Shooting the Messenger:
The Polarizing Effects of Political Attacks on the Press*

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Abstract

Government attacks on the press are increasingly common in polarized societies, yet their effects remain underexplored. We theorize that such attacks polarize politics by motivating journalists' production of anti-government content, increasing partisan media consumption and evaluation, and shifting citizens' political attitudes. We test this theory in Israel, a context marked by rising polarization and intensified attacks on the press. In Study 1, we estimate the effects of a government attack on radio hosts using an event-study design, finding suggestive evidence of an immediate but short-lived increase in anti-government content among targeted hosts. In Study 2, survey-based selection and evaluation tasks reveal that attacks on the press polarize both the consumption and evaluation of content produced by targeted journalists. In Study 3, an information experiment shows that learning about attacks on the press does not affect government supporters, but increases opposition supporters' preferences for social distance from outpartisans and concerns about democratic backsliding, while reducing their trust in government. Our findings highlight how attacks on the press shift media production, consumption, and political attitudes, creating a potentially reinforcing cycle of polarization.

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Introduction

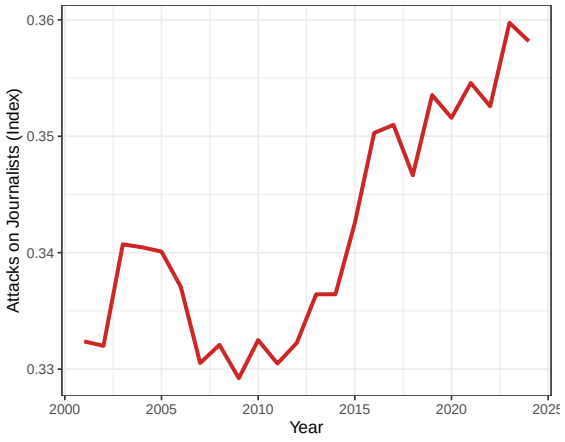
On December 1, 2024, a pro-government media outlet affiliated with the Likud—Israel's ruling party—published a list ranking national public radio hosts by their loyalty to the governing coalition. Most hosts were labeled as anti-government, and the ranking coincided with legislation to privatize parts of the public broadcaster. Golan Yochpaz, CEO of Kan—the Israel Public Broadcasting Corporation—condemned the labeling of journalists as a "McCarthyist" threat to the freedom of press and democracy.[1]

As demonstrated in Figure 1a, this attack on the Israeli press is part of a global pattern of political assaults on the media. Increasingly, across regime types, media outlets have been subject to various political attacks ranging from public vilification and legal pressure to economic manipulation and, in some contexts, violence against journalists (Relly and González de Bustamante, 2015; Reporters Without Borders, 2024; Howard and Hussain, 2010; Stoycheff and Nisbet, 2020; Benkler, Faris and Roberts, 2018; Bonello-Ghio and Nasreddin, 2022). Indeed, from Europe and the United States to Latin America and the Middle East, populist elites increasingly denounce the press, portraying mainstream journalism as biased or corrupt, and employing labels like "fake news" to cast reporters as "enemies of the people" (Davis and Sinnreich, 2018; Krämer, 2018; Vallone, Ross and Lepper, 1985; Panievsky, 2022*a*). Coinciding with the rise of attacks on the press, Figure 1b reports heightened levels of polarization across the globe, and Figure 1c suggests that these two patterns may be interrelated.

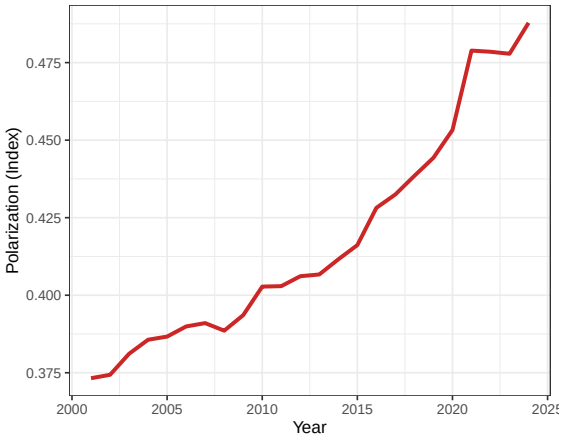
What are the political consequences of attacks on the press in polarized societies? We theorize that such attacks can polarize politics through three interrelated processes. First, attacks may shape the news content that journalists supply, either by discouraging engagement with sensitive political topics—resulting in a silencing effect—or by prompting more critical coverage of the government—resulting in a backlash effect. Second, attacks may shape media consumption and content evaluation patterns, encouraging selective exposure and biased interpretations of journalistic content, motivated by citizens' ex-ante political preferences. Finally, political attacks on the press—which are a salient form of democratic norm violation (Dahl, 2008; Norris, 2006)—can directly affect citizens' attitudes towards counter-partisans and shape their broader institutional trust and perceptions of democratic health.

To test the observable implications of our theoretical framework, we report three complementary studies including an event-study identifying the effects of government attacks on the content produced by targeted (and non-targeted) Israeli radio hosts, survey-embedded selection and evaluation tasks through which we document how government attacks on journalists shape consumption and evaluation of media content, and an information experiment through which we identify

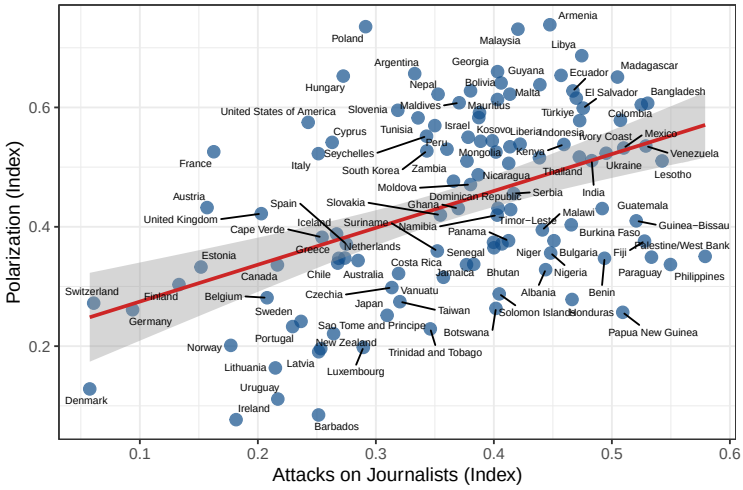
[1] For more information, see: https://www.calcalist.co.il/local_news/article/hjbmdcymjl. (Last accessed October 14, 2025.)



(a) Attacks on Journalists Over Time



(b) Polarization Over Time



(c) Attacks on Journalists and Polarization

Figure 1: **Attacks on Journalists and Polarization in Democracies Around the World.** This Figure reports data from V-Dem (Coppedge et al., 2025). Panel (a) reports the global average score of V-Dem's journalist harassment index over time (`v2meharjrn`), reverse-coded). Panel (b) reports the globael average score of V-Dem's political polarization index over time (`v2cacamps`). Panel (c) reports the bi-variate correlation of both variables ($\rho = 0.617$, $p < 0.001$). We focus on electoral and liberal democracies as coded by the `v2x_regime` variable in the V-Dem data.

the direct effects of information regarding a government attack on mass attitudes and preferences. These analyses yield three primary findings. First, we find no evidence that government attacks on the press have a chilling effect on journalists. Instead, we observe suggestive evidence of an immediate, short-lived increase in anti-government speech among targeted journalists, suggesting that government attacks on the press may lead to a modest, short-lived backlash effect. Second, attacks on journalists polarize the consumption and evaluation of media content, reducing government (opposition) supporters' selection of content produced by targeted journalists by 34% (10%) of the control group standard deviation, and polarizing broader evaluations of targeted journalists' media content. These effects are likely mutually reinforcing—expanding the overall supply of anti-government media content while simultaneously polarizing its consumption and evaluation along partisan lines. Our final result suggests that beyond shifting media supply and demand, government attacks on the press also have a direct effect on political attitudes. We find that exposure to depictions of government media attacks heightens opposition supporters' preferences for social distance from outpartisans and concerns about democratic backsliding by 14% and 10% of the control group standard deviation respectively, and reduces their trust in government by 9% of the control group standard deviation. Government supporters' attitudes remain unchanged after exposure to depictions of attacks on the press, suggesting that such attacks may fail to strengthen support within a politician's base and may even prove counterproductive—intensifying partisan animosity, reducing trust in government, and heightening perceptions of democratic backsliding among opposition supporters.

By theorizing how government attacks on the press reshape media production, media consumption, and citizens' political attitudes, and testing the observable implications of our theory with event-study analyses of large-scale radio data and a series of complementary experiments, we contribute to three lines of research. First, we contribute to work on the political causes and effects of attacks on the press (Gohdes and Carey, 2017; Carey and Gohdes, 2021). Recent accounts of this phenomena mostly focus on journalists' behaviors as an outcome, paying less attention to downstream effects on citizens' media consumption and evaluations or their broader political attitudes. Moreover, existing findings are mixed, with some studies pointing towards a silencing effect that minimize the prevalence of critical reporting (Holland and Rios, 2017; Bonello-Ghio and Nasreddin, 2022; Panievsky, 2022*a*), while others suggesting that attacks on the press motivate backlash resulting in more critical content (Ozawa et al., 2024). By developing a comprehensive theoretical framework that considers the consequences of attacks on the press for *both* media producers and consumers, and by employing a series of complementary research designs that account for the endogenous nature of such attacks and media consumption more broadly, we offer a nuanced perspective highlighting the potentially reinforcing effects of attacks on the press on both media content and its consumption.

Second, we contribute to the literature on selective exposure to partisan media (Stroud, 2011*a*; Prior, 2013; Levendusky, 2013*b*,*c*; Broockman and Kalla, 2025*b*; Peterson, Goel and Iyengar, 2021). Past studies mostly focus on documenting patterns of selective exposure (Peterson, Goel and Iyengar, 2021; Broockman and Kalla, 2025*b*; Weiss, Siegel and Scacco, 2025), and identifying their downstream political effects (Levendusky, 2013*c*,*a*; Broockman and Kalla, 2025*a*). Other work focuses on the causes of selective exposure to partisan media, highlighting how structural changes in the media landscape (Levendusky, 2013*b*; Hmielowski, Beam and Hutchens, 2016; Grossman, Margalit and Mitts, 2022), alongside individual-level factors including consumers' ideology and psychological predispositions (Peterson and Kagalwala, 2021; Stroud, 2008; Iyengar and Hahn, 2009), contribute to rising patterns of selective exposure to partisan news. Complementing these accounts, we propose that political elites—and specifically their attacks on the press, which can affect both the supply of and demand for political news—may be an underappreciated determinant of selective media exposure.

Finally, we contribute to the literature on partisan polarization and declining support for democratic norms and institutions (Iyengar et al., 2019; Gidron, Adams and Horne, 2020; Voelkel et al., 2024; Norris, van Es and Grömping, 2021; Lührmann and Lindberg, 2019; Graham and Svolik, 2022). Complementing recent studies demonstrating how elite rhetoric and behaviors may intensify partisan divides (Kim, 2025), or alternatively, depolarize the electorate (Weiss, Green and Willer, 2025; Huddy and Yair, 2021), our findings suggest that elite's attacks on the press are an underappreciated cause of polarization. As we show through our studies, such attacks not only affect partisan preferences directly—by increasing preferences for social distance and concerns about democratic health among opposition voters—but also shift patterns of media production and consumption. As we describe towards the end of this paper, these latter effects may very well reinforce one another—increasing consumption of critical media among opposition supporters, while decreasing it among government supporters—further exacerbating existing patterns of polarization in divided societies.

How Attacks on the Press Polarize Politics

Across regime types, governments deploy a wide range of tactics to influence, control, or punish the press. In authoritarian contexts, this can include overt repression such as imprisonment, physical violence, broadcast closures, and censorship of print or digital outlets (Relly and González de Bustamante, 2015; Reporters Without Borders, 2024). In hybrid regimes, legal harassment, licensing pressures, and economic manipulation often coexist with rhetorical de-legitimization (Howard and Hussain, 2010; Stoycheff and Nisbet, 2020). Even in established democracies with legal press protections, elected officials have increasingly employed various forms of attack, including public vilification of journalists, selective denial of access to reporters, strategic lawsuits against specific

outlets, and orchestrated online harassment of media personalities in an attempt to influence the nature of political reporting (Benkler, Faris and Roberts, 2018; Bonello-Ghio and Nasreddin, 2022; Krämer, 2018).

In democracies, government attacks on journalists are particularly consequential because they are levied by actors who have been elected by a significant majority of constituents, and are legally entrusted with upholding media freedom and independence. High-profile public rhetoric portraying journalists as "enemies of the people" or "biased partisans" with hidden political agendas can both erode public trust in the press and raise the personal and professional costs of reporting critically on the government (Ladd, 2012; Benkler, Faris and Roberts, 2018). Such attacks can also cue partisan audiences, influencing their consumption behaviors, and potentially legitimizing hostility toward the media (Nielsen and Graves, 2021; Posetti et al., 2022; Vallone, Ross and Lepper, 1985; Davis and Sinnreich, 2018).

A rich conceptual and mostly descriptive literature considers the relationship between anti-media populism and journalists' reporting behavior. Some studies clarify the nature and dynamics of populist attacks on the press (Krämer, 2018). Other work documents how journalists under-attack prefer to sustain objectivity standards, while moderating the political slant of their reporting (Panievsky, 2022*b*). Complementing such insights that focus primarily on the (self-reported) reactions of journalists under attack—without comparing them to journalists unconstrained by political pressure—other studies examine how elite attacks on the press may reduce media trust, lower accuracy judgments, or diminish self-reported intentions to use targeted outlets (Van Duyn and Collier, 2019; Archer and Peterson, 2025; Pingree et al., 2018; Smith, 2010; Watts et al., 1999).

Importantly, however, the consequences of political attacks on the press for both media producers and consumers are rarely examined together, despite their close relationship and potentially reinforcing effects. This limitation hinders our understanding of the broader political consequences of attacks on the press. If elite attacks on the press jointly affect the production of content by targeted journalists, as well as partisans' willingness to consume media produced by those targeted journalists, then elite attacks on the press may be especially polarizing. However, theoretical frameworks and empirical investigations that focus on journalists or media consumers in isolation, may underemphasize such dynamics, and, in turn, under-estimate the political consequences of elite attacks on the press. To address this and develop a more holistic theoretical framework, we examine how elite attacks on the press influence journalists content production (media supply), as well as the public's consumption and evaluation of political news (media demand), and their broader political attitudes and perceptions of democratic norms.

Media Supply

Elite attacks on the press can shape the production of news—and specifically journalistic content—in diverging ways. On the one hand, if posing a potential threat to journalists, political attacks can serve to silence criticism, reducing the volume of coverage on politically sensitive topics. Indeed, in the shadow of attacks journalists and editors may avoid engaging with reporting on topics that will provoke official retaliation from the government or affiliated political elites (UNESCO, 2021; Bonello-Ghio and Nasreddin, 2022). Even without influencing formal editorial decisions, political attacks on the press may induce self-censorship, altering the framing, tone, and sourcing of coverage in an attempt to reduce perceived risk (Besley and Prat, 2006; Pring and Canan, 1996). In some instances, these dynamics of re-framing and censorship can also lead journalists to "perform balance," which can actually bias coverage in favor of parties that levy attacks on the press (Krämer, 2018; Panievsky, 2022*b*).

Alternatively, political attacks on the press can spark backlash. Targeted journalists may double down on watchdog reporting, align more explicitly with democratic norms while portraying elites as a danger to democracy, and highlight threats to fellow journalists and opposition actors. Rather than self-censoring in response to government attacks, targeted journalists may increase their criticism of government officials. We view this response as especially plausible in polarized societies, but under-appreciated in the published literature examining populist attacks on the press.

Taken together, our theoretical discussion of supply-side consequences suggests that attacks on the press may either silence journalists or mobilize their critical reporting. The limited direct evidence identifying the effects of attacks on the press on targeted journalists' reporting patterns motivates the first component of our empirical analyses. However, as noted above, attacks on the press may affect not only the supply of, but also the demand for, critical journalism. We thus turn to theorize how attacks on the press shape media consumers' engagement with content produced by targeted journalists.

Media Demand and Evaluation

Turning to media demand and evaluation, we theorize that attacks on the press will shape which outlets and narratives citizens seek out and how they interpret coverage presented by targeted journalists. Our expectation draws on the literature on elite cues, which shows that politicians' rhetoric and behavior can shape partisan preferences (Lenz, 2009; Nicholson, 2012). Building on these findings, we anticipate that elite attacks on the press will serve as salient partisan and ideological signals, reinforcing and intensifying patterns of selective exposure.

In turn, supporters of the attacking government may come to view targeted outlets as further biased or illegitimate, leading them to turn toward explicitly partisan and government-aligned information sources (Stroud, 2011*b*; Levendusky, 2013*b*). Supporting this expectation, Archer and

Peterson (2025) find that exposure to a Republican politician's attack on a local newspaper dramatically reduces the public's trust in and intent to consume local news, particularly among Republican respondents. Conversely, opponents of the government may increasingly rely on attacked outlets as trusted watchdogs, reinforcing media diets concentrated on anti-government perspectives. Indeed, past research suggests that attacks on the press may increase loyalty, donations, or subscriptions among government opponents who seek to defend journalists under attack (Lichterman, 2016; Thompson, 2017; Fischer, 2017)

Importantly, such polarized responses to political attacks on the press may extend beyond outlet choice to the interpretation of content produced by various news organizations and media personalities. Past work suggests that exposure to attacks on journalists can activate hostile media perceptions, wherein audiences see neutral or balanced reporting as biased against their side (Vallone, Ross and Lepper, 1985; Gunther, 1992). Such polarization in engagement with, and interpretation of, journalistic content, can undermine the possibility of shared factual baselines, even when audiences consume the same information.

Drawing on these insights, our primary theoretical expectation is that political attacks on the press will increase politically motivated media selection and evaluations of journalistic content. These combined consequences, may lead partisan and counter-partisans to inhabit increasingly distinct information ecosystems, and interpret similar information in diverging ways. Before testing how attacks on the press shape patterns of media consumption and evaluation, we turn to lay out the final component of our theoretical framework considering the direct effects of such attacks on citizens' political attitudes and beliefs.

Political Attitudes and Beliefs

As noted above, attacks on the press may indirectly exacerbate polarization by altering patterns of journalistic reporting and shaping how consumers engage with and evaluate targeted journalists. However, we theorize that beyond these indirect consequences driven by mechanisms of media supply and demand, attacks on the press can have a direct effect on mass attitudes, further exacerbating political polarization and shaping citizens perceptions of democratic norms. We expect attacks on the press to directly polarize mass political attitudes for two primary reasons.

First, elite de-legitimization of the press can serve as a signal of weakened democratic norms, especially when combined with other forms of institutional erosion (Levitsky and Ziblatt, 2018; Benkler, Faris and Roberts, 2018). For pro-government audiences, such signals may normalize further restrictions on media freedom or justify legal and extra-legal measures against journalists. For opposition audiences, attacks on the press may be perceived as indicators of democratic backsliding, heightening concerns about authoritarian drift and potentially mobilizing resistance (Norris, van Es and Grömping, 2021; Waisbord, 2020).

Second, elite attacks on the press can undermine both trust in the media and beliefs about its legitimacy. The polarization of media trust and perceptions of press freedom can contribute to a broader cycle of democratic fragility. Citizens who view media freedom as under threat may interpret other government actions—such as changes to electoral laws, judicial appointments, or restrictions on civil society—through a backsliding lens, even when these actions have ambiguous democratic implications. Over time, this can widen the gap between partisan assessments of whether democracy is functioning, fostering parallel political realities and undermining the legitimacy of democratic institutions (Lührmann and Lindberg, 2019).

Observable Implications

In light of the theoretical framework above, we expect political attacks on the press to shape journalists' reporting behavior, audiences' consumption and evaluation of news, and the public's broader political preferences and democratic attitudes. To evaluate these expectations, we present a series of complementary empirical studies that examine the multiple, and potentially reinforcing, effects of government attacks on the press. Table 1 summarizes our theoretical expectations alongside the empirical strategies we use to test their observable implications.

Table 1: Theoretical Expectations and Study Descriptions

Study	**Estimand**	**Expectation**	**Research Design**
Study 1	Effects of attacks on media supply	Attacks on journalists may either silence or motivate journalists' criticism of government.	Event-study analysis identifying effects of government attacks on Israeli radio hosts' production of ant-government content.
Study 2	Effects of attacks on media demand	Attacks on journalists will polarize the consumption and evaluation of media content.	Survey-based media selection and evaluation tasks considering how attacks affect media consumption and evaluation of journalistic content.
Study 3	Effects of attacks on attitudes	Attacks on journalists will polarize mass political attitudes.	Information-based survey experiment identifying how information about an attack on the press shapes political attitudes.

The Israeli Context

To test the observable implications of our theoretical framework, we follow recent studies of political media intervention (Grossman, Margalit and Mitts, 2022), and turn to Israel. Our focus on the political consequences of Israeli elites' attacks on the press is motivated by three factors: (i) the increasingly politicized nature of Israeli media, (ii) persistent government attacks on journalists, and (iii) heightened levels of polarization in recent years.

Israel's contemporary media landscape features a mix of public and private platforms. Until the 1990s, however, television and radio services were provided exclusively by a centralized public broadcaster (Schejter, 2022). A series of reforms and legislative changes in the 1990s opened these markets to private enterprise, producing a more populous and somewhat diverse media environment (Peri, 2004). Nonetheless, emerging channels continue to depend heavily on government regulators (Panievsky, 2022*a*). This ongoing reliance has led some scholars to argue that "a major characteristic of both media regulatory actions and the actors in the media environment is the existence of purportedly commercial government mouthpieces" (Schejter, 2022, p. 152). In that sense, while freedom of press is legally protected in Israel (Schejter, 2022), direct and indirect forms of political influence on journalistic slant and content are by no means marginal (Grossman, Margalit and Mitts, 2022; Schejter, 2022).

Politicians' attempts to influence media reporting in Israel have taken different forms, ranging from alleged propositions to provide regulatory benefits in exchange for positive coverage (Schejter, 2022), through formal interference in headline selection in pro-government media outlets (Grossman, Margalit and Mitts, 2022), to outright attacks on critical journalists (Panievsky, 2022*a*). Indeed, a central feature of Netanyahu's cumulative eighteen years in office has been the persistent framing of mainstream private and public media outlets as politically biased, unrepresentative, and motivated by personal vendettas against him (Peri, 2004; Panievsky, 2022*a*). This populist sentiment, manifesting in countless explicit attacks on the press, has motivated the introduction of pro-government and right-wing outlets into the Israeli media ecosystem (Peri, 2004; Grossman, Margalit and Mitts, 2022). As outlets such as Israel Hayom, Channel 14, and i24 News gain increasing popularity, the Israeli media landscape has become increasing partisan.

Alongside rising politicization in the Israeli media landscape, mass polarization has intensified among the Israeli public in recent years (Bassan-Nygate and Weiss, 2022, 2020; Gidron, Sheffer and Mor, 2022). Such polarization, which has been extensively documented using longitudinal survey data (Amitai, Gidron and Yair, 2023), has gained especially high-salience in light of recent democratic backsliding reforms led by Netanyahu's 36th government (Gidron, 2023), and its contentious management of the Israel-Gaza war (Elran and Shapira, 2024). Whether or not rising polarization in Israeli society may be a consequence, at least in part, of political attacks on the press,

is a topic we now turn to empirically investigate.

Study 1: How Political Attacks Shape Media Supply

Building on the framework above, we posit that elite attacks can alter journalistic content. On one hand, such attacks may heighten the perceived costs of critical reporting and induce silencing or self-censorship—reducing general output rates or softening critical coverage. On the other, government targeting may provoke backlash, prompting journalists to double down and increase critical, anti-government content (UNESCO, 2021; Pring and Canan, 1996; Besley and Prat, 2006).

To adjudicate between these diverging expectations, we leverage a difference-in-differences event-study design centered on the publication of a Likud-affiliated list ranking journalists according to their level of government support. As noted in the motivation of this paper, on December 1, 2024, *Melukdim News*—a Likud-affiliated multi-media outlet managed by Avi Ravina, which delivers political content via a monthly journal, an online newsletter, 72 WhatsApp groups, Facebook, Tiktok and Telegram—published a list of Israeli radio hosts from the Kan network (Israel's main public network) ranked from 1-5 according to their level of support for Netanyahu's ruling government. As shown in Figure 2, the ranking focused on the majority of radio hosts in the Kan network. Most radio hosts were outed as "especially anti-government" (i.e., a rating of 5/5), while a few hosts were identified as neutral or modestly pro-government (i.e., a rating of 2-3/5), and not a single hosts was described as "especially pro-government" (i.e., a rating of 1/5).

Beyond its circulation in numerous Likud party and pro-government media channels, the list attracted a great deal of attention from the Israeli media establishment and was covered by most mainstream outlets as a serious assault on freedom of the press (Guetta, 2024; Cohen, 2024). We leverage the timing in which this list was distributed, as well as its differential implications for various radio hosts in Israel, to examine how attacks on the press affect radio content among targeted hosts. Specifically, we compare targeted radio hosts (who received a 3-5 score on the list depicted in Figure 2), with a sample of non-targeted radio hosts in the Kan network as well as other popular networks, and track the count of paragraphs containing polarized anti-government content (negative references to coalition actors or positive references to opposition actors). Under the parallel trends assumption, this design allows us to test whether attacks on the press primarily chill critical speech (silencing and self-censorship) or instead trigger a short-run increase in critical output (backlash).

Data and Measurement

The data for this study include the content of radio shows produced by targeted and non-targeted radio hosts between November 14 and December 14, 2024. Our unit of analysis is the radio show, and our data includes a total of 22 Kan radio shows hosted by journalists who were included on the publicly distributed list and were not labeled by the Likud as pro-government. Our control group

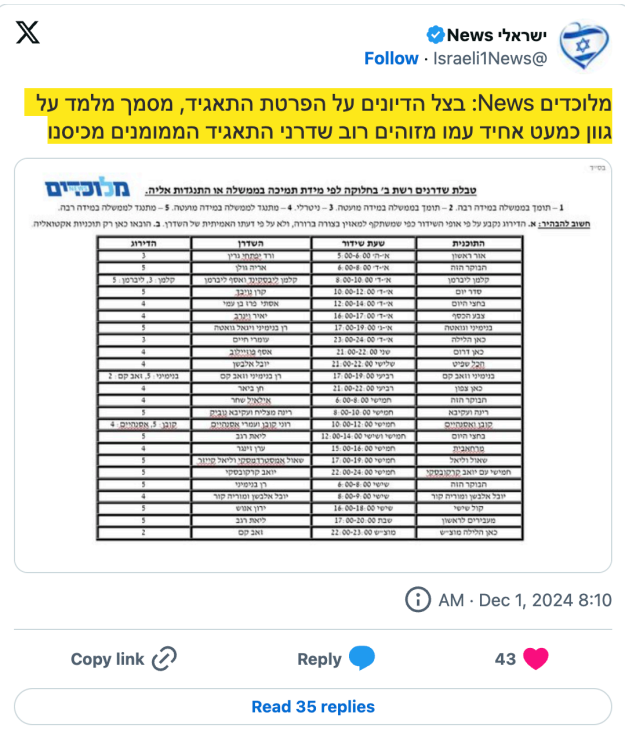


Figure 2: **Israeli radio host rankings published by Melukdaim News on December 1, 2024.**

includes shows hosted by radio journalists who received positive rating from the Likud, as well as shows hosted by radio journalists from two non-targeted stations—103FM and Galatz—that are similar in format and audience reach.

To compile this data, we built web scrapers to download MP3 audio files for all episodes produced by treated and control hosts between November 14 and December 14, 2024, covering the two weeks before and after the list was leaked. This yielded 184 treated episodes from 24 Kan shows, 5 control episodes from Kan shows, 136 control episodes from 19 Galatz shows, and 1,693 (shorter and more frequent) control episodes from 18 103FM shows. We transcribed all audio into Hebrew using Amazon Web Transcribe, producing 25,681 paragraphs (three-sentence chunks) from treated hosts and 44,347 from control hosts. This enables us to measure the total volume of content produced by targeted and non-targeted radio hosts over time.

Our primary outcome of interest is the prevalence of anti-government content produced by targeted and non-targeted radio hosts before and after the attack. To measure this, we first constructed Hebrew-language dictionaries to detect content relevant to the coalition government (keywords related to Prime Minister Netanyahu, his family, Likud ministers, and the Likud party).[2] Paragraphs containing at least one dictionary term were flagged as potentially relevant to our outcome of interest. We then applied the `openai` R package with the "gpt-4-turbo" model to classify each

[2]See Appendix A.1 for dictionary terms.

relevant paragraph's *topic polarity* (positive, negative, neutral) and *framing* toward the coalition government.[3] To measure anti-government content we count the number of paragraphs (3 sentence chunks) in each radio show containing negative references to government/coalition actors. Table 2 provides examples of paragraphs classified as anti-government, comprising our primary outcome of interest. We validate our LLM classification by training two native-Hebrew speaking RAs to manually code random samples of our data. We find that our RAs had on average 88% intercoder agreement with one another, compared to an average of 83% agreement between the RAs and the GPT codings. The high rate of agreement we observe between our RAs and the LLM coding, just 5% lower than the agreement we observe between trained graduate student RAs, increases our confidence in this measurement approach.[4]

Table 2: Examples of anti-government content with English translations (from randomly selected sentence ids).

Category	**Sentence ID**	**English translation**
Anti-government	11910	On this there is the greatest offense for which there is neither atonement nor forgiveness. Last night … with Yinon Magal and Sefi Ovadia on Galatz, Netanyahu's statement was a leadership failure.
Anti-government	23766	I work on this all the time. I think we have a government that is not okay... and I think this is truly the first time a government in Israel is abandoning hostages.
Anti-government	5299	I think this is a very bad law; anyone who grew up in the system understands that it... opens the door to interference by the political echelon, fundamentally changing the principles of the work that are tied to the job.

Empirical Strategy

Since most radio shows in our sample do not broadcast on a daily basis, we aggregate shows into five-day periods. Thus our primary outcome of interest is Y_{it}: the total amount of polarized anti-government content aired in show i at time period t. Our aggregation of data to five-day time periods balances two objectives: minimizing time periods where specific shows do not air (by increasing the temporal scope of each observation), while maximizing granularity in our data (by ensuring that temporal scope is not too coarse), allowing us to estimate the immediate effects of the Likud attack on radio content. As we show in Table A3, our main results are robust to alternative

[3] See Appendix A.2 for prompt text.

[4] To see agreement metrics broken down by each RA comapred to one another and GPT, see Table A2 in the Appendix.

time aggregations.

Using this data, and under the parallel trends assumption, we employ a standard event-study design. Our primary specification is:

$$Y_{it} = \sum_{k \neq -1} \beta_k \, \mathbf{1}\{t - t_0 = k\} \times \text{Treat}_i + \alpha_i + \gamma_t + \eta X_{it} + \varepsilon_{it},$$

where Y_{it} is the amount of polarized anti-government content produced by radio show i during period t. $\mathbf{1}\{t-t_0 = k\}$ are event-time dummies which we interact with Treat_i, a treatment indicator, yielding our primary parameters of interest: β_k, the time-specific effects of treatment. Our main specification also includes show fixed effects (α_i) and time fixed effects (γ_t), as well as a control for the number of radio shows aggregated in each time period (ηX_{it}). Standard errors are clustered by radio show, the level at which treatment was assigned (Abadie et al., 2023). Our estimation strategy compares changes in treated shows' outcomes before and after the attack to changes over the same period for control shows, allowing the dynamic β_k coefficients to trace treatment effects over time. The reference period is $k = -1$.

As a robustness check, we also estimate dynamic treatment effects using the Callaway and Sant'Anna difference-in-differences estimator (Callaway and Sant'Anna, 2021). This approach is less sensitive than two-way fixed effects event studies to bias from treatment effect heterogeneity and negative weighting, and is well suited to settings with treated and never-treated units and potentially unbalanced panels. We implement the estimator using never-treated shows as the control group. This provides an alternative estimate of the post-attack trajectory of anti-government content under the same core identifying assumption of parallel trends between treated shows and control shows.

In Appendix D, we additionally assess the sensitivity of our conclusions to plausible violations of parallel trends using the "Honest DiD" framework proposed by Rambachan and Roth (2023). This procedure evaluates how large post-treatment deviations from parallel trends would need to be—relative to the magnitude of observed pre-trend differences—to overturn inference about the early post-attack effect. Together, the Callaway and Sant'Anna estimates and the Honest DiD sensitivity analysis help clarify how much our conclusions depend on the two-way fixed effects functional form and on the strength of the parallel trends assumption. More generally, they provide a conservative check on our interpretation of the primary estimates reporeted below.

Results

Across specifications, our results provide no evidence of a chilling effect on anti-government content among targeted radio hosts. Instead, we find suggestive evidence of an immediate, short-lived increase in anti-government rhetoric following the December 1 attack, displayed in Figure 3. In

the first post-treatment period, the estimated effect of the attack is positive and statistically significant across all three aggregation schemes (3-day, 4-day, and 5-day specifications). Substantively, using treated shows' pre-treatment means as baselines, we observe an increase of 30.8% in antit-govenment content in our main specification ($\beta_0 \approx 8.33$; pre-treatment mean ≈ 27.0).[5]. Later post-treatment point estimates remain generally positive but are smaller and imprecisely estimated, suggesting that any backlash effect is concentrated in the immediate aftermath of the attack rather than sustained over time.

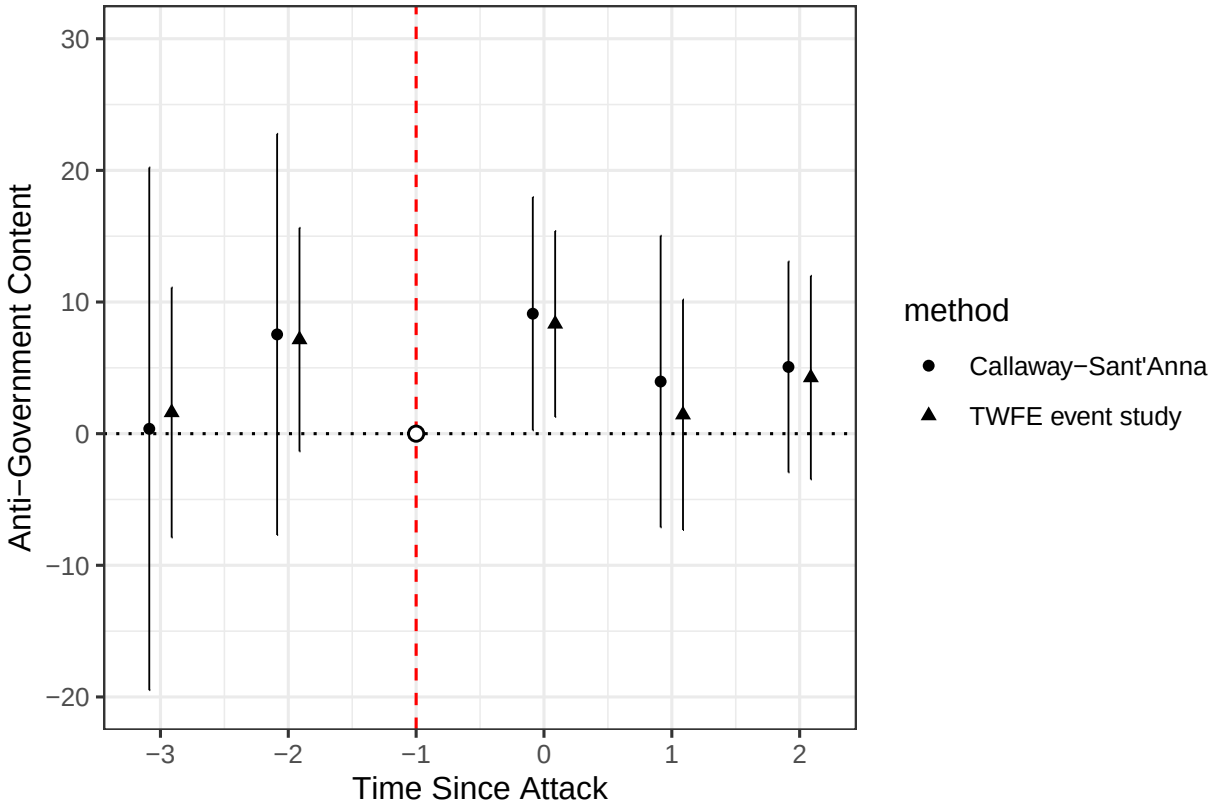


Figure 3: **Effect of the Likud attack on radio hosts' production of anti-government content.** This coefficient plot overlays dynamic estimates from two approaches using five-day aggregated data: 1) a two-way fixed effects (TWFE) difference-in-differences event-study specification with show and period fixed effects and a control for the number of shows included in each time period, and 2) the Callaway and Sant'Anna (CSA) DiD estimator. Standard errors are clustered at the show level. For the TWFE event study, estimates are shown relative to the reference period ($t = -1$). The CSA dynamic estimates do not rely on a single omitted reference period in the same way; however, we display them on the same event-time scale (including the visual marker at $t = -1$) to facilitate comparison across methods. The corresponding regression tables, along with results for alternative time aggregations, are reported in Tables A3 and A4 in the Appendix.

[5]In appendix D, we show that the immediate effects in response to treatment amount to 41.2% in the 3-day specification ($\beta_0 \approx 11.0$; pre-treatment mean ≈ 26.7), 33.8% in the 4-day specification ($\beta_0 \approx 9.16$; pre-treatment mean ≈ 27.1)

We complement our TWFE estimates with additional estimates using the Callaway and Sant'Anna framework, which is less sensitive than TWFE event studies to certain forms of specification bias and provides a useful robustness check even in a non-staggered setting. The Callaway-Sant'Anna estimates generally align with the TWFE results. The 5-day aggregation yields the clearest evidence of an immediate positive statistically significant post-attack response, while the 3- and 4-day versions point in the same direction but are estimated less precisely. Because the magnitude of several pre-treatment coefficients is nontrivial relative to the immediate post-treatment estimate in some specifications, we evaluate the sensitivity of our primary analysis to potential violations of parallel trends using the Rambachan and Roth HonestDiD "relative magnitudes" approach. This exercise asks how large post-treatment trend deviations would have to be—relative to observed pre-treatment deviations—to overturn inference about the immediate post-attack effect. The results, displayed in Appendix D, show that our 5-day aggregation primary analysis remains significant up to deviations of $M < 0.3$ of the magnitude of the pre-treatment violations.

Together, these results provide suggestive evidence of backlash dynamics in response to politicized attacks on the press. In contrast to existing theories that expect attacks on the press to depress critical journalistic content (Krämer, 2018; Panievsky, 2022*a*), we find no evidence that targeted radio hosts *reduce* their level of criticism towards the government. Instead, our evidence suggests that the Likud attacks temporarily emboldened targeted hosts to produce more content that negatively referenced the government or positively referenced opposition actors. In light of these findings, we now turn to examine whether, beyond their immediate effects on journalists, government attacks on the press influence consumers' demand for and evaluation of political media.

Study 2: How Political Attacks Shape Media Consumption

As we describe in our theoretical framework, attacks on the press may operate as salient cues that influence which media content respondents select to read, how they engage with it, assess its quality, and evaluate its tone. To test these propositions, and further evaluate the consequences of attacks on journalists we designed two complementary experiments. Study 2a focused on evaluating how attacks on journalists affect consumers' *selection* of media content, whereas Study 2b focused on how attacks on journalists affect consumers' *engagement* with media content.[6]

[6]Both studies reported in this section, as well as Study 3, were implemented in the same survey. We randomized the order of Studies 2a-2b, which were followed by Study 3. This design choice was taken in order to avoid order effects among the two complementary studies, and avoid priming respondents with the information treatment in study 3.

Study 2a: Attacks on Journalist and Patterns Media Selection

Research Design

As part of Study 2a, respondents were invited to participate in an incentivized knowledge task. In this task, respondents were informed that they will be asked three questions about contemporary events relating to either the Israeli Judicial System or Biotech Sector,[7] and that if they correctly answer all three questions, they will enter a 100 NIS lottery. Respondents were further informed that the questions they will answer are relatively specific, and therefore, they can select to read recent news articles on the topic from a variety of journalists.

After receiving information about the knowledge task, respondents were presented with a list of four (fictitious) Israeli journalists named Oren Faran, Tamar Aharon, Sharon Yashar, and Adam Levi. This list included a brief description of journalists' professional histories. In order to measure respondents' media consumption patterns and selection behavior directly, we informed respondents that as part of the knowledge task, they may select up to three journalists whose content they may read, to increase their likelihood of correctly answering the knowledge questions that follow. We combined a choice constraint—requiring participants to select three out of four journalists—with a meaningful financial incentive—amounting to 100 NIS lottery—in order to induce costly selection as part of our task.

To implement our primary treatment, we randomized whether Oren Faran's biographical information included a mention of him being attacked by various Likud officials, including prime-minister Netanyahu. Our primary outcome of interest records whether a respondent chooses to read content created by Oren Faran as part of the knowledge task. For ease of interpretation, we standardize this outcome so that the control group has a mean of 0 and a standard deviation of 1.

To estimate the effects of information regarding Likud attacks on respondents' selection of Oren Faran's journalistic content, we employ a pre-registered OLS regression, with HC2 robust standard errors. Specifically, we regress our primary outcome of interest (selection of Faran's media content as part of the knowledge task) over a binary treatment indicator (taking a value of 1 for respondents receiving information about the Likud's media attack, 0 otherwise), which is further interacted with demeaned covariates (ideology, age-category, religiosity, gender, locality) and secondary treatments (to which respondents is assigned at the start of the survey) to increase precision (Lin, 2013). Following our pre-registration plan, we report the ATE for the full sample, as well as CATEs for government and opposition supporters.

[7] We randomized the topic of the task to focus on either political or non-political domains, and report CATEs by domain in Appendix E.1.

Results

In Figure 4, we report our main estimates for the effects political attacks on respondents' choices in the knowledge task. The top panel of Figure 4 shows that respondents who received information about Likud attacks against Oren Faran were less likely to consume his content. However, this relatively modest effect, amounting to 7.6% of the control group standard deviation, masks significant heterogeneity in responses to treatment.

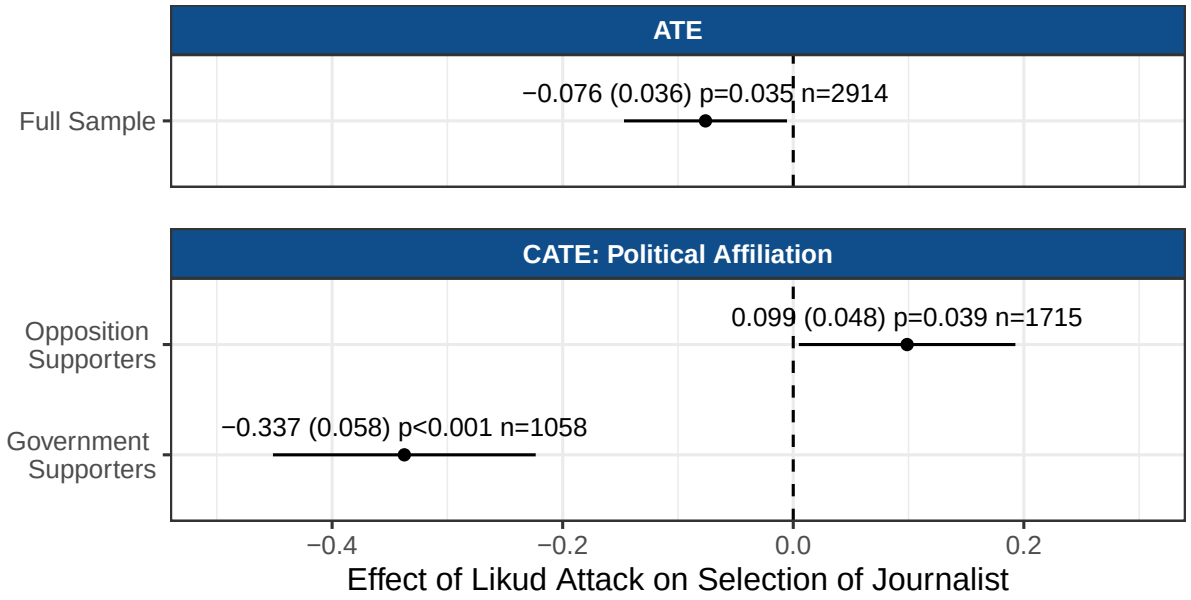


Figure 4: **Media attacks reduce overall consumption of articles written by targeted journalists, and polarize voters in their media selection.** This Figure reports point estimate and corresponding 95% confidence intervals representing the effects of the political attack treatment on the probability that respondents' select to read articles written by the targeted journalist. Table format results reported in Table A7.

Considering CATEs among government and opposition supporters reveals stark polarization in responses to treatment. These additional analyses suggest that our main negative estimate is driven by government supporters who, in response to treatment, reduce their selection of articles written by Oren Faran by over a third of the control group standard deviation. In contrast, effects on supporters of the opposition are positive, albeit more modest, increasing selection of articles written by Oren Faran by 9.9% of the control group standard deviation. Importantly, the estimated difference in CATEs across subgroups is not only substantively significant—amounting to 44% of the control group standard deviation—but also estimated with high levels of precision ($p<0.01$). We therefore interpret this evidence to suggest that attacks on journalists can polarize patterns of media consumption, reducing (increasing) consumption of content among government (opposition) supporters.

Study 2b: Attacks on Journalist and Evaluation of Media Content

Research Design

In Study 2b, we consider how attacks on journalists affect respondents' engagement with, and evaluation of their journalistic content. To do so, we designed an experiment where we asked respondents to read a brief news article regarding either the labor market impacts of AI or recent legal developments relating to Israel's contested conscription law.[8] Respondents were informed that the article they will read was written by a (fictitious) Israeli journalist named Yariv Levi. All respondents received basic information describing Levi's professional background, and treated respondents were further informed that Levi has been the subject of political attacks by government officials, including a senior minister.

After receiving information about the task, respondents were directed to read a brief article, and then report primary outcomes of interest, including: i) three reading comprehension questions (which we combine into an additive knowledge retention index ranging from 0-3), ii) four evaluative measures eliciting respondents' willingness to share the article, as well as their perceptions of article objectivity, journalistic standards, and informative value (which we average into an index ranging from 0-100), iii) a measure of perceived pro-government slant (single 0-100 item), and iv) a measure of perceived pro-opposition slant (single 0-100 item). We standardize all outcomes, so that the control group has a mean of 0 and a standard deviation of 1, and employ the same specification used in Study 2a to estimate our primary treatment effects, as well as CATEs by governments and opposition support.

Results

In Figure 5 we report our primary estimates demonstrating how political attacks on journalists affect respondent's engagement with and evaluation of their journalistic content. First, we find that treated respondents who receive information about political attacks, are less likely to retain the knowledge included in the article they read. On average control group respondents correctly answer 2.3 questions, compared with 2.2 correct answers among the treated. In Appendix E.1, we provide suggestive evidence that these effects are driven by treated respondents spending less time reading the article to which they are exposed. Considering CATEs by political support, we find that exposure to treatment has a negative, albeit imprecisely estimated effect for both government and oppostion supporters ($p > 0.1$), likely as a result of reduced power in our subgroup analyses, alongside generally small but homogeneous effects across subgroups.

[8] Like in Study 2a, we randomized the topic of the article to focus on either political or non-political domains, and report CATEs by domain in Appendix E.1.

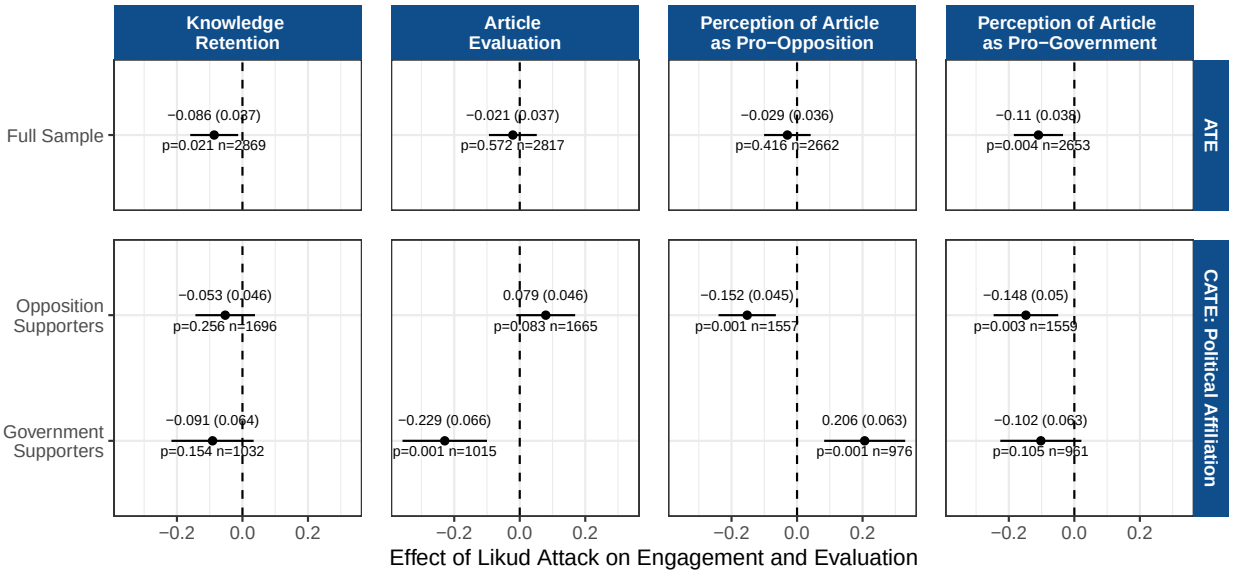


Figure 5: **Media attacks shape respondents' evaluations of journalistic content.** This Figure reports point estimate and corresponding 95% confidence intervals representing the effects of the political attacks treatment on respondents' evaluation of journalistic content. Table format results reported in Table A8.

Turning to subjective evaluations, estimates in the second panel of Figure 5 suggest an overall null effect of Likud attacks on respondents' evaluation of journalistic content. However, in line with the polarized pattern of results reported in Figure 4, we find that this null effect is driven by diverging responses to treatment among government and oppositions supporters. Indeed, in response to treatment government supporters report less favorable evaluations of the article to which they were exposed. This effect is equivalent to a decrease on the overall evaluative index which amounts to 22.9% of the control group standard deviation. In contrast, opposition supporters report a modest increase in positive evaluations of the article, equivalent to 7.9% of the control group standard deviation.

Turning to consider respondents evaluation of the article's pro-opposition slant, we find a similar pattern of polarization. The overall effects of the Likud attack treatment on perceptions of pro-opposition slant are null, but this null is driven by diverging responses among government and opposition supporters. Whereas the former evaluate the article as substantially more pro opposition under treatment—amounting to a change equivalent of 20% of the control group standard deviation—the latter view the article as less pro opposition under treatment—amounting to a change that is equivalent to 15% of the control group standard deviation. Finally, we find that treatment modestly reduced perceptions of the article's pro-government slant by 11% of the control group standard deviation. Importantly, this effect is relatively homogeneous among government and opposition supporters.

Overall, these results suggest that—irrespective of the content that journalists produce—-political attacks on the press may reduce consumers' in depth engagement with journalistic content (as measured through our knowledge retention index, as well as additional analysis of respondents' reading time reported in Appendix E.1), and lead them to evaluate the slant of the content as less favorable towards the government. Importantly however, political attacks on the press have additional polarizing consequences, leading government and opposition supporters to evaluate the quality of journalistic content and its political slant in diverging directions. Alongside the evidence from Study 2a, the findings reported in Figure 5 emphasize that political attacks on the press have the potential not only to shift patterns of media selection, but also change how consumers evaluate the actual content produced by journalists.

Additional Analyses

In Appendix E.1 we report several diagnostics and additional analyses. First, we provide descriptive statistics of our sample, and report balance on covariates for all treatments included in our survey. Second, we report minimal patterns of attrition throughout our survey, and rule out concerns about differential attrition by treatment. Third, we extend our findings from Figures 4-5, and report pre-registered analyses considering subgroup effects among respondents assigned to politicized and non-political journalistic content. Estimates from these analyses are measured with higher degree of uncertainty due to reduction in statistical power when considering subgroup effects. However, for the most part, results appear to be homogeneous across topics. These additional analyses, strengthen our confidence in the overall evidence reported in Figures 4-5.

Study 3: How Political Attacks Directly Shape Attitudes

Thus far, we have provided empirical evidence suggesting that politicized attacks on the press increase radio hosts' production of anti-government content and polarize patterns of media consumption and evaluation among the general public. However, we argue that government attacks on the press may have broader consequences that extend beyond shaping the supply and demand for political news. Indeed, such attacks may directly affect, and potentially polarize, citizens' political attitudes.

Research Design

To test this expectation, we implemented a final information experiment, exposing a subset of respondents to an article describing the December 1, 2024 Likud attack on Israeli journalists.[9] Af-

[9]Respondents in the control group read an article about future NASA expeditions to the moon. It is important to emphasize that our survey was implemented eight months following the initial government attack reported in the treatment news story. Considering this, we measured respondents' awareness to, and recall of the attack, at the end of our survey. Specifically, we asked all respondents whether they recall the December 1 attack on Kan 11 Journalists. As

ter reading the treatment or placebo article, respondents reported primary outcomes, including: i) three items eliciting respondents' perceptions of democratic backsliding in Israel averaged into a perceived democratic backsliding index ranging from 0-100, ii) five items measuring respondents' preferences to maintain various relationships with out-partisans averaged into a social distance index ranging from 0-100, and single measures of iii) trust in the Israeli government, and iv) opposition ranging from 0-100. All outcomes are standardized so that the control group mean is 0 and standard deviation is 1.[10] We use the same specification employed in study 2 to estimate the effects of exposure to the article depicting the Likud attack on Kan 11 journalists.

Results

In line with the evidence from studies 2a-b, the results reported in Figure 6, suggest that receiving information about political attacks on the press—and specifically reading an article depicting the Likud attack on Kan radio hosts—generated mostly polarized responses. As shown in the top-left panel of Figure 6, the effects of attacks on perceptions of democratic backsliding among our full sample are small and imprecisely estimated. However, this small positive estimate masks meaningful heterogeneity across government and opposition supporters. Specifically, exposure to treatment increases opposition supporters' perceptions of, and concerns about, democratic backsliding by 9.6% of the control group standard deviation. In contrast, the effects of attacks among government supporters are especially small and imprecisely estimated, amounting to less than 10% of the effect reported among opposition supporters.

Turning to consider the consequences of political attacks on citizens' preferences for relationships across the political divide, a somewhat similar pattern emerges. In general, we find that exposure to treatment reduced preferences for interpersonal relationships with outparty members by 10.3% of the control group standard deviation. However, this effect is mostly driven by opposition supporters who become substantially less open to relationships across party lines by over 14% of the control group standard deviation. While our CATE estimates for government supporters are also negative, they are imprecisely estimated, amounting to less then a third of the magnitude of effects among opposition supporters.

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we show in Appendix E.1, among control group respondents (who did not read about the attack), 7.3% of respondents recalled this event, and another 14.4% of respondents were unsure whether they recall the event. We further show demographic correlates of recall, suggesting hire rates of recall among opposition supporters and older respondents. This supplementary evidence suggests that our primary treatment was relatively novel for most respondents, but also underscores that the potential impact of public attacks on the press may be temporally concentrated, since, in the long run most respondents' do not recall the occurrence of specific events.

[10] We also collected measures of trust in various media channels and report treatment effects on these outcomes in Appendix E.1.

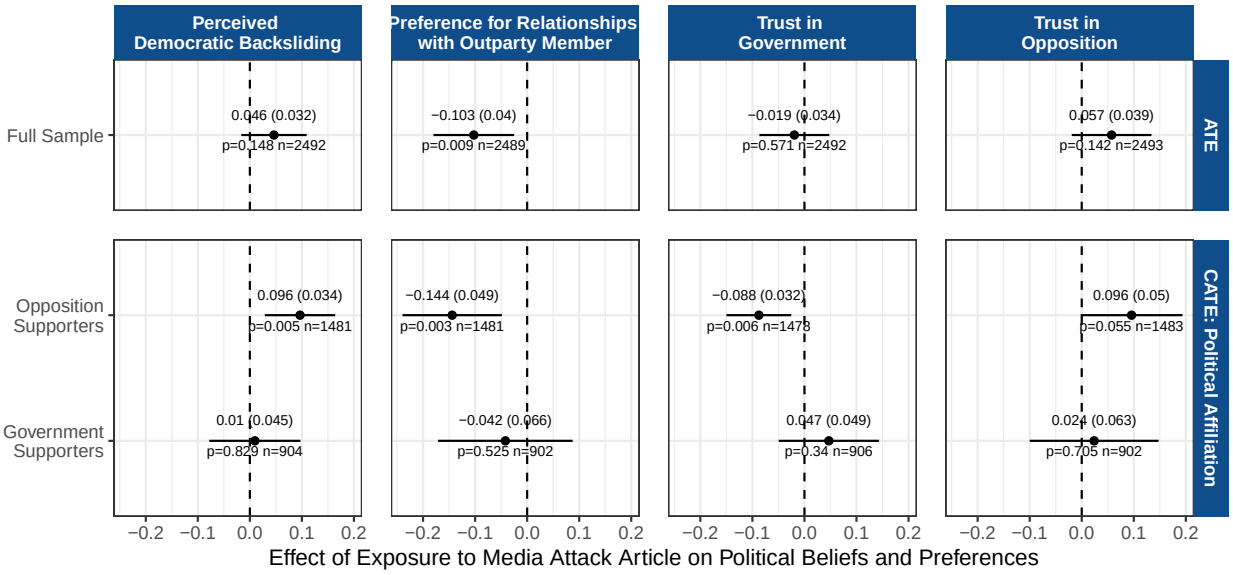

Figure 6: **Learning about the Likud Attacks on Kan 11 Radio Hosts Polarizes Citizens.** This Figure reports point estimate and corresponding 95% confidence intervals representing the effects of reading an article depicting the Likud's ranking of radio hosts by their level of government support. Table format results reported in Table A9.

In the third and fourth panels of Figure 6 we examine treatment effects on trust in the Israeli government and opposition. Our ATE estimates suggest small and imprecisely estimated responses to treatment. However, in line with the broader pattern of results reported thus far, these small ATE estimates mask meaningful heterogeneity. Whereas exposure to treatment does not shape government supporters' levels of trust in a substantial manner, effects among opposition supporters are relatively meaningful. Indeed, exposure to treatment reduces (increases) trust in the Israeli government (opposition) by 8.8% (9.6%) of the control group standard deviation.

Taken together, the evidence reported in Figure 6 emphasizes the broader polarizing effects of attacks on the press. Beyond their short-term effects that compel targeted journalists to produce more anti-government content, and their impact on patterns of selective and polarized engagement among the geneal public, such attacks have meaningful effects on the public's broader political beliefs and preferences. Though in aggregate our evidence points to limited effects, our analyses of CATEs shows that attacks on the press increase opposition supporters concerns over democratic backsliding and trust in institutions, and reduce their trust in the government as well as their preferences for social engagement with government supporters. At the same time, these attacks have little effect on shifting government supporters' beliefs or preferences in any direction.

Discussion

Our empirical analyses yield three primary findings. First, political attacks on the press do not exert a chilling effect and may instead increase the short-run prevalence of anti-government content produced by journalists. Second, such attacks polarize media consumption and evaluation, dissuading government supporters (and encouraging opponents) from consuming content produced by targeted journalists, while encouraging both groups to evaluate the credibility of media content in line with their ex-ante political preferences. Third, these attacks can shape political beliefs and policy preferences, particularly among opposition supporters. These results emphasize the importance of examining the broad consequences of politicized attacks on the press for multiple actors, including journalists and citizens. As we demonstrate in Figure 7, our combined results offer several nuanced theoretical insights and implications on the polarizing potential of political attacks on the press.

First, our findings from Studies 1–2 suggest that political attacks on the press—which simultaneously influence targeted journalists' reporting and the public's media consumption patterns—may generate reinforcing effects. By modestly altering both the content of journalists' coverage and the composition of their audience, such attacks may foster the emergence of echo chambers. In practice, our findings suggest that targeted journalists may respond to political attacks by intensifying their criticism of the ruling party and government. Yet, because attacks also reshape patterns of media selection among the general public, exposure to this additional critical media content is unevenly distributed. Specifically, government opponents, whose ex-ante evaluations of the government are already more negative, are disproportionately likely to consume it, while government supporters increasingly disengage from targeted journalists reducing their exposure to criticism of the attacking government. While Studies 1-2 do not allow us to speak directly to how this reinforcement process might polarize the electorate, empirically established insights from the literature on selective exposure to partisan news (Levendusky, 2013*a*; Broockman and Kalla, 2025*b*,*a*), emphasizes the potentially meaningful consequences of this process for broader patterns of societal polarization.

Second, beyond shifting media supply and demand, the evidence from Study 3 emphasizes that attacks on the press have direct consequences for political attitudes. Indeed, our results indicate that attacks on the press do not mobilize government supporters, but instead provoke backlash among opponents. Specifically, exposure to information regarding the Likud attack on Kan 11 had no discernible effect on government supporters' perceptions of democratic backsliding, their preferences for social relations with political opponents, or their trust in the coalition and opposition. In contrast, the same information significantly shaped opposition supporters' beliefs and preferences. Exposure to attacks reduced willingness for cross-party engagement, a patterns with direct

impactions for societal polarization. It also heightened concerns about the health of democracy and lowered trust in government—thereby broadening the partisan gap on these salient political issues.

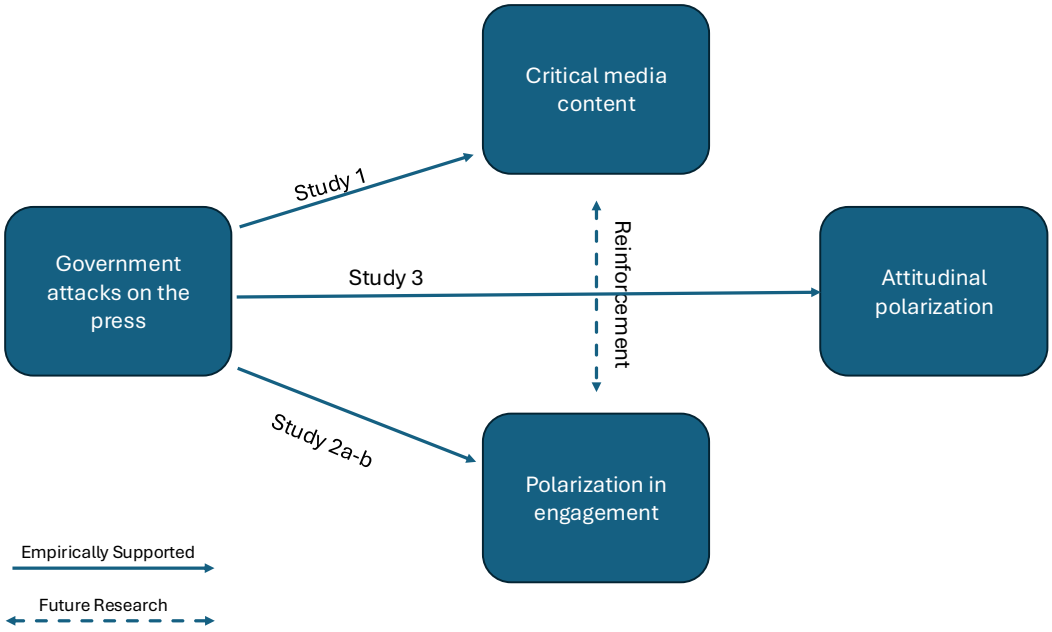

Figure 7: **Overview of Theoretical Framework and Implications.**

Our findings underscore the thus far under-examined polarizing consequences of government attacks on the press, complementing recent studies that emphasize elite behavior as a central cause of polarization (Bassan-Nygate and Weiss, 2022; Nicholson, 2012; Bäck et al., 2023). Moreover, these findings emphasize politicians' central role in (unintentionally) encouraging partisan slant in political news, as well as selective exposure to new sources by citizens. This emphasizes that elites may play a central role in the emergence well-documented patters of selective exposure to increasingly partisan news (Levendusky, 2013*a*; Broockman and Kalla, 2025*b*,*a*).

Conclusions

This paper advances theory, measurement, and empirical evidence on the political consequences of elite attacks on the press. Theoretically, we specify a multi-actor theoretical framework linking targeted political attacks to shifts in media supply (what journalists produce), demand (who selects to consume different news sources, and how audiences evaluate their content), and attitudes (how citizens perceive out-partisans, their elected officials and broader democratic health). Empirically, we pair an event-study of a salient attack on the press leveraging a novel corpus of Israeli radio broadcasts with three preregistered survey experiments to explore how attacks shape media sup-

ply, media demand, and general attitudes. Substantively, we show that political attacks on the press can increase anti-government coverage, polarize the consumption of content produced by targeted journalists, and trigger opposition backlash in terms of heightened concerns over democratic backsliding and rising preferences for social distance—widening the partisan gaps in selective exposure to news and broader political attitudes.

Despite these contributions, this work is not without limitations. First, our supply-side estimates are identified around a single episode in a short time-frame. While the event-study plots do not show strong systematic pre-trends, and are robust to multiple specifications, the Honest-DiD sensitivity analysis indicates that some specifications are more vulnerable to modest deviations from parallel trends. Accordingly, we interpret these results cautiously as consistent with a short-lived backlash effect, and we also view the patterns as descriptively informative evidence of how anti-government content increased in the days immediately surrounding the attack, even if contemporaneous shocks or spillovers to comparison outlets cannot be fully ruled out. Second, transcription and large-language-model classifications filtered by dictionaries may introduce noise or bias that could attenuate supply-side effects. We demonstrate that our use of large-language-models, however is aligned with human coders, a factor which should reduce concerns regarding measurement error. Third, our survey tasks rely on concise vignettes and fictitious bylines to isolate mechanisms. Real-world reactions shaped by reputation, repeated exposure, and social signaling may differ in magnitude. Similarly, our attitudinal outcomes are self-reported immediately post-treatment, raising questions about the longer-term effects of attacks beyond highly controlled survey environments. Thus, we encourage scholars to build on our theory and evidence to identify the effects of media attacks on news consumptions and attitudinal change using complementary, more naturalistic research designs (like the design employed in Study 1).

While our evidence is from the Israeli context—a theory-relevant case characterized by high polarization, visible public broadcasting, plural private media, and credible, public attacks by governing elites—we expect these findings to generalize to other polarized democracies and hybrid regimes. Specifically, in contexts where attacks are public and credible, targeted outlets are salient, substitution toward partisan media alternatives is easy, and press trust is already sorted by partisanship, we expect that political attacks on the press will have similar effects as those detailed above. Effects may be weaker (or potentially manifest in a chilling response) in closed autocracies, and more contingent where media salience and substitution are limited. Thus, we encourage researchers to employ suitable research designs (e.g., case selection via purposive sampling (Egami and Lee, 2024; Bassan-Nygate et al., 2025)), to interrogate questions of external validity.

These limitations notwithstanding, our results suggest that government attacks on the press do more than shift media supply in the short run. They shift incentives and audiences in ways that can entrench partisan information ecosystems. Future work should explore durability of these effects

under repeated attacks, compare effects by attack types (rhetorical, legal, regulatory, economic, harassment/violence), map cross-platform spillovers and audience flows with behavioral data, and test newsroom, civil-society, and platform responses that might mitigate polarization without hindering watchdog journalism.

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Shooting the Messenger:

The Polarizing Effects of Political Attacks on the Press

Supplementary Information

Contents

A Text Analysis

A.1 Dictionaries

Table A1: Hebrew keywords for dictionary-based analyses.

Category	Hebrew Keywords	English Translations
Government	רוהמ ,ראש הממשלה ,ביבי ,בנימין ,נתניהו, אשת ראש ,יאיר נתניהו ,שרה נתניהו ,משפחת נתניהו ,אבנר נתניהו ,הממשלה ,שר ,שרים ,קואליציה ,ליכוד ,ממשלה ,ממשלת.	Netanyahu, Benjamin, Bibi, Prime Minister, PM (abbreviation), Sara Netanyahu, Yair Netanyahu, Prime Minister's wife, Avner Netanyahu, Netanyahu family, government, Likud, coalition, ministers, minister, government of (construct form).

A.2 GPT Prompts

"You are labeling paragraphs from Israeli radio hosts that reference the government. For each paragraph, assess two dimensions:", "1) Topic Polarity: What is the nature of the content being discussed about the government or its members? Is it inherently positive, negative, or neutral? For example, neutrally mentioning a successful policy is a positive topic; neutrally mentioning policy failures or mentioning corruption is a negative topic; describing an event or decision that is neither positive nor negative is neutral. If the content is not relevant to the government/coalition actors, it should be classified as irrelevant. Be sure that the topic polarity is assessed only in reference to the government or particular government/coalition figures, not other actors or topics that might be discussed in the paragraph.", "2) Framing: How does the speaker frame the government or its actions? Is the speaker's tone or attitude positive, negative, or neutral? Positive framing includes praise or support; negative framing includes criticism or disapproval; neutral framing is descriptive or balanced. If the content is not relevant to the government/coalition actors, it should be classified as irrelevant. Be sure that the framing is assessed only in reference to the government or particular government figures, not other actors or topics that might be discussed in the paragraph." "Given

these instructions, how would you label the following paragraph: ”Use only one of: positive, negative, neutral, or irrelevant for each score. Then explain your reasoning by referring to specific words or tone from the paragraph.”

B Validation

Table A2: Percent agreement across coder pairs for topic framing vs. topic polarity.

Coder pair	Variable	Percent agreement
GPT vs RA1	Framing	0.844
GPT vs RA1	Polarity	0.788
GPT vs RA2	Framing	0.851
GPT vs RA2	Polarity	0.818
RA1 vs RA2	Framing	0.909
RA1 vs RA2	Polarity	0.860

C Descriptive Analyses of Radio Data

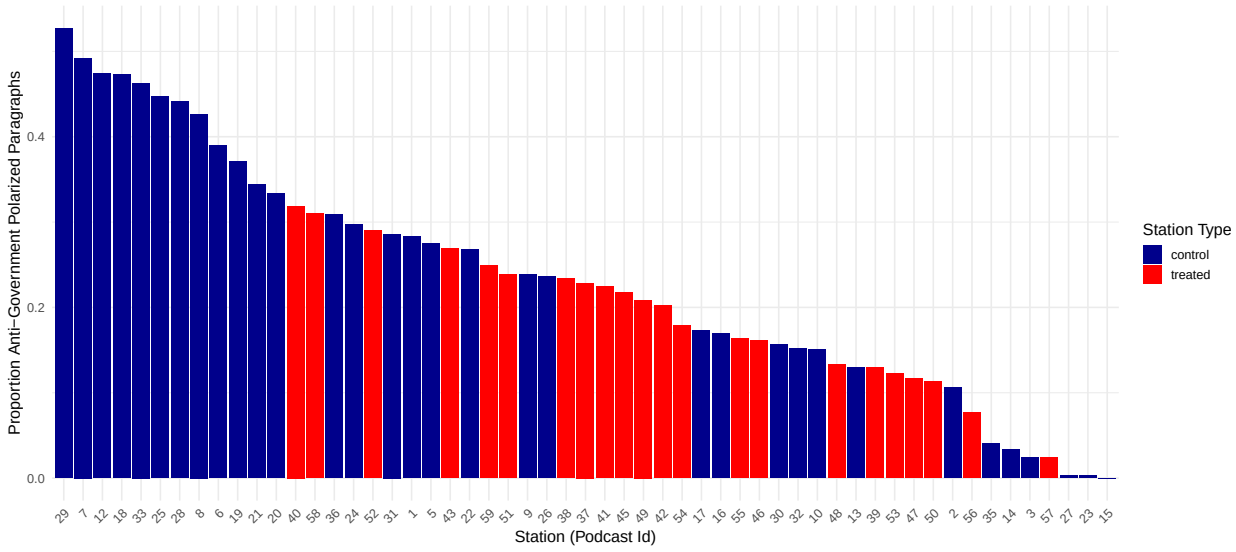


Figure A1: **Proportion of Anti-Government Content in the Pre-Treatment Period.** Anti-government content is measured as the sum of negative references to coalition actors. The proportion is calculated as anti-government paragraphs / total paragraphs.

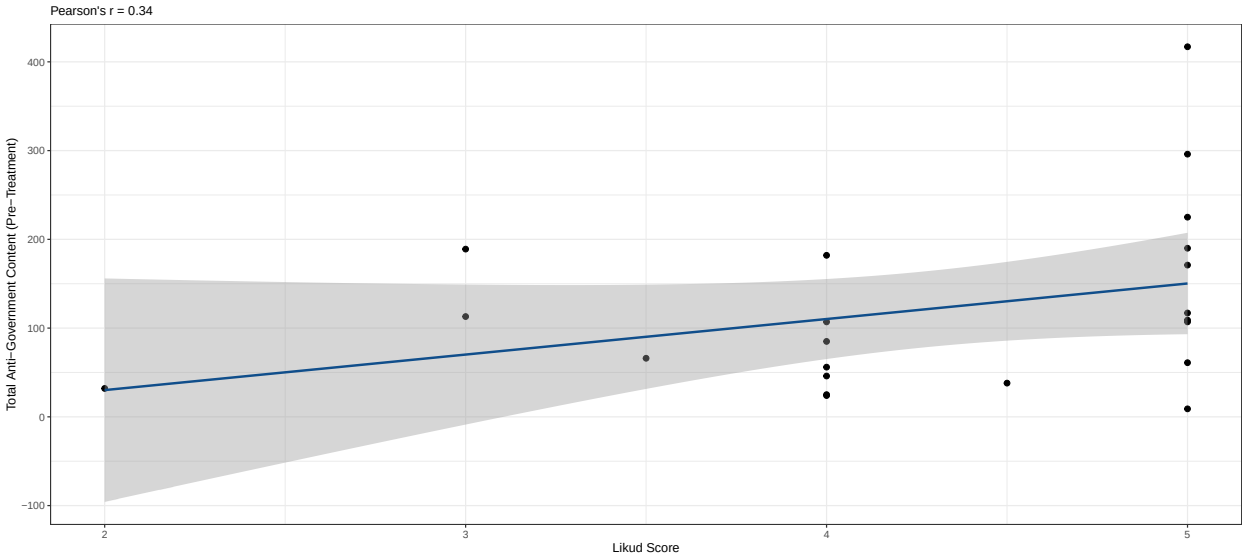


Figure A2: **Correlation between Anti-Government Content and Likud Score for Treated Podcasters in the Pre-Treatment Period.** Anti-government content is measured as negative references to coalition actors. Likud Scores were listed on leaked government list attacking podcasters.

D Event Study

Table A3: **Effect of attack on radio host production of polarized anti-government content across different event-time windows.** This table reports event-study estimates of the December 1 attack's effect on the share of polarized anti-government content in radio broadcasts, using 3-day, 4-day, and 5-day period definitions. Polarization is measured as the sum of positive references to opposition actors and negative references to coalition actors. All specifications include podcast, period, and show fixed effects, with standard errors clustered at the podcast level. Coefficients are reported relative to the reference period ($t = -1$).

Dependent Variable:	Anti-Government Polarized Content		
Model:	4-day window	3-day window	5-day window
Variables			
treatment_num2 × event_time = -6		7.524	
		(5.728)	
treatment_num2 × event_time = -5	7.117	1.614	
	(5.969)	(6.296)	
treatment_num2 × event_time = -4	-1.779	9.829	7.706
	(4.493)	(6.034)	(5.368)
treatment_num2 × event_time = -3	8.004	5.409	1.606
	(4.572)	(6.428)	(4.744)
treatment_num2 × event_time = -2	2.304	5.504	7.145
	(4.537)	(5.836)	(4.243)
treatment_num2 × event_time = 0	9.165*	10.97*	8.333*
	(4.483)	(5.048)	(3.529)
treatment_num2 × event_time = 1	4.138	6.550	1.435
	(5.055)	(5.320)	(4.373)
treatment_num2 × event_time = 2	2.573	3.061	4.259
	(4.396)	(6.680)	(3.864)
treatment_num2 × event_time = 3	3.582	7.848	
	(5.495)	(6.192)	
treatment_num2 × event_time = 4		5.319	
		(6.194)	
Fixed-effects			
podcast_id	Yes	Yes	Yes
period	Yes	Yes	Yes
shows	Yes	Yes	Yes
Fit statistics			
Observations	314	337	290
R^2	0.85361	0.85755	0.86699
Adjusted R^2	0.80418	0.81082	0.81953
Within R^2	0.03362	0.02586	0.03125

Clustered (podcast_id) standard errors in parentheses.

*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

Table A4: **Callaway-Sant'Anna dynamic effects around the intervention (anti-government content).** This table reports dynamic treatment effect estimates from the Callaway and Sant'Anna estimator for the effect of the December 1, 2024 Likud attack on radio hosts' production of anti-government content. We implement the estimator using 3-, 4-, and 5-day aggregations to accommodate irregular broadcast schedules and allow for an unbalanced panel. Standard errors are clustered at the show level. Unlike the TWFE event-study specification, the Callaway-Sant'Anna approach does not rely on a single omitted reference period; estimates are reported by event time for ease of comparison with the TWFE results.

spec	week	estimate	se	conf.low	conf.high
3 day window	-3	-2.932	8.416	-19.426	13.563
3 day window	-2	-1.320	7.900	-16.804	14.163
3 day window	-1	-3.241	9.346	-21.559	15.077
3 day window	0	6.843	8.044	-8.923	22.609
3 day window	1	2.815	5.716	-8.388	14.018
3 day window	2	5.033	8.404	-11.439	21.505
3 day window	3	7.131	8.042	-8.631	22.893
3 day window	4	12.414	9.677	-6.552	31.381
4 day window	-3	10.195	6.563	-2.668	23.058
4 day window	-2	-1.582	7.619	-16.516	13.352
4 day window	-1	-6.529	6.839	-19.933	6.875
4 day window	0	7.959	6.345	-4.477	20.395
4 day window	1	6.400	5.177	-3.746	16.547
4 day window	2	5.375	4.681	-3.799	14.549
4 day window	3	11.893	9.873	-7.459	31.244
5 day window	-3	0.368	10.218	-19.660	20.396
5 day window	-2	7.539	7.818	-7.785	22.863
5 day window	-1	-9.912	4.543	-18.816	-1.007
5 day window	0	9.111	4.659	-0.021	18.243
5 day window	1	3.959	5.556	-6.931	14.849
5 day window	2	5.068	3.391	-1.578	11.715

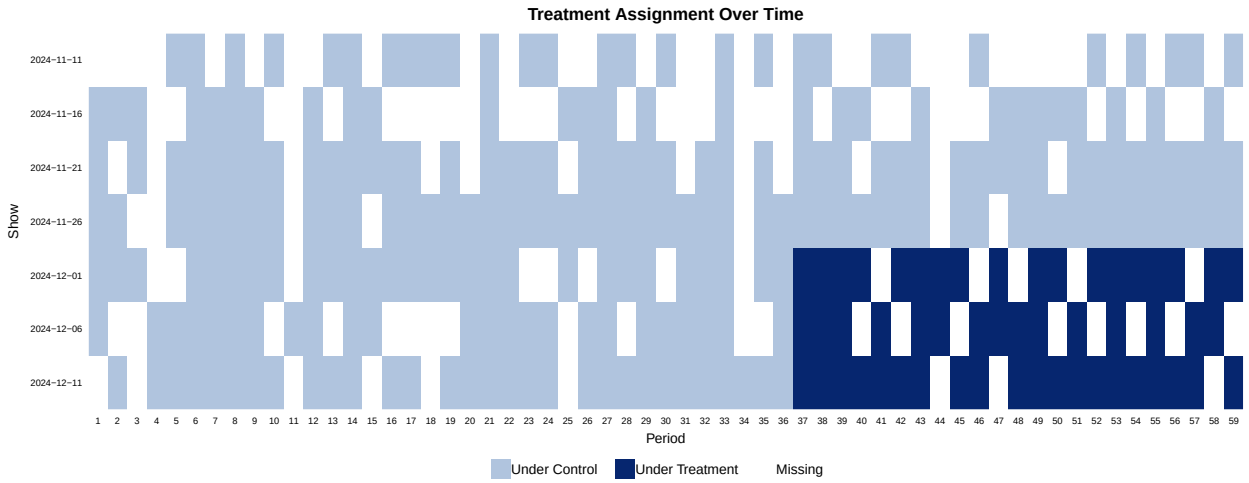


Figure A3: **Treatment assignment over time (five-day aggregation).** This panel-view figure visualizes treatment status across radio shows and time using our five-day period definition, which is the main aggregation used in the analysis. Each row corresponds to a show and each column to a five-day period. The figure illustrates the shift into treatment for targeted shows beginning with the period anchored on December 1, 2024, while comparison shows remain under control; blank cells indicate periods with missing observations.

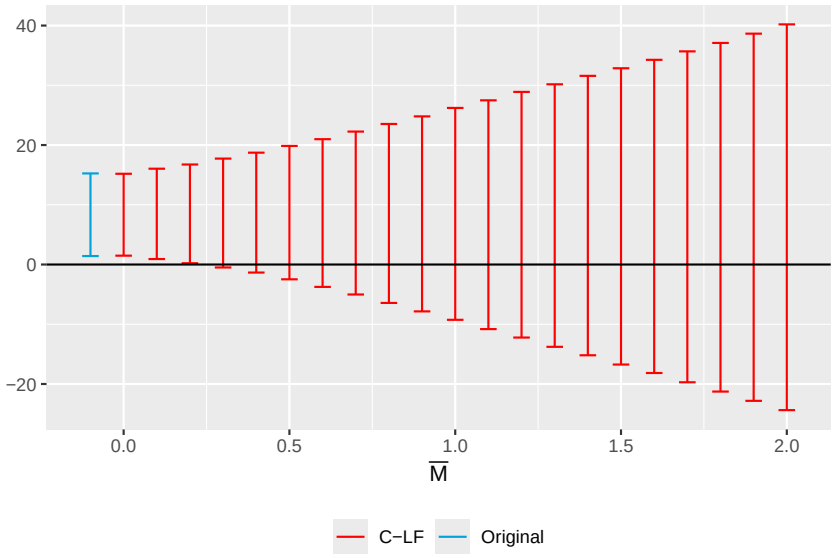


Figure A4: **Honest DiD Sensitivity Analysis for 5-Day Aggregation.** This figure reports the Rambachan and Roth (2023) relative-magnitude sensitivity analysis for the dynamic DiD estimate at $t = 0$, using the five-day aggregation that underpins our main specification. The horizontal axis, $\bar{M}$, indexes the size of post-treatment violations of parallel trends relative to the largest violation observed in the pre-treatment periods, while the vertical axis shows the corresponding confidence intervals for the estimated effect. The blue interval at $\bar{M} = 0$ corresponds to the conventional event-study confidence interval under parallel trends; the red intervals show how the confidence set widens as we allow for larger deviations from parallel trends. For violations $\bar{M} < 0.3$, the confidence interval remains above zero, indicating that the estimated increase in anti-government content is robust to small departures from the parallel trends assumption.

E Survey Studies

Studies 2a, 2b, and 3 in the main text were fielded using an online survey distributed by iPanel between July 22, 2025 and August 5, 2025. After screening on attention, a total of 3,268 respondents were assigned to the treatment or control conditions related to each study. To avoid order effects in selection and evaluation tasks we further randomized the order of studies 2a-b. Following iPanel guidance, we employed quota sampling, considering respondents age, religioisity, locality, and gender, to ensure that our sample mirrors the general Jewish population in terms of key covariates.

E.1 Survey Flow

- **Informed Consent and Introduction.**

- **Demographics and Attention Checks.** Respondents reported basic demographic information, including:
  - Ethnicity
  - Education level
  - Age group
  - Gender
  - Geographic location
  - Religious affiliation
  - Political ideology (7-point left–right scale)
  - News consumption sources
  - Government support
  - We embedded in this block two attention checks. Failure to pass checks resulted in exclusion from the sample.
- **Experiment 1: Journalist Selection.** Participants were told they would answer factual questions about current events and could choose up to three journalists whose articles to read beforehand. Within this task we randomize the topical domain of the exercise to focus on **Law** (judicial independence) or **Health and Technology** (Teva Pharmaceuticals). Moreover, one of four journalist profiles biography was randomized to include a mention of how they have been a subject of government led-political attacks in the past. While journalist names were all fictitious, we employed original new sources (often shortened) from main stream Israeli media outlets.
  - The primary outcome in this study is whether a respondent selects to read about a journalist who has been subject to attacks on the press.
- **Experiment 2: Article Evaluation.** All participants read a short news article written by a journalist described as "Yariv Levi." While Levi was a fictitious journalist, we employed

original, shortened new sources from main stream Israeli media outlets for the purpose of this experiment. Respondents were randomly assigned to one of two topics: AI and the labor market, or Military conscription law for ultra-Orthodox men. After reading the article, reported outcomes of interest, including:

  - *Knowledge Retention:* three knowledge questions (combined into an overall knowledge retention index).
  - *Article Evaluation metrics:* eliciting respondents evaluation of the article's informativeness, professionalism, novelty, and shareability (combined into a 4-item [0-100] evaluation scale).
  - *Perceived Bias:* measuring perceptions of article favorability toward government vs. opposition (single-item measures ranging from 0-100)

- **Experiment 3: Press Attack Treatment.** In the final experiment, participants were randomly assigned to read one of two short vignettes. **Treatment:** a news article depicting the Likud-affiliated campaign attacking Kan radio hosts (https://www.calcalist.co.il/local_news/article/hjbmdcymjl#google_vignette). **Control:** a news article about NASA's plans to return to the moon (https://www.calcalist.co.il/internet/articles/0,7340,L-3850937,00.html). After reading this vignette, respondents completed several outcome batteries measured on 100-point slider scales:

  - *Trust in Institutions:* trust in the government, opposition, Kan, and Channel 14
  - *Perceived Democratic Backsliding:* elciting respondents' pereceptions regarding existing threats to judicial independence, press freedom, and democracy (combined into 3-item index)
  - *Social Distance*: eliciting willingness to accept outparty supporters as close friends, family members, work-supervisors, teachers, or government officials (combined into five-items scale)

– *Recall:* We also asked all respondents whether they recall a recent event in which a Likud-affiliated media outlet targeted Kan radio hosts, ranking them by their level of government support.

Descriptive Statistics and Diagnostics

In Table A5, we report descriptive statistics for our full sample. In Figure A5 we further report balance covariates (top panel), as well as balance across treatment conditions (bottom panel). We show that our three primary treatments do not correlate with pre-treatment covariates or treatments from other studies fielded on the same survey.

In Figure A6 we further visualize patterns of attrition in our survey. Given the length of our survey which included three different studies, we are faced with noticeable patterns of attrition. Importantly, however, as we show in Figure A7, these patterns unlikely pose a threat to attrition. Non of our treatment consistently correlate with non-response to post-treatment outcomes, reducing concerns regarding differential attrition in response to treatment. We do, however, find that attrition correlates with gender for all outcomes, and political preferences for a subset of outcomes. Thus, we encourage readers to interpret our estiamtes as sample average treatment effects.

Table A5: Survey Descriptive Statistics

	Mean	SD	Min	Max	N
Ideology	2.96	1.45	1.00	7.00	3268
Age	3.22	1.58	1.00	6.00	3268
Religiosity	1.68	0.82	1.00	4.00	3268
Female	0.53	0.50	0.00	1.00	3268
Male	0.47	0.50	0.00	1.00	3268
Experiment 1: Selection	0.38	0.48	0.00	1.00	2914
Experiment 2: Number of Correct Answers	2.25	0.88	0.00	3.00	2869
Experiment 2: Article Evaluation	54.42	22.06	0.00	100.00	2817
Experiment 2: Pro-Gov Salience	25.54	25.19	0.00	100.00	2653
Experiment 2: Pro-Opp Salience	35.31	29.90	0.00	100.00	2662
Experiment 3: Backsliding Perceptions	54.30	35.13	0.00	100.00	2492
Experiment 3: Social Distance	53.64	27.98	0.00	100.00	2489
Experiment 3: Trust Gov	37.58	33.08	0.00	100.00	2492
Experiment 3: Trust Opp	33.03	27.02	0.00	100.00	2493
Experiment 3: Trust Channel 11	53.70	31.15	0.00	100.00	2498
Experiment 3: Trust Channel 14	34.67	35.44	0.00	100.00	2491

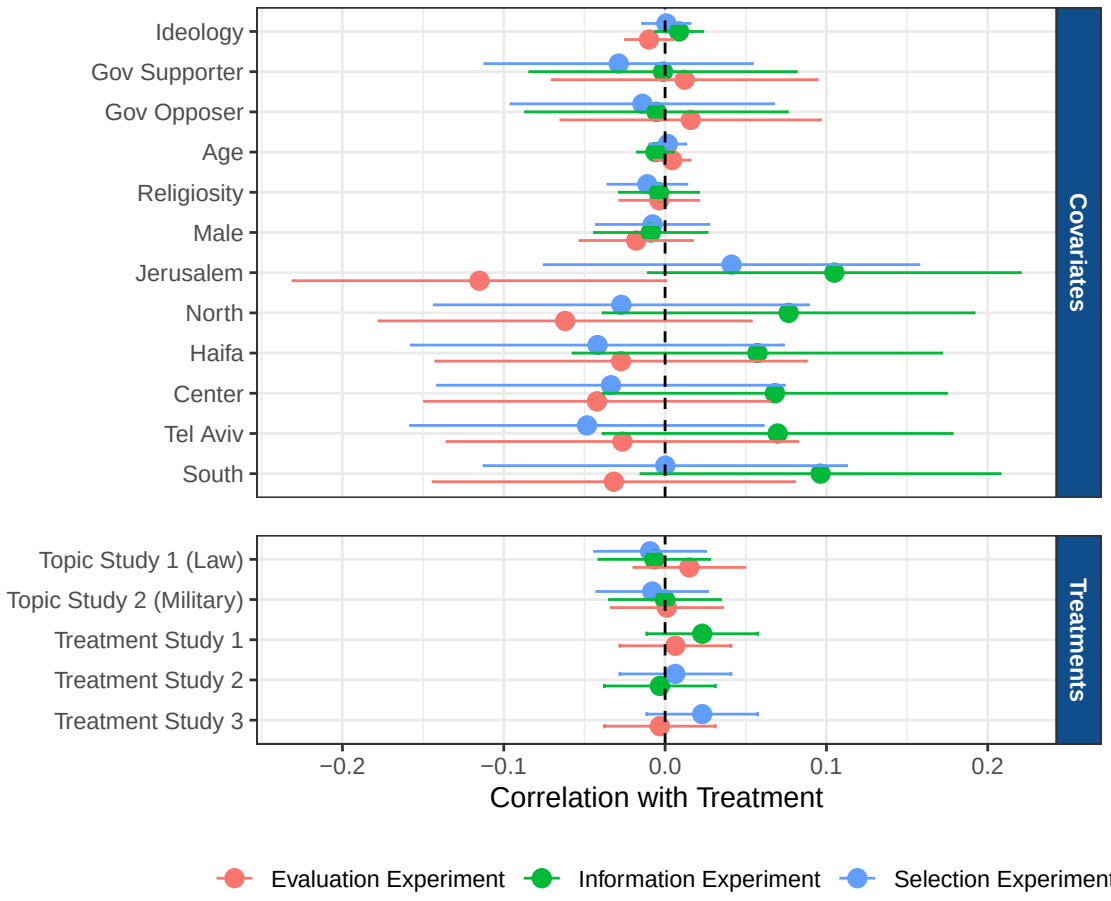


Figure A5: **Balance on Covariates and Additional Treatments.** This Figure reports balance for all three experiments. Point estimates and corresponding 95% CI are extracted from three separated regressions in which a treatment indicator is regressed over covariates and additional treatments. Table format results reported in Table A6.

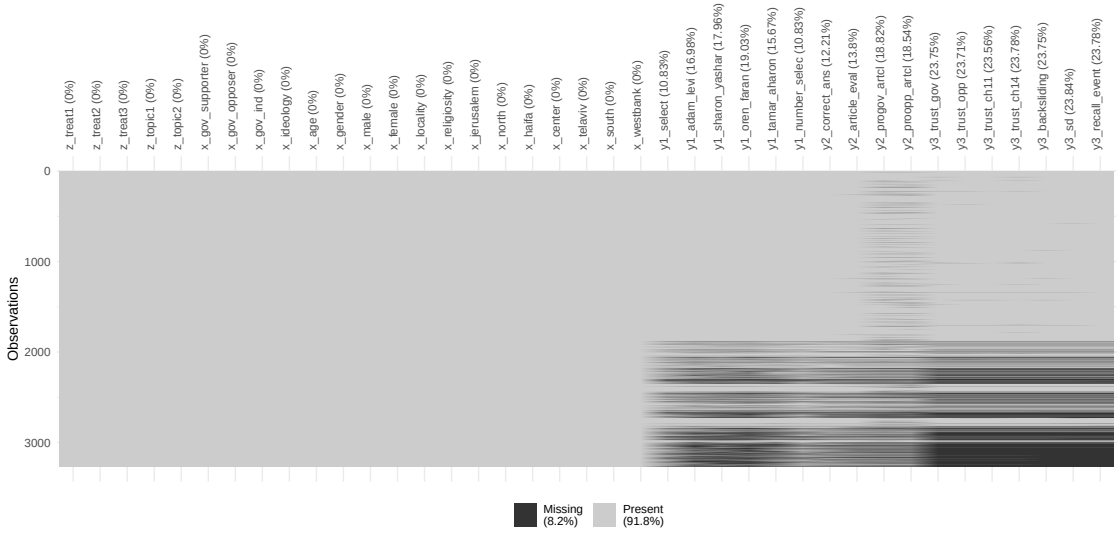


Figure A6: **Patterns of Attrition.** This Figure reports the overall pattern of attrition in our survey, and the stage at which each respondent attrites.

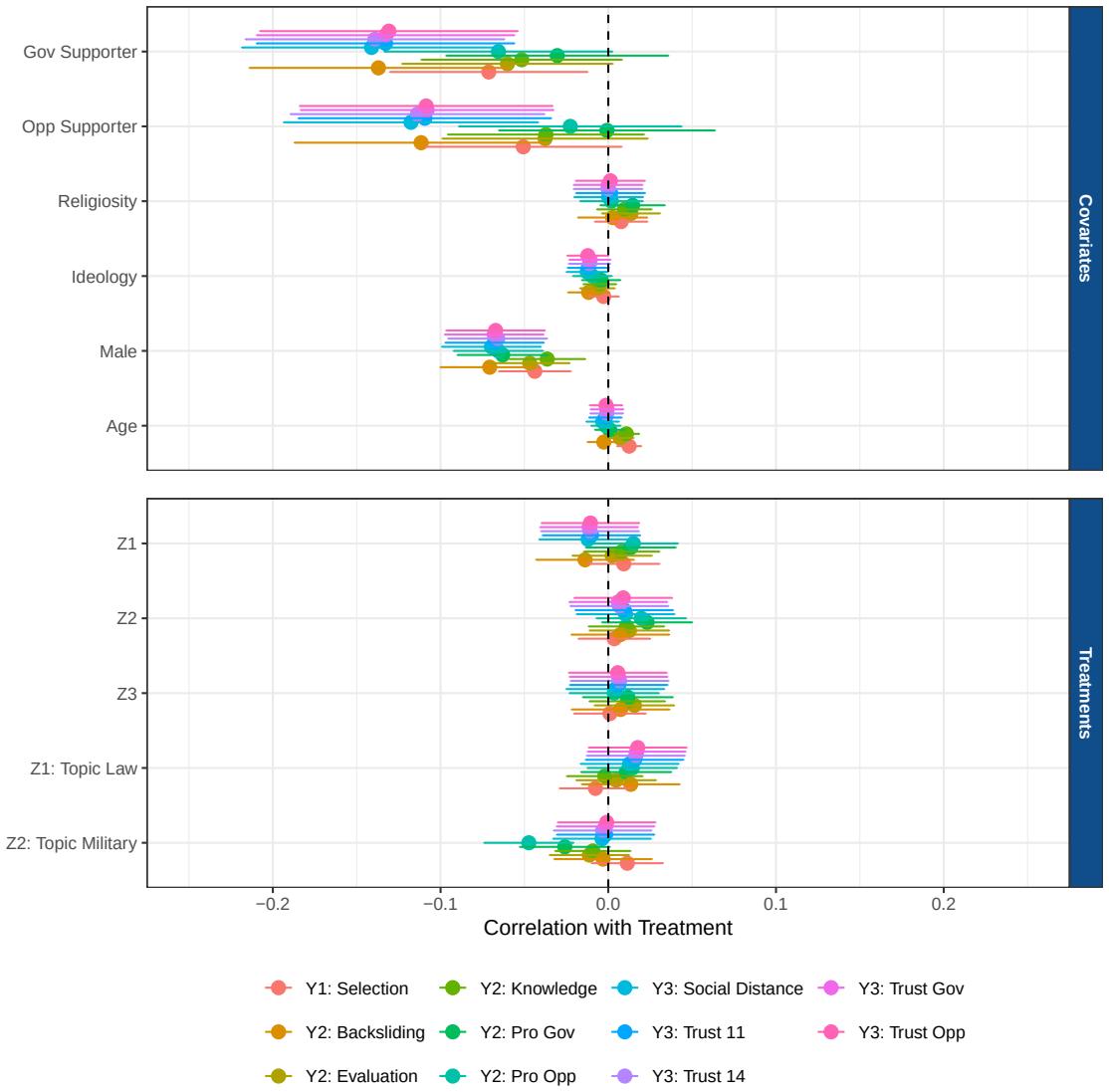


Figure A7: **Correlates of Attrition.** This Figure reports estimates from OLS regressions in which we regress a measure of non-response to each primary outcome, over all treatments and key covariates. We report point estimates alongside 95% confidence intervals showing no meaningful pattern of differential attrition by treatment, but some differential attrition by select pre-treatment covariates.

Additional analyses

In this section, we report additional analyses which we pre-registered in our pre-analysis plan.

Selection Experiment

In Figure A8 we consider whether the effects that attacks on the press have on consumers' selection behavior vary by topic of coverage. Note that we randomized the topic about which respondents were asked to read, in order to consider the extent to which attacks shape selection behavior when consumers focus on political or a-political content. All estimates reported in Figure A8 are directionally similar. While the magnitude of effects among opposition supporters reading about political topics is almost double in magnitude when benchmarked against opposition supporters reading about non-political topics, these differences are not statistically significant. We thus do not find strong evidence to suggest that selection behavior varies across experimental topics.

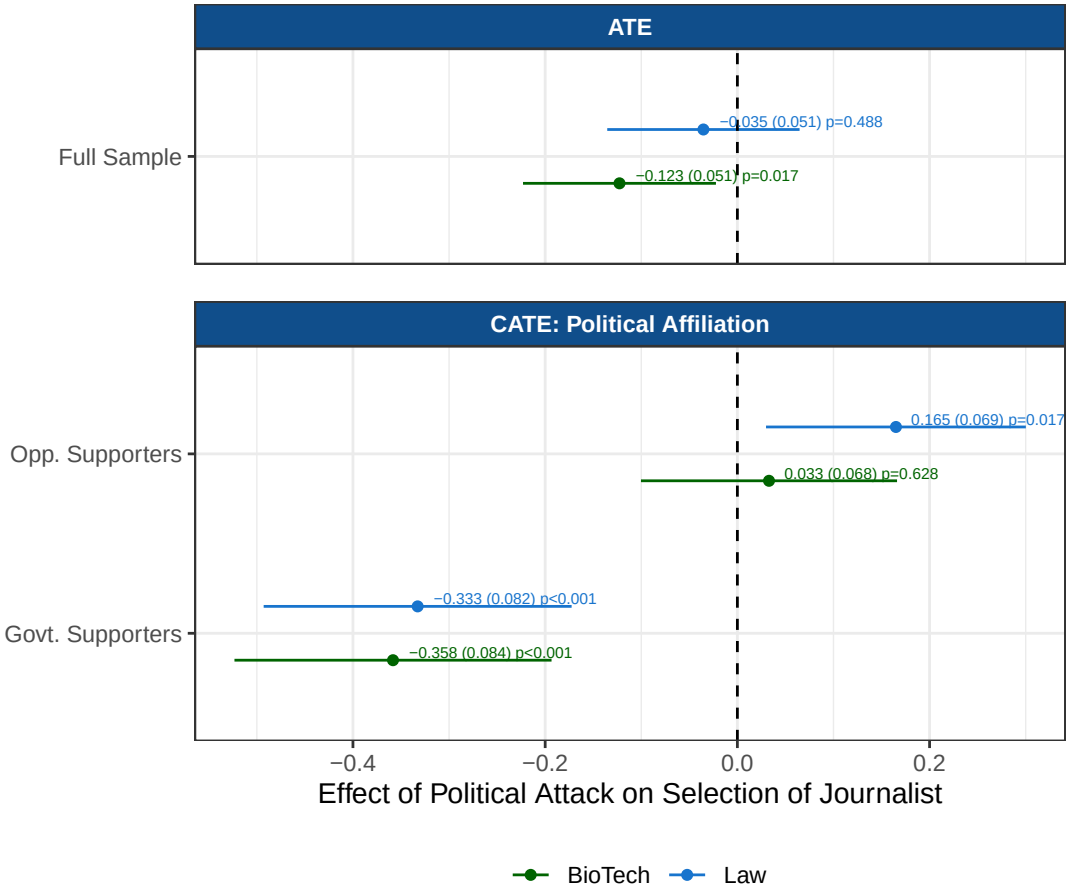


Figure A8: **Effects of Attack on Media Selection by Topic.**

Evaluation Experiment

In Figure A9 we consider variation in how attacks on the press influence evaluation of journalistic content, driven by our topical treatment. These analyses generate several interesting insights. First, among opposition supporters, the negative (positive) effects on knowledge retention (article evaluation) are driven by respondents assigned to a political (non-political) content. Second, effects on perceptions of aricles' political slant are mostly homogenous across topics. When considering effects among government supporters, we find that for all outcomes, other than knowledge retention, government supporters are more sensitive to treatment under the political topic treatment.

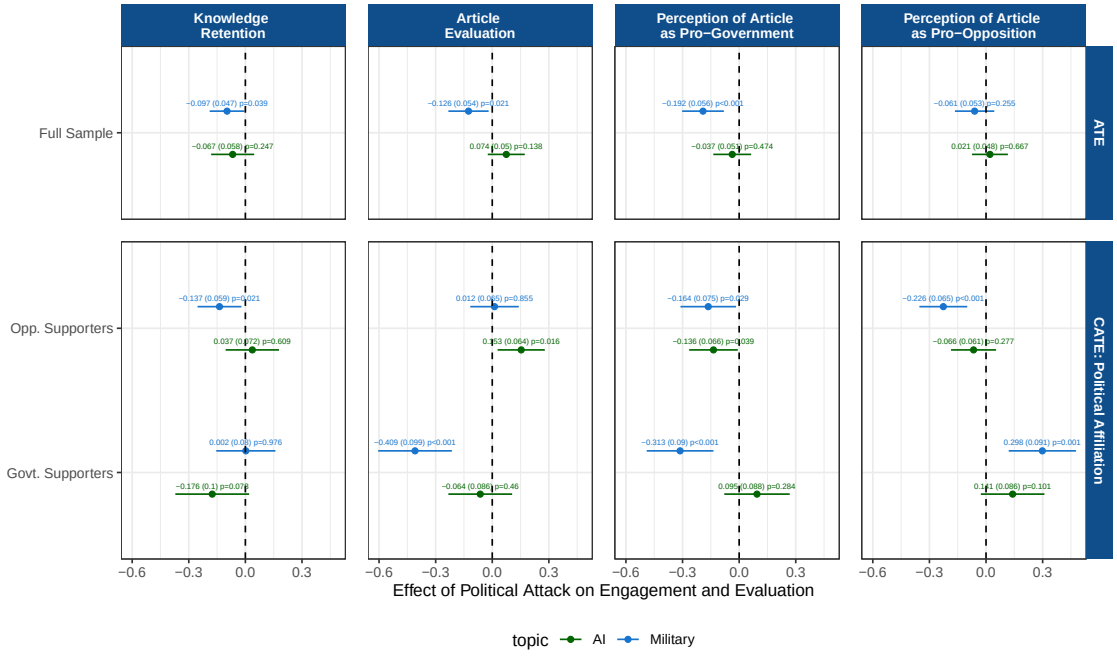


Figure A9: **Effects of Attack on Media Evaluation by Topic.**

To explore the underlying mechanism driving effects on knowledge retention, in Figure A10 we consider the effects of the government attack treatment on the amount of time respondents' spend reading journalistic content. We find that government attacks reduce respondents' time spent on articles, and that this effect is primarily driven by opposition supporters.

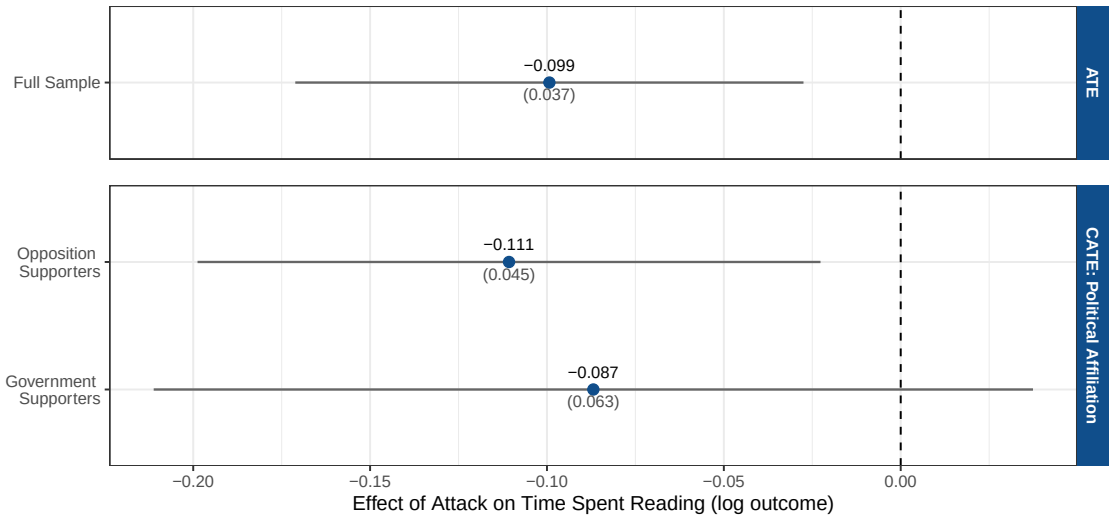


Figure A10: **Effects of Attack on Time Spent on Article.**

Information Experiment

In Figure A11 we report additional results from our information experiment, examining how learning about the Likud attack on Kan radio hosts affect respondents trust in the Kan network, as well as channel 14, a competing pro-government media channel. Overall, we do not find strong evidence that treatment shifted trust in media.

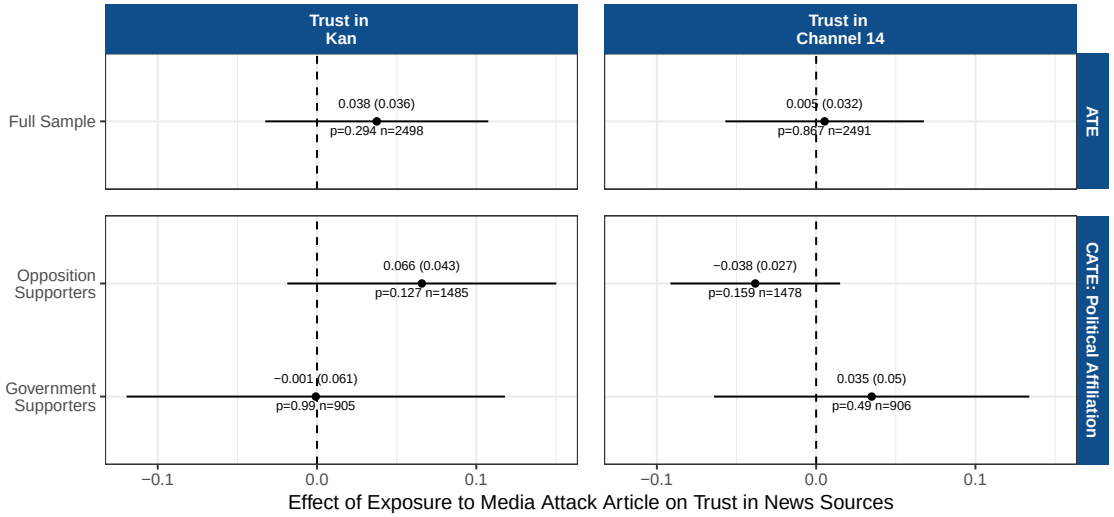
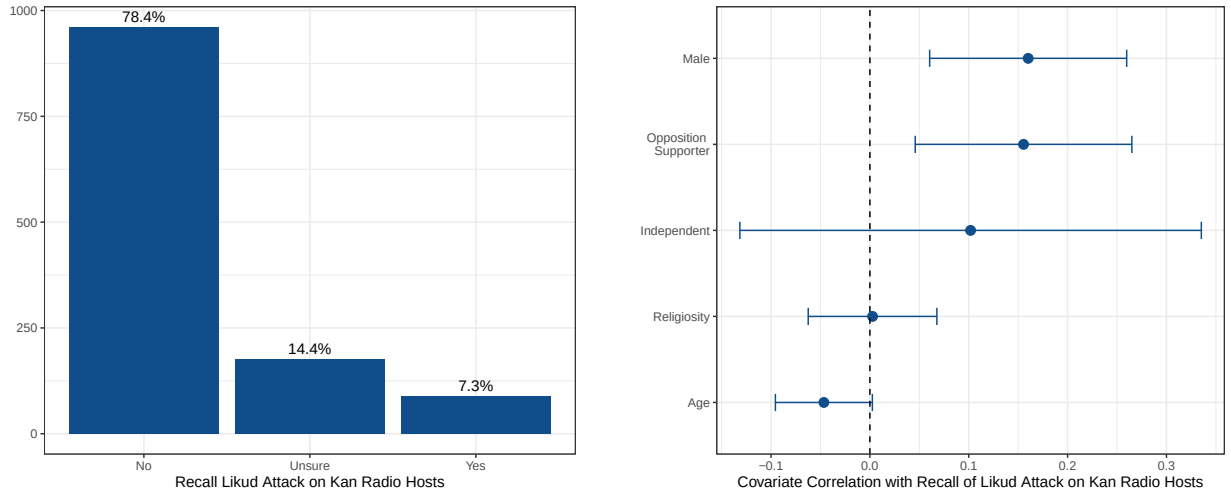


Figure A11: **Effects of Attack on Trust in News Sources.**

Finally, in Figure A12 we examine whether control group respondents who did not receive

exposure to the article depicting attacks on Kan radio hosts recall the event. We show that only 7.3% of respondents recall the event eight months post-treatment, and that men and opposition supporters were more likely to recall these attacks.



(a) Recall of Likud Attack After Eight Months

(b) Demographic Correlates Likud Attack Recall

Figure A12: **Recall Patterns of Likud Attack Against Kan11 Journalists After Eigth Months.** This figure reports descriptive survey data from all respondents who were not exposed to information about the Likud attack on radio hosts in December 2024.

F Table Format Results

Table A6: Balance Tests for Experiments

	Selection Study	Evaluation Study	Information Study
z_topic1Law	−0.009	0.015	−0.007
	(0.018)	(0.018)	(0.018)
z_topic2Military	−0.008	0.001	0.000
	(0.018)	(0.018)	(0.018)
z_treat2	0.006		−0.003
	(0.018)		(0.018)
z_treat3	0.023	−0.003	
	(0.018)	(0.018)	
x_ideology	0.001	−0.010	0.009
	(0.008)	(0.007)	(0.007)
x_age	0.002	0.004	−0.006
	(0.006)	(0.006)	(0.006)
x_religiosity	−0.011	−0.004	−0.004
	(0.012)	(0.012)	(0.013)
x_gov_supporter	−0.029	0.012	−0.001
	(0.042)	(0.042)	(0.042)
x_gov_opposer	−0.014	0.016	−0.005
	(0.042)	(0.041)	(0.041)
x_male	−0.008	−0.018	−0.009
	(0.018)	(0.018)	(0.018)
x_jerusalem	0.041	−0.115+	0.105+
	(0.059)	(0.059)	(0.059)
x_north	−0.027	−0.062	0.077
	(0.059)	(0.059)	(0.059)
x_haifa	−0.042	−0.027	0.057
	(0.059)	(0.059)	(0.058)
x_center	−0.034	−0.042	0.068
	(0.055)	(0.055)	(0.054)
x_telaviv	−0.048	−0.026	0.070
	(0.056)	(0.056)	(0.055)
x_south	0.000	−0.032	0.096+
	(0.057)	(0.057)	(0.057)
z_treat1		0.006	0.023
		(0.018)	(0.018)
Num.Obs.	3268	3268	3268
R2	0.004	0.004	0.003
RMSE	0.50	0.50	0.50

Each regression estimates the correlation between a study's primary treatment and demographic covariates. Models also include other study treatments assigned at the start of the survey.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A7: Effects of Government Attacks on Media Selection

	All respondents	Government supporters	Government opposers
z_treat1	−0.076*	−0.337***	0.099*
z_topic1Law_c	−0.036	−0.013	−0.039
x_ideology_c	0.047*	0.076	0.051+
x_age_c	−0.036*	−0.076**	−0.011
x_religiosity_c	0.039	−0.012	0.032
x_gender_c	−0.231***	−0.153+	−0.288***
(as.factor(x_locality)2)_c	−0.405***	−0.263	−0.545**
(as.factor(x_locality)3)_c	−0.306*	0.023	−0.579***
(as.factor(x_locality)4)_c	−0.376***	−0.255	−0.509***
(as.factor(x_locality)5)_c	−0.335**	−0.224	−0.462***
(as.factor(x_locality)6)_c	−0.339**	−0.135	−0.510**
(as.factor(x_locality)7)_c	−0.186	−0.166	−0.363
z_treat1 × z_topic1Law_c	0.093	0.028	0.142
z_treat1 × x_ideology_c	0.097***	0.082	0.049
z_treat1 × x_age_c	0.003	0.041	−0.021
z_treat1 × x_religiosity_c	−0.109*	0.041	−0.150+
z_treat1 × x_gender_c	0.097	−0.001	0.150
z_treat1 × (as.factor(x_locality)2)_c	0.292+	0.176	0.405+
z_treat1 × (as.factor(x_locality)3)_c	0.064	−0.229	0.252
z_treat1 × (as.factor(x_locality)4)_c	0.292*	0.150	0.370*
z_treat1 × (as.factor(x_locality)5)_c	0.234+	0.013	0.355+
z_treat1 × (as.factor(x_locality)6)_c	0.220	0.269	0.210
z_treat1 × (as.factor(x_locality)7)_c	−0.018	0.124	−0.079
Num.Obs.	2914	1058	1715
R2	0.049	0.074	0.047
RMSE	0.97	0.93	0.98

Each regression estimates the effects of the government attack treatment on a binary indicator taking a value of 1 if a respondent selected to read an article written by a targeted journalist.
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8: Effects of Attacks on Article Evaluation

	Knlg Retention	Knlg Retention	Knlg Retention	Article Eval	Article Eval	Article Eval	Perceive Progov	Perceive Progov	Perceive Progov	Perceive Proopp	Perceive Proopp	Perceive Proopp
z_treat2	−0.086*	−0.091	−0.053	−0.021	−0.229***	0.079+	−0.110**	−0.102	−0.148**	−0.029	0.206**	−0.152***
	(0.037)	(0.064)	(0.046)	(0.037)	(0.066)	(0.046)	(0.038)	(0.063)	(0.050)	(0.036)	(0.063)	(0.045)
z_topic2Military_c	0.351***	0.344***	0.332***	−0.096+	−0.294**	0.030	0.355***	0.286**	0.411***	0.739***	0.959***	0.603***
	(0.051)	(0.090)	(0.064)	(0.052)	(0.094)	(0.063)	(0.053)	(0.088)	(0.070)	(0.049)	(0.087)	(0.062)
x_ideology_c	0.020	−0.016	−0.012	0.029	−0.037	0.030	−0.020	0.043	−0.035	−0.067***	−0.067	−0.025
	(0.020)	(0.046)	(0.026)	(0.020)	(0.047)	(0.025)	(0.021)	(0.047)	(0.028)	(0.019)	(0.045)	(0.025)
x_age_c	0.044**	0.028	0.050*	0.019	−0.012	0.023	0.005	−0.035	0.035	−0.018	0.012	−0.036+
	(0.017)	(0.029)	(0.021)	(0.017)	(0.031)	(0.021)	(0.017)	(0.029)	(0.024)	(0.016)	(0.029)	(0.020)
x_gender_c	−0.085	−0.181*	−0.036	0.035	0.165+	−0.068	0.147**	0.192*	0.131+	0.060	0.051	0.092
	(0.052)	(0.089)	(0.065)	(0.052)	(0.095)	(0.064)	(0.053)	(0.090)	(0.071)	(0.050)	(0.086)	(0.062)
x_locality_c	0.001	0.026	−0.024	−0.011	−0.015	−0.010	0.013	−0.019	0.028	0.030+	0.001	0.047*
	(0.017)	(0.027)	(0.021)	(0.016)	(0.027)	(0.020)	(0.016)	(0.025)	(0.023)	(0.015)	(0.026)	(0.019)
x_religiosity_c	−0.032	0.071	−0.118*	−0.159***	−0.199***	−0.067	−0.041	−0.083	0.000	0.089**	0.068	0.043
	(0.034)	(0.050)	(0.055)	(0.036)	(0.055)	(0.056)	(0.034)	(0.050)	(0.055)	(0.034)	(0.049)	(0.054)
z_treat2 × z_topic2Military_c	−0.032	0.158	−0.181+	−0.193**	−0.357**	−0.132	−0.152*	−0.392**	−0.026	−0.080	0.166	−0.175*
	(0.075)	(0.128)	(0.093)	(0.074)	(0.131)	(0.092)	(0.076)	(0.126)	(0.099)	(0.072)	(0.126)	(0.089)
z_treat2 × x_ideology_c	0.044	−0.010	0.086*	0.080**	0.052	0.028	−0.018	0.036	−0.058	−0.076**	0.059	−0.073*
	(0.029)	(0.068)	(0.037)	(0.029)	(0.071)	(0.037)	(0.030)	(0.071)	(0.041)	(0.028)	(0.067)	(0.036)
z_treat2 × x_age_c	0.020	0.000	0.025	0.033	−0.041	0.074*	−0.008	−0.011	−0.013	0.007	0.033	0.004
	(0.025)	(0.043)	(0.030)	(0.025)	(0.044)	(0.029)	(0.025)	(0.041)	(0.033)	(0.024)	(0.043)	(0.029)
z_treat2 × x_gender_c	0.032	0.164	−0.036	−0.042	−0.332*	0.103	0.037	−0.173	0.136	−0.024	0.013	−0.023
	(0.075)	(0.127)	(0.094)	(0.076)	(0.132)	(0.093)	(0.078)	(0.129)	(0.101)	(0.074)	(0.128)	(0.091)
z_treat2 × x_locality_c	−0.016	−0.041	0.019	0.018	0.013	0.019	−0.009	0.035	−0.037	−0.021	0.035	−0.026
	(0.024)	(0.038)	(0.032)	(0.024)	(0.040)	(0.030)	(0.024)	(0.036)	(0.033)	(0.023)	(0.038)	(0.029)
z_treat2 × x_religiosity_c	0.058	−0.070	0.211**	−0.010	0.070	−0.066	0.040	0.022	−0.004	0.095+	0.081	0.015
	(0.051)	(0.073)	(0.081)	(0.052)	(0.076)	(0.082)	(0.051)	(0.075)	(0.082)	(0.050)	(0.072)	(0.083)
Num.Obs.	2869	1032	1696	2817	1015	1665	2653	961	1559	2662	976	1557
R2	0.044	0.053	0.041	0.058	0.085	0.031	0.033	0.032	0.061	0.171	0.230	0.111
RMSE	0.99	1.02	0.94	0.98	1.03	0.92	0.98	0.97	0.98	0.93	0.97	0.87
Subgroup	Pooled	Gov Sup	Opp Sup	Pooled	Gov Sup	Opp Sup	Pooled	Gov Sup	Opp Sup	Pooled	Gov Sup	Opp Sup

Each regression estimates the effects of the government attack treatment on a different measure of respondents' evaluation of an article.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: Effects of Exposure to Media Attack Article on Political Attitudes and Preferences

	Perceived Backsliding	Perceived Backsliding	Perceived Backsliding	Outparty Soc Dis	Outparty Soc Dis	Outparty Soc Dis	Trust Gov	Trust Gov	Trust Gov	Trust Opp	Trust Opp	Trust Opp
z_treat3	0.046	0.010	0.096**	−0.103**	−0.042	−0.144**	−0.019	0.047	−0.088**	0.057	0.024	0.096+
	(0.032)	(0.045)	(0.034)	(0.040)	(0.066)	(0.049)	(0.034)	(0.049)	(0.032)	(0.039)	(0.063)	(0.050)
x_ideology_c	0.291***	0.031	0.173***	0.020	0.174**	−0.108***	−0.276***	−0.044	−0.105***	0.141***	0.015	0.092**
	(0.019)	(0.036)	(0.020)	(0.024)	(0.060)	(0.030)	(0.019)	(0.036)	(0.017)	(0.022)	(0.046)	(0.029)
x_age_c	0.032*	−0.070***	0.099***	−0.022	−0.021	−0.028	0.002	0.085***	−0.051***	0.045*	−0.082**	0.117***
	(0.015)	(0.021)	(0.016)	(0.019)	(0.032)	(0.023)	(0.016)	(0.021)	(0.015)	(0.018)	(0.027)	(0.022)
x_gender_c	0.082+	0.146*	0.003	−0.022	−0.165+	0.050	−0.012	−0.069	0.075	0.057	0.210*	−0.048
	(0.048)	(0.065)	(0.051)	(0.058)	(0.099)	(0.071)	(0.050)	(0.066)	(0.048)	(0.055)	(0.090)	(0.070)
x_locality_c	0.017	0.029	0.001	−0.015	−0.008	−0.028	0.019	0.018	0.023	0.022	0.010	0.038+
	(0.015)	(0.019)	(0.017)	(0.019)	(0.030)	(0.024)	(0.016)	(0.018)	(0.015)	(0.017)	(0.025)	(0.022)
x_religiosity_c	−0.325***	−0.075*	−0.226***	−0.026	−0.073	0.172**	0.273***	−0.028	0.117**	−0.159***	−0.061	−0.164**
	(0.032)	(0.038)	(0.050)	(0.040)	(0.061)	(0.061)	(0.034)	(0.040)	(0.043)	(0.035)	(0.050)	(0.056)
z_treat3 × x_ideology_c	0.021	0.089+	−0.013	−0.013	−0.012	0.034	−0.033	−0.076	0.020	0.028	0.009	0.043
	(0.025)	(0.051)	(0.028)	(0.032)	(0.078)	(0.042)	(0.027)	(0.055)	(0.023)	(0.031)	(0.067)	(0.043)
z_treat3 × x_age_c	−0.008	0.038	−0.026	0.014	0.046	0.010	0.017	−0.017	0.005	−0.007	−0.001	−0.001
	(0.021)	(0.030)	(0.022)	(0.026)	(0.043)	(0.032)	(0.023)	(0.032)	(0.020)	(0.025)	(0.039)	(0.033)
z_treat3 × x_gender_c	0.064	0.156+	0.032	−0.074	0.059	−0.154	0.041	0.065	−0.034	−0.052	−0.068	−0.065
	(0.066)	(0.091)	(0.071)	(0.081)	(0.134)	(0.100)	(0.070)	(0.098)	(0.064)	(0.079)	(0.128)	(0.102)
z_treat3 × x_locality_c	−0.006	0.008	−0.010	0.023	0.052	0.011	−0.035	−0.056*	−0.018	0.015	0.040	−0.011
	(0.020)	(0.026)	(0.023)	(0.026)	(0.040)	(0.034)	(0.023)	(0.028)	(0.021)	(0.024)	(0.037)	(0.032)
z_treat3 × x_religiosity_c	−0.054	−0.082	−0.017	0.038	−0.059	0.109	0.036	0.035	0.024	−0.056	−0.110	−0.012
	(0.045)	(0.053)	(0.071)	(0.056)	(0.081)	(0.087)	(0.049)	(0.059)	(0.061)	(0.051)	(0.070)	(0.083)
Num.Obs.	2492	904	1481	2489	902	1481	2492	906	1478	2493	902	1483
R2	0.383	0.082	0.216	0.005	0.048	0.061	0.307	0.046	0.102	0.112	0.042	0.095
RMSE	0.80	0.67	0.66	0.98	0.98	0.93	0.85	0.73	0.60	0.97	0.94	0.95
Subgroup	Pooled	Gov Sup	Opp Sup	Pooled	Gov Sup	Opp Sup	Pooled	Gov Sup	Opp Sup	Pooled	Gov Sup	Opp Sup

Each regression estimates the effects of the government attack treatment on a different attitudinal outcome.
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

G Pre-Analysis Plan

Consequences of Media Attacks: Evidence from Israel (#231711)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has 2 authors.

Pre-registered on: 2025/06/03 - 11:20 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
STUDY 1: How does exposure to government attacks on journalists affect selection to consume media produced by those journalists?

STUDY 2: How does exposure to government attacks on journalists affect retention of factual information, as well as perceptions of quality and bias of media produced by those journalists?

STUDY 3: How does exposure to government attacks on journalists affect trust in media, trust in government institutions, partisan animosity, and perceptions of democratic backsliding?

3) Describe the key dependent variable(s) specifying how they will be measured.
STUDY 1: The dependent variable is a binary indicator recording whether or not respondents select to read an article by a fictitious journalist named Oren Faran.

STUDY 2: The first dependent variable measures factual information retention. This is measured with three multiple choice questions (on a 0-3 scale) designed to capture objective information from each article. This will be measured as an index.

The second dependent variable is perceptions of media quality. This is measured as an index constructed from the following multiple choice question: To what extent do you agree or disagree with the following statements? (on a 0-100 scale) a. This article provides objective information and facts; b. The article I read adheres to the highest standard of journalism; c. I learned new information from the article I just read; d. I would share this article with my friends and family.

The third dependent variable captures perceptions of media bias. This is measured with the following multiple choice question: To what extent do you agree or disagree with the following statements? (on a 0-100 scale) a. The article I read is politically biased in favor of the government; b. The article I read is politically biased in favor of the opposition.

STUDY 3: The first dependent variable measures trust in media and political coalitions with the following multiple choice question: On a scale of 0-100 how much you personally trust each of the following: a. The government; b. The opposition; c. Channel 14; d. Kan 11.

The second dependent variable measures perceptions of democratic backsliding as an index with the following multiple choice question: How much do you agree or disagree with the following statements? (on a 0-100 scale). a. The independence of the Israeli civil service is currently under threat; b. Freedom of the press is currently under threat in Israel; c. Israeli democracy is currently under threat.

The third dependent variable measures partisan animosity as an index with the following multiple choice question: Would you be comfortable with someone who strongly supports [Likud / Opposition] in the following roles? (0 = Not at all comfortable, 100 = Completely comfortable): a. As a close friend; b. As your child's teacher; c. As your boss or manager; d. As a romantic partner of your child; e. As a government official making decisions that affect your life

4) How many and which conditions will participants be assigned to?
STUDY 1: There are two main conditions. Respondents assigned to treatment will read that Oren Faran has been attacked by the government, and respondents assigned to control will not have information about whether or not Oren Faran has been attacked by the government. We further randomize the topic of the articles that respondents read to focus on a polarizing or non-polarizing topic.

STUDY 2: There are two main conditions. Respondents assigned to treatment will be informed that the author of the article to which they will be exposed has been attacked by the Israeli government. Respondents in the control group will not receive this information. We further randomize the topic of the article that respondents read to focus on a polarizing or non-polarizing topic.

STUDY 3: There are two conditions. Respondents assigned to treatment will read a real article about government attacks on journalists. Respondents assigned to control will read an article about recent expeditions to the moon.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
We will measure average treatment effects using OLS regression. Our main specifications will adjust for pre-treatment covariates (ideology, age, gender, locality, religiosity, and experiment topic when relevant) using the Lin (2013) estimator. We will report robust standard errors. Note that for all indices we will report the aggregated index as well as the individual items.

For studies 1 and 2 we focus on six primary analyses: 1) the full sample, 2) full sample with polarizing topic, 3) full sample with non-polarizing topic, 4) government supporters only, 5) opposition supporters only, and 6) independents only.

For study 3, we focus on four primary analyses: 1) the full sample, 2) government supporters only, 3) opposition supporters only, and 4) independents only.

For all three studies, in secondary analyses, we will also examine treatment effects by party and experimental topic.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
NA

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
2500 respondents

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
Nothing else to pre-register.