

THE JOKE'S ON YOU: THE EFFECTS OF DISPARAGING POLITICAL HUMOR ON  
YOUNG CITIZENS' ATTITUDES AND BEHAVIORS

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A dissertation submitted to the faculty of the University of North Carolina at Chapel Hill in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the School of Journalism and Mass Communication.

Chapel Hill  
2010

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## **ABSTRACT**

**JASON MOLDOFF: The Joke's on You: The Effects of Disparaging Political Humor on Young Citizens' Attitudes and Behaviors**  
(Under the direction of Anne Johnston)

Two experiments were run to test whether young voters were susceptible to stereotype threat effects under a variety of conditions. Results for Experiment 1 indicate that making the age of young citizens salient before taking a political knowledge test did not affect their sense of political information efficacy or performance. Making the diagnostic nature of the test salient did result in a significant decrease in performance, but this difference was eliminated when the age salience manipulation was also included. Results for Experiment 2 revealed a significant 2-way interaction between the expectation of humor, exposure to disparaging humor, and performance on the political knowledge test. Participants performed significantly worse on the political knowledge test when they were unexpectedly exposed to humorous as compared to non-humorous disparagement. Participants made to expect humor performed significantly worse on the political knowledge test when they did not receive humor than when they did receive humor. Implications for the study of political humor are discussed and opportunities for future research are detailed.

## ACKNOWLEDGEMENTS

I grew up surrounded by humor. Whereas some children ask their fathers to read them the same bedtime stories night after night, I cherished memorizing the jokes and stories my father told, and just as importantly, the way he told them. I copied his mannerisms, the way he set up his jokes, his pacing, and his vocal tones, all of which were key to a successful delivery. Success in our family was making my mother give a high-pitched squeal while she laughed, “I’m going to pee in my pants!” Mom was the barometer of what was funny and what was an appropriate target for parody or ridicule. As part of our cultural upbringing, my parents introduced my brother and I to Borscht Belt comedians like Mel Brooks. While I didn’t realize it then, this style of humor taught us a valuable lesson: that comedy could be used as a social corrective, to put down the abuser (e.g. Brooks’ constant mockery of the Nazis) without taking up arms. Together, my parents raised two boys with strong values and quick wits.

I am fortunate that humor continues to be an important part of my life, in my scholarship, and in my relationships. To be able to study what you love is a precious gift for which I am grateful. This project in particular benefited from the generosity of my doctoral committee, led by my chair, Anne Johnston. Anne, thank you for sticking with me these past three years: keeping me grounded and focused, and challenging me to express myself clearly on paper. To Sri Kalyanaraman, your class on experimental design was undoubtedly the most intense and worthwhile experience I had in the Journalism School. Learning to improve and

defend my research has made me a better scholar. Rhonda Gibson, thank you for your support from Day 1 in so many ways and for being a fantastic editor. Keith Payne, the theoretical justification for this research stems from the work I did in your Social Cognition class. I am so proud to have you on my committee and thank you for helping make this project successful. Andrew Perrin, thank you for taking a chance to serve on my committee. I truly believe that this project and my future research will be improved because of the work I did for your comprehensive exam question.

My thanks to Tab and Jon at Odum for their statistical assistance and to Scott Dunn for being my mentor here and at Virginia Tech. Thank you to Cindy Anderson for your masterful command of the graduate program. Thank you to my students in JOMC 101 and 240 for two enlightening and fulfilling semesters.

Finally, I am so blessed to have someone in my life that shares my love for learning and laughter. To my wife, Talya: you are my social calendar, my therapist, and my tour guide. Thank you for joining me in the foreign lands of western Virginia (Hokies?) and North Carolina (Grits?) as we pursued our graduate degrees together. I don't know where we'll end up next, but wherever it is, it'll be home, and it'll be funny.

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## CHAPTER I

### INTRODUCTION AND LITERATURE REVIEW

Every once in a while, a news story appears that includes the results of a survey showing some startling findings about the lack of political knowledge among Americans in general, and young Americans in particular. For instance, several media outlets picked up on a Pew Research report released in 2007 that found only 61% of Americans age 18-29 could name the vice-president (Pew, 2007), while in the lead-up to the invasion of Iraq in 2003, a widely cited National Geographic-Roper survey reported that only 13% of young people could locate Iraq on a map (National Geographic, 2002).

News coverage of young voters as poorly informed continued through the 2008 presidential campaign. In an opinion piece for *The Tundra Drums*, an Alaskan newspaper, Richard Brake reported that a political knowledge survey found, “our future voters and leaders are graduating with little knowledge about how our system works and how it has performed over time” (Brake, 2008, para. 13). Among the statistics cited, Brake noted that less than half the college-age respondents knew the meaning of federalism, while “fewer than 40 percent knew the basics of monetary and fiscal policy.” (Brake, 2008, para. 14). As part of a segment on the television show *20/20*, host John Stossel conducted an informal survey and came to the conclusion that “many of the young voters didn’t seem very informed,” leading him to propose that perhaps uninformed voters should stay away from the polls on Election Day (Stossel, 2008).

In addition to news and editorial coverage, the stereotype of young voters as politically uninformed figures prominently in the entertainment media. Americans seems fascinated with the seeming idiocy of their countrypersons. There are popular television programs and late-night segments dedicated to fulfilling our desire to poke fun at the not-so-bright side of all of us (e.g., *Are You Smarter than a Fifth Grader*, Jaywalking segment on *The Tonight Show with Jay Leno* and *The Jay Leno Show*). One of the most popular outlets for political information amongst young people, *The Daily Show with Jon Stewart*, has repeatedly mocked young people for their lack of knowledge and engagement.

For instance, on February 28th, 2008, in a segment called “Trendspotting-Youthquake,” Demetri Martin interviewed Itay Hod from CBS News about the tactics political candidates need to use to reach young voters. Hod says that as opposed to speaking logically and rationally to young voters about issues, “the more graphics you have on the screen, the more appealing it is to kids” (Stewart, 2008). Additionally, candidates need to “learn how to speak their language,” which according to Hod includes saying “What’s up?” instead of “Hey, you’ve got to vote for Obama” (Stewart, 2008). The scene then shifts to a group of “correspondents” as they relentlessly and fruitlessly ask young people at a bar about their political opinions (Stewart, 2008).

It seems commonplace now for news and entertainment programming to address or poke fun at young voters’ low levels of political knowledge. While there are a few studies on the effects of late-night humor about politicians on young voters’ attitudes (e.g., Baumgartner, 2007; Baumgartner & Morris, 2006; 2008; Young, 2004; 2006), there is little research to date examining the effects of political humor about young voters on young voters. Articles written by young people during the election indicate an awareness of the way their

age group is portrayed in the press and perceived by the public. In an opinion piece to the *Raleigh News and Observer*, Ph.D. student Justin Martin noted, “Oh, how common are lamentations about young Americans’ lack of political knowledge. Young people in this country, we often hear, are abysmally uninformed and would rather punch messages on i-Phones, listen to MP3s or hit bunker shots on a Wii than consume political news” (Martin, 2008, para. 1). Richard Wood, a columnist for the University of South Carolina’s paper *The Daily Gamecock*, wrote, “What good is it to encourage young people to vote if they don’t know much about the candidates or their positions? Or what if they lack an even more basic knowledge of how American government works?” (Wood, 2008, para. 3). While these anecdotes are interesting, little systematic evidence exists to determine if or how negative portrayals of young voters in humorous and non-humorous outlets affect young voters. This research seeks to fill that void in the literature.

For political scientists and scholars of political communication, the representation of voters as politically inept has serious implications. Delli Carpini (1999, p. 6) argued that the continued recitation of the notion of an ill-informed public has detrimental effects:

Books such as Allan Bloom’s *The Closing of the American Mind* (1987), Diane Ravitch’s and Chester Finn’s *What Do Our 17-Year-Olds Know?* (1987), and E. D. Hirsch’s *Cultural Literacy* (1988) have also contributed to our negative image of the American public.<sup>1</sup> Indeed, D. Charles Whitney and Ellen Wartella conclude that a ‘virtual cottage industry has arisen in the past few years in making out the American public as a bunch of ignoramuses’ (1989, p. 99). This characterization is so well-established that, according to John Ferejohn, ‘Nothing strikes the student of public opinion and democracy more forcefully than the paucity of information most people possess about politics’ (Ferejohn, 1990, p. 3).

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<sup>1</sup> Three more recent examples of these books are Richard Shenkman’s *Just How Stupid Are We?* (2008), Mark Bauerlein’s *The Dumbest Generation* (2008), and Susan Jacoby’s *The Age of American Unreason* (2008).

As a review of the literature will attest, many scholars are particularly concerned about the effects of political humor on young voters, as this generation increasingly turns to late-night comedy for political information. In a national survey conducted by the Pew Internet and Life Project (Pew, 2004), 44% of young people ages 18-29 reported at least sometimes if not regularly learning about the presidential campaign and the candidates from late-night shows such as *The Late Show with David Letterman* or *The Tonight Show with Jay Leno*, while 50% of this age demographic reported the same for comedy shows such as *The Daily Show with Jon Stewart* or *Saturday Night Live*. As young voters are exposed to disparaging humor about their age group, they may come to learn how little is expected of them. The question is whether exposure to disparaging political humor has different effects on young voters' attitudes and behaviors than exposure to non-humorous disparagement or whether either has any effect at all.

This research tested whether exposure to nearly identical disparaging messages told in a humorous or non-humorous manner had different effects on young voters' political attitudes and performance on a political knowledge test, using the growing psychological literature on stereotype threat (Steele & Aronson, 1995) as its theoretical foundation. This theory states that when presented with threatening information about the stereotypical inability of one's group to perform a skill, one tends to perform in accordance with that stereotype to a greater degree than one would without exposure to the threat. While young people are disparaged for performing poorly on political knowledge tests, this research sought to determine if exposure to such disparagement actually causes poor performance. Young voters' level of political knowledge is an important object of study because it is

strongly correlated with positive civic outcomes, including voting (Delli Carpini & Keeter, 1996).

One of the reasons why performance may decrease upon exposure to disparaging messages may be due to decreased self-esteem. There is some research to suggest that one of the reasons young people do not vote in greater numbers is that they lack political information efficacy, or the feeling that they know enough to participate in politics. Using national survey data from 2000 and 2002, Kaid, McKinney, and Tedesco (2007) found that confidence in one's level of political knowledge accounted for a small but significant amount of the variance (between 6% and 10%) in determining whether a young voter would turn up at the polls. Therefore, this research examined the effects of exposure to disparaging messages on political information efficacy.

Although young voters are disparaged in humorous and non-humorous outlets for their often unreliable, uninspired and uninformed civic behavior, little evidence exists that this disparagement causes apathetic attitudes or inhibits civic participation. To understand why these portrayals could affect young voters, one first needs to understand the literature on the effects of political humor. Second, to test whether messages about young voters' low levels of political knowledge affect their attitudes and behaviors, one must review the literature on stereotype threat.

### **Political Humor**

Throughout recorded history, the effects and functions of humor have been the subject of great debate. As young voters turn to late-night shows for political information, scholars continue to study the effects of political humor. How large or negative an influence is exposure to political humor on democracy in general or on young voters in particular?

Plato (Philebus 48-50, cited in Morreall, 1987, p. 11) argued that laughter and humor provide us with feelings of superiority that rational humans should not experience. In laughing at politicians' mistakes or at the foibles of the press, we erroneously imagine ourselves to be better persons. The superiority theory of humor, as it is referred to in the literature, gained prominence in the work of Thomas Hobbes. In summary of his treatise, he wrote, "the passion of laughter is nothing else but the sudden glory arising from some sudden conception of some eminency in ourselves, by comparison with the infirmity of others, or with our own formerly..." (Hobbes, 1840, summarized in Morreall, 1987, p. 19). One of the main criticisms of contemporary political humor is that the targets of ridicule are often elected officials or government institutions, and exposure to this type of humor may create a cynical, distrustful, or perhaps apathetic citizenry. Hart and Hartelius (2007) argue that Jon Stewart in particular makes being cynical popular. Through mockery of the political system, Stewart invites viewers to unite behind his anti-political diatribes. As a unified group of cynics, his audience holds an imagined sense of superiority over the political sphere they mock. It is quite plausible that people who watch late-night political humor shows enjoy the camaraderie and feelings of superiority associated with the constant disparagement of the political process at the expense of more positive civic participation.

There is scant empirical data about the effects of political humor on political attitudes. The studies that do exist often employ surveys, thus leaving open the issue of causality. In an exploration of political humor on the Internet, Baumgartner (2007) had survey participants at two universities answer questions about their use of Internet humor sites (e.g., jibjab.com) and their attitudes about politics and government. He found that participants who reported visiting these sites regularly were more likely to express feelings of political distrust.



Whether exposure to political humor caused these feelings could not be determined through use of a survey.

There are some experiments that purport to show negative effects of political humor on citizens' attitudes. Baumgartner and Morris (2006) randomly assigned participants to watch either eight minutes of edited jokes or news about President George W. Bush and John Kerry or no video (control group). The authors found that exposure to jokes about presidential candidates made participants more cynical, and paradoxically, more efficacious about politics. The weakness of this design is that the degree of equivalence between the two stimuli is questionable. It is unclear what about the humorous material affected the participants. Among the potential factors are the messages themselves, the presence of audience laughter, and the expectation of humor that comes with attention to late-night shows. Baumgartner and Morris (2006) note that the degree of equivalence is merely "adequate" (p. 348).

Similarly, Baumgartner and Morris (2008) had participants answer questions about their attitudes towards both political parties after watching a series of clips of either *The Colbert Report (TCR)*, the show it was fashioned after, *The O'Reilly Factor*, or watching no clips (control group). While the subject of the clips was similar between the two programs, the language, delivery and technical features of the clips contained numerous differences. Still, noting a significantly higher level of self-reported trust in Republicans after viewing a series of clips from *TCR* as opposed to the no video control group, the authors surmised that, "Exposure to *TCR* is positively associated with one's tendency to agree that they trust the Republicans in Congress to do the right thing" (p. 632). As should be evident, it is not necessarily the show itself that causes these attitudes but the types and topics of humor

present on late-night shows, along with variables such as audience laughter, visual cues, and humor expectancy.

In addition to creating a culture of cynicism, critics of humor note that disparaging jokes often rely on stereotypes, and thus contribute to misunderstandings and social division (Berger, 1993). Janes and Olson (2000) provide a general definition of disparagement humor as “any humor that derogates or provides negative information about someone or something” (p. 474). Ferguson and Ford (2008) define disparaging humor as, “remarks that (are intended to) elicit amusement through the denigration, derogation, or belittlement of a given target (e.g., individuals, social groups, political ideologies, material possessions) (pp. 283-284). The comic conception of groups based on exaggeration and caricature is theorized to likely continue to foster the “isms” of race, class, gender, age and American isolation (Ross & York, 2007). If this is correct, then exposure to disparaging humor about their age group may influence young citizens’ self-perception, though prior to the present research little empirical evidence exists to support this idea.

Most of the research in this area examines the effects of disparaging humor about others on one’s attitude towards the group being disparaged. For instance, Olson, Maio and Hobden (1999) found virtually no relationship between exposure to disparaging humor and the accessibility or extremity of stereotypical beliefs as compared to exposure to neutral humor or non-humorous disparagement, although the targets of the humor in their experiments were socially accepted as powerful, men and lawyers. However, Ford and Ferguson (2004) found that exposure to disparaging humor resulted in an increased acceptance of discrimination, particularly for people already prejudiced in some way.

Contemporary critics also argue that in addition to creating cynical citizens, laughing about society's problems may release the energy needed to motivate people to actually solve them (Purdie, 1993). According to the relief or catharsis theory of humor, most commonly attributed to the work of Sigmund Freud (1905/1960), humor serves as a means of expressing hostile feelings in a socially acceptable manner. Laughter and smiling, the physical manifestations of the enjoyment of humor, are physiological responses to the dissipation of psychological tension. It is possible that following these cathartic responses to humor, people are less inclined to give serious thought to the substance of the message behind the jokes. Therefore, laughing at disparaging humor about oneself or a group to which one belongs might diminish the recipient's motivation to take any action to correct whatever fault was highlighted in the joke.

However, data indicate that rather than giving up on politics, citizens exposed to late-night political humor are likely to subsequently seek out more traditional news coverage. Feldman and Young (2008) examined a series of cross sectional surveys from the National Annenberg Election Survey (NAES) to assess the relationship between late-night viewership and traditional news viewership. After controlling for demographic variables, the researchers found increased attention to late-night humor (*Leno*, *Letterman*, or *The Daily Show*) was positively associated with a subsequent increase in attention to traditional campaign coverage. Young and Tisinger (2006) and Cao and Brewer (2008) reported similar findings using Pew and NAES data, respectively.

In an experiment, Xenos and Becker (2009) tested whether exposure to humorous information about an issue resulted in greater information-seeking behavior or learning than exposure to a "more serious" version of that information. Subjects watched a 5-minute clip

constructed by the experimenters from nightly newscasts about the troop surge in Iraq, and/or a 5-minute clip from *The Daily Show* that “featured much of the same footage” as the news broadcasts (Xenos & Becker, 2009, p. 321). Participants were then given the opportunity to use a web browser to read stories about Iraq and foreign policy, domestic issues, sports, or entertainment after watching the clip. Participants who watched the comedy clip tended to spend more time reading stories about Iraq and foreign policy than the other groups, leading the authors to give moderate support to the idea that humor can act as a gateway to greater information seeking. However, once again the degree of equivalence between the two sets of stimuli is questionable.

Not all philosophers and scholars see humor as detrimental to democracy and discourse (For a review of philosopher’s thoughts on humor throughout the 18<sup>th</sup> and 19<sup>th</sup> centuries, see Morreall, 1987). For example, Kant (1892/1987) argued that rather than attending to humor to feel relieved of the need to act or to feel good about oneself in relation to others, people are often amused and prone to laughter when contrary or seemingly incompatible ideas are juxtaposed. The incongruity theory of humor posits that laughter results from a violation of expectations, which may be manifest in surprise, in wordplay, or in creative juxtaposition. Contemporary proponents of political humor argue that in attending to comedic messages, people are engaging in creative and critical thinking, exploring new ways of understanding the world, and may, as a result, find themselves motivated to learn more or engage with the issue involved (Bennett, 2007; Hariman, 2007; 2008). For instance, when Jon Stewart plays a series of edited video clips of politicians making contradictory statements side by side, the audience may learn while it laughs. While the lesson learned from exposure to such a series of clips might influence one’s opinions of a particular

politician in a negative manner, the act of learning from political humor can be a positive democratic outcome.

The idea that exposure to political humor can lead to positive outcomes such as increased political knowledge has received some attention in the form of survey research. In 2004, the Annenberg Public Policy Center found that survey respondents who indicated watching *The Daily Show* in the week prior answered an average of 3.59 out of six political knowledge questions correctly, as compared to 2.62 for people who watched no late-night shows that week, and 2.91 and 2.95 for those who reported watching Letterman and Leno, respectively (NAES, 2004). Similarly, a 2007 report from Pew found that 54% of regular *Daily Show* viewers scored in the high knowledge group (answering at least 15 out of 23 political knowledge questions correctly), as compared with 35% of the general population, 38% of regular network news viewers, and 43% of regularly daily newspaper readers (Pew, 2007). Using Pew data, Cao (2008) found that young people who regularly watched late-night shows scored modestly higher on the political knowledge questions than non-regular viewers. However, the four “political knowledge” questions pulled from the Pew data dealt with candidate familiarity more than knowledge per se. Also using Pew data, Hollander (2005) found that viewing late-night programming contributed a small degree to recognition but not recall of campaign information. It is unclear from these survey data the degree to which watching late-night humor caused an increase in political knowledge or whether those who scored higher on such tests tended to watch these shows.

There are other reasons to believe that humor can be a powerful force for good. In bringing to light the incongruities in politics, humor can show us the way life ought to be.

Nachman (2004) details the careers of comedians such as Mort Sahl and Lenny Bruce, who challenged the status quo in the 1950s and 1960s with their aggressive brand of humor. Satirists maintain a critical distance from the newsmakers often too familiar with ways of manipulating the traditional press (Baym, 2005, p. 265). In the words of author Murray Davis, "Satire, in short, focuses on social units that fall short of their ideal" (Davis, 1993, p. 219). In Peterson's (2008) opinion, satire can "raise awareness," "function as democracy's feedback loop," and "sound the alarm" about problems with its targets, be they the government, private industry, the press, or members of the public (p. 19). Similarly, Gray, Jones, and Thompson (2009) argue that satire is "provocative" and "empowering" (p. 13). Furthermore, while not everyone is interested in understanding the finer points of political arguments, many people can enjoy and engage in political humor, thus broadening the public sphere to include voices and audiences that otherwise would be disenfranchised (Hariman, 2008).

Contemporary scholars of political communication and political science remain divided over whether exposure to political humor benefits or harms positive democratic outcomes such as level of political knowledge, political attitudes, or voting intention. According to the relief theory, people expend their energy being entertained instead of being active in the expression of hostility through laughter, while the superiority theory predicts that humor is a likely cause of citizen apathy and disdain for politics and a source of social divisions. On the other hand, the incongruity theory posits that exposure to political humor can increase political knowledge.

There is some evidence from surveys and experiments that exposure to political humor is associated with and perhaps increases both political knowledge and political

information-seeking behaviors. There is scant yet mixed evidence about the effects of exposure to political humor on attitudes towards politics and one's own ability to affect change.

The majority of the research on the subject is based on survey data, leaving open the question of whether exposure to political humor affects political knowledge or political attitudes or whether these variables influence people's interest in and exposure to political humor. Additionally, the stimuli used to assess the effects of political humor in experimental research tend to have numerous differences in their humorous vs. non-humorous stimuli, making comparisons between groups suspect.

Next, studies about the effects of political humor fail to take into account the idea that people tend to approach humor with a different mindset. In other words, when people tune in to political humor, they are more likely to expect to be entertained. Wouldn't the effect of humorous messages about young citizens or politicians be different if the humorous intent of the speaker was cued in advance? If a humorous mindset leads to dismissal of disparaging messages, then negative portrayals of groups on late-night humor programs may not be as harmful as originally thought.

### **Humor Expectancy**

Literature in psychology suggests that expectations can influence the way information is interpreted and even unconsciously taken in through the senses (e.g., Balcetis & Dunning, 2006). For instance, according to the Affective Expectation Model (Wilson, Lisle, Kraft & Wetzel, 1989), our beliefs about how much we will like or dislike an impending event are key determinants of how we experience and interpret the event. Unless a discrepancy

between expectation and reality is noticed, people will assimilate their attitudes in line with their expectations. (Geers & Lassiter, 2005).

Similarly, it is thought that people engage in different types of information processing when cues about the impending presence of humor are evident. Research indicates that people may disregard a message that is unclear or incongruous when made to interpret that information in a playful way (McGhee, 1972). Likewise, any perceived threat can be interpreted playfully and dismissed when the humorous mindset is activated (Zillmann, 1983). Research by Pexman and colleagues (Katz & Pexman, 1997; Pexman & Olineck, 2002) found that information was more likely to be interpreted ironically if presented from a humorous source. Similarly, Nabi, Moyer Guse, and Byrne (2007) found that messages about social issues purportedly from comedian Chris Rock were judged to be funnier than the same messages from an anonymous source. Increased perception of humorousness resulted in reduced counterarguing, deeper processing and increased message discounting as reported in post-test measures.

This research adds to the literature on the effects of humor expectancy. Cues that forthcoming information will be humorous will affect how information is processed in at least three measurable ways. First, the expectation of humor will result in an increased desire to “get the joke,” thus leading people to give the information that follows greater attention. Second, people expect humor to be “based on truth,” and thus will tend to accept the message’s underlying meaning with less counterarguing than they would without the expectation of humor. Third, people made to expect humor will be more likely to dismiss threatening information as “just a joke.” Concerns about the negative effects of political humor, in this case disparaging humor about young people, may be supported if people are



shown to give greater attention and credence to information that they believed was going to be funny, and negated if it is shown that the expectation of humor reduces or eliminates the impact of the message on political attitudes and behaviors through message dismissal.

Finally, little research to date has assessed the effects of humor about young voters on young voters. It seems plausible that the type of humor most likely to affect young voters' political attitudes and voting intentions would be that which addresses young voters' ability to function as proper citizens. As newcomers to the world of political participation, young voters may be particularly influenced by negative humor about their age group. Hertlzer (1970) wrote that, "When newcomers are in the process of assimilation in a new community of society, the individuals try to avoid being laughed at because of their ignorance or clumsiness" (pp. 162-63).

### **Stereotype Threat**

One way to test whether messages about a group of people affect members of that group is through a theory called stereotype threat. Stereotype threat theory (Steele, 1997; Steele & Aronson, 1995) posits that when one's membership in a negatively stereotyped group is made salient, one tends to inadvertently confirm the stereotype. Stereotype threat theory builds off research in social identity theory (Tajfel & Turner, 1986), which predicts that when a group to which one belongs is threatened by comparison with a superior group, one actively works to minimize the negative and restore positive in-group distinctiveness. Stereotype threat theory states that this added motivation and stress actually inhibit performance. Acknowledgement of the threat to one's identity tends to result in a self-fulfilling prophecy. Self-fulfilling prophecies occur when "people hold expectancies that lead them to alter their behavior which in turn causes the expected behaviors to be exhibited by

people who are targets of the expectancies” (Hilton & von Hippel, 1996, p. 244). In other words, people behave in accordance with the salient expectations of how they will behave.

The most frequent subjects of stereotype threat research are women and minorities. There exists an “achievement gap” in applied settings between how these groups perform in comparison to men and white people, respectively. For instance, the initial study (Steele & Aronson, 1995) focused on the stereotype that African Americans do not perform as well on IQ tests as their white counterparts. Researchers are interested in identifying the causes of these gaps and any remedies for reducing them. The stereotype of young voters also has applied and as yet unexplored implications. This research will examine if young voters experience stereotype threat effects when exposed to political humor about their stereotypically low levels of political knowledge.

While there are scattered criticisms of the theory, particularly in the way results are reported in the press (e.g., Sackett, Hardison, & Cullen, 2004) and general real world applicability (e.g., Cullen, Hardison, & Sackett, 2004; Cullen, Waters, & Sackett, 2006), the majority of articles serve as extensions and refinements of the original study (Steele & Aronson, 1995). Sometimes journalists report that the gap between, say, African Americans and Caucasian Americans is eliminated in the absence of stereotype threat, or that the removal of stereotype threat would be a panacea for the stereotyped group. The research tends to more specifically indicate only that the stereotyped group performs better than it does in the absence of that threat. In other words, the gap between the performance of the stereotyped and the majority group often still exists in the absence of threat, though it may be reduced. Furthermore, scholars readily acknowledge that there are larger societal issues at play that cause these stereotypes to exist. Similarly, it is unclear whether or how frequently

people are primed about or asked to give information about their affiliation with a stereotyped group before taking tests such as the SAT, GRE, etc., leading skeptics to question the generalizability of the studies' findings. Even with these criticisms acknowledged, what Steele and Aronson (1995) found, and researchers in the fields of gender (e.g., Cadinu, Maass, Lombardo & Frigerio, 2006; Marx & Stapel, 2006; McGlone, Aronson & Kobrynowicz, 2006), race (e.g., Shih, Ambady, Richeson, & Fujita, 2002; Stone, Lynch, Sjomeling, & Darley, 1999), and age (senior citizens) (e.g., Abrams, Eller, & Bryant, 2006; Andreolleti & Lachman, 2004; Chasteen, Bhattacharyya, Horhota, Tam, & Hasher, 2005) have confirmed, is that stereotype threat can create a significant amount of variation in performance on measures where people feel threatened about what their performance will say about themselves and their group.

The vast majority of studies on stereotype threat examine the effects on performance on math tests. Among the other dependent variables used in stereotype threat research are scores on social sensitivity in men (Koenig & Eagly, 2005), political knowledge in women (McGlone et al., 2006), ability to balance (Chalabaev, Stone, Sarrazin, & Croizet, 2008) and performance at soccer (Chalabaev, Sarrazin, Stone, & Cury, 2008) and golf (Stone et al., 1999; Stone & McWhinnie, 2008).

The way threat is manipulated in stereotype threat research varies widely. For instance, Steele and Aronson (1995) had half of their African American participants select their race from a list before beginning their math test, thus subtly making that part of their identity more salient. Having participants select their age ("18-24/young voter") prior to taking part in a political knowledge test may similarly make that part of their identity more salient, resulting in a decrease in performance. Other ways of inducing feelings of threat

include having women take a test with a group of men present (Inzlicht & Ben Zeev, 2000), placing a derogatory cartoon about women's math skills in the room with women as they took a test (Oswald and Harvey, 2000/2001), and varying the race and gender of the experimenter (Marx & Goff, 2005; Stone & McWhinnie, 2008).

Another way of creating threat is by telling participants that the test they are about to take is a powerful and reliable diagnostic tool, one whose results will reveal something important about the test taker. By increasing the perceived diagnosticity of the test, theorists argue that stereotype threat effects are more likely to occur (Frantz, Cuddy, Burnett, Ray, & Hart, 2004; Kray, Thompson, & Galinsky, 2001; Marx, Stapel, & Muller, 2005; Steele & Aronson, 1995).

Although the results continue to be replicated, the precise mechanisms of the theory remain unclear. This review details several of the purported mediators and moderators of stereotype threat effects. Although calls for greater attention to the mediating factors in stereotype threat research are often made, studies employing mediational designs produce non-significant or even contradictory results (Smith, 2004). Generally, theorists take the position that task achievement is challenged when people are cognitively or affectively occupied. In their summary of the literature, Schmader, Johns, and Forbes (2008) note that whatever the mechanism (e.g., stress, self monitoring, counterarguing), the underlying phenomenon causing inhibited performance may be a reduction in working memory capacity.

One of the most frequently used mediators examined in stereotype threat research is anxiety. It would seem that the natural and most observable response to a threat would be anxiety. However, as frequently as it is used, results indicate a lack of consistent evidence that self-reported anxiety levels mediate stereotype threat effects (Aronson, Quinn, &

Spencer, 1998; Osborne, 2001; Spencer, Steele, & Quinn, 1999; Steele & Aronson, 1995; Stone et al., 1999). For instance, Aronson et al. (1998) found that although participants in a stereotype threat condition did perform worse on a math test than participants in a no threat condition, there was no difference in the level of state anxiety between conditions. However, Osborne (2001) found that controlling for state anxiety did reduce the differences in scores between black and white math test takers and also between male and female test takers.

Although not frequently used as a mediator in stereotype threat research, message counterarguing may cause performance detriments. In accordance with the working memory model, counterarguing increases cognitive load thus leading to decreased performance on a task requiring cognition (For research on cognitive load manipulation effects, see for example Gilbert & Hixon, 1991; Pontari & Schlenker, 2000).

Another mediator of interest is motivation/effort (Aronson et al., 1999; Beilock, Rydell, & McConnell, 2007; Jamieson & Harkins, 2007; Osborne, 2007; Schmader & Johns, 2003; Stone, 2002). On the one hand, decreased effort may be a sign of self-handicapping (Stone, 2002). People may give up on a task as a means of protecting themselves from the possibility of performing poorly on a task that is important to their identity. Stone (2002) found that participants who cared about sports practiced golf swings for an upcoming task for far less time when under stereotype threat.

Too much motivation can also inhibit performance, through what is commonly referred to as “choking” (Baumeister, 1984). Although it might seem that trying harder on a test would correlate with higher test scores, some researchers posit that too much pressure to act appropriately or diminish the feeling of threat can interfere with optimal performance, as when a basketball player misses clutch free throws.

The final mediator of interest to the present research is attitudes about the self, in this case political information efficacy. However, in prior research self-esteem was not found to mediate the relationship between stereotyping and performance detriments in elderly people (Levy & Langer, 1994) or between gender and performance on a math test (Heatherton & Polivy, 1991).

In addition to mediating variables, scholars are interested in understanding the individual differences that predict how people will react to stereotype threat. The moderators of most interest here are the related concepts of group and domain/topic identification, belief and awareness of the stereotype, and overall efficacy. Domain identification refers to the degree to which a person feels that the topic or issue being stereotyped is important to his or her sense of self. For instance, performance on a math test may be a more important domain for some people than for others. Some research indicates that stronger identification with a group or domain/topic being threatened will result in more dramatic effects (e.g., Aronson et al., 1999; Luhtanen & Crocker, 1992; Schmader, 2002; Smith & White, 2002). However, other research indicates that simply priming or exposing individuals to cues about groups with stereotypes can influence individuals' behavior in accordance with that stereotype (e.g., Ambady, Pail, Steele, Owen-Smith, & Mitchell, 2004; Bergeron, Block, & Echtenkamp, 2006; Levy, 1996). These studies find that one does not necessarily need to belong to or feel a close sense of affiliation with a stereotyped group or domain for effects to occur. In terms of belief and awareness of the stereotype, research does indicate that being "chronically self conscious" of the stereotype in question may moderate the strength of stereotype threat effects (Brown & Pinel, 2003; Pinel, 1999). Finally, one's overall sense of confidence, or

efficacy (Schwarzer & Jerusalem, 1995) may influence the way that stereotype threat effects influence a person's attitudes or behaviors.

As noted, the most frequently used dependent variable in stereotype threat research is performance on a math test. The present research focuses on political knowledge as its outcome variable of interest, though it is not the first to do so (McGlone et al., 2006). If stereotype threat is found to affect young voters' attitudes and performance on a political knowledge test, then interventions could be designed to prevent or reduce these effects. For instance, stereotype threat effects can be reduced by having people think about what makes them an individual, separate from the group (Ambady et al., 2004). McGlone et al. (2006) determined that having women focus on another part of their identity (e.g., where they were from) reduced the effects of stereotype threat. If political knowledge and attitudes are affected by negative portrayals of young voters, it is possible that encouraging newcomers to the political sphere to think of themselves in terms other than "young voters" may reduce stereotype effects.

Stereotype threat research is related to a larger stream of inquiry in psychology, wherein subjects' attitudes and behaviors are influenced by exposure to subliminal or supraliminal information. It is argued that information that precedes an object or behavior can "prime" the way one feels toward that object or conducts that behavior. The outcomes may change in accordance with the prime (e.g., performing better on a knowledge test after being primed with "professor"), or in opposition to the prime (e.g., performing worse on the test after being primed with "Einstein"). Attitudinal or behavioral assimilation is said to occur when people's attitudes or behaviors are found to be in closer alignment with the prime. On the other hand, when people's behavior is diametrically opposite from that

expected by exposure to a prime, contrast effects are said to have occurred (Dijksterhuis, Spears, Postmes, Stapel, Koomen, von Knippenberg, & Scheepers, 1998). Research indicates that exposure to trait primes or stereotypes (professor) often leads to assimilation effects (better performance, e.g., Bargh, Chen, & Burrows, 1996), whereas exposure to exemplar primes (Einstein) leads to contrast effects (worse performance, e.g., Dijksterhuis et al., 1998). The present research contributes to the literature on assimilation effects by testing whether general stereotypical information about young voters' stereotypical low levels of political knowledge affects their behavior in accordance with that stereotype.

### **Political Knowledge and Political Information Efficacy**

Virtually every journal article and book chapter dealing with political knowledge includes a statement about the importance of this concept as an indicator of a healthy democracy. For instance, Berelson, Lazarsfeld and McPhee (1954) argued, "The democratic citizen is expected to be well-informed about political affairs. He is supposed to know what the issues are, what their history is, what the relevant facts are, what alternatives are proposed, what the party stands for, what the likely consequences are" (p. 308). According to this early philosophy, it is imperative that as proper citizens, people be as well informed about as many issues as possible.

When we say that someone is politically knowledgeable, what do we mean? As Shenkman (2008) argues "If, say, half the respondents do not know that the Constitution was drafted in Philadelphia, as happens to be the case, does that entitle one to conclude that The People are stupid? Or is a higher percentage required –say, 51 percent? And if we are to grade the public in this manner, what shall we say constitutes a passing or failing grade?" (p.16). Mondak and Davis (2001) offered four "levels" of political knowledge:



(1) fully informed (i.e., the respondent truly does know the answer to our question); (2) partially informed (the respondent either possesses an incomplete understanding, or the respondent can rule out an incorrect choice option on a multiple-choice item); (3) misinformed (the respondent believes he or she knows the correct answer, but is mistaken); and (4) uninformed (the respondent holds no knowledge pertinent to the question) (p. 201, 202).

Throughout the 1950s, researchers refrained from studying political knowledge more thoroughly for several reasons (Lambert, Curtis, Kay, & Brown, 1988). First, the earliest voting studies found low levels of political knowledge as it was measured among respondents (Berelson et al., 1954; Campbell, Converse, Miller & Stokes, 1960; Campbell, Gurin & Miller, 1954; Campbell & Kahn, 1952; Lazarsfeld, Berelson & Gaudet, 1944), and an insignificant correlation was evident between these levels and either overall attitudes towards politics or political behaviors. Through interviews and surveys of voters in Michigan, Campbell and colleagues (1952, 1960) found that few people knew a lot about politics, about half knew enough to answer basic questions, while the rest knew little at all. Worse still, the percentage of people falling into the latter category was growing, particularly among young people. With low levels of political knowledge and insignificant relationships between the concept and other attitudes and behaviors, this area of research was mostly abandoned.

A second reason is also cited for the decrease in the study of political knowledge. In order to maintain a respectful relationship with participants, researchers refrained from posing questions about what people knew about politics for fear that this could demoralize or confuse participants and hinder their willingness to answer other questions (Lambert et al., 1988). To compensate for this belief, some researchers prefaced their questions with statements such as, “Many people are unfamiliar with <insert issue or politician>.”

A final reason focused on research designs. It was believed that people who may not have any real opinion or knowledge about an issue may feel inclined to give an opinion anyway, what Neuman (1986) called a “pseudo opinion” (p. 22), either to satisfy their desire to please the researcher or to feel better about themselves.

Although political knowledge became the focus of scholarly research again in the 1980s, to this day ways to measure political knowledge remain in serious dispute. Zaller (1990) wrote, “variables purporting to measure ‘political awareness,’ ‘political expertise,’ ‘political sophistication,’ ‘cognitive sophistication,’ ‘political information,’ ‘political involvement,’ ‘media exposure,’ and ‘political interest’ appear regularly in the public opinion literature and are used (along with education) more or less interchangeably to explain the same family of dependent variables” (p. 126). Kuklinksi and Quirk (2001) described the creative ways scholars assess citizens’ political knowledge (competence in their language):

They have considered whether citizens hold consistent positions across issues; whether they hold stable positions across time; whether they know relevant facts from a policy debate; whether they maintain their positions when given different framings of the same issue; whether their preferences are correlated with their values; whether their preferences resemble those of others who are well informed; and whether they effectively take cues from parties, politicians, interest group, and other citizens (Kuklinksi & Quirk, 2001, p.286).

A lot of research focuses on political knowledge trends, with a goal of identifying if there are significant differences between generations or groups of people (e.g. Delli Carpini & Keeter, 1991; Jennings, 1996). Ideally, researchers would be able to compare levels of political knowledge across generations and between groups of people. However, researchers often fail to use items that allow for these comparisons. For instance, Jennings (1996) divided political knowledge into textbook knowledge, surveillance knowledge, and historical

knowledge. For textbook knowledge, he included measures about government mechanics such as, “About how many years does a U.S. Senator serve? Do you happen to know how many members there are on the United States Supreme Court?” These are consistent measures of political knowledge. As measures of surveillance or “current events” knowledge, he asked, “Marshall Tito is a leader of what country? Who is governor of [name of state] now?” Finally, to assess historical knowledge he asked, “Do you happen to remember whether President Franklin Delano Roosevelt was a Republican or a Democrat? During World War II, which nation had a great many concentration camps for Jews? Who succeeded John Kennedy as president? Do you know a country that borders on North or South Vietnam?” As is evident, the current events are subject to changes over time, and the historical questions become more and more distant to each generation.

At the most basic level, political knowledge is an understanding of “what government is and does” (Barber, 1969, p. 38). To be politically knowledgeable, one must know “the basic structure of government – its basic values, such as citizen participation, majority rule, separation of powers, civil liberties, and its basic elements, such as the two-party system, the two houses of Congress, the role of the judiciary, and the organization of the cabinet” (Neuman, 1986, p. 186). Delli Carpini and Keeter (1993) surveyed political scientists to assess what was believed to be important for the average citizen to know about politics. The “essential” and “important” topics to political scientists were 1) institutions and processes, 2) issues and policies, 3) history, and 4) current political alignments.

In any given study, the conceptualization and measurement of a concept such as political knowledge are open to the interpretation of the author. While there is certainly a large amount of subjectivity of opinion with regard to issues, there also exist certain

undeniable truths. Knowledge for the purposes of this paper refers to the accumulation of proven, objective facts and concepts. Therefore, a core measure of political knowledge must include awareness or recognition of political parties or of candidates, the mechanics of government, and cognizance of personally relevant issues (Delli Carpini & Keeter, 1993). (See Appendix A for scale).

There is serious debate about whether political knowledge as measured this way accurately reflects what a person knows about politics, or whether it is important for a functioning citizenry. First, measuring political knowledge as a collection of facts limits the way we define a knowledgeable citizen. People are often able to make accurate decisions in their own best interest with limited information, using cues or heuristics such as party affiliation when making political judgments (Brady & Sniderman, 1985; Popkin, 1991). Whether or not someone can name the Secretary of State may not say much about his or her level of political interest or involvement. In addition, other outcomes (political talk, volunteerism, democratic imaginations) may be even more valuable (Perrin, 2006), but are less easily quantified or measured in experiments. This research does not make the case that political knowledge as measured is a valid construct, only that as a carefully crafted, standardized product, it might reveal some quantifiable difference between groups of people.

In addition to measuring political knowledge, this research examined the effect of exposure to disparaging political statements on young citizens' perceived level of political knowledge. Confidence in one's abilities to perform the functions of a citizen may be just as important an indicator of likely civic behavior as more objectively measured political knowledge.

Political efficacy is defined as “the feeling that individual political action does have or can have an impact upon the political process...”(Campbell, Gurin & Miller, 1954, p. 187). Kaid, McKinney, and Tedesco (2007) recently developed a related scale to assess how confident people are in their level of political knowledge. Kaid et al. (2007) state that political information efficacy, “focuses solely on the voter’s confidence in his or her own political knowledge and its sufficiency to engage in the political process (to vote)” (p. 1096). These authors found that “young voters who do not feel confident in their knowledge levels are less likely to vote than those who feel more confident” (p. 1103). Other research indicates that confidence in one’s abilities is strongly related to political participation. Solhaug (2006) found that self-efficacy, knowledge, and motivation all have significant impacts on young people’s political participation and political attitudes. Bandura (1986; 1997) found a strong positive correlation between a person’s level of efficacy and his or her participation in politics. Similarly, McClusky, Deshpande, Shah & McLeod (2004) found that the size of the “gap” between a person’s desired and perceived level of efficacy influenced whether he or she was politically active.

Political knowledge and political information efficacy may be strong predictors of political engagement. To this point there has been little research comparing performance on a political knowledge test with political information efficacy scores. As a measure of one’s self-confidence, it fits in well with the stereotype threat research as a possible mediator of the effects of disparaging statements on performance on a political knowledge test.

Young voters are commonly disparaged for their low levels of political knowledge in both the traditional press as well as in entertainment programming. This is a concern since some young voters feel that they do not know enough to participate in politics, possibly

inhibiting their motivation to be politically active. It is possible that the way young voters are discussed and represented in the media may cause feelings of inadequacy. While there are several studies about the effects of political humor about candidates on young voters' attitudes, little research to date has examined the effects of disparaging messages about young voters on young voters. While some scholars argue that late-night political humor shows create cynical and ill-informed citizens, it is possible that people dismiss information from these shows as "just jokes," or their attitudes towards the material is influenced by their affective expectations. This research examined the mediating role of humor expectancy on political attitudes and the mechanisms thought to increase stereotype threat effects.

The present experiments add to the literature by testing the effects of stereotype threat on young adults with regards to their levels of political knowledge. Stereotype threat is a phenomenon whereby people made cognizant of a negative stereotype about a group unintentionally confirm that stereotype in a subsequent task. Experiment 1 was run to document the phenomenon of stereotype threat in young voters with regard to their level of political knowledge. The research question for experiment 1 was as follows:

RQ1: For young people, what is the relationship between having one's age made salient and the purported diagnosticity of the test on political information efficacy and performance on a political knowledge test?

Prior literature indicates that indicating affiliation with a stereotyped group (salience) can increase stereotype threat effects, as can performing a task described as indicative of one's abilities (purported diagnosticity) (e.g., Steele & Aronson, 1995). Based on the literature on stereotype threat, the following hypotheses were offered:

H1a: Making the age group of young voters salient prior to a political knowledge test will negatively affect political information efficacy and political knowledge scores.

H1b: Anxiety and motivation will mediate the effects of age salience on political information efficacy and performance on the political knowledge test.

The presence of the age salience manipulation will increase scores on the anxiety and motivation measures. Increases in these mediating variables will in turn result in decreases in political information efficacy and performance on the political knowledge test. The inclusion of anxiety and motivation in the regression models will reduce or eliminate the effects of the age salience manipulation on the dependent variables.

H1c: Political information efficacy will mediate the effects of age salience on the political knowledge test.

The presence of the age salience manipulation will cause a decrease in scores on the political information efficacy scale. This decrease will lead to subsequent decreases in performance on the political knowledge test. The inclusion of PIE in the regression model will reduce or eliminate the effects of the age salience manipulation on performance on the political knowledge test.

H1d: Belief in the stereotype, awareness of the stereotype, and group and domain identification will moderate the influence of age salience on political information efficacy scores and political knowledge performance.

People who report lower scores on any of the moderating variables will not be as affected by the presence of the age salience manipulation as people who report higher scores on those variables.

H2a: Describing the test as diagnostic will negatively affect political information efficacy and political knowledge scores.

H2b: Anxiety and motivation will mediate the effects of the diagnostic salience manipulation on political information efficacy and performance on the political knowledge test.

The presence of the diagnostic salience manipulation will increase scores on the anxiety and motivation measures. Increases in these mediating variables will in turn result in decreases in political information efficacy and performance on the political knowledge test. The inclusion of anxiety and motivation in the regression models will reduce or eliminate the effects of the diagnostic salience manipulation on the dependent variables.

H2c: Political information efficacy will mediate the effects of diagnostic salience on the political knowledge test.

The presence of the diagnostic salience manipulation will cause a decrease in scores on the political information efficacy scale. This decrease will lead to subsequent decreases in performance on the political knowledge test. The inclusion of PIE in the regression model will reduce or eliminate the effects of the diagnostic salience manipulation on performance on the political knowledge test.

H2d: Belief in the stereotype, awareness of the stereotype, and group and domain identification will moderate the influence of diagnostic salience on political information efficacy scores and political knowledge performance.

People who report lower scores on any of the moderating variables will not be as affected by the presence of the diagnostic salience manipulation as people who report higher scores on those variables.



H3: The presence of both the age salience and diagnostic salience manipulations together will affect attitudes and performance more than either independent variable in isolation.

## **Experiment 2**

The goal of the second experiment was to test whether exposure to disparaging political humor or political statements influenced young citizens' attitudes and performance in a similar manner as the first experiment and whether the expectation of humor influences these effects.

For experiment 2, the following research question was asked:

RQ2: For young voters, what is the relationship between exposure to disparaging political statements or political humor on political information efficacy and performance on a political knowledge test?

Prior theorizing indicates that exposure to political humor may have negative effects on individuals' political attitudes (e.g., Hart & Hartelius, 2007). There is some evidence that exposure to political humor about politicians may cause feelings of cynicism (e.g.,

Baumgartner & Morris, 2006; 2008). Therefore, the following hypothesis was offered:

H4: Exposure to humorous disparagement will have a greater negative effect on political information efficacy and performance on the political knowledge test than will exposure to non-humorous disparagement.

However, there is reason to think that concerns about the effects of political humor on young citizens are unwarranted, since people cued about the presence of humor (as are late-night humor viewers) approach messages with different expectations (Katz & Pexman, 1997; McGhee, 1972; Pexman & Olineck, 2002; Zillmann, 1983). Therefore, the following hypothesis was offered:

H5: Participants in the humor expectancy conditions will indicate greater political information efficacy and perform better on the political knowledge test than will participants in the no expectation conditions.

H6: Anxiety, motivation, and message counterarguing will mediate the effects of humor expectancy and exposure to disparaging political statements or humor on political information efficacy and performance on the political knowledge test.

Hypothesis 6 sought to identify the mechanisms through which expected or unexpected exposure to humor affects attitudes and performance on the political knowledge test. Participants exposed to humorous disparagement will indicate higher scores on the mediating variables than will participants exposed to non-humorous disparagement only when the humor is unexpected. This difference will account for the lower scores in the humorous disparagement conditions (Hypothesis 4). In other words, if humor is a negative force on individuals' attitudes and behaviors, then people exposed to humorous disparagement will feel anxious, feel motivated, and engage in more counterarguing than people exposed to non-humorous disparagement, and thus perform worse on the political knowledge test. On the other hand, participants in the humor expectancy conditions will have lower scores on the mediating variables than will participants in the no expectation conditions. This difference will account for the greater efficacy and better performance on the political knowledge test in the expectancy conditions (Hypothesis 5). In other words, the cue that humor is forthcoming cues people that the information to follow is not to be taken seriously, thereby reducing anxiety, motivation, and counterarguing, and in so doing, eliminate or reduce stereotype threat effects.

H7: Political information efficacy will mediate the effects of humor expectancy and exposure to disparaging political statements or humor on performance on a political knowledge test.

Participants exposed to humorous disparagement will report lower political information efficacy than will participants in the non-humorous disparagement conditions, only when it is unexpected. This decrease in efficacy will account for the subsequent decrease in performance on the political knowledge test (Hypothesis 4). Participants in the humor expectancy conditions will report higher levels of efficacy than will participants in the no expectation conditions, and this difference will account for the improved performance on the political knowledge test (Hypothesis 5).

H8: Belief in the stereotype, awareness of the stereotype, and group and domain identification will moderate the influence of the disparaging statements or humor on political information efficacy and political knowledge performance.

People who report lower scores on any of the moderating variables will not be as affected by exposure to disparaging political statements or political humor as will people who report higher scores on those variables.

H9: An interaction effect between humor expectancy and exposure to disparaging political humor on political knowledge and political information efficacy is predicted. Regardless of whether participants are exposed to the disparaging humor or statement, participants in the humor expectancy conditions will perform equally as well on the political knowledge test and indicate equal levels of PIE. The effect of political humor will only be significant (resulting in a decrease in scores on both dependent measures) under the condition where it is unexpected.

## **CHAPTER II**

### **EXPERIMENT 1 METHOD**

Two experiments were run to test the effects of disparaging political humor on young voters' political attitudes and performance on a political knowledge test. Participants in both experiments were drawn from undergraduate classes at the University of North Carolina at Chapel Hill to take part in a "Media Diet" study. Subjects entered the lab, were greeted by the principal investigator, asked to sign in to receive course credit, handed an informed consent document with a random letter (A through D) written prominently on top, and directed to an available computer terminal. The sessions began a few minutes after their scheduled time, with groups as small as five and as large as 25 participating at any time.

Experiment 1 asked, "what is the relationship between having one's age made salient and the purported diagnosticity of the test on political information efficacy and performance on a political knowledge test?" This experiment used a between subjects factorial design, with age salience (present vs. absent) and diagnosticity (present vs. absent) as the independent variables in the 2x2 design. Four separate computers "surveys" were constructed, with the only differences being the independent variables of age and diagnostic salience. On the screen of each computer terminal was a Word document with A, B, C & D listed in blue letters. Participants were instructed to look at the top of their informed consent document to find out which of these four surveys they had been randomly assigned to take. Upon clicking the letter on their computer screen, all participants read the following

message: “Thank you for participating in this research. You have been assigned to a study about politics and political knowledge. To proceed, please click the forward arrow.”

Participants in the age salience conditions (A & B) were then asked the following question: To begin, please select the age category to which you belong; (18-24) (Young voter), 25-39 (Adult voter), 40-60 (Middle Age voter), or 61+ (Senior voter). In the age salience absent conditions (C & D), participants did not see this question.

Participants in the diagnostic conditions (A & C) then read the following message: “The political knowledge test you are about to take is a useful tool for comparing the intelligence of groups. At the end of this study, you will receive feedback about your performance. To begin the experiment, please click the forward arrow.” In the conditions without diagnostic salience (B & D), there was no mention of the tests’ diagnostic nature. A table indicating the cells is below.

Experiment 1 Design

	Age Salience	No Age Salience
Diagnostic	(A) High Threat	(C) Mod Threat
No mention of Diagnostics	(B) Mod Threat	(D) Control

A pre-test of an earlier version of this experiment was conducted the summer before this experiment was run. Half of the subjects were randomly assigned to select their age and then read the following statement before taking a political knowledge test.

“It is widely believed that people your age are less informed about politics than most people. It has been shown that people your age are less politically informed than prior generations. The media often cover stories about how young voters like you do very poorly on political knowledge tests.

We would like for you to take a test of your level of political knowledge.  
Your results will be compared with those of people of other age groups.”

Results of the pre-test indicated that participants exposed to this threat answered fewer questions correctly than those not exposed to the threat, although the difference was not statistically significant,  $F(1, 50) = 3.44, p=.069$ . The present research built off the pre-test by separating the test diagnosticity from the age salience factor and making changes to the political knowledge test.

The dependent measures for both experiments were political knowledge and political information efficacy. Smith (1989) argued that political knowledge indices and scales typically are structured in one of two ways. First, researchers may count the total number or percentage of correct responses (forced choice) to get a measure of a respondents’ political knowledge. Secondly, they might enlist free recall or memory measures, such as asking respondents for the names of political candidates, who controls the House of Representatives, the differences between Republicans and Democrats, or the ability to recognize the meaning of terms such as “conservative” or “liberal.” This research counted as correct or incorrect a series of fill in the blank, recall questions based on rules developed *a priori*. Appendix A lists the 13 questions that were used in the political knowledge test along with the answers that were accepted as correct. Test anxiety effects are more likely to be found when the test is cognitively taxing (Baumeister, 1984), therefore having people recall from memory without the aid of multiple choice should increase the likelihood that the hypothesized effects occur. The questions used to assess political knowledge build off the work of Delli Carpini and Keeter (1993) and National Election Studies questionnaires.

After completing the political knowledge test, participants completed the political information efficacy scale (Appendix B). The political information efficacy scale (Kaid et al.,

2007) consists of four, five-point Likert scale measures. These four items showed high levels of reliability (Cronbach's Alpha of .87) and together accounted for up to ten percent of the variance in youth voting behavior in 2000 and 2002 (Kaid et al., 2007).

After completing the two dependent measures, participants answered questions about what mental processes occurred while they were taking the political knowledge test. The most common state anxiety measures are intended to be taken before the test of performance abilities or they lack the specificity needed to provide a real understanding of the process. The present research measured anxiety using two scales. The first was a five measure semantic differential scale created by Mattson (1960), and used in stereotype threat research by Stone et al. (1999). For instance, participants were asked to indicate on a seven-point scale how they felt during the test from uneasy to easy, and uncomfortable to comfortable. The scale was tested for reliability and the items summed to create an anxiety score.

The second scale of anxiety consisted of eight exploratory items created for this research. Participants were asked their level of agreement on a five-point Likert scale with statements such as "While taking the test, I was worried about confirming the stereotype that young voters are uninformed about politics," and, "When I didn't know the answer to a question, I was able to stay calm" (reverse scored). This scale was also tested for reliability and a score created for each participant.

Motivation and effort have been measured by self report, by the time spent on each question, and by the number of questions attempted, with mixed results (e.g., Aronson et al., 1999; Beilock, Rydell, & McConnell, 2007; Jamieson and Harkins, 2007; Osborne, 2007; Schmader & Johns, 2003). The present research used a five-item Likert type scale to assess how much effort participants put into completing the test. Among the questions in the scale

(Appendix C) were, “When I didn’t know the answer to a question, I tried harder on the next one,” and, “I felt the need to distance myself from the stereotype that young voters are uninformed about politics.”

The belief in stereotype scale consisted of three questions to assess how strongly people believed prior to taking the test that young people are typically uninformed about politics. The awareness of stereotype scale, created by Pinel (1999), is capable of assessing awareness of any stereotype about any demographic group. For instance, one of the statements participants are asked about reads, “Before taking the test, I believed the stereotype that *blank* tend to be *blank*.” In this case the phrases “young voters” and “uninformed about politics” makes the scale applicable to the present research. Appendix E contains the scales for the moderating variables of belief in and awareness of the stereotype.

The next two moderators were the degree to which participants felt that the group or domain in question was an important part of their identity. To assess group identification, Luhtanen and Crocker’s (1992) scale of collective self-esteem was adapted. Participants were asked their level of agreement on a five-point Likert scale with questions such as, “In general, being a young voter is an important part of my self-image.”

The domain identification scale was adapted from Spencer et al. (1999) and Aronson et al. (1999). Participants were asked to identify on a five item, five-point Likert scale their level of agreement with statements such as, “Knowledge of politics is important to me,” and “I like politics.” Next, participants answered questions designed to assess their overall level of efficacy, using a widely used and reliable scale (Schwarzer & Jerusalem, 1995). Finally, participants in all conditions answered basic demographic questions about their age, gender and political affiliation (See Appendix E).



## **CHAPTER III**

### **EXPERIMENT 1 RESULTS**

#### **Sample**

One hundred and thirty subjects participated in experiment 1. Participants ranged in age from 18 to 23, with a mean age of 20.39. The sample was largely female (84.6%). Participants identified themselves as Caucasian (80.7%), of mixed race (8.5%), Black (3.8%), Asian (3.8%), and Hispanic (3%). Most participants identified themselves as Democrats (47%), followed by the Republicans (29%), Independent (14%), Libertarians (6%), Other (2%), and Socialists (2%).

#### **Scale Reliability**

The political knowledge scale showed moderate reliability, Cronbach's  $\alpha = .679$ . The PIE scale exhibited strong reliability ( $\alpha = .837$ ). The established anxiety scale (Mattson, 1960, referred to as Anxiety1) was highly reliable ( $\alpha = .905$ ), while the measure created for this study (Anxiety2) was weaker but still strong ( $\alpha = .773$ ). The scale for motivation exhibited very low reliability ( $\alpha = .420$ ). Three items on that scale were highly correlated and served as the basis of a more reliable scale ( $\alpha = .695$ ). These were, "I felt the need to counter the stereotype that young voters are uninformed about politics," "I tried to counter the stereotype that young voters are uninformed about politics," and "When I didn't know the answer to a question, I tried harder on the next one." The three question index of participants' belief in the stereotype was the least reliable ( $\alpha = .640$ ). The stereotype awareness scale was reliable

( $\alpha=.754$ ), as was the measure of group identification ( $\alpha=.731$ ). The domain identification scale was reliable ( $\alpha=.811$ ), as was the index of overall efficacy ( $\alpha=.822$ ).

### Descriptive Statistics

Table 1 displays the means and standard deviations for each of the three experimental conditions and the control group on the dependent variables of political knowledge and PIE.

Table 1

*Means and Standard Deviations for each Condition on the Dependent Variables*

	Political knowledge (out of 15)	PIE (out of 20)
Age Salience ( $N=33$ )	(10.82, 1.91)	(13.03, 3.32)
Diagnostic Salience ( $N=32$ )	(9.19, 3.11)	(12.38, 3.36)
Age+Diag ( $N=35$ )	(10.86, 2.43)	(13.49, 3.28)
Control ( $N=30$ )	(10.40, 2.01)	(12.30, 3.37)
Total ( $N=130$ )	(10.33, 2.48)	(12.82, 3.37)

Table 2 displays the means and standard deviations for each condition on the mediating variables.

Table 2

*Means and Standard Deviations for each Condition on the Mediating Variables*

	Anxiety 1 (out of 35)	Anxiety 2 (out of 40)	Motivation (out of 15)
Age Salience ( $N=33$ )	(17.03, 7.57)	(23.52, 3.89)	(8.67, 2.34)
Diagnostic Salience ( $N=32$ )	(18.00, 6.90)	(23.75, 5.06)	(9.34, 1.62)
Age+Diag ( $N=35$ )	(17.09, 7.37)	(24.40, 4.52)	(10.00, 2.17)
Control ( $N=30$ )	(18.53, 6.16)	(24.77, 5.11)	(9.53, 1.94)
Total ( $N=130$ )	(17.63, 6.99)	(24.1, 4.62)	(9.39, 2.08)

Table 3 displays the means and standard deviations for each condition on the moderating variables.

Table 3

*Means and Standard Deviations for each Condition on the Moderating Variables*

	Awareness (50)	Belief (17)	Domain (25)	Group (20)	Efficacy (40)
Age Salience ( $N=33$ )	(27.58, 5.20)	(9.55, 2.55)	(14.49, 3.67)	(8.91, 3.21)	(32.58, 3.48)
Diagnostic Salience ( $N=32$ )	(29.47, 4.44)	(10.13, 2.89)	(14.53, 3.93)	(9.66, 2.96)	(32.50, 3.45)
Age+Diag ( $N=35$ )	(28.71, 4.85)	(10.51, 2.21)	(15.23, 3.92)	(9.11, 2.03)	(33.17, 3.29)
Control ( $N=30$ )	(29.7, 6.02)	(10.20, 2.65)	(13.90, 3.21)	(9.40, 2.74)	(30.90, 2.58)
Total ( $N=130$ )	(28.84, 5.15)	(10.10, 2.57)	(14.56, 3.69)	(9.26, 2.74)	(32.33, 3.30)

## Hypothesis Testing

Hypothesis 1a stated that making age salient to participants would result in a decrease in political information efficacy and political knowledge scores. Hypothesis 1a was not supported. As Table 4 indicates, participants in the age salience condition scored slightly higher on the political knowledge test ( $M=10.82$ ) and PIE measure ( $M=13.03$ ) than participants in the control condition ( $M=10.40$  for political knowledge and 12.30 for PIE). Results indicate that, contrary to Hypothesis 1a, the presence of age salience resulted in increases in scores on the PIE measure ( $\beta=.730$ ,  $t=.842$ ,  $p=.403$ ), and political knowledge measure ( $\beta=.418$ ,  $t=.846$ ,  $p=.401$ ) as compared to the control group, though neither increase was significant.<sup>2</sup>

While age salience alone did not have a significant direct effect on the dependent variables, it is possible that it could have influenced the mediators. Recently, scholars have

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<sup>2</sup> In order to remain consistent throughout this paper, regressions were used whenever appropriate. The same results would be attained through ANOVA or ANCOVA.

argued that the absence of direct effects does not preclude mediation or “indirect effects” from occurring (Hayes, 2009). Hypothesis 1b posited that the anxiety and motivation scores would mediate the relationship between the presence of the age salience manipulation and scores on the dependent measures. Hypothesis 1b was not supported. The means in Table 2 show that whereas participants in the age salience condition were predicted to have higher scores on the mediating variables, participants in the control group actually indicated higher scores on these measures. To test whether these differences were significant, the mediators of anxiety (two measures) and motivation were regressed on the age salience manipulation. The presence of age salience led to non-significant decreases on Anxiety1 ( $\beta=-1.50$ ,  $t=-.859$ ,  $p=.394$ ), Anxiety2 ( $\beta=-1.25$ ,  $t=1.1$ ,  $p=.276$ ), and motivation ( $\beta=-.867$ ,  $t=-1.59$ ,  $p=.117$ ) as compared to the control group.

To determine if the presence of age salience interacted with the mediating variables in predicting PIE and political knowledge scores, several regression models were run. Since the two anxiety measures were highly correlated ( $r=.567$ ,  $p<.01$ ), only the established scale was used in the full regressions. The models in Tables 4 and 5 indicate that regardless of whether the analysis included any or all of the mediating variables, age salience did not have a significant impact on PIE scores or political knowledge scores as compared to the control group. Similarly, Hypothesis 1c, which predicted the PIE would mediate the effects of age salience on political knowledge scores, is not supported. Figures 1 and 2 displays the lack of mediation predicted in Hypotheses 1b and 1c.

Results for Hypothesis 1a, 1b, and 1c indicate that the presence of age salience alone did not directly affect PIE, political knowledge scores or the proposed mediating variables.

Neither did the addition of the mediating variables to the models reduce or change the effect of the age salience manipulation on the dependent variables.

Table 4

*Summary of Multiple Regression Analysis for PIE Scores (N=63)*

VARIABLES	a	b	c	d	e
(Constant)	12.3***	9.67***	15.42**	14.96**	11.93***
Age_alone	.730	.875	.478	.596	.686
Motivation		.276			.426*
Anx1Score			-.168**		-.199**
Anx2Score				-.108	-
R <sup>2</sup> =	.19				

Note: Model a contains age only, b contains age and motivation, c contains age and anx1, d contains age and anx2, e contains age, motivation, and anx1.

\*p<.05; \*\*p<.01; \*\*\*p<.001

Figure 1: Analysis of relationship between age salience, anxiety, motivation, and PIE scores

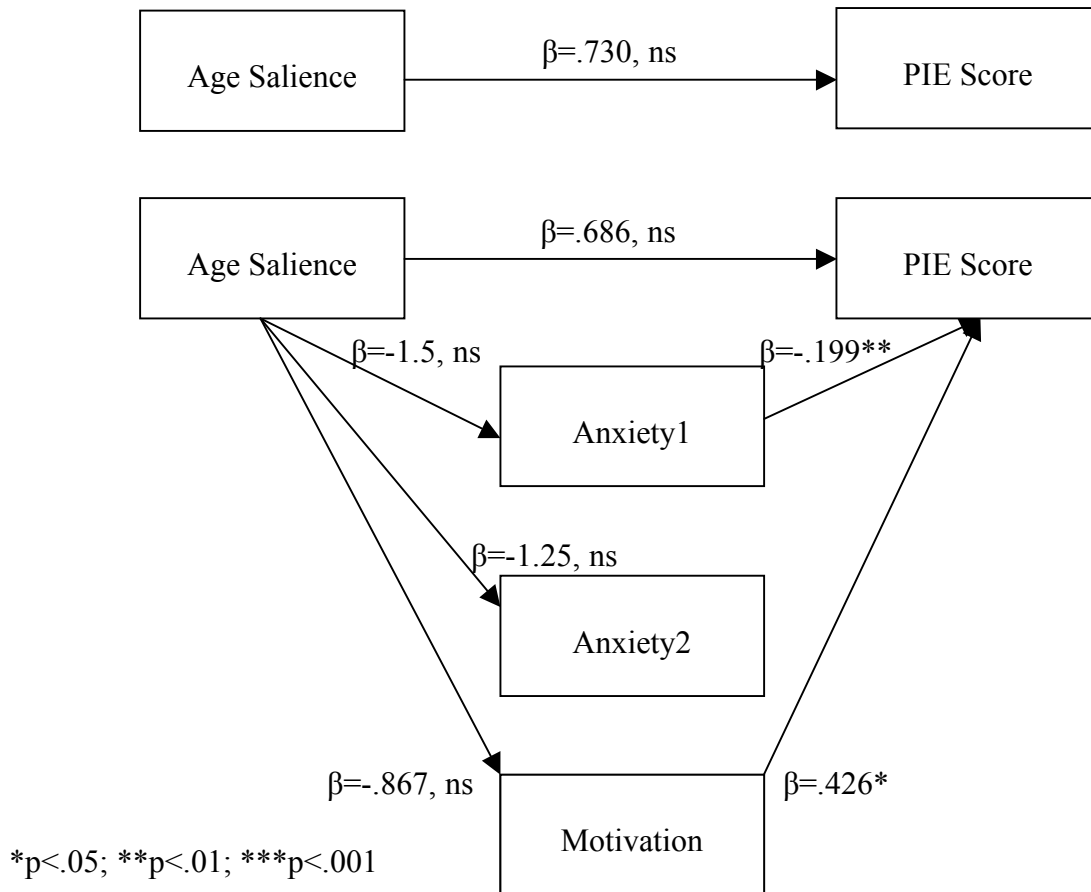


Table 5

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=63)*

VARIABLES	a	b	c	d	e	f
(Constant)	10.4***	10.85***	12.06***	12.08***	6.71***	8.45***
Age salience	.418	.377	.283	.333	.199	.069
Motivation		-.047				-.101
Anx1Score			-.089*			-.034
Anx2Score				-.068		
PIE_Score					.300***	.288***
R <sup>2</sup> =.32						

Note: Model a contains age only, b contains age and motivation, c contains age and anx1, d contains age and anx2, e contains age and PIE score, f contains age, motivation, anx1, and PIE score.

\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

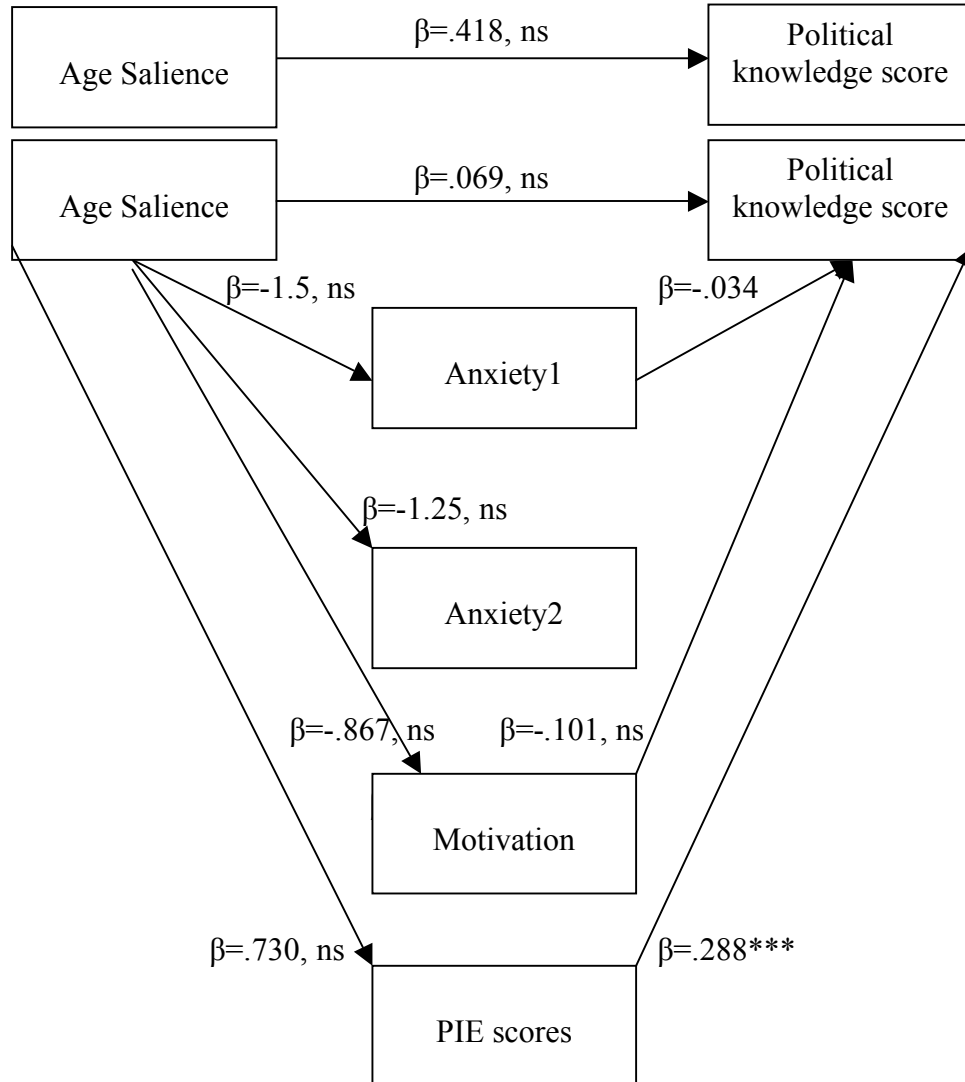
Next, Hypothesis 1d stated that the effect of age salience on PIE and political knowledge scores would be stronger as scores on the moderating variables increased. Hypothesis 1d was not supported. First, it was determined that the presence of the age salience manipulation alone did not cause a significant change in any of the moderators (stereotype belief or awareness, domain or group identification), except for overall efficacy ( $\beta = 1.68$ ,  $t = 2.15$ ,  $p = .035$ ). Participants in the age salience condition indicated higher levels of overall efficacy than participants in the control condition.

The effect of age salience on political knowledge scores was no different for participants regardless of their scores on the domain identification scale ( $\beta = -.094$ ,  $t = -.671$ ,  $p = .505$ ), group identification scale ( $\beta = -.192$ ,  $t = -1.12$ ,  $p = .266$ ), stereotype awareness scale ( $\beta = .136$ ,  $t = 1.518$ ,  $p = .134$ ), or stereotype belief scale ( $\beta = -.208$ ,  $t = -1.082$ ,  $p = .284$ ).

Subsequent analyses were performed to test the effects of these individual difference variables on the relationship between exposure to the age salience manipulation and PIE scores. The effect of age salience on PIE scores was no different for participants regardless of

their scores on the domain identification scale ( $\beta=.042$ ,  $t=.221$ ,  $p=.826$ ), group identification scale ( $\beta=.081$ ,  $t=.299$ ,  $p=.766$ ), stereotype awareness scale ( $\beta=.118$ ,  $t=.737$ ,  $p=.464$ ), or stereotype belief scale ( $\beta=.042$ ,  $t=.127$ ,  $p=.899$ ).

*Figure 2: Analysis of relationship between age salience, anxiety, motivation, and political knowledge scores*



\* $p<.05$ ; \*\* $p<.01$ ; \*\*\* $p<.001$

Hypothesis 2a stated that making the diagnostic nature of the test salient would result in a decrease in both the PIE and political knowledge measures. Hypothesis 2a was partially supported. The means on the PIE measure were nearly identical ( $M=12.38$  for the diagnostic

condition and 12.30 for the control condition). Participants in the diagnostic condition averaged 9.19 correct answers to the political knowledge test, as compared to 10.40 for participants in the control group. These differences were tested using linear regression. Results indicate that the presence of the diagnostic salience manipulation did not effect PIE scores ( $\beta=.075$ ,  $t=.085$ ,  $p=.932$ ), but did cause a marginally significant decrease in scores on the political knowledge test ( $\beta=-1.213$ ,  $t=-1.81$ ,  $p=.075$ , See Table 6) as compared to the control group.

Table 6

*Summary of Regression Analysis for Political Knowledge Scores (N=62)*

VARIABLES	B	SE( $\beta$ )	$\beta$	$t$	Sig.( $p$ )
(Constant)	10.40	.481		21.63	.000
Diagnostic Salience Alone	-1.213	(0.669)	-0.228	-1.81	0.075
$R^2=0.052$					

While diagnostic salience alone did not have a significant direct effect on PIE scores, it had a marginal effect on the political knowledge scores. It is possible that either of these relationships was mediated by the effect of diagnostic salience on the mediating variables. Hypothesis 2b posited that the anxiety and motivation scores would mediate the relationship between the presence of the diagnostic salience manipulation and scores on the dependent measures. Hypothesis 2b was not supported. First, the means in Table 1 show that whereas participants in the diagnostic salience condition were predicted to have higher scores on the mediating variables, participants in the control group actually indicated higher scores on these measures. To test whether these differences were significant, the mediators of anxiety (two measures) and motivation were regressed on the diagnostic salience manipulation. The presence of diagnostic salience led to non-significant decreases on the established anxiety



measure ( $\beta=-5.33$ ,  $t=-.310$ ,  $p=.750$ ), new anxiety measure ( $\beta=-1.017$ ,  $t=-.787$ ,  $p=.435$ ) and the motivation score ( $\beta=-.190$ ,  $t=-.418$ ,  $p=.677$ ) as compared to the control group.

The models in Table 7 indicate that regardless of whether the analysis included any or all of the mediating variables, diagnostic salience did not have a significant impact on PIE scores as compared to the control group. Figure 3 displays this lack of mediation graphically. Anxiety and motivation did not mediate the relationship between diagnostic salience and PIE scores.

Table 7

*Summary of Multiple Regression Analysis for PIE Scores (N=62)*

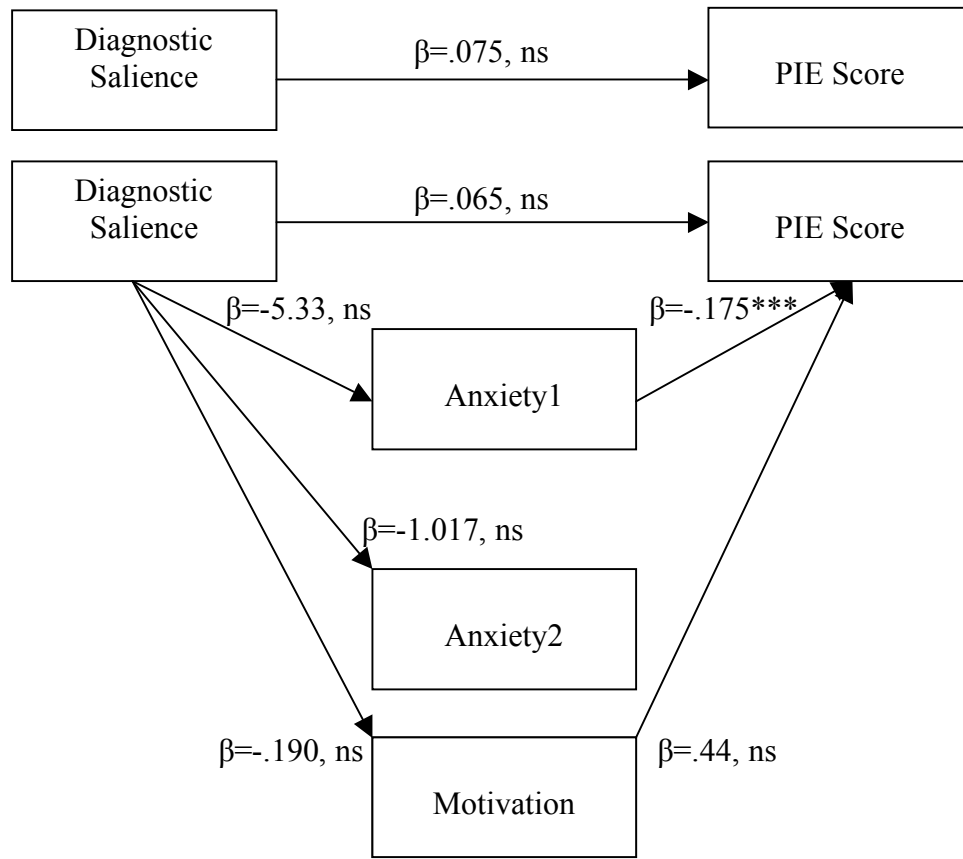
VARIABLES	a	b	c	d	e
(Constant)	12.3***	9.097***	15.19***	14.53***	11.35***
Diagnostic_alone	.075	.139	-.008	-.017	.065
Motivation		.336			.44
Anxiety1Score			-.156*		-.175***
Anxiety2Score				-.090	-
R <sup>2</sup> =.14					

Note: Model a contains diagnostic only, b contains diagnostic and motivation, c contains diagnostic and anx1, d contains diagnostic and anx2, e contains diagnostic, motivation, and anx1.

\* $p<.05$ ; \*\* $p<.01$ ; \*\*\* $p<.001$

Table 8 shows that when controlling for the anxiety measures or PIE scores, the coefficient for diagnostic salience became more negative and crossed the  $p<.05$  threshold for significance. This is likely an instance of statistical suppression due to correlations between the mediators and the dependent variable of political knowledge. Therefore, Hypotheses 2b and 2c are not supported. Figure 4 displays the relationship between diagnostic salience, the mediating variables, and political knowledge scores.

Figure 3: Analysis of relationship between diagnostic salience, anxiety, motivation, and PIE scores



\* $p<.05$ ; \*\* $p<.01$ ; \*\*\* $p<.001$

Results for Hypothesis 2a, 2b, and 2c indicate that the presence of diagnostic salience alone did not directly affect PIE or the proposed mediating variables. However, as compared to the control group, the presence of diagnostic salience did result in a marginally significant decrease in political knowledge scores.

Next, Hypothesis 2d stated that the effect of diagnostic salience on PIE and political knowledge scores would be stronger as scores on the moderating variables increased. Hypothesis 2d received partial support. As with age salience, the presence of diagnostic salience did not cause a significant change in any of the other mediators or moderators except for overall efficacy ( $\beta=1.6, t=2.06, p=.044$ ). Participants told that the test was diagnostic

performed worse on the political knowledge test, particularly when controlling for anxiety and PIE scores, yet surprisingly reported significantly higher levels of overall efficacy than participants in the control group.

Table 8

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=62)*

VARIABLES	a	b	c	d	e	f
(Constant)	10.4***	11.77***	13.78***	13.06***	4.61***	9.18***
Diagnostic_alone	-1.213	-1.24	-1.31*	-1.32*	-1.25*	-1.34**
Motivation		-.144				-.227
Anxiety1Score			-.182***			-.105
Anxiety2Score				-.108***		-
PIE_Score					.471***	.423***
R <sup>2</sup> =.51						

Note: Model a contains diagnostic only, b contains diagnostic and motivation, c contains diagnostic and anx1, d contains diagnostic and anx2, e contains diagnostic and PIE score, f contains diagnostic, motivation, anx1, and PIE score.

\*p<.05; \*\*p<.01; \*\*\*p<.001

The effect of diagnostic salience on political knowledge scores was no different for participants regardless of their scores on the domain identification scale ( $\beta=.241$ ,  $t=1.49$ ,  $p=.140$ ), group identification scale ( $\beta=-.278$ ,  $t=-1.16$ ,  $p=.253$ ), stereotype awareness scale ( $\beta=-.086$ ,  $t=-.658$ ,  $p=.513$ ), or stereotype belief scale ( $\beta=.158$ ,  $t=.631$ ,  $p=.530$ ). The effect of diagnostic salience on PIE scores did not differ for participants with higher levels of domain identification ( $\beta=-.11$ ,  $t=-.544$ ,  $p=.589$ ). Stronger identification with young voters resulted in a marginal change in the effect of diagnostic salience on PIE scores ( $\beta=-.599$ ,  $t=-1.95$ ,  $p=.057$ , See Table 9). Increased awareness of the stereotype did not affect the relationship between diagnostic salience and PIE scores ( $\beta=.111$ ,  $t=.625$ ,  $p=.535$ ), but higher self-reported belief in the stereotype did have a marginal effect on the relationship ( $\beta=.612$ ,  $t=1.92$ ,  $p=.060$ , See Table 10 ).

Table 9

*Summary of Regression Analysis for PIE Scores (N=62)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	7.74	2.24		3.45	.000
Diagnostic Salience Alone	5.74	(3.05)	.842	1.88	.065
Group Score	.485	(.229)	.401	2.12	.039
Diagnostic*Group	-.599	(.308)	-.926	-1.95	.057
R <sup>2</sup> =0.076					

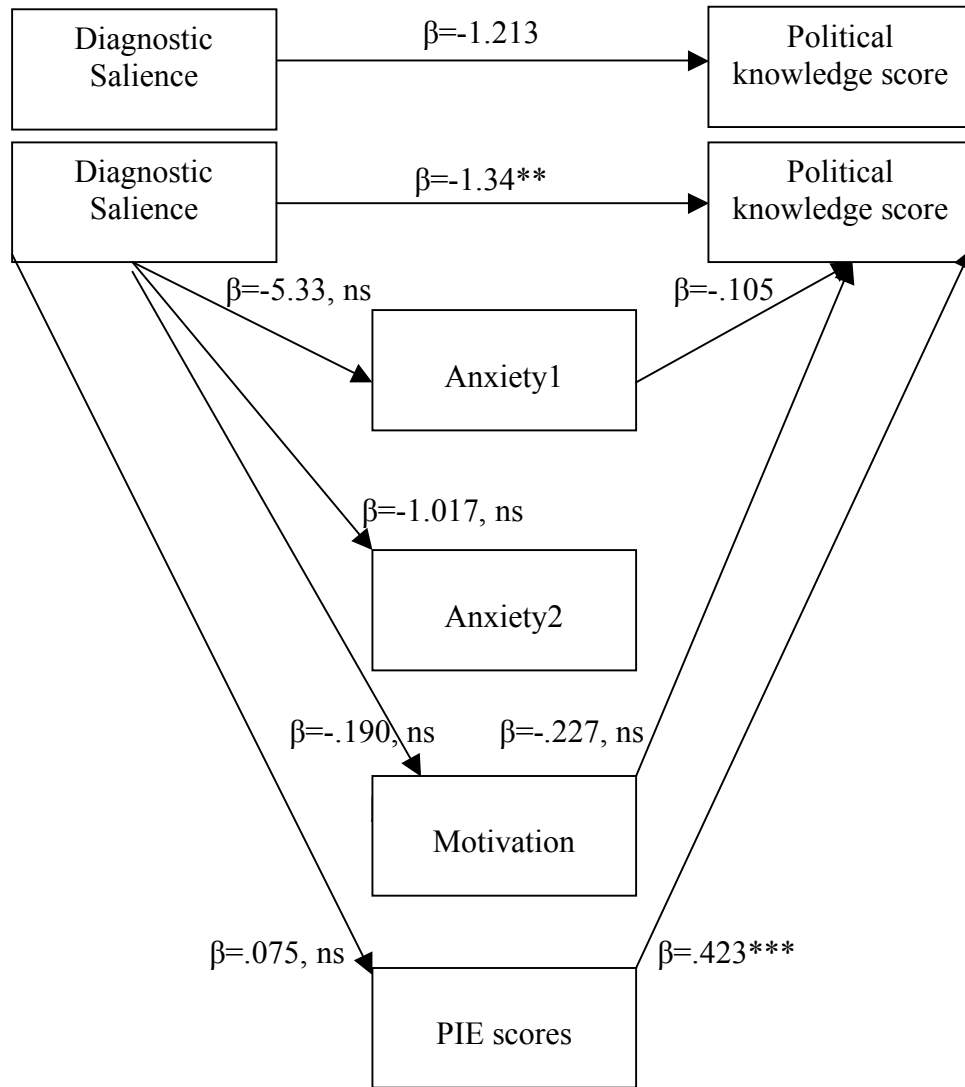
Table 10

*Summary of Regression Analysis for PIE Scores (N=62)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	16.87	2.51		6.71	.000
Diagnostic Salience Alone	-6.15	(3.35)	-.903	-1.84	.072
Belief Score	-.448	(.239)	-.359	-1.88	.066
Diagnostic*Belief	.612	(.319)	.981	1.92	.060
R <sup>2</sup> =0.066					

Hypothesis 3 stated that the presence of both independent variables would result in the strongest decreases in the dependent variables. While a review of Table 1 clearly indicates that Hypothesis 3 is not supported (participants receiving both treatments averaged higher scores on both dependent measures than participants receiving only one of the two treatments), the data were reviewed to see if the two independent variables interacted with one another to influence PIE or political knowledge scores. Scores on the dependent measures were compared between participants receiving both treatments and those receiving only one or neither treatment.

Figure 4: Analysis of relationship between diagnostic salience, anxiety, motivation, and political knowledge scores



\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

First, scores on the PIE and political knowledge measures were compared between participants in the age salience only condition to the age/diagnostic condition. Adding diagnostic salience did not result in the predicted decrease in PIE scores ( $\beta = .455, t = .569, p = .571$ ) or political knowledge scores ( $\beta = .039, t = .073, p = .942$ ). Adding diagnostic salience to age salience did not result in an increase in the established anxiety scale (Mattson, 1960) ( $\beta = .055, t = .031, p = .976$ ), or new anxiety scale ( $\beta = .885, t = .863, p = .391$ ). However, the

addition of diagnostic salience did increase the motivation of the participants significantly, ( $\beta=1.33$ ,  $t=2.44$ ,  $p=.017$ ; See Table 11).

Table 11

*Summary of Regression Analysis for Motivation Measure (N=68)*

VARIABLES	B	SE( $\beta$ )	$\beta$	$t$	Sig.( $p$ )
(Constant)	8.67	.392		22.09	.000
Adding Diagnostic Salience to Age	1.33	(.547)	.287	2.44	.017
R <sup>2</sup> =0.083					

The increase in motivation caused by adding diagnostic salience did not translate into differences in scores on the PIE or political knowledge measures. The presence of the mediating variables in the model did not change the effect of adding diagnostic salience to participants in the age salient conditions (See Tables 12 and 13).

Next, this analysis compared the scores of participants in the diagnostic salience alone condition and the age/diagnostic condition. What was the impact of having had ones' age made salient on the effect of diagnostic salience? Participants asked to select their age before being told that the test was diagnostic indicated slightly higher PIE scores than participants not first asked to select their age ( $\beta=1.11$ ,  $t=1.37$ ,  $p=.175$ ) and performed significantly better on the political knowledge test ( $\beta=1.67$ ,  $t=2.46$ ,  $p=.016$ ; See Table 14).

The negative effect of diagnostic salience was significantly stronger in the absence of the age salience manipulation. In other words, participants in the condition with both treatments performed significantly better on the political knowledge test than participants in the diagnostic treatment alone condition. Therefore, a closer examination of the scores on the mediating variables in these two groups was warranted.

Table 12

*Summary of Multiple Regression Analysis for PIE Scores (N=68)*

VARIABLES	a	b	c	d	e
(Constant)	13.03***	11.19***	16.58***	15.51***	13.73***
Adding Diagnostic Salience to Age	.455	.172	.467	.549	-.033
Motivation		.213			.376
Anxiety1Score			-.209***		-.233***
Anxiety2Score				-.106	-
R <sup>2</sup> =.29					

Note: Model a contains age only, b contains age and motivation, c contains age and anx1, d contains age and anx2, e contains age, motivation, and anx1.

\*p<.05; \*\*p<.01; \*\*\*p<.001

Table 13

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=68)*

VARIABLES	a	b	c	d	e	f
(Constant)	10.82***	11.83***	13.33***	14.25***	5.59***	8.710***
Adding Diagnostic Salience to Age	.039	.194	.047	.168	-.144	.076
Motivation		-.116				-.143
Anxiety1Score			-.147***			-.067*
Anxiety2Score				-.146*		-
PIE_Score					.400***	.344***
R <sup>2</sup> =.44						

Note: Model a contains diagnostic only, b contains diagnostic and motivation, c contains diagnostic and anx1, d contains diagnostic and anx2, e contains diagnostic and PIE score, f contains diagnostic, motivation, anx1, and PIE score.

\*p<.05; \*\*p<.01; \*\*\*p<.001

Table 14

*Summary of Regression Analysis for Political Knowledge Scores (N=67)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	9.19	.490		18.76	.000
Adding Age Salience to Diagnostic	1.67	(.678)	.292	2.46	.016
$R^2=0.085$					

Adding age salience to diagnostic salience did not result in the differences on the established anxiety scale ( $\beta=-.914$ ,  $t=-.523$ ,  $p=.603$ ), new anxiety scale ( $\beta=.650$ ,  $t=.555$ ,  $p=.581$ ), or motivation scale ( $\beta=.656$ ,  $t=1.39$ ,  $p=.168$ ). The addition of the mediators to the model did not change the effect of adding age salience to participants in the diagnostic condition on PIE scores (Table 15) or reduce the effect on political knowledge scores (Table 15). Therefore, while participants in the age salience/diagnostic salience condition performed significantly better on the political knowledge test than participants in the no age salience/diagnostic condition, this difference is unaccounted for by the mediating variables. Results indicate that there is an effect of having both manipulations as opposed to either in isolation. However, the data show that rather than working together to decrease performance or increase anxiety and motivation, the presence of age salience removed the negative effect of diagnostic salience on scores on the political knowledge test.

Next, an omnibus regression model with both independent variables and the mediating variables was run. As Table 17 indicates, participants in the diagnostic alone condition performed significantly worse on the political knowledge test even when the other factors were included in the model.



Table 15

*Summary of Multiple Regression Analysis for PIE Scores (N=67)*

VARIABLES	a	b	c	d	e
(Constant)	12.38***	10.17***	16.04***	14.46***	12.96***
Adding Age	1.11	.956	.924	1.17	.677
Salience to Diagnostic Motivation		.235			-.357
Anxiety1Score			-.204***		-.218***
Anxiety2Score				-.088	-
R <sup>2</sup> =.40					

Note: Model a contains age only, b contains age and motivation, c contains age and anx1, d contains age and anx2, e contains age, motivation, and anx1.

\*p<.05; \*\*p<.01; \*\*\*p<.001

Table 16

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=67)*

VARIABLES	a	b	c	d	e	f
(Constant)	9.19***	11.24***	13.24***	13.25***	2.09*	7.79***
Adding Age	1.67*	1.81*	1.46*	1.78**	1.03*	1.20**
Salience to Diagnostic Motivation		-.220				-.270*
Anxiety1Score			-.226***			-.116**
Anxiety2Score				-.171*		-
PIE_Score					.573***	.485***
R <sup>2</sup> =.63						

Note: Model a contains age only, b contains age and motivation, c contains age and anx1, d contains age and anx2, e contains age and PIE score, f contains age, motivation, anx1 and PIE score.

\*p<.05; \*\*p<.01; \*\*\*p<.001

Next, the data were analyzed to determine, using the measured variables, the best model to predict political knowledge scores. A four variable model proved to be the strongest and most parsimonious predictor of political knowledge scores. Just under half of the variance in political knowledge scores could be accounted for by including PIE scores, anxiety scores, motivation scores, and domain identification scores. Participants with higher

PIE and domain scores and lower anxiety and motivation scores performed better on the political knowledge test. Finally, a summary of hypotheses and findings for Experiment 1 is found in Table 19.

Table 17

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=130)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	8.76	1.09		8.05	.000
Age_alone	-.135	(.462)	-.024	-.292	.771
Diagnostic_alone	-1.32	(.458)	-.230	-2.88	.005
Age*Diagnostic	-.037	(.453)	-.007	-.081	.936
PIE Score	.387	(.053)	.527	7.26	.000
Anxiety1Score	-.080	(.026)	-.225	-3.09	.002
Motivation	-.173	(.083)	-.145	-2.09	.038
$R^2=0.497$					

Table 18

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=130)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	7.48	1.10		6.80	.000
PIE Score	.299	(.068)	.407	4.37	.000
Anxiety1Score	-.065	(.027)	-.183	-2.41	.018
Motivation	-.218	(.085)	-.183	-2.58	.011
Domain Score	.151	(.064)	.225	2.34	.020
$R^2=0.472$					

Table 19

## Summary of Hypotheses for Experiment 1

Hypothesis		Table 1	Table 4	Table 5	Tables 6, 17	Table 7	No Table
H1a	Making the age group of young voters salient prior to a pk test will negatively affect PIE and pk scores	-					
H1b	Anxiety and motivation will mediate the effects of age salience on PIE and performance on the pk test.		-	-			
H1c	PIE will mediate the effects of age salience on the pk test			-			
H1d	Belief in the stereotype, awareness of the stereotype, and group and domain identification will moderate the influence of age salience on PIE and pk scores.						-
H2a	Describing the test as diagnostic will negatively affect PIE and pk scores				+ for pk		
H2b	Anxiety and motivation will mediate the effects of the diagnostic salience manipulation on PIE and performance on the pk test.					-	
H2c	PIE will mediate the effects of diagnostic salience on the pk test.					-	
H2d	Belief in the stereotype, awareness of the stereotype, and group and domain identification will moderate the influence of diagnostic salience on PIE scores and pk performance.						-
H3	The presence of both the age salience and diagnostic salience manipulations together will affect attitudes and performance more than either independent variable in isolation.	-					

+ = supported, +? = limited support, - no support

## **CHAPTER IV**

### **EXPERIMENT 1 DISCUSSION**

Experiment 1 sought to determine if young voters' attitudes or performance on a political knowledge test could be affected by stereotype threat, induced by making group membership and test diagnosticity salient. Results for experiment 1 indicate that for this sample of participants making age salient did not result in the predicted decrease in scores on the political knowledge or PIE measures. Nor did age salience result in an increase in any of the mediating variables. In fact, when participants in the diagnostic condition (who performed worse on the political knowledge test than the control group) were first asked their age, the effect of diagnostic salience disappeared. It appears that the tests' purported power to reliably find differences between groups did affect performance, though not through the mediators explored in this study, and not in combination with increased age salience. It is likely that some other aspect of the participants' identity (e.g., their race or gender) was threatened in the diagnostic condition, though this is merely speculation, as strength of identification with these groups was not measured. It does appear, however, that whereas making the test's diagnostic nature salient after making age salient did not affect performance, the presence of age salience removed the negative effect of diagnostic salience.

There are at least two ways to interpret these results. First, for the participants involved, age salience may not have served as a symbol of personal weakness with regard to political knowledge. On the other hand, the strength of the age salience manipulation may

have been too weak to find the hypothesized effects. The second experiment sought to determine if exposing young voters to more explicit threatening information about their age group had different effects on their performance and attitudes.

## CHAPTER V

### EXPERIMENT 2 METHOD

Experiment 2 asked, “what is the relationship between exposure to disparaging political statements or political humor on political information efficacy and performance on a political knowledge test?” This experiment used a 2x2 between subjects factorial design with 1 control group. The manipulated independent variables were humor expectancy (Jon Stewart vs. *Minneapolis Star Tribune*<sup>3</sup>) and humorousness of content (humorous vs. non-humorous). Participants ( $N=150$ ) were randomly assigned to one of four experimental groups or the control group.

As with experiment 1, subjects participated in this study using a computer in a lab. On the screen of each computer was a Microsoft Word document with the letters E, F, G, H or I in blue letters. Clicking on any of the letters directed the subject to one of the five conditions. Each of the participants was given an informed consent document upon entering the lab with a randomly assigned letter written prominently on top of the page.. Upon clicking the letter on their computer screen, all participants read the following message: “Thank you for participating in this research. You have been assigned to a study about politics and political knowledge. To proceed, please click the forward arrow.”

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<sup>3</sup> *The Minneapolis Star Tribune* is a nationally recognizable name yet it does not have with it the “baggage” of being associated with a political philosophy or any memorable scandals (e.g. *The New York Times*).

Participants assigned to the humor expectancy conditions read the following statement: “Before you begin the survey, please read this recent quote from Jon Stewart, host of *The Daily Show* on Comedy Central.” Participants in the no expectation of humor condition read, “Before you begin the survey, please read this recent quote from David Jennings, journalist for *The Minneapolis Star Tribune*.” The control group was not made to expect a quote nor read a quote prior to taking the political knowledge test. (See table below).

Experiment 2 Design

	Humorous	Non-humorous
Jon Stewart (Humor expectancy)	Expect+Receive	Expect+Don’t receive
<i>Minneapolis Star Tribune</i>	Don’t Expect+ Receive	Don’t Expect+Don’t Receive

Participants then read either a humorous or non-humorous disparaging quote about young voters purportedly from the source. This experiment involved the creation of two “content equivalent” quotes disparaging young people for their stereotypically low levels of political knowledge and involvement. This procedure is widely used in humor and advertising research and is becoming more popular in political communication research as well (e.g., Nabi et al., 2007).

The non-humorous version of the disparaging statement read as follows:

“When it comes to politics, young people have no clue what’s going on, they’ve got no actual opinion, they’ve never affected the outcome, and they’d rather be sleeping.”

The humorous version of the statement read:

“When it comes to politics, young people are like Punxsutawney Phil on Groundhog’s Day. They have no clue what’s going on, they’ve got no actual opinion, they’ve never affected the outcome, and they’d rather be sleeping.”

A pre-test was run on this quote with 40 undergraduate students. Participants were told they were reading a quote from Jon Stewart. Results indicate that participants found the humorous statement to be significantly more humorous ( $M=5.35/9$ ) than the non-humorous statement ( $M=3.33/9$ ),  $F(1, 39)=11.345$ ,  $p=.002$ . This indicates that humor expectancy might have limited ability to influence perceptions of humorousness, although a comparison with a source other than Jon Stewart was not conducted. The post-test of the present experiment included manipulation checks of humor expectancy (Appendix G) and statement humorousness (Appendix F).

After reading the quote, participants read the following statement, “Next, we’d like to ask you some political knowledge questions.” Participants then took the political knowledge test, followed by the PIE scale as in Experiment 1.

In addition to the mediators from experiment 1, a new factor, statement counterarguing, was included in the analysis. Counterarguing of the message was assessed with a set of six items adapted from research by Nabi et al. (2007). Participants were asked to indicate the degree to which they agreed or disagreed with statements such as, “I thought of reasons why what Jon Stewart/David Jennings said was wrong,” and “The statement popped into my head while I was taking the test.” (See Appendix D). Greater counterarguing was hypothesized to lead to inhibited performance.

As with the first experiment, participants then completed the measures of individual difference variables, were thanked for their participation and debriefed before leaving the lab.



## **CHAPTER VI**

### **EXPERIMENT 2 RESULTS**

#### **Sample**

One hundred and fifty subjects participated in experiment 2. Participants ranged in age from 18 to 24, with a mean age of 20.62. The sample was largely female (79%). Participants identified themselves as Caucasian (84%), Black (5%), Hispanic (5%), Asian (4%), and of mixed race (2%). Most participants identified themselves as Democrats (42%), followed by the Republicans (31%), Independent (14%), Other (6%), Libertarians (5%), Green (1%) and Socialists (1%).

#### **Scale Reliability**

The political knowledge scale showed moderate reliability, Cronbach's  $\alpha = .652$ . The PIE scale exhibited strong reliability ( $\alpha = .814$ ). The established anxiety scale (Mattson, 1960, Anx1) was highly reliable ( $\alpha = .901$ ), while the measure created for this study (Anx2) was weaker but still strong ( $\alpha = .747$ ). The scale for motivation exhibited very low reliability ( $\alpha = .474$ ). As with the first experiment, three items on that scale were highly correlated and served as the basis of a more reliable scale ( $\alpha = .742$ ). These were, "I felt the need to counter the stereotype that young voters are uninformed about politics," "I tried to counter the stereotype that young voters are uninformed about politics," and "When I didn't know the answer to a question, I tried harder on the next one." The counterarguing scale exhibited weak reliability (.393). A two-item measure was constructed using "I actively disagreed with

what Jon Stewart/David Jennings said,” and “I thought of reasons why what Jon Stewart/David Jennings said was wrong.” This scale exhibited a reliability of .729.

Factor analyses were subsequently conducted to further determine whether the scales used for the mediating variables were uni-dimensional. As theorized, each of the mediating variables was found to load on a single factor with an eigenvalue greater than one. Furthermore, an omnibus factor analysis of all of the questions used to measure the mediating variables produced five distinct factors, corresponding well to the five proposed mediators (PIE, anxiety1, anxiety2, motivation, counterarguing).

The belief in the stereotype scale was the least reliable ( $\alpha=.533$ ). The stereotype awareness scale was reliable ( $\alpha=.728$ ), as was the group identification scale,  $\alpha=.824$ ). The domain identification scale was reliable ( $\alpha=.799$ ), as was the established index of overall efficacy ( $\alpha=.835$ ).

### **Manipulation Checks**

To ensure that the manipulations of humor expectancy and stimulus humorousness did work, several analyses were run. First, towards the end of the experiment, participants were asked to recall the quote to the best of their ability. Three subjects were unable to recall the quote at all, and were eliminated from the subsequent analysis.

Participants in the humorous/expected condition reported that the quote was somewhat funny ( $M=4.38/9$ ). This is quite lower than it was in the initial pre-test ( $M=5.35$ ). It is conceivable that this difference comes from the fact that while in the pre-test, participants were simply asked to read and rate the quote; in the present experiment, participants rated the quote after a long series of political knowledge and attitude questions. Perception of the quote’s humorousness may have been as high during the taking of the

political knowledge test, which immediately followed the manipulation, but trailed off as the experiment went on. Participants in the humorous/unexpected condition scored the quote as a little funny ( $M=3.59/9$ ). In the non-humorous conditions, the quote was rated 3.27 when humor was expected and 2.50 when unexpected. In summary, participants in the humorous conditions rated the quote as significantly funnier ( $M=3.98$ ) than participants in the non-humorous conditions ( $M=2.88$ ), ( $\beta=1.09$ ,  $t=3.37$ ,  $p=.001$ ). Participants in the humor expectancy condition also rated the quote as funnier ( $M=3.81$ ) than participants in the no expectation conditions ( $M=3.03$ ), ( $\beta=.780$ ,  $t=2.34$ ,  $p=.021$ ). However, there was not a significant interaction between the two independent variables and perceived humorousness.

As another check, participants were asked how clever they considered the quote. The results mirror those of the humorousness check, with slightly higher means. The tables below show the effect of the two conditions combined on the perceptions of humorousness and cleverness.

Table 20

*Summary of Multiple Regression Analysis for How Funny (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	$t$	Sig.( $p$ )
(Constant)	2.49	.275		9.08	.000
Humorous	1.09	(.319)	.299	3.45	.001
Disparagement					
Humor Expected	.780	(.319)	.212	2.44	.016
$R^2=0.134$					

There was no difference in how complicated or informative the quote was considered in the conditions. However, there was a marginally significant interaction between the two independent variables on perceptions of how confusing the quote was (See Table 22). Participants found the non-humorous quote from the *MST* more confusing than the humorous

quote from Jon Stewart. Also, the expectation of humor was associated with a significant decrease in the perceived negativity of the quote. Participants in the humor expectancy conditions considered the quote less negative than participants in the no expectation conditions (Table 23).

Table 21

*Summary of Multiple Regression Analysis for How Clever (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	2.69	.296		9.11	.000
Humorous	1.23	(.344)	.305	3.58	.001
Disparagement					
Humor Expected	1.10	(.344)	.272	3.2	.002
$R^2=0.167$					

Table 22

*Summary of Multiple Regression Analysis for How Confusing (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	1.67	.208		8.01	.000
Humorous	.333	(.297)	.146	1.12	.264
Disparagement					
Humor Expected	.133	(.294)	.058	.45	.651
Humorous*Expect	-.789	(.419)	-.297	-1.88	.063
$R^2=0.427$					

Table 23

*Summary of Multiple Regression Analysis for How Negative (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	6.91	.297		23.23	.000
Humorous	-.335	(.345)	-.085	-.97	.335
Disparagement					
Humor Expected	-1.22	(.345)	-.312	-3.53	.001
$R^2=0.105$					

Altogether, the humorous stimulus was rated as significantly funnier than the non-humorous stimulus. The non-humorous quote in the *MST* condition was rated as more confusing than the quote in the humorous/expected condition. Participants expecting humor rated the quote as less negative than participants not expecting humor.

The next manipulation check focused on whether stating the source of the quote influenced participants' mindsets before reading the quote. A humor expectancy scale with strong reliability ( $\alpha=.783$ ) was created using the following items: "I expected the quote to be funny," "I expected to have to work through a joke," "I was looking forward to reading the quote," "I assumed I would enjoy the quote," and "I expected the quote to be lighthearted." Participants in the expectation conditions reported expecting humor significantly more than participants in the no expectation conditions, ( $\beta=3.89$ ,  $t=8.06$ ,  $p<.001$ ; See Table 24).

Table 24

*Summary of Regression Analysis for Humor Expectancy Score (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	12.88	.342		37.67	.000
Jon Stewart (Humor Expected)	3.89	(.484)	.599	8.06	.000
$R^2=0.359$					

There was a marginally significant negative interaction between humor and expectancy on the humor expectancy score (See Table 25). Participants in the humor expectancy condition that did not get humor reported expecting humor more than participants that did get humor. Participants in the no expectation condition that did not get humor reported expecting humor less than participants given humor.

Table 25

*Summary of Multiple Regression Analysis for Humor Expectancy (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	12.3	.476		25.86	.000
Humorous	1.18	(.678)	.182	1.74	.084
Disparagement					
Humor Expected	4.8	(.673)	.738	7.14	.000
Humorous*Expect	-1.84	(.959)	-.243	-1.91	.058
$R^2=0.381$					

Altogether, participants told that Jon Stewart was the source of the forthcoming quote indicated significantly higher levels of humor expectancy. Participants reported expecting humor slightly more when it was expected but not received or unexpected and received.

**Descriptive Statistics**

Tables 26, 27, and 28 display the means and standard deviations of the groups' scores on each of the dependent, mediating, and moderating variables, respectively.

Table 26

*Means and Standard Deviations on Dependent Variables for each Condition*

	PK (out of 15)	PIE (out of 20)
Humor/Expect (N=29)	10.52, 1.88	12.86, 2.63
Humor/No Expect (N=29)	9.62, 2.51	12.55, 3.69
No Humor/No Expect (N=30)	10.53, 1.93	12.83, 2.89
No Humor/Expect (N=30)	9.37, 2.34	12.77, 2.81
Control (N=32)	10.53, 2.50	13.28, 3.28
Total (N=150)	10.12, 2.28	12.87, 3.05

Table 27

*Means and Standard Deviations on Mediating Variables for each Condition*

	Anxiety 1 (out of 35)	Anxiety 2 (out of 40)	Motivation (out of 15)	Counterarguing (out of 10)
Humor/Expect (N=29)	19.48, 6.69	25.48, 4.52	10.17, 2.00	6.14, 2.13
Humor/No Expect (N=29)	18.24, 6.27	25.10, 4.59	9.62, 2.87	6.79, 1.69
No Humor/No Expect (N=30)	19.63, 5.97	25.60, 4.46	10.07, 2.39	6.73, 1.41
No Humor/Expect (N=30)	17.57, 7.55	24.60, 5.00	10.00, 1.93	6.33, 1.85
Control (N=32)	18.97, 6.93	23.16, 4.03	9.39, 2.08	-
Total (N=150)	18.78, 6.66	24.76, 4.55	9.95, 2.31	6.50, 1.79

Table 28

*Means and Standard Deviations on Moderating Variables for each Condition*

	Awareness (out of 50)	Belief (out of 17)	Domain (out of 25)	Group (out of 20)	Efficacy (out of 40)
Humor/Expect (N=29)	29.76, 5.20	10.07, 2.67	15.07, 3.24	9.48, 3.65	33.31, 3.52
Humor/No Expect (N=29)	30.07, 4.59	10.00, 2.41	14.59, 3.77	9.66, 3.29	32.52, 3.25
No Humor/No Expect (N=30)	29.97, 5.89	10.23, 2.25	14.80, 3.81	9.37, 2.95	31.90, 3.36
No Humor/Expect (N=30)	30.07, 5.02	10.07, 1.99	14.57, 3.99	9.37, 2.79	32.17, 4.19
Control (N=32)	29.75, 5.09	10.47, 2.82	14.84, 3.72	9.69, 3.04	32.53, 3.42
Total (N=150)	29.92, 5.11	10.17, 2.42	14.77, 3.67	9.51, 3.12	32.38, 3.54

**Hypothesis Testing**

Figures 5 and 6 display the means of the four experimental conditions on measures of political knowledge and PIE, respectively.

Figure 5: Graph of mean political knowledge scores by condition

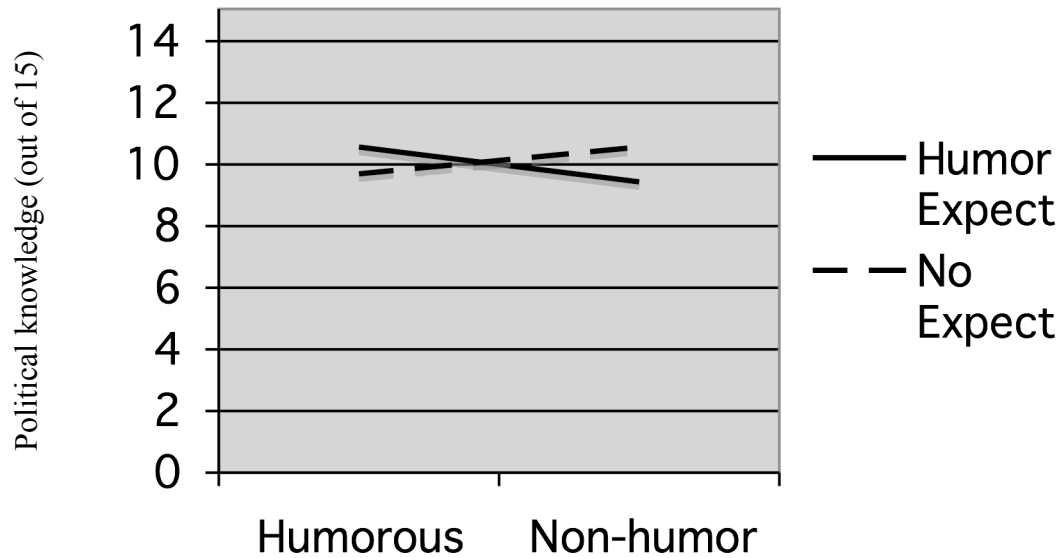
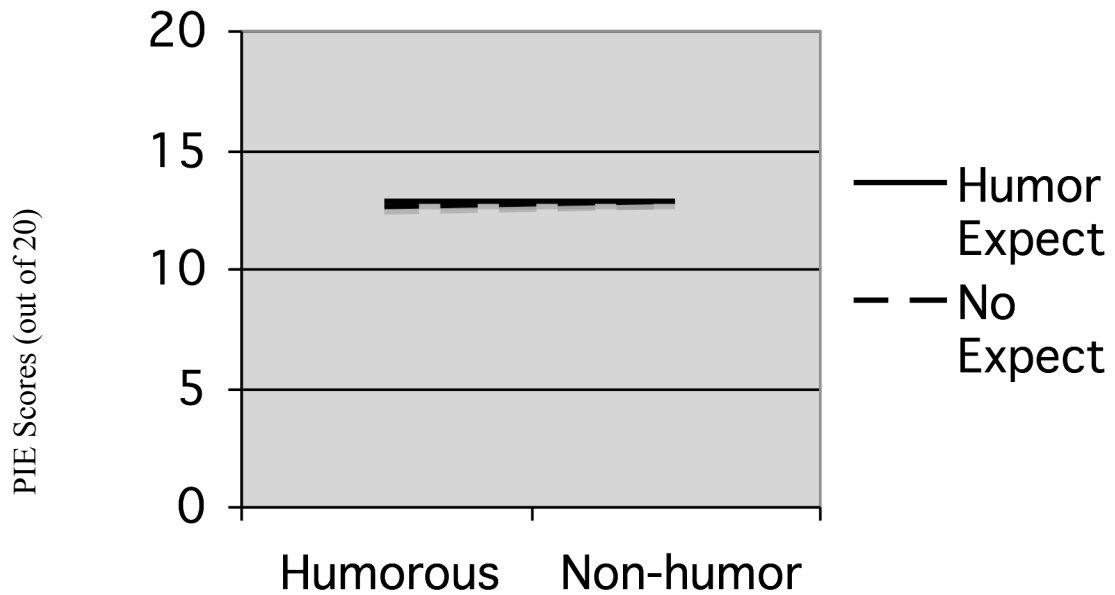


Figure 6: Graph of mean PIE scores by condition



Hypothesis 4 stated that exposure to disparaging political humor would have a greater negative effect on political information efficacy and performance on the political knowledge test than exposure to non-humorous disparagement. Hypothesis 4 was not supported.



To begin, scores were compared between participants exposed to either form of disparagement and the control group. First, political knowledge and PIE were compared between participants in the humorous disparagement conditions and the control group. Results indicate exposure to humorous disparagement resulted in a non-significant decrease in scores on the political knowledge test ( $\beta = -.462$ ,  $t = -.897$ ,  $p = .372$ ) and PIE ( $\beta = -.574$ ,  $t = -.811$ ,  $p = .420$ ) as compared to the control group. Next, scores were compared between participants in the non-humorous conditions and the control group. Results indicate that exposure to non-humorous disparagement also resulted in a non-significant decrease in scores on the political knowledge test ( $\beta = -.581$ ,  $t = -1.15$ ,  $p = .254$ ) and PIE ( $\beta = -.481$ ,  $t = -.736$ ,  $p = .464$ ) as compared to the control group. Participants in the humorous and non-humorous disparagement conditions all scored lower on the political knowledge and PIE measures than the control group, though these effects were not significant.

Next, scores were compared between participants in the humorous and the non-humorous conditions. Results indicate that there was no statistically significant difference between the political knowledge scores of participants based on this variable alone ( $\beta = .119$ ,  $t = .290$ ,  $p = .772$ ). Similarly, there was no significant difference in political information efficacy between participants exposed to humorous versus non-humorous disparagement ( $\beta = -.093$ ,  $t = -.168$ ,  $p = .867$ ).

Hypothesis 5 stated that participants in the humor expectancy conditions would indicate greater political information efficacy and perform better on the political knowledge test than participants in the no expectation conditions. Hypothesis 5 was not supported. First, scores were compared between participants made to expect humor and the control group. Results indicate humor expectancy resulted in a non-significant decrease in scores on the

political knowledge test ( $\beta=-.599$ ,  $t=-1.19$ ,  $p=.239$ ) and PIE ( $\beta=-.468$ ,  $t=-.731$ ,  $p=.467$ ) as compared to the control group.

Next, scores were compared between participants told that “David Jennings” was the source of the forthcoming quote (no expectation) and the control group. Results indicate participants not expecting humor performed slightly worse on scores on the political knowledge test ( $\beta=-.447$ ,  $t=-.866$ ,  $p=.389$ ) and PIE ( $\beta=-.586$ ,  $t=-.814$ ,  $p=.418$ ) as compared to the control group, although these differences were not significant.

Participants in the humor expectation and no expectation conditions all scored lower on the political knowledge and PIE measures than the control group, though these effects were not significant. Next, scores were compared between participants made to expect humor and those not expecting humor. Results indicate that participants in the expectation of humor conditions performed virtually the same as participants not made to expect humor ( $\beta=-.153$ ,  $t=-.372$ ,  $p=.710$ ) and indicated similar attitudes as well ( $\beta=.119$ ,  $t=.214$ ,  $p=.831$ ).

To test whether the effects of humor expectancy or exposure to humorous or non-humorous disparagement were mediated (Hypotheses 6 and 7) scores on the mediating variables were also compared between these groups. It was previously shown that the expectation of or exposure to disparaging humor or statements did not affect PIE, so Hypothesis 7 was rejected. Hypothesis 6 was also rejected. Results indicate that participants exposed to disparaging political humor scored no different on the first anxiety measure ( $\beta=-.107$ ,  $t=-.073$ ,  $p=.942$ ) or motivation measure ( $\beta=.022$ ,  $t=.040$ ,  $p=.968$ ) than the control group, but did report significantly more anxiety using the second measure (See Table 29).

Table 29

*Summary of Regression Analysis for Anxiety2 Scores (N=90)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	23.16	.769		30.10	.000
Humorous Disparagement	2.14	(.958)	.231	2.23	.028
$R^2=0.053$					

Similarly, participants exposed to non-humorous political disparagement scored no different on the first anxiety measure ( $\beta=-.369$ ,  $t=-.245$ ,  $p=.807$ ) or motivation measure ( $\beta=-.158$ ,  $t=.325$ ,  $p=.746$ ), but did report significantly more anxiety using the second measure than the control group (See Table 30).

Table 30

*Summary of Regression Analysis for Anxiety2 Scores (N=92)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	23.16	.795		29.12	.000
Non-humorous Disparagement	1.94	(.985)	.204	1.97	.051
$R^2=0.041$					

Participants in the humorous disparagement conditions did not report significantly different scores on the anxiety measures (Anx1,  $\beta=.262$ ,  $t=.214$ ,  $p=.831$ , Anx2,  $\beta=.193$ ,  $t=.227$ ,  $p=.821$ ) motivation measure ( $\beta=-.137$ ,  $t=-.321$ ,  $p=.749$ ) or counterarguing measure ( $\beta=-.068$ ,  $t=-.205$ ,  $p=.838$ ) than participants in the non-humorous disparagement conditions.

Participants made to expect humor scored no different on the first anxiety measure ( $\beta=-.460$ ,  $t=-.297$ ,  $p=.767$ ) or motivation measure ( $\beta=.210$ ,  $t=.455$ ,  $p=.650$ ) than the control group, but did report a nearly significant increase in anxiety using the second measure (See Table 31).

Table 31

*Summary of Regression Analysis for Anxiety2 Scores (N=91)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	23.16	.798		29.03	.000
Humor Expected	1.88	(.991)	.197	1.89	.061
$R^2=0.039$					

Results indicate that participants in the no expectation conditions scored no different on the first anxiety measure ( $\beta=-.020$ ,  $t=-.014$ ,  $p=.989$ ) or motivation measure ( $\beta=-.028$ ,  $t=-.050$ ,  $p=.961$ ) than the control group, but did report a significant increase in anxiety using the second measure (See Table 32).

Table 32

*Summary of Regression Analysis for Anxiety2 Scores (N=91)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	23.16	.767		30.21	.000
No Expectation of Humor	2.20	(.952)	.238	2.31	.023
$R^2=0.057$					

Participants in the humor expectancy conditions did not report significantly different scores on the anxiety measures (Anx1,  $\beta=-.441$ ,  $t=.360$ ,  $p=.719$ , Anx2,  $\beta=-.322$ ,  $t=-.378$ ,  $p=.706$ ) motivation measure ( $\beta=.237$ ,  $t=.557$ ,  $p=.578$ ) or counterarguing measure ( $\beta=-.525$ ,  $t=-1.61$ ,  $p=.111$ ) than participants in the no expectation conditions.

Seeing as the independent variables all affected Anxiety2 scores, two sets of regression equations were run to test whether the effects of expecting or experiencing humorous or non-humorous disparagement were mediated by anxiety. Tables 33 and 34 show that anxiety did not mediate or change the effects of exposure to disparaging humor versus disparaging political statements on political knowledge scores or PIE.

Table 33

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	12.25	1.14		10.73	.000
Humorous	.137	(.404)	.031	.338	.736
Disparagement					
Anxiety2	-.092	(.044)	-.190	-2.08	.040
$R^2=0.037$					

Table 34

*Summary of Multiple Regression Analysis for PIE Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	13.30	1.57		8.47	.000
Humorous	-.089	(.556)	-.015	-.160	.873
Disparagement					
Anxiety2	-.020	(.061)	-.031	-.331	.741
$R^2=0.001$					

Tables 35 and 36 indicate that the addition of anxiety did not affect the difference between participants in the expectation of humor conditions and no expectation conditions. Therefore, while anxiety was affected the independent variables, these effects did not influence political knowledge or PIE scores.

Hypothesis 8 predicted that the effect of humorous disparagement on political knowledge would be different for participants along dimensions of the moderator variables. Hypothesis 8 was not supported. Results indicate that no difference was found for participants exposed to humorous disparagement along levels of the measures of stereotype belief ( $\beta=.182$ ,  $t=.941$ ,  $p=.349$ ), stereotype awareness ( $\beta=-.045$ ,  $t=-.429$ ,  $p=.669$ ), group identification ( $\beta=-.214$ ,  $t=-1.30$ ,  $p=.197$ ), domain identification ( $\beta=-.048$ ,  $t=-.377$ ,  $p=.707$ ) or efficacy ( $\beta=.033$ ,  $t=.214$ ,  $p=.831$ ).

Table 35

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	12.42	1.15		10.76	.000
Humor Expected	-.182	(.404)	-.041	-.451	.653
Anxiety2	-.092	(.044)	-.191	-2.09	.039
$R^2=0.036$					

Table 36

*Summary of Multiple Regression Analysis for PIE Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	13.19	1.58		8.31	.000
Humor Expected	.112	(.556)	.019	.202	.840
Anxiety2	-.020	(.061)	-.031	-.327	.744
$R^2=0.001$					

Similarly, the effect of non-humorous disparagement on political knowledge as compared to the control group was no different for participants along levels of the measures of stereotype belief ( $\beta=.238$ ,  $t=1.18$ ,  $p=.250$ ), stereotype awareness ( $\beta=-.079$ ,  $t=-.805$ ,  $p=.423$ ), group identification ( $\beta=-.052$ ,  $t=-.302$ ,  $p=.763$ ), domain identification ( $\beta=-.081$ ,  $t=-.667$ ,  $p=.507$ ) or efficacy ( $\beta=-.146$ ,  $t=-1.00$ ,  $p=.320$ ).

The effect of humorous disparagement on PIE was no different for participants along dimensions of stereotype belief ( $\beta=.101$ ,  $t=.381$ ,  $p=.704$ ), stereotype awareness ( $\beta=.157$ ,  $t=1.09$ ,  $p=.227$ ), group identification ( $\beta=-.273$ ,  $t=-1.25$ ,  $p=.213$ ), or domain identification ( $\beta=-.099$ ,  $t=-.618$ ,  $p=.538$ ).

The effect of non-humorous disparagement on PIE was no different for participants along dimensions of the stereotype belief measure ( $\beta=.341$ ,  $t=1.29$ ,  $p=.200$ ), stereotype awareness measure ( $\beta=.155$ ,  $t=1.21$ ,  $p=.228$ ), or group identification measure ( $\beta=-.153$ ,  $t=-$

.731,  $p=.467$ ), but was different depending on how strongly participants identified politics as an important domain (See Table 37).

Table 37

*Summary of Multiple Regression Analysis for PIE Scores (N=92)*

VARIABLES	B	SE( $\beta$ )	$\beta$	$t$	Sig.( $p$ )
(Constant)	4.39	1.90		2.31	.023
Non-humorous disparagement Domain Score	4.36	(2.31)	.701	1.89	.063
Non-humor*Domain	-.599	(.124)	.762	4.81	.000
	-.323	(.152)	-.834	-2.13	.036
$R^2=0.279$					

Hypothesis 9 predicted an interaction effect between humor expectancy and exposure to disparaging political humor on political knowledge and PIE scores. It was hypothesized that participants would score equally well on the political knowledge and PIE measures under conditions of humor expectancy, regardless of whether the stimulus was humorous or not. When unexpectedly exposed to humor, they were predicted to perform worse and indicate lower PIE scores than when unexpectedly exposed to non-humorous disparagement. Hypothesis 9 revealed an interesting interaction of the two independent variables on political knowledge scores, though the predicted interaction was only partly supported.

First, political knowledge was regressed on both independent variables and no effect was found for either variable alone, controlling for the other (See Tables 38 and 39).

Table 38

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	10.02	.354		28.30	.000
Humorous	.119	(.411)	.027	.289	.773
Disparagement					
Humor Expected	-.153	(.411)	-.035	-.371	.711
R <sup>2</sup> =0.002					

Table 39

*Summary of Multiple Regression Analysis for PIE Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	12.74	.479		26.6	.000
Humorous	-.093	(.556)	-.016	-.167	.867
Disparagement					
Humor Expected	.119	(.556)	.020	.213	.831
R <sup>2</sup> =0.001					

Next, the dependent variables were regressed on the independent variables and the interaction term. Table 40 shows that the two independent variables did not have a significant interaction effect on PIE scores.

However, a significant interaction was found between the two independent variables and political knowledge scores (See Table 41). As Table 41 indicates, at each level of the humor expectation variable, political knowledge scores decreased for people that got humor as compared to people who did not get humor, though this difference was not significant ( $\beta = -.913$ ,  $t = -1.61$ ,  $p = .111$ ). Interestingly, the inclusion of the interaction term reveals that regardless of whether the participants received humorous or non-humorous disparagement, scores were significantly lower for people made to expect humor as compared to people not made to expect humor ( $\beta = -1.17$ ,  $t = -2.07$ ,  $p = .041$ ).



Table 40

*Summary of Multiple Regression Analysis for PIE Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	12.83	.554		23.181	.000
Humorous	-.282	(.790)	-.047	-.357	.722
Disparagement					
Humor Expected	-.067	(.783)	-.011	-.085	.932
Humorous*Expect	.377	(1.12)	.054	.338	.736
$R^2=0.002$					

Table 41

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	10.53	.398		26.5	.000
Humorous	-.913	(.568)	-.207	-1.61	.111
Disparagement					
Humor Expected	-1.17	(.563)	-.264	-2.07	.041
Humorous*Expect	2.06	(.803)	.402	2.57	.012
$R^2=0.057$					

Whereas by themselves the expectation and presence of humor, controlling for the other, resulted in significant and nearly significant decreases in political knowledge respectively, the combination of both humor expectancy and a humorous stimulus negated these decreases. In other words, although participants who expected humor performed significantly worse than participants not expecting humor, and participants exposed to humorous disparagement performed slightly worse than participants exposed to non-humorous disparagement, participants who both expected and received humor performed as well as participants not expecting humor and exposed to non-humorous disparagement, and better than those participants who either expected and did not receive humor or did not expect and did receive humor. The data indicate that the interaction counteracts the negative

effects of expecting or receiving humor. Thus, these apparent negative effects are only proxies for either (a) not receiving humor when it was expected, or (b) receiving humor when it was not expected.

Next, a full model regression was run to see if the interaction term was affected by any of the mediating variables. As Table 42 indicates, the strength of this interaction was unaffected by the inclusion of the mediators in the model.

Table 42

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=118)*

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	8.78	1.39		6.33	.000
Humorous	-.854	(.496)	-.193	-1.72	.088
Disparagement					
Humor Expected	-1.26	(.494)	-.285	-2.55	.012
Humorous*Expect	2.05	(.703)	.399	2.91	.004
PIE	.341	(.062)	.460	5.52	.000
Motivation	.020	(.095)	.020	.208	.836
Anx2	-.099	(.047)	-.205	-2.11	.037
Counterargue	-.043	(.102)	-.035	-.420	.675
$R^2=0.057$					

So, how can one explain why participants unexpectedly exposed to humorous disparagement performed slightly worse on a political knowledge test than participants unexpectedly exposed to non-humorous disparagement? Participants in the no expectation/non-humorous condition indicated they were more relaxed ( $M=4.03$ ) than participants in the no expectation/humorous condition ( $M=3.34$ ), a nearly significant difference ( $\beta=.689$ ,  $t=1.83$ ,  $p=.073$ ). Scores on one of the questions used to assess stereotype awareness were also different between these two groups. Participants in the humorous/no expectation condition reported significantly stronger disagreement with the statement “Stereotypes about young voters have not affected me personally” than participants in the

non-humorous/no expectation condition ( $\beta=.461$ ,  $t=1.87$ ,  $p=.067$ ). However feeling relaxed and that one has been personally affected did not reduce the effect of unexpected exposure to humor (See Table 43).

Table 43

*Summary of Multiple Regression Analysis for Political Knowledge Scores (N=59)*

VARIABLES	B	SE( $\beta$ )	$\beta$	$t$	Sig.( $p$ )
(Constant)	11.55	1.53		7.53	.000
Humor when Unexpected	-1.10	(.615)	-.245	1.79	.079
Relaxed	-.334	(.204)	-.218	-1.64	.108
Affected Personally	.091	(.312)	.039	.291	.772
$R^2=0.090$					

How can one explain the difference in scores between people expecting and receiving humor and expecting and not receiving humor? As compared to participants who expected and did not receive humor, participants who expected and received humor indicated that they more strongly agreed that they were relaxed, ( $\beta=.768$ ,  $t=1.70$ ,  $p=.094$ ), that they “put a lot of effort into answering the questions on the test,” ( $\beta=.321$ ,  $t=1.62$ ,  $p=.112$ ), and more strongly disagreed that “when I didn’t know the answer to a question, I didn’t care,” ( $\beta=-.424$ ,  $t=-1.59$ ,  $p=.118$ ). The inclusion of these variables did not reduce the effect of exposure to non-humorous disparagement when humor was expected (See Table 44).

Table 44

Summary of Multiple Regression Analysis for Political Knowledge Scores (N=59)

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	13.48	1.75		7.69	.000
Non-Humorous when Humor Expected	-1.45	(.568)	-.335	-2.56	.013
Relaxed	-.441	(.162)	-.354	-2.71	.009
Didn't Care	-.223	(.272)	-.106	-.820	.416
Put in Effort	-.187	(.358)	-.066	-.523	.603
$R^2=0.183$					

Next, the data were analyzed to determine, using the measured variables, the best model to predict political knowledge scores. A four variable model proved to be the strongest and most parsimonious predictor of political knowledge scores. Table 45 indicates that whereas experiment 1 found that a combination of PIE, anxiety, motivation and domain scores most fully predicted political knowledge performance, the best model for experiment 2 included group score instead of motivation. Lastly, Table 46 displays the hypotheses and findings for Experiment 2 in summary form.

Table 45

Summary of Multiple Regression Analysis for Political Knowledge Scores (N=150)

VARIABLES	B	SE( $\beta$ )	$\beta$	<i>t</i>	Sig.( <i>p</i> )
(Constant)	6.09	1.03		5.89	.000
PIE Score	.265	(.060)	.355	4.40	.000
Anxiety1 Score	-.062	(.024)	-.182	-2.54	.012
Group Score	-.095	(.053)	-.130	-1.79	.075
Domain Score	.182	(.050)	.293	3.66	.000
$R^2=0.383$					

Table 46

## Summary of Hypotheses for Experiment 2

Hypothesis		Tables 33-36	Table 37	Table 41	No Table
H4	Exposure to humorous disparagement will have a greater negative effect on PIE and performance on the political knowledge test than will exposure to non-humorous disparagement.				-
H5	Participants in the humor expectancy conditions will indicate greater PIE and perform better on the political knowledge test than will participants in the no expectation conditions.				-
H6	Anxiety, motivation, and message counterarguing will mediate the effects of humor expectancy and exposure to disparaging political statements or humor on PIE and performance on the political knowledge test.	-			
H7	PIE will mediate the effects of humor expectancy and exposure to disparaging political statements or humor on performance on a political knowledge test.				-
H8	Belief in the stereotype, awareness of the stereotype, and group and domain identification will moderate the influence of the disparaging statements or humor on PIE and political knowledge performance.		+? for domain and pk		
H9	An interaction effect between humor expectancy and exposure to disparaging political humor on pk and PIE is predicted. Regardless of whether participants are exposed to the disparaging humor or statement, participants in the humor expectancy conditions will perform equally as well on the pk test and indicate equal levels of PIE. The effect of political humor will only be significant (resulting in a decrease in scores on both dependent measures) under the condition where it is unexpected.			+?	

+= supported, +?=limited support, - no support

## **CHAPTER VII**

### **DISCUSSION**

Every night, millions of young citizens tune in to late-night television shows, where they watch and potentially learn as they laugh at the foibles of the press, politicians, and the public. In academic and journalistic circles, this trend towards infotainment as information source has scholars questioning the role of humor in a democracy (e.g., Hart & Hartelius, 2007). This research sought to determine whether exposure to disparaging political humor about their age group affected young voters' attitudes and performance on a political knowledge test.

Experiment 1 failed to support the hypothesis that young voters would feel less politically efficacious or perform worse as a result of exposure to a manipulation that made their membership in a stereotypically less knowledgeable demographic salient. While participants did perform significantly worse when the test was described as diagnostic, this effect was eliminated when age was made salient. It was reasoned that either being a young voter was not seen by the participants as a sign of weakness or that the manipulation of stereotype threat was not strong enough to find the anticipated effects. In both experiments, participants tended to neither agree nor disagree with the statements on the awareness, belief, domain, and group identification scales, and the effect of age and diagnostic salience or either form of disparagement was rarely found to differ along levels of these measures. Therefore, at least for the participants in this sample, being a young voter was not a strong or

negative part of their identity. However, since there were significant effects of disparagement in experiment 2 under certain conditions, it seems that the age salience manipulation was too weak to bring about the anticipated effects.

Something akin to stereotype threat effects did occur in experiment 2. Participants' performance on the political knowledge test assimilated towards the stereotype when humor was unexpectedly received or expected and not received. Humor therefore had two effects. Humor when expected aided in the dismissal of disparaging messages. Humor when unexpected aided in the fulfillment of the threat behind the disparaging message. Another way of looking at these results is that non-humorous disparagement had no effect on political knowledge scores when humor was unexpected, but did when humor was expected. Therefore, the absence of humor when expected also led to stereotype threat effects.

What caused participants to perform worse when unexpectedly exposed to humor may be different than what caused other participants to perform worse when not receiving the humor they expected. Participants not expecting humor were able to dismiss the threat in the non-humorous condition, but performed worse in the humorous condition, even though they rated the joke as more humorous. Humor here had a significant negative effect on performance, but not on attitudes. The data do not offer a clear answer for why performance was inhibited. Controlling for anxiety, motivation and counterarguing, or political information efficacy did not reduce the effects found in the interaction. More research is necessary to determine what factors contributed to the declines in performance.

Similarly, more research is needed to determine why people performed worse in the humor expectancy condition when humor was absent. The literature on affective expectations led to the prediction that all statements from Jon Stewart would be interpreted the same way.

Although statements from Jon Stewart were found to be more humorous than the statements coming from the journalist, participants who expected and did not receive humor rated the joke as less humorous than participants who expected and did receive humor, and performed worse on the political knowledge test. What the results indicate is that the absence of humor was noticed on some level in the humor expectancy/non-humorous condition. The expectation of humor (or a humorous mindset) was not enough to alleviate stereotype threat effects. The presence of humor was needed to resolve whatever unaccounted for feelings or thoughts were stirred up by the disparagement. Although affective expectations did influence explicit ratings of humorousness to some degree, expectations, at least of humor, were not found to be foolproof predictors of the way information affects behavior.

Although experiment 1 found that age salience did not create stereotype threat effects, even without an expressed allegiance or sense of connection with young voters, the more explicit disparagement in experiment 2 did cause stereotype threat effects under certain conditions. This provides further evidence that stereotype threat effects may be induced in people with little to no feelings of affiliation towards the stereotyped group or domain, or little belief in or awareness of the stereotype.

Another possibility exists to explain the results of experiment 2. It may be that the cause of the decrease in performance was due not to the presence or absence of humor but merely the violation of expectations. Prior research indicates that experiencing a violation of expectations can have dramatic effects on both physiological measures and task performance (e.g., Mendes, Hunter, Jost, Blascovich, & Lickel, 2007). Perhaps any violation of expectation, humorous or non-humorous, would have caused such a decrease. One way to test this in additional experiments could be to include conditions in which participants read



unexpected nonsensical or gibberish quotes about young voters or other subjects from either David Jennings or Jon Stewart. It could then be determined if a violation of expectations is sufficient for performance inhibition or whether humor is the key ingredient.

### **Limitations and Future Research**

There are several limitations to these experiments. First, people are not typically exposed to disparaging humor in a laboratory setting, nor do they take political knowledge tests (or any measure of performance) immediately after being exposed to disparagement. Second, people typically watch, rather than read statements from Jon Stewart. It is likely that different types of information processing occur when watching political humor as opposed to reading it. Additionally, the statements are not identical, as the non-humorous one contains 37 words and the humorous one 28.

One weakness of the designs of these experiments is that all of the potential mediators and moderators were completed after responses were given to the two main dependent variables of interest, political knowledge and political information efficacy. The political information efficacy measure was also in the position of coming after the main dependent variable, but serving as both a mediator of that variable and a dependent variable all its own. Another version of these experiments could isolate one mediator and have half of the participants answer those questions immediately before completing the dependent measures. The problem with doing so is that answering questions about anxiety for instance prior to taking the political knowledge test would seem to prime the participants about the purpose of the study. Additionally, the use of self-report in assessing the mediators of interest may be unreliable (Wilson & Nisbett, 1978). It would be interesting to include more physiological

measures of anxiety (e.g., blood pressure, skin conductance) and humor enjoyment or appreciation (e.g., smiling).

Another limitation that offers areas for future research is the homogeneity of the sample. In both experiments, participants were mostly white females, and all participants were college students at a highly competitive public university. The present research does not make the case that all young citizens would react the same to humorous or non-humorous disparagement, and thus leaves open the possibility for exploration of effects in other groups of young people. It is important to test these effects with groups that might be less likely to vote in the first place, including young citizens not enrolled in college. Along those same lines, future research should include post-test measures of how strongly participants identify with any number of social categories to which they might belong (e.g., race, gender, class). In experiment 1, it is unclear why participants in the diagnostic condition performed worse on the political knowledge test, yet this effect was removed when age salience was added. It was theorized that the general statement about test diagnosticity triggered gender-related stereotype threat in the largely female sample, as was found through more explicit means in prior research (McGlone et al., 2006). With proper measurement, this could be verified.

These studies mark the beginning of a line of research into the ways that young voters are affected by portrayals of their age group in the media. Experiment 2 lends strong support to the idea that stereotype threat research can be used as a foundation for this research. Before running experiment 2, additional disparaging jokes were considered for inclusion. The rationale was that significant effects would not be expected from exposure to one joke. Similarly, critics of humor might have argued that one joke was not enough to show the deleterious effects that humor can have. However, it was reasoned that stereotype threat

literature is full of examples wherein scholars found support for the theory simply by asking people to select their age or gender from a drop-down box, or by flashing words associated with a stereotyped group at a subliminal level. Therefore, if effects were not found based on one instance of disparagement, it would not mean that disparaging humor does not have negative effects in the short or certainly long term, but that more research would be needed. Similarly, if effects of exposure to one joke were found to have a significant effect on young citizens' attitudes or performance on a political knowledge test, it could be argued that they may not reveal the extent of the effect of continued exposure.

Having found an interesting interaction between the expectation of humor, exposure to disparaging humor and performance on a political knowledge test, this research area can expand to include more and different types of humor. It would be interesting to compare the effects of the moderately humorous comments used as the stimulus for experiment 2 with more humorous stimuli. Do the effects of unexpected exposure to disparaging humor increase as the stimuli become more humorous? Are people more likely to dismiss disparagement from a humorous source as it gets more humorous? Additionally, future research should include post-test measures of attitudes towards the source (e.g. source liking, credibility).

Finally, it would also be fitting to combine the two experiments and test the effects of exposure to disparagement in the high threat (age salience/diagnostic salience) condition as compared to the low threat (neither age nor diagnostic salience) condition.

## **Conclusion**

Although the candidate for whom the majority of young voters cast their ballots in 2008 was elected, the stereotype of an ill-informed youth did not end. A report (Ladner,

2009) issued by the Oklahoma Council of Public Affairs, a conservative think tank, and picked up by online sources such as Digg.com and Dailykos.com, detailed striking gaps in political knowledge amongst high school students in the state of Oklahoma. The survey found that less than three percent of those surveyed were able to correctly answer at least six out of ten questions from a United States citizenship exam. Notably, fourteen percent of respondents were able to name the author of the Declaration of Independence, and 23% correctly named the first George Washington as the first president of the United States.

Similarly, the election of Barack Obama did not mark the end of the stereotypical portrayal of an uninformed and unengaged youth in political satire. In the May 12th, 2009 episode of *The Daily Show*, Jason Jones traveled to Arizona State University after school officials refused to grant President Obama an honorary degree when he acted as commencement speaker. Three male students defended their university on camera. One student says honorary degrees should be reserved for important people and “heads of state.” When asked if they believe that Ben Franklin was a worthy enough president to receive an ASU honorary degree, two out of the three students say yes, not knowing he never held the office. When the third student catches their mistake, Jones congratulates him, then asks him about Alexander Hamilton. The student mistakenly agrees that Hamilton would have been a worthy enough president to receive an honorary degree at ASU.

This research found that concerns about the effects of humor on late-night shows on young citizens may not be warranted, since as long as the programs’ content is humorous, the expectation of humor that comes with attention to the program aids in the dismissal of information as “just joking.” However, Jon Stewart and other late-night hosts often make impassioned, non-humorous pleas during their programs. Audiences may notice that these

statements are non-humorous, and as a result interpret them differently. Non-humorous statements from humorous sources may be more persuasive or influential than they would be if they were humorous. Similarly, news broadcasts frequently include humorous material as a way to increase their audience size and seem more relatable. When young citizens' read the latest poll or news story about their age group's lack of political knowledge, they may not be susceptible to stereotype threat effects. However, if a journalist uses clever language or imagery, these news stories may impact future political performance. It is here, in the blurred line between news and entertainment, where concerns about humor may be appropriate. It is unclear when or how people make the decision that a certain program is to be attended to in a serious or non-serious manner, or whether they do so at all. Assuming that people approach their news and entertainment programming with certain expectations of what the tone of the content will be, this research found that the violation of those expectations can have dramatic effects.

## Appendix A

### Political knowledge test

What is the name of the U.S. Senate majority leader? \_\_\_\_\_  
(Harry Reid, Reid)

What specific position is held by John Roberts? \_\_\_\_\_  
(Chief Justice, Chief Justice of the Supreme Court, Supreme Court Justice)

Please list the three branches of government.

\_\_\_\_\_  
\_\_\_\_\_  
\_\_\_\_\_  
(legislative, judicial, executive)

How much of a majority is required for the U.S. Senate and House to override a presidential veto? \_\_\_\_\_  
(2/3) (66%) (67%)

Whose responsibility is it to determine if a law is constitutional or not? \_\_\_\_\_  
(The Supreme Court)

Which political party has the most members in the House of Representatives in Washington? \_\_\_\_\_  
(Democrat, Democrats, Democratic)

What is the term commonly used to refer to the first ten amendments of the U.S. Constitution? \_\_\_\_\_  
(Bill of Rights)

How many years is the term of office for a U.S. Senator? \_\_\_\_\_  
(Six, 6)

Which political party has the most members in the U.S. Senate? \_\_\_\_\_  
(Democrat, Democrats, Democratic)

What state do you consider to be your home state? \_\_\_\_\_ (not counted as right/wrong)

Please name one U.S. Senator from your home state. \_\_\_\_\_  
(Will be verified individually)

What political party does this Senator belong to? \_\_\_\_\_  
(Will be verified individually)

What is the name of the current Secretary of State? \_\_\_\_\_  
(Hillary Clinton, Clinton)

Which of the following issues is most important to you?

\_\_\_\_\_ Abortion/Reproductive Rights  
\_\_\_\_\_ Education  
\_\_\_\_\_ Environment  
\_\_\_\_\_ Gun Control  
\_\_\_\_\_ Taxes

For the issue selected as important, the person answered a question about which party traditionally held a certain viewpoint. If they selected Abortion, they were asked, “Which party traditionally supports a woman’s right to choose to have an abortion?” (Democratic) For education, “Which party traditionally supports the privatization of education?” (Republican) For those who choose environment, they were asked, “Which party traditionally supports tougher government regulations on emissions?” (Democratic) For gun control, “Which party traditionally supports stricter gun laws?” (Democratic) For taxes, “Which party traditionally supports lower taxes?” (Republican)

## Appendix B

### Political information efficacy scale (From Kaid, McKinney, & Tedesco, 2007)

I consider myself well-qualified to participate in politics.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I think that I am better informed about politics and government than most people.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I feel that I have a pretty good understanding of the important political issues facing our country.

1	2	3	4	5
Strongly Disagree				Strongly Agree

If a friend asked me about the election, I feel I would have enough information to help my friend figure out who to vote for.

1	2	3	4	5
Strongly Disagree				Strongly Agree



## Appendix C

### Mediators for Experiments 1 and 2

#### Anxiety/Threat (From Mattson (1960), used in Stone et al. (1999))

Please indicate how you felt while taking the test

1 2 3 4 5 6 7 (Reversed)  
Uneasy At ease

1 2 3 4 5 6 7  
Comfortable Uncomfortable

1 2 3 4 5 6 7 (Reversed)  
Upset Peaceful

1 2 3 4 5 6 7  
Relaxed Tense

1 2 3 4 5 6 7  
In Control Not in Control

Please answer the following questions about how you felt while taking the political knowledge test.

I was worried about confirming the stereotype that young voters are uninformed about politics.

1 2 3 4 5  
Strongly Disagree Strongly Agree

I was worried about how my performance would represent other young people.

1 2 3 4 5  
Strongly Disagree Strongly Agree

I was worried about performing up to my abilities.

1 2 3 4 5  
Strongly Disagree Strongly Agree

When I didn't know the answer to a question, I felt like I was confirming the stereotype that young voters are uninformed about politics.

1 2 3 4 5  
Strongly Disagree Strongly Agree

When I didn't know the answer to a question, I was able to stay calm. (reverse scored)

1 2 3 4 5  
Strongly Disagree Strongly Agree

When I didn't know the answer to a question, I was able to focus. (reverse scored)

I thought the test got harder as it went along.

When I didn't know the answer to a question, I didn't care. (reverse scored)

### Motivation/Effort

I felt the need to distance myself from the stereotype that young voters are uninformed about politics.

I tried to distance myself from the stereotype that young voters are uninformed about politics.

I wanted to finish the test as quickly as possible.

When I didn't know the answer to a question, I tried harder on the next one.

I put a lot of effort into answering the questions on the test.

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## Appendix D

### Mediator for Experiment 2

#### Statement counterarguing (adapted from Nabi et al. (2007))

I actively disagreed with what Jon Stewart/David Jennings said.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I thought of reasons why what Jon Stewart/David Jennings said was wrong

1	2	3	4	5
Strongly Disagree				Strongly Agree

I dismissed what Jon Stewart/David Jennings said as just a joke (reverse scored)

1	2	3	4	5
Strongly Disagree				Strongly Agree

Jon Stewart/David Jennings was serious about what he/they said

1	2	3	4	5
Strongly Disagree				Strongly Agree

Jon Stewart/David Jennings was trying to entertain more than persuade (reverse scored)

1	2	3	4	5
Strongly Disagree				Strongly Agree

The statement popped into my head while I was taking the test.

1	2	3	4	5
Strongly Disagree				Strongly Agree

### Moderators for Experiments 1 and 2

0	1	2	3	4	5	6
strongly disagree		neither agree nor disagree				strongly agree

I almost never think about the fact that I am a young voter. (R)

0	1	2	3	4	5	6
strongly disagree		neither agree nor disagree				strongly agree

My being a young voter does not influence how older Americans act with me. (R)

0	1	2	3	4	5	6
strongly disagree		neither agree nor disagree				strongly agree

Most older Americans have more anti- young voter thoughts than they actually express.

0	1	2	3	4	5	6
strongly disagree		neither agree nor disagree				strongly agree

I often think that older Americans are unfairly accused of being anti-young voter. (R)

0	1	2	3	4	5	6
strongly disagree		neither agree nor disagree				strongly agree

Most older Americans have a problem viewing young voters as equals.

0	1	2	3	4	5	6
strongly disagree		neither agree nor disagree				strongly agree

### Group Identification (Adapted from Luhtanen and Crocker (1992))

Overall, being a young voter has very little to do with how I feel about myself (reverse coded)

	1	2	3	4	5
Strongly Disagree					Strongly Agree

Being a young voter is an important reflection of who I am

	1	2	3	4	5
Strongly Disagree					Strongly Agree

Being a young voter is unimportant to my sense of what kind of person I am (reverse coded)

	1	2	3	4	5
Strongly Disagree					Strongly Agree

In general, being a young voter is an important part of my self-image

	1	2	3	4	5
Strongly Disagree					Strongly Agree

**Domain Identification (Adapted from Spencer et al. (1999) and Aronson et al. (1999))**

I like politics.

1	2	3	4	5
Strongly Disagree				Strongly Agree

Knowledge of politics is important to me.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I want to seek a career in politics.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I would describe myself as politically informed.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I would be embarrassed if I did not do well on a political knowledge test.

1	2	3	4	5
Strongly Disagree				Strongly Agree

**General self-efficacy (From Schwarzer & Jerusalem, 1995)**

How true are these statements for you?

1. I can always manage to solve difficult problems if I try hard enough.  
Not at all true    Hardly True    Moderately True    Exactly True
2. If someone opposes me, I can find the means and ways to get what I want.  
Not at all true    Hardly True    Moderately True    Exactly True
3. It is easier for me to stick to my aims and accomplish my goals.  
Not at all true    Hardly True    Moderately True    Exactly True
4. I am confident that I could deal efficiently with unexpected events.  
Not at all true    Hardly True    Moderately True    Exactly True
5. Thanks to my resourcefulness, I know how to handle unforeseen situations.  
Not at all true    Hardly True    Moderately True    Exactly True
6. I can solve most problems if I invest the necessary effort.  
Not at all true    Hardly True    Moderately True    Exactly True
7. I can remain calm when facing difficulties because I can rely on my coping abilities.  
Not at all true    Hardly True    Moderately True    Exactly True
8. When I am confronted with a problem, I can usually find several solutions.

- |  |                 |             |                 |              |
|--|-----------------|-------------|-----------------|--------------|
|  | Not at all true | Hardly True | Moderately True | Exactly True |
|--|-----------------|-------------|-----------------|--------------|
9. If I am in trouble, I can usually think of a solution.
- |  |                 |             |                 |              |
|--|-----------------|-------------|-----------------|--------------|
|  | Not at all true | Hardly True | Moderately True | Exactly True |
|--|-----------------|-------------|-----------------|--------------|
10. I can usually handle whatever comes my way.
- |  |                 |             |                 |              |
|--|-----------------|-------------|-----------------|--------------|
|  | Not at all true | Hardly True | Moderately True | Exactly True |
|--|-----------------|-------------|-----------------|--------------|

### **Demographics**

Were you eligible to vote in the 2008 election?

Did you vote in the 2008 election?

Do you intend to vote in the 2012 election?

Please list your age \_\_\_\_

Please list your gender \_\_\_\_

Please list your race \_\_\_\_

Which of the following political parties best exemplifies your political beliefs?

Republican

Democrat

Independent

Green

Libertarian

Other

## Appendix F

### Humorousness manipulation check

How funny would you rate that statement?

0	1	2	3	4	5	6	7	8
Not at all		A little		Somewhat		Very		Extremely

How clever would you rate that statement?

0	1	2	3	4	5	6	7	8
Not at all		A little		Somewhat		Very		Extremely

How confusing would you rate that statement?

0	1	2	3	4	5	6	7	8
Not at all		A little		Somewhat		Very		Extremely

How complicated would you rate that statement?

0	1	2	3	4	5	6	7	8
Not at all		A little		Somewhat		Very		Extremely

How informative would you rate that statement?

0	1	2	3	4	5	6	7	8
Not at all		A little		Somewhat		Very		Extremely

How negative would you rate that statement?

0	1	2	3	4	5	6	7	8
Not at all		A little		Somewhat		Very		Extremely



## Appendix G

### Humor expectancy manipulation check

I expected the statement to be funny.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I expected to have to figure out/work through a joke.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I was looking forward to reading the statement.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I assumed I would enjoy the statement.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I assumed the statement would be important. (reverse coded)

1	2	3	4	5
Strongly Disagree				Strongly Agree

I assumed the statement would be lighthearted.

1	2	3	4	5
Strongly Disagree				Strongly Agree

I assumed the statement would be complex. (reverse coded)

1	2	3	4	5
Strongly Disagree				Strongly Agree

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