Unexpected effects of national social insurance on support for county-level public health policies

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ABSTRACT

In the U.S., the generosity of supplementary federal unemployment insurance (UI) was a controversial issue throughout the early part of the COVID-19 pandemic. The debate focused mostly on worries about economic disincentives for workers. However, federal UI may also have undermined support for local-level pandemic mitigation strategies. We quantify the effect of federal UI on the trade-offs that individuals are willing to make with respect to county-level pandemic policies. We use choice experiments from an online survey, and both model, and correct for, systematic response/non-response propensities. When respondents are asked to assume that federal UI will be zero, they tend to be averse to losses in average household income but favorably disposed toward increased unemployment. With positive federal UI payments, however, respondents become more willing to accept losses in average household income but view increased unemployment less favorably. The reversal with respect to losses in average household income is driven by younger, white, non-college and lower-income respondents. The reversal with respect to unemployment is driven by middle-aged and conservative respondents. Our findings demonstrate policy-relevant heterogeneity in support for county-level health policies as a function of national-level social safety net policy.

JEL classifications: H5, H73, J65, I18

Keywords: Social insurance; public health; pandemic policy; political economy; policy interactions; choice experiments

1 Introduction

The U.S. suffered major public health disruptions during the COVID-19 pandemic. Future episodes of pandemic disease are likely to be a continuing threat, especially as humans continue to encroach on wildlife habitats (Wilkinson et al., 2018; Grange et al., 2021). While the specific risks are unknown, future pandemics will again force societies to choose the extent to which they are willing to sacrifice economic and social activity for public health.

During the COVID-19 pandemic, the U.S. government delegated most policy decisions about non-pharmaceutical interventions (NPIs)—such as mask-wearing and social-distancing—to state, county and local-level authorities. The federal government, however, made important national-level decisions to enhance the overall social safety net, primarily by supplementing state-level unemployment insurance benefits with additional federal unemployment insurance (UI) benefits. When pandemic policies are instituted at different levels of government, however, it is important to consider the potential interactions between these policies.

Using a survey-based choice experiment with nearly 1,000 U.S. respondents in California, Oregon, and Washington State, we focus on how the generosity of the federal-level social safety net—namely, the availability of supplementary federal UI—shapes preferences for pandemic mitigation policies implemented at the county level.[1] In the absence of federal UI, respondents are significantly *averse to* net losses in county-level average household incomes, but they are *in favor of* increases in unemployment in their county caused by county-level pandemic mitigation policies. When federal UI is provided, however, respondents are *in favor of* net losses in county-level average household incomes, on average. They also become *indifferent to* increases in county-level unemployment, or even *opposed to* increases in county-level unemployment if we focus on the actual levels of federal UI that had most recently been experienced at the time of our survey. Thus, paradoxically, the presence of generous federal UI may have undermined support for local

[1]Best practices for survey-based stated-preference research are reviewed in Johnston et al. (2017). The federal UI program examined in this study is more formally known as Federal Pandemic Unemployment Compensation.

pandemic mitigation policies that could reasonably be expected to increase unemployment. Our results are consistent with the more-general notion that the federal policy environment can potentially affect public support for county-level policies more broadly.

One plausible explanation for this federal UI-induced change in county-level policy preferences might be a relatively widespread fear that unemployment benefits create perverse economic incentives, especially if federal UI, in numerous cases, exceeds the lost wages it is meant to replace. This view has had vocal adherents in the U.S. Congress. Senator Ron Johnson (R-WI) called the original $600-per-week unemployment benefits a "perverse incentive to keep people out of the economy," citing the proportion of recipients who earned more from unemployment insurance than they lost in wages (UPFRONT, 2020). House Minority Leader Kevin McCarthy (R-CA) made similar remarks in July 2020, saying, "We made a mistake when we overpaid on unemployment insurance where now it's hard for people to come back to work because they're making more on unemployment than they can working" (Stein and Werner, 2020). Indeed, concerns about the disincentive effects of the continuation of $300-per-week federal UI benefits became even more apparent in May of 2021, as reported by Sainato (2021) and Zeleny and Luhby (2021). Republican-led states began seeking to end these benefits earlier than the Biden administration had planned, citing workforce shortages.

Early evidence, such as that reported in Bartik et al. (2020), Marinescu et al. (2021) and Dube (2021), contrasts with the claims of Johnson and McCarthy by suggesting that high levels of federal UI during the first wave of the pandemic did not contribute to increased unemployment. Political commentator Sean Hannity raised a different objection in an on-air interview with then-Treasury Secretary Steve Mnuchin in March 2020, citing a widespread feeling of aggrievement, rather than the threat of distortionary incentives. Apparently speaking for his 3+ million *Hannity* viewers, the host explained, "This idea that you're going to make more money unemployed, that angers my audience. That angers me too. Why couldn't somebody just have to show a pay stub, and that's the money you're going to get?" (Concha, 2020). In short, Hannity argues that a segment of the

US population prefers that those who experience unemployment *not* be compensated beyond their previous earnings.

We find evidence consistent with Hannity's claim, especially for respondents with lower incomes and those who are politically conservative or moderate, and we show that these preferences have a significant effect on support for pandemic mitigation policies. We hypothesize that respondents infer that increased unemployment reduces social contact, limiting the spread of COVID-19 and making increased unemployment a desirable feature of a pandemic mitigation policy. In the presence of federal UI, however, this effect is offset by concerns about economic incentives or preferences over distributional fairness, and increased unemployment is, on net, an insignificant or even negative factor in respondents' decision-making. Conversely, larger decreases in county average household incomes due to unemployment—expected to be an unambiguous economic "bad"—may instead signal to concerned respondents that, even with federal UI payments, unemployment is appropriately burdensome.[2]

A growing body of survey-based choice experiments has already revealed some of the tradeoffs people are willing to make with respect to COVID-19 pandemic policies. Some of the key features of these studies are summarized in Table 1. Four studies were fielded during the so-called "first wave" of the pandemic—a Dutch study (Chorus et al., 2020), a French study (Blayac et al., 2021), a study for the entire U.S. (Reed et al., 2020), and a study just in the state of Missouri (Wilson et al., 2020).[3] These surveys may have been fielded too early to capture the effects of federal-level policies on local preferences. The actual dates for a fifth study (described in Genie et al. (2020) and introduced as a protocol for an upcoming survey) seem not yet to have been published. Our survey was fielded between January 13 and February 16, 2021, in the latter part of the so-called "third wave" of the pandemic. During this period, future levels for U.S. Federal Pandemic

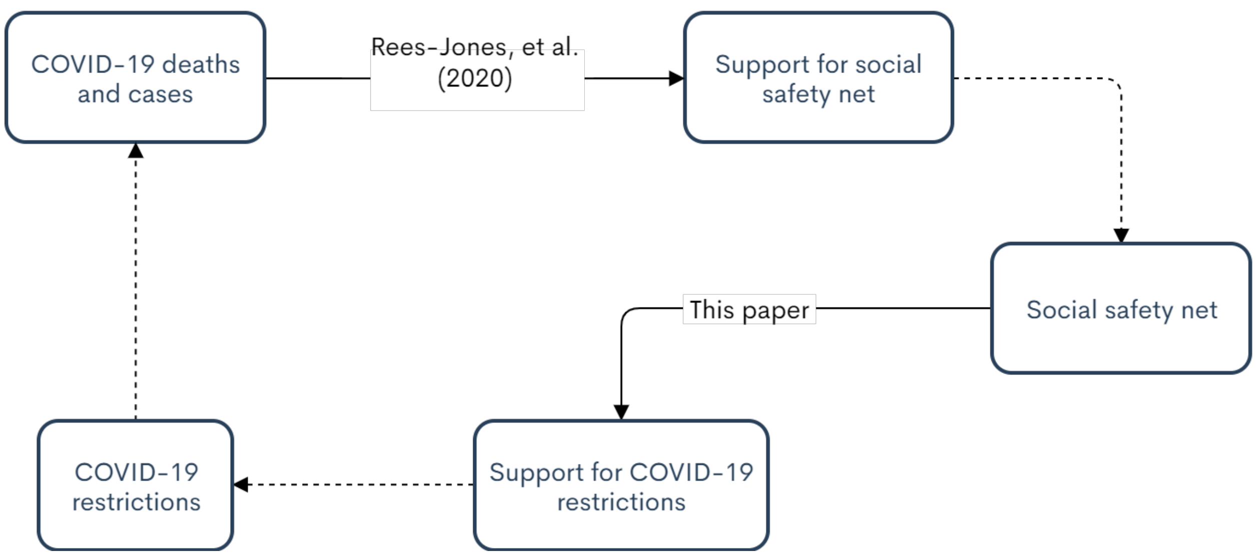
[2]When federal UI is not present, we find that respondents are on average strongly averse to policies that reduce average household income in their counties, as expected.

[3]For the very early Reed et al. (2020) survey, fielded between May 9 and May 20 of 2020, the authors warn that "If we had taken the time to follow standard, good-practice procedures, we almost certainly would have ended up with a different instrument."

Unemployment Compensation were still uncertain, allowing us to vary the levels of federal UI in our choice scenarios without straining credulity.[4]

Our work also complements Rees-Jones et al. (2020), who document increased support for federal safety-net programs, including federal UI, in response to the severity of COVID-19's negative effects on local (county-level) health and employment. We identify a related link in the causal chain: a strengthened federal social safety net reduces support for county-level NPI pandemic policies that (a) lead to unemployment and thereby (b) increase take-up rates for the federal UI that provides this social safety net. Figure 1 illustrates the different emphases of Rees-Jones et al. (2020) and the present paper.

Figure 1: Complementarity with Rees-Jones et al. (2020) study



Finally, our work contributes to the literature examining the relation between political partisanship and attitudes toward NPI policies and associated behaviors. Reed et al. (2020) note that on simple questions of concern or support, respondents seem to answer ideologically. In the context

[4]More-detailed summaries of these closely related studies are included in Online Appendix A, where we also describe a pre-COVID-19 study by Cook et al. (2018) in Singapore in the wake of the previous 2003 SARS-CoV and 2009 H1N1 influenza outbreaks.

Table 1: Other Covid-19 choice-experiment studies

	Chorus et al. (2020)	Blayac et al. (2021)	Reed et al. (2020)	Wilson et al. (2020)	Genie et al. (2021)
Health attributes	Number of deaths, physical injuries, mental injuries	None	Cases	Risk of infection	Excess deaths, infections, postponed non-pandemic medical care
Economic attributes	Lost income, taxes	Financial compensation	Percent below poverty line, time until economy recovers	Lost income	Job losses, ability to buy things
Other attributes	Educational disadvantages, healthcare worker stress	Duration, masks, restrictions on transportation, vacations, and bars/restaurants	Restrictions on non-essential business	Restrictions on gatherings, social venues, and schools	Policy duration, severity of lockdown (four tiers)
Method	Latent class (3 classes)	Heterogeneity by age, gender vulnerability	Latent class (4 classes)	Latent class (4 classes)	Mixed logit; heterogeneity by 5 core "moral foundations"
Region	Netherlands	France	whole U.S.	Missouri only	United Kingdom
N	1009	1154	5953	2428	4021

of their choice experiments, however, ideology "plays a more complicated role." They find that the preferences of self-identified Republicans and Democrats are more similar to each other than to the preferences of Independents. In related work, Allcott et al. (2020) use human mobility data (SafeGraph cell-phone GPS information) to show that people in areas with more Republicans are less likely to practice social-distancing, and Kahane (2021) uses survey data at the county level to detect lower mask-wearing in counties where Trump was strongly supported in the 2016 Presidential election.

In contrast to other pandemic policy choice experiments in the literature, we invited respondents to consider different baseline pandemic conditions with varied levels of expected cases and deaths, both without and with each pandemic policy in place. Furthermore, since state-level Health Authorities have tended to publish daily statistics on cases and deaths for each county, we express the numbers of cases and deaths in our choice scenarios in absolute numbers, in comparison to the population of the respondent's own county.[5] We also express the economic costs of each pandemic policy in terms of both the expected unemployment rate in the respondent's county and the average number of dollars lost per household. For this paper, one key policy attribute is the presence of federal UI, which drives a wedge between these two types of pandemic cost measures. Finally, we break out ten specific categories of restrictions to permit our respondents the opportunity to differentiate among policies that are relatively more or less restrictive for different types of activities.[6]

Our study also differs from earlier pandemic policy choice experiments in its attention to the possibility of systematic selection of potential survey respondents into the estimating sample. In the absence of sufficient lead time for researchers to develop, submit, and hear back about proposals for significant research funding from the usual sources, most existing studies have used modestly

[5]This required regular updating of actual cases and deaths for each county, based on daily county-level cases and deaths at https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/, and javascript code within the survey software to take randomized mixes of cases and deaths in our externally survey design, normalized for a population of 50,000, and scale these to the population of the county selected by each anonymous respondent.

[6]Preferences over these ten categories of restrictions will be the focus of a future paper. Here, these differences in restrictions are included merely as controls.

priced sampling strategies. Convenience samples can be inexpensive, but they present a significant risk of being systematically selected. Earlier survey-based studies of COVID-19 policy preferences have commented on the representativeness of their estimating samples, but none has proposed a strategy to correct for systematic selection into the estimating sample. In contrast, we model and correct for selection bias, in a systematic fashion.

2 Data

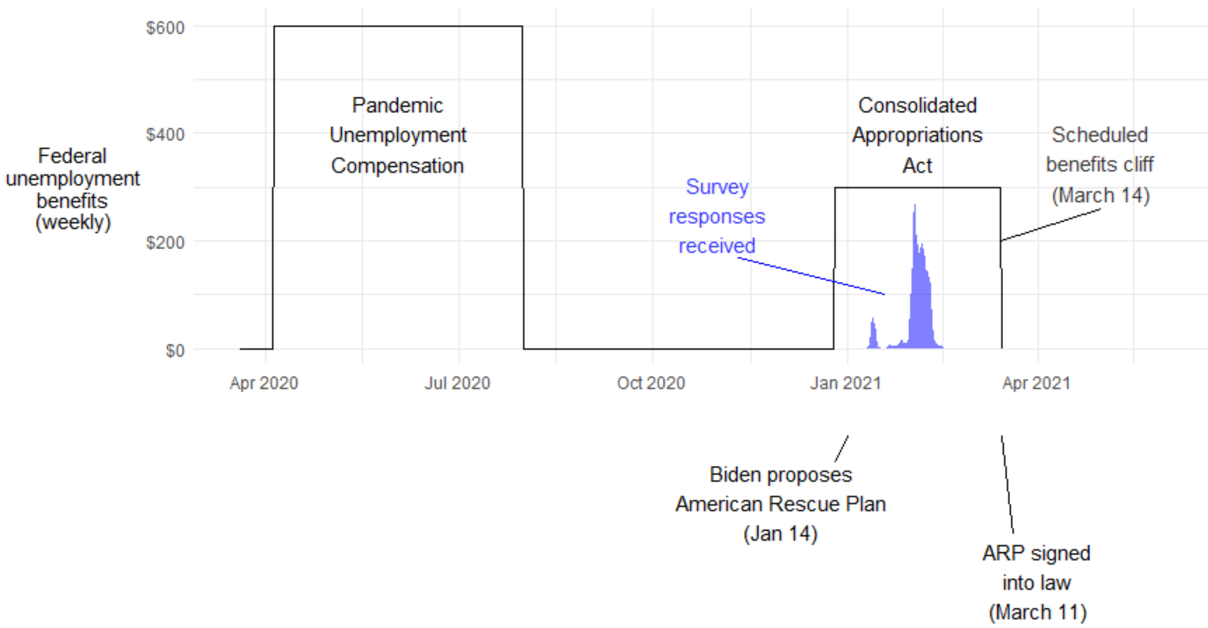
We use a survey instrument distributed via Qualtrics to residents of the west-coast U.S. states of California, Oregon and Washington during January 13–February 16, 2021. This timing is important for the present study because it coincides with a period of uncertainty about the future level of supplementary federal UI benefits. Figure 2 describes the federal policy context under which respondents participated in our choice experiments.[7]

We over-sampled the states of Oregon and Washington, relative to California, because of California's much larger population (and weight the resulting subsamples during estimation, to compensate). We requested quotas that matched the marginal distributions within each state of gender, age (18 to 34 years, 35 to 54 years, and 55 years and older), race (White, Black, Asian and other), and income (less than $50,000, $50,000 to $99,000 and $100,000 or more). We also asked about the respondent's zip code, with the explanation that we would need to assess whether eligible participants were broadly geographically representative. We asked these screening questions at the start of the survey to prevent wasted effort by respondents who turn out to be ineligible (due to their quotas having been reached). Respondents who were unwilling, for any reason, to provide data on their age or race bracket were automatically excused from the study.[8]

[7] The process of survey development is outlined in Online Appendix B. Our survey was approved under University of Oregon IRB Protocol Number 07022020.002. A codebook for the survey (including the wording of each question in generic format) may be viewed at pages.uoregon.edu/cameron/UO_COVID_survey.pdf (172 pp.)

[8] A reluctance to divulge such screening information could be systematically related to concerns about privacy in the general population, but regular participants in consumer surveys are presumably accustomed to providing this basic information.

Figure 2: Timeline of survey responses relative to the American Rescue Plan of 2021



2.1 Sample selection and response propensities

If a potential respondent was confirmed to be eligible to participate in the survey, they were then introduced to the topic of the survey during the preamble to the formal consent-to-participate question. While our 70 percent response rate is respectable, a critically important feature of our study is our ability to formally model the individual decisions of 993 of our 1,412 eligible respondents to complete our particular survey. In addition to the sociodemographic screening questions described above, the survey platform passively collects information about the operating system upon which the respondent began taking the survey, and the date and time when they initiated their session. The details of the sample-selection modeling for this particular survey forms the subject matter of Mitchell-Nelson and Cameron (2021), so we merely outline the process here.

The zip code data are especially valuable because they allow us to merge our sample with external data at the zip code level for all eligible panelists, and to associate with each eligible non-respondent some county-level variables that are otherwise available only for completed responses according to the state and county that each respondent selected in the body of our survey. Non-respondents who opted out of the survey after the screening process provided no information concerning the pandemic policy preferences that are the topic of our survey study. However, the screening questions for eligibility allow us to build a wide array of variables that explain systematic differences in propensities to respond to a survey about COVID-19 pandemic policies. Especially useful is our ability to link every eligible respondent to prevailing rates of COVID-19 cases and deaths in their *own* county over the duration of the pandemic leading up to our survey.

Candidate explanatory variables for the selection model include the sets of category indicators for individual screening sociodemographics as well as the device type and timing of the session. We also assemble a large inventory of potential explanatory variables at the zip code or county level for our sample-selection model. These candidate explanatory variables include zip code or county proportions for age groups, income groups, racial and ethnic groups, industries, rural/urban mix, ownership of computers and type of internet access. Relating to the local area's history with the current pandemic, we include county-level COVID-19 cases and deaths, by month, since the beginning of the pandemic. These variables include days since the first COVID-19 death in the potential respondent's county, and cases and deaths in the potential respondent's own county in the four weeks leading up to the date when they started the survey, which were duly quoted to actual respondents in the body of the survey. The variables that are retained in the final binary-outcome specification for selection are identified by LASSO methods. Online Appendix C reproduces the descriptive statistics for these retained variables and the parameter estimates for our selection model.

Here, we note only that there is considerable heterogeneity in the predicted response propensities from our selection model. In Figure 3 the distribution shown with a black outline describes the de-meaned fitted response propensities for the entire set of 1,412 eligible survey subjects. Figure 3 also shows the separate distributions of this variable for non-respondents and respondents. As expected, response propensities are lower for people who do not complete the survey and higher for those who do, but there is considerable overlap in the two distributions. We allow all of the estimated parameters in our subsequent policy-preference models to vary systematically with the de-meaned fitted response propensities for the estimating sample (consisting of the 993 actual respondents). Then we can simulate the parameter estimates that would obtain had everyone's de-meaned response propensity been exactly zero—that is, if everyone in the estimating sample shared the mean response propensity in the eligible population. Selection-correction, in general, seeks to identify preferences under counterfactual conditions where everyone from the population of interest is equally likely to show up in the estimating sample.[9]

2.2 Estimating sample for policy choice models

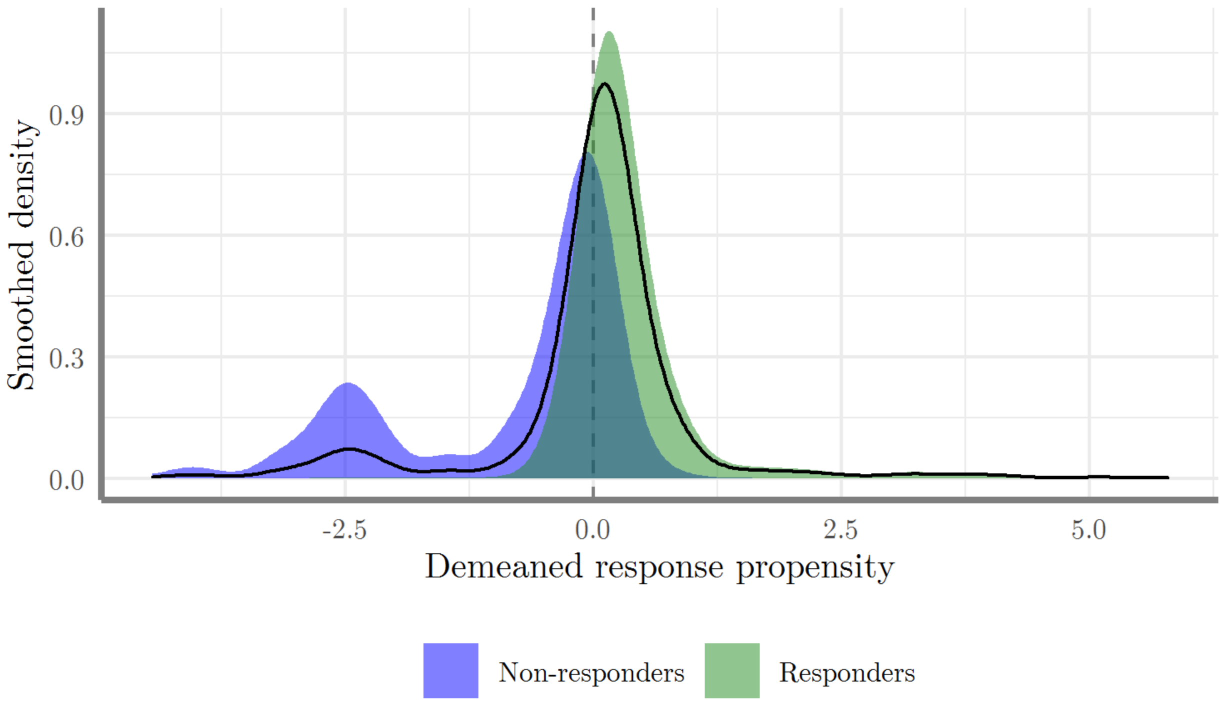
Our estimating sample for policy choice modeling consists of a total of 1986 choices by 993 respondents. Each policy scenario describes the baseline numbers of cases and deaths in the absence of any policy, the reductions in cases and deaths that would occur under each offered policy, the economic consequences of each policy for the respondent's county in terms of unemployment rates and the resulting average cost per household, and a set of restrictions (described in terms of levels 0 through 3) on each of ten different categories of activities or businesses.[10] Each county-level policy-choice scenario was presented in the context of a specific level of federal UI payments that would be available whether or not the county-level policy was implemented.[11]

[9]We acknowledge, of course, that this two-step correction process utilizes estimated quantities as interaction terms in the second step without correcting the parameter standard errors for the extra noise that the first stage introduces into the model. As of yet, however, there is still no packaged full-information maximum likelihood method for estimating a conditional logit outcome model jointly with a binary-outcome selection model. Conventional Heckman-like models, such as those employing inverse Mills ratio terms, are inappropriate when the error in the "outcome" equation is not normally distributed.

[10]D-efficient designs were employed for the ten sets of restrictions on activities and businesses, albeit in two batches of five because ten attributes with four levels each proved too time-consuming to implement at the scale required for our project.

[11]Policies were also described as remaining in effect for a specified duration. These durations were randomly assigned to be one or two months (40% each) or three months (20%). After standardizing total cases and total deaths with and without each policy to their per-month equivalents for our analysis, policy duration has no statistically discernible effect on respondents' propensities to choose any policy over the status quo, so we do not include policy duration in any of the models reported in this paper.

Figure 3: Fitted response propensities



LASSO model; n responders = 993; n non-responders = 419. Black line gives density for full sample (n = 1412).

Summary statistics for the key attributes of policy scenarios for this paper are presented in Table 2. The distributions of cases and premature deaths avoided were loosely conditioned on the severity of these restrictions, as were the average household costs and unemployment, to preserve plausibility of the policy scenarios.[12]

In this paper we present estimates based on just the first two dichotomous choices made by each respondent, since binary-choice formats are more incentive-compatible (Carson and Groves, 2007). Future work on choice experiment methodology will take advantage of the subsequent three-way and conditional two-way choices.[13]

2.3 Estimating specification

Conditional logit choice models based on random utility models (RUM) are by now familiar and algorithms for estimation are readily available in most standard econometric software packages. Thus we will not repeat here the basics of the RUM approach to preference estimation. The key part of the specification is the particular functional form used for indirect utility function under each alternative. The respondent is assumed to choose the alternative that yields the highest level of utility, up to an error term that is unobservable by the researcher.

Our model is based on the indirect utility of respondent i under policy A. In contrast to the usual specification in a random utility model, however, the cost of the policy is expressed in terms

[12] Randomizations were generated outside the survey and one instance from the inventory was dynamically selected at random, without replacement, for each respondent. The externally randomized attributes were scaled to a standardized county population of 50,000 and then scaled "on the fly" during the survey to match the population of the respondent's own county. This level of aggregation seemed appropriate because much public data on actual cases and deaths has been quoted at the county level. Extensive details about the randomized design of our policy choice scenarios are contained in the Online Supplementary Materials, available at http://pages.uoregon.edu/cameron/UO_COVID_description_of_randomizations.pdf.

[13] For each respondent, six different NPI pandemic policies (A–F) are described, individually or in pairs, with "No restrictions" (N) always included as the status quo alternative, where everyone merely takes whatever precautions they think are appropriate. Respondents' policy choice scenarios were presented first as two consecutive dichotomous choices: AN, BN. Only these two choices are employed in this paper. Policies C–F were combined into three-alternative choices with conditional pairwise follow-up choices, so we save their more-complex analysis for future research. One image of a choice set is provided in Online Appendix D, along with an example of the pop-up reminders about how to interpret the indicated level of restrictions on each activity or business.

Table 2: Descriptive statistics for featured variables in our randomized design

	Mean	SD	Min	Max
Federal policy context (does not vary by county-level policy):				
Federal UI ($/week)	193.15	138.9	0	400
Federal UI = 0	0.20			
Federal UI = 100	0.21			
Federal UI = 200	0.20			
Federal UI = 300	0.21			
Federal UI = 400	0.17			
Without policy (status quo):				
Unempl rate, federal UI = 0	4.34	1.65	2.3	10.4
Unempl rate, federal UI > 0	4.26	1.87	2.1	19.8
Avg. hhld cost/mo	0	0	0	0
Absolute ’00s cases/mo/50,000	9.74	3.31	3.08	22.2
Absolute deaths/mo/50,000	12.75	5.30	0	37.25
With policy:				
Unempl rate, federal UI = 0	14.96	4.13	6.00	30.10
Unempl rate, federal UI > 0	14.81	4.03	5.60	32.20
Avg. hhld cost/mo, federal UI = 0	355.	144.	60.0	930.
Avg. hhld cost/mo, federal UI > 0	238.	137.	5.00	895.
Absolute ’00s cases/mo/50,000	4.85	2.42	0.003	17.6
Absolute deaths/mo/50,000	6.25	3.56	0	27.42
Respondents	993			
Policies (choices)	1986			

Notes: We rely upon the designed-in independent variation in federal UI payments, unemployment rates, and average household costs per county. The joint distribution for the federal policy context variable and the two key attributes of the county-level policies is summarized in Online Appendix E, Figure E1. Cases and deaths are reported in terms of absolute levels of cases under each alternative. These rates are summarized per 50,000 people because 50,000 is roughly the average population of counties in our three-state region. In the survey itself, for each respondent, cases and deaths were scaled to the corresponding numbers for their own county.

of its effect on average household incomes in the respondent's county—namely, the *social* cost, as opposed to the *private* cost specifically to the respondent's own household. Our specification is additively separable in the respondent's income and in average household costs for the policy in the respondent's county:

$$(1) \quad V_i^A = \beta_0 Y_i + \beta_1 avcost_i^A + \beta_2 unempl_i^A + \beta_3 cases_i^A + \beta_4 deaths_i^A + \sum_{k=5}^{14} \left(\beta_k restr_{ki}^A\right) + \beta_{15} SQ^A$$
$$+ \gamma_0 Y_i \times \hat{RP}_i + \gamma_1 avcost_i^A \times \hat{RP}_i + \gamma_2 unempl_i^A \times \hat{RP}_i + \gamma_3 cases_i^A \times \hat{RP}_i + \gamma_4 deaths_i^A \times \hat{RP}_i$$
$$+ \sum_{k=5}^{14} \left(\gamma_k restr_{ki}^A \times \hat{RP}_i\right) + + \gamma_{15} SQ^A \times \hat{RP}_i + \varepsilon_i^A,$$

where the status quo indicator, SQ^A is zero for policy A and $\hat{RP}_i$ is the individual's de-meaned fitted response propensity from our preliminary selection model.

Indirect utility of respondent i under the status quo, V_i^N, involves no policy and therefore no decrease in average county-level incomes (so that $avcost_i^N = 0$), only baseline unemployment in the respondent's county and no extra unemployment related to Policy A (so that $unempl_i^N \neq 0$), and no pandemic restrictions (thus $restr_{ki}^N = 0$). This utility is given by:

$$(2) \quad V_i^N = \beta_0 Y_i + \beta_2 unempl_i^N + \beta_3 cases_i^N + \beta_4 deaths_i^N + \beta_{15} SQ^N$$
$$+ \gamma_0 Y_i \times \hat{RP}_i + \gamma_2 unempl_i^N \times \hat{RP}_i + \gamma_3 cases_i^N \times \hat{RP}_i + \gamma_4 deaths_i^N \times \hat{RP}_i$$
$$+ \gamma_{15} SQ^N \times \hat{RP}_i + \varepsilon_i^N,$$

where $SQ^N = 1$ for the status quo alternative.

Each respondent's choice between policy A and the status quo is determined by whether policy A yields greater utility. Let $\Delta V^A = V^A - V^N$ be the difference in indirect utilities from policy A and the status quo option N, so that policy A is chosen if and only if $V^A \geq V^N$ or $\Delta V^A > 0$. The individual's baseline level of income drops out, so our basic econometric specification is as follows.[14]

$$\Delta V^A = \beta_1 cost_i^A + \beta_2\, \Delta unempl_i^A + \beta_3\, \Delta cases_i^A + \beta_4\, \Delta deaths_i^A + \sum_{k=5}^{14}\left(\beta_k restr_{ki}^A\right) + \beta_{15}(-1)$$
$$+ \gamma_1 cost_i^A \times \hat{RP}_i + \gamma_2\, \Delta unempl_i^A \times \hat{RP}_i + \gamma_3\, \Delta cases_i^A \times \hat{RP}_i + \gamma_4\, \Delta deaths_i^A \times \hat{RP}_i$$
$$+ \sum_{k=5}^{14}\left(\gamma_k restr_{ki}^A \times \hat{RP}_i\right) + \gamma_{15}(-1) \times \hat{RP}_i + \varepsilon_i, \tag{3}$$

where $\Delta cases_i^A$ and $\Delta deaths_i^A$ are the differences in COVID-19 cases and deaths under policy A relative to N, $avcost_i^A$ is the average net loss in average household income in respondent i's county of residence under policy A, and $\Delta unempl_i^A$ is the *extra* unemployment in county A created by policy A. The specific levels of restrictions on the $k = 1, ..., 10$ different activities and businesses, are rendered as $restr_{ki}^A \in \{0, 1, 2, 3\}$ (and are treated in our different specifications as either continuous cardinal measures or sets of indicators). These restrictions are present under policy A but absent under the status quo. Given that $\hat{RP}_i$ is respondent i's fitted de-meaned response propensity, the γ terms serve to correct for sample selection bias, and $\hat{RP}_i$ will be counterfactually set to zero when we interpret the β coefficients as representative preferences.

Our approach departs from conventional choice models is one very important way. Our model is expressed in terms of *social costs*, rather than *private costs*, so this specification does not lend itself to straightforward calculations of private willingness-to-pay for private risk reductions. Our policy choices explicitly involve the respondent's willingness to impose *social* costs on their community, in the form of lost jobs and lost incomes, as well as restrictions on activities and businesses, to reduce the risk of cases and deaths in that same community.[15]

[14]We assume that the individual's disutility from county-level average household costs includes the probabilistic implication of this change for their own household's income. With a larger sample, it might have been possible to allow the marginal (dis)utility of average household costs to be greater for respondents who have greater exposure to job losses.

[15]This study is expressly NOT designed to yield an estimate of the value of a statistical life or the value of a statistical illness in the conventional sense. Our choice scenarios do not elicit the respondent's willingness to give up their own (private) household income to gain a reduction in their own (private) risk of illness or death. This study also differs from Bosworth et al. (2009) and Bosworth et al. (2015), where choice experiments elicited the respondent's

3 Results and Discussion

3.1 Homogeneous preferences

Selected parameter estimates for the model in equation (3), namely the estimates for (β_1, β_2, β_3, β_4 and $-\beta_{15}$) are shown as Model 1 in Table 3.[16] Model 2 in Table 3 replaces $avcost_i^A$ and $\Delta unempl_i^A$ in equation 3 with interactions of $avcost_i^A$ and $\Delta unempl_i^A$ with each of five indicator variables for the full set of five possible randomized levels of federal UI we asked respondents to assume. A simpler specification, Model 3 in Table 3, replaces $avcost_i^A$ and $\Delta unempl_i^A$ with interactions of $avcost_i^A$ and $\Delta unempl_i^A$ with just two indicators, in this case for the presence and absence of federal UI in the choice scenario. Models 4-6 in Table 3 are analogous to Models 1-3 but among the suppressed estimates, substitute sets of *indicator* variables to capture the effects of the policy's restrictions on activities or businesses, rather than treating these restrictions as continuous variables. In each model, all variables are also interacted with the respondent's fitted response propensity. Estimates for the corresponding γ parameters are provided in Online Appendix F. Note that Models 2, 3, 5 and 6 *omit the baseline levels* of for average household income lost and unemployment rate, and use instead the full set of interactions with the federal UI variables in each model. Thus the estimated coefficients are the *levels* of the effects in each case, rather than a base effect and a differential relative to that base effect.[17]

The basic specification of Model 1 in Table 3 suggests that respondents prefer policies that reduce the expected number of COVID-19 deaths in their county, as expected, though this result is significant only at the 10% level. Respondents are also highly averse to the status quo alternative,

willingness to give up their own (private) household income to pay for public health prevention programs and for public health treatment programs that would reduce illnesses and deaths in their communities.

[16]Coefficients on restrictions and response propensity interactions are suppressed in the body of this paper, since these features of the model are not our current focus. The full set of parameter estimates for each model in the body of the paper is provided in Online Appendix F.

[17]The estimates in Table 3 do not employ clustered standard errors. Online Appendix F, Table F5 shows the consequences for the key parameter estimates in Models 3 and 6 if we cluster at the respondent level. The qualitative results are the same (i.e., same-signed and statistically significant *differences* between contexts with and without federal UI supplements), so we focus on the simpler models in the body of the paper.

Table 3: Effects of Federal UI payments on preferences over pandemic policies; selected coefficients. (Complete models in Appendix F, Table F2)

	Dependent variable:					
	1=Preferred policy					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
NOTE: Coefficients for each specified condition for federal UI, rather than base coefficients and coefficient differentials						
(β_1) Avg. hhld cost for county ($100)	0.004			−0.018		
	(0.051)			(0.050)		
Avg. hhld cost for county (federal UI = 0)		-0.284^{***}	-0.291^{***}		-0.301^{***}	-0.310^{***}
		(0.093)	(0.093)		(0.098)	(0.098)
Avg. hhld cost for county (federal UI > 0)			0.174^{***}			0.153^{**}
			(0.066)			(0.065)
Avg. hhld cost for county (federal UI = 100)		0.125			0.088	
		(0.102)			(0.103)	
Avg. hhld cost for county (federal UI = 200)		0.131			0.105	
		(0.173)			(0.164)	
Avg. hhld cost for county (federal UI = 300)[a]		0.665^{***}			0.659^{***}	
		(0.124)			(0.124)	
Avg. hhld cost for county (federal UI = 400)		0.166			0.194	
		(0.144)			(0.138)	
(β_2) Unempl rate for county	−0.0003			0.002		
	(0.020)			(0.021)		
Unempl rate for county (federal UI = 0)		0.097^{**}	0.093^{**}		0.098^{**}	0.092^{**}
		(0.039)	(0.039)		(0.040)	(0.040)
Unempl rate for county (federal UI > 0)			−0.024			−0.023
			(0.020)			(0.020)
Unempl rate for county (federal UI = 100)		−0.028			−0.017	
		(0.034)			(0.035)	
Unempl rate for county (federal UI = 200)		−0.001			0.001	
		(0.045)			(0.043)	
Unempl rate for county (federal UI = 300)[a]		-0.079^{***}			-0.083^{***}	
		(0.026)			(0.026)	
Unempl rate for county (federal UI = 400)		−0.015			−0.014	
		(0.025)			(0.026)	
(β_3) Absolute ’00s cases/mo/50,000	−0.041	−0.036	−0.035	−0.042	−0.037	−0.036
	(0.032)	(0.031)	(0.031)	(0.032)	(0.032)	(0.031)
(β_4) Absolute deaths/mo/50,000	-0.034^{*}	-0.037^{*}	-0.038^{*}	−0.027	−0.026	−0.029
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
(β_{15}) 1=Status quo alternative	-1.982^{***}	-2.008^{***}	-1.994^{***}	-2.433^{***}	-2.552^{***}	-2.472^{***}
	(0.296)	(0.284)	(0.291)	(0.360)	(0.370)	(0.368)
Activity restrictions (continuous variables)	✓	✓	✓			
Activity restrictions (sets of indicators)				✓	✓	✓
All response propensity interactions	✓	✓	✓	✓	✓	✓
Total estimated coefs in model	30	46	34	68	84	72
Respondents	993	993	993	993	993	993
Choices	1986	1986	1986	1986	1986	1986
Log likelihood	-1205.77	-1184.09	-1194.52	-1180.75	-1158.56	-1169.80
AIC	2471.55	2460.17	2457.05	2497.5	2485.12	2483.61
BIC	2639.36	2717.49	2647.24	2877.88	2955	2886.37

Notes: See equation 3 for the econometric specification for Model 1. To reduce leading 0s in coefficient estimates, average household costs are measured in 100s of dollars per month, cases are measured per 50 county residents, and deaths are measured per 50,000 county residents. [a]Federal UI payments of $300 were the most-recently experienced level of generosity.

in which no restrictions are placed on activities or businesses and everyone is allowed to decide for themselves what precautions to take, if any. We do not find statistically significant evidence that subjects, on average, respond to expected reductions in COVID-19 cases. We do find, however, that the presence of federal unemployment insurance shapes the effects of lost income and increased unemployment on respondents' decisions (Table 3, Models 2-3 and 5-6).

3.2 Role of federal unemployment insurance

Respondents were not given the opportunity to vote for or against any level of federal unemployment insurance. They were simply instructed to assume that federal unemployment benefits would take a given level ($0, $100, $200, $300 or $400 in additional benefits per week) no matter which policy they selected. Respondents were told, "**Assume that any Federal unemployment benefits, as described, will be in place regardless of any pandemic rules that apply in [respondent's county]**" (bold in original). This design allows us to determine how the level of federal unemployment insurance affects preferences over our two measures of a policy's economic costs: increases in county-level unemployment and corresponding reductions in county-level average household income. We emphasized to respondents that average household incomes in their county would fall mostly as a result of the increased unemployment. We also instructed respondents that the economic costs would be unevenly distributed across residents of their county, with some households losing a lot of income and others losing almost no income, so they should consider their own household's chances of losing income under each policy.[18]

Our survey was launched January 13, 2021, one day before the American Rescue Plan (ARP) was first proposed. We concluded data collection February 16, 2021, while the inclusion of federal UI in the ARP (and its level) were still being debated, and more than three weeks before the

[18]We included a comprehension check to ensure respondents understood the relationship between unemployment and reduced household income. If they failed the comprehension check, they were reminded, "These 'Average $/month lost' because of a policy are mostly a RESULT of unemployment and lost business earnings. They are not an extra cost on top of that."

ARP was signed into law. The ARP ultimately extended the existing $300/week in additional unemployment insurance provided by the federal government, which would otherwise have expired March 14, 2021. We are fortunate to have fielded our survey during a period when future federal contributions to unemployment insurance were still uncertain, permitting us to vary the levels of federal aid in our choice scenarios without straining credulity.[19]

Casual intuition and observation suggest competing hypotheses about the likely effects of increased unemployment insurance on preferences. On the one hand, it seems reasonable to surmise that, because unemployment insurance functions to smooth consumption over negative income shocks, increased assistance to the unemployed should make a policy's "unemployment costs" less painful and therefore less salient. Furthermore, those who lose their jobs because of a coordinated response to a global pandemic—and through no fault of their own—may be considered especially deserving of aid. On the other hand, lawmakers and public figures, especially on the political right, have voiced concerns that overly generous unemployment insurance packages create perverse incentives for workers.[20] If these concerns resonate with with a broad swath of Americans, we may find that increases in unemployment are less tolerable in the presence of federal unemployment insurance. Our results are consistent with the latter of these two hypotheses, though we find a great deal of heterogeneity across groups in the effect of federal UI on preferences over the effects of these policies on unemployment and average household costs.

In the absence of additional federal UI, we find that respondents have a negative and statistically significant response to a policy's monetary cost, expressed as the average lost income over the

[19]For context, respondents were told: "During the first part of the current pandemic, there was an extra unemployment benefit of $600 per week from the Federal government under the CARES Act. These benefits made the pandemic's 'Average $/month lost' much lower than they would normally be, for any given level of unemployment. The $600/week extra benefit ended July 31. A $300/week extra benefit was then provided in December. The incoming Administration is proposing $400/week. It is not yet clear whether extra unemployment benefits will continue to be available, at what level, or for how long, as the pandemic drags on."

[20]In addition to examples given in Section 1, South Carolina Sen. Lindsey Graham said of 2020's stimulus bill, "You're literally incentivizing taking people out of the workforce at a time when we need critical infrastructure supplied with workers." https://www.cnn.com/2020/03/25/politics/senate-stimulus-unemployment-benefits-coronavirus/index.html

households in their county (Table 3, Models 3 and 6). Again in the absence of supplemental federal UI, we find that respondents have a *positive* and statistically significant response to increases in unemployment caused by a given policy. In contrast, when respondents are instructed to assume a *non-zero* level of federal UI (i.e., \$100, \$200, \$300 or \$400 in additional benefits per week), the effects of these economic costs change considerably. In the presence of federal UI, respondents are significantly *in favor of* lost household income and indifferent to increases in unemployment. Federal UI, then, shapes preferences for local pandemic policies. We discuss possible mechanisms for this apparent preference reversal in Section 4.

The results in Table 3 also raise another question: Why would respondents ever respond positively to losses in average household incomes (as they do when federal UI is present) or to increased unemployment (as they tend to do when federal UI is absent)? Attitudes toward the possibly perverse incentives of large federal UI payments or a sense of aggrievement over benefits perceived to be unfairly generous could plausibly explain the first result. If generous federal UI payments seem to be unfair or distortionary, then respondents may view lost income due to unemployment as an important mechanism to maintain desirable economic incentives, and therefore a beneficial feature of a pandemic mitigation policy. A relatively large increase in lost income may specifically signal to respondents that the level of federal UI is sufficiently low that most folks cannot earn more money on unemployment benefits than they would make in wages. Subjective scenario adjustment on the part of respondents could account for the second result. Respondents may suppose that high unemployment is associated with reduced social contact, slowing the spread of COVID-19 and resulting in greater reductions in cases and deaths than are specified in the choice scenario.[21]

The point estimates for the effects of \$100, \$200, \$300 and \$400/week federal UI policies on preferences over increased unemployment and average household costs do not display monotonicity, as can be seen in Models 2 and 5 in Table 3. For both average household costs and unemployment rates, we see the largest (and the only individually statistically significant) effects

[21] Scenario adjustment in stated preference research is specifically addressed in Cameron et al. (2011).

of federal UI at the \$300/week level. Respondents perhaps found this level to be more plausible than other positive levels, given that \$300/week was the current true level of federal UI at the time the survey was fielded. A likelihood ratio test suggests that Model 5, with indicators for each level of federal UI, dominates Model 6, with indicators for only the presence or absence of federal UI. However, we select the Model 6 as our preferred (conservative) specification because the Akaike Information Criterion yields the opposite conclusion.[22] Overall, subjects appear to respond mostly to the presence, rather than the specific level, of federal unemployment insurance, while anchoring on the very salient \$300/week level when it is presented. Given this pattern, in subsequent specifications we interact policies' economic costs (both average household costs and unemployment) with indicators for the presence or absence of federal UI described for their the choice scenario.

Among the other coefficients featured for the models with homogeneous preferences in Table 3, the number of COVID-19 cases does not appear to affect support for pandemic policies. COVID-19 deaths affect policy preferences only in Models 1–3 that capture the extent of our ten categories of restrictions on activities and business as continuous variables. In the fully saturated model with each of these ten types of restrictions included as sets of indicators, the marginal (dis)utility of COVID-19 deaths becomes statistically insignificant. However, for all specifications, the coefficient on the status quo indicator is negative and strongly statistically significant, indicating that on average, respondents prefer *some policy* to *no policy*, regardless of the policy's attributes.

3.3 Preference heterogeneity

Three of the other choice-experiment studies of pandemic policies summarized in Table 1 employ latent-class analysis when they consider heterogeneity in policy preferences. However, in this paper, we emphasize *systematic* sources of heterogeneity in policy preferences, because it seems likely that preferences are more complicated than just a finite mixture of some small number of

[22] Model selection based on AIC yields AIC(Model 5) = 2485.1; AIC(Model 6) = 2483.6.

preference classes.[23] In Table 4, we split our sample according to different sociodemographic characteristics to illustrate numerous dimensions of preference heterogeneity with respect to the economic impacts of pandemic policy according to the presence or absence of federal UI.

Panels A and B in Table 4 present split-sample estimates for separate demographic groups. Each set of columns (e.g. "Age Groups," comprising columns (1)–(3) in Table 4) shows the results from a *single* specification. Each such specification includes indicator variables for membership in the relevant groups (e.g. *18–34*, *35–64*, or *65+*) interacted with the attributes of each alternative. The non-interacted attribute levels are again excluded from the models. Significance levels for coefficients for each group are determined with respect to zero, rather than with respect to an arbitrarily chosen reference group.[24]

In Table 4, panels A and B reveal significant preference reversals with respect to average household costs and unemployment rates in the presence of federal UI for both men and women, and for both white and non-white racial groups. When partitioning the sample by age, political ideology, and income, however, we find mixed effects of federal UI.

The young (18–34) and middle (35–64) age groups are significantly averse to higher average household costs when federal UI is absent and significantly in favor of higher average household costs when federal UI is present. The preference reversal with respect to unemployment for these two age groups is less stark but is still apparent. For respondents 65 years and older, however, the coefficients for cost and unemployment are statistically indistinguishable from zero, whether or not federal UI is present. This may reflect the availability of social security and other retirement income that insulates retirees from the risk of unemployment.

Political moderates and conservatives also show strong evidence of a preference reversal in

[23]We report on our explorations of alternative models, including latent class and mixed-logit specifications, in Online Appendix G

[24]An alternative to this mode of estimation is to estimate completely separate specifications for each group. However, the estimated parameters from a conditional logit model are identified only up to an unknown scale parameter, which may differ across groups. By estimating the parameters for all groups with a single specification, we constrain this scale parameter to be the same across groups, permitting legitimate comparisons of same-scale utility-function parameters across groups.

Table 4: Heterogeneity in preferences across socioeconomic groups; generalizations of Model 6 in Table 3; selected coefficients. (Complete models in Appendix F, Tables F3 and F4)

Panel A

Heterogeneity by:	(1) Age			(2) Race		(3) Gender	
	18 to 34	35 to 64	65 +	Non-white	White	Women	Men
Dep. var: 1=Preferred policy	(a)	(b)	(c)	(a)	(b)	(a)	(b)
Avg. hhld cost (federal UI = 0)	-0.597^{***}	-0.382^{***}	-0.307	-0.842^{***}	-0.228^{**}	-0.497^{***}	-0.220^{*}
	(0.224)	(0.139)	(0.376)	(0.294)	(0.112)	(0.165)	(0.114)
Avg. hhld cost (federal UI > 0)	0.251^{**}	0.212^{*}	0.181	0.052	0.216^{***}	0.155	0.134
	(0.124)	(0.119)	(0.140)	(0.126)	(0.079)	(0.106)	(0.082)
Unempl rate (federal UI = 0)	0.251^{***}	0.080	0.219	0.268^{**}	0.090^{*}	0.141^{**}	0.093
	(0.093)	(0.055)	(0.161)	(0.118)	(0.046)	(0.064)	(0.057)
Unempl rate (federal UI > 0)	0.023	-0.066^{**}	-0.012	0.011	-0.029	-0.049	0.020
	(0.044)	(0.031)	(0.069)	(0.050)	(0.024)	(0.036)	(0.029)
Absolute ’00s cases/mo/50,000	-0.047	0.068	-0.045	-0.055	-0.048	-0.086^{*}	0.008
	(0.055)	(0.049)	(0.099)	(0.071)	(0.040)	(0.044)	(0.049)
Absolute deaths/mo/50,000	-0.074^{**}	-0.011	-0.059	-0.070^{*}	-0.001	0.001	-0.061^{**}
	(0.033)	(0.035)	(0.056)	(0.039)	(0.027)	(0.032)	(0.028)
1=Status quo alternative	-4.533^{***}	-1.943^{***}	0.966	-2.603^{***}	-2.584^{***}	-2.925^{***}	-2.039^{***}
	(0.729)	(0.559)	(1.211)	(0.761)	(0.443)	(0.557)	(0.508)
Number of respondents	317	453	223	295	698	507	480
Number of choices	634	906	446	590	1396	1014	960
Total estimated coefficients		102		68		68	

Panel B

Heterogeneity by:	(4) Ideology			(5) Education		(6) Income	
	Liberal	Moderate	Conservative	Non-college	College	< \$75k/yr	> \$75k/yr
Dep. var: 1=Preferred policy	(a)	(b)	(c)	(a)	(b)	(a)	(b)
Avg. hhld cost (federal UI = 0)	0.094	-0.891^{***}	-0.411^{**}	-0.347^{*}	-0.239^{*}	-0.452^{***}	-0.192
	(0.212)	(0.229)	(0.173)	(0.210)	(0.123)	(0.157)	(0.143)
Avg. hhld cost (federal UI > 0)	-0.081	0.123	0.123	0.150^{*}	0.119	0.367^{***}	0.028
	(0.207)	(0.118)	(0.108)	(0.082)	(0.109)	(0.091)	(0.087)
Unempl rate (federal UI = 0)	-0.116	0.316^{***}	0.089	0.140	0.015	0.250^{***}	-0.015
	(0.087)	(0.104)	(0.068)	(0.086)	(0.054)	(0.073)	(0.055)
Unempl rate (federal UI > 0)	0.063	-0.047	-0.082^{***}	-0.042	-0.016	-0.017	-0.022
	(0.078)	(0.040)	(0.031)	(0.034)	(0.031)	(0.033)	(0.028)
Absolute ’00s cases/mo/50,000	-0.036	-0.146^{**}	-0.081	-0.164^{***}	0.081^{**}	-0.092^{*}	-0.012
	(0.105)	(0.057)	(0.061)	(0.061)	(0.037)	(0.055)	(0.045)
Absolute deaths/mo/50,000	-0.035	0.028	-0.083^{***}	-0.029	-0.029	-0.028	-0.014
	(0.068)	(0.044)	(0.030)	(0.034)	(0.026)	(0.030)	(0.029)
1=Status quo alternative	-5.006^{***}	-3.647^{***}	-2.854^{***}	-2.497^{***}	-2.869^{***}	-2.705^{***}	-2.321^{***}
	(1.017)	(0.832)	(0.650)	(0.514)	(0.559)	(0.504)	(0.578)
Number of respondents	338	309	303	398	589	489	475
Number of choices	676	618	606	796	1178	978	950
Total estimated coefficients		102		68		68	

Notes: Panels A and B each contain three models, split into multiple columns for easier comparisons of these marginal utilities across groups. Each model includes interactions between all variables and indicators for membership in each group (e.g. “18–34”, “35–64,” and “65+”). To avoid the dummy variable trap, the non-interacted base levels of these variables are not included in the models. Similarly, average household cost and unemployment rate are interacted with indicators for the presence or absence of federal UI, but the uninteracted cost and unemployment variables are excluded from the models to avoid the dummy variable trap. All models are corrected for sample selection (see section 2.1), and include indicators for the levels of restrictions on each of the 10 categories of activities or businesses in our choice experiment. Five of the six models, compared with Model 6 in Table 3, have significant likelihood-ratio statistics ($p < 0.05$). The model that differentiates by race (Panel A, Model 2, columns (a) and (b)) does not offer a statistically significant improvement over Model 6 in Table 3. Average household costs are measured in \$100.

the presence of federal UI, although liberals do not. Liberals have a larger point estimate for the coefficient for unemployment when federal UI is present than when it is absent, although this difference is not statistically significant. Nevertheless, across the dimensions of heterogeneity presented here, this is the only instance that stands apart from the prevailing pattern where the availability of federal UI is associated with greater aversion to unemployment. We also see strong evidence for preference reversals among respondents whose household income is less than $75k per year, but not among those whose household income is greater than $75k per year.

Several other differences in preferences across groups warrant comment. The main benefits of these policies—reduced cases and deaths from COVID-19—are more important to some groups than to others. Table 4 reveals that COVID-19 *cases* are of concern to women, political moderates, and lower-income individuals. Somewhat perplexing is the result that college-educated respondents appear to prefer more COVID-19 cases to fewer. Positive point estimates for the marginal (dis)utility of COVID-19 cases are also obtained for the 35–64 age group and for men, but these estimates are not statistically different from zero. COVID-19 *deaths* are of concern to the 18–34 age group, to Whites, to men, and to conservatives.

Three subgroups in Table 4 display no systematic responsiveness to average household costs, unemployment rates, cases, or deaths. These subgroups are: people aged 65 and above (who may anticipate their imminent eligibility for the vaccine), liberals, and those with annual household incomes greater than $75,000. People with predictable retirement income and high-income households are likely to find issues of unemployment and its costs to be less salient. While the liberal group certainly includes young people with lower incomes, the most notable feature of their preferences is the greatest aversion to the status quo across all groups in Table 4. This group simply prefers *any* policy to no policy, regardless of the economic impact on their county or the morbidity/mortality consequences.

Political moderates wish to reduce *cases* (but not necessarily deaths), while conservatives are significantly motivated to reduce *deaths*. Every demographic group we examine, with the exception of those 65 or older, exhibits a statistically significant aversion to the status quo alternative. While there is considerable disagreement about the ideal features of a pandemic mitigation policy, we find a near consensus across groups that, in the face of a pandemic, something must be done to limit its spread.

Online Appendix G provides results for a number of alternative specifications and well as some sensitivity analyses that explore heterogeneity across split samples according to (a) time spent reading the instructions about assumptions concerning federal UI for the first choice task; (b) the salience of unemployment as measured by whether the worst monthly actual unemployment rate during the pandemic in the respondent's own county was better or worse than the median worst unemployment rate across all respondents in the sample; (c) whether the respondent who made no mistakes, versus any mistakes, on two key comprehension questions in the survey; and (d) whether the respondent's household income was more or less than the median income in the same zip code. The point estimates and levels of significance for the coefficients on our four key interaction terms vary from case to case, but the typical pattern of signs persists.

4 Conclusions

Harmonization of local-level policies or programs with federal-level counterparts can be challenging. We specifically explore the relationship between (a) the federal social safety net and (b) preferences over county-level pandemic mitigation strategies. However, we note that adjustments to federal policies may change people's preferences for related policies across jurisdictional levels and policy domains. Sometimes a straight line can be drawn from federal policies to state or local policy preferences. For example, a number of localities passed "sanctuary laws" in 2017 in response to the Trump administration's deportation policies. Similarly, state-level efforts to pass "heartbeat bills" that restrict abortion access are almost certainly a response to the legal status of abortion at the federal level. Less directly, changes in federal immigration policy may affect preferences for state-level voter ID requirements. Or, increased trade liberalization could plausibly be expected to change preferences for legal protections for labor unions that preserve domestic manufacturing jobs.

In this study, we employ a new survey, fielded between mid-January and mid-February of 2021, to elicit preferences over alternative county-level pandemic policies under different assumptions about the generosity of supplementary federal unemployment insurance. Each county-level policy is described in terms of its individually randomized effects on pandemic cases and deaths in the respondent's own county, the economic consequences (namely, the average cost per household and unemployment rates) and the levels of restrictions on a set of ten activities or businesses that would be imposed at the county level to achieve the specified expected reductions in cases and deaths.

We focus in this paper on the role played by supplementary federal unemployment insurance. More specifically, we highlight the effect of federal UI supplements on the level of disutility that respondents associate with (a) the county-level average costs of these pandemic policies, and (b) the unemployment that these policies generate. Federal unemployment insurance is intended to alleviate the economic hardship caused by pandemic restrictions.

What might one expect, intuitively? *Without federal UI*, unemployment rates will be highest for people who cannot work from home, so that unemployment rates may also proxy for avoidance of workplace exposure to infection for essential workers. This socially desirable aspect of higher unemployment may dominate people's policy preferences. But unemployment also imposes costs on households, and these costs can be expected to make people worse off. *With federal UI*, if these payments succeed in completely removing people's concerns about the economic impact of a county-level pandemic policy, we might expect that neither average household costs, nor the unemployment rate from a policy, would then be viewed as a bad thing. Greater unemployment might even be desirable if its role in social distancing is perceived to be sufficiently great.

In the following discussion, we summarize our findings. Some of the results from our analysis may seem counter-intuitive at first. In some cases, we speculate about possible mechanisms behind these effects, but we emphasize that additional data and further research will likely be necessary to confirm or refute these conjectures.

What do we find, in models with homogeneous preferences? *Without federal UI*, it appears that people dislike policies that result in higher average household costs, although they are in favor of policies that cause more county-level unemployment (again, perhaps because unemployment is associated with greater social distancing). *With federal UI*, policies that result in higher average household costs become acceptable, but greater unemployment becomes undesirable (perhaps because respondents object to people being paid by the federal government *not* to work). The difference is especially discernible when federal UI is described as matching the $300-per-week regime that had most recently been experienced by respondents.

What do we find, in models with heterogeneous preferences, for average household costs? *Without federal UI*, people tend to derive disutility from county-level policies with higher average household costs. This marginal utility is statistically significantly negative for all of the subgroups we considered except those 65 years and older, liberals, and those with incomes in excess of $75,000 per year. A negative marginal utility from higher average household costs can be interpreted as concern about one's own livelihood or the livelihoods of others in the community. *With federal UI*, however, people derive statistically significant *positive* utility from higher average household costs in their county. This marginal utility is statistically significantly positive for people under 65 years of age, for Whites, for those without college educations and for people earning less than $75,000 per year. Positive marginal utility from higher policy costs, ordinarily, would be unexpected. With generous federal UI benefits, however, a subset of respondents may be concerned that some unemployed workers will earn more money from unemployment benefits than those workers could have earned at their jobs. For these respondents, a larger average household cost may make a given policy more attractive because it signals that fewer workers find unemployment to be more lucrative than working.

What do we find, in models with heterogeneous preferences, for county-level unemploy-

ment rates? *Without federal UI*, people derive positive marginal utility from higher county-level unemployment. These effects are statistically significant for the 18–34 age group, for both racial groups we consider, for women, for moderates and for people with incomes less than $75,000 per year. These positive marginal utilities may stem from the perception that unemployment corresponds to reduced exposure to COVID-19. *With federal UI*, people may suffer a loss in utility from higher county-level unemployment rates, although this effect is not always statistically significant. This disutility from policies that result in more unemployment is statistically significant, however, for the 35–64 age group and for conservatives. The loudest warnings about undesirable economic incentives of federal UI have come from the political right, which is consistent with a sentiment that higher unemployment is a bad thing.

What do we find for COVID-19 cases and deaths? In our models with *homogeneous* preferences, all specifications yield negative point estimates for the disutility associated with COVID-19 cases and deaths in one's county, but only the disutility from additional deaths is statistically significant, and only in some model variants. In our models with *heterogeneous* preferences, there is greater variety. All statistically significant groupwise marginal disutilities from cases and deaths are negative, although there is one puzzling exception where college-educated respondents appear to prefer *more* COVID-19 cases. If college-educated respondents are themselves less likely to contract COVID-19, one could speculate that they might favor higher infection rates because this would hasten "herd immunity," but we have no evidence to support this conjecture.

What do we find for status quo effects? Except for the 65+ age group in our split-sample models with heterogeneous preferences, respondents are (in all cases) strongly statistically significantly more likely to choose some policy over the status quo, regardless of the attributes of that policy.

What is our most notable empirical insight? When respondents are asked to assume that federal UI will be zero, they tend to be averse to losses in average household income in their county but favorably disposed toward increased unemployment. With positive federal UI payments, however, respondents are more willing to accept losses in average household income but view increased unemployment less favorably. The first reversal, with respect to losses in average household income, is driven primarily by younger, white, non-college and lower-income respondents. The second reversal, with respect to unemployment, is driven primarily by middle-aged and conservative respondents.

Overall? Our results suggest that the generosity of the social safety net at the federal level can alter support for county-level public health policies when these county-level policies will have substantial economic impacts. This insight suggests that policymakers should carefully consider the potential effects of federal-level economic stimulus and unemployment insurance policies on support for other policies where authority devolves to lower-level jurisdictions. If too many people are concerned that federal UI is distortionary or unfair, then "stimulus checks" that are independent of employment status may represent a better solution. Alternatively, if federal UI is capped at some fraction of previous income (as are unemployment payments in many states), this may circumvent the objection that paying folks more to stay home than to work creates an especially perverse incentive. Unconditional stimulus checks, or federal UI that is indexed to previous income, might give state and local governments more flexibility to enact pandemic mitigation policies—or other emergency policies that foreseeably increase reliance on public assistance—while retaining popular support.

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A Online Appendix: Other Pandemic Policy Choice-Experiment Surveys

Chorus et al. (2020) collected their entire sample of 1009 completed surveys on April 22, 2020 using an online survey panel (Kantar Public) intended to be representative of the Dutch population in terms of gender, age, and education level. Rather than using specific types of pandemic restrictions as their policy attributes, they elect to use the generic consequences (impacts) of differing policy measures, making theirs a so-called "unlabelled" or "unbranded" choice experiment. Specifically, they described the increase in the number of deaths (8k, 11.5k, 15k and 18.5k), the increase in people with lasting injuries (30k, 80k, 130k and 180k), the increase in people with lasting mental injuries (30k, 80k, 140k and 200k), the increase in the number of children with educational disadvantages (10k, 90k, 170k, and 250k), the increase in households with a net income loss of more than 15% for more than three years (400k, 700k, 1M, 1.3M), a one-time tax per household in 2023 (1k, 2.5k, 4k and 5.5k Euros), and four levels of work pressure in the health-care sector. Their latent-class models differentiate preferences by gender, education level, and age. Their estimated preference classes suggest deontological preferences for some people, and consequentialist preferences for others. In separate specifications, they also explore the effect of having had a relative infected with COVID-19, and the effect of choices that involve "the taboo tradeoff" that involves simultaneously both a lower tax and a higher number of fatalities.

Blayac et al. (2021) report on survey-based choice experiments fielded in France during May 4 to May 16, 2020, yielding 1154 respondents with three pair-wise policy choices each. Both options, in each case, were active options (i.e. there was no status quo, or no-intervention, policy). Their choice scenarios featured seven policy attributes: (1) weeks of additional lockdown (no extension, one week or three weeks, treated as continuous), (2) mask requirements (indicators for mandatory in public places or mandatory everywhere), (3) bar and restaurant closure (indicator for closed all the summer season), (4) daily public transportation, urban and regional (indicator for limited to working hours), (5) vacation and leisure travels (indicators for limited to France or limited to within 100km), (6) digital tracking (an indicator for a free app), (7) financial compensation (in Euros, 0, 500, 1500 or 2200, treated as continuous). They differentiate their estimated preferences according to clinical vulnerability, age, and gender.

Reed et al. (2020) surveyed 5953 adults across all 50 U.S. states between May 8 and May 20 of 2020, during the first wave of the pandemic. They collaborated with a health care market-research firm, SurveyHealthcareGlobus, which sent email invitations to adults throughout the U.S., with oversampling in New York, California, Texas and North Carolina. The choice scenarios that they presented to respondents distinguished between just four attributes: The month when all non-essential businesses would be re-opened, the total number of COVID-19 cases per 100 people by January 2021, the percent of families below the poverty line, and when the economy would recover. Outside of the choice tasks, their survey did include a ranking exercise, where respondents were asked to rank their subjective importance of relaxing six categories of restrictions. In order of importance their respondents prioritized the relaxation of restrictions on non-essential businesses, schools and colleges, restaurants, parks and museums, religious ceremonies, and then sporting events.

These authors focus on latent class analysis for their main results. About 40% of the people in their sample are classed as "risk-minimizers" who are not willing to accept any level of risk brought about by the re-opening of non-essential businesses or reducing the impact of the pandemic on the economy. There is also a class of "waiters" who are less concerned about the health risks, but also do not want to re-open the economy in any hurry. About 25% are "recovery-supporters," and about the remainder are labelled as "openers" who are unconcerned about health risks or any impacts upon the poor. The individual characteristics that have systematic effects on membership in these latent preference classes include Democrat/Republican/Independent political affiliations, Employment status, income in each of three brackets, and whether the respondent is non-white.

During May and June of 2020, Wilson et al. (2020) recruited respondents through "randomly allocated social media advertising on Facebook and Instagram." They also sought greater diversity in their sample by recruiting by email through a community health organization. They argue that Missouri broadly represents the U.S. Midwest and South in terms of socioeconomic characteristics. Like Reed et al. (2020), Wilson et al. (2020) identify four latent preference classes in this population, dubbing them "risk eliminators," "risk balancers," "altruistic," and "risk takers." They detect that younger respondents (18-24 years) have a stronger relative preference for keeping schools and social venues open and to prevent income loss, compared to older age groups. Men, in contrast to women, prefer to keep outdoor recreational facilities and social/lifestyle venues open, to allow large gatherings, to minimizing lost incomes, and to have shorter policy durations. Whites, similar to men, but in contrast to other racial groups, want to keep services open, to minimize lost incomes, and to have shorter policy durations.

Genie et al. (2020) published their protocol for a choice experiment study of pandemic policy preferences, not including results, in November of 2020. Their choice experiments feature seven attributes: (1) restrictiveness of the lockdown (summarized as one of four levels, each associated with a fixed pattern of restrictions), (2) lockdown duration (3, 6, 10 or 16 weeks), (3) postponement of non-urgent medical care (for no, some, or all non-urgent care), (4) excess deaths (1, 4, 9, or 13 additional people per 10,000), (5) number of infections (100, 600, 1300 or 2000 people infected per 10,000), (6) ability to buy things (illustrated as 100%, 90%, 80% or 70% of current purchases) and (7) the proportion of people who will lose their job (0, 4, 15 or 25 out of 100).

One limitation of the Genie et al. (2020) choice set design is that their four overall levels of activity restrictions are aggregated into just "green," "yellow," "amber," and "red." The key to interpretation of their four levels of "lockdown" references the extent to which level requires (or limits) staying at home, group sizes for social gatherings, non-essential activities other than groceries and work-related trips, schools and youth activities, businesses and shops, and outdoor activities, but there is no variation within each of these four overall levels in the relative severity of the different constituent categories of restrictions.

Genie et al. (2020) plan to explore observed heterogeneity according to the usual array of sociodemographic variables, but include a novel measure of moral attitudes. They plan to use mixed logit models to test for unobserved preference heterogeneity.

There have also been a few stated-preference surveys concerning people's preferences concerning COVID-19 vaccines. Cerda and Garcia (2021) using a sample of 531 individuals from middle- and high-income sociodemographic groups in Chile. They use a double-bounded dichotomous choice elicitation format. Their survey was in the field between July 10 and August 10 of 2020. Muqattash et al. (2020) describe data from a Google Forms survey about vaccine preferences, fielded between July 4 and August 4 of 2020, administered to respondents in the UAE and garnering 1109 responses, but no models or statistical results are reported in the paper. Kreps et al. (2020) report a choice-experiment survey fielded on July 9, 2020, using the Lucid survey platform, which contacted 3708 U.S. adults and yielded 2000 responses. This study also concerns tradeoffs between COVID-19 vaccine attributes.

We should not omit to mention a pre-COVID-19 study by Cook et al. (2018) using a survey conducted with 500 respondents in Singapore between November 2012 and February 2013 in the wake of the previous 2003 SARS-CoV and 2009 H1N1 influenza outbreaks. Their policy options concerned seven attributes, including quarantines (mandatory or voluntary), isolation of actual cases (voluntary or mandatory), cancellation of mass gatherings (none, just schools, or also other gatherings of 30 or more), temperature screenings (none, at border checkpoints, at border checkpoints and internally), a fee to fund public health measures (S$15, S$20, S$40 or S$50), the expected number of infections (200, 500, 1k, 10k or 1M), and the expected number of infection-related deaths (0, 30, 80, 120, or 180).

B Online Appendix: Survey development

Survey development took place over the summer and early fall of 2020. Several generations of choice scenarios were assessed using sequential one-on-one think-aloud protocols with test subjects who helped us to understand the most efficient and accessible ways to present the tutorial information in the survey.

Each paragraph of the survey was processed through MS Word's reading-level utility, where we pursued revisions until passages fell into the 60-70 range on the Flesch Reading Ease test, and achieved a Flesch-Kincaid Grade Level score between 7.0 and 8.0.

Using javascript in the background of the survey, modal popups are used extensively to make access to optional explanations , or the review of key concepts, as easy as possible for respondents.

Information about the population of the respondent's own county, as well as actual COVID-19 cases and deaths in the respondent's county, was supplied dynamically. Values were keyed to the respondent's selection of their county of residence and updated with the most-recent four weeks of COVID-19 data every few days over the course of the survey.

Information about unemployment insurance that underlies our scenarios about unemployment rates and average household income lost in the respondent's county was keyed to their state's formula for state-level unemployment insurance and the baseline level of unemployment in their county, according to the Bureau of Labor Statistics, just prior to the start of the pandemic.

The survey's scenarios about potential future baseline pandemic conditions were based on draws from the distributions of county-level cases and deaths per 50,000 people during a recent four-week period across all counties in the U.S. These randomized draws of actual rates of cases and deaths were converted into the numbers of cases and deaths that would be implied for the respondent's own county.

Respondents were reminded of actual cases and deaths in their own county during a recent four-week period. They were also informed about what would have been the cases and deaths over the last four weeks in their own county if their county's rates had corresponded to the 90th percentiles, across all counties, during that recent four-week period. These actual data about potential COVID-19 risks were important for conveying the scope of the pandemic elsewhere in the country. Before we provided this contextual information, several test subjects had no real idea just how bad the pandemic could be if their county had been among the worst 10% across all U.S. counties (i.e. approximately the worst 300 counties) in terms of COVID-19 cases and deaths. These benchmarks were important for preventing rejection of choice scenarios as being implausible.

All of our randomized choice scenarios were generated outside the survey in rates per 50,000 people (for baseline cases and deaths as well as the reductions in these baselines that would be achieved by each proposed pandemic policy). All of the fields for the standardized choice scenarios were converted to json format and uploaded to Qualtrics via the survey platform's Web Service utility, making use of a block of php code designed to draw instances of these standardized choice scenarios, without replacement, from the universe of randomized scenarios.

One instance of the information for the choice scenarios was selected when a respondent accessed the survey, and javacript was used to convert the baseline rates for cases and deaths, and for reductions in cases and deaths in each policy choice scenario from rates per 50,000 to absolute counts of cases and deaths for their own county's population, as given by the 2018 5-year

American Community Survey. The quoted costs of each policy were systematically related to the stringency of the restrictions that the policy would place on each of ten activities or businesses, for plausibility, and these costs were associated with rates of unemployment that would be consistent with these costs for a single-earner household with the median annual income in that county (after state-level unemployment insurance). The quoted weekly amounts of federal UI were incorporated dynamically into the specific choice scenario, reducing these costs but having no effect on unemployment levels.

Each level of restrictions on each of our ten types of activities or businesses was associated with a verbal description appropriate to that activity or business, and these descriptions were available via popup associated with the terse name for that type of restriction. Each activity/business could have restrictions at level 0, 1, 2 or 3, except for "Grocery, essential retail," which was never subjected to level 3 restrictions.

The survey was designed to start with two binary choices between a specified policy and a No Policy alternative, described as being a policy where everyone is simply allowed to take whatever pandemic precautions that they deem to be appropriate. Two further choice scenarios were employed, where each involved first a three-way choice between either of two policies and a status quo alternative. If the status quo was not chosen, the respondent was then given a choice between the other, non-chosen policy versus the status quo. These three-way choices, with a two-way conditional follow-up, are more complex and require more complex statistical analysis, so we reserve their analysis for subsequent research and focus on the two binary (and most incentive-compatible) choice sets for this analysis.

The survey contains a substantial number of follow-up questions about respondent's pandemic perceptions and attitudes, for which detailed analysis will also be addressed in later research.

C Online Appendix: Selection model

C.1 Variables available for selection model

We designed our survey to permit extensive modeling of the decisions by eligible respondents about whether or not to complete the survey after they learned its topic (or at any subsequent point). In a separate paper, Mitchell-Nelson and Cameron (2021), we explain the survey design considerations and the variety of sources from which additional explanatory variables can be recruited. We used LASSO methods to identify the most important regressors to use in a model to yield fitted response propensities. In our models reported in the main paper, we employ a full set of interaction terms between every explanatory variable and these response propensities, which have first been de-meaned relative to the average response propensity across all eligible respondents, regardless of whether they completed the survey and ended up in the estimating sample.

Briefly, we built our selection model using the following process. First, we assembled a set of all candidate variables and their interactions, dropping variables that are perfectly collinear with other variables, e.g. an indicator and that same indicator squared. We used 10-fold cross-validation to select the value of lambda for LASSO that minimizes prediction error for a binary logit selection model, and retained the list of variables selected by LASSO at this value of lambda. Finally, we estimate a conventional logit selection model using these variables, supplemented by all of the base variables employed in any retained interaction terms, even if these base variables were not themselves retained by our LASSO model.

Table C1: Descriptive statistics: Basic variables for selection model, retained by LASSO model, either as individual variables or as part of a pairwise interaction term.

	mean	sd	min	max
Days since Jan 13, 2021 launch	20.307	6.658	0	33
CDC SVI cnty-Minority, language	0.855	0.166	0.256	0.998
CDC SVI cnty-Hsg type, transp.	0.759	0.183	0.063	0.999
1=Own gender female	0.511	0.5	0	1
1=Own race black	0.046	0.21	0	1
1=Own race white	0.703	0.457	0	1
1=Own age is 25 to 34	0.193	0.395	0	1
1=Own age is 35 to 44	0.229	0.42	0	1
1=Own age is 45 to 54	0.115	0.319	0	1
1=Own age is 55 and up	0.056	0.231	0	1
1=Own hhld inc less than 20K	0.115	0.319	0	1
1=Own hhld inc 20K to 25K	0.055	0.229	0	1
1=Own hhld inc 30K to 50K	0.117	0.321	0	1
1=Own hhld inc 100K to 125K	0.109	0.311	0	1
1=Own hhld inc 125K to 150K	0.09	0.286	0	1
1=Started survey on Monday	0.298	0.458	0	1
1=Started survey on Tuesday	0.206	0.405	0	1
1=Started survey on Wednesday	0.142	0.349	0	1

Continued on next page

Table C1 – continued from previous page

1=Started survey on Friday	0.109	0.311	0	1
1=Started survey on Saturday	0.103	0.304	0	1
1=Start: hour ending at 7:00	0.017	0.13	0	1
1=Start: hour ending at 9:00	0.047	0.212	0	1
1=Start: hour ending at 14:00	0.069	0.254	0	1
1=Start: hour ending at 15:00	0.082	0.274	0	1
1=Start: hour ending at 17:00	0.077	0.266	0	1
1=Start: hour ending at 19:00	0.045	0.208	0	1
1=Start: hour ending at 21:00	0.039	0.194	0	1
1=Start: hour ending at 23:00	0.031	0.174	0	1
Zip pr: vote republican	0.353	0.143	0.093	0.769
Days since first case, in 100s	3.438	0.24	2.43	3.89
1=At least one county death	0.995	0.071	0	1
Days since first death, in 100s	3.045	0.543	0	3.68
Cases/50K last 4 weeks	786.802	505.472	60.705	2835.951
Zip pr: age 18-24	0.084	0.04	0	0.359
Zip pr: age 55-64	0.117	0.039	0	0.385
Zip pr: race black	0.047	0.06	0	0.662
Zip pr: race asian	0.108	0.124	0	0.746
Zip pr: indus = agric.	0.019	0.042	0	0.54
Zip pr: indus = wholes.	0.027	0.015	0	0.122
Zip pr: indus = retail.	0.108	0.036	0	0.396
Zip pr: indus = transp.	0.049	0.029	0	0.28
Zip pr: indus = edu. serv.	0.205	0.066	0	0.66
Zip pr: indus = arts, ent.	0.098	0.042	0	0.316
County unemp rate Mar’20	5.411	2.055	2.8	23.5
County unemp rate Nov’20	6.904	2.141	3.7	16.2
County cases/50k Jun 2020	111.564	115.721	0	1278.466
County cases/50k Jul 2020	239.921	180.576	0	943.458
County cases/50k Aug 2020	188.713	132.845	0	1042.168
County cases/50k Oct 2020	141.259	63.762	0	537.281
County cases/50k Jan 2021	851.739	566.377	60.705	1808.252
County deaths/50k Jul 2020	2.872	3.009	0	32.461
County deaths/50k Aug 2020	3.489	3.038	0	23.028
County deaths/50k Sep 2020	2.533	2.171	0	18.074
Zip pr. urban (if havzip==1)	0.872	0.264	0	1
Zip pr. rural (if havzip==1)	0.089	0.198	0	1
County pr. other intnt	0.007	0.007	0	0.045
County pr. intnt not subscr	0.02	0.01	0	0.054
County pr. dialup cmptr	0.003	0.002	0	0.034
County pr. broadband cmptr	0.86	0.041	0.641	0.914
County pr. no intnt cmptr	0.066	0.019	0.038	0.192
County 2018 population	2652.225	3578.59	1.146	10098.052
1=Used a mobile device	0.534	0.499	0	1
1=Have ZIP code data	0.957	0.204	0	1

C.2 Coefficient estimates for selection model

Table C2 reports parameter estimates for our sample selection model. All available variables or sets of indicators, along with their pairwise interactions, were employed in a LASSO model for binary logit specifications. This algorithm employed ten-fold cross-validation to winnow down this extensive set of regressors to the set that makes the most accurate cross-validation predictions of which respondents who passed the screening phase will go on to complete the survey. This model yields the de-meaned fitted response propensities that serve as the variable $\hat{RP}_i$ in equation (3) in the paper. The coefficients on these interaction terms are not reported in the body of the paper because the relevant counterfactual simulation involves setting all of these de-meaned response propensities to zero, which is equivalent to dropping these terms from the model.

Table C2: Estimated coefficients for selection model, a binary logit specification employing all variables retained by the preliminary LASSO model (incompletely sorted)

Variable or interaction term	Coef. estimate	Std. error
Days since Jan 13, 2021 launch	−0.023	(0.060)
Days since Jan 13, 2021 launch × Zip pr: age 18-24	0.200	(0.335)
Days since Jan 13, 2021 launch × Zip pr: indus = edu. serv.	−0.004	(0.250)
Days since Jan 13, 2021 launch × Zip pr: indus = arts, ent.	0.260	(0.327)
CDC SVI cnty-Minority, language	4.263	(4.557)
CDC SVI cnty-Minority, language × Zip pr: age 55-64	−35.881	(32.166)
CDC SVI cnty-Hsg type, transp.	6.012	(4.449)
CDC SVI cnty-Hsg type, transp. × 1=Have ZIP code data	−6.224	(4.437)
1=Own gender female	1.213*	(0.621)
1=Own gender female × 1=Start: hour ending at 23:00	−1.126	(0.940)
1=Own gender female × 1=Own hhld inc 30K to 50K	−0.336	(0.541)
1=Own gender female × County unemp rate Nov'20	−0.086	(0.097)
1=Own gender female × County cases/50k Aug 2020	−0.004*	(0.002)
Zip pr: race black	3.374*	(1.863)
Zip pr: race black × 1=Start: hour ending at 17:00	−12.186**	(5.011)
1=Own race black	0.257	(0.508)
1=Own race black × 1=Own age is 55 and up	−18.577	(1,130.179)
1=Own race black × Zip pr: indus = agric.	−33.493	(48.676)
County pr. rural × 1=Own race black	−6.035*	(3.664)
1=Own race white	−0.218	(0.231)
Zip pr: race asian	−0.113	(0.739)
1=Started survey on Friday × Zip pr: race asian	13.397**	(5.317)
Zip pr: race asian × 1=Own hhld inc 20K to 25K	−7.906**	(3.292)
1=Own age is 25 to 34	0.007	(0.265)
1=Own age is 25 to 34 × 1=Start: hour ending at 9:00	5.385	(6.881)
1=Own age is 25 to 34 × 1=Start: hour ending at 17:00	−1.244*	(0.723)
1=Own hhld inc 30K to 50K × 1=Own age is 25 to 34	−0.355	(0.576)
1=Own age is 25 to 34 × 1=Own hhld inc 100K to 125K	2.198*	(1.163)
1=Own age is 35 to 44	0.216	(0.247)
1=Own age is 35 to 44 × 1=Start: hour ending at 19:00	−3.231*	(1.770)
1=Own age is 35 to 44 × 1=Start: hour ending at 21:00	−2.837***	(0.972)

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Table C2 – continued from previous page

1=Own age is 45 to 54	0.205	(0.290)
1=Own age is 55 and up	0.153	(0.385)
1=Own hhld inc less than 20K	0.404	(0.562)
1=Own hhld inc less than 20K × 1=Used a mobile device	−0.493	(0.540)
1=Own hhld inc less than 20K × County deaths/50k Jul 2020	−0.182**	(0.093)
1=Own hhld inc 20K to 25K	1.963*	(1.029)
1=Own hhld inc 20K to 25K × 1=Start: hour ending at 15:00	−1.521	(1.571)
1=Own hhld inc 20K to 25K × 1=Own race white	−1.576*	(0.864)
1=Own hhld inc 20K to 25K × County pr. other intnt	−52.940	(57.599)
1=Own hhld inc 30K to 50K	0.921	(0.674)
1=Started survey on Saturday × 1=Own hhld inc 30K to 50K	−1.369**	(0.651)
1=Own hhld inc 30K to 50K × County cases/50k Jul 2020	−0.005**	(0.002)
1=Own hhld inc 30K to 50K × County cases/50k Jan 2021	0.0003	(0.001)
1=Own hhld inc 100K to 125K	−0.784**	(0.374)
1=Used a mobile device × 1=Own hhld inc 100K to 125K	1.287**	(0.643)
1=Own hhld inc 125K to 150K	−5.479**	(2.343)
1=Start: hour ending at 7:00 × 1=Own hhld inc 125K to 150K	−8.139***	(3.003)
1=Own hhld inc 125K to 150K × County unemp rate Mar'20	0.298	(0.381)
1=Own hhld inc 125K to 150K × County pr. rural	18.081*	(10.176)
1=Own hhld inc 125K to 150K × County pr. dialup cmptr	222.663	(558.748)
1=Own hhld inc 125K to 150K × County pr. no intnt cmptr	61.696	(42.617)
1=Started survey on Monday	0.287	(0.352)
1=Started survey on Monday × County 2018 population	−0.0001*	(0.00005)
1=Started survey on Tuesday	−0.653	(0.683)
1=Started survey on Tuesday × 1=Start: hour ending at 7:00	−18.173	(1,702.151)
1=Started survey on Tuesday × Zip pr: age 18-24	4.800	(6.288)
1=Started survey on Tuesday × County deaths/50k Sep 2020	0.205	(0.128)
1=Started survey on Wednesday	−0.264	(0.394)
1=Started survey on Wednesday × 1=Start: hour ending at 14:00	−17.882	(1,715.373)
1=Started survey on Wednesday × 1=Start: hour ending at 17:00	−1.002	(0.638)
1=Started survey on Friday	−0.986*	(0.507)
1=Started survey on Saturday	0.507	(0.418)
1=Start: hour ending at 21:00	−0.262	(0.457)
1=Start: hour ending at 23:00	0.156	(0.815)
1=Start: hour ending at 23:00 × County pr. rural	−2.050	(2.416)
1=Start: hour ending at 7:00	0.268	(0.771)
1=Start: hour ending at 9:00	−0.541	(0.438)
1=Start: hour ending at 14:00	−0.334	(0.292)
1=Start: hour ending at 15:00	−0.238	(0.307)
1=Start: hour ending at 15:00 × 1=Own age is 45 to 54	−1.754***	(0.678)
1=Start: hour ending at 17:00	1.394**	(0.639)
1=Start: hour ending at 17:00 × 1=Own race white	−1.546***	(0.598)
1=Start: hour ending at 19:00	−0.412	(0.391)
Zip pr: vote republican	−3.379**	(1.722)
1=Used a mobile device × Zip pr: vote republican	0.022	(1.425)
Days since first case, in 100s	1.771	(3.932)
Days since first case, in 100s × 1=Have ZIP code data	−0.243	(3.734)
1=At least one county death	7.406	(8.193)
Zip pr: age 55-64 × 1=At least one county death	−16.974	(45.388)

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Table C2 – continued from previous page

Days since first death, in 100s	−4.556**	(2.058)
1=Have ZIP code data × Days since first death, in 100s	4.711**	(2.086)
Cases/50K last 4 weeks	0.003	(0.002)
Zip pr: age 55-64 × Cases/50K last 4 weeks	0.007	(0.008)
Cases/50K last 4 weeks × Zip pr: indus = transp.	−0.002	(0.010)
Zip pr: age 18-24	−1.292	(6.471)
Zip pr: age 55-64	50.383	(44.900)
Zip pr. urban (if havzip==1) × Zip pr: age 55-64	6.561	(16.008)
Zip pr: indus = agric.	−0.705	(2.785)
Zip pr: indus = wholes.	−16.712	(20.162)
Zip pr: indus = wholes. × County cases/50k Jun 2020	0.074	(0.080)
Zip pr: indus = wholes. × County pr. intnt not subscr	1,010.169	(872.524)
Zip pr: indus = retail.	−14.775*	(8.659)
Zip pr: indus = retail. × County cases/50k Oct 2020	0.129**	(0.059)
Zip pr: indus = transp.	−17.816**	(7.992)
Zip pr: indus = transp. × County deaths/50k Aug 2020	3.813**	(1.934)
Zip pr: indus = edu. serv.	1.974	(5.301)
Zip pr: indus = arts, ent.	−1.380	(6.897)
County unemp rate Nov’20	−0.027	(0.183)
County unemp rate Mar’20	0.053	(0.085)
County cases/50k Jun 2020	0.002	(0.003)
County cases/50k Jul 2020	0.0004	(0.002)
County cases/50k Aug 2020	0.0002	(0.003)
County cases/50k Oct 2020	−0.014**	(0.007)
County cases/50k Jan 2021	−0.002**	(0.001)
County deaths/50k Jul 2020	0.031	(0.107)
County deaths/50k Aug 2020	−0.179	(0.110)
County deaths/50k Sep 2020	0.014	(0.087)
Zip pr. urban (if havzip==1)	−24.499	(717.006)
1=Used a mobile device × Zip pr. urban (if havzip==1)	15.607	(716.903)
Zip pr. urban (if havzip==1) × County pr. broadband cmptr	−1.740	(13.642)
County pr. other intnt	20.184	(19.224)
County pr. intnt not subscr	−11.215	(24.551)
County pr. dialup cmptr	−100.434	(106.977)
County pr. broadband cmptr	−6.598	(16.954)
County pr. no intnt cmptr	2.704	(20.853)
County 2018 population	−0.0002	(0.0001)
County pr. rural	2.437	(1.723)
1=Used a mobile device	−16.092	(716.903)
Zip pr. rural (if havzip==1)	−22.074	(716.910)
1=Have ZIP code data	−2.147	(9.896)
1=Used a mobile device × Zip pr. rural (if havzip==1)	13.761	(716.905)
Constant	19.985	(717.258)
Observations	1,412	
Log Likelihood	−504.736	
Akaike Inf. Crit.	1,255.472	

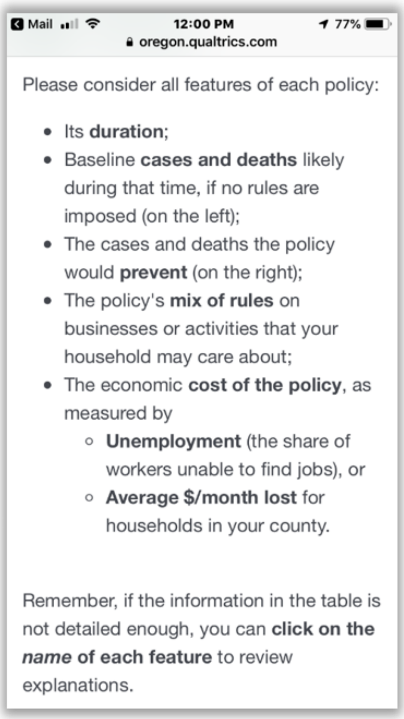
D Online Appendix: One example of a choice set, with pop-ups

D.1 One example of a policy-choice summary table

This is a two-month policy, with total cases and deaths over that two-month period, without and with Policy A. We convert these to per-month cases and deaths for analysis, commensurate with county unemployment rate and average cost per household stemming from this unemployment. This instance has very high county-level unemployment, but average $/month lost for county households is limited by unemployment insurance and federal unemployment insurance supplements, in the amount of $200/week, in this instance.

The sets of images below show the first choice task in the survey, for this one instance of the randomizations, as displayed on the screen of a mobile device. These tasks are preceded by an extensive tutorial about how to interpret these compact policy scenarios.

Figure D1: Immediate preamble to first summary table (one example)



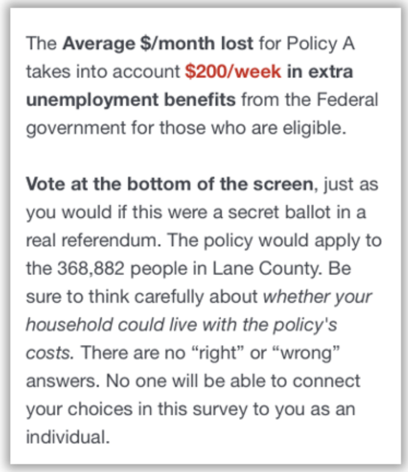


Figure D2: One instance of Policy A; contents of first three pop-ups

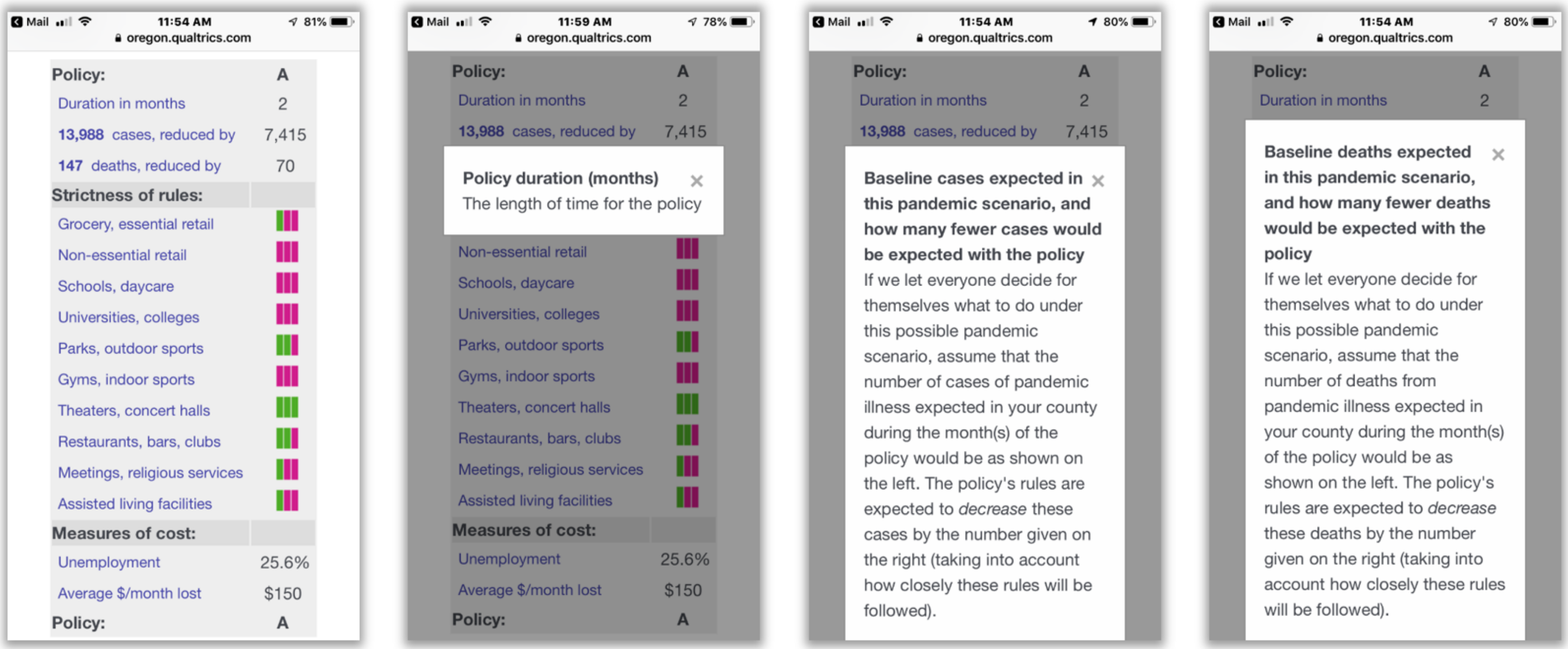


Figure D3: Contents of fourth through seventh popups

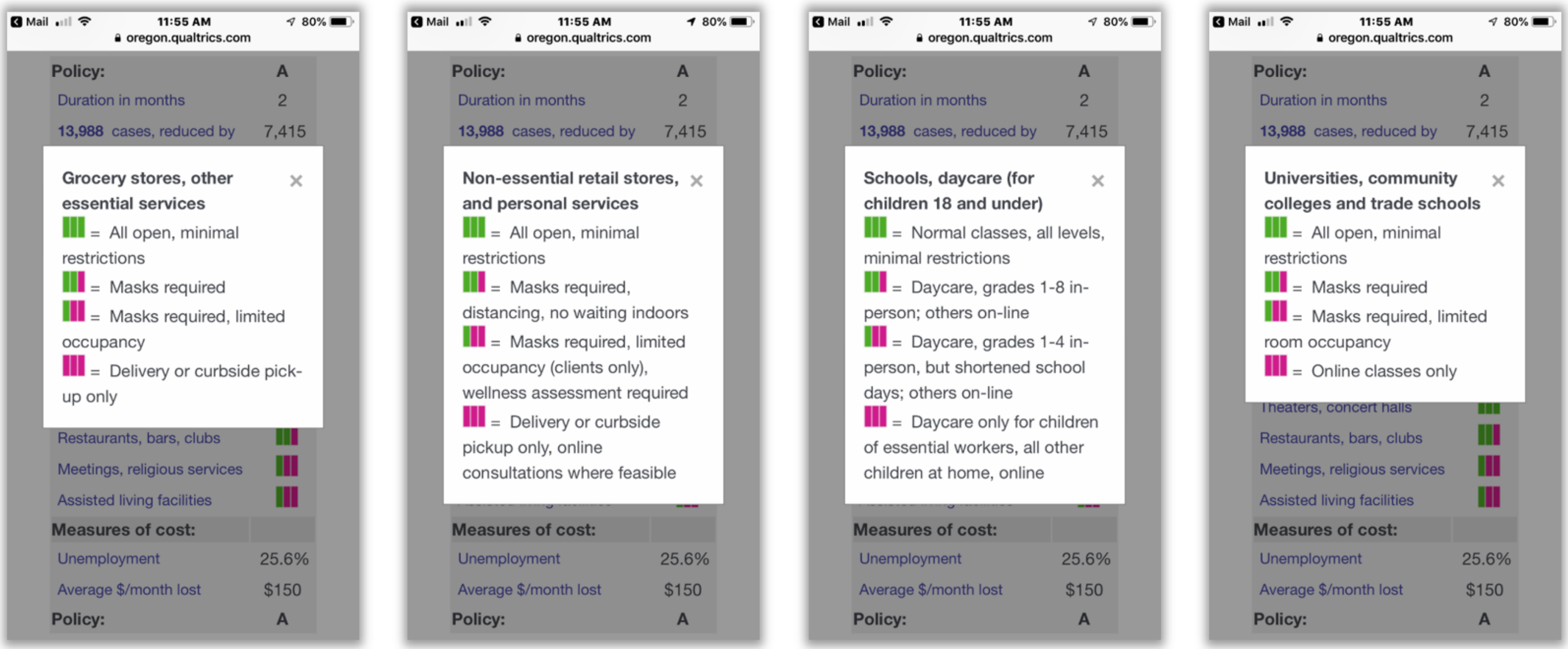


Figure D4: Contents of eighth through eleventh popups

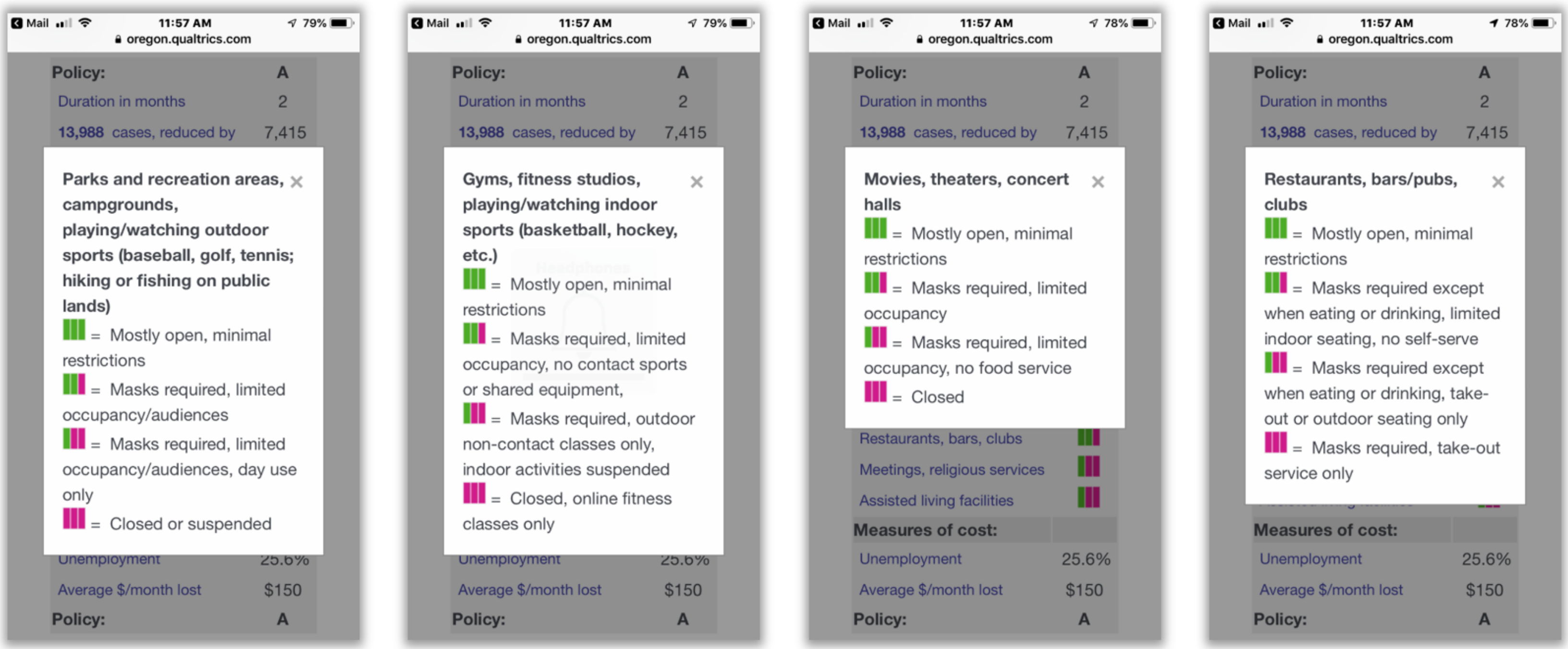


Figure D5: Contents of twelfth through 15th popups

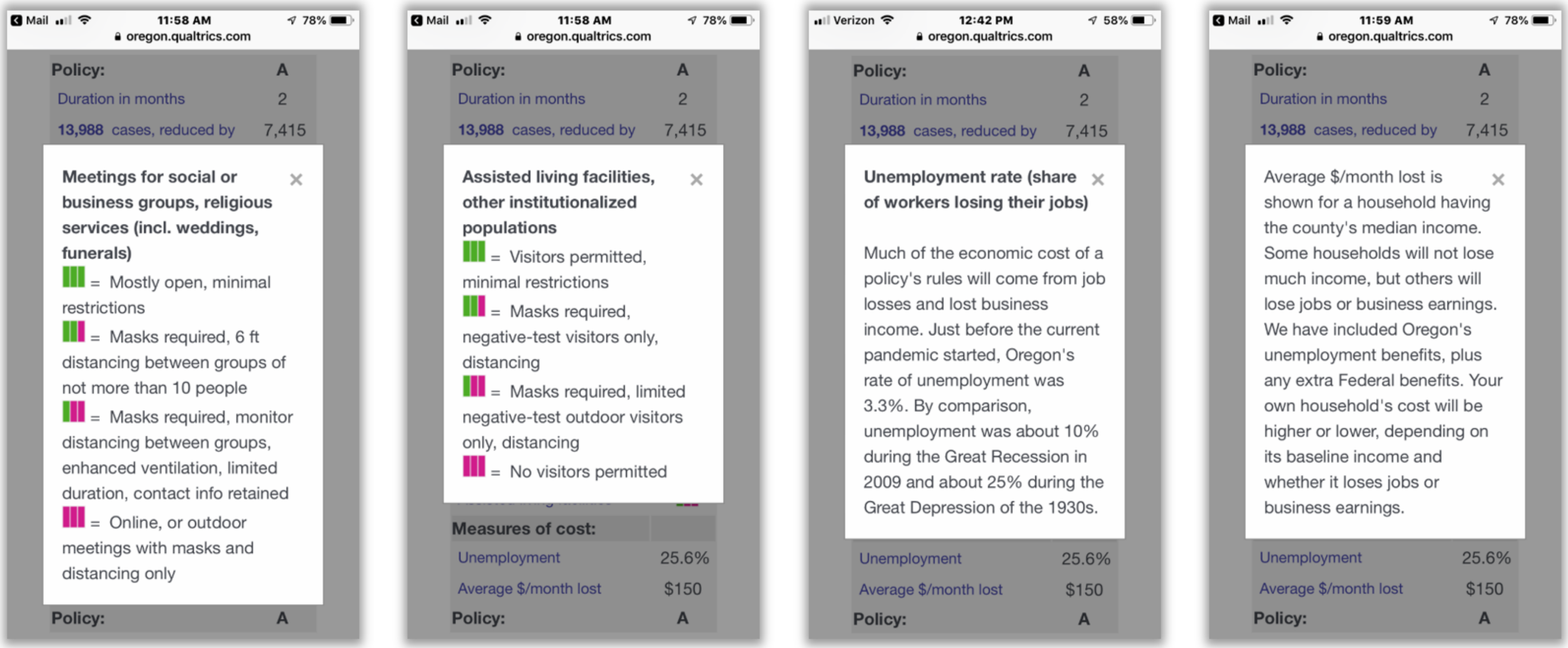
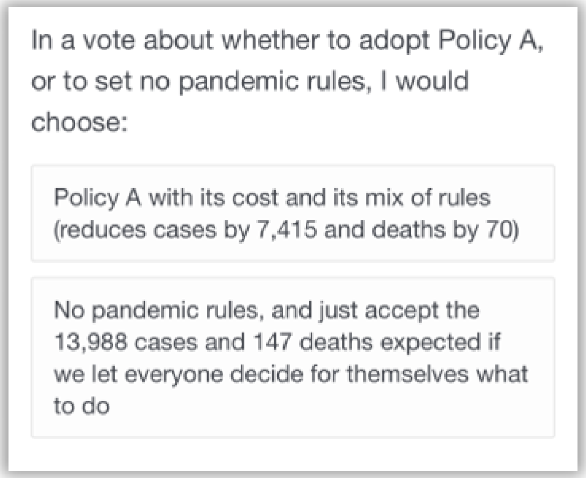


Figure D6: Choice question that immediately follows the table

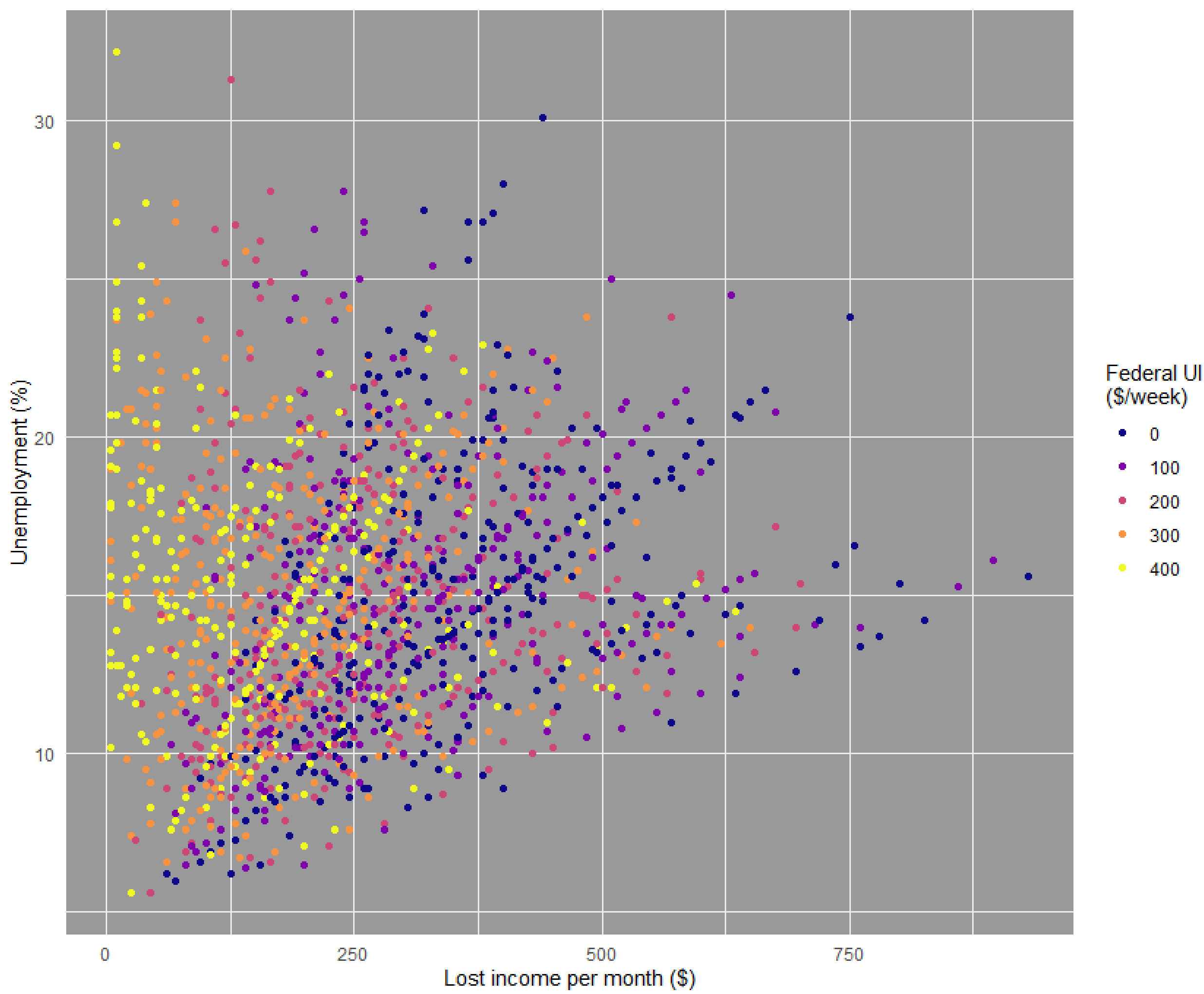

In a vote about whether to adopt Policy A, or to set no pandemic rules, I would choose:

Policy A with its cost and its mix of rules (reduces cases by 7,415 and deaths by 70)

No pandemic rules, and just accept the 13,988 cases and 147 deaths expected if we let everyone decide for themselves what to do

E Online Appendix: Joint distribution, key choice-set design features

Figure E1: Independent variation in average household costs and unemployment, by level of federal UI supplement



F Online Appendix: Complete estimation results

Table F1 provides descriptive statistics, across all offered county-level pandemic policies, for the levels of restrictions placed on ten different categories of activities and businesses. The statistics in this table assume that the levels of restrictions (level 0, 1, 2, or 3) are a cardinal variable. For each policy, these restriction levels are randomized, although some implausible combinations are excluded. For details, see the online supplementary materials that describe the attribute randomization in detail.[25]

Controls for these restrictions are included in all specifications in the body of the paper, either as cardinal variables (for example, Model 3 in Table 3), or as sets of indicators for each level (for example, Model 6 in Table 3). The coefficients on these variables (and interactions with these variables) are contained amongst the full sets of parameter estimates in Tables F2, F3 and F4 in this appendix.

Table F1: Descriptive statistics for restrictions on activities, across offered pandemic policies (included in this paper as incidental controls)

	Mean	SD	Min	Max
Grocery, essential retail	1.18	0.85	0	2
Non-essential retail	1.65	1.11	0	3
Schools, daycare	1.32	1.09	0	3
Parks, outdoor sports	1.34	1.09	0	3
Gyms, indoor sports	1.61	1.11	0	3
Theaters, concert halls	1.48	1.12	0	3
Restaurants, bars, clubs	1.53	1.11	0	3
Meetings, religious services	1.51	1.1	0	3
Assisted living facilities	1.62	1.11	0	3
Universities, colleges	1.49	1.12	0	3

[25]The randomization process for the choice sets is described at http://pages.uoregon.edu/cameron/UO_COVID_description_of_randomizations.pdf. The joint distributions of the randomized design variables are shown in the document at http://pages.uoregon.edu/cameron/UO_COVID_survey_orthogonality.pdf

Table F2: Full set of parameter estimates for the models reported in Table 3 in the body of the paper: Effects of Federal UI (selected coefficients)

	Dependent variable:					
	1=Preferred policy					
	(1)	(2)	(3)	(4)	(5)	(6)
Selected coefficients, as reported in Table 3 in the body of the paper:						
Avg. hhld cost for county	0.004			−0.018		
	(0.051)			(0.050)		
Avg. hhld cost for county (federal UI = 0)		−0.284***	−0.291***		−0.301***	−0.310***
		(0.093)	(0.093)		(0.098)	(0.098)
Avg. hhld cost for county (federal UI > 0)			0.174***			0.153**
			(0.066)			(0.065)
Avg. hhld cost for county (federal UI = 100)		0.125			0.088	
		(0.102)			(0.103)	
Avg. hhld cost for county (federal UI = 200)		0.131			0.105	
		(0.173)			(0.164)	
Avg. hhld cost for county (federal UI = 300)		0.665***			0.659***	
		(0.124)			(0.124)	
Avg. hhld cost for county (federal UI = 400)		0.166			0.194	
		(0.144)			(0.138)	
Unempl rate for county	−0.0003			0.002		
	(0.020)			(0.021)		
Unempl rate for county (federal UI = 0)		0.097**	0.093**		0.098**	0.092**
		(0.039)	(0.039)		(0.040)	(0.040)
Unempl rate for county (federal UI > 0)			−0.024			−0.023
			(0.020)			(0.020)
Unempl rate for county (federal UI = 100)		−0.028			−0.017	
		(0.034)			(0.035)	
Unempl rate for county (federal UI = 200)		−0.001			0.001	
		(0.045)			(0.043)	
Unempl rate for county (federal UI = 300)		−0.079***			−0.083***	
		(0.026)			(0.026)	
Unempl rate for county (federal UI = 400)		−0.015			−0.014	
		(0.025)			(0.026)	
Absolute ’00s cases/mo/50,000	−0.041	−0.036	−0.035	−0.042	−0.037	−0.036
	(0.032)	(0.031)	(0.031)	(0.032)	(0.032)	(0.031)
Absolute deaths/mo/50,000	−0.034*	−0.037*	−0.038*	−0.027	−0.026	−0.029
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
1=Status quo alternative	−1.982***	−2.008***	−1.994***	−2.433***	−2.552***	−2.472***
	(0.296)	(0.284)	(0.291)	(0.360)	(0.370)	(0.368)
Other coefficients, suppressed in Table 3 in the body of the paper:						
Grocery, essential retail	−0.045	−0.072	−0.056			
	(0.078)	(0.079)	(0.078)			
Non-essential retail	−0.059	−0.078	−0.069			
	(0.061)	(0.062)	(0.062)			
Schools, daycare	−0.138**	−0.153**	−0.144**			
	(0.067)	(0.069)	(0.068)			
Universities, colleges	−0.023	−0.025	−0.025			
	(0.059)	(0.060)	(0.059)			
Parks, outdoor sports	−0.058	−0.088	−0.080			
	(0.061)	(0.062)	(0.061)			
Gyms, indoor sports	−0.067	−0.078	−0.082			
	(0.059)	(0.060)	(0.059)			

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Table F2 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
Theaters, concert halls	−0.121**	−0.128**	−0.127**			
	(0.060)	(0.060)	(0.059)			
Restaurants, bars, clubs	−0.070	−0.085	−0.069			
	(0.062)	(0.063)	(0.063)			
Meetings, religious services	−0.021	−0.033	−0.028			
	(0.062)	(0.061)	(0.061)			
Assisted living facilities	0.059	0.052	0.060			
	(0.061)	(0.062)	(0.062)			
factor(Grocery, essential retail)1				−0.103	−0.165	−0.143
				(0.179)	(0.185)	(0.181)
factor(Grocery, essential retail)2				−0.155	−0.234	−0.185
				(0.162)	(0.167)	(0.164)
factor(Non-essential retail)1				−0.430**	−0.496**	−0.453**
				(0.198)	(0.201)	(0.200)
factor(Non-essential retail)2				0.023	−0.012	0.017
				(0.212)	(0.219)	(0.217)
factor(Non-essential retail)3				−0.378*	−0.440**	−0.394**
				(0.197)	(0.202)	(0.200)
factor(Schools, daycare)1				−0.114	−0.124	−0.111
				(0.180)	(0.182)	(0.178)
factor(Schools, daycare)2				−0.077	−0.078	−0.070
				(0.206)	(0.211)	(0.209)
factor(Schools, daycare)3				−0.466**	−0.473**	−0.457**
				(0.207)	(0.214)	(0.209)
factor(Universities, colleges)1				−0.190	−0.221	−0.219
				(0.176)	(0.180)	(0.179)
factor(Universities, colleges)2				−0.124	−0.117	−0.117
				(0.202)	(0.204)	(0.204)
factor(Universities, colleges)3				−0.134	−0.152	−0.155
				(0.195)	(0.199)	(0.197)
factor(Parks, outdoor sports)1				−0.117	−0.181	−0.154
				(0.180)	(0.184)	(0.181)
factor(Parks, outdoor sports)2				−0.206	−0.248	−0.223
				(0.185)	(0.190)	(0.187)
factor(Parks, outdoor sports)3				−0.112	−0.217	−0.178
				(0.209)	(0.210)	(0.207)
factor(Gyms, indoor sports)1				−0.028	−0.076	−0.072
				(0.211)	(0.215)	(0.211)
factor(Gyms, indoor sports)2				−0.334*	−0.360*	−0.361*
				(0.201)	(0.205)	(0.204)
factor(Gyms, indoor sports)3				−0.181	−0.203	−0.220
				(0.201)	(0.208)	(0.203)
factor(Theaters, concert halls)1				−0.038	−0.048	−0.030
				(0.191)	(0.194)	(0.190)
factor(Theaters, concert halls)2				−0.287	−0.249	−0.257
				(0.188)	(0.193)	(0.192)
factor(Theaters, concert halls)3				−0.281	−0.327*	−0.307
				(0.191)	(0.196)	(0.191)
factor(Restaurants, bars, clubs)1				−0.104	−0.128	−0.108
				(0.196)	(0.202)	(0.197)
factor(Restaurants, bars, clubs)2				−0.076	−0.152	−0.124
				(0.190)	(0.194)	(0.192)
factor(Restaurants, bars, clubs)3				−0.196	−0.260	−0.197
				(0.202)	(0.210)	(0.207)
factor(Meetings, religious services)1				−0.221	−0.247	−0.226
				(0.186)	(0.189)	(0.186)
factor(Meetings, religious services)2				−0.156	−0.162	−0.158
				(0.194)	(0.196)	(0.192)
factor(Meetings, religious services)3				−0.072	−0.095	−0.077

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Table F2 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
				(0.204)	(0.200)	(0.200)
factor(Assisted living facilities)1				−0.124	−0.166	−0.122
				(0.197)	(0.202)	(0.199)
factor(Assisted living facilities)2				0.047	0.010	0.044
				(0.206)	(0.208)	(0.207)
factor(Assisted living facilities)3				0.129	0.083	0.127
				(0.201)	(0.210)	(0.206)
$\hat{RP}$ × Avg. hhld costs	0.085***			0.087***		
	(0.031)			(0.032)		
$\hat{RP}$ × Avg. hhld costs (federal UI = 0)		0.396*	0.377*		0.497*	0.476*
		(0.216)	(0.216)		(0.273)	(0.271)
$\hat{RP}$ × Avg. hhld costs (federal UI = 100)		−0.004			−0.015	
		(0.165)			(0.188)	
$\hat{RP}$ × Avg. hhld costs (federal UI = 200)		−0.241			−0.150	
		(0.193)			(0.183)	
$\hat{RP}$ × Avg. hhld costs (federal UI = 300)		−0.167			0.0005	
		(0.209)			(0.214)	
$\hat{RP}$ × Avg. hhld costs (federal UI = 400)		0.183			0.105	
		(0.214)			(0.243)	
$\hat{RP}$ × Avg. hhld costs (federal UI > 0)			−0.067			0.002
			(0.092)			(0.097)
$\hat{RP}$ × Unempl rate	0.104			0.193***		
	(0.085)			(0.074)		
$\hat{RP}$ × Unempl rate (federal UI = 0)		0.024	0.023		0.023	0.025
		(0.071)	(0.071)		(0.092)	(0.090)
$\hat{RP}$ × Unempl rate (federal UI = 100)		0.108*			0.103*	
		(0.058)			(0.059)	
$\hat{RP}$ × Unempl rate (federal UI = 200)		0.152**			0.147***	
		(0.062)			(0.049)	
$\hat{RP}$ × Unempl rate (federal UI = 300)		0.108**			0.106*	
		(0.047)			(0.058)	
$\hat{RP}$ × Unempl rate (federal UI = 400)		0.079**			0.083**	
		(0.031)			(0.037)	
$\hat{RP}$ × Unempl rate (federal UI > 0)			0.101***			0.104***
			(0.032)			(0.033)
$\hat{RP}$ × '00s cases/mo/50,000	−0.319	−0.271	−0.343	−0.553	−0.507	−0.575
	(0.485)	(0.353)	(0.369)	(0.388)	(0.421)	(0.367)
$\hat{RP}$ × deaths/mo/50,000	−0.014	−0.023	−0.022	−0.040	−0.066**	−0.056*
	(0.024)	(0.024)	(0.026)	(0.027)	(0.033)	(0.030)
$\hat{RP}$ × 1=Status quo alternative	0.616	0.655**	0.573	1.165**	1.360**	1.111**
	(0.436)	(0.326)	(0.372)	(0.504)	(0.543)	(0.522)
$\hat{RP}$ × Grocery, essential retail	−0.147	−0.135	−0.138			
	(0.097)	(0.100)	(0.104)			
$\hat{RP}$ × Non-essential retail	0.114	0.105	0.099			
	(0.082)	(0.085)	(0.080)			
$\hat{RP}$ × Schools, daycare	−0.067	−0.070	−0.071			
	(0.097)	(0.098)	(0.097)			
$\hat{RP}$ × Universities, colleges	−0.070	−0.089	−0.083			
	(0.083)	(0.085)	(0.079)			
$\hat{RP}$ × Parks, outdoor sports	0.007	0.037	0.025			
	(0.096)	(0.090)	(0.087)			
$\hat{RP}$ × Gyms, indoor sports	−0.012	0.038	0.034			
	(0.078)	(0.087)	(0.083)			
$\hat{RP}$ × Theaters, concert halls	−0.032	−0.014	−0.014			
	(0.083)	(0.076)	(0.077)			
$\hat{RP}$ × Restaurants, bars, clubs	−0.068	−0.075	−0.095			
	(0.084)	(0.080)	(0.086)			
$\hat{RP}$ × Meetings, religious services	−0.084	−0.057	−0.062			
	(0.084)	(0.084)	(0.083)			

Continued on next page

Table F2 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{RP}$ × Assisted living facilities	−0.199**	−0.239***	−0.228***			
	(0.081)	(0.079)	(0.080)			
$\hat{RP}$ × factor(Grocery, essential retail)1				−0.269	−0.210	−0.256
				(0.242)	(0.270)	(0.241)
$\hat{RP}$ × factor(Grocery, essential retail)2				−0.234	−0.130	−0.241
				(0.245)	(0.254)	(0.249)
$\hat{RP}$ × factor(Non-essential retail)1				0.936***	0.988***	0.951***
				(0.310)	(0.318)	(0.305)
$\hat{RP}$ × factor(Non-essential retail)2				0.876***	0.828**	0.821***
				(0.327)	(0.327)	(0.316)
$\hat{RP}$ × factor(Non-essential retail)3				0.676**	0.635**	0.590*
				(0.313)	(0.319)	(0.309)
$\hat{RP}$ × factor(Schools, daycare)1				−0.024	−0.056	−0.018
				(0.266)	(0.283)	(0.263)
$\hat{RP}$ × factor(Schools, daycare)2				−0.268	−0.349	−0.278
				(0.298)	(0.312)	(0.292)
$\hat{RP}$ × factor(Schools, daycare)3				−0.229	−0.419	−0.339
				(0.311)	(0.340)	(0.312)
$\hat{RP}$ × factor(Universities, colleges)1				−0.055	0.063	0.022
				(0.261)	(0.263)	(0.258)
$\hat{RP}$ × factor(Universities, colleges)2				0.117	0.067	0.056
				(0.354)	(0.324)	(0.326)
$\hat{RP}$ × factor(Universities, colleges)3				−0.361	−0.391	−0.332
				(0.299)	(0.294)	(0.275)
$\hat{RP}$ × factor(Parks, outdoor sports)1				0.509**	0.581**	0.504**
				(0.254)	(0.292)	(0.249)
$\hat{RP}$ × factor(Parks, outdoor sports)2				0.594**	0.724**	0.599**
				(0.292)	(0.316)	(0.291)
$\hat{RP}$ × factor(Parks, outdoor sports)3				−0.166	−0.062	−0.109
				(0.321)	(0.327)	(0.316)
$\hat{RP}$ × factor(Gyms, indoor sports)1				−0.229	−0.023	−0.073
				(0.299)	(0.309)	(0.283)
$\hat{RP}$ × factor(Gyms, indoor sports)2				0.642**	0.746**	0.765**
				(0.294)	(0.311)	(0.301)
$\hat{RP}$ × factor(Gyms, indoor sports)3				−0.162	−0.042	−0.050
				(0.305)	(0.319)	(0.309)
$\hat{RP}$ × factor(Theaters, concert halls)1				−0.647**	−0.710**	−0.692***
				(0.268)	(0.285)	(0.259)
$\hat{RP}$ × factor(Theaters, concert halls)2				−0.308	−0.483*	−0.405
				(0.270)	(0.270)	(0.262)
$\hat{RP}$ × factor(Theaters, concert halls)3				−0.559*	−0.436	−0.462
				(0.297)	(0.307)	(0.291)
$\hat{RP}$ × factor(Restaurants, bars, clubs)1				−0.107	−0.094	−0.111
				(0.316)	(0.317)	(0.308)
$\hat{RP}$ × factor(Restaurants, bars, clubs)2				−0.567**	−0.455	−0.526*
				(0.271)	(0.280)	(0.277)
$\hat{RP}$ × factor(Restaurants, bars, clubs)3				−0.421	−0.357	−0.433
				(0.307)	(0.310)	(0.308)
$\hat{RP}$ × factor(Meetings, religious services)1				0.137	0.092	0.054
				(0.311)	(0.344)	(0.310)
$\hat{RP}$ × factor(Meetings, religious services)2				−0.265	−0.280	−0.296
				(0.283)	(0.303)	(0.278)
$\hat{RP}$ × factor(Meetings, religious services)3				−0.262	−0.236	−0.291
				(0.310)	(0.317)	(0.308)
$\hat{RP}$ × factor(Assisted living facilities)1				−0.468	−0.430	−0.509
				(0.349)	(0.352)	(0.342)
$\hat{RP}$ × factor(Assisted living facilities)2				−0.292	−0.297	−0.366
				(0.333)	(0.341)	(0.328)

Continued on next page

Table F2 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{RP}$ × factor(Assisted living facilities)3				-0.988^{***}	-1.033^{***}	-1.063^{***}
				(0.302)	(0.315)	(0.306)
Restrictions on activities (continuous variables)	✓	✓	✓			
Restrictions on activities (sets of indicators)				✓	✓	✓
Respondents	993	993	993	993	993	993
Choices	1986	1986	1986	1986	1986	1986
Log likelihood	-1205.77	-1184.09	-1194.52	-1180.75	-1158.56	-1169.8

Notes: *p<0.1; **p<0.05; ***p<0.01

Table F3: Full sets of parameter estimates for the models reported in Panel A of Table 4 in the body of the paper: Heterogeneity in preferences across socioeconomic groups

	18 to 34	35 to 64	65 +	Non-white	White	Women	Men
Dep. var: 1=Preferred policy	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Avg. hhld cost (federal UI = 0)	-0.597^{***}	-0.382^{***}	-0.307	-0.842^{***}	-0.228^{**}	-0.497^{***}	-0.220^{*}
	(0.224)	(0.139)	(0.376)	(0.294)	(0.112)	(0.165)	(0.114)
Avg. hhld cost (federal UI > 0)	0.251^{**}	0.212^{*}	0.181	0.052	0.216^{***}	0.155	0.134
	(0.124)	(0.119)	(0.140)	(0.126)	(0.079)	(0.106)	(0.082)
Unempl rate (federal UI = 0)	0.251^{***}	0.080	0.219	0.268^{**}	0.090^{*}	0.141^{**}	0.093
	(0.093)	(0.055)	(0.161)	(0.118)	(0.046)	(0.064)	(0.057)
Unempl rate (federal UI > 0)	0.023	-0.066^{**}	-0.012	0.011	-0.029	-0.049	0.020
	(0.044)	(0.031)	(0.069)	(0.050)	(0.024)	(0.036)	(0.029)
Absolute ’00s cases/mo/50,000	-0.047	0.068	-0.045	-0.055	-0.048	-0.086^{*}	0.008
	(0.055)	(0.049)	(0.099)	(0.071)	(0.040)	(0.044)	(0.049)
Absolute deaths/mo/50,000	-0.074^{**}	-0.011	-0.059	-0.070^{*}	-0.001	0.001	-0.061^{**}
	(0.033)	(0.035)	(0.056)	(0.039)	(0.027)	(0.032)	(0.028)
1=Status quo alternative	-4.533^{***}	-1.943^{***}	0.966	-2.603^{***}	-2.584^{***}	-2.925^{***}	-2.039^{***}
	(0.729)	(0.559)	(1.211)	(0.761)	(0.443)	(0.557)	(0.508)
Other coefficients, suppressed in Table 4 Panel A in the body of the paper:							
Grocery, essential retail.1	-0.556	0.375	-0.835	0.110	-0.313	-0.058	-0.055
	(0.342)	(0.295)	(0.537)	(0.334)	(0.229)	(0.258)	(0.276)
Grocery, essential retail.2	-1.117^{***}	0.251	2.156^{***}	0.149	-0.239	-0.192	-0.217
	(0.373)	(0.335)	(0.624)	(0.410)	(0.265)	(0.311)	(0.291)
Non-essential retail.1	-0.656^{**}	-0.022	-0.825	0.374	-0.476^{**}	-0.011	-0.382
	(0.300)	(0.254)	(0.540)	(0.317)	(0.202)	(0.234)	(0.240)
Non-essential retail.2	-0.454	-0.019	0.762	-0.625	0.281	-0.522	0.336
	(0.410)	(0.293)	(0.539)	(0.497)	(0.250)	(0.338)	(0.291)
Non-essential retail.3	-0.954^{**}	0.230	-0.360	-0.276	-0.462^{**}	-0.543^{**}	0.112
	(0.371)	(0.277)	(0.514)	(0.366)	(0.226)	(0.269)	(0.272)
Schools, daycare.1	-0.127	-0.677^{**}	-0.700	-0.165	-0.777^{***}	-0.325	-0.424
	(0.374)	(0.282)	(0.631)	(0.378)	(0.258)	(0.308)	(0.287)
Schools, daycare.2	-0.558	-0.131	0.077	-0.762	-0.163	-0.668^{**}	0.005
	(0.398)	(0.301)	(0.659)	(0.465)	(0.237)	(0.327)	(0.277)
Schools, daycare.3	-1.117^{***}	0.297	0.962	-0.059	-0.058	-0.228	-0.039
	(0.351)	(0.295)	(0.686)	(0.358)	(0.250)	(0.299)	(0.285)
Universities, colleges.1	-1.016^{***}	0.119	1.374^{**}	-0.005	-0.291	-0.331	-0.0003
	(0.357)	(0.277)	(0.563)	(0.383)	(0.234)	(0.283)	(0.249)
Universities, colleges.2	-0.254	-0.301	1.165^{*}	-0.030	-0.114	-0.074	-0.356
	(0.342)	(0.344)	(0.648)	(0.421)	(0.245)	(0.323)	(0.294)
Universities, colleges.3	-1.201^{***}	0.104	0.164	-0.134	-0.197	0.068	-0.602^{**}
	(0.413)	(0.317)	(0.606)	(0.422)	(0.237)	(0.315)	(0.292)
Parks, outdoor sports.1	-0.153	0.072	0.751	0.503	-0.289	0.157	-0.005
	(0.384)	(0.376)	(0.667)	(0.445)	(0.266)	(0.325)	(0.300)
Parks, outdoor sports.2	-0.837^{**}	0.094	-0.260	-0.391	-0.093	-0.612^{*}	0.026
	(0.381)	(0.300)	(0.698)	(0.458)	(0.231)	(0.313)	(0.284)
Parks, outdoor sports.3	-0.403	0.208	-0.277	0.153	-0.240	0.152	-0.396
	(0.389)	(0.277)	(0.434)	(0.432)	(0.242)	(0.306)	(0.277)
Gyms, indoor sports.1	-0.876^{**}	-0.163	-0.806	0.405	-0.768^{***}	-0.181	-0.506^{*}
	(0.399)	(0.311)	(0.508)	(0.397)	(0.251)	(0.317)	(0.284)
Gyms, indoor sports.2	-0.583^{*}	0.329	0.114	-0.780^{*}	0.157	-0.065	-0.111
	(0.325)	(0.289)	(0.457)	(0.429)	(0.229)	(0.269)	(0.304)
Gyms, indoor sports.3	0.809^{**}	-0.645^{**}	0.426	-0.236	-0.227	0.259	-0.546^{*}
	(0.343)	(0.305)	(0.595)	(0.384)	(0.249)	(0.283)	(0.295)
Theaters, concert halls.1	-0.861^{***}	0.316	-0.315	-0.428	-0.096	-0.433^{*}	0.326

Continued on next page

Table F3 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(0.315)	(0.285)	(0.567)	(0.376)	(0.225)	(0.251)	(0.258)
Theaters, concert halls.2	-0.888^{***}	-0.072	1.426^{**}	-0.864^{**}	-0.014	-0.093	-0.598^{**}
	(0.325)	(0.312)	(0.667)	(0.420)	(0.239)	(0.276)	(0.289)
Theaters, concert halls.3	0.742^{**}	-0.242	0.674	0.134	0.042	0.500	-0.473
	(0.361)	(0.319)	(0.627)	(0.446)	(0.254)	(0.310)	(0.305)
Restaurants, bars, clubs.1	-0.561	-0.016	0.892	-0.378	-0.074	-0.089	0.061
	(0.373)	(0.320)	(0.678)	(0.420)	(0.255)	(0.332)	(0.307)
Restaurants, bars, clubs.2	-1.023^{***}	0.093	-1.016^{*}	-1.098^{***}	-0.067	-0.134	-0.735^{***}
	(0.361)	(0.303)	(0.596)	(0.374)	(0.248)	(0.270)	(0.284)
Restaurants, bars, clubs.3	0.734^{**}	-0.077	1.056	0.028	0.072	0.387	-0.305
	(0.358)	(0.327)	(0.672)	(0.412)	(0.246)	(0.302)	(0.294)
Meetings, religious services.1	-0.919^{**}	-0.071	-0.379	-0.823^{**}	-0.332	-0.579^{*}	-0.449
	(0.397)	(0.333)	(0.732)	(0.407)	(0.261)	(0.311)	(0.296)
Meetings, religious services.2	-0.224	0.614	0.290	-0.219	-0.020	-0.376	0.209
	(0.340)	(0.384)	(0.531)	(0.399)	(0.242)	(0.267)	(0.315)
Meetings, religious services.3	-1.345^{***}	0.098	0.049	-0.533	-0.172	-0.491^{*}	-0.127
	(0.403)	(0.286)	(0.510)	(0.371)	(0.223)	(0.270)	(0.274)
Assisted living facilities.1	-0.860^{**}	0.305	1.611^{***}	0.139	-0.198	-0.248	0.117
	(0.360)	(0.288)	(0.493)	(0.351)	(0.226)	(0.277)	(0.268)
Assisted living facilities.2	0.222	-0.204	1.291^{*}	-0.018	-0.118	-0.234	0.132
	(0.371)	(0.349)	(0.696)	(0.384)	(0.230)	(0.284)	(0.289)
Assisted living facilities.3	-1.445^{***}	0.338	2.134^{***}	-0.725^{*}	0.177	-0.444	0.175
	(0.403)	(0.324)	(0.613)	(0.405)	(0.260)	(0.311)	(0.317)
Avg. hhld cost (federal UI > 0) × $\hat{RP}$	0.221	-0.094	-1.737^{***}	-0.428	0.017	0.717^{***}	-0.229^{*}
	(0.186)	(0.208)	(0.456)	(0.401)	(0.112)	(0.250)	(0.130)
Avg. hhld cost (federal UI = 0) × $\hat{RP}$	0.573	0.304	0.017	1.201	0.301	1.106^{***}	0.087
	(0.725)	(0.370)	(0.979)	(0.778)	(0.349)	(0.406)	(0.426)
Unempl rate (federal UI > 0) × $\hat{RP}$	-0.016	0.291^{***}	0.307	0.103	0.144^{***}	0.161^{***}	0.029
	(0.080)	(0.069)	(0.234)	(0.103)	(0.043)	(0.053)	(0.069)
Unempl rate (federal UI = 0) × $\hat{RP}$	-0.182	0.288^{*}	-0.364	-0.389	0.143	-0.108	0.081
	(0.259)	(0.152)	(0.279)	(0.291)	(0.125)	(0.144)	(0.161)
Absolute ’00s cases/mo/50,000 × $\hat{RP}$	-0.882	-2.055^{***}	-4.835^{*}	0.166	-0.795^{*}	-0.158	-1.185^{*}
	(1.126)	(0.663)	(2.804)	(1.792)	(0.420)	(0.861)	(0.609)
Absolute deaths/mo/50,000 × $\hat{RP}$	0.030	-0.160^{**}	-0.152	-0.085	-0.081^{**}	-0.168^{***}	-0.042
	(0.068)	(0.066)	(0.135)	(0.105)	(0.032)	(0.059)	(0.044)
Grocery, essential retail.1 × $\hat{RP}$	-0.329	-1.112^{**}	4.400^{***}	-0.112	-0.082	-0.904^{*}	0.081
	(0.616)	(0.538)	(1.667)	(0.966)	(0.299)	(0.499)	(0.457)
Grocery, essential retail.2 × $\hat{RP}$	2.067^{***}	-1.724^{**}	-5.040^{***}	-2.513^{**}	-0.005	-0.906	-0.331
	(0.629)	(0.681)	(1.508)	(1.211)	(0.372)	(0.643)	(0.455)
Non-essential retail.1 × $\hat{RP}$	0.018	-0.422	5.049^{***}	-1.585^{*}	0.222	-0.432	-0.217
	(0.519)	(0.507)	(1.539)	(0.936)	(0.312)	(0.487)	(0.375)
Non-essential retail.2 × $\hat{RP}$	-0.799	1.160^{**}	-2.743^{*}	-0.063	-0.393	0.413	-0.583
	(0.693)	(0.507)	(1.405)	(1.165)	(0.330)	(0.545)	(0.431)
Non-essential retail.3 × $\hat{RP}$	1.068^{*}	-1.011^{*}	-0.305	1.204	0.097	0.275	-0.487
	(0.623)	(0.529)	(1.331)	(1.108)	(0.353)	(0.558)	(0.475)
Schools, daycare.1 × $\hat{RP}$	1.621^{**}	1.389^{***}	1.950	1.222	1.149^{***}	-0.265	1.166^{***}
	(0.717)	(0.462)	(1.637)	(0.987)	(0.393)	(0.634)	(0.436)
Schools, daycare.2 × $\hat{RP}$	0.828	-0.059	-0.337	1.900^{**}	0.425	1.665^{***}	-0.750
	(0.860)	(0.542)	(1.407)	(0.966)	(0.399)	(0.600)	(0.530)
Schools, daycare.3 × $\hat{RP}$	1.412^{**}	-1.460^{***}	-3.425^{**}	-0.927	-0.519	-0.848	-0.640
	(0.590)	(0.552)	(1.588)	(1.038)	(0.327)	(0.577)	(0.506)
Universities, colleges.1 × $\hat{RP}$	0.956	0.077	-0.130	-0.086	0.372	0.436	0.749^{*}
	(0.654)	(0.451)	(1.502)	(0.982)	(0.382)	(0.517)	(0.431)
Universities, colleges.2 × $\hat{RP}$	0.126	-1.597^{***}	-1.851	-2.014^{*}	-0.475	-1.136^{*}	-0.133
	(0.636)	(0.536)	(1.433)	(1.051)	(0.337)	(0.653)	(0.490)
Universities, colleges.3 × $\hat{RP}$	0.760	0.248	-4.176^{***}	0.813	-0.109	0.542	-0.018
	(0.534)	(0.559)	(1.473)	(0.973)	(0.345)	(0.529)	(0.469)

Continued on next page

Table F3 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Parks, outdoor sports.1 × $\hat{RP}$	2.042^{***}	0.811	0.029	0.177	1.102^{***}	−0.307	1.444^{***}
	(0.694)	(0.611)	(1.771)	(1.075)	(0.369)	(0.627)	(0.506)
Parks, outdoor sports.2 × $\hat{RP}$	−0.223	-1.389^{**}	3.066^{*}	−0.936	−0.216	−0.674	0.123
	(0.717)	(0.554)	(1.789)	(0.980)	(0.348)	(0.534)	(0.513)
Parks, outdoor sports.3 × $\hat{RP}$	0.429	-1.967^{***}	−0.050	−0.620	−0.171	−0.785	0.305
	(0.621)	(0.556)	(1.818)	(1.050)	(0.361)	(0.551)	(0.482)
Gyms, indoor sports.1 × $\hat{RP}$	1.468^{**}	0.315	1.910	−1.472	1.164^{***}	-1.293^{**}	1.414^{***}
	(0.677)	(0.606)	(1.273)	(1.077)	(0.350)	(0.658)	(0.500)
Gyms, indoor sports.2 × $\hat{RP}$	0.483	-1.798^{***}	−1.117	0.649	-1.113^{***}	−0.678	−0.289
	(0.636)	(0.478)	(1.217)	(0.974)	(0.314)	(0.506)	(0.511)
Gyms, indoor sports.3 × $\hat{RP}$	-2.048^{***}	0.022	-4.689^{***}	0.448	−0.447	−0.712	−0.177
	(0.626)	(0.546)	(1.538)	(0.964)	(0.405)	(0.531)	(0.577)
Theaters, concert halls.1 × $\hat{RP}$	1.064^{*}	0.161	−0.057	1.218	0.044	0.810	−0.842
	(0.567)	(0.440)	(1.461)	(0.906)	(0.361)	(0.505)	(0.532)
Theaters, concert halls.2 × $\hat{RP}$	0.365	−0.402	-7.383^{***}	0.636	-0.931^{***}	-1.101^{**}	0.633
	(0.519)	(0.563)	(1.711)	(1.035)	(0.303)	(0.520)	(0.493)
Theaters, concert halls.3 × $\hat{RP}$	-1.235^{*}	−0.284	-5.552^{***}	0.041	−0.478	0.082	-0.757^{*}
	(0.711)	(0.505)	(1.892)	(1.036)	(0.342)	(0.525)	(0.454)
Restaurants, bars, clubs.1 × $\hat{RP}$	−0.086	0.137	−2.260	2.002^{*}	−0.543	0.162	-1.314^{**}
	(0.600)	(0.712)	(1.725)	(1.149)	(0.342)	(0.652)	(0.543)
Restaurants, bars, clubs.2 × $\hat{RP}$	1.004^{*}	−0.775	−2.085	1.024	-0.789^{**}	−0.302	0.659
	(0.602)	(0.563)	(1.533)	(0.955)	(0.358)	(0.589)	(0.481)
Restaurants, bars, clubs.3 × $\hat{RP}$	-2.326^{***}	-1.385^{**}	-3.882^{**}	−0.467	-0.968^{***}	-1.059^{**}	-0.900^{**}
	(0.712)	(0.599)	(1.771)	(1.005)	(0.335)	(0.530)	(0.422)
Meetings, religious services.1 × $\hat{RP}$	0.022	-1.406^{***}	−2.001	1.530	-0.763^{**}	0.023	−0.462
	(0.619)	(0.535)	(2.164)	(1.044)	(0.357)	(0.530)	(0.528)
Meetings, religious services.2 × $\hat{RP}$	−0.354	-1.449^{**}	0.172	−1.399	0.008	−0.189	−0.182
	(0.653)	(0.584)	(1.302)	(1.090)	(0.372)	(0.518)	(0.570)
Meetings, religious services.3 × $\hat{RP}$	1.414^{**}	1.074^{**}	−0.111	0.945	−0.279	1.734^{***}	−0.403
	(0.630)	(0.494)	(1.114)	(0.877)	(0.325)	(0.537)	(0.400)
Assisted living facilities.1 × $\hat{RP}$	2.711^{***}	-1.940^{***}	0.293	−1.515	0.407	0.233	0.037
	(0.656)	(0.502)	(1.255)	(0.969)	(0.282)	(0.521)	(0.393)
Assisted living facilities.2 × $\hat{RP}$	−0.525	−0.347	-5.309^{***}	−0.998	-0.557^{*}	−0.651	−0.589
	(0.554)	(0.603)	(1.721)	(0.887)	(0.313)	(0.502)	(0.574)
Assisted living facilities.3 × $\hat{RP}$	0.760	0.248	-4.176^{***}	0.813	−0.109	0.542	−0.018
	(0.534)	(0.559)	(1.473)	(0.973)	(0.345)	(0.529)	(0.469)
1=Status quo alternative × $\hat{RP}$	0.756	0.468	−1.521	−0.378	−0.298	−0.294	0.189
	(0.657)	(0.499)	(1.434)	(0.966)	(0.339)	(0.650)	(0.416)
Respondents	317	453	223	295	698	507	480
Choices	634	906	446	590	1396	1014	960

Notes: *p<0.1; **p<0.05; ***p<0.01

Table F4: Full sets of parameter estimates for the models reported in Panel B of Table 4 in the body of the paper: Heterogeneity in preferences across socioeconomic groups

Dep. var: 1=Preferred policy	Liberal (1)	Moderate (2)	Conservative (3)	Non-college (4)	College (5)	< \$75k/yr (6)	> \$75k/yr (7)
Avg. hhld cost (federal UI = 0)	0.094	-0.891^{***}	-0.411^{**}	-0.347^{*}	-0.239^{*}	-0.452^{***}	−0.192
	(0.212)	(0.229)	(0.173)	(0.210)	(0.123)	(0.157)	(0.143)
Avg. hhld cost (federal UI > 0)	−0.081	0.123	0.123	0.150^{*}	0.119	0.367^{***}	0.028
	(0.207)	(0.118)	(0.108)	(0.082)	(0.109)	(0.091)	(0.087)
Unempl rate (federal UI = 0)	−0.116	0.316^{***}	0.089	0.140	0.015	0.250^{***}	−0.015
	(0.087)	(0.104)	(0.068)	(0.086)	(0.054)	(0.073)	(0.055)
Unempl rate (federal UI > 0)	0.063	−0.047	-0.082^{***}	−0.042	−0.016	−0.017	−0.022
	(0.078)	(0.040)	(0.031)	(0.034)	(0.031)	(0.033)	(0.028)
Absolute ’00s cases/mo/50,000	−0.036	-0.146^{**}	−0.081	-0.164^{***}	0.081^{**}	-0.092^{*}	−0.012
	(0.105)	(0.057)	(0.061)	(0.061)	(0.037)	(0.055)	(0.045)
Absolute deaths/mo/50,000	−0.035	0.028	-0.083^{***}	−0.029	−0.029	−0.028	−0.014
	(0.068)	(0.044)	(0.030)	(0.034)	(0.026)	(0.030)	(0.029)
1=Status quo alternative	-5.006^{***}	-3.647^{***}	-2.854^{***}	-2.497^{***}	-2.869^{***}	-2.705^{***}	-2.321^{***}
	(1.017)	(0.832)	(0.650)	(0.514)	(0.559)	(0.504)	(0.578)
Other coefficients, suppressed in Table 4 Panel B in the body of the paper:							
Grocery, essential retail.1	0.077	0.441	−0.361	0.093	−0.398	-0.493^{*}	0.096
	(0.527)	(0.329)	(0.293)	(0.289)	(0.252)	(0.264)	(0.276)
Non-essential retail.1	0.230	−0.381	−0.214	−0.352	−0.066	−0.413	−0.103
	(0.406)	(0.322)	(0.279)	(0.236)	(0.256)	(0.266)	(0.248)
Schools, daycare.1	−0.546	-1.181^{***}	−0.152	−0.103	-0.554^{**}	−0.449	-0.703^{**}
	(0.530)	(0.384)	(0.357)	(0.319)	(0.270)	(0.302)	(0.296)
Parks, outdoor sports.1	0.583	0.563	-0.760^{**}	0.157	0.080	−0.119	0.009
	(0.649)	(0.441)	(0.358)	(0.324)	(0.313)	(0.333)	(0.342)
Gyms, indoor sports.1	−0.250	−0.092	-0.715^{**}	−0.269	−0.268	−0.503	−0.274
	(0.555)	(0.423)	(0.335)	(0.318)	(0.280)	(0.316)	(0.305)
Theaters, concert halls.1	−0.929	−0.437	0.841^{***}	−0.062	−0.078	−0.416	0.053
	(0.569)	(0.336)	(0.296)	(0.307)	(0.249)	(0.286)	(0.264)
Restaurants, bars, clubs.1	−0.574	0.588	−0.048	0.104	−0.253	−0.315	−0.091
	(0.574)	(0.444)	(0.333)	(0.309)	(0.294)	(0.311)	(0.298)
Meetings, religious services.1	−0.603	−0.251	−0.572	-0.679^{**}	−0.455	-0.987^{***}	−0.242
	(0.554)	(0.450)	(0.350)	(0.325)	(0.294)	(0.321)	(0.307)
Assisted living facilities.1	-1.370^{*}	0.066	−0.075	-0.501^{*}	0.121	0.182	−0.372
	(0.774)	(0.385)	(0.301)	(0.296)	(0.254)	(0.286)	(0.251)
Universities, colleges.1	-2.786^{***}	−0.140	0.475	-0.641^{**}	0.137	−0.355	−0.063
	(0.715)	(0.349)	(0.289)	(0.293)	(0.260)	(0.276)	(0.276)
Grocery, essential retail.2	-2.384^{***}	−0.325	−0.181	−0.395	−0.042	−0.202	−0.189
	(0.724)	(0.415)	(0.301)	(0.319)	(0.293)	(0.306)	(0.307)
Non-essential retail.2	−0.823	−0.054	−0.445	−0.424	0.105	−0.503	0.270
	(0.562)	(0.404)	(0.357)	(0.379)	(0.288)	(0.387)	(0.279)
Schools, daycare.2	−0.916	-1.020^{**}	−0.184	-0.928^{***}	−0.059	-0.983^{***}	0.126
	(0.595)	(0.397)	(0.336)	(0.340)	(0.290)	(0.365)	(0.313)
Parks, outdoor sports.2	−0.019	−0.526	−0.002	-0.805^{**}	0.146	-0.905^{***}	0.510^{*}
	(0.519)	(0.363)	(0.332)	(0.335)	(0.288)	(0.346)	(0.287)
Gyms, indoor sports.2	0.786	−0.308	−0.239	0.403	−0.269	0.314	−0.487
	(0.625)	(0.377)	(0.328)	(0.294)	(0.274)	(0.290)	(0.310)
Theaters, concert halls.2	−0.212	-0.747^{*}	-0.885^{***}	−0.160	−0.210	−0.142	-0.567^{*}
	(0.548)	(0.390)	(0.343)	(0.292)	(0.277)	(0.293)	(0.318)
Restaurants, bars, clubs.2	0.152	-0.823^{**}	−0.358	−0.401	−0.266	-0.508^{*}	−0.312
	(0.587)	(0.389)	(0.322)	(0.308)	(0.287)	(0.282)	(0.324)
Meetings, religious services.2	-1.072^{*}	−0.266	0.253	-0.758^{**}	0.174	-0.652^{**}	0.787^{**}
	(0.576)	(0.401)	(0.328)	(0.310)	(0.283)	(0.289)	(0.322)
Assisted living facilities.2	−0.361	−0.312	−0.067	−0.398	0.045	-0.485^{*}	0.436
	(0.494)	(0.387)	(0.336)	(0.303)	(0.276)	(0.290)	(0.301)

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Table F4 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Universities, colleges.2	−0.521	−0.052	−0.404	−0.756**	0.161	−0.314	0.273
	(0.594)	(0.403)	(0.359)	(0.335)	(0.301)	(0.330)	(0.301)
Non-essential retail.3	−0.408	−0.298	−0.058	−0.011	−0.486*	0.117	−0.671**
	(0.528)	(0.360)	(0.329)	(0.326)	(0.266)	(0.285)	(0.285)
Schools, daycare.3	0.210	−0.403	−0.410	−0.171	−0.184	−0.294	−0.021
	(0.571)	(0.348)	(0.321)	(0.310)	(0.290)	(0.269)	(0.319)
Parks, outdoor sports.3	0.904	0.131	−0.666*	−0.120	−0.074	0.093	−0.256
	(0.623)	(0.384)	(0.344)	(0.340)	(0.276)	(0.326)	(0.291)
Gyms, indoor sports.3	−0.191	−0.540	−0.798**	0.252	−0.455	0.268	−0.557*
	(0.545)	(0.410)	(0.348)	(0.328)	(0.284)	(0.319)	(0.297)
Theaters, concert halls.3	1.044*	−0.508	−0.956***	0.278	−0.027	0.080	−0.044
	(0.623)	(0.436)	(0.336)	(0.325)	(0.283)	(0.326)	(0.307)
Restaurants, bars, clubs.3	0.494	−0.020	−0.422	0.252	0.231	0.136	0.263
	(0.570)	(0.438)	(0.353)	(0.294)	(0.305)	(0.312)	(0.312)
Meetings, religious services.3	−0.185	−0.522	−0.139	0.060	−0.388	−0.039	−0.067
	(0.560)	(0.389)	(0.328)	(0.290)	(0.246)	(0.290)	(0.279)
Assisted living facilities.3	0.166	−0.359	−0.042	−0.332	0.027	−0.406	0.100
	(0.476)	(0.398)	(0.333)	(0.318)	(0.284)	(0.305)	(0.322)
Universities, colleges.3	0.143	−0.557	−0.604*	−0.199	−0.193	−0.085	−0.356
	(0.665)	(0.404)	(0.341)	(0.314)	(0.276)	(0.315)	(0.302)
Absolute deaths/mo/50,000 × $\hat{RP}$	−0.203**	−0.327***	0.068	0.036	−0.102**	−0.265***	−0.010
	(0.094)	(0.101)	(0.050)	(0.077)	(0.048)	(0.079)	(0.032)
Absolute ’00s cases/mo/50,000 × $\hat{RP}$	−0.034	0.616	0.537	−0.532	−1.170***	1.114	−1.156**
	(1.626)	(1.049)	(1.137)	(1.304)	(0.447)	(0.982)	(0.500)
Avg. hhld cost (federal UI > 0) × $\hat{RP}$	0.100	−0.133	0.054	0.211	−0.091	−0.498**	0.205
	(0.309)	(0.237)	(0.237)	(0.263)	(0.143)	(0.250)	(0.126)
Avg. hhld cost (federal UI = 0) × $\hat{RP}$	0.196	2.234***	−0.199	0.745	0.257	1.007**	0.002
	(0.604)	(0.678)	(0.417)	(0.646)	(0.338)	(0.405)	(0.505)
Unempl rate (federal UI > 0) × $\hat{RP}$	0.508***	0.305***	−0.011	0.184***	0.100*	0.223***	0.104**
	(0.126)	(0.089)	(0.068)	(0.065)	(0.053)	(0.065)	(0.046)
Unempl rate (federal UI = 0) × $\hat{RP}$	0.756***	−0.601***	0.110	0.007	0.120	−0.321**	0.291
	(0.195)	(0.227)	(0.157)	(0.242)	(0.125)	(0.140)	(0.186)
Grocery, essential retail.1 × $\hat{RP}$	0.346	−2.302***	0.542	1.116	−0.547*	−0.473	−0.586**
	(0.990)	(0.580)	(0.514)	(0.745)	(0.313)	(0.705)	(0.298)
Non-essential retail.1 × $\hat{RP}$	−1.729**	0.358	−0.060	−0.016	−0.270	−1.172*	−0.058
	(0.833)	(0.629)	(0.521)	(0.651)	(0.397)	(0.689)	(0.322)
Schools, daycare.1 × $\hat{RP}$	2.755**	1.585***	0.471	−0.444	1.069***	2.382***	1.293***
	(1.203)	(0.596)	(0.734)	(0.930)	(0.353)	(0.733)	(0.403)
Parks, outdoor sports.1 × $\hat{RP}$	2.382**	0.642	3.326***	−0.490	1.058**	1.740**	0.828*
	(1.096)	(0.697)	(0.747)	(0.962)	(0.461)	(0.708)	(0.442)
Gyms, indoor sports.1 × $\hat{RP}$	−0.142	1.333	1.363**	−0.555	0.380	1.148*	0.814*
	(1.093)	(0.882)	(0.564)	(1.008)	(0.415)	(0.683)	(0.426)
Theaters, concert halls.1 × $\hat{RP}$	−1.024	−0.567	0.257	1.521**	−0.559	0.968	−0.028
	(0.861)	(0.668)	(0.683)	(0.763)	(0.378)	(0.699)	(0.402)
Restaurants, bars, clubs.1 × $\hat{RP}$	−2.070*	−2.446**	1.171	−0.741	−0.092	0.061	−0.169
	(1.123)	(1.013)	(0.783)	(0.772)	(0.401)	(0.610)	(0.431)
Meetings, religious services.1 × $\hat{RP}$	−2.574**	−1.814**	2.490***	0.691	−0.615	1.569**	−0.983**
	(1.206)	(0.804)	(0.804)	(0.803)	(0.404)	(0.706)	(0.446)
Assisted living facilities.1 × $\hat{RP}$	3.990***	−0.254	0.822	0.058	0.372	0.668	0.156
	(1.132)	(0.683)	(0.616)	(0.701)	(0.327)	(0.620)	(0.348)
Universities, colleges.1 × $\hat{RP}$	4.064***	0.905	0.894	0.523	0.316	1.341*	0.070
	(1.261)	(0.633)	(0.671)	(0.793)	(0.410)	(0.758)	(0.371)
Grocery, essential retail.2 × $\hat{RP}$	2.065	0.598	0.256	−0.534	−0.410	−0.320	0.158
	(1.262)	(0.794)	(0.750)	(0.871)	(0.473)	(0.724)	(0.507)
Non-essential retail.2 × $\hat{RP}$	−3.299***	−0.207	1.411**	1.527*	−0.386	−0.059	0.149
	(1.048)	(0.621)	(0.622)	(0.861)	(0.369)	(0.829)	(0.377)
Schools, daycare.2 × $\hat{RP}$	−0.136	2.793***	−0.013	2.057***	0.466	0.844	0.847
	(1.120)	(0.888)	(0.659)	(0.774)	(0.430)	(0.784)	(0.516)
Parks, outdoor sports.2 × $\hat{RP}$	−4.274***	−0.179	−0.114	1.608**	−0.450	−0.553	−0.120
	(1.423)	(0.700)	(0.584)	(0.780)	(0.410)	(0.744)	(0.385)
Gyms, indoor sports.2 × $\hat{RP}$	−3.966***	−1.551**	−0.599	−0.731	−0.440	−0.088	−0.591*

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Table F4 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(0.952)	(0.778)	(0.574)	(0.751)	(0.379)	(0.688)	(0.346)
Theaters, concert halls.2 × $\hat{RP}$	−1.635*	−0.338	0.382	−1.810**	−0.112	−0.669	0.073
	(0.972)	(0.588)	(0.612)	(0.787)	(0.343)	(0.749)	(0.349)
Restaurants, bars, clubs.2 × $\hat{RP}$	−4.357***	−0.070	−0.221	−0.124	−0.135	−0.127	−0.345
	(1.260)	(0.860)	(0.604)	(0.771)	(0.423)	(0.648)	(0.465)
Meetings, religious services.2 × $\hat{RP}$	3.316***	−0.481	−0.059	0.341	0.342	−0.494	−0.930**
	(1.123)	(0.733)	(0.564)	(0.770)	(0.414)	(0.695)	(0.391)
Assisted living facilities.2 × $\hat{RP}$	−1.318	−0.424	−0.968*	−0.599	−0.474	−0.889	−0.999**
	(1.071)	(0.691)	(0.587)	(0.751)	(0.403)	(0.611)	(0.393)
Universities, colleges.2 × $\hat{RP}$	−2.083*	−0.992	−0.180	−1.108	−0.326	−1.305*	−0.869*
	(1.099)	(0.741)	(0.545)	(0.784)	(0.462)	(0.679)	(0.465)
Non-essential retail.3 × $\hat{RP}$	−1.130	1.867***	−1.249*	−0.178	0.478	−0.302	0.532
	(0.956)	(0.629)	(0.648)	(0.998)	(0.395)	(0.638)	(0.397)
Schools, daycare.3 × $\hat{RP}$	−1.820*	1.969**	0.083	−0.605	−0.290	−1.137*	−0.475
	(0.992)	(0.842)	(0.690)	(0.944)	(0.372)	(0.622)	(0.366)
Parks, outdoor sports.3 × $\hat{RP}$	−1.325	0.228	0.668	−0.499	0.072	−0.960	−0.052
	(1.036)	(0.791)	(0.738)	(0.893)	(0.428)	(0.662)	(0.429)
Gyms, indoor sports.3 × $\hat{RP}$	−0.283	0.584	1.047	−1.571*	0.130	−1.736**	−0.341
	(1.284)	(0.817)	(0.686)	(0.919)	(0.473)	(0.728)	(0.433)
Theaters, concert halls.3 × $\hat{RP}$	−2.990**	1.352*	2.048***	−0.534	−0.417	−0.686	−0.751*
	(1.316)	(0.747)	(0.633)	(0.909)	(0.392)	(0.724)	(0.442)
Restaurants, bars, clubs.3 × $\hat{RP}$	−1.687	−1.629**	−0.090	−0.591	−1.322***	−1.007	−1.658***
	(1.048)	(0.698)	(0.707)	(0.801)	(0.377)	(0.693)	(0.468)
Meetings, religious services.3 × $\hat{RP}$	0.695	0.272	0.026	0.521	0.365	0.442	−0.031
	(1.172)	(0.645)	(0.635)	(0.631)	(0.363)	(0.704)	(0.361)
Assisted living facilities.3 × $\hat{RP}$	0.293	−1.658**	0.011	0.285	0.171	0.754	−0.146
	(0.980)	(0.694)	(0.688)	(0.809)	(0.450)	(0.810)	(0.433)
Universities, colleges.3 × $\hat{RP}$	0.293	−1.658**	0.011	0.285	0.171	0.754	−0.146
	(0.980)	(0.694)	(0.688)	(0.809)	(0.450)	(0.810)	(0.433)
1=Status quo alternative × $\hat{RP}$	−0.947	0.407	−0.176	−0.750	−0.082	−0.002	0.131
	(1.127)	(0.633)	(0.759)	(0.781)	(0.361)	(0.752)	(0.383)
Respondents	338	309	303	398	589	489	475
Choices	676	618	606	796	1178	978	950

Notes: *p<0.1; **p<0.05; ***p<0.01

Table F5: Standard errors clustered by respondent, with federal UI entering as in Models 3 and 6 in Table 3 (Panel A) and in “baseline plus interactions” form (Panel B).

	Dependent variable:	
	1=Preferred policy	
Panel A Model:	(3)	(6)
Avg. hhld cost for county (federal UI = 0)	-0.291^{**}	-0.310^{**}
	(0.142)	(0.147)
Avg. hhld cost for county (federal UI > 0)	0.174^{*}	0.153^{*}
	(0.092)	(0.091)
Unempl rate for county (federal UI = 0)	0.093	0.092
	(0.057)	(0.058)
Unempl rate for county (federal UI > 0)	-0.024	-0.023
	(0.028)	(0.028)
Absolute ’00s cases/mo/50,000	-0.035	-0.036
	(0.038)	(0.039)
Absolute deaths/mo/50,000	-0.038	-0.029
	(0.025)	(0.025)
1=Status quo alternative	-1.994^{***}	-2.472^{***}
	(0.324)	(0.380)
Panel B Model:	(3’)	(6’)
Avg. hhld cost for county (baseline)	-0.291^{**}	-0.310^{**}
	(0.142)	(0.147)
× 1=Non-zero federal UI (differential)	0.465^{***}	0.463^{***}
	(0.163)	(0.166)
Unempl rate for county (baseline)	0.093	0.092
	(0.057)	(0.058)
× 1=Non-zero federal UI (differential)	-0.116^{**}	-0.115^{**}
	(0.055)	(0.057)
Absolute ’00s cases/mo/50,000	-0.035	-0.036
	(0.382)	(0.389)
Absolute deaths/mo/50,000	-0.038	-0.029
	(0.025)	(0.025)
1=Status quo alternative	-1.994^{***}	-2.472^{***}
	(0.324)	(0.380)
Respondents	993	993
Choices	1,986	1,986

Notes: *p<0.1; **p<0.05; ***p<0.01. All models are corrected for sample selection and include relevant variables to control for the 10 categories of activities or businesses. Panel A reports estimates for key coefficients with standard errors clustered at the level of the respondent. Models 3 and 6 here are comparable to Models 3 and 6 in Table 3. The results in Panel B are equivalent to those of Panel A, except that results for cost and unemployment are reported in terms of base levels and shifters in Panel B. Panel B makes it clear that, while the two coefficients for unemployment are not significantly different from zero (in Panel A), they are significantly different from each other.

G Online Appendix: Other types of choice models

G.1 Mixed logit models

Mixed logit models allow some marginal utility coefficients in a choice model to be distributed randomly across the population, while others are held fixed. Each random parameter must be assigned a specific type of distribution. In Table G1, Model 1 assumes independent normal distributions for each of the seven key parameters in our basic model, while Model 2 assumes a log-normal distribution for the coefficients on cases and deaths and normal distributions for the other five key parameters. Cases and deaths are transformed to be negative for Model 2 so that their positive coefficients (after exponentiation) are sensible.

For each parameter, the mixed-logit algorithm produces both an estimate of the mean of that distribution (with its standard error) and the standard deviation of that distribution (likewise with its standard error). When the standard deviation of a random parameter is statistically significantly different from zero, there is unobserved heterogeneity in that parameter across the population. Table G1 shows that a zero value is well within two standard deviations of all except the effect of average household cost when federal UI is not present (*Avg. hhld cost for county (federal UI = 0)*), though the standard deviation is only significantly different from 0 for this variable and the status quo effect. Model 2 differs from Model 1 in that the log-normal distributions for (negative) cases and deaths do not permit indirect utility to increase with additional cases and deaths. Model 2 suggests more *heterogeneity* in the marginal disutility associated with unemployment rates, household costs when federal UI is present, and cases than does Model 1.

Both models in Table G1 concur with the results in Model 6 in Table 3 for the *signs* of the means for the first four coefficients (for the terms in average household costs and unemployment).

We note that yet another alternative for a mixed logit specification employs triangular distributions for random parameters, often used to prevent inappropriate signs. We estimated the means and standard deviations of a set of triangular random parameters for our seven featured marginal disutilities. The means of the triangular random parameters all bear the expected signs, consistent with Models 3 and 6 in Table 3. All point estimates of these means are statistically significantly different from zero and there is statistically significant unobserved heterogeneity in each of these parameters as well. However, the magnitudes of all of the resulting parameters are dramatically larger than in all other specifications considered in this paper, a result we are not yet able to explain. Thus we do not pursue this specification in the current paper.

Table G1: Two mixed logit specifications

	Dependent variable:			
	1=Preferred policy			
	(1)		(2)	
	Coefficients all normal		Cases/deaths lognormal	
	Mean	SD	Mean	SD
Avg. hhld cost for county (federal UI = 0)	-0.604^{**}	0.096	-0.583^{*}	0.194
	(0.289)	(0.273)	(0.316)	(0.266)
Avg. hhld cost for county (federal UI > 0)	0.388^{**}	0.230	0.679^{***}	0.617^{***}
	(0.181)	(0.185)	(0.262)	(0.201)
Unempl rate for county (federal UI = 0)	0.230^{*}	0.122	0.249^{*}	0.150^{*}
	(0.128)	(0.075)	(0.136)	(0.086)
Unempl rate for county (federal UI > 0)	-0.001	0.166^{***}	-0.029	0.136^{*}
	(0.063)	(0.054)	(0.067)	(0.073)
Absolute ’00s cases/mo/50,000	-0.066	0.074		
	(0.069)	(0.078)		
Absolute deaths/mo/50,000	-0.044	0.065		
	(0.045)	(0.056)		
Absolute ’00s cases/mo/50,000 × (-1)			-2.986^{**}	2.557^{**}
			(1.454)	(1.016)
Absolute deaths/mo/50,000 × (-1)			-3.569^{*}	0.829
			(1.962)	(0.960)
1=Status quo alternative	-3.250^{***}	2.511^{***}	-2.455^{***}	1.930^{***}
	(0.600)	(0.348)	(0.764)	(0.616)
Respondents	993	993	993	993
Choices	1,986	1,986	1,986	1,986

Coefficients on reported variables are normally distributed in Model 1. Coefficients on *Absolute ’00s cases/mo/50,000 × (1)* and *Absolute deaths/mo/50,000 × (-1)* are distributed log-normal in Model 2, while the rest of the coefficients in Model 2 are normally distributed. All other variables in Model 3 of Table 3 (i.e. continuous controls for business restrictions and response propensity interactions) are included in estimation of both models but are not permitted to vary.

G.2 Latent class models

Given the emphasis on latent class models in the previous literature on pandemic policy choice experiments, we have estimated some 2-class and 3-class models (4-class models would not converge), but only for specifications analogous to Model 3, where the ten types of restrictions enter as continuous variables. For Model 6, where the ten types of restrictions enter non-parametrically as sets of indicators, the specification has 29, rather than just ten, parameters associated with the restrictions on activities and businesses (implying 19 more coefficient in 2-class models, or 28 more in 3-class models). Latent class generalization of Model 6 would not converge.

It appears not to be possible to allow every parameter in our homogeneous specifications to differ across latent classes. It proved necessary to constrain the nuisance parameters on the interaction terms between the policy attributes and the fitted response propensity to be the same for both/all preference classes. In our latent class models, the separating equation for the different classes is permitted to depend upon the same set of characteristics that we employ to split the sample in our analysis of heterogeneity in the body of the paper.

The estimation algorithms are somewhat finicky and require very high-quality starting values. We resort to Stata's lclogit2 with its EM algorithm, followed up by lclogitml2 by maximum likelihood once a simplified model can be induced to converge, because unlike the latent class algorithm in R, these latent-class estimators in Stata permit some of the preference coefficients to remain fixed while others are allowed to differ across some specified number of latent classes). We have managed to achieve convergence for latent class models with either two or three classes, but for a sample slightly larger than our final estimating sample.

Table G2 describes our 2-class model. As other researchers have tended to find, preferences over pandemic policies differ across political ideologies. For this two-class model, the only statistically significant coefficients in the class-separating equation are the indicators for "liberal" or "conservative" ideologies (relative to "moderate" ideology).

These estimates are similar in spirit to those reported in Online Appendix Table F4. Conservatives are more likely to belong to preference class 1, and liberals to class 2. Liberals are again more likely to vote for any policy, regardless of its characteristics. Conservatives pay more attention to death rates. The preferences of moderates seem to be subsumed with those of liberals in preference class 2 (reflecting their disutility from average household costs and positive utility from unemployment rates in the absence of federal UI payments). Conservatives object to restrictions on non-essential retail, parks and outdoor sports, and meetings and religious services. Liberals object to restrictions on schools and daycare, etc. Finally, for either group, people who are more likely to complete the survey tend to derive less disutility from restrictions on gyms and indoor sports, and greater disutility from restrictions on assisted living facilities.

However, Table 4 in the body of the paper reveals that there are likely to be more than just two or three classes of preferences, given the heterogeneity in preference parameters across the different splits of the sample. Latent class models with two or three classes appear to be too restrictive to reveal all of the interesting heterogeneity in preferences in these data. Latent class models with three latent preference classes (but without corrections for response propensities) do bring age, gender, and college graduation status into one or both of the class-separating equations. However, for the third class of preferences, only two of the 17 coefficients are statistically significantly different from zero, and only at the 10 percent level. Three-class models analogous to our two-class model (namely, including the interaction terms involving response propensities) have been reluctant to converge, so we do not pursue them in this paper.

Table G2: Specification with two latent preference classes, based on Model 3

Dependent variable: 1=Preferred policy

Class separating equation: Propensity to exhibit Class 1 preferences = f(respondent characteristics)

	coef.	(s.e.)
Age less than 35 years	0.379	(0.252)
Age greater than 64 years	-0.377	(0.288)
Non-white	-0.419	(0.261)
Remale	0.31	(0.227)
Liberal ideology	-1.567	(0.393)***
Conservative ideology	0.889	(0.237)***
College graduate	-0.401	(0.267)
Income $75,000 or more	0.105	(0.239)
Constant	-1.471	(0.372)***

Main preference parameters	Class 1		Class 2	
	coef.	(s.e.)	coef.	(s.e.)
Avg. hhld cost for county (federal UI = 0)	0.00292	(0.00503)	-0.00455	(0.00188)***
Avg. hhld cost for county (federal UI > 0)	-0.00074	(0.00215)	0.00021	(0.0019)
Unempl rate for county (federal UI = 0)	-0.222	(0.178)	0.207	(0.081)***
Unempl rate for county (federal UI > 0)	-0.043	(0.058)	0.087	(0.056)
Absolute ’00s cases/mo/50,000	-0.039	(0.096)	-0.045	(0.065)
Absolute deaths/mo/50,000	-0.167	(0.065)***	-0.012	(0.045)
Grocery, essential retail	0.21	(0.348)	-0.193	(0.186)
Non-essential retail	-0.6	(0.225)***	0.017	(0.159)
Schools, daycare	-0.107	(0.188)	-0.39	(0.144)***
Universities, colleges	-0.00141	(0.262)	-0.122	(0.156)
Parks, outdoor sports	-0.514	(0.227)**	-0.043	(0.169)
Gyms, indoor sports	-0.407	(0.278)	-0.077	(0.143)
Theaters, concert halls	0.003	(0.183)	-0.096	(0.129)
Restaurants, bars, clubs	-0.04	(0.219)	-0.041	(0.134)
Meetings, religious services	-0.619	(0.252)***	0.245	(0.154)
Assisted living facilities	0.256	(0.305)	0.15	(0.135)
1=Status quo alternative	-0.738	(1.269)	-2.366	(0.755)

Interactions with fitted response propensities, $\hat{RP}$

	coef.	(s.e.)
$\hat{RP}$ × Avg. hhld cost for county (federal UI = 0)	0.00298	(0.00574)
$\hat{RP}$ × Avg. hhld cost for county (federal UI > 0)	0.00021	(0.00187)
$\hat{RP}$ × Unempl rate for county (federal UI = 0)	0.105	(0.183)
$\hat{RP}$ × Unempl rate for county (federal UI > 0)	0.026	(0.058)
$\hat{RP}$ × Absolute ’00s cases/mo/50,000	-0.047	(0.079)
$\hat{RP}$ × Absolute deaths/mo/50,000	-0.052	(0.04)
$\hat{RP}$ × Grocery, essential retail	-0.128	(0.184)
$\hat{RP}$ × Non-essential retail	0.078	(0.164)
$\hat{RP}$ × Schools, daycare	-0.031	(0.156)
$\hat{RP}$ × Universities, colleges	-0.047	(0.141)
$\hat{RP}$ × Parks, outdoor sports	0.163	(0.174)
$\hat{RP}$ × Gyms, indoor sports	0.266	(0.155)*
$\hat{RP}$ × Theaters, concert halls	0.023	(0.135)
$\hat{RP}$ × Restaurants, bars, clubs	-0.131	(0.133)
$\hat{RP}$ × Meetings, religious services	-0.006	(0.152)
$\hat{RP}$ × Assisted living facilities	-0.363	(0.138)***
1=Status quo alternative	0.442	(0.57)
Log likelihood	-825.60747	

Notes: *p<0.1; **p<0.05; ***p<0.01. Even by starting with a minimal specification, it was extremely difficult to nurse these models towards convergence due to the large number of preference parameters to be estimated. These estimates employ a sample that is slightly larger than the 993 respondents used for our main results because we had not yet excluded 34 respondents who rejected pandemic policy(ies) because they rejected the choice scenario, rather than for reasons related to preferences or economic considerations. For analogous models employing only the 993 respondents, it was even more difficult to achieve convergence. Given that our models with heterogeneous preferences reveal that there are many different types of preferences, we gave up on latent-class models (despite their popularity in other studies of pandemic-policy preferences.

G.3 Sensitivity analysis: Preferences as a function of time spent on first-choice preamble about federal UI assumption

Some respondents read survey screens at roughly the average reading speed in the population. Others spend less time than this on each page, reinforcing the importance of providing them with succinct information. When respondents are offered several choices in a similar format, it can be expected that they will develop choice heuristics that will allow them to process choice tasks more quickly. In our survey, the tutorial section used that individual's (unique) first choice set for training purposes, highlighting and explaining each section of the choice task summary table in turn, so respondents will have had an opportunity, in advance of their first choice, to think about which policy attributes matter the most to them and where in the choice table they will be able to see those attributes. Our use of "meters" for the severity of restrictions on activities and businesses allows respondents to see these restrictions without having to read any numbers. The pop-up descriptions explain how to interpret each meter reading in the context of that particular type of restriction.

It is generally prudent to determine the extent to which there may be systematically different apparent preferences between (a) people who read survey pages quickly and (b) people who read more deliberatively. In this study, we are particularly concerned about whether respondents took the time to review the preamble to the first choice set, where the the assumptions to be made concerning federal UI benefits were described. These benefits were also described as a fixed characteristic of the policy choice context, invariant to the respondent's choice of whether or not to "vote" for a given policy.

Table G3 shows that while there is some movement in the point estimates for our four coefficients on the average household cost and unemployment variables and their interactions with federal UI payments, all of these coefficients retain their signs and significance levels when they are accompanied by their interactions with each of two measures of the respondent's time on that page. None of the four interaction terms is individually statistically significant, and neither are the four extra terms jointly statistically significant from zero.

Table G3: Reading time

	Dependent variable:		
	1=Preferred policy		
	(1)	(2)	(3)
Avg. hhld cost for county (federal UI = 0)	-0.310^{***}	-0.292^{**}	-0.336^{**}
	(0.121)	(0.164)	(0.171)
× Time on page		-0.0002	
		(0.006)	
× Time on page (censored)			0.002
			(0.006)
Avg. hhld cost for county (federal UI > 0)	0.153^{**}	0.137^{*}	0.137^{*}
	(0.068)	(0.081)	(0.086)
× Time on page		0.001	
		(0.002)	
× Time on page (censored)			0.001
			(0.003)
Unempl rate for county (federal UI = 0)	0.092^{**}	0.085	0.096^{*}
	(0.052)	(0.068)	(0.071)
× Time on page		0.0004	
		(0.002)	
× Time on page (censored)			-0.0001
			(0.003)
Unempl rate for county (federal UI > 0)	-0.023	-0.026	-0.033
	(0.025)	(0.027)	(0.029)
× Time on page		0.0001	
		(0.001)	
× Time on page (censored)			0.0005
			(0.001)
Absolute '00s cases/mo/50,000	-0.036	-0.029	-0.026
	(0.033)	(0.033)	(0.033)
Absolute deaths/mo/50,000	-0.029	-0.030	-0.030
	(0.022)	(0.023)	(0.023)
1=Status quo alternative	-2.472^{***}	-2.509^{***}	-2.501^{***}
	(0.399)	(0.402)	(0.401)
Respondents	993	993	993
Choices	1986	1986	1986
Log Likelihood	$-1,169.803$	$-1,165.983$	$-1,165.560$

Notes: *p<0.1; **p<0.05; ***p<0.01. Column 1 reproduces our preferred specification (Model 6 in Table 3) for easy comparison. Column 2 includes interactions of our four variables of interest with the amount of time (in seconds) respondents spent on the survey page that instructed them to "**Assume that any Federal unemployment benefits, as described, will be in place regardless of any pandemic rules that apply in [respondent's county]**." A few respondents spent long periods of time (e.g. more than 10 minutes) on that page. To ensure that these outliers are not driving our results, we also interact each variable of interest with a right-censored measure of time. This measure replaces all values above 92 seconds (i.e. double the time it would take a typical reader to read the page) with 92.

G.4 Sensitivity analysis: Preferences as a function of highest pandemic-month unemployment rate in respondent's county relative to sample median

The extent to which a respondent responds to a policy that changes the unemployment rate may depend on unemployment rates in that respondent's own county relative to the unemployment rates experienced by other respondents. For each respondent, we calculate the highest monthly county-level unemployment rate experienced over March 2020 through December 2020. To split the sample into two roughly equal groups, we calculate the median of these highest unemployment rates across all respondents. We then split the sample according to whether the respondent's highest pandemic-era monthly county-level unemployment rate is higher or lower than the sample median.

The estimates in Table G4 are all from the same model, as in our models for heterogeneous preferences across partitions of the sample by sociodemographic groups. Column 1 shows our seven featured parameter estimates for respondents who have experienced lower-than-median "worst" pandemic-era unemployment rates. Column 2 shows the corresponding parameter estimates for respondents who have experienced higher-than-median "worst" pandemic-era unemployment rates. For both groups, the first four coefficients retain the same signs that they exhibit in our other specifications.

Without federal UI, the disutility from average household costs is about three times greater for the group that has experienced higher-than-median "worst" unemployment levels. Decreasing marginal utilities of income may explain some of this disparity. Additionally, recent experience with high local unemployment may make people more cost averse. With federal UI, the marginal utility from average household costs is positive (as usual), but is statistically significant only for the group with lower-than-median "worst" unemployment levels during the pandemic.

Without federal UI, higher unemployment rates confer statistically significant positive utility only for the group with higher-than-median "worst" unemployment levels during the pandemic. Counties that have already experienced very high unemployment are likely to have higher shares of workers in sectors that cannot transition to remote work and require in-person interaction, so residents of those counties may perceive a larger public health benefit associated with unemployment. With federal UI, both groups dislike higher unemployment rates, but nether point estimate is statistically significantly different from zero.

The disutility from greater numbers of deaths is marginally statistically significant only for the group with higher-than-median "worst" unemployment rates during the pandemic. The worst unemployment rates are likely experienced in regions where the pandemic has been especially severe, which may increase the salience of pandemic deaths.

Table G4: Worst month's unemployment rate relative to median worst pandemic unemployment rate prior to survey

	Dependent variable:	
	1=Preferred policy	
	Better Unemp	Worse Unemp
	(1)	(2)
Avg. hhld cost for county (federal UI = 0)	-0.205^{*}	-0.641^{**}
	(0.109)	(0.253)
Avg. hhld cost for county (federal UI > 0)	0.190^{**}	0.122
	(0.092)	(0.120)
Unempl rate for county (federal UI = 0)	0.035	0.212^{**}
	(0.049)	(0.086)
Unempl rate for county (federal UI > 0)	−0.038	−0.031
	(0.033)	(0.029)
Absolute '00s cases/mo/50,000	−0.040	−0.063
	(0.042)	(0.056)
Absolute deaths/mo/50,000	0.004	-0.052^{*}
	(0.031)	(0.029)
1=Status quo alternative	-2.258^{***}	-3.000^{***}
	(0.500)	(0.582)
Respondents	503	490
Choices	1006	980

Notes: *p<0.1; **p<0.05; ***p<0.01. Models are corrected for sample selection and include relevant variables to control for the 10 categories of activities or businesses.

G.5 Sensitivity analysis: Preferences as a function of highest pandemic-month unemployment rate in respondent's county relative to sample median

Our survey employs occasional comprehension questions to assess whether the respondent is paying attention to the tutorial portion of the survey. One such question was: "We need to be sure that everyone interprets their choice tasks the same way. Thus there are several points at which we 'check your understanding.' For example, can we be sure that exactly X people will get sick and Y will die, if no rules are in place?" In the tutorial, the wildcard amounts X and Y are set to their values in the respondent's first choice task.[26]

A second comprehension question was: "For your Policy A, the 'Average $/month lost' across all households will be $X. Does that mean your household, and every other household in [your county], will end up losing $X of income each month during the policy? In addition to any unemployment?"[27]

For this sensitivity analysis, we split the sample according to whether the respondent was one of the 370 respondents who answered both of these questions correctly, or whether they were one of the 623 respondents who answered at least one question incorrectly. Note that the subsequent screen in the survey either confirms the respondent's correct answer, or goes into more detail to explain the correct answer if the respondent's answer is either incorrect or "Don't know/not sure." Thus an incorrect answer does not imply that the respondent makes their policy choices based on incorrect information, only that they needed more help to fully understand important points that were being made in the tutorial portion of the survey.

Again, the first four coefficients in both columns of Table G5 display the familiar pattern in their signs. Respondents who were more attentive to the information in the tutorial have less precise estimates for the first three coefficients, but with federal UI, their marginal disutility from unemployment is strongly statistically significantly negative, whereas this disutility for respondents who made at least one mistake in these two comprehension questions shows no discernible response to unemployment rates when federal UI is non-zero. Respondents who are more engaged with the survey may be more concerned about county-level pandemic policies in general, including their interactions with federal policies.

[26]The correct answer here is *no*. We instructed respondents that X and Y are to be treated as the best available estimates of cases and deaths.

[27]Again, the correct answer is *no*. We instructed respondents that X is an average over households that will lose much more than X in income and households that lose less—or no—income.

Table G5: Heterogeneity according to correctness of answers on two comprehension questions during the survey's tutorial

	Dependent variable:	
	1=Preferred policy	
	Both answers right	At least one answer wrong
	(1)	(2)
Avg. hhld for county (federal UI = 0)	-0.447^{*}	-0.333^{***}
	(0.258)	(0.109)
Avg. hhld for county (federal UI > 0)	0.123	0.195^{**}
	(0.133)	(0.077)
Unempl rate for county (federal UI = 0)	0.066	0.137^{***}
	(0.102)	(0.047)
Unempl rate for county (federal UI > 0)	-0.095^{**}	-0.005
	(0.044)	(0.025)
Absolute '00s cases/mo/50,000	-0.105	-0.011
	(0.070)	(0.039)
Absolute deaths/mo/50,000	-0.019	-0.033
	(0.039)	(0.025)
1=Status quo alternative	-3.205^{***}	-2.284^{***}
	(0.811)	(0.426)
Respondents	370	623
Choices	740	1246

Notes: *p<0.1; **p<0.05; ***p<0.01. Models are corrected for sample selection and include relevant variables to control for the 10 categories of activities or businesses.

G.6 Sensitivity analysis: Preferences as a function of household income relative median household income in the respondents ZIP code

It is possible that preferences for pandemic policy attribute may vary systematically according to whether the respondent's household income is higher or lower than the median household income in their ZIP code. We merge ZIP code level sociodemographic data, including median household income, with our survey response data. In Table G6, column 1 gives our seven featured parameter estimates for respondents with household incomes below their ZIP code median ("Poorer") and column 2 gives the corresponding estimates for respondents with household incomes above their ZIP code median ("Richer").

This model reveals that most of the action on the average household cost and unemployment rate variables is driven by the preferences of relatively poorer household. These estimates are complementary to those in Table 4 where we separate respondents according to the absolute level of their household income, using $75,000/year as the dividing line. We estimate the model in Table G6 because median household incomes differ substantially across ZIP codes. It is sometimes argued that "relative income" compared to one's neighbors may be a stronger determinant of preferences than the absolute level of income, it seemed prudent to check the effects of relative household income within ZIP codes.

The main difference between the estimates in Table G6 and those in Table 4 in the body of the paper, is that in Table G6, the marginal disutility from average household costs in the absence of federal UI is marginally statistically significant, whereas in Table 4, none but the coefficient on the indicator for the status quo alternative is statistically significantly different from zero for the group of households distinguished by having an absolute income greater than $75,000.

Table G6: Zip code relative income. Household income below median zip code income (Poorer) versus above median zip code income (Richer)

	Dependent variable:	
	1=Preferred policy	
	Poorer	Richer
	(1)	(2)
Avg. hhld for county (federal UI = 0)	-0.514^{***}	-0.236^{*}
	(0.162)	(0.138)
Avg. hhld for county (federal UI > 0)	0.319^{***}	0.026
	(0.105)	(0.084)
Unempl rate for county (federal UI = 0)	0.268^{***}	0.009
	(0.077)	(0.052)
Unempl rate for county (federal UI > 0)	0.010	−0.041
	(0.039)	(0.025)
Absolute '00s cases/mo/50,000	-0.094^{*}	−0.016
	(0.057)	(0.045)
Absolute deaths/mo/50,000	0.006	-0.048^{*}
	(0.030)	(0.028)
1=Status quo alternative	-2.830^{***}	-2.444^{***}
	(0.577)	(0.542)
Respondents	440	524
Choices	880	1048

Notes: *p<0.1; **p<0.05; ***p<0.01. Models are corrected for sample selection and include relevant variables to control for the 10 categories of activities or businesses.