media



(d) Verify through traditional media

Figure A6: 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. 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

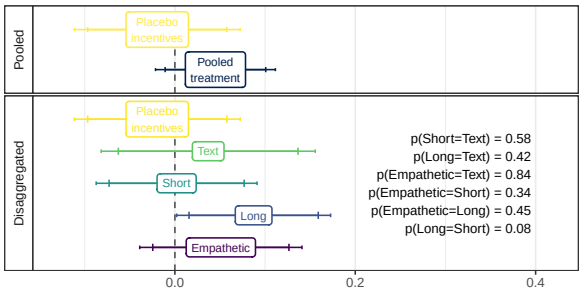


Figure A7: Being alerted about fake news

Notes: Outcome is standardized: How often participant is alerted about fake news. 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.

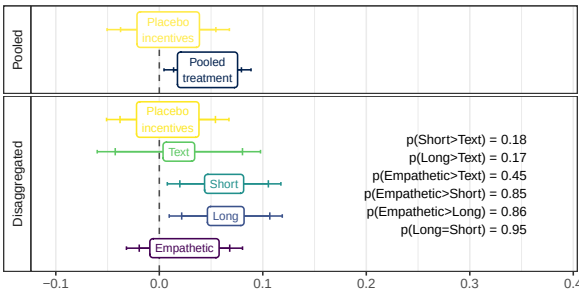
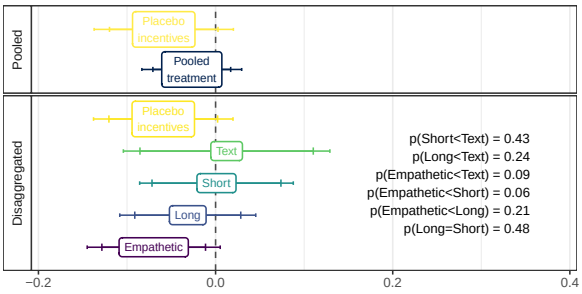
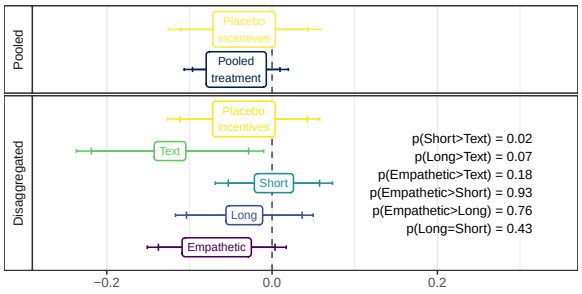


Figure A8: Alerting others about fake news

Notes: Outcome is standardized: How often participant reports alerting others about misinformation. 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.



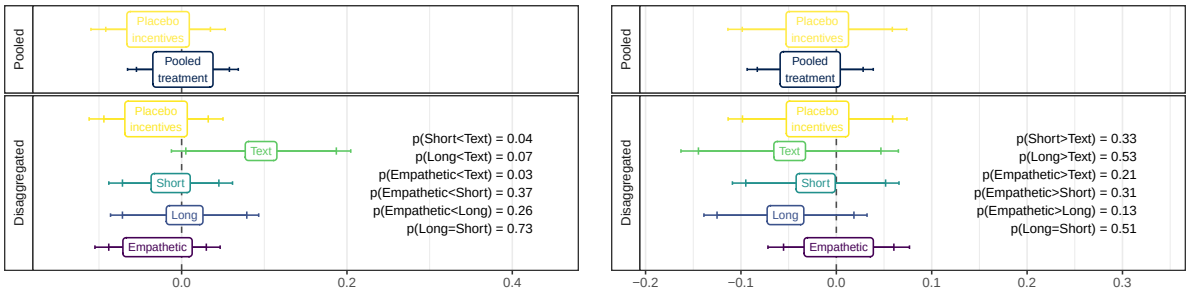
(a) Trust in traditional media



(b) Trust information sent by close ties

Figure A9: 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. 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 A10: 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. 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.

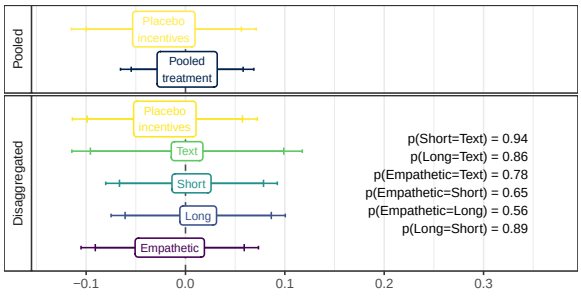


Figure A11: Perceptions of government capacity

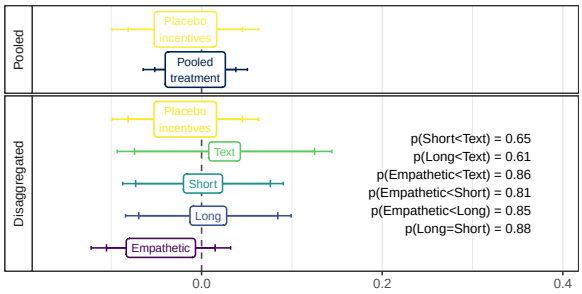


Figure A12: Populist attitudes

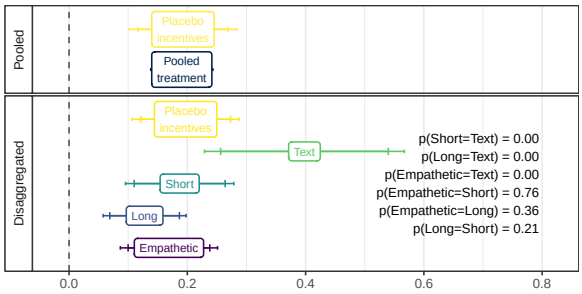


Figure A13: Fact-check requests

Notes: **Fig A11:** Outcome is standardized inverse covariance-weighted index comprising perception of government capacity to provide roads; perception of government capacity to supply electricity. **Fig A12:** Outcome is standardized inverse covariance-weighted index comprising perception of policies benefit elites; perception that ordinary people have no influence over policy. **Fig A13:** Outcome is a standardized indicator for participant submitting a fact-check request to Africa Check. 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 A4: 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 A1 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 3a.

Table A5: 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 A1 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 3b.

Table A6: 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 A1 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 3c.

Table A7: Discernment

	ICW: Discernment		Alcohol and COVID (true)		Foreign restaurant workers (false)		How COVID spreads (true)		Matric marks (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 A1 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 A8: Identification 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 A1 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 A9: 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 A1 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 A10: 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 A1 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 A11: 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 A1 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 A12: 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 A1 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 A13: 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 A1 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 A14: 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 A1 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 A15: 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 A1 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 A16: Views and attitudes about the government

	ICW: Government attitudes		General govt performance		Govt handled COVID well		How true: Info from politicians		Trust most for info: Govt		Trust most for info: Politicians		Trust: Info from politicians		Vote: Local incumbent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
A. Pooled estimation																
Placebo incentives	0.097	0.090	0.079	0.074	0.026	0.022	-0.030	-0.034	0.009	0.009	0.019	0.018	-0.013	-0.024	0.060	0.057
	(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)
	[0.027]	[0.028]	[0.089]	[0.096]	[0.334]	[0.353]	[0.525]	[0.466]	[0.343]	[0.339]	[0.132]	[0.139]	[0.827]	[0.671]	[0.002]	[0.003]
Pooled treatment	0.061	0.058	0.051	0.042	0.027	0.029	-0.033	-0.032	0.021	0.021	0.020	0.020	-0.035	-0.035	0.020	0.020
	(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)
	[0.039]	[0.041]	[0.109]	[0.150]	[0.264]	[0.244]	[0.323]	[0.326]	[0.090]	[0.096]	[0.046]	[0.040]	[0.396]	[0.378]	[0.081]	[0.081]
B. Disaggregated estimation																
Placebo incentives	0.097	0.090	0.079	0.072	0.027	0.024	-0.030	-0.034	0.009	0.009	0.019	0.018	-0.013	-0.024	0.059	0.056
	(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)
	[0.027]	[0.028]	[0.089]	[0.103]	[0.331]	[0.344]	[0.528]	[0.459]	[0.342]	[0.341]	[0.131]	[0.139]	[0.827]	[0.669]	[0.003]	[0.004]
Text information	0.075	0.083	0.033	0.032	-0.062	-0.048	0.037	0.050	0.047	0.049	0.000	0.004	-0.009	0.006	0.055	0.059
	(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)
	[0.111]	[0.075]	[0.316]	[0.319]	[0.405]	[0.509]	[0.258]	[0.187]	[0.057]	[0.046]	[0.492]	[0.424]	[0.910]	[0.468]	[0.017]	[0.011]
Short podcast	0.121	0.114	0.095	0.085	0.118	0.112	0.015	0.016	0.032	0.028	0.026	0.027	0.017	0.013	0.032	0.029
	(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)
	[0.004]	[0.005]	[0.043]	[0.055]	[0.017]	[0.021]	[0.361]	[0.349]	[0.067]	[0.093]	[0.046]	[0.039]	[0.377]	[0.399]	[0.050]	[0.064]
Long podcast	0.040	0.030	0.032	0.014	-0.013	-0.013	-0.098	-0.108	0.000	0.001	0.020	0.019	-0.062	-0.073	0.034	0.032
	(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)
	[0.203]	[0.257]	[0.283]	[0.397]	[0.822]	[0.822]	[0.028]	[0.014]	[0.993]	[0.484]	[0.103]	[0.117]	[0.270]	[0.183]	[0.041]	[0.049]
Empathetic podcast	0.015	0.017	0.034	0.031	0.013	0.019	-0.049	-0.044	0.020	0.020	0.021	0.022	-0.075	-0.066	-0.023	-0.020
	(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)
	[0.376]	[0.354]	[0.269]	[0.278]	[0.407]	[0.366]	[0.276]	[0.316]	[0.169]	[0.173]	[0.085]	[0.078]	[0.173]	[0.213]	[0.219]	[0.276]
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
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
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
Observations	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543	4543

Notes: See Table A1 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.