# Foreign Policy Debates Shape Refugees' Psychological Integration *

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**Abstract**

Refugee integration is a core policy concern. Expanding on prior research on domestic policies and elite cues, we explore whether host country debates about military aid to refugees' homeland shape the psychological integration of refugees. We implement a survey experiment with 2,631 Ukrainian refugees in Germany. Participants viewed authentic statements by German politicians with varying support for military aid. While average effects of the intervention are null, this masks substantial heterogeneity. Linking our experiment to day-to-day variation in national news coverage, we find that, when media salience of the conflict is low, exposure to any political statement on military aid, whether supportive or opposing, reduces psychological integration. These effects disappear when the issue is already prominent in the news. We argue these patterns reflect a contestation mechanism: refugees treat the mere visibility of political debate, rather than its specific content, as a signal that host country support is contested and therefore less secure.

**Keywords:** Military aid, Refugee integration, Ukraine, Germany, Survey experiment

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# 1 Introduction

Refugee integration is a pressing global concern that carries profound implications for both displaced populations and host societies (UNHCR, 2023). As conflicts worldwide continue to displace millions, understanding the determinants of refugee integration is essential for promoting social cohesion, economic development, and humanitarian objectives (Martén, Hainmueller and Hangartner, 2019; Harder et al., 2018).

After Russia's full-scale invasion of Ukraine, European countries faced the dual challenges of responding to an inflow of refugees and deciding on the extent of military engagement in the ongoing Russo-Ukrainian War. Germany, in particular, remains the second most popular destination for Ukrainian refugees within the EU (Kinkartz, 2024), while also playing an important role in determining the level of military support to Ukraine. While the German government provides social programs to support Ukrainian refugees and extends military, economic, and humanitarian aid to Ukraine, the degree to which Germany should provide military support has been frequently debated. The German population is also divided on the topic (Hopkins, 2023). This debate is still ongoing: in March 2025, amid the uncertainty about the US military aid, the EU Commission proposed increased support for Ukraine,[1] and the US's recent decision to reduce its own commitments has heightened the pressure on European countries to maintain or step up their engagement.[2]

Prior research has examined the role of direct policies and political messaging about refugees themselves on integration (Hainmueller, Hangartner and Pietrantuono, 2015; Fouka, 2023; Zonszein, 2025). However, just as domestic policies of host countries can affect refugee integration outcomes, national debates over military engagement in the conflict could affect refugees' perceptions of support and attitudes towards the host country by signaling overall (lack of) support for their country of origin and its citizens. When politicians publicly oppose

[1] https://www.europarl.europa.eu/news/en/press-room/20250310IPR27219/meps-debate-future-of-eu-defence-and-support-for-ukraine

[2] https://www.theguardian.com/world/2025/mar/04/us-military-aid-ukraine-pause-trump-zelenskyy-updates

military aid to refugees' country of origin, it may lead to group-based resentment towards the host country reduced perceptions of psychological safety and trust in the host society, hindering successful integration (Phillimore, 2012; Kuhn and Maxwell, 2023; Weiss, 2025). Conversely, political support for military aid may signal acceptance and inclusion, potentially increasing refugees' motivation to integrate (Hainmueller, Hangartner and Pietrantuono, 2015).

In this paper, we provide the first evidence on this topic by examining the effect of German debates about military support for Ukraine on refugees' feelings of belonging, perceptions of the host country more generally, and their intentions to stay. To do so, we conducted a survey experiment on a sample of 2,631 Ukrainian refugees who resided in Germany as of March 2024. Given the difficulty of reaching this population, we used targeted online ads to recruit our study participants and expose participants to authentic statements by members of the German government, with varying levels of support for increased military support for Ukraine.

To assess the effect of these treatments, we construct three outcome indices based on respondents' self-reported (i) sense of belonging, (ii) perceptions of general refugee support, and (iii) intentions to leave Germany. While these are not long-term behavioral integration outcomes per se, prior work shows that perceived acceptance and support impact downstream outcomes—including labor market participation, language acquisition, and civic engagement (Schilling and Stillman, 2024; Djogbenou, Adjiwanou and Lardoux, 2025; Szaflarski and Bauldry, 2019).

In the full sample, there is little evidence that the treatments shifted these outcomes. Investigating mechanisms, we find that these null results are *not* driven by respondents who see military support as counterproductive. Similarly, self-reported consumption of German mainstream news does not moderate effects.

We then investigate whether short-term fluctuations in the information environment, rather than long-term media consumption levels, may explain the full-sample null results.

Building on work showing that individuals' attitudes are influenced most significantly by recent news coverage (Tesler, 2015; Zaller, 1992), we contend that high salience of debates over military support of Ukraine in German media shortly before the survey may have saturated the information environment and muted the effects of our experimental treatments. Put differently, the short-term information environment may condition the effects of the political messages we present respondents with.

To test our argument, we leverage daily variation in coverage of Ukraine in German media, proxied by coverage in the *Tagesschau*, Germany's most-watched TV news program. For each respondent, we code whether Ukraine was mentioned in the Tagesschau on the evening before she took the survey. Based on secondary evidence, we argue that Tagesschau coverage provides a good proxy for the salience of the topic at the time the respondent completed the survey (Krämer and Schroll, 2009). This proxy for day-to-day variation in German news coverage thus allows us to assess whether treatment effects are conditional on contemporaneous media salience.

Our findings reveal two patterns that support the information saturation argument. First, we find suggestive evidence that the national information environment independently shifts baseline perceptions among control group respondents. Following coverage in the national news, control group respondents reported lower perceptions of military support, general support for Ukrainians, belonging in German as well as increased return intentions. Moreover, we find that these patterns are independent of the valence of national media coverage, i.e. any reporting on Ukraine is associated with the same attitudinal effect.

Second, among respondents surveyed on days when Ukraine was not covered in the news, clear experimental effects emerge. In this *low-salience environment*, all three treatments (supportive, ambivalent, and oppositional) reduced perceived German support, increased return intentions, and decreased feelings of belonging in Germany. These shifts parallel how control-group perceptions differ between no-mention and mention days. By contrast, when Ukraine received any media coverage the preceding evening, treatment effects are near zero

across all outcomes. This pattern is consistent with an information-environment saturation mechanism explaining the pooled null effects.[3]

Additional checks reduce concerns about unobserved confounding. First, days with and without a Tagesschau mention of Ukraine are balanced on a wide range of respondent characteristics. Second, estimates are robust to adding a linear time trend and to alternative standard error calculations. Third, a permutation test using placebo coverage dates supports the main heterogeneity result. Finally, a partial replication with an independent survey of Ukrainian refugees in Germany reproduces the valence-independent effect of media coverage on attitudes.

We therefore conclude that the treatment had statistically and substantively significant effects when ambient media coverage of the debate was low, and these experimental results are closely mirrored by observational shifts of control-group baselines depending on the ecological information environment.

Our main contribution, therefore, is to show that debates about military support shape refugees' feelings of belonging, perceptions of the host country, and intentions to stay or return. Contrary to what we expected based on previous research, we document a valence-independent pattern: under low salience, all kinds of cues—supportive, ambivalent, and oppositional—shift outcomes in the same direction. This departs from arguments that emphasize directional updating under elite disagreement (Zaller, 1992; Chong and Druckman, 2007; Druckman, Peterson and Slothuus, 2013). Instead, our results suggest that the mere presence of debate may act as a signal of contestation that drives perceptions of support.

More broadly, our work contributes to several strands of research on refugee integration, political messaging, and conflict attitudes. First, our findings broaden the scope of research on how political rhetoric shapes the attitudes of immigrants and other minority groups (Pérez, 2015; Grewal and Hamid, 2024; Secen and Öztürk, 2024; Tyrberg, 2024). We identify an indirect channel through which elite discourse about a homeland conflict affects perceptions

[3] Most interviews occurred on days following Ukraine coverage in German news.

of acceptance and inclusion in the host society. Unlike prior work, which has studied rhetoric that explicitly targets immigrants or minorities, we focus on political messages that address military support to refugees' country of origin. We focus on this debate because, first, the timing of a war makes military support questions especially salient and dominant in public discourse, and second, such debates are rooted in the broader question of whether host societies feel responsible for refugees—shaping how refugees interpret themselves as valued or ignored. We show that such messages, despite not mentioning refugees directly, can nonetheless influence perceptions of belonging and return intentions under certain conditions. Our findings suggest that refugees interpret political cues about the conflict they fled not only as foreign policy positions, but also as signals of their social status and inclusion in the host society.

Second, we contribute to research on the conditional effects of political communication by showing that elite cue effects may hinge on short-term salience in mainstream news. We find that when ambient media coverage does not mention Ukraine, brief elite statements move perceived host support, belonging, and return intentions. When Ukraine is covered, effects vanish, consistent with saturation and cue competition (Prior, 2007; Bullock, 2011) and qualifying previous research that finds that experimental treatments are more effective than real-world coverage (Jerit and Barabas, 2012). This also underscores the importance of measuring short-term media fluctuations as a potential moderator of political persuasion, distinct from self-reported, long-term media consumption.

Methodologically, we provide a blueprint of how to investigate this short-term salience mechanism. We rely on time-varying measures of the ambient media environment, matched with survey completion dates, to investigate how treatment effects may be conditional on the short-term media salience of a specific topic. We contend that this approach—combing experimental cues and observational media exposure data—enables researchers to distinguish true null effects of informational cues from false negatives driven by pre-treatment exposure through the organic media environment.

## 2 Why Might Military Aid Debates Might Matter?

Refugee attitudes are shaped not only by integration policies but also by signals of acceptance inferred from broader political discourse (Berry et al., 2006; Phillimore, 2012; Harder et al., 2018; Fouka, 2023; Zonszein, 2025). Such signals are often conveyed through elite cues—politicians' statements or policies—that refugees may use as heuristics for wider public attitudes, updating perceptions of belonging and intentions to remain (Hainmueller, Hangartner and Pietrantuono, 2015; Hall and Werner, 2022). Elite rhetoric shapes attention and evaluation, and in cue-taking models, visible disagreement can dampen persuasion by activating counter-considerations (McCombs, Shaw and Weaver, 2014; Tesler, 2015; Zaller, 1992; Druckman and Leeper, 2012; Druckman, 2022).

Even brief political messages shift beliefs and behaviors among minority groups. Negative ads increased political engagement among Latino Americans (Besco et al., 2022; Pérez, 2015); inclusive statements by Chancellor Merkel reduced perceived discrimination among Muslims in Germany (Grewal and Hamid, 2024); and welcoming versus hostile remarks altered Syrian refugees' political engagement and sense of belonging (Secen and Öztürk, 2024; Tyrberg, 2024). Across these settings, elite rhetoric functions as a signal of group standing (Kuhn and Maxwell, 2023).

A key limitation of this work is its focus on direct rhetoric toward established minorities (e.g., Grewal and Hamid, 2024; Pérez, 2015). We extend this research to the context of indirect cues through military support debates, where the symbolic inclusion or exclusion communicated by elite rhetoric may be particularly potent. Lacking deep-rooted knowledge of host-country politics, and with the fate of their homeland at stake, recent refugees of war are highly motivated to scan prominent elite debates—such as those over military aid to Ukraine—for signals about their social standing and the durability of their welcome. This focus highlights a novel pathway through which elite discourse shapes integration outcomes.

## 2.1 Symbolic Role of Military Aid

While debates over military aid are, on their surface, about geopolitics, they may function as powerful symbolic signals for affected refugee populations. Arguments over German aid to Ukraine bundle concrete policy with an implicit question of continued German material and moral investment. Under conditions of high uncertainty and personal threat, Ukrainian refugees are likely to interpret this symbolic layer as diagnostic information about expected empathy, solidarity, and long-run commitment, shaping their sense of belonging and future prospects (Sasse and Lackner, 2020; Harm Adema et al., 2023; Al Husein and Wagner, 2023).

Two channels make this plausible. First, minorities use elite statements as heuristics for mass sentiment. Second, social identity and system-justification perspectives hold that individuals scan elite discourse for cues to their group's esteem and future security (Tajfel and Turner, 2004). As a result aid debates can serve a double symbolic role: they communicate not only the degree of domestic solidarity with displaced Ukrainians but also the depth of Germany's geopolitical alignment with their national in-group (e.g., Fearon, 1994; Tomz, 2007). Both layers can then be projected by refugees' onto their own status and safety. Supportive rhetoric, by signaling sustained commitment abroad and empathy at home, reassures psychological security, while oppositional rhetoric primes uncertainty about protection or hospitality (Stevens and Thijs, 2018; Hall and Werner, 2022)[4].

Applied to our case, statements favoring increased military assistance can transmit a compound signal about material commitment and moral alignment. This should elevate perceptions of support and belonging by affirming refugees' social status as a deserving group and reinforcing their sense of psychological safety. Stronger perceived host commitment should then reduce near-term return intentions by increasing expected integration payoffs (Al Husein and Wagner, 2023).

[4] Prior experimental studies show that even brief cues on unrelated topics can shift minorities' trust and belonging (Grewal and Hamid, 2024; Pérez, 2015; Secen and Öztürk, 2024; Tyrberg, 2024), consistent with heightened threat sensitivity among displaced groups (Jaschke, Sardoschau and Tabellini, 2022; Weiss, Siegel and Romney, 2023).

Conversely, while opposition to aid could be framed as pragmatic caution, for the affected group it is more plausibly interpreted as a signal of waning solidarity. It suggests a declining willingness to bear costs on behalf of Ukrainians, which can weaken belonging by casting doubt on the durability of their welcome. Such cues could also alter expectations about conflict duration, but the primary update concerns *host solidarity*, not war timelines. Identity-relevant status signals shift affect and belonging quickly (Stevens and Thijs, 2018; Hall and Werner, 2022; Secen and Öztürk, 2024), shaping our expectation of reduced perceived support and belonging and (weakly) higher return intentions.

In summary, the symbolic-signal logic yields clear directional expectations for pro- and anti-aid statements. These predictions follow from a framework that treats foreign policy not as separate from domestic inclusion, but as a critical site where belonging is negotiated for diaspora communities, thus identifying an overlooked pathway through which elite discourse shapes minority integration. Based on these theoretical considerations, we derive our primary hypothesis[5]:

**H1:** Exposure to German politicians' statements supporting/opposing military aid to Ukraine increases/decreases Ukrainian refugees' perceptions of overall German support, feelings of belonging, and intentions of staying in Germany, relative to non-political messaging.

We pre-registered two additional moderation hypotheses. First, some refugees may view military aid as undesirable, believing it prolongs conflict and signals German intent to send them home. Such reactions likely depend on preferences for war termination: those favoring negotiations may respond differently than those supporting continued combat (Fabbe, Hazlett and Sınmazdemir, 2019; Lyall, Blair and Imai, 2013). Second, prior media exposure may condition treatment effects. Refugees who frequently consume German news already have strong perceptions of the government's stance, reducing the novelty and persuasiveness of our treatments (Druckman and Leeper, 2012; Druckman, 2022). Based on these theoretical

[5] Hypotheses presented here are simplified compared to the ones we pre-registered. See Appendix A.2 for a discussion of the deviations in hypotheses we state here from the pre-analysis plan.

considerations, we derive two additional hypotheses relating to effect moderation by war termination preferences (**H2**) and self-reported consumption of German media (**H3**).

**H2:** Exposure to German politicians' statements supporting/opposing military aid has larger positive/negative effect among (a) *refugees who prefer continued combat to protect all of Ukraine's territories over negotiations*; and (b) *refugees who do not consume German news.*

## 2.2 Alternative Mechanisms

While the above *symbolic inclusion* mechanism provides our pre-registered baseline expectations, other pathways may operate even if refugees do not treat aid debates as solidarity cues.

**Contestation.** Political debates can carry informational value that derives less from the valence of any single statement and more from the publicly observable fact of sustained *contestation*. Work on framing and cue competition in American politics shows that when multiple elite positions circulate, audiences update not only on policy content but also on meta-information about issue security, coalition cohesion, and likely durability of support (Slothuus, 2010; Leeper and Slothuus, 2014; Druckman, Levendusky and McLain, 2018). Studies of information saturation and selective exposure further suggest that once an issue is repeatedly cued, marginal messages increasingly function as signals that support is not unanimous (Guess, Nyhan and Reifler, 2018; Bullock, 2011; Nicholson, 2011). Unlike the symbolic mechanism, which implies directional effects based on the positivity or negativity of aid rhetoric, this "contestation mechanism" predicts valence-independent effects: what matters is not the content of the message but the public visibility of disagreement.[6]

[6] To our knowledge, this valence-independent pathway has not previously been shown to operate in the immigration or refugee integration context, and evidence of it would provide a novel explanation for why supportive and opposing cues can generate parallel negative downstream effects on integration outcomes. Below we document evidence of valence-independent effects on low-salience days consistent with this mechanism.

**Backfire.** Supportive rhetoric could also reduce, rather than increase, integration prospects. First, refugees may interpret pro-aid statements not as lasting solidarity but as signals that protections are temporary and contingent on the continuation of the war. A large literature shows that insecure status discourages early investments in language, employment, and community ties (Kosyakova and Brenzel, 2020; Menjívar, Agadjanian and Oh, 2022). Related U.S. evidence on the "public charge" debates demonstrates that even rhetorical threats of future retrenchment can depress engagement with host institutions (Bernstein et al., 2019). Second, refugees may construe military assistance itself not primarily as a sign of solidarity but as part of a broader effort aimed at reducing refugee inflow to Germany–similar to how European foreign aid to African nations is often framed as migration deterrence (Clemens and Postel, 2018). From this perspective, support for aid may signal a less welcoming environment, while opposition might confer that Ukrainians are welcome (to stay) in Germany.

Table 1 summarizes the distinct empirical predictions of the three mechanisms we posit above. If military aid is interpreted as a directional signal about the support for Ukrainians, pro-aid statements should increase psychological markers of integration, and anti-aid statements should have the opposite effect. If any statement–regardless of its valence–acts as a sign of contestation and lack of consensus, even supportive or ambivalent statements should reduce perceptions of support and staying intentions. If military aid is seen as an attempt to quickly restore piece in order to force refugees to return and prevent future refugee inflow, supportive statements may have negative effects on perceptions of support and staying intentions; opposing statements should have the opposite effect.

Table 1: Empirical predictions by mechanism

| Mechanism | Support | Ambivalent | Opposition |
|---|---|---|---|
| Symbolic | $\uparrow$ | $\approx 0$ | $\downarrow$ |
| Contestation | $\downarrow$ | $\downarrow$ | $\downarrow$ |
| Backfire | $\downarrow$ | $\approx 0$ | $\uparrow$ |

*Notes:* Arrows indicate expected direction of effects across perceived overall support, belonging, and staying intentions.

# 3 Experimental Design

## 3.1 Sample Recruitment and Intervention

To test these empirical predictions with Ukrainian refugees in Germany, we conducted an online survey experiment. Since it is impossible to reach this population with conventional sampling methods, participants were recruited through targeted advertisements on Meta's platforms, Facebook and Instagram. Recent surveys indicate that 96% of Ukrainian refugees in Germany with internet access use these platforms at least occasionally, and 74% use them frequently (European Centre for Press and Media Freedom, 2023). This high engagement rate among our target population suggests recruitment via Meta ads likely avoids significant selection biases common to online surveys of migrants in other contexts (Pötzschke, 2022).

To minimize entries from ineligible respondents, ads were geographically restricted to users currently located in Germany and presented only in Ukrainian and Russian—the two predominant languages among Ukrainian refugees. Ads were further stratified by region (South, East, North/West), age (above or below 40), and primary language. Participants were incentivized by entering a raffle for 10 Amazon vouchers. Recruitment ran from March 9 to April 16, 2024. Our sample is restricted to respondents who (i) resided in Ukraine before February 24, 2022 and (ii) resided in Germany at the time of the survey. Jointly, these inclusion criteria make it all but certain that our survey respondents are Ukrainian

war refugees, since virtually all Ukrainians who moved to Germany after 24 February 2022 received temporary protection.[7] The total number of participants after applying these criteria is 2,631.

Due to the absence of pre-existing sampling frames or panels for Ukrainian refugees in Germany, we employed a two-stage sampling approach. First, we recruited participants via targeted Meta ads based on geographic location, language, age, and gender. Second, we used initial filtering questions to verify respondents' eligibility concerning their previous residence in Ukraine and current residence in Germany.

The resulting sample consists of 2,631 Ukrainian refugees living across various regions in Germany, covering both urban and rural areas. Participants range in age from 18 to over 60 (median age of 40). Summary statistics of these characteristics are provided in Appendix F. 81 % of the sample are female, which mirrors the population profile of Ukrainian refugees in Germany: administrative data from the Ausländerzentralregister show that about 62 % of all adult Ukrainians registered as refugees are female (Mediendienst Integration, 2025). A probability-based IAB–BAMF–SOEP panel survey reports an even higher share, noting that "three quarters of adult respondents were women" (Kosyakova, Rother and Zinn, 2025). This gender imbalance is structural rather than a sampling artifact: at the time of the study the martial law enstated on 24 February 2022, prohibited Ukrainian men aged 18–60 to leave Ukraine with rare exceptions.

Female-majority profiles are typical for conflict-driven displacement: UNHCR notes that 75% of Sudanese refugees and 79% of those now in South Sudan are women and children, and more than 75% of Rohingya refugees in Bangladesh are likewise women and children (UNHCR, N.d., 2025). Where exit is constrained by conscription or hazardous routes, a female-dominated refugee stream is therefore the norm.

[7] Eurostat, "4.1 million people under temporary protection in July (2024)," https://ec.europa.eu/eurostat/web/products-eurostat-news/w/ddn-20240910-1; Eurostat, "Temporary protection for persons fleeing Ukraine — monthly statistics," https://ec.europa.eu/eurostat/statistics-explained/index.php/Temporary_protection_for_persons_fleeing_Ukraine_-_monthly_statistics.

Participants were randomly assigned via simple randomization in Qualtrics to one of four experimental conditions.[8] In three treatment conditions, respondents watched a short clip with authentic statements by German politicians previously broadcast by German media. These statements were selected to be similar in style and length, and to focus explicitly on military aid rather than other forms of aid (e.g., humanitarian). The clips varied primarily in the stance the politicians took on military aid to Ukraine. In the *Oppose* condition, participants viewed the prominent politician Lars Klingbeil explicitly rejecting weapons deliveries to Ukraine while affirming continued economic support—a clear signal of opposition to military aid. In the *Ambivalent* condition, participants saw Chancellor Olaf Scholz decline to send Taurus missiles while acknowledging other military support. This condition was designed to operationalize *weak opposition* to military aid; expectations for this group mirror the *Oppose* condition but with attenuated effects. In the *Support* condition, participants saw Chancellor Scholz call on European partners to increase military support for Ukraine, an unambiguous appeal for military aid. In addition, to avoid potential confounding from partisan or media-slant effects, we selected videos with politicians from the same political party, the SPD,[9] and obscured media sources in all videos with Ukrainian subtitles. In the control condition, participants watched a neutral news segment—a weather forecast—unrelated to the war in Ukraine or politics more generally.[10]

Our main outcome indices were the German Support Index, which measures perceived German governmental and public support for Ukraine beyond military aid; the Belonging Index, which assesses feelings of belonging and integration within German society; and the Return Intentions Index, which gauges intentions to stay in Germany or return to Ukraine. These indices were constructed from specific survey items detailed in the Appendix J.2 and

[8] Full video transcripts are provided in Appendix J.1.

[9] Although Lars Klingbeil was likely less recognizable to refugees than Chancellor Scholz, he was the SPD party's co-leader and a prominent national figure at the time.

[10] Prior studies have shown that weather forecasts can operate as an incidental mood cue (e.g., Schwarz and Clore, 1983, 2003). The specific clip we used reported bad weather conditions, and thus any possible spillovers into outcomes would be negative. This would bias the control group downward and attenuate estimated Oppose treatment effects. Thus, any negative effects we estimate are conservative, whereas positive effects might be overstated by this placebo choice.

??.[11] We also included a manipulation check measuring perceptions of German military support for Ukraine, which corresponds to the belief that our treatment is supposed to manipulate.

These measures align with widely used frameworks of immigrant integration in both academic research and immigration policy. For instance, the IPL *Integration Index* operationalizes "psychological" integration with items on belonging and connectedness (Harder et al., 2018). In a similar vein, the OECD's *Settling In* indicators include a social integration pillar with subjective metrics, such as perceived discrimination and trust, that capture host-society support (OECD and European Commission, 2023). Our "perceived support" index therefore taps the "context of reception" that classic theory treats as central to immigrants' adaptation (Portes and Rumbaut, 2014). Finally, stated return intentions are a standard, policy-salient proxy in migration research, featuring prominently in models of temporary and repeat migration and having been empirically linked to subsequent behavior (Dustmann and Görlach, 2016; Carling and Pettersen, 2014). In addition to their connection to these established measures, our attitudinal outcomes are well-suited for this study as they are potentially manipulable in the context of a survey experiment.

## 3.2 Estimation

To assess the effect of the treatment video clips, we estimated the following model:

$$Y_{ijk} = \alpha_k + \sum_{T \in \mathcal{T}} \beta_T D_i^T + X_i'\gamma + W_j'\delta + \varepsilon_{ijk} \tag{1}$$

In this model, $Y_{ijk}$ represents the outcome of interest for respondent $i$ in county $j$ and state $k$. The term $\alpha_k$ is a state-specific intercept to control for unobserved state-level factors. The variable $D_i^T$ is a dummy variable indicating whether individual $i$ was assigned to treatment condition $T$, with the control group ($C$) serving as the reference category.

[11] Our main outcome indices are constructed from multiple items to capture distinct facets of broad concepts like belonging and perceived support. The rationale for this design and a full diagnostic of the relationships between index items are provided in Appendix B.1.

The vector $X_i'$ includes individual-level control variables. These are: reason for leaving the city/town in Ukraine where the respondent used to reside, age, gender, marital status, current employment status, current financial situation, highest level of education, and vote choice in the 2019 Ukrainian election. The vector $W_j'$ includes county-level control variables. These are population density, GDP per capita, unemployment rate, and household income. The error term is denoted by $\varepsilon_{ijk}$.

The set $\mathcal{T}$ includes all treatment conditions to which a respondent can be assigned, specifically $\mathcal{T} = \{\text{Oppose}, \text{Ambivalent}, \text{Support}\}$. The coefficients $\beta_T$ capture the average treatment effect of being assigned to treatment $T$ relative to the control group.

# 4 Main Results

We present our main results using dot-whisker plots with points representing OLS effect estimates and lines representing 95% confidence intervals. All outcomes are standardized using the control group mean and standard deviation. The main estimates are shown in Figure 1. We first assessed whether the experimental treatments effectively shifted participants' perceptions of German military support for Ukraine. Participants exposed to statements opposing military aid (*Oppose*) reported significantly lower perceptions of German military support than the control group. The size of this effect–at 0.39 control-group standard deviations–is meaningful. As we expected, the weaker statement used in the *Ambivalent* treatment had a weaker effect that did not reach statistical significance. Results are similar without covariates; see Appendix Figure C.1 and the item-level estimates in Figure C.2.

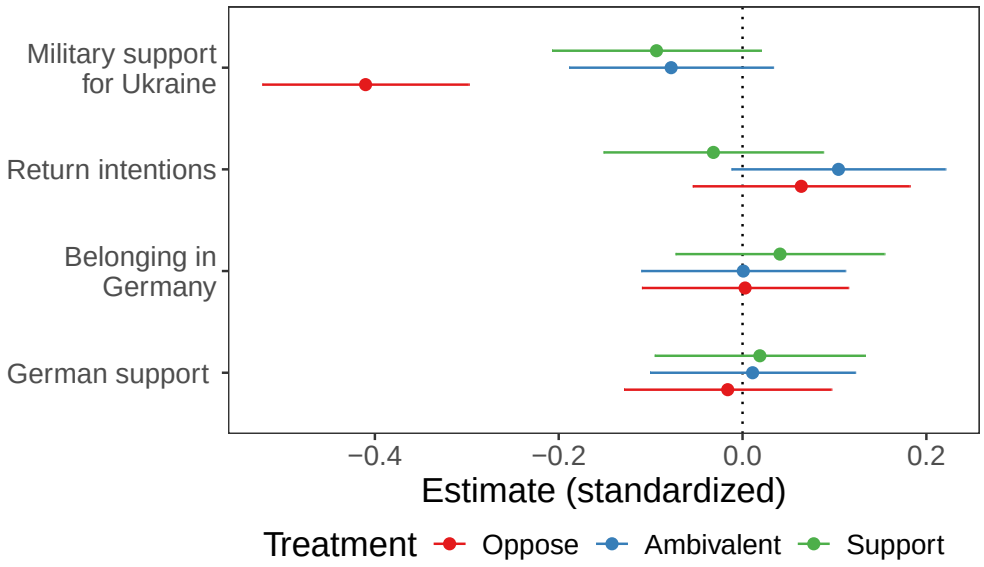


Figure 1: Estimates of the Treatment Effects on Perceptions and Integration Outcomes. *Estimates correspond to the effect of treatment relative to the control condition (weather report). See Section C for the corresponding regression table.*

Despite these shifts in perceptions of military support, we found no significant effects of the treatments on our main outcomes. The treatments did not significantly affect participants' overall perceptions of German support for Ukrainians beyond military aid, as measured by the German Support Index. Similarly, there were no notable differences in feelings of belonging or integration within German society, according to the Belonging Index, across the different treatment conditions. For both of these outcomes, the effects are indistinguishable from zero. While the treatments also had no significant effect on the Return Intentions Index, we note that the point estimate for the *Ambivalent* treatment is slightly positive. However, this effect is small—at 0.1 standard deviations—and not statistically significant. Given that the other treatment effects are tightly clustered around zero, we conclude that this finding likely reflects statistical noise rather than a true causal effect. These results suggest that the overall effect of exposure to political debates about military support on Ukrainian refugees' integration-related attitudes is not distinguishable from zero.

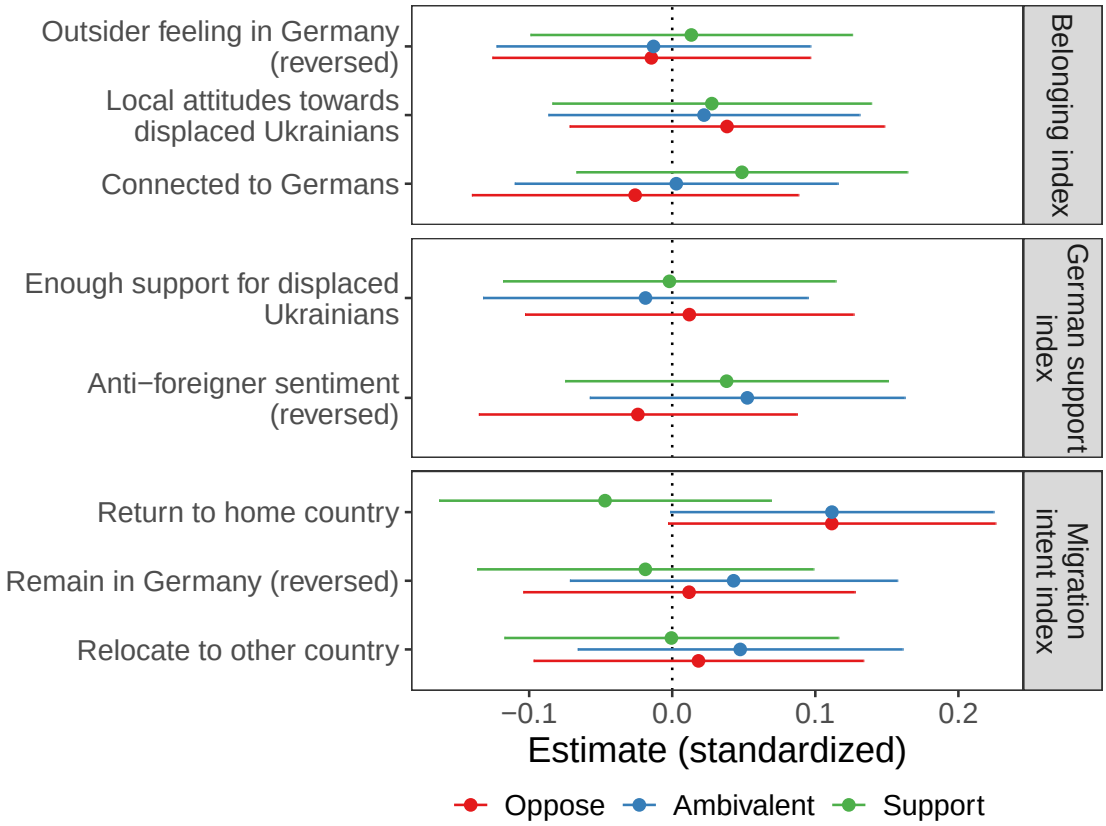


Figure 2: Estimates of the treatment effects for index components. *Estimates correspond to the effect of treatment relative to the control condition (weather report).*

In the pre-analysis plan we registered several diagnostic tests for potential assumption violations that could threaten our experimental inferences. These included: (i) an attention check embedded in the survey (passed by 95.6% of the final study sample); (ii) tests of balance on pre-treatment covariates across experimental groups (see Appendix B.3); (iii) a manipulation check using a direct item on perceived German military support for Ukraine (see Figure 1); (iv) tests related to the Missing-At-Random (MAR) assumption for attrition (see Appendix E.4); (v) an experimenter demand test using an endline question about the study's purpose (see Appendix E.3) and (vi) a check for whether respondents correctly identified the topic of the video treatments after seeing them (correctly identified by 87.5% of respondents). Across these diagnostics, we find no evidence of violations that would undermine validity of our experimental estimates. Although inattentiveness and effects of treatment on correctly

guessing the study aims are non-zero, their magnitudes are small and unlikely to affect the results we report in the paper.

## 4.1 Pre-registered mechanisms

Our main specification shows no evidence that information about opposition to military support for Ukraine changes feelings of belonging, perceptions of general refugee support, or intentions to leave the host country. We now discuss and test several mechanisms to explain these overall null results.

First, some refugees might view military support as counter-productive, preferring diplomacy. We surveyed this preference, finding a sizable minority (38%) favors negotiations over continued fighting. Second, sustained engagement with German media could create differential baseline responsiveness to our treatments.[12] Investigating heterogeneity based on these potential moderators, our analysis provides no support for either mechanism. The results, presented in Figure D.1, show no evidence of effect heterogeneity on the main integration outcomes across any of these subgroups. Therefore, neither differing war preferences nor prior media consumption appears to explain the overall null results.

However, long-term self-reported media consumption might not accurately capture the relevant dynamics for a number of reasons. First, self-reported consumption has been shown to be a poor measure of actual consumption (Konitzer et al., 2021). Secondly, individuals' attitudes may be shaped through indirect exposure because "those who watch [...] talk to and persuade others who did not watch" (Druckman, Levendusky and McLain, 2018, p. 99). Third, especially in fast-developing and highly volatile debates such as those over a recently erupted military conflict, short-term changes in the information environment may be more consequential than long-term consumption levels.

[12] We define this measure based on a survey item that asks respondents to indicate whether they consume mainstream German news media. This includes, e.g., the Tagesschau or newspapers like FAZ or Die Zeit. Respondents can select from a list of predefined mainstream outlets and/or fill out a text entry field. Our moderator is defined as one if respondents select any of the predefined outlets or if the text entry fields list a mainstream outlet. If neither of those is the case, the moderator is defined as zero.

We therefore posit an additional, exploratory explanation of our overall null results that was not part of the pre-analysis plan: an "information environment saturation" mechanism, whereby short-term fluctuations in the level of support communicated by the media may condition treatment effects.

# 5 Effect Heterogeneity by Information Environment Saturation

While our pooled estimates suggest little evidence that our treatments significantly shifted integration-related outcomes, the null results we report above simply show that the *marginal signal* respondents receive from our treatments, on average, do not have a detectable effect. However, respondents do not enter the survey experimental environment as a blank slate; rather, they arrive with attitudes already shaped by the broader information environment to which they have recently been exposed, including news coverage, public debates, and other political signals circulating in their daily lives. This pre-existing exposure can amplify, mute, or crowd out the influence of our experimental cues, meaning that the effects of political debates over military support may only manifest under certain conditions—specifically, when the surrounding information environment is relatively sparse.[13]

The most direct test of moderation by the overall information environment draws on short-term measures of media reporting. To proxy for the overall media environment, we draw on transcripts of the 8 p.m. edition of the *Tagesschau*, Germany's most-watched daily TV news program. Based on these transcripts, we code whether Ukraine was mentioned and, if so, whether German politicians expressed support, opposition, or ambivalence toward military aid. This approach allows us to assess whether the salience and content of contemporaneous media coverage moderate our experimental treatment effects.

[13] We have decided to present the theoretical and empirical discussion of this mechanism after the main results to transparently reflect the research process, since we derived this mechanism after collecting the data and obtaining the main results.

This analysis builds on research showing that media coverage creates shared informational contexts that fundamentally alter how individuals process information (Zaller, 1992; Prior, 2007). Such contexts define what issues are top-of-mind, shape the interpretive frames people use, and influence how they evaluate new information (Scheufele and Tewksbury, 2007; McCombs, Shaw and Weaver, 2014). When issues receive extensive coverage, the information environment becomes saturated, reducing the marginal effect of additional messages through two main pathways.

First, pre-treatment effects occur when ambient exposure to political information—through headlines, public discourse, or repeated references in multiple outlets—shapes individuals' baseline attitudes before any experimental intervention (Druckman and Leeper, 2012; Druckman, Peterson and Slothuus, 2013). This means that even respondents who do not directly consume the media source in question may still be indirectly exposed through interpersonal discussion, social media circulation, or spillovers from other outlets (Druckman, Levendusky and McLain, 2018; Barabas and Jerit, 2009). Such pre-treatment can narrow the scope for experimental messages to shift attitudes because the core considerations relevant to the issue are already activated.

Second, opinion crystallization occurs when repeated exposure to consistent messages stabilizes attitudes, making them more resistant to change (Tesler, 2015). Once individuals have formed a strong evaluative position, additional messages—whether congruent or incongruent—are less likely to move them, and may instead be filtered through motivated reasoning processes (Taber and Lodge, 2006; Jerit and Zhao, 2020). Over time, high-salience issues become "settled" in the minds of attentive audiences, reducing the capacity for new cues to generate measurable change.

This saturation dynamic is particularly relevant in highly politicized contexts, where media coverage not only supplies factual information but also signals the presence of elite contestation (Bullock, 2011; Nicholson, 2011). In such contexts, the marginal informational value of an additional statement—like those used in our treatments—depends critically on

whether it introduces genuinely novel content or simply joins a pre-existing canon of messages on an already salient topic.

Therefore, we propose (1) that *any* exposure to discussion of military support to Ukraine, whether experimental or ambient, may shift baseline perceptions of support; and (2) that experimental treatments will only have measurable effects when the information environment is relatively sparse (i.e., when Ukraine is not being actively discussed in media) but will be "crowded out" when the environment is already saturated with relevant content. This leads to two testable implications: first, baseline perceptions of support should differ systematically based on short-term fluctuations in the level of media coverage; and second, treatment effects should be moderated by the presence or absence of contemporaneous media coverage about Ukraine.

## 5.1 Media Coverage Data

To test the proposed mechanism, we examine treatment effect heterogeneity based on German media coverage of Ukraine on the day preceding survey participation. To measure media coverage, we rely on official transcripts from the 8:00 p.m. edition of the *Tagesschau*, which is Germany's most-watched daily TV news program. We code whether Ukraine was mentioned on a particular day and, if so, whether German politicians expressed support, opposition, or ambivalence toward military aid.[14] This approach allows us to assess whether the contemporaneous coverage moderates our experimental treatment effects.

The Tagesschau is Germany's most important and widely viewed national news broadcast, airing daily at 8:00 p.m. on the public television channel ARD. Its viewership consistently exceeds ten million, making it the single most influential news source in shaping public awareness of political events. Multiple sources indicate that refugees' consumption of German media is substantial (Appendix A.3). Because the Tagesschau sets the national news agenda for the following day, the topics covered on the Tagesschau reflect the broader informational

[14] We chose these categories to directly match the experimental treatment conditions.

environment to which most Germans, and also many refugees, are exposed (Deutschlandfunk, 2024; ARD Programmdirektion, 2025; Maier, Ruhrmann and Stengel, 2009; Krämer and Schroll, 2009; Mayerhöffer and Heft, 2022).

From the German public broadcaster ARD, we obtain daily transcripts of the 8:00 p.m. Tagesschau broadcast. We classify each transcript along two dimensions: (1) whether Ukraine was mentioned at all, and (2) whether coverage conveyed a German government stance on military aid (support, oppose, neutral, or no stance mentioned). We assign each respondent surveyed on day $t$ to the Tagesschau coverage on the previous day $t-1$.

To classify content, we used the OpenAI API (GPT-4o-mini). First, for each transcript we extract only text related to Ukraine (Stage 1). Second, conditional on Ukraine-related content, we classify whether the coverage signals German government support, opposition, neutrality, or contains no stance (Stage 2). Prompts are provided verbatim in Appendix G. The resulting stance label is mapped to four mutually exclusive categories: *support*, *oppose*, *neutral*, and *not mentioned*; we additionally construct a collapsed indicator for *Any mention* (support/oppose/mentioned without stance) versus *No mention*. As we document in more detail in Section 5.3, there is good balance along respondent background characteristics when comparing the No mention / any mention categories (Appendix G.6), and there is further no evidence that Ukraine mentions are merely a function of conflict intensity (Appendix G.11).

Several examples in Table G.2 illustrate the content we capture. Supportive stances frequently highlight resource commitments and coordination (e.g., Ramstein Air Base meetings, munitions packages, coalition pledges). Oppositional stances center on legal or operational constraints (e.g., debates over Taurus deliveries, "no boots/no targeting" positions). Finally, some Ukraine mentions provide situational updates or commemorations (e.g., Bucha remembrance, frontline summaries, diplomatic developments) without articulating a government stance.

To validate the automated media coding, three research assistants independently hand-coded the 32 Tagesschau episodes that aired while our survey was fielded. There was perfect

agreement between GPT-4o-mini and two of the three human coders regarding whether Ukraine was mentioned in a given edition of the Tagesschau, with near-perfect agreement (97%) for the third coder. Across the stance labels, which only apply for episodes where Ukraine was mentioned, agreement was between 0.76–1.00 (this is the fraction of editions that received the same label from GPT-4o-mini and the human coders). Inter-coder agreement was between 84–100%. We provide more details on this in Section G.2. Overall, human-coded labels are well-aligned with LLM-coded labels.

## 5.2 Baseline Shifting by Media Coverage

Using the data described above, we first we examine whether media coverage shifts baseline perceptions among control group respondents. Figure 3 presents control group means across our main outcomes, separated by the type of Tagesschau coverage on the previous day. The results reveal substantial baseline shifting: when Ukraine was not mentioned in the Tagesschau, control group respondents showed markedly different perceptions compared to days with any Ukraine coverage.

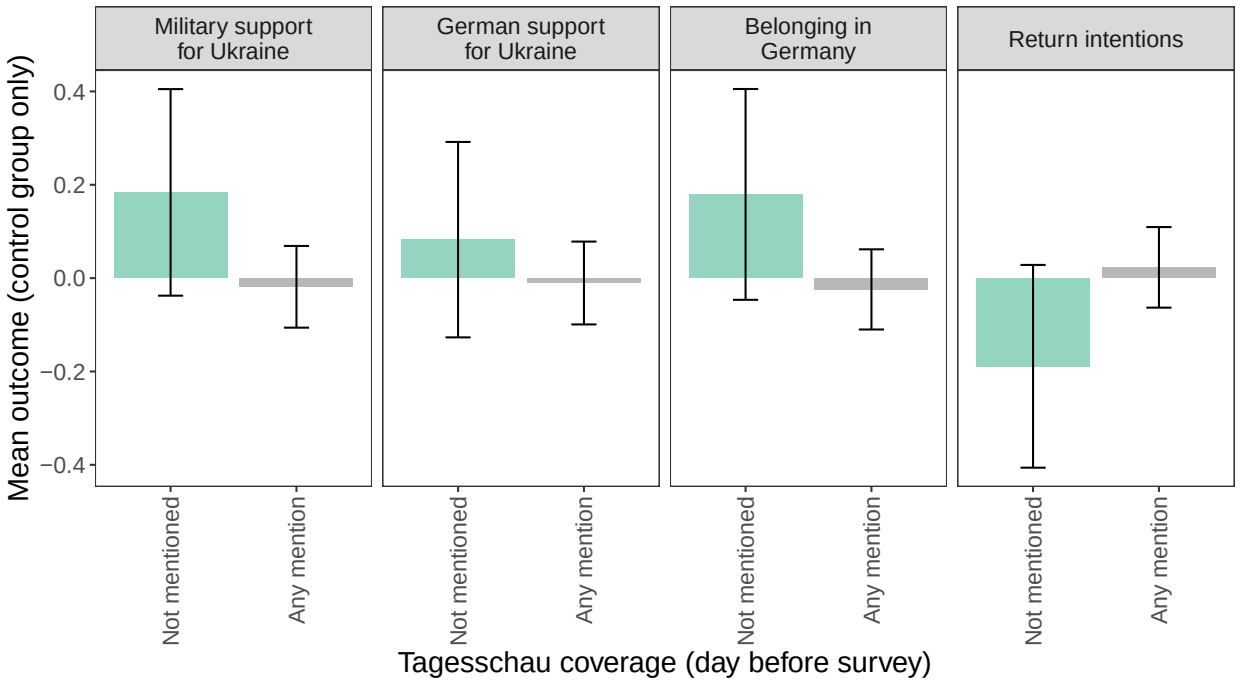


Figure 3: Baseline shifting by media coverage. *Control group means by type of Tagesschau coverage on the day before survey participation. Error bars represent 95% confidence intervals. This demonstrates how the information environment shapes baseline perceptions even before experimental treatment.*

When Ukraine was absent from media coverage, control group respondents exhibited markedly different baseline perceptions. Most notably, they reported higher perceptions of both military support ($+0.184 vs. -0.019$) and general German support for Ukraine ($+0.082 vs. -0.010$), higher feelings of belonging ($+0.179 vs. -0.024$), and lower return intentions ($-0.189 vs. +0.023$) compared to days with any Ukraine coverage. This pattern aligns with the proposed contestation signaling mechanism: in the presence of coverage, refugees may infer active political contestation, which lowers perceived support and increases return intentions.[15]

[15] We also conducted hypothesis tests for differences in means between coverage and no coverage groups. Given the smaller sample size due to focusing on the control group, standard errors are larger. Differences are significant at $\alpha = 0.1$ for belonging, German support and return intentions outcomes. The difference is not significant for the military support outcome.

## 5.3 Treatment Effects Conditional on Media Environment

To investigate whether our experimental treatments do affect outcomes during periods of low salience, we re-estimate our main specification on two subsamples: respondents sampled when Ukraine was not mentioned in the Tagesschau (14.2% of the total sample) versus when it was (85.8%).

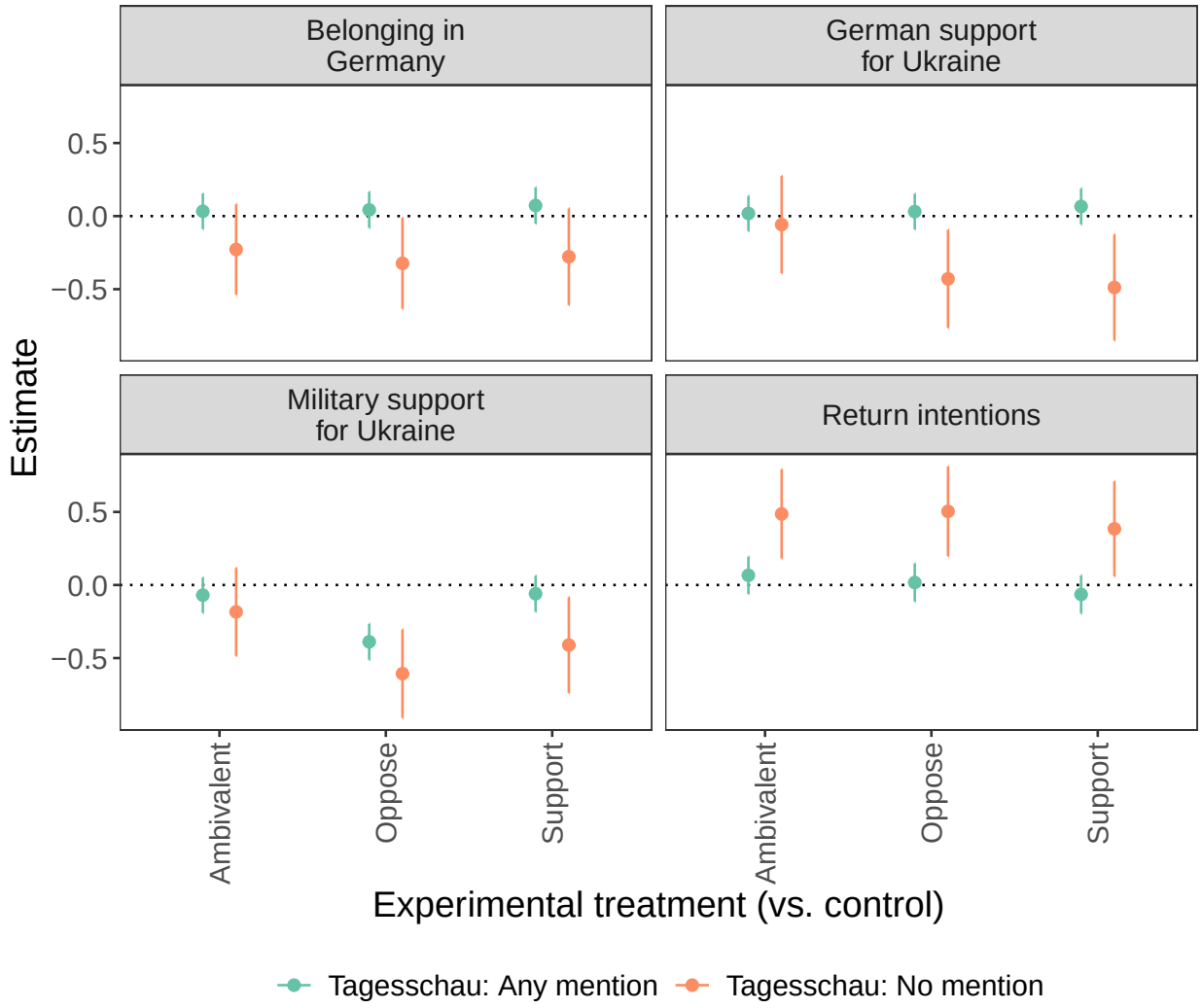


Figure 4: Treatment effects by media coverage condition. *Treatment effects (relative to control within each media condition) for outcomes excluding the manipulation check. Experimental treatments produce substantial effects only when Ukraine was not covered in German media the previous day. Error bars represent 95% confidence intervals. Corresponding interaction coefficients are reported in Table G.3. In the appendix, we report additional specifications with linear time trends (Figure G.3), standard errors clustered by day (Figure G.4), and Newey-West standard errors with a 3-day lag (Figure G.5).*

Figure 4 presents our main finding: experimental treatments produce substantial effects only in the absence of prior media coverage. When Ukraine was not mentioned in the Tagesschau, all three treatments—oppose, support, and ambivalent—generated large, significant effects across multiple outcomes. Most strikingly, both supportive and oppositional treatments produced remarkably similar effects: reducing perceived German support by 0.43-0.49 standard deviations, increasing return intentions by 0.38-0.50 standard deviations, and decreasing feelings of belonging by about 0.23-0.32 standard deviations. This valence-independent pattern strongly supports a contestation signaling mechanism. Any political attention to military aid, regardless of valence, conveys that support is debated rather than assured.

In contrast, when Ukraine received any media coverage (85.8% of our sample), treatment effects became small and statistically indistinguishable from zero across all outcomes. The oppose treatment reduced German support perceptions (without manipulation check) by only 0.03 standard deviations and increased return intentions by just 0.02 standard deviations—substantially smaller than the effects observed without coverage. Treatment effects across all outcomes ranged from $-0.18$ to $+0.08$ standard deviations with most being statistically insignificant. This pattern holds regardless of whether the coverage was supportive, opposing, or neutral, indicating that the mere presence of Ukraine in the media environment, rather than its specific content, drives the moderation effect.

Disaggregating coverage further into support, oppose, and mentioned-without-stance yields the same conclusion: There are sizable treatment effects only on no-coverage days, whereas on coverage days effects are small and largely indistinguishable from zero across stances. We present the disaggregated specifications in Appendix Figure G.8.

Importantly, we note that the direction and magnitude of the experimental results in the no-coverage subsample closely align with the observational control-group mean differences presented in Figure 3: media coverage of military support debates decreases perceived levels of military support and general levels of support for Ukrainians, and increases return intentions

while decreasing feelings of belonging in Germany, regardless of message content.

For completeness, we also estimate the corresponding interaction models that pool the two subsets and interact each treatment with an indicator for "No mention." The interaction coefficients, reported in Table G.3, mirror the visual pattern in Figure 4: treatment effects are concentrated in the no-coverage condition. columns (1)-(4) report models without covariates; columns (5)-(8) add controls. Results are highly similar across specifications.

Taken together, these findings suggest that political debates over military support affect Ukrainian refugees' feelings of general host-country support as well as their return intentions and feelings of belonging in Germany. Interestingly, the magnitude and direction of these effects does not vary by the valence of the information: any coverage of military support debates—whether supportive or oppositional—signals that support is contested rather than universally agreed upon, aligning with research showing that the mere presence of political debate conveys important information about political divisions on the issue (Bullock, 2011; Nicholson, 2011).

In our specific case, debates about military aid to refugees' homeland carry particular significance. Social identity theory suggests that group members are especially attentive to signals about their group's status and acceptance (Tajfel and Turner, 2001). When political elites debate support for Ukraine, refugees may interpret this as evidence that their cause lacks unanimous backing, regardless of individual politicians' positions. This creates a paradoxical effect, whereby even supportive statements can reduce perceived support by highlighting the existence of opposition.

## 5.4 Internal and External Validity

To assess the robustness of these findings, we first note that since treatment assignment remains random within each media coverage subset, the subgroup effects presented in Figure 4 are unbiased within both the no-coverage and any-coverage subsamples.

However, to interpret the difference in treatment effects between subsamples as evidence of

information environment saturation, we require an additional assumption: that assignment to media coverage conditions is uncorrelated with potential outcomes. In other words, whether a respondent completed the survey following (no) Ukraine coverage in the Tagesschau must be 'as-if random' with respect to their latent responsiveness to the treatments.

**Addressing potential confounding.** While not directly testable, we provide three pieces of evidence in support of this assumption in Appendix G. First, we demonstrate that days with or without Ukraine coverage are not systematically clustered but distributed relatively evenly across our survey period (see Figure G.1). This temporal distribution suggests that coverage patterns reflect day-to-day editorial decisions rather than systematic trends that could confound our results. Second, we show that respondents in both subsamples are balanced on a variety of pre-treatment covariates, including demographics, region, move motivation, vote choice in Ukraine, prior media exposure, prior integration, and war termination preferences (Figure G.2).

Third, we address the concern of unobserved confounding that is correlated with the survey date. We re-estimate the split-sample specification in Figure 4 using a linear time trend (see Figure G.3). The results of this robustness check are very similar to the original pattern, which alleviates concerns that unobserved, time-varying confounding drives our findings. Together, these tests suggest that our findings reflect a genuine interaction between experimental treatments and the broader information environment rather than a spurious correlation driven by unobserved confounders.

**Permutation evidence based on placebo coverage dates.** Complementing these checks, we implement a permutation-style check in Appendix G.10. If the information environment effects are spurious, we would expect similar coefficients as shown in Figure 4 using "placebo" coverage dates – that is, coverage on dates further in the past or even in the future. Instead of assigning coverage to a respondent surveyed on day $t$ based on the *Tagesschau* edition on day $t-1$, we construct a reference distribution by shifting the coverage

window over days $t-k$ with $k \in [-10, 10]$, $k \neq 1$. This means that, instead of the previous evening, we assess treatment effect moderation by coverage that occurred several days before or after the survey date $t$. Coverage on these placebo dates should not moderate our main treatment effects.[16]

We then compare the estimated moderation effect to the placebo distribution. We find that the observed moderation in Figure 4 is unusually large. We calculate two-sided $p$-values by counting the number of placebo estimates with absolute magnitude at least as large as the observed effect. We obtain $p = 0.000$ for perceived German support, belonging, and return intentions, and $p = 0.200$ for military support. We interpret this as additional evidence that the moderation effects in Figure 4 derive from the short-term information environment rather than from spurious fluctuations in treatment effects across different days.

**Conflict intensity and media coverage.** We further consider the possibility of a bundled treatment: coverage may be more likely precisely when the conflict is more severe, so Tagesschau mentions could proxy recent conflict intensity. To address this, we use daily casualties from UCDP GED (v25.1) to proxy for conflict intensity. We compute windowed averages over the previous $k$ days ($k = 1, 2, 3$) for each Tagesschau day (the evening before respondents' survey date) and compare casualties preceding mention or no-mention days. Across all casualty measures, the differences in total casualties are small. This indicates that mentions in the Tagesschau do not merely track short-run conflict intensity (for more details and results, see Appendix G.11 and Figure G.7).

**Alternative standard errors.** Additionally, to account for possible within-day dependence and short-run serial correlation, we implement two complementary adjustments: first, we cluster standard errors by survey day, which modestly widens confidence intervals without altering substantive conclusions (see Appendix G.8 and Figure G.4). Second, we com-

[16] Coverage on earlier days, such as $t-2$ or $t-3$, may still moderate treatment effects through lingering salience or carryover, but the expected magnitude is smaller. If moderation reflects the *Tagesschau* news cycle, the effect at $t-1$ should be largest.

pute heteroskedasticity- and autocorrelation-consistent Newey-West standard errors with a three-day lag based on the survey date to accommodate a smoothly evolving information environment, which yields substantively identical results (see Appendix G.9 and Figure G.5).

**Multiple comparisons correction:** To address multiplicity, we pre-specified two families of tests, main treatment effects and Tagesschau interaction effects, and applied Benjamini-Hochberg false discovery rate control at $q = 0.10$ within each family. After adjustment, no average (pooled-sample) treatment effects on the three primary indices are significant, reinforcing the conclusion of null average effects. In contrast, several treatment-by-low-salience (no Tagesschau mention) interactions remain significant after FDR correction—specifically, decreases in perceived German support (Oppose, Support), increases in return intentions (all three treatments), and a decline in belonging (Oppose). This suggests that the salience-conditioned findings are not artifacts of multiple testing.

## 5.5 Partial replication using SOEP survey data.

Finally, we replicate some of our results using a different survey data set: the IAB-BiB/FReDA-BAMF-SOEP survey of Ukrainian refugees. Using data from 2023 and outcomes on intentions to stay in Germany or return to Ukraine, we again merge survey responses to Tagesschau coverage on the preceding day. Comparing respondents who are either exposed or not exposed to Ukraine coverage, we again find that media attention to Ukraine (any mention) is associated with increased return intentions and lower intention to stay, regardless of message valence. The results using this additional dataset reproduce the sign and pattern of the findings in Figure 3, which reduces concern that the results are sample- or measurement-specific. Details and specification are provided in Appendix I and Figure I.1.

# 6 Discussion

In this paper, we set out to examine whether German political debates about military support for Ukraine affected Ukrainian refugees' integration-related attitudes. While following the pre-analysis plan we document null effects of the exposure to political statements on the pulled sample, we find strong evidence that these results mask heterogeneity. Specifically, during periods of low ambient media coverage of Ukraine, the treatment significantly shifted attitudinal markers of integration. Importantly, the direction and magnitude of these effects on social integration are consistent, regardless of the valence content of the message: supportive or oppositional statements by German elite politicians all lowered perceptions of German support of Ukrainian refugees and feelings of belonging in Germany, while increasing return intentions. These effects are closely mirrored by observational shifts of control-group baselines on days with low media salience, lending further credence to the validity of our findings. Taken together, our results suggest that any exposure to debates over military aid to refugees' home country–irrespective of content–affects outcomes by signaling contestation.

This study makes several contributions to the broader literature. Conceptually, we recast elite debates as evidence of contestation, whereby any elite cue, regardless of valence, can have negative effects on perceived levels of support by signaling disagreement. This valence-independence suggests that even supportive statements can reduce perceived support and belonging among migrants and nudge return migration plans. Furthermore, substantively, we widen the lens from rhetoric *about* immigrants to broader elite discourse *about refugees' home-country*. Foreign-policy debates that never mention refugees nonetheless carry information that shapes perceived acceptance and migration intentions. This connects research on minority politics to work on wartime public opinion and alliance credibility, showing that refugees interpret domestic contestation over military aid as a signal about future support for their group.

Methodologically, the study demonstrates the importance of accounting for information environment saturation when designing survey experiments on politically salient topics. Our

findings suggest that experimental treatments may appear ineffective when pooled across all respondents, even when accounting for self-reported long-term media consumption, but can reveal substantial effects when disaggregated by the ambient, short-term media environment. This highlights a broader challenge in experimental research: treatments that seem to "fail" may actually succeed under specific informational conditions, but these effects can be masked by ambient pre-treatment exposure similar to the experimental stimuli. Our approach of linking daily media content to individual survey responses offers a template for future studies seeking to understand how experimental interventions interact with naturally occurring information flows. By coding the content and timing of mainstream media coverage and matching it to survey participation dates, researchers can better isolate the marginal effects of experimental treatments from the background information environment that shapes baseline attitudes.

Using authentic political statements (Appendix A.4) rather than vignettes improves external validity without sacrificing experimental control. Pairing real rhetoric with day-level measures of the information environment strengthens inference about how debates shape refugee integration and offers a template for other cases, such as immigration policy, trade affecting ethnic communities, and diaspora-relevant foreign policy.

Finally, we discuss generalizability of our findings. The mechanism we document should travel to settings where two conditions occur. First, the relevant foreign policy debate has become contentious in mainstream news (i.e., not a quiet consensus but an openly contested issue). Second, the affected refugee or diaspora group faces uncertainty about the durability of host support, for instance, through temporary protections, budget fights, or shifting political coalitions. We contend that these conditions are common. The high-salience debates over Ukraine aid in national legislatures, such as the recent discussions in the German Bundestag following the new government's coalition agreement, illustrate elite contestation. At the same time, periodic policy shifts and budget reassessments by European governments create uncertainty that refugees with temporary status are likely to perceive. Where either condition

is absent, because the issue is settled or the diaspora enjoys secure, long-term status, our effects should not appear. Framed this way, our contribution is general: in contexts where contentious elite debate exist, and where refugees are exposed to uncertainty, we should expect similar, valence-independent shifts in perceived support, belonging, and migration intentions.

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# Appendix

# A Additional information

## A.1 Ethical Considerations

Given that our study targets a vulnerable population of Ukrainian refugees who may have limited economic resources, we implemented several ethical precautions to ensure participant welfare and autonomy. While we offered entry into a raffle for Amazon vouchers as compensation, we mitigated undue influence by specifying in the consent form that participation in the raffle was possible without completing the survey. Our recruitment materials emphasized the opportunity to share opinions and experiences, mentioning the raffle only subtly to avoid coercion. To prevent any unintended negative consequences of the intervention–especially since it could affect respondents' migration intentions–we included a debriefing at the end of the survey explaining the study's purpose and experimental nature, allowing participants to readjust any influenced attitudes. Participants were informed of their right to withdraw at any time, and we provided contact information for support services, including a hotline for Ukrainians in Germany, should they experience any distress during the survey.

## A.2 Deviations from Pre-Analysis Plan

We note three main deviations from the pre-analysis plan (https://osf.io/32h9n).

First, because the data did not support our preregistered directional hypotheses, we report two-sided tests and corresponding confidence intervals throughout; Hypotheses H1-H3 in Section 2 are stated accordingly.

Second, the preregistered German support index originally bundled the military support perception item that also serves as our manipulation check. Since that item is mechanically most proximate to the treatment, we report versions of the German support index both including and excluding it and use the standalone military support item as the manipulation check.

Third, the plan included an observational design leveraging quasi-random initial regional allocation of refugees in Germany. By the time of fieldwork, substantial secondary moves and pre-arranged placements meant too few respondents still resided in their originally assigned localities; consequently, we did not implement or report that observational component.

## A.3 Media Consumption among Ukrainian Refugees

Several independent data sources indicate that a substantial share of Ukrainian refugees in Germany routinely follow domestic news media. A commissioned survey by the European Centre for Press and Media Freedom (ECPMF) and the JX Fund, fielded by FORSA between April and July 2023 ($N = 508$), shows that 42% of respondents report using at least one German-language outlet for political information; the most frequently named sources are Deutsche Welle (23%), Tagesschau (13%), *ZDF heute* (9%), and Der Spiegel (13%) (European Centre for Press and Media Freedom, 2023). Qualitative follow-ups in the same study suggest that the turn to German outlets is motivated by a search for "more diverse perspectives" and by the desire to practice language skills.

Complementary evidence arises from administrative and panel data. In the IAB–BAMF–SOEP refugee panel, wave 2 (July 2023-January 2024), approximately 70% of Ukrainian respondents report having completed an integration or language course; among these participants, "regular watching of German news programs or reading German press" is documented as one of the most common voluntary activities undertaken to reinforce classroom learning (Brücker et al., 2024). A separate evaluation of the Federal Office for Migration and Refugees' Erstorientierungskurse likewise finds that course instructors systematically recommend public-broadcast news formats for self-study, and that participants subsequently adopt these media as everyday information sources (für Migration und Flüchtlinge, 2023). Taken together, the survey and administrative records demonstrate that exposure to German public-service news is neither incidental nor marginal but forms an integral component of many refugees' information environments. This prevalence strengthens the plausibility that elite cues broadcast via Tagesschau can reach and influence a sizable segment of the Ukrainian refugee population.

## A.4 Video Treatment Selection

The videos we use across treatment arms intentionally mirror the prevailing elite register in Germany during early 2024: a cautious, legal-operational framing that emphasized escalation risks, alliance coordination, and the categorical exclusion of any Bundeswehr involvement in targeting. During the Taurus debate, Chancellor Scholz repeatedly rejected deliveries with references to prudence and to the requirement that German soldiers must not be associated with target selection; he defended this stance in the Bundestag and in press appearances.[17] Contemporary reporting likewise framed the dispute in terms of proportionality, legal constraints, and alliance management, rather than hostile affect toward Ukrainians.[18] Because the public debate rarely employed emotive or outgroup-directed language, we used muted, authentic clips that preserved this register.

## A.5 Minimum Detectable Effect Size

The minimum detectable effect size (MDES) quantifies the smallest standardized average treatment effect that our design could detect with 80% power using a two-sided test at $\alpha = 0.05$ (Bloom, 1995). Let $\widehat{\mathrm{SE}}$ denote the heteroskedastic-robust standard error of the treatment coefficient. Then

$$\mathrm{MDES} \;=\; \big(z_{\alpha/2} + z_{1-\beta}\big)\,\widehat{\mathrm{SE}} \;=\; (1.96 + 0.84)\,\widehat{\mathrm{SE}} \;=\; 2.80\,\widehat{\mathrm{SE}}.$$

Using the largest standard error reported in Table C.1 (column 3, $\widehat{\mathrm{SE}} = 0.060$) gives

$$\mathrm{MDES} \;=\; 2.80 \times 0.060 \;=\; 0.168 \approx 0.17 \text{ SD}.$$

Repeating the calculation for the other models ($\widehat{\mathrm{SE}} \approx 0.057$) yields MDE values of 0.16 SD. Because all outcomes are standardized using the control-group standard deviation, these figures are directly interpretable: the experiment was powered to detect effects smaller than ±0.17 SD,.

# B Descriptive

## B.1 Index Diagnostics: Item-Item Correlations

We report correlation matrices among the components of each index (three indices shown: German support without the manipulation item, belonging, and migration intentions).

Table B.1: Correlation matrix among components of German support (no manipulation item)

| Item | Enough support for displaced Ukrainians | Anti-foreigner sentiment (reversed) |
|---|---|---|
| Enough support for displaced Ukrainians | 1.000 | 0.134 |
| Anti-foreigner sentiment (reversed) | 0.134 | 1.000 |

As shown in Table B.1, the correlation between the two items of the German support index is positive but weak (0.134). This pattern reflects the fact that the items are designed to capture different aspects of support: one measures explicit endorsement of assistance for displaced Ukrainians, while the other taps the absence of anti-foreigner sentiment. The low correlation therefore signals complementarity rather than inconsistency—together, the items cover both positive and negative dimensions of the underlying construct.

[17] Deutscher Bundestag, Regierungsbefragung, 13 March 2024, "Nichtbeteiligung deutscher Soldaten sicherstellen."

[18] AP News, 26 February 2024, "Germany's Scholz explains his reluctance to send Taurus long-range missiles to Ukraine."; AP News, 13 March 2024, "Germany's Scholz defends his refusal to send Taurus missiles to Ukraine."; *Süddeutsche Zeitung*, 13 March 2024, "Scholz bekräftigt Nein zu Lieferung von Taurus."; *El País*, 28 February 2024, "Scholz's refusal to send Taurus missiles to Ukraine divides German government."; Politico Europe, 10 April 2024, "Zelenskyy blasts Scholz's reason for not sending German Taurus missiles."

Table B.2: Correlation matrix among components of Belonging

| Item | Connected to Germans | Outsider feeling in Germany (reversed) | Local attitudes towards displaced Ukrainians |
|---|---|---|---|
| Connected to Germans | 1.000 | 0.635 | 0.325 |
| Outsider feeling in Germany (reversed) | 0.635 | 1.000 | 0.408 |
| Local attitudes towards displaced Ukrainians | 0.325 | 0.408 | 1.000 |

Table B.3: Correlation matrix among components of Migration intentions

| Item | Return to home country | Remain in Germany (reversed) | Relocate to other country |
|---|---|---|---|
| Return to home country | 1.000 | 0.473 | 0.008 |
| Remain in Germany (reversed) | 0.473 | 1.000 | -0.020 |
| Relocate to other country | 0.008 | -0.020 | 1.000 |

By contrast, the belonging index in Table B.2 shows stronger associations among items, with correlations ranging from 0.325 to 0.635, indicating that measures of connection to Germans, outsider feelings, and local attitudes toward Ukrainians capture related but non-identical facets of social attachment. Migration intentions (Table B.3) display lower correlations, especially between "return to home country" and "relocate to other country" (0.008), which is expected since these represent distinct choices rather than positions along a single continuum. In sum, the indices demonstrate internal coherence where theory predicts it, while preserving conceptual breadth by avoiding redundant items.

## B.2 Distribution of Outcomes

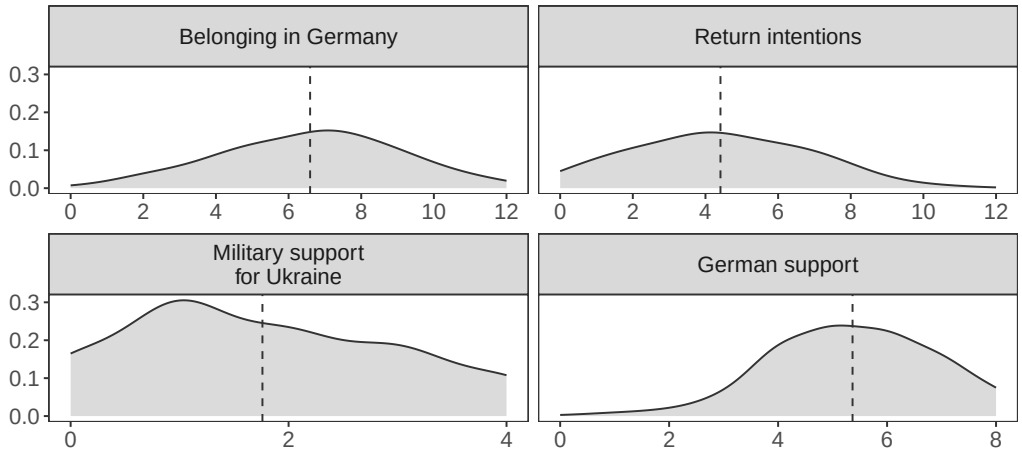


Figure B.1: Raw Distributions of Perceptions and Integration Outcomes. *Unlike the paper, the outcomes are not standardized to show the observed (raw) distribution of the main outcomes used in the paper. Dashed lines indicate the mean response.*

## B.3 Balance

Table B.4: Treatment Balance by Covariates. Each cell in the table shows RI p-values for the relationship between specific treatment assignment (as opposed to control group) and baseline covariate based on 10,000 permutation of treatment vector.

| | P-value by treatment | | |
|---|---|---|---|
| **Variable** | **Oppose** | **Ambivalent** | **Support** |
| Age | 0.068 | 0.726 | 0.096 |
| NA | 0.434 | 0.394 | 0.434 |
| Employment: Employed | 0.756 | 0.216 | 0.668 |
| Employment: Odd jobs | 0.152 | 0.438 | 0.370 |
| Employment: Other | 0.112 | 0.908 | 0.520 |
| Employment: Pensioner | 0.464 | 0.256 | 0.902 |
| Employment: Running household | 0.826 | 0.628 | 0.674 |
| Employment: Self-employed | 0.978 | 0.288 | 0.638 |
| Employment: Student | 0.356 | 0.808 | 0.498 |
| Employment: Unemployed | 0.316 | 0.028 | 0.216 |
| Female | 0.910 | 0.358 | 0.498 |
| Male | 0.846 | 0.328 | 0.550 |
| Other gender | 0.704 | 0.732 | 0.670 |
| Income | 0.768 | 0.512 | 0.700 |
| Married | 0.618 | 0.798 | 0.804 |
| Move motivation: assigned | 0.988 | 0.858 | 0.938 |
| Move motivation: family | 0.086 | 0.108 | 0.286 |
| Move motivation: host family | 0.874 | 0.690 | 0.588 |
| Move motivation: other | 0.096 | 0.022 | 0.408 |
| State: Schleswig-Holstein | 0.500 | 0.640 | 0.230 |
| State: Hamburg | 0.326 | 0.424 | 0.352 |
| State: Niedersachsen | 0.706 | 0.144 | 0.968 |
| State: Bremen | 0.798 | 0.796 | 0.724 |
| State: North Rhine-Westphalia | 0.580 | 0.160 | 0.408 |
| State: Hesse | 0.988 | 0.336 | 0.838 |
| State: Rhineland-Palatinate | 0.882 | 0.200 | 0.906 |
| State: Baden-Württemberg | 0.232 | 0.852 | 0.494 |
| State: Bavaria | 0.772 | 0.724 | 0.586 |
| State: Saarland | 0.976 | 0.848 | 0.876 |
| State: Berlin | 0.250 | 0.914 | 0.562 |
| State: Brandenburg | 0.044 | 0.298 | 0.128 |
| State: Mecklenburg-Vorpommern | 0.818 | 0.528 | 0.008 |
| State: Saxony | 0.208 | 0.346 | 0.106 |
| State: Saxony-Anhalt | 0.470 | 0.602 | 0.640 |
| State: Thuringia | 0.246 | 0.656 | 0.768 |
| State: missing | 0.830 | 0.990 | 0.344 |
| Vote choice in Ukraine: no vote | 0.382 | 0.926 | 0.688 |
| Vote choice in Ukraine: Poroshenko | 0.296 | 0.780 | 0.916 |
| Vote choice in Ukraine: Zelensky | 0.908 | 0.898 | 0.798 |

# C Regression Tables with Main Results

Table C.1: Main results

| Dependent Variables: | German support (1) | Belonging (2) | Return intentions (3) | Military support (4) |
|---|---|---|---|---|
| *Variables* | | | | |
| Oppose | -0.016 | 0.003 | 0.064 | -0.410*** |
| | (0.058) | (0.057) | (0.060) | (0.057) |
| Ambivalent | 0.011 | 0.0008 | 0.104* | -0.078 |
| | (0.057) | (0.057) | (0.059) | (0.057) |
| Support | 0.019 | 0.041 | -0.032 | -0.094 |
| | (0.058) | (0.058) | (0.061) | (0.058) |
| Observations | 2,196 | 2,250 | 2,215 | 2,215 |
| $R^2$ | 0.101 | 0.069 | 0.051 | 0.081 |

*IID standard-errors in parentheses*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table C.2: Heterogeneous treatment effects by war termination preference

| Dependent Variables: | German support | Belonging | Return intentions | Military support | German support | Belonging | Return intentions | Military support |
|---|---|---|---|---|---|---|---|---|
| War termination pref. | Pref. territorial loss | | | | Pref. war | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Variables* | | | | | | | | |
| Oppose | 0.072 | 0.024 | 0.141 | -0.260*** | -0.088 | -0.003 | -0.009 | -0.440*** |
| | (0.089) | (0.095) | (0.099) | (0.096) | (0.077) | (0.076) | (0.080) | (0.073) |
| Ambivalent | 0.082 | 0.064 | 0.032 | -0.050 | -0.026 | -0.021 | 0.113 | -0.043 |
| | (0.089) | (0.094) | (0.099) | (0.095) | (0.076) | (0.075) | (0.078) | (0.073) |
| Support | 0.107 | -0.032 | -0.181* | -0.135 | 0.0008 | 0.096 | 0.042 | -0.036 |
| | (0.089) | (0.094) | (0.099) | (0.096) | (0.079) | (0.077) | (0.082) | (0.075) |
| Prefers war over territorial loss | No | No | No | No | Yes | Yes | Yes | Yes |
| Observations | 788 | 800 | 794 | 789 | 1,341 | 1,376 | 1,349 | 1,355 |
| $R^2$ | 0.129 | 0.103 | 0.094 | 0.105 | 0.117 | 0.078 | 0.060 | 0.100 |

*IID standard-errors in parentheses*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table C.3: Heterogeneous treatment effects by mainstream media exposure

| Dependent Variables: | German support | Belonging | Return intentions | Military support | German support | Belonging | Return intentions | Military support |
|---|---|---|---|---|---|---|---|---|
| Mainstream media | No mainstream media | | | | Any mainstream media | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Variables* | | | | | | | | |
| Oppose | -0.002 | 0.049 | 0.095 | -0.387*** | -0.048 | -0.040 | 0.039 | -0.420*** |
| | (0.082) | (0.081) | (0.085) | (0.079) | (0.082) | (0.080) | (0.086) | (0.084) |
| Ambivalent | 0.027 | 0.059 | 0.153* | 0.022 | -0.015 | -0.046 | 0.041 | -0.192** |
| | (0.079) | (0.078) | (0.081) | (0.076) | (0.083) | (0.081) | (0.087) | (0.085) |
| Support | 0.014 | 0.008 | 0.142* | -0.060 | 0.028 | 0.059 | -0.222** | -0.141* |
| | (0.084) | (0.082) | (0.086) | (0.080) | (0.083) | (0.081) | (0.087) | (0.085) |
| Exposed to mainstream media | No | No | No | No | Yes | Yes | Yes | Yes |
| Observations | 1,173 | 1,207 | 1,190 | 1,186 | 1,023 | 1,043 | 1,025 | 1,029 |
| $R^2$ | 0.108 | 0.073 | 0.062 | 0.096 | 0.124 | 0.111 | 0.077 | 0.105 |

*IID standard-errors in parentheses*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*



Table C.4: Heterogeneous treatment effects by prior integration level

| Dependent Variables: | German support | Belonging | Return intentions | Military support | German support | Belonging | Return intentions | Military support |
|---|---|---|---|---|---|---|---|---|
| Prior integration | Low integration | | | | High integration | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Variables* | | | | | | | | |
| Oppose | 0.047 | -0.018 | 0.054 | -0.448*** | -0.019 | 0.128* | 0.002 | -0.329*** |
| | (0.088) | (0.086) | (0.091) | (0.087) | (0.078) | (0.072) | (0.082) | (0.079) |
| Ambivalent | 0.187** | 0.072 | -0.021 | -0.032 | -0.069 | -0.014 | 0.152* | -0.091 |
| | (0.089) | (0.086) | (0.091) | (0.087) | (0.076) | (0.070) | (0.080) | (0.077) |
| Support | 0.149* | 0.104 | -0.120 | -0.113 | 0.0002 | 0.049 | -0.009 | -0.023 |
| | (0.090) | (0.087) | (0.092) | (0.088) | (0.078) | (0.073) | (0.083) | (0.079) |
| Prior integration level | Low | Low | Low | Low | High | High | High | High |
| Observations | 952 | 975 | 963 | 959 | 1,178 | 1,188 | 1,181 | 1,182 |
| $R^2$ | 0.137 | 0.080 | 0.058 | 0.122 | 0.121 | 0.118 | 0.082 | 0.095 |

*IID standard-errors in parentheses*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

## C.1 Results Without Covariates

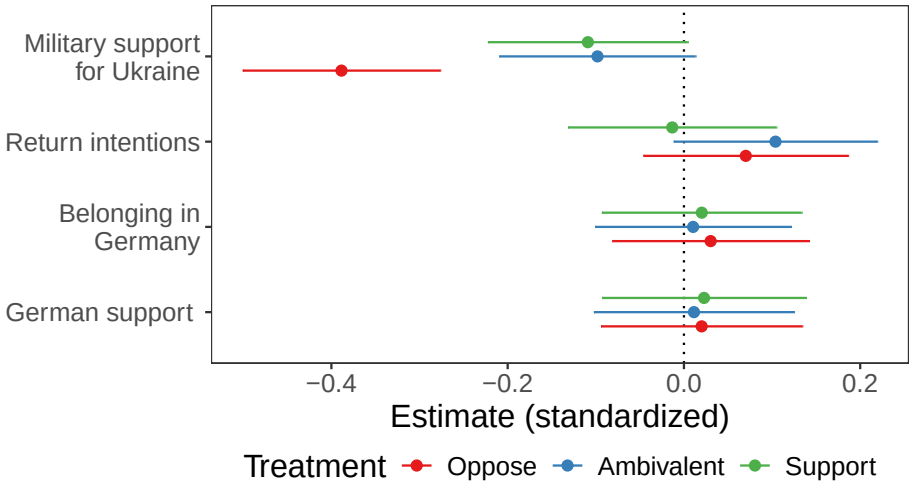


Figure C.1: Main estimates without covariates. Points show OLS estimates; whiskers show 95% CIs. Outcomes are standardized using the control-group mean and SD.

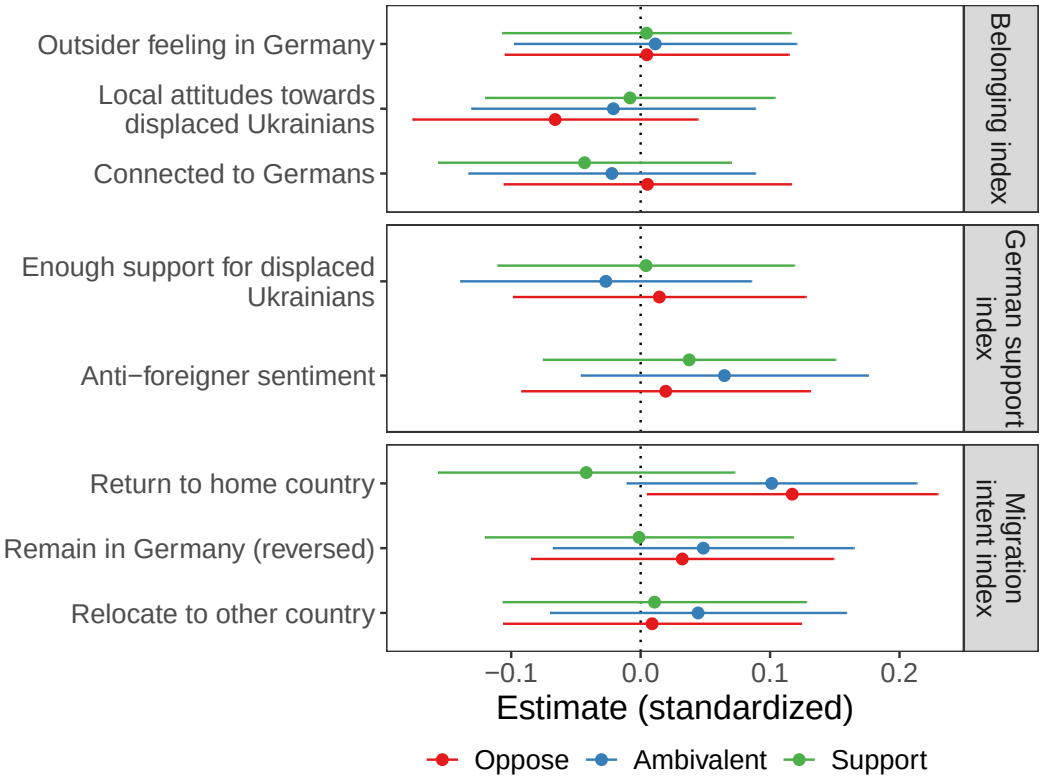


Figure C.2: Item-level estimates without covariates. Points show OLS estimates; whiskers show 95% CIs. Items are standardized using the control-group mean and SD.

# D Pre-Registered Mechanisms Tests

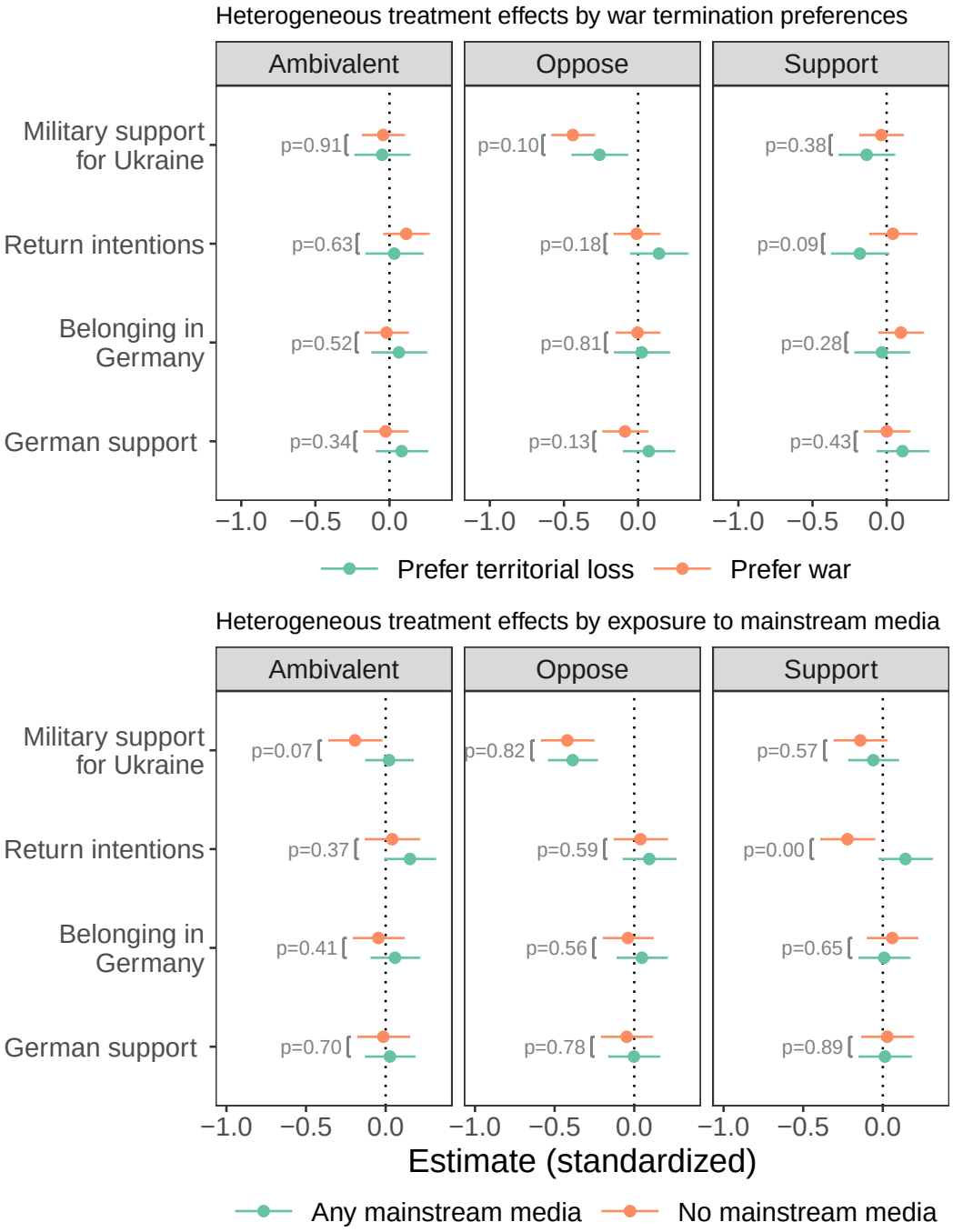


Figure D.1: Estimates of the Treatment Effects Heterogeneity by War Termination Preferences (top panel) and Prior Media Consumption (bottom panel). *Estimates correspond to the effect of treatment relative to the control condition (weather report). See Section C for the corresponding regression table.*

# E Additional Results

## E.1 Heterogeneity by Prior Integration

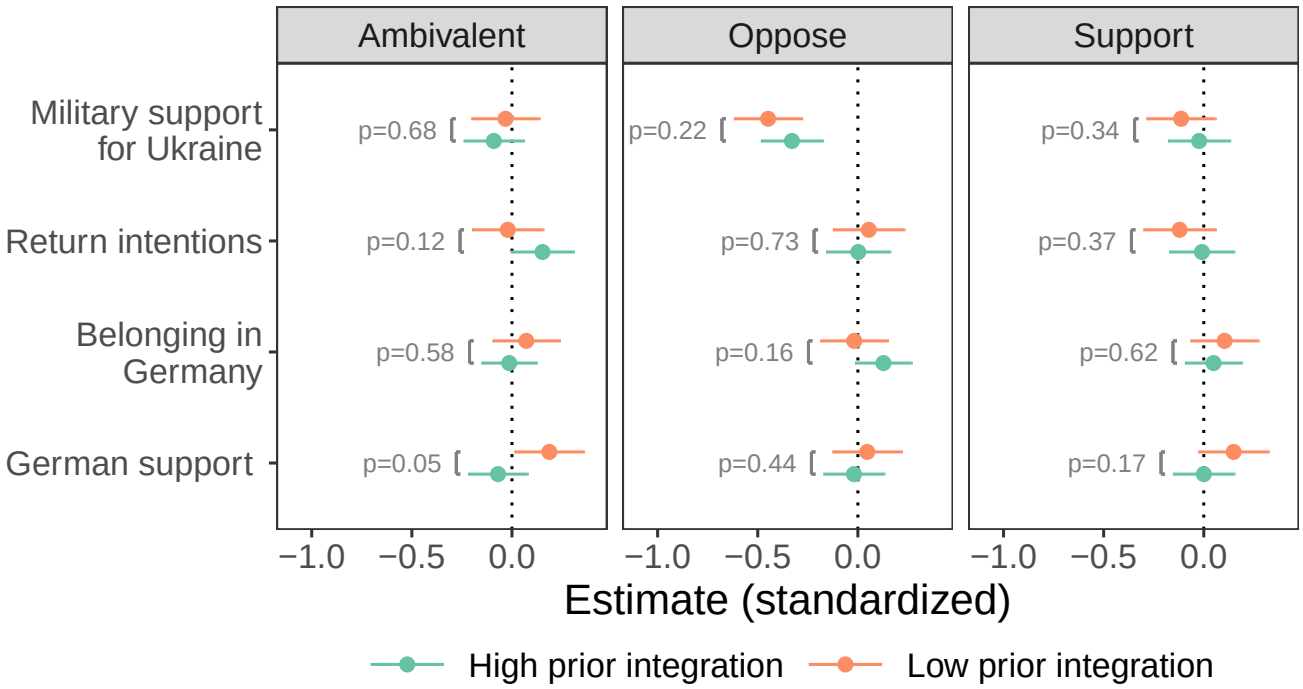


Figure E.1: Estimates of Treatment Effects Heterogeneity by Prior Integration Index. *High and low groups are based on a median split.*

## E.2 Heterogeneity by War Termination Preferences

Similar to the pre-registered analyses, we can expect that the treatment effects we observe on low-saturation days are moderated by war-termination preferences (fight to retake all territory vs. negotiate even with territorial concessions). We therefore restrict to the no-mention subsample and re-estimate the model separately for these two groups. Because Figure 4 shows the three treatments move outcomes in the same direction with similar magnitudes under low salience, we pool them and compare the combined treatment group to the placebo control group. Figure E.2 displays the estimates across war termination preferences subgroups.

The results on no-mention subsample should be read cautiously given small sample sizes, but a clear pattern emerges. Among *"hawks"* (those who prefer fight to retake all territory) the treatment sharply lowers perceived German military support and broader German support for Ukrainians; this shift is absent among *"doves"* (those who favor negotiations and possible territorial concessions). In contrast, the increase in return intentions is larger for doves—driven by a higher stated likelihood of returning to Ukraine. Belonging declines modestly and similarly for both groups, primarily via weaker reported connection to Germans and stronger outsider feelings.

Taken together, the pattern is consistent with the valence-independent contestation mechanism. Under low salience, any elite cue (regardless of valence) appears to function as a contestation signal. Hawks respond primarily by sharply downgrading perceived, military and broader, German support—consistent with interpreting visible debate as wavering allied resolve. Doves show comparatively little belief updating about support but do increase their return intentions, suggesting they translate contestation into revised migration timing rather than into support re-assessment. Both groups register modest declines in belonging, in line with the idea that debate visibility-rather than its directional content-signals a less secure social status. Given small subgroup Ns, these heterogeneity estimates are imprecise and should be viewed as suggestive.

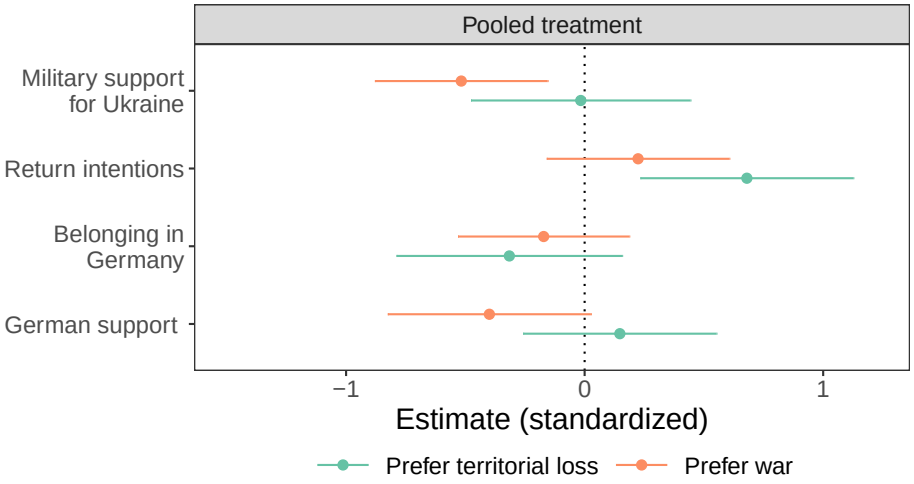


Figure E.2: No-mention subgroup treatment effects by war termination preferences. *Points show OLS estimates; whiskers show 95% confidence intervals. Outcomes are standardized using the control-group mean and SD. All three treatments are pooled and compared to control within the no-mention subset.*

## E.3 Experimenter Demand Effects

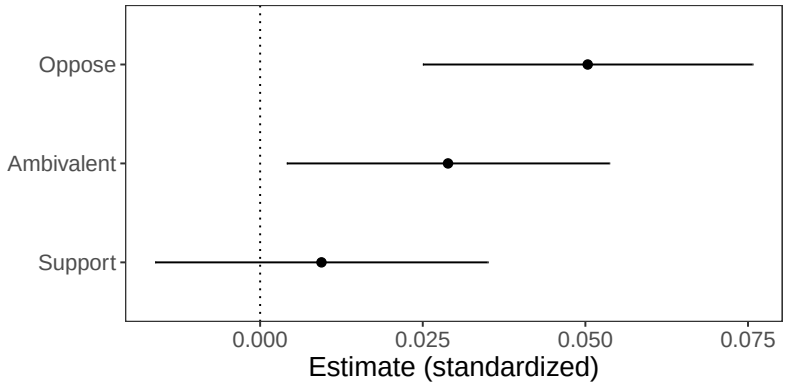


Figure E.3: Estimates of treatment effect on guessing the aim of the study. *Rates of correctly guessing the outcome were 1.86% in the control condition, 6.58% in the oppose condition, 4.64% in the ambivalent condition, and 2.84% in the support condition.*

## E.4 Attrition

For our main attrition indicator, we coded an observation as missing if the respondent reached the treatment assignment in the survey, but all main outcomes were missing. Based on this indicator, we observe an attrition rate of approximately 8.9%. Table D.1 below presents the results of pre-registered attrition tests of missingness related to treatment assignment—unequal variance $t$-tests and $F$-tests of the relationship between attrition and treatment-by-covariate interactions. The results indicate that our main treatments, *Oppose* and *Ambivalent*, did not show attrition rates different from the control condition. However, attrition rates in the *Support* treatment were systematically different from all other experimental groups, suggesting that the video shown in this condition prompted respondents to drop out.

Table E.1: t and F tests of relationship between treatment assignment and attrition

| Outcome | $t$-test $p$-values | | | | | | $F$-test $p$-value |
|---|---|---|---|---|---|---|---|
| | control vs oppose | control vs support | control vs ambival | oppose vs support | oppose vs ambival | support vs ambival | |
| Missing Military Support | 0.907 | 0.004 | 0.195 | 0.006 | 0.154 | 0.001 | 0.338 |
| Missing German Support | 0.862 | 0.003 | 0.240 | 0.006 | 0.160 | 0.000 | 0.244 |
| Missing Belonging in Germany | 0.841 | 0.001 | 0.240 | 0.008 | 0.184 | 0.000 | 0.828 |
| Missing Return Intentions | 0.692 | 0.000 | 0.408 | 0.001 | 0.235 | 0.000 | 0.828 |
| Missing All Outcomes | 0.715 | 0.000 | 0.387 | 0.000 | 0.656 | 0.000 | |

Each p-value corresponds to separate test of independence of treatment assignment from attrition pattern we observe. The F-test is conducted excluding Support treatment that is not included in the analyses and exhibits different patterns of attrition. Covariates included in the model for the F-test are all covariates used in our main regression model specification and indicator of passing attention check.

# F Summary Statistics

Summary statistics for treatments, outcomes, and moderators

| Variable | Mean | Median | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Treatment: Oppose | 0.25 | 0.00 | 0.43 | 0.00 | 1.00 |
| Treatment: Ambivalent | 0.25 | 0.00 | 0.43 | 0.00 | 1.00 |
| Treatment: Support | 0.25 | 0.00 | 0.43 | 0.00 | 1.00 |
| Treatment: Control | 0.25 | 0.00 | 0.43 | 0.00 | 1.00 |
| German support | 5.37 | 5.00 | 1.48 | 0.00 | 8.00 |
| Belonging in Germany | 6.59 | 7.00 | 2.49 | 0.00 | 12.00 |
| Return intentions | 4.41 | 4.00 | 2.42 | 0.00 | 12.00 |
| Military support for Ukraine | 1.76 | 2.00 | 1.24 | 0.00 | 4.00 |
| War termination preference: prefer war | 0.62 | 1.00 | 0.49 | 0.00 | 1.00 |
| Any mainstream media | 0.45 | 0.00 | 0.50 | 0.00 | 1.00 |
| Prior integration | 9.34 | 9.00 | 4.45 | 0.00 | 26.00 |

Summary statistics for control variables

| Variable | Mean | Median | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Age | 41.72 | 40.00 | 12.79 | 18.00 | 100.00 |
| Education | 3.37 | 4.00 | 1.23 | 0.00 | 5.00 |
| Employment: Employed | 0.22 | 0.00 | 0.41 | 0.00 | 1.00 |
| Employment: Odd jobs | 0.05 | 0.00 | 0.23 | 0.00 | 1.00 |
| Employment: Other | 0.17 | 0.00 | 0.37 | 0.00 | 1.00 |
| Employment: Pensioner | 0.07 | 0.00 | 0.26 | 0.00 | 1.00 |
| Employment: Running household | 0.06 | 0.00 | 0.24 | 0.00 | 1.00 |
| Employment: Self-employed | 0.03 | 0.00 | 0.16 | 0.00 | 1.00 |
| Employment: Student | 0.06 | 0.00 | 0.23 | 0.00 | 1.00 |
| Employment: Unemployed | 0.34 | 0.00 | 0.47 | 0.00 | 1.00 |
| Female | 0.81 | 1.00 | 0.39 | 0.00 | 1.00 |
| Male | 0.19 | 0.00 | 0.39 | 0.00 | 1.00 |
| Other gender | 0.00 | 0.00 | 0.05 | 0.00 | 1.00 |
| Income | 3.34 | 3.00 | 0.97 | 1.00 | 6.00 |
| Married | 0.51 | 0.51 | 0.47 | 0.00 | 1.00 |
| Move motivation: assigned | 0.18 | 0.00 | 0.39 | 0.00 | 1.00 |
| Move motivation: family | 0.51 | 1.00 | 0.50 | 0.00 | 1.00 |
| Move motivation: host family | 0.18 | 0.00 | 0.39 | 0.00 | 1.00 |
| Move motivation: other | 0.16 | 0.00 | 0.37 | 0.00 | 1.00 |
| State: Schleswig-Holstein | 0.03 | 0.00 | 0.17 | 0.00 | 1.00 |
| State: Hamburg | 0.03 | 0.00 | 0.18 | 0.00 | 1.00 |
| State: Niedersachsen | 0.08 | 0.00 | 0.27 | 0.00 | 1.00 |
| State: Bremen | 0.01 | 0.00 | 0.10 | 0.00 | 1.00 |
| State: North Rhine-Westphalia | 0.21 | 0.00 | 0.41 | 0.00 | 1.00 |
| State: Hesse | 0.08 | 0.00 | 0.28 | 0.00 | 1.00 |
| State: Rhineland-Palatinate | 0.03 | 0.00 | 0.17 | 0.00 | 1.00 |
| State: Baden-Württemberg | 0.14 | 0.00 | 0.35 | 0.00 | 1.00 |
| State: Bavaria | 0.17 | 0.00 | 0.37 | 0.00 | 1.00 |
| State: Saarland | 0.01 | 0.00 | 0.12 | 0.00 | 1.00 |
| State: Berlin | 0.07 | 0.00 | 0.25 | 0.00 | 1.00 |
| State: Brandenburg | 0.02 | 0.00 | 0.15 | 0.00 | 1.00 |
| State: Mecklenburg-Vorpommern | 0.02 | 0.00 | 0.13 | 0.00 | 1.00 |
| State: Saxony | 0.05 | 0.00 | 0.22 | 0.00 | 1.00 |
| State: Saxony-Anhalt | 0.02 | 0.00 | 0.15 | 0.00 | 1.00 |
| State: Thuringia | 0.02 | 0.00 | 0.14 | 0.00 | 1.00 |
| State: missing | 0.03 | 0.00 | 0.16 | 0.00 | 1.00 |
| Vote choice in Ukraine: no vote | 0.27 | 0.00 | 0.44 | 0.00 | 1.00 |
| Vote choice in Ukraine: Poroshenko | 0.20 | 0.00 | 0.40 | 0.00 | 1.00 |
| Vote choice in Ukraine: Zelensky | 0.53 | 1.00 | 0.50 | 0.00 | 1.00 |

Note: This table presents summary statistics for all control variables used in the analysis.

# G Tagesschau Media Coverage Analysis

This Section provides comprehensive documentation of our analysis of German media coverage using transcripts from the *Tagesschau*, Germany's most-watched daily news program. We use this analysis to examine how the information environment moderates treatment effects in our experiment.

## G.1 Data Collection and Classification Procedure

We obtained transcripts for the 20:00 edition of *Tagesschau* covering our entire survey period (March 8–April 9, 2024). The *Tagesschau* is the most important news program in Germany and generally reports on the most significant news of the day. If there is sufficiently salient news related to the conflict between Ukraine and Russia as well as the German government's stance on this topic, the *Tagesschau* will report on it.

We employed a two-stage classification process using GPT-4o-mini:

**Stage 1: Ukraine Content Extraction** – We first extracted any portions of each transcript that discussed Ukraine, the Ukrainian people, or the war in Ukraine.

**Stage 2: German Government Stance Analysis** – For episodes containing Ukraine-related content, we classified whether the coverage signaled German government support, opposition, or neutrality regarding military aid to Ukraine, or contained no mention of the German government's stance.

## G.2 Validation Using Human Coders

We conducted a validation exercise in which three research assistants independently hand-coded a subset of *Tagesschau* episodes using the same four stance categories as the automated classifier: support, opposition, neutral, and no stance mentioned, plus a binary indicator for Ukraine mentioned. Coders worked from full transcripts, followed a short written codebook aligned with the prompts in §G, and were blinded to the automated labels and to each other's assignments. The validation set comprised 32 episodes from the *Tagesschau* broadcasts that overlapped with our survey fielding period for the mention indicator.

Agreement between each human coder and the automated classifier is high (Table G.1). For the mention indicator, agreement is perfect (1.00) for two coders and near-perfect (0.97) for the third coder. For stance, agreement ranges from 0.76–1.00 across categories and coders, with the lowest values for "no stance mentioned" (0.76–0.83) and the highest values for "neutral" (0.92–1.00). Inter-coder reliability between the three research assistants was also high, with 84–100 % agreement across stance categories, providing additional confidence in the manual coding quality. These results indicate close correspondence between the automated and human classifications for the purposes of our analysis.

Table G.1: Agreement rates by individual coder versus automated classifier

| | Agreement rate | | |
|---|---|---|---|
| Variable | Coder A | Coder B | Coder C |
| Ukraine mentioned | 1.00 | 0.97 | 1.00 |
| Support stance | 0.84 | 0.84 | 0.79 |
| Opposition stance | 0.88 | 0.92 | 0.92 |
| Neutral stance | 0.92 | 1.00 | 0.96 |
| No stance mentioned | 0.76 | 0.76 | 0.83 |

*Notes:*
Entries are the proportion of episodes with exact label match between manual coding by research assistants and automated classifier (GPT-4o-mini), on a 0–1 scale. Stance categories include support, opposition, neutral, and no stance mentioned. N = 32 episodes for Ukraine mentioned; N = 25 episodes for stance variables (episodes where Ukraine was mentioned).

## G.3 Classification Rules and Prompts

The exact prompts used for our GPT-4o-mini classification are provided below, in both German (original) and English (translation).

### Stage 1: Ukraine Content Extraction German Original:

*Extrahiere nur die Teile dieses deutschen Nachrichtentextes, die sich auf die Ukraine, das ukrainische Volk oder den Krieg in der Ukraine beziehen. Gib nur den extrahierten Text zurück, sonst nichts. Wenn nichts über die Ukraine vorhanden ist, gib 'NONE' zurück. Text: [TRANSCRIPT]*

**English Translation:**

*Extract only the parts of this German news text that relate to Ukraine, the Ukrainian people, or the war in Ukraine. Return only the extracted text, nothing else. If nothing about Ukraine is present, return 'NONE'. Text: [TRANSCRIPT]*

### Stage 2: German Government Stance Analysis German Original:

*Bitte analysieren Sie den folgenden deutschen Nachrichtentext. Bestimmen Sie, ob darin Hinweise darauf enthalten sind, ob die deutsche Regierung Unterstützung für die Ukraine im Konflikt mit Russland signalisiert oder nicht, sei es direkt oder indirekt. Falls ja, geben Sie bitte die Haltung der deutschen Regierung an, indem Sie eine der folgenden Optionen auswählen:*

*1. Ein Signal militärischer Unterstützung für die Ukraine, auch wenn das Signal schwach ist [support]*

*2. Ein Signal gegen militärische Unterstützung für die Ukraine, auch wenn das Signal schwach ist [oppose]*

*3. Eine völlig neutrale Haltung zu diesem Thema [neutral]*

*4. Der Text enthält keine Hinweise auf die Haltung der deutschen Regierung zu diesem Thema [not_mentioned]*

*Bitte antworten Sie ausschließlich mit dem entsprechenden Text in Klammern und fügen Sie nichts Weiteres hinzu.*

*Text: [UKRAINE_CONTENT]*

**English Translation:**

*Please analyze the following German news text. Determine whether it contains indications of whether the German government signals support for Ukraine in the conflict with Russia or not, either directly or indirectly. If so, please indicate the German government's stance by selecting one of the following options:*

*1. A signal of military support for Ukraine, even if the signal is weak [support]*

*2. A signal against military support for Ukraine, even if the signal is weak [oppose]*

*3. A completely neutral stance on this topic [neutral]*

*4. The text contains no indications of the German government's stance on this topic [not_mentioned]*

*Please respond exclusively with the corresponding text in brackets and add nothing further.*

*Text: [UKRAINE_CONTENT]*

## G.4 Coverage Examples During Survey Period

To illustrate the variation in German media coverage of military support for Ukraine during our study period, Table G.2 presents examples of *Tagesschau* episodes classified by their stance toward German government support. These examples are drawn from episodes that aired on survey days or the day before survey days during our survey period.

Table G.2: Examples of *Tagesschau* Coverage by Stance Classification

| Stance | Date | German Original | English Translation |
|---|---|---|---|
| Support | March 19, 2024 | Mehr als zwei Jahre nach Beginn des russischen Überfalls auf die Ukraine gerät das Land militärisch stärker unter Druck. Wie die westliche Hilfe ausgebaut werden kann, darüber hat heute die Ukraine-Kontaktgruppe in Ramstein beraten. Kiew benötigt mehr Waffen und Munition, um den russischen Vormarsch zu stoppen... | More than two years after the beginning of the Russian attack on Ukraine, the country is coming under stronger military pressure. The Ukraine Contact Group met in Ramstein today to discuss how Western aid can be expanded. Kyiv needs more weapons and ammunition to stop the Russian advance... |
| Oppose | March 13, 2024 | Bundeskanzler Scholz hat im Bundestag erneut seine Haltung verteidigt, keine Taurus-Lenkflugkörper an die Ukraine zu liefern. Die Union will zum Marschflugkörper Taurus fragen und warum Scholz ihn nicht an die Ukraine liefern will. Die Union unterstellt dem Kanzler, der Ukraine zu misstrauen... | Chancellor Scholz has once again defended his position in the Bundestag not to deliver Taurus cruise missiles to Ukraine. The Union wants to ask about the Taurus cruise missile and why Scholz does not want to deliver it to Ukraine. The Union accuses the Chancellor of distrusting Ukraine... |
| Mentioned but no stance | March 31, 2024 | In der Ukraine gedenken die Menschen heute der Opfer von Butscha. In der Kleinstadt wurden Hunderte Zivilisten ermordet. Als die russischen Besatzer vor genau zwei Jahren abzogen, lagen Leichen auf den Straßen — viele gefesselt und gefoltert. Die Ukraine wirft Moskau Kriegsverbrechen vor... | In Ukraine, people are commemorating the victims of Bucha today. Hundreds of civilians were murdered in the small town. When the Russian occupiers withdrew exactly two years ago, bodies lay on the streets — many bound and tortured. Ukraine accuses Moscow of war crimes... |

## G.5 Temporal Distribution of Coverage Types

During our survey period, among the 31 *Tagesschau* episodes on survey-relevant dates, 41.9% were classified as "Mentioned but no stance" (coverage of Ukraine without reference to German government stance), 25.8% as "Support" (signaling government support for military aid), 22.6% as "Not mentioned" (no Ukraine coverage), and 9.7% as "Oppose" (signaling government opposition to military aid, primarily regarding Taurus missile deliveries). No episodes were classified as taking a neutral stance during this period.

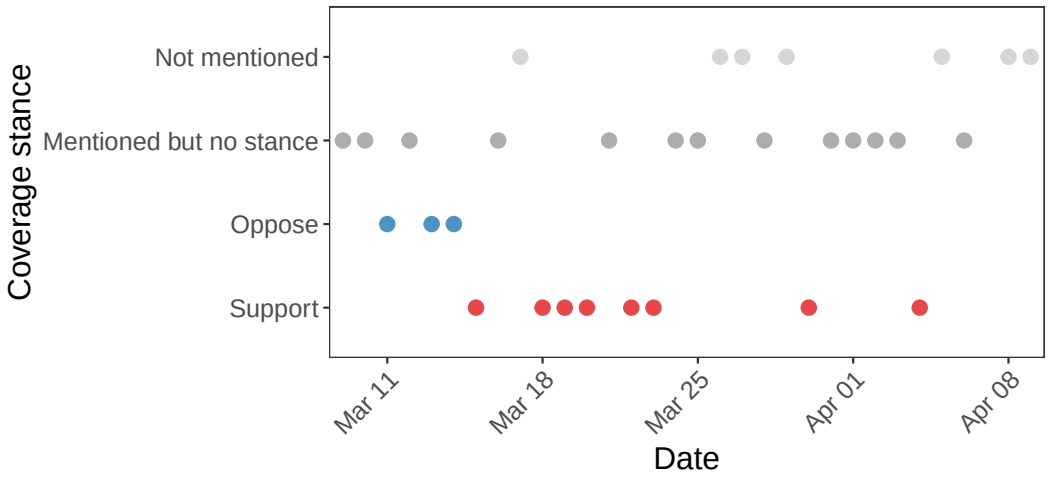


Figure G.1: *Tagesschau* Coverage by Date During Survey Period

Figure G.1 shows the temporal distribution of coverage types across the survey period. The plot illustrates how different types of *Tagesschau* coverage varied day by day, providing context for understanding the media environment that survey respondents experienced.

## G.6 Balance Across Media Coverage Conditions

To assess whether our sample exhibits systematic differences based on media coverage patterns that could confound our analysis, we examine balance across covariates by *Tagesschau* coverage on the day preceding survey participation. Figure G.2 presents standardized differences in means between respondents who participated when Ukraine received any media coverage versus those who participated when Ukraine was not mentioned in German media.


Figure G.2: Balance Check: Any Ukraine Mention vs. No Mention in *Tagesschau*. *Standardized differences in means between respondents surveyed when Ukraine received any media coverage versus when Ukraine was not mentioned. Points represent standardized differences; error bars show 95% confidence intervals. Dashed lines indicate conventional imbalance thresholds (±0.1 and ±0.25 standard deviations). Stars indicate statistical significance: *** p<0.001, ** p<0.01, * p<0.05, . p<0.1. Legend denotes significance (filled = significant, open = not significant).*

The balance analysis reveals no systematic evidence of selection into our sample based on media coverage patterns. Across 29 demographic, socioeconomic, geographic, and moderator covariates (including war termination preferences, prior media exposure, and prior integration), we observe only one statistically significant difference (3.4% of tests) and one marginally significant difference (3.4% of tests), with 93.1% of comparisons showing no significant imbalance. Most importantly, all standardized differences fall well within conventional balance thresholds, with no differences exceeding 0.25 standard deviations and only a few exceeding the more stringent 0.1 standard deviation threshold.

This balance indicates that respondents who participated during periods of high versus low Ukraine media salience are comparable across observed characteristics. Therefore, the differential treatment effects we observe by media coverage condition are unlikely to be driven by systematic differences in sample composition.

We also conducted balance tests across the more granular, uncollapsed Tagesschau coverage categories (support, oppose, mentioned but no stance, and not mentioned). These analyses similarly revealed only very few statistically significant differences across covariates, with small substantive differences that fall well within conventional balance thresholds. This additional evidence further confirms that our sample composition does not vary systematically by the specific type of media coverage on survey days.

## G.7 Time-Trend Adjusted Treatment Effects by Media Coverage

To address potential unobserved time-varying confounding, we re-estimate the treatment effects by Tagesschau coverage with a linear time trend added to the regression model. Specifically, we include a covariate equal to the number of days since the first survey date and replicate the split-sample specification from Figure 4.

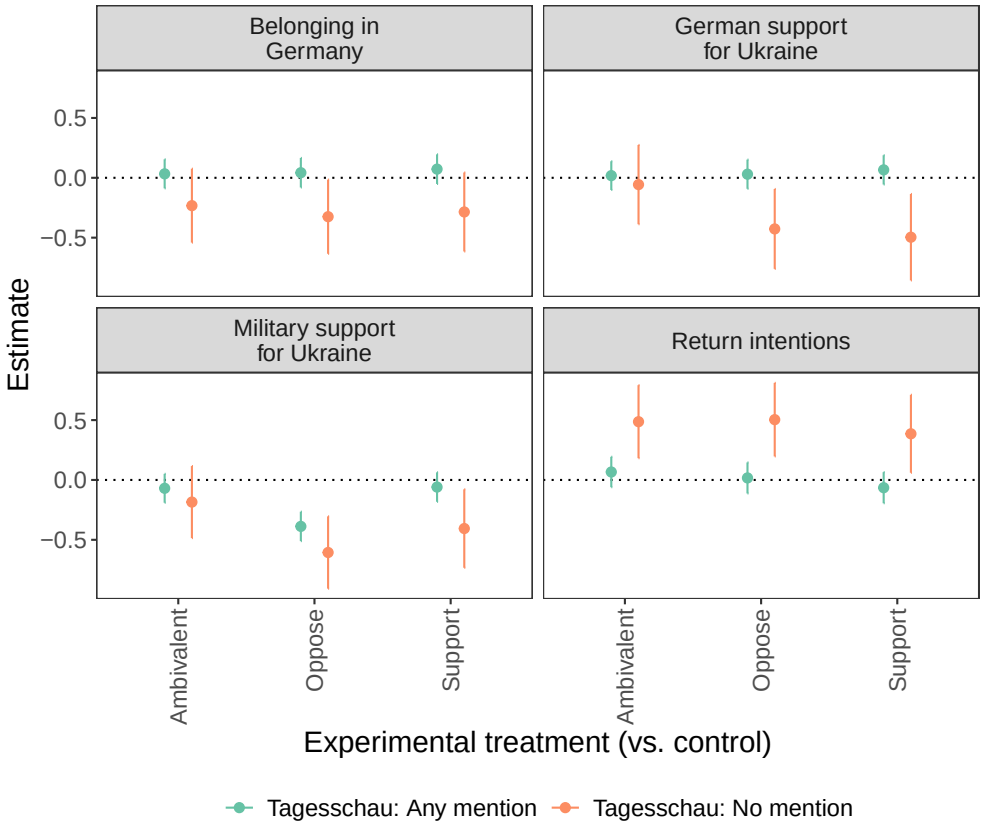


Figure G.3: Treatment effects by Tagesschau coverage (any vs. none) controlling for a linear time trend. Points show OLS estimates; whiskers show 95% CIs. Outcomes exclude the manipulation check.

We report the results in Figure G.3. The time-trend adjusted results closely mirror the unadjusted results: sizable negative effects on perceived German support and increased return intentions, with similar magnitudes, remain

concentrated in the no-coverage subsample. Effects remain small and imprecise when coverage is present. If anything, adding the time trend slightly tightens several confidence intervals without changing qualitative conclusions.

## G.8 Clustered-by-Day Treatment Effects by Media Coverage

As an additional check for within-day correlation in outcomes, we re-estimate the split-sample specification clustering standard errors by survey day. Figure G.4 shows the resulting estimates.

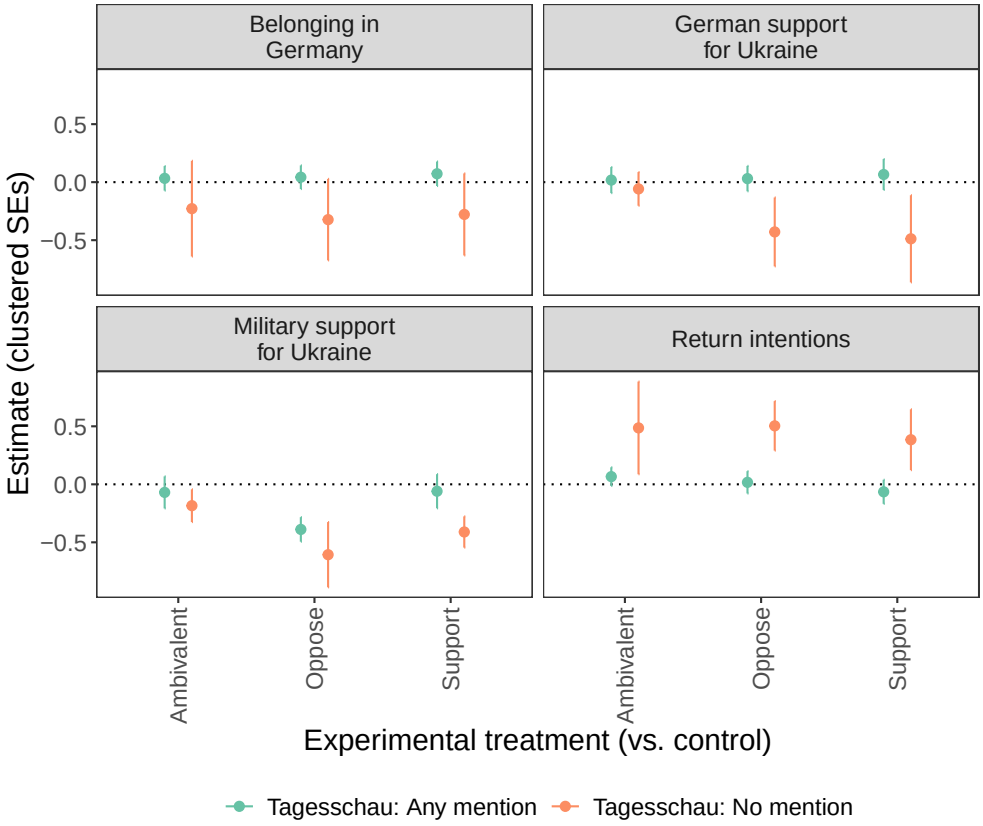


Figure G.4: Treatment effects by Tagesschau coverage (any vs. none) with standard errors clustered by survey day. Points show OLS estimates; whiskers show 95% clustered CIs.

Clustering by survey day modestly widens some confidence intervals, as expected, but leaves the substantive pattern intact: effects are concentrated in the no-coverage condition and remain small and imprecise under any coverage. We find no indication that the main conclusions hinge on not clustering by day.

## G.9 Newey–West (HAC) Standard Errors by Media Coverage

As an additional robustness check for serial correlation across survey days, we re-estimate the split-sample specification using Newey–West heteroskedasticity- and autocorrelation-consistent (HAC) standard errors with a 3-day lag, using the survey date as the time index and respondent `id` as the panel identifier (Figure G.5). The estimates closely match the baseline and clustered-by-day results: confidence intervals widen minimally and the substantive conclusions remain unchanged.

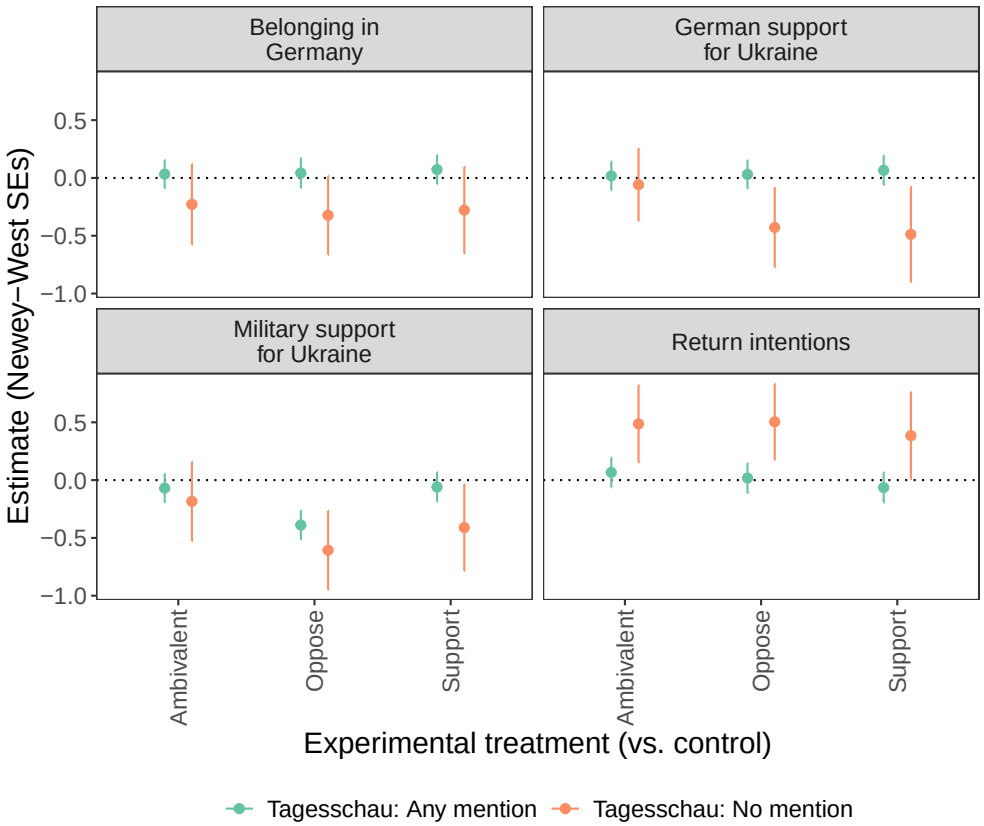


Figure G.5: Treatment effects by Tagesschau coverage (any vs. none) with Newey–West (3-day lag) standard errors. Points show OLS estimates; whiskers show 95% HAC CIs. Outcomes exclude the manipulation check.

## G.10 Permutation Test Using Placebo Coverage Dates

We assess whether the sharp difference in treatment effects between "Any mention" and "No mention" at lag $k$=1 reflects a genuine alignment with the news environment, rather than an artifact of generic day-to-day fluctuations. For each integer shift $k$ in a symmetric window around the survey date (we use $k \in [-10, 10]$ and keep only shifts with available *Tagesschau* data), we recompute the pooled treatment effect (treated vs. control) separately within the two media conditions and take the difference $\Delta_k \equiv \widehat{\tau}_{\text{Any}}(k) - \widehat{\tau}_{\text{None}}(k)$. By construction, positive $k$ are lags (coverage dated $k$ days before the survey date); thus $k$=1 is the design-relevant case where *Tagesschau* coverage is measured on the evening before a respondent completes the survey. Negative $k$ are leads (i.e., using coverage from dates after the interview). The rationale for examining these placebo shifts is straightforward: coverage many days before the interview, or any coverage dated after it, should not systematically change the treatment-control difference. If our interpretation is correct, only the previous-evening case ($k$=1) should stand out. We therefore compare $\Delta_{k=1}$ to the empirical distribution of $\{\Delta_k : k \neq 1\}$ and report a two-sided empirical $p$-value $\hat{p} = \frac{1}{|\mathcal{K}|} \sum_{k \neq 1} \mathbb{1}\{\, |\Delta_k| \geq |\Delta_1| \,\}$.

Figure G.6 shows the permutation distributions (histograms of $\Delta_k$ for $k \neq 1$) with the observed statistic $\Delta_1$ indicated by a dashed line. The empirical two-sided $p$-values are: 0.000 for German support (without the manipulation item), 0.000 for return intentions, 0.000 for belonging, and 0.200 for military support. These values indicate that the $k$=1 alignment is unusually large relative to other shifts for support, belonging, and return intentions, consistent with a short-term information-saturation interpretation. We take this as evidence that the observed alignment at $k$=1 reflects a genuine effect rather than spurious timing.

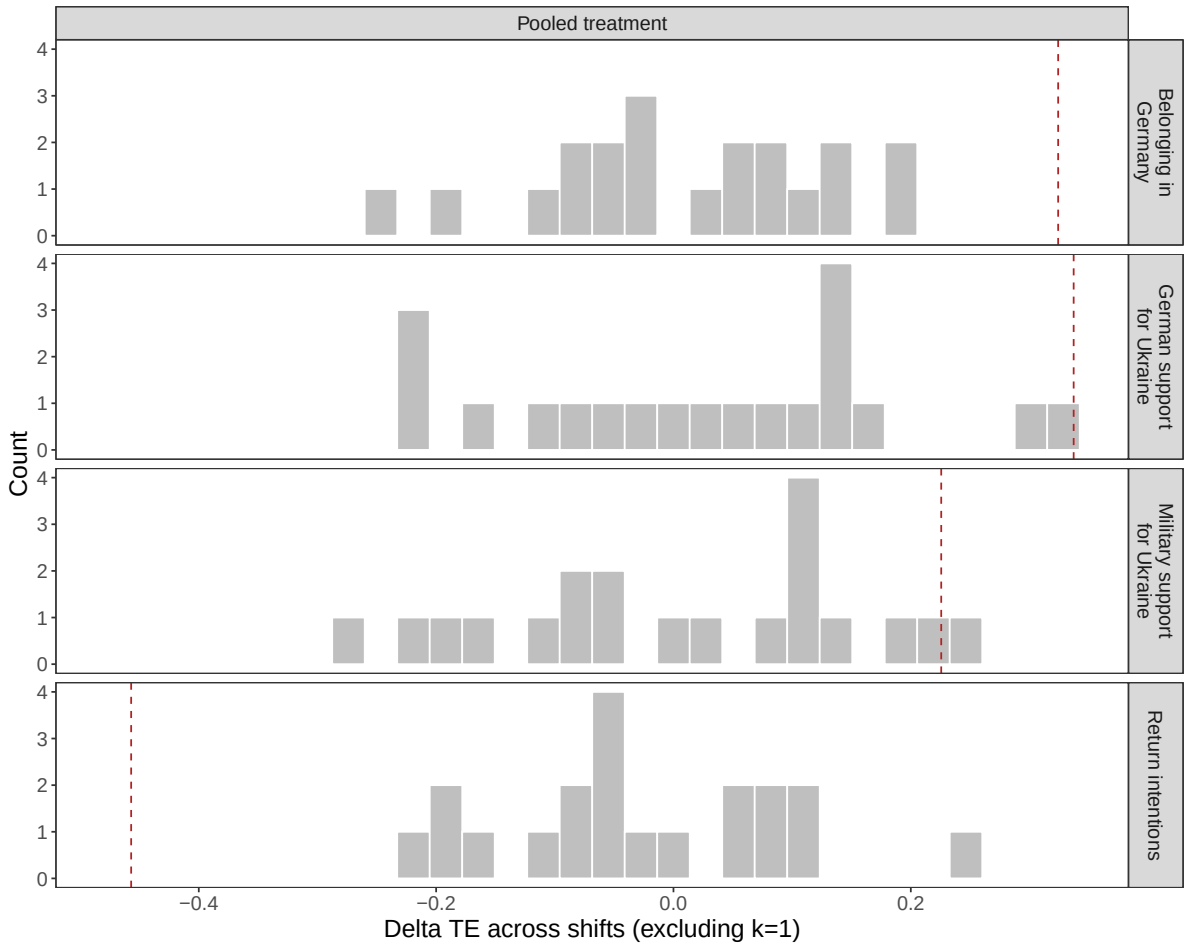


Figure G.6: Permutation distributions of $\Delta_k$ for $k \neq 1$ with observed $\Delta_1$ (dashed). Facets by outcome and pooled treatment.

## G.11 Tagesschau Coverage and War Casualties

We assess whether news coverage simply follows conflict intensity, which we track using daily casualties from the Uppsala Conflict Data Program's Georeferenced Event Dataset (GED, v25.1). We construct a daily series by converting event-level fatalities to the day level (evenly allocating fatalities across multi-day events) and use GED *fatalities (best)* as our casualty proxy.

For each Tagesschau day linked to our respondent sample (the evening before the survey date), we compute the average number of daily fatalities in the $k$-day window $[t-k+1, t]$ for $k \in \{1, 2, 3\}$.

Let $C_t$ denote GED fatalities (best) on day $t$, and define the $k$-day window average

$$\bar{C}_k(t) \equiv \frac{1}{k} \sum_{\tau=t-k+1}^{t} C_\tau.$$

Let $D_t \in \{0, 1\}$ indicate whether the Tagesschau mentioned Ukraine on day $t$ (1 = mention). We compare, for $k \in \{1, 2, 3\}$,

$$E\big(\bar{C}_k(t) \mid D_t = 1\big) \quad \text{vs.} \quad E\big(\bar{C}_k(t) \mid D_t = 0\big).$$

As shown in Figure G.7, differences are small and CIs overlap across $k$. For example, mean prior fatalities are 158 (mention) vs. 155 (no mention) for $k$=1, 159 vs. 155 for $k$=2, and 158 vs. 159 for $k$=3. These results indicate that Tagesschau mentions are not mechanically tracking short-run casualty fluctuations in this period.

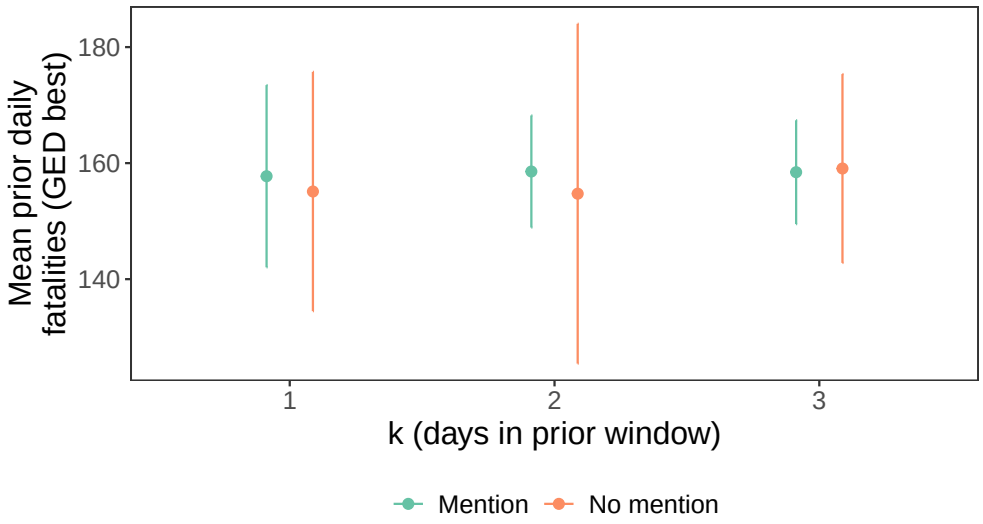


Figure G.7: GED *fatalities (best)*: Mean prior casualties in $[t-k+1, t]$ by Tagesschau mention status, with 95% CIs.

## G.12 Tagesschau Interaction Results (Four Categories)

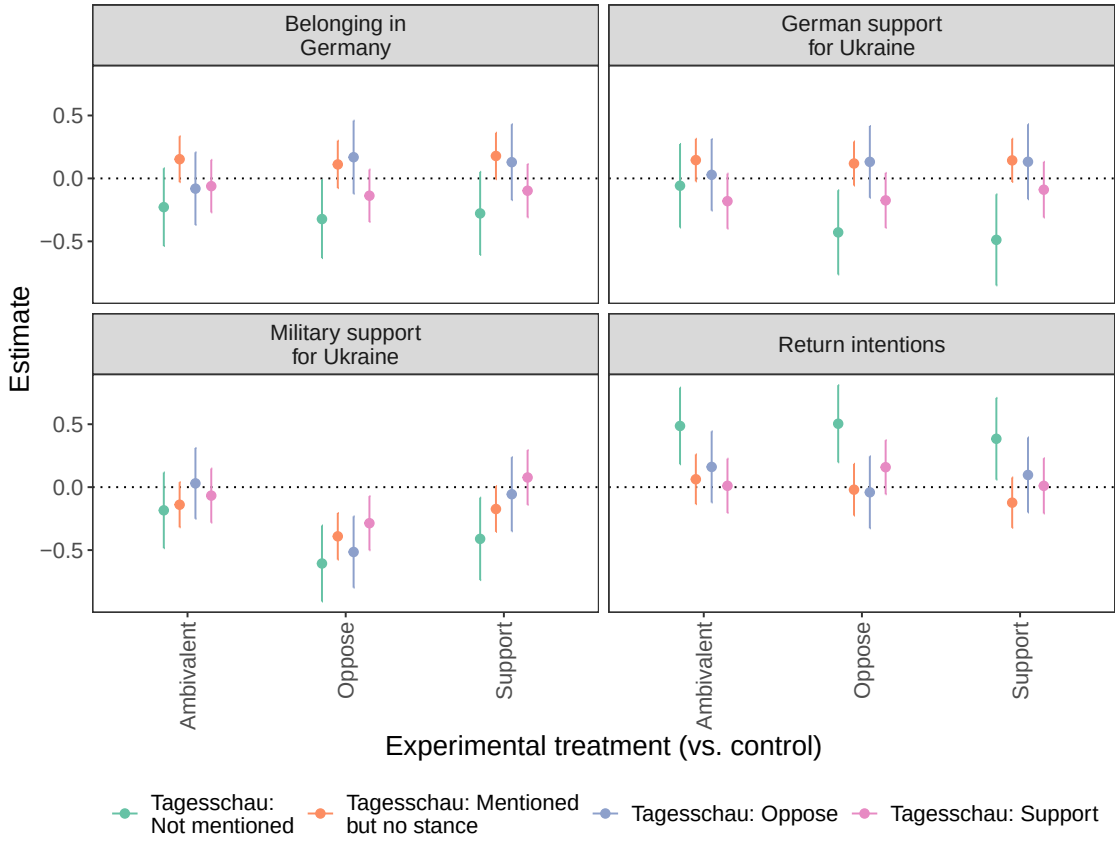


Figure G.8: Treatment effects by Tagesschau coverage type (Support, Oppose, Mentioned without stance, Not mentioned). Outcomes exclude the manipulation check. Effects are sizeable only on no-coverage days and remain small across coverage stances.

## G.13 Tagesschau Interaction Results (Pooled)

Table G.3: Treatment effects interacted with Tagesschau: No mention

| Dependent Variables: | German support (1) | Belonging (2) | Return intentions (3) | Military support (4) | German support (5) | Belonging (6) | Return intentions (7) | Military support (8) |
|---|---|---|---|---|---|---|---|---|
| *Variables* | | | | | | | | |
| Oppose | 0.061 | 0.073 | 0.028 | -0.362*** | 0.032 | 0.048 | 0.012 | -0.386*** |
| | (0.063) | (0.062) | (0.064) | (0.062) | (0.062) | (0.062) | (0.065) | (0.062) |
| Ambivalent | 0.023 | 0.045 | 0.062 | -0.099 | 0.021 | 0.033 | 0.065 | -0.070 |
| | (0.063) | (0.062) | (0.064) | (0.061) | (0.062) | (0.061) | (0.064) | (0.061) |
| Support | 0.066 | 0.050 | -0.050 | -0.081 | 0.063 | 0.074 | -0.068 | -0.058 |
| | (0.064) | (0.063) | (0.065) | (0.062) | (0.063) | (0.063) | (0.066) | (0.062) |
| Oppose × No mention | -0.318* | -0.337** | 0.362** | -0.250 | -0.362** | -0.339** | 0.449*** | -0.204 |
| | (0.169) | (0.164) | (0.171) | (0.165) | (0.167) | (0.165) | (0.173) | (0.165) |
| Ambivalent × No mention | -0.085 | -0.238 | 0.343** | -0.043 | -0.099 | -0.238 | 0.366** | -0.087 |
| | (0.169) | (0.164) | (0.170) | (0.164) | (0.167) | (0.165) | (0.173) | (0.165) |
| Support × No mention | -0.374** | -0.239 | 0.345* | -0.251 | -0.420** | -0.282 | 0.369** | -0.316* |
| | (0.175) | (0.171) | (0.176) | (0.171) | (0.176) | (0.173) | (0.181) | (0.175) |
| Controls included | No | No | No | No | Yes | Yes | Yes | Yes |
| Observations | 2,256 | 2,344 | 2,312 | 2,285 | 2,170 | 2,222 | 2,188 | 2,188 |
| $R^2$ | 0.004 | 0.002 | 0.005 | 0.025 | 0.107 | 0.071 | 0.056 | 0.083 |

*IID standard-errors in parentheses*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table H.4: Main effects: FDR (BH) q-values (3 indices × 3 treatments)

| Outcome | Treatment | Estimate | p | BH q-value |
|---|---|---|---|---|
| Belonging | Ambivalent | 0.001 | 0.988 | 0.988 |
| Belonging | Oppose | 0.003 | 0.961 | 0.988 |
| Belonging | Support | 0.041 | 0.482 | 0.988 |
| German support | Ambivalent | 0.011 | 0.848 | 0.988 |
| German support | Oppose | -0.016 | 0.779 | 0.988 |
| German support | Support | 0.019 | 0.747 | 0.988 |
| Return intentions | Ambivalent | 0.104 | 0.079 | 0.710 |
| Return intentions | Oppose | 0.064 | 0.289 | 0.988 |
| Return intentions | Support | -0.032 | 0.603 | 0.988 |

# H Multiple-Comparisons Adjustments

To guard against false positives from testing several outcomes and treatments, we pre-specified two families and controlled the false discovery rate (FDR) at 10% using the Benjamini-Hochberg procedure. We report BH $q$-values in Tables H.4 and H.5. A q-value is the Benjamini-Hochberg-adjusted p-value and can be read as the minimum FDR level at which the test would be called significant; at our preregistered 10% threshold, results with $q \leq 0.10$ are treated as discoveries. Unlike Bonferroni, this controls the expected share of false positives among the rejections rather than the probability of any false positive in the family.

The first family (main effects) contains the three primary indices (German support, Belonging, Return intentions) crossed with the three treatment contrasts (Oppose, Ambivalent, Support vs. Control), for nine tests. The second family (Tagesschau moderation) contains the same indices crossed with the three treatment-by-No mention interactions, again nine tests.

After FDR correction, none of the main-effect coefficients in Family 1 are significant (Table H.4). This supports the conclusion that there are no detectable average treatment effects across the full sample.

In contrast, several interaction terms in Family 2 remain significant after the same FDR control (Table H.5). When Ukraine was not mentioned in the Tagesschau on the previous day, return intentions increase for all three treatments; perceived German support decreases for the Oppose and Support treatments; and belonging decreases for the Oppose treatment. This aligns with the Figures and reinforces our interpretation that effects emerge in low-salience news environments and are largely independent of message valence. We conclude that multiple testing is not driving our results.

# I Evidence from the SOEP Survey

**Data and sample.** We use the IAB-BiB/FReDA-BAMF-SOEP Survey of Refugees (which we refer to as SOEP)[19] and restrict to the 2023 wave, which includes a dedicated sample of Ukrainian refugees residing in Germany. We merge SOEP interview dates to daily *Tagesschau* transcripts, in the same way as described in Section (Appendix 5.1). We again code whether Ukraine was mentioned on day $t-1$ ("Any mention"), using the same procedure described in Appendix G.

**Outcomes.** We focus on migration intention outcomes because SOEP provides the closest analogues to our survey for these measures. These outcomes are whether a respondent (i) plans to return to Ukraine (binary), (ii) intends to leave Germany (binary: return or move to third country vs. live in both), and (iii) reports higher stay intentions (1–4; higher means longer stay). Other attitudinal constructs (e.g., belonging) have less direct counterparts.

[19] See project description: https://iab.de/teilnehmerinfo/gefluechtete-aus-der-ukraine-in-deutschland-iab-bib-freda-bamf-soep-befragung/.

Table H.5: Tagesschau interaction (No mention): FDR (BH) q-values (with controls)

| Outcome | Term | Estimate | p | BH q-value |
|---|---|---|---|---|
| Belonging | Ambivalent × No mention | -0.238 | 0.149 | 0.199 |
| Belonging | Oppose × No mention | -0.339 | 0.041 | 0.082 |
| Belonging | Support × No mention | -0.282 | 0.104 | 0.156 |
| German support | Ambivalent × No mention | -0.099 | 0.555 | 0.598 |
| German support | Oppose × No mention | -0.362 | 0.031 | 0.082 |
| German support | Support × No mention | -0.420 | 0.017 | 0.082 |
| Return intentions | Ambivalent × No mention | 0.366 | 0.034 | 0.082 |
| Return intentions | Oppose × No mention | 0.449 | 0.009 | 0.082 |
| Return intentions | Support × No mention | 0.369 | 0.041 | 0.082 |

**Specification.** For each outcome, we estimate OLS models of the form $Y_i = \beta \cdot \text{Mention}_{t-1} + X_i\gamma + \alpha_{s(i)} + \alpha_{m(t)} + \varepsilon_i$, where $\alpha_{s(i)}$ are state fixed effects and $\alpha_{m(t)}$ are month fixed effects based on the interview date. Covariates $X_i$ include age, gender, marital status, employment status, and education. Outcomes are standardized using the no-mention group's mean and standard deviation.

**Results.** Figure I.1 shows estimates from the specification described above. A prior-day Tagesschau mention of Ukraine is associated with higher intentions to leave (positive effects on return plans and leave intentions) and a lower intention to stay longer in Germany. These observational results align with our main-study mechanism: media attention serves as a contestation signal increasing the salience of return/relocations.

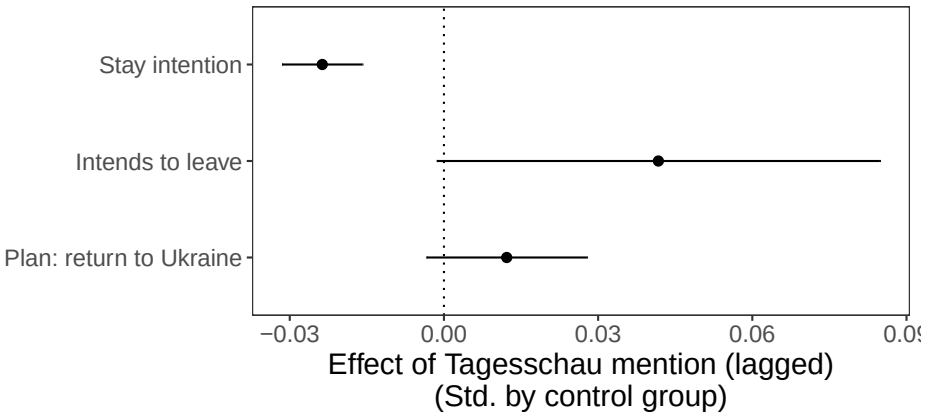


Figure I.1: SOEP 2023: Relationship between lagged Tagesschau mentions (t-1) on migration intention outcomes. Outcomes standardized by the no-mention group.

# J Study Materials

## J.1 Treatment Video Transcripts

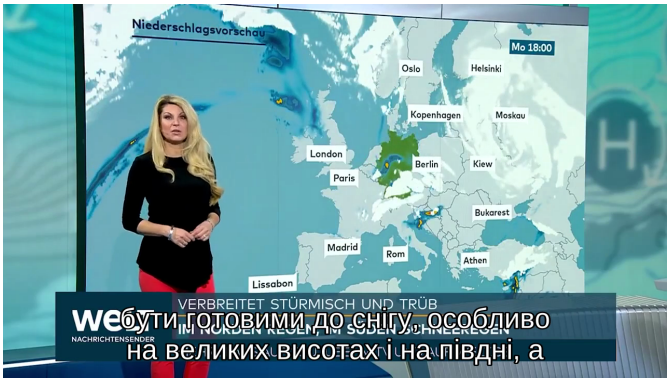


Figure J.1: Control (Placebo) condition video clip.
**[Weather presenter]:** *After the calm night, a new low is causing new precipitation and a freshening wind starting this morning, especially now in rush hour traffic, you should first be prepared for snow, especially at higher altitudes and also in the south, and therefore also for slippery roads in the south. This also applies to the lower altitudes, so here too you have to expect wintry road conditions. Only in the extreme east and southeast it is nicer in the morning and here the sun even comes out sometimes. In the western low mountain ranges the wind also picks up quite a bit. In the afternoon this blows along the western low mountain ranges up to the Alps and then continues strongly on the Alpine peaks, in some cases severe squalls are possible here.*


Figure J.2: Oppose condition video clip.
**[Lars Klingbeil (SPD)]:** *I am fundamentally convinced that it will be of no use if we supply weapons and, especially in the current situation, it would lead to us opening a door that we may no longer be able to close and I believe that supplying arms would send a completely wrong signal that would not lead to that that we come to a diplomatic easing of the situation. We support Ukraine economically, we support you diplomatically, reviving the Normandy format is a very important step, so Germany is doing a lot, but arms deliveries are the wrong way. I have just made clear my fundamental skepticism about arms deliveries to crisis areas. It's now about organizing peace, it's about conversations, it's about diplomacy, it's also about sitting down at the table. I don't believe in supplying weapons to crisis areas, no matter which country they come from.*


Figure J.3: Ambivalent (Weak Opppose) condition video clip.
**[Interviewer]:** *So you're saying that in order to control the [Taurus] targets, German soldiers should be sent to Ukraine, but you don't want that?*
**[Chancellor Olaf Scholz]:** *German soldiers should not be associated with the targets that this system [Taurus] achieves, anywhere.*
**[Interviewer]:** *So that's off the table for Germany? And sending Taurus missiles to Ukraine is out of the question?*
**[Chancellor Olaf Scholz]:** *I said very clearly that there are reasons why our government is providing the most support to Ukraine in Europe. But right now, sending Taurus missiles to support Ukraine is not on the agenda for us, and it's also clear.*


Figure J.4: Support condition video clip.
**[Chancellor Olaf Scholz]:** *Ladies and gentlemen, no matter how important our German contribution is, it alone will not be enough to ensure Ukraine's long-term security. Therefore, I call on our allies in the European Union to strengthen their efforts in support of Ukraine—efforts currently undertaken by most EU member states. The planned arms deliveries to Ukraine by the member states are too small. We need to accurately overview the exact contributions of our European partners to supporting Ukraine this year, at the latest before the meeting of the European Council on February 1.*

## J.2 Measurement of Outcomes and Moderators

This Section details the construction of outcomes and moderators we use in the main analyses in the paper.

We elicit a total of nine main outcome items. To increase precision and mitigate issues arising due to family-wise error rates (FWER), we combine these into three core outcome indices by first recoding them to go in the same direction, then standartizing them using control group means and standard deviations, and then summing them up into index. The three main outcome indices are:

1. **Return intentions:** This is the index of the three sub-items asked in question 22 as shown in the Appendix **??**. Each sub-item is included in such a way that the resulting index will measure the likelihood of returning to Ukraine (i.e., higher values will translate to a higher likelihood of intending to return).
2. **Belonging in Germany:** This index measures the perceived sense of belonging in Germany based on questions 23-25 as shown in the Appendix **??**. Higher values will measure a greater sense of belonging.
3. **German support:** This index measures the degree to which respondents perceive the German government and the German population to be supportive of Ukraine. It is the average of the first and third sub-items of question 26 as shown in the Appendix **??**. Higher values indicate greater perceivedGerman support for Ukraine.

In addition to the main outcome indices, we use responses to the second sub-item of question 26 (see Appendix **??**) as a manipulation check. This item directly measures respondents' perceptions of German military aid to Ukraine, which is the primary focus of our treatment videos. This also constitutes a departure from our pre-analysis plan as we initially planned to include answers to this question into the German support index described above.

Finally, we rely on a number of pre-treatment question in our survey to measure moderators we consider in Section 4.1. We use questions 6 and 15 to measure prior consumption of mainstream German media and preferences for territorial loss respectively. To measure prior integration we construct an index of all sub-items in questions 16 and 17 and second sub-item of question 18.