

Partisan Media and Engagement: A Field Experiment in a Newly Liberalized System

Jeffrey Conroy-Krutz
Assistant Professor
Department of Political Science,
Michigan State University
368 Farm Lane, S303
East Lansing, MI 48824
517-353-8486
conroyk6@msu.edu

Devra C. Moehler
Assistant Professor
Annenberg School for Communication
University of Pennsylvania
3620 Walnut Street
Philadelphia, PA 19104
215-512-2696
dmoehler@asc.upenn.edu

Abstract:

How does media exposure affect political engagement in newly liberalized systems? Some celebrate newly vibrant and diverse media, believing that they mobilize citizens. Others worry that these outlets, which are often partisan, dampen engagement. We theorize that exposure to political programming engenders interest in politics irrespective of program bias, but that interest does not necessarily beget action. Partisan media affect participation only when altering attitude strength, and thus motivations. To evaluate media effects on interest and participation, we conducted a field experiment in Ghana, in which subjects in *tro-tros* (commuter vans) were randomly exposed to different types of live talk radio. We find that partisan and non-partisan media increased political interest, but not participation. Instead, exposure to alternate perspectives on cross-cutting media (i.e., those biased against subjects' partisan preferences) heightened ambivalence and dampened participation, measured as signing an SMS petition to parties. Partisan media simultaneously increased interest and decreased participation.

Acknowledgements:

The authors contributed equally to this project. Data collection was carried out by DAK Consult in Ghana, and we thank all interviewers, team leaders, translators, and data management staff for their hard work, with special thanks to our superb project manager, Ruth Ahinei Essuman. We also thank Ebenezer Ansah for excellent research assistance in Ghana, and Andrew Daniller and Eleanor Marchant for research assistance in the United States. We would like to thank Susanna Dilliplane, John McCauley, Gwyneth McClendon, Dan Young, and members of Experiments in Governance and Politics for their helpful comments. We gratefully acknowledge funding provided by the Annenberg School for Communication, the Annenberg Public Policy Center, the Annenberg Center for Global Communication Studies, and BBC Media Action.

How does media exposure affect political engagement in settings that have recently undergone media liberalization? New outlets emerge after the easing of legal restrictions, expanding and enlivening discourse. However, many newly established outlets are owned by politicians and affiliated businesspeople who use them to espouse partisan views. Some observers celebrate opinionated media on the belief that they mobilize political involvement. However, others worry that partisan media foster apathy and dampen participation. Given that new democratic institutions are often fragile, such effects can be very consequential. Either scenario is plausible, and extant literature does not provide theory or evidence to adjudicate between them. We address this by examining the effects of partisan media on two different dimensions of engagement—psychological and behavioral—in a post-liberalization setting.

We theorize that media exposure will engender psychological engagement irrespective of program bias. Given that open and pluralistic discussion had been blocked for so long, citizens will likely find political discourse in the media to be important and exciting. Crucially, however, increased interest will not necessarily lead to action; individuals also need to be properly motivated to incur costs of participation. Motivation is particularly important in post-liberalization settings, because individuals there are often less able to bear the costs of participation due to poverty and inexperience. We posit that media will affect participation only when they alter attitude strength, which is a significant determinant of motivation. Given that different types of messages—like-minded, cross-cutting, and neutral—can have different effects on attitude strength, the influence of media on participation likely varies depending on content.

To test the effects of media on psychological and behavioral engagement, we conducted a field experiment during the 2012 electoral campaign in Ghana, a country that recently experienced media liberalization. Our subjects were passengers traveling in *tro-tros*, which are

minibuses that provide the most popular form of transport in Ghana. Under normal circumstances, passengers are exposed to radio programs selected by the drivers; under our design, drivers played randomly assigned programs. Three treatments involved live political talk programming—from a pro-government, pro-opposition, or non-partisan station—while other *tro-tros* were assigned to a no-radio control. After their commute, subjects were interviewed about their interest in politics. In addition, they were subsequently contacted via SMS and invited to sign a petition to political parties. We use this component to measure political participation.

The results provide support for our theory of how partisan media affect engagement. First, both partisan and non-partisan media led to greater interest in politics. However, this was not accompanied by increased behavioral engagement. In fact, exposure to media biased against the subject's partisan preferences reduced participation—subjects in the cross-cutting condition had response rates 44% lower than those in the control—while like-minded and neutral media had no effect. These results are consistent with other research demonstrating that cross-cutting media moderate partisan attitudes in environments where partisan-motivated reasoning is low ([forthcoming by authors]). In short, partisan media simultaneously stimulated interest and discouraged action.

Our experiment also has methodological implications. Common approaches might mistakenly lead to predictions that partisan media increase participation. Observational studies risk confusing selective exposure for media effects, while laboratory experiments may yield treatment effects different from those that occur in the real world (Jerit *et al.* 2013). In contrast, subjects in our field experiment were exposed to live broadcasts in a natural setting. We used radio programs that are commonly played on *tro-tros*, and the nature of the *tro-tro* system means that drivers and riders were likely strangers to each other. Subjects were not alerted to the

treatment until they were debriefed after data had been collected. The design has high ecological validity, while still maintaining the inferential benefits of random assignment.

Media and Engagement

Democratic development requires politically active citizens. At least two preconditions must be met for participation to grow: individuals must have sufficient interest in politics, and they must be motivated to act. Mass media can affect interest and motivation and, thus, participation. However, most scholars of post-liberalization politics have overlooked the role that media might play, instead focusing on the mobilizing effects of selective incentives (De La O 2013; Gans-Morse *et al.* 2014) and identity-based appeals (Horowitz 1985; Wilkinson 2004).

Media liberalization was an important component of many countries' political reforms in the early 1990s. Without legal protections, state-owned monopolies gave way to systems dominated by privately owned outlets (Bourgault 1995; Hydén *et al.* 2003; Nyamnjoh 2005). These outlets expanded discourse through debates, call-in programs, and commentary, usually in vernaculars, and this programming may catalyze interest in politics.

However, many of these emergent outlets were established by politicians or affiliated entrepreneurs who infuse political content with partisan biases and calls to action (Lawson 2002; Nyamnjoh 2005; Snyder & Ballentine 1996). While political discussion of any kind might stimulate psychological engagement, the type of bias to which audiences are exposed might affect motivation, and thus behavioral engagement, in different ways.

There are only a handful of studies on the mobilizing effects of partisan media in advanced democracies,¹ and we know of no such research in newly liberalized polities. We thus develop our own theory by drawing on two related literatures. The first investigates the combined effect of balanced and opinionated media on engagement, without distinguishing

between the two. The second explores the effects of interpersonal communication. While these two literatures lead to differing predictions for how partisan media affect engagement (Dilliplane 2011), they offer a useful starting point for theoretical development.

The first pertinent literature examines how news exposure affects engagement without disaggregating exposure based on the direction or amount of partisan bias (de Vreese & Boomgaarden 2006; Kuenzi & Lambright 2007; Martin 2008; Nisbet 2008; Scheufele 2002). Some scholars argue that exposure to political content in the media increases interest (Atkin *et al.* 1976; Lupia & Philpot 2005; Martin 2008; Strömbäck & Shehata 2010), which then fosters participation (Butler & De La O 2010).² Media often highlight the importance that politics has on wellbeing. As such, media exposure can make politics more salient, thus increasing interest.

The second germane literature focuses on interpersonal communication. This research suggests that the (de)mobilizing effects of political discussion depend on the orientations of discussion partners and the homogeneity of social networks (Huckfeldt *et al.* 2004; Klofstad *et al.* 2013; Lazarsfeld *et al.* 1948; Mutz 2002, 2006; Nir 2011). Scholars argue that like-minded exposure increases engagement, while cross-cutting messages have the opposite effect.

A likely explanation for the divergent effects of congenial and counter-attitudinal discussions on participation is their effect on attitude strength and, thus, motivations. Individuals who are certain that their side is correct and that governance by others would have deleterious consequences will be more motivated to participate than those who are ambivalent and see both sides as equally (in)capable (Lavine 2001; Mutz 2002; Nir 2005; Nir & Druckman 2008). Extending these findings to the field of media studies, scholars have found that exposure to like-minded media in the United States increases attitude extremity (Arceneaux *et al.* 2012; Levendusky 2013). The effect of cross-cutting media on attitude strength has received less

attention, but existing work suggests that it can lead to extremism when partisan-motivated reasoning is high, and ambivalence when partisan-motivated reasoning is low ([forthcoming by authors]; Levendusky 2013).³ In sum, the size and direction of effects on participation may differ according to media orientation, and according to how bias affects attitude strength.

Importantly, these literatures do not distinguish between psychological and behavioral dimensions of mobilization. Most research evaluates only participation, or combines interest and participation into a single measure of engagement (e.g., Scheufele 2002). However, the two dimensions of engagement should not be conflated. The causal relationship between media and interest may not be the same as that between media and participation.

Our theory holds that exposure to political content in the media stimulates interest, regardless of partisan spin. Discussions of hot political topics, candidate strengths or weaknesses, and competitions over power draw attention to the entertaining aspects of politics. We posit the following hypotheses:

- H1: Like-minded media will increase interest in politics
- H2: Cross-cutting media will increase interest in politics
- H3: Neutral media will increase interest in politics

In short, we expect that media of all types will increase political interest.⁴

Notably, we do not expect that media-induced interest determines participation. Discussions can be captivating without being motivating. Even when media increase interest, many individuals will be content to watch, without getting involved. We theorize that partisan media affect participation only when they alter attitude strength. Individuals are most likely to incur the costs of participation when they believe something is at stake and they have a clear idea of their preferred outcomes. When they are ambivalent, they avoid costly actions. In short, media mobilize when they strengthen attitudes and demobilize when they moderate attitudes.

This theory leads to a specific set of expectations for our case of a newly liberalized media system. In another article based on the data also analyzed here, we found that cross-cutting media led to less extreme partisan attitudes, while like-minded and neutral media had no effect on attitude extremity ([forthcoming by authors]).⁵ Contrary to much of the research on the United States, individuals in most newly liberalized polities are motivated by concerns other than partisan identity. Biased information processing along partisan lines—accepting messages from one’s favored party and arguing against those from the other party—requires significant political knowledge and an inclination to dismiss a partisan “other.” However, limited education and experience with competitive electoral politics means that these conditions are not prevalent in many post-liberalization societies. There, individuals are more likely to be persuaded by arguments from the other party than they are to be radicalized by them. Subjects in our experiment became more sanguine about parties and candidates when exposed to alternate perspectives in cross-cutting media, but were unaffected by the familiar arguments in like-minded media or the nuanced ones in neutral media. Given that cross-cutting media reduced attitude strength, but like-minded and neutral media had no effect on attitudes, we hypothesize:

H4: Like-minded media will have no effect on participation

H5: Cross-cutting media will have a negative effect on participation

H6: Neutral media will have no effect on participation

These hypothesized effects are especially likely in post-liberalization settings. Open, vibrant, and diverse political discussions will garner significant attention given their novelty after the uniform and staid programming previously provided by government monopolies. Therefore, media (especially talk radio) are likely to increase interest in politics. However, the costs of participation will be especially limiting in post-liberalization settings. Most such polities are in

the developing world, where citizens have fewer resources to devote to participation, and less experience with it. Interest alone is less likely to induce participation when the costs are high.

Previous scholarship on partisan media does not provide empirical evidence that would allow us to evaluate our hypotheses, for four reasons. First, most research on partisan media has focused solely on like-minded exposure (DellaVigna & Kaplan 2007; Jamieson & Cappella 2008; Hofstetter 1998; Stroud 2007). Only two studies have examined how cross-cutting media affect participation, and while they suggest negative effects, results are not robust.⁶ Second, this scholarship focuses on how partisan media affect participation, but not interest. Third, research on the participatory effects of cross-cutting media, and most of that on like-minded media, is observational. Causal inference with survey data is especially challenging in this domain since media exposure, interest, attitude strength, and participation are likely endogenous. Fourth, there is no research about partisan media effects on engagement outside of the US. The few studies on media and participation in post-liberalization settings in places like Africa and Asia do not distinguish between partisan and neutral media (Aker *et al.* 2013; Kuenzi & Lambright 2007; Olken 2009; Nisbet 2008). Results from the US may not generalize to locations with severe resource constraints, new multiparty systems, and recently pluralized media.

This paper evaluates how different types of messages—like-minded, cross-cutting, and neutral—affect interest and participation. We utilize evidence from a field experiment in Ghana, a country where recent reforms yielded a media system dominated by partisan outlets.

Case Background: Partisan Media in Ghana

Like many other developing countries, Ghana liberalized its media as part of political reforms in the early 1990s. Authoritarian governments previously restricted media, to limit discourse and build legitimacy (Asante 1996; Hachten 1971: 167-70; Hasty 2005: 33-4).

President Jerry Rawlings called for multiparty elections in 1992 and established the National Democratic Congress (NDC) to compete in those contests. The government also eased restrictions on private broadcasters, thereby ending the long-standing monopoly of the state-run Ghana Broadcasting Corporation (GBC). Private radio stations proliferated (Temin & Smith 2002), and today there are some 225 FM stations, the vast majority being commercial ventures.⁷

Many of these new stations have clear political biases, with some advocating for the NDC and others for the other major party, the New Patriotic Party (NPP). Political-talk programming is popular, and many stations devote several hours daily to news and politics. Some political programs feature balanced debates, while others are led by polemical hosts, several of whom attract significant notoriety and large audiences.

Partisan broadcasts are popular, and we expect many individuals are regularly exposed to cross-cutting messages, as well as like-minded or neutral ones, though survey data on the topic are lacking. In countries like Ghana, individuals spend considerable time in public or semi-public settings where they are exposed to media not of their choosing (Nyamnjoh 2005: 16-17). *Tro-tros*, for example, commonly play talk radio, and such vehicles are the modal form of conveyance (Abane 2011). In other words, individuals in such settings cannot engage in selective avoidance, even if so inclined, making exposure to all kinds of media more likely.

In fact, our survey (described below) suggests that regular exposure to various types of messages is common in Ghana. 32% of partisans said they had listened within the last week to the specific cross-cutting morning talk show used in our experiment. And 66% of partisans reported that they had listened to the neutral program (i.e., Peace FM's *Kokrokoo*).⁸ These figures underrepresent actual cross-cutting and neutral exposure, given that subjects were only asked about three of many possible cross-cutting and neutral programs available.

Experimental Design and Data Collection

To test the effects of various types of partisan media, we conducted a field experiment in Ghana's largest city, Accra, in the weeks before the December 2012 election. Subjects were exposed to one of four treatments: political-talk programming on pro-government radio, pro-opposition radio, or neutral radio, or no radio at all (the control). Accra is a politically divided city,⁹ and subjects had heterogeneous political leanings. As such, some subjects were *de facto* assigned to a like-minded treatment, while others were assigned to a cross-cutting or neutral one.

To administer the treatments, we took advantage of the fact that most residents of Accra spend considerable time exposed to radio while commuting in *tro-tros*, which are minibuses with capacities of 15-20 people. Although *tro-tros* are privately owned, they resemble public transportation: passengers enter the first available vehicle, they do not know the drivers, and they are generally unknown to one another. Drivers' route assignments vary and are made by the Ghana Private Road Transport Union. The *tro-tro* stations we used in our study typically contain hundreds—sometimes thousands—of commuters heading to myriad points during rush hour. Most were major transfer hubs, so passengers could not surmise the partisanship, location of residence, or social identity of fellow travelers arriving from multiple locations.

Our field experimental design has a number of significant advantages over observational studies, as well as over laboratory experiments. First, observational studies on media effects are prone to identification problems resulting from the fact that individuals may self-select media. Second, our experiment benefits from high ecological validity, given that subjects were exposed to treatments 1) in natural settings where they are often exposed to like-minded, cross-cutting, and neutral political media; 2) during their quotidian routines, with distractions that individuals normally experience when accessing media; and 3) using actual live broadcasts, rather than material created or repurposed by researchers. Further, our subjects were not aware, either at the

time of exposure or during data collection, that they had been involved in a study of media effects.¹⁰ As such, they were unlikely to be abnormally sensitive to source cues, content, and message biases, which might be the case in laboratory settings (Jerit *et al.* 2013).

Treatments were randomized by *tro-tro*, meaning that passengers within the same vehicle were assigned to the same category. We worked with 228 vans, covering 58 routes in Accra, between 16 October and 7 November 2012. 1200 passengers in these *tro-tros* were interviewed upon completion of their morning commute, though the main analyses of effects include only partisans—nonpartisans cannot be coded for like-minded and cross-cutting exposure.¹¹ In the following sub-sections we discuss procedures for selection of treatments, routes, and subjects.

Radio Treatment Selection: We used live broadcasts for our treatments, which greatly enhances ecological validity. We selected stations after discussions with Ghanaian academics, individuals in media-monitoring organizations, and radio personnel. Along with bias, we considered three selection criteria. First, we needed stations that simultaneously broadcast political programming to enable randomization. We focused on stations with political talk between 6 and 10 AM on weekdays. Second, we only selected stations that broadcast mainly in Twi, one of Accra's *lingua francas* and the language most commonly used in political programming, regardless of station bias, to facilitate broad comprehension. Third, we limited our selection to programs often played in *tro-tros*, to avoid alerting subjects to the manipulation.

We selected Oman FM and Radio Gold as the pro-opposition and pro-government stations, respectively. We chose Peace FM as the neutral station because of its reputation for balance and its popular, lively talk show. All three aired political-talk programming during our target hours. The programs contain news, interviews, audience responses, and commentary. Hosts introduce a news article or topic, which guests then discuss. Reactions are then collected

via calls, SMS, and website postings. Our research was conducted weeks prior to the 2012 elections, and topics included campaign activities, statements by politicians and supporters, candidate characteristics, policies, and current events. Guests included journalists, politicians, party agents, and issue experts, often chosen for their political stance.

Despite similar formats, the programs express dramatically different opinions. The quasi-governmental National Media Commission (NMC) monitored news stories during the 2012 campaigns, ranking Gold and Oman as the most biased. Radio professionals also attested to the editorial nature of the talk shows as compared to news bulletins. Talk-show hosts advocate for their side, guests are selected to support the partisan perspective, and listeners express opinionated viewpoints when joining the conversation. The top panel of Table 1 presents the approximate amount of coverage of the two main parties in news bulletins. We would expect bias to take a different form, but to be even stronger in talk shows.¹² The stations are also widely perceived as biased. The bottom panel of Table 1 shows that the majority of our subjects correctly identified a station's bias, and very few reported the opposite bias (e.g., the pro-government station as pro-opposition, etc.). In sum, content analysis, media experts, and our subjects indicate that our station selections are appropriate.¹³

[Table 1 about here]

Tro-Tro Route Selection: Two criteria guided route ($N=58$) selection: 1) expected travel of at least forty minutes, to facilitate significant treatment exposure, and 2) sufficient vehicles operating during study hours. Selected routes began at various points and ended at one of nine terminuses.¹⁴ Once the experiment was conducted using a given route, that route was never used on another day; this minimized the likelihood of including the same subject twice in our sample, as well as possible contamination if prior subjects shared their experiences with later recruits.

Driver Recruitment and Treatment Administration: We next recruited drivers to act as confederates ($N=228$). In return for 10 Ghanaian cedis (~\$5.26 US), drivers played the assigned station (or no station) for the duration of the ride at a volume that would be clearest to passengers, and without mention of these protocols to others. Drivers turned on the radio only after the vehicle departed, to ensure that individuals would not self-select into or out of treatments. One research assistant rode in each study *tro-tro* to ensure protocol adherence. Simple random assignment was used to assign each vehicle to a treatment.

Subject Recruitment: As a *tro-tro* neared its terminus, the research assistant announced that Ghanaian citizens at least eighteen years old who had been in the van for at least forty minutes were eligible to take a survey upon arrival. This survey would be about “experience with riding *tro-tros* in Accra, conditions faced by commuters in Accra, and what can be done to improve conditions for Ghanaians more generally,”¹⁵ and remuneration of two cedis (~\$1.05) was offered. Interviewers were waiting at several points around the terminus. 1200 commuters completed the survey, though we include only partisans who provided mobile phone numbers in our analyses here. Interview yields per contacted *tro-tro* ranged between 1 and 14, with a mean of 5.3 subjects per vehicle.¹⁶ As Table 2 reports, yields are balanced across treatments, and, as we discuss below, it is unlikely that our results are artifacts of self-selection into the survey.

Checks on Random Assignment and Manipulation: Table 2 reports on statistical balance, for paired comparisons between each radio treatment and the control, as well as aggregate checks.¹⁷ Given the large number of comparisons, we should expect some to be significant by chance, and the number of significant differences in our data is within these bounds. Randomization

succeeded in generating groups that were equivalent in terms of observable measures that were unlikely to be affected by the treatment. Differences in subject attitudes and behaviors between the treatment groups can therefore reliably be attributed to the assigned radio treatment.

[Table 2 about here]

Furthermore, self-reported behaviors that might have diverted attention from the broadcast—such as talking, using a phone or electronic device, or sleeping—did not vary significantly across the four conditions ($\chi^2=1.71, p=.64$), while reported listening to the vehicle's radio did not vary significantly across the radio treatments ($\chi^2=1.26, p=.53$). Thus, treatment effects are likely due to the content of the programming, rather than actions to avoid listening.

Finally, we conduct a manipulation check by asking subjects to identify what, if any, station was playing. 75% in the no-radio condition correctly stated that no radio was playing, while 79% in radio conditions stated that the radio was playing. Of those in the radio conditions who named a station, 76% identified it correctly. The most common incorrect responses were mislabeling partisan stations as neutral, and naming a station besides the three in our experiment. Only seven subjects assigned to the pro-opposition station said the pro-government station played, or vice versa. These results suggest correct application.

Nonetheless, 21% in a radio treatment incorrectly said the radio was not playing, while another 31% could not identify the station. These results are not surprising, since subjects were not cued that they should be attentive to the broadcast. Even those unable to identify the station were still likely affected, although it is possible that some ignored the broadcasts. A strength of our design is that subjects had no contrived reasons to pay attention to the broadcasts; thus, if we find significant effects, we can be more confident that individuals in the real world, who often consume media amidst myriad distractions, would respond similarly.¹⁸

Measurement

Our independent variables are the treatments transformed to record exposure to like-minded or cross-cutting radio, by virtue of subjects' partisan preference.¹⁹ The effects of neutral radio are also included. We use reported vote in the 2008 presidential election as a proxy for partisan preferences. We chose to use a post-treatment measure of partisanship so subjects would not be alerted to the study prior to the treatment.

The data indicate that our measure of partisan preference is valid. First, 2008 vote is balanced across treatments (Table 2), suggesting that responses were unaffected by the experiment. Second, reports of past behavior are less likely to be affected by the treatments than other indicators of partisanship, such as "closeness" to a party or planned vote. Third, given that the two leading presidential candidates in 2012 were also on major-party tickets in 2008, we would expect that 2008 vote would be a good indicator of preferences at the time of our experiment.²⁰ Indeed, our subjects' partisan preferences did not change much between 2008 and 2012. For subjects in the control, 88% of 2008 NDC voters who reported a vote choice for 2012 said they would vote for the NDC again; the figure for the NPP was 89%.²¹ As we note below, it is very unlikely that our findings are due to miscoded partisan preferences.

We use the measure of 2008 vote and the assigned treatments to create indicators of exposure to a station biased towards (*Like-minded*) or against (*Cross-cutting*) the subject's partisan preferences. Like-minded treatments included subjects exposed to: 1) pro-government radio (Gold), for government (NDC) voters, and 2) pro-opposition radio (Oman), for opposition (NPP) voters.²² Cross-cutting treatments included subjects exposed to: 1) pro-opposition radio (Oman), for government (NDC) voters, and 2) pro-government radio (Gold), for opposition (NPP) voters. The non-partisan radio treatment (*Neutral*) includes subjects exposed to Peace

FM. Our analyses include only those who reported voting for NDC or NPP in 2008 ($N=752$).²³ Partisans in the treatment groups are compared to partisans in the no-radio control.

The main outcomes are psychological and behavioral engagement. We measure the former through two questions, one measuring the perceived importance of politics and the other excitement about the campaign. We take the mean of these variables to create a scale (*Interest*), values of which range from zero to three.²⁴ Higher values indicate greater interest.

Our measure of participation (*Participation*) records whether subjects signed an SMS petition to parties. Subjects were asked to identify three problems with *tro-tros*. They were then told that we would deliver a petition to the Accra headquarters of major parties listing the most common concerns amongst all subjects.²⁵ We invited subjects to sign the petition and told them to expect an SMS with instructions. The following text was sent that evening or in subsequent days: “Thank you for taking our survey. To sign the petition to major parties, listing commonly named problems with trotros, please reply by texting YES”. 97 subjects (9.5%) texted YES.

This is a meaningful measure of political participation, in that the targets were political actors, the issue was salient, and the action incurred a cost to the subject, in the form of time and phone credit. It is also unobtrusive. The recruitment, survey, and interview location all referenced *tro-tros*, so a petition on transport seemed a natural request. We were able to collect data on participation without divulging the study's purpose until after the petition period.

Our measure offers several advantages. First, we minimize Hawthorne effects by not requiring participation under an enumerator's gaze. Subjects replied in private, at their own convenience. The physical separation from the enumerator reduces desirability bias.

Second, self-reported participation can be biased (Hammer *et al.* 2014). We directly observe participation, so there is no possibility of over-reporting. We measure how the

treatments affect behavior, and not induced differences in how subjects wish to be perceived, recall events, react affectively, or other sources of reporting bias. Therefore, we can accurately distinguish treatment effects on behavioral engagement from psychological engagement.

Third, we purposefully lagged our measure of participation from the time of the treatments and our measurement of interest. In real-world settings, most opportunities for participation occur hours or days after media exposure, and the potential for messages to impact behavior likely dissipates over this time. Measures taken immediately after the treatment are likely to result in inflated estimates of effects (Jerit *et al.* 2013). Finally, individuals in the real world are likely exposed to different types of media messages and other political stimuli between the time they receive a certain message and the opportunity to act. In lagging our measure, we therefore approximate real-world characteristics.

Finally, our subjects had to incur the monetary cost of sending the SMS and expend the effort to do so. The costliness of political action is critical to our theoretical argument about why interest might not translate into action. Given that 8% of our subjects responded, signing the petition seems to have been costly for subjects, but not prohibitively so.

Results

To examine the effects of media on engagement, we compare partisans exposed to like-minded, cross-cutting, or neutral radio to partisans not exposed to radio. In other words, the no-radio condition is the excluded category. Our estimated models follow this format:

$$\text{Interest or Participation} = \beta_0 + \beta_1 \text{Like-minded} + \beta_2 \text{Cross-cutting} + \beta_3 \text{Neutral} + \varepsilon$$

[Table 3 about here]

Results are presented in Table 3. Model 1 estimates treatment effects on interest. We use OLS regression since the outcome is continuous. Coefficients on all three treatments—like-

minded, cross-cutting, and neutral—are significant and positive, thus supporting H1, H2, and H3, which predict that partisan media heighten psychological engagement. Subjects in the like-minded treatment had estimated scores that were 18.0% higher than those in the no-radio control, while subjects in the cross-cutting treatment had scores that were 13.0% higher than those in the control. We also find that non-partisan radio increases psychological engagement; subjects in the neutral treatment had scores that were 6.2% higher than those in the control.²⁶ In short, we find significant evidence that all types of political discussion on the radio stimulate interest.²⁷

Next, we estimate treatment effects on participation, measured as signing the SMS petition. In model 2, we use logistic regression, given the dichotomous (i.e., signed/did not sign) outcome. Like-minded media have no estimated effect on participation, consistent with H4. Similarly, neutral radio has no significant effect on participation, as predicted in H6.

Most importantly, however, cross-cutting media decreased likelihood of signing the petition to parties, and the effect is statistically significant, as H5 predicted. Those in the cross-cutting treatment had a 44% lower response rate than those in the no-radio control; twelve percent of those in the cross-cutting treatment group signed the petition, as compared to seven percent in the control group. In sum, exposure to cross-cutting media increased political interest, but it also led to significant declines in actual participation.

The effects of partisan media on participation are in line with how they affected attitudes, rather than how they affected interest. In another paper using the same data ([forthcoming by authors]), we find that subjects exposed to cross-cutting treatments held less polarized attitudes about politicians and parties than those in the control. Moderation can reduce motivation to participate in politics by making individuals indifferent between potential outcomes.

We consider a possible alternate explanation for the apparent depressive effects of cross-cutting messages on participation. It is possible that subjects who received discordant messages became more cynical about politics as a result of hearing negative critiques of their own side. However, we find no evidence that the cross-cutting treatments decreased subjects' assessment of the trustworthiness of politicians, ability of elections to result in improvements in the country, or the likelihood of politicians' keeping their promises (Appendix E). Exposure to cross-cutting media did not make subjects turn away from politics in disgust.

Instead, the evidence suggests that exposure to cross-cutting media made subjects more sanguine about politics. After hearing discussions from the other side, they came to view out-party candidates more positively, thus closing the gap between their assessments of the two sides ([forthcoming by authors]). Cross-cutting media reduced participation because it made subjects more comfortable in the full range of potential office holders' abilities to perform adequately, even without their own input. Individuals became more complacent about government and more willing to acquiesce to the decisions of politicians from both sides.

Before concluding, we consider whether these results are likely to be due to subject self-selection or measurement error. We cannot think of a plausible scenario whereby treatment-induced differences in who agreed to be interviewed would generate these effects. *Tro-tro* yields were balanced across treatments (Table 2). For subject composition to be responsible for results, cross-cutting exposure would have to encourage less-participatory individuals to answer a survey about transport and discourage equal numbers of more-participatory individuals. Observables are also balanced, so participatory individuals encouraged and non-participatory individuals discouraged would have to be equivalent in demographics, partisanship, radio habits, and journey details. The logic is similar for interest. All kinds of radio would have to induce more

politically interested individuals to answer the survey while discouraging an equal number of identical (at least with respect to observables) uninterested individuals.

Finally, it seems unlikely that our results are due to error in our post-treatment measure of partisan preference. First, as mentioned earlier, reported vote choice in 2008 is balanced (Table 2), indicating that the treatment is unlikely to have affected our measure. Second, we can think of no reason why less-participatory listeners would report having voted for the party opposite the one favored by the station to which they had just been exposed, while more-participatory listeners would report the opposite. Third, the main effects of the radio treatments are significant and in line with what we would expect based on the results here. The main effects analyses are not dependent on the measure of partisan preferences.²⁸ In sum, we expect that the results are not artifacts of our research design or coding of type of exposure.

Conclusion

In many countries, government monopolies have recently given way to competitive media environments that allow for pluralistic discussion. However, many systems are also dominated by outlets with partisan biases. We provide the first investigation of the mobilizing, or demobilizing, effects of opinionated media in a post-liberalization setting, examining both psychological and behavioral engagement. We theorize that exposure to political discussions in the media heightens interest because pluralistic and open discussions are novel in such settings. However, we also posit that interest is not enough to motivate participation when costs are burdensome. Media affect participation only when they impact attitudinal intensity and, thus, motivation. In contrast to the uniformly positive effects of media on interest, we theorize that like-minded, cross-cutting, and neutral radio can have differential effects on participation.

To examine the effects of partisan media we conducted a field experiment in Ghana, which underwent media liberalization in the early 1990s and is now dominated by biased outlets. Subjects in Accra were exposed, at random, to pro-government, pro-opposition, or non-partisan live radio broadcasts during the last election campaign. Treatments were administered in a setting in which Ghanaians are often exposed to like-minded or cross-cutting messages: over the sound systems of *tro-tros*, during their morning commutes.

We find that exposure to political content increased interest, regardless of stations' partisan orientation. Yet, as hypothesized, cross-cutting media decreased participation, measured as whether subjects signed an SMS petition to parties. Elsewhere, we found that cross-cutting media moderated attitudes through exposure to divergent views ([forthcoming by authors]). Even though these subjects were more interested in politics, their ambivalence depressed action. As with most participation, our subjects had to spend time and money to sign the petition. The results suggest that increased interest alone did not encourage subjects to incur these costs.

This project makes an important methodological contribution to research on media effects. Many studies draw upon survey data, which can lead to identification problems. Citizens who are interested in politics, have stronger attitudes, and participate at higher rates may also consume more, and different types of, media. Other studies employ laboratory experiments, which often have limited ecological validity. Subjects in laboratory experiments are aware that they are being studied, so they might pay abnormally high attention to treatments and be more sensitive to desirability effects when being evaluated. Our field experiment minimizes these problems, by exposing subjects to real, live broadcasts in a natural setting, and without their knowledge during treatment or data collection that they were involved in a media-effects study.

Furthermore, we use a novel measure of actual participation that is not affected by biases associated with self-reported intended or past participation.

The results of this study also offer theoretical insights of import, for both post-liberalization societies and for advanced democracies. Few studies test whether partisan media affect engagement, and nearly all of these evaluate only like-minded exposure. We examine the effects of cross-cutting and neutral media alongside like-minded media, which facilitates interpretation of results. For example, we find that all three types of media heighten interest in politics, thus suggesting that factors other than targeted appeals to in-group members and discussion of attitude-congruent information can increase psychological engagement.

We also examine interest and participation as two distinct dimensions of engagement. The theoretical distinction between psychological and behavioral engagement is supported by the empirics: cross-cutting media increased interest in politics, but decreased participation.

Finally, in terms of normative implications, there are some promising findings. The animated and pluralistic programming that emerged in the aftermath of media liberalization seems to increase attention to politics. By heightening psychological engagement, media may provide benefits for democratic governance, such as increased scrutiny of elected officials. However, the outlook is not entirely positive. Interest did not beget action. While we find that cross-cutting media have the ostensibly beneficial effects of moderating political attitudes, the effects of these moderating influences on politics might be limited if moderation in turn causes individuals to reduce their involvement, as our evidence suggests. The effects of partisan media could be to create a more psychologically engaged, but also more quiescent population.

¹ This is especially true for cross-cutting media. We are aware of only two studies on the effects of cross-cutting media on participation, both based on the US (Barker 2002; Dilliplane 2011).

² An alternate literature, on media malaise, argues that news coverage can decrease political trust, thereby reducing engagement (Cappella & Jamieson 1996; O'Keefe 1980; Robinson 1976). However, we found the opposite was true in this case; exposure to partisan media increased trust ([forthcoming by authors]). Nonetheless, we use two-tailed tests, which are appropriate for evaluating whether media increased or decreased interest and participation.

³ In other words, when individuals have the tools and/or inclination to argue against cross-cutting messages, such messages might lead to a strengthening of positions. On the other hand, when individuals lack the tools and/or inclination, exposure to cross-cutting messages might lead to acceptance of discordant views, and thus, moderation.

⁴ We do not hypothesize that one type is more likely to increase interest, though this is possible.

⁵ The causal variables (like-minded, cross-cutting, and neutral exposure) were the same as those used in this article. We used five measures of the outcome of interest (attitude extremity). First, we find no statistically significant differences on any of our outcomes between partisans exposed to like-minded media and those not exposed to radio (i.e., the control). And we find no statistically significant differences between the neutral radio group and the control. However, we find that cross-cutting media did significantly moderate attitudes and reduce partisan polarization across all five measures. Cross-cutting media significantly: 1) reduced the gap in positive attitudes towards one's own party *vis-à-vis* the other party ($p=.00$); 2) led to more moderate attitudes about candidates from the governing party ($p=.04$); 3) led to more moderate attitudes about opposition party candidates ($p=.02$); 4) decreased aversion to voting for the other side ($p=.05$); and 5) increased selection of the keychain with a national symbol (i.e., the

Ghanaian flag) over one indicating partisan identity ($p=.10$). For this last outcome, subjects were presented with three keychains—NPP, NDC, and national flag—at the end of the survey as a “token of our gratitude,” and subjects were coded as displaying less-extreme partisan attitudes if they took the flag keychain rather than their party’s keychain. See Appendix F for the results.

⁶ Barker (2002) found a negative, but insignificant, correlation between exposure to Limbaugh and participation amongst liberals. Dilliplane (2011) finds some evidence of demobilization from cross-cutting media, but the results are not robust across estimation strategies.

⁷ Data from the National Communications Authority (NCA), at www.nca.org.gh.

⁸ We report responses only from the no-radio group, which were not affected by the treatments.

⁹ In the 2008 presidential runoff, 54.5% voted NDC and 45.5% voted NPP in Greater Accra.

¹⁰ As per IRB guidelines, subjects were debriefed following all data collection.

¹¹ Prior to the experiment, we conducted a power analysis to calculate the requisite sample sizes per treatment group for detecting any substantively meaningful effects with sufficient power.

¹² The NMC monitored only bulletins. We use these data to indicate station bias rather than content of programs under investigation. Professionalism dictates neutral presentation of bulletins, so bias results primarily from greater coverage of the favored party’s campaign rallies, statements, and the like. In contrast, talk shows use slanted rhetoric.

¹³ We focus on partisan bias, not inflammatory language, given the prevalence of the former and the relative scarcity of the latter. Content analysis by the Media Foundation for West Africa shows that “indecent expressions” constituted a small proportion of total political speech (calculations from MFWA 2012). Nonetheless, our partisan stations were the worst offenders.

¹⁴ In preparation, researchers conducted an enumeration of *tro-tro* routes. They contacted representatives of the Ghana Private Road Transport Union at the main terminuses to generate a

list of origin points that dispatched vehicles to each station during morning hours. They then visited the origin points and interviewed drivers to gather information about morning ridership.

¹⁵ Many questions addressed the transit system. We created English, Ga, and Twi instruments.

¹⁶ Because *tro-tro* capacity varies, and because we do not have ridership figures for each vehicle, we cannot determine what proportion of eligible passengers completed an interview. We surmise that passengers who chose to answer our survey were slightly less well-off and had fewer time constraints than those who did not answer the survey. As we discuss later, it does not seem plausible that self-selection is responsible for our findings.

¹⁷ Details on the balance checks and variables are in Appendices A and B.

¹⁸ We present an intention-to-treat (ITT) analysis of the effects of exposure, rather than a treatment-on-the-treated (TOT) analysis of the effects of listening, for several reasons. First, TOT requires a measure of treatment uptake, and we lack a validated measure of actual radio listening. Our best measure is whether subjects *said* they listened to the radio, but individuals may have forgotten that they listened. Second, this measure of reported listening is binary. Estimates from instrumental variable models with binary endogenous variables are only consistent under restrictive assumptions (Wooldridge 2010). For those who are interested in the estimated effects of listening, given these notable limitations, Appendix D presents the results of TOT analyses instrumented by reported listening. The results seemingly offer stronger support for our hypotheses than the ITT analysis; the TOT estimates are much larger and the p-value for cross-cutting listening is significant at the 95% rather than 90% level. Although the results are favorable, we urge utmost caution in drawing inferences from the TOT analyses.

¹⁹ See Appendix B for question wordings and Appendix C for descriptive statistics.

²⁰ The NPP fielded the same presidential candidate—Nana Akufo-Addo—in both races, while the NDC’s 2012 flagbearer—John Dramani Mahama—was the vice presidential candidate in 2008. He ascended to the presidency upon the death of President John Atta Mills in July 2012.

²¹ The correlation between 2008 vote and intended 2012 vote in the control group is strong ($r=.79$, $p=.00$ for 2008 NDC voters, $r=.78$, $p=.00$ for NPP). The similarity between these figures suggests that the 2008 measure as an indicator of partisan preferences at the time of the experiment was equally reliable for those coded as NDC and NPP partisans.

²² In 2008, NPP was the government party and NDC the opposition. To ease the discussion, we refer to the parties in relation to their status at the time of the experiment in 2012.

²³ Those who did not vote, did not report their choice or voted for a minor party were thereby excluded from the analysis, even if assigned to the neutral or no-radio conditions.

²⁴ The Cronbach’s alpha for the scale is .65.

²⁵ Enumerators collected mobile numbers from subjects, and 85.3% provided a contact. All analyses, whether on psychological or behavioral engagement, are limited to those individuals. Results for psychological engagement do not change significantly when conducted on all partisans, regardless of provision of a mobile phone number. With respect to subjects’ providing contacts, paired comparisons between the control and treatments are insignificant for pro-government (Gold) ($p = .66$) and neutral radio (Peace) ($p = .93$). The comparison of the control and pro-opposition radio (Oman) is significant ($p = .10$), but substantively the difference is small (88.7% of Oman listeners provided a cell, versus 84.4% of those in the control).

²⁶ Between-treatment differences in effects on interest are not statistically significant. We cannot confidently say that one type of media message has larger effects on interest than another.

²⁷ These results are robust to several alternate specifications of interest with respect to like-minded exposure, and to a lesser extent for cross-cutting and neutral media exposure. When we separate the scale into its component parts, like-minded, cross-cutting and neutral media each have a significant effect on excitement about campaigns. However, only the estimate for like-minded media is within conventional levels of significance for the single question about perceived importance. We also examined an additional measure of engagement: intention to get involved in campaigns. We view intention to participate as lying somewhere between psychological and behavioral engagement, but theoretically and empirically closer to the former. Stating intent is costless and often an expression of interest rather than a predictor of action. When we use a single scale for psychological engagement including perceived importance, excitement, and intention to attend a rally, talk to an official, display a party item, and vote ($\alpha = .75$), the effect of like-minded exposure is significant, but the effects of cross-cutting and neutral are not. Individually, like-minded media increase intention to attend a rally and display a party item at the .10 level, but have no effect on intention to talk to an official or vote.

²⁸ We ran a model in which we regressed political interest on a dummy indicator of partisan media (i.e., Radio Gold and Oman FM) and the dummy indicator for neutral media (i.e., Peace FM), using the entire sample (i.e., not restricting to major-party partisans) of mobile-number providers ($N=1023$). Partisan media significantly increase psychological engagement ($b=.12$, $s.e.=.06$, $p=.07$). Neutral media also increase psychological engagement, but not significantly so ($b=.10$, $s.e.=.08$, $p=.20$). When we ran the same model with participation as the outcome, partisan media had a negative, but non-significant, estimated effect ($b=-.27$, $s.e.=.25$, $p=.29$).

Works Cited

- Abane, A. (2011). Travel behaviour in Ghana. *Journal of Transport Geography*, 19(2), 313-22.
- Aker, J., Collier, P., & Vicente, P. (2013). Is information power? Retrieved from www.pedrovicente.org/cell.pdf.
- Arceneaux, K., Johnson, M. & Murphy, C. (2012). Polarized political communication, oppositional media hostility, and selective exposure. *Journal of Politics*, 74(1), 74-86.
- Asante, C. (1996). *The press in Ghana*. Lanham, MD: University Press of America.
- Atkin, C. K., Galloway, J., & Nayman, O. (1976). News media exposure, political knowledge and campaign interest. *Journalism Quarterly*, 53(2), 231-7.
- Barker, D. C. (2002). *Rushed to judgment*. New York: Columbia UP.
- Bourgault, L. (1995). *Mass media in sub-Saharan Africa*. Bloomington: Indiana UP.
- Butler, D., & De La O, A. (2010). The causal effect of media-driven political interest on political attitudes and behavior. *Quarterly Journal of Political Science*, 5(4), 321-37.
- Cappella, J., & Jamieson, K. (1996). News frames, political cynicism, and media cynicism. *Annals of the American Academy of Political and Social Science*, 546, 71-84.
- De La O, A. (2013). Do conditional cash transfers affect electoral behavior? *American Journal of Political Science*, 57(1), 1-14.
- De Vreese, C., & Boomgaarden, H. (2006). News, political knowledge and participation. *Acta Politica*, 41, 317-41.
- DellaVigna, S., & Kaplan, E. (2007). The Fox News effect. *Quarterly Journal of Economics*, 122(3), 1187-234.
- Dilliplane, S. (2011) All the news you want to hear. *Public Opinion Quarterly*, 75(2), 287-316.

- Gans-Morse, J., Mazzuca, S., & Nicther, S. (2014). Varieties of Clientelism. *American Journal of Political Science*, 58(2), 415-32.
- Hachten, W. (1971). *Muffled drums*. Ames: Iowa State University Press.
- Hammer, M., Banks, A., & White, I. (2014). Experiments to reduce the over-reporting of voting. *Political Analysis*, 22(1), 130-41.
- Hasty, J. (2005). *The press and political culture in Ghana*. Bloomington: Indiana UP.
- Hofstetter, R. (1998). Political talk radio, situational involvement, and political mobilization. *Social Science Quarterly*, 79(2), 273-86.
- Horowitz, D. (1985). *Ethnic groups in conflict*. Berkeley: University of California Press.
- Huckfeldt, R., Mendez, J., & Osborn, T. (2004). Disagreement, ambivalence, and engagement. *Political Psychology*, 25(1), 65-95.
- Hydén, G., Leslie, M., & Ogundimu, F.(Eds.). (2003). *Media and democracy in Africa*. London: Transaction Publishers.
- Jamieson, K., & Cappella, J. (2008). *Echo chamber*. New York: Oxford UP.
- Jerit, J., Barabas, J., & Clifford, S. (2013). Comparing contemporaneous laboratory and field experiments on media effects. *Public Opinion Quarterly*, 77(1), 256-82.
- Klofstad, C., Sokhey, A., & McClurg, S. (2013). Disagreeing about disagreement. *American Journal of Political Science*, 57(1), 120-34.
- Kuenzi, M., & Lambright, G. (2007). Voter turnout in Africa's multiparty regimes. *Comparative Political Studies*, 40(6), 665-90.
- Lavine, H. (2001). The electoral consequences of ambivalence toward presidential candidates. *American Journal of Political Science*, 45(4), 915-29.
- Lawson, C. (2002). *Building the fourth estate*. Berkeley: University of California Press.

- Lazarsfeld, P., Berelson, B. & Gaudet, H. (1948). *The people's choice*. New York: Columbia UP.
- Levendusky, M. (2013). Why do partisan media polarize viewers? *American Journal of Political Science*, 57(3), 611-23.
- Lupia, A., & Philpot, T. (2005). Views from inside the net. *Journal of Politics*, 67(4), 1122–42.
- Martin, P. S. (2008.) The mass media as sentinel. *Political Communication*, 25(2), 180–193.
- Mutz, D. (2002). The consequences of cross-cutting networks for political participation. *American Journal of Political Science*, 46(4), 838-55.
- Mutz, D. (2006). *Hearing the other side*. Cambridge: Cambridge UP.
- Nir, L. (2005). Ambivalent social networks and their consequences for participation. *International Journal of Public Opinion Research*, 17(4), 422-42.
- Nir, L. (2011). Disagreement and opposition in social networks. *Political Studies*, 59(3), 674-92.
- Nir, L., and Druckman, J. (2008). Campaign mixed-message flows and timing of vote decision.” *International Journal of Public Opinion Research*, 20(3), 326-46.
- Nisbet, E. (2008) Media use, democratic citizenship and communication gaps in a developing democracy. *International Journal of Public Opinion Research*, 20(4), 454-82.
- Nyamnjoh, F. (2005). *Africa's media*. London: Zed Books.
- O’Keefe, G. (1980). Political malaise and reliance on media. *Journalism Quarterly*, 57(1) 122-8.
- Olken, B. 2009. Do television and radio destroy social capital? *American Economic Journal: Applied Economics*, 1(4): 1-33.
- Robinson, M. (1976). Public affairs television and the growth of political malaise.” *American Political Science Review*, 70(2), 409-32.
- Scheufele, D. (2002). Examining differential gains from mass media and their implications for participatory behavior. *Communication Research*, 29(1), 46-95.

- Snyder, J., & Ballentine, K. (1996). Nationalism and the marketplace of ideas.” *International Security*, 21(2), 5-40.
- Strömbäck, J., & Shehata, A. (2010). Media malaise or a virtuous circle? *European Journal of Political Research*, 49(5), 575–597.
- Stroud, N. (2007). Media effects, selective exposure & *Fahrenheit 9/11*. *Political Communication*, 24(4), 415-32.
- Temin, J., & Smith, D. (2002). Media matters. *African Affairs*, 101(405), 585-605.
- Verba, S., Schlozman, K., & Brady, H. (1995). *Voice and equality*. Cambridge: Harvard UP.
- Wilkinson, S. (2006). *Votes and violence*. Cambridge: Cambridge UP.
- Wooldridge, J. (2010). *Econometric Analysis of Cross Section and Panel Data*. Cambridge: MIT Press.

Table 1: Partisan Bias by Station

Panel One: Percentage of News Stories about Main Parties (NMC 2013)

	Radio Gold (pro-government)	Oman FM (pro-opposition)	Peace FM (neutral)
NDC (government)	80.1	6.3	39.2
NPP (opposition)	8.1	88.4	32.5

Panel Two: Subjects' Perceptions of Station Bias (Authors' survey)

	Radio Gold (pro-government)	Oman FM (pro-opposition)	Peace FM (neutral)
Pro-Government	53.9	4.2	5.4
Pro-Opposition	2.4	58.6	25.3
Neutral	19.3	15.0	53.2
Don't know	24.3	22.3	16.1

Note: Coverage of minor parties not reported in Panel One.

Table 2: Balance Checks

Variable	Range	Mean Value for Treatment Group				p
		No Radio	Oman	Gold	Peace	
Demographics						
Sex	0-1	.33	.38	.36	.36	.72
Age	18-84	33.16	33.36	33.21	31.77	.32
Education	1-10	5.65	5.59	5.68	5.67	.93
Wealth index	0-5	3.17	3.17	3.17	3.06 *	.18
Ethnicity						
Akan	0-1	.48	.44	.48	.52	.32
Ewe	0-1	.22	.27	.21	.19	.08
Ga	0-1	.16	.16	.17	.21	.34
Language Ability						
English	0-3	2.23	2.23	2.28	2.27	.97
Twi	0-3	2.39	2.44	2.51	2.48	.30
Ewe	0-3	.86	1.00	.80	.86	.26
Ga	0-3	1.40	1.52	1.44	1.58	.37
2008 vote						
Voted 2008	0-1	.75	.75	.75	.70	.55
Voted NDC	0-1	.39	.43	.40	.40	.77
Voted NPP	0-1	.33	.28	.33	.27	.37
Refused response	0-1	.11	.14	.12	.19	.59
Radio listening habits						
General frequency	0-3	2.40	2.35	2.34	2.41	.65
Peace morning show	0-3	1.17	1.10	1.11	1.20	.56
Gold morning show	0-3	.59	.68	.64	.62	.55
Oman morning show	0-3	.71	.72	.65	.75	.71
Journey details						
Seat proximity to rear	1-4	2.28	2.80	2.79	2.85	.85
Duration (minutes)	24-110	55.90	51.76	53.88	52.89	.62
Start time (30-min. slots)	1-8	3.86	4.45	3.86	4.38	.50
Interviews per van	1-14	5.53	5.31	5.29	4.91	.69

Notes: Statistically significant comparisons between the no-radio control and other treatments are marked as follows: * $p < .10$; ** $p < .05$; *** $p < .01$. T-tests are conducted for continuous variables, Chi-square tests for categorical and dummy variables, and Wilcoxon-Mann-Whitney tests for ordinal variables. Right-hand column reports p values for tests of relationships between variables of interest and treatment categories.

Table 3: Effects of Radio Treatments on Political Interest & Participation

	(1) Interest	(2) Participation
Like-minded	.24*** (.09)	-.32 (.36)
Cross-cutting	.16* (.09)	-.63* (.38)
Neutral	.18** (.09)	-.14 (.37)
Intercept	1.66*** (.06)	-2.02*** (.23)
<i>N</i>	634	658
R-squared	.01	.01

Notes: Cell entries are OLS regression coefficients for Model 1 and logistic regression coefficients for Model 2. Standard errors are reported in parentheses. Adjusted R-square reported for Model 2. Coefficients that can be distinguished from zero are marked as follows: * ≤ 0.10 ; ** ≤ 0.05 ; *** ≤ 0.01 (for two-tailed tests). The excluded group is subjects assigned to the no-radio condition. Participation is measured by whether subjects signed the SMS petition to parties. All analyses performed on major-party partisans who provided mobile numbers.

Online Supplemental Material

Appendix A: Description of Balance Checks

In Table 2, we report on balance checks for nearly two dozen variables. Most of these variables are included because we did not expect that they would be affected by the treatments, while they might theoretically impact how individuals responded to the experimental treatments. These variables included demographic measures, such as sex, age, education, personal wealth, and ethnicity.

We also check for balance on variables that might have affected individuals' abilities to comprehend broadcasts, such as language ability (English, Twi, Ewe, and Ga), general frequency of radio listenership, and prior listenership to the morning shows included in the treatments. In addition, we include a variable measuring subjects' seating in the vehicle, in case individuals who were located closer to the rear were less able to hear the *tro-tro*'s sound system.

Other variables intended to measure *tro-tro* specific factors, such as the duration (in minutes) of the treatment application (as recorded by research staff who rode in the *tro-tros* included in the study), the starting time of the journey (in eight half-hour slots, running between 6 and 10 AM), and number of successful interviews conducted per contacted *tro-tro*, are also included.

Finally, as discussed in the paper, we also check for balance on variables measuring participation in and preferences regarding the 2008 presidential election (i.e., turnout, vote for NDC candidate, vote for NPP candidate, refusal to report vote).

Question wordings for variables included in the survey are listed in Appendix B. All variables except start time, duration, and interviews per *tro-tro* are measured at the individual level; these three variables are measured at the *tro-tro* level.

Appendix B: English-Language Survey Question Wordings

Dependent Variables

Interest

[3] Some people think politics isn't important to them, while others find it touches their daily lives a lot. How interested would you say that you are about politics? Would you say that you are very interested, moderately interested, only a little interested, or not interested at all?

[4] What about the campaigns currently going on in this country, for the December elections? Some people find campaigns exciting, even fun, to watch and follow, while others find them to be rather boring. How would you personally describe your feelings about the current campaigns? Do you think they are very exciting, somewhat exciting, somewhat boring, or very boring?

Participation

[63] In your opinion, what are the three biggest problems that you think need to be solved with the *tro-tro* system here in Accra?

Thank you. One of the things we will do after this survey is contact the major parties with a list of the most commonly named problems that respondents like you provided to our survey. We want to let the parties know what *tro-tro* riders in Accra think needs to be done to improve the system here. There is a chance you will receive an SMS message in the next hours or days asking you to respond with a simple text, in order to sign a petition that will be sent to the major parties, to urge them to consider the issues that people have raised in this survey. If you receive a text, all you will have to do is reply with the word "YES," and your phone number will be listed as a signer of the petition. This is an important topic, and we hope you will take the opportunity to help with this cause.

SMS message: "Thank you for taking our survey. To sign the petition to major parties, listing commonly named problems with trotros, please reply by texting YES"

Party Affiliation Measure

2008 Vote

[22] For which candidate did you vote in the first round of the 2008 presidential election? [*Candidates' names not read. If subject could not remember candidate's name, follow up*]: Do you remember of what party the candidate was a member? [*Question only asked of those who had previously reported having voted in 2008, in Question 21: Let's talk about political participation in the past. We know that many Ghanaians did not go to the polls in the last general elections, in 2008. Did you go to the polls to vote in the first round of the 2008 elections, when this country elected a president and parliament?*]

2012 Vote (to check for validity of 2008 measure)

[11] Whom would you vote for if the 2012 presidential election were today? [*Candidates' names not read. If respondent could not remember candidate's name, follow up*]: Do you remember which party the candidate is a member of? [*Question only asked of those who had*

previously reported expecting to vote in 2012, in Question 10: Understanding that some Ghanaians choose not to vote, how likely do you think it is that you will vote in the next general elections? Would you say you are totally sure you will vote, very likely to vote, somewhat likely to vote, not very likely to vote, or are you sure that you will not vote?]

Manipulation Checks

[58] People choose to do different things to pass the time when they are riding in *tro-tros*. I'm going to read you a list of things that people sometimes do. Can you tell me which you did during the *tro-tro* ride that brought you to this station? A) Talked to my fellow passengers; B) Talked on my mobile phone; C) Listened to the radio playing on the *tro-tro*; D) Listened to my own phone or device for entertainment; E) Slept.

[59] Was the radio playing in the *tro-tro*?

[61] Can you tell me which radio station was playing in the *tro-tro*? *[Options not read. Only asked of those who reported in Question 59 that the radio was playing.]*

[62] From what you know about radio in Accra, would you say that the presenters on the stations I'm going to read to you are more in favor of the government or the opposition, or are they neutral? A) Radio Gold? B) Peace FM? C) Oman FM?

Other Variables

[53] In the last week, how often would you say that you listen to the following morning shows? Every day, most days, a few days, or not at all? A) "Kokrokoo" on Peace FM? B) "Gold Power Drive" on Gold FM? C) "National Agenda" on Oman FM? *[Only asked of those who previously reported listening to radio, in Question 52: For each of these sources, please tell me how often you think you got your news from them in the last week. Every day, most days, a few days, or not at all? Radio?]*

Tro-tro Seating

[1] Where were you seated in the *tro-tro*? Were you seated in the front row with the driver; near the front; towards the middle; or towards the back of the *tro-tro*?

Age

[46] How old are you?

Education

[47] What is the highest level of education that you have completed?

Ethnic Group

[48] What is your ethnic group or tribe? *[Options not read.]*

Wealth

[56] I am going to read you a list of items. Please tell me which ones your household has. A) Piped water in your home? B) DVD player? C) Personal computer? D) Refrigerator? E) Motor vehicle?

Languages

[57] I'm going to read you a list of languages. Can you please tell me whether or not you could understand someone speaking in each one. Could you understand them extremely well; fairly well, with just a few problems; a little, but with many problems; or not at all? A) English? B) Twi? C) Fante? D) Ewe? E) Ga?

Cynicism

[9A] I am now going to read you a list of people and institutions in this country. Please tell me whether you trust them a lot, somewhat, just a little, or not at all. Politicians

[6] I am now going to read you two statements. Please tell me which one, A or B, you agree with most. A: No matter whom we vote for, things will not get better in this country in the future. B: We can use our power to choose leaders who will help us improve our lives. *[If subject answered A or B]:* And do you agree with *[A/B]* strongly, or only somewhat?

[27] In your opinion, how often do politicians in this country keep their campaign promises after elections? Do they keep them almost always, most of the time, about half of the time, once in a while, or never?

[5] And thinking about your own involvement in politics—how much of an effort do you think someone like you can have on politics in our society? Do you think you can have a great deal of impact, a moderate amount of impact, only a little impact, or no impact at all?

Extreme Partisan Attitudes and Behavior

[23] Among the political parties in this country, are there any which you would never vote for, under any circumstances? [Multiple answers possible; options not read.]

[29] I would like to ask you your opinions about some of the political parties in this country. First, please tell me how well you think these words or phrases describe candidates from the NATIONAL DEMOCRATIC CONGRESS (NDC)? Do you think they describe them extremely well, somewhat well, a little, or not at all? 1) Honest? 2) Strong leader? 3) Capable of bringing development to Ghana?

[30] Next, please tell me how well you think these words or phrases describe candidates from the NEW PATRIOTIC PARTY (NPP)? Do you think they describe them extremely well, somewhat well, a little, or not at all? 1) Honest? 2) Strong leader? 3) Capable of bringing development to Ghana?

[65] We would like to offer you another item as a thank you. Please feel free to take one of these key holders. [Interviewer offers display of three key holders and records subject's selection.]

Appendix C: Descriptive Statistics of Key Variables

Variable	Mean	Std. Dev.	Min	Max	N
Dependent Variables					
Interest					
<i>Importance of politics</i>	1.65	1.05	0	3	751
<i>Excitement about campaigns</i>	1.90	.87	0	3	724
Participation					
<i>SMS response</i>	.09	.29	0	1	752
Partisanship Measure (2008 vote) (1=NDC, 0=NPP)	.57	.450	0	1	752

Notes: For partisanship measure, votes for minor parties, refusals, and reported non-votes excluded. Statistics provided only for partisan subjects who provided mobile numbers.

**Appendix D: TOT Analysis of Radio Listening Effects on Interest and Participation,
Instrumented by Treatments**

Second Stage

Outcome	Interest			Participation		
Instrument	(1) Like- minded	(2) Cross- cutting	(3) Neutral	(4) Like- minded	(5) Cross- cutting	(6) Cross- cutting
Listening	0.62*** (.24)	0.57* (.29)	0.51* (.26)	-0.48 (0.43)	-0.95** (0.46)	-0.22 (0.52)
Constant	1.48*** (.12)	1.49*** (.13)	1.51*** (.12)	-1.01*** (0.28)	-0.80** (0.34)	-1.12*** (0.26)

First Stage

Outcome	Listening to Radio			Listening to Radio		
Like-minded	0.39*** (0.05)			0.40*** (0.05)		
Cross-cutting	0.30*** (0.05)			0.30*** (0.05)		
Neutral	0.36*** (0.05)			0.36*** (0.05)		
Constant	0.28*** (0.04)	0.28*** (0.04)	0.28*** (0.04)	0.27*** (0.03)	0.27*** (0.04)	0.27*** (0.03)
N	324	348	299	335	360	314

Notes: The table presents estimates from a TOT analysis of the effect of take up (reported listening to the radio) on interest or participation, instrumented by the experimental treatments. Standard errors are reported in parentheses. Columns 1, 2, and 3 show the estimated effects on interest, measured by a scale generated from questions about perceived importance and excitement. Columns 4, 5, and 6, show the estimated effects on participation, measured by whether subjects signed the SMS petition to parties. Listening is instrumented by the like-minded treatment, the cross-cutting treatment, and the neutral treatment, respectively, for each outcome. The excluded group is always the no-radio control. The first-stage results and the second-stage results predicting interest are based on regression analyses. The second-stage results predicting participation are based on probit analyses. Estimates that can be distinguished from zero are marked as follows: * ≤ 0.10 ; ** ≤ 0.05 ; *** ≤ 0.01 (for two-tailed tests).

Appendix E: Effects of Treatments on Cynicism

	(1) Trustworthiness of Politicians	(2) Ability of Elections to Make Improvements	(3) Likelihood of Politicians Keeping Promises
Like-minded	.24 (.20)	.20 (.24)	.12 (.20)
Cross-cutting	.00 (.19)	.00 (.23)	.10 (.20)
Neutral radio	.14 (.21)	-.16 (.24)	.10 (.21)
Cut points	-.69 (.14)	-1.14 (.16)	-2.40 (.19)
	.71 (.14)	-1.10 (.16)	-.64 (.14)
	2.94 (.21)	-.91 (.16)	1.58 (.16)
		-.83 (.16)	3.91 (.30)
N	658	657	657
Adjusted R²	.00	.00	.00

Notes: Cell entries are ordered logistic regression coefficients. Standard errors are in parentheses. Coefficients that can be distinguished from zero are marked as follows: * ≤ 0.10 ; ** ≤ 0.05 ; *** ≤ 0.01 (for a two-tailed test). The excluded baseline group in the analyses is subjects assigned to the no-radio condition. Analyses only performed on subjects who were major-party partisans and who provided mobile phone numbers.

Appendix F: Effects of Treatments on Partisan Attitude Extremity & Behavior

Note: This appendix is intended for use by editors and reviewers only. Since these results are appearing in a separate forthcoming publication, we are not proposing their inclusion in this publication. They are simply included here to assist editors and reviewers evaluate our claims with regard to the effects of our treatments on attitudinal extremity.

	(1) Own Party minus Other Party	(2) Government Candidates	(3) Opposition Candidates	(4) Never Vote for Other Party	(5) Party Keychain
Like-minded	0.11 (0.12)	0.02 (0.08)	0.10 (0.08)	0.04 (0.22)	- 0.14 (0.21)
Cross-cutting	- 0.38 *** (0.12)	- 0.17 ** (0.08)	- 0.19 ** (0.08)	- 0.41 ** (0.21)	- 0.32 * (0.19)
Neutral radio	- 0.08 (0.12)	- 0.03 (0.08)	- 0.04 (0.08)	0.13 (0.22)	- 0.10 (0.20)
Intercept	1.25 *** (0.08)	2.18 *** (0.05)	2.07 *** (0.05)	- 0.11 (0.15)	
Cut points					- 3.31 *** (0.21)
					- 0.26 *** (0.14)
N	729	742	734	683	744
R-Squared	.03	.01	.02	.01	.00

Notes: Cell entries are OLS regression coefficients for models 1, 2 and 3, logistic regression coefficients for model 4, and ordered logistic regression coefficients for model 5. Standard errors are in parentheses. Coefficients that can be distinguished from zero are marked as follows: * ≤ 0.10 ; ** ≤ 0.05 ; *** ≤ 0.01 (for two-tailed tests). The excluded baseline group in the analyses is subjects assigned to the no-radio condition.