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

DEVRA C. MOEHLER and JEFFREY CONROY-KRUTZ

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 nonpartisan 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 a petition to parties. Partisan media simultaneously increased interest and decreased participation.

Keywords Africa, engagement field experiment, participation, partisan media

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

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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 underwent 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 nonpartisan 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, subjects were subsequently sent a message via short message service (SMS) on their mobile phones, inviting them to send an SMS response to sign a petition, which would be delivered to political parties. We use this component to measure participation.

The results provide support for our theory of how partisan media affect engagement. First, both partisan and nonpartisan 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 43% lower participation rates than those in the control—while like-minded and neutral media had no effect. These results are consistent with other research showing that cross-cutting media moderate attitudes in environments where partisan-motivated reasoning is low (Conroy-Krutz & Moehler, 2015). 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, Barabas, & Clifford, 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, Mazzuca, & Nicther, 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, Leslie, & Ogundimu, 2003;

Nyamnjoh, 2005; Voltmer, 2013). 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; Voltmer, 2013). 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, and we know of no such research in newly liberalized polities.¹ 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; Tworzecki & Semetko, 2012). Some scholars argue that exposure to political content in the media increases interest (Atkin, Galloway, & Nayman, 1976; Lupia & Philpot, 2005; Martin, 2008; Strömbäck & Shehata, 2010), which then fosters participation (Butler & De La O, 2011).² Media often highlight the importance that politics has on well-being. 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, Mendez, & Osborn, 2004; Klofstad, Sokhey, & McClurg, 2013; Lazarsfeld, Berelson, & Gaudet, 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, Johnson, & Murphy, 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 (Conroy-Krutz & Moehler, 2015; 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 (Brundidge, Garrett, Rojas, & Gil De Zúñiga, 2014; Dilliplane, 2011; Wojcieszak, Bimber, Feldman, & Stroud, in press).

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 (Conroy-Krutz & Moehler, 2015).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 the following:

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 sufficient evidence to evaluate our hypotheses, for four reasons. First, much of the research has focused solely on like-minded exposure (DellaVigna & Kaplan, 2007; Hofstetter, 1998; Jamieson & Cappella, 2008; Stroud, 2007). Only a handful of studies have examined how cross-cutting media affect participation, and while they suggest negative effects, results are mixed (Barker, 2002; Brundidge et al., 2014; Dilliplane, 2011; Wojcieszak et al., in press). Second, this scholarship focuses on how partisan media affect participation, but not interest. Third, nearly all 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 endogenous. Fourth, there is no research about partisan media effects on engagement outside of the United States. 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, Collier, & Vicente, 2013; Kuenzi & Lambright, 2007; Nisbet, 2008; Olken, 2009). Results from the United States may not generalize to locations with severe resource constraints, new multiparty systems, and recently pluralized media.

This article 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, pp. 167–170; Hasty, 2005, pp. 33–34). 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 longstanding 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.⁶

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, pp. 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 are therefore more likely to be exposed to a range of media messages, including cross-cutting ones.

In fact, our survey (described later) suggests that regular exposure to various types of messages is common in Ghana. A total of 32% of partisans said they had listened within the past week to the specific cross-cutting morning talk show used in our experiment. In addition, 64% of partisans reported that they had listened to the neutral program (i.e., Peace

FM's *Kokrokoo*).⁷ 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, and subjects had heterogeneous political leanings.⁸ 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 to 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 (a) in natural settings where they are often exposed to like-minded, cross-cutting, and neutral political media; (b) during their quotidian routines, with distractions that individuals normally experience when accessing media; and (c) using actual live broadcasts, rather than material created or repurposed by researchers. Furthermore, 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.⁹ 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 October 16 and November 7, 2012. A total of 1,200 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.¹⁰ In the following subsections 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 a.m. 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.¹¹ 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.¹²

Tro-Tro Route Selection

Two criteria guided route ($N = 58$) selection: (a) expected travel of at least 40 minutes, to facilitate significant treatment exposure, and (b) sufficient vehicles operating during study

Table 1
Partisan bias by station

	Radio Gold (pro-government)	Oman FM (pro-opposition)	Peace FM (neutral)
<i>Panel 1: Percentage of News Stories About Main Parties (NMC 2013)</i>			
NDC (government)	80.1	6.3	39.2
NPP (opposition)	8.1	88.4	32.5
<i>Panel 2: Subjects' Perceptions of Station Bias (Authors' Survey)</i>			
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 1. Panel 2 results based on responses from 1,200 subjects.

hours. Selected routes began at various points and ended at one of nine terminuses.¹³ 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 18 years old who had been in the van for at least 40 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,” and remuneration of two cedis (~\$1.05) was offered.¹⁴ Interviewers were waiting at several points around the terminus. A total of 1,200 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.¹⁵ As Table 2 reports, yields are balanced across treatments, and, as we discuss next, 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.¹⁶ 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.

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 ($\chi^2 = 1.77, p = .62$), 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. A total of 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

Table 2
Balance checks

Variable	Range	Mean Value for Treatment Group				<i>p</i>
		No Radio	Oman	Gold	Peace	
<i>Demographics</i>						
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	2.87	2.88	2.87	3.00	.56
<i>Ethnicity</i>						
Akan	0–1	.48	.44	.48	.52	.32
Ewe	0–1	.22	.27	.21	.19	.08
Ga	0–1	.16	.16	.17	.21	.34
<i>Language Ability</i>						
English	0–3	2.23	2.23	2.27	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
<i>2008 Vote</i>						
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	.12	.81
<i>Radio Listening Habits</i>						
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
<i>Journey Details</i>						
Seat proximity to rear	1–4	2.82	2.80	2.79	2.85	.85
Duration (minutes)	24–110	55.90	51.76	53.88	52.89	.62
Start time (30-minute 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. No comparisons between no-radio control and other treatments were statistically significant at $p < .10$. ** $p < .05$; *** $p < .01$. *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.

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.¹⁷

Measurement

Our independent variables are the treatments transformed to record exposure to like-minded or cross-cutting radio, by virtue of subjects' partisan preference.¹⁸ 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, the 2008 vote is balanced across treatments (Table 2), suggesting that the treatments did not differentially affect responses on average. 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 the 2008 vote would be a good indicator of preferences at the time of our experiment.¹⁹ 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 92%.²⁰ As we note later, it is very unlikely that our findings are due to miscoded partisan preferences.

We use the measure of the 2008 vote and the assigned treatments to create indicators of exposure to a station biased toward (*Like-minded*) or against (*Cross-cutting*) the subject's partisan preferences. Like-minded treatments included subjects exposed to (a) pro-government radio (Gold), for government (NDC) voters, and (b) pro-opposition radio (Oman), for opposition (NPP) voters.²¹ Cross-cutting treatments included subjects exposed to (a) pro-opposition radio (Oman), for government (NDC) voters, and (b) pro-government radio (Gold), for opposition (NPP) voters. The nonpartisan radio treatment (*Neutral*) includes subjects exposed to Peace FM. Our analyses include only those who reported voting for NDC or NPP in 2008 ($N = 752$).²² 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.²³ 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.²⁴ 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 *tro-tros*, please reply by texting YES." A total of 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 (Hanmer, Banks, & White, 2014). We directly observe participation, so there is no possibility of overreporting. We measure how the treatments affect behavior, which is far less susceptible to reporting biases from how subjects wish to be perceived, recall events, react affectively, and the like. 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:

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

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 14.4% higher than those in the no-radio control, while subjects in the cross-cutting treatment had scores that were 9.7% higher than those in the control. We also find that nonpartisan radio increases psychological engagement; subjects in the neutral treatment had scores that were 10.7% higher than those in the control.²⁵ In short, we find significant evidence that all types of political discussion on the radio stimulate interest.²⁶

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 43% lower response rate than those in the no-radio control;

Table 3
Effects of radio treatments on political interest and participation

	Model 1: Interest	Model 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. Interest is measured by a scale from survey questions on the perceived importance of politics and excitement about the campaign, and it ranges from zero to three. Participation is measured by whether subjects signed the petition to parties via SMS. All analyses performed on major-party partisans who provided mobile numbers.

7% of those in the cross-cutting treatment group signed the petition, as compared to 12% 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 article using the same data (Conroy-Krutz & Moehler, 2015), 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 (Supplemental Material). 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 (Conroy-Krutz & Moehler, 2015). 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.²⁷ 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 nonpartisan 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 later signed a petition to parties. Elsewhere, we found that cross-cutting media moderated attitudes through exposure to divergent views (Conroy-Krutz & Moehler, 2015). 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 important contributions to the study of political communication, in the areas of theory, methodology, and policy. First, we theorize that media can have inconsistent effects on two dimensions of engagement: psychological and behavioral. Most

extant literature conflates the two, with many researchers combining them into a single scale. Our finding that media can dampen participation while simultaneously increasing interest highlights the theoretical and empirical need to separate behavioral and psychological components of engagement. While our study focuses on a particular post-liberalization setting, our theory suggests that such divergence between different types of engagement occurs more broadly, and with respect to political phenomena other than media effects. In sum, researchers should be precise in their conceptualization and measurement of engagement.

Furthermore, our theory and findings highlight the need for greater precision in the study of media effects, in developed democracies and elsewhere. Few studies in developed democracies have tested whether partisan media affect engagement, and most of these evaluate only like-minded exposure. And ours is the first study, of which we are aware, to examine the effects of exposure to different types of media on attitudes and behaviors in the developing world. Specifically, we examine the effects of cross-cutting and neutral media alongside like-minded media, which facilitates interpretation of results. Although we find that all types of media catalyze interest in politics, only certain types of messages affected behavior in our study.

Next, our field experimental design could provide a model for other data collection on political communications, in the developed and developing worlds. Many studies on media effects draw upon surveys, 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 or survey experiments, which often have limited ecological validity. Subjects in such 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.

Finally, our findings have normative implications that could be important to those interested in promoting democratic development. On the positive side, our evidence suggests that further encouraging the development of animated, pluralistic media in post-liberalization settings could encourage the emergence of populations that are interested in and excited about politics. Furthermore, these gains can occur from a range of different types of messages, suggesting that communications other than targeted appeals to in-group members and attitude-congruent information can increase psychological engagement. By heightening psychological engagement, media may provide benefits for democratic governance, such as increased scrutiny of 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.

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Supplemental Material

Supplemental data for this article can be accessed on the [publisher's website](#) at . . .

Notes

1. This is especially true for cross-cutting media; only a subset of studies evaluate the effects of both like-minded and cross-cutting exposure on participation (Barker, 2002; Brundidge et al., 2014; Dilliplane, 2011; Wojcieszak et al., in press).

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 (Conroy-Krutz & Moehler, 2015). 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). In addition, 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 (a) reduced the gap in positive attitudes toward one's own party vis-à-vis the other party ($p = .00$); (b) led to more moderate attitudes about candidates from the governing party ($p = .04$); (c) led to more moderate attitudes about opposition party candidates ($p = .02$); (d) decreased aversion to voting for the other side ($p = .05$); and (e) increased selection of the key chain 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 key chains—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 key chain rather than their party's key chain. Please see Conroy-Krutz and Moehler (2015) for the results.

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

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

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

9. As per institutional review board (IRB) guidelines, subjects were debriefed following all data collection.

10. 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.

11. 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.

12. 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 reports provided by the Media Foundation for West Africa). Nonetheless, our partisan stations were the worst offenders.

13. 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.

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

15. 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.

16. Details on the balance checks and variables are in the Supplemental Material.

17. 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, the Supplemental Material 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.

18. See the Supplemental Material for question wordings and for descriptive statistics.

19. The NPP fielded the same presidential candidate—Nana Akufo-Addo—in both races, while the NDC's 2012 flag bearer—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.

20. 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.

21. 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.

22. 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.

23. The Cronbach's alpha for the scale is .65.

24. 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).

25. 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.

26. 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 least the .10 level, but have no effect on intention to talk to an official or vote.

27. 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 ($N = 1,023$). 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 ($b = -.27$, $s.e. = .25$, $p = .29$).

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