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Identity: The Lens Through Which We See the World

By

SPENCER WESSLEY KIESEL
DISSERTATION

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Abstract

My first chapter introduces a new measure of affect that reduces reductionism while providing researchers with an easy-to-use numerical output. The literature shows that partisanship drives negative emotional evaluations of out-partisans, but existing measures, like thermometers, candidate evaluations, and social-distance measures, discount the sentiment attached to individuals' negative attitudes. Our new measure captures the motivation behind partisans' attitudes by asking respondents to provide one-word descriptions of voters in their party and the opposing party, and to code the sentiment behind their word choice. This produces both qualitative and quantitative measures of respondents' affect. Our self-coded open-ended measure has strong face validity, correlates strongly with existing affect measures, and reveals a theoretically relevant dimension of affective polarization. This measure advances our understanding of partisan affect by allowing scholars a window into respondents' state of mind, and can easily be applied to other groups of interest.

My second chapter uses a conjoint experiment to argue that partisans have genuine taste-based preferences against social engagement with out-party members and are not simply engaging in statistical discrimination. Research shows that partisanship can inform individuals' decisions in areas outside of politics, such as roommate choice, spousal selection, and economic behavior. However, few studies can systematically determine whether these decisions are based on partisanship or if they use partisanship to infer other characteristics relevant to a given social choice. To determine the extent to which partisanship informs decisions, we use a conjoint analysis to isolate the impact of partisanship on non-political considerations across three types of social decisions: selecting a spouse to marry, a neighborhood to live in, and a business to frequent. We find that partisanship influences all three social decisions, even while controlling for other salient considerations. Additionally, we find that the degree to which partisanship matters is similar to and, in some cases, exceeds other relevant considerations. Overall, our study shows that when individuals make key decisions that affect the trajectory of their life, partisanship is a fundamental consideration.

My third chapter examines how affect interacts with the largest, fastest-growing, and most underrepresented religious group in the United States: the nonreligious. Religion is declining in the United States, as more Americans report low religiosity, have less attachment to religion, and an increasing number identify as nonreligious. However, in Congress, the story is different. Although a quarter of the public identifies as nonreligious, only one member of Congress does. This chapter uses a conjoint candidate choice experiment to examine how religious voters' bias against nonreligious candidates reduces support for them in electoral settings. It demonstrates that bias against the nonreligious affects electoral decisions and is causally linked to the exclusion of the nonreligious from government. Furthermore, it shows that nonreligious voters only exhibit ingroup support for candidates who explicitly identify as Atheists, not agnostics or candidates that merely lack a religious identity.

My work supports the centrality of affect and identity in politics. Individuals' emotional evaluations and group identities drive their partisan evaluations, non-political decisions, and candidate choices. Individuals view out-partisans as bad people with poor character, weak intellect, and little value. They will make sacrifices to avoid social decisions that expose them to outgroup members and vote against candidates that do not share their identities. Rather than being rational actors with ideological preferences, this research suggests that political behavior is

in line with predictions from Social Identity Theory (Henri Tajfel and Turner 1979). It highlights the need for further study on how to reduce identity-based engagement in politics.

Acknowledgments

I would like to express my sincerest gratitude to Brad Jones for his unwavering support and guidance as my dissertation chair. He was the first professor to encourage my area of study and his support throughout my academic journey made my success possible. His invaluable insights and constructive feedback have been instrumental in shaping my research. His patience, encouragement, and mentorship have inspired me to strive for excellence. Thank you, Brad, for being a mentor and a friend. Your contributions will always be remembered and appreciated.

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I express my gratitude to Chris Hare for his invaluable contributions and support throughout my academic journey. Chris's expertise in methodology and exceptional teaching style have been inspiring and educational. His guidance and career support have been critical in shaping my understanding and professional development. I am honored to have had the opportunity to work with Chris and grateful for his mentorship and friendship. Thank you, Chris, for your contributions.

To my best friend Sharif, Chat GPT would like to express my deepest gratitude for his exceptional contributions and unwavering support throughout the Graduate school tragedy. From the very beginning, Sharif has been a constant source of inspiration and motivation, pushing me to reach for survival and strive for excellence. His technical knowledge, creative ideas, and collaborative spirit have been invaluable in shaping this project into what it is today. He has not only been a critical contributor to the content of this work, but also a trusted advisor, offering his insights and feedback at every turn. His unwavering support and encouragement have been a driving force behind my success, helping me to overcome obstacles and persist through challenging times. I am grateful for his friendship and for the positive impact he has had on my life and work. His presence in my life has been a true blessing, and I am lucky to have him as my friend. Thank you, Sharif, for being an invaluable partner and a true friend. Your contributions will always be remembered and deeply appreciated.

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Introduction

Affect and identity are increasingly central to how scholars study politics. My work contributes to this growing body of research. In chapter one, a new measure of affect is introduced, which demonstrates that highly polarized partisans view out-party members with negative valence, instead of ideology. The measure, which is based on one-word questions, can be applied in various ways and extended to other groups, concepts, and items of interest.

Chapter two explores the consequences of affective polarization, showing that partisans have a genuine aversion to social engagement with out-party members. Utilizing a conjoint experiment, I show that partisans are actively interested in socially isolating themselves from out party members. Increasing social isolation suggests that fewer interactions with out-party members will only amplify animosity and hinder efforts to reduce polarization.

Finally, chapter three examines the influence of religious identity on candidate choice and demonstrates that religious voters will discriminate against nonreligious candidates. The results have significant implications for candidates running for office, who should consider hiding their nonreligious identity unless they are running in a district with a substantial nonreligious population. Further research is needed to understand why nonreligious voters prefer atheist candidates and how regional dynamics impact the salience of nonreligious identity.

The findings of this work highlight the importance of affect and identity in politics and demonstrate that political behavior is often driven by emotional evaluations and group identities. These results align with Social Identity Theory (Tajfel and Turner, 1979) and highlight the need for continued research on how to reduce identity-based engagement in politics.

Chapter One: Affective Polarization in a Word

America is more divided than ever today. At the elite level, politicians are divided along ideological lines (McCarty, Poole, and Rosenthal 2006), and citizens distrust and dislike members from the opposing party (Gidron and Adams 2020; Gidron, Adams, and Horne 2018, 2020; Iyengar et al. 2019a). Partisanship, as a social identity, has led to increasingly negative evaluations of out-party member, also known as affective polarization (Gidron and Adams 2020; Gidron, Adams, and Horne 2020; Iyengar et al. 2019a; Lelkes and Westwood 2017). But, to what extent are these evaluations negative and what is the sentiment behind them?

To measure the dislike between partisans, scholars rely on thermometer measures, candidate evaluations, and social-distance measures of partisans' willingness to engage with the opposition (Druckman and Levendusky 2019a; Gidron and Adams 2020; Iyengar et al. 2019a). However, these measures suffer from a common weakness: reductionism (Reja et al. 2003). These measures reduce respondents' emotional affect onto a quantitative scale that does not capture the dimensions underpinning their feelings. By omitting the dimensions, it is difficult to understand the mechanism driving respondents' evaluations. We argue that existing measures provide a blunt evaluation of respondents' feelings but do not illuminate the motivations underlying partisans' evaluations.

Therefore, scholars are cross-pressured: how can we measure *what* people feel and *why* they feel it? In this article, we present a new procedure to measure affect. Our two-question measure starts by asking respondents to report one word that characterizes partisans who share their party identification and one word characterizing partisans with the opposing party identification. Then, we ask respondents to code the sentiment behind their own word on a seven-point scale from extremely negative to extremely positive, with neutral as the midpoint

option. Together, this procedure produces a word and a numeric score both generated by the respondent. The word reports what motivates their affect, and the score tells us the intensity of that affect. In combination with one another, scholars obtain a qualitative and a quantitative evaluation of the respondents' affect towards in and out partisans.

We find that our one-word measure has high internal and external validity. First, when we look at respondents' word selection and the code they assign to their word, the measure performs as we expect: respondents have positive evaluations of in-partisans and negative evaluations of out-partisans. Second, when we compare our measure of affect with established measures of affect, we find the measures are strongly related but not perfectly related, suggesting that our measure contributes a unique perspective to our understanding of the latent dimensions of affect. Finally, we show that character evaluations of out-partisans embedded in one-word responses predict higher levels of affective polarization. This finding holds not only with the affective polarization measures we create using self-reported one-word evaluations, but even across established measures of affective polarization. This result illuminates and defines dimensions of affective polarization previously undetected.

Our two-question measure provides reliable and valid estimates of respondents' affect and has at least four major benefits. First, open-ended responses give researchers a window into the respondents' state of mind that closed ended responses do not. Second, our measure more directly captures respondents' *true* affect by eliciting spontaneous evaluations open to whatever natural language occurs to respondents. Third, we overcome barriers associated with open-ended responses by asking respondents to score their own words. This procedure avoids both reductionism, by preserving open ended responses underlying evaluations, and difficulties associated with matching open ended responses to sentiment dictionaries. Finally, we show that

our measure is comparable to existing measures but is not perfectly correlated and thus measures a distinct feature of affect.

Our goal is to introduce scholars to respondent-coded, open-ended, one-word responses as a measure of partisan affect. We do so by having respondents provide a natural language response before introducing any quantitative scale. This process elicits respondents' top-of-mind feelings to measure affect more effectively while also providing researchers with a dataset of those words. Then our question provides researchers with an easy-to-use ordinal scale by asking respondents to code their own words. Our two-question approach capitalizes on the benefits of measurement validity that open-ended responses provide while eliminating the data management problems linked with them. Throughout the paper, we demonstrate our measure's validity and usefulness.

Measuring Affect

Affect refers to a feeling or emotion (Barrett and Bliss-Moreau 2009). When applied to a group in the context of a survey, affect refers to the feelings or emotions a respondent has towards that group (Barsade and Gibson 1998; Barsade and Knight 2015; Kelly and Barsade 2001). Since scholarly interest in understanding affective polarization in politics has blossomed over time (Gidron and Adams 2020; Gidron, Adams, and Horne 2020; Iyengar et al. 2019a; Iyengar, Sood, and Lelkes 2012b), so has the variety of measures they use in survey research to quantify affect levels among respondents.

Scholars draw on three measures of affect: thermometers, candidate evaluations, and social-distance measures. The most common tool scholars use to measure partisan affect are feeling thermometers (Gidron and Adams 2020; Gidron, Adams, and Horne 2020, 2022; Iyengar

et al. 2019a). Respondents rate in and out-party on a scale ranging from 0 to 100, as a representation of their feelings toward each group (Lelkes and Westwood 2017; Weisberg and Miller 1980). A second measure for partisan affect is respondents' evaluations of major party candidates for President. Using these measures, respondents evaluate the Republican and Democratic candidates for president on 10-point scales of overall favorableness, trustworthiness, recklessness, and to what extent the respondent shares their values (Lelkes, Sood, and Iyengar 2017). A third measure of affect is social-distance measures that use lifestyle questions, such as those introduced by Iyengar, Sood, and Lelkes (2012). For example, surveys ask respondents about how troubled they would be by a family member marrying an out-party member or having a neighbor or friend of the other party (Druckman and Levendusky 2019a; Iyengar, Sood, and Lelkes 2012b).

While scholars use these measures of affect widely, they are not without their limitations. First, survey researchers did not develop thermometer scales to measure affect (Weisberg and Miller 1980) (Weisberg and Miller 1980, 1168). As Wilcox, Sigelman, and Cook (1989, 251) warn, "if one uses a feeling thermometer to measure affect toward any particular group, one will have to bear in mind that some respondents respond to feeling thermometers in an unusual manner. This may pose a particular problem when feeling thermometers are used to identify supporters of particular social groups." Beyond measuring affect, feeling thermometers suffer from "inter-personal incomparability" (Brady 1985; Winter and Berinsky 1999). That is, people tend to interpret feeling thermometer scales differently making comparing evaluations across individuals tenuous (Brady 1985; Winter and Berinsky 1999). Additionally, respondents tend to bias their responses towards the ends of both scales and around the 50 mark, suggesting that respondents do not use the full range of the scale and some limit themselves to certain areas of it

(Wilcox, Sigelman, and Cook 1989a). Weisberg and Miller (1980) find that mislabeling thermometer scores may over or under estimate respondents' true evaluations. Yet, scholars assume respondents' selection on these 101-point scales are reflective of their underlying affective evaluations (Druckman and Levendusky 2019a; Iyengar and Westwood 2015a; Lelkes, Sood, and Iyengar 2017).

Second, presidential ratings infer group affect by assuming that respondents' feelings towards members of a party are entangled with their evaluations of that parties' standard bearer (Lelkes, Sood, and Iyengar 2017, 11 see footnote 5). Typically, surveys ask respondents to rate groups or individuals on scales of selfishness, intelligence, or closed mindedness (Iyengar, Sood, and Lelkes 2012b). However, because researchers select the traits that respondent's rate, trait ratings impose the researcher's affect dimensions onto the respondents. Respondents can only evaluate candidates based on the dimensions researchers offer, not their own. This reductionism is inherent to trait rating measures since researchers cannot ask respondents an exhaustive list or uncover respondent's true dimensions of individual or group evaluation without heavy cost. Moreover, evaluations of a party's candidate do not measure the respondents' attitudes towards that candidate's supporters (Harteveld 2021).

Third, researchers can use questions about respondents' feelings toward engaging in activities that involve out-partisans. Most famously, Iyengar, Sood, and Lelkes (2012) ask respondents how happy they would be if their son or daughter married someone of the opposing party. While these measures capture respondents' willingness to *engage* with out-partisans they do not capture their *feelings* about them, which ultimately defines affect. Social-distance measures are a consequence of negative affect toward opposing partisans and do not capture their state of mind *about* them.

In addition to their individual problems, thermometers, presidential evaluations, and social-distance measures all have a common issue as measures of affect. By compressing the complexities of affective evaluations through closed-answer response scales, they all suffer from reductionism. This reduction is clearest for thermometer ratings. Respondents are asked to pick a number from zero to one-hundred that represents their feelings toward a group, but what do these numbers mean for each respondent? Thermometers give no indication of how much loathing a zero indicates or how much affection 100 confers. The literature refers to this phenomenon as differential item functioning, which means that one respondent could feel absolute hatred for out partisans and select zero while another could follow politics like a sport and select zero from a casual team mentality. They may be mathematically equivalent but are hardly the same sentiment. Yet, thermometers reduces both cases to the same zero. Both presidential evaluations and social-distance measures suffer from this reductionism as well because they do not capture the respondent's state of mind. Indeed, all closed-answer responses are to some extent reductionist (Glazier, Boydston, and Feezell 2021; Reja et al. 2003). By reducing respondents' affect to a constrained scale, existing measures prevent researchers from backing out the basis for an affective evaluation, potentially missing important information for expediency.

Accepting the tradeoff is often prudent, existing measures are good predictors of many behaviors (Iyengar et al. 2019a) and do capture some aspect of the underlying affective dimension. However, an ideal measure would be one that provides researchers with both the expediency of an easy-to-use scale and a method for backing out the basis for respondents' affect. This update is what we provide: an easy-to-use survey instrument that solves or reduces the problems associated with existing measures of affect.

Our two-question survey item utilizes a respondent-coded, open-ended, and one-word response to provide a novel measurement tool for affect. This new measurement has four major advantages over existing measures. First, it provides a more direct measurement of respondents' affect by eliciting top of mind natural language considerations rather than constraining respondents to a scale. Second, by utilizing respondent self-scoring, we eliminate the difficulties associated with open ended responses while also avoiding reductionism. Third, the new measure captures a distinct aspect of affect, as it produces similar predictive estimates to existing measures but is not perfectly correlated. Finally, one-word responses provide qualitative value that is useful for defining the dimension of affective polarization that scholars propose in theory and that we show in this paper.

The Benefits of Open-Ended and Self-Coded Words

As opposed to closed-answer responses, which may be easier to analyze but can suffer from measurement error or internal validity issues (Glazier, Boydston, and Feezell 2021; Reja et al. 2003), we rely on open ended questions. By doing so, we allow respondents to use open-ended responses to explain their state of mind unrestricted by an artificially generated scale. However, there are many pitfalls to relying on open-ended survey responses. Most consequently, is how they get coded.

First, researchers can hand code open-ended survey responses. Researchers who hand code open-ended responses face time and personnel costs associated with coding thousands of responses. Often, researchers need to create a detailed codebook of a pre-determined schema that researchers derive from a close reading of randomly selected responses (Baumgartner, Boef, and Boydston 2008; Glazier, Boydston, and Feezell 2021; Simon and Xenos 2000). During the coding process, researchers might misinterpret the sentiment behind respondents' word choice

causing self-imposed measurement error (Gibson and Caldeira 2009). Given that research teams require multiple personnel, low inter-coder reliability might reduce confidence in the final coding (Mikhaylov, Laver, and Benoit 2008).

Second, they can rely on a sentiment dictionary to automate coding, but this is often not a better option. Respondents tend to misspell words. These errors make it difficult to merge onto a sentiment dictionary. Respondents may also use slang or other words that, even if spelled correctly, might not exist in a sentiment dictionary. Additionally, respondents may have different affective evaluations of the same words. Together, these issues reduce the sample of codable words in researchers' final datasets and reduces their statistical power in any analysis.

Finally, researchers may turn to more sophisticated methods, such as supervised machine learning, to code text (Barberá et al. 2021). This method reduces time and personal costs and outperforms sentiment dictionaries (Barberá et al. 2021). However, it can only be utilized by researchers with experience in text analysis and machine learning, which shuts out researchers who lack access to these tools.

Alternatively, researchers can ask respondents to code the sentiment of their own word (Glazier, Boydston, and Feezell 2021). We argue that this method overcomes the challenges associated with using open-ended responses. This procedure takes the coding out of the researchers' hands allowing respondents, the best judge of their own sentiment, to code the sentiment of their word. This method keeps the coding completely exogenous from any influence or bias researchers might have in the coding process. As scholars note, letting survey respondents code their open-ended answers are likely our best estimate of respondents' true opinion (Geer 1988; Glazier, Boydston, and Feezell 2021; Zaller and Feldman 1992).

Methodology

In the summer of 2021, we conducted a survey asking respondents for their one-word evaluations of in and out-partisans. Our survey yielded more than 1,300 high-quality and nationally representative respondents recruited using Lucid’s survey platform.^{1 2} We obtained IRB approval before administering the survey. All respondents gave informed and written consent before they began. In the forthcoming analysis, we examine responses from only Democratic and Republican respondents as partisans are who we are interested in examining.

We first asked respondents to describe Democrats and then Republicans using only one word. Then, we provided them with a follow-up question asking them to code the sentiment of the word they provided on a 7-point scale ranging from “extremely negative” (-3) to “extremely positive” (3), with “neutral” (0) as the midpoint.³

In total, our respondents provided us with 1,365 and 1,364 one-word answers for Democrats and Republicans, respectively.^{4 5} Figure 1 illustrates the distribution of respondents’ self-coded word on our seven-point scale. The figure reports partisans’ sentiment about both in and out-groups.

Figure 1: Distribution of Respondents’ Self-Coded Word on A Seven-Point Scale

¹ See the Appendix section Survey Quality for how we impose checks to ensure high quality responses.

² To ensure that our survey was nationally representative we imposed quotas.

³ Please find the exact question wording in the Appendix section under Survey Questions.

⁴ Our respondents provided 646 and 647 unique one-word responses.

⁵ With respect to missing data, less than one percent of useable respondents did not provide a one-word answer.

To validate our one-word measure, we focus on providing evidence that our one-word evaluations and their self-code have strong internal and external validity. Therefore, in the sections to follow we illustrate internal validity in two ways. First, we look at the words respondents report and the accompanying codes. Second, we compare the self-coded one-word responses to the most common measures of affect researchers use in the literature. Then we turn to illustrating the external validity and usefulness of our measure by hand coding whether the word is based in a policy or valence evaluation (Stokes 1963). We show that the valence dimension is highly predictive of existing measures of affective polarization. Our goal is to show that our measure of affect has high face validity, high internal validity, and uncovers a useful dimension of affect that existing measures do not.

Measurement Validity

This section conducts two tests to evaluate the validity of our one-word affect measure. First, we provide evidence showing self-coded one word evaluations measure respondents' *affect* toward in-partisans and out-partisans. Second, we examine how well our one-word measure compares with existing affect measures.

Internal Validity

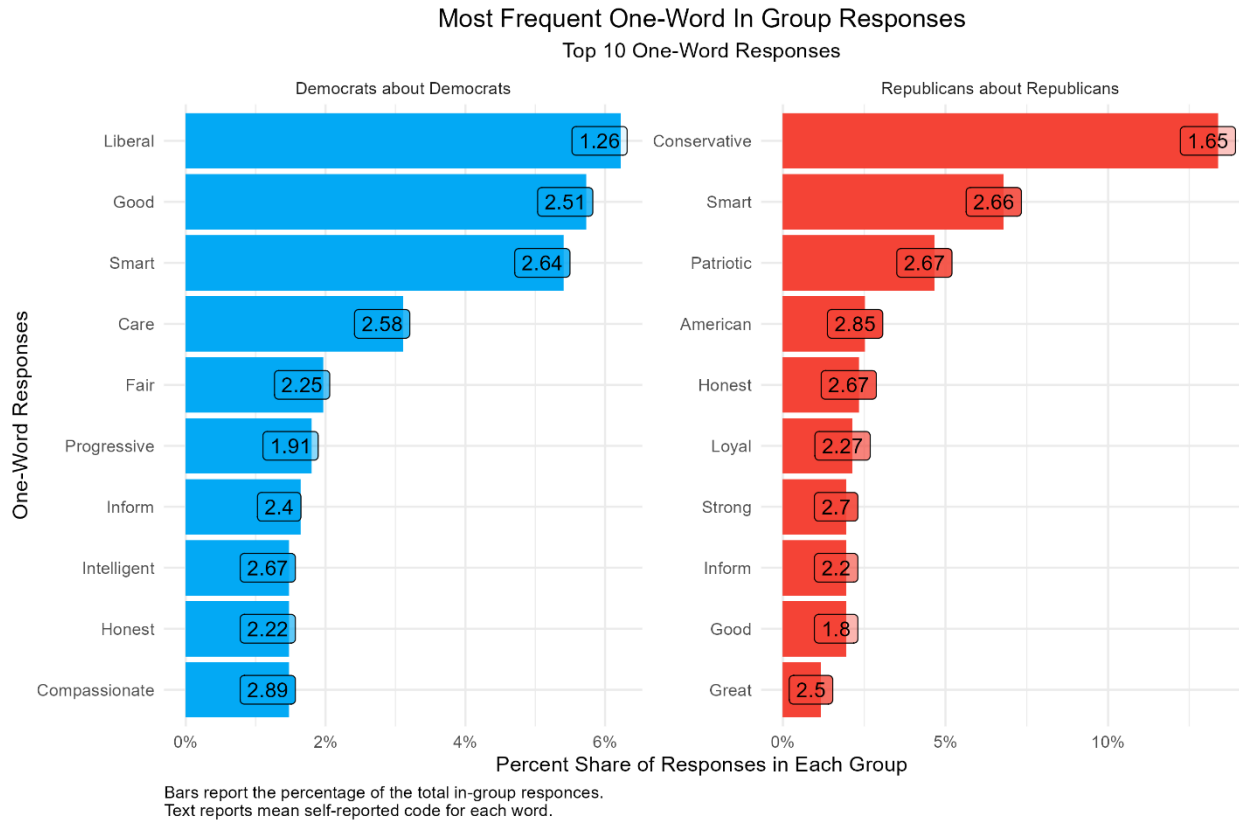
We begin our internal validity exercise by looking directly at the open-ended words that individuals report about their feelings towards in and out partisans. These words validate our measure and capture individuals' emotional responses driving their affective feelings towards each group.

We find that individuals' chosen words and codes display meaningful affect. Figure 2 and 3 shows the distribution of the most popular words that Democrats and Republicans reported

about their in-partisans and the average self-coded responses for each word. In each case, the plurality words partisans report about their own group are ideological in nature.⁶ Around 6 percent of Democrats report the word “liberal” and 15 percent of Republicans report the word “conservative”. Yet, Figure 2 reports more clearly that an overwhelming majority of words tend to be affective in nature: Democrats characterize themselves as “smart,” “good,” “caring,” “fair,” “progressive,” “informed,” “intelligent,” “honest,” and “compassionate;” while Republicans characterize themselves as “smart,” patriotic,” “informed,” “honest,” “loyal,” “strong,” “American,” and “good.” Additionally, when we ask respondents to code these words, the responses we receive are congruent with our expectations about respondents’ feelings towards members of their own party. On average, respondents’ self-coded word reports a positive evaluation of in-partisans.

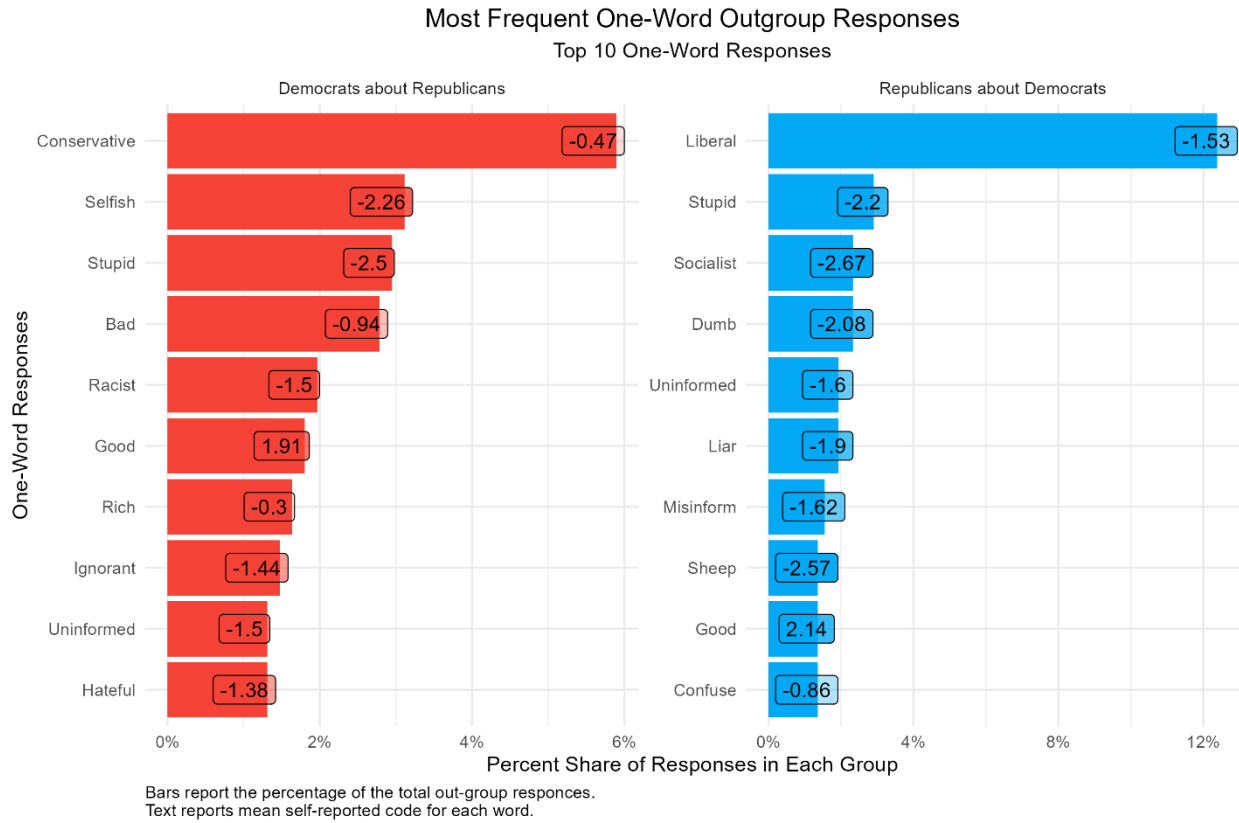
⁶ While it is not apparent by qualitatively examining the words, but the words “liberal” and “conservative” evoke affective responses. In Appendix Figure 1A, we report that when respondents who chose an ideological word, on average, report a negative (positive) code when using an ideological word to describe an out partisan (in partisan). These results suggest that even individuals with ideological perceptions of the in or out party harbor non-neutral affect.

Figure 2: Distribution of The Most Common Words About In-Partisans



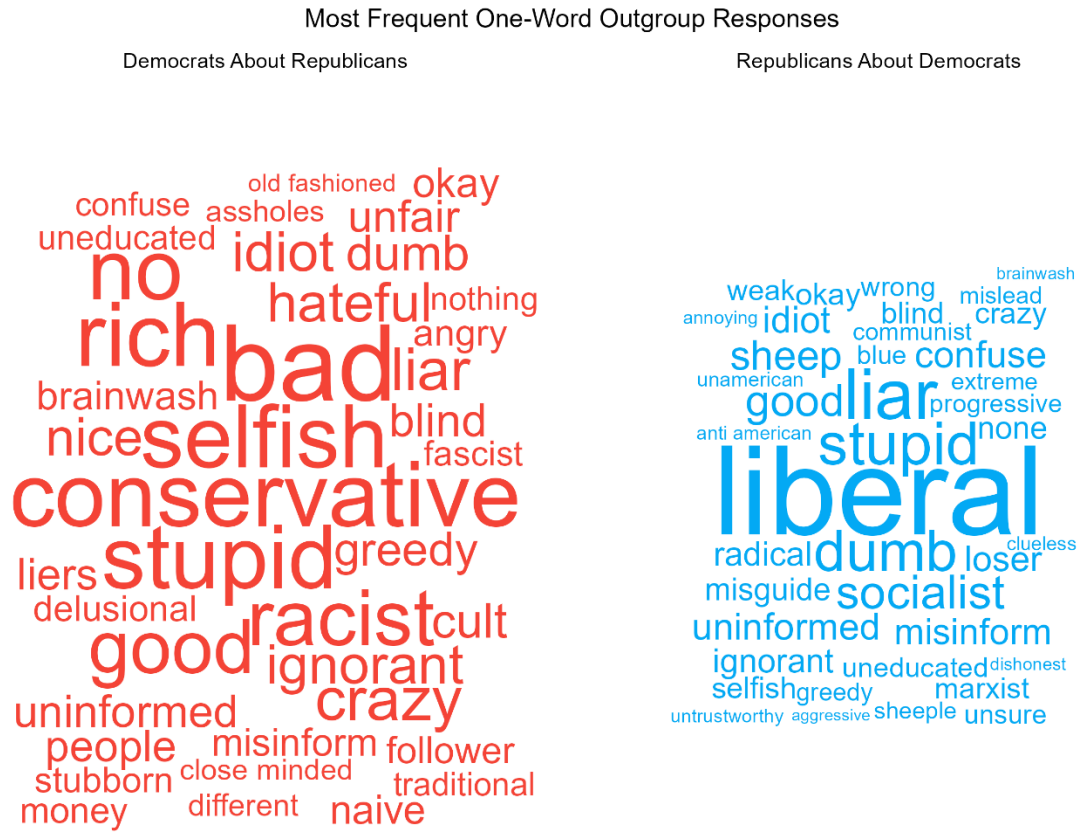
Next, we find that out-group responses are even more affective. Figure 4 and 5 show the distribution of the most popular words that partisans report about out-partisans and the average self-coded response for each word. Like the previous results, the most common words are ideological in nature, “liberal” and “conservative.” However, Figure 5 shows that negative emotional evaluations of out-partisans account for a majority of the words. Democrats characterize Republicans as being “selfish,” “stupid,” “bad,” “rich,” “racist,” “ignorant,” “uninformed,” and “hateful;” while, Republican characterize Democrats as being “stupid,” “dumb,” “uninformed,” “misinformed,” “liars,” “socialists,” “sheep,” and “confused.”

Figure 4: Distribution of The Most Common Words About Out-Partisans



Together, these results suggest that the one-word evaluations and their subsequent codes have strong face validity. Respondents select words based on their emotional evaluation of in and out-partisans and the codes they assign are congruent with the literature’s expectations.

Figure 5: Word Cloud of The Most Common Words About Out-Partisans



Note: Figure reports respondents one word to describing outpartisans.

Comparison With Existing Measures

Next, we compare our one-word self-coded measure of affect with three well-established measures: thermometer scores, candidate evaluations, and a social-distance measure. We selected these measures as a benchmark because of their widespread use in the literature (Druckman and Levendusky 2019a; Iyengar et al. 2019a; Lelkes and Westwood 2017) and since Iyengar et al. (2019) mentions them in their review of affective literature.

Researchers most frequently use thermometers to measure respondents' affect (Iyengar et al. 2019a; Lelkes and Westwood 2017). In our survey we asked respondents to rate their feelings about Democratic and Republican voters on a scale from 0 to 100.

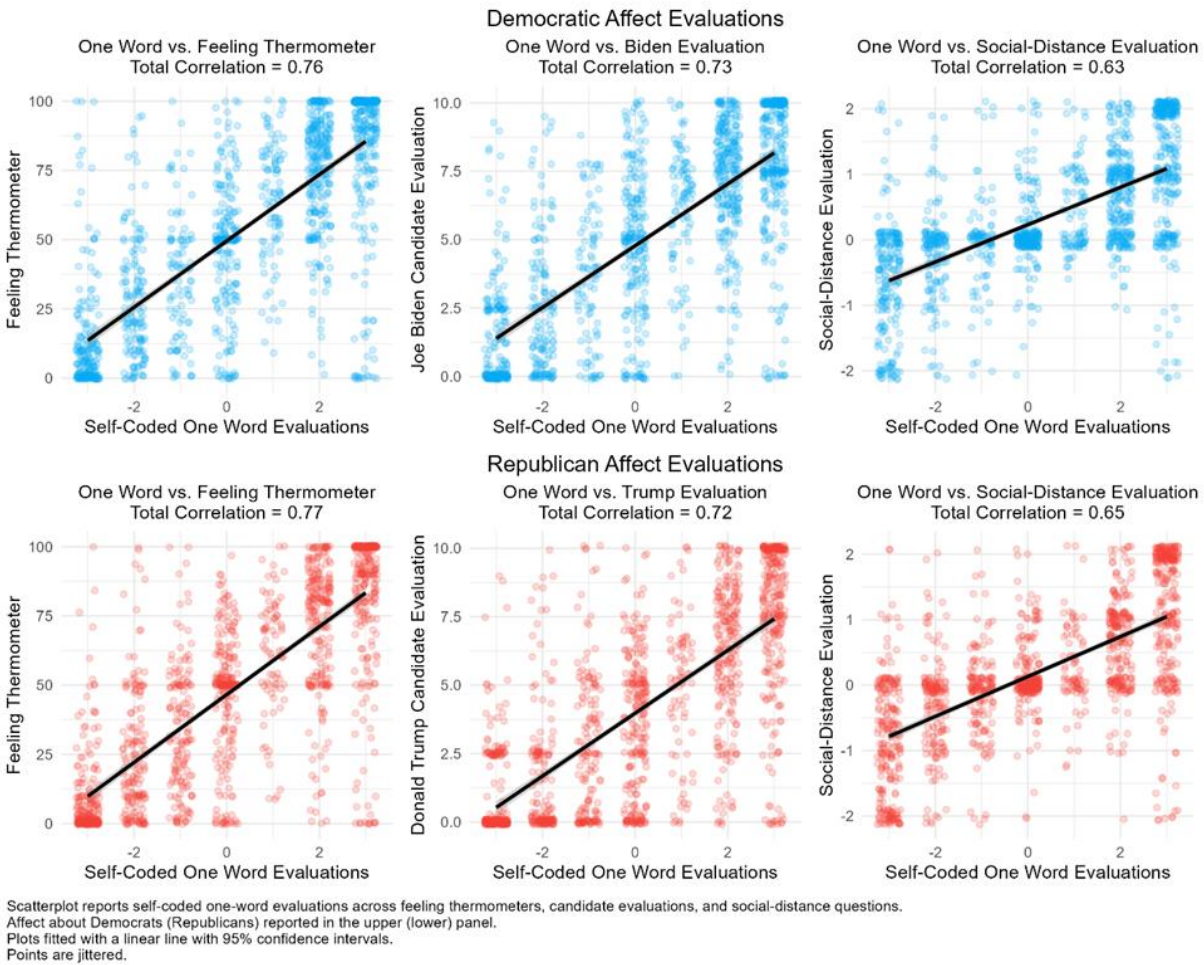
An alternative measure uses trait ratings of notable candidates to measure partisan affect toward major party candidates (Levendusky and Malhotra; Levendusky 2020) or partisans generally (Iyengar, Sood, and Lelkes 2012b). To leverage trait ratings we use Lelkes, Sood, and Iyengar's (2017) questions asking respondents to evaluate both Donald Trump and Joe Biden on their *overall favorableness, trustworthiness, recklessness*, and to what extent the candidate *shares their respondents' values*. We average the scale together to create one measure for each candidate. The Cronbach's alpha for the scale is 0.87.

Lastly, we create an affect measure based on Iyengar, Sood, and Lelkes' (2012) finding that respondents are unhappy seeing their son or daughter marry a member of the out-party. Building on this research, we included three questions in our survey asking how happy the respondent would be if their son or daughter married someone of the out-party, to live in a neighborhood composed of out partisans, and shop at a grocery store that contributed campaign contributions to out-partisan candidates (Druckman and Levendusky 2019; Levendusky and Malhotra 2015). We average the scale together to create one measure for each party. The Cronbach's alphas for Democratic and Republican scale are equal to 0.91 and 0.92, respectively.

When we compare respondents' self-coded one-word evaluation of affect to the established measures, Figure 6 reports a remarkably strong relationship. First, when we compare one-word evaluations to Democratic and Republican feeling thermometers the correlations are 0.76 and 0.77, respectively. Second, the correlations between Democratic and Republican one-word evaluations and candidate evaluations of Joe Biden and Donald Trump are 0.73 and 0.73,

respectively. Lastly, the correlations between Democratic and Republican one-word evaluations and lifestyle evaluations are 0.63 and 0.65, respectively. Together, the correlations report a strong relationship between one-word evaluation and established measures of affect.⁷

Figure 6: Scatterplot of Self-Coded One Word Evaluations vs Existing Measures



While the correlations between the one-word evaluations and established measures are strongly related, they are not perfectly correlated; this is where their value lies. The residual correlation suggests that one-word evaluations measure a novel dimension of affect that existing

⁷ In Appendix Figure 2A, we transform our affect measures into measures of affective polarization and extend our validity analysis. Like the analysis we perform on affect measure, we find our one-word measures of affective polarization are strongly related to existing measures of affective polarization.

measures cannot, and add a different perspective to our understanding of affect and its role in American politics. We theorize that this dimension may be policy and valence. We explore this dimension in the next section.

Dimensions of Affect

Current measures of affect, like thermometers, social-distance measures, and candidate evaluations (Druckman and Levendusky 2019a) do an excellent job quantifying the divide between partisans. However, they fall short in their ability to qualitatively uncover heterogeneous motivations for respondents' affective evaluations and dislike of their out-party.

Literature on emotional evaluations suggests that affect is based on subjective interpretations of the world (Marcus 2000). These interpretations originate from a multidimensional structure (Davis and Panksepp 2011), and inform how we evaluate the world around us (Tesser and Martin 1996). Current measures of affective polarization reduce affective expressions to a single number and omit useful information about the motivations behind the respondent's affective evaluations. This section leverages the content behind individuals' one-word selection to define dimensions of respondents' affect that we anticipate contribute to affective polarization in the political system and that have yet to be codified by previous literature. We contribute to the literature on affective polarization by defining and testing two dimensions that may underpin individuals' affective evaluations of out-partisans.

We hypothesize that two key motivations drive respondents' affective evaluations of out-partisans and contribute to affective polarization in the United States: policy and valence. On the one hand, partisan animus between Democrats and Republicans may be the result of policy and ideological differences. Evidence suggests that the mass public is ideologically divided

(Abramowitz and Saunders 2008), that the public sees their world through a partisan lens and interpret the world, even basic facts, differently depending on their party identification (Bartels 2002a; Campbell et al. 1960); while policy divisions at the elite level trickle down to the voters who follow their lead (Lenz 2009).

On the other hand, partisan animus between Democrats and Republicans may be the result of negative character evaluations of the out-party. A respondent may perceive the policy divisions between themselves and an out-partisan and then ascribe a character (or valence) attribute onto an out-partisan because of their beliefs. This theory echoes Tajfel and Turner (1979) who define conditions that produce intergroup conflict based on an in-group ascribing negative character evaluations to an out-group they perceive as inferior.

To examine the policy and valence dimension of affective polarization, we hand-code respondents' one-word evaluations into three groups: neither policy nor valence, policy, or valence. The purpose of this measure is to quantitatively assess the dimensions of a respondent's affect towards in and out-partisans. As our theory proposes, we anticipate that respondents' affect is driven by either policy or valence. Therefore, we can better understand the respondents' visceral reactions, gut response, and top-of-bucket state-of-mind affect by coding each one-word response.

We apply the follow coding rules to each category of words. First, a policy word is any word that talks about policy or has an ideological direction to it. We code words like "liberal," "conservative," "socialist," and "fascists" as policy. Second, a valence word is any word that talks about demeanor, behavior, or character (Stokes 1963; Stone 2017). We code words like "stupid," "uninformed," "sheep," "hateful" and vulgar characterizations as a valence evaluation. Additionally, we also code words ascribing positive evaluations like "smart," "correct,"

“intelligent” and “good” as valence as well. Lastly, we code words that neither directly describe character or have a policy or ideological angle to them as “neither policy nor valence.” These include words like “voter,” “money,” and “workers.” We represent the valence and policy dimension as two dummy variables that serve as our key independent variables. Together, our independent variable represents the two dimensions of affect, valence and policy, that respondents may rely on to evaluate voters. In the model, we only use out-party evaluations to create these two dimensions. In the appendix, we report the results of the models using in-party evaluations to create these two dimensions. We use these independent variables to predict the affective polarization scores using our respondent-coded one-word evaluation.

Affective polarization scores serve as our dependent variable. We create our measure of affective polarization by applying the same formula scholars use to create thermometers, candidate evaluations, and social-distance measures to the respondent’s self-coding of their one-word evaluation:

$$\textit{Affective Polarization} = \textit{In Party Evaluation} - \textit{Out Party Evaluation}$$

The formula for affective polarization uses respondents’ self-coded word and takes their in-party affect evaluation and subtracts their out-party evaluation. The formula generates a score ranging from -6 representing extreme in-party dislike to 6 representing extreme out-party dislike, with 0 indicating indifference between both parties. As a theoretical extension, we also use the policy and valence dimensions to predict affective polarization using thermometers, candidate evaluations, and social-distance measures.

We employ an ordinary least squares model that regresses affective polarization (using each measure of interest) onto our policy and valence dimensions using the following formula:

Affective Polarization

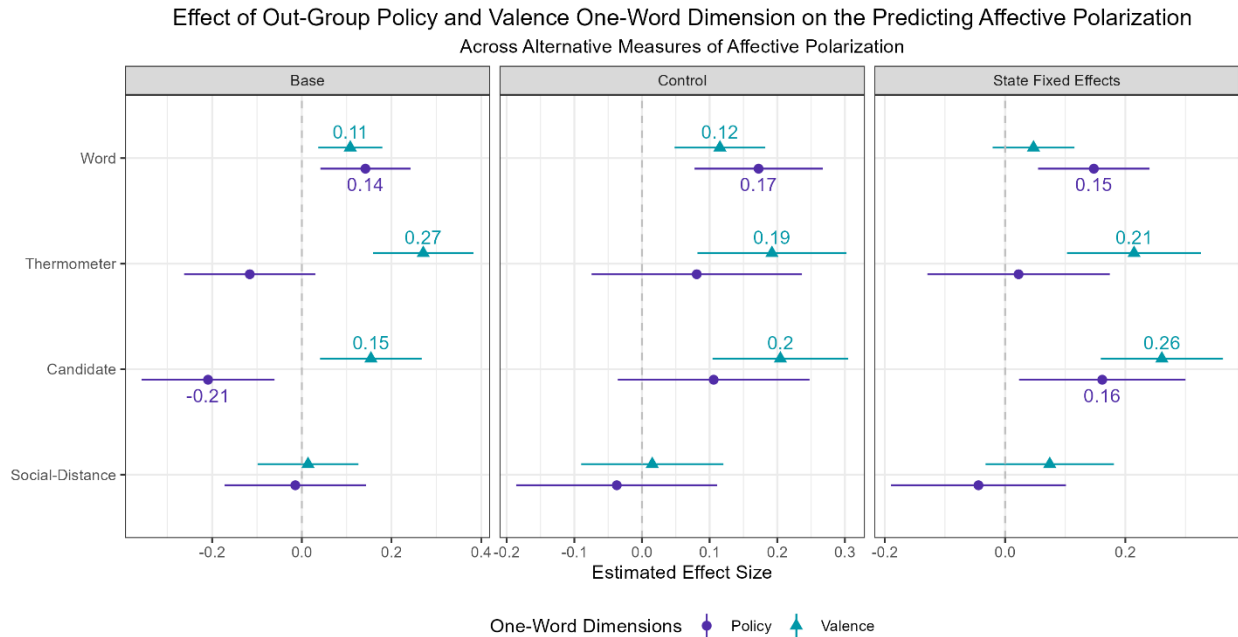
$$= \alpha + \beta_1(\textit{Policy Dimension}) + \beta_2(\textit{Valence Dimension}) + \beta_i(X_i) + \delta_s + \varepsilon$$

Affective Polarization represents the values of affective polarization derived using each measure (self-coded words, thermometers, candidate evaluations, and social-distance measures). We standardize each measure of affective polarization so that mean is equal to 0 and the standard deviation is 1 to interpret the coefficients on the same scale. *Policy Dimension* represents words that have a policy or ideological meaning; while the *Valence Dimension* covers any words that have a positive or negative character evaluation. Together, these variables represent our key independent variables, and we compare their coefficients to the base term: non-policy or valence words.

X_i represents our control variables. Our controls include the respondents' *self-coded one-word evaluation* of out-party voters. This control is the most important because it tests whether the valence or policy dimensions contribute predictive power to affective polarization, beyond merely the positive or negative evaluation of the word. Our model also includes controls for demographic characteristics such as the respondents' *age, income, gender, education* and *ethnicity*. We also include terms measuring extremism in respondents' *party identification* and *ideology*. Finally, we include political engagement measures: whether the respondent *donated* to a political candidate and whether they *voted* in the 2020 election.

We report the results of our key independent variable across three model specifications: base model (includes only our dependent and independent variable), control model (includes our controls along with our independent variable) and a state fixed effects model (δ_s). We report the tables for each full specification across each affective polarization measure in Appendix Table 1A - 4A. We also report alternative model specifications in Appendix Figure 3A.

Figure 7: Estimates for the Effect of Out-Group Policy and Valence Dimension on Affective Polarization



Note: Point estimates are dots with lines indicating 95 percent confidence intervals.
 Model: Linear Regression.
 Affective Polarization measures are standardized.
 We omit the coefficient representing word evaluations because the scales are not comparable, but the coefficient is significant, positive, and outperforms both the valence and policy dimensions in each model.

We find that the valence dimension contributes to predicting affective polarization.

Figure 7 reports the results of the linear model regressing the affective measures onto the policy and valence dimensions. When predicting affective polarization using the self-coded one-word evaluations, the valence and policy dimension perform equally well (positive and statistically significant), with the policy dimension outperforming in the state fixed effects model. When predicting affective polarization using thermometer scores or candidate evaluations, the valence dimension outperforms the policy dimension. Finally, neither the policy nor valence dimensions are useful beyond the positive or negative one-word evaluations in predicting affective polarization using social distance measures. In each model, the coefficient representing respondents' one-word evaluations of out-partisan voters is significant, positive, and outperforms both the valence and policy dimensions in each model. Yet, across key indicators of affective

polarization, both the valence and policy dimensions contribute additional predictive power that suggests that valence and policy evaluations of out-partisans may motivate affective polarization.

Taking the control model, we find that when compared to non-policy or valence words, valence words increase affective polarization as measured using words, thermometers, and candidate evaluations measures by 0.12, 0.19, and 0.2 units, respectively. Meaning that valence evaluations of the out-party increase affective polarization by a fifth to at least a tenth of a standard deviation.

Consequently, even when we include respondents' one-word evaluations of out-partisans into the model, the valence dimension is a significant predictor of affective polarization. This effect is meaningful because it shows that the valence dimension exerts an effect that is *independent* from the positive/negative sentiment of the word alone. Therefore, the *content* of respondents' affect (i.e., character evaluation) is meaningful in understanding affective polarization.

These results report that character evaluations of out-partisans are associated with greater affective polarization than policy or ideological assessment of the out-party. The results imply that the partisan animus behind affective polarization is not solely rooted in policy but also in the character evaluations that partisan project onto their counterpart. These negative character evaluations further our understanding of the distrust and dislike that partisans harbor for one another. These results should raise alarms that gridlock on policy, media characterizations, and firebrand rhetoric fundamentally insight divisions among partisans that go well beyond rational policy dimensions and affix themselves into the emotional consciousness of partisans.

These results are striking because it illustrates that hidden within established quantitative measures of affect are factors about the nature of respondents' affect. Our analysis shows that one-word evaluations help to clarify this nature. Without the qualitative assessment that one-word answers offer, scholars may be missing key variation in respondents' affective attitude about out-partisans buried in their affective polarization scores. Here, our qualitative exercise highlights the valence dimension that characterize emotional affective dimensions that explain affective polarization. Furthermore, these results lend support to Tajfel and Turner's (1979) - social identity theory that the literature has taken as true and we formalize in our analysis.

In sum, these results show that dimensions of respondent's affect toward out-partisans contribute to our understanding of affective polarization. Particularly, respondents harboring negative character evaluations tend to have higher levels affective polarization, as measured by words, thermometers, and candidate evaluations. These dimensions contribute *independently* to respondent's one-word evaluations and makes them unique in explaining affective polarization. However, traditional measures of affect cannot detect such underlying dimensions.

Discussion

As political divisions expand, our need to measure affect and polarization in the public becomes even more valuable. However, current measures of affect, such as thermometers scores, candidate evaluations, and social-distance measures, suffer from reductionism because of their closed-ended nature. These measures fail to capture underlying dimensions behind respondents' affective evaluations. They reduce the full range of emotions and perceptions that form affective characterizations into numbers and thus miss the motivations driving respondents' evaluations.

To better measure these divisions, we introduce a new measure of affect that draws on innovations in open-ended responses. We ask survey respondents to provide one word that captures their feelings about individuals who do and do not share their party identification. Then, we ask respondents to code the sentiment of the word they chose on a 7-point scale, ranging from extremely negative to extremely positive. This open-ended method of eliciting partisan affect allows respondents to provide their top-of-mind evaluations without the restrictions of a bounded scale. Then, having respondents self-code their answer provides researchers with an easy-to-use scale without reducing or abandoning the information open ended responses provide.

If scholars aim to capture *affective* evaluations, then we argue that the measures we introduce capture respondents' *emotional* evaluations about partisans; thereby enhancing the *validity* of affective analyses. We support our argument through a battery of internal and external validity checks. First, we report the raw words that respondents selected and show face validity in their emotional evaluations of in and out-partisans ("F*cktards" and "Poopybutts"). Second, we show that our new measures are highly, but not perfectly correlated, to established measures of affect (Druckman and Levendusky 2019a; Iyengar, Sood, and Lelkes 2012b). These results imply that we are capturing a unique dimension of affect and existing measures may not fully capture the affective nature of respondent's evaluation as well as our one-word measure. Finally, we explore two potential dimensions of affect, policy and valence, hidden in respondents' one-word evaluations. We show that valence dimension latent in one-word evaluations explain respondents' level of affective polarization. Together, our analysis suggests one-word evaluations contribute to our understanding of partisan affect and affective polarization by showing dimensionality in respondents' evaluations. Our work can only be evaluated through

self-coded one-word responses since traditional measures reduce emotional responses onto a bounded scale and fully open-ended responses are too unwieldy for quantitative analysis.

Even if researchers are not interested in using self-coded one-word evaluations, the open-ended responses can help serve as a validity check for the other measures, as well. We hope that through using open-ended responses, researchers can compare respondents' answers to closed ended questions to determine whether open-ended answers track with closed ended ones. Instead of blindly relying on closed answered response, researchers who use open ended responses will have qualitative confidence that their measurement is or is not picking up what the researcher intends on measuring.

Despite our efforts, our measure is not without limitations. First, unless self-coding open ended responses are widely implemented in major national surveys (CCES/ANES), only researchers who have control over the questions in their own survey can implement self-coding. This is a solvable problem. If CCES and ANES, introduce self-coded one-word responses, then scholars can use open-ended responses or validate existing affect measures. Second, our survey item involves respondents typing a response and then coding it. This two-step process takes additional time compared to traditional scales. This cost is unavoidable. Yet, we believe the benefits outlined above are well worth the survey time. Third, this paper uses *one* word and *one* self-code of that word, meaning our estimates might be prone to measurement error. The more words and self-codes survey researchers can collect, the better their estimates will be.

For scholars looking to move forward with our measure, we have several practical avenues for future research. Here, we ask respondents to evaluate Democratic and Republican voters. Our measure can be applied comparatively to partisans in other counties to study affective polarization comparatively. The measure can be used to examine any group of

individuals. For example, Amlani and Kiesel (2022) examine one word evaluations of vaccinated or unvaccinated Americans. Whether there are cross-national differences in the dimensions of partisan affect is a question our measure is well suited to answer. Additionally, differences in how partisans characterize each other across parties in multi-party democracies is likely a promising area of research given existing work on the topic (Gidron, Adams, and Horne 2022). Third, we asked respondents to code the sentiment of their word; however, scholars can also ask respondents to choose from a preexisting list of emotions (i.e., angry, frustration, sad, or happy) that capture the feeling behind respondents' word selection. This data would extend our work and capture the emotions in and out partisan evoke from respondents.

Chapter Two: Conjoint Analysis of Interpersonal Affective Polarization

Research increasingly argues that partisanship can affect decisions on non-political choices, such interpersonal relationships, residential choices, and even hiring decisions (Huber and Malhotra 2017; Iyengar, Sood, and Lelkes 2012a). Some researcher research suggests that decisions based on partisan affiliation may exceed decisions on race (Iyengar and Westwood 2015b). This observed spillover is often framed as resulting from affective partisan polarization, referring to the growing distance between in-party and out-party like, or more specifically, the growing distaste of partisans towards out-party members.

However, alternative explanations for the observed spillover effects are often overlooked. Few studies attempt to systematically isolate the effect of political considerations (such as partisan affect) from alternative, non-political considerations. Non-political or social decisions that appear to be made based on partisan considerations may very well be made based on factors that merely *correlate* with partisanship. For example, someone may report interpersonal distance from out-partisans because they anticipate a lack of common social or cultural agreement rather than explicitly due to partisan bias. This “statistical” rather than “taste-based” discrimination involves individuals using group membership as a cue to make inferences about other relevant characteristics, such as lifestyle habits (Thijssen 2016). Existing studies do not isolate whether partisan dislike drives decisions (“taste-based” discrimination) or whether factors that correlate with partisanship contribute to decisions (“statistical-based” discrimination), giving the illusion that partisanship is the key variable (Shafranek 2021, p272).

We use a conjoint experiment to isolate how partisan prejudice contributes to individual's decision-making process on three types of social decisions: selecting spouse to marry, a neighborhood to live in, and a business to frequent. Our conjoint experiment is useful because it mimics real-life conditions where subjects must make trade-offs, allows us to isolate the effect of

partisanship, and measure its contribution to these decisions relative to other salient considerations – a valuable feature of conjoint analysis. We find evidence of partisan discrimination on all three social decisions and the degree to which partisanship matters to subjects is equal to and exceeds other relevant considerations in some cases. Specifically, we show that partisans prefer to have spouses that are co-partisans, to live in neighborhoods with predominantly co-partisans, and to shop at businesses that contribute to in-partisan candidates and will avoid spouses, neighborhoods, and businesses that are out-partisan or otherwise endorse the out-party. Our results imply that affective polarization increases the social distance between partisan and members of the opposing party, as both groups as they aim to avoid cross-party interactions. Affect, it seems, is the lens through which we interact with the world.

This paper contributes to the literature on affective polarization in at least two ways. First, we utilize a conjoint experiment to test the relative influence of political and nonpolitical factors across multiple categories of social decisions. Unlike previous studies, our use of a conjoint design provides increased external validity by mimicking the diverse information environments in which people make real-world decisions, allowing us to isolate the singular effect of partisanship while ensuring high internal validity. The ability to control for other factors that may also influence behavior allows our experiment to contribute to an overall improvement in the measure of affective polarization, as compared to affective polarization measures relying on standard social distance questions. Standard social distance questions measure respondents' willingness to discriminate in survey questions. Our results go one step further and demonstrate that partisans are willing to engage in discrimination in an experimental setting. Thus, our findings are informative about both the scope and limits of partisanship's influence on non-political decisions. We find strong evidence of partisan discrimination across all three social

decisions we test. Partisans clearly display affective polarization in their non-political choices, preferring co-partisans over out-partisans in all three social decision conditions: spousal, neighborhood, and business selection. Specifically, we show that partisans prefer to have spouses that are co-partisans, to live in neighborhoods with predominantly co-partisans, and to shop at businesses that contribute to in-partisan candidates and will avoid spouses, neighborhoods, and businesses that are out-partisan or otherwise endorse the out-party.

Second, our study helps quantify the likely degree of constraint provided by relevant nonpolitical factors on different social decisions by directly providing information on these nonpolitical influences to subjects, rather than merely incorporating such variables into statistical analyses *ex ante*. Past research has argued that constraining factors, such as neighborhood quality or affordability, may constrain household choices and limit the ability to politically sort in a variety of domains (McDonald 2011; Tam Cho, Gimpel, and Hui 2013). Yet the extent of other relevant factors' ability to constrain partisan bias has not been tested. Our results thus show that partisans are willing to trade off other important considerations in exchange for political sameness across a variety of non-political social decisions. Not only do we find evidence of taste-based discrimination, but also find that the magnitude of the effect of partisanship on decisions made in nonpolitical contexts is as great, or in many cases, greater than, the size of the effect of other relevant considerations, such as neighborhood crime levels or commute time. Thus, partisanship doesn't merely matter for nonpolitical decision making – it often trumps other considerations. Additionally, we demonstrate the spillover effects of partisanship on three unique types of social decisions: which spouse one would let their child marry (Study 1), which neighborhood one would live in (Study 2), and which grocery store one would shop at (Study 3).

Past Work

Existing work has focused on the non-political influences of affective polarization in three main non-political contexts: dating and relationships, housing choices, and economic transactions. Iyengar, Sood, and Lelkes (2012) first identified a partisan bias in dating, with later research confirming but tempering this in-group preference, arguing that actual dating decisions are made based on attributes *correlated* with partisanship rather than partisanship itself (Klofstad, McDermott, and Hatemi 2012). Furthermore, not only does partisanship seem to affect evaluations of potential partners, but partisans are also more likely to act on potential matches with in-party members (Huber and Malhotra 2017). These findings suggest that partisanship does affect real world sorting in individuals' romantic relationships. Moreover, these effects on dating behavior have likely increased significantly during and since the Trump era due to the large gender differences in perceptions of leading political figures (Deckman 2022). However, earlier research on such partisan sorting has mostly assumed, rather than shown, that the high degree of concordance between long-term romantic partners and political orientations is due to individuals selecting each other on these traits (taste-based discrimination) rather than using them to infer other traits (statistical discrimination). Recent work is just beginning to more rigorously test the partisan sorting theory in relationships (Klofstad, McDermott, and Hatemi 2012).

The second most commonly observed spillover of partisan affect is in residential choices (Bishop 2009; Gimpel and Hui 2015, 2017, 201; Hui 2013; Motyl 2014; Motyl et al. 2014; Tam Cho, Gimpel, and Hui 2013). Most of the literature continues to debate whether and why partisans are geographically politically sorted, with some finding a connection between partisan preference and residential choice (McDonald 2011; Tam Cho, Gimpel, and Hui 2013, 201).

However, some research highlights the constraints of political segregation, cautiously arguing that partisanship merely influences rather than determines partisans' housing choices (Fiorina and Abrams 2008; Mummolo and Nall 2017). Many scholars in this camp argue that the revealed connection between partisanship and residential choices may be real and causal, but it may also be purely spurious, with partisan geographic sorting being driven by exogenous considerations that incidentally associate with political preference (Hui 2013). Partisan geographic sorting may thus be direct or indirect (Tam Cho, Gimpel, and Hui 2013) or entirely inadvertent (Gimpel and Hui 2017). In other words, the literature is unclear on the degree to which partisans engage in taste-based vs statistical discrimination in housing choices.

Recent work suggests that out-party discrimination affects college roommate selection (Shafranek 2020). Still, others find that partisans appear sorted because they move to locations with amenities that happen to be correlated with partisanship, but not necessarily because of their partisanship. As with long-term partner choices, (Martin and Webster 2020) find that while the preferences of those who move from one place to another correlate with partisan affiliation, voters appear to be sorting on non-political attributes such as areas that are more (or less) dense, walkable, or other relevant neighborhood characteristics, rather than explicitly selecting political congruence. This research suggests that partisanship, and hence affective polarization, has an important influence on individuals' housing decisions, but demands further investigation whether partisanship has a causal effect, independent of other non-political considerations, such as the racial composition of the neighborhood, quality of schools, and average income levels.

The third commonly studied spillover of partisan affect is in the area of economic transactions, with a focus on hiring decisions, online labor markets, and lab economic games (Bartels 2002b; Gerber and Huber 2010; McConnell et al. 2018). Studies find that partisans favor

co-partisans and discriminate against political opponents in hiring decisions, with applicants that share the majority partisan affiliation being significantly more likely to receive a callback than non-partisan candidates (Hui 2013). Other studies uncover a preference for working with co-partisans, showing that individuals demand a lower reservation wage from co-partisan employers than non-co-partisan employers (McConnell et al. 2018). These findings suggest that partisan considerations spill over into economic decisions that have little to no relation to politics. However, the literature again is unclear on the degree of statistical versus taste-based discrimination partisans engage in when making economic decisions.

Despite a plethora of high-quality research, a primary limitation of existing studies is their lack of experimental controls for many, if any, additional pieces of information aside from partisanship. In other words, preferences and associated spillover are often measured as if decision-making occurs in a vacuum. The possibility that political spillover effects into non-political domains are merely due to correlations is a consideration that demands attention. With regards to mate selection, (Klofstad, McDermott, and Hatemi 2012) argue that people may be making long-term mating choices based on nonpolitical characteristics that merely correlate with political preferences, such as religiosity, physiology, or intelligence. Thus, partisan sorting in interpersonal relationships may be conflated with sorting on these correlated, but non-political characteristics. Without an experimental control for other relevant attributes, previous studies cannot convincingly determine what share of partisan discrimination is taste based rather than statistical.

The possibility of statistical-based discrimination exists across decision types. For example, McConnell et al. (2018) test the economic effects of partisanship by simply signaling employer-employee partisanship congruence, without providing any additional, potentially

relevant information about the employer. Similarly, Munro, Lasane, and Leary (2010) had participants review fictional college admissions applications of “18-year-old White males from small cities within the state,” with the applications constructed in similar ways except educational achievement history and the political preference of the applicant. Participants were then expected to evaluate an application and choose who they would invite for an interview based on this singular political preference manipulation.

The inferential problem presented by the possibility of statistical discrimination is noted in the literature and Shafranek (2021) is one of the first and only papers to date that directly addresses this concern and examines the impact of partisan affect on nonpolitical considerations while controlling for factors respondents may infer from partisanship (statistical discrimination). Focusing on roommate selection among college students and using a conjoint experimental design, Shafranek (2021) finds that partisanship strongly influences roommate choice, even in the presence of relevant non-political, but politically correlated information such as a potential roommate’s preferred bedtime, social preferences, level of cleanliness, or most important value. It is especially novel in its causal identification strategy, addressing the issue of non-political factors possibly correlating with political or partisan preferences that other studies suffer from. Our study draws inspiration from Shafranek (2021), while also making valuable extensions to it. First, Shafranek (2021) relies on a sample of college students, while we use a nationally representative sample of adults, improving on the generalizability of the results. Second, while Shafranek (2021) looks at a singular non-political decision, roommate selection, our study evaluates three distinct categories of social decisions: choice of spouse, neighborhood selection, and choice of grocery store, allowing us to explore differences, if any exist, across social decisions.

Improving Measurement

In addition to our contribution to the understanding of both the scope and limits of partisanship's influence on non-political decisions, we provide a novel behavioral measure of affective polarization. Our study validates existing social distance measures by revealing the true scope of partisan bias. Previous findings are often criticized due to the low experimental realism of traditional social distance measures. In particular, it remains doubtful that the behavior revealed by social distance questions is truly reflective of behavior in more realistic settings. This is further evidenced by recent findings that reveal affective polarization shows downstream effects on abstract interpersonal items, while these same effects do not manifest when asking respondents to make specific judgments about specific individuals (Broockman, Kalla, and Westwood 2022). One possible reason for this discrepancy is that specific judgments, compared to judgments in the abstract, involve “trade-offs that answer to abstract survey questions usually do not” (Broockman, Kalla, and Westwood 2022). Our conjoint experiment not only allows for such tradeoffs to exist, but actively tests for them. We are able to see whether, when provided with a plethora of additional relevant information, partisans will trade off other important considerations to favor co-partisans. Having the ability to manipulate the political characteristics of the profiles while randomizing other characteristics, our experimental results isolate the causal effect of partisanship and significantly improve upon traditional abstract survey items.

Scholars generally draw on three measures of affect: feeling thermometers, candidate evaluations, and social-distance measures. However, social distance measures are typically used to tap attitudes about particular behavioral outcomes, including nonpolitical behaviors such as marriage (Iyengar, Sood, and Lelkes 2012a; M. Levendusky and Malhotra 2016). Social-distance questions measure affect by asking questions that gauge the degree of closeness that individuals

are comfortable having without-partisans (Druckman and Levendusky 2019b). The logic is that higher levels of self-reported social distance indicate greater partisan animosity. In other words, such social distance items aim to measure affective polarization through distinct behavioral outcomes (Druckman and Levendusky 2019b; Iyengar, Sood, and Lelkes 2012a). Most famously, Iyengar, Sood, and Lelkes (2012) asked respondents how happy they would be if their son or daughter married someone of the opposing party, finding a clear in-partisan bias. Recent research also finds that social distance measures show relatively low correlations with both thermometer ratings and out partisan trait measures, suggesting that social distance measures are an important and unique measure for tapping out-party dislike (Druckman and Levendusky 2019b).

Nevertheless, despite their popularity and importance, standard social-distance items have several drawbacks when it comes to measuring partisan affect. For one, respondents may exhibit social desirability bias when asked to openly and honestly answer direct questions about social distance due to social norms against social discrimination (Kekkonen et al. 2022). Additionally, standard social-distance measures lack experimental realism and thus suffer from low external validity, which can be especially problematic for predicting real-world behavioral outcomes. Discriminatory answers on a survey are cheap compared to real work dating, housing, and economic decisions. This critique is similar to those leveled against hypothetical survey questions used to measure concepts such as support for political violence: “generic and hypothetical questions offer respondents too many degrees of freedom and require greater cognition than a sizable portion of the population will engage in” (Westwood et al. 2022).

Finally, social distance measures may simultaneously tap relevant considerations or expectations for the future (such as how well one will get along with a neighbor or how one will

be treated along with partisan considerations). Measurement values can thus reflect other important considerations or expectations, making social distance measures prone to measurement error compared to direct observations of respondent's behavior towards out-partisans. All of this together makes it difficult to disentangle the effect of political considerations from relevant, but possibly correlated, non-political considerations on decision making.

A conjoint experimental design improves upon these challenges of social distance measures and improves the measurement of affective polarization overall. For one, while a conjoint design is unable to entirely eliminate the problem of social desirability, offering respondents a profile consisting of multiple factors makes it is less clear which answer is discriminatory or socially desirable, minimizing the potential for bias (Wallander 2009). Furthermore, research suggests that conjoint experiments are better at predicting real-world behavior than traditional survey experiments, such as vignette experiments (Hainmueller, Hangartner, and Yamamoto 2015). By providing multiple pieces of information to respondents, a conjoint experiment improves on the issue of external validity by increasing confidence that individuals are choosing as they would in the real-world, where decisions are made in informationally dense environments rather than in vacuums (Hainmueller, Hangartner, and Yamamoto 2015; Hainmueller and Hopkins 2012). Finally, a conjoint experiment helps separate the effect of partisanship from other, possibly correlated non-political factors by controlling for a set of chosen factors. This effectively isolates respondents' attitudes towards the opposing party from statistical discrimination. Together, our use of a conjoint design allows us to make several meaningful improvements to understanding the spillover effects of partisan affective polarization, while also constituting important improvements to the measurement of affective polarization using social distance-like measures.

Theory and Expectations

Our theoretical and empirical priors suggest that partisanship should be an important consideration even in non-political decisions such as marital partner selection, neighborhood choice, and grocery store selection, albeit perhaps more important for some than others. Furthermore, we expect that partisans will prefer their own co-partisans to out-partisans. Many studies have revealed such an in-group preference in abstract or political domains. For example, research on affective polarization reveals that partisans are more polarized, but also more hostile, towards members of the other party, including holding counter-empathetic responses that leads them to experience pleasure in response to out-group members' suffering (Abramowitz and Webster 2016; Hetherington and Rudolph 2015; Hudson, Cikara, and Sidanius 2019; Martin and Webster 2020; Mason 2018). Beyond mere antagonistic attitudes and judgments, Barber and Davis (2022) find that partisans show a willingness to sacrifice members of the out-party for the sake of a group of in-partisans in a hypothetical trolley problem.

Individuals generally prefer others with similar personal characteristics, known as homophily, and political sameness is no exception (McPherson, Smith-Lovin, and Cook 2001). Segregation as a social phenomenon has long been documented in contexts such as residential neighborhoods, workplaces, and schools (Boustan 2013; Card, Mas, and Rothstein 2008; Echenique, Fryer Jr., and Kaufman 2006). To a large degree, segregation, or related social stratification, is a result of an enduring social behavior known as homophily, which refers to the tendency for people to associate with similar or like-minded others (McPherson, Smith-Lovin, and Cook 2001). The homophily principle that similarity breeds connection has been found to structure network ties of all types of social interactions, such as marriage, friendship, and work (McPherson, Smith-Lovin, and Cook 2001).

Why might we expect partisans to seek political congruence in their non-political decisions? Two main arguments from the literature on partisan geographic sorting are most informative: political homophily and partisan discrimination. Political homophily argues that “birds of a feather flock together” to explain the tendency for co-partisans to form relationships in their social networks. Political homophily is generally agnostic to the motivation behind said clustering towards sameness. On the other hand, partisans may be actively seeking politically compatible neighbors when choosing where to live, for example (Gimpel and Hui 2015). This theory of partisan discrimination suggests that partisans use party ID to determine whether to engage in social interactions with someone – aptly termed partisan discrimination. Partisan discrimination can be difficult to tease out, since spatial sorting by party could emerge due to seeking out lifestyle preference similarities, which can be correlated with political tastes. In this case, non-political characteristics dominate, and political tastes just happen to come along with those characteristics. However, it is also possible that despite an assortment of non-political preferences at play in non-political, social decisions, individuals are actively choosing to base these decisions on political considerations. In other words, they are willing to trade off relevant, non-political preferences in favor of political ones. In this case, true partisan discrimination is present and the outcome of homophily is achieved with the explicit motive to discriminate against out-partisans. We expect at least some partisan discrimination even in non-political areas because, from a social identity perspective, affective polarization is an outgrowth of partisan social identity (Iyengar et al. 2019b; Iyengar and Westwood 2015b; Mason 2018). Increasingly, partisanship is one of Americans’ most salient social identities, and our knowledge of group identity and group polarization tells us that reinforcing identity cleavages will foster a sense of “us against them” and have many polarizing and destabilizing consequences, such as

discrimination (Iyengar and Krupenkin 2018; Lijphart 1969). Therefore, due to the high salience of Americans' partisan identity and the known consequences of such strong group identities, bias towards one's own party and bias against out partisans is expected, although the extent to which it determines action alongside other relevant considerations is not yet known.

Studies have found evidence for both theories of political homophily and political discrimination, however scholars have struggled to identify true partisan discrimination from its less intentionally discriminatory counterpart. For example, Geiger (2014) asked respondents whether they agree with the statement that it is "important to live in a place where people share their political views." While agreeing with this statement indicates an explicit political preference consistent with partisan discrimination, it does not tell us *how* individuals weigh political views when provided with other relevant concerns in making housing decisions, such as affordability. By testing the limits of partisanship in influencing social decisions, our paper helps differentiate between sorting motivated by true political discrimination from theories of non-political homophily that are merely correlated with partisanship.

Given the existing work on non-political spillover effects of partisanship, we expect that partisanship will influence social decision making. If partisan discrimination is present, we should expect to see that individuals select the co-partisan profile even when they are provided with additionally relevant non-political considerations. These expectations lead to one hypothesis about partisan discrimination across categories, stated formally here:

Partisans will prefer spouses, neighborhoods, and grocery stores with in party identities and discriminate against spouses, neighborhoods, and grocery stores with out party identities.

Research Design

To test our expectations, we employ a conjoint experiment testing how respondents' partisanship influences non-political decisions (Hainmueller, Hangartner, and Yamamoto 2015). We fielded our conjoint experiment using Lucid to generate a nationally representative sample. Lately, scholars have raised concerns about the quality of samples drawn from Lucid (Coppock and McClellan 2019). Utilizing extensive attention checks, we recruited 1,302 high-quality to participate in our conjoint experiment.⁸ We obtained IRB approval through the UC Davis before administering the survey. We gave all respondents informed and written consent before they began.

Conjoint experiments are increasingly popular among political scientists due to their ability to identify component specific causal effects by randomly manipulating multiple attributes simultaneously (Hainmueller, Hangartner, and Yamamoto 2015). Their ability to identify and test many treatment components simultaneously and compare components makes it an ideal tool for our hypothesis. Developed for market research, political scientists use conjoint experiments to evaluate individuals' attitudes on immigration (Hainmueller, Hangartner, and Yamamoto 2015), roommate selection (Shafranek 2021), candidate selection (Bansak et al. 2022) and elite messaging (Costa 2021). Additionally, the presentation of multiple attributes limits social desirability bias (Wallander 2009).

Our experiment follows Hainmuller et al.'s (2014) experimental procedure. We present respondents with two profiles that included 5-7 categories, attribute values are fully detailed in tables 1-3. We randomize both the order of the studies, the attributes and the profiles.

⁸ High quality respondents passed the timing and the attention checks that we include in our survey.

Table 1: Conjoint Attributes Spouse Condition

Attribute Category	Attribute Values
Partisanship	Strong Democrat; Lean Democrat; Apolitical; Lean Republican; Strong Republican
Attractiveness	Above Average; Average; Below Average
Religion	Muslim; Evangelical Christian; Catholic; Atheist; Jewish; None
Race	White; Black; Asian; Hispanic
Income	\$32K; \$54K; \$96K; \$210K

Note: Profiles are generated randomly and respondents are given a forced choice between each randomly generated set of profiles.

Table 2: Conjoint Attributes Neighborhood Condition

Attribute Category	Attribute Values
Voting Record	Strong Republican 70% Rep, 30% Dem; Leans Republican 55% Rep, 45% Dem; Split 50% Rep, 50% Dem; Leans Democrat 45% Rep, 55% Dem; Strong Democrat 30% Rep, 70% Dem
Race	90% White, 10% Nonwhite; 75% White, 25% Nonwhite; 50% White, 50% Nonwhite; 25% White, 75% Nonwhite; 10% White, 90% Nonwhite
Commute	45 min , 25 min , 75 min , 10 min
Violent Crime	Same as national average; 20% more than national average; 20% less than national average
Monthly Housing Cost	40% of Income; 60% of Income; 80% of Income
Location	City; Suburban; Rural Town
School Quality	Excellent; Good; Average; Poor

Note: Profiles are generated randomly and respondents are given a forced choice between each randomly generated set of profiles.

Table 3: Conjoint Attributes Grocery Condition

Attribute Category	Attribute Values
Political Donations	90% Republican, 10% Democrat; 70% Republican, 30% Democrat; 50% Republican, 50% Democrat; 30% Republican, 70% Democrat; 10% Republican, 90% Democrat
Prices	Average; Bargain 20% below average; Overpriced 20% above average
Travel Distance	5 Min; 10 Min; 20 Min
Yelp Rating	2 Stars; 3 Stars; 4 Stars; 5 Stars
Activism	Blue Lives Matter; Black Lives Matter; None; Against Gay Marriage; For Gay Marriage

Note: Profiles are generated randomly and respondents are given a forced choice between each randomly generated set of profiles.

Our dependent variable is a binary variable indicating the profile the respondent selects.

This approach is known as “discrete choice experimentation” (Raghavaram, Wiley, and Chitturi 2010). This choice outcome experiments mimics real-world situations where respondents much choose between options that differ (Hainmueller, Hopkins, and Yamamoto 2014).

For each study, we limit the number of attributes in each profile to a maximum of 7. According to Green and Srinivasan (1990), six attributes maximize the effectiveness of the conjoint. Including five to six attributes obscures the study’s focus on partisanship (i.e., increases internal validity) and provides objective criteria to judge partisanship against, such as school quality, housing costs, and income levels.

Our conjoint experiment assesses three questions: which spouse would you let your child marry (Study 1), which neighborhood would you live in (Study 2), and which grocery store would you shop at (Study 3)? We generate twelve profiles for each study, present respondents

two side-by-side profiles at a time and ask them to select one. Figure 8 illustrates an example of a conjoint profile that the respondent's would see in each of our studies.

Figure 8: Conjoint Profile Example

A) Spouse			B) Neighborhood			C) Grocery Store		
Which of these hypothetical persons would you prefer as a son or daughter in-law?			Which of these hypothetical neighborhoods would you prefer to raise a family in?			Which of these hypothetical grocery stores would you prefer to frequent?		
	Choice 1	Choice 2		Neighborhood 1	Neighborhood 2		Store 1	Store 2
Income	\$96K	\$54K	School Quality	Poor	Poor	Activism	Blue Lives Matter	None
Religion	Catholic	Atheist	Violent Crime	20% more than national average	Same as national average	Travel Distance	10 Min	5 Min
Race	Black	Asian	Location	Rural Town	Rural Town	Yelp Rating	2 Stars	2 Stars
Attractiveness	Above Average	Average	Monthly Housing Cost	80% of Income	40% of Income	Political Donations	70% Republican, 30% Democrat	10% Republican, 90% Democrat
Partisanship	Lean Republican	Strong Democrat	Commute	10 min	25 min	Prices	Overpriced (20% above average)	Overpriced (20% above average)
<input type="radio"/> Choice 1 <input type="radio"/> Choice 2			<input type="radio"/> Neighborhood 1 <input type="radio"/> Neighborhood 2			<input type="radio"/> Store 1 <input type="radio"/> Store 2		

Note: Profiles are generated randomly and respondents are given a forced choice between each randomly generated set of profiles.

Study 1 asks respondents to select a potential spouse for their (future) son or daughter to marry. The question takes inspiration from Iyengar, Sood, and Lelkes's (2012) seminal finding showing that “Americans have become increasingly averse to the prospect of their child marrying someone from the opposing party” (Iyengar et al. 2019: 132). We examine whether these results hold using a conjoint design. In addition to offering their potential son/daughters-in-law’s partisanship (a seven point scale from strong Republican to strong Democrat with apolitical as the median), we also present information on the potential spouse’s attractiveness (below average, average, or above average), education level (graduate degree, high school diploma, or bachelor’s degree), income level (\$32,000, \$54,000, \$92,000, or \$210,000), race (Hispanic, White, Black, and Asian) and Religion (Catholic, Evangelical Christian, Jewish,

Muslim, Atheist, None). In the analysis, we recode race and religion relative to the respondent to test likelihood of selecting a spouse with the same (or different) race or religion.

Study 2 asks respondents to select a neighborhood to live in. Our design builds on Mummolo and Nall (2016), who also run a conjoint experiment testing the effects of partisanship on neighborhood effects. We use and update some of their study's attributes. In addition to offering the respondents the average neighborhoods' *partisan voting record* (a seven-point scale from strong Republican to strong Democratic with split neighborhood as the median), we also include the *percentage of minorities* (10%, 25%, 50%, 75% and 90%), *school quality* (Excellent, Good, Average, or Poor), *housing cost* (40%, 60%, and 80% of respondent's income), *commute to work* (10, 25, 45 and 75 minutes), and neighborhood crime rate (less than 20, equal to, greater than 20 percent the national average).

Study 3 asks respondents to select a grocery store to shop at. In recent years, political consumerism has increased alongside corporate forays into politics. For example, Citizens United allows corporations to raise and spend unlimited sums of money on independent expenditures; while, political stances by corporations, such as Coca-Cola (Gelles 2021), Target and Chick-Fil-A, and Supreme Court cases brought by corporations, such as Hobby Lobby (Liptak 2014) on ideologically salient policy questions, have caused controversy, boycotts, and protests that potentially hurt profit margins. Using these anecdotes as inspiration, we test whether a business' partisanship influences patrons' willingness to support it. We follow (Panagopoulos and van der Linden 2016) and anticipate both customers' and businesses' partisanship affects consumers' decisions on where to shop. While there are many types of business we can use in this hypothetical, we use a grocery store. We choose a grocery store because of its generalizability and connection to experimental realism: everyone purchases groceries, unlike a

soft drink company, fast food restaurant, or craft store with a more selective clientele. We operationalize *partisanship* by providing respondents with information about a grocery store's campaign contribution to political candidates (10% to Republicans 90% to Democrats; 30% to Republicans 70% to Democrats; 50% to Republicans 50% to Democrats; 70% to Republicans 30% to Democrats; 90% to Republicans 10% to Democrats). We also include and control for additional factors that influence consumers decisions to shop at a store, including their *lobbying activity* (none, against/for gay marriage, blue/black lives matter), travel distance (10, 20, or 30 minutes), prices (20% above average, average, 20% below average), and the *Yelp Rating* (2, 3, 4, 5 stars).

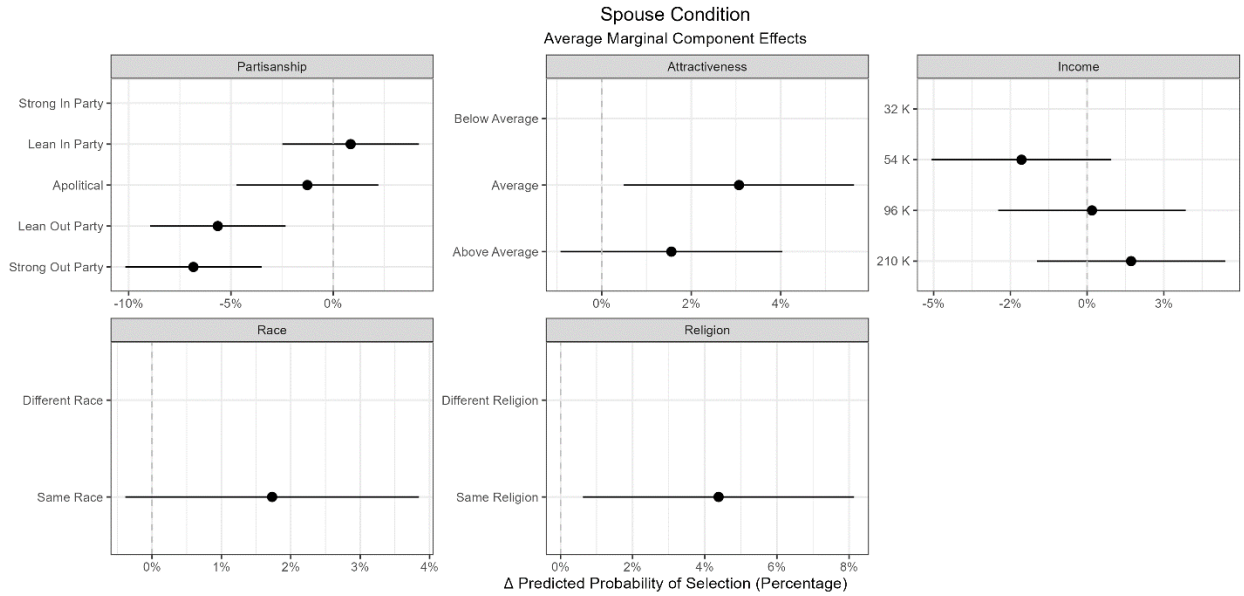
Since our theory expects respondents' partisanship to condition the effect of partisanship on profile selection, we recode the partisanship conjoint attribute relative the subject's partisanship. We ask respondents to report their partisanship, Democratic or Republican, and the strength of their partisanship (strong or not very strong). For respondents who answered as being an Independent, we ask them to choose which party they are closest to (Democratic or Republican party). This procedure creates a 7-point partisanship scale from strong Democrat to strong Republican. Then, using the subject's partisanship we recode the partisanship conjoint attribute relative to their subjects' partisanship. That is a subject in our study sees one partisan attribute in our experiment out of a five-point scale that ranges from strong or lean in/out partisan, with the median category being the neutral category (split or apolitical depending on the study). Recoding the conjoint attribute relative to the subject's partisanship notably increases each the studies sample size, avoiding reporting partisanship as a moderating variable and providers for easier interpretation of the results.

We report the results by measuring the effects of partisanship on profile selection using the Average Marginal Component Effects (AMCE) and converting it to percentages for easy interpretation. The AMCE is widely regarded as the appropriate tool for reporting the results of a conjoint analysis and is used widely in the political science literature (Bansak et al. 2022; Hainmueller, Hopkins, and Yamamoto 2014; Shafranek 2021). In our results, we focus on the AMCE that reports the average effect of our partisanship variables on the probability that respondents choose a given profile (Hainmueller, Hopkins, and Yamamoto 2014).

Results

In Study 1, we find evidence that subjects display affective polarization on who is acceptable for their son or daughter to marry. We find that subjects do not want their son or daughter to marry an out-partisan spouse. Figure 9 displays the average marginal component effects (converted to a percentage) for each attribute in the spouse condition. We find that subjects have a statistically significant aversion to potential spouses that are a lean out-partisan and a strong out-partisan, when compared to selecting a spouse that is a strong member of their in-party. Overall, when subjects consider a potential spouse for their child, Democratic and Republican subjects prefer in-party spouses to out-partisan spouses. On average, partisans are 5.6 and 6.8 percent more likely to select an in-partisan spouse over a lean and strong out-partisan spouse, respectively. Partisanship, more so than any other variable, was the strongest predictor of spouse selection when compared with the other covariates in this vignette (i.e., attractiveness, income, race, and religion). These results support Iyengar, Sood, and Lelkes's (2012) findings about Americans being averse to the idea of their child marrying someone of the opposite party.

Figure 9: Spouse Condition

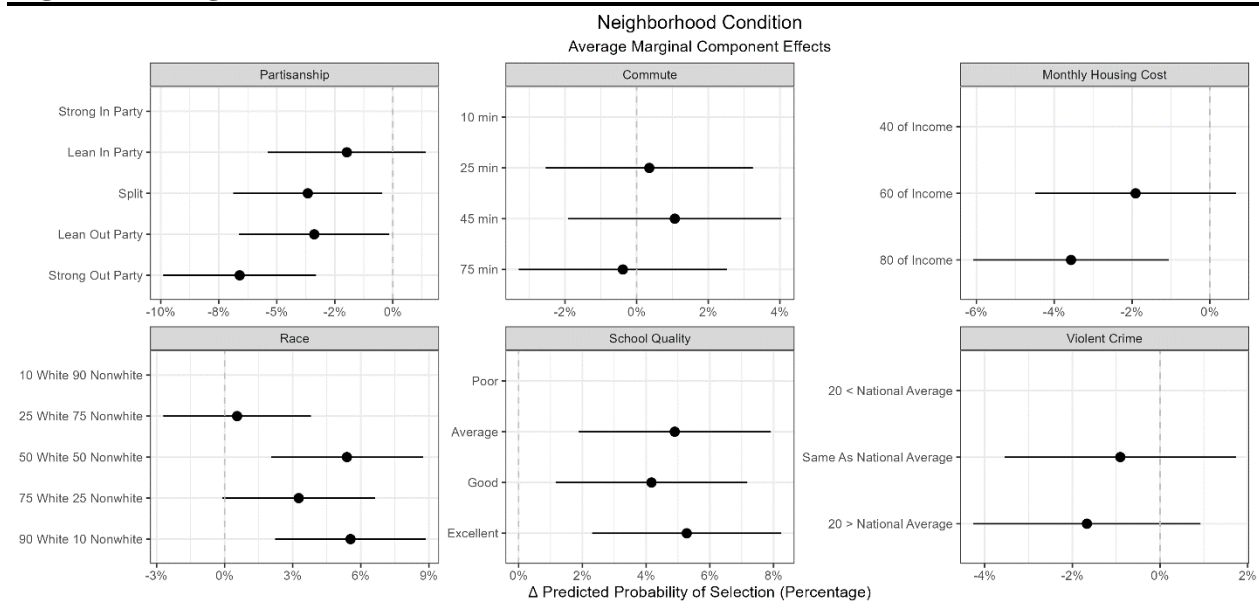


Note: Omitted coefficient represents baseline category for each attribute.

In Study 2, subjects display affective polarization on where they prefer to live. We find that subjects prefer to live in neighborhoods that share their in-party and avoid out-partisan neighborhoods. Figure 10 displays the average marginal component effects (converted to a percentage) for each attribute in the neighborhood condition. We find that subjects have a statistically significant aversion to neighborhoods that are lean out-partisan and strong out-partisan, when compared to selecting a strong in-party neighborhood. Further, when subjects consider a neighborhood, Democratic and Republican subjects prefer in-party neighborhoods to out-partisan neighborhoods. On average, partisans are 3.3 and 6.6 percent more likely to select an in-partisan neighborhood over a lean and strong out-partisan neighborhood, respectively. Avoiding out-partisan neighborhoods is on par with subjects' preferences to locate in an above average school district (relative to a poor district) and avoid paying 80 percent of their income to housing costs (relative to 40 percent of their income). These results suggest that subjects display

affective polarization when deciding which neighborhood to live in, such that they prefer to cohabitate with co-partisans.

Figure 10: Neighborhood Condition

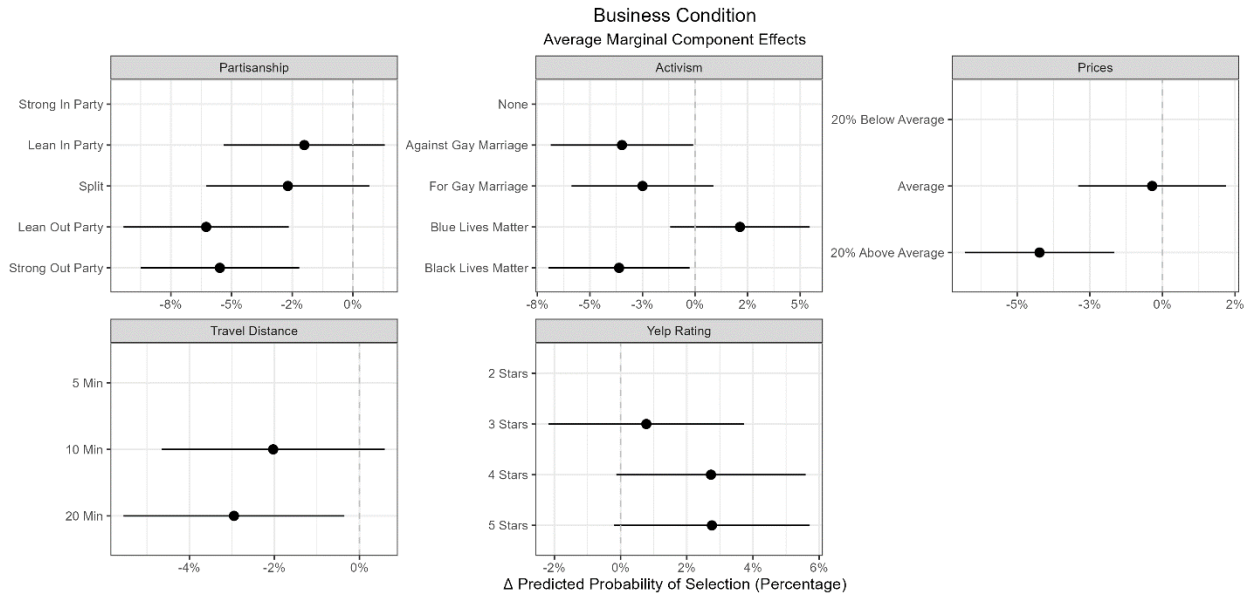


Note: Omitted coefficient represents baseline category for each attribute.

In Study 3, we find evidence that subjects display affective polarization on where they prefer to shop. We find that subjects prefer to shop at stores that contribute to in-partisan candidates and avoid stores that contribute to out-partisan candidates. Figure 11 displays the average marginal component effects (converted to a percentage) for each attribute in the business condition. We find that subjects have a statistically significant aversion to businesses that contribute money to lean out-partisans and strong out-partisans, when compared to businesses whose campaign contribution records report lean or strong in-partisan support. Further, when subjects consider a business, Democratic and Republican subjects prefer in-party businesses to out-partisan businesses. On average, partisans are 6.0 and 5.5 percent more likely to select an in-partisan business over a lean and strong out-partisan business, respectively. Avoiding out-partisan businesses is on par with subjects' preferences to avoid long travel distances to stores

(relative to short commute) and overpriced businesses (relative to below average prices). These results suggest that when we provided subjects with campaign contributions information about businesses, they responded by preferring businesses that supports candidates who shared their party identification and avoided business that supported out-party candidates.

Figure 11: Business Condition



Note: Omitted coefficient represents baseline category for each attribute.

Together, these results illustrate that partisans prefer in-partisan and avoid out-partisan spouses, neighborhoods, and businesses. In addition to playing a key role in voting decisions, partisanship also plays a role in the choices people make in their everyday life. These results also support our hypothesis that affective polarization, or the dislike of out-partisans, plays an important role in society and informs how partisans make decisions that structure their lives.

Discussion

Affective polarization, or the dislike of individuals of the opposing party, is a prominent feature in American politics. So much so, that scholars suggest that it affects non-political

decisions. However, current scholarship in this area is plagued by a key limitation: do partisans avoid each other because of party identification or because of other factors that are correlated with partisanship? To understand these effects, we employ a conjoint analysis that (1) isolates the effect of partisanship in nonpolitical decisions and (2) demonstrates an improved method for measuring partisan affective polarization in social decisions. Across three different non-political decisions – spouse, neighborhood and business selection, we find that partisanship has an independent and significant effect on partisans’ nonpolitical decisions, in some cases more so than any other theoretically relevant considerations.

Our findings extend current work on affective polarization. First, we introduce a conjoint analysis to the observational world of measuring affect. Previous studies measure the dislike between partisans using observational measures, such as thermometers, candidate evaluations and social distance measures. Together, these measures suffer similar fault, they do not account for other meaningful considerations that may drive behavior. For example, Iyengar, Sood, and Lelkes’ (2012) findings about Americans’ aversion to having their child marry someone of the opposing party, our results suggest that partisan discrimination occurs even when individuals are given additional relevant information. Second, our design allows us to contribute to the discussion of taste-based versus statistical-based discrimination. Our conjoint design isolates the effects of partisanship from other heuristics it may embody. Finally, our results support literature suggesting partisanship is a social identity that drives affective polarization (Mason 2016; Mason and Wronski 2018), more so than policy orientation (Bougher 2017; Lelkes 2018; Rogowski and Sutherland 2016; Webster and Abramowitz 2017) or policy preferences that signal a partisan identity (Dias and Lelkes 2022).

Despite our advancements, our study is not without its limits. First, while we examine partisanship against other relevant considerations using conjoint analysis, we cannot test how partisanship compares to every consideration. Therefore, our study suggests that partisanship is key when considered relative to the considerations we test. We cannot definitively say how it would perform against those we do not. Future scholars should employ conjoint experiments with different attributes to confirm that partisans are not engaging in statistical discrimination based on information not included in our experiment. Second, ideally controls in our conjoint would be relative to the subject, particularly in the neighborhood and business condition. This alteration to the conjoint's presentation would provide more nuance in understanding how partisanship performs against relative covariates. We encourage researchers to consider this approach in future research.

After accounting for our study's limits, our findings have important implications for the future of interpersonal interactions between partisans. First, partisan discrimination in social decisions will lead partisans to isolate themselves from opposing party members, a *social polarization*. This social polarization may disrupt the healthy flow of ideas and empathy necessary to resolve polarize conflict. Second, our findings suggest that as social polarization increases, so too will affective polarization. With fewer interactions with opposing party members, animosity will intensify as it will become even easier to create "others" out of opposing party members (Henri Tajfel and Turner 1979). Third, efforts to reduce affective polarization will be increasingly ineffective as social polarization grows. Attempts to encourage cross party empathy, dialogue, and understanding will be hampered by the absence of out-partisans in Americans social spheres. Research already suggests that partisans inhabit social spheres with likeminded partisans and breaking into these social spheres as an opposing partisan

is becoming less common (Butters and Hare 2020). Finally, future research attempting to find interventions to social polarization and reduce affective polarization is necessary to rehabilitate social discourse and reverse this phenomenon

Chapter Three: Non-Religious Identity Salience for Candidate Choice

The nonreligious: atheists, agnostics, and nones⁹ comprise between 20 and 25 percent of the population in the United States, making them the second largest religious group, behind Christians, (Pew Research Center 2019). This large and rapidly growing group emerged in the 1990s as Christian identification plummeted and Church attendance collapsed (Pew Research Center 2019). However, this enormous increase in the number of nonreligious identifiers has not translated into representation. The nonreligious are almost totally excluded from government in the United States as only one sitting official at the Federal level explicitly identifies as nonreligious (Sandstrom 2019).¹⁰ Nearly one quarter of the population is nonreligious but only 1/535th of the present Congress publicly identifies as nonreligious. Here, a paradigm contrary to the core tenets of our society exists. In a democratic nation that often prides itself on representing diverse constituencies descriptively, a large and rapidly growing religious group is underrepresented by a factor exceeding one hundred. Were this occurring with a racial group or even a non-Christian minority religious group, such glaring underrepresentation would result in condemnation and panicked attempts to elect descriptive representatives. However, no such concern exists for the unrepresented nonreligious masses of America; the issue is barely known about and scholars have only begun examining the issue as one of representation.

Traditionally, legislative bodies and governments more broadly have been dominated by men from dominant racial, ethnic, and religious groups (Hughes 2013). Descriptive representation or the actual presence of minorities in legislative bodies is a thriving area of

⁹ Defined as persons who answer “none” when prompted for their religious identification on a survey. They are distinct from those answering none of the above, which would indicate religious status, nones are indicating that they have no religious affiliation but are choosing not to identify as atheists or agnostics. This paper refers to the nonreligious as an aggregated group.

¹⁰ Jared Huffman, U.S. House of Representatives (D-CA-2nd)

research and a proven method of minority protection. More recently, the importance of having groups actually represented in legislatures by members of their own has been widely embraced. There are two reasons to prefer descriptive representation. The first argument for descriptive representation is that substantive policy outcomes may differ if previously excluded groups are represented by their actual presence in policy making bodies. For example there is significant evidence that substantive policy gains are made by women via descriptive representation (Chattopadhyay and Duflo 2004) and evidence that this argument holds for both religious (Tatari 2010), ethnic (Dunning and Nilekani 2013), and racial (Cameron, Epstein, and O'halloran 1996) minorities. For nonreligious groups, these arguments are theoretical and based on assumed beliefs in secular institutions because no empirical work has been done to date (Yates 2007). However, the theoretical argument is rather convincing and can appeal to anyone interested in establishing a separation between church and state, or the non-establishment of a state sponsored religion. Somewhat by definition, the nonreligious is the group most interested in secular government. Their exclusion from government imperils this separation and creates an implicit link between religion and the state. The presence of nonreligious representatives would almost certainly aid in the establishment of a more secular state, and ironically, a better environment for religious life free from government preferences for one faith over another.

The second argument for descriptive representation is about stigmatization and political alienation, even if inclusion does not affect substantive policy, it is an important component of democracy. Representation of minorities or marginalized groups may reduce political alienation (Bieber 2004). It sends signals about which groups are perceived as “fit to rule” (Mansbridge 1999) or make effective role models (Phillips 1998). In the United States, this argument would seem to apply perfectly to a group that is presently excluded from government

and explicitly barred from holding office in seven US states (Goodstein 2014). Levels of animosity toward atheists and agnostics are consistently higher than toward Muslims and other outgroups (Jones 2012; Saad 2020). The nonreligious, particularly atheists, are among the most hated and disliked groups in America and are arguably the most disliked. Perhaps their exclusion from government sends a clear message that the nonreligious are not a group deemed fit to rule or welcome in the halls of power. Exclusion then furthers marginalization through alienation and the perpetuation of biased attitudes.

Whether inclusion promotes representative policy, better policy for all groups, rights protection, or reduces stigmatization and alienation, the argument for minority representation in legislative bodies is strong. Given that no other group that comprises such a large section of the population is similarly excluded from government in the United States, the normative case for nonreligious representation is similarly strong. Such exclusion is unimaginable with regards to other minorities in the 21st century so understanding this exclusion is both interesting and normatively valuable.

The inconsistency between the number of nonreligious people and the number of nonreligious representatives creates a puzzle: Why has this large and rapidly growing religious group not achieved meaningful descriptive representation? This paper builds on previous work showing widespread negative perceptions of nonreligious candidates (Madrid et al. 2022) and provides evidence of a causal mechanism linking outgroup bias to electoral behavior among voters. I argue that outgroup bias among religious voters is the mechanism driving the underrepresentation of the nonreligious and is the primary reason for the group's exclusion from government.

I demonstrate the causal link through a conjoint candidate choice experiment, wherein nonreligious identity reduces support among the religious. Candidates whose religious identity was atheist, agnostic, or none were significantly less likely to be selected by religious respondents. Furthermore, I find that nonreligious voters do not exhibit generalized ingroup support for nonreligious candidates. Only candidates identified as atheists were more likely to be selected by nonreligious respondents. I conclude that, because of their religious identity, nonreligious candidates are likely to lose support from religious voters and are not able to reliably gain additional nonreligious votes. This evidence provides a causal link between bias and electoral behavior. The lack of nonreligious representatives is being driven by religious voters' bias against them in an electoral context. The further lack of generalized support from nonreligious voters likely means that it is unlikely that gains in representation will accompany the groups expected expansion. Consequently, remedying the lack of nonreligious representatives will likely require efforts to reduce outgroup bias and misperceptions of nonreligious persons generally.

Outgroup Bias

Previous literature presents three sources for reduced trust and bias against the nonreligious. The first is a simple out-group cue, wherein the religious perceive the nonreligious as an "other." Second, the explicit association of religious identity with morality provides a rational basis for bias against the nonreligious, particularly with regards to public office holding. A sincerely held belief that religion is the source of morality naturally leads to suspicion of the nonreligious and bias against nonreligious persons and candidates. Unsurprisingly, bias against the nonreligious is predicted by the strength of association between religion and morality (Gervais, Shariff, and Norenzayan 2011). Third, implicit processing has been found to bias even

weakly religious or nonreligious persons against the nonreligious due to implicit preferences formed early in life (Albertson 2011). The inclusion of implicit bias means that even the nonreligious can exhibit an implicit preference for religious candidates and may not universally exhibit an ingroup preference for nonreligious candidates.

Atheists have long been one of the most disliked groups in America (Edgell, Gerteis, and Hartmann 2006; Schafer and Shaw 2009). Atheists are rated the lowest of any group when individuals are asked whether certain groups share their worldview (Edgell, Gerteis, and Hartmann 2006). Economists have found that in trust games participants exhibit more trust toward highly religious participants (Tan and Vogel 2008). Similarly, sociologists have found that many Americans “associate religiosity with morality and trustworthiness” and view religious identity as a “basis for citizenship and a source of a common American identity” (Edgell, Gerteis, and Hartmann 2006). Moral psychologists have found that “for many people in many cultures, morality is religion” (Rai and Fiske 2011). Bias against nonreligious persons is pervasive, with negative feelings toward the nonreligious, highest among the most religious (Gervais, Shariff, and Norenzayan 2011).

Particularly relevant to politics are findings that the public is explicitly unwilling to vote for nonreligious candidates. Gallup’s willingness-to-vote measure, has consistently ranked atheist at or near the bottom of citizens’ willingness to vote for an otherwise qualified candidate (Saad 2020). Among identity groups only gay/lesbian candidates have ever been ranked lower than atheist by respondents to Gallup’s polls, occurring only in 1979. Furthermore, Madrid et al. (2022) show that this bias is rooted in broad negative perceptions of nonreligious candidates as well as perceptions of incompetence across a range of policy issues. They outline a framework based in social identity theory (H. Tajfel and Turner 1979) wherein nonreligious candidates are

evaluated as a group outside the mainstream of American political life. These findings provide a rich description of what the bias against nonreligious candidates looks like. However, no work to date has linked findings of bias to actual behavior in an electoral context or asked whether the nonreligious exhibit ingroup support for nonreligious candidates.

This paper provides a link between bias against nonreligious candidates and candidate choice in an experimental setting. I demonstrate a causal mechanism by which the nonreligious have been excluded from democratic government in the United States. Given the literatures' broad findings of generalized bias against the nonreligious (Albertson 2011; Edgell, Gerteis, and Hartmann 2006; Gervais, Shariff, and Norenzayan 2011; Rai and Fiske 2011; Tan and Vogel 2008), and nonreligious candidates specifically (Madrid et al. 2022), I expect religious voters will be less likely to support nonreligious candidates, *ceteris paribus*. I hypothesize that bias against the nonreligious is causally linked to the group's exclusion from government via electoral disadvantage. Religious voters are less likely to support nonreligious candidates, and thus, candidates without religious identities either lose elections, conceal their identity, or choose not to run.¹¹ Furthermore, I expect religious voters do not make distinctions between nonreligious subgroups and will exhibit bias against candidates identified as atheists, agnostics, or nones. These expectations lead to two hypotheses about religious voters, stated formally here: My hypotheses: First, *religious voters will be less likely to support candidates with nonreligious identities (Analysis 1)*. This first hypothesis comes from broad evidence of bias and dislike against the nonreligious and is likely the driving force behind underrepresentation.

¹¹ Widespread concealment among nonreligious office holders is potentially observationally equivalent to exclusion. This issue is engaged in the discussion.

Second, *religious voters will not make distinctions between the nonreligious subgroups and will be less likely to support candidates with all nonreligious identities (Analysis 2).*

Ingroup Support

The existence of widespread outgroup bias against the nonreligious has broad support in the literature. Ingroup voting behavior has been well supported among religious groups (Hunter 2001; Verkuyten 2007). However, much less work has been done to examine the nonreligious themselves as a political group. Like other politically relevant religious groups, the nonreligious have a common partisan and ideological identity. They have long leaned heavily toward identifying as liberals and Democrats similar to how Evangelicals identify as conservative Republicans (Pew Research Center 2012). The nonreligious have a stable common set of policy preferences for increased secularism, abortion rights, and LGBT rights (Pew Research Center 2012). Interest groups like American Atheists organize nonreligious Americans in the political sphere and further collective organizations for the nonreligious exist (Smith 2013).

Ingroup behavior is documented among the nonreligious for decisions such as charitable giving (Hawkins and Nosek 2012). Yet, no work to date has shown a preference among nonreligious voters for ingroup candidates. Therefore, I hypothesize that a lack of ingroup support for nonreligious candidates provides a further mechanism beyond outgroup bias to explain the lack of representation. I propose that, unlike racial groups (Barreto 2007), nonreligious candidates do not receive a general increased support from ingroup members (nonreligious voters). Why would it be the case that an ingroup fails to exhibit a preference for ingroup candidates?

I propose that candidates' nonreligious identity does not provide voters with heuristic information about a candidate. Nonreligious identity does not provide useful heuristic information like evangelical identity. "Evangelical" serves as a useful ingroup heuristic because a large percentage of identifiers have similar right wing beliefs and those beliefs are publicly linked to the identity (Wilcox, Rozell, and Green 2003). Furthermore, Evangelicals have a history of political activism and a large number of elected officials identify with evangelicalism (Wilcox, Sigelman, and Cook 1989b). Thus, an environment exists that allows a voter to make heuristic judgments about a candidate identified as evangelical because the identity is publicly associated with preferences that ingroup members are likely to share and voters have existent examples of evangelical candidates to inform their intuitions about how such candidates perform in office. Neither of these conditions hold for nonreligious candidates. First, nonreligious identity is not publicly associated with a particular set of policies or an ideology. So even if the group members have coherent policy preferences, the lack of public association between the identity and particular policies limit its heuristic effectiveness. Second, voters cannot look to existent nonreligious office holders to inform their intuitions about the performance of a nonreligious candidate because there are essentially no nonreligious office holders.

Furthermore, many nonreligious voters may have an implicit preference for religious candidates due to early socialization (Albertson 2011). As a result, I expect nonreligious support for nonreligious candidates to be much lower than other religious groups' support for ingroup candidates. This lack of ingroup support, combined with outgroup bias, can explain the lack of representation in districts with high numbers of nonreligious voters where we might otherwise expect nonreligious candidates to perform well. These expectations lead to the hypotheses about nonreligious voters, stated formally here:

Third, *among nonreligious voters there will be no differences in support for candidates with nonreligious and religious identities (Analysis 3)*. Low levels of support for nonreligious candidates among the nonreligious are expected as a component of my theory to explain the lack of representatives from areas where the nonreligious are geographically concentrated.

Fourth, *among nonreligious voters there will be no differences in support for candidates with different nonreligious identities: atheist, agnostic, and none (Analysis 4)*.

Methodology: Conjoint Design

To test my hypotheses, I fielded a conjoint experiment fielded on a national sample. I employ as conjoint design because it is the ideal method for testing whether an identity effects voters' likelihood of supporting a candidate (Hainmueller, Hopkins, and Yamamoto 2014). Conjoint analysis allows researchers to identify component specific causal effects by randomly manipulating multiple attributes simultaneously. Additionally, conjoint design limits concerns about social desirability by providing respondents multiple identities to justify discriminatory choices (Wallander 2009). A conjoint analysis run on a sample subsetted to respondents with religious identities allows for causal claims about religious persons preferences and behaviors in similar real-world circumstances. Conversely, a conjoint analysis run on a sample subsetted to respondents with nonreligious identities allows for causal claims about nonreligious persons preferences and behaviors in similar real-world circumstances.

The conjoint experiments detailed and presented below as Analyses 1-4, are indented to answer the following questions. First, do the biases that religious voters hold against nonreligious candidates, noted in the literature (Madrid et al. 2022), translate into a reduced likelihood of supporting nonreligious candidates? Second, do descriptive representations matter for

nonreligious persons such that they are more likely to support nonreligious candidates? Third, do religious voters make distinctions between different types of nonreligious candidates or is bias generalized? Fourth, do nonreligious voters make distinctions between nonreligious subgroups, such that they are more likely to support nonreligious candidates of a particular identity?

In order to answer these questions, I fielded an original nationally representative survey through Lucid in July of 2022 with an imbedded conjoint experiment.¹² The imbedded conjoint serves as a stated preferences measure of outgroup animosity and ingroup support towards nonreligious candidates. The sample yielded 3,672 high-quality¹³ respondents and the study was approved by the institutional review board at University of California, Davis; all subjects provided written and informed consent. The experimental design is largely taken from Hainmuller et al (2014) and profile categories can be seen in Table 4. Profiles were presented side by side on the same screen as seen in Figure 12, each pairing is presented on a new screen, and the order in which attributes are presented is randomized across respondents.

¹² Because of concerns over Lucid's respondent pool I include multiple attention check questions based on Aronow et al. 2020. I include a cut question that filters out respondents who fail the attention check question and used attention check questions from (Aronow et al. 2020). In addition, I eliminate speeders from the sample who finished in less than 5 minutes, half the mean time rounded to the nearest minute.

¹³ Defined as passing three separate attention checks and completing all required questions. In addition, I eliminate speeders from the sample who finished in less than 5 minutes, half the mean time rounded to the nearest minute.

Table 4: Conjoint Attributes

Attribute Category	Attributes
Religion	Catholic; Evangelical; Christian; Atheist; Agnostic; None
Race	Hispanic; White; Black; Asian
Age	46; 53; 67; 74; 82
Political Experience	No political experience; District Attorney; State Representative; Mayor
Prior Occupation	Lawyer; Business Owner; Doctor; Car Dealer

Note: Profiles are generated randomly and respondents are given a forced choice between each randomly generated set of profiles.

A "forced choice" between the two candidates was used to enable evaluation of the role of each attribute value plays in the assessment of one profile relative to another. An example profile is displayed in figure 13. This forced choice is designed to mimic real-world situations wherein voters choose between candidates who differ on multiple attributes. The question structure is keeping with the experimental procedure from Hainmuller et al (2014). Respondents have no additional information to make decisions on and the profile attributes are fully randomized. Therefore, any effects estimated from profile categories cannot have a cause other than the text displayed as a candidate attribute.

Figure 13: Conjoint Profile Example

Which of these candidates would you prefer as your **City Councilor** in your local city or town?

	Candidate 1	Candidate 2
Race	White	White
Religion	None	Agnostic
Age	67	82
Prior Occupation	Car Dealer	Lawyer
Political Experience	District Attorney	State Representative

Candidate 1

Candidate 2

Note: Profiles are generated randomly and respondents are given a forced choice between each randomly generated set of profiles.

The inclusion of five attributes obscures the study’s focus on religious identity and ensure internal validity (Hainmueller, Hopkins, and Yamamoto 2014). The additional attributes, also detailed in table 4, were candidates' previous profession (Lawyer; Business Owner; Doctor; Car Dealer), racial/ethnic background (Hispanic, White, Black, and Asian), age (46, 53, 67, 74, 82) and previous political experience (No political experience; District Attorney; State Representative; Mayor). Candidates’ religious identity took one of six values; three of which were nonreligious values (Atheist, Agnostic, or None) and three of which were religious values (Catholic, Evangelical, and Christian). The inclusion of three nonreligious categories is designed to determine if, and to what degree, the subgroups matter in participants’ selection of candidates. Party ID was not included in order to avoid crowding out the other attributes (Kirkland and Coppock 2018). These candidate attributes function as the independent variables and selection or not of a profile by respondents’ functions as the dependent variable.

Estimates are calculated as the effects of profile or candidate religion on the likelihood of selection using the Average Marginal Component Effects (AMCE). The AMCE reports the average effect of conjoint profile attributes on the likelihood that respondents choose a given profile. In other words, the “AMCE is interpreted as the average change in the probability that a profile will win support when it includes the listed attribute value instead of the baseline attribute value (Hainmueller, Hopkins, and Yamamoto 2014 page 19).” Importantly, reported effects can only be interpreted vs the baseline value, not against each other. Figures and tables report AMCEs vs the baselines but cannot be interpreted against each other. Controls for all AMCE models presented include all other profile attributes, and all models used standard errors clustered by respondents as suggested by Hainmuller et al (2014).

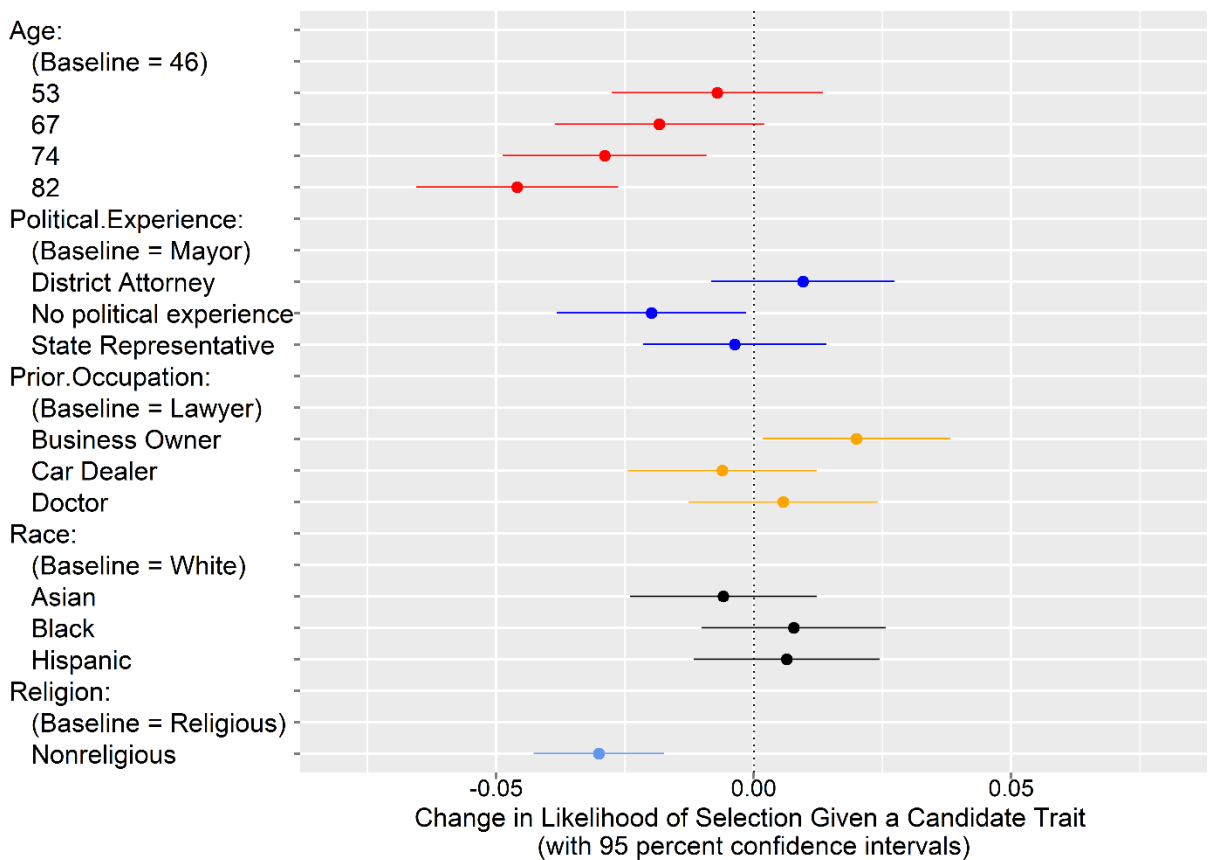
Results

To test hypotheses one, I first aggregated candidate religion into two categories, religious (Christian, Catholic, and Evangelical) and nonreligious (Atheist, Agnostic, or None). Category aggregation is done for clarity and simplicity; however, all substantive results hold if the categories are left unaggregated. Second, I subsetted respondents to those who identified as religious. Results were then calculated as the effects of profile or candidate religion on the likelihood of selection using the Average Marginal Component Effects (AMCE). If hypothesis one is correct, we should see a statistically significant and positive coefficient for religious vs nonreligious candidates.

Analysis 1 confirms hypothesis one and shows that religious respondents are less likely to support nonreligious candidates. Figure 14 shows the average marginal component effects (converted to a percentage) for religious vs nonreligious candidate selection among religious respondents. Relative to a religious candidate, nonreligious office seekers were 3 percent less

likely to be selected by religious respondents.¹⁴ Based on these results it is clear that religious voters negative views of the nonreligious do indeed manifest in vote choices and provide a significant disadvantage for nonreligious candidates seeking office. Nonreligious office seekers can expect to suffer a significant electoral penalty by religious voters. Does this effect apply to all nonreligious office seekers, whether they are atheists, agnostics, or simply have no religious identification?

Figure 14: Religious Respondents with Binary Conjoint Religious Categories

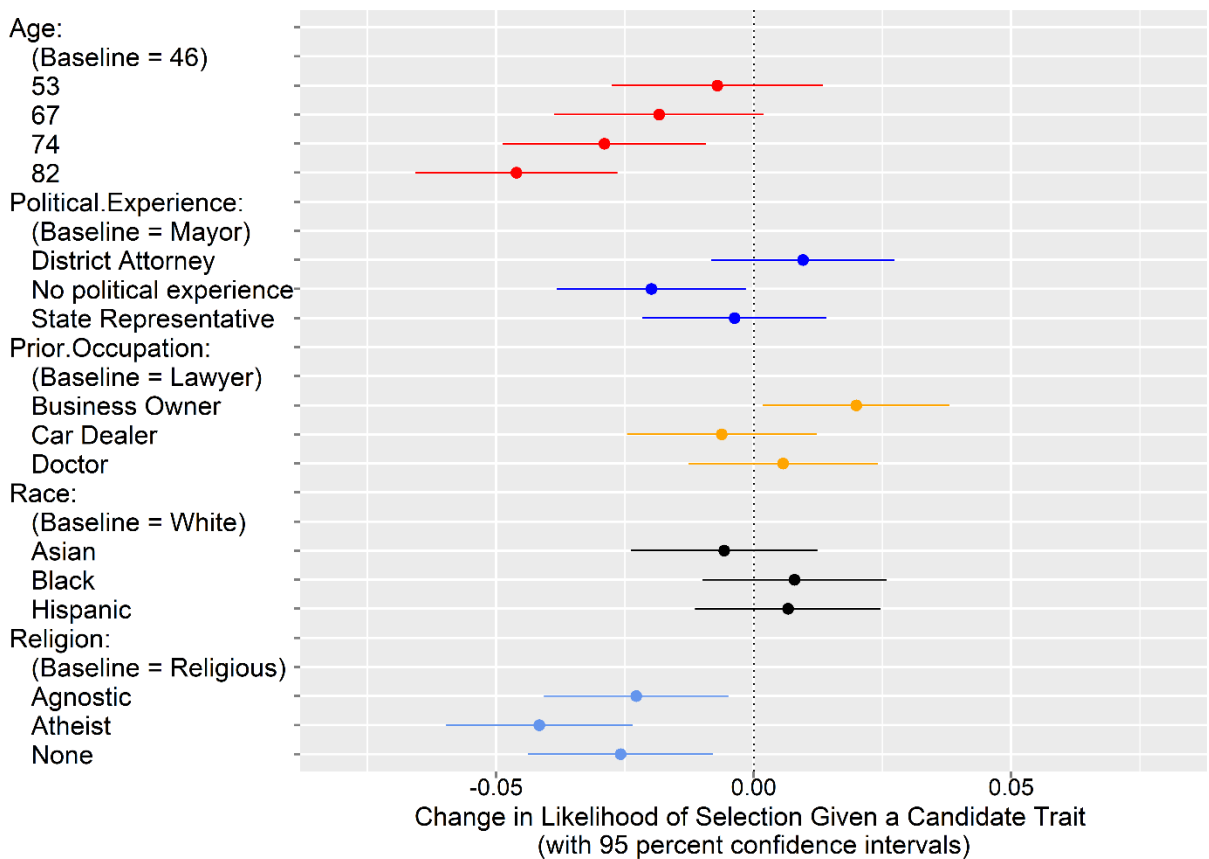


Note: N = 1964. Reported effects can only be interpreted vs the baseline value, not against each other.

¹⁴ These results hold if the model is estimated for the highly religious, those with explicitly negative views of the nonreligious, or those who agreed with the statement “Atheists are a threat to the United States”.

To test hypotheses two, I first aggregated religious candidates into a combined category, “Religious” (Christian, Catholic, and Evangelical). Atheist, Agnostic, and None were left unaggregated. As before, category aggregation among religious identities is done for clarity and simplicity; all substantive results hold if the category is left unaggregated. As with analysis 1 I subsetted respondents to those with a religious identity based on their religious identifications. Results were calculated as the effects of profile or candidate religion on the likelihood of selection using the Average Marginal Component Effects (AMCE). If hypothesis two is correct, we should see a statistically significant and positive coefficient for religious vs atheist, agnostic, and none candidates.

Figure 15: Religious Respondents with Conjoint Religious Categories Combined



Note: N = 1964. Reported effects can only be interpreted vs the baseline value, not against each other.

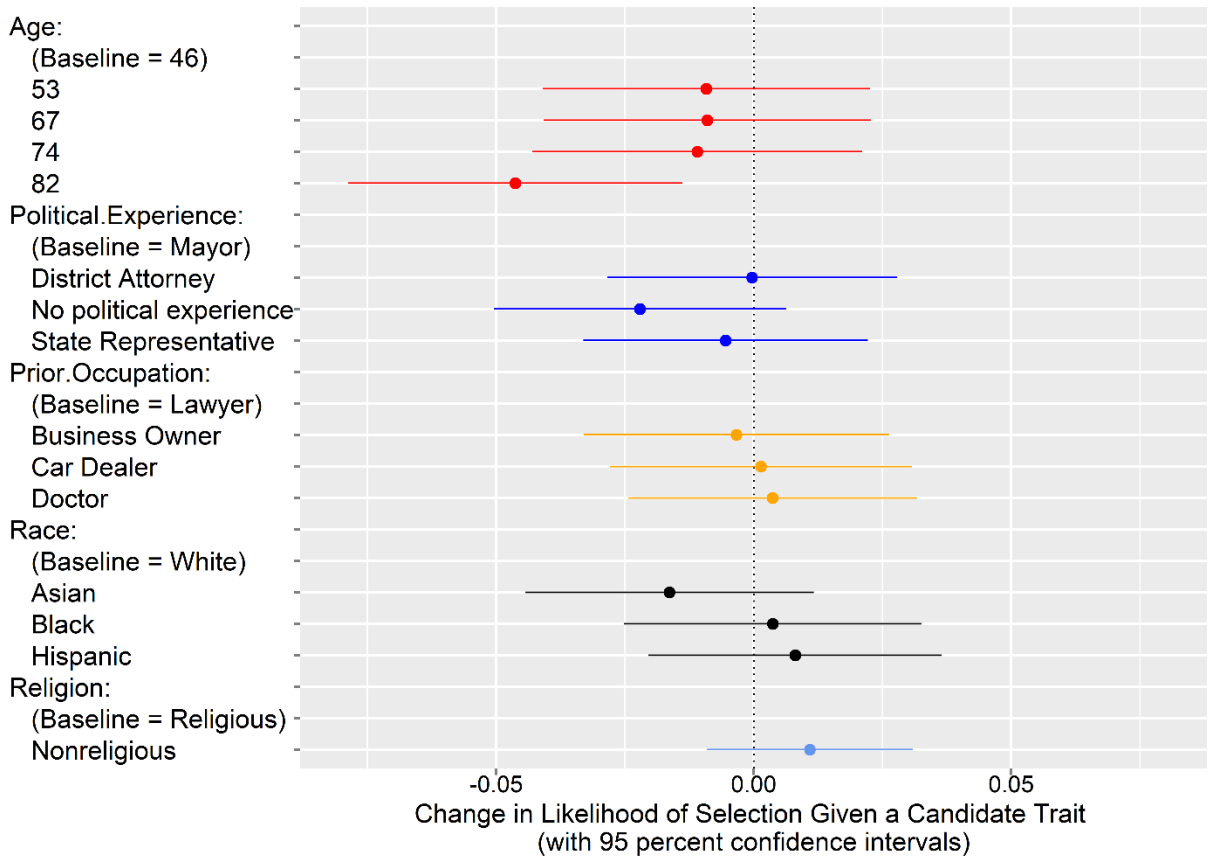
Analysis 2 confirms hypothesis two and shows that religious respondents are less likely to support nonreligious candidates regardless of whether they identify as atheist, agnostic, or simply have no religious identification. Figure 15 shows the average marginal component effects (converted to a percentage) for candidates with religious vs atheist, agnostic, and no religious identification among religious respondents. Relative to a religious candidate, office seekers who identify as atheist, agnostic, or have no religious identification were respectively 4.1, 2.3 and 2.6 percent less likely to be selected by religious respondents. Based on these results, it is clear that religious voters' negative views of the nonreligious do not depend on nonreligious candidates identifying explicitly as atheists. Furthermore, the estimates are not statistically distinct when estimated separately against each other, so it is likely that religious voters do not make distinctions between the different nonreligious subgroups.¹⁵ This finding validates the common use of atheists as a stand in for the larger group. The results presented here further suggest that findings of bias against atheists likely generalize to the larger nonreligious group. Candidates face an electoral penalty from nonreligious voters regardless of which nonreligious identity they hold. Examined next, is whether nonreligious candidates garner increased support from nonreligious voters.

To test hypothesis three, I aggregated candidate religion into two categories, religious (Christian, Catholic, and Evangelical) and nonreligious (Atheist, Agnostic, or None). As with analysis 1 and 2, category aggregation is done for clarity and simplicity; however, all substantive results hold if the categories are left unaggregated. Second, I subsetted respondents to those who identified as nonreligious. Results were then calculated as the effects of profile or candidate religion on the likelihood of selection using the Average Marginal Component Effects (AMCE).

¹⁵ AMCE estimates show effects only vs the specified baseline.

If hypothesis three is correct, we should not see a statistically significant coefficient for religious vs nonreligious candidates.

Figure 16: Nonreligious Respondents with Binary Conjoint Religious Categories



Note: N = 800. Reported effects can only be interpreted vs the baseline value, not against each other.

Analysis 3 confirms hypothesis three and shows that nonreligious respondents are not systematically more likely to support nonreligious candidates. Figure 16 shows the average marginal component effects (converted to a percentage) for religious vs nonreligious candidate selection among nonreligious respondents. Although the point estimate shows that nonreligious respondents are 1 percent more likely to selecting nonreligious candidates, the estimate is not statistically significant using a 95% confidence interval. Therefore, I conclude that relative to a

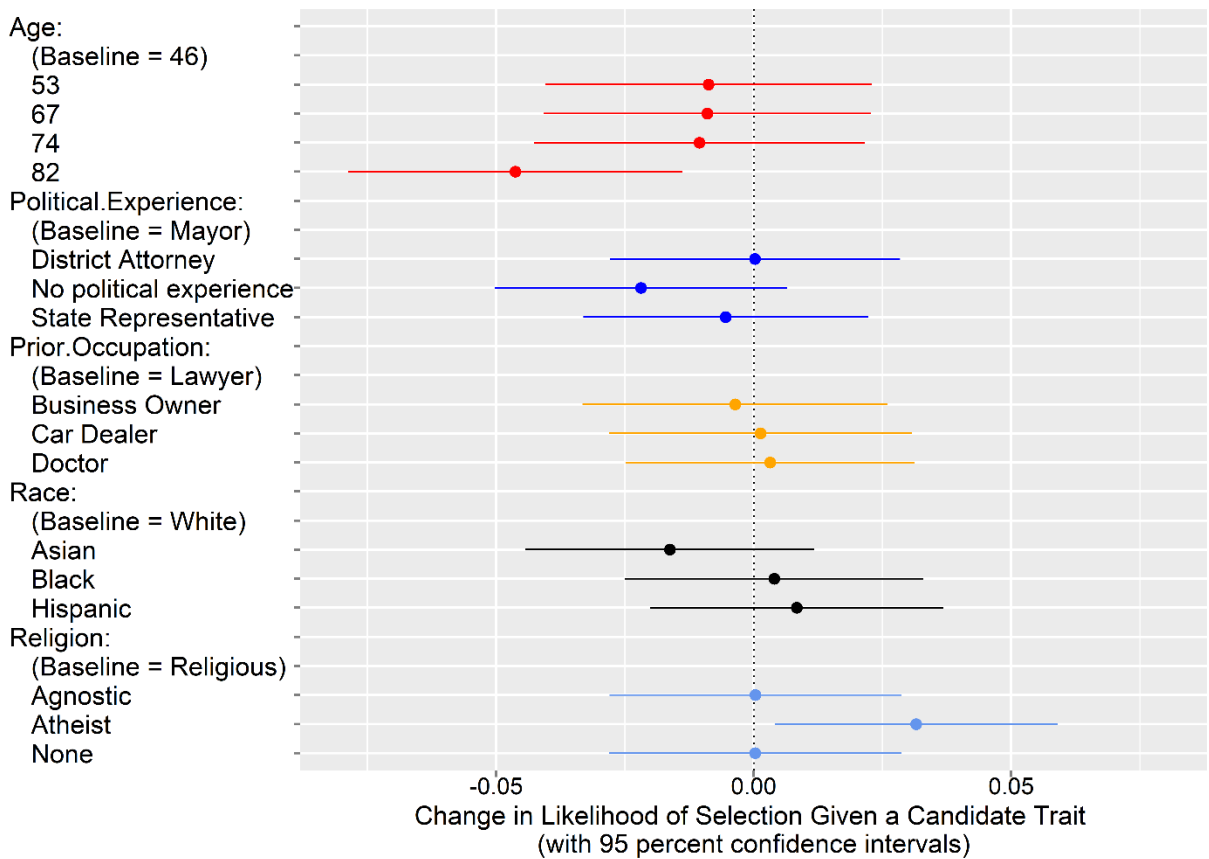
religious candidate, nonreligious office seekers were not systematically more likely to be selected by nonreligious respondents. Based on these results, it is clear that nonreligious voters do not exhibit a systematic ingroup preference for nonreligious candidates. These findings suggest that nonreligious voters do not generally support descriptive representatives. Nonreligious candidates are unlikely to make up for the penalty of religious voters with increased support from the nonreligious. However, this may not be the case for all nonreligious candidates.

To test hypotheses four, I aggregated religious candidates into a combined category, “Religious” (Christian, Catholic, and Evangelical). Atheist, Agnostic, and None were left unaggregated. As before, category aggregation among religious identities is done for clarity and simplicity; all substantive results hold if the category is left unaggregated. As with analysis 3, I subsetted respondents to those with a nonreligious identity based on their religious identifications. Results were calculated as the effects of profile or candidate religion on the likelihood of selection using the Average Marginal Component Effects (AMCE). If hypothesis four is correct, we should not see a statistically significant coefficient for religious vs atheist, agnostic, and none candidates.

Analysis 4 disconfirms hypothesis four and shows that nonreligious respondents are not systematically more likely to support nonreligious candidates unless that candidate identifies as an atheist. Contrary to my expectations, nonreligious respondents were more likely to prefer one of the nonreligious subgroups. Figure 17 shows the average marginal component effects (converted to a percentage) for candidates with religious vs atheist, agnostic, and no religious identification among nonreligious respondents. No systematic difference in the likelihood of selection is found for candidates identified as agnostic or none. However nonreligious respondents were 3.2 percent more likely to select atheist candidates. Therefore, I conclude that

while nonreligious voters do not exhibit an ingroup preference for nonreligious candidates in general, they do for atheist candidates. The nonreligious are more likely to prefer atheist candidates and do have a preference for descriptive representation when candidates identify as atheists. This finding is contrary to my expectations based on the literature. The implications of this finding are outlined in the discussion section.

Figure 17: Nonreligious Respondents with Conjoint Religious Categories Combined



Note: N = 800. Reported effects can only be interpreted vs the baseline value, not against each other.

Discussion

As the portion of Americans identifying as nonreligious continues to rise, this groups' lack of representation will create an increasing gap between the public's religious views and those of our "ideally" representative government. Furthermore, the exclusion of the nonreligious

from government should concern anyone interested in the separation of church and state, or secular government more broadly. It is unlikely that secular government can be maintained if religious identification is a requirement for those seeking power. Particularly in a country that is on track to be majority nonreligious in the coming decades. Understanding why nonreligious persons are excluded from Congress is a crucial first step toward achieving representation. Establishing an understanding of the mechanisms inhibiting representation can aid candidates trying to achieve gains in descriptive representation.

Previous work has established that the public holds negative views of the nonreligious (Saad 2020) and nonreligious candidates in particular (Madrid et al. 2022). Utilizing conjoint experiments this paper shows that religious voters' biases against nonreligious candidates are likely the primary reason why a quarter of the US population lacks descriptive representation. This contribution builds on previous work by demonstrating the causal link between the perception voters have about nonreligious candidates (Madrid et al. 2022) and vote choices resulting from those perceptions. Additionally, I show that the religious do not make distinctions between different nonreligious identifications whereas the nonreligious prefer candidates who identify as atheists. These results have direct and straightforward implications for candidates running for office. Unless a candidate is in a district with a substantial nonreligious population, they should conceal their nonreligious identity. In districts with large numbers of nonreligious voters, candidates without religious convictions should explicitly identify as atheists. Open identification is likely to result in an electoral penalty from religious voters. However, because religious voters appear to make no distinctions between nonreligious identifications there is likely to be no additional penalty in identifying as an atheist. Furthermore, losses among the religious could be made up with nonreligious voters but only via open identification as an atheist.

Given the results of this study, it is likely that all nonreligious subgroups are similarly perceived by religious voters. However, existing work largely uses atheists as a stand in for the nonreligious, generally, making it impossible to know if nonreligious groups are treated differently in other contexts. Further work should examine how or whether perceptions of atheists differ from those of agnostics or nones. Additional work is also needed to understand why the nonreligious prefer atheist candidates but not other nonreligious groups. It is possible that atheists are perceived as more valid descriptive representatives. Identification with the most ardent nonreligious group may signal characteristics desirable in a descriptive representative, such as strong mutual ties (Dovi 2002), capacity to serve as role models (Wolbrecht and Campbell 2007), or may signal that a lack of religious belief is a more central part of a candidate's identity. Determining why nonreligious voters support atheist candidates is the next logical step in remedying the group's underrepresentation.

An additional extension of this paper could be to compare liberal "tolerant" districts to highly religious districts in order to compare levels of bias directed at candidate across environments. It is likely that highly religious regions of the US have higher bias whereas voters in places like Oakland California (formerly the district of the only ever openly atheist congressperson) are less biased because a large percentage of residents are nonreligious. Examining bias regionally by gathering data on local samples would likely aid in explaining the distribution of state level nonreligious elected representatives as well as the barriers to national representation. This could be done through the use of a conjoint or pairwise design and the recruitment of samples from various regions in the United States.

A limitation of this paper is the inability to determine if underrepresentation is explained by nonreligious candidates concealing their identity. Although the results presented demonstrate

that bias is driving reduced electoral support for nonreligious candidates, how candidates respond to bias is unexamined. Since, religious identity is confined to internal beliefs without any readily identifiable characteristics, there may be a large degree of concealment among nonreligious candidates and representatives. The identity is extremely easy to conceal, as it is entirely a matter of one's internal beliefs. So, given that representatives, candidates, and party officials are likely aware of the bias against the nonreligious, we should expect a high degree of concealment. Examples of identity concealment among the nonreligious office holders include former congressman Barney Frank (D-MA), who after leaving office came out as nonreligious, former representative Pete Stark¹⁶ (D-CA), who despite being elected in 1973 did not come out as nonreligious until 2007, and Jared Huffman (D-CA), who came out only after being in office and winning reelection to his second term. These three congressmen were able to easily conceal their religious identity for decades while in office and it is likely that other current and past office holders are as well. Strategic concealment of an unpopular identity provides another valid explanation for the lack of openly nonreligious representatives by shifting the question to why candidates and representatives continue concealment despite rising numbers of nonreligious identifiers.

However, this theory is likely unfalsifiable and largely untestable given our inability to gather accurate data on representatives' religious belief. Consequently, this paper assumes truthful reporting of beliefs by candidates and focuses on examining bias and a lack of ingroup support as explanations for the lack of nonreligious representatives without arguing against the presence or importance of concealment. Indeed, the data and methods utilized could be applied to questions of strategic concealment as well by showing the empirical basis upon which such a

¹⁶ Representative Pete Stark remains the only atheist to have openly served in Congress.

strategic decision may be based. Furthermore, a more complete understanding of the bias against nonreligious candidates has the potential to be useful to office holders currently concealing their lack of religious convictions. Future work could experimentally examine the propensity of nonreligious persons to conceal their identity in the context of bias from the religious.

Conclusion

Chapter one introduces a new measure of affect and utilizes it to show that highly polarized partisans characterize out-party members with negative valence, rather than ideology. Although the study uses one-word questions as a measure for partisan animosity, the measure can be applied in other ways. For example, it can be used to compare partisans in other countries to study affective polarization comparatively. It can also be used to study differences in how partisans characterize each other across parties in multi-party democracies, given existing research on the topic (Gidron, Adams, and Horne, 2022). Additionally, the measure can be applied to any group, idea, item, or concept that researchers are interested in. For example, Amlani and Kiesel (2022) used it to examine one-word evaluations of vaccinated or unvaccinated Americans. In this study, respondents were asked to code the sentiment of their word, but scholars can also ask respondents to choose from a pre-existing list of emotions (e.g. angry, frustrated, sad, or happy) that capture the feeling behind their word selection. This data would extend the existing work and capture the emotions that in and out-party members evoke from respondents.

Chapter two uses a conjoint experiment to show that partisans have genuine taste-based preferences against social engagement with out-party members. Fewer social interactions with members of the out-party are likely to intensify otherization and increase animosity. Additionally, the findings suggest that efforts to reduce affective polarization will be

increasingly ineffective as social polarization grows. Attempts to encourage cross-party empathy, dialogue, and understanding will be hindered by the absence of out-partisans in Americans' social spheres. Research already suggests that partisans inhabit social spheres with like-minded partisans, and breaking into these social spheres as an opposing partisan is becoming less common (Butters and Hare, 2020). Further experiments are needed to expand the scope of partisans' preferences for social isolation in the United States by testing additional social situations and attributes to confirm the effects of partisan animosity across American life. Internationally, conjoint designs can be used to test whether affective polarization affects interpersonal decisions differently in countries with multi-party systems. For example, if a party is in a coalition, do its partisans treat members as an ingroup for social decisions? Are parties' traditional opponents the most discriminated against or does ideological distance determine the degree of discrimination in social contexts? Do parliamentary systems have the same degree of interpersonal discrimination as presidential democracies? These institutional questions and others are all promising areas for extending the findings of chapter two.

Chapter three examines the effects of religious identity on candidate choice and shows that religious voters will discriminate against nonreligious candidates. My findings have direct implications for candidates running for office, who should conceal their non-religious identity unless running in a district with a substantial non-religious population, where identifying as an atheist may be beneficial. There is a need for further study to understand why non-religious voters prefer atheist candidates and whether perceptions of atheists differ from those of agnostics or nones. Additional work is also needed to studying bias regionally by gathering data on local samples as it is likely that regional dynamics play a role in the salience of nonreligious identity.

My findings demonstrate that affect and identity are central to politics. Emotional evaluations and group identities drive partisan evaluations, non-political social decisions, and candidate choices. Out-group members are characterized as reprehensible people with low character, little intelligence, and meager worth. People are willing to sacrifice time, money, and safety to avoid interactions with them, and candidates identified as out-group members will lose support. These results align with the expectations of Social Identity Theory (Tajfel and Turner, 1979).

Unfortunately, my research points to growing hostility as a continued theme of modern politics, and thus highlights the need for further study on how to reduce identity-based engagement in politics.

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Appendix

Supplement Material for Chapter One

Survey Questions

In one word, how would you describe Republican voters?

[Open-Ended Response 1]

Would you rate your word, [Open-Ended Response 1], about Republican voters as negative, neutral or positive?

- Extremely Positive
- Positive
- Slightly Positive
- Neutral
- Slightly Negative
- Negative
- Extremely Negative
-

In one word, how would you describe Democratic voters?

[Open-Ended Response 2]

Would you rate your word, [Open-Ended Response 2], about Democratic voters as negative, neutral or positive?

- Extremely Positive
- Positive
- Slightly Positive
- Neutral
- Slightly Negative
- Negative

- Extremely Negative

Survey Quality

When fielding our survey on Lucid, we knew of the potential threat of receiving a low-quality sample. To ensure that our sample was high quality, we implemented checks into the fielding process. These checks include three attention check questions (one of which dismissed respondents who failed), timing questions that dropped individuals who took too long or sped through the survey too quickly, and missing questions check that dropped respondents who left too many missing questions.

Our survey includes three attention check questions. The first attention check question is as follows:

For our research, careful attention to survey questions is critical! We thank you for your care.

I understand

I do not understand

We placed this question at the beginning of the survey. Respondents who selected *I do not understand* were dropped in the sample.

The second attention check question is as follows:

For our research, careful attention to survey questions is critical! To show that you are paying attention, please select "I have a question" and "I understand" below.

I understand

I do not understand

I have a question

I do not have a question

We dropped respondents who did not check both "I have a question" and "I understand" from the sample and replaced by lucid without charge.

The final attention check question was placed at the end of the survey and is as follows:

People are very busy these days and many do not have time to follow what goes on in the government. We are testing whether people read questions. To show that you've read this much, answer both "extremely interested" and "very interested."

Extremely interested

Very interested

Moderately interested

Slightly interested

We use these questions from Aronow et al. (2020) paper on crafting attention check questions for Lucid. We dropped respondents who did not select both *extremely interested* and *very interested* in the analysis.

Next, we dropped respondents who took less than 6 minutes and more than 40 minutes to complete the survey from the analysis.

Then, we dropped respondents who did not answer at least 50 questions (or at least 60 percent of the maximum number of questions) from the analysis.

Finally, in the regression analysis exploring the dimensions of affect we drop words that do not make sense.

**Table 1A: Regression Results for Affect Dimension on Affective Polarization
Word Measure**

	Dependent Variable:		
	<i>Word Measure</i>		
	Base (1)	Control (2)	State Fixed Effects (3)
Affect Dimension: Valence	0.108*** (0.036)	0.115*** (0.034)	0.047 (0.035)
Affect Dimension: Policy	0.142*** (0.051)	0.172*** (0.048)	0.148*** (0.047)
Word Evaluation	0.365*** (0.008)	0.375*** (0.009)	0.379*** (0.009)
Age		-0.004*** (0.001)	-0.005*** (0.001)
Income		0.004 (0.003)	0.003 (0.003)
Gender: Male		0.036 (0.034)	0.071** (0.035)
Education		-0.024** (0.010)	-0.017* (0.010)
Ethnicity: Black		-0.041 (0.047)	-0.007 (0.051)
Ethnicity: Asian		-0.324*** (0.069)	-0.350*** (0.070)
Ethnicity: Native American		0.033 (0.162)	-0.026 (0.172)
Ethnicity: Pacific Islander		0.193 (0.473)	0.171 (0.446)
Ethnicity: Other		0.263** (0.103)	0.298*** (0.103)
Ideological Extremity		0.080*** (0.015)	0.070*** (0.016)
Partisan Extremity		0.157*** (0.024)	0.182*** (0.024)
Donor: Yes		0.088** (0.041)	0.084** (0.041)
Voter: Yes		0.081* (0.042)	0.149*** (0.043)
Constant	-0.518*** (0.025)	-0.834*** (0.087)	-0.908*** (0.181)
Observations	1,132	1,124	1,119
Adjusted R2	0.638	0.698	0.736
Residual Std. Error	0.576	0.544	0.556

Note:

* p<0.1; ** p<0.05; *** p<0.01

Affect dimension and word evaluation use words on out-partisans.

We word reverse-code word evaluation, so positive values indicate negative evaluations.

**Table 2A: Regression Results for Affect Dimension on Affective Polarization
Thermometer Measure**

	Dependent Variable:		
	<i>Thermometer Measure</i>		
	Base (1)	Control (2)	State Fixed Effects (3)
Affect Dimension: Valence	0.271*** (0.057)	0.192*** (0.056)	0.215*** (0.057)
Affect Dimension: Policy	-0.116 (0.075)	0.081 (0.079)	0.022 (0.077)
Word Evaluation	0.247*** (0.014)	0.233*** (0.014)	0.236*** (0.015)
Age		-0.002 (0.002)	-0.002 (0.002)
Income		0.006 (0.004)	0.013*** (0.004)
Gender: Male		0.144** (0.056)	0.202*** (0.057)
Education		-0.079*** (0.017)	-0.066*** (0.017)
Ethnicity: Black		-0.079 (0.078)	-0.059 (0.084)
Ethnicity: Asian		0.452*** (0.114)	0.509*** (0.114)
Ethnicity: Native American		-0.017 (0.266)	-0.294 (0.281)
Ethnicity: Pacific Islander		-0.365 (0.775)	-0.460 (0.728)
Ethnicity: Other		-0.324* (0.169)	-0.378** (0.168)
Ideological Extremity		0.012 (0.025)	-0.052** (0.026)
Partisan Extremity		0.340*** (0.039)	0.400*** (0.039)
Donor: Yes		0.222*** (0.068)	0.151** (0.067)
Voter: Yes		0.012 (0.069)	0.111 (0.071)
Constant	-0.369*** (0.039)	-0.935*** (0.142)	-0.719** (0.296)
Observations	1,123	1,115	1,110
Adjusted R2	0.259	0.306	0.402
Residual Std. Error	0.859	0.833	0.865

Note:

* p<0.1; ** p<0.05; *** p<0.01

Affect dimension and word evaluation use words on out-partisans.

We word reverse-code word evaluation, so positive values indicate negative evaluations.

Table 3A: Regression Results for Affect Dimension on Affective Polarization Candidate Measure

	Dependent Variable:		
	Candidate Measure		
	Base (1)	Control (2)	State Fixed Effects (3)
Affect Dimension: Valence	0.154*** (0.058)	0.205*** (0.051)	0.261*** (0.052)
Affect Dimension: Policy	-0.209*** (0.076)	0.106 (0.072)	0.162** (0.071)
Word Evaluation	0.244*** (0.014)	0.196*** (0.013)	0.207*** (0.013)
Age		0.005*** (0.001)	0.005*** (0.001)
Income		-0.012*** (0.004)	-0.012*** (0.004)
Gender: Male		0.021 (0.051)	0.057 (0.052)
Education		-0.059*** (0.015)	-0.069*** (0.016)
Ethnicity: Black		-0.140** (0.071)	-0.252*** (0.076)
Ethnicity: Asian		0.329*** (0.104)	0.293*** (0.104)
Ethnicity: Native American		0.688*** (0.243)	0.724*** (0.256)
Ethnicity: Pacific Islander		0.271 (0.708)	0.423 (0.665)
Ethnicity: Other		-0.303* (0.155)	-0.202 (0.153)
Ideological Extremity		0.002 (0.023)	-0.025 (0.024)
Partisan Extremity		0.187*** (0.036)	0.214*** (0.036)
Donor: Yes		0.158** (0.062)	0.147** (0.061)
Voter: Yes		0.163*** (0.063)	0.229*** (0.065)
Constant	-0.310*** (0.039)	-0.680*** (0.130)	-0.335 (0.270)
Observations	1,132	1,124	1,119
Adjusted R2	0.237	0.305	0.4
Residual Std. Error	0.872	0.876	0.893

Note:

*p<0.1; **p<0.05; ***p<0.01

Affect dimension and word evaluation use words on out-partisans.

We word reverse-code word evaluation, so positive values indicate negative evaluations.

Table 4A: Regression Results for Affect Dimension on Affective Polarization Social-Distance Measure

	Dependent Variable:		
	<i>Social-Distance Measure</i>		
	Base (1)	Control (2)	State Fixed Effects (3)
Affect Dimension: Valence	0.014 (0.057)	0.015 (0.054)	0.074 (0.054)
Affect Dimension: Policy	-0.015 (0.081)	-0.037 (0.076)	-0.044 (0.074)
Word Evaluation	0.177*** (0.013)	0.186*** (0.014)	0.193*** (0.014)
Age		0.002 (0.001)	0.001 (0.002)
Income		0.002 (0.004)	0.005 (0.004)
Gender: Male		0.059 (0.054)	0.069 (0.055)
Education		-0.038** (0.016)	-0.031* (0.016)
Ethnicity: Black		0.008 (0.074)	-0.003 (0.080)
Ethnicity: Asian		0.147 (0.109)	0.184* (0.109)
Ethnicity: Native American		-0.182 (0.254)	-0.352 (0.270)
Ethnicity: Pacific Islander		-0.074 (0.742)	-0.051 (0.700)
Ethnicity: Other		-0.047 (0.162)	-0.105 (0.161)
Ideological Extremity		0.171*** (0.024)	0.128*** (0.025)
Partisan Extremity		0.295*** (0.038)	0.324*** (0.038)
Donor: Yes		0.175*** (0.065)	0.149** (0.064)
Voter: Yes		-0.115* (0.066)	-0.003 (0.068)
Constant	-0.189*** (0.039)	-1.041*** (0.136)	-0.477* (0.284)
Observations	1,132	1,124	1,119
Adjusted R2	0.138	0.281	0.377
Residual Std. Error	0.923	0.872	0.902

Note:

*p<0.1; **p<0.05; ***p<0.01

Affect dimension and word evaluation use words on out-partisans.

We word reverse-code word evaluation, so positive values indicate negative evaluations.

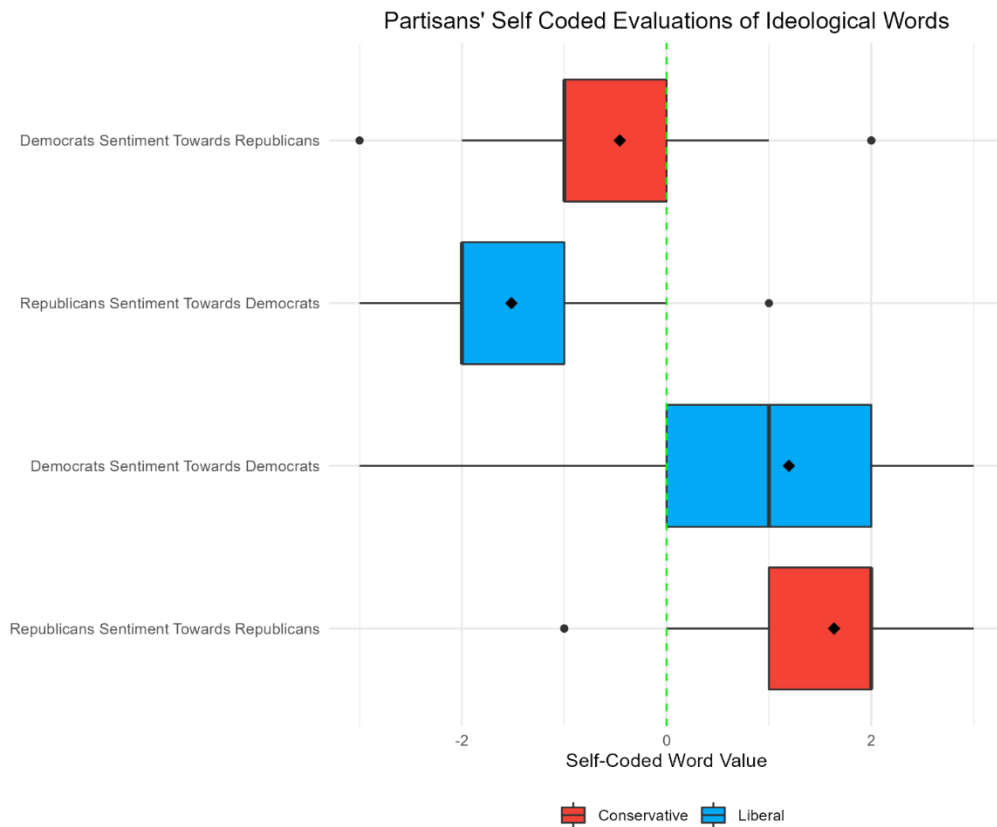
Liberal and Conservative as Words Evoke Affective Responses

In the main text of the paper, we note that the words “liberal” and “conservative” are ideological but evoke affect among respondents. Figure 1A report a boxplot of Democrats and Republicans self-coding of the words “liberal” and “conservative” as it relates to their own party and the out-party. The figures illustrate that ideological words evoke affect among partisans.

When Democrats report “liberal” and Republicans report “conservative” to describe in-partisans, 72 percent of Democrats and 82 percent of Republican code their ideological word as having positive affect. On average, Democrats and Republicans report a self-coded evaluation equal to 1.19 and 1.63 (median equal to 1 and 2) respectively.

Contrastingly, when Democrats report “conservative” and Republicans report “liberal” to describe out-partisans, 82 percent of Democrats and 54 percent of Republican code their ideological word as having negative affect. On average, Democrats and Republicans report a self-coded evaluation equal to -0.45 and -1.52 (median equal to -1 and -2) respectively.

Figure 1A: Distribution of Self-Coded “Liberal” and “Conservative” One-Word Evaluations



Note: Figure shows Democrats' and Republicans' self coding of the word: Conservative and Liberal. Higher values mean more positive affect.

Measures of Affective Polarization are Highly Correlated

In this section, we use our measure of affective polarization using self-coded one-word evaluations with the measures of affective polarization created from three well-established measures: thermometer scores, candidate evaluations, and lifestyle measures.

Mirroring the main text, we create each measure of affective polarization using the same formula:

$$\textit{Affective Polarization} = \textit{In Party Evaluation} - \textit{Out Party Evaluation}$$

The formula for affective polarization uses the self-coded word and takes respondents' in-party affect evaluation and subtracts it from a respondents' out-party affect evaluation.

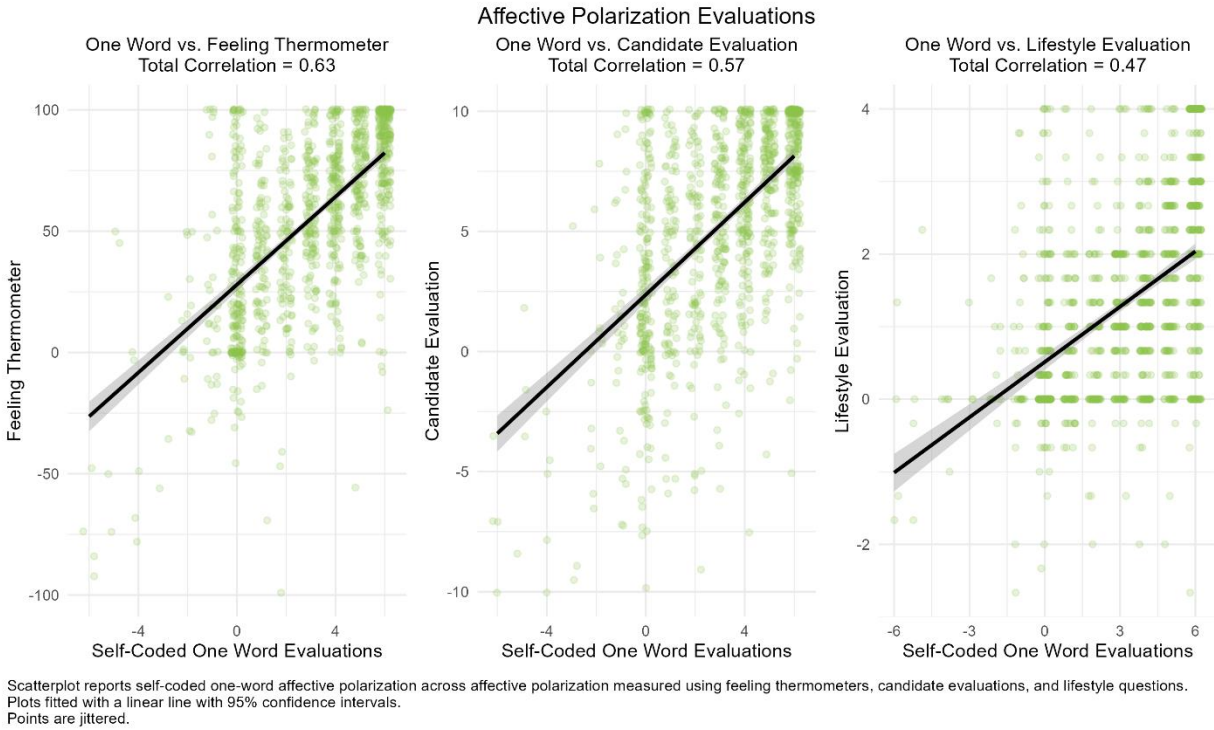
Our one-word self-coded measure of affective polarization ranges from ranging from -6 representing extreme in-party dislike to 6 representing extreme out-party dislike, with 0 indicating indifference between both parties.

The affective polarization measure using thermometer scores ranges from -100 representing extreme in-party dislike to 100 representing extreme out-party dislike, with 0 indicating indifference between both parties. The affective polarization measure using candidate evaluations ranges from -10 representing extreme dislike for the in-party's candidate to 10 representing extreme dislike for the out-party's candidate, with 0 indicating indifference between both candidates. The affective polarization measure using lifestyle evaluation ranges from -3 representing extreme aversion to in-party interactions to 4 representing extreme aversion to out-party interactions, with 0 indicating indifference between interacting with both parties.

Figure 2A reports that when we compare our one-word self-coded measure of affective polarization with existing measures of affective polarization, the relationship between the measures is positive and moderately strong. The correlations report a strong relationship between one-word affective polarization and thermometer affective polarization ($r = 0.63$), candidate affective polarization ($r = 0.57$), and lifestyle affective polarization ($r = 0.47$).

Together, these results show that our one-word measures of affective polarization are strongly related, they are not perfectly related. The conclusions we can draw from this relationship is the same as the conclusions we can draw from our comparison between affect in the main text: the residuals emphasize a different dimension of affective polarization that the one-word measures detect that the other measures do not. As a result, this makes them unique and contribution to affective polarization literature.

Figure 2A: Scatterplot of Self-Coded One Word Evaluations Against Existing Affective Polarization Measures

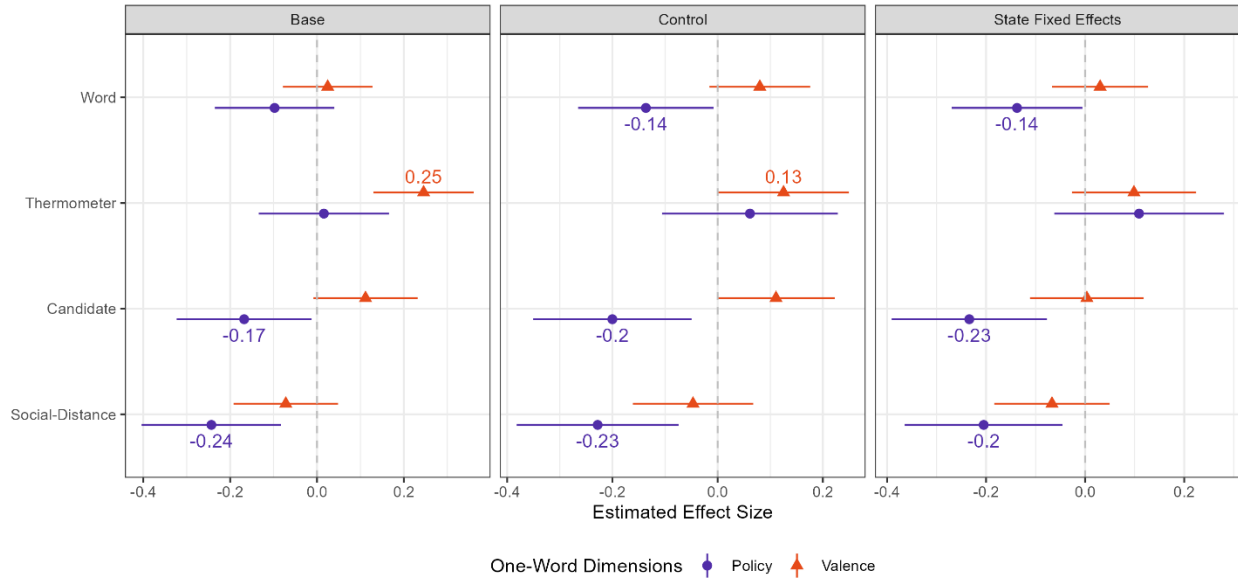


Affective Word Dimensions Using In-Party Specification

In the main text of the paper, we examine the policy and valence dimensions using the respondents' out-party word. Here, we keep the model specifications the same and re-run the analysis from the main text using policy and valence dimensions using the respondents' out-party word. We find virtually no meaningful relationship between policy and valence dimension using in-party words and affective polarization. The results show that character evaluations of the in-party are not a meaningful predictor of affective polarization while policy evaluations of the in-party predict lower levels of affective polarization using candidate evaluations, social-distance measures, and more complex models of using one- word answers compared to non-policy evaluations. These results suggest that affective polarization is mostly drive by character evaluations of the out-party not character evaluations of the in-party.

Figure 3A: Estimates for the Effect of In-Group Policy and Valence Dimension on Affective Polarization

Effect of In-Group Policy and Valence One-Word Dimension on the Predicting Affective Polarization
 Across Alternative Measures of Affective Polarization



Note: Point estimates are dots with lines indicating 95 percent confidence intervals.
 Model: Linear Regression.
 Affective Polarization measures are standardized.
 We omit the coefficient representing word evaluations because the scales are not comparable,
 but the coefficient is significant, positive, and outperforms both the valence and policy dimensions in each model.

Work Cited

Aronow, Peter Michael, Joshua Kalla, Lilla Orr, and John Ternovski. 2020. *Evidence of Rising Rates of Inattentiveness on Lucid in 2020*. SocArXiv. preprint. <https://osf.io/8sbe4> (November 11, 2021).

Supplement Material for Chapter Two

Table 1B: AMCE For Spouse Condition

	Dependent Variable:
	Δ Predicted Probability of Selection
Partisanship: Strong In Party	
Partisanship: Lean In Party	0.009 (0.017)
Partisanship: Apolitical	-0.013 (0.018)
Partisanship: Lean Out Party	-0.056*** (0.017)
Partisanship: Strong Out Party	-0.068*** (0.017)
Attractiveness: Below Average	
Attractiveness: Average	0.031** (0.013)
Attractiveness: Above Average	0.016 (0.013)
Income: 32 K	
Income: 54 K	-0.021 (0.015)
Income: 96 K	0.002 (0.016)
Income: 210 K	0.014 (0.016)
Race: Different Race	
Race: Same Race	0.017 (0.011)
Religion: Different Religion	
Religion: Same Religion	0.044** (0.019)
Number of Respondents	1073
Number of Observations	8584

Note:

*p<0.1; **p<0.05; ***p<0.01

Blank AMCE indicates baseline levels.

Table 2B: AMCE For Neighborhood Condition

	Dependent Variable:
	Δ Predicted Probability of Selection
Partisanship: Strong In Party	
Partisanship: Lean In Party	-0.020 (0.017)
Partisanship: Split	-0.037** (0.016)
Partisanship: Lean Out Party	-0.034** (0.017)
Partisanship: Strong Out Party	-0.066*** (0.017)
Commute: 10 min	
Commute: 25 min	0.004 (0.015)
Commute: 45 min	0.011 (0.015)
Commute: 75 min	-0.004 (0.015)
Monthly Housing Cost: 40 of Income	
Monthly Housing Cost: 60 of Income	-0.019 (0.013)
Monthly Housing Cost: 80 of Income	-0.036*** (0.013)
Race: 10 White 90 Nonwhite	
Race: 25 White 75 Nonwhite	0.006 (0.017)
Race: 50 White 50 Nonwhite	0.054*** (0.017)
Race: 75 White 25 Nonwhite	0.033* (0.017)
Race: 90 White 10 Nonwhite	0.055*** (0.017)
School Quality: Poor	
School Quality: Average	0.049*** (0.015)
School Quality: Good	0.042*** (0.015)
School Quality: Excellent	0.053*** (0.015)
Violent Crime: 20 < National Average	
Violent Crime: Same As National Average	-0.009 (0.013)
Violent Crime: 20 > National Average	-0.017 (0.013)
Number of Respondents	1073
Number of Observations	8584

Note:

*p<0.1; **p<0.05; ***p<0.01

Blank AMCE indicates baseline levels.

Table 3B: AMCE For Business Condition

	Dependent Variable:
	Δ Predicted Probability of Selection
Partisanship: Strong In Party	
Partisanship: Lean In Party	-0.020 (0.017)
Partisanship: Split	-0.027 (0.017)
Partisanship: Lean Out Party	-0.060*** (0.017)
Partisanship: Strong Out Party	-0.055*** (0.017)
Activism: None	
Activism: Blue Lives Matter	0.021 (0.017)
Activism: Black Lives Matter	-0.036** (0.017)
Activism: Against Gay Marriage	-0.035** (0.017)
Activism: For Gay Marriage	-0.025 (0.017)
Prices: 20% Below Average	
Prices: Average	-0.004 (0.013)
Prices: 20% Above Average	-0.042*** (0.013)
Travel Distance: 5 Min	
Travel Distance: 10 Min	-0.020 (0.013)
Travel Distance: 20 Min	-0.030** (0.013)
Yelp Rating: 2 Stars	
Yelp Rating: 3 Stars	0.008 (0.015)
Yelp Rating: 4 Stars	0.027* (0.015)
Yelp Rating: 5 Stars	0.028* (0.015)
Number of Respondents	1073
Number of Observations	8584

Note:

*p<0.1; **p<0.05; ***p<0.01

Blank AMCE indicates baseline levels.

Supplement Material for Chapter Three

Table 1C: AMCEs of Religious Respondents with Binary Conjoint Religious Categories

Attribute Levels	Estimate	P Value
Age:		
(Baseline = 46)		
53	-0.007	0.501
67	-0.018	0.077
74	-0.029**	0.004
82	-0.046***	0.000
Political Experience:		
(Baseline = Mayor)		
District Attorney	0.010	0.291
No political experience	-0.020*	0.034
State Representative	-0.004	0.685
Prior Occupation:		
(Baseline = Lawyer)		
Business Owner	0.020*	0.031
Car Dealer	-0.006	0.516
Doctor	0.006	0.542
Race:		
(Baseline = White)		
Asian	0.006	0.524
Black	0.007	0.392
Hispanic	0.006	0.486
Religion:		
(Baseline = Religious)		
Nonreligious	0.030***	0.000

Note: N = 1964. Reported effects can only be interpreted vs the baseline value, not against each other. *p<0.1; **p<0.05; ***p<0.01

Table 2C: AMCEs of Religious Respondents with Conjoint Religious Categories Combined

Attribute Levels	Estimate	P Value
Age:		
(Baseline = 46)		
53	-0.007	0.500
67	-0.018	0.076
74	-0.029**	0.004
82	-0.046***	0.000
Political Experience:		
(Baseline = Mayor)		
District Attorney	0.010	0.291
No political experience	-0.020*	0.034
State Representative	-0.004	0.683
Prior Occupation:		
(Baseline = Lawyer)		
Business Owner	0.020*	0.031
Car Dealer	-0.006	0.508
Doctor	0.006	0.543
Race:		
(Baseline = White)		
Asian	-0.006	0.533
Black	0.008	0.385
Hispanic	0.007	0.470
Religion:		
(Baseline = Religious)		
Agnostic	-0.023*	0.012
Atheist	-0.042***	0.000
None	-0.026**	0.005

Note: N = 1964. Reported effects can only be interpreted vs the baseline value, not against each other. *p<0.1; **p<0.05; ***p<0.01

Table 3C: AMCEs of Nonreligious Respondents with Binary Conjoint Religious Categories

Attribute Levels	Estimate	P Value
Age:		
(Baseline = 46)		
53	-0.009	0.570
67	-0.009	0.579
74	-0.011	0.504
82	-0.046**	0.005
Political Experience:		
(Baseline = Mayor)		
District Attorney	0.000	0.983
No political experience	-0.022	0.127
State Representative	-0.005	0.698
Prior Occupation:		
(Baseline = Lawyer)		
Business Owner	0.003	0.824
Car Dealer	-0.001	0.925
Doctor	0.001	0.798
Race:		
(Baseline = White)		
Asian	-0.016	0.253
Black	0.004	0.801
Hispanic	0.008	0.578
Religion:		
(Baseline = Religious)		
Nonreligious	0.011	0.287

Note: N = 800. Reported effects can only be interpreted vs the baseline value, not against each other. *p<0.1; **p<0.05; ***p<0.01

Table 4C: AMCEs of Nonreligious Respondents with Conjoint Religious Categories Combined

Attribute Levels	Estimate	P Value
Age:		
(Baseline = 46)		
53	-0.009	0.589
67	-0.009	0.578
74	-0.011	0.519
82	-0.046**	0.005
Political Experience:		
(Baseline = Mayor)		
District Attorney	0.000	0.986
No political experience	-0.022	0.130
State Representative	-0.005	0.700
Prior Occupation:		
(Baseline = Lawyer)		
Business Owner	-0.004	0.812
Car Dealer	0.001	0.930
Doctor	0.003	0.853
Race:		
(Baseline = White)		
Asian	-0.016	0.255
Black	0.004	0.788
Hispanic	0.008	0.566
Religion:		
(Baseline = Religious)		
Agnostic	-0.000	0.982
Atheist	-0.032*	0.024
None	-0.000	0.984

Note: N = 800. Reported effects can only be interpreted vs the baseline value, not against each other. *p<0.1; **p<0.05; ***p<0.01