Shy Women Voters:
Private Preferences and Public Expression in the 2024 U.S. Presidential Election

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Abstract

Can appeals targeted to women shift their voting preferences across party lines? And can such appeals increase women's willingness to publicly express those preferences? Despite the widespread use of targeted appeals to women in political campaigns, their effectiveness in changing private attitudes remains uncertain—and even less is known about their effect on public expression, which is central to broader political mobilization. Using experiments embedded in an online survey of 2,049 women conducted in the final days of the 2024 presidential election, we evaluate the effects of a real campaign ad targeting Republican and Independent women. The ad reduced private support for Trump. However, by randomly varying whether respondents reported their preferences privately or to a co-partisan "virtual enumerator", we find that this effect did not extend to public expression. These results demonstrate that even when persuasive appeals shift private preferences, social pressures can inhibit public expression, limiting opportunities for political momentum.

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This paper investigates two related questions: Can campaign appeals targeted to women persuade them to shift their vote preferences? And can such appeals increase women's willingness to express those preferences publicly? These questions are critical because, although campaigns routinely target women, there is limited evidence on whether such appeals change women's private political attitudes—and even less on whether they encourage public expression, which is central to broader political mobilization.

Political campaigns increasingly "micro-target" appeals to women and other social groups, yet whether—and under what conditions—such appeals shift preferences remains contested (Erfort, 2023; Hersh and Schaffner, 2013). Group-targeted appeals may emphasize shared identity or solidarity, highlight issues presumed to be salient to the group, or provide cues about what other group members are likely to do—an important consideration when individuals are embedded in networks that favor the opposing candidate, where even privately dissenting can carry psychological costs (White, Laird and Allen, 2014; Afifi et al., 2020).

Importantly, while group-targeted appeals are prevalent, the evidence of their persuasive effects among women is mixed. Some research shows that appeals from female candidates directed at women voters can prime gender identity and increase support (Holman, Schneider and Pondel, 2015). Others emphasize the limits of gender-based mobilization, noting that women are not a monolithic political bloc and that other identities—e.g. based on race, ethnicity, class, or partisanship—often take precedence (Beckwith, 2005; Htun, 2004; Huddy, Cassese and Lizotte, 2008; Wineinger, 2025). Consistent with this view, targeted appeals to women can backfire when they heighten partisan divisions (Klar, 2018), and women rarely cross party lines to support female candidates in U.S. elections (Junn and Masuoka, 2024).

Even less is known about whether such appeals influence public expression—the outward display of one's attitudes or preferences. Public expression is plausibly harder to shift than private preferences because voicing dissent can entail real or perceived social costs within one's social network or partisan community. Yet public expression is central to theories of social signaling and social change (Granovetter, 1973; Hale and Colton, 2017; Lohmann, 1994; de Mesquita,

Shadmehr et al., 2023). Because public expression—e.g. voicing an opinion in conversation or via sharing on social media—is observable, it conveys social information about prevailing norms. When individuals publicly express new or dissenting attitudes, they can reveal emerging shifts in opinion and lower the perceived costs of dissent for others. Conversely, preference falsification—hiding or misrepresenting updated views—sustains misperceived norms and dampens political momentum (Kuran, 1995; Bursztyn, González and Yanagizawa-Drott, 2020; Bursztyn, Egorov and Fiorin, 2020). As a result, even substantial private attitude change may fail to translate into broader social or political change absent visible public expression.

We examine both persuasion and public expression by studying exposure to a real-world campaign ad from the 2024 U.S. presidential election in a pre-registered study (see Appendix M). The ad—produced, paid for, and released by Vote Common Good, and narrated by Julia Roberts—depicted two women deciding against voting for Trump and instead voting for Harris, signaling this choice to each other while concealing it from their spouses (Vote Common Good, 2025a,b). It aired in the campaign's final week amid speculation that Republican and Independent women disillusioned with Trump might quietly defect to Harris (ABC News, 2024b). These women were often described as "shy voters"—individuals who privately preferred Harris or opposed Trump but hesitated to act on these preferences due to the psychological or social costs of deviating from partners or partisan communities (NPR, 2024). The strong reaction 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 Vote Common Good ad exemplifies a campaign appeal designed to shift both private candidate preferences and perceived social norms, potentially encouraging women to express changed views publicly to other women. The ad plausibly influenced private preferences by emphasizing the personal stakes of the election—a common persuasive tactic in political advertising (Hewitt et al., 2024)—while also affirming the secrecy of the ballot, which could reduce the psychological discomfort associated with private dissent (Afifi et al., 2020). Additionally, by depicting other Republican and Independent women defecting from Trump, the ad may have

lowered the psychological costs of deviating from group norms, which could inhibit even private attitude change. (Bicchieri, 2005; Cialdini, Reno and Kallgren, 1990; White, Laird and Allen, 2014).

The ad also plausibly shifted public expression *to other women*. While it encouraged preference falsification vis-à-vis partners by emphasizing that vote choices could remain secret—including from Trump-supporting husbands—it simultaneously portrayed women defecting from Trump and *signaling this choice to one another*. In doing so, the ad suggested that such behavior was neither rare nor stigmatized but part of a broader trend among Republican and Independent women. This signal may have shifted perceived social norms among co-partisan women, making public expressions of support for Harris to other women feel safer and more legitimate, thereby potentially amplifying the ad's effects through social signaling (Lohmann, 1994; Bursztyn, Egorov and Fiorin, 2020).

To test the effects of the Vote Common Good ad on women's private and public preferences, we embedded an experiment in an online survey conducted with 2,049 primarily Republican and Independent women voters in the final days before the U.S. presidential election. We randomly assigned respondents to a treatment group that watched the ad and a pure control group that did not.[1]

To distinguish the ad's effects on private preferences from its effects on public expression, we cross-randomized exposure to the campaign ad with a virtual enumerator experiment that varied whether respondents reported their preferences privately or to a co-partisan female (or male) enumerator. This design allows us to compare private reporting to a context that primes public expression to a co-partisan woman (see details in the Research Design section).

We find that the campaign ad significantly reduced private support for Trump. Unexpectedly, this shift did not translate into greater support for Harris; instead it increased support

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[1]The 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 small scale and nationwide recruitment, it is unlikely that participation influenced electoral outcomes (Slough, 2024).

for third-party candidates. Moreover, while the ad influenced private support for Trump, exploratory analysis shows that it did *not* make women more willing to express reduced support to other women, as captured by responses to a virtual female co-partisan enumerator. These findings suggest that while targeted appeals can shift women's private support for candidates, leveraging these changes to trigger broader social cascades may be significantly constrained.

Research Design

We recruited 2,056 women—of whom 2,049 passed an attention check and completed data collection—through Cloud Research's Connect platform between October 31 and November 3, 2024, in the final days before the U.S. presidential election. Participants were eligible if they were U.S. citizens, of voting age, identified as women, and had not yet voted. Recruitment focused on Republican women during the first two days of fielding and expanded to include Independent and weak or leaning Democratic women during the final two days, reflecting the intended target audience of the campaign ad.[2] Overall, 37 percent of respondents identified as strong Republicans, 32 percent as weak or leaning Republicans, 27 percent as Independent, and 5 percent as weak or leaning Democrats. Roughly 20 percent of respondents reported some uncertainty about their vote choice, suggesting meaningful scope for persuasion. Additional details on recruitment and sample characteristics are provided in Appendix B.

In the **campaign ad experiment**, those assigned to treatment watched the 30 second Vote Common Good ad and completed a few reaction questions before moving on to the post-ad survey. Those assigned to the pure control group did not watch a video and proceeded directly to the post-ad survey. By using an actual ad, the design trades some experimental control over specific elements (such as message content, narrator, visuals, and tone) for ecological validity—an approach common in studies that evaluate real political advertisements (e.g. Clinton and Lapinski, 2004; Kalla and Broockman, 2018; Coppock, Hill and Vavreck, 2020; Hewitt et al., 2024). We accept this tradeoff because our goal is not to isolate which specific component

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[2] We updated our pre-analysis plan to reflect this change (see Appendix L).

drives persuasion but rather to provide a proof-of-concept test of whether private attitude change occurs and translates into public expression, as captured by the virtual enumerator experiment.

In the **virtual enumerator experiment**, respondents assigned to the *private* condition completed the survey using a standard online format. Those assigned to the *public* condition were primed to think they were reporting responses to a virtual female (or male) co-partisan enumerator.[3] The prime took the form of an image of a female or male enumerator who (using chat bubbles) introduced themselves at the beginning of the survey; to maintain treatment salience, the image appeared above almost all subsequent survey questions (see Appendix C for more design details).

This design leverages differences in survey mode—where responses in enumerator-administered surveys are generally more susceptible to social expectations (Blair, Coppock and Moor, 2020)—to determine if the treatment induced differential reporting in private versus to a co-partisan woman in general. The approach builds on prior efforts to detect the presence of shy Trump voters in the 2016 election (Pew Research Center, 2017), as well as on evidence that the gender of an enumerator can affect responses (Benstead, 2014); that private and publicly expressed political opinions on surveys and in focus groups often diverge (Valentim, 2024; Lindsey, 2023); and that individuals respond similarly to virtual and in-person enumerators (Krysan and Couper, 2003). We return in the conclusion to discussing how these effects might generalize to expression within respondents' actual social network.[4]

Overall, randomization succeeded in obtaining balance across treatment and control (see

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[3]Respondents in the public condition were further assigned with equal probability to one of three female or three male virtual enumerators. All enumerators were white, reflecting the anticipated racial composition of the sample; Appendix I.2 shows that responses did not differ systematically by race.

[4]We did not manipulate expression to real network members because doing so would have required revealing respondents' survey answers, raising ethical and practical concerns.

Appendix D). Furthermore, treatment checks reported in Appendix E indicate that respondents successfully registered the treatments as intended, and that there is no differential attrition due to treatment assignment in our sample.

Our 2×2 factorial design allows us to estimate the effect of the ad on private preferences (in the private condition), on public expression (in the public condition), and whether these effects differ. This comparison distinguishes among three theoretically meaningful patterns: (1) larger effects on private preferences than on public expression, consistent with social barriers to political momentum;[5] (2) similar effects across conditions, suggesting favorable conditions for social momentum; or (3) larger effects on public expression than on private preferences, consistent with shifting social norms even in the absence of individual persuasion. In what follows, we focus on an exploratory comparison between the private condition and the public *female enumerator* condition, which best aligns with the ad's signal of gendered solidarity among women.[6]

We examine the effects of the campaign ad on two primary measures of candidate support—candidate favorability and intended vote choice—which we analyze both separately and as pre-registered candidate-specific indices. We estimate treatment effects using regression specifications from our pre-analysis plan (see Appendix G).[7]

Results

Our main results are reported in Figure 1 (and Appendix Table I.1), which shows the effect of the ad on private support (Panel A), public expression (Panel B), and the difference between

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[5]This corresponds to our pre-registered hypothesis; see Appendix G.

[6]While we pre-registered comparisons pooling male and female enumerators, we focus on the female enumerator condition here; see Appendix L.

[7]We also collected a behavioral measure of willingness to donate to a candidate's campaign, but only five percent of respondents donated, limiting statistical power. For details on outcome construction, see Appendix F; for the full set of pre-registered results, see Appendix H.
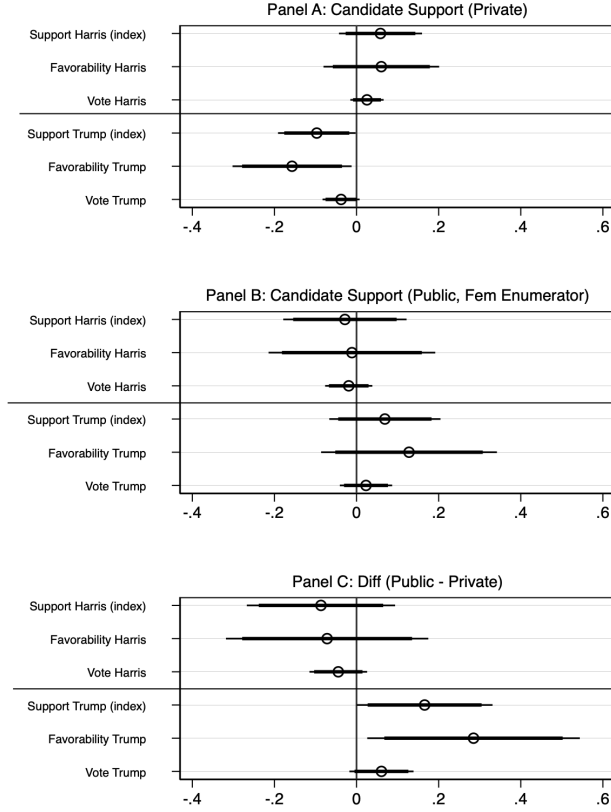
the two (Panel C). We find that the campaign ad caused a noticeable decline in private support for Trump: a .10 standard deviation decrease in the overall index of Trump support ($p = .044$). This effect is driven by a .16 unit decrease in favorability ($p = .033$) and a four percentage point reduction in likelihood of voting for Trump ($p = .102$). The decline in private support for Trump was driven primarily by Independents (Appendix H), consistent with evidence that Independents may be more "persuadable" than self-identifying partisans (Klar and Krupnikov, 2016).

Contrary to expectations, the decline in Trump support did not translate into a significant increase in support for Harris. As shown in Panel A, effects on Harris support are directionally positive but not statistically significant: the treatment produced a 0.06 standard deviation increase in the two-variable index of Harris support ($p = .256$), including a three percentage point rise in willingness to vote for Harris ($p = .217$). Appendix H suggests that this muted effect is partly due to a three percentage point increase in support for third-party candidates, while Appendix I shows no corresponding decline in respondents' likelihood of voting. Taken together, these results indicate that the ad reduced support for Trump and increased support for non-Harris alternatives.

Yet, the results in Panel B show that the ad's effect on Trump support disappears entirely in the public condition. If anything, the coefficients are slightly positive, suggesting the ad may have increased respondents' willingness to express greater support for Trump to co-partisan women (see Appendix Table I.1). As shown in Panel C, this divergence produces a significant gap between private and public support for Trump, captured by statistically significant interaction effects. Specifically, the ad generated a 0.17 standard deviation difference in the Trump support index ($p = .049$), driven by a 0.28 point difference in favorability ratings ($p = .031$) and a six percentage point difference in vote intention ($p = .128$) between the private and public conditions.

These results—a reduction in private but not public support for Trump—raise the question of why the ad failed to influence public expression among women. One possibility is that

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Figure 1: Private Support vs. Public Expression

Panel A: Candidate Support (Private)

Panel B: Candidate Support (Public, Fem Enumerator)

Panel C: Diff (Public - Private)

Notes: Figure shows the effects of the campaign ad treatment on support for Harris and Trump in the private condition versus the public (female enumerator) condition. The support indices exclude the donation outcome. Confidence intervals reflect two-sided tests at the 90% and 95% confidence levels.

women internalized the ad's privacy message—namely that vote choices could be hidden from one's partner—and generalized it to public expression more broadly. Yet, among partnered respondents, we find no evidence that exposure to the ad affected expectations of privacy, suggesting this mechanism is unlikely (see Appendix J).

Another possibility is that the ad did not sufficiently shift perceived social norms to meaningfully reduce the social costs of public expression. However, we find that the ad increased the perceived proportion of women voting for Harris, indicating some updating of perceived copartisan norms, particularly among Independents (see Appendix J). Nevertheless, these shifts were limited: among Independent women in the control group (private condition), only 25 percent believed that a majority of Independent women would vote for Harris; exposure to the ad raised this figure to 35 percent, but most respondents still perceived Harris support as a

minority position. Consequently, Independent respondents likely continued to assume that a typical co-partisan woman—as represented by the virtual female enumerator—did not support Harris, sustaining high perceived social costs of public expression. That these patterns are most pronounced among Independent women is notable; they suggest that norm perceptions among Independents may be more diffuse and responsive to social cues (see Appendix J) and are consistent with evidence that Independent voters might be reluctant to engage in publicly partisan behavior despite privately updated preferences (Klar and Krupnikov, 2016).

Discussion

This study contributes to research on political persuasion, women's political behavior, and the social dynamics of public expression. We provide new evidence that a real-world campaign ad targeted at women can shift private candidate support, even in the final days of a highly polarized election. While it is difficult to generalize from a single ad or to isolate which components drove persuasion, estimating the impact of an actual campaign ad offers ecological validity that researcher-designed treatments outside election contexts often lack (Hewitt et al., 2024). The fact that the ad moved private attitudes at all is notable given skepticism about the effectiveness of cross-partisan appeals targeting women (Klar, 2018; Junn and Masuoka, 2024) and evidence that persuasion effects are typically weakest close to Election Day (Broockman and Kalla, 2023). In this case, persuasion may have been facilitated by the broader electoral context (Hewitt et al., 2024): given the heightened salience of gender in the 2024 election, cross-pressured women may have been unusually receptive to such appeals.

The paper's primary contribution, however, is to offer a proof-of-concept test of whether targeted messages that shift private attitudes also translate into public expression. Although the ad depicted women supporting Harris and signaling that support to one another, we find that respondents updated their private attitudes without revising what they were willing to express to another co-partisan woman, indicating preference falsification *even among women.* These findings underscore the importance of treating private persuasion and public expression

as distinct outcomes and highlight the difficulty of eliciting visible expressions of dissent within ingroups, even when an ad conveys an explicit social message. This challenge is particularly relevant in contexts where campaigns rely on social sharing for amplification but where the social costs of speaking out remain high.

The virtual enumerator experiment captures public expression to a stranger representing a modal co-partisan, raising questions about generalizability to expression to women within an individual's actual social network. Expression to network members likely carries higher reputational stakes, and social costs may vary substantially across networks characterized by different levels of trust and conformity. That the ad failed to shift public expression even in this relatively low-stakes context underscores how difficult it may be to generate the visible signals necessary for social norm change and broader social cascades.

What do these findings imply for voting behavior? While survey measures of candidate support and vote intent may not map directly onto real-world voting, our results offer insights into how social concerns may shape vote choice when individuals do not view voting as a private act. Prior research shows that many voters believe others will learn how they voted, regardless of voting mode (Dowling et al., 2019; Gerber et al., 2013). Consistent with this, nearly 75 percent of partnered respondents in our sample believed that people close to them would find out how they voted, and exposure to the ad—which emphasized ballot secrecy—did not alter this perception. Persuasion strategies that shift private preferences may therefore fail to translate into changed voting behavior when perceived social costs of expression remain high.

Although we focus on shy *women* voters, the framework developed here may generalize to other cross-pressured voters—within the United States and elsewhere—who face social costs to publicly expressing their views. In addition, while we do not study men directly, the public backlash to the ad among prominent conservative men (ABC News, 2024a) highlights the potential for offsetting effects and unintended consequences of targeted appeals among non-target groups. Examining these dynamics in future work would help clarify the broader political impact of such appeals.

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