Salience and Government Messaging During Crisis

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This dissertation is a collection of three essays that investigate the impact of risk salience, government messaging, and political ideology on individuals' opinions and behavior. The first essay studies the relationship between extreme weather and climate opinion. This paper focuses on the impact of variation in salience and its impact on opinion. It finds that Republicans can be shocked into adopting a favorable climate opinion by exposure to intense weather and both Democrats and Republicans suffer negative impacts from overexposure to extreme weather. The second essay looks at the relationship between evacuation orders and death during a wildfire in Paradise, California in 2018. Here, the focus is on the impact of variation in government orders on evacuation behavior while holding salience constant. It finds that evacuation orders have no impact on the probability of dying and that evacuation orders were issued with systemic bias against communities of color and low-income communities. The final essay probes the relationship between messaging and salience during the early months of the COVID-19 pandemic with an experiment on individuals' mask valuation. The focus here is on the variation of both salience and messaging – answering questions raised in the empirical studies that came from only varying one of the two independent variables. It finds that government messaging is more impactful in low-salience conditions and when those messages provide action-oriented directives rather than simply providing information. Results also suggest that Liberals may be more likely to adhere to government guidance around mask-use regardless of whether the directive is "to mask" or "not to mask". Together these essays suggest that messages from the government are most impactful when they contain a clear directive and occur early when risk-salience is low, and that ideology shapes an individual's response.

Table of Contents

List of Tables

List of Figures

Preface

This dissertation investigates the relationship between salience, opinions, behavior, and government messaging. I probe these relationships to gain a better understanding of what conditions make people more responsive to directives from their government. The first essay establishes an important baseline: that risk salience is a relevant condition. It studies the impact of salient extreme weather on individuals' climate opinion - investigating the impact of variation in salience on belief. The second looks at resident response to evacuation orders during a high-risk wildfire – holding salience constant and varying government messaging. And the third essay explores the efficacy of government messaging as a function of salience on the valuation of masks during the early months of the COVID-19 pandemic – focusing on variation of both salience and messaging.

The first essay, **Extreme Weather**, is placed in the context of the increasing polarization of the climate crisis. I seek to understand the relationship between exposure to extreme weather and individuals' opinions on climate change and the role politics plays in mediating that relationship. This study looks at how political ideology and politicized framing mediate the relationship between salient weather and climate opinion. I find that Republicans who live in areas with more shockingly salient events will respond favorably when climate issues are framed using non-politicized rhetoric. And that both Democrats' and Republicans' opinions are negatively impacted when they are oversaturated by repeated exposure to extreme weather.

While the first essay varied salience, the second essay, ***Paradise Lost,*** turns to focus on the role of government messaging when salience is constant. It explores how residents of Paradise California responded to explicit, strategic evacuation orders that were only issued to parts of the affected area. During natural disasters governments issue evacuation orders to encourage safe evacuation behavior. But not all residents take the desired action, putting themselves at risk by staying when told to leave, and putting others (and themselves) at risk by evacuating when instructed to stay. This study uses a regression discontinuity design to compare zones that received orders to those that did not. Findings suggest that evacuation orders had no significant impact on deaths. Further investigation uncovered that though older residents were disproportionately represented among the fatalities, they were also more likely to be issued evacuation orders sooner than their younger counterparts. And that low-income and BIPOC residents were less likely to be issued orders.

The final essay, ***To Mask or Not to Mask***, takes an experimental approach to answer questions about salience and messaging that were raised in the first two essays. Here, I vary both salience and messaging to see how they impact people's valuation of personal protective equipment during the COVID-19 pandemic. We test the interactive effects of displaying state COVID-19 statistics and government messages (e.g., recommendations on mask-wearing) on Americans' valuation for masks at a time when messaging about the appropriate mitigating actions was very much in flux. Our findings suggest that sending an action-oriented message early is crucial for effective government messaging. We also find that liberals are more responsive to action-oriented messaging than conservatives in both directions.

Together these studies show evidence that there are effects of partisanship, salience, and government messaging on both beliefs and behavior, but those effects are strongest in low-salience conditions. This body of work contributes to the topics of salience, government messaging efficacy, and political ideology influence on belief in the literatures of public administration, emergency and disaster management, and behavioral economics.

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1.0 Extreme Weather: Salient Weather and Climate Opinion in the United States

1.1 Introduction

The frequency and intensity of natural disasters due to anthropogenic climate change has been consistently increasing, yet as of 2018, over 25 percent of U.S. residents deny its occurrence and do not acknowledge it as a serious problem (National Surveys on Energy and Environment [NSEE], 2018). Recent research has demonstrated that even as the percent of 'climate deniers' decreases, limited risk perception means that residents routinely discount the impending risk of climate crisis as being a 'future problem' or impacting 'other places' (Ballew et al., 2019). But meaningful environmental policy to address climate change requires, at a minimum, sufficient level of belief within the public that a problem exists. One challenge to reaching a baseline level of belief among the public is that climate change is not equally salient to all people and may not be salient at all for some. While people living in states exposed to extreme wildfires like California, Oregon, and Washington may be more acutely aware of the climate crisis, some areas of the country are relatively isolated from its direct impacts and therefore may be less convinced of its urgency. Further, in an era of heightened polarization of climate issues, the problem is likely also compounded by politics.

Numerous studies have outlined a trend of increasing polarization around environmental policy beginning with the Regan administration and widening substantially over the last two decades (Dunlap & McCright, 2008; Dunlap et al., 2016; and Egan & Mullin, 2017). As the trend continues, polarization now factors heavily into environmental policy. Politics create two challenges: first, through ideologically prescribed belief that dictates opinions on climate issues, and second, through the politicization of framing climate issues that determines how new information, such as experience of extreme weather events, is interpreted. Framing effects and the inherent politicization of the language used has shown to influence how partisans respond when asked about climate issues (Schuldt & Roh, 2014). These effects are further complicated when considering how residents take cues from political elites, often strengthening the effects via political confirmation bias (Millner & Ollivier, 2020). While Konisky et al. (2016) have shown that individuals' direct exposure to extreme weather increases the likelihood of belief that climate change is occurring, Rolfe-Redding et al. (2011) show that ideological conservatives are less likely to believe that global warming is a problem to be concerned about. Following this line of inquiry, this paper explores how politics mediate the link between salient information and beliefs at the individual level. Using survey data of opinions about climate change from respondents that self-identified as Republicans or Democrats, I compare the impact of recent extreme weather events in a respondent's state to their response to climate change questions when 1) the question is framed non-politically (*"Has weather been getting more extreme over the past 40 years?"),* and 2) the question is framed politically (*"Is there scientific evidence of a global warming trend over the past 40 years?").*

While the existing literature tends to address these issues in isolation, it does not explicitly investigate these factors together. This paper explores the relationships between salience, framing, and beliefs that are mediated by individual ideology. I use extreme weather data from the National Oceanic and Atmospheric Administration (NOAA) and public opinion data from the National Surveys on Energy and the Environment (NSEE) to conduct a systematic exploration of the relationship between salience: variations in the occurrence of extreme weather events; framing: variation in how survey questions are asked, and belief: opinion about climate issues. I use a series of ordinary least squares regressions with interacted terms for a pooled sample and a sample that splits respondents by self-identified partisanship (Republicans and Democrats).

This paper answers the question*: how do politics mediate the link between information and beliefs at the individual level?* To form my hypothesis about how beliefs change with attention, I draw on a large literature in behavioral economics on saliency. "Salience refers to the phenomenon when one's attention is differentially directed to one portion on the environment rather than to others" (Taylor & Thompson, 1982, p. 175). In this context, I consider extreme weather events as a plausible exogenous shock (McCoy & Walsh, 2018) that draws individuals' attention. Studies show that weather consistently has an impact on climate opinion (Deryugina, 2013; Owen et al., 2012; Konisky et al., 2016) and on behavior (Herrnstadt & Muehlegger, 2014). More specifically, focusing of attention happens when that experience "is odd, different, or unusual." (Kahneman, 2011, p. 324), which suggests that climate opinions are impacted when attention is drawn to unusual weather. Absent politics, I expect that increased frequency and intensity of weather events would impact individual climate opinion in a positive direction.

However, I also expect the relationship between the experience of weather and belief to be mediated by ideology. Many studies point to partisanship or political identity as an indicator of opinion (Borick & Rabe, 2014; McCright et al., 2016; Shao & Goidel, 2016; Shao, 2017; Palm et al., 2017). A separate literature focused on framing brings more nuance to the impact of politics. Republicans are shown to be less primed to think politically about climate issues when the rhetoric is either framed non-politically (van der Linden et al., 2015, Schuldt et al., 2011) or more broadly, when their partisanship is not made salient (Unsworth & Feilding, 2014). Therefore, I expect that triggering respondents with political framing activates ideology effects with salience among Republicans and Democrats differently.

For this study salience is measured by proxy using two measures of extreme weather roughly following the methodology used in Konisky et al. (2016) (addressing temporal effects) and Borick & Rabe (2010) (addressing intensity effects). These measures are constructed from an unweighted sum of extreme weather events and of weather-related deaths that occurred in a respondents' state within a period leading up to the respondents' survey date. To address the role of politics, I test two lines of inquiry: the effects of ideology (using sample split by self-identified partisanship) and the effects of politicized framing (using survey responses from two questions with a politicized and non-politicized frame). With partisan groups situated in the political and non-political frames, I hypothesize three possible effects: a ***Baseline/Temporal*** effect – where recent exposure to events or weather-related deaths increases the likelihood of belief in global warming; a ***Shock/Intensity*** effect – where shocking out-of-the-ordinary events (i.e. when places with few extreme weather events experience a weather event with one or more fatality) is what increases belief in global warming; and an ***Oversaturation*** effect – where respondents are saturated by bad news about extreme weather and respond unfavorably to global warming.

I find that when asked to consider "extreme weather" Republicans and Democrats show no evidence of a Baseline effect, but Republicans do when asked to consider "global warming". Only Republicans show evidence of a positive Shock/Intensity effect when asked to consider "extreme weather", but no group shows evidence when asked about "global warming". Oversaturation effects were present for both Republicans and Democrats when asked about "extreme weather", with those effects disappearing when the issue is politicized and referred to as "global warming". To summarize, Democrats may be less likely to be impacted by salience and framing because their beliefs about climate change are already at the upper boundary. Republicans, however, present as a movable group because they are less primed to think about salient weather as a politically charged climate issue when the rhetoric is non-political (as consistent with van der Linden et al., 2015). The results highlight an important phenomenon occurring for policymakers in this context: that framing of climate issues may matter the most for the movable group (Republicans). Republicans who live in areas with more shockingly salient events (deaths across fewer events) will respond favorably when climate issues are framed using non-politicized rhetoric. But Republicans also will move closer to scientific consensus when they are exposed to weather-related deaths and the issue is politicized – an effect seen that was not captured in the predictions of this paper.

The implications of these findings highlight the difference in responses among partisans and by framing effects. They suggest that policymakers may need to craft rhetorically careful climate policy that reflects the role of salience and ideology. The broad policy-relevant recommendation from this study is to decouple climate issues from partisan politics and to develop a keener awareness of how the target audience is being impacted by extreme weather. This creates an opportunity to leverage naturally occurring variations salience to increase belief in global warming among the public. This paper contributes to the framing and belief literature, showing that politicization of frames matters in the context of climate opinion. And to the mounting evidence across literatures that partisanship impacts beliefs. It also serves to bridge the gap between these disparate bodies of literature, pinpointing a unique relationship that has not been explored fully in previous work.

1.2 Literature

1.2.1 Politics

There are three notable ways that politics influence climate opinion: first through the inherent polarization of climate and environmental issues, second individual political ideology (which interacts with polarization), and third framing effects of presenting climate issues to partisans.

Dunlap et al., (2016) have provided evidence of climate policy becoming an increasingly polarizing political issue between 2001 and 2008. But this trend began with the Regan administration and saw substantial widening of the gap as the Bush administration responded to Clinton-era involvement in the Kyoto Protocol (Dunlap & McCright, 2008). Anecdotally, this trend likely has continued as evident from the Trump administration's exit of the Paris Agreement in 2016. This polarization, though enhanced by cues from political elites, continues to be impacted by direct experience (Egan & Mullin, 2017). As such, there is now wide consensus among researchers that politics play a substantial role in climate opinion. A meta-analysis of this literature shows that studies consistently demonstrate that political ideology and party affiliation are accurate predictors of climate opinion and often overshadow the effects of direct experience with weather (Hornsey et al., 2016). One study demonstrates that Democratic values are a consistent predictor of climate opinion in a cross-national sample (Lewis et al., 2018). Another confirms that concern about global warming decreases as ideology shifts from liberal to conservative (Zia & Todd, 2010).

Researchers have also explored the role of ideology as a mediator between direct experience with climate change and opinion. One such study shows that the beliefs of Independents tend to change with the weather they experience while Democrats and Republicans are relatively unaffected (Hamilton & Stampone, 2013). Another shows that political orientation conditions the degree to which residents think weather is changing in relation to longer-term weather patterns (Shao & Goidel, 2016). Partisan impacts, though framed differently among studies tend to show ideological conservatives being less likely to accept scientific evidence of a global warming trend.

In an adjacent literature, studies have explored how the framing of climate issues impacts belief among partisans. Schuldt et al. show that the wording of questions in an experimental setting elicited difference responses among Republicans and Democrats. They find that Democrats are largely unaffected by rhetorical changes from "global warming" to "climate change" while Republicans were more willing to accept that "climate change" was occurring than "global warming" (Schuldt et al., 2011). Schuldt and Roh similarly found in a web experiment that Republicans were more primed to associate warming trends and climatic phenomena with "global warming" than with "climate change" while Democrats were equally primed to think about the two phrases (Schuldt & Roh, 2014). Interestingly, one study finds that while Republicans are more likely to react negatively to framing devices, they react even worse to the same frame that cites scientific studies (Singh & Swanson, 2017). These studies show that, though Democrats are more likely to be primed to think politically about climate- and weather-related language, their opinion is less affected by rhetorical changes. This phenomenon can likely be attributed to Democrats' movability being capped with a majority already believing that climate change is a concerning occurrence. Republicans, on the other hand, are much less likely to find climate change concerning and therefore have room to change their opinions. This goes curiously against the literature that explores confirmation bias in climate opinion – where 'climate deniers' (who are typically conservative Republicans) were shown to hold more bias in their message processing and react with extreme position polarization when presented with non-confirmational climate messages (Zhou & Shen, 2021). Another study similarly argues that individuals will weigh values they share with others like themselves more heavily than hazards associated with climate change (Kahan et al., 2015). Considering confirmation bias alone does not adequately explain climate opinion. Were that to be true, partisans on either side of the aisle would be immovable in their opinion with polarization happening by confirmatory and by non-confirmatory messaging. Instead, trends show that despite increased polarization, there has been a steady increase of support for prioritizing climate action between 2008 and 2020 (Funk & Kennedy, 2022).

Taken together, these studies suggest that while Democrats may be unmovable because they have hit a cap with sufficiently high belief, Republicans, despite having strong polarized reactions, are positioned to shift their opinion in alignment with scientific consensus.

1.2.2 Salience

Studies conclude that information gathered from personal experience of weather events can influence risk perception about climate change (Howe et al., 2013; Ackerlof et al., 2013). And within a sub-genre of the literature, studies have demonstrated that salience of weather is an important determinant of climate opinion. Meaning that the more an individual's attention is drawn to a weather event, the more that event influences their climate opinion. While in the literature there is consensus around accounting for political ideology as a mediator, there is not agreement on what makes weather salient to individuals. Some studies focus on temporal salience effects, showing that events that occurred more recently impact one's concern about climate change more than temporally distant events (Konisky et al., 2016) or that short-term temperature fluctuations have less impact than longer-term ones (Deryugina, 2013). However, these studies seem to agree, that these effects are present at around about a month of compounded weather. The first hypothesis I test checks for ***Baseline/Temporal effects*** as consistent with this body of literature, where salience effects are present from any weather present within a four-week period leading up to the respondents' survey date.

Other scholars consider intensity a determinant of salience, where more intense storms have a stronger impact on climate opinion than less intense ones (Borick & Rabe, 2010). They later showed that individuals commonly refer to weather patterns when explaining their opinions on climate change – seasonal snowfall anomalies being among the more commonly reported events (Borick & Rabe, 2014). One study points to an "unusual lack of snow" leading to an increase in Google searches for "climate change" and "global warming" (Herrnstadt & Muehlegger, 2014) which highlights that unusualness can also come from a lack of weather. The second hypothesis I test checks for these ***Shock/Intensity effects*** where salience effects are present when weather is particularly intense.

A behavioral body of literature looks at a related salience effect that this paper refers to as an Oversaturation effect. Researchers have provided evidence that individuals normalize abnormal weather quickly, calling this a "boiling frog" effect (Moore et al., 2019; Zhongming et al., 2019). For example, individuals who are repeatedly exposed to extreme temperatures show a decline in weather-related posting (Zhongming et al., 2019). And a sentiment analysis in 2019 showed evidence of the effect where the remarkability of extreme temperatures changes with repeated exposure: individuals normalize abnormal conditions quickly and stop commenting on them (Moore et al., 2019). Both studies mentioned here show that while the remarkability of weather declines with repeated exposure, people still report grumpiness and other ill effects from that exposure. This suggests, intuitively, that oversaturation or overexposure to extreme weather may present as an overall null effect on opinion. When extreme weather has been normalized, people may have an adverse effect to being asked about it and simply shut down. I test a third hypothesis in this line of inquiry that checks for ***Oversaturation Effects*** present in places with more events and deaths.

1.3 Data

This study uses cross-sectional data oriented around individual responses from the National Surveys on Energy and the Environment (NSEE, 2018) from 2014-2016. NSEE data provide the two main outcome variables: climate opinion within a non-politicized frame and opinion in a politicized frame, and the individual-level demographic controls (age, education, income, and partisanship). I also use data from the NOAA (National Oceanic and Atmospheric Administration [NOAA], 2018) storm records aggregated to the state level. The storm data make up the two main salience variables: events and deaths. Summary statistics describing these data are found in Table 1.1.

Table 1.1 Summary Statistics

n = 1,267	Variables	Mean	Median	Std.Dev.	Range
Non-Politicized Question	Has weather been getting more extreme over the past 40 years?[2] (More extreme = 1, not more extreme = 0)	0.73	1	0.44	0-1
	More Extreme	0.73	1	0.44	0-1
	Less Extreme	0.03	0	0.16	0-1
	No Change	0.24	0	0.43	0-1
Politicized Question	Is there scientific evidence of a global warming trend over the last 40 years? (1 = "Yes" 0 = "No"/"Not Sure")	0.63	1	0.48	0-1
	Yes	0.63	1	0.48	0-1
	No	0.23	0	0.42	0-1
	Not Sure	0.14	0	0.35	0-1
Salience Measures	**Deaths** (# of weather-related deaths that occurred during the 4 weeks leading up to survey date)	0.59	0	1.33	0-10
	Any Deaths (1 if at least 1 death occurred in the 4 weeks leading up to the survey date)	0.25	0	0.43	0-1
	Events (raw events during the 4 weeks leading up to the survey date	8.07	6	8.3	0-60
	Over Median Events (1 if the number of events for the four weeks leading up to survey was >= 6)	0.52	1	0.5	0-1
Controls	**Low Income** (1 if respondent's income is <$20k)	0.09	0	0.29	0-1
	College (1 if respondent has college degree or higher)	0.47	0	0.5	0-1
	Respondent Party (1 if respondent identified as Republican)	0.42	0	0.49	0-1
	State Party (1 if state government is under Republican control for the year the survey was taken)	0.59	1	0.49	0-1
	Over 65 (1 if respondent is over 65 years old at the time of survey)	0.36	0	0.48	0-1
Lagged Salience Measures	**Lagged Any Death** (29% of respondents had weather related deaths occurring during the same time last year)	0.3	0.10	0.35	0-1
	Lagged Over Median Events (47% of respondents had more than 6 events occurring in their state the same time the previous year)	0.5	0.41	0.36	0-1

1.3.1 National Surveys on Energy and the Environment (NSEE)

The first outcome variable - climate opinion in a non-politicized frame – is from respondents who were asked "*Has weather been getting more extreme over the past forty years?*". The data show that overall, 71 percent of respondents believed that weather has been getting more extreme, 25 percent believed it was about the same, while a negligible 2.5 percent believed that weather was getting less extreme. Table 1.2 shows the breakdown of responses split by party. While the literature suggests that Democrats may be more inherently primed to associate weather-related content with global warming, Republicans are much less likely to make the same association (Schuldt & Roh, 2014). Therefore, I use the question framed about extreme weather as the non-political frame to see if any effects are present for Republicans.

The second outcome variable – climate opinion within a politicized frame – comes from the question "*Is there scientific evidence of a global warming trend over the past forty years?*". Unsurprisingly, within the politicized context, 63 percent of respondents believed that there was evidence of global warming, while 23 percent believed there was no evidence, and a relatively small 14 percent were unsure. With the explicit mention of global warming, under a polarized political climate around environmental issues, this question easily triggers Republicans to think politically about this question.

The NSEE data were also used for controls, all of which are coded as dummy variables. In summary, 9 percent of respondents were considered low-income (earning under $20,000 annually), 47 percent had a college degree or higher for their educational attainment, 42 percent self-identified as Republicans, and 36 percent were over the age of 65. Table 1.1 shows the specific breakdown of respondents by party identification for Republicans and Democrats only[1].

1.3.2 National Oceanic and Atmospheric Administration Storm Data (NOAA)

Drawing from the literature I constructed the two salience variables that aim at accounting for temporal and intensity effects. The NOAA records 48 different types of weather phenomena from heavy rain to volcanic ashfall. For this study, I omit four types of events: sneaker waves, dense fog, seiche, and astronomical low tide. These are omitted because they are hyper-localized, have no associated fatalities or damages, and are likely to only have minimal exposure to residents. A sneaker wave, for example, may only be witnessed by a dozen or so people and tend to dissipate after ten minutes (US Department of Commerce: NOAA, 2021). This omits 533 events (most of which are

[1] The main tables in this paper use a pooled sample of 1,267 respondents who identified themselves as either Republican or Democrat when asked about their partisanship. That sample omits anyone who identified as "Independent", "Other", "Not Sure", and those who refused to answer. Tables in Appendix A (Tables A.4 and A.5) recreate the main tables using a larger pooled sample that includes self-identified "Independents", increasing the sample size by 609 respondents.

fog) accounting for only 2 percent of the total number of events over the three-year period (26,256 events). Appendix Table A.1 shows the breakdown of events and deaths by event type between 2014-2016. Note that the raw NOAA data designates unique identifiers "episode ID" and "event ID" to each observation. To ensure that multi-state weather events are accurately counted: deaths and events are aggregated by their episode number. This means that a hurricane (denoted in the original data as a single episode ID) that impacts 10 counties in Florida and 10 parishes in Louisiana would appear as 20 'events' in the original data. By aggregating by 'episode ID' that single hurricane is logged once with Louisiana deaths attributed to Louisiana, and again for Florida with Florida deaths attributed there. This avoids over counting for county-level impacts.

From the remaining 25,729 events, I take a snapshot of a resident's state for the four weeks leading up to their survey response date for their state. For example, a respondent from Texas who answered the survey on October 1, 2015 would have measures that aggregate from weather events between September 2, 2015 and September 30, 2015 that occurred in Texas. The *Events* measure takes the unweighted sum of events that occurred during that four-week period within the respondent's state and codes it as a one if that number is greater than the median number of events for that state. For reference, the range of events for these periods was from between 0 and 60 with the median at 6 events (mean at 8.07 events). Therefore, ***Over Median Events***, codes as one if the respondent experienced six or more events during the month leading up to their survey, zero if they experienced less than six events.[2] The second salience measure, *Deaths*, were measured in the same four-week period by state aggregated relative to the respondent's survey date but were coded one if ***Any Deaths*** occurred during that period, zero if there were no weather-related deaths. The range among the full sample was between 0 and 10, with about 25 percent of respondents being in states that experienced one or more deaths leading up to their survey date. (Please note these values are scaled by 100 for readability, therefore coefficients in regression models can be interpreted as straight percentages).

To account for longer-term weather patterns, I constructed lagged versions of the salience variables that calculated the mean for the previous year for the respondents' state. However, lagged salience measures for *Any Deaths* and *Over Median Events* were not included in the main results tables.

[2] Note that the median of six events was the lowest threshold for events where results were seen. I checked in the 75th and 90th percentiles and found that the results were robust.

Appendix Tables A.2 and A.3 demonstrate that including these terms in the main tables makes a negligible difference in effects.

1.4 Hypotheses

With this data I expect that Democrats and Republicans respond differently to salient weather. This section describes the mechanics of how ideology contributes to beliefs about climate change in a non-politicized framing context. When belief is already high (meaning that respondents are already convinced that weather has been getting more extreme or that there is evidence of global warming) respondents have no room to move their opinion any higher. Thus, in a sample where beliefs are already at a high level, I expect to see no evidence of Baseline or Shock effects. However, when belief is low (meaning individuals do not believe that weather has been getting more extreme), they can be convinced in response to experiencing unusual events. Among a sample where climate opinion is relatively low, I expect to see evidence of Baseline and Shock effects. For the pooled sample, with 73.7 percent of respondents believing that weather has been getting more extreme, I expect that belief may be low enough to show evidence of a Baseline or Shock effect. Splitting that sample by partisanship, however, will help pinpoint which subset of the population is movable in their opinion about climate change.

Looking at Table 1.2, 82 percent of Democrats believe that weather has been getting more extreme (with 2.7 percent believing it is getting less extreme, and about 14.9 percent thinking it has been unchanged over the past 40 years) – therefore, among Democrats, where belief if high, I expect to see no evidence of Baseline or Shock effects. Among Republicans, however, where belief is considerably lower with 56.9 percent believing weather has been getting more extreme (2.4 percent reporting less extreme weather patterns, and 37.3 percent reporting no change), I expect to see evidence of Baseline and Shock effects.

Table 1.2 Responses to Outcome Variable Survey Questions Split by Party

	Response	Dem	Dem %	Rep	Rep %	Total	Total %
Non-Politicized Frame: "Has weather in your state been getting more or less	More extreme	606	82.3%	320	56.9%	926	73.7%
	About the same	110	14.9%	198	37.3%	308	24.3%
	Less extreme	20	2.7%	13	2.4%	33	2.6%

extreme over the past 40 years"	Total	736		531		1,267	
	Response	Dem		Rep		Total	
Politicized Frame: "Is there scientific evidence of a global warming trend over the past four decades?"	Yes, Evidence	574	78%	224	42.2%	798	62.9%
	Not sure	91	12.4%	85	16%	176	13.9%
	No Evidence	71	9.6%	222	41.8%	293	23.1%
	Total	736		531		1,267	

While Baseline and Shock effects are expected to impact only respondents with beliefs low enough to be moved, Oversaturation should impact partisans more evenly. Because Oversaturation is a negative effect, both Republicans and Democrats have sufficiently high belief to be movable in a negative direction. Therefore, within the non-politicized frame I expect to see evidence of Oversaturation for both Republicans and Democrats.

Turning to the politicized frame, I predict that politicization makes both Democrats and Republicans become resolute in their partisan beliefs and become immovable. If this is the case, I expect to see no Baseline, Shock, or Oversaturation effects because people's political identity has been activated and overrides their personal experience of weather. To test these theories, I use the econometric model outlined below in Equation 3.1.

$$CO_i^{frame} = \alpha + \beta_1 E_{its} + \beta_2 D_{its} + \beta_3 D_{its} E_{its} + M_i + S_{ist} + \lambda_{iw} + \varepsilon \qquad (1.1)$$

Where:

CO_i^{frame} = Respondent's climate opinion within the specified frame (politicized, non-politicized)

E_{its} = 1 if >6 events occurred in respondent's (i) state (s) during the 4 weeks before the survey (t)

D_{its} = 1 if >0 deaths occurred in respondent's (i) state (s) during 4 weeks before the survey (t)

$D_{its}E_{its}$ = interaction term between Over Median Events and Any Deaths $(D_{its} * E_{its})$

M_i = individual level demographic indicators (income, age, education, partisanship)

S_{st} = gubernatorial party (1 if Republican) for individual (i)'s state (s) during year (t)

λ_{iws} = Fixed effects for state (s)and survey wave (w) for individual (i)'s response

1.4.1 Baseline Effects: $\boldsymbol{\beta_1 > 0}$

Individuals who experience either above median number of events *or* any deaths in their state during the four weeks leading up to their survey response will be more likely to think that weather has been getting more extreme. If there is evidence of a Baseline effect, then we can expect to see $\beta_1 > 0$. This is tested in a non-interaction model where the interaction term (β_3) drops from Equation 1. Baseline effects can be expected in places that experienced more events. Evidence of an effect here indicates that belief about the extremeness of weather increases with increased occurrences of extreme semi-local weather. In this case, people's attention is drawn toward weather when it is more frequent. Democrats should not show signs of experiencing Baseline effects in either frame. Republicans, as the movable group should show evidence of effects in the non-political frame but not in the political frame.

1.4.2 Shock Effects: $\boldsymbol{\beta_2 > 0}$

Individuals who experience any deaths across fewer events in their state during the four weeks leading up to their survey response will be *more* likely to think that weather has been getting more extreme. If there is evidence of a Shock/Intensity effect, then we can expect to see from Equation 1: $\beta_2 > 0$. Shock/Intensity effects can be expected in places with deaths but fewer events. Here, people's attention is drawn to the 'unusualness' of the events: few intense events that resulted in fatalities. Intuitively, for places that typically experience mild weather but have a single weather event with fatalities, that event is likely to draw attention more effectively than places that routinely have weather-related deaths (such as areas prone to wildfire and hurricanes). In this case, the attention directed to the intensity of a weather event serves to highlight it as a problem. Democrats should not show signs Shock/Intensity effects in either frame. Republicans, as the movable group, should show evidence of Shock/Intensity effects in the non-political frame but not in the political frame.

1.4.3 Oversaturation Effects: $\boldsymbol{\beta_3 < 0}$

Individuals who experience any deaths *and* more than six events in their state during the four weeks leading up to their survey response will be *less* likely to think that weather has been getting more extreme. Evidence of Oversaturation effects will be indicated by a negative coefficient on the interaction term: $\beta_3 < 0$. This effect can be expected in places with more events and deaths where people may have normalized extreme weather. In this case people's attention has been drawn to the weather over an extended time and are experiencing the negative effects on climate opinion from over-exposure. Democrats and Republicans should both show signs of an Oversaturation effect in the non-political frame, but neither should show signs in the politicized frame.

1.5 Results

1.5.1 Non-Politicized Frame:

Table 1.3, Models 1-2 show the coefficients for *Any Deaths* and *Events* on their own among the pooled sample, with no significant results. Models 3-5 show results for the non-interacted model where evidence of a Baseline effect would be present. Among the pooled sample (Model 3), Republicans (Model 4), and Democrats (Model 5) I fail to reject that $\beta_1 = 0$, suggesting no evidence of a Baseline effect. While it was expected that Democrats, as the non-movable group, should not show evidence of this effect, Republicans might have. The lack of evidence, however, does not necessarily go against the extant literature. Since this study uses a definition of 'semi-local' weather as occurring at the state-level (rather than county-level), this may simply indicate that using a broader geographic identifier might not be salient enough.

When looking at the interactive effects of *Any Deaths* and *Events* on the opinion, there is evidence of Shock/Intensity effects. For the pooled sample (Model 6) there is evidence of a Shock/Intensity effect where $\beta_2 = .1$ (p= .068). This suggests that respondents who live in areas that had fewer than median events and experienced any weather-related deaths were more likely (by .1 percent) to think that weather has been getting more extreme. This effect, however, is shown to be driven entirely by Republicans as seen in Model 7 where $\beta_2 = .19$ (p= .054), while there was no for Democrats alone (Model 8 where I fail to reject that $\beta_2 = 0$). This effect is consistent with two parts of the extant literature – first where salience, defined by its shockingness or intensity, impacts climate opinion. And second, where Republicans, as the movable group and when climate change related content is framed without politically charged rhetoric, will demonstrate evidence of salience effects.

Similarly, Table 1.3, Models 6-8 test for Oversaturation effects. In all models, $\beta_3 < 0$ which suggests consistent with the literature, that people respond negatively when they have normalized extreme weather. For the pooled sample (Model 6), where $\beta_3 = -.16$ (p= .02) and among Republicans (Model 7) where $\beta_3 = -.25$ (p= .06). Models 6 and 7 therefore show evidence of a negative Oversaturation effect for Republicans and among the pooled sample. Testing the linear combination of any deaths and the interaction term ($\beta_2+\beta_3 = 0$) suggests that overall, there is no significant effect. Democrats, however, show in Model 8 show that $\beta_3 = -.18$ (p= .02), indicating presence of Oversaturation and $\beta_2+\beta_3 = -.11$ (p= .04) where the overall effect for Democrats is negative. The results for both Republicans and Democrats seem consistent with the extant literature, where people normalize extreme weather as a usual occurrence and react less to it.

Table 1.3 Weather Impact on Non-Politicized Opinion - Split by Party

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Pooled Sample	Pooled Sample	Pooled Sample	Reps	Dems	Pooled Sample	Reps	Dems
Over Median Events	-0.04		-0.03	-0.02	-0.00	-0.00	-0.00	0.03
	(0.04)		(0.04)	(0.08)	(0.04)	(0.04)	(0.08)	(0.05)
Any Deaths		-0.04	-0.03	-0.02	-0.06	0.10*	0.19*	0.07
		(0.05)	(0.06)	(0.09)	(0.05)	(0.05)	(0.10)	(0.06)
Over Med Events * Any Deaths						-0.16**	-0.25*	-0.18**
						(0.07)	(0.13)	(0.08)
State Party	0.02	-0.03	0.03	-0.02	0.07*	0.05	-0.01	0.08**
	(0.05)	(0.05)	(0.05)	(0.08)	(0.04)	(0.05)	(0.09)	(0.03)
Respondent Party R	-0.22	-0.22	-0.23***			-0.22***		
	(.02)	(0.03)	(0.02)			(0.02)		
College	0.06	0.06	0.06**	0.03	0.08**	0.06**	0.03	0.08**
	(0.02)	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)
Low Income	0.01	.001	0.01	0.10	-0.03	0.01	0.10	-0.03

	(0.04)	(0.04)	(0.04)	(0.08)	(0.05)	(0.04)	(0.08)	(0.05)
Over 65	-0.01	-0.01	-0.01	-0.06	0.01	-0.01	-0.05	0.02
	(0.03)	(0.03)	(0.03)	(0.06)	(0.03)	(0.03)	(0.06)	(0.03)
Constant	1.02***	1.02***	1.21***	1.06***	0.87***	1.15***	0.98***	0.83***
	(0.04)	(0.04)	(0.06)	(0.10)	(0.05)	(0.06)	(0.10)	(0.05)
Linear Combination: B2 + B3						-0.07	-0.06	-0.11**
						(0.06)	(0.09)	(0.05)
Observations	1,267	1,267	1,267	531	736	1,267	531	736
R-squared	0.09	0.09	0.09	0.09	0.07	0.10	0.09	0.07

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

1.5.2 Politicized Frame

Table 1.4 similarly shows the coefficients for *Any Deaths* and *Events* on their own among the pooled sample, with no significant results in Models 1 and 2. While the pooled sample in Model 3 shows no evidence of a Baseline effect (fail to reject that $\beta_1 = 0$), Republicans show evidence of being impacted by the presence of any weather-related deaths in Model 4. Here, $\beta_2 = .15$ (p= .026) indicates that Republicans exposed to weather-related deaths are .15 percent more likely to believe that there is scientific evidence of global warming. While not part of the explicit hypotheses, these results seem intuitively consistent with the literature: perhaps weather-related deaths (regardless of the number events they occurred in) are salient. While these results would be more at home in the non-politicized frame, here we are seeing that Republicans who are activated to think about climate change from a political standpoint and live in areas with weather-related deaths are curiously more movable than when politics are left aside. Model 5 confirms that there is no Baseline effect for Democrats (fail to reject that $\beta_1 = 0$) as expected.

Models 6-8, report results that include the interaction term between *Over Median Events* and *Any Deaths*, show no evidence of Shock/Intensity effects and I fail to reject that $\beta_2 = 0$ for all models. In this context, politicizing the framing of the question activates Republicans' political ideology and the intensity of effects is overridden by conservative rhetoric that confirms their existing bias.

Democrats, since they are unmovable due to an existing high level of belief, may be experiencing a similar effect but are already maxed out in their ability to present as movable. However, this speculation cannot be confirmed with the clumsiness of the dummy variable capturing opinion (see the following section for additional tests of robustness that address this issue). Models 6-8 show no evidence of Oversaturation effects, where I also fail to reject that $\beta_3 = 0$ for all models (and the linear combination for Model 7) confirms the overall impact of deaths for Republicans shown in Model 4).

While many of the findings are consistent with extant literature, one anomaly worth noting is the positive coefficient ($\beta_2 = .15$ in Model 4, p= .026) among Republicans. This may suggest that the overall effect of being exposed to any weather-related deaths is positive for Republicans when they are asked to consider global warming. This is perhaps an anomaly in the data, but it may warrant further investigation.

Table 1.4 Weather Impact on Politicized Opinion - Split by Party

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Pooled Sample	Pooled Sample	Pooled Sample	Reps	Dems	Pooled Sample	Reps	Dems
Over Median Events	0.02		0.00	-0.03	0.07	-0.01	-0.02	0.05
	(0.05)		(0.05)	(0.07)	(0.06)	(0.05)	(0.07)	(0.05)
Any Deaths		0.06	0.06	0.15**	-0.02	-0.03	0.22	-0.11
		(0.04)	(0.04)	(0.06)	(0.04)	(0.09)	(0.17)	(0.08)
Over Med Events * Any Deaths						0.11	-0.09	0.14
						(0.11)	(0.18)	(0.11)
State Party	-0.05	-0.06	-0.06	-0.13	-0.08*	-0.07	-0.12	-0.09**
	(0.05)	(0.05)	(0.05)	(0.12)	(0.04)	(0.05)	(0.12)	(0.04)
Respondent Party R	-0.36	-0.36	-0.36***			-0.36***		
	(0.03)	(0.03)	(0.03)			(0.03)		
College	0.12	0.12	0.12***	0.10**	0.16***	0.12***	0.10**	0.16***
	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.04)
Low Income	0.00	.001	0.00	-0.02	0.01	0.00	-0.02	0.01
	(0.04)	(0.04)	(0.04)	(0.10)	(0.04)	(0.04)	(0.10)	(0.04)
Over 65	0.01	0.01	0.01	-0.05	0.07**	0.01	-0.05	0.07*
	(0.03)	(0.03)	(0.03)	(0.06)	(0.03)	(0.03)	(0.06)	(0.03)
Constant	.78***	.79***	0.73***	0.48***	0.72***	0.77***	0.45***	0.75***
	(0.06)	(0.05)	(0.07)	(0.13)	(0.07)	(0.08)	(0.14)	(0.07)

Linear Combination: B2 + B3						0.08	0.13**	0.02
						(0.06)	(0.06)	(0.06)
Observations	1,267	1,267	1,267	531	736	1,267	531	736
R-squared	0.11	0.10	0.20	0.12	0.16	0.20	0.12	0.16

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

1.5.3 Robustness Checks

I tested these results for robustness along four dimensions. First to confirm that the effects seen from one answer to the outcome variable question were going to be consistent with the other answers. For the Non-politicized frame, the question "*Has the weather been getting more or less extreme over the past 40 years?*" could be answered with "*Yes, more extreme*", "*No change*", and "*No – less extreme*". Appendix Table A.2 shows the outcome variables as dummies – Models 1 and 2 show the same results seen in the main tables for the pooled samples: no Baseline or Oversaturation effects and evidence of a Shock/Intensity effect. Models 3 and 4 change the outcome variable to be the effects on respondents who thought weather was either *Unchanged* (Model 3) or *Less Extreme* (Model 4). The significant effect in Model 3 where $\beta_2 = -.13$ suggests that people exposed to shocking/intense weather (fewer than six events and any death) are less likely to think that weather is unchanged. I assert that the shock/intensity effect being present here helps to bolster the results seen in the main table. Appendix Table A.3 confirms the results seen in Table 1.3 for the politicized frame – here the dummy responses are "*Yes, evidence of global warming*", "*No evidence*", and "*Not sure if there's evidence*" when asked "*Is there scientific evidence of a global warming trend over the past four decades?*". Here, consistent with the results above, there is no evidence of a significant Oversaturation effect.

The second consideration I checked was to account for longer-term weather trends. Model 5 of Appendix Tables A.2 and A.3 also added a lagged version of the measures for events and deaths (more details provided with Appendix Table A.2). Briefly, this check determined that the results seen when accounting for lagged salience measures were not different enough to justify their inclusion in the main model specifications.

Third, I checked to see what impact omitting respondents who self-identified as independents had on the results. While the literature tends to reference Democratic and Republican partisanship considerably more than other parties, there is also a clear trend that the effects attributed to partisanship may be more accurately linked to ideology. Since the NSEE survey collects partisan specific data, and because there is no supplemental question to deal with an ideological degree, independents are wildcards in how they will respond in this context. As mentioned earlier, one study showed that independents' climate opinion is movable by direct experience while Democrats and Republicans are unaffected (Hamilton & Stampone, 2013). If independents are movable, I would imagine them to be movable along the same axes of ideology: more conservative-leaning independents should respond like Republicans and more liberal-leaning independents like Democrats. For this data, Appendix Table A.4 reports non-politicized framing results where there is no evidence of Baseline effects but curiously, an overall effect from *Any Deaths* on independents ($\beta_2 = .1$ significant at 5% level). Like results from the main table, there was evidence of Shock/Intensity effects among the pooled sample and Republicans with the added effect present among independents. Oversaturation, too, was seen in present among the pooled sample, Republicans, and Democrats, but not among independents. In the politicized frame (shown in Appendix Table A.5), similar results are bolstered with the addition of independents – Baseline effects are absent, but the overall effect of deaths become widespread enough to show up in the pooled sample when Republicans and independents are both experiencing them. Finally, there is no evidence of Shock/Intensity or Oversaturation effects, consistent with the main results.

Lastly, I tested for negligible effects of null results to determine if the lack of effects were both statistically insignificant and substantively meaningless. To accomplish this, I constructed a 90 percent confidence interval around the coefficients of interest with null results (Appendix Table A.6 lists these tests with coefficients, standard errors, and confidence intervals). Rainey suggests identifying the 'smallest substantively meaningful' value of the outcome variable's estimate and identifying if the confidence interval around the estimate contains such a value (Rainey, 2014). However, in this instance, where both independent and dependent variables are binary, it is more useful to consider if the extents of the confidence intervals are, themselves, substantively meaningful values. The largest (in magnitude) value among these limits is found on the β_2 Coefficient in Table 1.4, Model 7, where a significant coefficient would suggest evidence of a Shock effects. Here, where $\beta_2 = .22$ and $SE = .17$, the confidence interval extends to .49965 on its upper limit. This suggests that being in areas where there were any deaths, but less than six events would impact the likelihood of adopting a favorable climate opinion by just less than half of a percent. I argue that for a coefficient to be substantively meaningful, it would need to be greater than half of a percent (in absolute value). Therefore, without any of the confidence intervals containing such a value, it is likely that these are truly null effects rather than imprecision in the estimation (Rainey, 2014).

1.6 Discussion

Before looking at the implications of these findings – I first ask why Republicans are not acting as expected. In a non-politicized context, we see that they are not as movable as hypothesized. One explanation is that there is not a sufficient difference in framing to constitute a non-political context. By nature, a survey called "National Survey on Energy and the Environment" may have already primed Republicans (and Democrats) to be thinking politically about these questions regardless of phrasing. If we consider that all respondents are primed and there is no difference except in wording between the two frames – then Republicans are acting very closely to how we would expect. Their ideology has made them resolute in their opinion and they become movable only when shocked by extreme weather (in the 'non-political' context) or exposed to weather-related deaths (in the 'political' context). If we shift perspective and consider the frames analogous in their degree of politicization, these results suggest simply that Republicans' opinions about weather may be movable by exposure to more intense events, and their opinions about climate change may be movable by exposure to weather-related deaths. In both cases, state-level exposure to death is a necessary condition for movability.

If we are convinced that the frame that uses extreme weather language is sufficiently non-political to constitute a meaningful difference in framing, then the implications of this study track more closely with the extant literature. Politicization of climate issues serves mainly as a divisive mechanism that encourages partisans to dig their heels in and align more closely with their existing beliefs, but this study suggests that salience can override that effect. Climate-related deaths are particularly impactful for Republicans, but there is a noticeable difference in how deaths impact beliefs between the frames.

For policymakers, these findings suggest that developing an awareness of audience may be a useful course to encourage pro-environmental beliefs. Simple shifts in how climate issues are framed in ballot referenda that accounts for a region's exposure to extreme weather could have meaningful impact. Application in practice of these findings, however, seems largely impractical for the typical government operation. These findings require a clear understanding of constituents' ideology, a detailed picture of semi-local weather conditions, and careful attention to rhetorical choices when constructing ballot measures. While it may seem that the simpler solution is to use non-political language when engaging the public on climate issues, the literature has demonstrated that over the past thirty years the polarization of climate issues has lowered the threshold at which partisans' ideology is activated. Thirty years ago, where one would have to explicitly say "global warming" to trigger an ideologically powered response, now adjacent language of 'climate change' and 'extreme weather' may trigger a similar response.

Going forward, more research should be conducted either in an experimental setting where researchers have a higher degree of control over framing or as a longitudinal study where individuals climate opinions are checked repeatedly over time. A substantial limitation to using cross-sectional data is that it provides a blurry picture of an overall effect among the public, longitudinal data could provide a clearer picture of how individuals opinion changes over time.

2.0 Paradise Lost: Evacuation Order Efficacy During the 2018 Paradise, CA Wildfire

2.1 Introduction

During crisis, one of the most important roles of government is to provide guidance to residents. This messaging role can serve a life-saving purpose during extreme natural disaster events such as hurricanes and wildfires, when residents are often faced with the difficult and costly decision to evacuate or shelter in place. However, one common problem is that residents do not always follow evacuation orders. Either they go against direct orders (stay when told to evacuate), or having not been issued orders, decide to evacuate anyway. During hurricane Katrina's mandatory evacuation order an estimated 20 percent of residents did not evacuate ahead of the storm (Johnson, 2006, p. ii). Many who stayed were trapped on rooftops awaiting search and rescue teams (Committee on Homeland Security and Governmental Affairs, 2006, p. 8). Conversely, when residents choose to evacuate prematurely (before they are ordered) or when their area is not issued orders, traffic congestion causes increased danger on roadways. Bottlenecking of evacuation routes has been documented during natural disasters such as hurricane Rita where 6.3 million Florida residents evacuated on only three major highways (Harten et al., 2018). Fatality analysis of Rita showed that 51 percent of deaths attributed to the storm were residents found non-responsive in their vehicle (Zachria & Patel, 2006). Understanding how residents respond to government messaging during crisis is crucial to developing effective disaster management strategies.

Trying to identify the causal link between government evacuation orders and residents' evacuation decisions is an ongoing challenge of researchers and practitioners. In this paper, I study the effect of evacuation orders on resident safety in the case of the deadliest wildfire in California's history: the 2018 Camp Fire. Due to the unique conditions present in this fire, I use deaths geo-located inside of residential structures as an unbiased measure of non-evacuation. Residents who were ordered to evacuate were presumably at higher risk than those who were not, explaining why they were ordered to evacuate. Therefore, without a strategy to isolate the effect of orders on evacuation, there will be a non-causal positive correlation between receiving an order and evacuation status that makes causal inference challenging. To address this issue, I identify areas within the perimeter of the fire that are comparable in their exposure to risk by selecting residential structures that fall on either side of evacuation zone boundaries. This identification strategy allows for comparison of reasonably similar groups by exploiting the change in evacuation orders while holding actual risk constant. To accomplish this, I employ a regression discontinuity design with controls for socio-demographic indicators. Another issue is the notorious challenge of accurate evacuation information – studies commonly rely on ex-post self-reports in survey data. By using death as an outcome to measure evacuation status, rather than self-report, I overcome the problem of recall and social desirability bias associated with those studies.

I find that evacuation orders had no significant impact on death. Structures inside the burn perimeter that were issued an order were equally likely to have fatalities occur there as their counterparts that were not issued orders. This result is robust to various specifications and bandwidths. However, further research is needed to explore these results as the null findings may be imprecisely estimated.

To follow the main findings, I explore potential bias in where and when orders were issued and the impact of socio-demographics on deaths. I find that residents over 65 years of age were disproportionately represented among the fatalities (as is consistent with previous research), and that census blocks with a higher percentage of elderly residents and those with higher population density were more likely to be issued an order to evacuate. Further, that areas with higher percentages of low-income and BIPOC-identifying residents were *less* likely to be issued orders (and waited longer for orders when they were issued). The implications of the follow-up questions suggest two things: first that the effect of the evacuation order on evacuations could be underestimated. However, after controlling for age and other demographics explicitly, I still find no significant effect of evacuation orders on deaths. And second, that there was systematic bias in how orders were issued.

This paper provides evidence that sometimes evacuation orders do not affect evacuation behavior. In cases where the impending risk is clear, residents may act regardless of government messaging. In the case of the Paradise wildfire, where risk is highly salient (residents can see smoke and flames approaching), one possibility is that residents chose evacuation without prompting from the government, because the threat was so clear and messaging from the government was not needed. Another possibility is that the orders themselves were not salient and residents were not aware of the orders. Both possibilities could help to explain why there was no clear effect of evacuation orders on resident safety. Because of the severity of the fire, whether residents were ordered to leave within the studied time or not, they would have had to evacuate to survive. So, it is quite good that those who did not receive explicit orders chose evacuation, otherwise the death toll would have been astronomical. This feature sets Paradise as a disaster with a clear need for residents to evacuate, which highlights the importance of salience in a way that many studies of disaster do not. Unlike other studies that have found a significant effect of orders on evacuation (Kim & Oh, 2015; Houts et al., 2010; McLennan et al., 2019) the findings of this work suggest that other studies may have suffered from omitted variable bias, interpreting excess noise as an effect. With the inclusion of a measure of saliency of the impending disaster, the null findings provide a more complete estimate and thus may be an important area for future study as a determinant of order efficacy.

Theoretically, when salience, or top-of-the-mind awareness of risk, is low among residents, government messaging plays an important role in informing residents of a danger that they may not be aware of previously. Messages in this context also importantly provide information about corrective action to take. But as salience increases, information is provided to residents from multiple sources (news, media, and peers) and thus the need for government as an information provider is diluted. To begin understanding the role of salience, this paper provides one point of information in a case where salience is high and constant. To develop a more robust understanding of the role of salience and messaging on behavior, future work will need to consider conditions where salience of risk is varied across the affected population. Armed with a better understanding of the limits of compliance during crisis and a foundation for understanding the role salience plays, public administrators may be more prepared to coordinate residents' risk-mitigating behavior through government messages.

2.2 Literature

The broad question this paper seeks to address is understanding the efficacy of government messaging on risk mitigation. Messaging may come in the form of orders, recommendations or guidelines from areas including consumer protection, food and drug warnings, public health, or disaster preparedness. But there is considerable debate about whether government orders are effective and what conditions impact that efficacy. Natural disaster provides a unique context because large geographic areas are often exposed to risk without impunity and the orders themselves are often clear, action-oriented, and apply to all residents within a defined population. While there may be differences in how those exposed are prepared for or recover from disaster, there is typically consistency among exposure to the disaster and the issuance of orders. Focusing on natural disaster compliance narrows the scope of this study to uncovering the causal relationship between government orders, compliance, and outcome. Debate among scholars as to when and why orders are effective has produced a growing literature on evacuation compliance. A study of hurricane Katrina showed that order compliance was dependent on an individual's confidence in the capacity of Federal Emergency Management Agency (FEMA) and their awareness of local disaster preparedness policy (Kim & Oh, 2015). Others have considered the *type* of government message as predictive of compliance, finding that mandatory orders, instead of voluntary recommendations, have shown to increase the compliance rate even without enforcement (McLennan et al., 2019). During the 1979 nuclear accident at Three-Mile Island, evacuation compliance with voluntary recommendation was only at 8 percent over a three-day period but when it became a mandate, compliance rose immediately to 40 percent on the first day (Houts etal., 2010 via Dombroski et al., 2006).

2.2.1 Trust in Government

However, even mandatory orders do not guarantee compliance. Evidence of such is seen in the cases of Hurricane Katrina where evacuation rates were around 80 percent (Johnson, 2006, pg. 35) and during the nine-mile island incident rates were also at around 80 percent at their peak (Houts et al., 2010). The literature proposes three general explanations for non-compliance. First, residents may not find their government to be a credible source of information. Research suggests that governments that take an overly cautious approach to disaster, relying on widespread evacuation default, build a reputation for "crying wolf" which ultimately erodes residents' confidence that evacuation orders are serious (Dow & Cutter, 1998). Similarly, confidence in mandates erode when governments fail to issue evacuations when natural disasters cause severe damage. Importantly, this trade-off between type I (failing to recognize severity of risk) and type II (crying wolf) errors (Sobel & Leeson, 2006) is context-specific and built on repeated resident observations of government orders and the outcomes they experience. Importantly, trust in government serves as a mechanism that links partisanship to compliance with government recommendation. A study of vaccination compliance showed that compliance with federal recommendations to vaccinate was dependent on the incumbent executive office (Krupenkin, 2021). Political views have also been shown to impact evacuation compliance, showing that residents' partisanship was reliable indicator of evacuation after Rush Limbaugh's famously anti-preparedness rhetoric that followed Hurricane Irma (Long et al., 2019). In this case, conservative residents were more likely to be sympathetic with Limbaugh's messaging, and thus less likely to evacuate ahead of Irma (Long et al., 2019).

2.2.2 Relationship with Risk

A second explanation for non-compliance is that residents' behavior is also driven by their relationship with risk. Studies that approach compliance from a risk perspective show that residents' decisions to evacuate rely on past experiences (Riad et al., 1999; Tinsley et al 2012; Meyer et al., 2018) their perception of current risk (Dash & Gladwin, 2008), and the opinions and actions of trusted peers and family (Mileti et al., 1992). In a study of U.S. wildfires, findings suggest that resident response depends on attitudes toward risk and the type of cues residents receive such as messages from media and government (McCaffrey et al., 2018). The role of risk perception is illustrated nicely in a study that showed 11 percent of the sample intended to stay to defend their property during wildfire evacuation regardless of the risk and roughly half intended to stay until the impending threat was imminent (McCaffrey & Winter, 2011; Vogt et al., 2011). The implication is that residents have differing evacuation thresholds as related to their relationship with risk. One potential flaw with risk-oriented studies is that perceived risk can be a subjective and biased measure if residents have different experience with natural disaster and their overall risk preferences. Therefore, this study uses an unbiased measure of risk to move away from the potentially misleading conclusions drawn from those that use perceived risk.

2.2.3 Difference in Ability

The literature also argues that there are socio-demographic characteristics that prevent residents from evacuating, even if it is their preference to do so. Studies show that low socioeconomic status residents are more likely to face hardships in executing an evacuation (Fothergill & Peek, 2004), and so are women (particularly those with children), the elderly, unhoused people, and communities of color (Flanagan et al., 2011). During Hurricane Katrina, compliance with evacuation mandates was considerably lower among African Americans (Elder et al., 2007) and low-education residents (Thiede & Brown, 2013). Age has also been identified as a consistent predictor: The Center for Disease Control found that over 70 percent of victims of Katrina were over the age of 60 despite making up only 15 percent of New Orleans residents (Benson, 2015). Income, education, race, ethnicity, and partisanship have also been shown to be predictive socio-demographic indicators of evacuation compliance (Long et al., 2019). Therefore, in this study, I control for many of these indicators to ensure that collinearity is not skewing results.

2.2.4 Lack of Awareness

Finally, a fourth explanation for non-compliance with orders may be explained by a lack of awareness of the orders. A study of Hurricane Irene in North Carolina showed that around 66 percent of residents were unaware of any order to evacuate (Wallace et al., 2016). However, despite such a low rate of awareness, around 28 percent of those surveyed evacuated before the storm made landfall (Wallace et al., 2016). This indicates that residents were using other factors to make decisions (such as experience, neighbors, etc.). This can be set in contrast with another study of community response to flood warnings in the United Kingdom which showed that 90 percent of residents ignored evacuation orders (despite being in a clearly risky position) but demonstrated that only three percent of those people were unaware of evacuation orders (Pfister, 2002). These cases together begin to illustrate how evacuations happen when residents are not aware of official orders. Studies have shown that evacuations (or sheltering in place) can been considered contagious or cascading behavior (Stein et al., 2010), where residents, without being aware of the 'official' recommendation, use neighbors as a heuristic (Bikhchandani et al., 1992). Tversky and Kahneman (1974) identify this phenomenon as anchoring heuristic for response to risk: residents would be more likely to evacuate if their neighbors do because they trust that the shared experience during past events more accurately motivates the behavior they are observing. Keeping this in mind, in the case of Paradise, it is possible that very few people were aware of the explicit government orders but instead used their neighbors as a gauge for risk.

2.2.5 Gap in the Literature

With empirical disaster studies being a relatively newer trend, there are several gaps in the literature. First, the reasons for noncompliance discussed above are usually studied separately and often even studied in separate fields. Additionally, earlier studies are often theoretical because outcome behavior is difficult to observe. Without direct observation of evacuation, many empirical researchers rely on the use of potentially biased measures (Meyer et al., 2018; Dash & Gladwin, 2007) such as survey data collected after the event to compare characteristics of residents who evacuated to those who did not (Stephens et al., 2009). While such data has the benefit of gaining individual-level measures of socio-demographic indicators, it suffers from reporting bias, inaccurate recollections of actions and decision-making processes, and the exclusion of non-evacuees who died. One recent study, Long et al. (2019), has utilized cell phone geolocation data to obtain an independent measure of evacuation for residents' movement before a hurricane – this type of data, however, is not widely available to researchers. That particular study also did not adequately control for the level of damage or danger faced for residents, and hence are unable to eliminate inherent differences in disaster risks between those who received evacuation orders and those who did not. This method could have led to a biased estimate of the effectiveness of orders if those who receive an order were subject to higher risk if they had chosen not to evacuate than those who did not receive an order.

2.3 Background

With a history of cyclical drought, wildfires, and flash-flooding, Butte County in Northern California, has developed comprehensive emergency and evacuation plans to mitigate the impact of such emergencies. The Butte County Office of Emergency Management (OEM) maintains zone-oriented evacuation plans in the event of widespread hazards. These evacuation plans divide the City of Paradise and surrounding communities to the North into 33 distinct evacuation zones that segment the region along major roadways and natural boundaries. They were developed to better serve the community by providing an organized approach to evacuating to avoid roadway congestion during area-wide evacuation efforts.

After Northern California experienced an exceptionally dry fall, environmental conditions in the area were ideal for a fast-moving wildfire that would be difficult to contain. On the morning of November 8th, 2018 just before 6:30am, Butte County emergency responders received reports of a small fire under powerlines. By 8:00am the fire had reached the town limits of Paradise roughly six miles away. In the first four hours of the fire, the Butte County Sheriff's department issued orders to zones perceived to be in the most immediate danger. Strategically issued orders that target residents by zone are meant to reduce roadway congestion that had occurred with town-wide evacuation orders in previous fires (St. John et al., 2018). While many areas in the country use geolocated push-notifications from cell phone towers through the National Weather Service, Butte County employs a privately owned, opt-in alert system called Code Red to relay its messages. The system has been criticized as having minimal reach: only about 25 percent of Paradise residents were enrolled at the time of the fire and only about a quarter of those enrolled were sent an order during the fire (St. John et al., 2018). The messages are often amplified through social media, such as Twitter, which provides more accessible source of evacuation orders. The diffuse reach of alerts via social media provides the benefit of being more widely available and unincumbered by the limitations of auto-dialer systems, but that wide reach may mean that those who were not directly issued orders may have followed orders for neighboring zones instead.

By noon CALFire's Chief conceded that the 18.6 square miles of Paradise had been lost to the blaze (Boghani, 2019). Over the next seventeen days the fire continued to spread and smolder throughout the region, only reaching full containment on November 25 after burning over 150,000 acres, causing $16.5 billion in damages (Ruiz-Grossman, 2019), destroying over 18,000 structures, and claiming 85 lives (Butte County District Attorney's Office, 2020). It has been characterized among the top twenty most destructive wildfires on record and the most deadly and destructive in California's history.

2.4 Methods

To effectively study the impact of evacuation orders on behavior I use a regression discontinuity design (RDD) to establish the efficacy of orders when government credibility and risk faced is constant. The primary goal of the RDD is to isolate the effect of orders and establish a causal relationship between receiving an order, not evacuating, and dying as a result. To accomplish this, I engage in a structure-level analysis of residences within the burn perimeter. By using the borders between zones that received dissimilar orders as the discontinuity cut-off and examining only those structures that fall within an optimal bandwidth surrounding the border, I establish a comparison between the treatment group (those who received evacuation orders) and control (those who did not receive orders).

This study uses a measure of evacuation behavior, deaths in residential structures, by exploiting the damage to the affected area to create a proxy for evacuation. Given the high level of damage residential structures sustained across the burn area, with 96 percent of structures destroyed, I use deaths geolocated to residences as a proxy for evacuation attempts. Structures that were destroyed but did not have a death occur there are assumed to have their inhabitants evacuated. Those residences that had at least one fatality occur were assumed to house residents who did not evacuate. This measure is discussed in further detail in the data section.

Figure 2.1 lays out the general RDD concept applied to this case with a structure's distance from the border of interest (measured in miles) on the x-axis, and probability of a residential structure containing at least one death on the y-axis. The cutoff point is marked as zero with positive distance values to the right representing the treatment group (orders issued) and negative distances to the left as the control (no orders issued). By restricting the sample of structures to a bandwidth surrounding those borders of interest, I controlled for several important confounding factors. First, residents on either side of dissimilar zone borders within that narrow bandwidth share similar experience with their government in the context of evacuation, allowing us to hold the credibility of government constant. Second, residents within proximity to one another on opposite sides of evacuation zone borders face similar actual risk from the fire. This allows me to test for a causal link between evacuation orders and attempts to evacuate, as measured by death inside a residence.

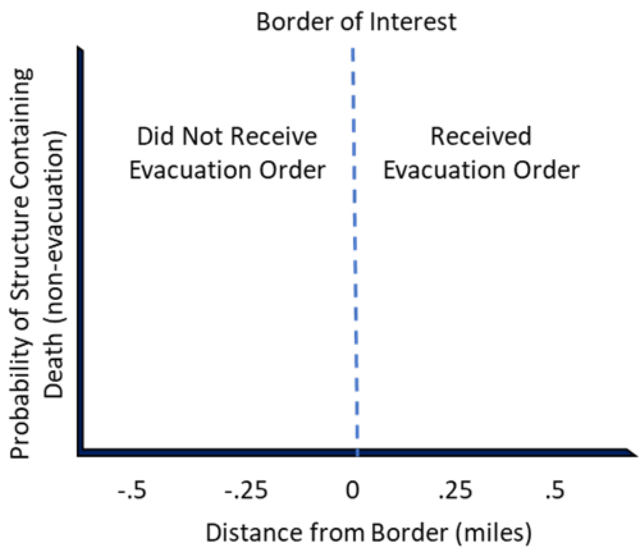

Figure 2.1 Regression Discontinuity Setup

I use Calonico, Cattaneo, and Titiunik's (2014) method for bandwidth selection which employs Stata package 'rdbwselect' to generate an optimal bandwidth (.45 miles on either side of the cut point) based on the full data set. To deal with the potential issue of this selection method leading to bandwidths that are wider than standard confidence intervals can validate, I performed robustness checks among other bandwidths. First, I selected a smaller-than-optimal bandwidth (.25-miles surrounding cut point) which 'undersmooths' the estimate to validate the results at the optimal bandwidth (Keele & Titiunik, 2015). I also test amongst a larger bandwidth (1-mile surrounding the cut point) to demonstrate variation among a larger sample. And finally, I provide estimates for the full sample as a means of comparison. Appendix Figure C.1 identify borders of interest for the optimal bandwidth (.45-miles) and among those selected for robustness checks (.25-miles and 1-mile).

2.5 Data

The unit of analysis for this study is residential structures. Using GIS software, I geolocated each of the 19,000+ structures to points within the 33 evacuation zones defined by the Butte County OEM. Limiting those structures to residential types, I was left with 13,668 unique residential structures, which I then overlaid on evacuation zone boundaries. Each residential structure was assigned three binary indicators: whether they were ordered to evacuate or not, whether a death occurred there, and whether the structure was over 50 percent destroyed from the fire. Using the zone boundaries with evacuation orders assigned to each polygon, I then identified borders of interest (BOI) which were borders shared by zones with dissimilar orders. Each structure was then assigned to its closest BOI in naïve distance[3] so that comparisons were made across zone borders within that bandwidth. Table 2.1 provides a summary of the variables used in this study.

I combined data from four sources to construct the novel data set used in this study: Twitter data for reconstructing a timeline of evacuation orders, Sheriff's Department fatality notices to geolocate deaths, CALFire Structural Assessments to assign damages (and death) to each residential structure, and Census Bureau socio-demographics at the block level for covariates. Each data source is described in more detail below.

[3] Naïve distance measures the shortest distance between a point (residence) and the cutoff point (border of interest). The term "naïve" refers to the treatment of geospatial variables as being measured only in a single plane rather than along a coordinate plane (Calonico et al., 2014)

Table 2.1 Summary Statistics by Data Source

	Description	Full Sample Mean	Std. Dev.	Range
Twitter: Butte County Sheriff Evacuation Order				
Order	1 if the structure was in a zone that received an evacuation order	0.6	0.49	0-1
Time	1-5 specifies the period in which an evacuation order was issued	2.25	0.8	1-5
Butte County Sheriff Fatality Notices				
Death	1 if at least 1 death occurred in a structure, 0 otherwise (scaled by 100: interpreted as %)	0.34	5.85	0-100
CALFire Structural Assessments				
Distance (absolute value)	Naïve distance in miles structure is from nearest zone border of interest	0.22	0.13	.0007-8.63
Distance	Naïve distance from BOI: + if in zones with orders, - if in zones without orders	-.018	1.03	-8.63-2.09
Damage	1 if the structure was >50% damaged	0.97	0.18	0-1
Structure Type	Categorical variable specifying residential structure type			
Census Bureau Block Group Demographics				
Over 65	% census block group over the age of 65	0.25	0.07	0.14-.38
Low Income	% census block group under poverty line for Butte Co.	0.11	0.07	0-.24
BIPOC	% census block group identified as non-white	0.08	0.05	0-.67
Population Density	# of residents in census block per 1 mile (scaled by .001)	2.13	1.35	0-.17.52

2.5.1 Twitter: Evacuation Orders

I systematically reviewed Twitter to construct a timeline of evacuation orders issued by zone. This process was accomplished by using R-studio package ‘rtweet’ to scrape Twitter’s API for tweets that occurred during the first six hours of the fire. Appendix A provides detailed information about process used to identify the Tweets used in this study. Briefly, I first compiled a list of 192 Twitter accounts from the area from news & media, government and non-governmental organizations, fire departments, and fire scanners (bots that scrape other accounts). From these 4,200 unique tweets, 147 tweets contained explicit evacuation orders for a zone or region to evacuate. Tracing these tweets to the initial evacuation order resulted in the eight Tweets issued by the Butte County Sheriff account in Table 2.2. These are listed chronologically along with a breakdown of how many structures within the burn perimeter were targeted by each Tweet. Figure 2.2 shows a map of the burn area with evacuation zones overlaid -- the number specified on the map designates the period orders were issued (e.g., 1= First 30 minutes, 2 = Second 30 minutes, etc.). The zones marked with a 0 did not receive an order within the studied timeframe. Table 2.1 shows that 60 percent of the residential structures in this study were in areas that receive an evacuation order, with the average order arriving after the second hour after the fire reached the city limits of Paradise.

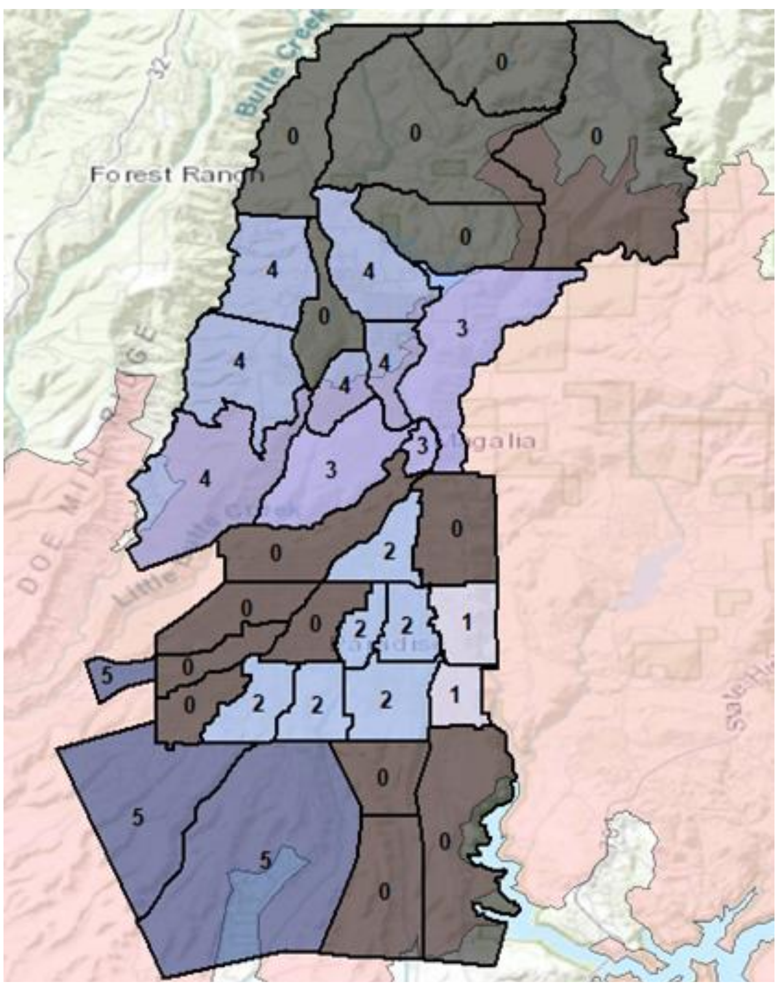

Figure 2.2 Zone Map: Chronology of order issuance.

Table 2.2 Twitter Alert Timeline: Tweets used to construct *time* and *order* variables shown with structures targeted by tweets.

<table>
<tr><th colspan="4">Tweets used to construct timeline (All tweets occurred on 11/8/2018 from @ButteSheriff)</th></tr>
<tr><th>Time Issued</th><th>Period</th><th>Text</th><th>% Structures Targeted</th></tr>
<tr><td>9:03</td><td>1</td><td>“EVACUATION ORDER: Due to a fire in the area, an evacuation order has been issued for all of Pentz road in Paradise East to Highway 70.” (Butte County Sheriff, 2018(a))</td><td>7.2%</td></tr>
<tr><td>9:41</td><td>2</td><td>“EVACUATION ORDER: Due to the fire in the area, an evacuation order has been issued for zones 2, 6, 7 and 13. If assistance is needed in evacuating, please call 911” (Butte County Sheriff, 2018(b))</td><td rowspan="2">33.7%</td></tr>
<tr><td>9:55</td><td>2</td><td>“EVACUATION WARNING: 8:51 AM- an evacuation warning has been issued for zones 11 and 12. If you need assistance in evacuating, please call 9 1 1” (Butte County Sheriff, 2018(c))</td></tr>
<tr><td>10:25</td><td>3</td><td>“EVACUATION ORDER 9:22 AM-an evacuation order has been issued for the South Pine Zone, Old Magalia Zone and the South Coutelenc Zone. If assistance is needed to evacuate, please call 9 1 1” (Butte County Sheriff, 2018(d))</td><td>11.6%</td></tr>
<tr><td>10:33</td><td>4</td><td>“EVACUATION ORDER 9:33 AM- an evacuation order has been issued for the Carnegie Zone, North Pines Zone, North Fir Haven Zone and South Fir Haven Zone. If assistance is needed to evacuate, please call 911” (Butte County Sheriff, 2018(e))</td><td>3.7%</td></tr>
<tr><td>11:01</td><td>5</td><td>“EVACUATION WARNING 10:00 AM- an evacuation warning has been issued for the Nimshew Zone. If assistance is needed to evacuate, please call 911.#ButteSheriff #CampFire” (Butte County Sheriff, 2018(f))</td><td rowspan="3">0.7%</td></tr>
<tr><td>11:09</td><td>5</td><td>“EVACUATION WARNING 10:08 AM-an evacuation warning has been issued for the Lower Clark and Lower Skyway zones. #ButteSheriff #CampFire” (Butte County Sheriff, 2018(g))</td></tr>
<tr><td>11:13</td><td>5</td><td>“EVACUATION WARNING 10:12 AM-an evacuation warning has been issued for the Lower Neal and Upper Honey Run zones.” (Butte County Sheriff, 2018(h))</td></tr>
</table>

2.5.2 Sheriff's Department Fatality Announcements

To create the outcome variable *Death*, I first established if each structure within the burn perimeter had a fatality occur there. To determine this, I combined data from the Butte County Sheriff's Department (Butte County Sheriff's Office, 2018) and news outlets (Enterprise-Record, 2018). These were comprised of press releases that detailed the fatalities, their characteristics, and place of death with geographic indicator (street address or coordinates). A press release typically specified age and name, while the news outlets provided an interactive-geolocated map with notes like the general location of death such as "inside their residence", "outside residence", or "on a roadway". I used this indicator to limit deaths to those that occurred at a residential structure. By excluding deaths that occurred on roadways, I was able to use death as proxy for whether an evacuation was attempted from a residence. If a death occurred within a residence, it was assumed that the resident did not evacuate. Those who died on roadways presumably responded to evacuation orders by trying to evacuate but were unsuccessful. Table 2.1 shows that 0.34 percent of structures has at least one death occur in them.

2.5.3 CALFire Structural Assessments

This data set details the location, structure type, and level of damage sustained by each structure in the burn perimeter after the fire. Damages ranged from "Affected (1-9% damaged)" to "Destroyed (>50% damaged)" with just under 97 percent of residences in the sample being coded as destroyed. Again, using GIS software, the structures were mapped within OEM evacuation zones and tagged with whether an order was issued to it, whether a death occurred there, and the amount of damage sustained. Using the fatalities from the Sheriff's Department, I geolocated deaths to structures shown in Figure 2.3 where structures used in this study are displayed as dark grey points, and structures that had at least one death occur in them are designated with a larger red point.

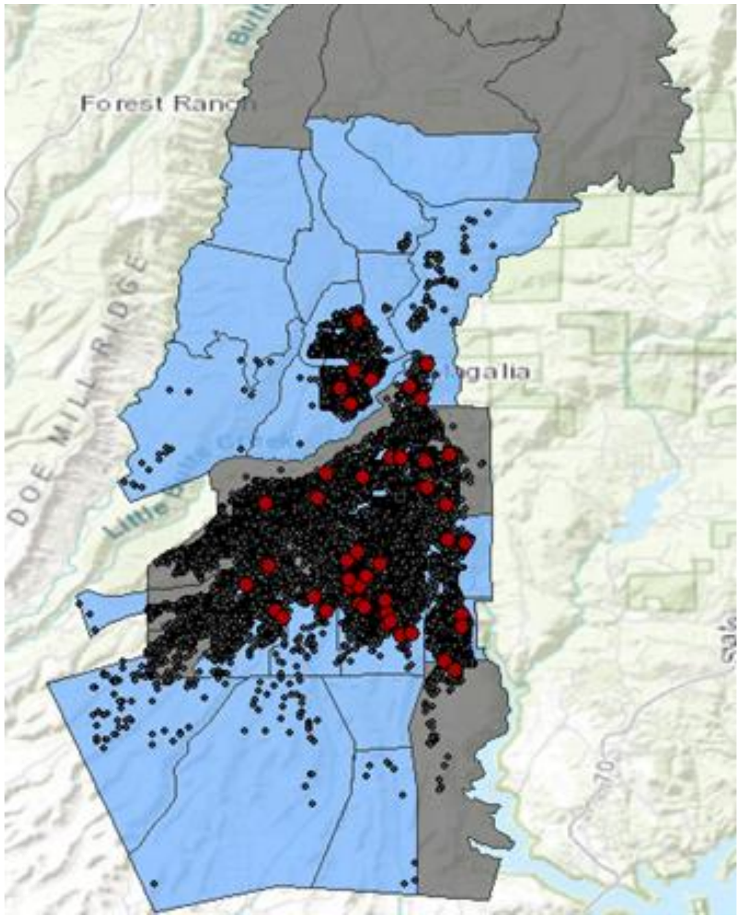

Figure 2.3 Map of Structures and Deaths

2.5.4 Census Bureau Demographic Controls

The Census Bureau data set provided age, income, race, and population density information at the census block group level. All structures within the same block group were assigned the same demographic information. Summaries for these indicators are found in Table 2.1. The variable *Over 65* indicates that on average people aged 65 and older make up about 25 percent of the combined block groups. *Low Income* indicates that about 11 percent of the block group populations are living under the poverty line. *BIPOC* measures the percentage of the block group who identified as a racial groups Black, Indigenous, or Persons of Color and shows that 8 percent of the population impacted were BIPOC. *Population Density* is the number of residents per square mile, scaled by .001 for readability.

2.6 Analysis

As mentioned above, to validate the use of RDD in the context of this study there is an important consideration regarding the cutoff point. The selection of the cutoff (in this case, the OEM zone boundaries) should be determined such that the assignment of evacuation orders is as-if random. This is done so that the boundaries divide the sample 'randomly' so that there is balance among covariates within a bandwidth surrounding the boundaries.

2.6.1 Random Assignment of Boundaries

To determine the validity of the OEM zone borders used as the cutoff point in this study, I compare evacuation zones to three influencing factors: first, natural land barriers and roadways, political precinct lines, and census block groups. Appendix Figure C.2 shows important local roadways and topographical features against zone boundaries. A visual comparison shows that the natural topography and water ways likely played a role in determining zone borders. For example, zone borders to the east follow ridge lines and water features closely. Roadways also seem to play a significant role, with the area's major roadways bisecting many of the zones such that residents have equitable access to main evacuation routes through smaller ancillary roadways. These commonalities suggest that equal access to evacuation may have factored into establishing zones. A visual comparison of political precincts lines and census block groups to zone borders shows little shared geometry. In Appendix Figure C.3, we see that shared boundaries mainly occur along Paradise city's southern limit splitting the densely populated city from a largely unpopulated area to the south with similar borders occurring along the eastern limits of Paradise and the rural area to the east. Appendix Figure C.4 illustrates census block groups lines against OEM zones in red, shared lines (circled in purple) are limited to the eastern border of Paradise City extending north splitting the town of Magalia from the largely uninhabited rural area to the east. While topography and access to evacuation routes may have played a role in defining zone boundaries, there is no evidence that suggests that zone borders are politicized. This brief visual analysis suggests that the zone borders are not politicized and thus can be considered an adequately 'random' placement.

2.6.2 Balance Testing

Testing for balance is intended to demonstrate that as the bandwidth narrows, treatment covariates become more similar (Keele & Titiunik, 2015). Without such balance, making a causal argument is not validated under RD as other factors may be influencing the outcome. To test this assumption, I ran a series of local linear regressions that follow the general form of Equation 2.1. If the covariates are balanced around the cut point, then β_2 should be equal to zero.

$$M_{ijk} = \alpha + \beta_1 D_i + \beta_2 T_{ij} + \beta_3 D_i T_{ij} + \varepsilon_i \qquad (2.1)$$

Where:

M_{ijk} = demographic indicator associated with census block group *k* assigned to structure *i* in zone *j*[4]

D_i = Naïve distance from BOI: the measure is positive if structure (i) is in a zone with an order, negative if in a zone without an order.

T_{ij} = 1 if structure (*i*) is in a zone (*j*) that received an order, 0 otherwise

ε_i = error

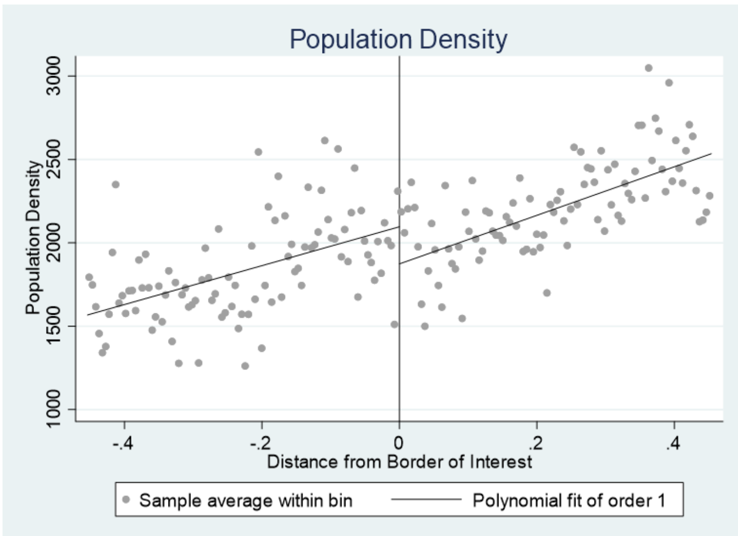

Figure 2.4 Regression Discontinuity Balance Check for Population Density

[4] Controls included indicators included % 65+, % BIPOC, % low income, population density, and mobile homes. Mobile homes were included in this balance check due to the disproportionate impact of the fire on housing stock of this type. In future regressions Mobile Homes are considered as part of a fixed effect for Structure Type.

Figure 2.4 shows the discontinuity plot for the population density by census block group. The distinct discontinuity featured around the cut point (paired with the results of Table 2.3, Model 1) indicates that areas that were more densely populated were less likely to be issued orders where the coefficient on order (β_2) is equal to -.19 (p=.026). Appendix Figures C.5-C.9 show the discontinuity plots that correspond with the coefficients on the other socio-demographic indicators featured in Table 2.4. The significant coefficients reported were on *% Low Income* (β_2 = .0123, p=.065), *% BIPOC* (β_2 = -.017, p=.001), and *Mobile Homes* (β_2=-.148, p=.001). This indicates that lower income areas, areas with a higher BIPOC population, and areas with more mobile homes were not balanced around the cut point. Non-significant coefficients were both positive: *% 65+* (β_2 =-.007, p=.254) and damages (β_2=.008, p=.478). Since β_2 was non-significant for these indicators, it suggests that there is balance around the dispersion of damages and the elderly population surrounding the cut point. Therefore, damages (i.e., risk-salience) was constant across zones as well as the distribution of the elderly population. Appendix Table C.1 shows that these results are robust to the other bandwidth selections and among the full sample. These results demonstrate the need to follow the main discontinuity model with a model that controls for the unbalanced indicators. This method is validated by Black in her 1999 paper for use in cases where the conditions of an RD are met except where covariates are not constant on either side of the cut point.

Table 2.3 Regression Discontinuity Estimates for Optimal Bandwidth: Testing balance of sociodemographic indicators at cutoff.

VARIABLES	Model (1) **Pop. Density**	Model (2) **% Poverty**	Model (3) **% BIPOC**	Model (4) **%65+**	Model (5) **Mobile Home**	Model (6) **Damage**
RD Estimate on Order Optimal Bandwidth (.45 Miles)	-0.190**	-0.012*	-0.017***	-0.007	-0.149***	0.008
	(0.085)	(0.007)	(0.005)	(0.006)	(0.046)	(0.011)
Observations	13,660	13,660	13,661	13,661	13,668	13,668

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specifications: Kernel: uniform, Polynomial degree: 1st

2.7 Results

2.7.1 Regression Discontinuity

The main discontinuity results can be found in Table 2.4 specified by Equation 2.2 below. The RD estimates for all models are non-parametric local-linear estimation that employ a uniform kernel.[5]

$$Y_{ijk} = \alpha + \beta_1 D_i + \beta_2 T_{ij} + \beta_1 D_i T_{ij} + \varepsilon_i \tag{2.2}$$

Here, the dependent variable, Y_{ijk}, *death,* is the proxy for evacuation at structure *i* in zone *j* in block group *k* as a function of zone *j* receiving an evacuation order (T_{ij}) and that structure's proximity to the cutoff point (D_i). Figure 2.4 plots this model and the corresponding results in Table 2.4, Model 1 reports a coefficient on Order (β_2=.069, p=.826), both showing no significant discontinuity around the zone borders. While non-significant, it does show an interesting pattern: that at there is a slightly negative relationship between orders and deaths – the further from zone borders a structure is located, the greater chance it will contain a death. Figure 2.4 visualizes this clearly with the modest V-shape emanating from the cut-off point.

Table 2.4 Regression Discontinuity Output: Impact of *Order* on *Deaths*

VARIABLES	Model 1 **Optimal BW**	Model 2 **.25-mile**	Model 3 **1-mile**	Model 4 **Full Sample**
RD Estimate on Order	-0.0697 (0.316)	-0.043 (0.409)	0.0998 (0.187)	0.165 (0.158)
Observations in BW	6,844	3,908	11,263	13,668

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specifications: Kernel: uniform, Polynomial degree: 1st

[5] Results in all main tables use a uniform kernel, weighting each structure the same regardless of distance from the zone border. Appendix Table C.1 and the accompanying text validate this selection and show estimates using a non-parametric local-linear regression with triangular kernel weighting as a robustness check. Notably, there is no change in significance among any of the specified bandwidths.

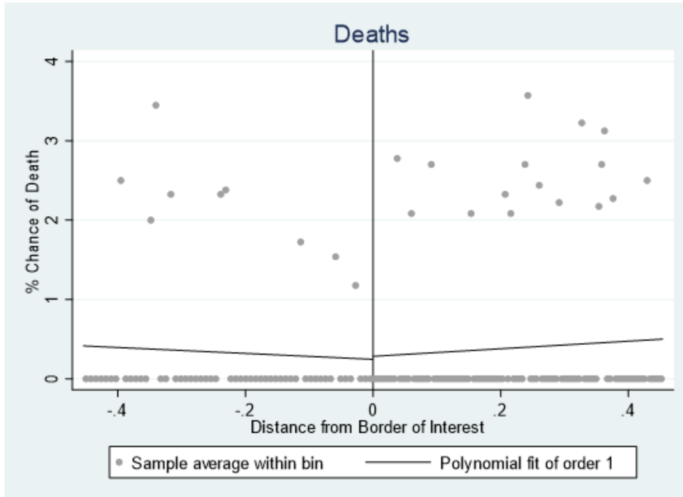

Figure 2.5 Discontinuity Plot: Impact of *Order* on *Deaths*

2.7.2 Regression Discontinuity with Controls

Since the balance check from the previous section showed that covariates surrounding the cut point were not evenly dispersed on either side of the borders, I tested for validity with Equation 2.3 using Black's (1999) method. The model presented below specifies an RD with controls with results displayed Table 2.5.

$$Y_{ijk} = \alpha + \beta_1 D_i + \beta_2 T_{ij} + \beta_3 D_i T_{ij} + M_{ijk} + A_i + \lambda_{ts} + \varepsilon_i \tag{2.3}$$

Equation 2.3 has the same base as the original RD model (Equation 2.2) but accounts for unbalanced covariates: socio-demographics are controlled for with M_{ijk} which assigns values to structure *i* in census block group *k* situated in zone *j*. *Damages* (A_i) are assigned to structure *i,* and fixed effects enter as λ_{ts} for the *Time* the order was issued (t) and the *Structure Type* (s). Fixed effects on time were an important consideration as orders were not issued simultaneously to all zones but instead were staggered over the first six hours of the fire. Structure type was included to account for any variability associated with the type of residential structure that may have impacted the level of damage sustained, the need for this was demonstrated by the balance test for *Mobile Homes* that showed an uneven distribution across zones.

The results presented in Table 2.5 are consistent in significance but not in direction with those in Table 2.4. The coefficient on Order (β_2= .038, p=.896) suggests that structures that received orders (and were relatively close to the BOI) were .038 percent more likely to contain a fatality (or less likely to evacuate) but the effect is non-significant. Models 2-4 show robustness checks among the other specified bandwidths with inconsistent directions, but consistently non-significant results. For results on all coefficients and more details, see Appendix Table C.1.3.

Table 2.5 Regression Discontinuity With Controls - Order & Distance Impact on Death

VARIABLES	(1) Optimal BW	(2) .25-mile	(3) 1-mile	(4) Full Sample
Order	0.038	-0.3096	-0.252	-0.063
	(0.292)	(0.394)	(0.261)	(0.196)
Observations	6,844	3,908	11,263	13,660
R-squared	0.003	0.005	0.003	0.004

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2.7.3 Results Take-Aways

The results from Table 2.4, suggested that structures in zones that were issued orders were slightly (but not significantly) more likely to be evacuated (less likely to die by .06 percent). Table 2.5 shows that when controlling for covariates, those who were issued orders and relatively closer to BOIs were *less* likely to evacuate (*more* likely to die by .04 percent), but not significantly so. To put these results in perspective, an increase of .01 percent in the probability of dying translates to an increase of less than one death (.0088). While a .01 percent increase in probability is not statistically significant, it is perhaps subjective whether a single life is practically significant.

Given the null results and the lack of variation in the level of damage across the burn area, I tested the precision of the main estimate. To accomplish this, I constructed a 90 percent confidence interval surrounding the explanatory variable's estimate (0.04 from Table 2.5, Model 1) which allows for a more precise estimate on null findings (Rainey, 2014). I begin by identifying the 'smallest substantively meaningful' value of *Distance* as .1 (or one tenth of a mile or roughly two city blocks). This threshold allows for the possibility that within city limits (where most of structures were located), that a two-block difference may create a different evacuation route for residents. The simulated estimate provides a confidence interval on *Distance* between -.428 and .501, which contains the 'smallest meaningful' value of .1. While there is evidence that the effect is not statistically significant, there is no evidence that this null is precisely estimated. These findings suggest that there may be merit in improving the data or testing the same method in a different contextual disaster. Therefore, there are inconclusive results suggesting that the null findings could have a substantive (but statistically non-significant) effect, but further research would be needed.

2.8 Follow-up

The imbalance of socio-demographic indicators around the cut point combined with the null results inspired a closer look at the related theory that ability, which is tied to those indicators, may have hindered some residents' ability to evacuate regardless of the orders they were issued. The literature suggests that elderly, lower-income, and communities of color tend to face additional challenges during evacuations, so I test two possible avenues for how ability (socio-demographics indicators) factors into the Paradise case. First, I look to uncover any relationship between indicators and fatalities (deaths as a function of demographics). Then, I look for systematic bias in which socio-demographic groups may have been targeted to receive an evacuation order and when (orders and time as a function of demographics). Model specifications, tables, figures, and details can be found in Appendix D. Briefly, the results are consistent with the extant literature suggesting that areas with a higher percentage of population over the age of 65 were less likely to evacuate (and thus more likely to die). The underlying reasons for difficulty evacuating may range from mobility issues common among older populations (Butte County District Attorney's Office, 2020, p. 11-13) or lack of awareness of the evacuation orders: older populations use technology less frequently than younger populations. Higher population density and higher percentage of residents over 65 were significantly more likely to be issued orders while areas with higher percentage of BIPOC or low-income residents were less likely to be issued orders. Further exploration with a Weibull duration model showed that older residents were more likely to receive orders sooner. These results suggest that despite being more likely to be issued evacuation orders sooner, residents over 65 were less likely to evacuate and thus more likely to die as the result of the fire.

2.9 Conclusions and Limitations

The results suggests that contrary to the extant literature, evacuation orders have no statistically significant effect on resident evacuations, but these results may be imprecisely estimated. Refining the method to explicitly control for risk salience and socio-demographic indicators produced similar null results sparking the need to explore this relationship further. However, the results from the follow-up questions were consistent with previous studies that highlight the disproportionate effects of disaster on the elderly, low-income, and communities of color. Together the secondary results help to frame the Paradise case as non-exceptional in the scope of disaster studies despite it being a particularly damaging fire.

One consideration for practice is in determining if orders can prevent a single death by prompting an evacuation (but that one death is not statistically significant), would it be colloquially significant to overestimate risk and avoid a single fatality? Taking a step back to consider the implications for practitioners, these results should not be interpreted as the smoking gun that does away with government mandates during crisis. More accurately, it suggests that there may be conditions under which government orders may not be a necessary mechanism for getting people to evacuate. The case of Paradise was exceptional in the amount of damage it caused but not in how it impacted residents of varying socio-demographic stations. What it provides is evidence that when risk is highly salient and the disaster is fast moving, government messaging may get lost in the noise of media.[6] This is not to say that governments should not weigh in during high profile events, but rather their role may be less pronounced. Since the mechanical ease of issuing orders is incredibly low cost with the prevalence of social media and widespread news coverage, governments should more carefully consider the hidden cost of credibility and pay closer attention to the efficacy of such orders. Perhaps government orders need to come sooner, before widespread coverage of the risk is saturating residents, to be impactful.

The inconclusiveness of the results may simply be the product of poorly designed and implemented evacuation orders rather than the result of high salience. As mentioned earlier, the official evacuation alert system was significantly flawed with minimal reach and no reliable estimate

[6] Consider that among 192 non-private Twitter accounts tweeting +4,200 times during the evacuation window with information about the fire, only 33 of those were relevant government accounts issuing orders. This ignores individual users and accounts for only a single social media platform.

on how many people who were contacted received the alert. Alerts issued via Twitter and media outlets may have more diffuse reach but have similar reach limits. There is a distinct possibility that residents, regardless of what zone they were in, paid more attention to their direct experience: seeing smoke, flames, or their neighbors preparing for evacuation. Residents located in zones that did not receive direct orders to evacuate but left anyway made the 'correct' decision – and we cannot accuse them of going against orders, they simply weren't issued any. Had they waited for orders to be issued to them, they would have been at greater risk of dying considering the widespread damage across the burn perimeter within the first six hours of the fire. This suggests that perhaps a further line of inquiry could be in looking not just at whether a resident evacuated, but more precisely whether residents followed the order within the necessary time frame. The differentiation here being that the former considers deaths as non-compliance, while the latter separates deaths in zones with orders and non-deaths in zones without orders as non-compliance. While this gets at a slightly different question, it is worth noting the importance of such distinction for future work.

In summary, this study contributes evidence of systemic bias in government messaging and impact of age and density on evacuation behavior. Further, it provides a novel method for constructing an unbiased measure of risk-salience and evacuation behavior. Future research on the efficacy of evacuation orders (and government messaging more broadly) will need to consider an array of contexts to help build our understanding of disaster preparedness and I look forward to continuing this work.

3.0 To Mask or Not to Mask: Salience of Risk and the Efficacy of Government Messaging

(Coauthored with Daniel Jones and Sera Linardi)

3.1 Introduction

In the face of crisis – such as global warming, disease, or, specifically, the recent COVID-19 pandemic -- what drives *individuals* to respond and take mitigating action? This paper probes the related roles of *salience of risk* and *government messaging about risk* in driving responses. While there is existing evidence that individuals are more likely to respond to a risk when it is highly salient to them, many of the largest challenges that society faces require many individuals to take action *prior to the effects of risk becoming salient.* Such as the case with global warming or early evacuation action during hurricanes and other natural disasters. It is therefore critical to understand how government messaging operates both in the presence or absence of heightened awareness about a risk, the explicit aim of our paper.

We take advantage of a window of time when messaging from the Center for Disease Control (CDC) in the United States regarding the appropriate mitigating actions was very much in flux. Specifically, as late as late March, the CDC was still advising that masks should not be worn, but one week later shifted their position, recommending that they should be. We conducted an online experiment during this period, either highlighting the local severity of the pandemic or not and also exposing participants to either the CDC's anti-mask or pro-mask messaging. Following those treatments, we measured individuals' demand for purchasing a mask, at a time when masks were otherwise difficult to obtain. We find that the effects of government messaging (both for and against wearing a mask) is more pronounced in treatments where the risk is *not* made especially salient.

Our focus on salience draws on a large literature originating in behavioral economics. To conserve their attentional resources, humans usually focus on only a subset of available data to make judgements, a phenomenon that psychologists refer to as salience detection (Taylor & Thompson, 1982). Chetty et al. (2009) show that taxes included in posted prices reduce alcohol consumption significantly more than increases in taxes applied at the register, even though consumers know the tax status of products. California voters within five kilometers of a wildfire are significantly more likely to vote in favor of costly, climate-related ballot propositions, but voting behavior of individuals just fifteen kilometers from the same fires was unchanged (Hazlett & Mildenberger, 2020).[7] "(W)hatever is odd, different or unusual." (Kahneman, 2011, p. 324) usually is what draws the decision maker's attention and enters disproportionately into payoff-relevant consideration, thus influencing behavior. This suggests that people are inattentive, leading to suboptimal behavior that can be corrected by manipulating what they pay attention to.

This paper brings together the literature on salience with a related, but largely separate, literature on the effect of government communication directed at individual behavior. While there is evidence that government policy recommendations have a positive effect in reducing smoking and increasing quit attempts, increasing flood evacuation compliance, and reducing the purchases of sugar-sweetened beverages (Azagba & Sharaf, 2013; Glock et al., 2012; Molinari & Handmer 2011; Roberto et al., 2016), the literature has also found that government messages have no impact on behavior with regards to alcohol, dietary supplements, gambling, and wildfire evacuation compliance (Andrews, 1995; Mason et al., 2007; Steenberg et al., 2004; Gillespie 2020). Differences in salience may be partly responsible for these divergent results. Theoretical models of salience (Bordalo et al., 2012) suggest that there is diminishing sensitivity to increased salience. In other words, bringing additional attention to what is already on top of someone's mind will not change behavior much. The effect of smoking, preparing for floods, and drinking sweetened beverages may take some time to emerge and hence may not enter people's minds. On the other hand, drinking, gambling, and wildfires have relatively more immediate effects; because they are already salient, government messaging may appear to have minimal additional effect on behavior.

In this paper, we investigate the role of salience in the efficacy of government messaging about a crisis. We use an online platform to quickly recruit 512 participants across twelve states that were affected by COVID-19 to a varying degree in early April 2020 and randomly expose them to actual government messages in a 2x5 design. We first randomize half of the participants into the (high salience) *StateStats* treatment, where they saw their state's current confirmed COVID cases, tests performed, and death statistics (which is widely available and shared in the media). All subjects were then randomly assigned into one of five federal messaging groups: No Message, Anti-Mask or Pro-Mask – displaying CDC's official position on wearing a mask before and after April 3, 2020, and

[7] Also see Borick and Rabe (2010); and Deryugina (2013). Konisky et al. (2016) show that individual's level concern about climate change is affected by recent experience of extreme weather events (e.g., excessive heat, droughts) but does not change with prolonged extreme weather events

Optimistic Forecast and Pessimistic Forecast – the positive and negative framing of IHME's national fatality forecast. After observing the messages, they allocate lottery tickets between a large Starbucks gift certificate ($75) and an out-of-stock face mask (N99).[8] Participants then completed an exit survey on their perception of the risks of COVID-19 and support for masks before they were debriefed with current CDC recommendations.

We first test that the *StateStats* treatment indeed works as a salience shock (bringing the pandemic to the front of the mind) rather than an *informational* shock (shifting participants' prior beliefs about the level of severity of the pandemic in their state).[9] Participants post-treatment beliefs did not depend on the information they were provided, with subjects from states that were more impacted by COVID perceiving no more risk than those in less impacted states, thus confirming that *StateStats* treatment functions by increasing salience. Moving to the government messages, we find that the forecast framing had no effect while the action-oriented messages did. Participants' support for and willingness to allocate any tickets to the mask is significantly higher in the Pro-Mask treatment group compared to the Anti-Mask group. This was driven by the *NoStateStates* treatment, suggesting that government messages are more effective when salience of the event is not already at a high level.

In short, our study provides evidence that the nature of government messaging about a risk matters, *especially* in cases where there is room for the decision maker to bring additional attention to the issue. We find that action-oriented messages matter in both directions: discouragement of a mitigating behavior indeed leads to less uptake of that behavior relative to no government recommendation at all; government messaging that *encourages* the behavior, on the other hand, leads to significantly more uptake than the discouraging message. These findings highlight that communications of policymakers can play a pivotal role in shifting behavior in the face of risk, but that the nature of their messages must be carefully considered.

[8] Our survey revealed that lack of interest in bidding for masks cannot be explained by already having one. While 45 percent of participants focused on stocking up on food and water, only 5% focused on masks and thermometers.

[9] For example, Chetty et al. (2009) had to overcome this issue by surveying consumers about their knowledge of sales taxes and found that individuals do know the tax status of the products, but just focus on the posted price when shopping.

3.2 Experimental Design and Measures

3.2.1 Sampling

The experiment was run on April 13, 2020 through online survey hosting site "Prolific". Our aim was to collect a geographically diverse sample that represent the states that at that point had experienced only a moderate and varied impact of COVID-19. In highly impacted states (Florida, New York, California), exposure to information about the virus and government recommendations may have been too great to expect a response to experimental intervention. To ensure an even geographic coverage, we chose two states each from the six regions of United States: West, Southwest, Southeast, Northeast, Mountain, and Midwest. Within regions, we chose two states, based on two factors: (1) we aimed to choose states that are relatively similar with regards to demographics and economic characteristics (achieved using the fivethirtyeight.com state similarity score index) and (2) we aimed to choose pairs of states such that one had a relatively lower and one had a relatively higher number of COVID-19 cases at the time. Table 3.1 shows the states sampled, number of cases, deaths, and tests conducted as of April 13, 2020, and the number of participants recruited from each state. We recruited about 43 participants from each of the 12 states, resulting in a total of 512 participants.

Table 3.1 Sampled States with COVID-19 Severity at the Time of Experiment

Region	State	Cases 4/13/2020	Deaths 4/13/2020	Tests 4/13/2020	Respondents
West	WA	10,530	508	92,999	43
	OR	1,527	52	29,758	43
Southwest	AZ	3,539	115	42,109	42
	NM	1,245	26	30,515	44
Southeast	TN	5,308	101	70,747	41
	KY	1,963	97	25,866	45
Northeast	MA	25,475	756	116,730	41
	CT	12,035	554	41,220	42
Mountain	CO	7,303	290	37,153	44
	NV	2,836	112	29,579	43
Midwest	MI	24,638	1,487	76,014	42
	OH	6,604	253	63,243	42

3.2.2 Survey Flow and Treatments

Our experimental design is illustrated in Table 3.2. Participants started from a baseline survey that focused on demographics, employment, risk factors and behavior (e.g., stocking up). COVID-19 was not explicitly mentioned in the questions. They were then randomized into treatments in two stages, and after viewing the messages associated with the treatments, completed a choice task and an additional exit survey before being debriefed about the treatment. We describe each stage in detail below.

Table 3.2 Survey Flow for Prolific Experiment

<table>
<tr><td colspan="10">Consent & Baseline Survey
Survey collects demographics, health conditions and behavior.</td></tr>
<tr><td colspan="10">First Stage Random Assignment: SALIENCE treatments</td></tr>
<tr><td colspan="5">No State Stats: N= 255</td><td colspan="5">State Stats: N = 257</td></tr>
<tr><td colspan="10">Second Stage Random Assignment: MESSAGE treatments</td></tr>
<tr><td colspan="2">No message:
N = 103</td><td colspan="2">Anti-Mask:
N = 111</td><td colspan="2">Pro-Mask:
N = 101</td><td colspan="2">Optimistic Forecast:
N = 111</td><td colspan="2">Pessimistic Forecast:
N = 86</td></tr>
<tr><td colspan="10">Choice Task
Distribute tickets between $75 Starbucks gift card and N99 face mask</td></tr>
<tr><td colspan="10">Exit Survey
COVID-19 specific beliefs and Behavior</td></tr>
<tr><td colspan="10">Debrief
most current proper CDC guidelines on masking (link to CDC website)</td></tr>
</table>

3.2.2.1 Salience Treatment

Following the baseline survey, we randomized participants into one of our 2x5 treatment cells. First, the 512 participants were split between two groups: *StateStats* (N =257) and *NoStateStats* (N=255). The *StateStats* group was shown information about the number of confirmed cases, deaths, and tests performed in their state as of the day of the survey. For example, participants from Connecticut saw the following:

"As of 4/13/2020 there have been 554 deaths in the state of Connecticut. The number of cases confirmed in your state has reached 12,035 with 41,220 tests performed."

This information was widely available in local news media in all states. Our treatment is motivated by the idea that displaying widely known information at the time of decision making can have an effect of increasing top-of-mind awareness such as the display of (known) sales taxes in Chetty et al. (2009). We presented the statistics as a raw number instead of as per-capita to remain consistent with the way that media sources were presenting COVID-19 statistics at the time (Lutton, 2020; Chidambaram, 2020). The *NoStateStats* group received no message in this stage.

3.2.2.2 Government Messaging Treatment

After the randomization into the two salience treatments, all participants were further randomly assigned into one of five messaging treatments: No Message (N = 103), a control group; Anti-mask (N= 111), who received the official CDC statement on masks when their position was *against* their use; Pro-mask (N=101), who received the official CDC statement after reversing their position to now encourage face mask use; Pessimistic Forecast (N=111), who were shown the predicted number of *nationwide* COVID-related deaths expected by August framed negatively; and Optimistic Forecast (N=86), who were shown the same forecast numbers framed positively.

The language used in Anti-mask and Pro-mask messages was taken directly from the recommendations posted on the CDC COVID-19 website on March 30, 2020 and April 3, 2020 respectively. Anti-mask participants were shown: *"If you are NOT sick: You do not need to wear a face mask unless you are caring for someone who is sick (and they are not able to wear a face mask)"* (CDC, March 2020) while Pro-mask participants were shown: *"The CDC recommends that residents wear cloth face coverings or face masks in public settings where other social distancing measures are difficult to maintain (e.g., grocery stores and pharmacies),* ***especially*** *in areas of significant community-based transmission* (CDC, April 2020). Pessimistic Forecast participants were shown *"The forecasting model used by the White House was recently updated and* *predicts that by August 2020 the United States should expect to have 60,400 corona virus deaths. The growth rate of confirmed cases and deaths in the past three weeks has been the highest in the world with social distancing measure in place.*" and Optimistic Forecast participants were shown *"The forecasting model used by the White House was recently updated and predicts that by August 2020 the Unites States should expect to have 20,000 fewer deaths than earlier forecasting models predicted."*[10]

3.2.2.3 Choice Task

After viewing the messages, all participants were given 100 tickets and asked to distribute the tickets between two choices. First was a Monato brand N99 face mask that, at the time, was not available for purchase through online sellers. The description includes the health function of the face mask, its reusability, its current unavailability, and the timeline where items will be shipped. The second option is a $75 Starbucks gift certificate, which was chosen because - unlike a cash card or an Amazon gift card - the value cannot be exchanged for a face mask.

3.2.2.4 Exit Survey: Beliefs and Opinions

After completing the task, participants filled out an exit survey. Most pertinent to our interest here are questions that ask about their beliefs and perception about the risk of COVID-19 and the role of preventive measures. We used responses to these questions to understand if and how the treatments work.

- *How do you think your state compares to others on the COVID-19 cases and deaths?* (Respondents choose between: in the top 10, bottom 10, or somewhere in the middle.)
- *If you were to consider the risk that COVID-19 poses to your own health on a scale of 1 to 10 - where 1 is completely unconcerned and 10 means you have serious concerns about your health and well-being - how would you rate your risk level?* (This is followed by the same question about "*the health of others in the community*").
- *How important do you feel it is to wear a face mask in public under the current circumstances?* (Respondents choose between extremely important, moderately important, slightly important, and not at all important.)

[10] Forecasting messages were taken from media reports that used IHME models published in early April2020 and were subsequently used by the White House (Wan & Johnson, 2020; Al-Arshani, 2020)

3.2.2.5 Debriefing

Finally, to avoid confusion about the current recommendations from the CDC, all participants were shown a debriefing screen that clarified proper safety precautions for limiting the spread of COVID-19 with a link to the current CDC precautions website.

3.2.3 Hypothesis

We first discuss our hypothesized effect of the salience treatment (*StateStats* vs *NoStateStats*) before considering how government messages will interact with salience. As mentioned earlier, the effect of salience is driven by the focusing of attention on what is unusual. Because of this, models of decision-making under risk that incorporate salience (Bordalo et al., 2012) feature diminishing sensitivity to increased salience. For the same reason we avoided states with the highest incidence of COVID-19 in participant recruitment, we expect residents in states with higher COVID-19 severity to be less affected by our display of state COVID-19 statistics. Note that this is the opposite from what would be expected if the state statistics have informational value to the respondents – participants from states with higher COVID-19 rates would react most strongly to our *StateStats* treatment since they will be seeing displays of higher incidence of COVID-19.

$$y_{ij} = \alpha + \beta_1 StateStats_{ij} + \beta_2 HighDeath_j x StateStats_{ij} + \beta_M M_{ij} + \beta_X X_i + \lambda_j Z_j + \epsilon_{ij} \tag{3.1}$$

More formally testing this with a regression model in Equation (3.1), we indicate the response of person i from state j as y_{ij}, and let M_{ij} be controls for other treatments, X_i to be demographic controls, and Z_j to be state fixed effects. In estimating this model, we will take on a variety of outcome variables, largely drawn from the post-treatment survey portion of the experiment, e.g., whether individuals perceive their state to be highly impacted by COVID-19 relative to other states. Taking that particular outcome as an example, if the *StateStats* treatment functions simply as an informational treatment for subjects, β_2 will be positive; participants who live in a high death state and are exposed to that information via the StateStats treatment would be more likely to report that they perceive their state as especially impacted. However, if *StateStats* functions as a salience treatment, $\beta_1 > 0$ and $\beta_2 \leq 0$; that is, by simply raising the presence of any local deaths to the front of participants' minds, the

StateStats treatment will increase their perception of local risk, regardless of actual local conditions. We note that the state fixed effects $\lambda_j Z_j$ are particularly valuable as they strengthen the interpretation of the StateStats treatment as a salience shock rather than an informational shock; by including the fixed effects, we are comparing participants within the same states, therefore with the same death counts, but where some are alerted of the death count and others are not.

We now turn to the effect of government messaging. The "Mask" message treatments were intended to test the government's action-oriented messages while the "Forecast" treatments test the effect of optimistically or pessimistically framing the future outlook of the pandemic in the United States generally (as opposed to the current local condition in the *StateStats* treatment). To illustrate the broader intent of these messages, consider the context of global warming, where action-oriented messages might entail suggesting that the public should or should not drive less to reduce emissions, while a parallel to the forecast messages might entail informing the public that society is or is not on track to achieving carbon emission targets.

$$y_{ij} = \alpha + \beta_1 StateStats_{ij} + \beta_2 AntiMask_{ij} + \beta_3 ProMask_j + \beta_4 OptForecast_i + \beta_5 PessForecast_i + \beta_M M_{ij} + \beta_X X_i + \lambda_j Z_j + \epsilon_{ij} \tag{3.2}$$

We will use the regression model in Equation (3.2) to test the effect of the treatment. The omitted category is the "No Message" or control treatment. However, of interest is not just the difference between each messaging treatment with the absence of messaging, but also comparison between types of messaging, e.g., Anti-Mask vs. Pro-Mask and Optimistic vs. Pessimistic Forecast. Thus, the tables that follow, report β_1 through β_5, but also the difference $\beta_3 - \beta_2$ (Pro- vs. Anti-mask) and $\beta_5 - \beta_4$ (Pessimistic vs. Optimistic Forecast). We have no specific hypothesis with regards to the action-oriented vs future trajectory framing, but we expect that due to the same diminishing sensitivity to increased salience, the messages will have more of an effect when saliency is low – specifically, in the *NoStateStats* treatment.

When thinking about our main dependent variable y_{ij}, which measures participants' willingness to take preventative action against the pandemic in the form of allocating tickets towards a mask, note there are several ways to interpret the outcome of the choice task. If participants know how much they value the face mask, they should put all their tickets into the Starbucks gift card if this value is below $75 and all their tickets into the face mask if this value is **above** $75 (or the dollar equivalent of utility received from $75 in Starbucks purchases). However, given that COVID-19 is an unprecedented situation, it is much more likely that participants have a distribution of possible values for the face masks that include many unknown factors, such as the future severity of the pandemic in their location or the likelihood another mask will be available when they need it, which would result in allocating the tickets between the two options. What we do know is that a participant that allocates **any** tickets to the face mask can envision scenarios where the face mask will be of value to her.[11] In a time where there was not much social acceptance for face masks in the United States (which continues to the time of writing) (Leung, 2020), this behavioral proxy for one's openness to wear a face mask is an important and meaningful measure. Moreover, as noted below, ticket allocations were highly bimodal. For these reasons, our main outcome measure is a binary variable indicating whether participants allocated *any* share of tickets towards the mask as our main outcome measure.

3.3 Results

3.3.1 Demographics and Summary Statistics

We recruited a total of 512 participants from 12 states. Fifty-seven percent of the respondents are female and 27 percent reside in urban areas. Fifty-two percent identified as Democrats/Green party, 17 percent identified as Republican/Libertarian, and the rest as Independents. Sixty-three percent of participants were between 18 and 35, 29 percent between 36 and 55, and 8 percent older than 55. Most participants rate their health as good: average self-reported health rating was 7.6 in a scale of 1-10. For the most part, all these demographics are balanced across treatments. See Appendix Table E.1 for a balance test across treatments. We use these demographics as control variables X_{ij} in Equations (3.1) and (3.2) for the regressions in this paper.

Participants are very well-informed: 87 percent of respondents indicated that they read news at least once a day, suggesting that almost all can be expected to be aware of the threat of COVID-19

[11] A low probability event may become significantly overweighted if the salience of the payoff is increased (Bordalo et al., 2012).

and how their states are doing since this was the issue that dominated the news cycle at that time. Preparation for COVID-19, however, was varied with 40 percent indicating that they had not stocked up on anything, 45 percent focused on stocking up on food and water, and only 5.5 percent focused on stocking up on medical supplies such as masks and thermometers. This suggests to us that most participants do not yet have a face mask, and that a lack of valuation for the face mask cannot be explained by already having one at home.

Moving on to the choice task, 493 (96 percent) respondents completed the task correctly - indicating an allocation of tickets that added up to 100. We drop the remaining 4 percent from our sample. The distribution of ticket allocations was bimodal: 43 percent of participants allocated their tickets entirely to the gift card and 16 percent entirely to the mask. However, a substantial number of people (41 percent) allocated their tickets to both options (10 percent at 50-50). As noted above, we create a binary variable (*Alloc. tix for mask*) that is 1 if participants allocate any tickets to the mask.[12]

3.3.2 Effect of StateStats Treatment

In Section 2.3 we hypothesized that the StateStats treatment, which displays widely available state-specific statistics on COVID-19 incidence, death, and testing information, will have an effect of increasing the salience of the virus' threat rather than inform participants of something new. To test this, in Table 3.3 we run the regression in Equation 3.1 against measures of risk perception and mask importance elicited in the exit survey (Section 3.2.2.4), and the behavioral measure *Alloc. tix for mask*.

Table 3.3 Effect of Displaying State's COVID-19 Statistics to Participants

	(1) State is bad (=1)	(2) State is bad (=1)	(3) State is bad (=1)	(4) Risk to self [0-1]	(5) Mask extr. imp. (=1)	(6) Alloc. tix for mask (=1)
StateStats	0.07** (0.03)	0.05 (0.03)	0.07** (0.03)	0.52 (0.32)	0.10 (0.06)	0.12* (0.06)
StateStats x high death		0.04 (0.06)		-0.47 (0.44)	-0.13 (0.09)	-0.16* (0.09)

[12] Results taking the continuous share of tickets allocated are qualitatively similar. Those results are reported in Appendix Table E.3.

StateStats x very high death			-0.01 (0.08)			
Obs. R-squared	493 0.42	493 0.42	493 0.42	493 0.13	493 0.10	493 0.05

Outcomes: Column 1-3: Respondent agrees that her state has one of ten highest states in death counts as of survey date (1=yes, 0=no). Column 4: Rating of risk to self [scaled from 1-10]. Column 5: Agrees that masks are extremely important (1=yes, 0=no). Column 6: allocate any tickets to the mask (1=yes, 0=no).
High death states are those whose death rates are above the median of the 12 states sampled. Very high death states are those whose death rates are above the top 25%. All specifications include indicator variables for gender, self-identification as Republican or Libertarian, being over 65 or under 35, and living in an urban area, as well as state fixed effects and controls for other treatments. The full regression table is in Appendix Table E.4 and a modified version of this table that uses a continuous measure of *share of tickets allocated to the mask* as the outcome variable can be found in Appendix Table E.3.
Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Columns 1-3 draw on the survey question "*How do you think your state compares to others on the COVID-19 cases and deaths?*", specifically asking whether they think their state is one of the worst ten in the country. Column 1 shows that participants in *StateStats* treatment (β_1) are 7 percent more likely than participants in NoStateStats to think that their state is doing badly, suggesting that the treatment impacts beliefs. In Column 2, we interact the treatment dummy *StateStats* with the binary variable *high death* (which indicate that a state's fatality is above the median of the 12 sampled states). We expect the interaction term to be insignificant or negative ($\beta_2 \leq 0$) if *StateStats* increases salience of COVID risk due to its diminishing marginal sensitivity.[13] We find that the coefficient is positive, but not significant. To test this further, we interact *StateStats* with *Very high death* (=1 if a state's fatality is among the top quartile) in Column 3 and find that while *StateStats* remain significant and positive, the interaction term is negative and insignificant. The same can be seen when we look at other outcome measures, such as a participant's perception of the risk of COVID-19 to herself (*Risk to self*, Column 4), the importance of wearing masks (*Mask extremely important*, Column 5), and most importantly, the participants' willingness to invest any resources in obtaining the mask (*Alloc. tix for mask*, Column 6). The *StateStats* treatment induced participants from less impacted states to be 12 percent to enter the bid for the mask. It has little effect on participants in the more affected states – all linear combination of $\beta_1 + \beta_2$ were not significant in any of the models.

[13] For example, an individual's level of concern about climate change is affected by recent experience of extreme weather but does not change with prolonged extreme weather events (Konisky et al., 2016).

Overall, we see that the State Stats treatment increased the perception of risk and support for preventive measures in the least affected states, which suggests that observing these widely available state statistics increases their top-of-mind awareness of the risk from the pandemic. With this in mind, we now move on to investigate the effect of messages from the federal government and its interaction with salience.

3.3.3 Effect of Government Messages

We begin our discussion of results by first examining how the simple average of our main outcome measure (*Alloc. tix for mask*) varies across treatments in Figure 3.1. Panel A of Figure 3.1 pools all participants. There is some variation in demand for a mask across treatments: participants who see the CDC's message promoting mask usage ("Pro-Mask" treatment) are 14 percentage points more likely to have demand for a mask than participants who read the CDC's earlier message discouraging mask usage ("Anti-Mask" treatment), a difference that is statistically significant at the 5 percent level. There is not a notable difference between the "Optimistic Forecast" and "Pessimistic Forecast" treatments, which will be true in much of the analysis, and is also true when we investigated their effect on perception of risk or support for masks, suggesting that the positive and negative framing that was used to present the IMHE projections at that time were too vague to significantly influence behavior. For that reason, much of the discussion will focus on the mask messaging treatments.

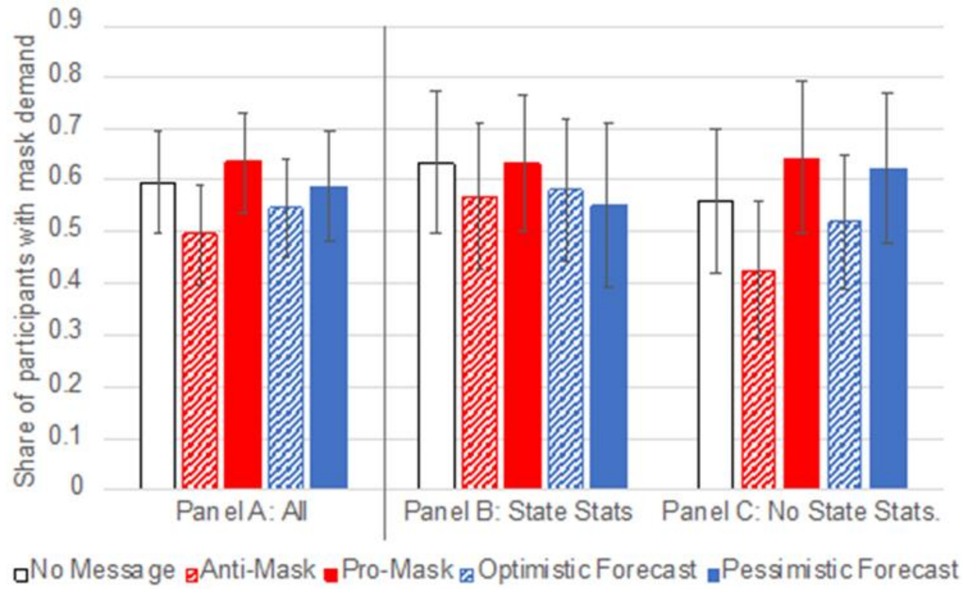

Figure 3.1 Share of Participants that Allocated Any Tickets to Masks in the Choice Task

Next, Panels B and C report the same simple averages split into the two salience treatments. We see that there is more variation in average outcomes across second-stage treatments for participants in the "NoStateStats" treatment, while messaging about masks or forecasts appear to have smaller effects for participants in the "StateStats" treatment. This is consistent with what we have observed all throughout – which is that beliefs and behavior exhibits diminishing marginal sensitivity to additional inducement to pay attention to an issue.

Turning now to the regression analysis specified in Section 2.3, we run Eq. (3.2) against *Alloc. tix for mask*. The coefficients β_1 through β_5, for Info treatment to Negative Forecast treatment, are reported in Table 3.4. Post estimation comparisons between treatments ($\beta_3 - \beta_2$ (Pro- vs. Anti-mask) and $\beta_5 - \beta_4$ (Pessimistic vs. Optimistic Forecast)), which are of equal importance to the main coefficients, are reported on the bottom of the table. Note that the results we will present are qualitatively similar when we take a survey response indicating that participants believe that it is important to wear masks as the dependent variable (Appendix Table E.2), suggesting that these messages affect behavior through beliefs about masks.[14]

[14] One important difference is that stated support for masks increased with government messages across both saliency condition, while actual behavioral changes was only seen in the low saliency (NoStateStats) condition.

Table 3.4 Impact of treatment on likelihood to allocate any tickets to the mask

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: Allocate any tickets towards mask (=1)					
	Full Sample	NoStateStats	StateStats	Full Sample	NoStateStats	StateStats
StateStats (v. No StateStats)	0.044			0.032		
	(0.045)			(0.050)		
Anti-Mask (v. No Msg.)	-0.094	-0.133	-0.083	-0.137*	-0.218**	-0.104
	(0.070)	(0.100)	(0.103)	(0.077)	(0.110)	(0.112)
Pro-Mask (v. No Msg.)	0.030	0.096	-0.029	0.029	0.120	-0.039
	(0.072)	(0.108)	(0.101)	(0.077)	(0.114)	(0.109)
Pos. Forecast (v. No Msg.)	-0.027	0.004	-0.042	-0.034	-0.033	-0.006
	(0.070)	(0.101)	(0.103)	(0.077)	(0.111)	(0.115)
Neg. Forecast (v. No Msg.)	-0.003	0.083	-0.074	0.028	0.080	-0.021
	(0.075)	(0.107)	(0.112)	(0.082)	(0.113)	(0.125)
Post-Estimation Comparisons						
Pro-Mask (v. Anti-Mask)	0.124*	0.229**	0.054	0.167**	0.338***	0.065
	(0.072)	(0.107)	(0.101)	(0.078)	(0.116)	(0.111)
Neg. F'cast (v. Pos. F'cast)	0.024	0.079	-0.032	0.062	0.114	-0.015
	(0.074)	(0.105)	(0.111)	(0.083)	(0.111)	(0.130)
Excludes Repubs. & Libertarians				Yes	Yes	Yes
Observations	493	249	244	407	206	201
R-squared	0.040	0.089	0.060	0.040	0.135	0.040

All specifications include controls for participant gender, an indicator for self-identifying as Republican or Libertarian, indicator variables for being over 65 or under 35, an indicator variable for living in an urban area, and state fixed effects. The outcome variable is equal to 1 if any tickets were allocated to the mask, and 0 otherwise.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Results are reported in Table 3.4, Columns 1-3. In Column 1, we pool both salience treatments together. None of the federal government messages change behavior such that there was a statistically significant different from the no message treatment, however, the Pro-Mask vs. Anti-Mask comparison $\beta_3 - \beta_2$ revealed that participants receiving Pro-Mask CDC messages are 12.4 percent more likely to enter the bid for the mask than participants who were exposed to CDC's Anti-Mask messages. When we split the sample by the StateStats treatment, we see that this effect was coming from the NoStateStats treatment, where Pro-Mask subjects are 22.9 percent more likely to allocate some of their lottery tickets to the mask than Anti-Masks subjects. The estimate in the Pro-Mask vs. Anti-Mask comparison ($\beta_3 - \beta_2$) was much smaller and not significant in the StateStats treatment, suggesting that government messages matters most when the risk has not been made salient to participants.[15] This is an intuitive finding, but also important from a policy standpoint: messaging about taking risk-mitigating action may have the largest marginal effect where people are not already thinking frequently about the looming risk. One can imagine implications of this finding not just for mitigating the effects of COVID-19, but also other risks with varying levels of salience across different groups and geographies, such as climate change.

The analysis of Columns 1-3 tests how people may shift their behavior in response to government messaging. However, there has already been some evidence in the context of the COVID-19 pandemic that political partisan affiliation shapes individuals' willingness to adopt mitigating behaviors. Specifically, Painter & Qiu (2020) and Allcott et al. (2020) find that Democrats are substantially more likely than Republicans to follow social distancing guidelines. To the extent that that is generally true, it may mean that some portion of our sample is generally non-responsive to our treatments. We therefore re-estimate the specifications of Table 3.4 Columns 1-3, but with a sample where we have dropped self-identified Republicans and Libertarians (16 percent). Despite dropping observations, as expected, results are more pronounced in this sample (Column 4-6). In the low salience condition, Pro-mask messaging increases the fraction of participants valuing the mask by 33 percent; anti-mask messaging now significantly decreases likelihood of valuing masks by 22 percent relative to the no message treatment. This suggests that both positive and negative messaging matters: suggesting mask-wearing *can* increase the demand for masks, and likewise minimizing the importance of mask-wearing *can* decrease the demand for masks.

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[15] Though splitting the sample to Models (2) and (3) eases interpretation, we have also estimated a model where StateStats is interacted with the four second-stage treatments. The difference between Pro-vs. Anti-mask is large (17 percent) but not statistically significant (p-val.=0.225).

3.4 Conclusions

The main findings of this study can be summarized in two key points. First, action-oriented government messages have an impact on both perceptions about risk and on behavior.[16] And second, that the impact of messaging on behavior is strongest when risk is least salient. These findings clarify the conditions under which we can expect government messaging to have the greatest impact and explain the variation in effects across the literature. Our clarification implies that policymakers should craft messages that use action-oriented directives that explicitly encourage (or discourage) the desired action and that they may find more success in conditions where risks are less known.

Though our experiment was conducted in the context of the COVID-19 pandemic with findings related to willingness to pay for personal protective equipment, there are implications for policy in other arenas. For example, government messaging about climate change could be constructed as action-oriented directives targeting individuals' behavior rather than broader messages about future danger. Policies in the arena of public health may find use for such messaging to encourage vaccination among residents or discourage unhealthy eating habits. Disaster management could similarly benefit from designing clear and directive messages in lower information conditions to encourage evacuation or sheltering in place among residents.

[16] Schotter (2003) remarked that "If given a choice between getting advice or the information upon which that advice was based, subjects tend to opt for the advice, indicating a kind of under-confidence in their decision-making abilities that is counter to the usual egocentric bias or overconfidence observed by psychologists." (p.199).

Appendix A Extreme Weather – Additional Tables and Figures

Table A.1 Summarizing Events and Deaths for the period 2014-2016 across all states

Event Type	Deaths	Events
Avalanche	39	66
Blizzard	2	165
Coastal Flood	1	143
Cold/Wind Chill	93	285
Debris Flow	47	39
Dense Smoke	0	1
Drought	0	691
Dust Devil	0	28
Dust Storm	4	75
Excessive Heat	64	125
Extreme Cold/Wind Chill	26	322
Flash Flood	65	1261
Flood	66	1212
Freezing Fog	1	18
Frost/Freeze	0	254
Funnel Cloud	3	214
Hail	17	2440
Heat	95	236
Heavy Rain	36	788
Heavy Snow	4	802
High Surf	22	195
High Wind	18	1573
Ice Storm	0	81
Lake-Effect Snow	1	129
Lakeshore Flood	0	3
Lightning	62	444
Rip Current	106	136
Sleet	0	10
Storm Surge/Tide	3	4
Strong Wind	32	594
Thunderstorm Wind	169	8109
Tornado	211	1436
Tropical Depression	1	2
Tropical Storm	22	49

Tsunami	0	1
Wildfire	13	717
Winter Storm	27	970
Winter Weather	39	2111
Total Events	1289	25729

Table A.2 Weather Impact on Non-Politicized Opinion

Shows the impact that weather events have on a respondents' opinion that weather has been getting more extreme over the past 40 years. This table uses Equation A1 for Model 5 to check the robustness of the results in the Non-Politicized Frame when accounting for lagged salience measures and their interaction. Appendix Tables A.2 and A.3 demonstrate that including these terms in the main tables makes a negligible difference in effects. Models 2-3 drop the lagged variables out, and Model 1 drops both the lagged variables and the interaction between Any Deaths and Over Median Events (to check the Baseline Model).

$$CO_i^{frame} = \alpha + \beta_1 E_{its} + \beta_2 D_{its} + \beta_3 D_{its}E_{its} + M_i + S_{ist} + \lambda_{iw} + LE_{st-1} + LD_{sy-1} + LE_{sy-1}LD_{sy-1} + \varepsilon \quad \text{(A1)}$$

Where:

$CO_i^{frame} = Respondent's\ climate\ opinion\ within\ the\ specified\ frame$

$E_{its} = 1\ if > 6\ events\ occured\ in\ respondent\ (i)\ state\ (s)\ during\ 4\ weeks\ leading\ to\ survey\ (t)$

$D_{its} = 1\ if > 0\ deaths\ occured\ in\ respondent\ (i)\ state\ (s)\ during\ 4\ weeks\ leading\ to\ survey\ (t)$

$D_{its}E_{its} = interaction\ term\ between\ Over\ Median\ Events\ and\ Any\ Deaths\ (D_{its} * E_{its})$

$M_i = individual\ level\ demographic\ indicators\ (income, age, education, partisanship)$

$S_{st} = gubernatorial\ partisanship\ for\ individual\ (i)'s\ state\ (s)\ during\ year\ (t)$

$\lambda_{iws} = Fixed\ effects\ for\ state\ (s)\ and\ survey\ wave\ (w)\ individual\ (i)\ responded\ during$

$LE_{isy-1} = mean\ of\ Over\ Median\ Events\ for\ the\ individual's\ state\ (s)\ lagged\ by\ 1\ year$

$LD_{isy-1} = mean\ of\ Any\ Deaths\ for\ the\ individual's\ state\ (s)\ lagged\ by\ 1\ year$

$LE_{sy-1}LD_{st-1} = interaction\ between\ lagged\ terms$

	(1)	(2)	(3)	(4)	(5)
VARIABLES	More Extreme	More Extreme	Less Extreme	Same Extreme	More Extreme
Over Median Events	-0.03	-0.00	-0.00	0.00	-0.00
	(0.04)	(0.04)	(0.02)	(0.04)	(0.05)
Any Deaths	-0.03	0.10*	0.03	-0.13***	0.10
	(0.06)	(0.05)	(0.03)	(0.04)	(0.06)
Death * Events		-0.16**	-0.01	0.18***	-0.16**
		(0.07)	(0.03)	(0.06)	(0.07)
Lagged Over Median Events					0.01
					(0.06)
Lagged Any Deaths					0.05

					(0.11)
Lagged Events * Lagged Deaths					-0.10
					(0.11)
State Party	0.03	0.05	-0.04***	-0.01	0.04
	(0.05)	(0.05)	(0.01)	(0.04)	(0.05)
Respondent Party	-0.23***	-0.22***	-0.00	0.23***	-0.22***
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
College	0.06**	0.06**	-0.00	-0.05**	0.06**
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Low Income	0.01	0.01	0.01	-0.01	0.01
	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)
Over 65	-0.01	-0.01	-0.00	0.01	-0.01
	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)
Constant	1.21***	1.15***	0.03	-0.18***	1.15***
	(0.06)	(0.06)	(0.02)	(0.06)	(0.06)
Lincom B2 + B3		-0.04	0.01	0.03	-0.03
		0.04	0.01	0.03	0.05
Observations	1,267	1,267	1,267	1,267	1,267
R-squared	0.09	0.10	0.04	0.11	0.10

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.3 Weather Impact on Politicized Opinion

Shows the impact that weather events have on a respondents' opinion that there is scientific evidence of global warming. This table uses Equation A1 in the same fashion detailed for Appendix Table A.2 but for the Politicized Frame.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Yes, Evidence	Yes, Evidence	No Evidence	Not Sure	Yes, Evidence
Over Median Events	0.00	-0.01	0.00	0.01	-0.04
	(0.05)	(0.05)	(0.03)	(0.03)	(0.05)
Any Deaths	0.06	-0.03	-0.09	0.12	0.01
	(0.04)	(0.09)	(0.09)	(0.10)	(0.09)
Events * Deaths		0.11	0.02	-0.12	0.10
		(0.11)	(0.09)	(0.12)	(0.11)
Lagged Over Median Events					0.14
					(0.10)
Lagged Any Deaths					-0.13
					(0.16)
Lagged Events * Lagged Deaths					-0.04
					(0.15)
State Party	-0.06	-0.07	0.07*	0.00	-0.08
	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)
Respondent Party	-0.36***	-0.36***	0.32***	0.04	-0.36***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)

College	0.12***	0.12***	-0.06**	-0.07***	0.13***
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Low Income	0.00	0.00	0.01	-0.01	-0.00
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Over 65	0.01	0.01	0.00	-0.01	0.01
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Constant	0.73***	0.77***	-0.01	0.24***	0.76***
	(0.07)	(0.08)	(0.06)	(0.06)	(0.08)
Lincom B2 + B3		0.10***	0.10***	0	0.12***
		0.04	0.03	0.03	0.04
Observations	1,267	1,267	1,267	1,267	1,267
R-squared	0.20	0.20	0.21	0.07	0.21
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1					

Table A.4 Recreating Main Table 1.3 with sample that includes independents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Pooled	Dem	Rep	Ind	Pooled	Dem	Rep	Ind
Over Median Events	-0.04	-0.00	-0.02	-0.04	-0.01	0.03	-0.00	-0.04
	(0.03)	(0.04)	(0.08)	(0.05)	(0.04)	(0.05)	(0.08)	(0.06)
Any Deaths	0.02	-0.06	-0.02	0.10**	0.14***	0.07	0.19*	0.14**
	(0.04)	(0.05)	(0.09)	(0.05)	(0.04)	(0.06)	(0.10)	(0.06)
Any Deaths * Over Med Events					-0.16***	-0.18**	-0.25*	-0.04
					(0.05)	(0.08)	(0.13)	(0.07)
State Party	0.04	0.07*	-0.02	0.05	0.05	0.08**	-0.01	0.05
	(0.04)	(0.04)	(0.08)	(0.08)	(0.04)	(0.03)	(0.09)	(0.08)
Respondent Party R	0.04*	0.08**	0.03	0.02	0.04*	0.08**	0.03	0.02
	(0.02)	(0.03)	(0.04)	(0.04)	(0.02)	(0.03)	(0.04)	(0.04)
College	0.03	-0.03	0.10	-0.01	0.03	-0.03	0.10	-0.01
	(0.04)	(0.05)	(0.08)	(0.07)	(0.04)	(0.05)	(0.08)	(0.07)
Low Income	-0.03	0.01	-0.06	-0.05	-0.02	0.02	-0.05	-0.05
	(0.02)	(0.03)	(0.06)	(0.04)	(0.02)	(0.03)	(0.06)	(0.04)
Over 65	0.94***	0.87***	1.06***	0.86***	0.87***	0.83***	0.98***	0.84***
	(0.04)	(0.05)	(0.10)	(0.10)	(0.04)	(0.05)	(0.10)	(0.10)
Observations	1,876	736	531	609	1,876	736	531	609
R-squared	0.03	0.07	0.09	0.09	0.03	0.07	0.09	0.09
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1								

Table A.5 Recreating Main Table 1.4 with Sample that includes Independents

VARIABLES	(1) Pooled	(2) Dem	(3) Rep	(4) Ind	(5) Pooled	(6) Dem	(7) Rep	(8) Ind
Over Median Events	0.00	0.07	-0.03	-0.01	-0.01	0.05	-0.02	-0.03
	(0.03)	(0.06)	(0.07)	(0.07)	(0.03)	(0.05)	(0.07)	(0.07)
Any Deaths	0.11***	-0.02	0.15**	0.16**	0.04	-0.11	0.22	0.02
	(0.03)	(0.04)	(0.06)	(0.07)	(0.05)	(0.08)	(0.17)	(0.14)
Any Deaths * Over Med Events					0.09	0.14	-0.09	0.17
					(0.06)	(0.11)	(0.18)	(0.15)
State Party	-0.03	-0.08*	-0.13	0.08	-0.04	-0.09**	-0.12	0.07
	(0.06)	(0.04)	(0.12)	(0.10)	(0.06)	(0.04)	(0.12)	(0.10)
Respondent Party R	0.10***	0.16***	0.10**	0.03	0.10***	0.16***	0.10**	0.03
	(0.02)	(0.04)	(0.05)	(0.04)	(0.02)	(0.04)	(0.05)	(0.04)
College	0.02	0.01	-0.02	-0.04	0.02	0.01	-0.02	-0.04
	(0.03)	(0.04)	(0.10)	(0.06)	(0.03)	(0.04)	(0.10)	(0.06)
Low Income	-0.04	0.07**	-0.05	-0.11*	-0.04	0.07*	-0.05	-0.11*
	(0.03)	(0.03)	(0.06)	(0.06)	(0.03)	(0.03)	(0.06)	(0.06)
Over 65	0.66***	0.72***	0.48***	0.81***	0.70***	0.75***	0.45***	0.88***
	(0.06)	(0.07)	(0.13)	(0.11)	(0.06)	(0.07)	(0.14)	(0.13)
Observations	1,876	736	531	609	1,876	736	531	609
R-squared	0.05	0.16	0.12	0.12	0.05	0.16	0.12	0.12

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.6 Confidence Intervals for Negligible Effects Tests

Table 1.3		Coeff.	SE	Confidence Interval
Baseline	Model 3 - B1: Pooled Sample	-0.03	0.04	-0.0958 to 0.0358
	Model 4 - B1: Republicans	-0.02	0.08	-0.1516 to 0.1116
	Model 5 - B1: Democrats	0	0.04	-0.0658 to 0.0658
Shock	Model 8 - B2: Democrats	0.07	0.06	-0.0287 to 0.1687
Table 1.4				
Baseline	Model 3 - B1: Pooled Sample	0	0.05	-0.08225 to 0.08225
	Model 4 - B1: Republicans	-0.03	0.07	-0.14515 to 0.08515
	Model 5 - B1: Democrats	0.07	0.06	-0.0287 to 0.1687
Shock	Model 6 - B2: Pooled Sample	-0.03	0.09	-0.17805 to 0.11805
	Model 7 - B2: Republicans	0.22	0.17	-0.05965 to 0.49965
	Model 8 - B2: Democrats	-0.11	0.08	-0.2416 to 0.0216
Oversaturation	Model 6 - B3: Pooled Sample	0.11	0.11	-0.07095 to 0.29095
	Model 7 - B3: Republicans	-0.09	0.18	-0.3861 to 0.2061
	Model 8 - B3: Democrats	0.14	0.11	-0.04095 to 0.32095

Appendix B Paradise Lost – Detailed Process for Cleaning Tweets

The intervention variable enters the RD and OLS models as a measure of distance that a residence was from the BOI, *Directional Distance*. This is a positive measure in zones that received an order and negative in zones that did not. The data used to determine if a structure received an order comes from Twitter, which I systematically reviewed to construct a timeline of evacuation orders issued by zone. This process was accomplished by using R-studio package 'rtweet' to scrape Twitter's API for tweets that occurred during the first six hours of the fire. Figure B.1 maps the process used to identify the Tweets used in this study. I first compiled a list of 192 Twitter accounts from the area that belonged to six categories: news & media, government organizations, fire departments, fire scanners (bots that scrape other accounts), and non-governmental organizations. This produced over 4,200 unique tweets, which I narrowed to a set of 1,375 that contained key words indicating information about the fire. These words were: fire, wildfire, evacuate, evacuation, warning, order, and zone. Below is a sample tweet that was included in the creation of the intervention variable:

"EVACUATION ORDER 9:22 AM-an evacuation order has been issued for the South Pine Zone, Old Magalia Zone and the South Coutelenc Zone. If assistance is needed to evacuate, please call 911 #ButteSheriff #CampFire" (Butte County Sheriff, 2018)

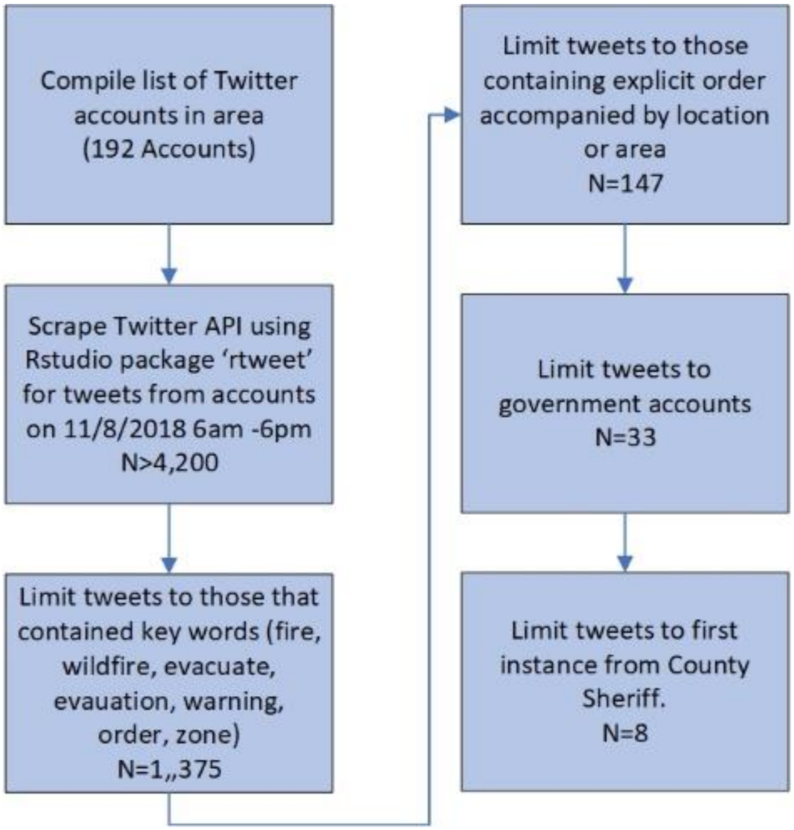

Figure B.1 Process for cleaning tweets from 4,200 to the 8 used for the time and order variables

From the remaining 1,375 tweets I then narrowed those to ones that contained explicit evacuation orders (147 tweets) but also identified a zone or region to evacuate. This effectively restricted the initial sample of tweets to those issued by the Butte County Sheriff account. Among the 33 evacuation-relevant tweets from government officials, 8 of them ultimately served to construct two elements of this study: the main intervention variable: *Distance* and *Order*. To summarize, the eight tweets used to construct the timeline met the following criteria:

1. Posted within the first six hours of the fire;
2. by an official government account;
3. that had been retweeted by multiple news sources;
4. contained the key word "Evacuation", and;
5. specified a zone or zone-equivalent area to evacuate.

The eight Tweets that were ultimately used for the study are found in Table B.1 along with a breakdown of how many structures within the burn perimeter were targeted by each Tweet. Figure

B.2 shows a basic map of the burn perimeter with evacuation zones overlaid. The zones colored blue were issued evacuation orders while those that did not are shown in gray.

For the OLS models and event history, I addressed the time issues were ordered more explicitly, the variable was constructed by ordering the timestamp on each of the tweets chronologically and separating into five periods over the first six hours of the fire. I restrict to tweets within the first five hours because it encompasses the full time that residents would have had to potentially evacuate the area.

Looking again to Table 2.2, many tweets (like the one quoted above), begin with "EVACUATION ORDER" and are immediately followed by a time. The time shown there (bold in the table) is when the order was officially announced, and as the table demonstrates, those times mirror the official order but experience some lag. In Table 2.2, *Period* groups those times in half-hour segments such that the first order was issued at 9:03PST and belongs to Period 1, the second and third tweets were issued at 9:41PST and 9:55PST respectively and belonged to period 2. By grouping and ordering these chronologically I created a variable suitable for adding into the OLS models as a fixed effect. Figure 2.2 again shows a map of the burn area with evacuation zones outlined, but zones that received an order are shaded in accordance with their chronological zone orders (darker colors representing later periods) while zones shaded grey were not issued orders within the analyzed timeframe. The number specified on the map designates the period orders were issued. The zones marked with a 0 did not receive an order.

The decision to use Sheriff tweets was made from necessity involving data availability. The official evacuation warning system used a county-level, privately owned, opt-in alert system called Code Red. The system has been criticized as having minimal reach: only about 25 percent of Paradise residents were enrolled at the time of the fire and only about a quarter of those enrolled were sent an order during the fire (St. John et al., 2018). Still, several months after the fire, there had been no substantive claims about how many residents who were enrolled in the system actually received the order via Code Red. Twitter data, although unconventional, provided an accurate timeline of orders when considering the barriers to using Code Red's auto-dialer system. Code Red data is proprietary and only able to reach a limited number of enrolled residents per minute, creating substantial questions about timing, access, and efficacy even if it were used. Twitter, though it faces similar if not more restricted reach, is not further limited by reach or delay within those who are on twitter.

Appendix C Paradise Lost – Additional Tables and Tables

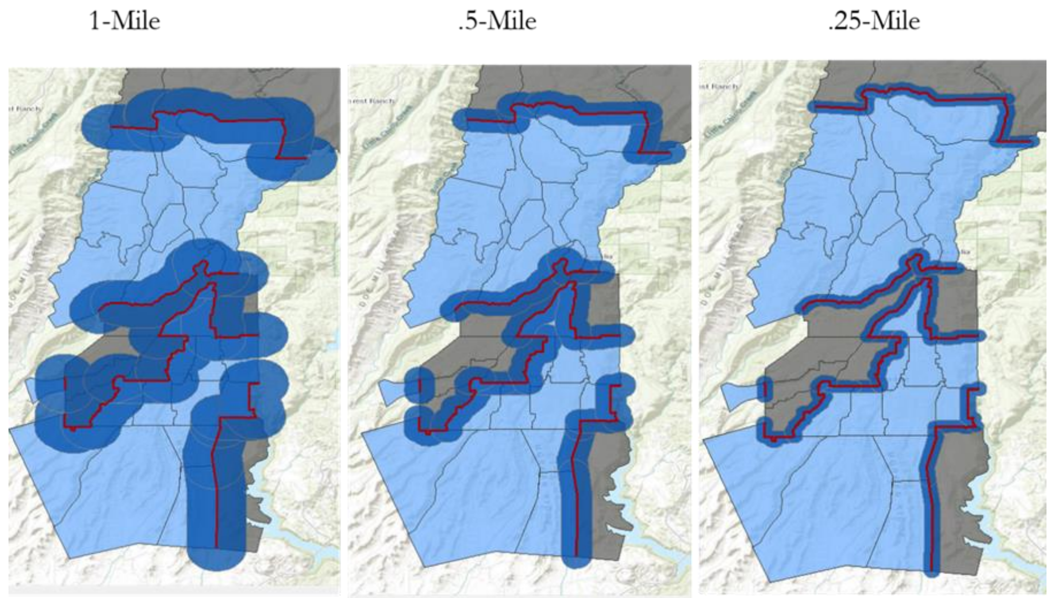

Figure C.1 1-mile, .45-mile, and .25-mile bandwidths surrounding borders of interest

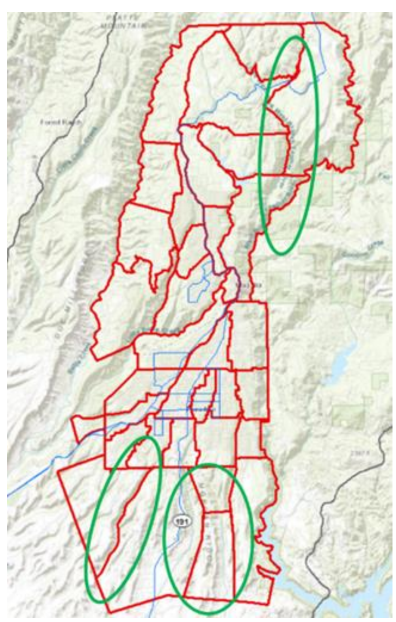

Figure C.2 Border Validation – Topography and Roadways

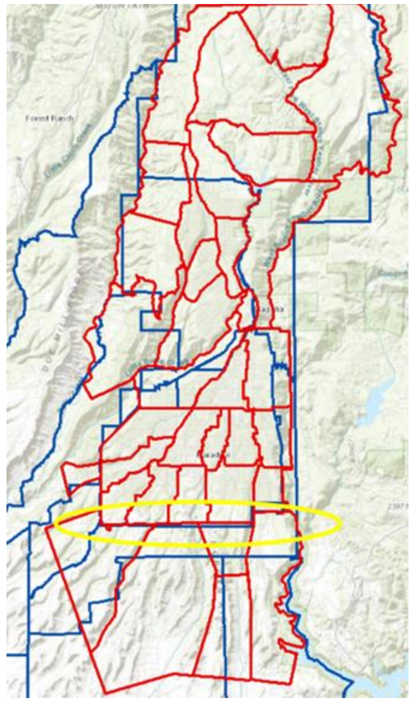

Figure C.3 Border Validation - Precincts Groups

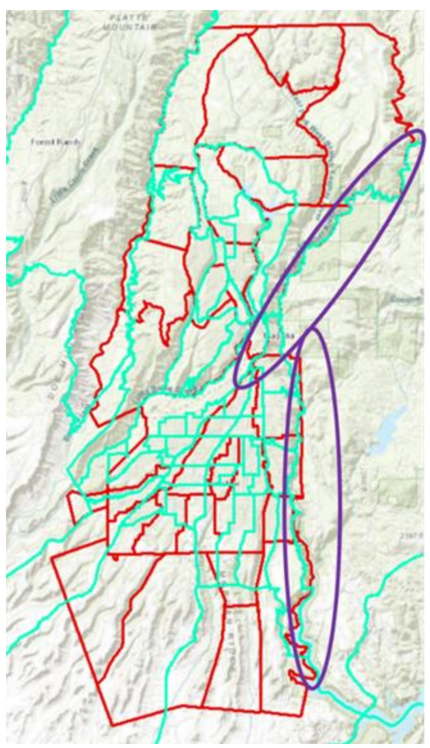

Figure C.4 Border Validation - Census Blocks

Red lines identify the BOIs between zones with dissimilar zone orders. Light grey zones did not receive an order while light blue zones received an order to evacuate. Dark blue areas surrounding red lines demarcate the 1-mile, .45-mile, and .25-mile bandwidths surrounding BOIs.

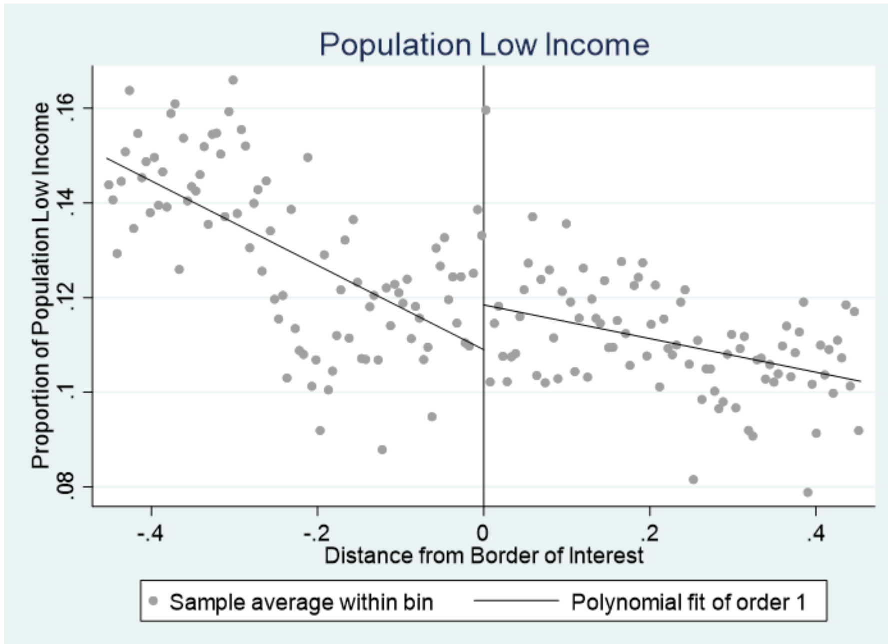

Figure C.5 Regression Discontinuity Balance Check for % Low Income

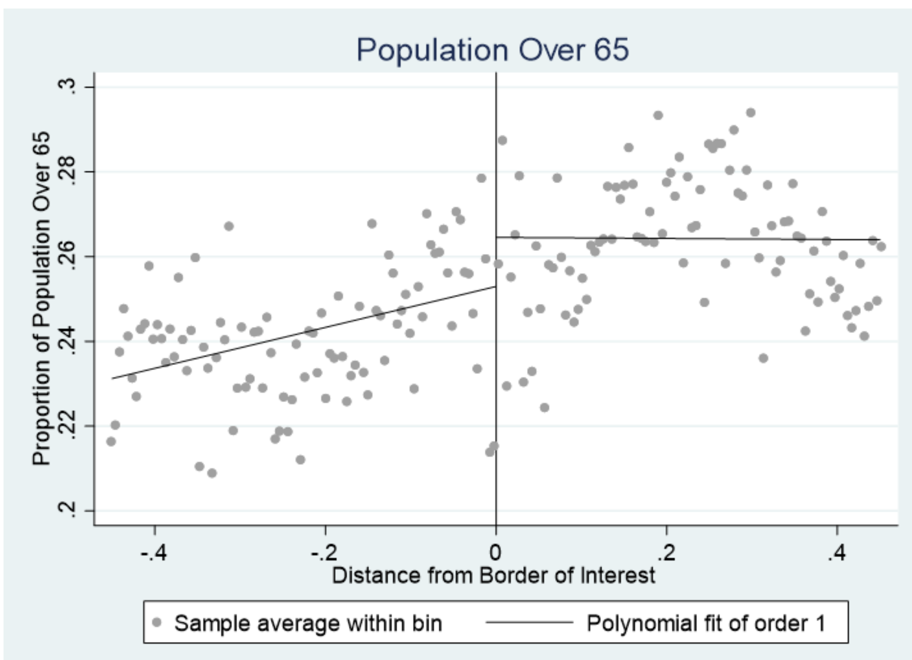

Figure C.6 Regression Discontinuity Balance Check fo %65+

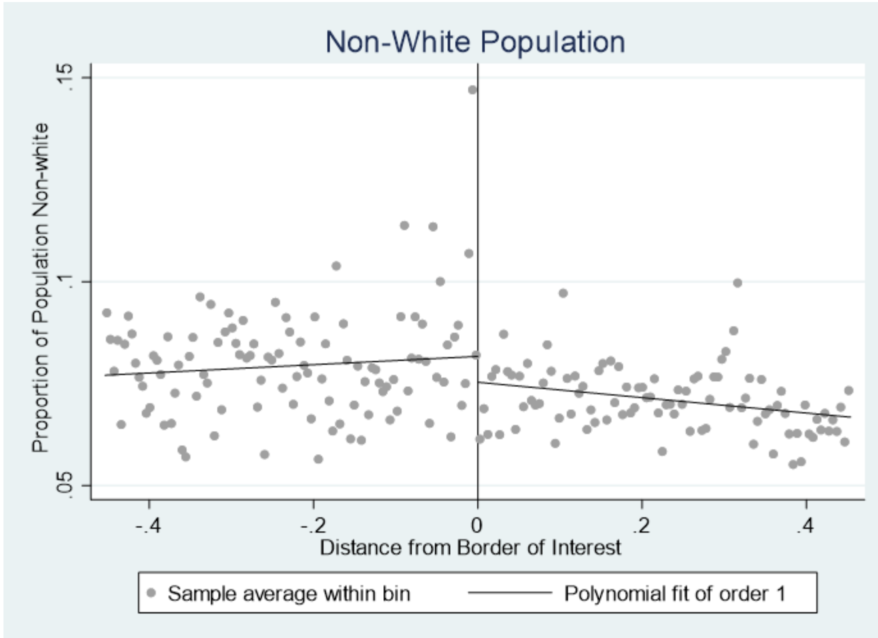

Figure C.7 Regression Discontinuity Balance Check for BIPOC

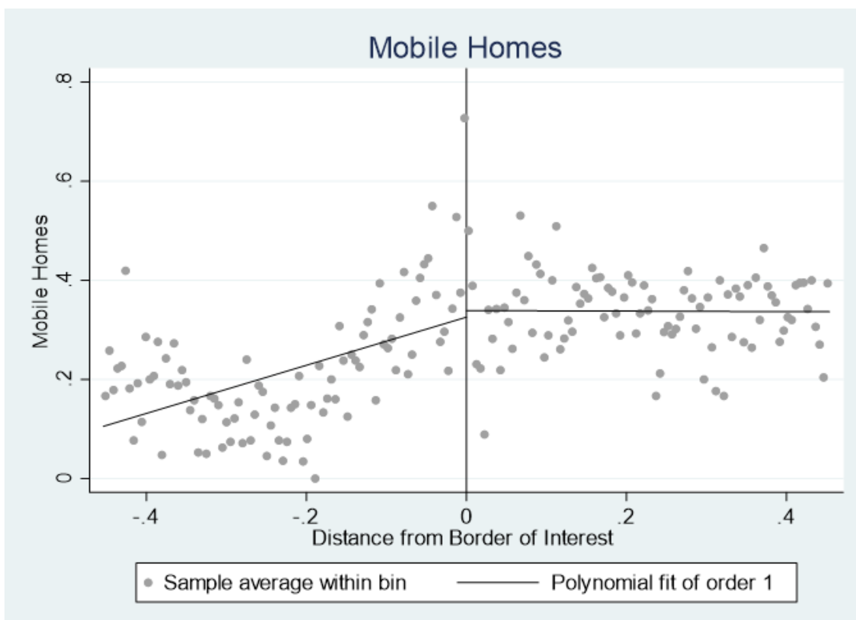

Figure C.8 Regression Discontinuity Balance Check for Mobile Homes

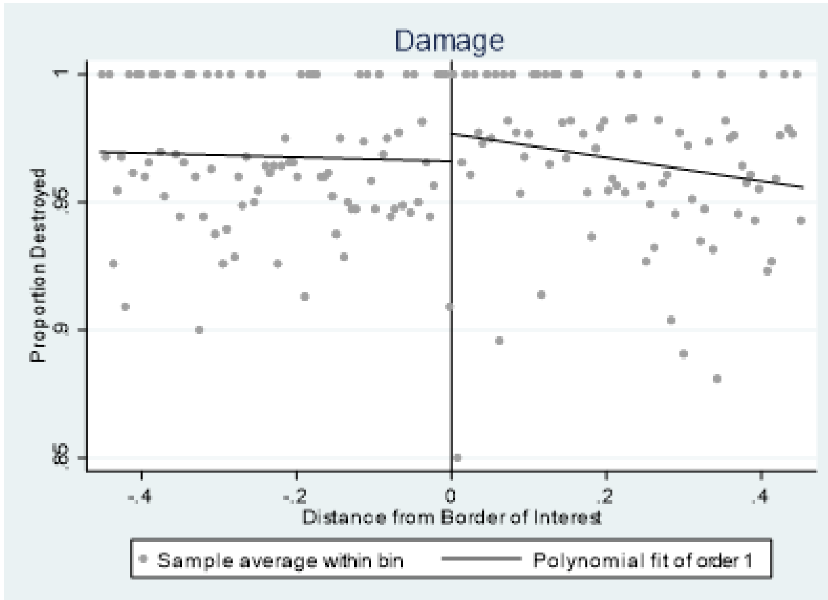

Figure C.9 Regression Discontinuity Balance Check for Mobile Homes

Table C.1 Regression Discontinuity Estimate Robustness Check: Using triangular kernel

VARIABLES	Model 1 **Optimal BW**	Model 2 **.25-mile**	Model 3 **1-mile**	Model 4 **Full Sample**
RD Estimate	0.0036 (0.2945)	-0.0597 (0.3920)	-0.0481 (0.2047)	0.1577 (0.1587)
Observations in BW	6,844	3,908	11,263	13,668
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1				

The tables in the main text use uniform kernel in the non-parametric local-linear regressions. The reasoning behind this is because the absolute value of distance a structure is relative to zone borders within the specified bandwidth should have little to no impact on their likelihood of survival. Weighting each uniformly assumes that zone borders were drawn with access to evacuation routes in mind. If using a triangular kernel, structures closer to the border would be weighted more than those farther away, which is counter intuitive since distance is not particularly relevant to the question.

Table C.2 Robustness Check for Main Table 2.4

VARIABLES	Model (1) Pop. Density	Model (2) % Poverty	Model (3) % BIPOC	Model (4) % 65+	Model (5) Mobile Home	Model (6) Damage
RD Estimate	-0.1940**-	-0.0027	-0.0099**	0.0115**	-0.1022***	-0.1022***
.25-mile BW	(0.0844)	(0.0041)	(0.0041)	(0.0046)	(0.0308)	(0.0308)
Observations in BW	3,908	3,908	3,908	3,908	3,908	3,908
RD Estimate	0.2651***	-0.0064***	-0.0113***	0.0153***	0.0702***	-0.0035
1-mile BW	(0.0448)	(0.0023)	(0.0019)	(0.0023)	(0.0151)	(0.0061)
Observations in BW	11,263	11,263	11,263	11,263	11,263	11,263
RD Estimate	0.1609***	-0.0385***	-0.0156***	0.0176***	0.0417***	0.0040
Full Sample	(0.0395)	(0.0019)	(0.0016)	(0.0019)	(0.0125)	(0.0054)
Observations in BW	13,668	13,668	13,668	13,668	13,668	13,668

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.3 Full Report of Coefficients for RD with Controls

VARIABLES	(1) Optimal BW	(2) .25-mile	(3) 1-mile	(4) Full Sample
Order	0.0382	-0.3096	-0.2521	-0.0629
	(0.2916)	(0.3943)	(0.2606)	(0.1962)
Distance	-0.5598	-1.4797	0.0299	0.0780
	(0.8656)	(2.6510)	(0.2702)	(0.1398)
Order* Distance	1.0800	3.1823	0.2943	-0.0950
	(1.1079)	(3.5194)	(0.4151)	(0.2246)
Damage	0.3326***	0.3874**	0.3403***	0.3003***
	(0.0980)	(0.1580)	(0.0792)	(0.0645)
% Low Inc	0.8310	1.6174*	0.5685	0.6077
	(0.6841)	(0.9670)	(0.7093)	(0.6280)
% 65+	2.7725**	2.4223	1.9873**	2.2189**
	(1.3203)	(1.9414)	(0.9926)	(0.9504)
%BIPOC	1.0957	1.5609	1.6540	1.3404
	(1.9713)	(2.9089)	(1.2752)	(1.1131)
Pop. Density	0.0808	-0.0554	0.0149	0.0188
	(0.0846)	(0.0572)	(0.0644)	(0.0512)

Single Wide Mobile	0.3677	0.1216	-0.1345	0.1934
	(0.4744)	(0.4644)	(0.3737)	(0.3841)
Triple Wide Mobile	0.5884	0.9556	-0.1130	-0.1551
	(1.0358)	(1.3496)	(0.6785)	(0.4892)
Motor Home	-0.4363	-0.1398	-0.6970***	-0.5776***
	(0.3002)	(0.2993)	(0.2234)	(0.1820)
Multi-Family, Multi-Story	-0.4608*	-0.2239	-0.6886***	-0.5657***
	(0.2479)	(0.2421)	(0.2167)	(0.1908)
Multi-Family, Single-Story	0.9530	1.5903	1.0369	1.4219
	(0.9608)	(1.3166)	(0.9101)	(0.9298)
Single-Family, Multi-Story	-0.1269	-0.0701	-0.4630*	-0.3503*
	(0.2572)	(0.2602)	(0.2362)	(0.1981)
Single-Family, Single-Story	-0.1017	0.0081	-0.4770**	-0.3420*
	(0.2198)	(0.2476)	(0.2184)	(0.1803)
Period 1	0.1549	0.0621	0.1173	0.1145
	(0.3094)	(0.3794)	(0.2393)	(0.2252)
Period 2	0.4206	0.3248	0.0194	0.0359
	(0.4257)	(0.6064)	(0.2844)	(0.2627)
Period 3	0.2250	-0.0880	-0.1324	-0.1473
	(0.3789)	(0.6488)	(0.2532)	(0.3509)
Period 4	0.1336	-0.2715	-0.1908	-0.0832
	(0.3572)	(0.4233)	(0.2573)	(0.2470)
Period 5	0.2277	-0.1853	-0.1725	-0.0876
	(0.4157)	(0.5120)	(0.3018)	(0.2861)
Constant	-1.3209	-0.9146	-0.2310	-0.4365
	(0.8212)	(1.0611)	(0.5876)	(0.5620)
Observations	6,844	3,908	11,263	13,660
R-squared	0.0031	0.0045	0.0032	0.0038

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The coefficient on Distance, B1= .56 (p=.518), though also not significant, suggests that as distance from BOIs increases in zones that were not issued orders so too does the likelihood of that structure containing a fatality (meaning those structures were less likely to evacuate). Finally, the coefficient on the interaction term (Order * Distance) is negative and non-significant, B3= -.039 (p=.970). This indicates that for zones that were issued orders as distance from BOIs increased, those structures were more likely to be evacuated (or more precisely, less likely to contain deaths by .039%).

Appendix D Paradise Lost: Follow-up Results

D.1 Deaths as a Function of Demographics

To investigate the impact of demographics on deaths, I conducted a regression analysis using Equation D2, where Y_{ijk} represents death at structure *i* in zone *j* in block group *k* as a function of socio-demographic indicators (M_{ijk}). The indicators tested are *% Over 65, % Low Income, % BIPOC*, and *Population Density* within census block group *k*. The model controls for *Damages* (A_i) at structure *i* and considers the fixed effects (λ_{ts}) on Time *(t)* and Structure Type *(s)*.

$$Y_{ijk} = \alpha + \beta_n M_{ijk} + A_i + \lambda_{ts} + \varepsilon_i \tag{D1}$$

Models 1 and 2 in Table D.1 summarize these effects with and without controlling for damages. Model 2 shows a coefficient of 2.25 (p=.013) which indicates that areas with a higher percentage of residents over the age of 65 were more likely to experience deaths at them, significant at the 95% level. Unsurprisingly, these results are consistent with the extant literature that suggests that elderly populations are at the increased risk during disaster.

Table D.1 Demographic Impact on Deaths and Orders

VARIABLES	Model (1)	Model (2)	Model (3)	Model (4)
	Outcome: Deaths		Outcome: Order	
% 65+	2.26**	2.25**	1.03***	1.03***
	(0.90)	(0.90)	(0.07)	(0.07)
% Low Income	0.66	0.64	-0.33***	-0.33***
	(0.75)	(0.75)	(0.06)	(0.06)
% BIPOC	1.39	1.36	-0.29***	-0.29***
	(1.05)	(1.05)	(0.08)	(0.08)
Population Density	0.02	0.02	0.03***	0.03***
	(0.04)	(0.04)	(0.00)	(0.00)
Damage		0.30		-0.01
		(0.28)		(0.02)
Constant	-0.44	-0.72*	0.35***	0.36***
	(0.30)	(0.40)	(0.02)	(0.03)
Fixed Effects:	Time & Structure Type		Structure Type	
Observations	13,660	13,660	13,660	13,660

R-squared	0.00	0.00	0.07	0.07
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1				

Appendix D.2 Orders as a Function of Demographics

To test if demographics have an impact on who was issued orders, I employ Equation D2 to test if demographic indicators had an impact on where orders were issued.

$$Z_{ijk} = \alpha + \beta_n M_{ijk} + A_i + \lambda_s + \varepsilon_i \quad \text{(D2)}$$

Here, Z_{ij} represents an evacuation order at at structure *i*, in zone *j* as a function of demographic indicators within block group *k*. Fixed effects are specified for *Structure Type* by λ_i (Time is omitted to avoid multicollinearity with order) Models 3 and 4 of Table D.1, show the effects of demographics on orders with and without a control for damages. The significant positive coefficient on *% 65+*, 1.03 (p=.000) suggests that areas with a higher percentage of residents over 65 were more likely to be issued orders. Similarly, the coefficient on Population Density, .03 (p=.000) suggests that more densely populated areas were more likely to be issued orders. Meanwhile, the coefficients on BIPOC (-.29, p=.001) and Low-Income (-.33, p=.000) showed that areas with higher percentage of those populations were less likely to be issued an order to evacuate. Results suggest that there was, in fact, bias in *where* orders were issued. Additional investigation shows that there was also systematic bias in *when* orders were issued. Using a Weibull duration model, I estimated the amount of time these groups waited before receiving an evacuation order (if at all).

Using the time variable derived from the Twitter timestamps, I calculated the duration variable that measured the period the order was issued minus the extent of the periods. Zones that did not receive an order are coded with a period outside of range of the extent. For example, the zones with orders have been assigned their chronological order 1-5, the extent of the model is set to 6, and those zones that did not receive an order would be coded as 10, designated as a 'censor' to identify zones that never received orders as a failure.

Figure D.1 shows duration model results plotted for the Over 65 population with the percentage of the block group over 65 on the x-axis and the median time before an order was issued on the y-axis. This plot suggests that zones with higher percentages of elderly residents were issued orders sooner (if at all). Figures D.1-D.4 show tests for *% BIPOC*, *% low-income*, and *Population Density*. Figure D.2 suggests that higher density areas were also issued orders sooner while Figure D.3 and D.4 show that areas with a higher percentage of BIPOC residents or higher percentage of lower-income areas waited longer to receive order.

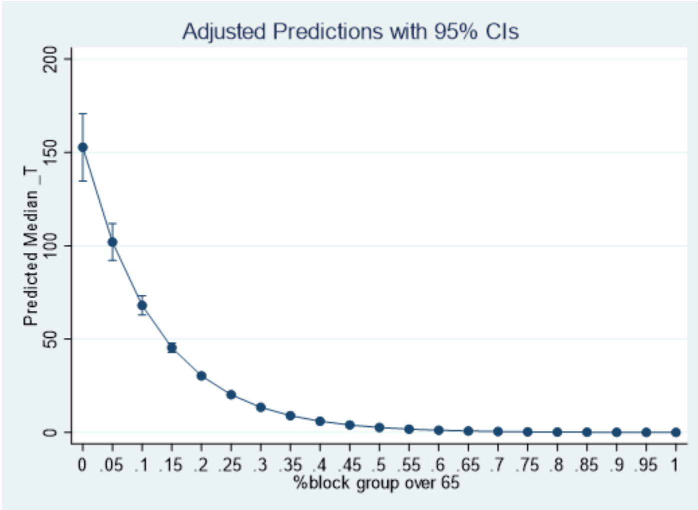

Figure D.1 Weibull Duration for Over 65

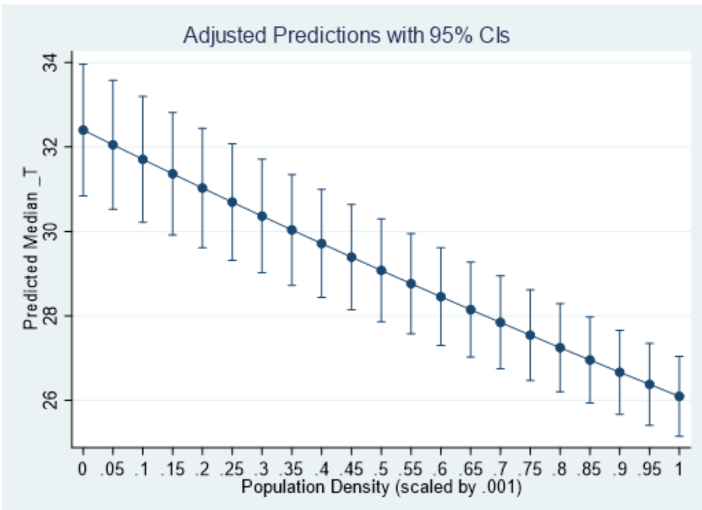

Figure D.2 Weibull Duration for Population Densit

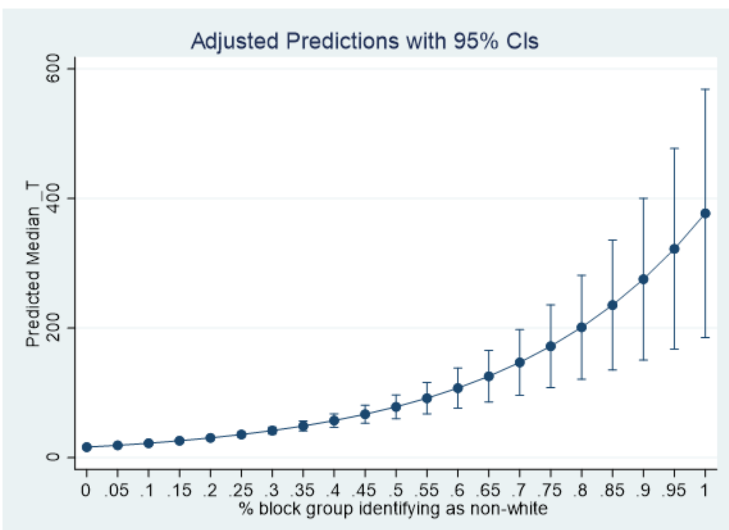

Figure D.3 Weibulul Duration for BIPOC

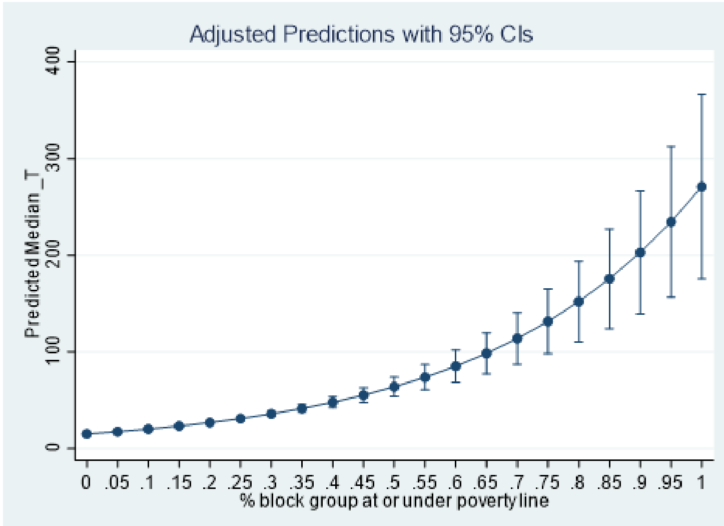

Figure D.4 Weibull Duration for Poverty Line

To test the duration models more explicitly, Equation D3 is presented in Table D.2. In this iteration, P_{ijk} is the Period or time that orders were issued, M_{ijk} substitutes in demographic controls (*% 65+*, *% BIPOC*, *% Low-income*, and *Population Density*), A_i controls for damages, and λ_s includes fixed effects on structure type (s).

$$P_{ij} = \alpha + \beta_n M_{ijk} + A_i + \lambda_s + \varepsilon_i \quad \text{(D3)}$$

Model 1 confirms what Figures A.11-A.14 show graphically, that areas with more residents over the age of 65 and more densely populated areas were more likely to be issued orders sooner where the coefficients were 9.45 (p=.000) and .79 (p=.000) respectively. And areas with more BIPOC and lower-income residents were less likely to receive orders sooner (if at all) with coefficients reported at 9.08 (p=.000) and 12.53 (p=.000).

Table D.2 Testing Duration Models with Outcome Variables (Death and Orders)

VARIABLES	Model (1)	Model (2)	Model (3)	Model (4)
	Outcome: Deaths		Outcome: Order	
% 65+	2.26**	2.25**	1.03***	1.03***
	(0.90)	(0.90)	(0.07)	(0.07)
% Low Income	0.66	0.64	-0.33***	-0.33***
	(0.75)	(0.75)	(0.06)	(0.06)
% BIPOC	1.39	1.36	-0.29***	-0.29***
	(1.05)	(1.05)	(0.08)	(0.08)
Population Density	0.02	0.02	0.03***	0.03***
	(0.04)	(0.04)	(0.00)	(0.00)
Damage		0.30		-0.01
		(0.28)		(0.02)
Constant	-0.44	-0.72*	0.35***	0.36***
	(0.30)	(0.40)	(0.02)	(0.03)
Fixed Effects:	Time & Structure Type		Structure Type	
Observations	13,660	13,660	13,660	13,660
R-squared	0.00	0.00	0.07	0.07

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix E To Mask or Not To Mask – Additional Tables & Figures

Table E.1 Demographics Balanced Across Treatments

VARIABLES	(1) Female [=1]	(2) Conservative [=1]	(3) Over 65 [=1]	(4) Under 35 [=1]	(5) Lives in urban area [=1]	(6) Lives in state with above median case count [=1]
State Stats	0.02	0.01	0.01	-0.01	0.04	0.02
	(0.04)	(0.03)	(0.01)	(0.04)	(0.04)	(0.05)
Anti-mask	-0.04	0.06	-0.01	-0.01	0.07	-0.00
	(0.07)	(0.05)	(0.02)	(0.07)	(0.07)	(0.07)
Pro-mask	-0.07	0.01	0.02	-0.06	-0.06	-0.06
	(0.07)	(0.05)	(0.02)	(0.07)	(0.06)	(0.07)
Pess. Forecast	-0.15**	0.09*	-0.01	-0.05	-0.06	-0.02
	(0.07)	(0.05)	(0.02)	(0.07)	(0.06)	(0.07)
Optim. Forecast	0.10	0.05	-0.02	-0.11	-0.03	-0.04
	(0.07)	(0.05)	(0.01)	(0.07)	(0.07)	(0.07)
Observations	493	493	493	493	493	493
R-squared	0.03	0.01	0.02	0.01	0.01	0.00

Table E.2 Main Regression Specification: Taking a measure of self-reported perception of importance of wearing masks (measured after treatments) as outcome

	Model (1) Full Sample	Model (2) No StateStats.	Model (3) StateStats.
StateStats. (v. No StateStats.)	0.030		
	(0.044)		
Anti-Mask (v. No Msg.)	-0.019	-0.086	0.028
	(0.068)	(0.097)	(0.101)
Pro-Mask (v. No Msg.)	0.232***	0.165	0.304***
	(0.070)	(0.105)	(0.099)
Opt. Forecast (v. No Msg.)	0.061	0.014	0.113
	(0.068)	(0.098)	(0.101)
Pess. Forecast (v. No Msg.)	0.007	0.003	0.024
	(0.073)	(0.104)	(0.109)
Post-Estimation Comparisons			
Pro-Mask (v. Anti-Mask)	0.251***	0.251**	0.276***
	(0.069)	(0.104)	(0.099)

Pess. F'cast (v. Opt. F'cast)	-0.053	-0.012	-0.089
	(0.072)	(0.102)	(0.109)
Observations	493	249	244
R-squared	0.096	0.116	0.118

All specifications include controls for participant gender, an indicator for self-identifying as Republican or Libertarian, indicator variables for being over 65 or under 35, an indicator variable for living in an urban area, and state fixed effects. The outcome variable is equal to 1 if the participant identifies the importance of wearing a face mask when in public as "Extremely Important", 0 if otherwise identified.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.3 Main Specification - taking continuous share of tickets allocated to mask as outcome

	Model (1) Full Sample	Model (2) No StateStats.	Model (3) State Stats.
StateStats. (v. No StateStats.)	0.066*		
	(0.036)		
Anti-Mask (v. No Msg.)	-0.009	-0.027	0.002
	(0.056)	(0.077)	(0.085)
Pro-Mask (v. No Msg.)	0.029	0.056	-0.008
	(0.057)	(0.083)	(0.083)
Opt. Forecast (v. No Msg.)	-0.011	0.059	-0.066
	(0.056)	(0.078)	(0.085)
Pess. Forecast (v. No Msg.)	-0.024	0.014	-0.021
	(0.060)	(0.083)	(0.092)
Post-Estimation Comparisons			
Pro-Mask (v. Anti-Mask)	0.038	0.083	-0.010
	(0.057)	(0.082)	(0.083)
Pess. F'cast (v. Opt. F'cast)	-0.013	-0.044	0.046
	(0.059)	(0.081)	(0.092)
Observations	493	249	244
R-squared	0.069	0.079	0.106

All specifications include controls for participant gender, an indicator for self-identifying as Republican or Libertarian, indicator variables for being over 65 or under 35, and an indicator variable for living in an urban area. The outcome variable is continuous between 0 and 1 representing the proportion of tickets (out of 100) that were allocated to the mask.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.4 Full Regression Results for Main Table 3.3

	(1) State is bad (=1)	(2) State is bad (=1)	(3) State is bad (=1)	(4) Risk to self [0-1]	(5) Mask extr. imp. (=1)	(6) Alloc. tix for mask (=1)
StateStats.	0.07**	0.05	0.07**	0.52	0.10	0.12*
	(0.03)	(0.03)	(0.03)	(0.32)	(0.06)	(0.06)
StateStats. x high death		0.04		-0.47	-0.13	-0.16*
		(0.06)		(0.44)	(0.09)	(0.09)
StateStats. x very high death			-0.01			
			(0.08)			
Female	0.01	0.01	0.01	0.26	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.23)	(0.04)	(0.05)
Conservatives	0.02	0.02	0.02	-0.91***	-0.13**	-0.10*
	(0.04)	(0.04)	(0.04)	(0.31)	(0.06)	(0.06)
Over 65	0.16	0.16	0.16	1.22	0.27	0.26*
	(0.14)	(0.14)	(0.14)	(1.15)	(0.17)	(0.14)
Under 35	0.11***	0.11***	0.11***	-1.07***	-0.04	-0.03
	(0.03)	(0.03)	(0.03)	(0.24)	(0.05)	(0.05)
Urban	-0.01	-0.01	-0.01	0.25	0.09*	0.02
	(0.04)	(0.04)	(0.04)	(0.24)	(0.05)	(0.05)
Anti-mask	-0.03	-0.03	-0.03	0.31	-0.02	-0.09
	(0.05)	(0.05)	(0.05)	(0.36)	(0.07)	(0.07)
Pro-mask	-0.00	-0.00	-0.00	-0.11	0.23***	0.03
	(0.05)	(0.05)	(0.05)	(0.37)	(0.07)	(0.07)
Optimistic Forecast	-0.00	-0.00	-0.00	-0.08	0.06	-0.02
	(0.05)	(0.05)	(0.05)	(0.33)	(0.07)	(0.07)
Pessimistic Forecast	0.08	0.07	0.08	0.19	0.02	0.01
	(0.05)	(0.05)	(0.05)	(0.37)	(0.07)	(0.07)
Observations	493	493	493	493	493	493
R-squared	0.42	0.42	0.42	0.13	0.10	0.05

Outcomes: Column 1-3: Respondent agrees that her state has one of ten highest death counts as of survey date (1=yes, 0=no). Column 4: Rating of risk to self [scaled from 1-10]. Column 5: Agrees that masks are extremely important (1=yes, 0=no). Column 6: allocate any tickets to the mask (1=yes, 0=no). All specifications include indicator variables for gender, self-identification as Republican or Libertarian, being over 65 or under 35, and living in an urban area, as well as state fixed effects.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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