Moderation from Bias: A Field Experiment on Partisan Media in a New Democracy

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Abstract
Partisan media are often blamed for polarization in newly liberalized regimes. However, there is little empirical work on the subject, and information-processing theories suggest that extreme position taking is only one possible response to opinionated news. Rather, we theorize that partisan media may cause moderation in post-liberalization settings, because low political sophistication and shifting political landscapes discourage partisan-motivated reasoning. We conducted a field experiment in Ghana in which *tro-tros* (commuter mini-buses) were randomly assigned to one of four conditions. Passengers heard live talk-radio from a pro-government, pro-opposition, or neutral station, or were in a no-radio control. We find no effect of like-minded media on polarization, but significant evidence of moderation from cross-cutting broadcasts, indicating that rival arguments persuaded subjects. Partisan broadcasts also encouraged displays of national over partisan identity. Rather than fueling extremism, we argue that partisan media can moderate by exposing citizens to alternate perspectives.

Moderation from Bias: A Field Experiment on Partisan Media in a New Democracy[1]

How do partisan media affect polarization in newly liberalized regimes? Opinionated media, which often emerge after media liberalization, are frequently blamed for discord and instability. Observers worry that bias polarizes citizens and threatens democracy. However, many democratic theorists argue that exposure to diverse views fosters moderation, tolerance, and compromise (Barber 1984; Habermas 1989; Mill [1859] 1999), which are crucial for progress in polities with histories of authoritarianism. Since individuals are unlikely to encounter opposing attitudes within homogenous social networks, partisan media may be the most prevalent source of alternate perspectives (Gentzkow and Shapiro 2011; Mutz and Martin 2001). According to this reasoning, partisan media may help, rather than harm, democracy and stability.

We theorize that partisan media are likely to have salutary effects in post-liberalization settings, even though observers fear such contexts are especially vulnerable to polarization from partisan media. The dominant perspective on partisan media predicts polarization because it assumes that individuals engage in motivated reasoning, in that they are more persuaded by their own side and/or counter-argue with the other side (Levendusky 2013; Pomerantz *et al.* 1995; Taber and Lodge 1996).[2] We argue, however, that extreme position-taking is an unlikely response to partisan media in post-liberalization settings, which are usually marked by shifting political landscapes and populations with limited political sophistication. Individuals in such settings typically lack the inclination or tools to engage in counter-argument, so they accept, rather than refute, discordant messages. Contrary to expectations in the literature, we posit that partisan media cause moderation, not polarization, in post-liberalization environments.

This article examines the effects of partisan media on attitudes about candidates, as well as on behavioral displays of partisan over national affiliation, in a newly liberalized setting. We evaluate whether exposure to media favoring one's own side (i.e., like-minded exposure) increases

attitudinal and behavioral extremity. Importantly, we also test the effects of content challenging preferences (i.e., cross-cutting exposure), which has received far less attention.

To do so we conducted a novel field experiment weeks prior to the 2012 elections in Ghana, an emerging democracy where many fear that partisan media contribute to polarization. We made use of captive audiences traveling in *tro-tros*, which are minibuses that serve as the most common form of public transport in Ghana. Typically, riders are exposed to radio of the driver's choosing; under our design, drivers played randomly assigned programs. There were four conditions: pro-government, pro-opposition, or neutral political talk radio, or a no-radio control. Upon completing their commute, 1200 subjects from 228 *tro-tros* were interviewed.

We find that partisan media moderate attitudes. There is no difference in attitudinal extremity between subjects exposed to like-minded media and those not exposed to radio. Instead, we find that exposure to cross-cutting broadcasts begets partisan ambivalence and encourages displays of national over partisan affinities. Rather than fueling extremism, our evidence suggests that partisan media provoked reconsideration of initial positions.

Our results have important theoretical and methodological implications for scholarship on partisan media. First, existing theories fail to consider how the effects of partisan media on polarization might vary according to context. While Levendusky (2013) empirically evaluated how individual-level factors affect response to partisan media, scholars have not considered responses in settings where system-level factors, such as institutional flux and weak educational infrastructures, might make motivated reasoning less likely than it seems to be in advanced democracies (specifically, the United States).

Our paper also makes an important methodological contribution. Existing approaches might overestimate the polarizing effects of partisan media. Observational studies risk mistaking selective exposure for media effects, while laboratory experiments can increase counter-arguing if

subjects are more attentive to partisan labels than when they passively consume media on a day-to-day basis. We introduce an alternate approach. Subjects in our field experiment were exposed to live broadcasts in a natural setting, giving the design high external validity, while still maintaining the inferential benefits of random assignment.

This paper proceeds in six sections. The first lays out competing theoretical expectations about the polarizing or moderating potential of partisan media. We then introduce the Ghanaian case. Third, we discuss our experimental design and data collection. The fourth section presents the results of our analyses, which the fifth discusses. We conclude with a discussion of the potential for moderating effects of partisan media in newly liberalized environments.

Theorizing the Effects of Partisan Media in Post-Liberalization Settings

Media liberalization yielded privatized and pluralized environments in most post-authoritarian countries where state-run outlets once dominated. While necessary for enhanced political competition and accountability, many believe these reforms have Janus-faced qualities. Newly established outlets are often owned by politicians or their allies, who use these mouthpieces to propagate their views (Nyamnjoh 2005: 56-9; Snyder and Ballentine 1996).

The predominant view is that biased media exacerbate partisan cleavages, which could add to the rise of anti-democratic elites (Linz 1978; Sartori 1976; Valenzuela 1978), intra-state conflict (Esteban and Ray 1999), and weak economic performance (Frye 2002). Samantha Power wrote of killers in Rwanda who "carried a machete in one hand and a radio transistor in the other" (2001: 89), referencing the anti-Tutsi propaganda by *Radio Télévision Libre des Milles Collines* (RTLM), the first private radio station established after media liberalization. Academics and policymakers typically focus on extreme cases, such as Rwanda, Kenya, and the Balkans, where violence

coincided with indecent broadcasts (Abdi and Deane 2008; IRIN 2008; Palmer 2001; Sofos 1999, Thompson 1999), to surmise the dangers of biased media elsewhere.

The widespread expectation of polarization from partisan media in post-liberalization settings is echoed by most scholars of the subject in the United States (Allen and Moehler 2013; Baum 2012; DellaVigna and Kaplan 2007; Dilliplane 2011, 2014; Jacobson 2010; Jamieson and Cappella 2008; Prior 2007; Stroud 2001; Sunstein 2009; Williams and Delli Carpini 2011). Like-minded media might generate more extreme views by augmenting argument repertoires, boosting confidence in the probity of beliefs, intensifying feelings, and exacerbating in/out-group sentiments (Arceneaux *et al.* 2013; Levendusky 2013). Cross-cutting messages have received far less attention, but the most common expectation is that individuals dismiss or argue against perspectives that are not compatible with their pre-existing beliefs, thus making their initial attitudes even more extreme (Kunda 1990; Lodge and Taber 2000; Redlawsk 2002).

According to this perspective, partisan media polarize because, on average, the reinforcing effects of like-minded exposure are larger than any persuading powers of cross-cutting exposure, and/or cross-cutting exposure provokes counter-argument and intensification of biases. Importantly, this reaction requires some degree of partisan-motivated reasoning, such that individuals find their party's arguments most convincing, ignore discordant views, or argue against the other side's claims (Levendusky 2013). However, this expectation of polarization might not hold universally. Rather, we theorize that partisan media are more likely to moderate in post-liberalization settings. We establish this expectation drawing on two literatures: one on effects of cross-cutting messages, and the other on biased information processing.

First, much of the scholarship on interpersonal discussion across party lines and cross-cutting social networks highlights how such interactions can foster mutual understanding, reevaluation of positions, and moderation (Huckfeldt *et al.* 2004; Klofstad *et al.* 2013; Mutz 2002a,

2002b, 2006; Nir 2011). The empirical findings of this literature, in turn, support a venerable tradition within democratic theory that argues that democracy is strengthened when citizens are exposed to myriad views (Barber 1984; Habermas 1989; Mill [1859] 1999).

Although these arguments about interpersonal contact are rarely applied to partisan media,[3] it is important to recognize that mass media are important sources of exposure to opposing political views (Gentzkow and Shapiro 2011; Mutz and Martin 2001). This is especially true when personal networks are ethnically and politically segregated, as they often are in the post-liberalization settings of the developing world. In such environments, cross-cutting media might be especially influential because they provide novel perspectives, whereas like-minded media duplicate arguments heard elsewhere (Morley and Walker 1987). Furthermore, partisan media might be more effective in delivering alternate perspectives than non-partisan outlets, because partisan media present arguments in captivating, unidirectional, and straightforward ways (Druckman *et al.* 2010; Feldman 2011; Zaller 1992). Strident partisan programming can help citizens pay attention to and understand arguments from the other side.

Cross-cutting media facilitate exposure to alternate views, but moderation will not result if individuals reject or argue against them, as literature on biased information processing expects. Individuals reject discordant views when they are motivated by strong attitudes and attachments, and equipped with arguments to defend their positions (Arceneaux *et al.* 2013; Levendusky 2013; Pomerantz *et al.* 1995; Taber and Lodge 1996; Zaller 1992).[4] Absent these conditions, individuals will be more willing to accept, and less able to reject, discordant messages.

The polarizing effect of like-minded media is also likely contingent on motivated reasoning. When motivated reasoning is high, bias evokes emotions and amplifies group attachments. Affective responses, in turn, reinforce attitudes (Taber and Lodge 2006). Partisans are also attentive to and trusting of congenial media. Their confidence is bolstered when trusted

sources repeat their views on air. When motivated reasoning is low, like-minded media do not grab attention, excite, heighten identification, and lend additional credence to views by virtue of the messenger. Discussions on like-minded media duplicate those in homogeneous social networks, and are uninformative and uninfluential when accuracy goals surpass partisan ones.

We contribute to the information-processing literature by arguing that the likelihood of motivated reasoning varies by political development. The strong priors and levels of political sophistication in advanced democracies are not found in post-liberalization settings, where shifting alliances make politics harder to evaluate (Bielasiak 2002; Ferree 2010; Kuenzi and Lambright 2001; Mainwaring and Scully 1995), and education rates are low. Individuals thus often lack the inclination and tools to resist messages from opponents. Therefore, we expect that individuals are open to persuasion by cross-cutting media in post-liberalization settings. Redundant arguments in like-minded media will not be as potent. Paradoxically, observers are particularly worried about of partisan media in post-liberalization societies, but these settings might actually be most likely to foster moderating media effects, rather than polarizing ones.

This possibility of moderation has been overlooked by the existing scholarship on media effects, which has not incorporated how broader contexts might affect responses to biased messages. And to our knowledge, there have been no studies of how privately owned partisan media affect attitude extremity in post-liberalization settings using survey or experimental evidence.[5] In the next section, we discuss the case of Ghana, where media liberalization has meant a rise in partisan media that many fear are exacerbating inter-party divisions.

Case Background: Partisan Media and Polarization in Ghana

Ghana is a useful case for studying partisan media and polarization in post-liberalization settings. Media liberalization resulted in the emergence of myriad partisan outlets, particularly in

print and FM radio. Most of Ghana's post-independence history was marked by single-party or military rule, under which media were significantly restricted (Asante 1996; Hachten 1971: 167-70; Hasty 2005: 33-4). Multipartyism's return in 1992 was accompanied by the end of the state-run Ghana Broadcasting Corporation's *de jure* radio monopoly (Temin and Smith 2002). By October 2012, 225 FM stations were operating, 70% of which were commercially owned.[6]

These outlets operate in an environment of fierce, albeit mostly peaceful, political competition. Two parties—the National Democratic Congress (NDC) and the New Patriotic Party (NPP)—have dominated the current multiparty regime, and they are quite evenly matched. Partisan control of the presidency and Parliament each changed hands twice (2000, 2008). In 2008 and 2012 the presidential winner enjoyed margins of only 0.5% and 3.0%, respectively.

Media are commonly combatants in this inter-party competition. Station owners often have well-known allegiances and use their broadcasters as partisan mouthpieces. Observers have accused talk radio of fanning partisan animosities and widening cleavages to an extent that it threatens stability (Ghana News Agency 2008). Following the 2008 elections, a Ghanaian professor of communication studies, Dr. Audrey Gadzekpo, warned: "Some of the media houses, especially the FM stations such as Oman FM and Radio Gold… heightened tension and nearly plunged the country into chaos" (*Daily Graphic* 2009). Condemnations of media bias continued into the next election cycle. Former President Jerry Rawlings (1981-2001) warned:

> [O]pen political bias and falsehood have eaten into our media practice… [T]alk radio has led to all sorts of characters with no capacity to discuss issues of national importance being given the opportunity to shout hoarse on our airwaves, throwing abuse and insults and feeding us with shallow arguments ("JJ Blasts Media," *Daily Guide*, 2012).

Representatives from NGOs told us that opinionated content was so problematic that governmental curbs on speech might be warranted. However, despite the widespread belief that biased media are polarizing Ghanaians, evidence of a causal relationship is lacking so far.

Experimental Design and Data Collection

To test the effects of exposure to partisan media on attitude extremity in Ghana, we conducted a field experiment in which we randomly exposed individuals to one of four treatments: two political-talk programs on partisan radio stations (one pro-government, one pro-opposition), one political-talk program on a neutral station, and no radio (the control). Since our subject population included government and opposition supporters, this design allows us to measure the effects of like-minded and cross-cutting messages.

We administered our treatments in *tro-tros*, which are small buses, usually with capacities of 15-20 people.[7] They form the backbone of inter- and intra-city transport in Ghana (Abane 2011), and most of the four million residents of metropolitan Accra regularly travel in them. Although privately owned, they operate more like public transportation in that passengers board whichever *tro-tro* happens to be available, drivers are typically unfamiliar to passengers, and passengers are generally anonymous to one another. Our subject population was morning riders in Accra. Discussions with Ghanaians, and our experience with transport in various African countries (including Ghana), indicated that commuters are captive to drivers' musical or talk-radio preferences. Given that Ghanaians are frequently exposed to like-minded or cross-cutting messages in these settings, *tro-tros* seemed ideal for the administration of treatments.

In the study of media effects, experimental research has advantages because it avoids identification problems that occur with self-selection. Our field-based design also has significant advantages in terms of external validity as compared to previous experimental work, much of which was conducted in laboratories. We exposed subjects to treatments in a setting where they often hear both like-minded and cross-cutting messages; with content created by actual outlets, and in an unobtrusive manner (i.e., without subjects' knowledge that they were being included in a study on media effects). This last feature is particularly important. Alternate designs, such as

laboratory studies, might artificially raise subjects' sensitivity to biased content and source cues (Jerit *et al.* 2013), thus increasing probability of argument against cross-cutting messages and diminishing these messages' persuasive potential. Our design also minimizes Hawthorne effects.

The randomization occurred at the level of the *tro-tro*, such that each van in the study was randomly assigned to one of the four conditions. Thus, all passengers in a given van were *de facto* assigned to that condition. We interviewed 1200 respondents,[8] who rode in 228 vans, plying 58 routes, during 15 days of research (16 October-7 November 2012). The remainder of this section describes the selection of radio treatments, *tro-tro* routes, vans, and respondents, as well as the procedures used to execute and verify random assignment of treatments.

Selection of radio treatments: live, political, popular, and biased broadcasts

Our treatments included live broadcasts, rather than tailored or simulated stimuli. This strategy accounts for real-world variation in programming.[9] Bias can be subtle, and coverage includes discussions of apolitical topics, such as sports or celebrities. If our data do allow us to reject null hypotheses, we can be more confident that similar effects operate beyond our study.

To select the stations, we discussed options with Ghanaian academics, journalists, staff of media-monitoring organizations, and radio directors. Three criteria, in addition to the partisan reputation of the station, guided our selection. First, we sought stations that are often played on *tro-tros* so that subjects would not recognize differences from their normal commutes. Second, although some stations have afternoon, evening, or weekend political programming, weekday mornings are the primary time when talk-radio programs overlap across stations. To facilitate randomization, we focused on stations with political programming between 6 and 10 AM. Third, we selected stations that mainly broadcast in Twi, the *lingua franca* in Accra.

On these bases, we chose Radio Gold as the pro-government station and Oman FM as the pro-opposition station. We chose Peace FM as the neutral station because it also has a lively

morning political-talk show, a large listenership, and a reputation for balance. These programs—*Gold Power Drive* on Gold, *National Agenda* on Oman, and *Kokrokoo* on Peace—contain news, interviews, call-in segments, and commentary. Talk shows often begin with the presentation of a newspaper article or topic. In-studio guests discuss the issue, and then the conversation is opened to listener reactions via phone calls, SMS, and Internet postings. The study was conducted weeks prior to the December 2012 elections, and topics included campaign activities, statements by politicians and their supporters, candidate traits, policies, and current events. Invited guests included journalists, politicians, party agents, issue experts, and analysts.

Although the stations use similar programing formats, they express markedly different opinions. The quasi-governmental National Media Commission (NMC) monitored news stories over several months prior to the 2012 elections, and ranked Gold and Oman as the most biased. The top panel of Table 1 presents the approximate amount of time devoted to coverage of the two main parties in news bulletins, though not in talk shows.[10] The stations are also widely perceived as biased. As reported in the bottom panel of Table 1, the majority of subjects in our experiment identified the bias of the stations, while few reported the opposite bias for our partisan stations.[11] Even radio professionals attested to the editorial nature of the talk-show programs, which they contrasted with news bulletins. The talk-show hosts regularly take positions, guests are often chosen based on their views, and listeners express partisan viewpoints when they join the conversation. In sum, observations by media experts, content analysis of bias, and survey responses from our subjects indicate that our station selections are appropriate.[12]

Selection of tro-tro routes

The first sampling stage involved selecting *tro-tro* routes (N=58). We selected those with 1) an expected minimum travel time of forty minutes to ensure subjects had significant exposure to the treatment, and 2) a sufficient number of *tro-tros* plying the route during our study hours to

facilitate efficient distribution of our staff—some were stationed at departures, while others worked at the destinations. We conducted an enumeration of routes in Accra, with assistants visiting the city's nine main terminuses and interviewing Ghana Private Road Transport Union staff to identify all points that dispatched *tro-tros* to that station on a normal weekday morning. Assistants then visited these points and interviewed drivers about trip duration and ridership. To minimize the probability of individuals being included in our sample twice, or of subjects being in contact with others from earlier days, we never worked on the same route over multiple days.

Recruiting tro-tro drivers, random assignment and administration of treatment

Next, we recruited drivers as our confederates (N=228). We used simple random assignment to determine the treatment for each *tro-tro* before passengers boarded. In return for ten cedis (~$5.26 US), drivers played their assigned station (or if in the control, no station), without interruption, at a volume that would make the broadcast as clear as possible;[13] and without mentioning that they had received such instructions. To ensure these protocols, a staff member (the "recruiter") traveled in the *tro-tro*. Finally, drivers did not turn on the radio until after departure, to minimize the possibility that individuals would hear a certain station, and thus self-select into or out of certain treatments. 49 vans were assigned to the pro-government station, 65 to the pro-opposition station, 55 to the neutral station, and 59 to the no-radio control.

Recruitment of subjects

The last stage involved subject recruitment (N=1200). As *tro-tros* neared destinations, recruiters announced that Ghanaian citizens who were at least eighteen-years old and who had been in the vehicle for at least forty minutes could complete a survey "about your 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."[14] Respondents were promised two cedis (~$1.05 US). Interviewers were waiting at several points near the destination. Contacted *tro-tros* yielded a range

of interviews (1 to 14), with a mean of 5.3 subjects per *tro-tro* (3.3 partisan subjects per *tro-tro*).[15] Yields do not differ significantly by treatment, suggesting condition assignment did not affect willingness to participate (Appendix A).

Verification of random assignment and manipulation check

To examine whether the randomization was successfully executed, we check for statistical balance across treatment groups for observable indicators that were expected not to be affected by the treatment, including: demographics (sex, age, education, wealth); ethnicity; language ability; 2008 vote; radio-listening habits; and *tro-tro* journey details (seat location, duration, start time, interviews per vehicle).[16] Appendix A reports the balance tests between the radio treatment and the no-radio control as well as aggregate balance checks. The number of significant differences is well within the bounds of what is expected due to chance. The results indicate that the randomization procedure was well executed and treatments effects are unlikely to be associated with confounding variables. Differences in subject attitudes and behaviors between the treatment groups can therefore reliably be attributed to the assigned radio treatment.

As a manipulation check, subjects were asked whether the van's radio was playing and, if so, what station.[17] Of those assigned to the control, 75% reported the radio was not playing. Of those assigned to a radio condition, 79% reported a station was playing. Furthermore, 76% of subjects who named a station (and were assigned to a radio condition) correctly named the station. The most common discrepancies were mislabeling partisan stations as neutral, and naming a station other than our three. Only seven subjects assigned to the pro-government station said the pro-opposition station played or vice versa. The high percentage who accurately reported the assigned treatment suggests correct application. Since subjects were not previously told of the experiment, we would expect some to have forgotten what was being played.

Nonetheless, it is important to note that, of those assigned to a radio treatment, 21% said that the radio was not playing, and another 31% did not name a station. Many of these subjects were likely still affected even if they could not identify the specific treatment after the fact. It is also possible that they ignored the treatment. One of the greatest strengths of our design is that individuals were exposed to media in a real-world setting with everyday distractions. Individuals had no contrived reasons to pay attention to the stimulus. If we find significant effects under these conditions, we can be more confident that partisan media are consequential in reality.[18]

Measurement

Our independent variables are the experimental treatments converted to indicate whether subjects were exposed to like-minded or cross-cutting radio, by virtue of their partisan preference. We also include the effects of neutral radio. Here, we measure subject partisan preference as reported vote in the 2008 election.[19] We chose to measure partisan preference in a post-treatment survey so as not to alert subjects to the study prior to the treatment.

Evidence suggests that our measure of partisan preference is valid. First, reported 2008 vote is balanced across conditions, indicating that it was not affected by the treatments (Appendix A). Responses about pre-treatment behavior are less likely to be affected than other possible measures of partisan preferences, such as "closeness" to a party or planned vote in 2012. Second, vote choice tends to be stable over time, so behavior in 2008 is a good proxy for preferences at the time of our experiment. Our sample's partisan proclivities did not change much between 2008 and 2012. Amongst those in the control who reported a preference for 2012, 88% of 2008 NDC voters said they would vote for NDC again; the figure was 92% for NPP.[20] As we explain later, miscoded partisan preferences are unlikely to be responsible for our results.

The measure of partisan preference was combined with the treatments to create measures of exposure to a station biased towards (like-minded) or against (cross-cutting) a subject's 2008 vote.[21] Like-minded treatments included: 1) subjects who voted for the government (NDC) and were exposed to pro-government radio (Gold), and 2) subjects who voted for the opposition (NPP) and were exposed to pro-opposition radio (Oman).[22] Cross-cutting treatments included: 1) subjects who voted for the government (NDC) and were exposed to pro-opposition radio (Oman), and 2) subjects who voted for the opposition (NPP) and were exposed to pro-government radio (Gold). We also create an indicator of neutral exposure. Our analysis includes only those who reported voting for NDC or NPP in 2008 (N=752).[23] Partisan subjects in these three treatment groups are always compared to partisan subjects in the no-radio control.

We have five outcomes: four based on reported attitudes about candidates, and one based on subject behavior. The first measures relative support for a subject's own party as opposed to the other. Subjects were asked in three separate questions if they thought NDC candidates were 1) honest, 2) strong leaders, and 3) capable of developing Ghana; they were asked the same questions about NPP candidates.[24] Responses are combined into a single measure by subtracting mean attitudes about the other party from mean attitudes about one's own party. Higher values indicate polarization (i.e., strongly positive feelings about copartisans and strongly negative feelings about non-copartisans); lower values indicate moderation. Scores range from 3 to -3.[25]

This variable allows us to measure how media affect disparities in relative attitudes,[26] but does not allow us to discern differential effects by party. It is possible that partisan media only change evaluations of incumbent performance or, instead, attitudes about lesser-known opposition candidates. Therefore, we create two additional variables: one measuring attitudes about government party (NDC) candidates and the other attitudes about opposition party (NPP) ones. Using evaluations of candidates' honesty, leadership, and capability, we create folded scales in

which higher values indicate attitudes consistent with the subject's partisan leanings (i.e., positive attitudes if they voted for the party in 2008, negative attitudes if they did not), and lower values indicate attitudes out of line with them (i.e., negative attitudes if they voted for the party in 2008, positive attitudes if they did not). Each scale ranges from 0 to 3.[27]

The fourth outcome is a folded dichotomous variable that measures whether respondents said they would never vote for the opposing party "under any circumstances." NDC supporters were coded one if they mentioned NPP as a party for which they would never vote, and zero if they did not mention NPP. NPP supporters were coded one if they mentioned NDC as a party for which they would never vote, and zero if they did not mention NDC. In short, subjects were coded as more extreme if they said voting for the other party was unthinkable.

The final measure is a behavioral one indicating whether a subject was inclined to display a partisan preference. After the survey subjects were shown three keychains—one prominently displaying the NDC logo, one the NPP logo, and one the Ghanaian flag—and encouraged to select one as a gift. 23% took the NDC keychain, 24% the NPP keychain, and 50% the keychain with the Ghanaian flag. Subjects received a 2 when they took a keychain with their own party logo, 1 for the flag, and 0 for the other party logo. In other words, strong partisan attitudes are revealed when subjects choose to display their own party preference over their national identity.

Results

We compare partisans exposed to like-minded, cross-cutting, or neutral radio to partisans not exposed to radio. The no-radio condition is the excluded category. We estimate the equation:

Extreme attitudes or behavior $= \beta_0 + \beta_1$ *Like-minded* $+ \beta_2$ *Cross-cutting* $+ \beta_3$ *Neutral* $+ \varepsilon$

We run separate analyses for each outcome: 1) relative feelings towards own party *vis-à-vis* the other party; 2) attitudes about NDC candidates; 3) attitudes about NPP candidates; 4) aversion to voting for the other side; and 5) keychain selection.

Positive estimated coefficients on like-minded (β_1), cross-cutting (β_2), or neutral (β_3) radio exposure would indicate that subjects in the relevant treatment had more extreme attitudes in line with their party preference than those in the no-radio condition. Negative estimated coefficients would indicate that subjects exposed to the relevant radio treatments had less-extreme attitudes than subjects in the no-radio condition. Thus, positive estimated coefficients indicate polarization due to the treatments, and negative coefficients indicate moderation.

Table 2 reports results from two-tailed tests of treatment effects on attitudes about one's own party relative to the other, NDC and NPP candidates, willingness to vote for the other side, and keychain selection. The first row indicates that exposure to like-minded radio is not significantly related to attitude extremity or behavioral change. It seems that listeners are not affected when they are exposed to media matching their partisan leanings.

Importantly, the results in the second row reveal that cross-cutting media did significantly affect attitudes. The estimated coefficients are negative, indicating that cross-cutting radio moderated attitudes and reduced partisan polarization. The negative coefficient on the first dependent variable suggests that cross-cutting media reduced the gap in positive attitudes towards one's own party *vis-à-vis* the other party, while the second and third suggest that these moderating effects are general, and not a function of changing attitudes towards just the governing or opposition party. Effects are not limited to retrospective evaluations of government, nor to assessments of lesser-known challengers. Cross-cutting exposure also decreased aversion to voting for the other side. Furthermore, the negative estimated coefficient on party keychain suggests that cross-cutting radio induced respondents to prefer the symbol of national identity over partisan identity.[28] On balance, exposure to cross-cutting radio reduced partisan cleavages by encouraging moderation.

Finally, the third row in Table 2 indicates that neutral political-talk radio had no significant effect on partisan attitudes or behavior. This suggests that cross-cutting exposure, and only cross-cutting exposure, produces the moderating effect.

Figure 1 illustrates these results. The height of each bar represents the difference of means between the relevant type of radio exposure and the no-radio condition. As in the analyses above, the outcomes are depicted so that positive values indicate greater extremity of attitudes and action in the radio treatments than in the no-radio group, and negative values indicate weaker ones. Note that the cross-cutting treatment has negative estimated values across all five dependent variables, which indicates that subjects exposed to cross-cutting radio have more moderate attitudes than co-partisan subjects who were not exposed to radio of any kind.

Our conclusions are not the product of a particular estimation procedure, model specification or variable construction. The results remain largely the same when we cluster standard errors by *tro-tro*, estimate the effect of like-minded or cross-cutting media relative to neutral radio (rather than the no-radio control), and control for subject ethnicity.[29] And, results hold when we construct a global scale of measured attitudes and behavior together.[30]

Moreover, the null results for like-minded and neutral media are not likely due to ceiling effects, sample size, or duration outliers. First, most subjects scored below the maximum on all outcomes, indicating that they had space to become more extreme.[31] Second, the estimated effects are substantively close to zero. Even with a larger sample, and any associated reduction in standard errors, we expect null results. Third, the null results are not a product of particularly short dosages. Like-minded and neutral media still have insignificant effects on all five outcomes when excluding rides of 35 minutes or less (one standard deviation below the mean).[32]

Next, it seems highly unlikely that the results are due to error with respect to our post-treatment measure of partisan preference. First, as mentioned earlier, reported vote choice in 2008

is balanced across the treatment groups, indicating that the treatment is unlikely to have affected our measure of partisan preference. Second, we can think of no reason why moderate listeners would report having voted against the party favored by their treatment station. Third, the main effects of the radio treatments are significant and in line with what we would expect based on the results of the like-minded and cross-cutting analysis. On average, pro-government radio increased support for government officials while opposition radio increased support for opposition politicians. Each partisan station shifted attitudes in the direction of the media bias. Crucially, the main effects analyses do not depend on the measure of partisan preferences.

Finally, we also cannot think of a scenario whereby treatment-induced differences in who agreed to be interviewed would generate these effects. *Tro-tro* yields were balanced across treatments. For subject composition to be responsible, cross-cutting exposure would have had to encourage moderates to answer a survey about transport and discourage equal numbers of extremists. Observables are also balanced, so moderates encouraged and extremists discouraged would have had to be equivalent in demographics, partisanship, radio habits, and journey details. Although we cannot definitively rule out the possibility that the treatments induced different types of individuals to agree to the survey, such a scenario is unlikely. In sum, we expect that the results are of real-world import and not artifacts of our research design, model, or analysis.

Discussion

Before concluding, we consider why our findings in Ghana differ from the expectations of extant literature, and their generalizability. First, the dominant perspective expects that partisan media polarize because it assumes that most individuals engage in motivated reasoning by internalizing arguments from their own side and arguing against discordant views. However, the incidence of partisan-motivated reasoning may be atypically high in the United States, where most

research on partisan media has been conducted. Strong psychological attachments to parties as social groups (Green *et al.* 2002) mean that US partisans have strong inclinations to find validation in familiar arguments and dismiss information that might threaten their internalized identity. This dissonance reduction strategy can lead to widespread perceptions that cross-cutting sources are untrustworthy, further increasing antipathy to the other side. Additionally, education levels and access to political information facilitate counter-argument.

Such conditions do not hold in many post-liberalization settings, including Ghana. The multiparty system there is young, meaning that partisan identities, reinforced over generations in the US, are relatively weak. Thus, Ghanaians do not possess the same inclinations to distrust, and therefore argue against, cross-cutting sources. Only 27% of partisan subjects in the control said that they had no trust in the cross-cutting station, and 38% said they would like listening to the cross-cutting station during their morning commute. Finally, due to low school enrollment, many lack the political sophistication necessary to counter-argue with cross-cutting messages, even if so inclined.[33]

The evidence is consistent with our theory that moderation occurs when motivated reasoning is low. Cross-cutting media moderate the attitudes of less politically knowledgeable subjects, but not of those who are knowledgeable and thus better equipped to resist persuasion from uncongenial sources (Appendix E). We use a scale measuring knowledge about the number of MPs, the minister of finance, and ECOWAS. Non-sophisticates are those with below-average knowledge (n=398), and sophisticates are those with above-average knowledge (n=354).[34] For non-sophisticates, cross-cutting media significantly decrease fondness for one's own party *vis-à-vis* the other (p=.01), extremity of attitudes about opposition candidates (p=.03), aversion to voting for the other side (p=.03), party keychain selection (p=.03), and the global scale (p=.01). The estimated effect on attitudes about government candidates is also negative, but not significant

(p=.11). In contrast, cross-cutting media had no significant effect on sophisticates. However, we cannot say with confidence that motivated reasoning moderates partisan media effects because the interaction terms between the treatments and knowledge are generally not significant. Furthermore, our scale for knowledge proxies only part of what generates motivated reasoning. We cannot test whether partisan identification moderates effects because we lack a pre-treatment measure of partisan strength. We also cannot evaluate whether experience with multiparty politics matters, since democracy is equally new for all subjects.

Next, we consider whether the findings are likely to be consequential in reality. Our research satisfies two conditions for real-world applicability: 1) the causal processes identified are at work beyond the experiment; and 2) the stimuli administered occur frequently in reality. First, our design replicates actual conditions by exposing subjects to live broadcasts in an unobtrusive way amongst everyday distractions. Other commonly used methods—surveys and laboratory experiments—are more likely to overestimate the polarizing effects of partisan media. Observational studies risk conflating selective exposure with media effects. Those most prone to extremism are also those most likely to consume partisan media. Laboratory experiments might overestimate polarization by inducing counter-argument at higher-than-typical levels. Subjects will be more attentive to partisan cues and biases when they are insulated from distractions and when they know they are being observed. Attentive subjects are more likely to identify and react negatively to cross-cutting media than they do when passively consuming media on a day-to-day basis. Our subjects were unaware that they were exposed to, or questioned about, experimental treatments. We were able to measure how individuals typically react to incidental media exposure, while still maintaining the benefits of experimental inference.

Second, the moderating effects of cross-cutting exposure are of real-world import, because it seems that individuals are exposed to media from the other side on a regular basis. We are not

aware of representative surveys measuring cross-cutting media exposure in the developing world, but there are theoretical and empirical reasons to expect that it is common.

From a theoretical standpoint, selective exposure is one outcome of motivated reasoning (Iyengar and Hahn 2009; Stroud 2011). Given that we find little evidence of partisan-motivated information processing, we expect limited selective exposure; individuals lack the sophistication and motivation to select media based on partisan preferences. Second, we expect that conditions in the developing world beget incidental exposure. Individuals spend substantial time in public or semi-public settings where they are exposed to media not of their choosing (Nyamnjoh 2005: 16-17). For example, most individuals travel within and between population centers by *tro-tros* rather than by private vehicles (Abane 2011). Selective avoidance in such settings is difficult.

Empirically, our data suggest considerable exposure to cross-cutting media amongst our subjects. 32% of partisans in the control said they listened to the cross-cutting program at least a few times in the previous week. This figure likely underestimates how often subjects heard discordant messages, since it does not include less-frequent exposure (once a week or less), nor does it account for exposure to the myriad partisan programs and stations beyond the two morning shows included in our survey.[35] More research is needed, but what relevant evidence we have suggests that cross-cutting exposure is common enough to be consequential.

How well might these conclusions from Ghana generalize to other contexts? While we cannot be certain about generalizability beyond our experimental setting without additional research, we can develop informed expectations. We anticipate that counter-argument with cross-cutting messages would be lowest, and the persuasive effects of cross-cutting media would be greatest, where partisan identities are weak (Levendusky 2013; Taber and Lodge 2006). Citizens should be especially susceptible to the moderating influence of cross-cutting media in settings with less-institutionalized party systems, such as Benin, Bulgaria, Guatemala, Latvia, Madagascar,

Malawi, Mali, Niger, Peru, and Senegal. Moderation might therefore be the most likely outcome of partisan media exposure in most post-liberalization settings.

Importantly, we do not expect the Ghanaian results to generalize to all post-liberalization settings. While partisan voting occurs in Ghana, it is far from absolute (Fridy 2007; Lindberg and Morrison 2005, 2008; Weghorst and Lindberg 2013). In contrast, group differences have been reified by violence in places such as Kenya, Mozambique, and Zimbabwe (LeBas 2011; Levitsky and Way 2012). In these situations, exposure to cross-cutting media might be less common due to increased self-selection. And when cross-cutting exposure does occur, the solidified identities prevalent in post-conflict settings might mitigate against moderating effects. In sum, we expect that most citizens in post-liberalization settings will be susceptible to the persuasive (and thus moderating) effects of cross-cutting media, but such salutatory effects are unlikely where conflict has widened political divides. Unfortunately, the polities most in need of moderation might also be immune to the potential benefits of exposure to cross-cutting media.

Conclusion

Throughout much of Africa, Asia, Latin America and Eastern Europe, liberalization ushered in media systems dominated by partisan outlets. Many worry that opinionated media polarize citizens and threaten democracy. Extant scholarship, based primarily on the US, similarly predicts that partisan media lead to more extreme views. Scholars assert that partisan media polarize because individuals readily accept their own side's arguments, while dismissing or arguing against the other's (Levendusky 2013). We argue that an alternate theoretical framework is more useful where conditions are unfavorable to partisan-motivated reasoning. Partisan media can moderate attitudes when individuals are open to persuasion by arguments from the other side.

While an established literature cites the democratic benefits of cross-cutting interpersonal communications, the same logic can be applied to the study of partisan media.

To evaluate the effect of partisan media in a newly liberalized polity, we conducted a novel field experiment in Ghana in which commuter mini-buses were randomly assigned to one of four conditions. Passengers heard live talk-radio from a pro-government, pro-opposition, or neutral station, or were in a no-radio control. We find that partisan media moderated attitudes because exposure to like-minded and neutral media had no estimated effect on attitudes, while cross-cutting media decreased extreme positions. Cross-cutting broadcasts also encouraged displays of national over partisan affinities. Rather than fueling extremism, our evidence indicates that partisan media tempered attitudes by exposing citizens to alternate perspectives.

The study has important methodological and theoretical implications. First, our design offers an important innovation in its administration of live media treatments in a natural, unobtrusive manner. This approach decreases the likelihood of lab-induced hypersensitivity to bias and source cues while maintaining the inferential benefits of random assignment. Researchers should reconsider how much motivated reasoning occurs in real-world settings.

Second, we argue that a theory of partisan media should be attentive to contextual factors. Partisan media can have opposite effects depending on whether political and demographic environments favor motivated reasoning. While existing theory and evidence from the US context suggests that partisan media polarize, we found the reverse in a newly liberalized polity. We theorize that moderating effects are likely in most, though not all, newly liberalized polities. Low levels of political sophistication, shifting political alliances, and homogeneous social networks mean that unfamiliar arguments from cross-cutting media may be especially persuasive. In many hybrid regimes and new democracies, partisan media may be more likely to help democracy by moderating attitudes, than to harm democracy by fueling extremism.

[2] An additional consequence of motivated reasoning is selective exposure (Stroud 2011), which we note in the discussion section. Our study is designed to measure effects of exposure, rather than one's choice of media. Nonetheless, cross-cutting exposure is seemingly common in post-liberalization settings, as we discuss with reference to implications of our results.

[3] Several scholars mention that cross-cutting media can persuade at times, but they theorize that, on average, cross-cutting media persuade less than like-minded media, or that partisan media persuade irrespective of partisanship (see Dilliplane 2014; Druckman and Parkin 2005; Feldman 2011; Levendusky 2013). We theorize that, in post-liberalization settings, cross-cutting media should be more influential than like-minded media, and cross-cutting media can persuade even when like-minded media do not. In short, other scholars expect polarization or parallel effects, on average, while we posit that moderation is also a plausible outcome for most individuals.

[4] However, we lack empirical evidence on inter-individual differences in responses to partisan media in the United States (for an exception, see Levendusky 2013).

[5] Two studies on Rwanda come to differing conclusions about the role of the media in violence (Straus 2007; Yanagizawa-Drott 2010), but neither includes individual-level analysis of attitudes.

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

[7] Analogues include the *car rapide* in Senegal, *danfo* in Nigeria, *dala dala* in Tanzania, *dolmuş* in Turkey, *jeepney* in the Philippines, *louage* in Tunisia, *matatu* in Kenya and Uganda, and *tap tap* in Haiti. Vehicles usually follow fixed routes, but not schedules, departing only when full.

[8] We include only the 752 partisan subjects in our sample for analyses, because only partisans can be coded for like-minded and cross-cutting exposure.

[9] Live broadcasts also ensured that subjects would not be alerted to the experiment.

[10] The NMC monitored only news bulletins. We present these data to indicate station bias rather than the content of programs under investigation. Professionalism dictates neutral presentation of news, and bias results primarily from greater coverage of a favored party's campaign rallies, statements, and the like. In contrast, talk shows use tone to express editorial positions.

[11] Appendix B reports the wordings for all the survey questions in this article.

[12] We focus on partisan bias, not inflammatory language. Content analysis by Media Foundation for West Africa shows that "indecent expressions" constituted a small proportion of total political speech (calculations from MFWA 2012).

[13] We required that all vehicles had working sound systems, even if assigned to the control.

[14] Many questions concerned transport. The instrument was translated into English, Ga and Twi.

[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.

[16] See Appendix A for justifications for balance checks and Appendix B for question wordings.

[17] These questions were asked at the end of the survey (as part of a set of questions about the *tro-tro* ride), so as to avoid priming subjects on the study's purpose.

[18] In other words, we evaluate effects for all subjects using an intention-to-treat (ITT) analysis, rather than for only those who listened (i.e., treatment-on-the-treated (TOT)). We designed the study to determine real-world impact, but we could not also measure listenership. We can identify some active listeners as those who correctly identified stations, but we cannot distinguish passive listeners and those with poor recall from non-listeners. Thus, we cannot isolate listeners. A TOT

analysis that codes only active listeners as treated would be problematic if active listeners were more inclined to engage in counter-argument and reject rival broadcasts.

[19] See Appendix B for question wordings and Appendix C for key variable descriptive statistics.

[20] The correlation between 2008 vote and intended 2012 vote in the control group is strong (r=.77, p=.00 for 2008 NDC voters, r=.74, 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.

[21] Use of variables coded as like-minded and cross-cutting is common (Arceneaux *et al.* 2012; Dilliplane 2011, 2014; Klofstad *et al.* 2013; Levendusky 2013; Nir 2011).

[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 they were assigned to the neutral or no-radio conditions.

[24] Higher values indicate ratings of candidates from the party as extremely honest, strong, and capable; lower values indicate ratings of candidates as not at all honest, strong, or capable.

[25] The absolute lowest (i.e., negative) values represent individuals who have favorable attitudes about the other party, and negative attitudes about their own party. Only 6% of subjects in the control group registered negative values.

[26] Measures of affective polarization are common in survey research, and have also been used in experiments on polarization on the U.S. (for examples, see Prior 2007 and Levendusky 2013).

[27] For example, for the NDC scale, 2008 NDC voters were coded highest if they rated NDC candidates as extremely honest, strong, and capable, and lowest if they rated NDC candidates as not at all honest, strong, and incapable. 2008 NPP voters were coded highest if they rated NDC

politicians as not at all honest, strong and capable, and lowest if they rated NDC candidates as extremely honest, strong, and capable. Cronbach's alphas for the scales are both 0.87.

[28] We also estimated the model with attitudes about the president as an outcome. The directions of effects were consistent but were not statistically significant. Attitudes about the president are plausibly more stable than attitudes about candidates generally. (See working paper by authors).

[29] See Appendix D for the results and justifications for these checks. The only notable differences are: the effect of cross-cutting radio on keychain selection falls just beyond conventional levels of significance when including the clustered standard errors (p=.116), and cross-cutting radio is not significantly different from neutral radio for this measure.

[30] The global scale is constructed by summing four measures: attitudes about the government candidates; attitudes about opposition candidates; never vote for the other party; and keychain selection (Cronbach's alpha = .61). Results reported in this paper are from a scale that does not include the first outcome--own party minus other party--so that attitudes about candidates are not given undue weight. However, the results are robust regardless of the measures used.

[31] The mean values amongst partisans in the control are: 1.15 (out of 3) for own minus other-party; 2.18 (out of 3) for government candidates; 2.07 (out of 3) for opposition candidates; 0.31 (out of 1) for aversion to voting for another party; and 1.54 (out of 2) for keychain.

[32] Also, cross-cutting effects are not driven by particularly lengthy rides; results hold even when excluding rides over 70 minutes (one standard deviation above the mean).

[33] Only 46% of Ghanaians attended secondary school (World Development Indicators, 2009).

[34] The scale is balanced across treatments, which is expected given the questions we asked.

[35] Some suggest that cross-cutting exposure is common even in the US (Garrett *et al*. 2013; LaCour 2013; Levendusky 2013), though its extent is a subject of debate.

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Table 1: Partisan Bias by Station

Panel One: Percentage of News Stories about Main Parties (National Media Commission 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: Effects of Treatments on Attitude Extremity & Behavior

	(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: $* \leq 0.10$; $** \leq 0.05$; $*** \leq 0.01$ (for two-tailed tests). The excluded baseline group in the analyses is subjects assigned to the no-radio condition.

Figure 1: Difference of Means between Radio Treatments and No Radio
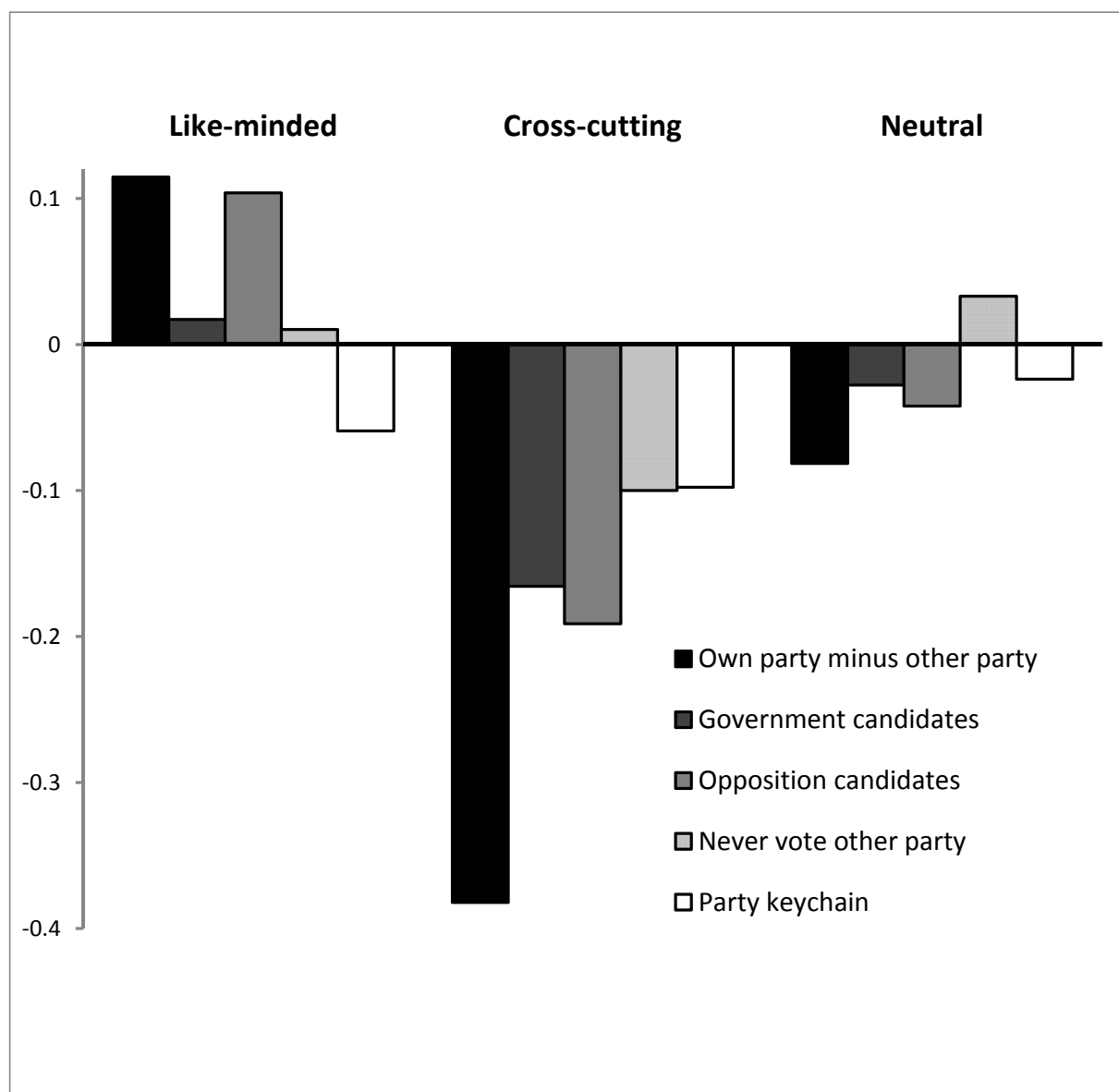
Notes: Positive bars indicate more extreme attitudes and selection of partisan keychain in the treatment conditions relative to the no-radio control condition. Negative bars indicate more moderate attitudes in the treatment conditions relative to the no-radio control condition.

Supplemental Materials

Appendix A: Balance Check Results and Justification

Variable	Range	Mean Value for Treatment Group				p
		No Radio	Oman	Gold	Peace	
Demographics						
Sex	0-1	.33	.38	.36	.36	.79
Age	18-84	33.16	33.36	33.21	31.77	.42
Education	1-10	5.65	5.59	5.68	5.67	.62
Wealth index	0-5	3.17	3.17	3.17	3.06 *	.69
Ethnicity						
Akan	0-1	0.48	0.44	0.48	0.52	.41
Ewe	0-1	0.22	0.27	0.21	0.19	.12
Ga	0-1	0.16	0.16	0.17	0.21	.45
Language Ability						
English	0-3	2.23	2.23	2.28	2.27	.83
Twi	0-3	2.39	2.44	2.51 *	2.48	.42
Ewe	0-3	0.86	1.00	0.80	0.86	.66
Ga	0-3	1.40	1.52	1.44	1.58	.44
2008 vote						
Voted 2008	0-1	0.75	0.75	0.75	0.70	.57
Voted NDC	0-1	0.39	0.43	0.40	0.40	.80
Voted NPP	0-1	0.33	0.28	0.33	0.27	.37
Refused response	0-1	0.11	0.14	0.12	0.19	.89
Radio listening habits						
General frequency	0-3	2.40	2.35	2.34	2.41	.24
Peace morning show	0-3	1.17	1.10	1.11	1.20	.28
Gold morning show	0-3	0.59	0.68	0.64	0.62	.25
Oman morning show	0-3	0.71	0.72	0.65	0.75	.46
Journey details						
Seat proximity to rear	1-4	2.28	2.80	2.79	2.85	.51
Duration (minutes)	24-110	55.90	51.76	53.88	52.89	.60
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	.70

Notes: Statistically significant t-tests comparing group means for the no-radio control and other treatments are marked as follows: * *p*<.10; ** *p*<.05; *** *p*<.01. Right-hand column reports *p* values for tests of relationships between variables of interest and treatment categories. Chi-square tests are conducted for categorical and dummy variables; Kruskal-Wallis tests are conducted for ordinal and continuous variables. For variables measured at the individual level, all tests take into account clustering at the *tro-tro* level.

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 are 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

Parties For Which Subject Would Never Vote?
[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.]*

Opinions of Government (NDC) Candidates
[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?

Opinions of Opposition (NPP) Candidates
[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?

Keychain Selection
[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.]*

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

[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?

Regular Exposure to and Attitudes towards Like-Minded and Cross-Cutting Radio

[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?]*

[54] In general, how much do you trust and have confidence in [*radio station*] when it comes to reporting the news fully, accurately, and fairly? A great deal, a fair amount, not very much, or none at all? A) Radio Gold? B) Peace FM? C) Oman FM?

[62] How much would you like to listen to the following radio stations during your morning *tro-tro* commute? Would you like it a lot, like it somewhat, dislike it somewhat, or dislike it a lot? A) Oman FM? B) Radio Gold? C) Peace FM?

Other Variables

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?

Knowledge

[43] Can you tell me how many seats there are currently in the Parliament of Ghana?

[44] Do you happen to know what job or political office is now held by Dr. Kwabena Duffour?

[45] What does the acronym ECOWAS stand for?

Appendix C: Descriptive Statistics of Key Variables

Variable	Mean	Std. Dev.	Min	Max	N
Dependent Variables					
Own party minus other party	1.15	1.18	-3	3	729
NDC candidates scale (folded)	2.13	.790	0	3	742
Honest (folded)	2.08	.861	0	3	747
Strong leader (folded)	2.12	.923	0	3	747
Development capability (folded)	2.16	.890	0	3	748
NPP candidates scale (folded)	2.03	.792	0	3	734
Honest (folded)	2.03	.822	0	3	745
Strong leader (folded)	2.02	.934	0	3	741
Development capability (folded)	2.03	.903	0	3	742
Refusal to vote for opposing party (1=Indicated refusal, 0=Did not indicate refusal)	.455	.498	0	1	683
Keychain selection (0=Other party, 1=Flag, 2=Own party)	1.49	.575	0	2	744
Party Preference Measure (2008 vote) (1=NDC=1, 0=NPP)	.573	.495	0	1	752

Notes: Folded measures recoded such that higher values represent favorable attitudes towards co-partisans and negative attitudes towards non co-partisans. For party preference measure, votes for minor parties, refusals, and reported non-votes excluded.

Appendix D: Robustness Checks

	Baseline			*Tro-Tro* Clustered SE			Neutral Radio			Ethnic Controls		
	β	SE	P-value	B	SE	P-value	β	SE	P-value	β	SE	P-value
Own party minus other party (OLS regression)												
Like-minded treatment	0.11	0.12	0.32	0.11	0.12	0.33	0.20	0.12	0.10	0.12	0.12	0.31
Cross-cutting treatment	-0.38	0.12	0.00	-0.38	0.12	0.00	-0.30	0.13	0.02	-0.39	0.12	0.00
Neutral treatment	-0.08	0.12	0.48	-0.08	0.11	0.46	-0.08	0.12	0.48	-0.08	0.12	0.49
No radio treatment												
Ethnic Akan										0.06	0.14	0.68
Ethnic Ewe										0.00	0.15	0.99
Ethnic Ga										-0.13	0.16	0.40
Intercept	1.25	0.08	0.00	1.25	0.07	0.00	1.25	0.09	0.00	1.24	0.14	0.00
Government (NDC) candidates (OLS regression)												
Like-minded treatment	0.02	0.08	0.83	0.02	0.08	0.83	0.04	0.08	0.58	0.02	0.08	0.84
Cross-cutting treatment	-0.17	0.08	0.04	-0.17	0.08	0.04	-0.14	0.08	0.10	-0.17	0.08	0.03
Neutral treatment	-0.03	0.08	0.73	-0.03	0.07	0.71				-0.01	0.08	0.90
No radio treatment							0.03	0.08	0.73			
Ethnic Akan										-0.21	0.09	0.01
Ethnic Ewe										-0.01	0.09	0.95
Ethnic Ga										-0.14	0.10	0.17
Intercept	2.18	0.05	0.00	2.18	0.05	0.00	2.15	0.06	0.00	2.30	0.09	0.00
Opposition (NPP) candidates (OLS regression)												
Like-minded treatment	0.10	0.08	0.19	0.10	0.08	0.21	0.15	0.08	0.08	0.11	0.08	0.18
Cross-cutting treatment	-0.19	0.08	0.02	-0.19	0.08	0.02	-0.15	0.08	0.08	-0.19	0.08	0.02
Neutral treatment	-0.04	0.08	0.60	-0.04	0.07	0.55				-0.06	0.08	0.46
No radio treatment							0.04	0.08	0.60			
Ethnic Akan										0.25	0.10	0.02
Ethnic Ewe										-0.01	0.11	0.89
Ethnic Ga										-0.03	0.12	0.79
Intercept	2.07	0.05	0.00	2.07	0.04	0.00	2.03	0.06	0.00	1.96	0.10	0.00

	Baseline			*Tro-Tro* Clustered SE			Neutral Radio			Ethnic Controls		
	B	SE	P-value	B	SE	P-value	B	SE	P-value	β	SE	P-value
Never vote for other party (Logistic regression)												
Like-minded treatment	0.04	0.22	0.85	0.04	0.22	0.85	-0.09	0.23	0.69	0.04	0.22	0.86
Cross-cutting treatment	-0.41	0.21	0.05	-0.41	0.20	0.04	-0.54	0.22	0.02	-0.41	0.21	0.05
Neutral treatment	0.13	0.22	0.55	0.13	0.22	0.55				0.12	0.22	0.58
No radio treatment							-0.13	0.22	0.55			
Ethnic Akan										0.12	0.26	0.65
Ethnic Ewe										0.02	0.28	0.96
Ethnic Ga										-0.01	0.30	0.98
Intercept	-0.11	0.15	0.47	-0.11	0.14	0.47	0.03	0.17	0.87	-0.16	0.26	0.53
Party keychain (Ordered Logistic regression)												
Like-minded treatment	-0.14	0.21	0.52	-0.14	0.23	0.55	-0.04	0.22	0.87	-0.14	0.21	0.52
Cross-cutting treatment	-0.32	0.19	0.10	-0.32	0.20	0.12	-0.22	0.20	0.29	-0.32	0.19	0.10
Neutral treatment	-0.10	0.20	0.62	-0.10	0.20	0.63				-0.10	0.20	0.61
No radio treatment							0.10	0.20	0.62			
Ethnic Akan										0.00	0.24	0.99
Ethnic Ewe										-0.08	0.26	0.75
Ethnic Ga										-0.09	0.29	0.76
Cut 1	-3.31	0.21		-3.31	0.23		-3.22	0.22		-3.35	0.30	
Cut 2	-0.26	0.14		-0.26	0.14		-0.16	0.15		-0.30	0.25	
Global partisan attitudes and behavior scale (OLS regression)												
Like-minded treatment	0.04	0.10	0.70	0.04	0.11	0.71	0.03	0.10	0.74	0.04	0.10	0.71
Cross-cutting treatment	-0.25	0.10	0.01	-0.25	0.10	0.01	-0.26	0.10	0.01	-0.26	0.10	0.01
Neutral treatment	0.00	0.10	0.97	0.00	0.10	0.97				0.00	0.10	0.98
No radio treatment							0.00	0.10	0.97			
Ethnic Akan										0.02	0.12	0.85
Ethnic Ewe										-0.01	0.13	0.94
Ethnic Ga										-0.11	0.14	0.44
Intercept	2.67	0.07	0.00	2.67	0.06	0.00	2.67	0.07	0.00	2.68	0.12	0.00

Notes: The excluded baseline group in the "baseline," "*tro-tro*-clustered standard errors," and "ethnic controls" models are subjects assigned to the no-radio condition. The excluded baseline group in the "neutral radio" model includes subjects assigned to the neutral radio condition. P-values are for two-tailed tests.

Explanation for robustness checks

First, we cluster standard errors to account for randomization at the level of the *tro-tro*.

Second, we use neutral radio as the excluded category to provide suggestive evidence about effects of bias separately from political discussion. Our baseline has the no-radio control because we cannot be certain that the content of the neutral station is actually perfectly neutral for all subjects. No radio is a "cleaner" control and theoretically closer to the counterfactual because Ghanaians might not listen to neutral political radio if partisan radio did not exist.

Third, we include ethnic controls to demonstrate that observable traits do not affect the results. We provide the results for ethnicity because ethnicity is strongly related to political attitudes in Ghana. The results are similar with other controls.

Fourth, we create a scale of the four outcome variables (Cronbach's alpha=0.61) to provide a holistic assessment of the effects of the treatments.

Appendix E: Results by High and Low Political Sophistication

	(1) Own Party minus Other Party	(2) Government Candidates	(3) Opposition Candidates	(4) Never Vote for Other Party	(5) Party Keychain	(6) Scale of Four Outcomes
Low Knowledge						
Like-minded	0.05 (0.16)	0.00 (0.11)	0.07 (0.11)	− 0.21 (0.30)	− 0.46 (0.29)	− 0.12 (0.14)
Cross-cutting	− 0.48 *** (0.17)	− 0.18 (0.12)	− 0.24 ** (0.11)	− 0.62 ** (0.28)	− 0.59 ** (0.27)	− 0.38 *** (0.14)
Neutral radio	− 0.08 (0.17)	0.01 (0.12)	− 0.06 (0.12)	0.33 (0.31)	− 0.40 (0.29)	0.00 (0.14)
Intercept	1.24 *** (0.11)	2.17 *** (0.07)	2.05 *** (0.08)	0.040 (0.20)		2.77 *** (0.09)
Cut points					− 3.30 *** (0.29)	
					− 0.68 *** (0.2)	
N	385	390	390	362	393	348
R-Squared	0.03	0.01	0.02	0.02	0.01	0.03

	(1) Own Party minus Other Party	(2) Government Candidates	(3) Opposition Candidates	(4) Never Vote for Other Party	(5) Party Keychain	(6) Scale of Four Outcomes
High Knowledge						
Like-minded	0.18 (0.16)	0.03 (0.11)	0.14 (0.12)	0.33 (0.32)	0.23 (0.31)	0.23 (0.14)
Cross-cutting	− 0.27 (0.17)	− 0.14 (0.11)	− 0.13 (0.12)	− 0.16 (0.31)	− 0.01 (0.28)	− 0.11 (0.14)
Neutral radio	− 0.09 (0.16)	− 0.07 (0.11)	− 0.03 (0.11)	− 0.02 (0.32)	0.26 (0.29)	0.04 (0.14)
Intercept	1.25 *** (0.11)	2.18 *** (0.08)	2.09 *** (0.08)	− 0.27 (0.22)		2.54 *** (0.10)
Cut points					− 3.43 *** (0.34)	
					0.21 (0.20)	
N	310	352	344	362	321	351
R-Squared	0.02	0.01	0.02	0.02	0.01	0.00

Notes: Cell entries are OLS regression coefficients for models 1, 2, 3 and 6; 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: $* \leq .10$; $** \leq .05$; $*** \leq .01$ (for two-tailed tests). The excluded baseline group in the analyses is subjects assigned to the no-radio condition. Knowledge is a scale measuring accuracy in answering questions about the number of seats in parliament, the minister of finance, and ECOWAS, Non-sophisticates are those with below average knowledge, and sophisticates are those with above average knowledge.