

Private Preferences, Public Expression, and Campaign Persuasion: Experimental Evidence from the 2024 U.S. Presidential Election

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Abstract

Research on electoral and campaign persuasion typically examines how appeals change what voters think, but appeals can also change what voters are willing to express to others. Because public expression depends on social incentives as well as personal beliefs, campaign appeals can affect private preferences and public expression differently, creating, widening, or narrowing gaps between them. Existing research rarely examines these outcomes together however. We address this by studying a campaign ad targeting women during the 2024 U.S. presidential election using experiments embedded in a pre-election survey of 2,056 primarily Republican and Independent women. By cross-randomizing ad exposure and whether respondents express their candidate preferences privately in an online survey or publicly to a co-partisan virtual enumerator, we identify the ad’s effects in each domain. The ad reduced private Trump support significantly more than support expressed to a female co-partisan. We examine whether this asymmetry reflects social costs among Republican women, as anticipated by the “shy voter” account. Instead, the pattern is most evident among Independent women and is consistent with social benefits: these women initially expressed less Trump support publicly than privately, and the ad brought their private preferences into closer alignment with their public expression. These findings show how the same private effect without a corresponding public effect can have different interpretations depending on the social environment. Studying both private preferences and public expression provides a fuller account of what persuasion changes and of its potential—and limits—as a source of electoral change.

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1 Introduction

A vast literature on electoral and campaign persuasion examines how appeals change voters’ preferences, attitudes, candidate evaluations, and vote intentions (Huber and Arceneaux, 2007; Gerber et al., 2011; Hewitt et al., 2024; Coppock, Hill and Vavreck, 2020; Kalla and Broockman, 2018; Broockman and Kalla, 2023). These outcomes are understood to reflect voters’ underlying political views, which can shape vote choice and other political behaviors (Druckman, 2022). From this perspective, a campaign message succeeds to the extent that it shifts those underlying views in favor of a candidate.

Yet campaign persuasion can also influence what individuals are willing to express publicly. Public expression—the outward display of attitudes or preferences—can amplify the effects of a campaign message through political discussion, interpersonal persuasion, social media sharing, and other visible forms of political action (Druckman, Levendusky and McLain, 2018; Carlson, 2019). Campaign ads may encourage such expression by conveying social information: an ad that depicts people expressing support for or opposition to a candidate signals both what others are doing and what they regard as acceptable or desirable (Cialdini, Reno and Kallgren, 1990). By shifting perceived norms, these signals can reduce the social costs or increase the social benefits of expressing a view (Bursztyn, Egorov and Fiorin, 2020; Carlson and Settle, 2016; Suhay, 2015). As more people express that view, they in turn signal its acceptability to others, creating the potential for broader political momentum (Noelle-Neumann, 1974; Mutz, 1992; Bond et al., 2012; Valentim, 2024).

Despite the importance of private preferences and public expression as distinct outcomes of campaign persuasion, existing research rarely examines them together. This matters because these outcomes need not move together. Changing private preferences involves changing personal beliefs—how someone thinks or feels about a candidate—while public expression additionally depends on the social incentives associated with communicating those beliefs to others (Kuran, 1995; Blair, Coppock and Moor, 2020; Levitan and Verhulst, 2016). Campaign ads may change personal beliefs and social incentives to different degrees, producing effects on both private preferences and public expression, neither, or larger effects on one than the other. Investigating these patterns is critical to understanding the effects of persuasive appeals.

These public–private effect patterns also matter for understanding campaign persuasion as a

source of electoral change. Consider an ad that moves an individual’s private preferences toward a candidate but has no effect on public expression. This pattern can have different interpretations depending on the social environment. When individuals face social costs—disapproval or sanctions for publicly supporting the candidate—it could reflect a private change that does not carry over into public expression, indicating limited change. In contrast, when public support is socially rewarded, individuals may already express support they do not privately hold (Blinder, Ford and Ivarsflaten, 2013; Willer, Kuwabara and Macy, 2009; Pickup, Kimbrough and de Rooij, 2022). The ad could then narrow this pre-existing gap, bringing private preferences, and potentially private behavior, into closer alignment with what individuals already express publicly. Thus, interpreting the private and public effects of campaign persuasion requires understanding the social incentives individuals face and how an appeal reshapes the relationship between private preferences and public expression.

We examine these questions in the context of campaign persuasion targeting Republican and Independent women during the 2024 U.S. presidential election, a setting in which commentators and campaign strategists openly speculated about divergences between women’s private political preferences and public expression (Washington Post, 2024; CNN, 2024). Specifically, we study a campaign ad produced and funded by Vote Common Good and released in the final days of the campaign, encouraging these women to defect from Trump and support Harris. It depicted a woman who arrives at a polling place with her Trump-supporting husband, votes for Harris while concealing her choice from him, and exchanges a knowing glance with another woman.¹ By emphasizing the stakes of the election and encouraging women to make their own choice, the ad may have shifted private preferences away from Trump and toward Harris. By portraying women signaling shared opposition to Trump to one another, it may also have changed the perceived social incentives for expressing that opposition. The ad thus provides an opportunity for examining how a campaign appeal affects private preferences and public expression, especially to other co-partisan women.

The electoral context also highlights the importance of examining whether these effects widen or narrow the gap between private preferences and public expression. The ad aired amid widespread speculation about “shy” women voters: Republican women who privately opposed Trump but were reluctant to say so publicly due to perceived social costs (NPR, 2024). The ad could have

¹The ad can be viewed here: <https://www.youtube.com/watch?v=FaCPck2qDhk>.

increased the pool of such voters by shifting private preferences while social costs continued to deter public expression, even if the ad reduced those costs. This reasoning motivated our preregistered hypotheses: that the ad would reduce private support for Trump and increase private support for Harris, with larger effects on private preferences than on public expression.

We test these hypotheses in an online survey of 2,056 primarily Republican and Independent women conducted immediately before the election. Respondents were randomly assigned to watch the ad or to a pure control condition. To distinguish effects on private preferences and public expression, we cross-randomized respondents to a *private* condition, in which they expressed their views in a standard online survey, or a *public* condition, in which they expressed their views to a virtual female or male copartisan enumerator. This design builds on evidence that anonymous online surveys better capture private preferences and that socially relevant audiences shape expressed responses (Tourangeau and Yan, 2007; Huddy et al., 1997; Krysan and Couper, 2003; Adida et al., 2016; Blair, Coppock and Moor, 2020; Bursztyn et al., 2025). It captures a key feature of public expression: communicating attitudes to a potentially socially relevant other. Including male enumerators allows us to examine whether the ad’s effects on public expression depend on the audience’s gender, as we would expect if its portrayal of shared opposition among women changes the social incentives for expressing opposition to Trump to other women. Respondents expressed their views in their assigned condition both before and after ad exposure, allowing us to examine whether they initially expressed different levels of candidate support privately and publicly, whether the ad affected each domain differently, and whether those effects widened or narrowed the gap between private preferences and public expression.

Consistent with the direction of our preregistered predictions, the ad reduced private support for Trump by 0.10 standard deviations, and this effect was significantly larger than its effect on expression to the female enumerator, with a public–private difference of 0.17 standard deviations. The effects on Harris support point in the predicted direction but are smaller and not statistically significant; the ad also shifted private support toward third-party candidates. Although the pattern emerges primarily for one candidate, the results show that a campaign ad can change private preferences more than public expression.

We then examine whether this pattern reflects the social costs anticipated by the “shy voter” account, which should be most pronounced among Republican women. We find no detectable ad

effects on their privately held or publicly expressed Trump support. Instead, effects were most clearly evident among Independent women, where the results point to social benefits rather than social costs. Without the ad, Independent women already expressed more opposition to Trump to a female copartisan than they held privately, and the ad reduced their private support without a detectable additional effect on public expression. The ad’s primary effect was thus to bring private preferences into closer alignment with already elevated public opposition to Trump. Differences between private and public expression before ad exposure, together with the ad’s effects on Independent women’s perceptions of Harris support among other Independent women, provide further evidence consistent with this social benefits interpretation.

This paper contributes to research on both campaign persuasion and social influences on political expression. First, it shows why studying private preferences and public expression together matters for understanding how campaign appeals work. Research on campaign persuasion examines how appeals change voters’ preferences and how their effects spread through social networks ([Druckman, Levendusky and McLain, 2018](#); [Druckman, 2022](#)), while research on political expression examines how social incentives shape what people express ([Carlson and Settle, 2016](#); [Levitan and Verhulst, 2016](#)). Bringing these literatures together raises a question rarely examined by either: whether an appeal can change what voters privately prefer, what they express to others, or both. The answer matters because the two outcomes play distinct roles in elections. Private preferences can inform vote choice and other political behaviors perceived as private, whereas public expression shapes what others learn about prevailing views and can extend a message’s influence beyond those who encounter it directly.² Studying either outcome alone can therefore give an incomplete account of what campaign persuasion changes.

Second, the paper connects campaign persuasion to the micro-foundations of social change by examining how appeals reshape the relationship between private preferences and public expression. Large literatures examine how social pressure produces gaps between the two through concealment or public conformity—forms of preference falsification in which people hide or misrepresent their private preferences in response to perceived social pressures ([Kuran, 1995](#); [Hayes, Scheufele and](#)

²Voting may not always be perceived as private ([Gerber et al., 2013](#)), in which case social incentives may also shape how private preferences translate into vote choice.

Huge, 2006; Carlson and Settle, 2016; Levitan and Verhulst, 2016; Willer, Kuwabara and Macy, 2009; Blinder, Ford and Ivarsflaten, 2013; Pickup, Kimbrough and de Rooij, 2022; Valentim, 2024). These literatures also identify how such gaps change. Bringing concealed views into public can reveal that support is more widespread than believed, prompting further expression and change (Kuran, 1995; Prentice and Miller, 1993; Bursztyn, González and Yanagizawa-Drott, 2020). Alternatively, social change can begin with shifts in public expression: changes in perceived norms may lead people to express views they do not yet hold privately, a form of norm compliance (Paluck, 2009). As people internalize these norms, private preferences may move toward public expression, narrowing the gap (Tankard and Paluck, 2016) and making public expression less dependent on social rewards and more durable (Kelman, 1958, 2006).

Less attention has been paid to how campaign persuasion itself creates, deepens, or reduces these gaps. Importantly, the same pattern of effects can widen or narrow these gaps. A larger effect on private preferences than on public expression could deepen preference falsification where social costs prevail, yet in our setting it narrowed an existing gap among Independent women, consistent with a shift from norm compliance toward internalization. Campaign persuasion can therefore affect not only what voters privately prefer and publicly express, but whether the two move closer together or further apart.

We do not expect this specific pattern to recur in every setting. Which pattern of effects a campaign appeal produces depends on whether it changes personal beliefs, social incentives, or both, as well as on the social environment an individual faces. Studying private and public outcomes together, alongside the social incentives that discourage or encourage public expression, can clarify both the potential and the limits of campaign persuasion as a source of electoral change.

2 The Private and Public Effects of Campaign Persuasion

This section develops our argument about how campaign appeals affect private preferences and public expression, and why the implications depend on the social environment. We explain how different patterns of effects arise and when they widen or narrow the gap between private preferences and public expression, then turn to the considerations that motivated our predictions for the ad studied here.

We distinguish the preferences people hold privately from the support they express publicly. An

individual may privately support a candidate yet be unwilling to express that support to others, or may express support publicly without privately preferring that candidate. Here we consider public expression to ingroup members, an audience whose approval people value and who are especially likely to sanction deviation from group norms (Hogg and Reid, 2006; Connors, 2020). The ingroup can be defined by gender (Huddy et al., 1997; Klar, 2013), partisanship (Suhay, 2015), or another shared identity.

A campaign appeal may change whether an individual privately supports a candidate and whether they express that support publicly.³ A person’s private support for a candidate depends on the personal value of supporting that candidate weighed against the nonsocial costs, such as the psychological discomfort of departing from one’s usual partisan choices even when that choice is not observed by others. Public support depends on these same considerations *and* on social incentives: whether expressing that support to others brings social costs or social benefits. When social norms convey disapproval, publicly expressed support may lag behind privately held support. When they convey approval, publicly expressed support may exceed privately held support, so that people may express support they do not privately hold. A campaign appeal can therefore have different effects on private preferences and public expression because the two outcomes depend on overlapping but distinct considerations.

Campaign appeals can affect private preferences and public expression through two pathways. First, appeals can change *personal beliefs*, altering the personal value of supporting a candidate (Druckman, 2022). They can do so by providing information that changes candidate evaluations (Lodge, Steenbergen and Brau, 1995), increasing the salience of particular issues (Chong and Druckman, 2007), eliciting emotions (Brader, 2006), or activating identities (Klar, 2013; Holman,

³For simplicity, we consider binary decisions: whether to support a candidate privately and whether to express support publicly. For empirical purposes, the binary logic can be extended to ordered and continuous outcomes by treating each successively higher level of support as a separate decision about whether to support the candidate at least that much privately or express at least that much support publicly. Changes in the level of support then reflect changes in these decisions. At the population level, effects aggregate changes across individuals, so even binary individual decisions can yield continuously varying average effects.

Schneider and Pondel, 2015). Because personal value shapes both private preferences and public expression, such changes can affect either or both outcomes.

Second, appeals can change *perceived social norms*. Campaign appeals often convey social signals by depicting ingroup members supporting or opposing a candidate.⁴ By shifting perceived descriptive or injunctive norms, these appeals can lower the social costs or increase the social benefits of public expression (Cialdini, Reno and Kallgren, 1990; Bursztyn, Egorov and Fiorin, 2020). They can therefore change public expression without changing the personal value of supporting a candidate and thus need not change private preferences.⁵ An appeal that lowers perceived social costs without eliminating them makes public expression less costly, but expressing support publicly still requires enough personal value to outweigh both nonsocial and remaining social costs. When public expression instead brings net social benefits, those benefits can lead people to express support they would not hold privately.

The argument thus far builds on established accounts of why private preferences and public expression differ and how campaign appeals can change personal beliefs and perceived norms. Our contribution is to show how changes in personal beliefs and social incentives produce different public-private effect patterns and how these patterns reshape the relationship between private preferences and public expression. We assume that any changes in personal beliefs or social incentives operate in the direction encouraged by the message, although either pathway may remain unchanged.⁶ This logic implies four possible patterns: similar effects on both outcomes, no effect

⁴For example, the Trump campaign released a video in 2024 featuring more than a dozen Black women declaring “I’m not with her” (NBC News, 2024). By depicting Black women publicly opposing Harris, the video could challenge the perception that members of this group were expected to support the Democratic candidate.

⁵We do not assume that changes in perceived norms are internalized. Norm shifts may change the personal value people place on supporting a candidate, but they may also produce compliance, in which people publicly conform to social norms they do not personally support (Blinder, Ford and Iversflaten, 2013; Willer, Kuwabara and Macy, 2009; Pickup, Kimbrough and de Rooij, 2022).

⁶In our empirical context, this would be increased support for Harris and increased opposition to Trump.

on either, a larger effect on private preferences than on public expression, or a larger effect on public expression than on private preferences. We refer to the first two as *symmetric effects* and the latter two as *asymmetric effects*.

An appeal encouraging support for a candidate can have symmetric effects when individuals initially support the candidate neither privately nor publicly and the message changes their personal beliefs enough—alone or alongside a change in perceived norms—to lead them to support the candidate in both domains. Such effects indicate that the appeal can advance electoral change through both private preferences and public expression, without being confined to either domain. Alternatively, an appeal may have symmetric null effects, leaving both outcomes unchanged. This could occur when individuals already support the candidate privately and publicly, or when the appeal is insufficiently strong to change support in either domain.

At the individual level, asymmetric effects arise when an appeal changes support in one domain but not the other.⁷ The asymmetric pattern of central interest here is a larger effect on private preferences than on public expression. This pattern has different implications depending on the social environment. Consider first individuals who face social costs for expressing support and support a candidate neither privately nor publicly. An appeal that increases the personal value of supporting the candidate may lead them to support the candidate privately without expressing that support publicly—even if it also reduces social costs—provided the remaining costs are sufficient to discourage public support. In this way, campaign persuasion can create new gaps between private preferences and public expression and, in the aggregate, deepen preference falsification (Kuran, 1995).

Under social benefits, by contrast, publicly expressed support may initially exceed private support: people express support for the candidate because doing so is rewarded, even though they do not privately support the candidate. A message that raises the personal value of supporting the candidate can shift private preferences toward the position people already express publicly, while public expression remains unchanged. Persuasion then narrows the gap, a pattern consistent with movement from norm compliance towards norm internalization (Kelman, 1958, 2006; Wood, 2000;

⁷Because each support decision is binary, a message cannot increase support in a domain where an individual already supports the candidate.

Willer, Kuwabara and Macy, 2009; Blinder, Ford and Ivarsflaten, 2013; White and Laird, 2020). A central takeaway is that the same pattern of effects—larger effects on private preferences than on public expression—can widen a public–private gap under social costs or narrow one under social benefits.

An asymmetry in the other direction is also possible, with a message having a larger effect on public expression than on private preferences. Under social costs, private support may initially exceed public expression, consistent with pre-existing preference falsification (Kuran, 1995). By increasing the personal value of supporting the candidate or reducing social costs, an appeal can lead individuals to express support they already hold privately, changing public expression while leaving private support unchanged. Under social benefits, individuals who initially support the candidate neither privately nor publicly may come to express support only publicly: an appeal may make support worthwhile when socially rewarded but not in private, either by raising its personal value or by increasing those rewards. Larger effects on public than on private support can thus indicate a narrowing gap when social costs prevail or a widening gap when social benefits prevail.

Overall, the framework yields two implications. First, campaign appeals can affect private preferences and public expression symmetrically or asymmetrically, depending on the intervention pathway, the social environment, and the initial public–private relationship. Second, the social environment and the initial public–private relationship help explain whether a given asymmetry brings private preferences and public expression closer together or moves them further apart. Our preregistered hypotheses focused on larger private effects under social costs, potentially widening the gap. Although we did not anticipate the social benefits interpretation, the framework also explains how larger private effects could narrow a gap where public expression already exceeds private support. The next section describes the ad and electoral context that motivated our predictions.

3 The Campaign Ad

We examine the effects of campaign persuasion on private preferences and public expression using a real-world campaign ad from the 2024 U.S. presidential election in a preregistered study (see Appendix K). The ad, narrated by Julia Roberts, aired during the final week of the campaign amid widespread speculation that some Republican and Independent women disillusioned with Trump might quietly defect to Harris (ABC News, 2024). Reactions to the ad across the political spectrum

suggests it was perceived as especially resonant, even in an already saturated media environment ([Newsweek, 2024](#); [Washington Post, 2024](#)).

The ad contained messages that could plausibly shift personal beliefs about the candidates. It emphasized the personal stakes of the election and encouraged women to make their own choice, potentially reducing psychological barriers to privately defecting from Trump among women conflicted about their vote choice. The persuasive potential of these appeals among Republican and Independent women was nevertheless uncertain. Gender-targeted appeals can activate gender identity and increase support among women voters ([Holman, Schneider and Pondel, 2015](#)), but partisan identity often outweighs gender identity in shaping political behavior ([Huddy, Cassese and Lizotte, 2008](#)). Such appeals can also activate countervailing partisan identities ([Klar, 2018](#)), and women in the United States rarely cross party lines on the basis of shared gender identity alone ([Junn and Masuoka, 2024](#)). Whether this ad could overcome these barriers only days before the election was therefore an open empirical question.

The ad also conveyed a social signal that could affect public expression. Although it reassured women that dissenting vote choices could remain hidden from their husbands, it depicted women signaling their opposition to Trump to one another. This suggested that opposition among other women was neither isolated nor socially unacceptable. The ad could therefore have reduced the perceived costs or increased the perceived benefits of expressing opposition to Trump to other women, potentially changing public expression independently of any change in private preferences. The “shy voter” account nevertheless led us to anticipate that Republican women in particular might face social costs substantial enough to constrain public expression even if the ad reduced them, motivating our prediction of larger effects in private than in public.

3.1 The Experiment

We test the ad’s effects by embedding it in an online survey experiment. Respondents assigned to treatment watched the 30-second ad and answered several reaction questions before proceeding to the post-treatment survey. Respondents assigned to the no-ad control condition proceeded directly

to the post-treatment survey without viewing the ad or answering the reaction questions.⁸ Among respondents assigned to treatment, 88 percent reported that they had not previously seen the ad, and 72 percent correctly answered a post-treatment attention check (Appendix C.2).

Using an actual campaign ad trades experimental control over specific features—such as its message, narrator, visuals, and tone—for ecological validity, an approach common in research on political advertising (Clinton and Lapinski, 2004; Coppock, Hill and Vavreck, 2020; Gerber et al., 2011; Huber and Arceneaux, 2007; Hewitt et al., 2024). Our goal is not to isolate which component of the ad produced persuasion, but to investigate whether the ad had differential effects on private versus public expression.

4 Research Design

As detailed in Appendix A, we fielded the online survey with the campaign ad experiment during the final days of the 2024 U.S. presidential election. Eligible participants were voting-age U.S. citizens who identified as women and had not yet voted. Recruitment initially focused on Republican women and was subsequently expanded to Independent and weak or leaning Democratic women.⁹ The resulting sample included 2,056 respondents: 37 percent identified as strong Republicans, 31 percent as weak or leaning Republicans, 27 percent as Independents, and 5 percent as weak or leaning Democrats.

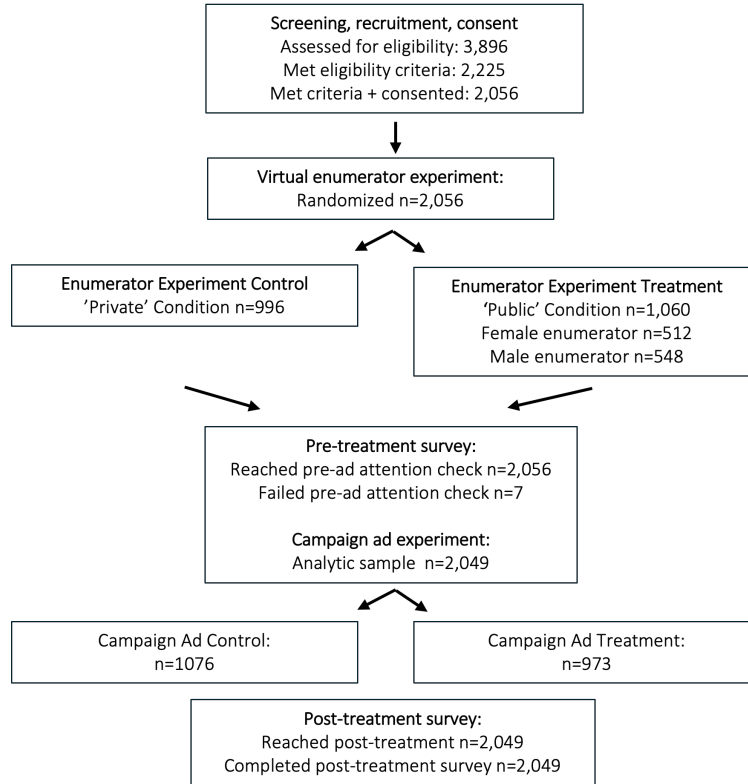
Figure 1 summarizes the overall research design. Following recruitment and consent, participants were randomly assigned to the private or public condition of the virtual enumerator experiment, described below. They then completed a brief pre-ad survey, followed by the campaign ad

⁸The ad had already been publicly released and widely discussed in the media at the time of the study (Newsweek, 2024; Washington Post, 2024). Given the study’s relatively small sample and nationwide recruitment strategy, participation was unlikely to have influenced electoral outcomes (Slough, 2024).

⁹The study was approved by the authors’ institutional review board (Appendix J) and preregistered. The pre-analysis plan was updated to reflect the change to the sample. The anonymized preregistration is available [here](#); Appendix K documents correspondence between the manuscript and the preregistration plan.

experiment. In the post-ad survey, respondents in the public condition again answered questions posed by a virtual enumerator, while those in the private condition completed the online survey on their own. The survey ended with a measure of private campaign donation behavior. All 2,056 participants who began the study were randomized in the campaign ad experiment. Following the pre-analysis plan, we excluded seven who failed the pre-ad attention check, yielding an analytic sample of 2,049.¹⁰

Figure 1: Research Design Overview



4.1 The Virtual Enumerator Experiment

The virtual enumerator experiment serves two purposes: to examine differences between private preferences and public expression before ad exposure, and to compare the ad’s effects on these outcomes while holding all other features of the attitude measures constant. Before the pre-ad

¹⁰Balance checks show no statistically significant joint imbalance between the ad and control or female-enumerator and private conditions, although some baseline characteristics differed between the male-enumerator and private conditions (Appendix C.1).

survey, respondents were randomly assigned to a *private* condition, in which they answered a standard self-administered online survey, or a *public* condition, in which they answered the same questions posed by a female or male virtual co-partisan enumerator. Varying the enumerator’s gender allows us to examine whether expression and ad effects differ between female and male audiences, as discussed below. Respondents in the public condition were further randomized among six enumerators, three women and three men, so that the manipulation was not confounded with the characteristics of any single individual (see Appendix A.2). The enumerator was introduced through an image and a brief chat-style exchange, and the image remained visible throughout the survey. Across conditions, respondents answered the same direct questions, with identical response options and formats; the virtual enumerator was present only in the public condition, throughout both the pre-ad and post-ad surveys.¹¹

The private condition provides the closest observable approximation of respondents’ explicitly reported private preferences. This interpretation is consistent with evidence that self-administered surveys are perceived as more private and tend to elicit more candid answers to sensitive questions than interviewer-administered modes (Tourangeau and Yan, 2007; Holbrook and Krosnick, 2010; Krumpal, 2013; Bursztyn et al., 2025). It does not require assuming that anonymous self-administration perfectly reveals respondents’ true beliefs. Respondents may anticipate broader audiences or otherwise adapt their answers even when completing surveys privately (Lindsey, 2023; Krupnikov, Piston and Bauer, 2016). The relevant claim is comparative: anonymous self-administration more closely approximates private expression than reporting the same views in the presence of a socially identified audience.

The public condition introduces a socially relevant audience for political expression. A large literature shows that respondents adjust survey answers to perceived audiences and their expectations, and that interviewer characteristics such as gender, race, and ethnicity can affect the views respondents express (Tourangeau and Yan, 2007; Huddy et al., 1997; Adida et al., 2016; Blair, Coppock and Moor, 2020; Bursztyn et al., 2025). Interviewers can serve as salient social referents

¹¹Because the enumerator was present before ad exposure, audience assignment could also have influenced message processing. Interpreting differences in ad effects solely as differences in expression assumes that the audience did not differentially change respondents’ underlying response to the ad.

when respondents infer that they hold expectations about which attitudes are appropriate to express (Blair, Coppock and Moor, 2020). Responses may therefore reflect perceived norms and the anticipated costs or benefits of expressing particular views.¹² Research also finds that respondents react to virtual interviewers in socially meaningful ways (Krysan and Couper, 2003).

We thus interpret the virtual enumerator as introducing a perceived audience for social evaluation into an otherwise anonymous survey. Specifically, the enumerator captures expression to a general ingroup member: an unfamiliar person whose social relevance derives from shared group membership—partisanship and, for female enumerators, gender—rather than from a personal relationship. This approach has two advantages for our purposes. First, because respondents have no history with the enumerator, their responses can reflect perceived norms of the shared group rather than the views or anticipated reactions of particular friends or family members, which vary across respondents and cannot be randomly assigned.¹³ Second, a general ingroup member is the audience the ad’s social signal addresses: the ad depicts women signaling their choices to other women, not to particular members of a viewer’s network. In the conclusion, we discuss the extent to which findings from this design generalize to public expression within respondents’ own social networks.

We identified all virtual enumerators as co-partisans to reduce ambiguity about the audience’s partisan affiliation. Without this information, some respondents might have perceived the enumerator as a co-partisan and others as a non-co-partisan, introducing uncontrolled variation in the social incentives surrounding expression. Second, doing so provided a substantively relevant setting for political expression, given that political discussion often occurs within relatively homogeneous partisan networks (McPherson, Smith-Lovin and Cook, 2001; Mutz, 2006). Shared partisanship did not necessarily define the audience’s relevance, however: respondents could perceive the enumerator as an ingroup member on the basis of partisanship, gender, or both.

¹²The design does not distinguish among anticipated social evaluation, activation of partisan or gender identity, and heightened awareness of group norms. We therefore interpret differences across conditions broadly as audience effects rather than attributing them to a single psychological process.

¹³Manipulating expression to respondents’ actual network members would also have required disclosing their survey responses to friends or family, posing substantial ethical and practical barriers.

Our primary comparison is between the private and female enumerator conditions because the ad’s portrayal of women signaling their choices to one another should most directly affect political expression to another woman. At the same time, the portrayal of women concealing their choices from their husbands could reinforce perceived costs of expressing opposition to Trump to men, further motivating the comparison between female and male audiences. The male enumerator condition allows us to examine whether expression and ad effects vary with audience gender while holding stated partisan affiliation constant. Because both female and male virtual enumerators introduced a socially identified audience and disclosed a shared partisan affiliation, differences between them are less readily attributable to the mere presence of a virtual enumerator or to a general reminder of partisan identity.

Treatment checks indicate that respondents registered the manipulation, although they identified the enumerator’s gender more accurately than partisan affiliation (Appendix C.2). This supports interpreting gender as a salient audience cue, while leaving open the extent to which shared partisanship also shaped responses. We therefore describe the public conditions as expression to female or male enumerators identified as co-partisans, without assuming that respondents attached equal weight to both identities.

4.2 Outcome Measures, Hypotheses, and Estimation

We examine the effect of the ad on private and publicly expressed support for Trump and Harris. Our primary outcomes are the preregistered candidate support indices, which combine candidate favorability and intended vote choice. We also report the individual components to clarify the patterns underlying the index effects. A behavioral measure of willingness to make a private donation provides a secondary outcome, which we discuss where relevant to the interpretation of the main findings. Details on outcome measurement and index construction are provided in Appendix D.

Overall, the design allows us to estimate the ad’s effects on preferences as privately held versus publicly expressed to a virtual enumerator. Following our pre-registered predictions (see Appendix E.1), we assess the following main hypotheses:

- The ad decreases private support for Trump.

- The ad increases private support for Harris.
- The ad decreases Trump support more in private than in public.
- The ad increases Harris support more in private than in public.

The key quantity for the latter two predictions is the public-minus-private difference in ad effects. In our main analysis below, we define public expression as expression to the female virtual enumerator because the ad depicts women signaling their choices to one another. The pre-analysis plan pooled expression to female and male enumerators and separately predicted larger ad effects on expression to female than to male enumerators, but did not explicitly specify the female-public versus private hypotheses (see Appendix K). Our focus on these contrasts follows the substantive reasoning behind those predictions. They are evaluated using coefficients in the prespecified regression summarized below, requiring no change to the model or estimation procedure, but remain distinct from the explicitly preregistered pooled-public comparisons, which we also report.

We intentionally did not preregister a separate prediction for the public effect alone because the predictions for the private effect and the public-private difference already capture the asymmetry of interest. The public effect may be null or move in the intended direction, provided that it is smaller in magnitude than the private effect. Although these predictions reflect our preregistered expectations in the “shy voter” context, the design allows us to assess any of the public-private effect patterns described in the theory section. Appendix E.1 details our preregistered expectations about baseline public-private differences and heterogeneity by respondent party.

As detailed in Appendix E.2, we estimate the campaign ad’s effects on privately held and publicly expressed preferences using the following pre-registered specification:

$$Y_i = \alpha + \beta_1 Ad_i + \beta_2 (Ad_i \times PubFem_i) + \beta_3 PubFem_i \\ + \beta_4 (Ad_i \times PubMale_i) + \beta_5 PubMale_i + X_i' \psi + \mu_{d[i]} + \epsilon_i.$$

where Y_i is the outcome for respondent i ; Ad_i indicates assignment to the campaign ad; and $PubFem_i$ and $PubMale_i$ indicate assignment to the female or male virtual enumerator, respectively.

The private condition is the reference category. β_1 estimates the ad’s effect in the private condition, and β_2 estimates the public-minus-private difference in ad effects for expression to the female enumerator. For Harris support, we predict a positive private effect ($\beta_1 > 0$) and a negative

public-minus-private difference ($\beta_2 < 0$), indicating a smaller increase in support in public than in private. For Trump support, we predict a negative private effect ($\beta_1 < 0$) and a positive public-minus-private difference ($\beta_2 > 0$), indicating a smaller decrease in support in public than in private.¹⁴ We also report the ad’s effect in the female-public condition, $\beta_1 + \beta_2$, for completeness. β_3 estimates the effect of expression to the female enumerator relative to private expression among respondents in the no-ad control condition, which we use to assess social incentives in the absence of the ad. All specifications include the prespecified demographic controls X_i and fixed effects $\mu_{d[i]}$ for the day on which respondent i completed the study, and use heteroskedasticity-robust standard errors.

To examine whether these effects vary by respondent party, we estimate a fully interacted specification that includes the triple interaction of the ad indicator, the enumerator indicators, and party identification. This allows both the private ad effect and the public-minus-private difference to vary between Republican and Independent women. We calculate the party-specific effects as linear combinations of the relevant coefficients, and the triple interaction coefficient tests whether the public-minus-private difference differs between the two groups. We report party specific effects in the main text and respective appendices.

5 Descriptive Statistics

Appendix B presents descriptive statistics using pre-ad data from respondents in the private condition. These suggest potential scope for persuasion even in the final days of the campaign. Overall, 68 percent were very certain of their vote choice, leaving nearly one-third less certain. Certainty varied sharply by party: 81 percent of Republican women were very certain, compared with 38 percent of Independent women.¹⁵ Similarly, 83 percent of Republican women said they were very likely to vote for Trump, compared with about 25 percent of Independent women. Independent

¹⁴Because these hypotheses make directional predictions, the pre-analysis plan specifies one-sided tests. We nonetheless report conventional two-sided p -values throughout, which are more conservative for estimates in the predicted direction. Multiple hypothesis correction tests in Appendix I report corrections using both one-sided and two-sided p -values.

¹⁵We do not discuss weak/leaning Democrats here due to their small sample size.

women thus entered the experiment with less settled vote choices and substantially weaker Trump support.

Republican and Independent women also described different social environments. Republican women’s networks were predominantly Trump-supporting: 77 percent said most of their friends and family were voting for Trump, and among those with a partner, 89 percent said their partner would vote for Trump. Independent women’s networks were more mixed: the corresponding figures were 34 and 41 percent. These differences suggest that the social incentives surrounding public expression could vary by party. Republican women’s predominantly Trump-supporting networks could make expressing opposition socially costly, whereas Independent women’s more mixed networks leave open whether such expression carried costs or benefits.

6 Ad Effects on Private and Public Candidate Support

Figure 2 presents the main public–private campaign ad treatment effects, showing effects in the private condition (top), public (female enumerator) condition (middle), and the public–private difference (bottom). Appendix Table G.2.1 reports the full regression results.

The results show clear evidence of the predicted asymmetry for Trump. The campaign ad reduced private Trump support by .10 SD on the index ($p = .044$). Both components declined, with favorability falling by .16 units ($p = .034$) and intended vote by four percentage points ($p = .102$). However, these effects did not appear in the public (female enumerator) condition: the ad had no detectable effect on publicly expressed Trump support on the index (.07 SD, $p = .318$) or either component. The public–private difference in the index effect was therefore .17 SD ($p = .049$), consistent with the expectation that the ad would cause a bigger reduction in private relative to public support.¹⁶ The explicitly preregistered private–public contrasts pooling female and male enumerators show a similar pattern, with slightly smaller differences in ad effects (Appendix G.1).

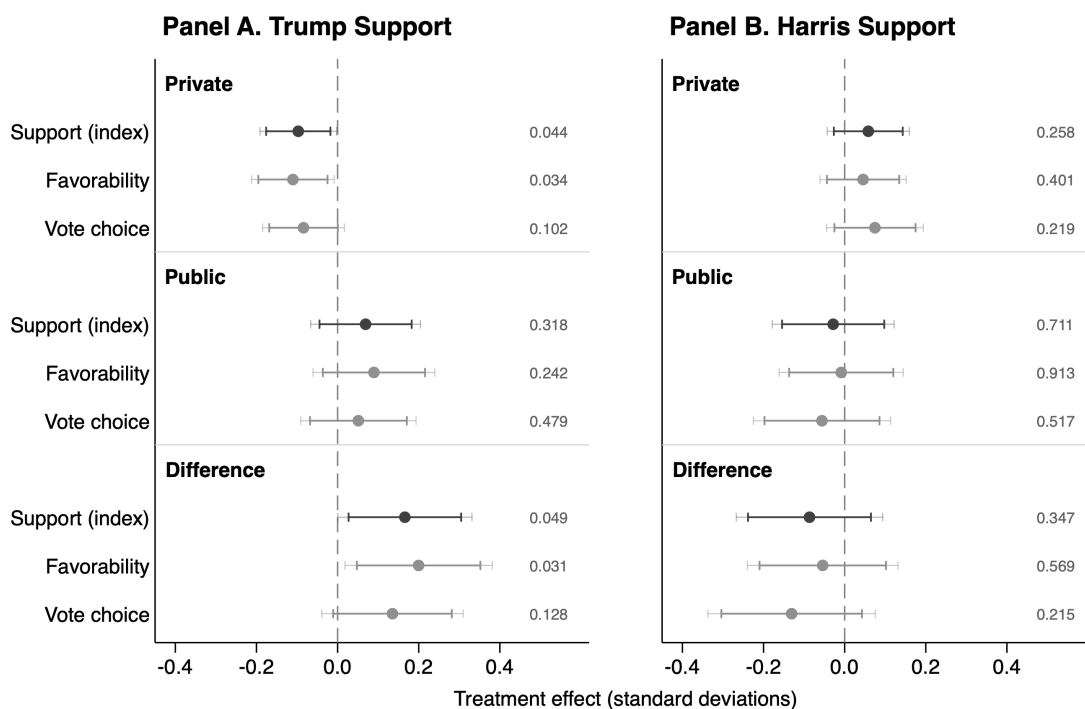
The Harris results suggest a mirror-image pattern, though estimates are less precise. In the private condition, the index effect is positive but not statistically significant (.06 SD, $p = .258$). One potential explanation for this null result is that the ad shifted private support towards third-party

¹⁶Appendix I reports sharpened false discovery rate q-values for the primary persuasion tests and exploratory supporting analyses.

candidates: results in Appendix Table G.2.1 show the ad increased the likelihood of voting for a third-party candidate by three percentage points ($p = .019$). However, similar to the Trump results, the ad had no detectable effect on publicly expressed support for Harris or likelihood of voting for a third party. The coefficient on the public-private difference in the Harris index is correspondingly negative—reflecting the expected smaller increase in public relative to private Harris support—but not statistically significant ($-.09$ SD, $p = .347$); its components follow the same pattern.

Overall, these results suggest that the campaign ad affected privately held and publicly expressed attitudes asymmetrically in the full sample, most clearly for Trump support. Although the pattern is less clear for Harris—in part because shifts in private support away from Trump appear to have benefited third-party candidates as well—it nonetheless demonstrates that the same campaign message can affect private preferences and public expression differently. We turn next to examining what accounts for this asymmetry.

Figure 2: Ad Effects on Private and Public Candidate Support



Notes: Points are estimated treatment effects of exposure to the ad based on regressions with fixed effects, controls, and heteroskedasticity-robust standard errors. All outcomes are standardized so their effects are in standard deviation units. Private estimates are treatment effects among respondents in the private condition. Public estimates are treatment effects among respondents who answered to a female virtual enumerator. The difference rows show the public effect minus the private effect, estimated as the interaction between treatment and the female-enumerator condition. Dark circles indicate the main support index, which combines the favorability and vote choice components (light circles). Thick bars show 90 percent confidence intervals and thin bars show 95 percent confidence intervals. Numbers at right are two-sided p -values.

7 Explaining Ad Effects on Private but not Public Support

Why did the campaign ad change private but not public Trump support? Our preregistered expectation attributed such an asymmetry to social costs for Republican women: the ad would shift private preferences, but even if it shifted norms and reduced those costs, the remaining costs would deter public opposition to Trump.

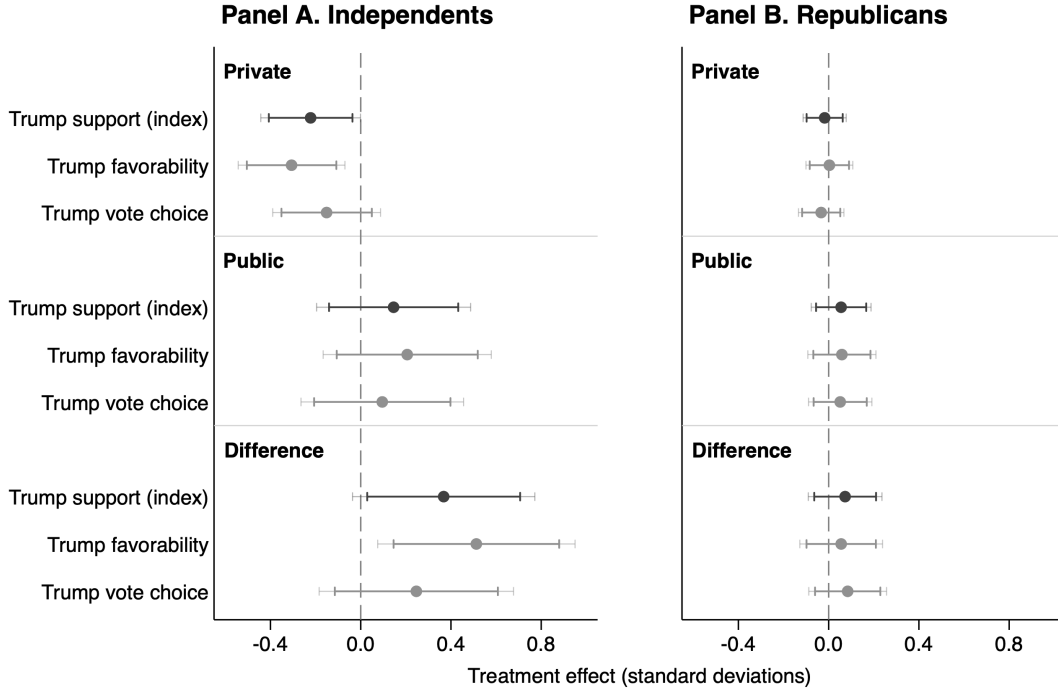
We begin by disaggregating the main results by respondent party.¹⁷ Figure 3 presents the party-specific Trump results; detailed regression estimates and the corresponding Harris results appear in Appendix G.3. As can be seen in Panel B, the ad had no detectable effect on either private or public Trump support among Republican women. We therefore find no evidence of the predicted “shy voters” pattern. Instead, the asymmetric effects were most clearly evident among Independent women. The ad reduced their private Trump support index by .22 standard deviations ($p = .049$), reflecting declines in both favorability and vote choice. It also reduced Independent women’s likelihood of clicking through to Trump’s donation page in the private condition by 3 percentage points ($p = .033$; Appendix Table G.3.1), indicating that the shift in privately held attitudes extended to private behavior.

Yet the ad had no detectable effect on Independents’ publicly expressed Trump support. The resulting public–private difference was .37 SD ($p = .074$), indicating a larger decline in private relative to public support. The difference in this contrast between Independent and Republican women is not statistically significant (.24 SD, $p = .284$), so we do not claim that the two groups differ (see Appendix Table G.3.2). Our claim is narrower: the pooled pattern is most clearly evident among Independent women. Their private response is consistent with evidence that gender-based appeals in campaign advertisements can persuade Independents (Kam, Archer and Geer, 2017). It leaves open, however, why their public expression did not change.

One possibility is that Independent women, like the shy voters we anticipated among Republican women, faced social costs that kept them from expressing a change in private preferences. The descriptive statistics give some reason for doubt: Independent women described more politically

¹⁷We preregistered examining heterogeneous effects by party as an exploratory analysis (see Appendix K). We exclude weak and leaning Democrats because of their small sample size.

Figure 3: Ad Effects on Private and Public Trump Support by Party



Notes: Points are estimated treatment effects of exposure to the ad based on regressions with fixed effects, controls, and heteroskedasticity-robust standard errors, estimated separately for Independents (Panel A, $n = 555$) and Republicans (Panel B, $n = 1399$). All items are standardized so their effects are in standard deviation units. Private estimates are treatment effects among respondents in the private condition. Public estimates are treatment effects among respondents who answered to a female virtual enumerator. The difference rows show the public effect minus the private effect, estimated as the interaction between treatment and the female-enumerator condition. Dark circles indicate the main support index, which combines the favorability and vote choice components (light circles). Thick bars show 90 percent confidence intervals and thin bars show 95 percent confidence intervals.

mixed social environments than Republican women, suggesting that opposing Trump was less likely to carry social costs.

Figure 4 provides a more direct test. It plots mean post-ad Trump support in the ad and control groups, both as privately held and as publicly expressed to the copartisan female enumerator. If women faced social costs for opposing Trump, we would expect two features. First, without the ad, publicly expressed Trump support should match or exceed privately held support: social costs could lead some women to express more support for Trump than they privately held, but would give them no reason to express less. Second, if the ad reduced private Trump support while social costs remained, women who came to oppose Trump would keep that opposition to themselves, so public support would stay near its level without the ad while private support fell below it. We expected this pattern primarily among Republican women, for whom the prevailing ingroup norm favors Trump. As the bottom panel shows, however, their Trump support was similar across all

four conditions, so the ad produced no private shift for social costs to keep from public view.

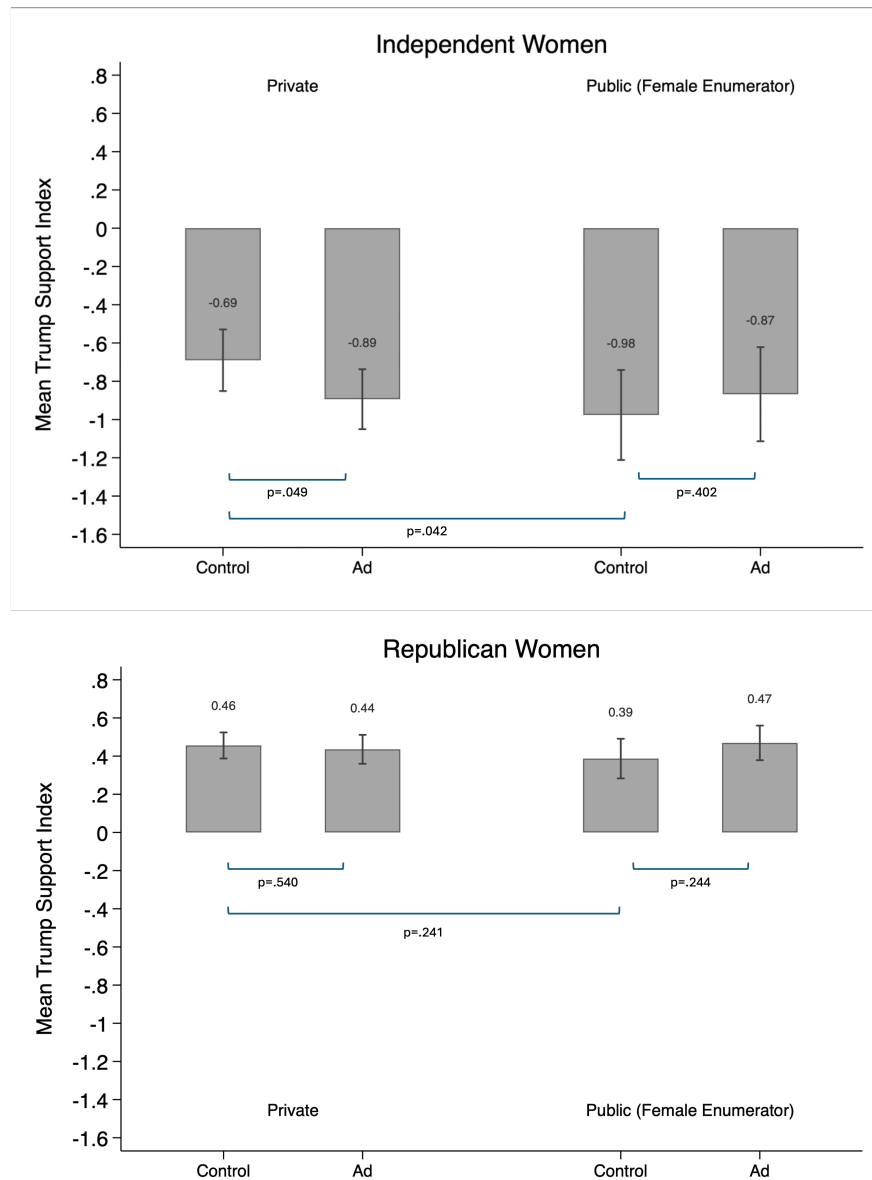
The top panel shows that Independent women departed from both expectations. Among these women, privately held Trump support was lower in the ad condition than in the control condition ($-.89$ versus $-.69$), consistent with the regression-based treatment effect reported above. Publicly expressed support, however, was already low in the absence of the ad: in the control condition, Independent women expressed less support for Trump to a female copartisan than they held privately ($-.98$ versus $-.69$, $p = .042$; Appendix Table G.3.1). Relative to this public control level, the ad had no additional detectable effect on publicly expressed support ($-.87$ versus $-.98$, $p = .402$). Following exposure, privately held and publicly expressed support were at similar levels ($-.89$ and $-.87$). This pattern does not fit the shy voter account in which social costs prevent women from publicly expressing an ad-induced private change: under that account, publicly expressed support would remain above privately held support. Instead, the ad reduced privately held support toward the level Independent women already expressed publicly.

7.1 The Effects of Public Expression (Pre-Ad)

These patterns are consistent with social benefits, rather than costs, to expressing opposition to Trump to a female copartisan, but they rest on comparisons within the post-ad experiment. Additional evidence comes from responses collected before respondents saw any campaign ad. Because these outcomes were measured before ad exposure, any difference between public and private responses reflects the audience alone. If Independent women perceived social benefits to expressing opposition to Trump to another Independent woman, then expressing their views to the co-partisan female enumerator should reduce Trump support relative to private expression. That effect should also be specific to a female audience, rather than appearing for any co-partisan enumerator. We therefore estimate the effects of public expression before the ad experiment, comparing responses to the female and male enumerators with private expression. Figure 5 presents these effects, and Table H.1.1 reports the corresponding regression results.

The figure shows that reporting to the female copartisan enumerator shifted Independent women's responses in a direction consistent with social incentives to express opposition to Trump and concern for reproductive rights. Relative to the private condition, expression to the female virtual enumerator reduced their reported likelihood of voting for Trump by .38 points on a 1-5

Figure 4: Mean Levels of Trump Support by Ad and Public Female Enumerator Treatment Conditions

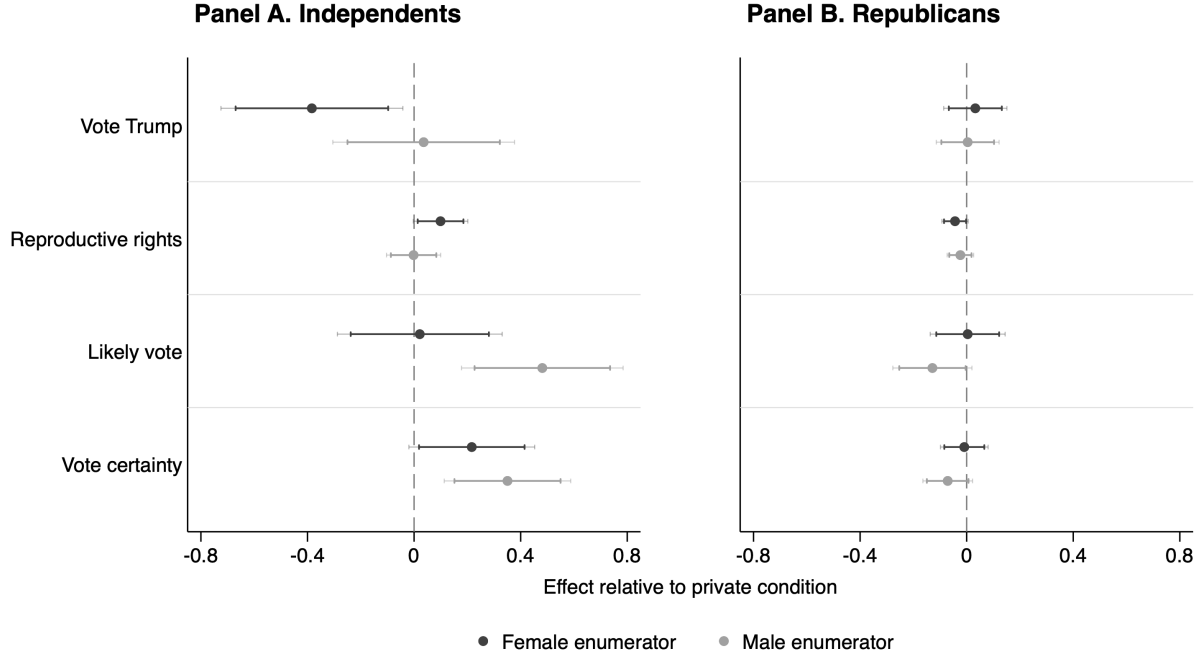


Note: Bars report raw mean values of the Trump support index by treatment condition separately for Independent and Republican women. The private condition measures preferences reported directly in the survey, while the public condition measures preferences reported to a female virtual enumerator. Error bars represent 95 percent confidence intervals. Reported p-values are from regression models corresponding to Appendix Table G.3.1.

scale ($p = .028$). The pattern extended to issue priorities: reporting to the co-partisan female enumerator increased the probability that Independent women named reproductive rights as one of the country's three most important issues by 10 percentage points ($p = .057$). These pre-ad results closely resemble the public expression patterns among Independent women in the no-ad condition.

These shifts depended on the gender of the audience. Expression to the male copartisan enumer-

Figure 5: Effects of Public Expression to Female and Male Enumerators, by Party



Notes: Points are estimated effects of expression to a virtual enumerator based on regressions with fixed effects, controls, and heteroskedasticity-robust standard errors, estimated separately for Independents (Panel A) and Republicans (Panel B). Controls exclude party identification. All effects are relative to the private condition and are in the original units of each outcome. Female enumerator estimates are effects among respondents who answered to a female virtual enumerator, and male enumerator estimates are effects among respondents who answered to a male virtual enumerator. Estimates pool respondents across the ad treatment and control conditions. Thick bars show 90 percent confidence intervals and thin bars show 95 percent confidence intervals.

ator had no detectable effect on Independent women’s likelihood of voting for Trump (.04 points, $p = .836$), and the female–male difference was statistically significant ($-.42$ points, $p = .045$). The male enumerator likewise had no detectable effect on naming reproductive rights as a top issue (0 percentage points, $p = .972$), although the female–male difference was less precisely estimated (10 percentage points, $p = .100$).

The absence of these shifts with the male enumerator did not reflect a general lack of responsiveness to that audience. Relative to private expression, expression to the male co-partisan enumerator increased Independent women’s expressed likelihood of voting and certainty about their vote choice. The male audience thus affected expressions of general political engagement, while lower Trump support and greater concern about reproductive rights appeared only with the female audience. Nor do we find evidence that social costs of expressing opposition to Trump or concern about reproductive rights to the male audience account for the female–male difference: relative to private

expression, the male enumerator produced no detectable increase in expressed Trump support or decrease in concern about reproductive rights. The contrast instead reflects lower Trump support and greater concern about reproductive rights with the female audience, consistent with social benefits to expressing these views to another woman identified as an Independent.

These results also challenge an account in which a general reluctance to appear partisan operates similarly across audiences. Many self-identified Independents hold partisan preferences but mask them in social settings because partisanship is perceived as socially undesirable (Klar and Krupnikov, 2016). Independent women might therefore understate their Trump support to any audience simply to avoid appearing partisan. That account predicts lower expressed Trump support with both enumerators. Instead, the shift appeared only with the female enumerator, indicating patterns are inconsistent with a general reluctance to appear partisan.

We do not see the same pattern among Republican women. Expression to the co-partisan female enumerator had no detectable effect on their expressed likelihood of voting for Trump (.03 points, $p = .591$) and, if anything, reduced the probability that they named reproductive rights among their top issues by 4 percentage points ($p = .083$). The female-enumerator effects differed between Independent and Republican women by $-.42$ points for likelihood of voting for Trump ($p = .020$) and by 14 percentage points for naming reproductive rights ($p = .017$). Although between-party differences are not central to our argument, these results suggest that the shifts among Independent women reflected the particular co-partisan audience they faced, rather than a general response to expressing political views to another woman.

7.2 Evidence for Social Norm Change

We also consider whether the absence of an ad effect on public expression reflects a failure to change perceived ingroup norms. The ad’s social signal was expected to shift perceptions of prevailing norms among women and, in turn, to reduce the social costs or increase the social benefits of public expression to other women. If the ad did not shift those perceptions, that failure could explain its null effect on publicly expressed Trump support. Appendix H.2 examines the ad’s effects on respondents’ perceived descriptive norms, measured as the belief that more than half of Independent women, Republican women, or Independent men would vote for Harris.

The results suggest that the ad shifted perceived norms. Among Independent women, it in-

creased the proportion expressing privately that more than half of Independent women would vote for Harris by 10 percentage points, significant at the 10 percent level ($p = .054$). The absence of a detectable public effect therefore does not appear to reflect simply a failure to shift perceived norms.

Rather, the evidence shows that publicly expressed perceptions of Harris support were already elevated in the absence of the ad. Among Independent women in the ad control condition, expression to the female copartisan enumerator increased the likelihood of expressing that more than half of Independent women would vote for Harris by 19 percentage points relative to private expression ($p = .005$). This pattern is consistent with social benefits: in telling another Independent woman that most Independent women would vote for Harris, respondents may have been affirming what they took to be their group's norm. The norms results thus parallel those for Trump support: the ad shifted privately expressed perceptions toward an already elevated public level, with no detectable effect on public expression (-13 percentage points, $p = .131$). The resulting public-private difference in ad effects was -23 percentage points ($p = .020$).

The specificity of the estimates provides additional support for this interpretation: the pattern is most evident when Independent women express beliefs about their own group to another Independent woman. We find no evidence of such asymmetric ad effects when they express beliefs about Independent women to a male copartisan enumerator (Appendix Table [H.2.1](#)). This asymmetry also differs significantly from the corresponding differences for beliefs about Republican women and Independent men (Appendix [H.2](#)). These comparisons support the interpretation that the pattern reflects public expression about an ingroup to an audience sharing both party and gender.

Taken together, the audience effects and perceived-norm results support a social benefits interpretation of the asymmetry among Independent women. Their public opposition to Trump exceeded private opposition without the ad, and exposure brought private preferences closer to that existing public position. This pattern contrasts with the anticipated account in which social costs leave ad-induced private opposition concealed. Appendix [H.3](#) addresses additional alternative explanations.

8 Conclusion

This paper addresses two central questions. Does persuasion change what people privately believe, what they are willing to express publicly, or both? And how do these effects reshape the relationship between private preferences and public expression? We show that a campaign ad reduced privately held support for Trump but had no detectable effect on support expressed to a female copartisan enumerator, producing a significant difference between its private and public effects. The evidence does not support the preregistered expectation that social costs among Republican women prevented private preference change from translating into public expression. Instead, in the absence of the ad, Independent women expressed less support for Trump publicly than privately—a pattern consistent with social benefits of expressing opposition to another Independent woman—and the ad brought their private preferences closer to what they already expressed publicly. These findings show that campaign persuasion can have asymmetric effects on private preferences and public expression. Studying both reveals what a message changes and whether it widens or narrows the gap between the two, providing a fuller account of how campaign persuasion can contribute to electoral change.

Our findings suggest that the relationship between persuasion, private preferences, and public expression varies across groups and contexts. The contrast between Independent and Republican women is instructive. Independent women entered the study with weaker partisan attachments, greater uncertainty about their vote choice, and more politically heterogeneous social environments—conditions that may have increased the scope for persuasion for this group. More broadly, generalizing the public–private effect pattern requires considering whether the social environment rewards or penalizes the position expressed and the initial relationship between private preferences and public expression. Together, these conditions shape which effect patterns are possible and whether persuasion widens or narrows a gap. Our study thus responds to calls for greater attention to the contexts of persuasion ([Druckman, 2022](#)) by identifying social incentives and the preexisting public–private relationship as contextual features that help explain what campaign appeals change.

Just as the relationship between private preferences and public expression is likely to vary across groups, it is also likely to vary with the audience. Here, public expression was captured by what

respondents were willing to express to an unfamiliar virtual enumerator. We provide substantial evidence that the female virtual enumerator was perceived as a socially salient audience. Still, these results should be interpreted as public expression shaped by generally perceived ingroup norms rather than as public expression to an actual network member. Generalizing beyond this setting likely requires considering two aspects of social incentives: their direction (whether expressing a position brings social benefits or costs) and their weight (how much those benefits or costs matter to the individual). Our findings suggest that direction mattered and that asymmetric effects emerged even in a setting where relational stakes were limited. Within real-world networks, the direction of social pressure may vary not only with the political views of an individual audience (Carlson and Settle, 2016) but also with the homogeneity or heterogeneity of the network itself (McPherson, Smith-Lovin and Cook, 2001). The weight of social incentives may also vary. It may be limited when individuals are relatively unaffected by others’ reactions or when trust makes disagreement less threatening. Conversely, richer relationships, repeated interactions, and greater reputational stakes may give social incentives more weight. Where real-world relationships carry more weight, asymmetries of the kind observed here may be more pronounced, and investigating when this holds is an important task for future research.

Beyond whether these patterns extend to other audiences, their electoral significance depends on whether they translate into behavior and persist over time. Although our outcomes are primarily attitudinal, the ad also reduced clicks to Trump’s donation page among Independent women, providing evidence of a behavioral consequence. We do not directly observe vote choice or social influence, but existing research suggests plausible links to both. Because attitudes predict behavior best under corresponding conditions (Ajzen and Fishbein, 1977), private preferences may be especially relevant to behaviors undertaken outside others’ view, including vote choice (Kelley and Mirer, 1974; Glasman and Albarracín, 2006), while publicly expressed positions may be more closely related to visible behavior. Public expression is also political behavior in its own right (Makse and Sokhey, 2014): it can shape perceived norms and interpersonal persuasion (Kuran, 1995; Mutz, 2006) and commit people to later action (Pallak, Cook and Sullivan, 1980; Lokhorst et al., 2013).

These consequences also depend on persistence. Persuasion effects often decay quickly (Hill et al., 2013; Gerber et al., 2011), although some interventions produce attitude change that lasts for weeks or months (Broockman and Kalla, 2016; Coppock, Ekins and Kirby, 2018), and private

preferences and public expression may change at different rates (Kelman, 1958; Prentice and Miller, 1993; Kuran, 1995). If the private shift we observe fades while public expression holds steady, the preexisting gap would widen again. Our immediate measures show where the ad initially produces change, not where its effects ultimately settle. Measuring both private preferences and public expression repeatedly over time would help establish how the relationship between the two continues to evolve and where the barriers to change lie. More broadly, our findings show why measuring both outcomes matters: a larger effect on private preferences can narrow, rather than widen, an existing public-private gap. Understanding campaign persuasion therefore requires attention to what changes privately, what changes publicly, and how the social environment shapes the relationship between the two.

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Appendix

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A Research Design

Figure 1 in the main text presents an overview of the complete research design, described in detail below.

A.1 Screening, Recruitment, Consent

We recruited participants through CloudResearch Connect, an online platform through which researchers recruit adults to complete paid studies. Connect screens participants and monitors the quality of its participant pool. Recruitment took place over four days, from October 31 to November 3, 2024. Of the 3,896 individuals screened, 2,056 met the eligibility criteria and consented to participate. Eligible respondents identified as women, were at least 18 years old and U.S. citizens, had not yet voted in the 2024 presidential election, and did not identify as strong Democrats.

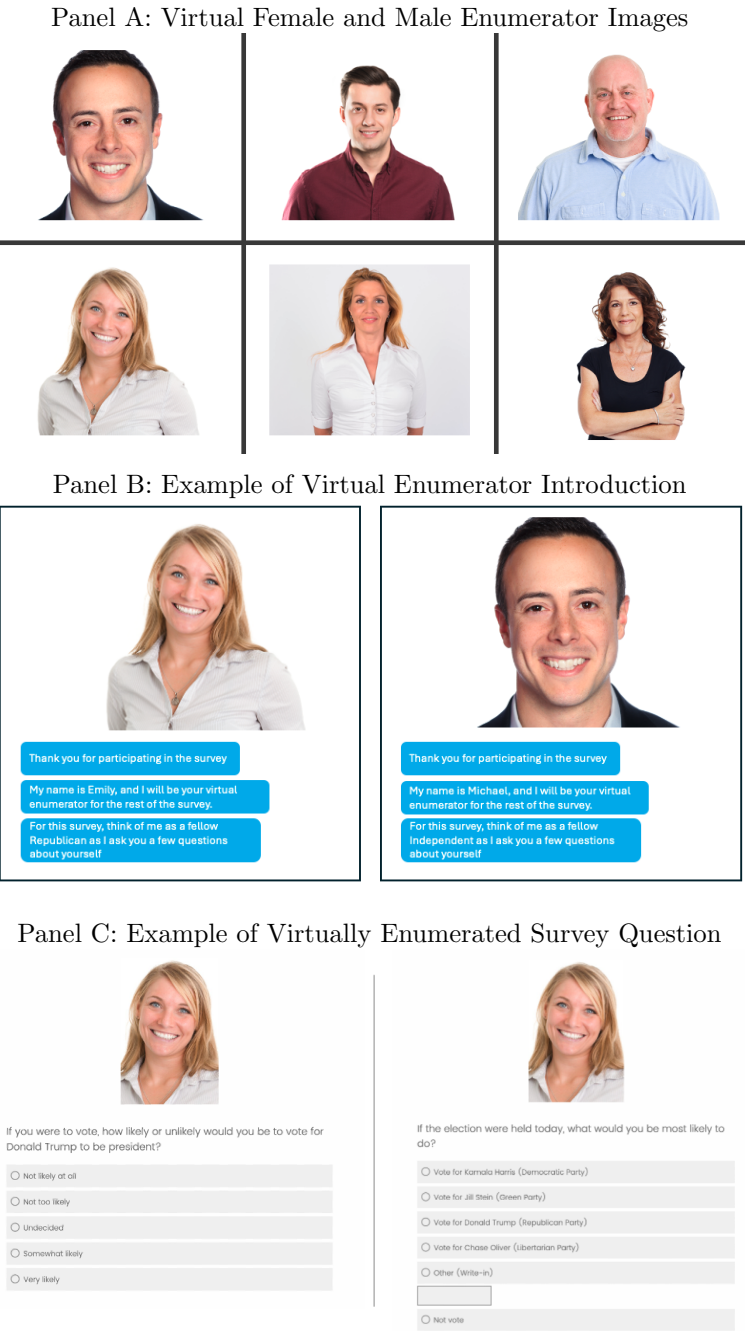
A.2 The Virtual Enumerator Experiment

As described in Section 4.1, eligible respondents who consented were assigned with equal probability to the *private* or *public* condition of the virtual enumerator experiment. Respondents in the private condition completed a standard online survey. Those in the public condition reported responses to a virtual co-partisan enumerator. Respondents in the public condition were further randomly assigned to one of six enumerators who varied in gender and age (Panel A, Figure A.2.1), so that the manipulation did not depend on the characteristics of any one enumerator.¹ Each enumerator introduced themselves as a co-partisan for reasons discussed in the main text.

The enumerator was introduced through an image and chat bubbles (Panel B, Figure A.2.1). In the public condition, the image remained visible above subsequent questions throughout the pre-ad and post-ad survey, as shown in Panel C of Figure A.2.1. Respondents in the private condition answered the same questions without an enumerator image.

¹All virtual enumerators were White, reflecting the anticipated racial composition of the sample. Appendix H.3 shows that treatment effects did not differ systematically by respondent race.

Figure A.2.1: Virtual Enumerator Experimental Interface



Note: Screenshots illustrate components of the virtual enumerator interface used in the experiment.

A.3 Pre-Treatment Survey

Following the introduction of the virtual enumerator, all participants completed a brief pre-treatment survey. It included measures used as controls, for balance checks, to describe the sample, and to assess the effect of public expression before the campaign ad experiment. Descriptive statistics for these measures appear in Appendix B, and balance checks appear in Appendix C.1. The complete survey instrument is available [here](#).

A.4 The Campaign Ad Experiment

An attention check was administered at the end of the pre-treatment survey, before exposure to the campaign ad. The 2,049 participants who passed were randomly assigned with equal probability to the ad treatment or control condition, as described in Section 3.1.² Table A.4.1 summarizes the factorial design and reports the number of respondents in each cell, distinguishing assignments to female and male virtual enumerators within the public condition.

Table A.4.1: Experiment Design

		Campaign Experiment		
		Control	Treatment	Total
Virtual Enumerator Experiment	Private	518	475	993
	Public	558	498	1056
	<i>Female enumerator:</i>	<i>(263)</i>	<i>(247)</i>	<i>(510)</i>
	<i>Male enumerator:</i>	<i>(295)</i>	<i>(251)</i>	<i>(546)</i>
	Total	1076	973	2049

Notes: Table shows the 2x2 factorial design for the campaign and virtual enumerator experiments. Realized sample sizes in cells.

A.5 Post-Ad Survey and Behavioral Outcome

Post-treatment included a survey and behavioral outcome as described in Appendix D. For those assigned to the public condition in the enumerator experiment, enumerator image continued throughout the post-treatment survey.

B Summary and Descriptive Statistics

B.1 Summary Statistics

Table B.1.1 presents summary statistics for all pre-treatment variables. Panels I–III report demographic and background measures used as controls and to assess balance (Appendix C.1). The partisan measures in Panel III were collected during screening, before assignment to the virtual enumerator experiment. Panel IV reports attitudinal measures collected after that assignment, which may themselves be affected by the enumerator manipulation. We use these measures to assess the effects of public expression before the campaign ad experiment.

²Consistent with our pre-analysis plan, the seven who failed were excluded from analyses of post-ad outcomes.

Table B.1.1: Summary Statistics for Pre-Treatment Measures

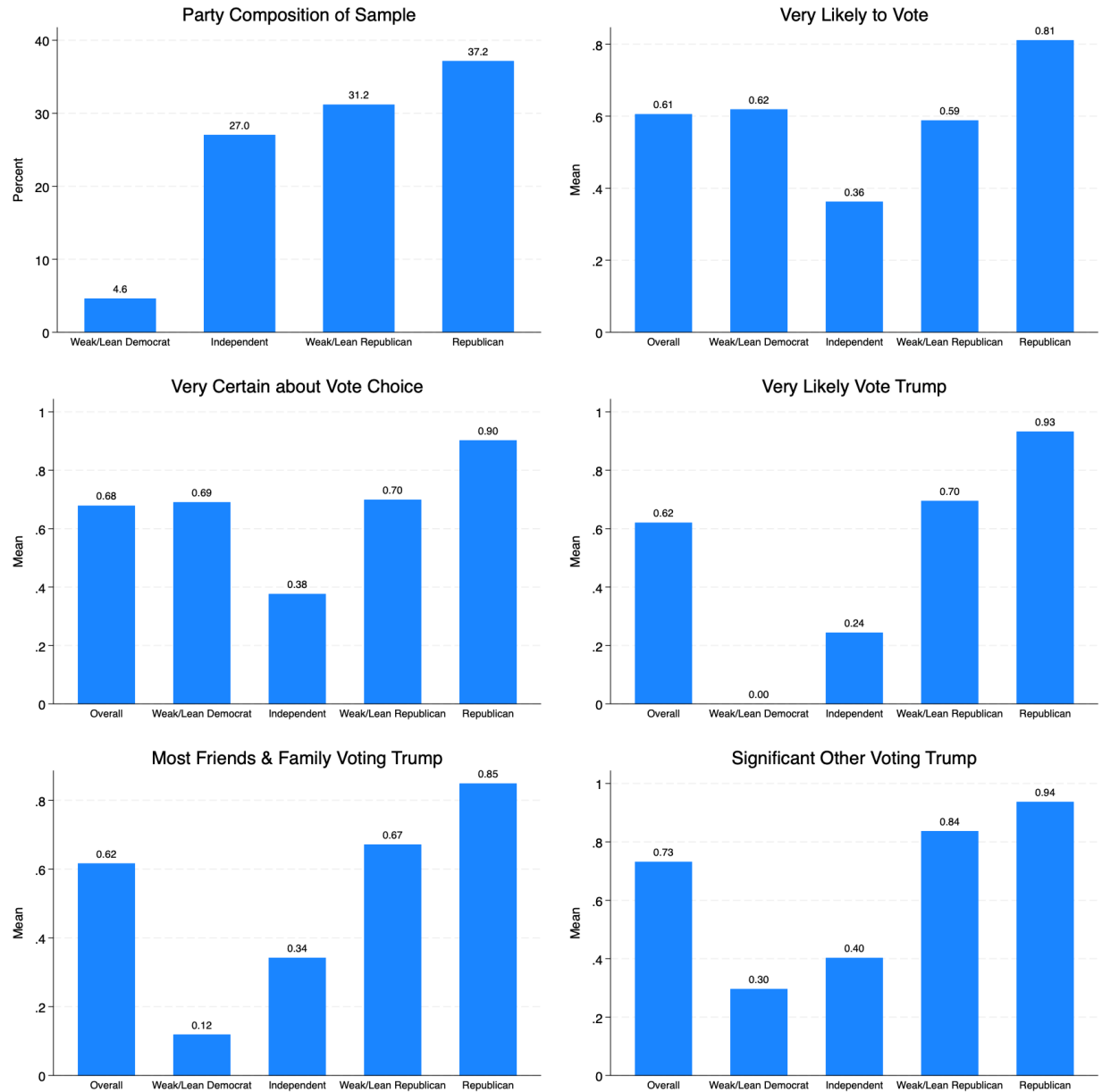
	Min	Max	Mean	Std.Dev
I. Demographics				
Age (Yrs)	18	97	47.46	15.02
Marital Status (Married/Partnered/In Relationship)	0	1	0.54	0.50
College Educated	0	1	0.19	0.39
Race: White Non-Hispanic	0	1	0.82	0.38
Religion: Christian	0	1	0.60	0.49
Employed (Fulltime/Parttime/Self)	0	1	0.50	0.50
Income Quantile	1	5	2.92	1.44
II. Region				
Northeast	0	1	0.15	0.36
Midwest	0	1	0.27	0.44
South	0	1	0.45	0.50
West	0	1	0.13	0.33
III. Partisanship				
Party ID (1-7)	2	7	5.48	1.37
Weak Democrat	0	1	0.01	0.10
Lean Democrat	0	1	0.04	0.19
Independent	0	1	0.27	0.44
Lean Republican	0	1	0.21	0.40
Weak Republican	0	1	0.11	0.31
Strong Republican	0	1	0.37	0.48
IV. Attitudes				
Reproductive rights in top 3 issues	0	1	0.27	0.44
Likelihood of voting in 2024	1	5	1.92	1.37
Certainty about vote choice	1	4	1.57	0.97
Likelihood of voting for Trump	1	5	2.02	1.54
Think friends and family will mostly vote Trump	0	1	0.60	0.49
Think significant other likely to vote Trump	0	1	0.40	0.49
Comfort with others' disapproval	1	4	2.24	0.98
N=2056				

B.2 Descriptive Statistics

Figure B.2.1 presents descriptive statistics for key pre-ad measures, calculated using only respondents in the private condition to minimize pressures associated with public expression. The top-left panel shows the sample's partisan composition: 37 percent identify as strong Republicans, 31 percent as weak or leaning Republicans, 27 percent as Independents, and 5 percent as weak or leaning Democrats. The remaining panels show respondents' likelihood of voting, certainty of vote choice, likelihood of voting for Trump, and reports of whether friends, family, and significant others would vote for Trump. These measures vary across Democrats, Independents, and Republicans as discussed in Section 5.

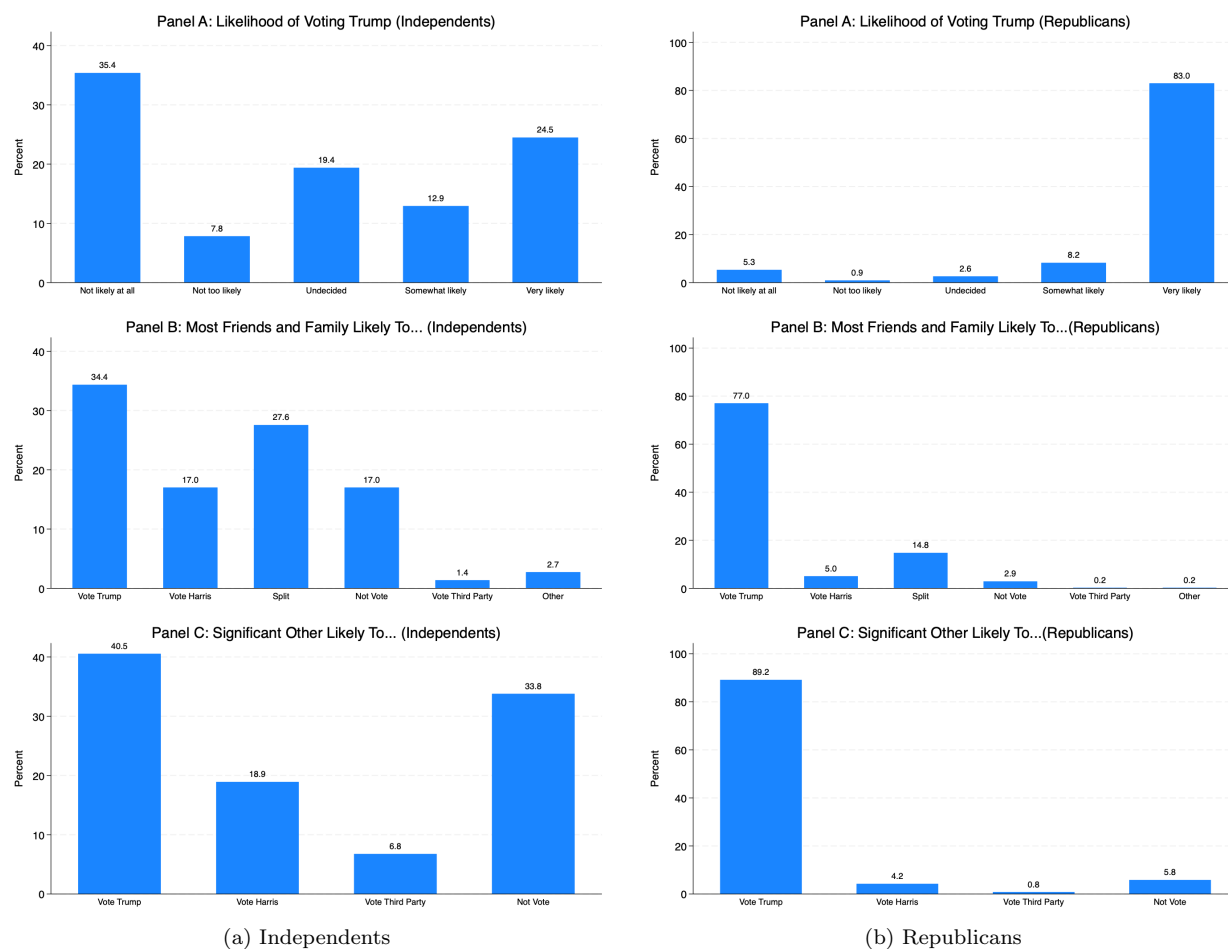
Figure B.2.2 examines differences in personal preferences and social environments in more detail. Its top row shows variation in the likelihood of voting for Trump among Independent and Republican respondents. The remaining panels show variation in the reported voting preferences of people close to respondents, with Independents reporting particularly heterogeneous social environments.

Figure B.2.1: Descriptive Statistics (Private Condition)



Note: Figure presents descriptive statistics for pre-treatment measures. All estimates are calculated using respondents in the private condition only ($n = 993$), which minimizes public expression pressures. “Significant Other Voting Trump” is further restricted to respondents who report being married, in a relationship, or in a domestic partnership ($n = 553$).

Figure B.2.2: Descriptive Statistics by Party Identification (Private Condition)



Note: Figures present descriptive statistics for pre-treatment measures. All estimates are calculated using respondents in the private condition only, which minimizes public expression pressures. “Significant Other Voting Trump” is further restricted to respondents who report being married, in a relationship, or in a domestic partnership.

C Checks

C.1 Balance Checks

Balance checks come from screening and pre-treatment survey demographic questions that should have been unaffected by the enumerator (Table B.1.1, Panels I-III). These are also the core variables used as controls in the entire study.

Table C.1.1: Balance on Enumerator Treatment

Variable	(1) Private Mean/SE	(2) Female Enumerator Mean/SE	(3) Male Enumerator Mean/SE	t-test p-value (1)-(2)	t-test p-value (1)-(3)
Age in Years	47.238 [0.468]	47.713 [0.647]	47.628 [0.676]	0.553	0.629
Married, Partnered or in Relationship	0.557 [0.016]	0.557 [0.022]	0.484 [0.021]	0.983	0.006***
College Educated	0.180 [0.012]	0.178 [0.017]	0.221 [0.018]	0.924	0.051*
Race: White, non-Hispanic	0.827 [0.012]	0.812 [0.017]	0.823 [0.016]	0.477	0.831
Religion: Christian	0.596 [0.016]	0.586 [0.022]	0.608 [0.021]	0.696	0.665
Employed	0.499 [0.016]	0.502 [0.022]	0.496 [0.021]	0.913	0.921
Income Quintile	2.890 [0.046]	2.936 [0.064]	2.953 [0.062]	0.558	0.413
Party ID					
Democrat	0.042 [0.006]	0.045 [0.009]	0.055 [0.010]	0.803	0.262
Independent	0.296 [0.014]	0.252 [0.019]	0.241 [0.018]	0.070*	0.020**
Republican	0.662 [0.015]	0.703 [0.020]	0.704 [0.020]	0.103	0.086*
Region					
Northeast	0.147 [0.011]	0.145 [0.016]	0.161 [0.016]	0.915	0.463
Midwest	0.276 [0.014]	0.262 [0.019]	0.263 [0.019]	0.552	0.573
South	0.441 [0.016]	0.480 [0.022]	0.449 [0.021]	0.143	0.758
West	0.137 [0.011]	0.113 [0.014]	0.128 [0.014]	0.202	0.626
Passed Attn Check	0.997 [0.002]	0.996 [0.003]	0.996 [0.003]	0.775	0.833
N	996	512	548		
F-test of joint significance (F-stat)				0.727	1.837**
F-test, number of observations				1508	1544

Notes: The value displayed for t-tests are p-values; The value displayed for F-tests are the F-statistics;
***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table C.1.2: Balance on Campaign Ad Video Treatment

Variable	(1) Control Mean/SE	(2) Ad Treatment Mean/SE	t-test p-value (1)-(2)
Age in Years	47.983 [0.461]	46.881 [0.476]	0.097*
Married, Partnered or in Relationship	0.526 [0.015]	0.550 [0.016]	0.270
College Educated	0.194 [0.012]	0.186 [0.012]	0.685
Race: White, non-Hispanic	0.826 [0.012]	0.819 [0.012]	0.666
Religion: Christian	0.615 [0.015]	0.577 [0.016]	0.080*
Employed	0.489 [0.015]	0.510 [0.016]	0.334
Income Quintile	2.886 [0.044]	2.953 [0.046]	0.296
Party ID			
Democrat	0.044 [0.006]	0.049 [0.007]	0.542
Independent	0.266 [0.013]	0.276 [0.014]	0.615
Republican	0.691 [0.014]	0.675 [0.015]	0.450
Region			
Region: Northeast	0.148 [0.011]	0.152 [0.011]	0.825
Region: Midwest	0.278 [0.014]	0.259 [0.014]	0.344
Region: South	0.447 [0.015]	0.459 [0.016]	0.592
Region: West	0.127 [0.010]	0.130 [0.011]	0.825
N	1080	976	
F-test of joint significance (F-stat)			0.692
F-test, number of observations			2056

Notes: Table reports balance checks for the full sample, including the seven later dropped for failing the attention check. The value displayed for t-tests are p-values; The value displayed for F-tests are the F-statistics;

***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

C.2 Treatment Checks

Virtual enumerator experiment. Table C.2.1 presents treatment checks for the virtual enumerator experiment. For those assigned to the private condition about 72 percent correctly indicated that they did not have an enumerator or they did not know the sex of their virtual enumerator. Respondents assigned to a public condition correctly reported their enumerator sex at high rates: about 93 (87) percent of those assigned to a female (male) enumerator correctly recalled their enumerator’s sex. By contrast, 51 (54) percent of respondents assigned to the female (male) enumerator condition correctly recalled the virtual enumerator’s partisanship. This implies that gender may have been a more salient cue than co-partisanship, implying that responses to the virtual enumerator may have been shaped more strongly by gender-based than partisan considerations.

Campaign ad experiment. Table C.2.2 presents treatment checks for the campaign ad experiment. In response to a treatment check question asking how many women were featured in the video, 86 percent in the

control correctly stated they did not see a video while 72 percent in treatment correctly recalled the number of women in the ad. The checks also indicate that 88 percent of respondents had not seen the campaign ad previously and almost all watched the ad alone.

Table C.2.1: Treatment Checks: Virtual Enumerator Experiment

	Private (No Enumerator)	Female Enumerator	Male Enumerator
ENUMERATOR TREATMENT			
Sex of virtual interviewer			
I didn't have a virtual interviewer	604 (60.8%)	21 (4.1%)	23 (4.2%)
Male	48 (4.8%)	6 (1.2%)	475 (87.0%)
Female	232 (23.4%)	472 (92.5%)	38 (7.0%)
Don't know	109 (11.0%)	11 (2.2%)	10 (1.8%)
Partisanship of virtual enumerator			
Correct	665 (67.0%)	262 (51.4%)	294 (53.8%)
VIDEO TREATMENT			
Number of women in video			
Measure 1: Correct	817 (82.3%)	354 (69.4%)	455 (83.3%)
Measure 2: Correct	817 (82.3%)	426 (83.5%)	455 (83.3%)
SURVEY			
Duration (mins)	11.06 (10.09)	11.84 (11.55)	11.20 (9.24)
Overall experience with survey			
Very positive	346 (35.7%)	237 (47.9%)	250 (47.1%)
Somewhat positive	243 (25.1%)	145 (29.3%)	141 (26.6%)
Neither positive nor negative	302 (31.2%)	105 (21.2%)	122 (23.0%)
Somewhat negative	48 (5.0%)	5 (1.0%)	12 (2.3%)
Very negative	29 (3.0%)	3 (0.6%)	6 (1.1%)
N	993 (48.5%)	510 (24.9%)	546 (26.6%)

Notes: Table presents treatment checks by assignment to the private and public conditions in the virtual enumerator experiment. Cell values in **bold** indicate correct response for the assigned treatment status.

Table C.2.2: Treatment Checks: Campaign Ad Experiment

	Control (No Video)	Treatment (Video Ad)
Number of women featured in the video that you watched		
I didn't see a video	926 (86.1%)	21 (2.2%)
One	89 (8.3%)	98 (10.1%)
Two	21 (2.0%)	700 (71.9%)
Three	21 (2.0%)	99 (10.2%)
Four	5 (0.5%)	27 (2.8%)
Five	3 (0.3%)	16 (1.6%)
Other (Write In)	11 (1.0%)	12 (1.2%)
Have you seen this video before?		
Yes	.	95 (9.8%)
Maybe	.	21 (2.2%)
No	.	857 (88.1%)
Viewing experience		
Watched alone	.	836 (85.9%)
With others around, but they did not see or hear it	.	96 (9.9%)
With people around, and they saw or heard it	.	41 (4.2%)
Overall experience with survey		
Very positive	487 (46.6%)	346 (36.5%)
Somewhat positive	284 (27.2%)	245 (25.8%)
Neither positive nor negative	258 (24.7%)	271 (28.6%)
Somewhat negative	14 (1.3%)	51 (5.4%)
Very negative	2 (0.2%)	36 (3.8%)
N	1,076 (52.5%)	973 (47.5%)

Notes: Table presents treatment checks by assignment to the campaign ad treatment and control groups. Cell values in **bold** indicate correct response for the assigned treatment status.

D Outcome Measures and Summary Statistics

D.1 Outcomes

Table D.1.1 reports summary statistics for all outcome measures by experimental condition.

Main attitudinal outcomes. Our main outcomes are attitudinal measures of candidate support. The primary pre-registered outcomes for the private-public contrasts are candidate-specific indices of support for Harris and Trump. In the results we also present individual index components—candidate favorability and intended vote choice—for insights into what is driving the index changes. Candidate favorability is measured on a five-point Likert scale ranging from 1 = very unfavorable to 5 = very favorable. Intended vote choice is measured using the question: “If the election were held today, what would you be most likely to do?” Response options included vote Harris, vote Trump, vote Stein, vote Oliver, vote other, and not vote. From these responses, we constructed binary indicators for likely vote Trump and likely vote Harris. For exploratory purposes and to help interpret underlying patterns of defection from Trump support, we also constructed indicators for likely vote third party, likely vote third party or Harris, and likely not vote.

Behavioral outcome. We obtained a behavioral measure of candidate support by giving respondents an opportunity to donate to one of the presidential campaigns at the end of the survey. This behavioral measure was introduced on the final screen of the survey, where respondents saw the following text: “*Thank you for participating in this study. Election Day is Tuesday, November 5. This could be one of your last chances to donate to the candidate of your choice. Remember, you can donate as little as \$1 and every bit will help your candidate. Also remember any donation you make can be kept private.*” Respondents were then given the option to donate to one of the four presidential candidates by clicking through to the candidate’s campaign donation page or to exit the survey. We record clicks on the donation links as the behavioral outcome measure. Importantly, the prompt explicitly emphasized that donation decisions would remain private. As pre-registered, we treat this measure primarily as a behavioral indicator of private candidate support. Accordingly, we include donations in the overall index of candidate support when estimating average treatment effects of the campaign ad (Table F.1.1), but analyze donation behavior separately only within the private condition.

Perceived social norms. To assess whether the ad shifts perceived social norms, we measured respondents’ beliefs about candidate support across partisan and gender groups. Specifically, we elicited perceived support for Harris among Democratic, Independent, and Republican men and women using six items of the form: “To the best of your knowledge, of all [GROUP; e.g., Republican women], how many do you think will vote for Harris?” Responses were recorded on an eight-point Likert scale ranging from 1 (“none”) to 8 (“all”), with 4 corresponding to “about half.” For ease of interpretation, we also construct a binary measure coded 1 if respondents believed that a more than half of the group would vote for Harris (responses 5–8) and 0 otherwise (responses 1–4).

Coding. As pre-registered, prior to analysis, all outcome and mechanism variables were recoded so that higher values correspond to more favorable outcomes (e.g., greater favorability). Variables were analyzed using their original scales. For items pre-registered as measuring the same construct, we constructed indices using inverse covariance weighting following Anderson (2008), implemented using code produced by Cyrus Samii.³

³<https://cyrussamii.com/?p=2656>.

Table D.1.1: Summary Statistics for Outcome Measures, by Experimental Condition

Measure	Ad Control			Ad Treatment		
	Private	Female Enum	Male Enum	Private	Female Enum	Male Enum
	mean (sd)					
Support Indices (Standardized)						
Support for Harris (Index v1)	-0.03 (1.03)	0.04 (1.13)	0.02 (1.10)	-0.01 (0.85)	-0.08 (0.78)	0.09 (1.14)
Support for Harris (Index v2)	-0.06 (0.93)	0.02 (1.05)	-0.02 (0.99)	0.04 (1.00)	-0.05 (0.99)	0.11 (1.10)
Support for Trump (Index v1)	0.05 (0.98)	-0.04 (1.01)	0.02 (1.04)	-0.04 (1.04)	0.03 (0.94)	-0.03 (0.97)
Support for Trump (Index v2)	0.07 (0.96)	-0.05 (1.02)	0.01 (1.01)	-0.07 (1.03)	0.07 (0.96)	-0.02 (1.02)
Candidate Favorability (5pt Scale; 1: Very Unfavorable - 5: Very Favorable)						
Favorability: Trump	3.94 (1.43)	3.73 (1.58)	3.83 (1.50)	3.72 (1.55)	3.94 (1.44)	3.79 (1.54)
Favorability: Harris	1.95 (1.33)	1.99 (1.40)	1.94 (1.34)	2.06 (1.39)	1.91 (1.36)	2.12 (1.48)
Favorability: Stein	2.64 (0.85)	2.71 (0.81)	2.64 (0.85)	2.60 (0.86)	2.63 (0.83)	2.75 (0.76)
Favorability: Oliver	2.64 (0.85)	2.74 (0.73)	2.69 (0.83)	2.61 (0.84)	2.69 (0.78)	2.72 (0.76)
Stated Vote Preference (Binary, =1 if Prefer to vote for [Candidate])						
Vote Preference: Harris	0.13 (0.34)	0.19 (0.39)	0.17 (0.37)	0.17 (0.38)	0.15 (0.36)	0.21 (0.41)
Vote Preference: Trump	0.72 (0.45)	0.68 (0.47)	0.70 (0.46)	0.67 (0.47)	0.72 (0.45)	0.69 (0.46)
Vote Preference: Other	0.02 (0.14)	0.02 (0.15)	0.05 (0.21)	0.05 (0.21)	0.03 (0.18)	0.04 (0.21)
Vote Preference: Would Not Vote	0.13 (0.33)	0.11 (0.31)	0.08 (0.28)	0.12 (0.32)	0.09 (0.29)	0.06 (0.23)
Clicked to Donate (Binary, =1 if Clicked to donate to [Candidate])						
Donation: Trump	0.03 (0.18)	0.03 (0.17)	0.04 (0.19)	0.03 (0.18)	0.03 (0.17)	0.03 (0.16)
Donation: Harris	0.01 (0.12)	0.02 (0.12)	0.02 (0.13)	0.01 (0.08)	0.00 (0.06)	0.02 (0.13)
Donation: Other	0.00 (0.04)	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.01 (0.11)
Perception of Others' Vote for Harris (8 pt Scale; 1: None - 8: All)						
Perceived Harris Vote: Republican Women	2.50 (1.62)	2.59 (1.50)	2.70 (1.72)	2.61 (1.56)	2.67 (1.57)	2.69 (1.59)
Perceived Harris Vote: Republican Men	2.23 (1.50)	2.33 (1.55)	2.37 (1.61)	2.32 (1.56)	2.30 (1.50)	2.22 (1.41)
Perceived Harris Vote: Independent Women	3.21 (1.56)	3.41 (1.61)	3.50 (1.67)	3.44 (1.69)	3.36 (1.53)	3.67 (1.61)
Perceived Harris Vote: Independent Men	2.67 (1.36)	2.92 (1.44)	2.85 (1.44)	2.86 (1.42)	2.87 (1.31)	3.00 (1.38)
N	518	263	295	475	247	251
Perceived Harris Vote: Democratic Women	4.47 (2.07)	4.67 (2.10)	4.77 (1.85)	4.78 (2.02)	4.91 (1.91)	5.23 (1.75)
Perceived Harris Vote: Democratic Men	3.97 (1.97)	4.05 (2.07)	4.05 (1.72)	4.15 (2.00)	4.31 (1.84)	4.44 (1.85)
N	299	157	149	282	143	141
Other Beliefs about Voting (4pt Scale: 1:Very Unlikely - 4: Very Likely)						
Believe Vote Choice Known to Close Ppl	3.12 (1.03)	3.08 (0.99)	3.18 (0.98)	3.07 (1.08)	2.99 (1.10)	3.09 (1.02)
N	268	142	155	285	142	108
Conflict Over Vote Choice (5pt Scale: 1:Strongly Disagree - 5: Strongly Agree)						
Feel Conflicted About Vote Choice	2.23 (1.48)	2.22 (1.43)	2.14 (1.46)	2.35 (1.51)	2.11 (1.37)	2.10 (1.45)
N	518	263	295	475	247	251

Notes: Differences in sample size reflect variation in the analytic samples across measures. The survey question "To the best of your knowledge, of all Republican/Independent/Democratic (wo)men, how many do you think will vote for Harris?" was only asked about Democrat men and women after the expansion of the sample to include Democrat and weakly leaning Democrat survey respondents in the second wave of implementation. The survey question "How likely or unlikely is it that people close to you will learn who you vote for?" was only asked of married or partnered respondents.

E Hypotheses and Estimation

E.1 Hypotheses

Table E.1.1 summarizes all hypotheses. The main hypotheses tested in this paper are bolded.

H1 and H2 are hypotheses about the effects of public expression to a virtual enumerator in the absence of the ad. They apply to responses collected after assignment to the virtual enumerator condition but before exposure to the campaign ad. The pre-analysis plan specified H1 before the sample was expanded to include Independents and weak/leaning Democrats. The updated pre-registration plan allowed for the possibility that, with the expanded sample, expression to a co-partisan enumerator could instead favor Harris and reduce expressed Trump support and stated this would be investigated through heterogeneous effects by party ID. H2 compares responses to female and male enumerators; although the pre-analysis plan stated a directional expectation, it designated this comparison as exploratory. Results for H1 and H2 appear in Appendix H.1.

H3–H5 concern the average effects of the campaign ad. H6 and H7 predict effects in the private condition, while H8 and H9 predict larger effects in private than in the pooled public condition. H10 and H11 predict that the ad will have larger effects on Harris and Trump support, respectively, with a female than with a male enumerator.

$H8_{fem}$ and $H9_{fem}$ are additional tests of preregistered hypotheses (see Appendix K). The preregistered hypotheses compare the private condition with the pooled female and male virtual enumerator conditions; $H8_{fem}$ and $H9_{fem}$ compare the private condition with the public female enumerator condition specifically. Omitting these hypotheses was an oversight: pooling the public conditions can obscure the enumerator-gender heterogeneity anticipated by H10 and H11. The private–public female contrasts are already estimated by the preregistered regression (Equation E.5). We therefore report them as additional tests from the planned analysis, while distinguishing them from the explicitly preregistered tests of H8 and H9.

Finally, the pre-analysis plan specified analyses of heterogeneous ad effects. It stated an expectation of larger effects among women with weaker support for Trump, greater uncertainty about their vote choice, and weaker Republican identification (p. 5), and also specified examining heterogeneity by party identification. As the descriptive statistics in Appendix B show, Independents are especially relevant to these expectations: relative to Republicans in our sample, they report weaker Trump support, greater vote-choice uncertainty, and weaker Republican identification. The comparison between Independents and Republicans therefore follows both the stated directional expectations and the planned analysis by party identification.

Table E.1.1: Hypotheses

A. Public-private difference hypotheses (pre-ad)	
H1	Vote choice certainty and Trump support will be higher in the public than private conditions in the virtual enumerator experiment
H2	Vote choice certainty and Trump support will be lower with a female versus male virtual enumerator.
B. Campaign ad average treatment effect hypotheses	
H3	The ad will increase Harris support.
H4	The ad will reduce Trump support.
H5a	The ad will increase the perceived proportion of women (Republican, Independent, and Democratic) women supporting Harris.
H5b	The ad will have no effect on the perceived proportion of men (Republican, Independent, and Democratic) men supporting Harris.
C. Main ad effect on private preferences hypotheses	
H6	The ad will increase Harris support in the private condition.
H7	The ad will reduce Trump support in the private condition.
D. Main ad effect on public-private difference hypotheses	
H8	The ad will cause a bigger increase in Harris support in the private relative to public (pooled) condition
H8 _{fem}	The ad will cause a bigger increase in Harris support in the private relative to public (female enumerator) condition
H9	The ad will cause a bigger reduction in Trump support in the private relative to public (pooled) condition
H9 _{fem}	The ad will cause a bigger reduction in Trump support in the private relative to public (female enumerator) condition
E. Additional ad effect on public expression hypotheses	
H10	The ad will cause a bigger increase in Harris support in the public (female enumerator) relative to public (male enumerator) condition
H11	The ad will cause a bigger reduction in Trump support in the public (female enumerator) relative to public (male enumerator) condition

The table reports all pre-registered hypotheses and exploratory hypotheses. $H8_{fem}$ and $H9_{fem}$ denote exploratory versions of $H8$ and $H9$ that focus specifically on the preferred comparisons between the private condition and the female-enumerator condition.

E.2 Estimation

We test the hypotheses using the regression specifications below. The pre-analysis plan specified one-sided tests for directional, non-exploratory hypotheses. In the text, we report conventional two-sided p -values. A non-significant test cannot, by itself, establish the absence of an effect, as predicted by H5b.

E.2.1 Virtual Enumerator Tests Using Pre-Ad Data

We test H1 and examine H2 using outcomes measured after assignment to the virtual enumerator condition but before exposure to the campaign ad:

$$Y_i = \alpha + \delta Pub_i + X_i' \psi + \mu_j + \epsilon_i, \quad (E.1)$$

$$Y_i = \alpha + \beta_1 PubFem_i + \beta_2 PubMale_i + X_i' \psi + \mu_j + \epsilon_i. \quad (E.2)$$

Here, Pub_i indicates assignment to either public condition, with the private condition as the reference category. In Equation E.2, $PubFem_i$ and $PubMale_i$ indicate assignment to a female or male enumerator, respectively; the private condition is again the reference category. X_i is the vector of controls described below, μ_j denotes implementation-day fixed effects, and ϵ_i is an individual error term. H1 predicts $\delta > 0$ for both vote-choice certainty and likelihood of voting for Trump, since higher values of these outcomes indicate greater certainty and greater likelihood, respectively. H2 was designated exploratory in the pre-analysis plan, which anticipated lower values of both outcomes with a female than with a male enumerator ($\beta_1 < \beta_2$). We evaluate H2 using a test of that coefficient difference.

E.2.2 Average Treatment Effect Tests

We test H3–H5b in the full analytic sample using:

$$Y_i = \alpha + \delta Ad_i + X_i' \psi + \mu_j + \epsilon_i, \quad (E.3)$$

where Ad_i indicates assignment to the campaign ad rather than the control condition. The outcome Y_i varies across tests. Positive values of δ support the predicted increase in Harris support (H3) and in perceived support for Harris among women (H5a); negative values support the predicted reduction in Trump support (H4). For perceived support for Harris among men, H5b predicts no effect. We report the estimate and its uncertainty rather than interpreting a non-significant result as proof of no effect.

All specifications include implementation-day fixed effects to account for potential developments during recruitment, which ran from October 31 through November 3, 2024. The pre-specified controls, X_i , are shown in Panels I–III of Table B.1.1 and include age, partisan identification, marital status, education, race, religion, employment status, income, and region. Party identification was measured during screening, before enumerator assignment. The other controls were collected afterward but describe background characteristics that we do not expect enumerator assignment to change.

E.2.3 Public–Private Hypothesis Tests

We test H6–H9 by interacting ad assignment with assignment to the pooled public condition:

$$Y_i = \alpha + \beta_1 Ad_i + \beta_2 Pub_i + \beta_3 (Ad_i \times Pub_i) + X_i' \psi + \mu_j + \epsilon_i. \quad (E.4)$$

The ad effect in private is β_1 , the ad effect in public is $\beta_1 + \beta_3$, and β_3 is the public-minus-private difference in ad effects. The directional predictions are:

- H6: $\beta_1 > 0$ for Harris support in private.
- H7: $\beta_1 < 0$ for Trump support in private.
- H8: $\beta_3 < 0$ for Harris support, indicating a larger increase in private than in public.
- H9: $\beta_3 > 0$ for Trump support, indicating a larger reduction in private than in public.

To distinguish the two public enumerator conditions, we also estimate the following model specified in the pre-analysis plan:

$$\begin{aligned} Y_i = & \alpha + \beta_1 Ad_i + \beta_2 (Ad_i \times PubFem_i) + \beta_3 PubFem_i \\ & + \beta_4 (Ad_i \times PubMale_i) + \beta_5 PubMale_i \\ & + X_i' \psi + \mu_j + \epsilon_i. \end{aligned} \quad (E.5)$$

The main coefficients of interest are β_1 , which is the effect of the ad in private, and β_2 , which is the effect of the ad in private versus public (female enumerator) conditions, testing H8_{fem} and H9_{fem}. We also use this equation for the pre-specified tests of H10 and H11:

- H8_{fem}: $\beta_2 < 0$ indicating a larger increase in Harris support in the private relative to public (female enumerator) condition.
- H9_{fem}: $\beta_2 > 0$ for Trump support, indicating a larger reduction in Trump support in the private relative to public (female enumerator) condition.
- H10: $\beta_2 > \beta_4$ for Harris support, indicating a larger increase in Harris support when reporting to a female versus male enumerator.
- H11: $\beta_2 < \beta_4$ for Trump support, indicating a larger reduction Trump support when reporting to a female versus male enumerator.

E.2.4 Heterogeneity by Party Identification

We examine ad effects separately among Independents and Republicans by estimating Equation E.5 in each group. To test whether the estimated differences between enumerator conditions vary by party identification, we also estimate the following model in the Independent and Republican subsample:

$$\begin{aligned} Y_i = & \alpha + \beta_1 Ad_i + \beta_2 (Ad_i \times PubFem_i) + \beta_3 PubFem_i \\ & + \beta_4 (Ad_i \times PubMale_i) + \beta_5 PubMale_i \\ & + \beta_6 Ind_i + \beta_7 (Ad_i \times Ind_i) + \beta_8 (PubFem_i \times Ind_i) \\ & + \beta_9 (Ad_i \times PubFem_i \times Ind_i) \\ & + \beta_{10} (PubMale_i \times Ind_i) + \beta_{11} (Ad_i \times PubMale_i \times Ind_i) \\ & + X_i' \psi + \mu_j + \epsilon_i. \end{aligned} \quad (E.6)$$

Ind_i equals one for Independents and zero for Republicans. Among Republicans, β_2 is the difference in ad effects between the public female enumerator and private conditions; among Independents, that difference is $\beta_2 + \beta_9$. Thus, β_9 tests whether the public-female-minus-private difference varies between Independents and Republicans. Similarly, β_{11} tests whether the public-male-minus-private difference varies by party group. We present estimates from the separate party samples to show the size and direction of effects within each group and use the interaction tests when assessing whether those effects differ statistically.

F Campaign Ad Average Treatment Effects

F.1 Candidate Support

Table F.1.1 reports the average treatment effects of the campaign ad on Harris and Trump support (H3–H4). Although our focus is on differences between private and public expression, we report these results to follow the pre-analysis plan. The index and component estimates for Trump support are in the hypothesized direction but are not statistically significant. The same is true of the Harris support estimates for the v2 index and its components, although Panel C shows we find increases in the the likelihood of supporting a third-party candidate by approximately one percentage point ($p = .093$) and the likelihood of supporting either Harris or a third-party candidate by approximately three percentage points ($p = .045$). Nevertheless, as the analyses below show, these average estimates mask variation across private and public reporting conditions.

Table F.1.1: Campaign Ad Average Treatment Effects

PANEL A: TRUMP SUPPORT					
	Index v1	Index v2	Favorability	Vote	Donate
Ad Effect	-0.04	-0.03	-0.05	-0.01	0.00
<i>s.e.</i>	(0.04)	(0.03)	(0.05)	(0.02)	(0.01)
<i>pval</i>	[0.332]	[0.365]	[0.338]	[0.461]	[0.563]
Control Mean	0.02	0.02	3.86	0.71	0.03
PANEL B: HARRIS SUPPORT					
	Index v1	Index v2	Favorability	Vote	Donate
Ad Effect	-0.01	0.05	0.06	0.02	-0.01
<i>s.e.</i>	(0.04)	(0.04)	(0.05)	(0.01)	(0.00)
<i>pval</i>	[0.735]	[0.179]	[0.216]	[0.230]	[0.135]
Control Mean	0.00	-0.03	1.96	0.16	0.02
PANEL C: ALTERNATIVE ELECTORAL RESPONSES					
	Vote 3rd	Vote Harris/3rd	Note Vote		
Ad Effect	0.01*	0.03**	-0.02		
<i>s.e.</i>	(0.01)	(0.02)	(0.01)		
<i>pval</i>	[0.093]	[0.045]	[0.132]		
Control Mean	0.03	0.18	0.11		
N	2049	2049	2049	2049	2049
Controls	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$ based on two-sided p-values. Each column and panel reports results from a separate regression, estimated following Equation 1. Index.v1 is a pre-registered standardized index combining favorability, vote intention, and donation outcomes. Index.v2 is a standardized index combining favorability and vote intention outcomes only. Favorability is measured on a five-point scale ranging from 1 = very unfavorable to 5 = very favorable. Vote and donation outcomes are binary indicators coded 1 = yes and 0 = no. Control means report means in the pure control condition (private condition, no ad treatment).

F.2 Social Norms

For completeness with the pre-analysis plan, Table F.2.1 reports the average treatment effect of the ad on perceived descriptive norms (H5a and H5b). As expected, the ad increased the perceived proportion of women voting for Harris, primarily through increases in perceived support among Democratic and Independent women rather than Republican women. The ad also increased perceived support among Democratic and Independent men, which we had not anticipated. These estimates show that the ad shifted perceptions of how both women and men would be voting, but they do not indicate whether it changed respondents'

perceptions of the norm among women (versus men) in their own party group. Appendix H.2 examines perceived support among copartisan women and men to assess that more specific and relevant pattern.

Table F.2.1: Ad Effect on Perceived Descriptive Norms (ATE)

Panel A: Perceived Proportion of [...] Women Voting Harris					
	Index (v1)	Index (v2)	Democratic	Independent	Republican
Ad Effect	0.15***	0.08*	0.35***	0.14**	0.08
<i>s.e.</i>	(0.06)	(0.04)	(0.11)	(0.07)	(0.07)
<i>pval</i>	[0.008]	[0.073]	[0.002]	[0.047]	[0.257]
Control Mean	-0.07	-0.04	4.60	3.34	2.58
N	1171	2049	1171	2049	2049
Controls	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes

Panel B: Perceived Proportion of [...] Men Voting Harris					
	Index (v1)	Index (v2)	Democratic	Independent	Republican
Ad Effect	0.13**	0.05	0.24**	0.12**	0.00
<i>s.e.</i>	(0.06)	(0.04)	(0.11)	(0.06)	(0.07)
<i>pval</i>	[0.025]	[0.250]	[0.029]	[0.048]	[0.992]
Control Mean	-0.059	-0.02	4.012	2.78	2.30
N	1171	2049	1171	2049	2049
Controls	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes

Notes: * p<.10, ** p<.05, *** p<.01 based on two-sided p-values. Standard errors are shown in parentheses and two-sided p-values in brackets. Each outcome measures respondents' perceived proportion of women (Panel A) or men (Panel B) in the indicated party group who would vote for Harris. The Democratic, Independent, and Republican outcomes use an eight-point scale, from 1 (none) to 8 (all). Index (v1) combines perceptions for all three party groups; Index (v2) combines perceptions for Independents and Republicans only. The Democratic item, and therefore Index (v1), has a smaller sample because the item was added when the sample was expanded partway through implementation. Entries are estimated effects of the campaign ad relative to the control condition.

G Ad Effects on Private and Public Attitudes

G.1 Ad Effects in Private vs. Public (Pooled) Conditions

Table G.1.1 reports the effects of the campaign ad in the private and pooled public conditions (H6–H9 in Table E.1.1). These estimates are smaller in magnitude than the private–public female enumerator contrasts reported below, but their pattern is broadly consistent with the predictions. The ad reduced privately expressed support for Trump, while its effect on publicly expressed support was smaller, yielding a positive public–private difference. The Harris estimates point in the opposite direction but are less precise, partly because the ad also appears to have increased support for third-party candidates. Finally, the Public indicator is statistically significant for some outcomes, suggesting that public reporting affected responses even in the control condition. We examine these public–private differences in Appendix H.1.

Table G.1.1: Ad Effects in Private vs. Public (Pooled) Conditions

		TRUMP SUPPORT				HARRIS SUPPORT				ALTERNATIVE ELECTORAL RESPONSES			
		Index	Favorability	Vote	Donate	Index	Favorability	Vote	Donate	Vote 3rd	Vote Harris/3rd	Vote Not	Donate Harris/3rd
Panel A: Regression results													
(1)	Ad Effect (Private) (B1)	-0.10**	-0.16**	-0.04	0.00	0.06	0.06	0.03	-0.01	0.03**	0.05**	-0.01	-0.01
	<i>s.e.</i>	(0.05)	(0.07)	(0.02)	(0.01)	(0.05)	(0.07)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
	<i>pval</i>	[0.044]	[0.033]	[0.102]	[0.852]	[0.256]	[0.398]	[0.217]	[0.200]	[0.019]	[0.017]	[0.454]	[0.436]
(2)	Public (B2)	-0.07	-0.12*	-0.03		0.05	0.00	0.04*		0.02*	0.05**	-0.02	
	<i>s.e.</i>	(0.05)	(0.07)	(0.02)		(0.05)	(0.07)	(0.02)		(0.01)	(0.02)	(0.02)	
	<i>pval</i>	[0.128]	[0.091]	[0.241]		[0.378]	[0.962]	[0.076]		[0.081]	[0.012]	[0.151]	
(3)	Ad x Public (B3)	0.13*	0.21**	0.05		-0.02	0.00	-0.01		-0.03	-0.04	-0.01	
	<i>s.e.</i>	(0.07)	(0.10)	(0.03)		(0.07)	(0.10)	(0.03)		(0.02)	(0.03)	(0.03)	
	<i>pval</i>	[0.059]	[0.047]	[0.122]		[0.824]	[0.968]	[0.609]		[0.119]	[0.190]	[0.713]	
Panel B: Calculated results													
(4)	Ad Effect Public	0.03	0.05	0.01		0.04	0.06	0.01		0.00	0.01	-0.03	
	<i>s.e.</i>	(0.05)	(0.07)	(0.02)		(0.05)	(0.07)	(0.02)		(0.01)	(0.02)	(0.02)	
	<i>pval</i>	[0.512]	[0.492]	[0.589]		[0.428]	[0.365]	[0.611]		[0.926]	[0.593]	[0.131]	
Control Mean		0.07	3.94	0.72	0.03	-0.06	1.95	0.13	0.01	0.02	0.15	0.125	0.02
N		2049	2049	2049	2049	2049	2049	2049	2049	2049	2049	2049	2049
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * p<.10, ** p<.05, *** p<.01 based on two-sided p-values. Each column reports results from a separate interaction regression of the ad treatment and public-condition assignment (virtual enumerator), estimated following Equation E.4. “Ad Effect (Private)” captures the effect of the ad in the private condition; “Ad x Public” reports the interaction coefficient; “Public” captures the effect of assignment to a virtual enumerator in the no ad condition; and “Ad Effect | Public” is the calculated marginal effect of the ad in the public condition. The support indices are pre-registered standardized indices combining candidate favorability and vote intention. Favorability is measured on a five-point scale ranging from 1 = very unfavorable to 5 = very favorable. Vote intention is a binary indicator coded 1 = yes and 0 = no. Donate is a behavioral measure pre-registered to capture the effect of the ad on private donation behavior and is presented as a behavioral corollary to the effect of the ad on private preferences. Control means report means in the pure control condition (private condition, no ad treatment).

G.2 Ad Effects in Private vs. Public (Female and Male Enumerator) Conditions

Table G.2.1 reports the effects of the campaign ad in the private condition and in the public female and male enumerator conditions. Our primary focus is the private–public female enumerator comparison (H6–H7 and $H8_{fem}-H9_{fem}$). These estimates appear in bold and correspond to Figure 2 in the main text. We also report the male enumerator results to assess H10–H11.

As discussed in the main text, the Trump results show the predicted asymmetry: the ad reduced privately expressed support but had little effect on support expressed to a female enumerator, yielding a statistically significant public–private difference in ad effects. The Harris estimates point in the predicted opposite direction but are less precise, partly because some private movement away from Trump appears to have gone to third-party candidates.

The ad also had little effect on support expressed to a male enumerator. The female–male differences in ad effects (row 8) are not significant, providing no clear evidence for H10 or H11. Appendix H.1 shows, however, that even in the absence of the ad, women expressed different levels of candidate support to female and male enumerators. Enumerator gender thus shaped public expression in the absence of the ad, although we do not find clear evidence that it changed the effect of the ad.

Table G.2.1: Ad Effects in Private vs. Public (Female and Male Enumerator) Conditions

	TRUMP SUPPORT				HARRIS SUPPORT				ALTERNATIVE OUTCOMES		
	Index	Favorability	Vote	Donate	Index	Favorability	Vote	Donate	Vote 3rd	Vote Harris or 3rd	Note Vote
Panel A: Regression Results											
(1) Ad Effect (Private) (B1)	-0.10**	-0.16**	-0.04	0.00	0.06	0.06	0.03	-0.01	0.03**	0.05**	-0.01
<i>s.e.</i>	(0.05)	(0.07)	(0.02)	(0.01)	(0.05)	(0.07)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
<i>pval</i>	[0.044]	[0.034]	[0.102]	[0.851]	[0.258]	[0.401]	[0.219]	[0.198]	[0.019]	[0.018]	[0.458]
(2) Ad x Public (Female Enumerator) (B2)	0.17**	0.28**	0.06		-0.09	-0.07	-0.04		-0.02	-0.06	0.00
<i>s.e.</i>	(0.08)	(0.13)	(0.04)		(0.09)	(0.13)	(0.04)		(0.02)	(0.04)	(0.03)
<i>pval</i>	[0.049]	[0.031]	[0.128]		[0.347]	[0.569]	[0.215]		[0.401]	[0.112]	[0.983]
(3) Public (Female Enumerator) (B3)	-0.09	-0.17*	-0.03		0.06	0.01	0.04*		0.00	0.05*	-0.02
<i>s.e.</i>	(0.06)	(0.09)	(0.03)		(0.07)	(0.09)	(0.03)		(0.01)	(0.03)	(0.02)
<i>pval</i>	[0.125]	[0.072]	[0.279]		[0.389]	[0.935]	[0.085]		[0.767]	[0.078]	[0.465]
(4) Ad x Public (Male Enumerator) (B4)	0.09	0.14	0.04		0.05	0.08	0.01		-0.03	-0.02	-0.02
<i>s.e.</i>	(0.08)	(0.12)	(0.04)		(0.09)	(0.12)	(0.04)		(0.02)	(0.04)	(0.03)
<i>pval</i>	[0.258]	[0.277]	[0.312]		[0.559]	[0.517]	[0.703]		[0.110]	[0.593]	[0.508]
(5) Public (Male Enumerator) (B5)	-0.05	-0.08	-0.02		0.03	0.00	0.03		0.03**	0.06**	-0.04*
<i>s.e.</i>	(0.06)	(0.09)	(0.03)		(0.06)	(0.08)	(0.02)		(0.01)	(0.03)	(0.02)
<i>pval</i>	[0.333]	[0.333]	[0.401]		[0.570]	[0.996]	[0.242]		[0.027]	[0.021]	[0.078]
Panel B: Calculated Effects											
(6) Ad Effect Public (Female Enum) (B1+B2)	0.07	0.13	0.02		-0.03	-0.01	-0.02		0.01	-0.01	-0.02
<i>s.e.</i>	(0.07)	(0.11)	(0.03)		(0.08)	(0.10)	(0.03)		(0.01)	(0.03)	(0.03)
<i>pval</i>	[0.318]	[0.242]	[0.479]		[0.711]	[0.913]	[0.517]		[0.450]	[0.796]	[0.553]
(7) Ad Effect Public (Male Enum) (B1+B4)	0.00	-0.02	0.00		0.11	0.14	0.04		-0.01	0.03	-0.03
<i>s.e.</i>	(0.07)	(0.10)	(0.03)		(0.07)	(0.10)	(0.03)		(0.02)	(0.03)	(0.02)
<i>pval</i>	[0.949]	[0.826]	[0.951]		[0.135]	[0.161]	[0.184]		[0.672]	[0.304]	[0.123]
(8) Test: Ad Effect in Female vs. Male Public Conditions (B2-B4)	0.07	0.15	0.02		-0.14	-0.15	-0.06		0.02	-0.04	0.02
<i>s.e.</i>	(0.10)	(0.15)	(0.05)		(0.11)	(0.14)	(0.04)		(0.02)	(0.04)	(0.03)
<i>pval</i>	[0.444]	[0.312]	[0.643]		[0.192]	[0.294]	[0.163]		[0.419]	[0.363]	[0.579]
Control mean	0.07	3.94	0.72	0.03	-0.06	1.95	0.13	0.01	0.02	0.15	0.13
N	2049	2049	2049	2049	2049	2049	2049	2049	2049	2049	2049
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * p<.10, ** p<.05, *** p<.01 based on two-sided p-values. Standard errors are shown in parentheses and two-sided p-values in brackets. Pre-registered one-sided p-values are not shown but are equal to one-half of the corresponding two-sided p-values for tests in the hypothesized direction. Each column reports results from a separate interaction regression estimated following Equation E.5 with the corresponding coefficient estimates shown in parentheses. Bolded results correspond to those in Figure 2 in the main text. Index is a pre-registered standardized index combining candidate favorability and vote intention. Favorability is measured on a five-point scale ranging from 1 = very unfavorable to 5 = very favorable. Vote intention is a binary indicator coded 1 = yes and 0 = no. Donate is a behavioral measure pre-registered to capture the effect of the ad on private donation behavior and is presented as a behavioral corollary to the effect of the ad on private preferences. Control means report means in the pure control condition (private condition, no ad treatment). All models include pre-registered covariates and day fixed effects.

G.3 Private and Public Effects By Respondent Party

Table G.3.1 presents the effects of the campaign ad on private versus public Trump support by respondent party. Bolded results correspond to the main results presented in Figure 3. As reported in the main text, Table G.3.1 shows a private–public asymmetry in ad effects on Trump support among Independent women. We do not detect the same pattern among Republican women, although this difference in statistical significance is not itself evidence that the groups differ. Table G.3.2 provides the formal comparison: the three-way interaction estimates are in the direction of a larger asymmetry among Independents, but none is statistically significant. The subgroup estimates therefore locate the observed pattern among Independents without establishing that the ad affected Independents differently from Republicans.

Additionally, we check the distribution of Trump support for Independents. Figure G.3.1 displays the Trump support index across the private and female enumerator conditions among Independent women. The lower support observed in the private ad condition does not appear to be driven by a small number of extreme values. We present this descriptive check alongside the regression results, which provide the formal tests of the treatment effects.

Table G.3.3 reports the corresponding results for Harris support. Among Independent women, the private ad effects on the Harris support index, favorability, and vote intention are positive but imprecise. For Independents, the ad increased private willingness to vote for either Harris or a third-party candidate by 10 percentage points ($p = .051$), consistent with the decline in private Trump support discussed in the main text. We find little evidence of an effect on support expressed to a female enumerator; the private–public differences are generally in the predicted negative direction but also imprecisely estimated. Among Republican women, the estimated ad effects are small and statistically insignificant in both the private and public conditions.

Table G.3.1: Ad Effects on **Trump Support** in Private vs. Public (Female and Male Enumerator) Conditions, By Respondent Party

		Independents				Republicans			
		Index	Favorability	Vote	Donate	Index	Favorability	Vote	Donate
Panel A: Regression Results									
(1)	Ad Effect (Private) (B1)	-0.22**	-0.44**	-0.07	-0.03**	-0.03	-0.02	-0.02	0.01
	<i>s.e.</i>	(0.11)	(0.17)	(0.05)	(0.02)	(0.05)	(0.08)	(0.02)	(0.02)
	<i>pval</i>	[0.049]	[0.011]	[0.215]	[0.033]	[0.540]	[0.812]	[0.402]	[0.418]
(2)	Ad x Public (Female Enumerator) (B2)	0.37*	0.73**	0.11		0.11	0.15	0.05	
	<i>s.e.</i>	(0.21)	(0.32)	(0.10)		(0.09)	(0.14)	(0.04)	
	<i>pval</i>	[0.074]	[0.022]	[0.261]		[0.194]	[0.292]	[0.196]	
(3)	Public (Female Enumerator) (B3)	-0.30**	-0.54**	-0.10		-0.07	-0.11	-0.03	
	<i>s.e.</i>	(0.14)	(0.22)	(0.07)		(0.06)	(0.10)	(0.03)	
	<i>pval</i>	[0.042]	[0.016]	[0.144]		[0.241]	[0.265]	[0.300]	
(4)	Ad x Public (Male Enumerator) (B4)	0.21	0.23	0.10		0.05	0.08	0.02	
	<i>s.e.</i>	(0.21)	(0.31)	(0.10)		(0.09)	(0.13)	(0.04)	
	<i>pval</i>	[0.320]	[0.460]	[0.291]		[0.557]	[0.565]	[0.603]	
(5)	Public (Male Enumerator) (B5)	-0.11	-0.18	-0.04		-0.06	-0.06	-0.03	
	<i>s.e.</i>	(0.14)	(0.22)	(0.07)		(0.06)	(0.09)	(0.03)	
	<i>pval</i>	[0.418]	[0.404]	[0.514]		[0.319]	[0.498]	[0.255]	
Panel B: Calculated Effects									
(6)	Ad Effect Public (Female Enum) (B1+B2)	0.15	0.29	0.04		0.08	0.13	0.03	
	<i>s.e.</i>	(0.17)	(0.27)	(0.08)		(0.07)	(0.11)	(0.03)	
	<i>pval</i>	[0.402]	[0.277]	[0.602]		[0.244]	[0.257]	[0.322]	
(7)	Ad Effect Public (Male Enum) (B1+B4)	-0.02	-0.20	0.04		0.02	0.06	0.00	
	<i>s.e.</i>	(0.17)	(0.26)	(0.08)		(0.07)	(0.11)	(0.03)	
	<i>pval</i>	[0.926]	[0.438]	[0.660]		[0.781]	[0.589]	[0.967]	
(8)	Test: Ad Effect in Female vs. Male Public Conditions (B2-B4)	0.16	0.50	0.01		0.06	0.07	0.03	
	<i>s.e.</i>	(0.25)	(0.38)	(0.12)		(0.10)	(0.15)	(0.05)	
	<i>pval</i>	[0.510]	[0.186]	[0.956]		[0.535]	[0.656]	[0.508]	
Control mean		-0.69	2.97	0.37	0.03	0.46	4.45	0.90	0.04
N		555	555	555	555	1399	1399	1399	1399
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * p<.10, ** p<.05, *** p<.01 based on two-sided p-values. Standard errors are shown in parentheses and two-sided p-values in brackets. Each column reports results from a separate interaction regression estimated using Equation E.5, with the corresponding coefficient estimates shown in parentheses. Results are reported separately for Independent and Republican respondents. Bolded results correspond to those in Figure 3 in the main text. Index is a pre-registered standardized index combining candidate favorability and vote intention. Favorability is measured on a five-point scale ranging from 1 = very unfavorable to 5 = very favorable. Vote intention is a binary indicator coded 1 = yes and 0 = no. Donate is a behavioral measure pre-registered to capture the effect of the ad on private donation behavior and is presented as a behavioral corollary to the effect of the ad on private preferences. Control means report means in the pure control condition (private condition, no ad treatment). All models include pre-registered covariates and day fixed effects.

Table G.3.2: Ad Effects on **Trump Support** by Public–Private Condition and Respondent Party: Three-Way Interaction

	Trump Index b/se/p	Trump Favorability b/se/p	Trump Vote b/se/p
Ad (Private) (B1)	-0.04 (0.05) [0.498]	-0.03 (0.08) [0.744]	-0.02 (0.02) [0.383]
Ad*Public (Female Enumerator) (B2)	0.11 (0.09) [0.203]	0.15 (0.14) [0.294]	0.05 (0.04) [0.211]
Public (Female Enumerator) (B3)	-0.07 (0.06) [0.253]	-0.11 (0.10) [0.272]	-0.03 (0.03) [0.317]
Ad*Public (Male Enumerator) (B4)	0.06 (0.09) [0.512]	0.10 (0.13) [0.472]	0.02 (0.04) [0.604]
Public (Male Enumerator) (B5)	-0.06 (0.06) [0.320]	-0.07 (0.09) [0.478]	-0.03 (0.03) [0.268]
Independent (B6)	-1.08*** (0.09) [0.000]	-1.42*** (0.14) [0.000]	-0.50*** (0.04) [0.000]
Ad*Independent (B7)	-0.19 (0.12) [0.113]	-0.40** (0.19) [0.035]	-0.06 (0.06) [0.341]
Public (Female Enumerator)*Independent (B8)	-0.22 (0.16) [0.153]	-0.42* (0.24) [0.088]	-0.07 (0.07) [0.319]
Ad*Public (Female Enumerator)*Independent (B9)	0.24 (0.22) [0.284]	0.54 (0.34) [0.120]	0.06 (0.11) [0.600]
Public (Male Enumerator)*Independent (B10)	-0.06 (0.15) [0.670]	-0.14 (0.24) [0.558]	-0.02 (0.07) [0.814]
Ad*Public (Male Enumerator)*Independent (B11)	0.15 (0.22) [0.512]	0.10 (0.34) [0.760]	0.09 (0.11) [0.402]
Constant	0.18	4.23	0.74
N	1954	1954	1954

Notes: Each column reports a regression of Trump support on the ad treatment, public reporting condition, party ID, and their interactions (Equation E.6). The sample includes Republican and Independent women; Republicans and the private condition are the reference categories. The main effect of interest is B9 tests whether this difference in Trump support is larger among Independents than Republicans. Standard errors are in parentheses and two-sided p -values are in brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure G.3.1: Distribution of Trump Support Among Independent Women

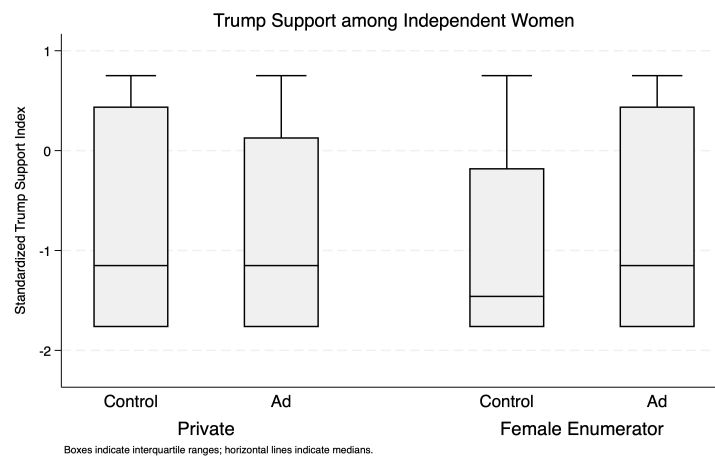


Table G.3.3: Ad Effects on **Harris Support** in Private vs. Public (Female and Male Enumerator) Conditions, By Respondent Party

	Independents					Republicans				
	Index	Favorability	Vote Harris	Vote Harris/3rd	Donate	Index	Favorability	Vote Harris	Vote Harris/3rd	Donate
Panel A: Regression Results										
(1) Ad Effect (Private) (B1)	0.10	0.09	0.05	0.10*	-0.02	0.04	0.08	0.01	0.02	0.00
s.e.	(0.12)	(0.16)	(0.05)	(0.05)	(0.02)	(0.05)	(0.08)	(0.02)	(0.02)	(0.00)
pval	[0.392]	[0.595]	[0.295]	[0.051]	[0.113]	[0.411]	[0.357]	[0.652]	[0.241]	[0.826]
(2) Ad x Public (Female Enumerator) (B2)	-0.04	0.05	-0.05	-0.10		-0.11	-0.15	-0.03	-0.05	
s.e.	(0.24)	(0.31)	(0.10)	(0.10)		(0.09)	(0.13)	(0.03)	(0.03)	
pval	[0.881]	[0.869]	[0.621]	[0.330]		[0.206]	[0.239]	[0.269]	[0.163]	
(3) Public (Female Enumerator) (B3)	0.22	0.11	0.14**	0.13*		0.02	0.03	0.01	0.02	
s.e.	(0.17)	(0.22)	(0.07)	(0.07)		(0.06)	(0.09)	(0.02)	(0.02)	
pval	[0.193]	[0.615]	[0.046]	[0.060]		[0.705]	[0.764]	[0.672]	[0.392]	
(4) Ad x Public (Male Enumerator) (B4)	0.25	0.25	0.11	0.01		-0.05	-0.03	-0.03	-0.03	
s.e.	(0.24)	(0.31)	(0.10)	(0.10)		(0.09)	(0.13)	(0.03)	(0.04)	
pval	[0.301]	[0.415]	[0.260]	[0.940]		[0.547]	[0.803]	[0.305]	[0.347]	
(5) Public (Male Enumerator) (B5)	0.08	-0.07	0.09	0.15**		0.05	0.06	0.02	0.03	
s.e.	(0.16)	(0.21)	(0.07)	(0.07)		(0.06)	(0.09)	(0.02)	(0.02)	
pval	[0.610]	[0.743]	[0.167]	[0.028]		[0.459]	[0.508]	[0.481]	[0.206]	
Panel B: Calculated Effects										
(6) Ad Effect Public (Female Enum) (B1+B2)	0.07	0.14	0.00	0.00		-0.07	-0.08	-0.02	-0.02	
s.e.	(0.21)	(0.27)	(0.09)	(0.09)		(0.07)	(0.10)	(0.02)	(0.03)	
pval	[0.754]	[0.615]	[0.964]	[0.960]		[0.340]	[0.444]	[0.297]	[0.394]	
(7) Ad Effect Public (Male Enum) (B1+B4)	0.36*	0.34	0.17*	0.11		-0.01	0.04	-0.02	-0.01	
s.e.	(0.21)	(0.26)	(0.09)	(0.09)		(0.07)	(0.11)	(0.02)	(0.03)	
pval	[0.095]	[0.200]	[0.058]	[0.197]		[0.895]	[0.697]	[0.343]	[0.750]	
(8) Test: Ad Effect in Female vs. Male Public Conditions (B2-B4)	-0.29	-0.20	-0.16	-0.11		-0.06	-0.12	0.00	-0.01	
s.e.	(0.30)	(0.38)	(0.12)	(0.12)		(0.10)	(0.15)	(0.03)	(0.04)	
pval	[0.336]	[0.598]	[0.184]	[0.382]		[0.570]	[0.420]	[0.959]	[0.724]	
Control mean	0.44	2.77	0.23	0.29	0.03	-0.36	1.48	0.06	0.06	0.00
N	555	555	555	555	555	1399	1399	1399	1399	1399
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * p<.10, ** p<.05, *** p<.01 based on two-sided p-values. Standard errors are shown in parentheses and two-sided p-values in brackets. Each column reports results from a separate interaction regression estimated using Equation E.5, with the corresponding coefficient estimates shown in parentheses. Results are reported separately for Independent and Republican respondents. Index is a pre-registered standardized index combining candidate favorability and vote intention. Favorability is measured on a five-point scale ranging from 1 = very unfavorable to 5 = very favorable. Vote intention is a binary indicator coded 1 = yes and 0 = no. Donate is a behavioral measure pre-registered to capture the effect of the ad on private donation behavior and is presented as a behavioral corollary to the effect of the ad on private preferences. Control means report means in the pure control condition (private condition, no ad treatment). All models include pre-registered covariates and day fixed effects.

H Explaining Ad Effects on Private but not Public Support

H.1 The Effects of Public Expression (Pre-Ad)

Table H.1.1 reports the subgroup regressions underlying Figure 5. Panel A compares expression to female and male virtual enumerators with private expression; Panel B tests the female–male enumerator differences. Results are shown for Independents, Republicans, and the full sample, which also includes weak and leaning Democrats. Table H.1.2 formally tests whether the female-enumerator effect relative to private expression differs between Independents and Republicans for likelihood of voting for Trump and naming reproductive rights as a top issue.

These tests support two features of the pattern described in the main text. Among Independent women, the effects on Trump vote likelihood and reproductive rights differ between the female and male co-partisan enumerators, although the reproductive-rights contrast is less precise. The effect of expression to a female enumerator also differs significantly between Independent and Republican women for both outcomes. Together, the results show that the pre-ad shifts in these responses are concentrated in the Independent female-enumerator condition.

Table H.1.1: Effects of Public Expression to the Female and Male Virtual Enumerators (Pre-Ad)

	Independents				Republicans				Full Sample			
	Vote Trump (1)	Rights (2)	Likely vote (3)	Certain vote (4)	Vote Trump (5)	Rights (6)	Likely vote (7)	Certain vote (8)	Vote Trump (9)	Rights (10)	Likely vote (11)	Certain vote (12)
Panel A: Effects of Public Expression (Pre-Ad)												
Female enumerator (vs. private)	-0.38**	0.10*	0.02	0.22*	0.03	-0.04*	0.00	-0.01	0.01	-0.02	0.05	0.10**
<i>s.e.</i>	(0.17)	(0.05)	(0.16)	(0.12)	(0.06)	(0.03)	(0.07)	(0.05)	(0.08)	(0.02)	(0.07)	(0.05)
<i>pval</i>	[0.028]	[0.057]	[0.892]	[0.072]	[0.591]	[0.083]	[0.957]	[0.845]	[0.864]	[0.479]	[0.507]	[0.047]
Male enumerator (vs. private)	0.04	0.00	0.48***	0.35***	0.00	-0.02	-0.13*	-0.07	0.04	-0.02	0.05	0.07
<i>s.e.</i>	(0.17)	(0.05)	(0.15)	(0.12)	(0.06)	(0.03)	(0.08)	(0.05)	(0.08)	(0.02)	(0.07)	(0.05)
<i>pval</i>	[0.836]	[0.972]	[0.002]	[0.004]	[0.949]	[0.352]	[0.090]	[0.134]	[0.623]	[0.510]	[0.438]	[0.181]
Panel B: Calculated Effects												
Test: Female vs. Male Enumerator Effect	-0.42**	0.10	-0.46**	-0.13	0.03	-0.02	0.13	0.06	-0.02	0.00	-0.01	0.03
<i>s.e.</i>	(0.21)	(0.06)	(0.18)	(0.14)	(0.07)	(0.03)	(0.08)	(0.05)	(0.09)	(0.03)	(0.08)	(0.05)
<i>pval</i>	0.045	0.100	0.012	0.334	0.667	0.475	0.113	0.234	0.789	0.953	0.924	0.552
Control mean	2.83	0.38	3.26	2.67	4.62	0.22	4.38	3.69	3.96	0.28	4.04	3.38
N	556	556	556	556	1405	1405	1405	1405	2056	2056	2056	2056

Notes: * p<.10, ** p<.05, *** p<.01 based on two-sided p-values. Standard errors are shown in parentheses and two-sided p-values in brackets. Full sample includes small group of democrats. "Rights" is coded 1 if reproductive rights were a top three issue and 0 otherwise. Panel B reports a formal test of the difference between the female and male enumerator effects (female minus male).

Table H.1.2: Effects of Public Expression to the Female and Male Virtual Enumerators (Pre-Ad)

	Likely Trump b/se/p	Reproductive Rights b/se/p
Public (Female Enumerator)	0.03 (0.06) [0.584]	-0.04* (0.03) [0.085]
Independent	-1.70*** (0.10) [0.000]	0.12*** (0.03) [0.000]
Public (Female Enumerator)*Independent	-0.42** (0.18) [0.020]	0.14** (0.06) [0.017]
Public (Male Enumerator)	0.01 (0.06) [0.827]	-0.02 (0.03) [0.362]
Public (Male Enumerator)*Independent	0.01 (0.18) [0.953]	0.02 (0.06) [0.665]
Constant	4.22	0.49
N	1961	1961

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Each column reports a regression of a pre-ad outcome on assignment to a female or male virtual enumerator, respondent party ID, and their interactions. Republican women in the private condition are the reference group. The Public (Female Enumerator) $\times$ Independent coefficient tests whether the female-enumerator-versus-private difference is larger or smaller among Independent women than among Republican women. It is statistically significant for both likely Trump vote and reproductive rights, indicating that the effect of public expression to a female enumerator differs by party ID. The corresponding interactions for the male enumerator are not statistically significant. Robust standard errors appear in parentheses and two-sided p -values in brackets.

H.2 The Role of Social Norms

Table H.2.1 reports the results discussed in the main text. Column 1 examines Independent women’s beliefs about Harris support among other Independent women. In the private condition, the ad increased the probability of saying that more than half of Independent women would vote for Harris by 10 percentage points ($p = .054$; Panel A, row 1), suggesting that it shifted perceived ingroup norms. Yet the effect was 23 percentage points smaller when respondents expressed this belief to a female copartisan enumerator ($p = .020$; row 2). As with Trump support, the asymmetry coincides with a shift in expression even without the ad: assignment to the female copartisan enumerator increased the probability of expressing this belief by 19 percentage points relative to private reporting ($p = .005$; Panel A, row 3). Thus, the ad moved privately held beliefs toward a level that women in the no-ad group were already expressing publicly.

The asymmetry depends on the audience. When Independent women expressed their beliefs about Independent women to a male copartisan enumerator, the public–private difference in the ad effect was approximately zero (0.00, $p = .973$; column 1, row 4). The difference in expression to the female versus male virtual enumerator is -0.23 ($p = .056$), estimated as a linear combination of the column 1 coefficients. This indicates that the pattern depends not just on copartisanship but on the gender of the copartisan enumerator.

The asymmetry also depends on the group being assessed. When Independent women expressed their beliefs to the female copartisan enumerator about Republican women, the public–private difference in the ad effect was positive and not statistically distinguishable from zero (0.12, $p = .138$; column 2, row 2), indicating that the pattern is not a general effect of the female enumerator. When they expressed their beliefs to the same enumerator about Independent men, the difference was approximately zero (0.00, $p = .994$; column 3, row 2), indicating that a shared partisan identity with the assessed group is not sufficient.

We formally compare these interactions across outcomes using seemingly unrelated estimation, which accounts for the correlation among responses from the same respondents (see the replication files). The interaction for beliefs about Independent women differs from the interaction for beliefs about Republican women by 35 percentage points ($p = .0004$) and from the interaction for beliefs about Independent men by 23 percentage points ($p = .018$), and a joint test rejects equality of the three interactions ($p = .0016$).

Taken together, the results suggest that public-private differences in expression are most evident when the respondent, audience, and assessed group share both gender and partisanship, consistent with social benefits from expressing these views to a fellow Independent woman.

Table H.2.1: Ad and Enumerator Effects on Perceived Social Norms by Respondent Party

	INDEPENDENT WOMEN SAMPLE			REPUBLICAN WOMEN SAMPLE		
	Perceived Harris Support Among [...]			Perceived Harris Support Among [...]		
	Independent Women	Republican Women	Independent Men (Placebo)	Republican Women	Independent Women	Republican Men (Placebo)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Regression Results						
(1) Ad Effect (Private) (B1)	0.10*	-0.05	0.05	0.01	0.06**	0.01
<i>s.e.</i>	(0.05)	(0.04)	(0.04)	(0.02)	(0.03)	(0.02)
<i>pval</i>	[0.054]	[0.296]	[0.258]	[0.810]	[0.028]	[0.696]
(2) Ad x Public (Female Enumerator) (B2)	-0.23**	0.12	0.00	0.01	-0.06	-0.03
<i>s.e.</i>	(0.10)	(0.08)	(0.08)	(0.04)	(0.05)	(0.04)
<i>pval</i>	[0.020]	[0.138]	[0.994]	[0.776]	[0.201]	[0.348]
(3) Public (Female Enumerator) (B3)	0.19***	-0.03	0.04	-0.03	0.06*	0.01
<i>s.e.</i>	(0.07)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)
<i>pval</i>	[0.005]	[0.602]	[0.423]	[0.270]	[0.072]	[0.743]
(4) Ad x Public (Male Enumerator) (B4)	0.00	0.03	-0.03	-0.02	-0.10*	-0.04
<i>s.e.</i>	(0.10)	(0.08)	(0.08)	(0.04)	(0.05)	(0.04)
<i>pval</i>	[0.973]	[0.720]	[0.686]	[0.626]	[0.052]	[0.302]
(5) Public (Male Enumerator) (B5)	0.07	0.00	0.02	0.02	0.09***	0.03
<i>s.e.</i>	(0.07)	(0.06)	(0.05)	(0.03)	(0.03)	(0.03)
<i>pval</i>	[0.315]	[0.996]	[0.674]	[0.384]	[0.006]	[0.295]
Panel B: Calculated Effects						
(6) Ad Effect Public (Female Enum) (B1+B2)	-0.13	0.08	0.05	0.02	0.00	-0.02
<i>s.e.</i>	(0.08)	(0.07)	(0.07)	(0.03)	(0.04)	(0.03)
<i>pval</i>	[0.131]	[0.281]	[0.487]	[0.569]	[0.972]	[0.379]
(7) Ad Effect Public (Male Enum) (B1+B4)	0.10	-0.02	0.02	-0.01	-0.04	-0.03
<i>s.e.</i>	(0.09)	(0.07)	(0.07)	(0.03)	(0.04)	(0.03)
<i>pval</i>	[0.246]	[0.804]	[0.803]	[0.669]	[0.388]	[0.320]
Control mean	0.25	0.18	0.14	0.10	0.12	0.08
N	555	555	555	1399	1399	1399
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$ based on two-sided p-values. Standard errors are shown in parentheses and two-sided p-values in brackets. Full sample includes small group of democrats. "Rights" is coded 1 if reproductive rights were a top three issue and 0 otherwise.

H.3 Alternative Explanations

We consider additional explanations for the null results on public expression: a generalized privacy message, virtual enumerator distraction, and race effects.

Generalized privacy. We consider one possible explanation for the null results in the public condition: women internalized the ad's general privacy of ballot message and generalized it to all forms of public expression. To address this, we can test whether the campaign ad shifted privacy expectations using a question that was asked only of partnered respondents: "How likely or unlikely is it that people close to you will learn who you vote for?" The ad had no detectable effects on responses to this question (private condition: $b = .02$, $p = .833$; public condition: $b = .11$, $p = .195$, interaction: $b = .09$, $p = .443$), suggesting this alternative explanation is unlikely.

Virtual enumerator distraction. We also check whether the enumerator treatment distracted respondents from the ad treatment, which could have caused the null results in the public condition. If the enumerator caused distraction from the ad, we would expect lower correctness in the enumerator condition(s). The ad treatment check asked respondents how many women appeared in the video. We evaluate this measure in two ways. In the first coding, "I didn't watch a video" is coded as correct for control respondents and "2 women" as correct for treated respondents. Under this coding, correctness appears somewhat lower in the female-enumerator condition, which could suggest distraction. However, in the second coding, we allow "1

woman” to be considered correct for respondents in the ad-control condition or in the female enumerator condition, to account for the possibility that respondents were referring to the single woman shown in the enumerator image rather than the video. Using this coding, which we believe is reasonable, we find no differences in correctness across conditions.

We also find no evidence that the public condition reduced engagement. Respondents assigned to the female enumerator condition spent slightly more time on the survey—0.77 minutes (about 46 seconds) longer on average—but this difference is small and statistically indistinguishable from the private condition ($p = 0.201$). Taken together, these results indicate that distraction from the enumerator is unlikely to explain the null effect on public expression.

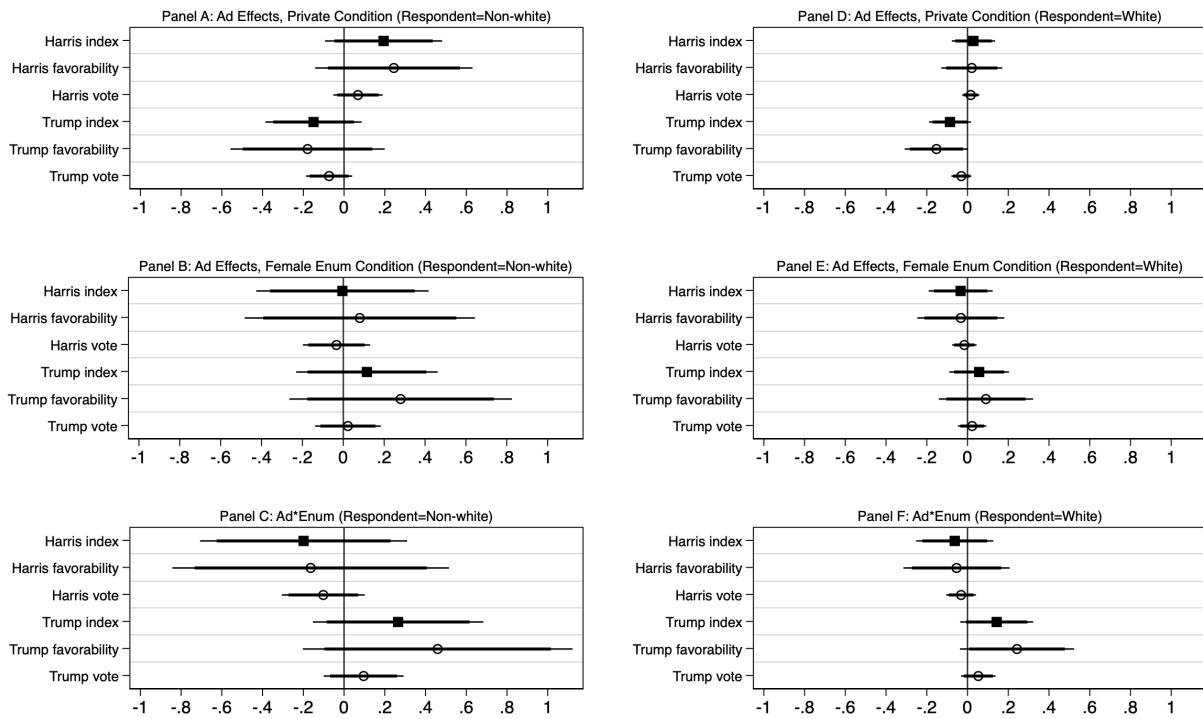
Virtual enumerator race. We also examine whether responses to the virtual female enumerator vary by respondent race. This analysis is important because all virtual enumerators were white. We made this design choice anticipating that our sample—Republican and Independent women—would be predominantly white, which the data confirm. Of 2,049 respondents, 82 percent identify as white (non-Hispanic), 5 percent as Black, 5 percent as Latina, and the remaining 7 percent as other racial or ethnic minorities.

Although the number of non-white respondents is relatively small, this analysis helps assess an alternative interpretation of the null results in public expression. Specifically, if non-white respondents identified less strongly with the white enumerators, weaker treatment effects in the public condition could reflect reduced identification with the messenger rather than a broader divergence between private attitudes and public expression. Conversely, if gender identification dominates racial identification, or if even limited social visibility is sufficient to shape public expression across groups, we would expect similar patterns among white and non-white respondents.

As shown in Figure H.3.1, the results exhibit strikingly similar patterns across racial groups. In the private conditions, both non-white (Panel A) and white (Panel D) respondents show reduced support for Trump in response to the treatment. In contrast, under the public female enumerator condition, coefficients are positive for both non-white (Panel B) and white (Panel E) respondents, yielding a similar interaction pattern across groups. These results suggest that the null results in public expression are unlikely to be driven primarily by the racial identity of the enumerator. If weaker identification with the white enumerator among non-white respondents explained the null public effects, we would expect substantially different interaction patterns across racial groups. Instead, the divergence between private and public outcomes appears in both groups.

The analysis also sheds light on whether the campaign ad itself—featuring two white women—resonated differently across racial groups. If non-white women failed to identify with the characters in the ad, we would expect weaker treatment effects on their private attitudes and behavioral intentions. However, Panels A and D suggest otherwise. The reduction in private support for Trump is similar across white and non-white respondents, and the increase in support for Harris is, if anything, suggestively larger among non-white women. This pattern indicates that the ad’s persuasive effects on private attitudes were not confined to white respondents despite the racial composition of the ad’s protagonists.

Figure H.3.1: Effect of campaign ad in ‘private’ vs. ‘public’ female enumerator conditions by respondent race



I Multiple Hypothesis Correction

This appendix reports multiple hypothesis corrections using sharpened false discovery rate (FDR) q-values following [Anderson \(2008\)](#). Because the manuscript examines several distinct theoretical questions, we apply corrections within conceptually related families of tests rather than across all analyses in the paper.

I.1 Corrections for Primary Campaign Persuasion Results

Table [I.1.1](#) reports corrections for the main analyses of campaign persuasion. The hypotheses concerning ad effects in the private condition were preregistered. The corresponding hypotheses comparing private effects with effects in the public female-enumerator condition were omitted from the pre-analysis plan, although the contrasts follow from the prespecified regression model; these hypotheses were subsequently specified and are treated as exploratory. Panel A reports results for the full sample; Panel B reports exploratory subgroup results for Independent women, among whom the asymmetric effects are most clearly evident. Within each panel, corrections are applied separately to the private ad effects and the public-private differences in ad effects. Each family contains two tests, corresponding to the primary preregistered outcomes: the Trump support index and the Harris support index. The table reports both two-sided tests and one-sided tests in the hypothesized directions, with the latter following the directional-testing convention specified in the pre-analysis plan. The q-values therefore address multiplicity across the two outcomes within each estimand and analysis population, rather than across the full-sample and subgroup analyses jointly.

Table [I.1.2](#) provides a broader robustness check on the private persuasion results among Independent women. It applies corrections across the two primary candidate-support indices, clicks on Trump and Harris donation pages, and intended vote for Harris or a third-party candidate. This family considers the private attitudinal and behavioral findings together while retaining the two-index correction as the primary analysis. Using two-sided tests, the reductions in Trump support and clicks on the Trump donation page, as well as the increase in intended vote for Harris or a third-party candidate, remain significant at the 10 percent FDR level ($q = .093$).

Table I.1.1: Multiple Hypothesis Correction for Private vs. Public (Female Enumerator) Results

	Ad Effect (Private)				Ad*Public (Female Enumerator)			
	two-sided		one-sided		two-sided		one-sided	
	pval	qval	pval	qval	pval	qval	pval	qval
Panel A: Full Sample								
Trump support index	0.044	0.097	0.022	0.047	0.049	0.109	0.025	0.052
Harris support index	0.258	0.149	0.129	0.069	0.347	0.210	0.174	0.095
Panel B Independents Only								
Trump support index	0.049	0.109	0.025	0.053	0.074	0.174	0.037	0.080
Harris support index	0.392	0.244	0.196	0.109	0.881	0.788	0.441	0.283

Notes: Table reports sharpened false discovery rate (FDR) q-values following Anderson (2008) for the manuscript's primary analyses of campaign persuasion. Corrections are applied separately within each estimand (Ad Effect in Private; Ad $\times$ Public Female Enumerator) across the two primary outcome measures: the Trump support index and Harris support index. Panel A reports results for the full sample; Panel B reports exploratory subgroup treatment effects for Independent women. One-sided tests follow the directional-hypothesis testing convention specified in the pre-analysis plan. Directional hypotheses for the Ad Effect (H6–H7) were pre-registered; directional hypotheses for the Ad $\times$ Public interaction ($H8_{fem}$) and ($H9_{fem}$) were specified post-registration and are treated as exploratory.

I.2 Corrections for Effects of Public Expression (Pre-Ad)

Table [I.2.1](#) reports corrections for the exploratory pre-ad audience analyses used to assess the social-benefits interpretation of the campaign persuasion results. These analyses examine whether Independent women's political expression differed between the private condition and the public female- and male-enumerator conditions before exposure to the ad, whether the audience effects differed by enumerator gender, and whether the female-enumerator effects differed between Independent and Republican women. Columns 1–6 report results among Independent women for the female-versus-private, male-versus-private, and female-male comparisons; Columns 7–8 report Independent-Republican differences in female-enumerator effects relative to

Table I.1.2: Multiple Hypothesis Correction for Private Effects (Independents)

	Independents	
	pval	qval
Trump support index	0.049	0.093
Harris support index	0.392	0.165
Trump donation	0.033	0.093
Harris donation	0.113	0.093
Vote Harris/3rd	0.051	0.093

Notes: Table reports two-sided p-values and sharpened false discovery rate (FDR) q-values following Anderson (2008) for private ad effects among Independent women. Corrections are calculated across five outcomes: the Trump support index, Harris support index, clicks on the Trump and Harris donation pages, and intended vote for Harris or a third-party candidate. Donation opportunities were framed as private; this table estimates ad effects among respondents assigned to the private survey condition. This broader correction supplements the primary correction across the two candidate-support indices.

private expression. Corrections are applied separately within each contrast across four outcomes: support for Trump and vote certainty, the two preregistered outcomes in *H1*, and reproductive-rights salience and likelihood of voting, which also contribute to the interpretation developed in the text. All tests are two-sided.

Table I.2.1: Multiple Hypothesis Correction for Effects of Public Expression (Pre-Ad)

	Independents						Ind. vs. Republican	
	Private vs. Public (Female Enumerator)		Private vs. Public (Male Enumerator)		Female vs. Male Enumerator		Private vs. Public (Female Enumerator)	
	pval	qval	pval	qval	pval	qval	pval	qval
Vote Trump	0.028	0.107	0.836	0.946	0.045	0.073	0.020	0.042
Rights	0.057	0.107	0.972	0.946	0.100	0.099	0.017	0.042
Likely Vote	0.892	0.288	0.002	0.009	0.012	0.051	0.812	0.255
Certain Vote	0.072	0.107	0.004	0.009	0.334	0.154	0.064	0.045

Notes: Table reports two-sided p-values and sharpened false discovery rate (FDR) q-values following Anderson (2008) for exploratory analyses of pre-ad audience effects. The first three column pairs report, among Independent women, female-enumerator versus private effects, male-enumerator versus private effects, and female-male enumerator differences. The final column pair reports Independent-Republican differences in female-enumerator versus private effects. Corrections are calculated separately for each of these four contrasts across the four outcomes shown. The q-values therefore address multiplicity within each contrast, rather than across all tests in the table.

We do not apply multiple-hypothesis corrections to all additional analyses reported in the manuscript. Perceived-norm results and other supplementary analyses are presented as exploratory supporting evidence and are not covered by the correction families reported here. We correct separately for campaign persuasion and pre-ad audience effects because they address different substantive questions. The reported q-values should therefore be interpreted as addressing multiplicity within the specified families of tests rather than across the manuscript as a whole.

J Research Ethics and IRB

The research design is consistent with the American Political Science Association’s *Principles and Guidance for Human Subjects Research* and the ethical principles outlined in the Belmont Report, including respect for persons, beneficence, and justice. Participants provided informed consent, were free to withdraw at any time, and were debriefed at the conclusion of the study. The study posed minimal risk, protected participant confidentiality, and compensated participants at standard survey platform rates.

This study was reviewed and approved by the Institutional Review Board at XXX University. All participants provided informed consent prior to participating in the study (see consent script below) and were informed that participation was voluntary and that they could withdraw at any time. Participants were compensated for completing the survey through the Cloud Research Connect platform.

To avoid potential demand effects, the study was described to participants as being conducted by researchers affiliated with the Media Research Project rather than identifying the university affiliation of the investigators. At the conclusion of the survey, participants were debriefed and provided with contact information for the research team at XXX University. Participants were also given the opportunity to withdraw their responses following the debriefing.

The study posed minimal risk to participants. Responses were collected anonymously and stored on secure servers accessible only to the research team. Data were analyzed and reported only in aggregate form, and no information identifying individual respondents is included in the dataset or publications resulting from this research.

Election context. Because the study involved exposure to a political campaign ad during an ongoing election, we considered the potential ethical implications of influencing vote preferences. As mentioned in the main text, the campaign ad used in the study had already been publicly released and widely discussed in national media at the time the study was conducted. Also, given the study’s modest scale and nationwide recruitment, it is unlikely that participation meaningfully influenced electoral outcomes.

Consent script:

Please read the following information closely. At the end you will be asked if you consent to participate.

This study is being conducted by researchers affiliated with The Media Research Project. The goal of the study is to better understand how voters are thinking about the upcoming 2024 presidential election.

Your participation has several benefits. Your responses will help to improve understanding of how people are voting in the 2024 presidential election. You will also receive compensation if you complete the survey.

There are minimal risks to participating. Your participation is completely voluntary. You can choose to withdraw from the study at any time. If you choose to participate, your responses will be kept confidential, combined with those from other participants, and published in the form of generalizations about responses as a whole. All data will be stored in a secure location accessible only to the research team. The research team retains the right to use and publish data that contains no information that would reveal the identity of respondents.

Consent. *I have read this consent form and understand what is being requested of me as a participant in this study. I have been given satisfactory answers to my questions. I certify that I am at least 18 years of age.*

K The Pre-Analysis Plan and Deviations

We pre-registered our study at OSF on October 31, 2024 and updated the PAP on November 3, 2024. The anonymized pre-registration document is available [here](#). The manuscript closely follows the pre-analysis plan (see Appendix E.1); below we discuss deviations between the PAP and the manuscript.

The sample. After the first two days of implementation we revised the PAP to expand the sample beyond Republican women. About 60 percent of the initial Republican sample identified as Strong Republicans, whom we expected to be difficult to move. We also recognized that the ad might be especially relevant to women whose husbands supported Trump but who themselves identified as Independents or weak or leaning

Democrats. We therefore expanded eligibility to all women except Strong Democrats and increased the target sample to 2,000. The revised PAP also required updating some of the hypotheses as predictions varied by party. We thus also stated we would investigate differences by party.

The private vs. public (female enumerator) comparison. The PAP specified H8 and H9 as comparisons between the private condition and the pooled public enumerator conditions, but did not separately identify the private–public female contrasts as hypothesis tests. This omission was an oversight, given that H10 and H11 anticipated differences by enumerator gender: pooling the public conditions can obscure the contrast with the female enumerator. We therefore report the female enumerator contrasts as additional tests of H8 and H9. This is a limited departure from the PAP: the contrasts are coefficients in the preregistered regression (Equation E.5), so reporting them requires no change to the specified model or estimation procedure. We distinguish these additional tests from the explicitly preregistered tests throughout.

Heterogeneous effects by party ID. The original pre-analysis plan anticipated that the ad would have “a bigger effect on women who have weaker support for Trump, are more undecided, and have weaker Republican party ID” (p. 5). After the sample was expanded, the revised plan stated: “Given that we expanded the sample to include weak/leaning Democrats and Independents, we will also look at hetero effects by party ID...” (p. 5). Consistent with that plan, we examine results separately for Independent and Republican women. Independents broadly fit the profile of women with weaker support for Trump and greater uncertainty; we exclude Democrats from the subgroup analysis because of their small sample size. Our primary interest is in identifying which group drives the overall pattern of results and how the ad’s effects differ between private and public reporting within each group. We therefore place greater interpretive weight on these within-group patterns than on formal comparisons between Independents and Republicans, although we report the latter as well.