

# Threats of Violence and Political Ambition: Experimental Evidence

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## Abstract

Politicians and candidates for political office are increasingly subject to threats, harassment and outright violence. Does anticipated exposure to such violence affect political ambition? I present evidence from an original survey fielded with a large sample of US citizens ( $n = 4582$ ) to document that concerns about exposure to violent speech, harassment and physical violence are salient, that Americans on average overestimate the risk of such exposure relative to a scholarly benchmark, and that overestimation is particularly pronounced among women, queer and nonwhite citizens. I then estimate the effect of a randomized intervention which exposes individuals to corrective aggregate information about the risk of facing violence when in office. Downward-correcting aggregate information has a small positive effect on individual willingness to run for office, and a large positive effect on the willingness to support a hypothetical peer running for office. The increase in support for peer ambition is nearly twice as large for hypothetical female peers compared to hypothetical male peers. The intervention provided effective reassurance that women, queer and nonwhite citizens can safely pursue careers in politics.

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*"We cannot ask people to serve in public life if the cost is risking the safety of their families and those they love."*

– Nancy Pelosi, *The Art of Power*, 2024, p. 33.

## 1 Introduction

Getting involved in politics and running for office requires a thick skin. By virtue of their prominent position in society and sometimes significant media exposure, candidates for office and office holders face intense scrutiny, hostility and sometimes outright violence from the publics they serve. While toxicity and violence have historically been part of democratic life, a string of recent high-profile attacks or attempted attacks on senior politicians has reinvigorated interest in violence against elected officials. Extremist attacks on Steve Scalise (R, LA)<sup>1</sup> and Paul Pelosi<sup>2</sup>, husband of former Speaker of the House Nancy Pelosi (D, CA), the attempted kidnapping of Michigan governor Gretchen Whitmer (D, MI)<sup>3</sup>, as well as the assassinations of former Japanese prime minister Shinzo Abe<sup>4</sup>, British MPs Jo Cox<sup>5</sup> and David Amess<sup>6</sup>, and prominent Christian Democrat Walter Lübcke<sup>7</sup> in Germany stand out among many recent examples of violence against politicians in mature democracies. Senior, professional politicians are not alone in being targeted: In both the United States<sup>8</sup>

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1. Williams, Moe and Ortiz. "Congressman Steve Scalise, Three Others Shot at Alexandria, Virginia, Baseball Field", *NBC News*, Sep 15, 2017. URL: <https://www.nbcnews.com/news/us-news/congressman-steve-scalise-shot-alexandria-virginia-park-n772111>, accessed Jan 17, 2024.

2. Arango and Secon. "Paul Pelosi Describes the Night He Was Attacked at Home", *New York Times*, Nov 13, 2023. URL: <https://www.nytimes.com/2023/11/13/us/paul-pelosi-david-depape-trial.html>, accessed Jan 17, 2024.

3. Bogel-Burroughs, Dewan and Gray. "F.B.I. Says Michigan Anti-Government Group Plotted to Kidnap Gov. Gretchen Whitmer", *New York Times*, Oct 8, 2020. URL: <https://www.nytimes.com/2020/10/08/us/gretchen-whitmer-michigan-militia.html>, accessed Jan 17, 2024.

4. Slodkowski and Inagaki. "Japan's former prime minister Shinzo Abe shot and killed", *Financial Times*, Jul 8, 2022. URL: <https://www.ft.com/content/de7116d5-f68b-46c9-9681-368f97f7ad15>, accessed Jan 17, 2024.

5. Cobain and Taylor. "Far-right terrorist Thomas Mair jailed for life for Jo Cox murder", *The Guardian*, Nov 23, 2016. URL: <https://www.theguardian.com/uk-news/2016/nov/23/thomas-mair-found-guilty-of-jo-cox-murder>, accessed Jan 17, 2024.

6. Croft. "'Radicalised Islamist terrorist' convicted of murdering Tory MP David Amess", *Financial Times*, Apr 11, 2022. URL: <https://www.ft.com/content/a71e5ea5-80a9-4d27-b4d0-ccba02969ca2>, accessed Jan 17, 2024.

7. Oltermann. "German neo-Nazi jailed for murder of pro-immigration politician", *The Guardian*, Jan 28, 2021. URL: <https://www.theguardian.com/world/2021/jan/28/german-neo-nazi-jailed-for-murder-of-pro-immigration-politician-walter-lubcke>, accessed Jan 17, 2024.

8. Borter, Ax, Tanfani. "School boards get death threats amid rage over race, gender, mask policies", *Reuters*, Feb 15, 2022. URL <https://www.reuters.com/investigates/special-report/usa-education-threats/>, accessed Jan 17, 2024.

and Germany<sup>9</sup>, local politicians have sounded the alarm as they face harassment, threats and physical violence.

Scholars have accumulated systematic evidence to demonstrate that violence against politicians is widespread even in mature democracies. In a large-scale survey of municipal politicians in Sweden conducted from 2012 to 2016, 25% of respondents reported experiencing either physical or psychological violence in any given year (Håkansson 2021, 527). Elite surveys conducted in the United States confirm that a substantial share of politicians is subject to psychological or physical violence (Thomas et al. 2019; Herrick and Thomas 2021a, 2021b). Recent evidence indicates that violence is on the rise: Between 2017 and 2021, the share of mayors of large U.S. cities reporting experiences of psychological violence has increased from 72.7% to 95.7%, while reports of physical violence have increased from 9.3% to 13.9% (Herrick and Thomas 2023, 86). Moreover, a growing body of evidence suggests that citizens from historically marginalized groups, such as women or ethnic minorities, experience particularly intense hostility when serving in office (Herrick et al. 2021; Håkansson 2021; Herrick and Thomas 2022; Daniele, Dipoppa, and Pulejo 2023) or during everyday campaign activities such as canvassing (Yan and Bernhard 2023).

In light of these findings, scholars and observers have expressed concerns that violence may dissuade citizens from running for office and thus degrade the quality of democratic representation. Recent gains in the representation of women and ethnic or sexual minorities are thought to be particularly threatened by violent backlash. This paper presents novel survey evidence to examine how concerns about exposure to violence when running for or serving in elected office shape political ambition among citizens and downstream representation. First, I explore to what extent concerns about violence and violent threats are *salient* to citizens as they reflect on the possibility of getting involved in politics, and how they perceive the safety risks faced by those serving in office. I find that a majority of respondents report being concerned about the safety risks that come with political careers, and that only financial considerations and concerns about privacy loss are reported to be more salient than safety. I then zoom in on one particular type of violence politicians face: death threats to themselves or their families. On average, respondents *overestimate* the risk of facing such threats when in office. Relative to a benchmark estimate derived from elite surveys, more than 3 in 4 respondents overestimate the risk of office-holders facing serious threats of violence. I also show that both concerns about personal safety and overestimation of safety risks are concentrated among women, queer and nonwhite citizens.

To understand how shifting risk perceptions *causally* affect who runs for office, I embedded a pre-registered<sup>10</sup> information provision experiment in my survey. The intervention

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9. Groll. "Hass gegen Politiker: Bedroht zu werden, gehört zum Mandat", *Die Zeit Online*, 25 Jun 2019. URL: <https://www.zeit.de/gesellschaft/zeitgeschehen/2019-06/hass-politiker-kommunalpolitik-rechte-gewalt-uebergrieffe-umfrage>, accessed Jan 17, 2024.

10. The Pre-Registration Plan is available here: <https://osf.io/f3pgd>. This paper tests the basic directional hypothesis as well as the historically marginalized groups hypotheses and network support hypothe-

allows me to estimate the causal effect of receiving downward-correcting aggregate information on the risk of facing violent threats – learning that such threats are less common than initially assumed – on two outcomes of interest: Personal political ambition, and the willingness to support peers in their political ambitions. To evaluate if support for peers varies with peer gender, I cross-randomize the gender identity of a hypothetical peer with my information intervention.

I find that receiving downward-corrective information has a small positive effect on individual willingness to run, and a large positive effect on the willingness to support politically ambitious peers in their desire to run for office. This peer support effect is twice as large for female compared to male peers. Further examination of auxiliary outcomes suggests that the intervention was successful in persuading respondents that women, queer and nonwhite citizens could safely pursue careers in politics.

## 2 Contribution

My paper contributes to scholarship on the effects of violence on political selection, the representation of women and marginalized groups among political elites, and the effects of repression on political participation. Recently, scholars have taken great interest in better understanding the effects of violence against politicians in democracies. Existing work on this issue has made great progress in conceptualizing violence against politicians – and in particular women in politics – as a distinct form of political violence (Krook 2020; Krook and Restrepo Sanín 2020). A wealth of descriptive work has provided evidence on important correlates of exposure to violence (Bjarnegård, Håkansson, and Zetterberg 2020; Håkansson 2021; Herrick et al. 2021; Thomas et al. 2019; Collignon and Rüdiger 2020). Additionally, scholars have examined how exposure to violence affects politician behavior and perceptions, ranging from the willingness to interact with media outlets to the discussion of particularly sensitive policy issues (Håkansson 2023), exit decisions and feelings of personal agency (Erikson, Håkansson, and Josefsson 2023; Pulejo and Querubin 2023). Public opinion scholars have examined how violence against politicians affects public opinion and voter preferences (Krakowski, Morales, and Sandu 2022). Finally, existing scholarship that explicitly focuses on how violence against politicians affects entry into politics has studied how violence perpetrated by organized criminal groups affects various aspects of political selection, employing observational research designs (Daniele and Dipoppa 2017; Daniele 2019; Alesina, Piccolo, and Pinotti 2019; Dal Bò, Dal Bò, and Tella 2006).

I innovate with respect to this body of scholarship in three important ways. First, to my knowledge, my paper is the first to provide systematic quantitative evidence on citizen *perceptions* of safety risks faced by politicians. Information on perceptions matters as prior

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ses. The other results are reported in a separate paper. See Appendix D for more information on the PAP.

research has documented that perceptions of violent crime – both in terms of levels and trends – often diverge dramatically from fundamentals, likely due to disproportionate media attention and the particularly high salience of violence (Romer, Jamieson, and Aday 2003; Velásquez et al. 2020).

Second, my paper is the first to use an experimental design to pin down the causal effects of information on safety risks to politicians on willingness to run for office, overcoming the limitations inherent in scholarship using observational research designs. Third, successful and sustainable political careers – in particular by ordinary citizens – are contingent on supportive peer and family networks. My survey is the first to measure such network support directly. I demonstrate that network support for politically ambitious peers responds strongly to information about the safety risks of political engagement, particularly when the peer in question is a woman. My paper thus clarifies that fear of exposure to violence can effect representation directly, by lowering individual political ambition, or indirectly, by reducing network support for political candidacies.

By interrogating how political violence affects marginalized groups, my paper contributes to scholarship examining the causes of persistent underrepresentation of women (Lawless 2015), queer citizens (Magni and Reynolds 2021) and black and minority ethnic citizens (Dancygier et al. 2021) among politicians and candidates for office in the United States and other mature democracies. Underrepresentation is consequential: A large body of work has shown that various dimensions of politician identity causally affect policy (Chatopadhyay and Duflo 2004; Carnes 2013; Hyytinen et al. 2017; Gulzar, Haas, and Pasquale 2020) and social norms (Kuipers 2020). The underrepresentation of women is particularly puzzling, since a wealth of experimental evidence suggests that on average, voters in the United States prefer female to male candidates for office (Schwarz and Coppock 2020, for a meta-analysis). Scholars have provided compelling evidence that women and other marginalized groups face numerous obstacles on the path to elected office, ranging from hostile party gatekeepers (Broockman 2014; Dancygier et al. 2015; Butler and Preece 2016) to fear of media scrutiny (Sutter 2006; Wagner 2021), competition aversion (Kanthak and Woon 2015; Dynes et al. 2021) and material constraints (Carnes 2018; Bernhard, Shames, and Teele 2021).

My work extends this scholarship in two important ways: First, I show that women, queer citizens and black and minority ethnic citizens perceive the safety risks of engaging in politics as more severe than their male, straight and white peers. Interventions that *demonstrably* curb the risk of exposure to violence in politics have the potential to lead to broad gains in representation for a variety of different social groups. Perceptibly hostile and violent political environments – such as the contemporary United States (Kleinfeld 2021; Kalmoe and Mason 2022; Zeitzoff 2023) – are unlikely to achieve broad representation of various social groups, undermining a key tenet of representative democracy. Second, my findings imply that the representation of women is particularly adversely affected by con-

cerns about violence against office-holders. Prior research has compellingly demonstrated that the gendered distribution of care work in the United States and other mature democracies renders politically ambitious women particularly dependent on the support of family and peer networks (Rosenbluth, Kalla, and Teele 2018). I demonstrate that as the perceived risk of violence increases, respondents withdraw support from their politically ambitious peers. I estimate this network effect to be twice as large for female compared to male peers.

Finally, a large body of scholarship in political science has examined how individual political participation and activism respond to repression and violence (Wood 2003; Young 2019; Aytac and Stokes 2019; Cantoni et al. 2022). My paper speaks to recent work that has identified group-level legacies of violent repression as an important determinant of responses to the threat of political violence (Rozenas and Zhukov 2019; Walden and Zhukov 2020). This line of work has focused on documenting how violent events give rise to intergroup variation in political behaviour in the long run – however, less attention has been paid to the individual-level mechanisms through which histories of repression affect present-day political participation (Davenport et al. 2019, 374). My paper makes an attempt to clarify how exactly legacies of violent repression shape how marginalized individuals and their social environment reflect on political participation. First, marginalization affects how individuals acquire information about risks of violence: Individuals who identify with marginalized groups are more likely to overestimate safety risks of political engagement, thus depressing their personal willingness to run for office. Second, a widespread perception that political careers are laden with safety risks reduces social support for the candidacies *particularly* of marginalized individuals.

The remainder of this paper proceeds as follows. The next section introduces my survey instrument and describes the data used in this paper. Section three investigates whether or not safety risks are salient to Americans as they reflect on running for office, and how safety risks are perceived. The next two sections discuss my information intervention. I conclude by discussing implications for policy and avenues for further research.

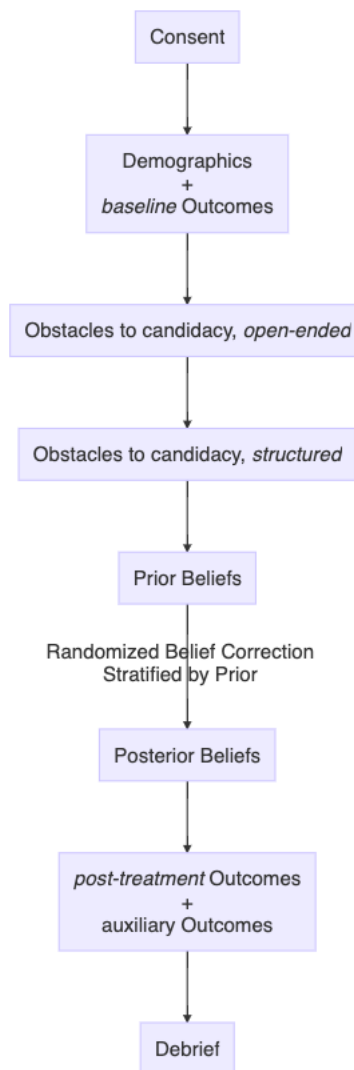
### 3 Data

How salient are concerns about safety to Americans as they reflect on the possibility of running for elected office? How do they perceive safety risks, and do risk perceptions vary systematically across social groups? Does exposure to information about safety risks affect how Americans think about running for office?

To answer these questions, I developed an original survey instrument. The first part of the survey leverages open-text responses and theoretically motivated structured survey questions to gauge how *salient* the risk of exposure to violence is for respondents as they reflect on the possibility of running for office. I then elicit more specific information on how respondents perceive the risk of facing violence when running for office. To do so, I

focus on a specific form of aggression: Death threats against politicians or their families. Finally, I embed an experimental component in my survey that allows me to pin down the causal effect of changing information on the risk of exposure to violence on my outcomes of interest.

The survey consists of nine modules, shown in Figure 1.



**Figure 1:** Survey Flow

After obtaining consent from respondents, the survey first elicits information on respon-

dent demographics. I then elicit information on salience of safety concerns to respondents as well as their risk perceptions. I first ask respondents to spontaneously reflect on why they have not run for political office in an open-ended survey question.<sup>11</sup> The question is designed to elicit "first-order" concerns of respondents as they reflect on the possibility of serving in public office (Ferrario and Stantcheva 2022). It appeared near the beginning of the survey flow to avoid priming respondents to think of particular aspects of running for office. After the open-ended module, I introduce a structured survey module to elicit which obstacles to running for office individuals rate as particularly relevant for them. I rely on prior work examining obstacles to political candidacies to develop these survey items.<sup>12</sup>

In the next step, I elicit information on prior beliefs regarding the risk of violence.<sup>13</sup> A video vignette is used to introduce the survey item to improve respondent understanding and maximize attention.<sup>14</sup> I elicit beliefs and provide information on a specific type of violence: Death threats against politicians or their families, focusing on the experience of local politicians. This choice is motivated by two considerations. First, while previous research has shown that politicians can be subject to a wide range of violent attacks ranging from property destruction to stalking or unwanted attention, using concepts such as "psychological violence" in a survey of non-specialists raises concerns that respondents might interpret questions or the intervention in very different ways, or might not be able to understand them at all (Westwood et al. 2022). Concerns about interpretability and respondent comprehension are mitigated by the focus on death threats. Second, I elicit beliefs regarding the safety risks to *local* politicians specifically. Most political careers in multilevel polities such as the United States begin and end in local government, and local elected officials such as school board members (Macartney and Singleton 2018), sheriffs (Farris and Holman 2017), tax assessors (Sances 2016) or election administrators (Furstenberg-Beckman, Degen, and Wang 2021) take decisions of great consequence for their communities. With its wide range of local elected offices – 96% of all elected offices in the country are part of local government (Lawless 2011, 33) – the United States are what one could call a "high maintenance democracy", premised on a large number of citizens getting involved in politics not just as passive voters, but by competing for and holding office. Additionally, those who do compete for state or federal elected office often begin their political careers in local government (Fox and Lawless 2005, 643).

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11. The question reads: "Why have you personally not run for office? Please write down a few keywords to help us understand why you have not run for office." Note that I previously ask respondents whether they have in fact run for office, and I exclude respondents who have from answering this question. 28 respondents, corresponding to 0.6% of respondents who answered the question, had previously run for office. Although I asked for keywords, many respondents answered in prose.

12. See Appendix B.8 for more information on these survey items.

13. See appendix B.1 for measurement and coding.

14. The video vignette is available at this URL: <https://youtu.be/bB5vHzHT5hw>. See Appendix B.7 for the spoken text of the vignette.



### 3.1 Sample

I fielded my survey with a sample of 4,582 American citizens in October 2023. The citizenship restriction was imposed to ensure that those surveyed were theoretically eligible for office in the United States.<sup>15</sup> Respondents were recruited online on the survey platform Prolific, and compensated for their participation in line with Prolific’s recommended compensation rate. I imposed restrictions on access to produce a gender-balanced sample. No other quotas were applied. Descriptive statistics are shown in Appendix C.

Respondents were presented with two pre-treatment factual attention checks. The attention checks were designed to comply with Prolific’s attention check policy. In line with the policy, respondents who failed *both* attention checks were screened out of the study.<sup>16</sup> The paper thus presents results from respondents who have passed at least one pre-treatment attention check. Since the survey used video vignettes, I also conducted a pre-treatment video and audio check to ensure that assignment to treatment corresponded with treatment delivery. Respondents who failed the check, which requires them to watch a brief video, correctly identify objects displayed, and correctly identify sounds, were screened out.<sup>17</sup>

As a final check on correct treatment delivery, I ask respondents to self-report each time they are shown a video whether video and audio worked correctly on their device. Fortunately, reports of video or audio issues were very rare.<sup>18</sup>

In contrast to my paper, most prior experimental and observational work on entry into politics focuses on samples of so-called “marginal politicians” (Gulzar 2021, 57) or subgroups of respondents assumed to be particularly likely to run for office in the future. Marginal politicians may be party members, activists, or members of campaign training organizations (Broockman 2014; Preece and Stoddard 2015; Bernhard, Shames, and Teele 2021), university students (Silbermann 2015; Foos and Gilardi 2020) or those working in “feeder professions” such as legal services (Fox and Lawless 2005). The rationale for using marginal politician samples is that running for office is a low-probability event (Gulzar 2021), as the overwhelming majority of individuals would never consider entering politics in the first place. This implies that large samples are needed to detect meaningful treatment effects, especially when the analyst is interested in effect heterogeneity.

However, there are four significant issues with surveying “marginal politicians”. The first is that these populations are hard to survey. Thus, work that relies on “marginal politician” samples will typically have to work with reduced statistical power.<sup>19</sup> Low sta-

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15. Eight respondents were screened out after indicating that they did not hold U.S. citizenship.

16. No respondents failed both attention checks.

17. Thirty-four respondents were screened out because they failed the device check.

18. Fortunately, only 0.6% of respondents assigned to treatment reported that the video delivering treatment did not work correctly. See Appendix A.1.2 for a robustness check.

19. For comparison, the above studies use samples of between 450 and 600 (Foos and Gilardi 2020),

tistical power, in turn, raises serious questions regarding replicability. The second issue with using a “marginal politician” sample is that respondents may already have “priced in” the disamenities of participation in politics, meaning that selection based on expectations of exposure to violence may already have taken place. The decision to join a political party or to attend a campaign training event is likely preceded by significant reflection and prior lower-level experience with political engagement, maybe in the form of phone-banking, canvassing or engaging in political debate on social media. Given recent evidence that such activities expose politically active individuals to hostility (e.g. Yan and Bernhard 2023), it is unlikely that people join political parties or attend campaign training events – and thus enter typical “marginal politician” populations – without reflecting on the potential drawbacks of political engagement. Third, a key concern of this paper is how safety concerns might relate to the underrepresentation of certain social groups in politics. This underrepresentation is difficult to study with typical “marginal politician” samples which are themselves products of socially exclusive selection processes. Surveying a sample of students at an elite private college, the composition of which is the result of socially exclusive admissions strategies, is likely uninformative about the reasons why more ordinary citizens are not considering entering politics. Fourth, whether an ambitious individual ends up actually running for office is often a function of how much support that individual receives from peers and close network contacts. Understanding who receives support from peers and who does not requires a broader focus on the general population rather than simply “marginal politicians”. For all these reasons, I do not require “marginal politician” status for inclusion in the sample.

## 4 Salience and Risk Perceptions

Is fear of exposure to violence *salient* for respondents as they reflect on the possibility of running for office? To gauge salience, I asked respondents to reflect on why they had not previously run for office, and respond by providing the most important reasons in the form of keywords or prose. The responses reveal a wide range of concerns that prevent respondents from considering a candidacy. In a first step, I qualitatively analyze responses that explicitly mention fear of violence or harassment as important reasons to stay away from politics, and highlight common themes in these responses.

When respondents mention safety concerns as a reason to stay out of politics, they frequently discuss how social, gender or partisan identities factored into their decision. Respondents relate safety concerns to their identities as women (# 2094), queer people (# 1987), or to their particular partisan identity (# 3887). Others emphasize that their

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317 (Preece and Stoddard 2015), 702 (Bernhard, Shames, and Teele 2021) and 1,387 (Silbermann 2015). My sample size – even when restricted to those exposed to downward-correcting information – is between approximately 2.5 and 10 times larger.

political views lie far outside the mainstream in their local communities (# 218, # 2737). A perception that one deviates from local norms – either due to identity or political views – motivates respondents’ safety concerns.

WHY HAVE YOU PERSONALLY NOT RUN FOR OFFICE? PLEASE WRITE DOWN A FEW KEYWORDS TO HELP US UNDERSTAND WHY YOU HAVE NOT RUN FOR OFFICE.

“As a lesbian I would be killed” – # 1987, F 59, OR

“[...] Also, it’s very dangerous for a woman to be in office with as violently partisan as things are.[...]” – # 2094, F 48, MA

“Keywords: lack of confidence, safety concerns, unpopular opinions Though I have strong feelings about the political landscape, I recognize that my views are a bit farther to the left than most, so I don’t think I would appeal to enough people as a candidate. I also do not want to be in the public eye in this way for reasons of self-confidence as well as safety.” – # 2737, F 51, CA

“I feel politics today is to risky if you are on the wrong side of the aisle. Left wingers will harass and try and cancel people who disagree with them. Radicals also threaten people and harass their families. Politics is to [sic] dirty for me to even consider it. Plus it is so corrupt.” – # 3887, M 57, CA

“The town I live in is very conservative and republican, I am neither. There is no tolerance for other ideas and people are regularly abused in the town website for having a different opinion.[...]” – # 218, F 69, NH

Second, respondents voice concerns about various types of aggression or violence they fear being exposed to. Concerns about receiving hateful communications (# 1806), reputational damage due to defamation (# 2084) and being “canceled”, particularly on social media platforms (# 2349), were frequently mentioned. Other responses expressed concerns about physical harm (# 2278, # 3380).

“I do not want the stress. I think it would make me a target similar to people who have political bumper stickers on their cars. I do not want to attract crazy people or unstable people who disagree with my opinion and they could turn violent” – # 3380, F 37, TX

“[...] I know how vicious people can be in the comment sections of social media sites, and that frightens me.[...]” – # 2084, F 43, NC

"I don't like politics in it's current state where you have people that are looking for reasons to hate you on social media. They try to "cancel" you if you think differently, not worth the hassle in my opinion." – # 2349, M 37, TX

"Because I do not have a thick skin and I consider privacy my number 1 priority. MAGAts go terrorize politicians and need security details. This is Cochise County dude! 4 election supers quit in less than a year!![...]" – # 2278, F 62, AZ

Third, concerns about personal safety appear inextricably linked to privacy concerns and respondents' assessments about their own personality. Respondents perceive politics to entail significant loss of privacy (# 3756), and use highly charged and metaphorical language to describe their fear of living a life "under the microscope" (# 599). Some respondents relate loss of privacy to fear of further harm (# 599, # 2375). Others describe a perception that they lack the requisite personality traits to confront aggression in politics. In their view, politics requires a "thick skin" (# 2278, # 4391). Numerous responses mentioned the perceived risks of added scrutiny and aggression affecting not just candidates themselves but their families (# 599, # 3756).

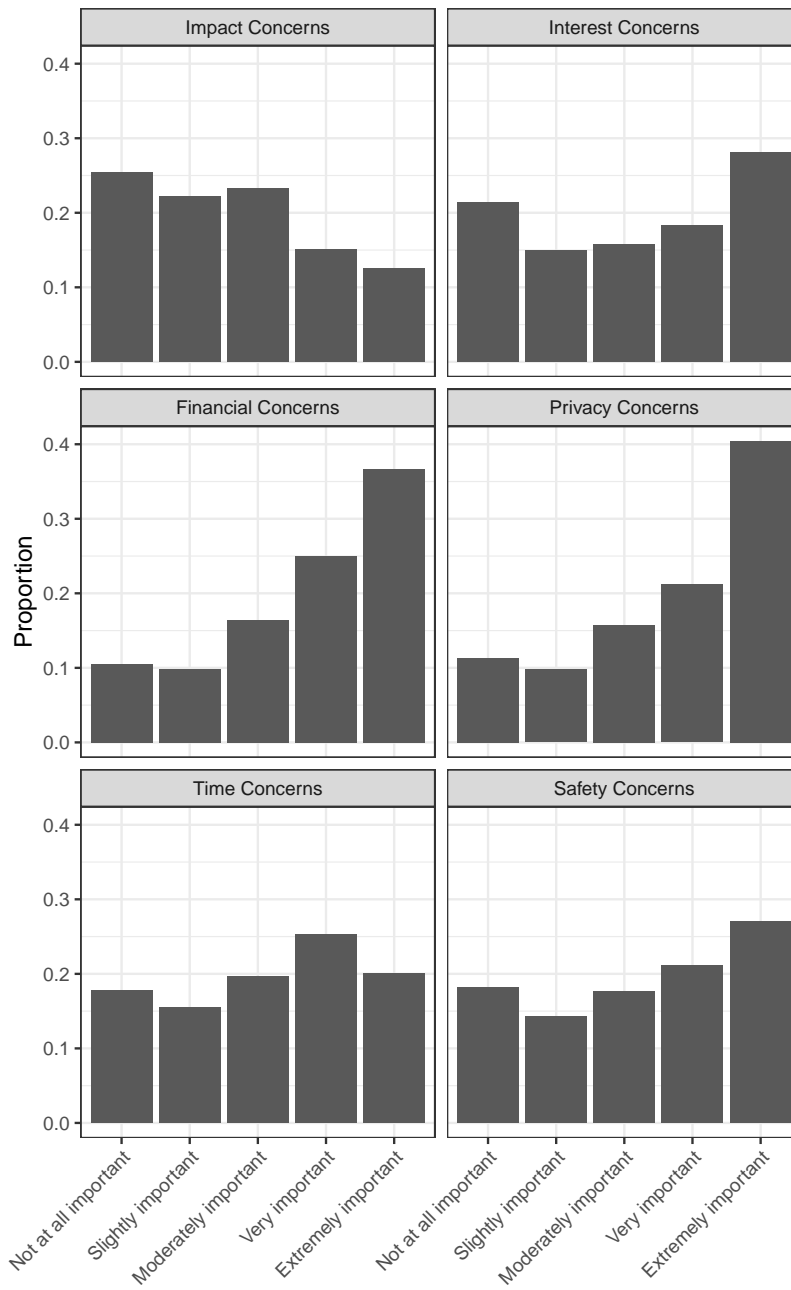
"media puts you and your family under a microscope, safety threatened by conspiracy theorists" – # 599, F 37, CO

"[...]Family life will be changed as they can be harassed or hurt by opponents. Personal life, choices and way of living will be scrutinized." – # 3756, F 57, NY

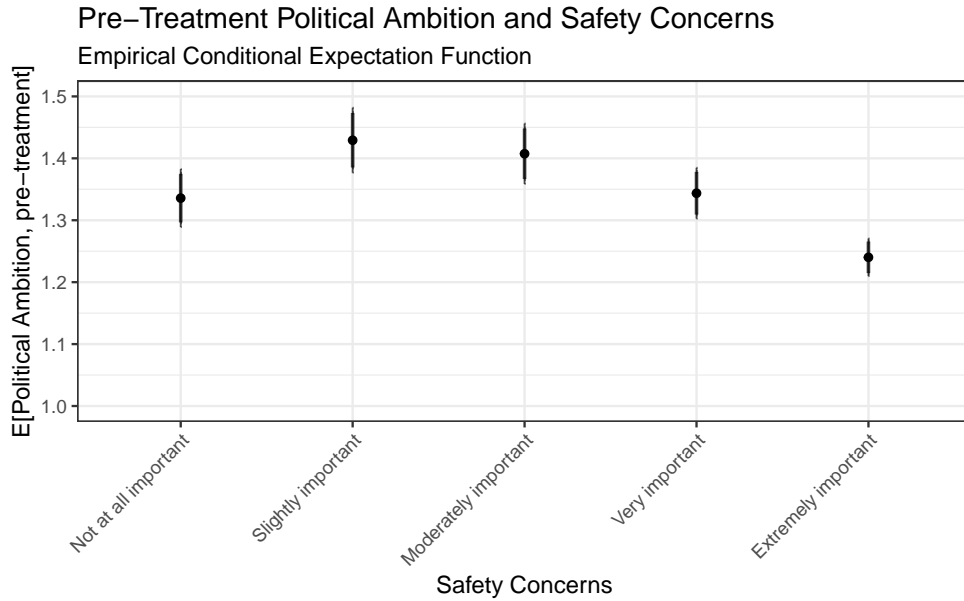
"Lack of privacy. My family would become targets. Maybe not so much small town, but have to ask if it's worth the trouble" – # 2375, M 52, MA

"Candidates are under a lot of scrutiny in my town. People enjoy being jerks online by saying mean things to people who run for office. I don't know if I'd have thick enough skin to deal with these type of people.[...]" – # 4391, F 59, IA

How salient are safety considerations in relation to other theoretically relevant obstacles to entry into politics? To gauge salience in a more structured fashion, I asked respondents to rate how concerned they were about experiencing violence or harassment in politics. I also asked them how important financial considerations, privacy concerns, lack of interest, lack of time, and expectations of impact or effectiveness were as they reflected on the possibility of running for office. Figure 2 shows how important each of these concerns were to respondents.



**Figure 2:** Salient Obstacles to Entry into Politics



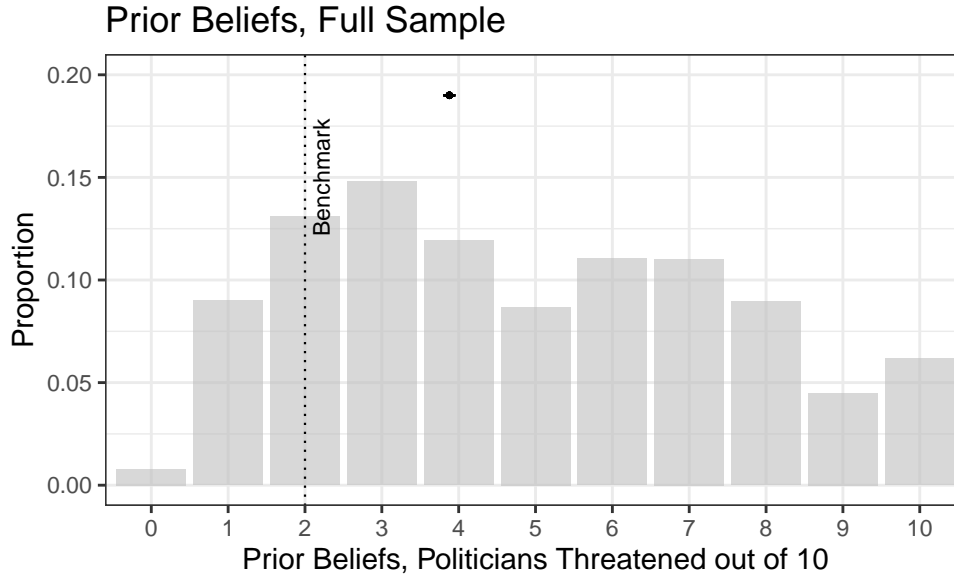
**Figure 3:** Safety Concerns and Baseline Political Ambition

Figure 2 reveals that financial and privacy considerations are reported to be by far the most salient obstacles to running for office, with 63% of respondents rating these concerns as very or extremely important. Among the concerns listed here, safety concerns rank third, with 49% of respondents rating safety concerns as very or extremely important. Lack of interest in politics (47%), concerns about inability to make an impact when in office (28%), or lack of time (46%) are all ranked as very or extremely important at comparable or lower rates, underscoring that safety concerns have to date been underappreciated as an obstacle to running for office. Moreover, the large share of respondents emphasizing the importance of privacy concerns is noteworthy. As the open-text responses reveal, safety and privacy concerns are viewed as related – respondents perceive that political careers force them into the public spotlight, which in turn might attract violence.

Does the salience of safety concerns correlate with political ambition? Figure 3 plots the empirical conditional expectation function of baseline political ambition with respect to the reported salience of safety concerns. The association between those variables is negative as expected – greater safety concerns correlate negatively with political ambition measured at baseline.

## 4.1 Risk Perception

To dig deeper into respondents’ perception of the risk of violence faced by those embarking on political careers in the United States, I elicited respondents’ prior beliefs on how many



**Figure 4:** Prior Beliefs, Full Sample

U.S. local politicians out of 10 faced death threats against them or their loved ones because of their work. I then compare these beliefs to a scholarly benchmark derived from elite surveys.<sup>20</sup>

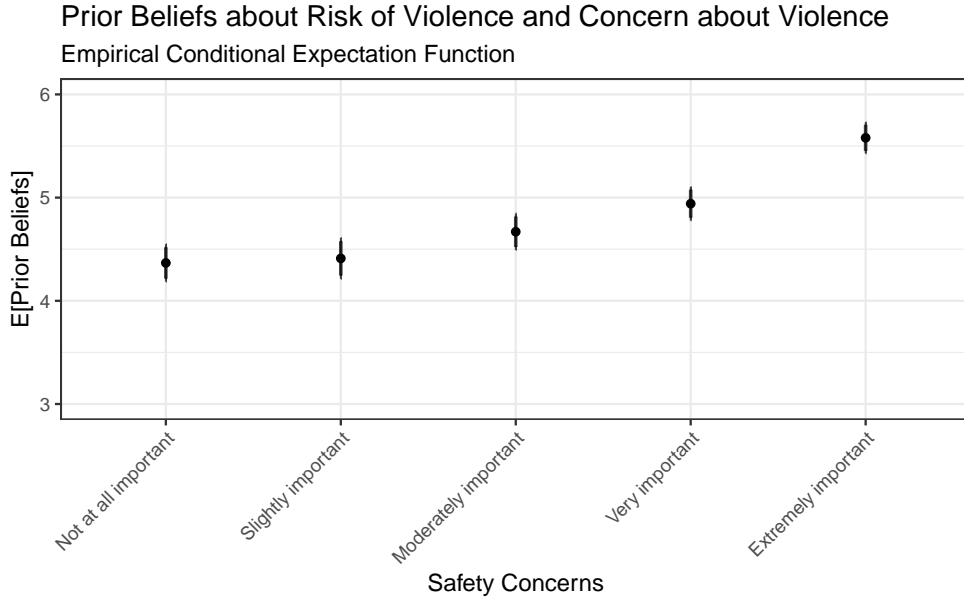
Figure 4 summarizes the prior beliefs of all respondents in a bar chart. Relative to the benchmark scholarly estimate, the majority of respondents holds overly pessimistic beliefs regarding the risk of experiencing violent threats while in office. The modal prior belief is that 3 out of 10 politicians have experienced threats, with a sample average prior of just below 4 out of 10.

Greater reported concern with personal safety is positively associated with perceived risk. Figure 5 shows the empirical conditional expectation function of prior beliefs with respect to the reported salience of safety concerns. The association between those variables is positive – greater safety concerns are associated with higher perceived risk to officeholders.

Do historically marginalized groups perceive risks of political engagement differently than historically dominant groups?<sup>21</sup> Figure 6 displays prior beliefs separately by gender, sexual orientation and ethnic identity. I display group averages and corresponding confidence intervals, as well as differences-in-means. Indeed, marginalized groups perceive a

20. The benchmark estimate – approximately 2 out of 10 local politicians in the U.S. have received death threats against them or their families – is derived from a 2021 survey of U.S. mayors conducted by Herrick and Thomas (2023). To protect the identity of respondents, replication data for this survey allowing me to compute this estimate myself were not available directly. I am grateful to Rebekah Herrick for providing me with the benchmark estimate.

21. This expectation was preregistered in the pre-analysis plan as  $H_{5Desc}$ .

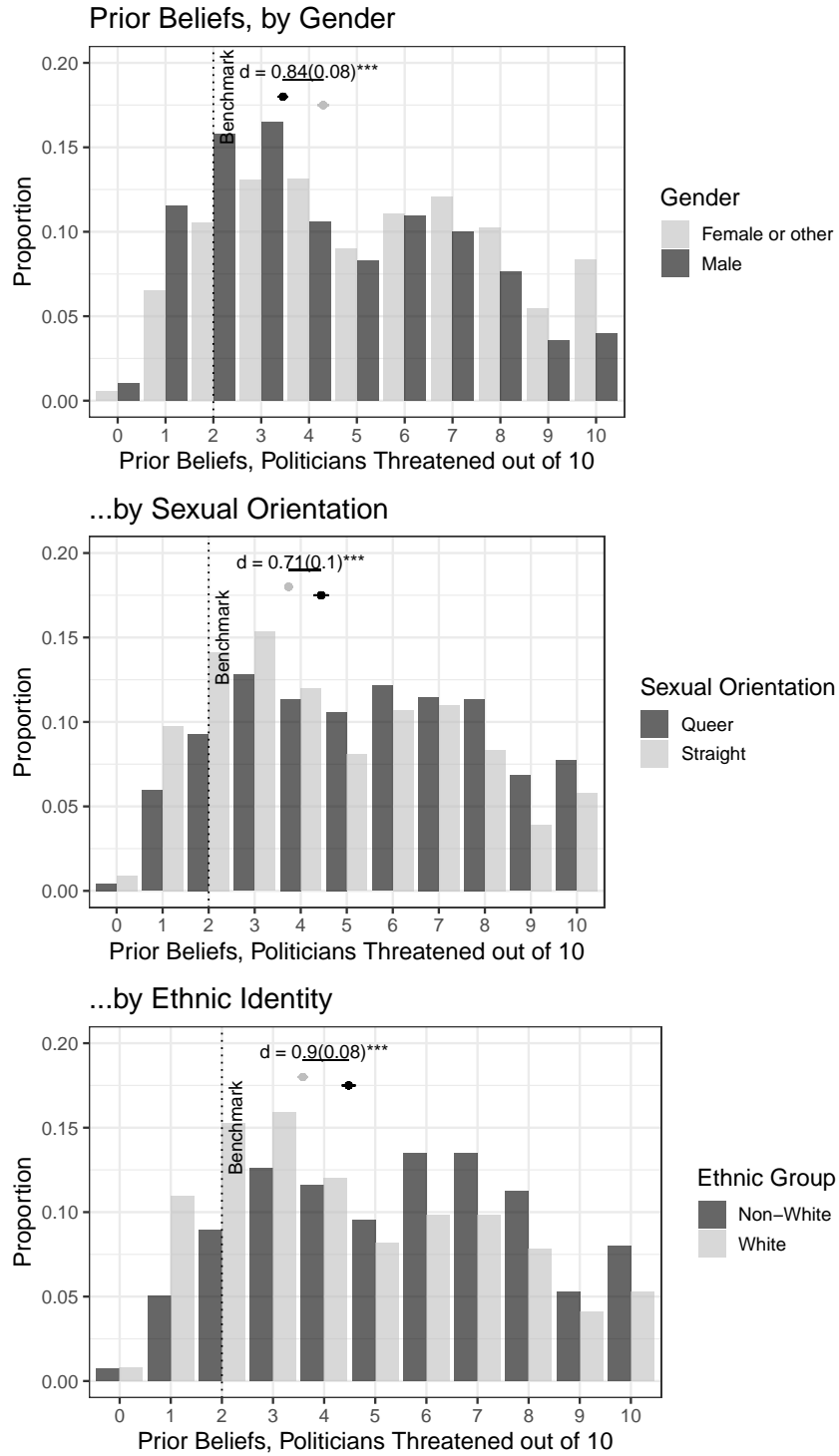


**Figure 5:** Safety Concerns and Prior Beliefs

higher risk of exposure to violence than dominant groups. Women hold higher average prior beliefs than men, queer respondents are more pessimistic than straight respondents, and non-white respondents on average hold higher prior beliefs than white respondents. In all cases, group differences are statistically significant at  $p < 0.01$ .

Is the association between marginalization status and prior beliefs robust to conditioning on other potential predictors of beliefs? Table 1 shows partial correlations between marginalization status and prior beliefs, conditioning on partisan preferences, reported news consumption habits and respondent demographics. Model (2) shows that the association between marginalization and prior beliefs is robust to conditioning on partisan preferences. Conditional on marginalization status, partisan preferences are not associated with prior beliefs. Model (3) additionally conditions on respondents' reported news consumption habits. The association between marginalization status and prior beliefs remains robust. Additionally, conditional on marginalization and partisan affiliation, getting news from print media is associated with *lower* prior beliefs, while getting news from social media is associated with *higher* prior beliefs on average. This raises the possibility that different news ecosystems relay information about political violence in different ways, leading to a divergence in respondent beliefs. Finally, model (4) conditions on basic respondent demographics. On average, and conditional on marginalization, partisan preferences and reported news consumption, greater age is associated with lower priors, and college-educated respondents report lower priors than non-college educated respondents. Again, the association between marginalization status and prior beliefs remains strong and robust.





**Figure 6: Prior Beliefs, by Group**

Note: Differences are estimated in a bivariate regression of priors on group identity. HC2 standard errors given in parentheses. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .

	<i>DV: Prior Beliefs</i>			
	(1)	(2)	(3)	(4)
Woman	0.829*** (0.078)	0.837*** (0.078)	0.810*** (0.078)	0.840*** (0.084)
Straight	-0.525*** (0.097)	-0.520*** (0.099)	-0.457*** (0.100)	-0.399*** (0.112)
White	-0.900*** (0.082)	-0.903*** (0.083)	-0.788*** (0.084)	-0.718*** (0.094)
Independent		0.136 (0.096)	0.138 (0.096)	0.075 (0.104)
<i>Reference: DEMOCRAT</i>				
No preference		0.062 (0.239)	0.119 (0.236)	0.154 (0.273)
Other party		0.323 (0.269)	0.368 (0.278)	0.542* (0.291)
Republican		0.022 (0.106)	0.038 (0.107)	0.021 (0.112)
TV			0.000 (0.042)	0.060 (0.045)
Radio			-0.039 (0.046)	0.066 (0.049)
Print			-0.140*** (0.045)	-0.072 (0.048)
Apps			0.080* (0.043)	0.113** (0.046)
Social Media			0.276*** (0.040)	0.168*** (0.043)
Above Median Income				-0.088 (0.089)
Age				-0.023*** (0.004)
Any College				-0.491*** (0.091)
Intercept	5.486*** (0.108)	5.431*** (0.113)	4.550*** (0.241)	5.455*** (0.296)
R <sup>2</sup>	0.058	0.058	0.073	0.083
Adj. R <sup>2</sup>	0.057	0.057	0.070	0.080
Num. obs.	4468	4468	4429	3884
RMSE	2.598	2.598	2.580	2.578

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ . Num. obs. varies due to missingness in the predictors.

**Table 1:** Unpacking Priors

## 5 Randomizing Information

Fear of aggression or violence looms large for many respondents as they reflect on the possibility of seeking office. Additionally, greater reported concern about exposure to violence is associated with a lower willingness to run for office. Is this relationship causal? This section is concerned with estimating the *causal* effect of a shift in safety risk perceptions on two key outcomes: Personal political ambition, as well as the willingness to support the political ambition of close peers.

After eliciting prior beliefs, I randomly assign respondents to a treatment condition, where they receive corrective information, or a control condition, where they do not receive corrective information, with equal probability.<sup>22</sup> I performed complete randomization of respondents *within* priors using the randomizer function in Qualtrics.<sup>23</sup> Respondents in the treatment condition are then shown a video containing corrective information.<sup>24</sup> Respondents in the control condition are not shown this information.

Following the information intervention, I elicit posterior beliefs regarding the risk of violence, post-treatment measures of my outcomes of interest,<sup>25</sup> as well as auxiliary outcomes.<sup>26</sup> As a last step, to ensure equitable treatment of all research participants, I provide the information to the respondents in the control condition in the post-survey debrief.

I estimate the effects of my intervention on personal political ambition, as well as the willingness to support close peers in their political ambition. To understand how effects on support for peer ambition vary with whether or not the peer in question identifies with a marginalized group, I cross-randomize peer gender – as communicated through an identifiably female or male name of the peer – prior to eliciting the pre-treatment outcome, with respondents being allocated a hypothetical female or hypothetical male peer with equal probability. Finally, I elicit information on auxiliary outcomes to understand exactly how respondents interpreted the information provided by the intervention.

To maximise statistical power, I elicit the main outcomes of interest both before and after the intervention. Pre-post experimental designs allow for substantial gains in statistical power without risking distortions due to experimenter demand effects (Mummolo and

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22. Treated respondents were shown a video presenting a benchmark estimate of the share of U.S. local politicians who have received death threats against themselves or their families. The benchmark estimate – approximately 2 out of 10 local politicians in the U.S. have received death threats against them or their families – is derived from a 2021 survey of U.S. mayors conducted by Herrick and Thomas (2023). To protect the identity of respondents, replication data for this survey allowing me to compute this estimate myself were not available directly. I am grateful to Rebekah Herrick for providing me with the benchmark estimate.

23. Randomization was performed within priors to protect against chance imbalances in treatment status within prior groups.

24. The video vignette is available at this URL: <https://youtu.be/Bsu9W7zJNqY>. See Appendix B.7 for the spoken text of the vignette.

25. See Appendix B.2 for coding and measurement.

26. See Appendix B.3 for coding and measurement.

Peterson 2019) or response consistency bias (Clifford, Sheagley, and Piston 2021).

## 5.1 Ethical Considerations

Conducting empirical research on political violence involving human subjects requires careful consideration of research ethics. My experimental intervention relies on eliciting prior beliefs regarding the risk of receiving threats of violence faced by those serving in local office in the United States, and then providing *verified information* on that risk drawn from previous scholarly work to a random treatment group of respondents. In designing the vignettes and survey questions, I took great care to prevent exposure to unnecessarily detailed or explicit accounts of violence or rely on fear-inducing or traumatizing stimuli. Most critically, the design does not rely on deceiving respondents. I provide all respondents with the same information derived from scholarly research. Respondents differ only in *when* they are exposed to this information – treated individuals are exposed before the elicitation of post-treatment outcomes, while control individuals receive the information during the debriefing module.

The choice of providing identical information to all respondents irrespective of their prior beliefs comes at a cost, as it complicates data analysis. As anticipated based on a pilot survey fielded in June 2023, a sizeable majority of respondents *overestimate* the frequency of violent threats against politicians. Hence, the design is well-powered to detect substantively meaningful effects or differences in effects of *downward* belief correction, but underpowered to estimate such effects or differences in effects for *upward* belief correction.<sup>27</sup> As announced in the pre-registration plan<sup>28</sup>, the primary focus of the results in the paper will be on respondents who overestimate the risk of violent threats. Results will be shown for other respondents as well, but they will not be central to the paper and should not be regarded as conclusive evidence indicating presence or absence of (heterogeneous) effects.

A different approach to the experimental component of the survey would have relied on providing upward updating information to all respondents irrespective of their prior beliefs. Subsequently, the use of deception could have been made transparent during a debriefing module. I judge this approach to be ethically problematic. First, providing deliberately deceptive information on an issue as sensitive as political violence in an era of generally rampant misinformation might induce real harm by limiting respondents' confidence in scholarly information, even when provisions for debriefing are made. Second, deceptive interventions that influence civic engagement are particularly problematic in an era where American democracy faces significant challenges. Risking that civic engagement is reduced through the deliberate provision of deceptive information is ethically impermissible in such a context. In sum, I contend that the analytical benefits of simplifying the design using

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27. See PAP, Section 11 for the full power analysis.

28. See PAP, page 8.

deception are outweighed by the cost of deceiving respondents.

My deception-free design provides respondents with valid and valuable information on the risk of experiencing threats of violence when serving in public office. Providing this information is arguably *beneficial* to subjects, as it enables them to make an informed decision regarding their level of engagement in public life. Hence, to ensure that my intervention does not violate the norm of equal treatment and empowers *all* research participants, and not just a random subgroup, to participate in civic life in an informed manner, the survey was followed by a debriefing section where respondents not previously exposed to verified information on the frequency of violence were provided that information.

## 5.2 Theoretical Expectations

My intervention exposes a randomly assigned treatment group to *aggregate* information about the risk of facing threats of harm when in elected office. I expect exposure to aggregate information to affect personal political ambition and the willingness to support close peers in their political ambition. I expect these effects to vary with respondents' own social identities and the experimentally manipulated social identities of their peers.

Safety from violence and aggression has long been recognized as a basic human need that trumps higher-order self-actualization benefits realized when running for office or otherwise serving one's community (Maslow 1943). On average, as individuals come to perceive political engagement as an activity that does not clash with the need to protect their physical and mental integrity, their ambition to engage in politics will increase. Conversely, fear of exposure to violence will depress engagement.

This prediction might appear to clash with scholarship documenting that individuals often do engage in political activities that put their safety at risk (Wood 2003; Cantoni et al. 2022), and that increasing the cost of political engagement does not invariably reduce political engagement by citizens.<sup>29</sup> As a result, recent theories of political participation posit that the relationship between the intensity of violence – a “cost” of participation – and the degree of participation is non-monotonic. At low levels of intensity, violence is argued to provoke defiance, activate approach emotions and increase participation. Only intense violence is successful at inducing compliance (Aytaç and Stokes 2019, 76-82).

Should we expect that common forms of violence against politicians in the United States – harassment, vandalism, threats of violence, and so on – induce defiance instead of compliance? I contend that extrapolating from motivations for voting and protest participation to motivations for seeking office overlooks a key distinction between *sporadic* and *continuous* forms of engagement. Empirically, primarily *sporadic* forms of engagement like voting or

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29. For example, research on voter ID laws in the United States suggests that policies which increase the cost of voting do not meaningfully depress turnout (Highton 2017; Cantoni and Pons 2021), and that such policies might even backfire by prompting countermobilization efforts (Valentino and Neuner 2017; Cantoni and Pons 2021).

attending protests have been shown to be subject to the defiance logic. In contrast, holding office is a *continuous* form of engagement. Even part-time local office requires office holders to invest significant time and effort over extended periods to fulfill their duties. Throughout their term, they are subject to scrutiny from their community, and, at higher levels of government, the media. The effort to show up to vote at a polling station on a few days a year or occasionally participate in marches or rallies may be substantial, but pales in comparison to the effort required to hold office. Hence, I expect that individuals will be encouraged to engage in continuous forms of political action as they come to perceive a lower risk of facing violence and aggression, and discouraged to engage when they perceive that risk to be increasing.

$H_1$ : All else equal, an increase in the perceived risk of experiencing threats of violence reduces political ambition, while a decrease in the perceived risk should increase political ambition.

Second, I expect aggregate information about the risk of facing violent threats in office to affect how willing individuals are to support the political ambitions of their peers. Peer support matters because political careers are *socially embedded*: They require not only individual intent to run for office, but equally supportive family, close friends and other peers. Given the significant time and financial investments a political career requires, network support is essential to managing the logistical demands of running for and serving in public office (Lawless 2011, 86-98).<sup>30</sup> Unsurprisingly, scholars have found that the support and encouragement of friends and family is strongly associated with greater political ambition, and that signals of support from close contacts matter more than encouragement from party personnel (Lawless 2011; Fox and Lawless 2023, 4).<sup>31</sup>

I argue that much like individual political ambition, the willingness to support the political ambition of close peers increases as the perceived risk of exposure to violence decreases, and decreases as that risk increases. First, individuals hold empathetic concerns for other individuals in their network, inducing them to protect others close to them from harm, such as exposure to aggression or violence. Second, even absent empathetic motivations, individuals may have made investments in their peers. For example, parents may invest in their children expecting support in later life, and spouses invest in each other for the purposes of raising children. Exposure to harm and violence may reduce the value of these investments by damaging the mental or physical integrity of the target. Conversely,

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30. Prior work on political ambition is unequivocal about the importance of network support for political ambition: According to Lawless (Lawless 2011, 96), “[t]he effect of a supportive personal environment cannot be overstated.”

31. Additionally, scholars have emphasized that signals of network support have a greater encouragement effect on women compared to men (Fox and Lawless 2023, 5).

peers stand to benefit when someone in their network wins political office.<sup>32</sup> The upshot is that a pacified political climate increases the willingness to support politically ambitious peers, and thus the likelihood that ambitious individuals receive signals of valuable network support.

*H<sub>2</sub>*: All else equal, an increase in the perceived risk of experiencing threats of violence reduces support for the political ambition of peers, while a decrease in the perceived risk increases support for the political ambition of peers.

Third, I expect that exposure to aggregate information exerts different effects on political ambition depending on the social identities of individuals. Existing scholarship has demonstrated that groups which have historically been subject to violent repression respond more strongly – again, in terms of voting and protest behavior – to changes in the repressive environment than groups with no established history of violent victimization (Rozenas and Zhukov 2019). Memories of past repression are argued to persist and shape downstream behavior due to family and community socialization (Bisin and Verdier 2001; Lupu and Peisakhin 2017). Older community members pass on particular norms and behavioral prescriptions to younger community members. In a repressive environment, parents in historically marginalized communities may, for example, model political quietude to their children, estimating that showing too much political ambition may put them in harm’s way.

Differential socialization – for example along gender lines – may lead individuals to respond differently to information regarding risks of violence. “Threat socialization” may, for example, instill a sense that information about risks of violence should be particularly important for one’s decision making, implying that individuals thus socialized respond more strongly to risk information than individuals socialized differently. Hence, I expect the individuals who identify with historically marginalized groups to respond more elastically to an information shock than individuals who identify with historically dominant groups.

*H<sub>3</sub>*: All else equal, a decrease in the perceived risk of facing threats of violence in office has a stronger positive effect on the political ambition of marginalized compared to dominant individuals.

Fourth, I expect that the effect of an aggregate information shock on support for peer ambition varies with whether or not the peer is marginalized. Differences in elasticity could result from (i) a widespread perception that exposure to violence has a greater negative

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32. For example, there is evidence that even local-level political connections are financially valuable to business owners, especially in moments of crisis (Acemoglu et al. 2016; Berger, Karakaplan, and Roman 2023).

effect on individuals belonging to historically marginalized groups – that marginalized individuals are less able to “cope”<sup>33</sup> – or (ii) the widespread belief that marginalized groups bear the brunt of violence, and that shifts in risk primarily represent shifts in risk to marginalized groups.

$H_4$ : All else equal, a decrease in the perceived risk of facing threats of violence in office has a stronger positive effect on the willingness to support marginalized peers in their political careers compared to dominant peers.

### 5.3 Estimation

As pre-registered, the main specifications focus on the effects of the intervention on respondents who hold prior beliefs greater than truth and are subject to downward belief correction. I first estimate intent-to-treat effects (ITTs) of providing downward-correcting information. The dependent variables are within-individual change in outcomes, computed by subtracting pre-treatment outcome measures from post-treatment outcome measures to obtain change scores. I then regress change scores on the treatment indicator. All effects are estimated using ordinary least squares with robust HC2 standard errors.

$$A_{i,post} - A_{i,pre} = \alpha + \beta Z_i + \epsilon \tag{1}$$

$$S_{i,post} - S_{i,pre} = \alpha + \beta Z_i + \epsilon \tag{2}$$

Where  $i$  indexes individual survey respondents.  $A_{i,post}$  denotes post-treatment personal ambition,  $A_{i,pre}$  pre-treatment personal ambition.  $S_{i,post}$  denotes post-treatment support for peer ambition,  $S_{i,pre}$  pre-treatment support for peer ambition.  $Z_i$  denotes the treatment indicator.

Adjusting for the pre-treatment outcome can either be done via computing the change score or by adjusting for the pre-treatment outcome by including it as a covariate using the estimator developed by Lin (2013). Crucially, both methods target the exact same estimand, although the Lin estimator has been argued to be more precise in an experimental setting.<sup>34</sup> I will show results using both the change score and Lin estimators.

I will present two additional sets of results. First, I will estimate ITTs separately by prior to understand how treatment effects vary by prior. Second, as a test of the directional

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33. When applied to women, this concern effectively represents a form of benevolent sexism (Glick and Fiske 1996), and more broadly, a form of benevolent prejudice.

34. DeclareDesign Blog, January 15, 2019. “Use change scores or control for pre-treatment outcomes? Depends on the true data generating process”, URL: <https://declaredesign.org/blog/posts/use-change-scores-or-control.html>, accessed December 21, 2023.



hypothesis using both respondents who overestimate and who underestimate the risk of violent threats, I use a sign-corrected composite estimand of the following form:

$$\beta_{sign-corrected} = \omega \times \beta_{priors > 2} - (1 - \omega) \times \beta_{priors < 2}$$

I estimate  $\beta_{sign-corrected}$  via plug-in estimation, where  $\beta_{priors > 2}$  and  $\beta_{priors < 2}$  are estimated as in equation 1, subsetting to respondents with priors greater than 2 and priors less than 2, respectively. The weight  $\omega \in (0, 1)$  is given by the sample proportion of respondents with priors greater than 2 as a share of all respondents with priors either greater or less than 2. Standard errors and confidence intervals are computed using the non-parametric bootstrap.

If the directional hypothesis is correct,  $\beta_{priors > 2} > 0$ , and  $\beta_{priors < 2} < 0$ , implying  $\beta_{sign-corrected} > 0$ . Thus, the composite estimand is another test of my directional hypothesis that uses data from all respondents apart from those with correct priors.<sup>35</sup>

To examine whether or not the effect of exposure to information varies depending on an individual’s marginalization status, I estimate conditional effects on *personal* political ambition as follows:

$$A_{i,post} - A_{i,pre} = \alpha + \gamma_1 M_i \times Z_i + \gamma_2 M_i + \gamma_3 Z_i + \epsilon \tag{3}$$

Notation is the same as above, and  $M_i$  denotes a placeholder for the binary moderator variable of theoretical interest.

As estimated in equation 5.3, differences in ITTs of downward correction will reflect both that (i) some groups of individuals may on average have higher priors than others and get a larger “dose” of the treatment, and (ii) any given dose may have a stronger effect on political ambition among some groups than among others. To isolate the second channel, I additionally estimate differences in conditional intent-to-treat effects for each prior group, and then compute a weighted average of differences in conditional intent-to-treat effects. Formally, I define:

$$\gamma_{Global} = \sum_{p=3}^{10} w_p \times \gamma_{1,p}$$

Where  $w_p$  is the proportion of respondents with prior  $p$  among all respondents with priors greater than two, and  $\gamma_{1,p}$  is the estimated difference in conditional intent-to-treat effects from a regression like equation 3, restricted to respondents with prior equal to  $p$ . Standard errors and confidence intervals are computed using the non-parametric bootstrap.

Focusing on support for peer political ambition, the estimation of conditional effects is straightforward because the moderator, a binary variable indicating the gender identity

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35. I am grateful to Fredrik Sävje for suggesting the use of this composite estimand.

of the hypothetical peer  $G_i$ , is randomly assigned. Hence, in expectation, prior beliefs are balanced between respondents assigned to a female and a male peer. To understand how the gender identity of the peer moderates the effect of belief correction on support for peer ambition, I thus simply estimate:

$$S_{i,post} - S_{i,pre} = \alpha + \gamma_1 G_i \times Z_i + \gamma_2 G_i + \gamma_3 Z_i + \epsilon \tag{4}$$

## 6 Effects of Belief-Correcting Information

How does exposure to belief-correcting information regarding the risk of experiencing violent threats affect individual political ambition and support for peer ambition? Table 2 summarizes the effect of the information treatment among respondents exposed to downward-correcting information. The first two models estimate effects on personal political ambition, while models 3 and 4 show effects on support for peer ambition. As hypothesized, the effect of downward-correcting information on personal ambition is positive. The estimated effect is substantively small: Downward-correcting information on average increases political ambition by .02 on a 4-point scale. The effect is equivalent to about one tenth of the estimated pre-treatment gender gap in personal political ambition.<sup>36</sup>

	<i>DV: Personal Ambition</i>		<i>DV: Support for Peer Ambition</i>	
	$\Delta$ -score	Lin	$\Delta$ -score	Lin
Information Treatment	0.018 (0.012)	0.020* (0.011)	0.137*** (0.023)	0.126*** (0.022)
Intercept	-0.005 (0.008)	1.345*** (0.008)	-0.157*** (0.016)	0.872*** (0.016)
Subgroup	priors > 2	priors > 2	priors > 2	priors > 2
R <sup>2</sup>	0.001	0.750	0.010	0.587
Adj. R <sup>2</sup>	0.000	0.749	0.010	0.586
Num. obs.	3499	3499	3499	3499
RMSE	0.344	0.331	0.669	0.644

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ . OLS estimates, HC2 standard errors in parentheses.

**Table 2:** Effects of Downward Correction on Political Ambition

The effect of downward-correcting information on support for peer ambition is positive and strongly significant. Downward-correcting information on average increases support for peer political ambition by .13 on a 5-point scale. This effect is equivalent to more than

<sup>36</sup> The difference in pre-treatment political ambition between women and men estimated in a bivariate regression is  $d = -0.190(0.019)$ \*\*\*.

	<i>DV: Personal Ambition</i>		<i>DV: Support for Peer Ambition</i>	
	$\Delta$ -score	Lin	$\Delta$ -score	Lin
$\hat{\beta}_{sign-corrected}^*$	0.013 (0.011)	0.014 (0.010)	0.138*** (0.021)	0.127*** (0.020)
Num. obs.	3944	3944	3944	3944
Bootstrap Iterations	10k	10k	10k	10k

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**Table 3:** Effects of Belief Correction, Composite Estimation

three times the estimated pre-treatment gap in support for peer ambition between a female and male peer.<sup>37</sup>

To investigate responses to belief-correcting information further, Table 3 displays the results of composite estimation. Positive estimates indicate that downward-correcting information increases ambition and upward-correcting information decreases ambition. While all estimated coefficients are indeed positive, only the coefficients for support for peer ambition are statistically significantly different from zero.

Figure 7 shows estimated effects of belief-correcting information on personal ambition (panel A) and support for peer ambition (panel B) separately by prior. Panel B demonstrates an effect pattern consistent with theory. Exposure to upward correcting information reduces support for peer ambition, while exposure to downward correcting information increases support for peer ambition. The pattern is less clear in Panel A. Fewer coefficients are clearly distinguishable from zero, and while estimated effects of downward-correcting information are indeed largely positive, estimated effects of upward-correcting information defy theoretical expectations.

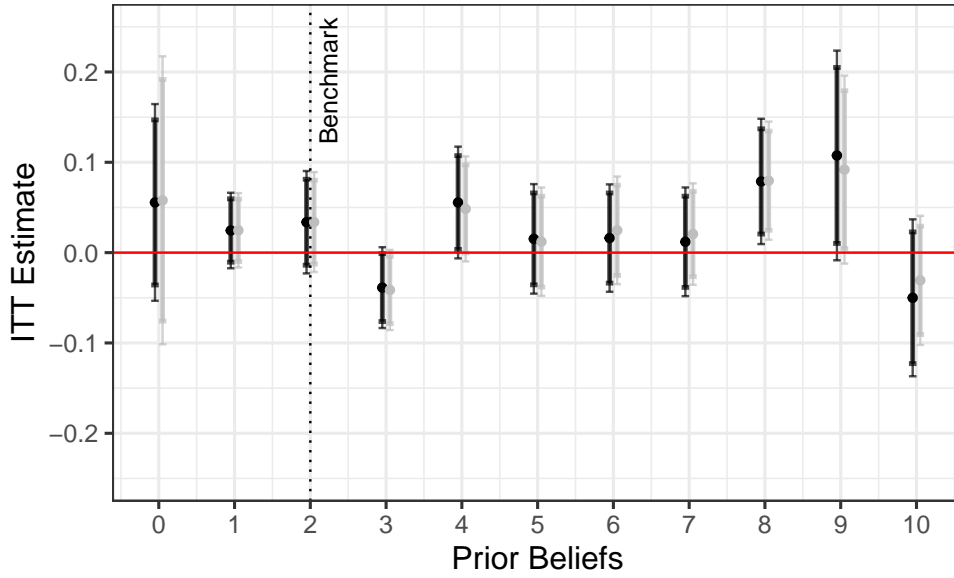
To summarize, belief-correcting information exerts substantively large effects on support for the political ambition of peers, with a positive [negative] response to downward [upward] correcting information. Personal political ambition, on the other hand, responds less predictably to information regarding the risk of violent threats. While downward-correcting information exerts a small positive effect on personal ambition, the response to upward-correcting information remains unclear.

## 6.1 Heterogeneous Effects

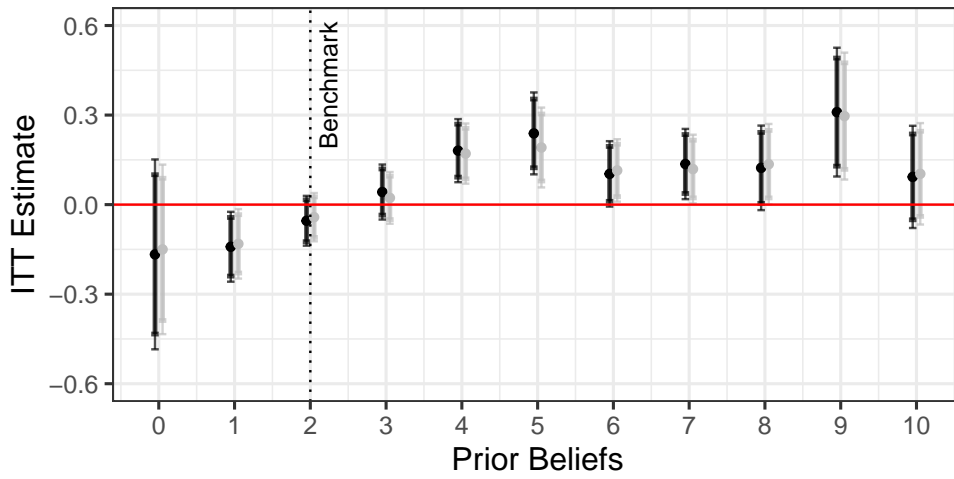
Does the effect of downward belief-correcting information on personal ambition vary with respondent characteristics? Figure 8 summarizes conditional effects of downward-correcting information on personal ambition by gender (panel A), sexual orientation (panel B) and

<sup>37</sup> The difference in pre-treatment support for peer ambition between a female and male peer estimated in a bivariate regression is  $d = 0.039(0.028)$ .

### A. ITT on Personal Ambition by Prior



### B. ITT on Support for Peer Ambition by Prior



Estimator ● Change Score ● Lin Estimator

Figure 7: ITT by Prior Beliefs

$\hat{\gamma}_{Global}^*$	<i>DV: Personal Ambition</i>		
Treatment $\times$ Gender	0.024		
<i>Reference: MALE</i>	(0.031)		
$\times$ Sexual Orientation		-0.018	
<i>Reference: QUEER</i>		(0.023)	
$\times$ Ethnic Identity			-0.017
<i>Reference: NON-WHITE</i>			(0.019)
Num. obs.	3483	3463	3470
Bootstrap Iterations	10k	10k	10k

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ . Num. obs. varies due to missingness in moderator variables.

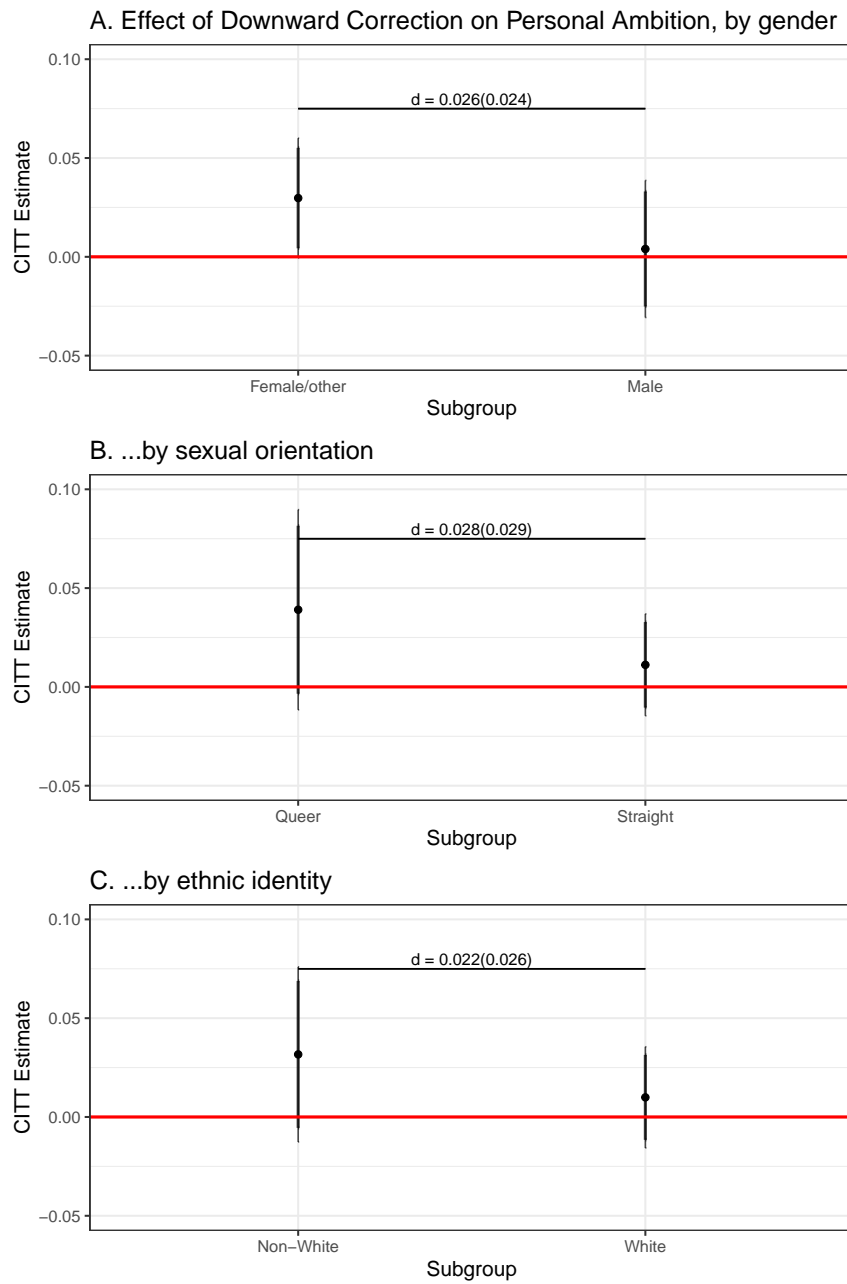
**Table 4:** Heterogeneous Effects of Downward Belief Correction

ethnic identity (panel C) to examine whether or not personal ambition is more elastic to information among marginalized groups. Estimated differences in conditional effects are displayed on the horizontal bars in the center of the plots. In all panels, estimated effects of downward belief correction on personal ambition are indeed *larger* for marginalized groups than politically dominant groups. The effect for women is estimated to be .030(.015)\* compared to .004(.018) for men, 0.039(.026) for queer respondents compared to .011(.013) for straight respondents, and .032(.023) for nonwhite respondents compared to .010(.013) for white respondents. While the effects of exposure of information are generally small – too small for differences-in-effects to be statistically significant – this analysis nonetheless points to effects being concentrated among marginalized respondents.<sup>38</sup>

Differences in conditional effects estimated so far may reflect both (i) unequal distributions of prior beliefs across respondents resulting in variation in treatment “dosage” and (ii) differences in the effects of given doses between respondents. As outlined in the estimation section, the  $\gamma_{Global}$  estimand isolates the second channel. Table 4 displays  $\gamma_{Global}$  weighted averages of differences in conditional effects. Again, estimated differences in effects between marginalized and dominant groups have the expected sign. However, they do not reach conventional levels of statistical significance.

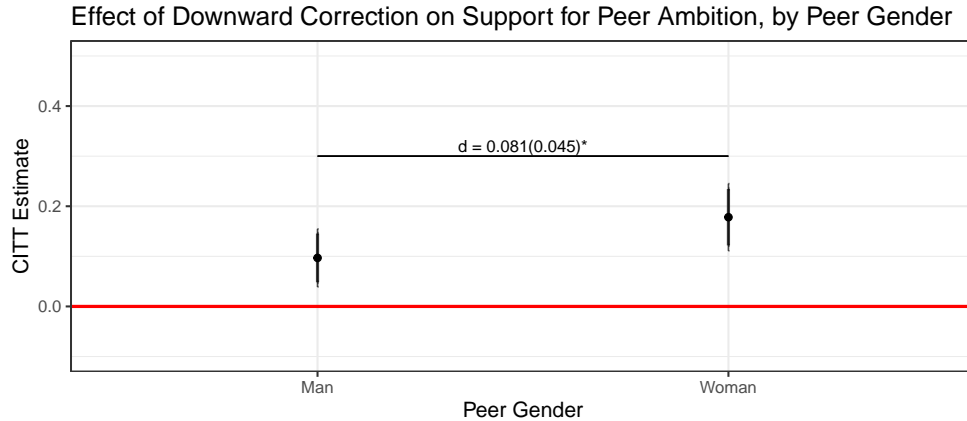
Finally, I examine whether or not the effect of belief-correcting information on support for peer ambition varies with the *randomized* peer characteristic, peer gender. Figure 9 shows conditional effects of downward-correcting information on support for peer ambition separately for male and female peers. The difference in conditional effects is displayed on the horizontal bar in the center of the plot. The plot shows that the effect of downward-correcting information on support for peer ambition is positive for both female and male peers. However, it is estimated to be close to twice as large for female peers at .178(.034)\*\*\*

38. Exploratory analysis using a combined marginalization indicator in Appendix A.2.1 suggests that effects are indeed concentrated among individuals identifying with marginalized groups.



**Figure 8: CITTs by Subgroup**

Note: OLS estimates with HC2 standard errors given in parentheses. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .



**Figure 9:** CITTs by Peer Gender

Note: OLS estimates with HC2 standard errors given in parentheses. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .

compared to male peers at  $.097(.030)^{***}$ . The difference in effects is statistically significant at conventional levels. As hypothesized, support for peer political ambition is much more elastic to downward-correcting information about the risk of violence when respondents are primed to think about female as opposed to male peers. The upshot is that an information shock leading individuals to downward-revise their beliefs on the safety risks to politicians makes it far more likely that they would be willing to support ambitious peers seeking office. The effect of such a shock is almost twice as large when respondents are primed to think about female peers running for office than when they are primed to think about male peers.

## 6.2 Mechanism Testing

Randomized exposure to downward belief-correcting information on average increases support for politically ambitious peers. The effect is particularly strong for female compared to male peers. Moreover, the intervention exerts a small positive effect on individual political ambition, an effect which appears to be concentrated among members of historically marginalized groups. What explains these findings?

In this section, I present additional pre-registered evidence to further probe how respondents interpreted the information they were given. The information I provided with my intervention was *aggregate* information about risks to the larger population of local U.S. office holders. Providing such information could have induced respondents to update their beliefs on the risk of violence on three different margins: (i) The overall risk of violence to the general population, (ii) the relative risk of violence to different social groups, or (iii) the proportions of different social groups among the population of office holders, a set of quantities often described as the base rates. “Good news” regarding the risk of violence

	<i>DV: Estimated Population %...</i>		
	Women	Gay	Non-White
Information Treatment	-0.923*	-0.574	-0.624
	(0.498)	(0.441)	(0.514)
Intercept	34.853***	14.338***	27.478***
	(0.358)	(0.326)	(0.373)
Subgroup	priors > 2	priors > 2	priors > 2
R <sup>2</sup>	0.001	0.000	0.000
Adj. R <sup>2</sup>	0.001	0.000	0.000
Num. obs.	3499	3499	3499
RMSE	14.716	13.042	15.201

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**Table 5:** Effects of Downward Correction on Base Rate Estimates

could induce respondents to lower general risk perceptions, change the perception of relative risk to high risk versus low risk groups, or even shift beliefs about how prevalent high risk groups are in the population of interest.

To examine how respondents interpreted information, I first estimate the effect of the downward-correcting information on the estimated share of women, gay people and non-white people among local U.S. officeholders. Table 5 shows that respondents updated their beliefs in the expected direction – downward-correcting information lowers the estimated share of women, gay people and nonwhite people among local U.S. officeholders. However, the effects are substantively small, ranging from half a percentage point for gay politicians to just below one percentage point for women politicians.<sup>39</sup> Only the estimate for women politicians reaches conventional levels of statistical significance.

Second, I estimate the effect of downward-correcting information on estimated relative risk of exposure to violence of female versus male politicians, gay versus straight politicians, black versus white politicians and Republican versus Democratic politicians. Relative risk is evaluated by respondents on a five-point scale.<sup>40</sup>

Table 6 shows that the treatment was effective in lowering perceived relative risk of exposure to violence for women, gay and black politicians relative to their male, straight and white counterparts. On average, and conditional on overestimating risk at baseline, respondents in the treatment group estimated a lower risk discrepancy between politicians from marginalized versus dominant groups than respondents in the control group. Note

39. For comparison, the estimated difference in the share of women politicians between female and male respondents in the control group was  $d = -2.175(0.608)$ \*\*\*.

40. Relative risk was not elicited quantitatively. Pre-testing of the survey revealed that respondents were confused about conventional quantitative measures of relative risk such as odds ratios.



	<i>DV: Relative Risk</i>			
	women vs men	gay vs straight	black vs white	R vs D
Information Treatment	-0.140*** (0.035)	-0.076*** (0.028)	-0.110*** (0.031)	-0.017 (0.035)
Intercept	0.844*** (0.025)	1.363*** (0.020)	1.073*** (0.022)	-0.299*** (0.024)
Subgroup	priors > 2	priors > 2	priors > 2	priors > 2
R <sup>2</sup>	0.004	0.002	0.004	0.000
Adj. R <sup>2</sup>	0.004	0.002	0.003	-0.000
Num. obs.	3499	3499	3499	3499
RMSE	1.047	0.836	0.918	1.024

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**Table 6:** Effects of Downward Correction on Relative Risk Estimates

that there was no effect on estimated relative risk for members of different partisan groups, suggesting that respondents interpret violence as a phenomenon affecting marginalized populations rather than Democrats or Republicans. In sum, the simple information intervention appears to have been effective in convincing respondents that political careers were not much more dangerous for individuals identifying with marginalized groups than for individuals identifying with dominant groups. Despite the fact that aggregate information on risks was provided, respondents interpreted the information as pertaining primarily to marginalized individuals, suggesting that the perceived importance of safety concerns is asymmetric. The perception that violence primarily affects marginalized individuals is so strong that exposure to a risk benchmark even prompted respondents to update their beliefs about the share of marginalized individuals in the population of interest, defying the well documented tendency of neglecting base rates (Bar-Hillel 1980).

## 7 Discussion

How does fear of violence as a result of political engagement affect democratic competition? How would policy interventions that perceptibly limit violence against office-holders and candidates for office affect representation? How do such interventions have to be designed to be effective?

The evidence provided in this paper generates three broad lessons. First, Americans on average hold overly pessimistic beliefs regarding the safety risks entailed by serving in political office – that is, they on average overestimate such risks relative to a benchmark derived from existing scholarship. Pessimistic beliefs are concentrated among members of

historically marginalized groups – women, non-white ethnic groups, and queer citizens.

Second, a simple randomized intervention that exposed respondents to a downward-correcting aggregate information shock produced a small increase in personal willingness to run for office, and a substantial increase in the willingness to support politically ambitious peers in seeking office. Peer gender moderates the relationship between downward-correcting information and willingness to support peer ambition. The positive effect of a downward-correcting aggregate information shock is twice as large for female compared to male peers.

Third, analysis of auxiliary outcomes revealed that the intervention was particularly effective in reducing the perceived risk discrepancy of holding office between members of historically marginalized and historically dominant groups. This finding is reflective of a widespread and strongly held perception that politically motivated violence is a phenomenon which primarily affects women, queer citizens and other marginalized groups.

Some mature democracies have implemented policies designed to address violence targeted at candidates for office or office-holders.<sup>41</sup> In the United States, policy interventions have focused on providing additional security for office holders and candidates for office, with the Federal Election Commission facilitating the use of campaign funds for personal security. FEC data show that campaign spending on personal safety has increased six-fold between the 2020 and 2022 federal elections.<sup>42</sup> A more repressive approach was taken by German lawmakers. Prompted by the assassination of prominent Christian-Democrat Walter Lübcke in 2019, the 2020 *Combating Far-Right Extremism and Hate Crimes Act*<sup>43</sup> passed by German Parliament widens the scope of illegal threats to include threats of grievous bodily harm and rape, extends existing legal protections against harassment and defamation to include local office holders, and requires social media companies to proactively report threatening or illegal content to law enforcement.<sup>44</sup>

My paper suggests that policy interventions which perceptibly improve the safety of elected officials from violence and threats of violence do not just protect current generations of candidates for office and office-holders from increasingly serious threats – by reassuring ordinary citizens that politics can safely be pursued by citizens from all walks of life, they make a central contribution to safeguarding democratic competition in the long run. Critically, such interventions are likely to be most impactful when they address a wide audience, rather than being narrowly targeted at prospective candidates for office. Political

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41. For a study of reform in Latin America, see Restrepo Sanín 2022.

42. Morton, Sotomayor and DeChalus. "Lawmakers are spending way more to keep themselves safe. Is it enough?", *Washington Post Online*, Sep 18, 2023. URL: <https://www.washingtonpost.com/politics/2023/09/18/congress-security-spending-violence-threats/>, accessed Jan 15, 2024.

43. German title: *Gesetz gegen Rechtsextremismus und Hasskriminalität*.

44. Deutscher Bundestag. "Gesetz gegen Rechtsextremismus und Hasskriminalität beschlossen", 18 Jun, 2020. URL: <https://www.bundestag.de/dokumente/textarchiv/2020/kw25-de-rechtsextremismus-701104>.

careers are socially embedded – hence, interventions should persuade not just those vying for office themselves that they can do so safely, but also provide such reassurance to their peers, friends and family. In line with previous research, my work suggests that interventions which explicitly address the concerns of dominant-group gatekeepers are particularly conducive to increasing participation among marginalized individuals (Cheema et al. 2023).

I see two promising avenues for further research. First, scholars should evaluate whether or not existing policy reforms successfully limit violence against candidates for office and office holders and improve democratic competition. Such investigations should also study whether these reforms stifle valuable forms of political expression, for example by creating uncertainty about the legality of certain speech acts. Combined with the findings of the present study, such policy evaluations can pave the way to develop more effective interventions that strengthen democratic competition while protecting robust political expression.

Second, the present study has thrown into sharp relief that legacies of marginalization structure responses to political violence. Women, queer citizens and nonwhite citizens are consistently more pessimistic about the likelihood of facing violence than their male, straight and white counterparts, an association that is as strong as it is robust. Exposure to information that political careers are safer than initially assumed has a greater effect on the willingness to support candidacies by female as opposed to male peers. In the mind of the average respondent, whether or not political activism raises safety concerns is a relevant question primarily for individuals belonging to historically marginalized groups.

Further work should do more to investigate why legacies of marginalization matter so much. Do legacies of marginalization affect media consumption habits and information acquisition? How, if at all, do marginalized identities differ in their effects on how individuals relate to political participation? How do dominant-group individuals reflect on the value of political engagement by their marginalized peers? These are but a few of the pressing questions raised by this paper that merit further study.

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# Supplementary Materials

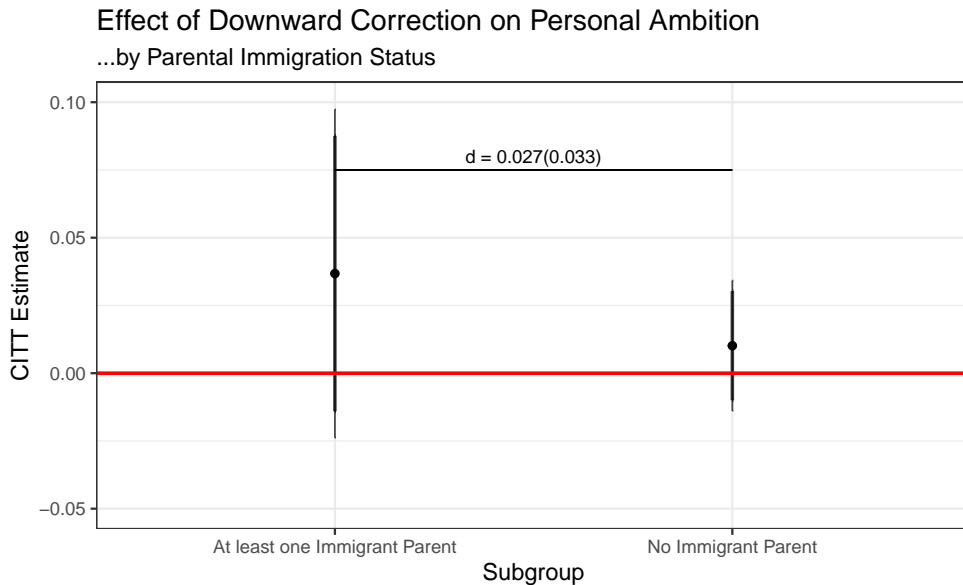
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# A Supplementary Analyses

## A.1 Pre-Registered Analyses

### A.1.1 Heterogeneous Effects by Parental Immigration Status

As part of my marginalization hypotheses, I expected individuals with recent immigration histories – such as those with parents who immigrated to the United States – to be more elastic to information about the risks of political engagement than individuals whose parents were born in the United States. Figure 10 shows estimated intent-to-treat effects separately for respondents with at least one immigrant parent and respondents with no immigrant parent. The estimated ITT for both subgroups of respondents is positive, with a slightly larger point estimate for respondents with at least one immigrant parent. However, the effect for this group is fairly imprecisely estimated, and the difference in effects does not reach conventional thresholds of statistical significance.



**Figure 10:** CITTs by Subgroup, Parental Immigration Status

I also reestimate  $\gamma_{Global}$  to remove differential effects due to different distributions of prior beliefs. The results are shown in table 7. Again, the effect of treatment is estimated to be larger for individuals with immigrant parents compared to respondents without immigrant parents. However, the estimated difference in effects is not statistically significantly different from zero.

$\hat{\gamma}_{Global}^*$	<i>DV: Personal Ambition</i>
Treatment $\times$ At least one Immigrant Parent	0.023
<i>Reference: NO IMMIGRANT PARENT</i>	(0.032)
Num. obs.	3458
Bootstrap Iterations	10k

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .

**Table 7:** Gamma Global, Parental Immigration Status

### A.1.2 Robustness, Video Treatment Delivery

The treatment – exposure to scholarly information about the frequency of threats directed at politicians – was delivered to treated individuals using a video. Embedding videos in surveys can cause problems if technical issues prevent respondents from following the content of the video. To minimize such issues, a video and audio check was implemented at the very beginning of the survey, requiring respondents to correctly identify objects displayed and sounds heard in an embedded video. Thirty-four respondents were screened out because they did not pass this device check.

I added a second layer of verification, asking respondents to self-report if the video treatment had been delivered correctly after being prompted to view it. Fortunately, only 15 of 2268 or 0.7% of respondents assigned to treatment (or 11 of 1748 respondents or 0.6% assigned to treatment with priors exceeding the benchmark) reported issues with the video delivery of treatment.

I reestimate table 2 from the main paper, excluding respondents who reported issues with treatment delivery. Table 8 displays the results. The point estimates and standard errors change only very marginally, if at all, when excluding respondents reporting failed treatment delivery.

## A.2 Exploratory Analyses

### A.2.1 Marginalization, Combined Indicator

In the paper, I present heterogeneity analyses using information on respondents’ gender, ethnic and sexual identities separately. Another analytical option would have been to combine information on these dimensions of marginalization to code a single indicator variable measuring group marginalization status. Here, I repeat the main heterogeneity analyses using a combined indicator, which takes the value 1 when a respondent’s gender is female or other *or* their sexual identity is not straight *or* their ethnic identity is nonwhite,

	<i>DV: Personal Ambition</i>		<i>DV: Support for Peer Ambition</i>	
	$\Delta$ -score	Lin	$\Delta$ -score	Lin
Information Treatment	0.018 (0.012)	0.019* (0.011)	0.137*** (0.023)	0.127*** (0.022)
Intercept	-0.005 (0.008)	1.343*** (0.008)	-0.157*** (0.016)	0.871*** (0.016)
Subgroup	priors > 2	priors > 2	priors > 2	priors > 2
Failed Delivery	Excluded	Excluded	Excluded	Excluded
R <sup>2</sup>	0.001	0.748	0.010	0.587
Adj. R <sup>2</sup>	0.000	0.748	0.010	0.587
Num. obs.	3488	3488	3488	3488
RMSE	0.344	0.331	0.669	0.644

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**Table 8:** Effects of Downward Correction on Political Ambition

$\hat{\gamma}_{Global}^*$	<i>DV: Personal Ambition</i>
Treatment $\times$ Marginalized	0.040
<i>Reference: DOMINANT</i>	(0.025)
Num. obs.	3441
Bootstrap Iterations	10k

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .

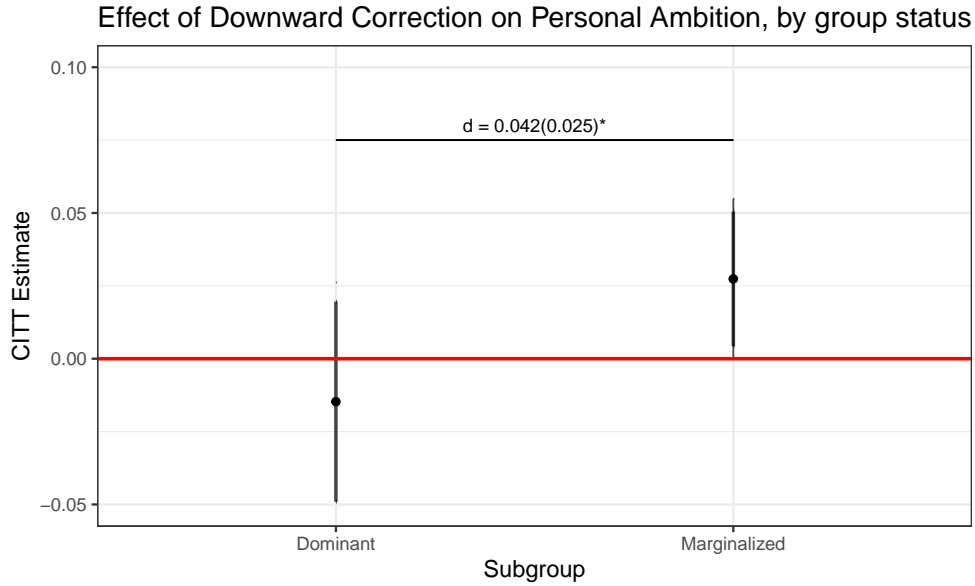
**Table 9:** Gamma Global, Marginalization, Combined Indicator

and takes the value 0 otherwise.<sup>45</sup>

Figure 11 shows estimated intent-to-treat effects separately for respondents identifying with dominant and marginalized groups. The estimated ITT for marginalized respondents is positive, while the same effect is estimated to be slightly below zero for dominant respondents, corroborating previous results. The difference in effects is statistically significantly different from zero, with  $p < .1$ .

I also reestimate  $\gamma_{Global}$  to remove differential effects due to different distributions of prior beliefs. The results are shown in table 9. Again, the effect of treatment is estimated to be larger for individuals identifying with marginalized groups compared to dominant groups. The difference in effects barely misses conventional thresholds for statistical significance.

45. I exclude respondents who have missingness in any of the three marginalization dimensions.



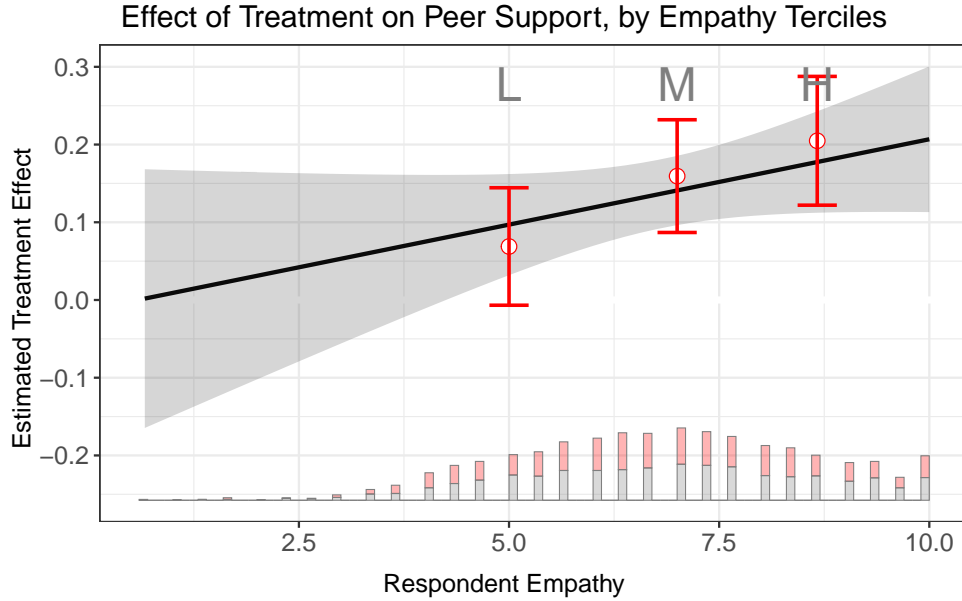
**Figure 11:** CITTs by Subgroup, Combined Indicator

### A.2.2 Peer Effects and Empathy

In the theory section of the paper, I conjecture that support for politically ambitious peers is elastic to information about the risks of running for office because individuals are empathetic towards their peers and seek to protect them from harm. Psychologists have documented that individuals differ in their capacity to feel empathy towards other human beings, and developed sophisticated tools to measure individual-level variation in empathy (Davis 1983). I use three questions from a survey module measuring empathy used by Clifford, Simas, and Kirkland 2021 to construct a composite individual-level measure of empathy.

If my conjecture about the role of empathetic concerns in explaining the treatment effect is correct, we should see that individual-level empathy moderates the effect of the information treatment on support for politically ambitious peers, with stronger treatment effects for more empathetic individuals. Figure 12 implements the tercile binning estimator (Hainmueller, Mummolo, and Xu 2019) to examine whether empathy indeed moderates the effect of exposure to downward-correcting information.

Figure 12 confirms my conjecture. Exposure to downward-correcting information on average increases support for politically ambitious peers by approximately 0.07 for respondents scoring in the bottom tercile on empathy, with the treatment effect tripling to 0.21 on average for respondents in the top empathy tercile. Put differently, the more empathetic respondents are, the more elastic their support for politically ambitious peers is to signals about the risk of harm.



**Figure 12:** Empathy Moderating Treatment Effect

### A.2.3 Priors and Social Media Sites

Further to the analysis in section 3.1., I also examined whether the association between marginalization status and prior beliefs is robust to conditioning on social media platform usage. The analysis also allows us to examine partial correlations between social media use habits and prior beliefs. Table 10 displays the results of a simple OLS regression of prior beliefs on marginalization variables, party preferences, basic demographics and indicator variables for social media use. Note that we only have information on social media use habits from respondents who indicated that they got news from social media at least rarely. This analysis is run on this subset of respondents.

Table 10 shows that among social media users, and when conditioning on partisan preferences, basic demographics, and social media use habits, the association between marginalization status and prior beliefs remains robust. Moreover, partial correlations suggest that particular social media use habits are associated with prior beliefs. On average, respondents who report using YouTube, TikTok, Instagram and Nextdoor to learn about the news have higher priors than respondents who report not using YouTube, TikTok, Instagram or Nextdoor, respectively. Of course, these partial correlations should not be interpreted as causal. They merely suggest that there is an association between different social media ecosystems and prior beliefs. The positive association between prior beliefs and reported usage of Nextdoor – a platform known for crime-related content – may reflect respondents particularly concerned about crime sorting into using Nextdoor, or respondents active on Nextdoor for idiosyncratic reasons growing more concerned about the risks of activism as



		<i>DV: Prior Beliefs</i>
		(1)
Woman		0.704*** (0.099)
Straight		-0.386*** (0.122)
White		-0.595*** (0.105)
Independent		0.119 (0.114)
No preference		0.136 (0.315)
Other party		0.422 (0.299)
Republican		-0.131 (0.125)
Facebook		0.053 (0.103)
YouTube		0.285*** (0.097)
Twitter/X		-0.095 (0.097)
Instagram		0.434*** (0.119)
Snapchat		-0.356 (0.230)
WhatsApp		-0.247 (0.226)
TikTok		0.566*** (0.120)
Reddit		-0.157 (0.101)
Twitch		0.030 (0.229)
Nextdoor		0.469*** (0.153)
Above Median Income		-0.115 (0.097)
Age		-0.017*** (0.004)
Any College		-0.486*** (0.099)
Intercept	49	5.909*** (0.264)
R <sup>2</sup>		0.087
Adj. R <sup>2</sup>		0.081
Num. obs.		3197
RMSE		2.560

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

a consequence of exposure to the Nextdoor information ecosystem.

## B Measurement and Coding

### B.1 Prior Beliefs

Respondents are asked to indicate their prior beliefs about the risk faced by politicians of receiving violent threats. Prior beliefs are elicited as follows:

#### *Prior Beliefs*

Out of ten local politicians in the United States, how many do you think have received death threats against them or their families because of their work?

Respondents indicate their prior beliefs using a slider with values ranging from 0 to 10 (integer values only). The task is introduced with a video vignette.

Respondents are asked to indicate their posterior beliefs as follows:

#### *Posterior Beliefs*

We would like to know again, to be sure: Out of ten local politicians in the United States, how many do you think have received death threats against them or their families because of their work?

Respondents indicate their posterior beliefs using a slider with values ranging from 0 to 10 (integer values only).

### B.2 Main Outcomes

Respondents are asked a standard political ambition survey item, first used by Fox and Lawless (2005). This item has been used extensively in research on political ambition, both in the United States and elsewhere. The question reads as follows:

Which best characterizes your attitude toward running for political office in the future?

Response options are:

- It is something I am unlikely to do (1)
- I would not rule it out forever, but I currently have no interest (2)

- It is something I might undertake if the opportunity presented itself (3)
- It is something I definitely would like to undertake in the future (4)

The question is asked in identical fashion both pre- and post-treatment.

The second outcome of interest measures how likely respondents would be to support others' political ambition. To elicit this information, I ask respondents how likely they would be to support a dear friend running for office in their town. The gender identity of the friend, conveyed through their first name and the pronouns used to refer to them, is cross-randomized with the information treatment. The question reads as follows:

Consider the following scenario: Your dear long-time friend **Tom/Sarah** is considering giving up **his/her** job and running for mayor in **his/her** town. **He/She** has asked for your opinion on this big life decision. How likely are you to support **him/her** running for office?

Response options are:

- Extremely unlikely (-2)
- Somewhat unlikely (-1)
- Neither likely nor unlikely (0)
- Somewhat likely (1)
- Extremely likely (2)

The question is asked in identical fashion both before and after the information treatment.

### B.3 Cross-Learning Outcomes

I elicit two placebo outcomes to better understand how respondents interpret the information provided by the treatment.<sup>46</sup>

First, it is conceivable that rather than updating their beliefs about the unconditional risk of experiencing violence in politics, respondents instead update their beliefs about the composition of U.S. local office holders. For example, upon learning corrective information suggesting that the proportion of office holders experiencing violence is lower than initially

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46. Cross-learning outcomes are only elicited after treatment, rather than pre- and post-treatment. I decided to remove pre-treatment elicitation of these cross-learning outcomes after receiving feedback from test survey takers that the questions caused confusion and attention loss, because the topic of threats against politicians had not yet been introduced in the survey flow. Obviously, this does not affect the causal interpretation of any effects on cross-learning outcomes, but merely the precision with which these effects can be estimated.

assumed, respondents might revise their beliefs about the composition of that population, rather than the unconditional risk of experiencing violence.<sup>47</sup> To verify whether or not respondents update in this way, I ask them to estimate the proportion of US local office-holders who are nonwhite, women or identify as gay:

Of all local politicians in the United States, how many percent do you think are members of the following groups?

Respondents then indicate their estimated proportions for (i) Black, Hispanic and Asian, (ii) women and (iii) identify as gay using sliders with possible values ranging from 0 to 100.

Second, respondents could interpret the information as relating to the relative risk of violence faced by different groups, rather than the unconditional risk of experiencing violence in politics. For example, upon learning corrective information suggesting that the proportion of office holders experiencing violence is lower than initially assumed, respondents might revise their beliefs about the relative risk of violence faced by the marginalized group relative to the dominant group.

To verify whether or not respondents update in this way, I ask them to estimate the relative risk faced by women, nonwhite politicians and politicians who identify as gay relative to men, white politicians and those who identify as straight:

Compared to white/male/straight politicians, do you think black/female/gay politicians are more or less likely to face threats of violence?

Response options:

- Much less likely (-2)
- Somewhat less likely (-1)
- Neither less or more likely (0)
- Somewhat more likely (1)
- Much more likely (2)

---

<sup>47</sup> The literature refers to such unintended updating as cross-learning (Haaland, Roth, and Wohlfart 2023, 20-21).

## B.4 Demographics

I investigate heterogeneity in treatment response between different demographic groups. I distinguish between individuals according to their gender identity, sexual orientation, ethnic identity, the immigration status of their parents, and household financial information. The questions are standard demographic questions for the United States taken from the Qualtrics Library. I will discuss each of the items in turn.

### *Gender*

How do you describe yourself?

Response options are:

- Male
- Female
- Non-binary/third gender
- Prefer to self-describe (open text)
- Prefer not to say

I create a binary variable taking the value 1 for respondents who are female and non-binary/third gender and who self-describe as a non cisgender identity, and 0 for respondents who describe themselves as male. Respondents who prefer not to disclose their identity are coded as missing.

As an alternative variable, I code respondents who are female as 1, and those who are male as 0. Respondents with other gender identities are excluded from this analysis.

### *Sexual Orientation*

Which of the following best describes your sexual orientation?

Response options are:

- Heterosexual (straight)
- Homosexual (gay)
- Bisexual
- Other
- Prefer not to say

I create a binary variable taking the value 0 for respondents who are gay, bisexual and other, and 1 for respondents who describe themselves as straight. Respondents who prefer not to disclose their sexual orientation are coded as missing.

I will check whether results are robust to coding respondents who indicate "other" as missing.

### *Ethnic Identity*

Choose one or more races that you consider yourself to be.

- White or Caucasian
- Black or African American
- American Indian/Native American or Alaska Native
- Asian
- Native Hawaiian or Other Pacific Islander
- Other
- Prefer not to say

Additionally, I ask:

Are you of Spanish, Hispanic, or Latino origin?

- Yes
- No

I code respondents who identify as White or Caucasian and do not identify as Latino as 1, and respondents who identify as African American as 0. Other respondents are excluded from this analysis. Respondents who indicate that they prefer not to disclose their identity are coded as missing.

An alternative binary coding codes all respondents who identify as white only and do not identify as Latino as 1, and codes *all other respondents* as 0. Again, those who prefer not to disclose their identity are coded as missing.

### *Parents Immigrants to U.S.*

I ask respondents:

Are one or both of your parents immigrants to the United States?

- Yes

- No
- Do not know/prefer not to say

I code respondents who answer "Yes" as 1, and those who answer "No" as 0. Respondents who do not know or prefer not to say are coded as missing.

*Pre-tax annual household income:*

Respondents were asked:

What was your total household income before taxes during the past 12 months?

Answer options were:

- Less than \$25,000
- \$25,000 to \$49,999
- \$50,000 to \$74,999
- \$75,000 to \$99,999
- \$100,000 to \$149,999
- \$150,000 or more
- Prefer not to say

This is the standard household income elicitation question proposed by Qualtrics. I exclude respondents who answer "prefer not to say". I code pre-tax annualized household income in two ways: First, I use an ordinal coding of the income brackets ("Less than \$25,000" coded as 1, "\$150,000 or more" coded as 6), imposing a linearity restriction on the analysis. Second, I create a binary variable with respondents with approximately above median household income (brackets of \$75,000 or greater) coded as 1 and respondents in lower brackets coded as zero. The median annual household income in the United States was \$70,784 in 2021, according to the US Census Bureau.<sup>48</sup>

*Highest Completed Education:* Respondents were asked:

What is the highest level of education you have completed?

- Some high school or less
- High school diploma or GED

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48. See <https://www.census.gov/library/publications/2022/demo/p60-276.html>, accessed 3 Jul 2023.

- Some college, but no degree
- Associates or technical degree
- Bachelor's degree
- Graduate or professional degree (MA, MS, MBA, PhD, JD, MD, DDS etc.)
- Prefer not to say

I code respondents whose highest completed level of education is either a bachelor's degree or a graduate or professional degree as 1, and all others as zero. Those who prefer not to say are coded as missing. I restrict the analysis to respondents over the age of 25, as younger respondents will still be in training.

*Children:* Respondents were asked:

Do you have care responsibility for any children under the age of 18? Please tell us how many children under the age of 18 you have care responsibility for.

- None
- One
- Two
- Three or more
- Prefer not to say

I code respondents who indicate having care responsibility for at least one child as 1, and all others as zero. Respondents who prefer not to say are coded as missing.

*Partisan Preferences:* Respondents were asked:

Generally speaking, do you think of yourself as a Republican, a Democrat, an Independent, or something else?

Answer options were:

- Republican
- Democrat
- Independent
- Other
- No preference

Respondents choosing "other" were invited to provide their own descriptors in an open-text field.



## B.5 News Consumption

I asked respondents about their principal sources of news. The exact items are loosely based on Pew Research Media Consumption Surveys<sup>49</sup>:

Now we would like to learn about where you get your news from. By news we mean information about events and issues that involve more than just your friends or family.

How often do you get news from...

The news sources mentioned were:

- Television
- Radio
- Newspapers and print
- Online news apps
- Online social media sites

Answer options were:

- Never (1)
- Rarely (2)
- Sometimes (3)
- Often (4)
- No answer (NA)

Respondents who indicated that they got news from online social media states at least rarely were also asked which specific sites they got news from:

Do you regularly get news or news headlines on any of the following social media sites or apps?

The sites mentioned were:

- Facebook
- YouTube
- Twitter

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49. An example of these surveys can be found here: <https://www.pewresearch.org/journalism/2020/12/08/measuring-news-consumption-in-a-digital-era/>, accessed Sep 25, 2023.

- Instagram
- Snapchat
- WhatsApp
- TikTok
- Reddit
- Twitch
- Nextdoor

Answer options were:

- No, don't regularly get news on this (0)
- Yes, regularly get news on this (1)
- No answer (NA)

## B.6 Empathy

I use three questions from a module measuring empathy by Clifford, Simas, and Kirkland 2021.

I ask respondents:

The following statements inquire about your thoughts and feelings in a variety of situations. For each item, indicate how well it describes you by choosing the appropriate letter on the scale: 0 (does not describe me well at all) to 10 (describes me very well).

The following statements are evaluated:

- “If I’m sure I’m right about something, I don’t waste much time listening to other people’s arguments.”
- “Other people’s misfortunes do not usually disturb me a great deal.”
- “I believe there are two sides to every question and try to look at both of them.”

Answers are given on an 11-point scale with values from 0 to 10 in increments of one. To construct a single measure of empathy, I invert the scales for the first two responses and then average over all three responses. Greater values on the final measure indicate greater empathy.

## B.7 Video Vignettes

Before eliciting prior beliefs from respondents, I introduce the task with a brief video vignette. The video can be seen under this URL: <https://youtu.be/bB5vHzHT5hw>. The spoken text is as follows:

Running for and serving in public office gives people across America an opportunity to contribute to their communities and work to improve lives. As mayors, members of school boards or local councils, elected officials get to make important decisions on education, transportation, or policing in the communities in which they live. It's an important job.

However, elected officials sometimes have to take decisions that not everyone agrees with. Disagreements can have serious consequences. Local politicians have faced harassment, abuse and even violence. They may be insulted, receive threats, or even be attacked for doing their jobs.

Some local politicians get death threats directed at them or their loved ones.

We want to know your best guess: Out of ten local politicians in the United States, how many do you think have received death threats against them or their loved ones because of their work?

To respondents in the treatment group, a second vignette is shown, containing a benchmark estimate derived from previous scholarship. The video can be seen under this URL: <https://youtu.be/Bsu9W7zJNqY>. The spoken text is as follows:

You indicated that you think **RESPONDENT PRIOR** local politicians in the United States have received death threats against them or their loved ones because of their work.

**Sadly/Fortunately**, threats are **more/less** common than you think. Research shows that 2 out of 10 local politicians have received death threats.

## B.8 Salience

First, I ask an open-ended question to understand what is salient to respondents as they reflect on the possibility of running for office:

Why have you personally not run for office? Please write down a few keywords to help us understand why you have not run for office.

Respondents were allowed space to formulate an open-ended response.

Second, I asked a more structured question to better understand which concerns respondents viewed as important to them as they reflected on the possibility of running for office.

We have collected some of the most common concerns people expressed when thinking about running for office.

Please indicate how important each of these concerns is to you.

The concerns were:

(1) "I don't think I have enough time to get involved in politics." (Time Concern) Time has long been identified – with money – as a key resource enabling or constraining political participation (Brady, Verba, and Schlozman 1995, 273). Particularly local-level offices often require significant time investments after work hours, and time emerges as an important constraining on office-seeking.

(2) "I am not interested enough in politics to make it a big part of my life." (Interest Concern) Holding office requires, at a minimum, interest in community governance and a willingness to embrace the routines of legislative or executive bodies.

(3) "I don't think I have enough money to get involved in politics." (Financial Concern) Money is the second key resource enabling or constraining political participation (273). Many local offices and even higher elected offices are typically poorly remunerated, creating financial obstacles to involvement in politics (Carnes 2018). Electoral campaigns also increasingly require candidates to engage in fundraising, an activity seen very negatively by many (Lawless 2011, 171-172).

(4) "I worry that I might be harassed, abused, threatened or attacked for getting involved in politics." (Safety Concern) Examining how important safety concerns are as an obstacle to seeking office is the key objective of this paper.

(5) "I don't think I could make a real difference to people's lives." (Impact Concern) The sense that politics are an effective avenue to produce positive change is an important motivation for seeking office (Gulzar and Khan 2024). Relatedly, fear of adverse policy changes coupled with a sense of underrepresentation can have powerful mobilizing effects on individuals (Clayton, O'Brien, and Piscopo 2023). Conversely, feeling that participation is irrelevant is likely to constrain political ambition.

(6) "I wouldn't feel comfortable being in the public spotlight." (Privacy Concern) Loss of privacy is often a direct consequence of candidacy, and has frequently appeared in work on political entry as a deterrent from running for office (Sutter 2006; Lawless 2011, 172-174). Privacy concerns are particularly acute for individuals who see themselves as living "nontraditional" lives such as sexual minorities (Wagner 2021).

Respondents rated each of these concerns on a five-point scale:

- Not at all important (-2)
- Slightly important (-1)
- Moderately important (0)
- Very important (1)

- Extremely important (2)

## C Sample Descriptives

## D Deviations from the Pre-Analysis Plan

The pre-analysis plan outlined a number of hypotheses grouped in different classes. This paper presents results for the **directional**, **marginalized groups** and **network support** hypotheses. The other hypotheses will be tested and results will be reported in a separate paper.

Here, I summarize changes from the PAP to this paper. The changes are mostly minor and were undertaken for presentational purposes, and do not touch on the substance of the hypotheses.

- $H_5$  and  $H_{5a}$ ,  $H_{5b}$ ,  $H_{5c}$  and  $H_{5d}$  from the PAP are subsumed under  $H_3$  in this paper.
- $H_{5Desc}$  is tested in the section on risk perceptions.
- $H_6$  from the PAP corresponds to  $H_2$  in this paper.
- $H_{6a}$  from the PAP corresponds to  $H_4$  in this paper.
- Line 3 of the code snippet on page 19 of the PAP contains an error, with the `lm_robust()` function taking an object outside of the scope of the function. The error is corrected in the replication code.

	<i>Min.</i>	<i>1st Quartile</i>	<i>Median</i>	<i>Mean</i>	<i>3rd Quartile</i>	<i>Max.</i>	<i>SD</i>	<i>NAs</i>	<i>Benchmark</i>
Age	18	30	38	40.89	50	99	13.53	0	39.00 <sup>a</sup>
Female or other	0	0	1	0.50	1	1	0.50	18	0.50 <sup>b</sup>
Straight	0	1	1	0.80	1	1	0.40	43	0.88 <sup>c</sup>
White	0	0	1	0.67	1	1	0.47	35	0.59 <sup>d</sup>
Democrats	0	0	0	0.49	1	1	0.50	0	0.29 <sup>e</sup>
Independents	0	0	0	0.26	1	1	0.44	0	0.40 <sup>e</sup>
Republicans	0	0	0	0.19	0	1	0.39	0	0.28 <sup>e</sup>
Left-Right Score	0	1	3	3.54	5	9	2.59	1	
Affective Polarization Score	0	16	41	42.38	66	100	29.38	1	
Personal Ambition (pre)	1	1	1	1.34	2	4	0.65	0	
Personal Ambition (post)	1	1	1	1.35	2	4	0.65	0	
Personal Ambition (change)	-3	0	0	0.00	0	3	0.33	0	
Support Ambition (pre)	-2	1	1	1.01	2	2	0.96	0	
Support Ambition (post)	-2	0	1	0.92	2	2	1.01	0	
Support Ambition (change)	-4	0	0	-0.09	0	4	0.65	0	
Prior Beliefs	0	3	5	4.88	7	10	2.67	0	
Posterior Beliefs	0	2	2	3.66	5	10	2.48	0	
Female Peer	0	0	1	0.50	1	1	0.50	0	
Treated	0	0	0	0.50	1	1	0.50	0	
Estimated % Women	0	25	35	34.42	40	100	14.19	0	
Estimated % Gays	0	5	10	13.46	17	100	12.60	0	
Estimated % Non-White	0	16	25	26.35	32	100	14.80	0	

**Notes:**

- a: Median age of U.S. population as estimated by U.S. Census Bureau in 2022, URL: <https://www.census.gov/newsroom/press-releases/2023/population-estimates-characteristics.html>.
- b: Share of women in U.S. population, U.S. Census Bureau in 2022, URL: <https://www.census.gov/quickfacts/fact/table/US/PST045222>.
- c: Share of U.S. population who identify as straight in U.S. Census Bureau Household Pulse Survey 2021, URL: <https://www.census.gov/library/visualizations/interactive/sexual-orientation-and-gender-identity.html>.
- d: Share of U.S. population "white alone" in 2022, URL: <https://www.census.gov/quickfacts/fact/table/US/PST045222>.
- e: Party Identification among U.S. citizens in Dec 2023, *Gallup*, URL: <https://news.gallup.com/poll/15370/party-affiliation.aspx>.

**Table 11:** Sample Descriptives

<b>Total N</b>	4582
... not U.S. citizens	8
... failed device check	34
... missing prior beliefs	1
<b>Usable N</b>	4539
Respondents with <i>priors</i> > 2	3499
Respondents with <i>priors</i> = 2	595
Respondents with <i>priors</i> < 2	445

**Table 12:** Observations