The Politics of Personal Crisis:
How Life Disruptions Shape Political Participation

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Abstract

Economic risk and instability are urgent and central facts in the lives of increasing numbers of Americans. Though experienced as "personal," the causes of life disruptions like unemployment, eviction, and loss of health insurance are also deeply political. In this paper, we build on existing "single crisis" studies to offer a comprehensive theoretical and empirical picture of how life disruptions shape political behavior. We use several large surveys to show that personal crises generally dampen turnout but sometimes spur other political acts. We also find that highly politicized crises (such as those related to COVID) boost all forms of participation. Our findings speak to the importance of considering life disruptions in the study of political behavior, particularly in an era when the lives of Americans are especially precarious.

Word count: 8,849

"The personal is political" was a popular mantra that emerged in the 1960s as a rallying cry of women's movements. This saying underscored that the challenges, dilemmas, and disruptions of life were not simply private affairs but were rooted in political choices and processes. During the 1960s and 70s, connecting the personal to the political was a strategy for spurring collective action (Combahee River Collective 1979; Nelson 2003). Since then, the popular saliency of "the personal" has ebbed and flowed but the basic logic holds: personal life can be profoundly political. Personal crises like unemployment, eviction, and financial distress are products of a changing political economy that has multiplied risk for those with fewer resources, deepened economic inequality, and left more Americans vulnerable to destabilizing financial loss. In this paper, we assess the political consequences of such personal crises.

Economic risk and instability are staples of American life (Hacker 2006; Western et al. 2012). Particularly since the 1970s, national crises around employment, housing, and healthcare have consistently generated interconnected and cumulative negative life events: job loss, disrupted health insurance, home foreclosure, and more. COVID-19 brought these realities into sharp relief. As the U.S. economy faltered under the weight of the pandemic, tens of millions of Americans faced resultant personal crises that were intensified by surging prices for gas, food, and rent as inflation hit a four-decade high.

We investigate the politics of personal crisis in this paper by exploring how disruptive life events affect political participation. Public opinion research demonstrates the importance of economic experiences for policy attitudes (Hacker, Rehm and Schlesinger 2013), while scholarship on participation typically focuses on the effects of economic crises like recessions (Bartels 2013) or specific life events like foreclosure (Hall et al. 2021; Shah and Wichowsky 2019), marriage (Stoker and Jennings 1995), childbirth (Kam, Kirshbaum and Chojnacki 2023),

or unemployment (Burden and Wichowsky 2014). We extend this research by examining a broader scope of life events involving work, finances, housing, health, and more. Despite their heterogeneity, such events are the product of larger political processes that structure the distribution of economic risk; they therefore have implications for democratic participation (Burns, Schlozman and Verba 2001). We contextualize our research in relation to the American economy, and we put our work in conversation with several theories of political participation to cull insights on how life disruptions can hinder or spur engagement.

We analyze data from the 2018 and 2020 Cooperative Election Study and the Democracy Fund Views of the Electorate Research survey to demonstrate that personal crises dampen voter turnout. Our analyses indicate that personal crises have heterogenous effects on political activity. As crises accumulate, voting declines for many citizens while participation in other political acts increase for a few citizens. These findings are consistent across elections before and during the COVID-19 pandemic for crises that are not related to the pandemic. Pandemic-related crises (e.g., contracting COVID-19 or losing one's job because of the pandemic) appear to spur all forms of participation, perhaps because of the political salience of the disease. This research underscores the case for more comprehensive attention to the participatory implications of personal crises engendered by a precarious political economy.

Charting the U.S. Crisis Economy

The COVID-19 pandemic disrupted the health and prosperity of the American people more than any event in living memory. Yet, instability and crisis are nothing new in American life. Recent breakdowns in the housing, labor, and healthcare markets widely imperil economic security. As Jacob Hacker wrote in the *New York Times* before the pandemic, "…many

Americans feel insecure. They may be doing well at the moment, but they fear that, however high they are on the economic ladder, a single step or bad event could cause them to slip" (Hacker 2019). We contextualize our study of how "a single step or bad event" dampens political participation by first discussing three pivotal contemporary crises.

The housing crisis of the 2000s was catalyzed by diminished affordable housing and skyrocketing foreclosures. The share of high debt/low equity loans rose sharply in the 1990s. When home prices began to drop in 2006, these high leverage loans defaulted at much higher rates, driving foreclosures (Corbae and Quintin 2015). The rental housing market was similarly dire. Between 2000 and 2010, inflation-adjusted median asking rent increased between 21% and 37% while median incomes rose by only a fraction as much (Desmond 2018). The combination of high leverage loans and increasing housing costs spurred the Great Recession, heightening economic instability for many Americans. Today, affordable housing continues to plague many Americans. As *Pew Research Center* noted in January 2022, "Prospective homebuyers and renters across the United States have seen prices surge and supply plummet during the coronavirus pandemic" (Schaeffer 2022).

Though rooted in the housing crisis, the Great Recession had severe labor market repercussions. The unemployment rate jumped from 5 percent to 9.5 percent by the end of the recession, making the employment decline associated with the Great Recession greater than that of any recession since 1980 (Bureau of Labor Statistics 2012). The sharp drop in employment across the country was due in part to a spike in the number of mass layoffs, which occur when at least 50 initial claims for unemployment insurance are filed against an establishment during a consecutive 5-week period. (Bureau of Labor Statistics 2012). Mass layoffs peaked in February

2009 when “employers took 3,059 mass layoff actions” affecting 326,392 workers (Bureau of Labor Statistics 2012). The specter of labor market instability extended beyond the Great Recession—even as standard unemployment returned to pre-recession levels, long-term unemployment and involuntary part-time employment remained high (Bureau of Labor Statistics 2018).

Most recently, the Covid-19 pandemic called attention to how easily the labor market could falter. In April 2020 the unemployment rate spiked to 14.8 percent, the highest rate observed since the U.S. began collecting official unemployment data in 1948 (Congressional Research Service 2021). For well over a year after that peak, the unemployment rate remained higher than pre-pandemic levels. Black and Latina women, younger workers, and people in employed in the service sector experienced especially high levels of job loss (Brower and Michener 2021; Congressional Research Service 2021).

Even as the labor market rebounds from the damage of the pandemic, those who find work in the modern economy do not benefit from the same stability and security that many of their parents did. Jobs that pay well and offer key non-wage benefits like health insurance and retirement coverage have grown increasingly scarce over the past several decades, even as the average American worker has become both older and more skilled (Schmitt and Jones 2012). American employment is increasingly “polarized,” with middle-wage jobs losing ground to low-wage jobs, and much of the growth in the latter taking place in service rather than manufacturing industries (Dwyer and Wright 2019). Meanwhile, the growth of non-standard employment and the so-called “gig economy” creates jobs with precarious work schedules, financial insecurity, and distrust among workers (Lambert, Henley, and Kim 2019). In short, work in the modern economy no longer guarantees financial security.

Making for a trifecta of crises, long standing predicaments around affordable healthcare came to a head just as the American economy faced breakdowns in the housing and employment sectors. Prior to the Affordable Care Act (ACA), the uninsured rate was as high as 18%, many Americans delayed medical care due to costs (Kenney et al. 2012), and medical debt ballooned (Doty et al. 2008). Like the housing and employment crises, problems with healthcare access and affordability created precarity. The ACA reduced the uninsured rate and improved access to care (Glied, Ma and Borja 2017), but these gains eroded during the Trump presidency (Galewitz 2019) as insurance premiums and deductibles grew more quickly than wages (Claxton et al 2018) and medical bankruptcy remained a serious threat (Himmelstein et al 2019).

The Political Impact of Personal Crises

These national crises highlight how the political economy spurs personal crises. But do personal crises in turn shape the way citizens engage with politics? We argue that personal crises are generally demobilizing but may spur political participation in some circumstances.

How Personal Crises Can Be Demobilizing

Numerous theories and empirical literatures point to economic and psychosocial explanations for why personal crises might be demobilizing. The data and analyses in this paper do not allow us to identify one or the other of these mechanisms as the driver of our results, but we nevertheless outline a range of existing theories that are consistent with the patterns we uncover between personal crises and political participation.

A robust literature in behavioral economics demonstrates the wide-ranging consequences of scarcity, and this logic can be applied to the resource model of political participation to understand the impact of personal crises. The resource model holds that participation requires

resources, such as time, civic skills, and money, and so "the presence or absence of resources contributes substantially to individual differences in political participation" (Verba et al. 1995, 274). Since the resources for participation are finite and in demand, individuals must evaluate the opportunity costs of how and when they should be deployed. Finiteness is the limit or bound to the supply of a resource. There are only 24 hours in a day and only so many dollars in a bank account. Once these hours or dollars have been spent on one activity they cannot be used again for another activity. Civic skills are less finite because they are not lost once used. The ability to write a letter to a representative can be deployed repeatedly without depleting its supply. Nevertheless, the deployment of civic skills depends on mental bandwidth—an individual's attentional and cognitive functioning (Mani et al 2013; Mullainathan and Shafir 2013)—which is finite. Few could routinely write several letters a day even if they found the time and supplies to do so. Importantly, the resources for political participation are also required of other activities. Money is required to make a political donation, but it is also required to purchase food and clothes, pay for shelter and transportation, and participate in leisure activities. Time and mental bandwidth are likewise in demand. Feeding oneself requires both time and attention in addition to money—planning meals, making a grocery list, purchasing goods, and then preparing the food for consumption are not costless activities.

That resources are finite and in demand means they are scarce, and this scarcity forces people to make decisions about how to allocate their limited supply of resources. While decisions over how to use resources will be somewhat idiosyncratic, individuals will generally give basic needs primacy over higher-order needs (Maslow 1943). Basic needs are shelter, food, clothing, utilities, and sanitation, intermediate needs are personal security, employment, health, property, family, and friendship, and higher order needs are status, leisure, and *political*

participation. Consider an individual who faces a home foreclosure as an illustrative example. The process of foreclosure will increase the demand for resources—money, time, and mental bandwidth to relocate; mental bandwidth and time to remain organized in the process of moving; time and energy to deal with emotional turmoil; and so forth. Resources expended for these purposes are no longer available for political participation. Simply put, the opportunity costs of participation are greater when basic needs lead to a heightened demand for resources, such as the demands created by personal crises.

There are also *psychosocial* reasons to think personal crises may be demobilizing. An abundance of research finds that the experience of stress, whether daily hassles or major traumas, can be deleterious to cognitive functioning, mental well-being, and physical health, especially as it intensifies, accumulates, and persists (Hammen 2005; Schneiderman et al. 2005; Sandi 2013). One review of the literature on stress and cognition concludes, "the general view that emerges is that exposure to high-to-very-high stress acutely (whether elicited by the cognitive task or experienced before being trained or tested in the task) or chronically impairs performance on explicit memory tasks that require complex, flexible reasoning while improving performance on implicit memory tasks, in simple declarative memories and in well-rehearsed tasks" (Sandi 2013, p. 255). This is important because these factors facilitate political participation (Denny and Doyle 2008; Landwehr and Ojeda 2021; Pacheco and Fletcher 2015), potentially because cognitive functioning is a resource for participation. To the extent these resources are diminished due to personal crises, participation would be expected to decline.

Personal crises, and their impact on cognition, mental well-being, and physical health, may also impair the motivation to participate. Depression is one indicator of poor mental well-being and has been found to reduce political interest and internal political efficacy, factors that

motivate political participation (Landwehr and Ojeda 2021; Ojeda and Slaughter 2019). Similarly, poor physical health has been linked to lower political trust (Mattila 2020), while reduced cognitive functioning has been found to diminish political trust (Hooghe et al. 2012) and political interest (Denny and Doyle 2008). In short, there are good reasons to think that personal crises may be demobilizing because they reduce our motivation and ability to participate.

When Personal Crises are Mobilizing

Just as there are economic and psychological reasons to think personal crises are demobilizing, there are economic and psychological reasons to think they may sometimes *mobilize* participation. Regarding the *economics* of personal crises, it is possible that extreme crises spur participation insofar as people see politics as the only route to solving their problem and ensuring their basic needs are met. However, such perceptions are likely only in cases where there is a direct relationship between the participatory act and the crisis. For instance, the threat of foreclosure may prompt a protest outside of the bank initiating the foreclosure or attendance at a local meeting about foreclosures in the neighborhood. It may also alter attitudes towards government regulation of banks or government programs designed to help homeowners. Absent such direction connections, the reality of finite resources in the context of a hierarchy of needs suggests crises will likely decrease participation. This is indeed borne out in empirical research (Estrada-Correa and Johnson 2012; Hall et al., 2021; Shah and Wichowsky 2019).

This dynamic is particularly germane to assessing how personal crises might affect different forms of political participation differently. While voting is a central component of democracy, it is also a generalized form of participation that does not ordinarily pertain to specific material needs, and so can be viewed as a higher-order activity. In contrast, other forms of participation (e.g., contacting a local official, attending a meeting, protesting) can sometimes

relate directly to basic needs and can thus provide a greater possibility of relief. We therefore expect crises to be demobilizing with respect to voting. We are less certain about how they might affect other participatory acts.

It is also possible that some personal crises place a demand on one resource while simultaneously increasing the availability of another resource. Unemployment, for instance, reduces money but increases free time (Aguiar et al. 2011). These "mixed" demands might be expected to have a diminished impact on political participation to the extent that the resources for participation are fungible (e.g., free time can be substituted for money) (Burden and Wichowsky 204; Cebula 2017).

From a psychosocial perspective, there are also good reasons to think personal crises will sometimes be mobilizing. In addition to diminishing cognitive bandwidth, mental well-being, and physical health, personal crises can evoke negative emotions like anger and anxiety (Spielberger et al. 1984). It is well-documented that anger, and to a lesser extent anxiety, has a mobilizing effect (Phoenix 2019; Valentino et al. 2011), creating the possibility that personal crises spur participation some of the time. However, for emotions to be systematically rather than merely incidentally mobilizing, they must be directed at government and politics. This seems possible in circumstances where personal crises are clearly connected to salient issues for which government is viewed as bearing some causal and/or treatment responsibility (Arcenaux 2003).

To summarize, based on existing economic and psychosocial explanations of political behavior, we argue that personal crises are likely to dampen participation by squeezing scarce resources and sapping motivation, *except* when the political act offers the potential for relief or when there is high political saliency, clarity of responsibility or perceived responsibility. This picture of disruptive events as politically demobilizing is in tension with notions of democratic

accountability—the idea that adverse societal outcomes mobilize citizens to punish incumbent politicians (Healy and Malhotra 2013). Our argument is not that catalyzing political action amid crisis is impossible, but rather that political action generally becomes more difficult when resources and motivation are depleted. Again, this may explain why research finds that experiencing foreclosure during the Great Recession dampened turnout (Hall et al. 2021)—although the salience of the Great Recession was high, perceptions about who was responsible were muddled (Parker-Stephen 2013) and motivated reasoning prevailed in the minds of voters (Bisgaard 2015).

In any case, we need not look far for evidence that crises dampen turnout. Studies find that a wide range of crises reduce turnout, including teen pregnancy and dropping out of school (Pacheco and Plutzer 2015), poor health (Burden et al. 2017; Ojeda and Pacheco 2017), the death of a spouse (Hobbs, Christakis, and Fowler 2014), divorce (Dehdari et al 2022), the loss of Medicaid (Haselswerdt and Michener 2019), unemployment (Burden and Wichowsky 2014), foreclosure (Hall et al 2021; Shah and Wichowsky 2019), acute financial hardship (Schaub 2021), loss of income (Schafer et al 2022), and the death of a loved one from opioid overdose (Kaufman and Hersh 2020). The analyses offered below build on these studies but take a broader approach by considering patterns across a wide range of crises, varying contexts of crises (before and during the COVID-19 pandemic), and different forms of political participation.

Methodology

We test our argument using data from the Cooperative Election Study (CES; formerly the Cooperative Congressional Election Study) Common Content and Modules in 2018 and 2020 as well as the Democracy Fund Views of the Electorate Research (VOTER) Survey. The CES is an

annual survey administered by YouGov and fielded to a nationally representative sample of 60,000+ respondents, with the opportunity for teams of researchers to field original questions to 1,000-respondent modules. We draw on questions from the 2018 and 2020 CES Common Content—supplemented with a battery of original questions on team modules in both years—that measure personal crises, such as loss of health insurance, loss of home, eviction, loss of eligibility for a government program, loss of eligibility for a tax break, trouble affording medical expenses, substantial increases in housing costs, and suspension or revocation of driver's license.

The VOTER Survey is an ongoing panel study of American adults administered by the Democracy Fund's Voter Study Group in cooperation with YouGov. The May 2018 wave of the survey include a series of questions on financial difficulties encountered in the twelve months preceding the survey, including the loss of job (self or spouse), problems paying mortgage or rent, problems paying a student loan, problems paying a car loan, problems paying off a credit card, and drop in household income. Table 1 summarizes the key features of each study. Appendix A elaborates on the data collection process, while Appendix B describes the measurement of all variables used in the analysis.

The primary dependent variable in all studies is *validated* voter turnout. Information about the voter-file matching procedure is reported in Appendix B. We also estimate models using self-reported non-voting forms of participation for the CES studies, including attending a political meeting, putting up a political sign, working for a candidate or campaign, attending a protest, contacting a public official, and donating money.

Table 1: Key Features of the Datasets

	CES 2018 Module	**CES 2018 Common**	**CES 2020 Module**	**CES 2020 Common**	**VOTER Survey**
Data	Pre/Post-	Pre/Post-	Pre/Post-	Pre/Post-	Panel

Type	Election Survey	Election Survey	Election Survey	Election Survey	survey
Time of Study	10/2018; 12/2018	10/2018; 12/2018	10/2020; 12/2020	10/2020; 12/2020	12/2016; 5/2018; 1/2019
Elections Years	2018	2018	2020	2020	2018
Total Crises	13	5	45	15	7
COVID Crises	-	-	9	9	-
Sample Size	1,000	60,000	1,000	61,000	3,691

We draw on stress research to inform the measurement of our independent variable, which we refer to as personal crises. We discuss three issues here: operationalization (i.e., how to define personal crises), measurement (e.g., how to generate data on personal crises), and aggregation (e.g., how to create a single variable out of data on many personal crises). Our focus here is explaining how we arrived at our independent variable; in the discussion, we describe other measurement approaches that could be used in future research.

The first issue is how to *operationalize* personal crises. Our focus is on "objective experiences that disrupt or threaten to disrupt an individual's usual activities" (Dohrenwend and Dohrenwend 1969, p. 133) and "cause a substantial readjustment in that person's behavior" (Thoits 1982, p. 342), such as divorce or losing a job.[1] Stress research consistently finds that the experience of personal crises leads to psychological distress and physical illness (Sheldon et al. 2019). Meanwhile, as we noted earlier, studies in political science have examined how particular personal crises, including traumas, impair participation (e.g., Marsh 2022; Hobbs et al. 2014).

[1] Scholars have long recognized different types of stress. Negative life events, or what we call personal crises, are dramatic experiences that can reshape a person's life. Traumas are a severe type of personal crisis that involve harm (or the threat of harm) to a person's physical security. Daily hassles are minor irritants that do not require serious life adjustments, such as traffic.

This makes personal crises a good starting point for thinking about how the cumulative stress burden affects political participation.

The development of personal crises checklists, such as the Social Readjustment Rating Scale (Holmes and Rahe 1967) or the updated and modified Stressful Life Events Scale (Hobson et al. 1998), have assisted scholars in identifying relevant, discrete, independent, and unbiased events across domains such as finances, family, health, and mortality. We included an abbreviated set of 13 personal crises in the 2018 CES Module and then expanded to 45 personal crises in the 2020 CES Module, which is nearly the full complement from the Stressful Life Events Scale. The CES Common Content and VOTER Survey, which we did not design, include a more limited sets of items, with the latter focused exclusively on financial difficulties.

The second issue is how to *measure* personal crises. The most common approach, and the one we use here, is to focus on incidence. Incidence refers to whether a person experienced a particular crisis, and it is shown to be predictive of negative outcomes (Monroe 2008). We measure incidence by asking respondents whether they experienced each crisis in the past year. Recognizing that not all crises are the same, scholars sometimes measure perceptions of frequency, length, and severity of crises (e.g., Friborg 2019). We generally lack the necessary data to categorize crises on these dimensions without making unsupported assumptions, but we address how future research can fruitfully tap into other dimensions of personal crises in the discussion. Importantly, a measurement strategy focused only on incidence likely represents the *lower bound* for the impact of crises on political participation. We might expect a stronger effect were we to account for other aspects of crises.

The final consideration is *aggregation*. We generate a single score for each respondent by counting the incidence of personal crises. In our case, a count is more appropriate than a factor

analysis (or a similar data reduction technique) because personal crises are the *cause* rather than the *effect* of stress. Factor analysis is useful when we expect indicators to be highly correlated because they are caused by the same latent variable; however, it can introduce bias when indicators (such as ours) are not strongly correlated nor expected to be (Bollen and Ting 2000). A count approximates the total stress burden over the past year, giving equal weight to each crisis. In the discussion, we consider how future research can explore alternative ways of aggregating crises, such as weighting by the severity or length of the crisis or its proximity to election day.

Altogether, the main independent variable is the total crises experienced by each respondent in the past year. However, we undertake a preliminary exploration of two different types of crises. We create separate scales in the CES 2020 for non-COVID and COVID crises to capture the unique politics surrounding the pandemic (Gadarian et al. 2022). We also present a supplementary analysis in the appendix that distinguishes between crises that more likely represent ongoing problems in a person's life from those that more likely represent significant changes to the status quo.

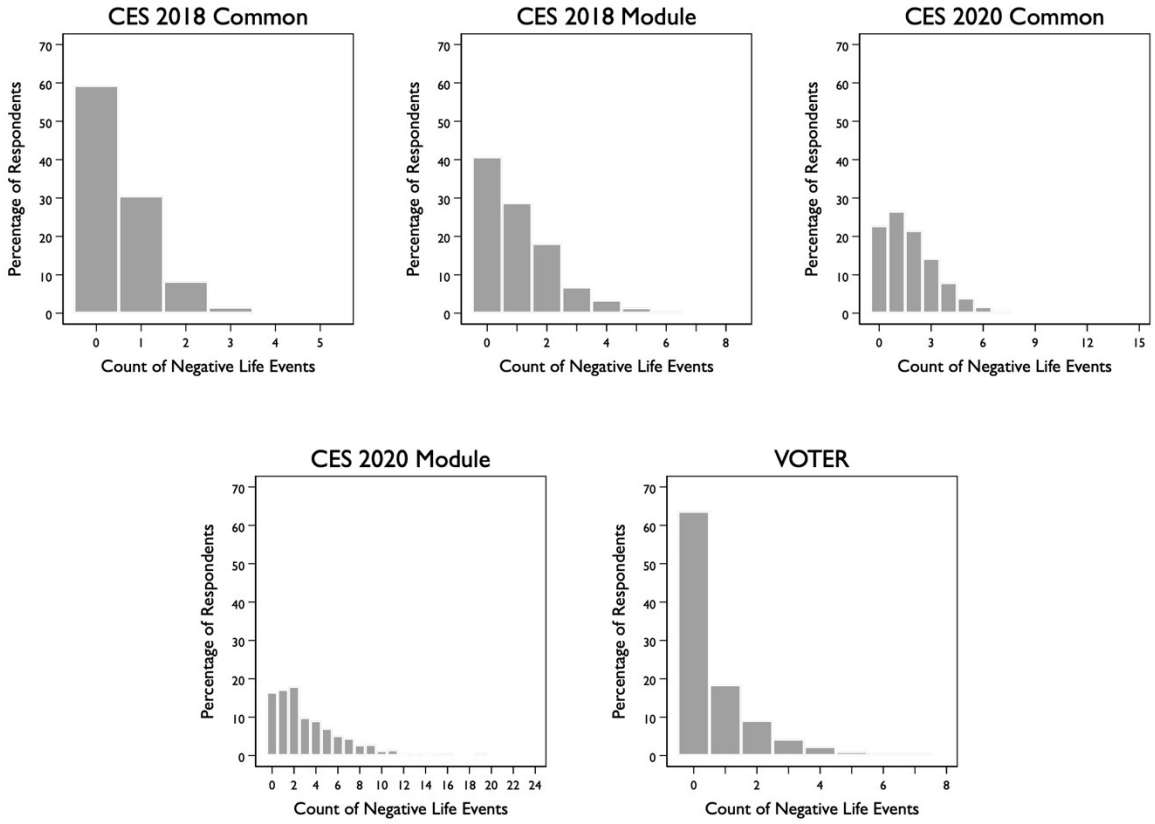
At least one personal crisis is reported by 42% of respondents in the 2018 CES Common Content, 60% in the 2018 CES Module, and 34% in the VOTER study. For the 2020 CES, at least one non-COVID crisis is reported by 48% of respondents in the Common Content and 69% in the Module, while at least one COVID crisis is reported by about 61% of respondents in the Common Content and 59% in the Module. The differences between surveys are largely a function of the total number of crises asked about on the survey—the more crises that are included, the smaller the percentage of respondents reporting no personal crises. As Figure 1

shows, the percentage of respondents reporting a given number of personal crises declines as the overall number of crises surveyed increases.

We estimate logistic regressions of validated voter turnout and other forms of self-reported participation on the count of personal crises. The CES 2020 models include separate variables for non-COVID and COVID crises. A negative and statistically significant coefficient would provide evidence that crises inhibit participation. All models employ survey weights and control for validated turnout in the 2016 election, age, education, income, gender, race, marital status, religious attendance, and the presence of children under 18 in the home.[2] The control variables in the CES are measured concurrently with the key dependent and independent variables, aside from validated 2016 turnout, while all control variables in the VOTER Survey are measured in 2016. Appendix B displays question wording and other measurement details, Appendix C reports descriptive statistics, and Appendix D reports the incidence of each crisis and distributions of the totals.

[2] We do not use a difference-in-difference approach because personal crises are only measured at one point in time. We therefore know prior levels of participation but not prior levels of personal crises, which precludes us from looking at how changes in crises affect changes in participation. Instead, we include prior voter turnout as a control variable in the model.

Figure 1: Distribution of Personal Crises

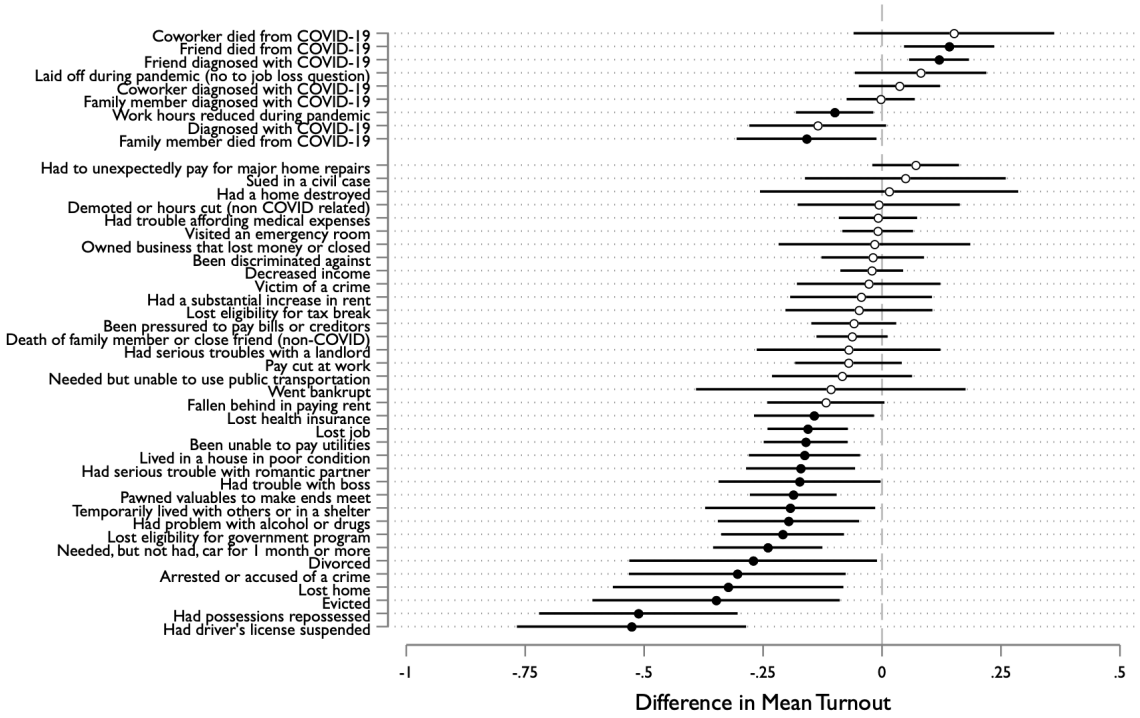


Results

We begin by examining the bivariate association between each personal crisis and turnout. Figure 2 displays the difference in mean turnout between respondents who did and did not experience each crisis in the CES 2020 Module, which included the most extensive battery of personal crises. The results show a consistently negative turnout gap for non-COVID crises: 33 of the 36 crises (about 92%) create a negative gap, while 19 of the 36 crises (about 53%) create a

negative *and* statistically significant gap. No non-COVID crises exhibit a positive and statistically significant turnout gap. A different pattern emerges for COVID crises: only 4 of the 9 crises (about 44%) are negative, while only 2 of the 9 crises (about 22%) are negative and statistically significant. Unlike with non-COVID crises, 2 of the COVID crises create a positive and statistically significant gap.

Figure 2. Association of Specific Personal Crises with the Probability of Voting



Note: Estimates represent the difference in mean between respondents who reported the personal crisis and respondents who did not report the personal crisis in the CES 2020 Module. The 95% confidence intervals are based on a two-tail t-test of the difference in means.

Table 2 summarizes a series of t-tests, like the ones reported in Figure 2, for all five studies. The results show that almost all non-COVID crises are negatively associated with turnout, and most of these associations are statistically significant. The differences in turnout for the five crises with a positive association are not statistically significant. These results are consistent with previous "single crisis" studies, but also point to a systematic pattern in which

personal crises are broadly consequential to voting. The COVID crises exhibit a mixed pattern, with fewer than half of the nine measured events showing a negative association with voting in both the 2020 CES Common Content and Module. Furthermore, some of the positively associated COVID crises are statistically significant.

Table 2. Bivariate Associations of Specific Personal Crises with Voting Across Studies

	Total Crises	Total Negative	Total Negative & Significant ($p < 0.05$)
CES 2018 Common	5	5 (100%)	5 (100%)
CES 2018 Module	13	11 (85%)	11 (85%)
CES 2020 Common – non-COVID	6	6 (100%)	6 (100%)
CES 2020 Common – COVID	9	4 (44%)	4 (44%)
CES 2020 Module – non-COVID	36	33 (92%)	19 (53%)
CES 2020 Module –COVID	9	4 (44%)	2 (22%)
VOTER Survey	7	7 (100%)	7 (100%)

Note: Item-by-item results are reported in Appendix D.

We now turn to the impact of crisis accumulation on political participation. Table 3 shows turnout levels across weighted tercile of total crises for each study. The results for non-COVID crises once again align with our expectations: those who experienced more crises are less likely to vote. This difference is substantial—as high as 19.5 percentage points in the 2018 CES module. For context, this disparity is roughly four-fifths the magnitude of that between non-Hispanic White and Hispanic respondents in the same survey (23.8 percentage points). The differences shown in Table 3 are statistically significant in all four CES surveys. In the VOTER Survey, the difference (though negative and monotonic) is smaller and not statistically

significant. These findings are consistent with the idea that the totality of disruptions to everyday life can be disruptive to turnout. While correlation is not causation, this pattern is noteworthy from a purely descriptive standpoint: people with unstable lives are systematically underrepresented at the ballot box. At the same time, we observe the opposite relationship for COVID-related crises, with higher turnout among those who experienced the greatest number of pandemic-related events, though the difference is only statistically significant in the 2020 CES Common Content.

Table 3: Voting Turnout Across Terciles of Personal Crises

		Crisis Tercile			
		1	**2**	**3**	*p*
CES 2018 Common	*Mean #*	0	1	2.2	
	Turnout	55.4%	49.2%	38.3%	< 0.001
CES 2018 Module	*Mean #*	0	1	2.7	
	Turnout	62.9%	50.9%	43.4%	< 0.001
CES 2020 Common – non-COVID	*Mean #*	0	1	2.3	
	Turnout	63.8%	56.3%	45.5%	< 0.001
CES 2020 Common - COVID	*Mean #*	0	1	2.6	
	Turnout	56.0%	59.0%	60.0%	< 0.001
CES 2020 Module – non-COVID	*Mean #*	0.4	2	6	
	Turnout	65.3%	68.7%	53.4%	0.008
CES 2020 Module – COVID	*Mean #*	0	1	2.6	
	Turnout	62.5%	56.6%	67.0%	0.122
VOTER Survey	*Mean #*	0	1	2.7	
	Turnout	67.3%	63.8%	62.8%	0.399

Note: All calculations use survey weights. *p*-values are derived from design-based F-statistics (corrected chi-squared statistics) of a test of independence.

We present the results of the multivariate models in Table 4, focusing only on the independent variables of interest; the full results are reported in Appendix E. Since these models control for validated past turnout in addition to demographic variables, they are less vulnerable to selection bias than the previous results. We observe a negative coefficient for non-COVID crises across all models. The coefficient magnitudes are consistent across models—ranging from

-0.091 to -0.109—and statistically significant in three of the five specifications, with the exceptions of the 2018 CES Module (p=0.30) and the VOTER Survey (p=0.12). These latter coefficients are comparable in size but not as precisely estimated. We speculate that the higher standard errors may be due to both smaller sample sizes and smaller batteries of crisis questions in those surveys. Unlike non-COVID crises, the COVID crises variable has positive and statistically significant coefficients in the two CES 2020 models. These findings support the idea that personal crises generally reduce turnout, while highly politicized ones may spur it.

Table 4: Logistic Regressions of Voting on Personal Crises

	CES 2018 Module	**CES 2018 Common**	**CES 2020 Module**	**CES 2020 Common**	**VOTER Survey**
Non-COVID Crises	-0.109	-0.104**	-0.091*	-0.109**	-0.107
	(.104)	(.028)	(.040)	(.021)	(.068)
COVID Crises	-	-	0.200+	0.061**	-
			(.112)	(.016)	
Observations	883	52,940	888	53,830	3,135
Survey Weights	ü	ü	ü	ü	ü
Control Variables	ü	ü	ü	ü	ü

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$
Note: Full results in Appendix E.

Using the models in Table 4, we estimate that a single non-COVID crisis reduces the probability of voting by about 1 percentage point, while a shift from 0 to the 95th percentile in crises reduces the probability of voting by as much as 10 percentage points in the CES 2020 Module and 13 percentage points in the CES 2020 Common Content. For context, these models predict an increase in turnout of 1.2 to 2.3 percentage points for a one-unit increase in the ordinal education measure (e.g., moving from "high school graduate" to "some college"), which is larger in magnitude than the dampening effect of experiencing one additional non-COVID crisis, but not dramatically so. Moreover, since the racial/ethnic and socioeconomic markers of

disadvantage accounted for in these models are also associated with increased incidence of crisis (see Appendix F), the coefficients reported in Table 4 may understate the dampening effect of these experiences. Indeed, such experiences may themselves constitute a partial explanation for racial, ethnic, or socioeconomic disparities in voting. Turning to COVID-related crises, a shift from 0 to 3 crises (the 95th percentile value) is associated with an increase in turnout of 4 percentage points in the Common Content and 12 percentage points in the Module.

We explore the possibility of heterogeneous effects in Appendix G and find that the negative relationship of non-COVID crises with voting is strongest among those who voted in 2016. The interaction of past voting with non-COVID crises is negative in all five models and statistically significant in four, including the VOTER study for which we found null effects in the original model. This suggests that crises may disrupt *habitual* turnout. We find no consistent evidence that effects are conditional on other covariates.

In a separate analysis of the 2020 CES Module, we distinguish between crises that could be ongoing in a respondent's life and those that more likely represent a new significant change. Since respondents were only asked to report on the past twelve months, we have no direct measure of whether a given problem is ongoing or new, and so the coding is based on our best judgment. To illustrate this distinction, "serious trouble with romantic partner" may represent an ongoing issue, while divorce is a specific qualitative change in a person's status. These results, and a more extensive explanation of our coding decisions, are reported in Appendix H. We find that the effect of ongoing problems approaches zero, perhaps because the past turnout control accounts for chronic effects, while recent change drives the significant and negative effect. This pattern may indicate that sudden changes or disruptions are more detrimental to voting.

We now turn our attention to how personal crises shape other forms of participation: attending a local political meeting, putting up a political sign, working for a candidate or campaign, attending a political protest, march, or demonstration, contacting a public official, and donating money to a candidate, campaign, or political organization.[3] The six participation variables scale well ($\alpha > 0.7$ in all four CES datasets), and so we also create an index of participatory acts. We estimate a regression model for each dependent variable—OLS for the index and logit for the dichotomous variables—using total personal crises as the key independent variable.[4] Table 5 displays the results for the personal crises variable in each model.

Table 5: Regressions of Non-Voting Political Acts on Personal Crises

Dependent Variable	CES 2018 Module	CES 2018 Common	CES 2020 Module	CES 2020 Common
Total # of acts	0.05 (0.04) p=0.21	0.08 (0.01)** p=0.00	0.03 (0.01)** p=0.00	0.10 (0.00)** p=0.00
Meeting	-0.01 (0.12) p=0.93	0.14 (0.03)** p=0.00	0.07 (0.03)* p=0.03	0.20 (0.01)** p=0.00
Sign	-0.06 (0.11) p=0.58	0.11 (0.02)** p=0.00	0.07 (0.03)* p=0.01	0.12 (0.01)** p=0.00
Work	0.23 (0.17) p=0.19	0.22 (0.04)** p=0.00	0.05 (0.05) p=0.35	0.21 (0.01)** p=0.00
Protest	0.20 (0.10)* p=0.04	0.22 (0.03)** p=0.00	0.12 (0.03)** p=0.00	0.21 (0.01)** p=0.00
Contact	0.24 (0.09)* p=0.01	0.17 (0.02)** p=0.00	0.09 (0.03)** p=0.00	0.18 (0.01)** p=0.00
Donate	-0.18 (0.11) p=0.10	0.05 (0.02)+ p=0.05	0.02 (0.03) p=0.43	0.13 (0.01)** p=0.00

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Note: The results are coefficients, standard errors, and p-values for the personal crises variable from separate regression models. All models include survey weights and control variables. The VOTER Survey is excluded since it did not ask about non-voting forms of participation.

[3] Participation in these activities is a dichotomous self-report based on the year preceding the interview. Since participation in the post-election survey is highly correlated with political participation, and since dropping respondents who did not take the post-election survey could introduce bias, we code all missing respondents as 0 for each of these variables.

[4] We find both non-COVID and COVID crises have positive relationships with each form of participation, and so we use the total count of all crises as the key independent variable.

The results show mostly *positive* and often statistically significant effects, consistent with the idea that non-voting political activities are more targeted than voting and therefore provide a potential solution to a personal crisis; in this sense, voting is best characterized as a higher-order activity that someone in desperate circumstances cannot readily afford. Other political acts may offer hope of relief, or a channel for expressing anger, and therefore be judged worthy of the resources they demand (Michener 2019).

Importantly, these other forms of participation are much rarer than voting – in 2020, for example, validated voter turnout in the CES was 62%, while self-reported participation ranged from 3% (working for a candidate or campaign) to 23% (donating to a candidate, campaign, or organization). Furthermore, validated voter turnout is very high (sometimes well over 80%) among politically engaged citizens who participate in one or more of these other acts. While the data are limited, turnout appeared to be similarly high among people who experienced one or more crises *and* engaged in one or more non-voting acts. It is important to avoid the ecological fallacy that these findings suggest at first glance: it is unlikely that personal crises make any *one* person less likely to vote *and* more likely to engage in other participation. Rather, we suspect that the depressive effect on voting and the positive effects on other forms of participation play out among different subsets of citizens.

A plausible alternative explanation for these findings, however, is that experiencing crises depresses all forms of political participation for those with less propensity to participate but boosts all forms of participation for those with a higher propensity. Recall, however, that our heterogeneous effects analysis of voter turnout found *negative* and statistically significant interactions of crisis incidence with validated past turnout, which militates against the argument

that crises increase turnout among those who are already engaged. We also looked for heterogeneous effects of crises across prior (pre-crisis) political interest in the VOTER survey. The results, which are reported in Appendix G, do not show evidence of an interactive effect ($p = 0.37$). These exploratory analyses suggest that there is no reason to believe that the negative effects of crisis on voter turnout are reversed for highly engaged citizens.

Discussion & Conclusion

The enduring recognition that the "personal is political" (Combahee River Collective 1979), along with important recent changes in our politics and economy are enough to warrant continued consideration of how personal crises affect political participation. In taking up this important topic, we are especially attuned to the broader political economy, the concrete realities of people's lives, and the multifaceted theoretical logic of political participation. Empirical tests reveal that most personal crises are associated with depressed voter turnout and that turnout declines as crises accumulate. The apparent relationships between these crises and turnout approach the magnitude of well-established disparities in the turnout literature (namely the gap between non-Hispanic White and Hispanic Americans and those of different levels of educational attainment). We also find robust evidence that these associations are larger among those who voted in the previous election.

At the same time, our findings show that crises do not always and everywhere lower engagement. We identify two scenarios where crises may spur participation in a way that is consistent with classic models of political accountability. First, the likelihood of voting may increase rather than decrease when a potential voter is affected by a crisis that is highly salient

and deeply political, such as the COVID-19 pandemic. Second, while crises in general may dampen the likelihood of voting, they may also make at least some citizens more likely to engage in other forms of participation, like protesting, contacting their elected officials, or volunteering for a campaign. In both scenarios, we suspect that the underlying mechanism is the same: the citizen can see participation as a means of addressing the problem that affected them. While voting is usually a very generalized form of political participation that is not tied to any one issue, that can change when a crisis is highly salient and highly politicized, as the COVID-19 pandemic has been. Other forms of participation, meanwhile, tend to be much more targeted to specific issues.

While we cannot make any dispositive claims, we do carefully and thoroughly leverage a wide range of available data to investigate the politics of life disruptions. Scholars can readily build on our findings through experimental, quasi-experimental, qualitative, and further observational approaches. Altogether, we offer a valuable contribution that advances knowledge of the politics of personal crises and provides a foundation on which future research can build.

While we situate our inquiries capaciously within both economic and psychological theories of political and social action, the analyses do not exhaust the full range of complexities and relationships worth understanding vis-à-vis the politics of personal crises. For instance, we do not comprehensively examine interactions between different kinds of crises, the distinct effects of combinations of crises, or the implications of the differential intensity of crises. It is quite likely that personal crises are not only cumulative but also interactive and interrelated. Moreover, even the same crises can be experienced at different levels of intensity by those facing them. Scholars seeking to better understand the political consequences of personal crises in relation to some of these complexities could gain traction by collecting more and better kinds of

quantitative data. For example, measuring personal crises in panel data sets that follow the same people over a long period of time would provide valuable empirical leverage, while including survey questions gauging respondents' sense of the intensity, length, and specific timing of crises would allow researchers to account for crisis heterogeneity. In addition to such survey based quantitative work, careful qualitative work can help to build knowledge of how crises are differentially perceived across stages of the life course, at varied levels of intensity, across relevant groups (e.g., people living in poverty versus middle class people), and more. Close qualitative investigation could also help scholars to chart new ways people articulate the connections between personal crises and political choices. Altogether, this topic is fertile ground for substantively important and methodologically wide-ranging research going forward.

Most immediately, our results demonstrate the value of examining the politics of personal crisis with nuance and comprehensiveness. Instead of focusing on a single category or a particular economic shock, we considered a wide and varied spectrum of challenges that emerge in the context of a precarious economy. While this research illuminates the political economy of personal crisis, continued elaboration of the mechanisms and processes structuring the connection between personal crisis and political outcomes remains an important task. As Americans cope with the devastating repercussions of a global pandemic, understanding the democratic consequences of personal crisis takes on a new and crucial importance.

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APPENDIX

The Politics of Personal Crisis:
How Life Disruptions Shape Political Participation

Christopher Ojeda, Jamila Michener, Jake Haselswerdt

Table of Content

Appendix A: Data Collection

We use five datasets: The 2018 & 2020 Cooperative Election Study Common Content (CES 2018 Common Content & CES 2020 Common Content), the 2018 & 2020 Cooperative Election Study Module (CES 2018 Module & CES 2020 Module), and the Democracy Fund Views of the Electorate Research (VOTER) Survey. The data from these studies are based on surveys of ordinary individuals living in the United States. In both the CES and VOTER surveys, individuals are part of a YouGov panel and their participation in the survey is compensated through a YouGov point system; once panelists have accumulated enough points, they can redeem them for material goods.

In the remainder of this section, we enumerate how these studies conform to the principles set forth in the American Political Science Association's (APSA) "Principles and Guidance for Human Subjects Research."

Power: The CES and VOTER studies are national representative surveys of adult living in the United States. This focus means that participants are not systematically sampled from either vulnerable communities (e.g., prisoners) or powerful parties (e.g., public officials). Participants may still be part of these groups, although this membership is coincidental.

Consent: The CES and VOTER studies seek out informed consent before administering the survey. Respondents are also given the option to stop taking the survey at any time.

Deception: The CES and VOTER studies did not include any deception (with respect to identity, activity, motivation, or misinformation) to the best of our knowledge.

Harm/Trauma: There is minimal risk to participants in the CES and VOTER studies.

Confidentiality: One important way that the CES and VOTER studies ensure confidentiality is by removing identifying information from the data files before making them publicly available. Indeed, the files we downloaded and analyzed for the paper were already stripped of identifying information, so we are not personally at risk of breaching the confidentiality of participants in these studies.

Impact: The CES and VOTER studies have little-to-no impact on real-time political processes as they do not include manipulations or interventions of the political world. It is possible these studies induce a "Hawthorne Effect", but this effect—to the extent it occurs—is more likely to affect the results of the study than ongoing politics. On the positive side, knowledge generated from these studies will ideally improve political processes in the future.

Laws, Regulations, and Prospective Review: The administration of the CES and VOTER studies is done in compliance with both local and federal laws.

Shared Responsibility: We recognize and acknowledge that we share responsibility in upholding ethical research, both in our personal work and in academia more broadly.

More information about the studies, and how they address the APSA's "Principles and Guidance for Human Subjects Research," can be found at the following links:

- CCES: https://cces.gov.harvard.edu/
- VOTER: https://www.voterstudygroup.org/ / YouGov: https://today.yougov.com/

Appendix B: Variable Measurement

CCES Common Content (2018 & 2020)

Voter turnout was measured using voter-file validation through the firm Catalist, which manages a dataset of voter files. Respondents who Catalist found to have voted in the 2018 and 2020 general elections were coded 1, while all others were coded 0, in accordance with the first approach recommended in the Guide to the 2018 CES. Additional details of the vote matching procedure are reported in the CES guide: Brian Schaffner; Stephen Ansolabehere; Sam Luks. 2019. "Guide to the 2018 Cooperative Congressional Election Survey." https://doi.org/10.7910/DVN/ZSBZ7K/WZWCZ1

Personal crises were measured by asking respondents in the 2018 and 2020 CES Common Content, "Over the past year have you…?" Responses options included yes and no for each of the following items:

1. Lost a job
2. Divorced
3. Had a reduction in income
4. Been a victim of a crime
5. Visited an emergency room

In addition, the CCES 2020 Common Content asked about other general negative life events as well as pandemic-related problems. The additional general negative life events were measured by asking respondents, "Over the past year have you…?"

1. Had a pay cut at work

In addition to these items, the CCES 2020 Common Content asked about pandemic-related problems. Respondents were asked the following questions (and response options):

- Have you or someone you know been diagnosed with the novel coronavirus (COVID-19) during the past year? (Select all that apply: yes I have, yes a family member, yes a friend, yes a co-worker, No I do not know anyone who has been diagnosed)
- Do you know anyone who died from the novel coronavirus (COVID-19)? (Select all that apply: yes a family member, yes a friend, yes a co-worker, no I do not know anyone who has died from coronavirus)
- How did your work status change this year as a result of the coronavirus pandemic? (Select all that apply: my hours have been reduced, my hours were reduced but they have been restored, I have been temporarily laid off, I was temporarily laid off but have now been re-hired, I had more than one job before the pandemic and lost one of them, I lost my job, I was not working when the pandemic began, my hours have increased, I have taken additional jobs since the pandemic, no nothing about my work has changed)

Responses to the pandemic-related problems are coded so that *each selection* for the first two constitutes a separate negative life event, while the third question is coded two negative life events for either losing a job or have work hours reduced. To avoid double counting a job loss, pandemic-related job loss is only coded if a person answer "no" to the job loss in the general negative life events battery.

Past Voting was measured with voter file validation in the same manner as turnout in the current election (see above). For both the 2018 and 2020 surveys, respondents were matched to voter files for the 2016 general presidential election. YouGov provided this data to the authors through a separate contract.

Gender was measured by asking respondents, "Are you…?" with response options of male or female.

Race was measured by asking respondents, "What racial or ethnic group best describes you?" Response options included White, Black or African-American, Hispanic or Latino, Asian or Asian-American, Native American, Middle Eastern, mixed race, and other.

Age was measured by asking respondents "In what year where you born?" and then subtracting the response from the interview year.

Education was measured by asking respondents, "What is the highest level of education you have completed?" Response options included did not graduate from high school, high school graduate, some college but no degree (yet), 2-year college degree, 4-year college degree, or postgraduate degree (MA, MBA, MD, JD, PhD, etc.).

Income was measured by asking respondents "Thinking back over the last year, what was your family's annual income?" Response options included the following categories:

- Less than $10,000
- $10,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $59,999
- $60,000 - $69,999
- $70,000 - $79,999
- $80,000 - $99,999
- $100,000 - $119,999
- $120,000 - $149,999
- $150,000 - $199,999
- $200,000 - $249,999
- $250,000 - $349,999
- $350,000 - $499,999

- $500,000 or more

Marital status was measured by asking respondents, "What is your marital status?" Response options included married, separated, divorced, widowed, and never married.

Religious attendance was measured by asking respondents, "Aside from weddings and funerals, how often do you attend religious service?" Response options included more than once a week, once a week, once or twice a month, a few times a year, seldom, never, and don't know.

CCES Module (2018 & 2020)

Voter turnout was measured using voter-file validation through the firm Catalist, which manages a dataset of voter files. Respondents who Catalist found to have voted in the 2018 and 2020 general elections were coded 1, while all others were coded 0, in accordance with the first approach recommended in the Guide to the 2018 CCES. Additional details of the vote matching procedure are reported in the CCES guide: Brian Schaffner; Stephen Ansolabehere; Sam Luks. 2019. "Guide to the 2018 Cooperative Congressional Election Survey." https://doi.org/10.7910/DVN/ZSBZ7K/WZWCZ1

Personal crises were measured both the 2018 and 2020 CCES Modules by asking respondents, "Over the past year have you…?" Responses options included yes and no for each of the following items:

1. Lost health insurance
2. Lost your home
3. Been evicted
4. Lost eligibility for a government program
5. Lost eligibility for a deduction, credit, or other tax break
6. Had trouble affording medical expenses
7. Had a substantial increase in rent or mortgage
8. Had your driver's license suspended or revoked
9. Divorced
10. Visited the emergency room
11. Had a reduction in income
12. Lost your job
13. Been the victim of a crime

In addition, the CCES 2020 Module asked about other general negative life events as well as pandemic-related problems. The additional general negative life events were measured by asking respondents, "Over the past year have you…?"

1. Been demoted or hours cut (not COVID-related)
2. Had trouble with boss
3. Owned business that lost money or closed
4. Went bankrupt

5. Had possessions repossessed
6. Pawned valuables to make ends meet
7. Been pressured to pay bills or creditors
8. Been unable to pay utilities
9. Had problem with alcohol or drugs
10. Death of family member or close friend (not COVID-related)
11. Had serious trouble with romantic partner
12. Arrested or accused of a crime
13. Sued in a civil case
14. Been discriminated against
15. Had a home destroyed
16. Lived in a house in poor condition
17. Had to unexpectedly pay for major home repairs
18. Had serious troubles with a landlord
19. Needed but unable to use public transportation
20. Needed, but not had, car for 1 month or more
21. Temporarily lived with others or in a shelter
22. Fallen behind in paying rent

Respondents were asked the following questions (and response options) for pandemic-related problems:

- Have you or someone you know been diagnosed with the novel coronavirus (COVID-19) during the past year? (Select all that apply: yes I have, yes a family member, yes a friend, yes a co-worker, No I do not know anyone who has been diagnosed)
- Do you know anyone who died from the novel coronavirus (COVID-19)? (Select all that apply: yes a family member, yes a friend, yes a co-worker, no I do not know anyone who has died from coronavirus)
- How did your work status change this year as a result of the coronavirus pandemic? (Select all that apply: my hours have been reduced, my hours were reduced but they have been restored, I have been temporarily laid off, I was temporarily laid off but have now been re-hired, I had more than one job before the pandemic and lost one of them, I lost my job, I was not working when the pandemic began, my hours have increased, I have taken additional jobs since the pandemic, no nothing about my work has changed)

Responses to the pandemic-related problems are coded so that *each selection* for the first two constitutes a separate negative life event, while the third question is coded two negative life events for either losing a job or have work hours reduced. To avoid double counting a job loss, pandemic-related job loss is only coded if a person answer "no" to the job loss in the general negative life events battery.

Past Voting was measured with voter file validation in the same manner as turnout in the current election (see above). For both the 2018 and 2020 surveys, respondents were matched to voter

files for the 2016 general presidential election. YouGov provided this data to the authors through a separate contract.

Gender was measured by asking respondents, "Are you…?" with response options of male or female.

Race was measured by asking respondents, "What racial or ethnic group best describes you?" Response options included White, Black or African-American, Hispanic or Latino, Asian or Asian-American, Native American, Middle Eastern, mixed race, and other.

Age was measured by asking respondents "In what year where you born?" and then subtracting the response from the interview year.

Education was measured by asking respondents, "What is the highest level of education you have completed?" Response options included did not graduate from high school, high school graduate, some college but no degree (yet), 2-year college degree, 4-year college degree, or postgraduate degree (MA, MBA, MD, JD, PhD, etc.).

Income was measured by asking respondents "Thinking back over the last year, what was your family's annual income?" Response options included the following categories:

- Less than $10,000
- $10,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $59,999
- $60,000 - $69,999
- $70,000 - $79,999
- $80,000 - $99,999
- $100,000 - $119,999
- $120,000 - $149,999
- $150,000 - $199,999
- $200,000 - $249,999
- $250,000 - $349,999
- $350,000 - $499,999
- $500,000 or more

Marital status was measured by asking respondents, "What is your marital status?" Response options included married, separated, divorced, widowed, and never married.

Religious attendance was measured by asking respondents, "Aside from weddings and funerals, how often do you attend religious service?" Response options included more than once a week, once a week, once or twice a month, a few times a year, seldom, never, and don't know.

VOTER Study

Voter turnout was measured using voter-file validation through the firm TargetSmart. Respondents who TargetSmart found to have voted in the 2018 general election were coded 1, while all others were coded 0.

Personal crises were measured by asking respondents, "In the last 12 months, have you: …?" Response options included yes or no for the following items.

1. Lost your job
2. Had a spouse or partner lose their job
3. Had difficulty making a mortgage payment or paying your rent
4. Had difficulty making a student loan payment
5. Had difficulty making a car payment
6. Had difficulty making a credit card payment
7. Experienced a drop in your household income

Past voting was measured using voter-file validation through the firm TargetSmart. Respondents who TargetSmart found to have voted in the 2016 general election were coded 1, while all others were coded 0.

Gender was measured by asking respondents, "Are you male or female?" Response options included male and female.

Race was measured by asking respondents, "What racial or ethnic group best describes you?" Response options included White, Black, Hispanic, Asian, Native American, mixed, other, and Middle Eastern.

Age was measured by asking respondents, "In what year were you born?" and then subtracting the response from the interview year.

Education was measured by asking respondents, "What is the highest level of education you have completed?" Response options included no high school, high school graduate, some college, 2-year college degree, 4-year college degree, postgraduate degree.

Income was measured by asking respondents "Thinking back over the last year, what was your family's annual income?" Response options included:

- Less than $10,000
- $10,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $59,999

- $60,000 - $69,999
- $70,000 - $79,999
- $80,000 - $99,999
- $100,000 - $119,999
- $120,000 - $149,999
- $150,000 - $199,999
- $200,000 - $249,999
- $250,000 - $349,999
- $350,000 - $499,999

Marital status was measured by asking respondents, "What is your marital status?" Response options included married, separated, divorced, widowed, never married, domestic/civil partnership.

Religious attendance was measured by asking respondents "Aside from weddings and funerals, how often do you attend religious services?" Response options included more than once a week, once a week, once or twice a month, a few times a year, seldom, never, and don't know.

Appendix C: Descriptive Statistics

CCES 2018 Common Content

Variable	N	Mean	SD	Min	Max
Vote	60,000	0.52	0.50	0	1
Personal Crises	59,878	0.54	0.75	0	5
Past Vote	60,000	0.53	0.50	0	1
Education	60,000	3.34	1.55	1	6
Female	60,000	0.51	0.50	0	1
Black	60,000	0.13	0.33	0	1
Hispanic	60,000	0.12	0.33	0	1
Other Race	60,000	0.08	0.27	0	1
Age	60,000	47.71	18.01	18	95
Income	53,769	6.00	3.37	1	16
Religious Attendance	58,844	2.92	1.72	1	6
Marital Status	60,000	0.47	0.50	0	1

CCES 2018 Module

Variable	N	Mean	SD	Min	Max
Vote	1,000	0.53	0.50	0	1
Personal Crises	997	1.16	1.27	0	8
Past vote	1,000	0.55	0.50	0	1
Education	1,000	3.32	1.55	1	6
Female	1,000	0.52	0.50	0	1
Black	1,000	0.13	0.33	0	1
Hispanic	1,000	0.12	0.33	0	1
Other Race	1,000	0.08	0.28	0	1
Age	1,000	47.29	18.17	18	87
Income	898	5.90	3.36	1	16
Religious Attendance	984	2.83	1.67	1	6
Married	1,000	0.45	0.50	0	1

CCES 2020 Common Content

Variable	N	Mean	SD	Min	Max
Vote	61,000	0.58	0.49	0	1
Personal Crises	60,910	1.78	1.61	0	14
COVID	61,000	1.04	1.14	0	8
Non-COVID	60,910	0.75	0.95	0	6
Past Vote	61,000	0.53	0.50	0	1
Education	61,000	3.37	1.55	1	6
Female	61,000	0.52	0.50	0	1
Black	61,000	0.13	0.33	0	1
Hispanic	61,000	0.13	0.34	0	1
Other Race	61,000	0.09	0.28	0	1
Age	61,000	48.21	18.08	18	95
Income	54,908	6.07	3.52	1	16
Religious Attendance	59,673	2.84	1.71	1	6
Marital Status	61,000	0.46	0.50	0	1

CCES 2020 Module

Variable	N	Mean	SD	Min	Max
Vote	1,000	0.62	0.49	0	1
Personal Crises	998	3.45	3.70	0	23
COVID	1000	1.02	1.16	0	7
Non-COVID	998	2.44	3.25	0	20
Past vote	1,000	0.68	0.47	0	1
Education	1,000	3.39	1.57	1	6
Female	1,000	0.51	0.50	0	1
Black	1,000	0.12	0.33	0	1
Hispanic	1,000	0.12	0.33	0	1
Other Race	1,000	0.09	0.28	0	1
Age	1,000	48.21	17.73	18	93
Income	904	5.84	3.45	1	16
Religious Attendance	977	2.70	1.63	1	6
Married	1,000	0.49	0.50	0	1

VOTER Study

Variable	N	Mean	SD	Min	Max
Voted	3,691	0.66	0.47	0	1
Negative Life Events	3,614	0.63	1.09	0	7
Past Vote	3,691	0.71	0.45	0	1
Education	3,691	3.36	1.48	1	6
Female	3,691	0.52	0.50	0	1
Black	3,683	0.12	0.33	0	1
Hispanic	3,683	0.11	0.32	0	1
Other Race	3,683	0.10	0.29	0	1
Age	3,691	55.08	15.68	25	94
Income	3,212	6.12	3.24	1	16
Religious Attendance	3,668	2.74	1.72	1	6
Married	3,691	0.56	0.50	0	1

Appendix D: Crisis Prevalence and Bivariate Association with Voting

CES 2018 Common Content

Crisis	% Reporting Crisis	% Voting		T-Statistic
		No Crisis	*Crisis*	
Had a reduction in income	17.34	57.33	51.33	11.16
Lost a job	7.81	57.40	40.67	20.80
Divorced	1.28	56.45	40.45	8.24
Been a victim of a crime	3.27	56.62	45.75	9.42
Visited an emergency room	24.78	57.71	51.89	12.40

CES 2018 Module

Crisis	% Reporting Crisis	% Voting		T-Statistic
		No Crisis	*Crisis*	
Lost health insurance	7.25	59.72	43.48	2.65
Lost a home	1.40	59.03	11.11	2.91
Been evicted	1.49	59.07	23.08	2.62
Lost eligibility for a government program	5.52	59.24	47.17	1.74
Lost eligibility for tax deduction, credit, or break	7.47	58.25	63.01	-0.79
Had trouble affording medical expenses	27.67	61.64	49.81	3.34
Had a substantial increase in rent or mortgage	10.22	59.93	47.71	2.45
Had driver's license suspended or revoked	2.26	58.92	35.71	1.75
Had a reduction in income	16.15	59.59	54.17	1.30
Lost a job	6.85	59.66	42.86	2.63
Divorced	1.01	58.56	61.54	-0.22
Been the victim of a crime	2.83	59.02	41.67	1.71
Visited the emergency room	26.70	59.07	57.14	0.53

CES 2020 Common Content

Crisis	% Reporting Crisis	% Voting		T-Statistic
		No Crisis	*Crisis*	
Had a reduction in income	27.26	66.16	59.46	15.59
Lost a job	15.71	66.67	50.70	29.69
Divorced	1.28	64.49	44.70	10.86
Been a victim of a crime	3.72	64.79	49.43	14.50

Visited an emergency room	18.85	64.98	61.15	7.73
Took a pay cut at work	8.07	64.95	56.50	11.90
Diagnosed with COVID-19	4.87	64.51	59.36	5.69
Family member diagnosed with COVID-19	21.06	63.82	65.86	-4.30
Friend diagnosed with COVID-19	29.33	61.25	70.76	-22.92
Co-worker diagnosed with COVID-19	29.33	63.50	69.66	-10.35
Family member died from COVID-19	4.97	64.43	60.99	3.86
Friend died from COVID-19	9.18	63.41	72.11	-13.34
Co-worker died from COVID-19	1.53	64.25	65.10	-0.56
Work hours reduced during the pandemic	16.38	66.24	54.16	23.12
Laid off during the pandemic	5.05	64.61	57.47	7.94

CES 2020 Module

		% Voting		
Crisis	**% Reporting Crisis**	*No Crisis*	*Crisis*	**T-Statistic**
Lost eligibility for government program	6.00	66.35	45.45	3.18
Lost eligibility for a tax break	3.14	65.38	60.53	0.62
Demoted or hours cut (not COVID-related)	3.36	65.22	64.52	0.08
Had trouble with boss	2.66	65.73	48.39	2.00
Owned business that lost money or closed	1.84	65.23	63.64	0.16
Went bankrupt	1.45	65.32	54.55	0.75
Had possessions repossessed	1.99	66.22	15.00	4.81
Pawned valuables to make ends meet	12.62	67.38	48.72	4.01
Been pressured to pay bills or creditors	11.80	65.94	60.00	1.30
Been unable to pay utilities	12.54	67.24	51.18	3.57
Lost health insurance	6.66	66.03	51.72	2.22
Had trouble affording medical expenses	14.31	65.33	64.47	0.20
Had problem with alcohol or drugs	4.56	66.01	46.34	2.59
Death of a family member or close friend (not COVID-related)	18.96	66.42	60.10	1.66
Had serious trouble with romantic partner	7.30	66.42	49.30	2.93
Arrested or accused of a crime	1.91	65.72	35.29	2.62
Sued in a civil case	1.96	65.10	70.00	-0.45
Been discriminated against	8.42	65.36	63.42	0.35
Had a home destroyed	1.12	65.18	66.67	-0.11
Lived in a house in poor condition	6.82	66.31	50.00	2.73
Had to unexpectedly pay for major home repairs	11.64	64.36	71.43	-1.52

Had serious troubles with landlord	2.10	65.37	58.33	0.71
Needed but unable to use public transportation	3.46	65.55	57.14	1.12
Needed, but not had, car for 1 month or more	7.45	68.88	42.86	4.10
Had driver's license suspended	1.28	65.99	13.33	4.28
Temporarily lived with others or in a shelter	3.00	65.74	46.43	2.12
Lost home	1.41	65.69	33.33	2.62
Evicted	1.58	65.65	30.77	2.63
Had a substantial increase in rent	3.73	65.38	60.98	0.58
Fallen behind in paying rent	5.71	65.92	54.10	1.88
Had a reduction in income	26.57	65.83	63.67	0.64
Lost a job	14.98	67.40	51.77	3.63
Divorced	1.20	65.55	38.46	2.04
Been a victim of a crime	3.93	65.31	62.50	0.37
Visited an emergency room	19.30	65.38	64.47	0.24
Took a pay cut at work	6.66	65.73	58.67	1.23
Diagnosed with COVID-19	4.40	65.79	52.27	1.84
Family member diagnosed with COVID-19	20.15	65.26	64.98	0.08
Friend diagnosed with COVID-19	30.14	61.38	73.35	-3.73
Co-worker diagnosed with COVID-19	30.14	64.69	68.35	-0.84
Family member died from COVID-19	3.58	65.87	50.00	2.12
Friend died from COVID-19	9.38	63.68	77.78	-2.91
Co-worker died from COVID-19	1.99	64.90	80.00	-1.40
Work hours reduced during the pandemic	14.68	66.75	56.77	2.40
Laid off during the pandemic	4.34	64.81	72.92	-1.15

VOTER Study

		% Voting		
Crisis	**% Reporting Crisis**	*No Crisis*	*Crisis*	**T-Statistic**
Experienced a drop in household income	4.96	60.39	41.38	7.03
Spouse or partner lost job	3.86	60.44	36.03	8.39
Difficulty making a credit card payment	9.13	61.11	42.19	9.01
Lost your job	5.07	60.49	38.66	8.18
Difficulty making a student loan	3.94	60.64	36.39	9.00
Difficulty making a mortgage or rent payment	13.06	60.62	50.00	5.59
Difficulty making a car payment	23.59	60.02	56.96	2.10

Appendix E: Full Multivariate Results

	CES 2018 Module	CES 2018 Common	CES 2020 Module	CES 2020 Common	VOTER Survey
Non-COVID Crises	-0.109 (.104)	-0.104** (.028)	-0.091* (.040)	-0.109** (.021)	-0.107 (.068)
COVID Crises	-	-	0.200+ (.112)	0.061** (.016)	-
Past voting	4.151** (.263)	4.296** (.042)	3.475** (.273)	3.850** (.046)	2.431** (.161)
Female	-0.202 (.259)	-0.116** (.040)	0.023 (.251)	0.123** (.039)	-0.220 (.152)
Black	-0.911+ (.520)	-0.166* (.078)	-0.617 (.428)	-0.238** (.065)	-0.447 (.277)
Hispanic	-0.590 (.418)	-0.170* (.072)	-0.157 (.370)	-0.287** (.067)	-0.363 (.300)
Other race	-.940* (.429)	-0.149+ (.080)	-0.132 (.452)	-0.381** (.077)	-0.146 (.294)
Age	0.003 (.010)	0.017** (.001)	0.008 (.008)	0.011** (.001)	0.019** (.006)
Education	0.139 (.085)	0.140** (.015)	0.171 (.107)	0.125** (.014)	0.147** (.056)
Income	0.011 (.052)	0.049** (.007)	-0.068 (.049)	0.045** (.007)	0.002 (.026)
Married	-0.118 (.271)	0.055 (.047)	-0.222 (.296)	-0.016 (.048)	0.200 (.158)
Religious attendance	-0.052 (.079)	0.015 (.012)	0.005 (.073)	-0.019 (.012)	0.028 (.048)
Children under 18	-0.001 (.285)	-0.411** (.051)	0.178 (0.263)	-0.230** (.048)	0.232 (.210)
Constant	-2.075** (.673)	-3.617** (.101)	-1.434* (.639)	-2.354** (.096)	-2.438** (.449)
Observations	*883*	*52,940*	*888*	*53,830*	*3,135*

+p<.10 *p<.05 **p<.01

Appendix F: The Demographics of Personal Crises

The Distribution of Non-COVID Crises Across Demographic Groups

	CES 2018 Common	CES 2018 Module	CES 2020 Common	CES 2020 Module	VOTER Study
Gender					
Women	0.59	1.22	0.77	2.50	0.67
Men	0.50	0.11	0.73	2.36	0.52
Age					
18-35	0.64	1.35	0.98	3.11	0.54
36-55	0.58	1.17	0.83	2.95	0.79
56 or older	0.44	0.99	0.52	1.54	0.70
Race/Ethnicity					
White	0.51	1.06	0.69	2.15	0.59
Black	0.69	1.33	0.89	3.39	0.84
Hispanic	0.61	1.71	0.89	3.38	0.98
Other	0.53	1.11	0.87	3.39	0.78
Education					
No HS Degree	0.69	1.90	0.88	2.07	0.79
HS Degree	0.58	1.20	0.74	2.29	0.62
Some College	0.59	1.17	0.84	2.60	0.72
College Degree	0.46	0.94	0.68	2.53	0.64
Income Quintile					
First	0.77	1.60	0.98	3.49	1.05
Second	0.56	1.26	0.80	3.11	0.83
Third	0.49	1.14	0.69	2.28	0.74
Fourth	0.44	0.95	0.61	1.65	0.57
Fifth	0.36	0.62	0.55	1.12	0.44

Note: Cell entries are the weighted average number of non-COVID crises reported by respondents in each group.

The Distribution of COVID Crisis Across Demographic Groups

	CES 2020 Common	CES 2020 Module
Gender		
Women	1.04	1.05
Men	1.04	0.99
Age		
18-35	1.17	1.14
36-55	1.14	1.15
56 or older	0.86	0.82
Race/Ethnicity		
White	1.00	0.95
Black	1.13	1.24
Hispanic	1.27	1.14
Other	0.96	1.13
Education		
No HS Degree	0.73	0.43
HS Degree	0.85	0.77
Some College	1.11	1.11
College Degree	1.20	1.26
Income Quintile		
First	0.84	0.78
Second	1.02	1.00
Third	1.12	1.11
Fourth	1.18	1.08
Fifth	1.27	1.27

Note: Cell entries are the weighted average number of COVID crises reported by respondents in each group.

Appendix G: The Heterogeneous Effects of Personal Crises

This section reports the results of analyses exploring possible heterogeneous effects of personal crises on voter turnout. We re-estimate the main logistic regression models with all control variables from Table 3 of the main text but add an interaction term between personal crises and each covariate in the regression model. Separate regression models are estimated for each interaction in each study. As in the main text, we distinguish between non-COVID and COVID crises in the 2020 CES models, including interaction terms for each of these variables in the same regression model.

The interaction terms allow us to test for heterogeneous effects of personal crises across values of past voting, gender, race (measured using White, Black, and Hispanic dummy variables), children under 18 (dichotomous), age, income, and education for all surveys. For the VOTER panel, we also include a model testing for heterogeneous effects with prior political interest. Political interest is measured with a question that asks respondents how often they would say they "follow what's going on in government and public affairs." The response options are "Most of the time," "Some of the time," "Only now and then," "Hardly at all," and "Don't know." We code this as an ordinal variable ranging from 1 to 4, with "Hardly at all" and "Don't know" both coded as 1 and "Most of the time" coded as 4. We use the respondent's response to this question in the 2017 wave of the survey, before the twelve-month lookback window for the financial difficulties question.

The table below displays the coefficient, standard error, and p-value for each interaction term.

Covariate	CES 2018 Module	CES 2018 Common	CES 2020 Module - COVID	CES 2020 Module - non-COVID	CES 2020 Common - COVID	CES 2020 Common - non-COVID	VOTER Survey
Past voting	-.24 (.19) p=.22	-.31** (.05) p<.001	.003 (.24) p=.99	-.08+ (.04) p=.06	.12** (.04) p<.001	-.27** (.04) p<.001	-.20+ (.12) p=.095
Female	.28 (.22) p=.22	-.11+ (.06) p=.06	-.008 (.21) p=.97	-.02 (.08) p=.79	.08* (.03) p=.01	.03 (.04) p=.52	-.02 (.14) p=.90
White	.08 (.21) p=.70	-.17* (.07) p=.01	.09 (.24) p=.71	-.07 (.07) p=.32	.10** (.03) p=.003	-.04 (.04) p=.36	-.05 (.15) p=.77
Black	.03 (.25) p=.89	.02 (.10) p=.84	.49 (.34) p=.14	-.06 (.09) p=.49	-.02 (.05) p=.67	-.01 (.06) p=.85	-.22 (.18) p=.23
Hispanic	-.70* (.34) p=.04	.21* (.09) p=.02	-.60* (.30) p=.05	.26** (.10) p=.006	-.14** (.04) p=.002	.02 (.06) p=.72	.11 (.23) p=.65
Children <18	.08 (.20) p=.67	.09 (.06) p=.13	.03 (.20) p=.90	-.05 (.08) p=.56	-.06+ (.03) p=.08	-.11** (.04) p=.006	.11 (.16) p=.50
Age	-.007 (.005) p=.18	.001 (.001) p=.47	.01 (.01) p=.15	.0002 (.004) p=.96	.0004 (.001) p=.67	.003* (.001) p=.04	.01 (.01) p=.10
Education	.14* (.07) p=.03	.02 (.02) p=.19	.07 (.07) p=.34	.05+ (.03) p=.07	-.01 (.01) p=.22	.05** (.01) p<.001	-.01 (.04) p=.80
Income	.04 (.02) p=.13	.003 (.008) p=.74	.01 (.03) p=.65	.01 (.01) p=.17	-.007+ (.004) p=.09	.02** (.01) p=.005	.07** (.02) p=.001
Past political interest	-	-	-	-	-	-	.07 (.08) p=.37

+p<.10 *p<.05 **p<.01

For the 2018 CES and VOTER surveys, each cell reports the results for an interaction term from a separate logistic regression. For each of the 2020 CES surveys, adjacent cells report the results for interaction terms for the COVID-specific and other negative life events variables from the same logistic regression. All analyses use survey weights. Standard errors in parentheses.

Appendix H: Effects of Changes vs. Possibly Ongoing Crises

The table in this appendix presents the results of a logit model of validated voter turnout using the 2020 CES Module data (which includes the most robust set of measures) and including variables that distinguish between crises that likely represent a significant change from the status quo in the current year and those that could represent ongoing problems. Since respondents were only asked about the past twelve months, we have no direct measure of whether the problem is ongoing or new, and so based the coding on our best judgment. To illustrate this distinction, "serious trouble with romantic partner" may represent an ongoing issue, while divorce is a specific qualitative change in a person's marital status. Divorce may represent the culmination of ongoing marital problems, but the event itself is qualitatively different from those ongoing problems. Similarly, "serious troubles with a landlord" may drag on for some time, while eviction represents a clear qualitative change in housing status. Complete lists of crises counted for each variable are included below. As in the 2020 CES analyses in the main text, crises related to COVID-19 are measured separately as well.

Personal crises – likely changes

- Had driver's license suspended
- Lost eligibility for government program
- Lost eligibility for tax break
- Demoted or hours cut (non-COVID related)
- Owned business that lost money or closed
- Went bankrupt
- Had possessions repossessed
- Lost health insurance
- Death of family member or close friend (non-COVID)
- Arrested or accused of a crime
- Sued in a civil case
- Had a home destroyed
- Had to unexpectedly pay for major home repairs
- Temporarily lived with others or in a shelter
- Lost home
- Evicted
- Had a substantial increase in rent
- Decreased income
- Lost job
- Divorced
- Victim of a crime
- Visited an emergency room
- Pay cut at work

Personal crises – possibly ongoing

- Had trouble with boss
- Pawned valuables to make ends meet

- Been pressured to pay bills or creditors
- Been unable to pay utilities
- Had trouble affording medical expenses
- Had problem with alcohol or drugs
- Had serious trouble with romantic partner
- Been discriminated against
- Lived in a house in poor condition
- Had serious troubles with a landlord
- Needed but unable to use public transportation
- Needed, but not had, car for 1 month or more
- Fallen behind in paying rent

Logistic Regression of Voting on Discrete vs Possibly Ongoing Crises

	CES 2020 Module
Non-COVID crises: likely changes	-.18* (.08)
Non-COVID crises: possibly ongoing	.01 (.08)
COVID crises	.21+ (.11)
Past voting	3.50** (.27)
Female	.01 (.25)
Black	-.60 (.43)
Hispanic	-.18 (.36)
Other race	-.11 (.46)
Age	.01 (.01)
Education	.09 (.06)
Income	-.07 (.05)
Married	.01 (.07)
Religious attendance	-.20 (.30)
Children under 18	.14 (.27)
Constant	-1.48** (.64)
Observations	*888*

+p<.10 *p<.05 **p<.01
All analyses use survey weights.