# Experience of the COVID-19 Pandemic and Support for Safety-Net Expansion

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**Abstract**

Did individuals' experiences with the harms of the COVID-19 pandemic influence their attitudes towards safety-net programs? To assess this question, we combine rich information about county-level impacts and individual-level perceptions of the early pandemic, repeated measurements of attitudes towards safety-net expansion, and pre-pandemic measurements of related political attitudes. Individuals facing higher county-level impact or greater perceived risks are more likely to support long-term expansions to unemployment insurance and government-provided healthcare when surveyed in June 2020. These differences persist across time, with experiences in the early months of the pandemic remaining strongly predictive of attitudes towards safety-net expansion in early 2021.

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Preregistration: https://aspredicted.org/5kc4x.pdf.

COVID-19 deeply disrupted American economic life. In response to the initial lock-downs and general panic, the April 2020 unemployment rate increased by over 14 percentage points. This unprecedented job loss translated into substantial economic hardship, with as much as 23% of households facing food insecurity and 7% receiving food assistance in a given week (Bitler, Hoynes and Schanzenbach, 2020). Due to American reliance on employer-sponsored health insurance, this precipitous increase in unemployment led to millions becoming uninsured at a very inopportune time (Bivens and Zipperer, 2020). Hardships like these strained the existing safety-net system, whose provisions did not replace the lost income for many American households (Moffitt and Ziliak, 2020).

In this paper we present evidence that Americans' experience in the early days of COVID-19 substantially altered their subsequent attitudes towards safety-net programs. In our data, individuals who objectively faced or subjectively perceived more severe early impacts of the pandemic are more likely to support safety-net expansions. These effects are both substantial in magnitude and persistent, pointing to the possibility that COVID-19 may influence American redistributive policy even after the disruption of the pandemic subsides.

In order to study the relationship between COVID-19's real or perceived impact and preferences for safety-net expansion, we create a dataset that merges a variety of surveys and county-level records. Our measures of attitudes towards safety-net programs come from a June 2020 survey of 2,516 members of the Understanding America Study (UAS). In this survey, we elicit respondents' support or opposition for long-term expansions to government-provided healthcare and unemployment insurance. While we are primarily interested in long-term government policy preferences, we additionally measure support for short-term government programs targeted towards COVID-19 relief. Our proxies for respondents' individual experience with COVID-19 come from objective measurements of the impact of COVID-19 in the respondents' counties as well as subjective measurements derived from the respondents' answers in a prior UAS Coronavirus tracking survey deployed in April 2020. To control for potentially important confounding factors, we match these data to other UAS surveys containing data on pre-pandemic political ideology, pre-pandemic support for government-provided healthcare, and a battery of demographic measures. To assess the persistence of effects, we match these data to a survey run in early 2021 that re-posed our

questions eliciting support for long-term safety-net reform.

We begin our analysis by predicting attitudes towards short- and long-term safety-net expansions with each item from our battery of COVID-19 impact proxies. With relatively few exceptions across the variety of measures used, individuals facing greater impact are predicted to have substantially greater support for policy expansion. These results suggest the possibility that COVID-19 impact has a quantitatively important relationship with policy preferences.

A challenge in succinctly quantifying these relationships arises from our examination of a comparatively large number of potential proxies for COVID-19's impact, with some measures having clear overlap. To help aggregate information across these measures, we apply principal component analysis to our full battery of proxies. We find these variables can be well summarized by a mere two principal components. The first principal component loads heavily on the three county-level objective measures: reported COVID-19 deaths and infections, as well as the change in the county unemployment rate. We thus use this first component as a measure of objective, county-level impact of the pandemic. The second principal component loads heavily on the subjective elicitations from the April 2020 Coronavirus tracking survey: perceived probabilities of contracting the Coronavirus, of dying of the Coronavirus conditional on contracting it, of losing their job, and of running out of money, as well as a non-probabilistic measure of the perceived danger associated with a battery of daily activities. We thus use this second component as a measure of the respondents' subjective assessment of the risks of the pandemic.

In our primary analysis, we predict support for policy expansion with the two measures generated by our principal component analysis. In our preferred version of this analysis we control both for pre-pandemic political ideology and for a large battery of individual demographics. In these analyses, we again find that individuals facing greater impacts of COVID-19 are more supportive of policy expansion. A 1-standard-deviation increase in respondents' county-level impact is associated with a 4.1pp increase in probability of support for expansion of unemployment insurance and a 3.6pp increase in probability of support for expansion of government-provided healthcare. A 1-standard-deviation increase in a respondents' subjective risk assessment is associated with a 2.0pp increase in probability

of support for expansion of unemployment insurance and a 2.1pp increase in probability of support for expansion of government-provided healthcare.

Are our results driven by prior skeptics of the value of safety-net programs who became supportive in the face of the need presented by the pandemic? Or alternatively, are they driven by prior supporters of safety-net expansion who, when observing the large federal response despite little need in their local area, became less supportive of government action? To inform this question, we identify individuals who expressed different attitudes regarding the expansion of government-provided healthcare when questioned about support for "Medicare for all" when surveyed prior to the pandemic. We find that attitudinal changes in both directions, and of roughly comparable magnitudes, are predicted by our measures of COVID-19's impact. In short, both types of substantive changes in position contribute to our effects.

The political importance of these changing attitudes depends on their persistence. Have these estimates identified a momentary shift in political attitudes experienced in June 2020? Did these effects last long enough to influence the 2020 election? Will these effects persist longer into the future? To partially inform these questions, we resurveyed respondents regarding preferences for long-term policy expansion in early 2021. With this more temporally distant data we continue to find strong associations with our measures of early-pandemic impact. Quantitatively, the average magnitude of effects is estimated to be 74% of the average of those estimated using June 2020 data. These results demonstrate that the differences we documented persisted through the 2020 election. The rate of decay suggests that they may remain relevant for some time to come.

If COVID-19 has led to a taste for changes in government policy, how would the populace like these changes to be funded? Our survey additionally included questions about preferences for short- and long-term changes to a variety of taxes, as well as preferences for deficit spending by the government. In contrast to our prior analyses—in which COVID-19 impact measures are typically strongly predictive—our tax-related analyses reveal few systematic effects on attitudes towards specific taxes. However, our analyses do reveal that individuals more impacted by COVID-19 are less likely to broadly oppose any tax increase, and are more likely to support deficit spending. These findings and analyses are reminiscent

of those in a long-standing literature assessing the coherence between preferences for policies and preferences for revenue collection (Mueller, 1963; Free and Cantril, 1967; Citrin, 1979; Ladd et al., 1979; Sears and Citrin, 1982; Hansen, 1998; Page and Shapiro, 2010; Tuxhorn, D'Attoma and Steinmo, 2021).

Our work closely relates to a literature examining how COVID-19 has influenced support for the comparatively generous welfare states of Western Europe. Relative to that literature, we generate different findings and different conclusions. Daniele et al. (2020) (studying Italy, Spain, Germany and the Netherlands) find that the COVID-19 pandemic induced lower support for welfare spending. In contrast, Ares, Bürgisser and Häusermann (2021) (studying Germany, Sweden, and Spain) find that preferred levels of redistribution remained stable, despite the fact that the pandemic exacerbated ideological polarization about welfare state efficiency and capacity. Somewhat similarly, Bol et al. (2020) (studying 15 Western European nations) find that lockdowns did not appear to influence traditional left-right attitudes.[1] While we can only speculate why we find a qualitatively different response among Americans,[2] we believe a different response need not be surprising given the vastly different baseline safety-net programs that were experienced and observed throughout the pandemic. Beyond merely documenting different responses among Americans, we also technically contribute to this literature through our construction and analysis of an unusually rich dataset. Relative to existing work, we have substantial advantages in our ability to explore questions of persistence and in our ability to use richly measured cross-county or cross-individual variation in experiences rather than relying on, e.g., priming interventions.

Moving beyond the specific context of the COVID-19 pandemic, our work also relates to existing research concerning the strong relationships between formative personal experiences and later attitudes, behaviors, and beliefs. As illustrative examples, Malmendier and Nagel

[1]Note, however, that Bol et al. (2020) document important effects on voting intentions, trust in government, and satisfaction with democracy.

[2]While we have emphasized an apparent difference in conclusions for analysis of Western European respondents, our results have some convergence with those of Foremny, Sorribas-Navarro and Vall Castelló (2020). Among their Spanish respondents, information treatments regarding COVID-19 induced a desire for a greater fraction of government spending to be allocated to healthcare issues (relating to our finding of greater support for expansion of government-provided healthcare). However, these information treatments had little influence on willingness to pay for medical care, ICU beds, or vaccines (related to our lack of findings regarding support for increased taxes).

(2015) document that beliefs about future inflation overweight inflation observations from within one's own lifetime[3], Malmendier and Nagel (2011) document that personal experience with low stock-market returns influences later financial risk tolerance, and Becker et al. (2020) document that experiences with forced migration influence later investment in human capital. Within this literature, we relate most closely to papers documenting tastes for redistribution responding to salient life events such as Giuliano and Spilimbergo's (2014) demonstration that early life experiences predict later support for government redistribution. In the most closely quantitatively comparable study, Margalit (2013) shows that personal experience with economic hardship during the great recession influenced support for welfare spending.[4] Benchmarked against these findings, our results suggest large quantitative impacts. Based on our preferred estimates, the difference in taste for safety-net expansion attributed to being in a county at the 90th percentile of impact versus a county at the 10th percentile of impact is comparable to the impact of personally losing a job in the wake of the great recession (a 9.5pp impact in Margalit (2013)). Our results suggest that the intuitions suggested by this literature played out in response to the particularly focal and salient experience of COVID-19 in a quantitatively important way, modifying the degree of American support for safety-nets in the immediate vicinity of tightly contested election and in a period where significant safety-net expansions were being publicly considered.

# 1 Dataset Construction

This project relies on the combination of data from a variety of surveys deployed over a two-year window as well as county-level statistics on COVID-19 and unemployment. In this section, we describe all data sources and the details of dataset construction.

[3]Malmendier, Nagel and Yan (2021) demonstrate that this overweighting extends even to central bankers.

[4]For results supporting this conclusion, see Owens and Pedulla (2013) and Hacker, Rehm and Schlesinger (2013). For a broader survey of the literature about how the experience of economic shocks (e.g., job loss or sharp drop in income) affect individuals' political views and voting behavior, see Margalit (2019).

## 1.1 Measures of Policy Preferences

Our survey was deployed in the Understanding America Study (UAS), an online panel of American households. This panel has three critical advantages for the purposes of our study. First, it has well established infrastructure for reaching respondents across the United States (US). Second, it devotes substantial effort to achieve representative sampling. Third, it allows data from prior surveys to be merged with the data from our own study. This includes elicitations of political ideology and support for Medicare expansions, each measured prior to the beginning of the pandemic, as well as detailed demographic data.

Respondents are recruited to the UAS panel through address-based sampling, either in nationally representative waves or in waves aimed to target more specific subpopulations. Upon being randomly selected for recruitment, significant efforts are made to recruit the targeted respondent to the panel. The recruitment process is designed to help respondents overcome common barriers to survey participation; this includes providing a tablet and broadband access to individuals who would otherwise be unable to take online surveys and providing all materials in Spanish to allow the recruitment of solely Spanish-speaking respondents.[5]

Our survey was deployed in late June, 2020. To achieve our preregistered target sample size of 2,500, we invited 3,333 members of the UAS to take our survey. We limited recruitment to panelists who had responded to two prior UAS surveys that provide subjective measures of COVID-19 exposure and a pre-COVID-19 measure of political ideology. Data collection was terminated shortly after the target sample size was reached, yielding our final sample of 2,516.

Our survey consists of two groups of questions.[6] The first group measured individual preferences for expansions of government policy. The second group elicited preferences for means of funding government activities.

Our questions of primary interest concerned long-term expansion of government policy. The two key survey prompts were "Do you support or oppose long-term expansion of unemployment benefits?" and "Do you support or oppose long-term expansions of government-

[5]This summary of the UAS draws from that in Pathak, Rees-Jones and Sönmez (2020).

[6]Complete documentation of this survey is available at `https://uasdata.usc.edu/survey/UAS+296`.

provided healthcare?". Response options were "strongly oppose," "somewhat oppose," "neither oppose nor support," "somewhat support," and "strongly support." For each of these questions, we also included an explicitly short-term variant of the question targeted to help individuals impacted by COVID-19, as well as a question regarding support for personal behaviors that help reduce the need for these government programs (i.e., purchase of private health insurance and private savings for use in case one loses their job).[7] We additionally asked an overarching question: "Overall, do you support a bigger or smaller government?", with response options of "I support a smaller government," "I think the current government is about the right size," and "I support a bigger government."

These questions were followed by questions probing the means of funding such activities in both the short- and long-term. For each time period, subjects were asked if they support increases in income taxes on high-, medium-, and low-income earners, payroll taxes, corporate taxes, wealth taxes, and sales taxes. They could indicate support for as many of these options as they would like, or indicate that they do not support any of the tax-increase options. Support for taxes was additionally measured with the prompt "I am personally willing to pay more in taxes," with responses provided on a five-point scale ranging from "strongly disagree" to "strongly agree." Subjects were additionally asked about their support or opposition of either short- or long-term increases in government deficit, providing another means of achieving policy expansion without raising taxes.

Our survey was brief, taking on average 3 minutes to complete. Subjects were paid $2 for their participation.

### 1.1.1 Follow-up Survey to Assess the Persistence of Effects

A key question assessed in this paper is the persistence of changes in attitudes arising from one's experience with COVID-19. To assess this persistence, we reposed our two primary questions on long-term policy expansions. These questions were deployed in January and February 2021 as part of a regular wave of the UAS's Coronavirus tracking survey.[8]

[7]The text of these questions (and all measures from this survey not explicitly reproduced here) are available in Appendix Tables A1 and A2.

[8]Complete documentation of this survey is available at `https://uasdata.usc.edu/survey/UAS+280`.

## 1.2 Measures of Experience with COVID-19's Consequences

The goal of our analysis is to assess the relationship between the policy preferences described in Section 1.1 and measures of individual experience with COVID-19's consequences. We generate a battery of measures of, or proxies for, this individual experience by matching a variety of data sources to these survey responses. We detail each group of measures below.

### 1.2.1 County-Level Measures of COVID-19's Impact

We merge into our dataset three measures of the objective impact of COVID-19 experienced in the respondent's county. All such merging is based on respondents' postal records on file with the UAS.

First, to generate measures of the literal spread of COVID-19, we match each response with the number of COVID-19 infections and deaths reported in the respondent's county measured as of the first day our survey was fielded. Data are drawn from usafacts.org.

Next, to generate a proxy for the economic impact of COVID-19, we match each response with the change in the respondent's county's unemployment rate between April 2019 and April 2020, as measured in the Bureau of Labor Statistics' records.

### 1.2.2 Individual-Level Subjective Risk Perceptions

To assess individuals' subjective experience with COVID-19, we match our data with the April 2020 wave of the UAS Coronavirus tracking survey.[9] This survey, run around the peak of the US's "first wave" of infection, contains a battery of questions that give a broad picture of the respondent's degree of concern with the pandemic. This gives us access to several measures of perceived health risks: 1) the perceived chance of getting the virus in the next 3 months, 2) the perceived probability of death if the respondent contracts the virus, and 3) an index capturing how safe the subject feels in a variety of activities. This also gives us access to several measures of perceived economic risks stemming from the pandemic: 1) a measure of the perceived chance of running out of money due to the virus in the next 3 months, and 2) a measure of the perceived chance of losing their job due to the virus in the

[9]Complete documentation of this survey is available at `https://uasdata.usc.edu/survey/UAS+235`.

next 3 months. The text of these questions is presented in Table 1.

## 1.3 Auxiliary Data Sources

Three additional data sources provide important background information for use in our analysis.

### 1.3.1 Demographic Data

The UAS collects detailed demographic information on its panelists every quarter, which is useful both for understanding the members of our study and for making comparisons between similar individuals in our analysis. When assessing or controlling for demographics we use measures of gender, marital status, employment status, US citizenship status, race and ethnicity, education, income, and age. Appendix Tables A3 and A4 present summary statistics on these variables (and also document a general lack of selection into survey participation based on these observables). Whenever these data are used as control measures, we include dummy variables for each discrete value of each relevant categorical variable.

### 1.3.2 Pre-pandemic Political Ideology

We match our survey to a prior UAS study on political preferences fielded in January 2020 (prior to the beginning of the pandemic in the United States).[10] As a control for political ideology, we use responses to the question "Regardless of your political registration or affiliation, where would you place yourself on the political spectrum from extremely liberal to extremely conservative?" Responses are given on a nine-point scale ranging from extremely liberal to extremely conservative with a tenth option of "I don't think of myself that way." We include dummy variables for each discrete response option in all regression analyses using this measure as a control. By this measure of political ideology, our sample is reasonably balanced: 24% identified as liberal, 30% identified as moderate, and 28% identified as conservative, with the remaining 18% indicating that they do not think of themselves in these terms. See Appendix Fig. A1 for the full response distribution.

[10] Complete documentation of this survey is available at `https://uasdata.usc.edu/survey/UAS+221`.

### 1.3.3 Pre-pandemic Attitudes Towards Government-Provided Healthcare

Our measures of pre-pandemic attitudes towards government-provided healthcare come from a UAS survey fielded in August and September, 2019.[11] We examine responses to two questions related to support for Medicare expansion. One question assessed support for "Medicare for all," asking "To what extent do you support or oppose eliminating private health insurance and, in its place, providing a publicly run healthcare program similar to Medicare that all Americans would be eligible for?" The other question assessed support for allowing buy-in to Medicare, asking "To what extent do you support or oppose allowing any American younger than 65 to either buy into a publicly run healthcare program similar to Medicare, or keep the plan they have?" Both elicited responses on the same 5-point opposed-to-support scaled used above. Pre-pandemic attitudes towards "Medicare for all" were quite mixed, with 40% of respondents supporting the policy and 39% opposing it. Pre-pandemic support for allowing buy-in to Medicare was high, with 53% of respondents supporting the policy and only 15% opposing it.

## 1.4 Preregistration

Our study was preregistered on aspredicted.org.[12] This preregistration includes precise specification of our sample size, our policy preferences of interest, our selection and coding of our county-level impact measures and our individual-level subjective risk perceptions, our key hypotheses, and our empirical strategy.

For transparency, we note that the empirical analysis presented in this paper goes significantly beyond the specific analyses that were preregistered. Our initial intention when running the study was to produce a short paper narrowly focused on the analysis that we will present in Section 2.2 and variants of the analysis that we will present in Section 2.7. We later became particularly interested in exploring non-preregistered analyses regarding the persistence of our results (reported in Section 2.6) and how our answers compared to pre-pandemic attitudes towards government-provided healthcare (reported in Section 2.5). In the course of this analysis, we found it useful to pursue the principal-component-based

[11] Complete documentation of this survey is available at `https://uasdata.usc.edu/survey/UAS+199`.
[12] The preregistration is available here: `https://aspredicted.org/5kc4x.pdf`.

dimensionality reduction that we document in Sections 2.3 and 2.4. In all cases, the analysis that we add supports the conclusions that are drawn from the original preregistered analysis, but often add important additional detail and assessment of robustness of our claims. For a prior draft of the paper with the analysis more tightly aligned to the original preregistration, see Rees-Jones et al. (2020).

# 2 Analysis of COVID-19's Impact on Policy Preferences

## 2.1 Levels of Support for Safety-Net Expansion

We begin our analysis by characterizing the overall level of support for safety-net policy as measured in our survey. Table 2 presents the response distributions for short-term and long-term expansions of our programs of interest.

As is documented in the first two columns of this table, short-term expansions of both programs were popular policies in June 2020. Overall, 75% of respondents showed some degree of support for expansion of unemployment insurance (only 13% opposed) and 78% of respondents showed some degree of support for expansion of government-provided healthcare (only 10% opposed).

In contrast, longer-term expansions of these programs had more mixed support, although still a relatively high degree of popularity: 47% of respondents supported long-term expansion of unemployment insurance (with 37% opposing) and 61% of respondents supported long-term expansion of government-provided healthcare (with 26% opposing).

Overall, Americans broadly supported short-term government intervention to mitigate the harms of the pandemic, even if their desire to limit the extent of government intervention meant that they did not support the continuation of such intervention in "normal times." Supporting this latter claim, responses to the survey question regarding preferences for overall government expansion reflect the stereotypical American taste for small government: 49% of respondents explicitly support an overall smaller government, 39% think the current government is about the right size, and a mere 12% support a bigger government.

## 2.2 Association of Support for Safety-Net Expansion and Proxies for Experience with COVID-19

To what degree does experience with COVID-19 influence these policy preferences? We begin this investigation by examining the association between each measure of support for policy expansion and each of our various measures capturing the impact of COVID-19 on our respondents. To account for the ordered, categorical nature of the policy-expansion response scales, we apply ordered logistic regressions.[13] Results are reported in Table 3.

In this table, each cell reports an estimated average marginal effect arising from predicting the discrete answers reported in Table 2 with a single predictor variable indicated in each row. Because all predictor variables are normalized, the reported marginal effects can be interpreted as the estimated increase in probability of any degree of support for policy expansion associated with a 1-standard-deviation increase in the row's measure. When calculating standard errors throughout all analyses to follow, we cluster by county whenever county-level variables are used and use Huber-White standard errors whenever solely individual-level variables are used. We apply the delta method to calculate standard errors for implied marginal effects arising from our parameter estimates.

We first direct attention to the first two columns of this table, reporting regressions predicting support for short-term policy expansion. In these analyses we find reasonably consistent positive associations between support for policy expansion and our COVID-19 impact measures. For example, examining the first row, we see that a 1-standard-deviation increase in county infections is associated with a 3.5pp (s.e.=0.4pp) increase in probability of support for short-term expansion of unemployment insurance and a 4.3pp (s.e.=0.4pp) increase in probability of support for government-provided healthcare. Examining all the rows of the table, we observe that the associations found for all county-level measures (county infections, county deaths, and the county unemployment shock) are similarly positive, strongly significant, and comparable in magnitude. Turning next to the individual-specific subjective risk assessments (capturing perceived probability of infection, death, economic harm, and

[13] An earlier draft of this paper (Rees-Jones et al., 2020) additionally reports analogous results using ordinary logistic regression, coding support for policy expansion according to several alternative thresholds in the ordinal scale. These analyses lead to similar conclusions.

perceived safety), we find qualitatively comparable results, although generally smaller in magnitude and weaker in statistical significance. Considering all measures as substitutable proxies for experience with COVID-19, we summarize these results as providing clear statistical evidence of a relationship: positive associations that are significant at traditional $\alpha$-levels are found in 12 of the 16 regressions presented in the first two columns, and significant negative relationships are not found.

We next direct attention to the third and fourth columns of this table, reporting regressions predicting support for long-term policy expansion. In these analyses we again find consistent positive associations between support for policy expansion and our COVID-19 impact measures, typically of a larger magnitude and of stronger significance. For example, examining the first row, we see that a 1-standard-deviation increase in county infections is associated with a 8.5pp (s.e.=0.5pp) increase in probability of support for short-term expansion of unemployment insurance and a 9.2pp (s.e.=0.5pp) increase in probability of support for government-provided healthcare. Examining all the rows of the table, we again observe that the associations found for the county-level measures are large, strongly significant, and comparable in magnitude. We again observe that the individual-specific subjective risk assessments have qualitatively comparable results, although generally smaller in magnitude and weaker in statistical significance. We again summarize these results as providing clear statistical evidence of a relationship: positive associations that are significant at traditional $\alpha$-levels are found in 14 of the 16 regressions presented in these two columns, and significant negative relationships are not found.

In the final column we present similar regressions predicting responses to the question on the overall size of the government. The presented marginal effects indicate the relevant increase in probability of supporting a bigger government estimated from our ordered-logit model. In this column we again find evidence of a positive association and generally similar patterns, although statistical support is slightly weaker: positive associations that are significant at traditional $\alpha$-levels are found in 5 of the 8 regressions presented in this column. Again, significant negative relationships are not found.

Examining the results of this table as a whole, we draw attention to two important patterns.

First, notice that associations are generally larger in magnitude when considering long-term rather than short-term responses. We believe this contrast is natural given the contrast in overall support for each policy documented in the previous section. At the time of our survey, support for short-term policy expansions was widespread and opposition to such expansions was rare. In contrast, support for long-term safety-net expansion has historically been a contentious issue in American politics, and remained so at the time of our survey. Where more substantial differences exist, there is more scope to empirically explain such differences, and potentially more scope for salient individual experiences to sway opinions.

Second, notice that both the magnitude and significance of associations are generally higher for our objective, county-level measures of COVID-19's impact as compared to our subjective, individual-level measures of risk assessments. We believe that considerations surrounding measurement error likely contribute to this difference. Note that the timing of these two groups of measures differs: county-level measures are up-to-date at the time our survey was fielded, whereas individual-level measurements were drawn from a survey deployed in April 2020, two months prior to our survey on policy preferences. Our research design intentionally features a gap in time between these two surveys to help demonstrate persistence of effects and to eliminate the scope of some potential confounds. However, a consequence of that decision may be the introduction of additional measurement error due to beliefs changing over time, and that measurement error would be predicted to attenuate these effects. More broadly, survey elicitations of probabilities are often assumed to have significant measurement error regardless of the timing of elicitation.[14] Supporting the view that this measurement error is relevant, notice that the two measures that produce nearly all insignificant results—subjective assessments of the probability of infection or death—are closely related to measures studied by Heffetz and Ishai (2021). That work documents poorly calibrated answers to questions like these, though also notes their value in predicting health-protective behaviors.

[14]Factors contributing to this measurement error can include reporting heuristics (e.g., rounding probabilities to the nearest 10%), errors in entering answers (e.g., mistyping a number), inattentive answers (e.g., entering a random number rather than considering the question), or differences in interpretation of question prompts (e.g., interpreting questions about the probability an event will happen "because of the coronavirus" to narrowly mean the probability it was caused directly by a coronavirus infection, versus interpreting it to include events directly caused by government policies that were themselves caused by the coronavirus).

While these considerations point to differences in data quality and information content across our measures, the results of Table 3 as a whole point to a clear association between one's experience with COVID-19 and one's preferences for policy expansion. Of course, the results presented thus far are mere univariate relationships and do not control for important potential confounds. We will turn to analyses with such controls included in Section 2.4.

## 2.3 Forming Primary Measures of Experience with COVID-19

A strength of the dataset we have constructed is its inclusion of a battery of alternative measures of COVID-19's impact. Different measures may capture different aspects of one's experience with COVID-19, and thus our access to a comparatively large battery of measures gives us access to a potentially broader picture than would be available considering any one measure in isolation. Despite that benefit, our measures contain undeniable overlap. To illustrate, the correlation between county-level infections and county-level deaths is 0.97, illustrating the minimal value in treating these two measures as meaningfully distinct proxies for experience.

To aggregate the information available across these measures in a systematic way, to study their interrelationships, and to reduce the dimensionality of our analysis, we conduct principal component analysis on our full battery of measures.

An interesting finding of this analysis is that these data are reasonably well explained by a mere two principal components. The first two principal components have eigenvalues of 2.50 and 1.54, both exceeding the common Kaiser-rule value of 1. These two components explain 31% and 19% of the variance in the data, respectively, and thus together capture approximately half of the information content of the full battery of measures. In contrast, of the remaining components, only one has an eigenvalue satisfying the Kaiser rule (and only modestly so at 1.09), and that component explains 14% of the variance. A full Scree plot is available in Appendix Fig. A2.

Component loadings, presented in Table 4, illustrate a sensible pattern of information extraction. Examining the loadings for component 1, we see that this component is strongly associated with the three county-level measures and nearly unassociated with the individual-level subjective risk assessments. For component 2 the opposite pattern arises, with sub-

stantial loading on the five individual-level subjective risk assessments and minimal loading on the county-level measures. Based on these findings, we interpret the first principal component as an aggregated measure of county-level impact of the pandemic. We interpret the second component as an aggregated measure of the individual's subjective risk assessment of the overall dangers of the pandemic. Moving forward, we will use the two estimated principal components as the explanatory variables in our analysis while maintaining this interpretation.

## 2.4 Preferred Regression Analysis

We now turn to the primary analysis of the paper. In this exercise, we again predict policy preferences with measures of experience with COVID-19 in an ordered-logit framework. Unlike in the previous analysis, where individual measures were used as predictors in isolation, in all analyses to follow we include both of our principal component measures as predictors. In the results reported in the first panel of Table 5 we include only those two variables as predictors, and in later panels we sequentially introduce controls.

Turning attention to the first panel of this table, we see that our aggregated measures of county-level impact and subjective risk assessments yield now-familiar results. Across these measures we consistently see a positive association between each measure of experience with COVID-19 and support for policy expansion. The estimated average marginal effects are larger when considering county-level impact than when considering subjective risk assessments and when considering long-term rather than short-term policy expansions, mirroring prior discussions at the end of Section 2.2. In both regressions examining the long-term policy preferences of primary interest, associations with both measures of COVID-19's impact are strongly statistically significant and quantitatively large.

The primary confound of concern when interpreting these simple associations arises from the possibility that early COVID-19 exposure was more intense in areas with pre-existing preference for safety-net policies. To take a stark example, New York State was especially hard-hit in the early months of the pandemic, and this state is generally left-leaning in American politics.[15] To help control for this type of potential correlation between COVID-

[15]Moving beyond this illustrative anecdote, Allcott et al. (2020) documents that US combined statistical

19 exposure and pre-existing political preferences, the second panel of Table 5 presents regressions including the controls for pre-COVID-19 self-assessed political ideology introduced in Section 1.3.2. The inclusion of this control results in smaller estimated effect sizes, confirming that the non-orthogonal assignment of COVID-19's spread across counties does indeed influence the simple associations previously reported. However, even allowing for the reduction in effect sizes that occurs with pre-pandemic ideology controlled, both measures of COVID-19's impact remain strongly statistically significant and quantitatively large predictors of support for long-term safety-net expansions.[16]

We present our preferred estimates in the bottom panel of this table. In these regressions, we include not only our controls for political ideology, but also controls for all of the large battery of demographic variables that we summarized in Section 1.3.1. Focusing attention on the policy expansions of primary interest, we find that the estimated average marginal effects of a 1-standard-deviation increase in county-level impact are a 4.1pp (s.e.=0.6pp) increase in probability of supporting long-term expansion of unemployment insurance and a 3.6pp (s.e.=0.08) increase in probability of supporting long-term expansion of government-provided healthcare. For a 1-standard-deviation increase in individual-level subjective risk assessments, the corresponding marginal effects are 2.0pp (s.e.=0.8pp) and 2.1pp (s.e.=0.8pp), respectively. As in previous analyses, somewhat smaller (although still quantitatively important and typically significant) marginal effects are found for short-term policy expansions. In the analysis of preferences for government expansion, we find significantly smaller and statistically significant positive effects.[17]

areas (CSAs) with above-median rates of COVID-19 contact were disproportionately democratic.

[16]To further explore the impact of political controls, Appendix Table A5 reproduces the preferred estimates of Table 5 while additionally controlling for party affiliation and intended voting behavior in a sequence of 5 possible election scenarios (depending on the Democrat nominee). The impact of these additional measures on estimated coefficients is modest, suggesting that the combination of our preregistered political ideology variable and demographics jointly capture most relevant information in these variables.

[17]For the interested reader, Appendix Table A6 estimates our preferred model based on sample splits on income, education, or political ideology. While the point estimates suggest some potentially interesting differences, we caution the reader against inferring too much due to the lower power available for these tests. Illustrating this low power, the full-sample estimates of Table 5 fall within the 95% confidence intervals for 25 of the 28 estimates presented in Appendix Table A6.

## 2.5 Examining Changes from Pre-Pandemic Attitudes

The results of the prior section suggest that safety-net attitudes have been influenced by experiences during the first months of the pandemic. How often does this influence result in qualitative changes of attitudes from their pre-pandemic baseline? Unfortunately, because we did not anticipate the pandemic, we did not collect the pre-pandemic measurements of our primary survey questions that would ideally address this question. However, shortly before the pandemic (in August and September, 2019) the UAS deployed a survey collecting information on support for policies related to government-provided healthcare. By comparing changes in attitudes for policy expansion between that survey and our own we may provide a partial answer to this question of interest.

As we previously described in Section 1.3.3, the available pre-pandemic measures elicit respondents' support for "Medicare for all" and for plans to allow buy-in to Medicare for any American.[18] In the analysis that follows, we restrict the sample to respondents who *opposed* either Medicare-related policy in 2019, and then estimate the impact of the COVID-19 experience measures on the probability of *supporting* safety-net expansion in 2020. We also restrict the sample to respondents who *supported* either Medicare-related policy in 2019, and then estimate the impact of our COVID-19 experience measures on the probability of *opposing* safety-net expansion in 2020. In all such regressions we include our broadest possible list of controls, as was included in Panel C of Table 5.

Results of these analyses are presented in Table 6. Directing attention first to the first column, we see that individuals who opposed "Medicare for all" in 2019 are 5.8pp (s.e.=1.7pp) more likely to support policy expansion in 2020 if they faced a 1-standard-deviation higher county-level impact; they are 0.3pp (s.e.=1.7pp) more likely to support it in 2020 if they had a 1-standard-deviation higher subjective risk assessment. These findings suggest that some positions changed from opposition to support, and that this change is more likely if the individual had a harsher experience of the pandemic. Directing attention next to the second column, we see that individuals who supported "Medicare for all" in 2019 are 3.8pp (s.e.=1.2pp) less likely to oppose policy expansion in 2020 if they faced a 1-standard-deviation higher county-level impact; they are 1.3pp (s.e.=1.0pp) less likely to oppose it in

[18] Responses to these questions are available for 2,203 of the 2,516 respondents to our primary survey.

2020 if they had a 1-standard-deviation higher subjective risk assessment. These findings suggest that some positions changed from support to opposition, and that this change was more likely if the individual had a milder experience of the pandemic. We note that only the county-level impact variables in this analysis are statistically distinguishable from zero, but also that all estimated effects reported here are reporting changes in opinion consistent with the results of the prior section.

The third and fourth columns of Table 6 conduct the same exercise but use support for Medicare buy in as the pre-pandemic reference attitude. Similar patterns arise, although with notably worse power in the regression of column 3. Recall from Section 1.3.3 that "Medicare for all" had roughly equal support and opposition, resulting in roughly equal sample sizes for the regressions of columns 1 and 2 (n = 844 and 850, respectively). In contrast, opposition to the Medicare buy-in proposal was rare, resulting in a very small sample for the regression of column 3 that is restricted to those subjects (n = 302). Because of the detrimental consequences of this small sample size for standard errors and power, we view the analysis related to "Medicare for all" as more diagnostic and thus preferable, but we include analysis of both measures for transparency and completeness.

## 2.6 Assessing Persistence

The results presented thus far establish that June 2020 attitudes towards safety-net expansion responded to individuals' experiences with COVID-19. We next turn to an assessment of the persistence of these effects. To facilitate this assessment, we redeployed our long-term safety-net expansion questions to UAS panelists in a survey deployed in January and February 2021. With these data, we can examine whether the same measures used in prior analysis (capturing county-level impact as of June 2020 and capturing subjective risk assessments in April 2020) continue to predict attitudes towards policy expansion approximately 8 months later.

While a large fraction of respondents from our first survey participated in this follow-up, some did not. In the analysis that follows, we will analyze data for the 2,260 who completed both long-term safety-net questions in both waves of our data collection.

Fig. 1 summarizes the raw distribution of responses in both waves of data collection.

As is illustrated in this figure, support for long-term expansion of unemployment insurance grew over the intervening months of the pandemic. From mid 2020 to early 2021, support for policy expansion grew from 46% to 55% and opposition to policy expansion shrank from 37% to 24%, reflecting an overall increase in the popularity of this policy. In contrast, attitudes towards expansion of government-provided healthcare were more stable. Comparison of the data for mid 2020 and early 2021 reveals very little change in the degree of either strong support or strong opposition. To the extent that aggregate changes occurred, they take the form of individuals transitioning into being neutral from a previous position of marginal support or opposition.

Table 7 presents our statistical analysis of persistence. In the first two columns we reproduce the analysis of columns 3 and 4 from Table 5. The sole difference is the imposition of the sample restriction noted above, restricting the data to the 2,260 who completed both long-term safety-net questions in both waves of our data collection. Comparing the results of these columns to those of Table 5 reveals minimal differences, suggesting little impact of selection arising from focusing attention on respondents who completed both surveys. In columns 3 and 4, the dependent variables are replaced with their early 2021 measurements. Focusing attention on our preferred results in Panel C, we see that all four estimates imply a positive and qualitatively meaningful association between experience with COVID-19's impact and support for safety-net expansion, with strong statistical significance found for the county-level variables and mixed significance for the subjective risk assessments. Quantitatively, estimated effect sizes in columns 3 and 4 of panel C are on average 74% of their value in columns 1 and 2.

Overall, these results suggest that the cross-county and cross-person differences in safety-net support that arose in the early months of the pandemic continued to exist until at least early 2021. Notably, this means that these differences persisted through the hotly contested election of 2020. Turning to the future, we can only speculate how long these differences will continue beyond the end of our data collection, and whether they will continue when[19] the spread of COVID-19 fully ends. However, we believe that the modest rate of decay of the effect across the period we studied suggests the possibility that these differences could

[19]Or, more pessimistically, if.

be relevant for years to come.

## 2.7 Assessing Attitudes Towards Funding Options

To the extent that COVID-19 exposure has influenced taste for government programs, how would the influenced individuals like to fund these changes? To assess this question, we conduct a closely analogous series of regressions to those of Panel C of Table 5, using our measures of experience with COVID-19 to predict respondents' support for short- and long-term increases in various taxes. Results are presented in Table 8.

In contrast with prior analyses, where we consistently found that our COVID-19 experience measures were strongly related to our outcomes of interest, Table 8 is most succinctly summarized by a scarcity of significant results. Across the 32 estimated coefficients, 23 estimates are statistically insignificant at the 5% $\alpha$-level. Particularly when focusing on support for the long-term changes that are our primary interest, the sole clear finding regarding support for a specific tax is that greater exposure to the impacts of COVID-19 is associated with support for a wealth tax. Turning attention to support for *any* tax increase, the estimates of the rightmost column indicate that a 1-standard-deviation higher county-level impact is associated with a 1.2pp (s.e.=0.7pp; p=0.10) lower probability of not supporting any tax increases; similarly, a 1-standard-deviation higher subjective risk assessment is associated with a 2.0pp (s.e.=1.0pp; p=0.05) lower probability of not supporting any tax increases. In summary, while there is some indication that COVID-19 experience is associated with an increased chance of supporting an increase of *some* tax, these findings are at the border of traditional statistical significance thresholds and the patterns of support for specific taxes reveal little systematic change.

Analysis of two additional variables further complements these findings. First, we conduct our standard ordered-logit regression, including political ideology and demographic controls, to predict responses to the prompt "I am personally willing to pay more in taxes." Responses options range from strongly disagree to strongly agree. We find that neither COVID-19 impact measure serves as a statistically significant predictor in this framework.[20] Second,

[20]County-level impact: marginal effect=-0.4pp; s.e.=0.5pp; p=0.43. Subj. risk assessment: marginal effect=-0.8pp; s.e.=0.7pp; p=0.22.

we conduct our standard ordered-logit regression, again including political ideology and demographic controls, to predict responses to the prompt "Do you support or oppose long-term increases in the government deficit?" We find that a 1-standard-deviation higher county-level impact is associated with a 6.3pp (s.e.=0.5pp; p<0.01) higher probability of support and that a 1-standard-deviation higher subjective risk assessment is associated with a 5.1pp (s.e.=0.8pp; p<0.01) higher probability of support.[21]

Taken together, these results suggest that respondents view deficit spending as an important part of the strategy for funding the changes to safety-net policy that COVID-19 has motivated them to consider.

# 3 Discussion

The COVID-19 pandemic placed tremendous strain on America's social safety net. For academics, this served as an opportunity for reflection on safety-net policies. Many research teams have focused their efforts on documenting the interaction of the pandemic with safety-net systems and examining how such systems might be improved for future needs (see, e.g., Bitler, Hoynes and Schanzenbach, 2020; Catherine, Miller and Sarin, 2020; Ganong, Noel and Vavra, 2020; Mitman and Rabinovich, 2021; Moffitt and Ziliak, 2020).

Just as academics have seen the pandemic as a potentially important learning opportunity, so too has the American public. In this article we have demonstrated that the American populace's support for safety-net policy has reacted in quantitatively important ways to their experiences in the early months of the pandemic. This influence on policy preferences may have already had an impact on American politics, perhaps contributing to the finding that county-level infection and death counts predicted a loss of vote-share for incumbent President Trump in the 2020 presidential election (Baccini, Brodeur and Weymouth, 2021). Should differences of the magnitude documented in our paper persist for years, they may substantially influence the nature of political support for safety-net policies during a window where consideration of significant reform appears likely.

[21] Results for short-term increases in government deficits are qualitatively similar but smaller in magnitude and weaker in significance. County-level impact: marginal effect=2.4pp; s.e.=0.5pp; p<0.01. Subj. risk assessment: marginal effect=1.0pp; s.e.=1.0pp; p=0.32.

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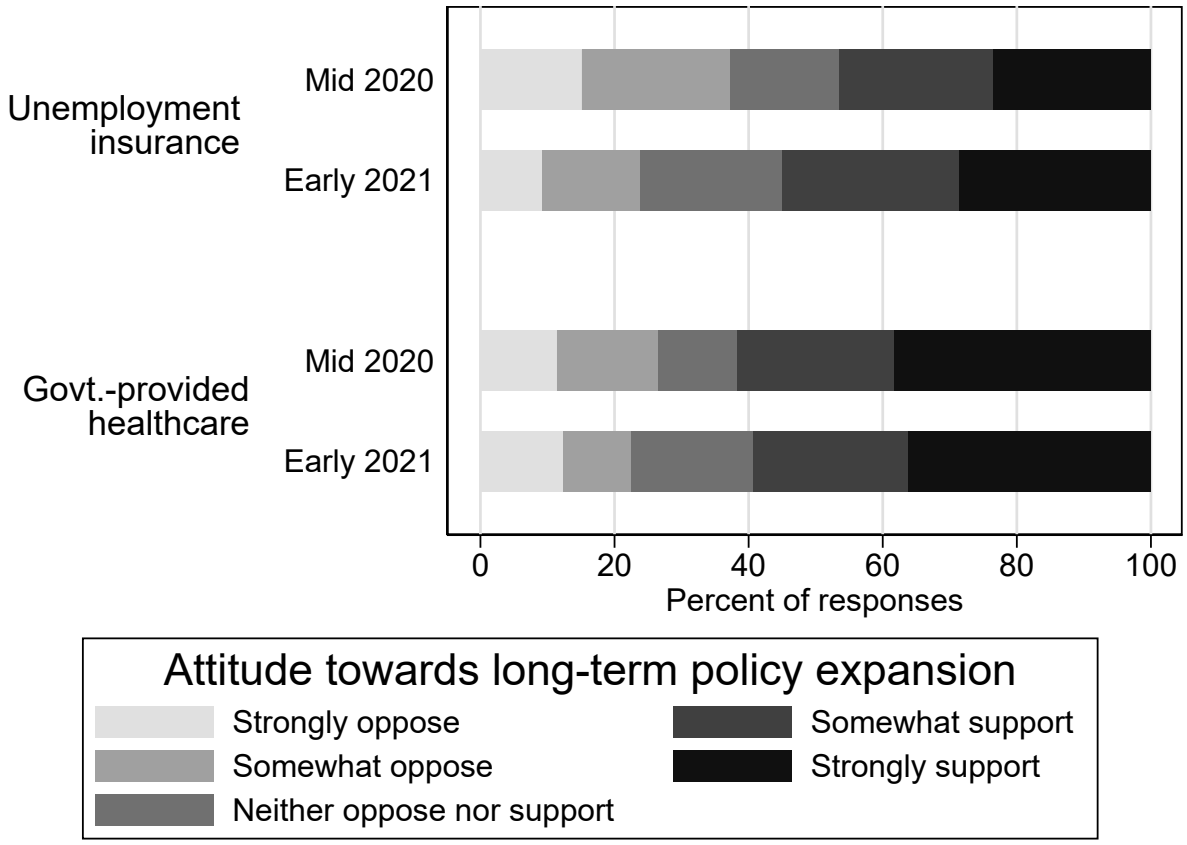
Figure 1: Evolution of Policy Support from Mid 2020 to Early 2021




Notes: This figure presents the distribution of responses to our primary policy support questions in both waves of our study. The distributions marked "mid 2020" come from our primary survey run in June, 2020. The distributions marked "early 2021" come from our follow-up survey run in January and February, 2021. The sample for this Figure is restricted to individuals who answered all four presented questions, and thus excludes individuals who did not participate in the follow-up survey. $n = 2,260$.

Table 1: Phrasing of Subjective Risk Assessments

| Measure Label | Survey Text |
|---|---|
| Subj. Risk of Infection | "On a scale of 0 to 100 percent, what is the chance that you will get the coronavirus in the next three months? If you're not sure, please give your best guess." |
| Subj. Risk of Death | "If you do get the coronavirus, what is the percent chance you will die from it? If you're not sure, please give your best guess." |
| Subj. Risk From Activities | Index formed from responses to the question "How safe or unsafe are the following actions for avoiding exposure to coronavirus?"* |
| Subj. Risk of Running Out of Money | "The coronavirus may cause economic challenges for some people regardless of whether they are actually infected. What is the percent chance you will run out of money because of the coronavirus in the next three months?" |
| Subj. Risk of Losing Job | "The coronavirus may cause economic challenges for some people regardless of whether they are actually infected. What is the percent chance that you will lose your job because of the coronavirus within the next three months?"** |

Notes: This table presents the text of the survey questions capturing subjective assessments of risks associated with the COVID-19 pandemic. All measures are drawn from UAS survey 235, deployed in April 2020.
*Actions considered were: grocery shopping; attending gatherings of more than 100 people; going to the hospital; dining in at restaurants; eating take-out meals from restaurants; visiting with relatives or friends in their home; handling packages that have been delivered; playing on playground equipment; touching door knobs, countertops, and other surfaces in your home; interacting closely with other members of your household; going outside to walk, hike, or exercise. Response options, with the numerical coding we adopted, were (1) extremely safe, (2) somewhat safe, (3) unsure, (4) somewhat unsafe, and (5) extremely unsafe. Our index is the average of these numerical values across all actions considered.
**This measure was not elicited from respondents who were not employed immediately prior to the survey. Respondents not presented with this question are therefore a mix of individuals not participating in the labor force prior to COVID-19 (thus having a 0% chance of losing their job due to COVID-19), as well as individuals who had already lost their jobs due to COVID-19 (thus having a 100% chance of losing their job due to COVID-19). We code individuals who were not employed at the time of the last quarterly demographic survey as having a 0% chance of losing their job due to COVID-19, and those who were employed at the time of the last quarterly demographic survey but who lost their job in the interim as having a 100% chance of losing their job due to COVID-19.

Table 2: Distribution of Tastes for Policy Expansions

| | Short-term expansion of... | | Long-term expansion of... | |
|---|---|---|---|---|
| | Unemployment insurance | Govt.-provided healthcare | Unemployment insurance | Govt.-provided healthcare |
| | % | % | % | % |
| 1 Strongly oppose | 4 | 4 | 15 | 11 |
| 2 Somewhat oppose | 9 | 6 | 22 | 15 |
| 3 Neither oppose nor support | 12 | 11 | 16 | 12 |
| 4 Somewhat support | 32 | 30 | 23 | 23 |
| 5 Strongly support | 43 | 48 | 24 | 38 |
| N | 2,514 | 2,509 | 2,513 | 2,514 |

Notes: This table presents summary statistics for our survey measures of support for policy expansion.

Table 3: Marginal Effects of Proxies for COVID-19's Impact on Taste for Policy Expansion

| | Short-term expansion of... | | Long-term expansion of... | | Bigger govt. |
|---|---|---|---|---|---|
| | Unemployment insurance | Govt.-provided healthcare | Unemployment insurance | Govt.-provided healthcare | |
| County infections | 3.5*** | 4.3*** | 8.5*** | 9.2*** | 2.5*** |
| | (0.4) | (0.4) | (0.5) | (0.5) | (0.2) |
| County deaths | 3.3*** | 4.4*** | 8.4*** | 9.2*** | 2.5*** |
| | (0.6) | (0.4) | (0.7) | (0.7) | (0.2) |
| County unemployment shock | 3.0*** | 3.7*** | 4.9* | 6.0** | 1.3 |
| | (1.0) | (1.0) | (2.5) | (2.3) | (0.8) |
| Subj. risk of infection | 0.2 | 1.3** | 0.9 | 1.9** | 1.0** |
| | (0.7) | (0.7) | (1.0) | (0.9) | (0.4) |
| Subj. risk of death | -0.2 | 1.0 | 1.4 | 2.4*** | 0.0 |
| | (0.7) | (0.6) | (0.9) | (0.9) | (0.4) |
| Subj. risk from activities | 2.1*** | 2.8*** | 5.7*** | 4.9*** | 2.0*** |
| | (0.7) | (0.6) | (0.9) | (0.9) | (0.4) |
| Subj. risk of running out of money | 0.8 | 2.3*** | 7.9*** | 7.7*** | 1.0** |
| | (0.8) | (0.7) | (0.9) | (0.8) | (0.4) |
| Subj. risk of losing job | 2.4*** | 1.7*** | 4.7*** | 3.5*** | 0.4 |
| | (0.7) | (0.6) | (0.9) | (0.8) | (0.4) |

Notes: This figure summarizes the results of ordered logit regressions predicting support for policy expansions using our battery of proxies for COVID-19's impact. The primary numbers reported are the estimated percentage point increase in support for the policy expansion indicated in the column. Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county in analysis with county-level predictors. Huber-White standard errors are used in analysis with individual-level predictors. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 4: Principal Component Analysis of COVID-19 Impact Measures

| | **Component 1** | **Component 2** |
|---|---|---|
| Interpretation | County-level impact | Subj. risk assessment |
| Eigenvalue | 2.50 | 1.54 |
| Explained variance | 31% | 19% |
| **Component loadings** | | |
| County infections | 0.60 | -0.09 |
| County deaths | 0.59 | -0.09 |
| County unemployment shock | 0.46 | -0.13 |
| Subj. risk of infection | 0.02 | 0.46 |
| Subj. risk of death | -0.05 | 0.52 |
| Subj. risk from activities | 0.12 | 0.42 |
| Subj. risk of running out of money | 0.19 | 0.48 |
| Subj. risk of losing job | 0.16 | 0.27 |

Notes: This table summarizes the first two principal components of our measures of exposure to the consequences of COVID-19. The top rows present our interpretation of the component (used for future labeling) as well as the associated eigenvalue and explained variance. The body of the table presents the component loadings. Notice that the first component primarily loads on the three county-level measures and that the second component primarily loads on the battery of subjective risk assessments. For a Scree plot presenting the eigenvalues of all components, see Appendix Figure A2. $n = 2,506$.

Table 5: Marginal Effects of COVID-19's Impact on Support for Policy Expansion

| | Short-term expansion of... | | Long-term expansion of... | | Bigger govt. |
|---|---|---|---|---|---|
| | Unemployment insurance | Govt.-provided healthcare | Unemployment insurance | Govt.-provided healthcare | |
| ***Panel A: No controls*** | | | | | |
| County-level impact | 4.0*** | 5.0*** | 9.6*** | 10.3*** | 2.5*** |
| | (0.5) | (0.5) | (0.5) | (0.6) | (0.2) |
| Subj. risk assessment | 0.7 | 2.2*** | 5.1*** | 5.2*** | 1.0*** |
| | (0.7) | (0.7) | (0.9) | (1.0) | (0.4) |
| N | 2,505 | 2,500 | 2,503 | 2,505 | 2,502 |
| ***Panel B: Political ideology controls*** | | | | | |
| County-level impact | 1.5*** | 2.2*** | 5.0*** | 4.6*** | 0.8*** |
| | (0.4) | (0.5) | (0.5) | (0.6) | (0.3) |
| Subj. risk assessment | 0.0 | 1.5** | 3.2*** | 2.9*** | 0.2 |
| | (0.6) | (0.6) | (0.8) | (0.9) | (0.4) |
| N | 2,504 | 2,499 | 2,502 | 2,504 | 2,501 |
| ***Panel C: Political ideology and demographic controls*** | | | | | |
| County-level impact | 2.5*** | 2.5*** | 4.1*** | 3.6*** | 0.2 |
| | (0.7) | (0.6) | (0.6) | (0.8) | (0.4) |
| Subj. risk assessment | 0.5 | 1.8*** | 2.0*** | 2.1** | 0.4 |
| | (0.7) | (0.7) | (0.8) | (0.8) | (0.5) |
| N | 2,481 | 2,476 | 2,479 | 2,481 | 2,479 |

Notes: This table summarizes the results of ordered logit regressions predicting support for policy expansions using the principal components reported in Table 4. Principal components are standardized prior to inclusion in these regressions. The primary numbers reported are the estimated percentage point increase in support for the policy expansion indicated in the column. Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 6: Changing Attitudes Towards Government-Provided Healthcare

| | "Medicare for all" | | Medicare buy in | |
|---|---|---|---|---|
| | Support expansion given prior opp. to... | Oppose expansion given prior sup. for... | Support expansion given prior opp. to... | Oppose expansion given prior sup. for... |
| County-level impact | 5.8*** | -3.8*** | 2.8 | -3.8*** |
| | (1.7) | (1.2) | (2.5) | (1.4) |
| Subj. risk assessment | 0.3 | -1.3 | 2.7 | -1.9 |
| | (1.7) | (1.0) | (2.4) | (1.2) |
| N | 844 | 850 | 302 | 1,143 |

Notes: This figure summarizes the results of logit regressions analogous to those of Panel C in Table 5. As in Panel C, all regressions include our full battery of controls for political ideology and for demographics. In each regression the sample is restricted to individuals who either opposed or supported expansions to Medicare when surveyed in 2019. The first two columns condition on opposition or support for "Medicare for all"; the second two columns condition on opposition or support for allowing individuals to buy in to the Medicare. The dependent variables were derived from the principal component analysis reported in Table 4 and standardized prior to inclusion in these regression. The primary numbers reported are the estimated percentage point increase in support or opposition for expansion of government-provided healthcare. Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 7: Persistence of COVID-19's Impact on Support for Policy Expansion

| | Mid 2020 | | Early 2021 | |
|---|---|---|---|---|
| | Unemployment insurance | Govt.-provided healthcare | Unemployment insurance | Govt.-provided healthcare |
| ***Panel A: No controls*** | | | | |
| County-level impact | 9.6*** | 10.7*** | 8.5*** | 10.2*** |
| | (0.5) | (0.7) | (0.6) | (0.6) |
| Subj. risk assessment | 5.2*** | 5.4*** | 3.7*** | 4.6*** |
| | (0.9) | (1.0) | (1.2) | (0.9) |
| N | 2,252 | 2,252 | 2,252 | 2,252 |
| ***Panel B: Political ideology controls*** | | | | |
| County-level impact | 5.1*** | 5.1*** | 3.7*** | 4.3*** |
| | (0.5) | (0.7) | (0.6) | (0.5) |
| Subj. risk assessment | 3.1*** | 3.1*** | 1.9* | 2.4*** |
| | (0.8) | (0.8) | (1.0) | (0.8) |
| N | 2,251 | 2,251 | 2,251 | 2,251 |
| ***Panel C: Political ideology and demographic controls*** | | | | |
| County-level impact | 4.5*** | 4.2*** | 4.4*** | 3.3*** |
| | (0.7) | (0.9) | (0.7) | (0.6) |
| Subj. risk assessment | 2.0** | 2.2*** | 1.1 | 1.5* |
| | (0.8) | (0.8) | (1.0) | (0.8) |
| N | 2,236 | 2,236 | 2,236 | 2,236 |

Notes: This table summarizes the results of ordered logit regressions predicting support for long-term policy expansions using the principal components reported in Table 4. The first two columns present results predicting the policy support reported in our primary survey (run in June, 2020). The next two columns present results predicting policy support reported in our follow-up survey (run in January and February, 2021). The estimation sample is restricted to respondents who answered all four policy-support questions considered in this table. Principal components are standardized prior to inclusion in these regressions. The primary numbers reported indicate estimated percentage point increase in support for the policy expansion indicated in the column. Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

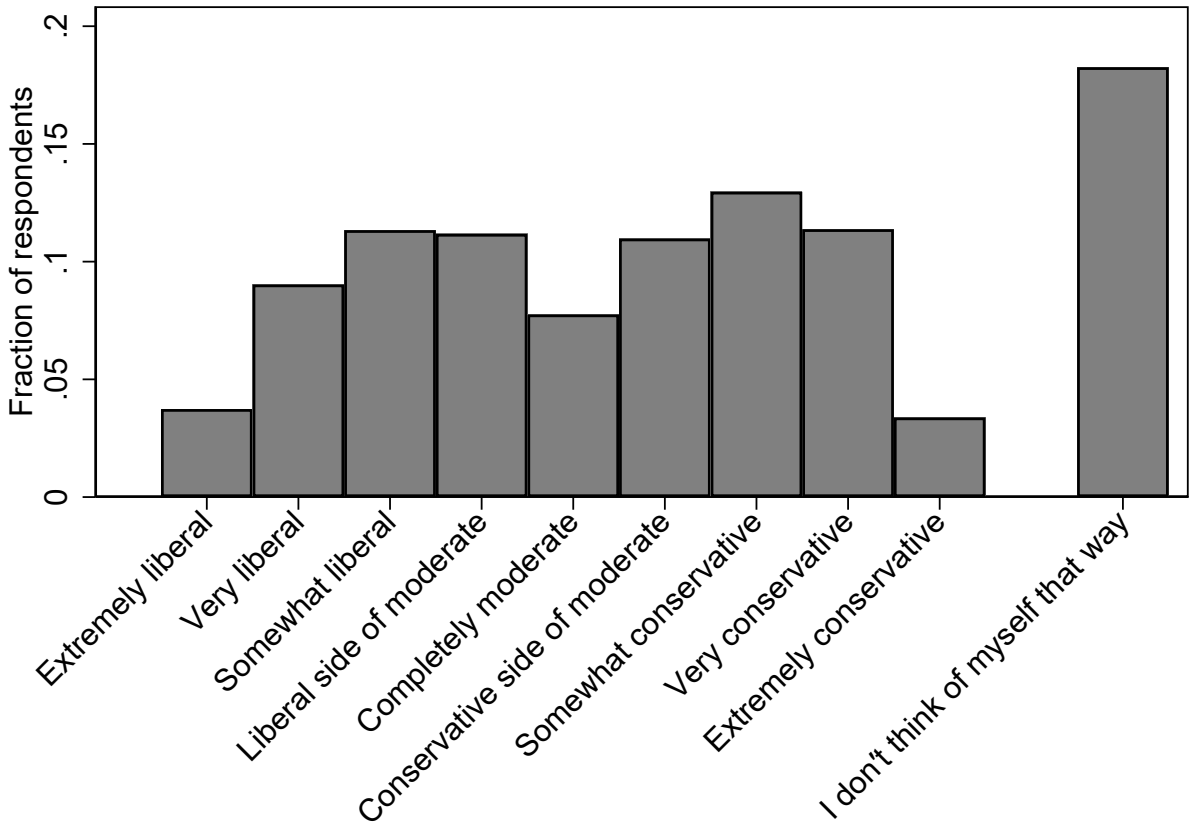
Table 8: Marginal Effects of COVID-19's Impact on Support for Tax Increases

| | Supports increase in... | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Income taxes on high-income earners | Income taxes on medium-income earners | Income taxes on low-income earners | Payroll taxes | Corporate taxes | Wealth taxes | Sales taxes | No tax increases |
| ***Panel A: Support for short-term changes (with political ideology and demographic controls)*** | | | | | | | | |
| County-level impact | 1.4 | -0.3 | -0.3 | -0.1 | 0.8 | 1.8** | -0.6 | -1.6** |
| | (0.9) | (0.7) | (0.3) | (0.4) | (0.8) | (0.9) | (0.6) | (0.7) |
| Subj. risk assessment | 0.8 | 1.2* | -0.1 | 1.1** | 2.3** | 1.4 | 1.3* | -2.2** |
| | (1.0) | (0.7) | (0.5) | (0.5) | (1.1) | (1.1) | (0.7) | (0.9) |
| N | 2,476 | 2,476 | 2,476 | 2,417 | 2,476 | 2,476 | 2,476 | 2,476 |
| ***Panel B: Support for long-term changes (with political ideology and demographic controls)*** | | | | | | | | |
| County-level impact | 1.3 | -0.3 | -0.4 | 0.2 | 1.5* | 2.8*** | -1.2** | -1.2 |
| | (1.1) | (0.4) | (0.3) | (0.5) | (0.8) | (0.9) | (0.5) | (0.7) |
| Subj. risk assessment | 0.6 | 0.1 | 0.5* | 0.0 | 1.7 | 2.3** | 0.1 | -2.0** |
| | (1.1) | (0.5) | (0.3) | (0.4) | (1.2) | (1.1) | (0.5) | (1.0) |
| N | 2,478 | 2,462 | 2,462 | 2,416 | 2,478 | 2,478 | 2,432 | 2,478 |

Notes: This table summarizes the results of logit regressions predicting support for tax increases using measures of experience with COVID-19's impacts. All regressions include our full battery of controls for political ideology and for demographics. The dependent variables were derived from the principal component analysis reported in Table 4 and standardized prior to inclusion in these regression. The primary numbers reported are the estimated average marginal effects on support for the relevant tax increase. Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

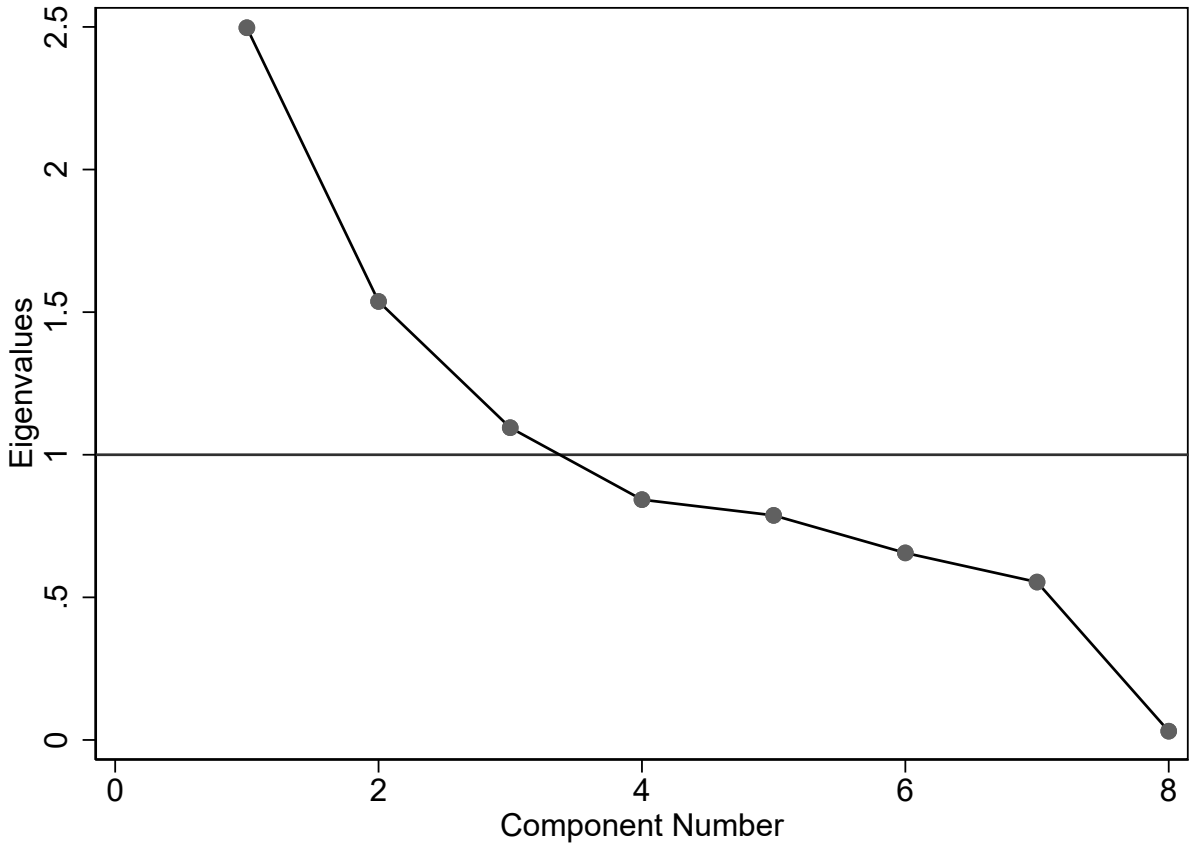
# A Appendix Analyses

Figure A1: Political Ideology of our Sample




Notes: This figure summarizes respondents' answer to the question "Regardless of your political registration or affiliation, where would you place yourself on the political spectrum from extremely liberal to extremely conservative?" Data are drawn from UAS survey 221, administered in January 2020.

Figure A2: Eigenvalues from PCA of COVID-19 Impact Measures




Notes: This figure presents a Scree plot of the Eigenvalues associated with all principal components estimated in the model from Table 4. Components with eigenvalues above the horizontal line satisfy the Kaiser criterion: these are the components with information value exceeding the average of the individual measures used as inputs.

Table A1: Distributions of Survey Responses

| | Strongly disagree | Somewhat disagree | Neither agree or disagree | Somewhat agree | Strongly agree |
|---|---|---|---|---|---|
| I believe it is important for individuals to have private savings for use in case they lose their job. | 2% | 4% | 16% | 29% | 50% |
| I believe it is important for individuals to purchase private health insurance. | 8% | 12% | 31% | 27% | 22% |
| I am personally willing to pay more in taxes. | 41% | 16% | 19% | 19% | 5% |

| **Do you support or oppose...** | Strongly oppose | Somewhat oppose | Neither oppose nor support | Somewhat support | Strongly support |
|---|---|---|---|---|---|
| Do you support or oppose... ...short-term increases in the government deficit to support costs associated with the COVID-19 pandemic? | 8% | 13% | 22% | 36% | 20% |
| ...long-term increases in the government deficit? | 25% | 26% | 24% | 17% | 8% |

| **Do you support a bigger or smaller government?** | |
|---|---|
| 1 I support a smaller government | 49% |
| 2 I think the current government is about the right size | 39% |
| 3 I support a bigger government | 12% |

Notes: This table presents the question text and response distributions for auxilliary questions included in our survey.

Table A2: Support for Tax Expansions

| | **Yes** |
|---|---|
| **To help offset costs associated with the COVID-19 pandemic, I support short term increases in...** | |
| ...Income taxes on high income earners | 52% |
| ...Income taxes on medium income earners | 11% |
| ...Income taxes on low income earners | 4% |
| ...Payroll taxes | 6% |
| ...Corporate taxes | 43% |
| ...Wealth taxes | 43% |
| ...Sales taxes | 11% |
| I don't support short-term tax increases | 36% |
| **I support long term increases in...** | |
| ...Income taxes on high income earners | 47% |
| ...Income taxes on medium income earners | 6% |
| ...Income taxes on low income earners | 3% |
| ...Payroll taxes | 5% |
| ...Corporate taxes | 41% |
| ...Wealth taxes | 39% |
| ...Sales taxes | 7% |
| I don't support long-term tax increases | 40% |

Notes: This table presents the question text and response distributions for our measures of tax preferences. Responses were elicited in multiple-choice format. Conditional on not selecting the final option indicating a lack of support for any tax increase, respondents could indicate support for as many tax increases as they would like.

Table A3: Demographics of our Sample: Part 1

| | Survey Completion Status | | | |
|---|---|---|---|---|
| | **Incomplete** | **Complete** | **Total** | **P-value** |
| **Gender** | | | | |
| Female | 56.8% | 56.3% | 56.4% | 0.806 |
| Male | 43.2% | 43.7% | 43.6% | |
| **Marital status** | | | | |
| Single | 43.9% | 43.8% | 43.9% | 0.959 |
| Married | 56.1% | 56.2% | 56.1% | |
| **Currently Working** | | | | |
| No | 40.4% | 47.3% | 45.6% | 0.001 |
| Yes | 59.6% | 52.7% | 54.4% | |
| **US Citizen** | | | | |
| No | 2.4% | 2.4% | 2.4% | 0.970 |
| Yes | 97.6% | 97.6% | 97.6% | |
| **Spanish/Hispanic/Latino** | | | | |
| No | 83.6% | 85.5% | 85.1% | 0.178 |
| Yes | 16.4% | 14.5% | 14.9% | |
| **Race** | | | | |
| White Only | 77.4% | 80.0% | 79.4% | 0.379 |
| Black Only | 8.0% | 8.2% | 8.1% | |
| Am. Indian or Alaska Native only | 1.8% | 1.8% | 1.8% | |
| Asian Only | 5.4% | 4.2% | 4.5% | |
| Hawaiian/Pacific Islander Only | 1.0% | 0.7% | 0.8% | |
| Mixed | 6.4% | 5.0% | 5.3% | |

Notes: This table presents demographic summary statistics for our sample. The first column presents the fraction of respondents in each demographic category among completed survey responses. The second column presents results for UAS participants who were invited to the study but did not complete it. The third column presents results for all invitees. The final column presents p-values for chi-squared tests of differences in the demographic variable by completion status, serving as a test for selection into the sample.

Table A4: Demographics of our Sample: Part 2

| | Survey Completion Status | | | |
|---|---|---|---|---|
| | **Incomplete** | **Complete** | **Total** | **P-value** |
| **Education** | | | | |
| <12th Grade | 5.0% | 5.1% | 5.1% | 0.702 |
| High School Graduate | 17.5% | 17.2% | 17.3% | |
| Some College | 24.7% | 22.7% | 23.2% | |
| Associate Degree | 11.8% | 13.7% | 13.2% | |
| Bachelor's Degree | 24.0% | 24.3% | 24.2% | |
| Master's Degree + | 17.0% | 17.0% | 17.0% | |
| **Income** | | | | |
| <$10,000 | 4.5% | 6.4% | 5.9% | 0.286 |
| $10,000 - $24,999 | 13.4% | 13.2% | 13.3% | |
| $25,000 - $49,999 | 23.0% | 21.6% | 22.0% | |
| $50,000 - $74,999 | 18.0% | 19.9% | 19.5% | |
| $75,000 - $99,999 | 14.8% | 13.7% | 14.0% | |
| $100,000 + | 26.2% | 25.1% | 25.4% | |
| **Age** | | | | |
| 18-29 | 10.8% | 8.5% | 9.0% | 0.013 |
| 30-39 | 19.5% | 16.4% | 17.2% | |
| 40-49 | 18.6% | 17.3% | 17.6% | |
| 50-59 | 17.9% | 19.8% | 19.3% | |
| 60+ | 33.2% | 38.1% | 36.9% | |

Notes: This table presents demographic summary statistics for our sample. The first column presents the fraction of respondents in each demographic category among completed survey responses. The second column presents results for UAS participants who were invited to the study but did not complete it. The third column presents results for all invitees. The final column presents p-values for chi-squared tests of differences in the demographic variable by completion status, serving as a test for selection into the sample.

Table A5: Panel C of Table 5 With Additional Political Controls

| | Short-term expansion of... | | Long-term expansion of... | | Bigger govt. |
|---|---|---|---|---|---|
| | Unemployment insurance | Govt.-provided healthcare | Unemployment insurance | Govt.-provided healthcare | |
| County-level impact | 2.4*** | 2.1*** | 3.5*** | 2.6*** | -0.0 |
| | (0.6) | (0.7) | (0.7) | (1.0) | (0.4) |
| Subj. risk assessment | 0.8 | 2.0*** | 2.0*** | 2.0*** | 0.5 |
| | (0.8) | (0.7) | (0.8) | (0.7) | (0.5) |
| N | 2,419 | 2,414 | 2,417 | 2,419 | 2,417 |

Notes: This table presents regression results building on those in Panel C of Table 5. The models estimated in this table include additional political-preference controls as well as the political ideology and demographic controls previously included in Panel C. The new measures come from UAS survey 221, fielded in January 2020. The measures we include are indicators for the individual's party affiliation (options: Democrats, Republicans, Independents, Libertarians, Green Party, "some other party," or "not aligned with any political party.") and indicators for intended voting behavior in a variety of potential elections pitting incumbent President Trump against potential Democrat nominees (candidates: Biden, Warren, Buttigieg, Sanders, or Klobuchar). Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table A6: Marginal Effects of COVID-19's Impact on Support for Policy Expansion: Sample Splits

| | Income | | Education | | Ideology | | |
|---|---|---|---|---|---|---|---|
| | $< \$50{,}000$ | $\geq \$50{,}000$ | < Associate Degree | ≥ Associate Degree | Liberal | Neutral/neither | Conservative |
| ***Panel A: Long-term expansion of unemployment insurance*** | | | | | | | |
| County-level impact | 4.7*** | 3.5*** | 3.6*** | 4.4*** | 4.0** | 6.6*** | 3.4*** |
| | (1.5) | (0.8) | (1.2) | (0.7) | (1.6) | (1.9) | (1.3) |
| Subj. risk assessment | 1.8 | 2.7** | 3.5*** | 0.4 | 1.5 | 1.2 | 3.2*** |
| | (1.1) | (1.2) | (1.1) | (1.0) | (1.5) | (1.6) | (1.2) |
| N | 1,019 | 1,460 | 1,109 | 1,370 | 868 | 643 | 968 |
| ***Panel B: Long-term expansion of govt.-provided healthcare*** | | | | | | | |
| County-level impact | 3.0*** | 4.4*** | 3.0*** | 4.1*** | 1.5** | 7.2*** | 4.8*** |
| | (0.9) | (1.0) | (1.0) | (1.1) | (0.7) | (1.8) | (1.8) |
| Subj. risk assessment | 1.8* | 2.4* | 3.8*** | 0.3 | 1.2 | -0.6 | 5.0*** |
| | (1.1) | (1.3) | (1.1) | (1.4) | (1.1) | (1.5) | (1.3) |
| N | 1,020 | 1,461 | 1,111 | 1,370 | 870 | 643 | 968 |

Notes: This table presents variants of our preferred estimates from Table 5. All models in Panel A predict support for long-term expansion of unemployment insurance, using the same specification as column 3 of Panel C of Table 5. All models in Panel B predict support for long-term expansion of government-provided healthcare, using the same specification as column 4 of Panel C of Table 5. Each model is estimated imposing the sample restriction indicated in the header of the table, splitting the population based on income, education, or political ideology. Standard errors for the estimated marginal effects, presented in parentheses, are calculated by the delta method. Standard errors for the primitive ordered logit coefficients, used as inputs for the application of the delta method, are clustered by county. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.