Reassessing the Effects of Emotions on Turnout

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Abstract

Discrete emotions such as anger, pride, worry, and hopefulness have been shown to predict candidate preferences, issue attitudes, reports of participation other than voting, and stated intention to participate in various civic and electoral activities. Yet we know very little about how emotions might impact the most fundamental individual act in a democracy: turning out to vote. Using original survey data linked to past and future validated turnout to form four three-wave panels, we find that worry was a significant mobilizer of turnout in the 2018 midterm election, while the impacts of enthusiasm were not. We also find that measures of discrete emotions have detectable impacts on turnout only when respondents are prompted to think about *political* stimuli. These results have implications for theory, measurement, and model specification that should inform future work on the effects of emotions on political participation generally.

Reassessing the Effects of Emotions on Turnout

Discrete emotions such as anger, pride, worry, or hopefulness predict many kinds of political participation (Best and Krueger 2011; Groenendyk and Banks 2014; Hassell and Wyler 2019; Lamprianou and Ellinas 2019; Valentino et al. 2009; 2011) as well as stated intention to participate (Aytac et al. 2020; Banks et al. 2020; Becker et al. 2011; Marcus et al. 2000; Valentino and Neuner 2017; Weber 2013). Yet, we know little about how emotions might impact the most fundamental individual act in a democracy: turning out to vote. Estimating the effects of emotions on turnout is the central focus of this paper.

To inquire about the impact of emotions on political participation requires grappling with three key questions. The first is in some sense preliminary, but critical to research design: Do emotions contribute to the mis-reporting of political participation? It seems plausible that those with high levels of pride or hopefulness (or enthusiasm more generally) might over-report their political participation through expressive responding. Since turnout is substantially over-reported, this looms as a particular concern that – to our knowledge – has never been investigated empirically.

The second question concerns timing. There are some situations when emotional arousal can impact the participation decision *in the moment*. For example, on January 6, 2021 it seems likely that emotions impacted Trump supporters' decisions to use violence, force their way through security lines, and enter the US Capitol. Likewise, someone can decide in the moment to email a representative, post a comment, or make a campaign contribution. But elections may not afford the opportunity to translate emotions into action instantaneously. Thus, we must ask whether emotions experienced weeks or months before an election can have measurable downstream impacts on future voter turnout.

The third question concerns the objects that make people proud or angry, hopeful or worried. For example, if someone is angry about the latest unemployment statistics, does this mobilize as much as being angry at the actions of an elected official whose policies contributed to slow economic growth? With only a few recent exceptions, much prior research has relied on emotions concerning major party presidential candidates, who represent an important but quite limited sample of the kinds of objects that might arouse citizens.

We also note a fourth issue that is specific to turnout among other participatory acts – the extent to which participation in a prior election is highly predictive of participation in the next. For reasons we explain below, turnout's inertia makes it imperative to account for participation in prior elections.

In this paper we place these questions in the context of previous research on emotions in politics and voter turnout. In line with Ladd and Lenz (2008)'s recommendation to use panel data in emotion research, we merge four cross-sectional surveys with both self-reports and validated turnout from one subsequent election and one prior election. This results in four three-wave panel datasets that we use to answer these questions. We find that expressive responding is a significant confounder, with models based on self-reported turnout leading to different conclusions than those using validated turnout measures. Contrary to much prior work, we find that citizens' levels of worry has a robust impact on validated turnout, anger has effects conditional on the emotional stimulus, and enthusiasm has modest effects that often fall short of conventional levels of statistical significance. More specifically, exploiting a survey experiment embedded in the panels, we find that emotions evoked by stories "recently in the news" have no impact on future turnout, whereas emotions associated with recent "politics" have large and

substantively important effects. Together, these findings both update and raise important issues in our understanding of how discrete emotions impact political participation.

How Might Emotions Affect Voter Turnout?

Within political science, the most influential emotions-focused research program was launched by George Marcus and Michael MacKuen (1993) and evolved into Affective Intelligence Theory, or AIT (Marcus, Neuman, and MacKuen 2000). AIT posits that neural systems linked to emotion have important consequences for political behavior. Feelings of anger or enthusiasm activate the disposition system, which regulates motivation to continue established patterns of behavior informed by preferences. Enthusiasm and anger – sometimes called "approach emotions" – mitigate collective action problems and other inhibitions by increasing motivations to act (Groenendyk 2019; Groenendyk and Banks 2014). Consistent with this expectation, high levels of anger and enthusiasm have been empirically linked to higher political participation (Banks et al. 2020; Becker et al. 2011; Best and Krueger 2011; Brader 2005; Groenendyk and Banks 2014; Hassell and Wyler 2019; Lamprianou and Ellinas 2019; Phoenix 2019; Valentino and Neuner 2017; Valentino et al. 2009). In some additional studies, the effects are evident only conditionally: at lower levels of civic duty (Collins and Block 2020), when people are not ambivalent about their political choices (Groenendyk 2019), or when people have sufficient political resources (Valentino et al. 2011). Phoenix (2019) also finds that the effects of anger and enthusiasm motivate citizens of color to participate in political action beyond voting, but the same emotions appear to motivate white citizens to vote.

In contrast, fear's expected effects on participation are less clear. According to AIT, fear activates the neural systems that monitor the environment for threats. In contrast to enthusiasm and anger, which sustain habit, activation of the surveillance system disrupts established

routines. Notably, fear serves an information-gathering function (Marcus and MacKuen 1993; Marcus, Neuman, and MacKuen 2000), with inconsistent participatory benefits (Valentino et al. 2011). Cognitive appraisal theory further argues that fear leads to *withdrawal* in response to threat (Lerner and Keltner 2000; 2001; Smith and Ellsworth 1985), implying that fear leads people to eschew highly costly political action (Best and Krueger 2011; Valentino et al. 2011; Weber 2013). On the other hand, there is evidence that fear predicts a higher likelihood of self-reported turnout, and, particularly among Asian-Americans (Phoenix 2019), less costly participation beyond voting (Phoenix 2019; Valentino et al. 2011).

Voting Differs from Other Forms of Political Participation

There are several reasons why we might not expect findings on other forms of political participation to generalize to voting. Unlike other forms of participation, people cannot vote as an immediate response to their emotions. When AIT and cognitive appraisal theory discuss emotions, they imply that emotions generate immediate effects. Anger and enthusiasm lead to immediate drives for action, fear to withdrawal from threat and reconsideration of routines. This makes sense for many forms of political participation. Citizens can quickly don a campaign button in response to their candidate doing well in the polls or order a yard sign; they can gather in the streets in the aftermath of a police shooting or post to social media.

In contrast, voting occurs at set intervals. Unless the source of political emotions occurs immediately before an election, emotions can only impact the immediate intention to vote. For example, a strongly felt arousal of enthusiasm might spur some to go online and complete a voter registration form. Likewise, acute fear might diminish the immediate motivation to display a yard sign or lead someone to tune in to a news broadcast. But emotions cannot immediately spur or discourage people to cast a vote if the election is months away. Campaign seasons last over a

year, generating a steady stream of stimuli to ignite political emotions, and a longer time horizon to act on them.

How Emotions Interact with Past Turnout Behavior

Another complication in generalizing from studies of other kinds of participation derives from the habitual character of turnout. High percentages of those voting in one election do so in the next (Fowler 2006), and only a small share of non-voters will transition to becoming regular voters in any given election cycle (Plutzer 2002). This has implications for the impact of fear. According to AIT (Brader et al. 2008; Huddy et al. 2005; Marcus and MacKuen 1993; Marcus et al. 2000; Vasilopoulos et al. 2019) and cognitive appraisal theory (Lerner and Keltner 2000; 2001; Smith and Ellsworth 1985), fear reduces reliance on previous habits when making decisions. When those who traditionally abstain from the political process feel worried or scared in response to a political threat, there is an opportunity to disrupt habits of abstention. Likewise, fear can also explain why peripheral voters who were previously unmotivated instead participated in the next election (Hansford and Gomez 2010). Extending this same logic to regular voters, fear and worry could lead habitual voters to reconsider their longstanding habit and reformulate their political ends and means.[1]

The roles of anger and enthusiasm in a habitual turnout framework are potentially more complex. AIT frames enthusiasm (Marcus et al. 2000) and anger (MacKuen et al. 2010) as

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[1] Voting also is subject to different norms than other forms of participation. People who abstain from voting may be subject to social sanctions (Gerber et al. 2016), which is not the case for those failing to protest or convince others to vote. If anything, there may be norms *against* such

emotions that sustain ongoing habits. It follows that among those who already vote, anger and enthusiasm can help sustain one's participation streak (Valentino et al. 2009). At the same time, anger and enthusiasm lend themselves to action (Lerner and Keltner 2000; 2001; Smith and Ellsworth 1985), so even previous non-voters who feel these emotions may end up being more likely to participate. Indeed, Lamprianou and Ellis (2019) find that inducing anger increases intentions to participate politically, and especially so among those with little history of participation.

On the other hand, high levels of anger and enthusiasm might be downstream consequences of prior electoral participation (Valentino et al. 2009). By voting in a previous election, citizens feel more plugged in to political life and group conflict, feeling more enthusiasm and anger in response (Groenendyk and Banks 2014; Phoenix 2019; Smith et al. 2007).

These two well-known phenomena – that regular voting can influence emotions and that voters often respond differently to campaign stimuli than non-voters – point to the necessity of controlling for prior voter turnout. Many have found that anger and enthusiasm can influence *intentions* to vote (Aytac et al. 2020; Brader 2005; Collins and Block 2020; Valentino and Neuner 2017; Weber 2013) or self-reports of voting in a recent election (Groenendyk 2019; Phoenix 2019). Yet, without controlling for past voting, the effects of these emotions might be artifacts of previous voting decisions.

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attempts at persuasion (Klar et al. 2018). Voting is also a potent form of expression of group membership (Dawson 1994) as well as re-affirmation of its importance in one's life (Gerber et al. 2009).

It is also possible, if anger and enthusiasm are signs of group attachment (Groenendyk and Banks 2014; Phoenix 2019; Smith et al. 2007), that people displaying these emotions could be particularly prone to overstating their intentions or feel particularly ashamed of abstaining and resolve it through over-reporting turnout. Prior work has acknowledged these limitations of using voting intentions (Banks et al. 2020; Brader 2005; Hassell and Wyler 2019; Valentino and Neuner 2017) and self-reports of turnout (Banks et al. 2020; Groenendyk 2019). Though other work has made use of multiple methods of assessing turnout to triangulate (Banks et al. 2020; Groenendyk 2019), it is still important to incorporate best practices. We do so by taking advantage of recent advances in the validation of voter turnout (Enamorado et al. 2019; Enamorado and Imai 2019) to distinguish the effect of emotions on actual turnout as opposed to self-reports of turnout. Our research design, described below, uses validated turnout in the current election as a dependent variable and validated turnout in the prior election as a control, allowing us to identify effects of emotions independently from past voting patterns and free of biases in self-reporting.

Can Different Political Stimuli Affect How Emotions Shape Turnout?

Our second research question concerns how emotions may have different effects on turnout depending on the stimulus. By definition, emotions concern specific objects (Mather 2007). From one perspective, emotions primarily produce action orientations towards the source of the stimulus (Lerner and Keltner 2000; 2001). It follows that as the source of the emotion becomes less relevant to politics, the less likely it is that the emotion would translate into political action. If I am angry because I feel cheated by a local contractor, I am far more likely to complain to the

Better Business Bureau than I am to register to vote. In other words, this position expects that emotions exert stronger effects on turnout when the stimulus is more relevant to the election.

On the other hand, there are reasons to expect that emotions aroused by one stimulus can impact actions toward another. The state-dependent theory of memory (Bower 1981) argues that when people experience an emotional state, it leads them to recall (and occasionally act on) affectively congruent stimuli. There is evidence this occurs in politics. For example, Banks (2014) and Banks and Valentino (2012) find that a non-political anger induction strengthens the association between symbolic racism (caused by anger at Black Americans for perceived norm violations) and opposition to racial policy (see also Webster 2018). Applying this logic to turnout, if I am angry because I feel cheated by a local contractor, I will also recall being angry at government actions that harmed my community, motivating me to participate. Phrased in the strongest way, this position expects an association between emotions and turnout, no matter the stimulus. If the transference is inefficient, however, the effects may be smaller (though still present) with a less direct stimulus.

Political psychology scholars have engaged the possibility that different stimuli can elicit different behavioral outcomes by varying the question wording of emotions questions on surveys. Valentino et al. (2011, Study 2) used a battery of emotion questions about "the way things are going on the country these days" (163). This battery is expanded in the 2018 American National Election Study pilot to include emotions directed at Trump and immigration policy. These batteries are undoubtedly improvements in measurement as they can gauge general emotions. However, asking respondents to consider the broad domain of "the way things are

going" may mask important heterogeneity in stimuli. Additionally, Trump and immigration policy, while important, are far from the only potential sources of emotions.[2]

Another method to gauge the general effects of emotions on turnout is experimental manipulation. For example, subjects may be asked to write about something that made them angry or hopeful, with measures of the dependent variable (such as voting intention) recorded shortly after this manipulation (Phoenix 2019; Valentino et al. 2011, Study 1). However, none of these experiments were designed to distinguish among the topics that subjects wrote about and their potentially heterogenous impacts on turnout intention. Moreover, it remains an empirical question how the observed effect of emotions on immediate participatory *intentions* can inform us about an actual campaign where people experience emotions throughout the campaign but vote much later.

Our original survey addresses several of the above issues. First, our use of validated turnout allows direct assessment of over-reporting. Second, respondents answer open-ended items about each emotion, making it possible to gauge what caused them to feel certain ways. Combined with an experiment designed to make certain emotional stimuli more salient than others we can assess how the object of the emotion conditions emotional effects on turnout.

Data and Methods

The ideal research design to test the role of emotions in turnout includes validated turnout in both the election before and after respondents register their emotions. Using this design, it is possible to parse out the effects of anger, worry, and enthusiasm on voting above and beyond any effect

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[2] Unfortunately, these items are also asked only on the 2018 post-election survey, after the act of voting has occurred.

turning out in the previous election had on emotions. This design also allows for testing whether emotions have different effects on turnout for voters and non-voters.

Our data come from a unique pooled dataset based on four independent samples from Penn State's Mood of the Nation Poll. Respondents were recruited by YouGov's non-probability panel, with field dates in August 2017, November 2017, February 2018, and September 2018, ranging from 3-15 months prior to the 2018 midterm election. Based on matching to Current Population Survey benchmarks and population estimates from other surveys, YouGov samples are demographically representative of the American voting population.[3] Each survey contains 1,000 respondents for a preliminary sample size of 4,000.[4] In each survey, respondents reported

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[3] We recognize the limitations of online samples. However, among 12 online polls tested by Kennedy and colleagues (2016), YouGov (organization "I" in the Pew benchmarking) performed the best overall and best in logistic models predicting volunteering, and third-best in predicted turnout in a local election relative to a Current Population Survey (CPS) benchmark equation, although CPS may not be the best benchmark as one respondent serves as proxy for an entire household. Overall, we feel confident that the YouGov platform is well-suited to the needs of the project.

[4] These respondents, after weighting, were 51.6% female, 48.4% male; 65.7% White, 14.8% Hispanic, 11.9% Black, 2.4% Mixed-Race, 2.4% Asian, 1.7% Other Race, 1.1% Native American, 0.2% Middle-Eastern; 44.0% Democrats (including leaners); 34.5% Republicans (including leaners), and 21.5% true Independents.

on four different emotions when originally interviewed. These samples are part of an ongoing series of surveys declared exempt by the Penn State Institutional Review Board.
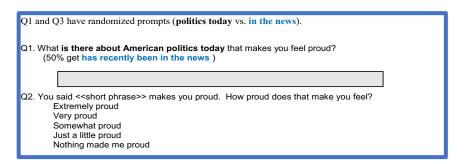
Measuring Turnout

Our primary dependent variable is validated turnout. In our 2018 survey, YouGov, working with TargetSmart, was able to match 2,393 of the 4,000 respondents (59.8%) to voter file records. The unmatched consist primarily of unregistered voters (true negative matches) and a smaller number of registered voters who were unable to be matched with vote records (false negative matches). Several factors can contribute to false negatives. For example, some panelists may identify themselves to YouGov using a nickname (Ronnie, rather than Ronald), some may provide YouGov with an address different from the one associated with their voter registration or may report their age based on an upcoming birthday rather than the most recent one. With probabilistic matching, a single minor anomaly may not prevent successful matching. Detailed audit studies by Pew (Igielnik et al. 2018), Enamorado et al. (2019), and Enamorado and Imai (2019) all estimate that 75% or more of the unmatched are true negatives (not registered). We restrict attention to matched respondents such that non-voting is operationalized as non-voting *among those successfully* matched. Overall validated turnout in the 2018 midterm election among the matched was 78.5%. An alternative approach is to treat non-matched individuals as un-registered, and (by definition) non-voters. If we assume all unmatched are non-voters, the estimated turnout rate is 42.2%. We report analyses using this alternative operationalization in the Appendix (Table A15), and these produce the same findings and conclusions.

Measuring Emotions

Our 2018 survey differs from the ANES measures as well as the innovative methods of Valentino and associates to measure emotions more generally. The ANES focuses on candidates while Valentino et al.'s prompt ("Generally speaking, how do you feel about the way things are going in the country these days?") essentially asks respondents to scan the social and political landscape and average ("generally speaking") across stimuli. In contrast, our questions ask respondents to scan these same stimuli, but to select the ones with the greatest emotional arousal. More specifically, as illustrated in Figure 1, we present respondents with open-ended questions tasking them with recalling and reporting one or more things in politics or the news that made them proud, and one item on anger (each followed by an intensity measure). Next, we asked questions about what makes them hopeful and worried about where America is headed.[5]

Figure 1: Core Emotions Questions. Pride in the Politics condition depicted here, but pattern is repeated for all four emotions.

Q1 and Q3 have randomized prompts (**politics today** vs. **in the news**).

Q1. What **is there about American politics today** that makes you feel proud?
   (50% get **has recently been in the news**)

Q2. You said <<short phrase>> makes you proud. How proud does that make you feel?
   Extremely proud
   Very proud
   Somewhat proud
   Just a little proud
   Nothing made me proud

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[5] Though we ask about worry in this survey, worry and fear are theoretically synonymous (Lazarus and Lazarus 1994) and correlate quite highly in studies that estimate both (Levy and Guttman 1976; Zebb and Beck 1998).

The prompts for pride and anger explicitly ask respondents to recall stimuli from the present or recent past that elicited these emotions, while the prompts for hope and worry explicitly elicit prospective answers. These wordings are consistent appraisal-based theories of emotion conceive of them. Pride and anger occur in the presence of certainty about stimuli (Lerner and Keltner 2000). In contrast, hope and worry are both reactions to uncertainty, which by definition is the perception of an unknown future outcome (Just, Crigler and Belt 2007).

Respondents first provide a free-text response to each open-ended question. These answers are then echoed back on the next screen, which contains a forced-choice five-point rating scale of the strength of that emotion, ranging from "nothing makes me proud" [angry/hopeful/worried] (scored 0) to "extremely proud" [angry/hopeful/worried] (scored 1). We use anger and worry as standalone measures of anger and fear, respectively, and combine hope and pride into an enthusiasm index (α=0.61).

The open-ended questions elicited a wide range of specific answers – some were just one or two words, most between 3 and 20 words, while a small number qualify as short essays. When asked "what makes you proud," however, the modal answer was "nothing" or variations such as "not much" or "I can't think of anything." Indeed, 46% of respondents could think of nothing in politics that made them proud and 37% said the same when prompted to think about events in the news. About 1 in 5 (22%) could think of nothing that made them hopeful, but over 95% provided a substantive answer when asked what made them angry or worried.

Unsurprisingly, about 1 in 5 respondents named President Donald Trump as an object of anger and worry during his first two years in office. But respondents named scores of other objects from the political world (e.g., gridlock, the role of money in politics, corruption,

advocacy groups like Black Lives Matter) and the news (recent mass shootings, police killings, crime, terrorism, and many instances of local or human-interest stories salient at the time).

Overall, respondents reported substantial variation in their emotional intensity as well. Table 1 summarizes the distribution of answers to the follow-up intensity measures. The table shows great emotional arousal to the objects named in response to the anger and worry questions, but with meaningful variation in all instances.

Table 1: Intensity of emotions associated with named objects. Surveys fielded in August 2017, November 2017, February 2018, and September 2018

	Proud (N=3,956)		Angry (N=3,973)		Hopeful (N=3,979)		Worried (N=3,980)	
Nothing made me	40.3	%	7.1	%	23.0	%	6.8	%
Just a little	4.0		2.9		8.9		3.3	
Somewhat	11.3		12.0		20.0		14.9	
Very	21.6		27.2		23.1		30.3	
Extremely	22.8		50.8		25.0		44.8	
	100.0		100.0		100.0		100.0	

Leveraging Question Wording to Assess the Effects of Objects of Emotions

This ongoing poll includes an implicit question wording experiment. For the pride and anger items, half the respondents were randomly assigned to what we will call the "politics" condition, getting the question "What is there about American politics today that makes you proud?" (and similarly, for anger). In the "news" condition, people were instead asked "What in the news today makes you proud?" (or angry). These different prompts were intended to direct respondents to scan different recent memories. In Figure 1, the randomly assigned prompts for questions 1 and 3 are indicated in bold font.

To show that the manipulation was effective, we can examine the relative frequency of words used in the open-ended answers to questions using each prompt. Specifically, we adapted the "fightin' words" metric developed by Monroe, Colaresi and Quinn (2008). Each word is assigned a standardized score that reflects its relative use in the news condition relative to the politics condition, adjusted for the overall frequency of word use so that a word used just once or twice is heavily discounted (more specifically the relative frequency is adjusted for sampling variance, with the metric of low-prevalence words shrunk towards the baseline).

Table 2 illustrates the impact of the random assignment on answers to the anger question from the September 2018 wave of the survey.[6] It shows that the news prompt elicited objects that were highlighted in news reports in the previous days or weeks (e.g., Donald Trump, Brett Kavanaugh, Supreme Court, along with Nike and Colin Kaepernick) while the objects of anger elicited by the politics prompt typically reflected more enduring features of political conflict (e.g., politicians, parties, we/they, racism). Objects of pride when asked about the news versus politics show a similar pattern. As the variance is higher than the null variance distribution range, the words elicited by different prompts for anger and pride cannot be random draws from the same population. Therefore, the treatment successfully directd respondents' attention to different kinds of recent memories.

Table 2. Relative frequency of word use, by question prompt (September 2018 wave). Numbers are z-scores.

	What Makes You Angry?	What Makes You Proud?
News Terms	8.50 donald_trump	4.55 news

	5.34 brett_kavanaugh	4.21 john_mccain
	4.42 news	3.77 nike
	4.40 supreme_court	3.41 colin_kaepernick
	4.15 anything	2.59 supreme_court
	4.14 hearings	2.44 economy
	3.72 nike	2.41 funeral
	3.12 john_mccain	2.25 new
	2.93 with	2.15 his
	2.74 hearing	2.08 brett_kavanaugh
Politics Terms	-2.93 politicians	-4.83 are
	-2.57 party	-4.46 im
	-2.46 is	-4.26 not
	-2.32 are	-4.07 we
	-2.28 they	-3.66 have
	-2.19 we	-3.59 and
	-2.10 parties	-3.54 people
	-1.90 racism	-3.42 politics
	-1.84 than	-3.12 not much
	-1.84 themselves	-3.12 it
Variance (all terms)	0.77	0.83
Null Variance, Mean	0.53	0.53
Variance, 99th %ile	0.60	0.61

Source: Monroe and Plutzer (2020)

The one seeming exception to this is the token "donald_trump" (this token includes variations such as "Trump," "President Trump," "Drumph" and so on), which occurs more often in the news. However, as we illustrate in Appendix Tables A8-A11, people talk about Trump differently in each condition. In the news condition, people referred to Trump's comments and tweets, while in the politics prompt, they referred to Trump's official actions and his engagement with government institutions. Taken together, this is strong evidence that the prompts successfully primed respondents to recall different sources of emotions.[7]

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[7] Although the prospective questions about hope and worry did not receive the same randomized prompts, they are nevertheless post-treatment and asked immediately after the pride

Results

Worry Is a Mobilizer

Table 3 reports estimates of emotions' effects on validated turnout, controlling for past validated turnout and a number of other demographic and political predictors of turnout. In contrast to participatory acts other than voting (Valentino et al. 2009; 2011), we fail to find a significant effect of anger on turnout (p=.11). We should note, however, that when we subset the dataset and examine only those given the "politics" prompt, a more substantial anger effect emerges, as we explain in more detail below.

Table 3: Logit model of observed effects of emotions on validated turnout among respondents matched to voter files. Controls not shown are partisan strength, education, income, church attendance, age, race/ethnicity, gender, marital status, parental status, and survey wave. The full model is reported in Table A12 of the Appendix.

	Dependent variable: Validated Turnout
Anger	0.488
	(0.307)
	p=.112
Fear/Worry	0.753
	(0.304)

and anger questions. It is not surprising, then, that the answers to these questions also reflect experimental assignment. For example, in the August 2017 wave, the token donald_trump occurs far more frequently in the news condition than the politics condition for both anger and worry. The impact of prompt, however, is less distinctive for the unprompted questions.

	p=.013
Enthusiasm	0.432
	(0.245)
	p=.078
Past Validated Turnout	3.318
	(0.176)
	p<.001
Constant	-5.062
	(0.498)
	p<.001
Controls	Yes
Observations	2,027
Log Likelihood	-615.689

Note: Standard errors in parentheses.

In contrast, the effect of fear and worry in our YouGov survey is positive and statistically significant (p=.01). Enthusiasm's effect on turnout is positive, but marginal in our survey (p=.08). Though the sizes of the coefficients do not differ significantly from one another (p*s*>.41), these results do place worry's effects on stronger empirical ground than prior work, particularly once we take into account past voting decisions. Because this effect runs counter to much previous literature, we must consider if this is due to the particular sample we use, or our unconventional method of measuring emotions. To explore this, we replicated the analysis using the validated vote measures and candidate emotional response measures available in the 2016 American National Election Study. As shown in Appendix Table A16, we find essentially the same pattern of results. Specifically, even with different sample and different questions we find

no effect of anger after accounting for past validated turnout. We also find positive effects of fear (though with p = 0.06) and enthusiasm.

Second, we tested whether our different findings were due to the principled changes we made to model specification. We estimated alternative models of turnout using our own data, but using self-reports instead of validated turnout, and alternating whether prior turnout is controlled for. In line with previous literature, Tables A13-A16 show that shifts in the predicted probability of voting from anger and enthusiasm are 2-3 times larger and statistically significant when self-reported turnout is employed rather than validated turnout, (ps<.001),while the effect for worry is of similar magnitude regardless of how turnout is measured (but only marginally significant in models that do not control for prior turnout, with p=.08). Comparing models 3 and 4 in Table A17 shows the danger of not accounting for prior turnout: anger and enthusiasm's effects are over-estimated, and worry's effects may be overlooked. That is, when we fail to control for lagged participation or rely on self-reports, we arrive at results very similar to the previous literature.

Emotional intensity is associated with overreporting turnout

These results raise the question of why anger and enthusiasm – shown to impact self-reported turnout in several studies – have an inconsistent impact in our analyses. One possibility is that self-reported turnout may be inflated by expressive reporting, and disproportionately so for those with the strongest emotional reactions to the political world. This could occur, for example, because anger and enthusiasm, if resulting from political identities, might increase the salience of norms of participation and instill shame in those who fail to follow through on it. To explore this, we restrict our analysis to respondents at risk of over-reporting: those whose voter file indicated did not vote. Among those for whom we have self-reports, we created a

dichotomous variable coded =1 if the respondent reported voting (and thus over-reported turnout), and 0 if the respondent correctly reported not voting. We predicted over-reporting of turnout using each of our emotion measures and controls from previous models, including past validated turnout.

Table 4: Logit models of observed effects of emotions on overreporting turnout (standard errors in parentheses). Controls not shown are partisan strength, education, income, church attendance, age, race/ethnicity, gender, marital status, parental status, and survey wave. Full model shown in Table A13 of the Appendix.

	Dependent variable: Overreporting Turnout
Anger	1.292
	(0.588)
	p=.028
Fear/Worry	0.654
	(0.618)
	p=.289
Enthusiasm	2.063
	(0.503)
	p<.001
Past Validated Turnout	1.752
	(0.375)
	p<.001
Constant	-3.064
	(0.925)
	p<.001
Controls	Yes
Observations	276

Log Likelihood	-143.005

As shown in Table 4, the effect of enthusiasm on overreporting is large, positive and statistically significant (p<.01). Moving from middling to maximum enthusiasm shifts the predicted probability of overreporting turnout from .31 to .55 among those who did not vote in 2016 and from .72 to .88 among those who did. In conjunction with the model in Table 3, these results suggest that enthusiasm potentially produces some modest participatory benefits, but it also may lead to fabrication of turnout for voters who fail to follow through. Table 4 also shows that anger also has a large and significant impact on overreporting. Moving from middling to maximum shifts the predicted probability of overreporting turnout from .23 to .36 among those who did not vote in 2016 and from .63 to .77 among those who did. In contrast, worry's estimated effect is smaller in magnitude and well short of conventional levels of significance (p=.29). Moving from middling to high worry shifts the predicted probability of overreporting turnout from .26 to .32 among those who did not vote in 2016, and from .67 to .73 among those who did. We do not find that the coefficient for anger in the equation predicting overreporting of turnout differs significantly from that of worry (p=.52) or enthusiasm (p=.33), while the coefficients for worry and enthusiasm differ marginally (p=.08).

Priming Political Considerations Increases Observed Effect of Emotions on Turnout

In our analyses so far, we have tested the effects of emotions on turnout without regard for item wording, pooling responses across the politics and news conditions. Now we turn to whether asking about political sources of emotion instead of more general "news" sources affects the correspondence between emotions and turnout. To do so, we run separate models for the

effects of emotions on turnout splitting the sample by experimental condition, along with a pooled model with the relevant treatment × emotion interactions.

Table 5: Logit models of observed effects of emotions on validated turnout (standard errors in parentheses). Controls not shown are partisan strength, education, income, church attendance, age, race/ethnicity, gender, marital status, parental status, and survey wave. See Appendix Table A14 for full results.

	Dependent variable:		
	Validated Turnout, 2018		
	News	Politics	Interactive
Anger	0.072	1.043	0.100
	(0.407)	(0.497)	(0.402)
	p=.860	p=.036	p=.804
Fear/Worry	0.437	1.151	0.462
	(0.410)	(0.475)	(0.403)
	p=.287	p=.015	p=.252
Enthusiasm	0.510	0.451	0.514
	(0.334)	(0.384)	(0.325)
	p=.126	p=.240	p=.114
Politics Prompt			-0.910
			(0.551)
			p=.099
Anger * Politics Prompt			0.881
			(0.625)
			p=.158
Fear/Worry * Politics Prompt			0.629
			(0.606)
			p=.300
Enthusiasm * Politics Prompt			-0.076
			(0.479)
			p=.874
Validated Turnout, 2016	3.317	3.460	3.321
	(0.243)	(0.280)	(0.177)
	p<.001	p<.001	p<.001

Constant	-4.750	-5.859	-4.717
	(0.645)	(0.834)	(0.538)
	p<.001	p<.001	p<.001
Observations	1,039	988	2,027
Log Likelihood	-340.227	-265.420	-613.205

The results, reported in Table 5, again shows that the effect of enthusiasm is small and falls short of statistical significance, regardless of prompt (ps>.10). The split sample models show that the marginal effects of both anger (p = 0.04) and worry (p = 0.02) are large and statistically significant when respondents are prompted to think about political stimuli, while none of the emotional intensity measures have an effect on turnout that is statistically distinguishable from zero (ps>.10) in the news condition.[8]

To illustrate the substantive meaning of the prompt effects, we report predicted probabilities of turnout among those who did not vote in 2016 and those who voted in 2016 (Table 6) at both the 50th percentile and the maximum level of that emotion. We document predicted probabilities separately by past vote due the strong effect of past turnout on future turnout (Plutzer 2002). Anger's effects show the same pattern by prompt among voters and non-voters alike. Among those who received the news prompt, the estimated effect of moving from medium to maximum anger is near-zero, but among those who received the politics prompt,

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[8] The coefficients for emotions do not differ significantly from one another in the news (ps>.44) or politics conditions (ps>.24). In addition, we note that none of the prompt × emotion interactions are statistically significant and the equivalent tests of coefficients across models fail to reject the null hypotheses that the estimates are identical in the population (p=.27).

likelihood of voting increases by 5-8 percentage points. Worry shows a similar pattern, where increases in turnout as a function of worry are much more visible among those who received the politics prompt. In contrast, enthusiasm shows relatively flat effects across prompt conditions.

Table 6: Predicted probabilities of validated turnout by emotion and prompt among previous non-voters and voters based on models in Table 5.

2016 Validated Turnout	Emotion	P(Turnout) at Middling Emotion	P(Turnout) at Maximum Emotion	Difference [a]
News prompt				
Non-voter	Anger	0.24	0.25	0.01
Non-voter	Worry	0.22	0.26	0.04
Non-voter	Enthusiasm	0.25	0.30	0.05
Voter	Anger	0.90	0.90	0.00
Voter	Worry	0.89	0.91	0.02
Voter	Enthusiasm	0.89	0.91	0.02
Politics prompt				
Non-voter	Anger	0.12	0.19	**0.07**
Non-voter	Worry	0.12	0.19	**0.07**
Non-voter	Enthusiasm	0.16	0.19	0.03
Voter	Anger	0.81	0.88	**0.07**
Voter	Worry	0.81	0.88	**0.07**
Voter	Enthusiasm	0.85	0.88	0.03

These results confirm that the object of emotion matters for whether it influences turnout. If a respondent is angry at a news story about an alligator in the creek eating a small child, at school shootings, or police shootings, this anger might not increase the impetus to turn out. If a respondent is angry about gridlock in Congress or President Donald Trump's actions, is associated with substantively large increases in turnout, especially for those who abstained in the previous election.

Exploring additional conditional effects

The models in Tables 3 and 5, by virtue of controlling for past turnout, capture the association between emotions and *changes* in turnout. The habitual turnout literature notes that the transition from non-participation to participation and the slide from participation back to non-participation may be driven by quite different factors (Plutzer 2002). To capture whether emotions differentially impact prior non-voters and prior voters in the Politics Prompt condition, in Table A21 of the Appendix we estimate a model with anger × prior turnout, fear/worry × prior turnout, and enthusiasm × prior turnout interactions. For ease of presentation, we also include separate models of non-voters and voters and derive predicted probabilities of turnout based on moving from middling to maximum levels of each emotion, depicted in Table A22.

The anger × prior turnout interaction is positive and near-marginally significant (p=.10). Among prior non-voters, the coefficient for anger is close to 0 with a large standard error (p=.66). Among prior voters, the effect is positive, nearly identical to that of worry, and statistically significant (p=.04). The predicted increases in probability of voting are similar for non-voters (.03) and voters (.04). In contrast, the worry × prior turnout interaction is nearly 0, with a large standard error (p=.95). The coefficient for worry is similar in the models for both prior non-voters and prior voters, but only significant for prior voters (p=.17 among non-voters, p=.02 among voters). Nevertheless, moving from medium to maximum worry is associated with a .09 increase in the predicted probability of turnout among non-voters, and .05 among voters. We believe the most likely reason for this low statistical power that comes from having considerably fewer non-voters (n=143) than prior voters (n=845). The enthusiasm × prior turnout interaction is not zero but does not approach significance (p=.72), and we do not find a significant effect for enthusiasm among prior non-voters (p=.70) or voters (p=.39). Moving from

middling to high enthusiasm also only has small effects on the predicted probability of turnout (.02 increase for non-voters, .01 for voters). These findings, while inconclusive for the role of enthusiasm, indicate that anger and worry predict sustained participation by prior voters. Worry may also help convert non-voters to voters, though we cannot say so with any certainty.

Summary and Implications

Decades of research have found a strong role for emotions in political participation, self-reported turnout, and intention to vote. We advance this research program by leveraging more robust research designs to examine how emotions affect validated turnout. Once we account for prior turnout, worry caused by political stimuli emerges as a strong predictor of turnout. The effect of anger is also strong when the expressed anger is in reaction to events the respondent views as political. But even intense anger about social conditions and recent events does not spur future mobilization. As to enthusiasm, we find its effects to be weak and inconsistent for actual turnout, but powerful as a predictor of over-reporting. There is value in studying vote intentions and self-reported turnout, but our results suggest that the results from these studies should not be uncritically extrapolated to actual electoral behavior.

Emotions and Political Participation: Theoretical Implications

Some regard anger and enthusiasm as "approach" emotions that stand in contrast to fear and worry, which induce caution. Though this may be the case for forms of political participation in which people can act on their emotions immediately, our results differ from this expectation for the delayed action of voting. Affective Intelligence Theory posits that worry and fear activate the surveillance system and prompt a reconsideration of routines. With a sample limited to registered voters, we provide evidence consistent with the idea that fear may (p=.17) disrupt routines of abstention and increase the probability of voting, even in a lower turnout midterm election. This

is consistent with AIT's expectation that anxiety can break habitual behaviors by stimulating reconsideration of prior habits and spurring a search for new information. However, it is also possible that fear spurs action more directly, something that future work may be able to tease out.

Implications for Turnout Measurement and Model Specification.

Whenever past behaviors increase the likelihood of repeating the behavior in the future, cross-sectional analyses can produce misleading effect estimates, a limitation other work has acknowledged. Because turnout is characterized by strong inertia, failure to account for prior participation leaves open the possibility that ostensibly mobilizing stimuli are themselves products of participation. Despite the use of panel data for the downstream effects of emotions on candidate evaluations (Ladd and Lenz 2008) and electoral participation other than voting (Valentino et al. 2009), panel data have not previously been utilized this way on studies of emotions and voter turnout. Our results show that anger's effects on turnout are reduced and enthusiasm's strong effects on turnout disappear after controlling for past turnout. This finding, found in both our own data and in the 2016 ANES, suggests that prior cross-sectional analyses have over-estimated the effects of anger and enthusiasm.

Implications for Measuring Discrete Emotions

For mdecades, the ANES candidate-centered measures were the primary tool for measuring discrete emotions in representative sample surveys of the US electorate. Recently, there have been several attempts to ask respondents about a much wider range of stimuli. Some of this research focuses on specific events, like the Charlie Hebdo murders in France (Vasilopoulos et al. 2019). We laud efforts to cast a wider net by asking about "Generally speaking, how do you

feel about the way things are going in the country these days," (Valentino et al. 2011). [9] A similar motivation underlies emotions questions in the Collaborative Multiracial Post-Election Survey, which asked about discrete emotions "During the 2016 election season." (Barretto et al. 2018).

We believe that these kinds of approaches can come closer to how emotions can impact participation, but the generality leaves researchers with little information on the stimuli that come to mind when respondents answer these questions. Our approach not only asks respondents to focus on salient stimuli, but to then name them and rate the levels of arousal they experienced. We believe this approach can serve as an additional instrument in the repertoire of emotions and politics researchers. The embedded survey experiment, which provided a subtle cue about the kinds of political objects to be rated, shows that estimated effects are very sensitive to the object that respondents have in mind. General questions that capture mood may under-estimate emotions' effects because they may lead respondents to focus on non-political considerations. Experiments like ours, used in different settings, will help political scientists gain a better understanding of the sensitivity of results to the objects stimulated by question stems.

Implications for Future Research.

Our conclusions raise important questions about how, and under what conditions, emotions elicited by political and non-political stimuli impact different political behaviors. Experimental work by Banks (2014) and Banks & Valentino (2012) show that anger aroused by non-political stimuli can impact political attitudes and behavioral intentions. Our results suggest that these

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[9] The American National Election Studies switched to Valentino et al. (2011)'s methods of asking about political emotions starting with the 2020 ANES Time Series Study.

displacement effects may be limited if the behavior is not enacted immediately after arousal. Studies designed to demark to scope of displacement effects would help to clarify this.

This study also took advantage of a survey experiment that was not specifically designed to test our particular hypotheses. While our conclusions about the need to account for prior turnout and our results on expressive over-reporting rest on strong empirical grounds, the effects of the news/politics prompts are better characterized as suggestive and point to the need for carefully designed studies that can offer controlled comparisons of different stimuli.

Finally, we examined respondents interviewed in the lead-up to the 2018 midterms in the United States, with corroborating results from the 2016 ANES. Prior work has not specified scope conditions that might imply that these elections are poor cases to test theories of emotions and participation. Nevertheless, the 2010s have been marked by high affective polarization (Iyengar et al. 2019). Thus, it will be important for other studies to replicate features of ours in other elections and in other settings. And it will be important for experimental work to identify more micro-processes at play – especially the ways fear serves to mobilize.

Our work does not conclusively overturn existing understandings of emotion's effects on political participation. Rather, it raises important questions on how contingent observed effects of emotions have been. In the case of turnout, they appear to promote expressive responding. The effects of anger and enthusiasm seem particularly vulnerable to changes in measurement of emotions (including small changes in prompt) and model specification. Worry's effect is more

resilient to the use of best practices. However, we hope this work invites further investigation into how robust or contingent effects of emotions are on downstream political behavior.

Acknowledgements

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