“The Kids Are Alright”: Environmental Concerns Of Young Adults In The Time Of COVID

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Abstract

We ask whether young adults have “finite pools of worries” that would cause concerns about the COVID-19 pandemic to crowd out environmental priorities, and report on the results of two pre-registered studies that provide direct evidence on this question. We find that “moral bandwidth” is quite robust – that is, for the young adults in our samples, environmental concerns were unaffected and remain strong.

Keywords: COVID-19, environmental concerns, finite pool of worries, young adults

JEL Classification: Q51, Q54

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1 Introduction

The sixth IPCC climate change report (Arias et al., 2021), described as the UN's "starkest warning" about "inevitable and irreversible" climate change (Harvey, 2021), and a "code red for humanity" (McGrath, 2021), was released in August 2021, in the midst of the COVID-19 pandemic. Given the timing, it was not clear whether there was sufficient "moral bandwidth" to assimilate the new information or whether, to borrow Weber's (2006) evocative earlier language, individuals' "finite pools of worries" would cause climate-related concerns to be crowded out. Some initial evidence suggested that climate concerns were not displaced: Evensen et al. (2021) found that even after the first wave of widespread lockdowns, more residents of the U.K. were concerned about climate change than COVID-19 (55% v. 35%). In other work, Sisco et al. (2020), who framed the pandemic as a natural experiment of sorts, discerned evidence of a "finite pool of attention" but not worries, which suggests that it is cognitive, not moral bandwidth, that matters in this context.

Our focus in this brief paper is on young adults, whose pre-pandemic engagement with climate issues differed, in both qualitative and quantitative terms, from previous generations. A recent Pew Research Center study (Tyson, Kennedy and Funk, 2021), for example, found that members of Gen Z were more concerned about climate than older generations, claimed it as their top priority more often, and were more likely to exploit social media to act on these concerns. The rapid expansion of Greta Thunberg's school strikes and the creation of a Youth Advisory Group on Climate Change by the UN Secretary-General (Harvey, 2020) are two familiar manifestations of this trend. On the other hand, Oved et al. (2021) found that the pandemic imposed higher emotional costs on young adults compared to older adults. Together, these prompted our basic question: How were the environmental priorities of this cohort affected by the COVID-19 pandemic?

We report on two separate studies, both of which suggest that the commitment of young adults in the United States to the environment was not displaced by the pandemic. Evidence from the first is indirect in the sense that it comes from a planned panel of students at a selective liberal arts college on environmental values and behaviors, in which the first wave was collected from incoming students in the fall before the pandemic. We modified our end-of-year follow-up to include questions about the salience of COVID-19 but because this was not our original purpose, and because the sample is not representative, we present the results as more suggestive than definitive. The second study exploits the new literature on information provision protocols (Haaland, Roth and Wohlfart, 2023) and induces random variation in beliefs about the economic and health effects of the pandemic to generate a causal estimate of the effect of interest for a broader population of young adults. Both projects were pre-registered at OSF, and the plan for the second reflects our expectation, based on the first study and the work of Sisco et al. (2020) and others, of a null effect.[1]

In both studies, when asked to compare to other political and social issues, most respondents agree that protection of the environment should be one of the most important priorities, and this belief seems unaffected by the global COVID-19 pandemic. Whether we consider variation in exposure to the consequences of the pandemic based on state-level variation in the spread of the disease, or the variation induced by random exposure to information about state-level health or economic outcomes, young Americans' "moral bandwidth" is robust – their environmental concerns are not displaced. Both of our studies were completed within the first year of the pandemic, which means that the null effects we find should be interpreted as a short-term response. Some will be concerned that the first of our studies relies on stated preferences, in which participants are not required to confront economic trade-offs (Johnston et al., 2017), even if it remains commonplace to elicit general policy preferences without incentives (Marlon et al., 2022). There is no such concern about the second, however. In this case, we both use an incentive-compatible mechanism to elicit expectations and offer participants a chance to donate some of their earnings to a known environment charity, a clear, if low stakes, trade-off. Donations (as a measure of behavior), for example, were used in a recent study of the stability of social sustainability concerns under the COVID-19

[1] The links for the two OSF registrations are `https://doi.org/10.17605/OSF.IO/YKUXT` and `https://doi.org/10.17605/OSF.IO/Z8TG6`.

pandemic (Blanco et al., 2022). This said, future work should explore both issues further.

Our results also have important political implications: young Americans, like those we sample, have tended to be pro-environment but low turnout voters, but turnout was high in the last (2022) midterm elections[2] and our results suggest that their concern for the environment has remained strong and inelastic.

2 Some Initial Evidence

Our initial evidence comes from the modification of an existing project designed to explore the evolution of environmental beliefs and behaviors of young adults as students and, later, alumni at a selective liberal arts college in New England. The project started, in August 2019, with a baseline survey e-mailed to all incoming first-year students: in addition to various demographics, the survey included questions about environmental beliefs and behaviors, as well as expectations about their college experience. On March 13, 2020, a week prior to spring break, the college stopped in-person learning in response to the COVID-19 pandemic and sent students home. There was substantial variation in the lived experiences of students after March 2020, and we added five additional pandemic-related questions to a planned follow-up survey in May and June of 2020. The survey protocols are included in the pre-registration plan, and a table of summary statistics and comparison of the two waves is available in the online appendix.

Of the 163 students who answered our survey and remained in the United States, 7% spent their first pandemic spring in New York, 6% in New Jersey, 14% in Massachusetts, 11% in California, and 7% in Vermont, with the others scattered across the remaining states. The corresponding cumulative death rates (per 100,000 residents) in these states on May 11, when the first invitations to participate in the follow-up were e-mailed, were 138, 105, 75, 7, and 8, respectively, consistent with wide variation in (at least) local experience with the pandemic at the time we surveyed them. More than half of the students reported knowing

[2] `https://circle.tufts.edu/2022-election-center`.

someone who had been infected, and as expected, this measure was correlated ($\rho = 0.19$) with state-level death rates. It is these two measures – death rates and knowing someone infected – that we use to measure COVID salience.

We focus on two outcomes, one of them also featured in the second experiment. The common outcome is *Priority Environment*, which measures, on a five-point Likert scale, how much respondents agree with the proposition: "Compared to other political and social issues, protection of the environment is one of the most important." The other is *Behavior Scale*, and is defined as the sum of responses to questions about changes in travel, water and energy consumption, food choice and recycling, with a maximum value of 25. (We do not consider changes in behavior in the second experiment because no time elapses between the information interventions and the questions that follow.)

Our results are reported in Table 1. We start with the first and fourth columns of the top panel, which estimate the respective responses of *Priority Environment* and *Behavior Scale* to variation in cumulative state death rates for the May/June cross-section of respondents. The estimates are both small and insignificant. To illustrate, consider the difference in the answers to *Priority Environment* between residents of Vermont (with the cumulative death rate per 100,000 residents of 8) and New York (with the cumulative death rate per 100,000 residents of 138), whose experiences were (then) close to the two extremes: our model predicts a difference of 0.011 ($p = 0.953$) on a five-point scale for which the mean response is 4.35. In a similar vein, the predicted Vermont-New York difference on the 25-point *Behavior Scale* is 1.01 ($p = 0.288$). The effect on behavior is perhaps a "less precise zero," but the real surprise is the absence of a strong(er) mechanical effect: with less travel in COVID-intensive states, for example, green options for travel or consumption were simply less available.

The first and fourth columns in the bottom panel of Table 1 report the analogous results for the case where COVID salience is measured as knowing someone who was infected, and are consistent with these findings. The effects on *Priority Environment* and *Behavior Scale* are -0.02 ($p = 0.913$) and -0.175 ($p = 0.768$), respectively, indicative of null effects.

	(1) OLS	(2) Diff	(3) Controls	(4) OLS	(5) Diff	(6) Controls
	Priority Environment			Behavior Scale		
Panel A: Cumulative Death Rate per 100,000 as of May 11, 2020						
COVID Exposure	-9.12e-05	-0.000830	-0.00143	-0.00776	-0.00874	-0.00890
	(0.00153)	(0.00201)	(0.00202)	(0.00727)	(0.00703)	(0.00743)
Female (=1)			-0.0492			0.422
			(0.203)			(0.674)
Liberal (1-5)			-0.193*			-0.252
			(0.112)			(0.388)
Constant	4.353***	0.0538	0.897*	18.19***	0.428	1.191
	(0.0962)	(0.161)	(0.496)	(0.376)	(0.372)	(1.822)
Observations	163	103	97	160	102	96
R-squared	0.000	0.001	0.027	0.008	0.019	0.030
Mean of outcome	4.35	0.02	0.01	17.89	0.07	0.07
St.dev. of outcome	0.92	1.07	1.08	3.68	2.88	2.90
	Priority Environment			Behavior Scale		
Panel B: =1 if know someone infected with COVID						
COVID Exposure	-0.0159	0.113	0.0601	-0.175	0.236	0.434
	(0.145)	(0.195)	(0.204)	(0.593)	(0.603)	(0.625)
Female (=1)			-0.0378			0.496
			(0.207)			(0.692)
Liberal (1-5)			-0.193*			-0.254
			(0.111)			(0.401)
Constant	4.359***	-0.0500	0.797	18***	-0.0769	0.527
	(0.110)	(0.118)	(0.529)	(0.455)	(0.491)	(1.896)
Observations	163	103	97	160	102	96
R-squared	0.000	0.003	0.024	0.001	0.002	0.016
Mean of outcome	4.35	0.02	0.01	17.90	0.07	0.07
St.dev. of outcome	0.92	1.07	1.08	3.68	2.88	2.90

Table 1: Impact of COVID-19 Exposure (College Sample)

*Notes: Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. COVID exposure is measured as state-level cumulative death rate per 100,000 as of May 11, 2020 in the top panel A and =1 if know someone infected with COVID in the bottom panel B. Dependent variables are Priority Environment ("Protecting the environment is one of the most important issues") in columns 1-3 and Behavior Scale ("Scale of environmental behaviors") in columns 4-6. For each dependent variable, the first column reports cross-sectional estimates for all students who remained in the United States and completed the second survey; the second column shows the within-person estimates for those who completed both the first (pre-pandemic) and second surveys, while the third includes within-person estimates with two controls (indicator variable for female and political leaning (1-5) with higher numbers corresponding to more liberal).*

To accommodate concerns about unobserved individual heterogeneities, we also looked at the subsample of 100 or so students who completed both fall and spring questionnaires. (This is not a subset of the fall sample: there was some attrition between fall and spring, of course, but some students participated for the first time in the spring.) The outcomes are now the differences in *Priority Environment* and *Behavior Scale* between the first and second waves, and we report the results without and with additional controls in the second and fourth (third and fifth, respectively) columns. To illustrate our findings, consider the Vermont-California example once again: with controls, the predicted differences in *Priority Environment* and *Behavior Scale* decrease by 0.1859 ($p = 0.481$) and 1.157 ($p = 0.234$). The estimated effects are larger than observed in the cross-section, but still insignificant.

Further, the bottom half of Table 1 reveals that for our alternative measure of COVID exposure, the effects are not just statistically insignificant but (from the standpoint of the crowding out hypothesis) wrong-signed: knowing someone who was infected is predicted to *increase* the *Priority Environment* and *Behavior Scale* scores by 0.0601 ($p = 0.769$) and 0.434 ($p = 0.490$), respectively.

In short, we find, consistent with some previous work on adults, that there is little evidence that the pandemic altered either students' environmental priorities or behaviors. But as we have also noted, this inference is based on a small and unrepresentative (of all young adults, that is) sample, and requires special assumptions about the spatial distribution of COVID salience, shortcomings we address in our second experiment, described in the next two sections.

3 Research Design

Studies of the health and economic consequences of COVID-19 based on observational data often turn on contestable claims about their random variation across space and/or time. Our approach in the remainder of the paper, inspired by recent developments in information provision protocols, is premised on the observation that even when this is not the case, information about the health and economic consequences can be shared at random. Haaland, Roth and Wohlfart (2023) provide an excellent overview of this framework, including two COVID-related contributions: Settele and Shupe (2020) evaluate the effects of research on the costs and benefits of shutdowns on support for such interventions, while Rafkin, Shreekumar and Vautrey (2021) ask whether changes in the US government's position affected its later credibility. In other work, Abel, Byker and Carpenter (2021) examine the effects of "de-biasing" beliefs about mortality risk on prosocial behavior.

Our own intervention was organized around a 2×2 protocol in which young adults either did, or did not, learn about the health and economic effects of the COVID-19 pandemic in their own states, at the time of their engagement with us. In all four treatments, the participants first answered questions about location, salience of the pandemic and their beliefs about the cumulative number of COVID-related deaths per 100,000 residents and the local unemployment rate, using an incentive-compatible method. And in all four, participants concluded with a series of questions about their environmental priorities and behaviors, the most salient of which is *Priority Environment*, the same measure we used in our first intervention, described in the previous section.

This was followed by a handful of demographic questions and, an important check on our design, the chance to donate some or all of their bonus compensation to a charity. They were given two options: Feeding America, a network of more than 200 food banks and a prominent source of pandemic relief, and The Nature Conservancy, a well-known environmental nonprofit that has protected more than 125 million acres of land worldwide. Participants were offered brief descriptions of the two charities and an opportunity to donate to one or both charities, or keep the bonus payment to themselves. While the participants' responses to questions about environmental priorities involved no economic trade-offs, the donation option does, and should be consistent with their revealed preferences.[3]

[3]There are more details about the survey, including incentives, in the OSF registrations.

The treatments differed in what happened in between. In the control, arm C, we provided no information. In treatment T^H, participants learned what the actual COVID-related morality rate was in their state, and then asked a question that encouraged them to compare their initial belief with the correct answer. In T^E, participants instead learned what the actual jobless rate was, and asked a similar follow-up question, while in T^{HE}, both pieces of information were shared, with two follow-up questions.

The baseline empirical specification, as described in our pre-analysis plan, is:

$$\begin{aligned}
Y_i = \beta_0 + \beta_1 T_i^H + \beta_2(Belief_i^H - Actual_i^H) + \beta_3 T_i^H(Belief_i^H - Actual_i^H) \\
+ \beta_4 T_i^E + \beta_5(Belief_i^E - Actual_i^E) + \beta_5 T_i^E(Belief_i^E - Actual_i^E) \\
+ \beta_6 T_i^{HE} + \beta_7 T_i^{HE}(Belief_i^H - Actual_i^H) + \beta_8 T_i^{HE}(Belief_i^E - Actual_i^E) + \beta\mathbf{X_i} + \epsilon_i, \quad (1)
\end{aligned}$$

where Y_i is the value of *Priority Environment* for individual i; T_i^H, T_i^E, and T_i^{HE} are treatment indicators, $(Belief_i^j - Actual_i^j)$ is i's standardized misbelief about j, the percentage difference between i's incentivized belief about j and the actual state of affairs $Actual_i^j$, whether or not the individual is shown this information, and $\mathbf{X}_i$ is a possible vector of controls.

Within this framework, the coefficients of interest are *not* those associated with the treatment indicators, that is, β_1, β_4, and β_6. For someone whose beliefs are correct, it is reasonable to suppose – and we confirm for our sample – that confirmation of those beliefs should have little effect. In our framework, the treatment effect should depend on the size of the misbelief, and the coefficients of interest are instead β_3, β_5, β_7, and β_8.

If the correction of each sort of belief is the same whether or not the other belief is (also) corrected, the data can be pooled, and the statistical model becomes:

$$\begin{aligned}
Y_i^k = \gamma_0 + \gamma_1 T_i^H + \gamma_2(Belief_i^H - Actual_i^H) + \gamma_3 T_i^H(Belief_i^H - Actual_i^H) \\
+ \gamma_4 T_i^E + \gamma_5(Belief_i^E - Actual_i^E) + \gamma_6 T_i^E(Belief_i^E - Actual_i^E) + \gamma\mathbf{X_i} + \epsilon_i, \quad (2)
\end{aligned}$$

where T_i^H is now equal to 1 if individual i received information about mortality rates in *any* treatment, which we shall henceforth refer to as the "health treatment," and likewise T_i^E, or the "economy treatment."

Our data come from a sample of 555 young adults aged 18 to 25, recruited online from Cloud Research's pool of "approved" participants (Litman and Robinson, 2020) between February and March 2021. In addition, we restricted participation to those with some experience (100 or more HITS) and a prior approval score of 95% or better, and required them to complete a set of attention and other checks. (Peer et al. (2022) show that with these filters, Cloud Research subjects are similar in quality to competing online platforms.) Participants were paid a fixed fee of \$2.00 and a bonus of \$1.00 on average for (almost) correct beliefs for less than 10 minutes work. We provide summary statistics, a balance table, and a map showing where our participants reside in the appendix but observe that, as intended, the sample is indeed more representative than our college student sample. In terms of outcomes, the mean *Priority Environment* score, across all conditions, was 4.09 (as opposed to 4.35 in our student sample) and about half made donations to Feeding America and/or The Nature Conservancy.

Figure 1 depicts the critical distributions of misbeliefs about COVID-19 mortality and unemployment, expressed as a percentage of the relevant state values. The median misbeliefs are not far from zero and, conditional on a natural lower bound at -100, the distributions are almost symmetric. It is the substantial variation in misbeliefs, however, that allows us to estimate the causal effect.

4 Results

Two preliminary tests of our protocol come to mind. First, it should be the case that information provision changes *other*, non-environmental concerns or behaviors. For example, we might expect those who learn about the significant economic effects of the pandemic to

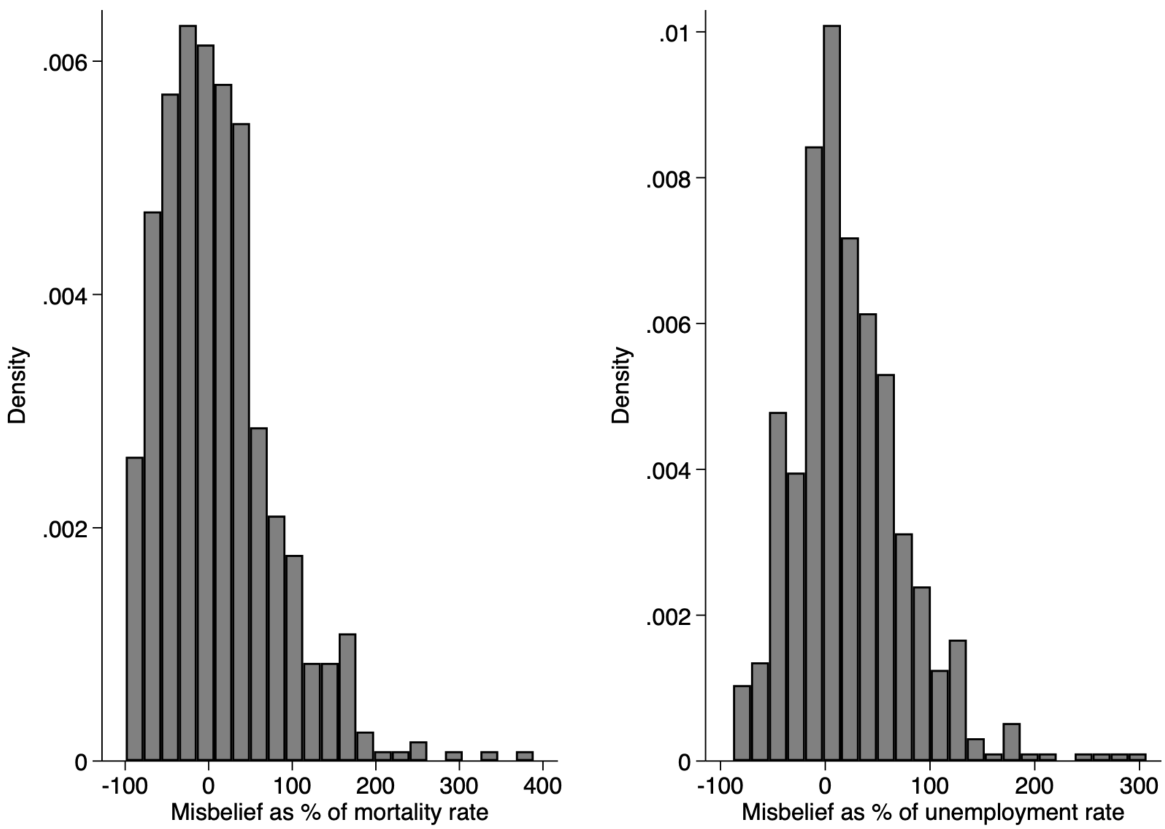

Figure 1: Standardized Misbeliefs In mTurk Data

Notes: These are the standardized (expressed as a percentage of the actual rate) misbeliefs for state-level COVID-19 mortality and unemployment rates for the online sample.

increase their donations to a charity providing economic relief. Second, it should also be true that for participants whose beliefs about local health and/or labor market consequences are more or less correct, *all* treatment effects should be zero, since no new information is acquired.

To this end, consider the left panel of Figure 2, which summarizes the estimated effect (relative to those who receive no information) on the likelihood of donation to Feeding America for those who receive information about unemployment or death rates, but not both, for three broad categories or bins of misperception. We start with the observation that for those whose beliefs are within 10 percentage points of the corresponding correct values – that is, those for whom the treatments have little information value – the effects are indeed close to zero: -0.017 ($p = 0.919$) for those who receive labor market information and -0.052 ($p = 0.786$) for those who are shown data on death rates.

The same panel also reveals, however, that information about unemployment rates did have an effect – asymmetric, perhaps – on the likelihood of donation to Feeding America. In particular, those who learned their beliefs about labor markets were 10% or more too optimistic – that is, those with large negative perception gaps – were 22.1 percentage points more likely ($p = 0.032$) to donate to Feeding America, a large and significant difference. Those who experienced positive surprises, on the other hand, were 8.72 ($p = 0.223$) percentage points less likely to donate, but the difference was not statistically significant at even the 10% level. The result is not just important in its own right, but because it demonstrates that our null findings on environmental effects cannot be attributed to ignoring or misunderstanding the information in our interventions. This said, the left panel also reveals that information about state-level death rates, positive or negative, had no effect on donations to Feeding America.

It is in this context that the findings on the right hand side panel of the same Figure 2 are striking. If bad economic news caused the likelihood of donation to Feeding America to rise, there is no discernible crowding out of donations to The Nature Conservancy. In

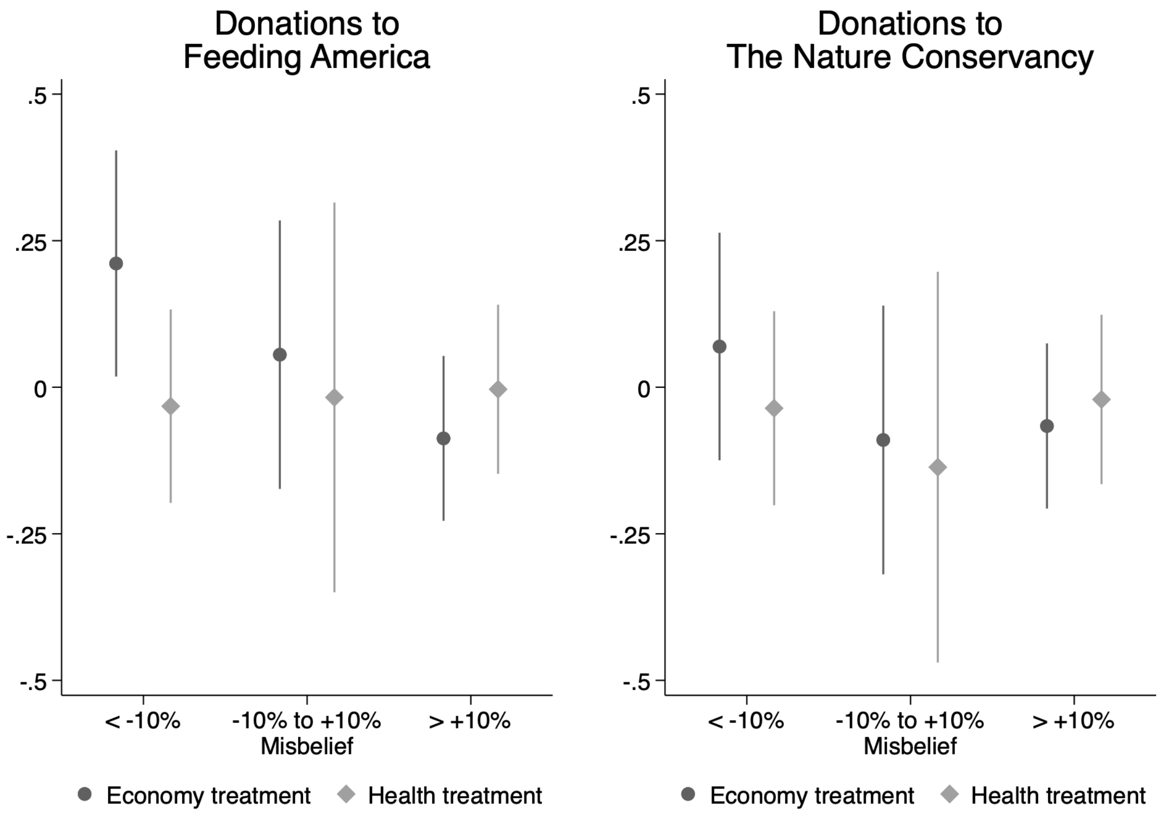

Figure 2: Impact of Information On Donations

Notes: Predicted likelihoods (relative to no information control) of donations by binned misperceptions of state-level unemployment and mortality rates, with 95% confidence intervals. The comparison here is to observations that did not receive relevant information. For example, for the left panel, donations to Feeding America of subjects that received information about state-level mortality rates (i.e., participants in T^H treatment) are compared to donations of subjects that did not receive information about mortality rates (i.e., participants in T^E treatment and in the control group). Subjects in T^{HE} treatment are not included in this analysis.

fact, the likelihood *increases*, but the effect is smaller and insignificant $(0.069, p = 0.482)$. Further, as with Feeding America donations, information about state-level death rates has no effect. This does not mean, of course, that our respondents do not care about The Nature Conservancy, but rather that their high donation rates (52.2% in the control arm, for example) are inelastic with respect to new information about health or labor markets.

With this in mind, we now turn to Table 2, where we report our main results on the effects of our information interventions on *Priority Environment*. The leftmost column contains the estimates for the unpooled model described in the previous section, without additional controls. We first note that none of the treatment indicators is statistically significant, consistent with the expectation that, conditional on correct beliefs, information provision should have no effect on environmental priorities. More important, perhaps, none of the three treatment interactions is large and/or significant, either. For those who received just information about COVID-19 mortality rates, for example, each percentage point "correction" was associated with a 0.0003 ($p = 0.77$) unit change in *Priority Environment*. Someone who discovers their belief was double the actual death count – that is, who was 100 percentage points too pessimistic – would increase their *Priority Environment* score 0.03 on a five-point scale, and this is the largest of the treatment effects. Furthermore, if these were small – as opposed to null – effects, we would expect the coefficients on misbeliefs and treatment interactions to be reverse-signed: if a belief that the death count was high caused one to reduce the weight attached to environmental concerns, the correction of that belief should produce the opposite effect. With one exception – the small (0.0008) and insignificant ($p = 0.67$) negative coefficient on the interaction of the combined information intervention and unemployment misperception – this is not the case, however.

It is nevertheless reasonable to wonder whether the data could be pooled – that is, the effect of a particular sort of information is the same, no matter what other information is shared – and, if so, whether the results would confirm or challenge our initial conclusions. The answer to the first question is, yes. In terms of the notation introduced in the previous section, the composite null hypothesis $\beta_3 - \beta_8 = \beta_6 - \beta_9 = 0$ can not be rejected at any reasonable level ($p = 0.40$). (The p-values for the separate hypotheses $\beta_3 = \beta_8$ and $\beta_6 = \beta_9$ are 0.83 and 0.18, respectively.)

	(1) Unpooled	(2) Pooled	(3) Controls	(4) Spline
Health treatment =1	0.0625			
	(0.1202)			
Pooled health treatment =1		0.0395	-0.0082	-0.1542
		(0.0841)	(0.0802)	(0.1102)
Misbelief as % of mortality rate	0.0004	0.0004	-0.0001	-0.0001
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Health treatment * Mortality misbelief	0.0003			
	(0.0012)			
Pooled health treatment * Mortality misbelief		0.0002	0.0006	-0.0036
		(0.0011)	(0.0011)	(0.0024)
Pooled health treatment * max(Mortality misbelief, 0)				0.0060*
				(0.0030)
Economy treatment =1	0.0194			
	(0.1275)			
Pooled economy treatment =1		0.0228	-0.0278	0.0152
		(0.0927)	(0.0897)	(0.1110)
Misbelief as % of unemployment rate	0.0004	0.0005	0.0002	0.0003
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Economy treatment * Unemployment misbelief	0.0021			
	(0.0019)			
Pooled economy treatment * Unemployment misbelief		0.0004	0.0005	0.0024
		(0.0016)	(0.0015)	(0.0036)
Pooled economy treatment * max(Unemployment misbelief, 0)				-0.0022
				(0.0042)
Health/economy treatment =1	0.0963			
	(0.1285)			
Health/economy treatment * Mortality misbelief	0.0000			
	(0.0015)			
Health/economy treatment * Unemployment misbelief	-0.0008			
	(0.0018)			
Female			0.0692	0.0834
			(0.0837)	(0.0844)
Liberal			0.2952***	0.2922***
			(0.0419)	(0.0413)
Constant	4.0314***	4.0435***	2.9715***	2.9686***
	(0.0847)	(0.0746)	(0.1763)	(0.1746)
Observations	555	555	536	536
R-squared	0.008	0.004	0.116	0.123
Control_Mean	4.04	4.04	4.04	4.04

Table 2: Impact of Two Kinds of Information Provision on Prioritization of Environment

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variable in all columns is *Priority Environment*, a five-point Likert scale. In the second, third, and fourth columns, the information indicator is equal to 1 if the relevant data was shared with the individual, whether or not other data also was.

The second column of Table 2, which contains estimates for the pooled specification (still without controls), shows that, if anything, our null effect(s) findings are reinforced. Each percentage point correction of death rate misbeliefs is now estimated to increase *Priority Environment* by 0.0002 ($p = 0.846$), a smaller and even less significant effect than the one just discussed. In a similar vein, each percentage point correction of a misbelief about unemployment rates is associated with a 0.0004 ($p = 0.804$) increase in *Priority Environment*. The third column adds two natural controls – a *Female* indicator and a five-point *Liberal* scale – to the specification. The treatment effect remains small and insignificant, and the demographic coefficients provide a different sort of robustness check: a one-point increase in *Liberal* is associated with a mean increase of 0.295 ($p < 0.001$) in *Priority Environment*, a 7.1% increase relative to the control mean. The estimated coefficient on *Female* is positive (0.069) – consistent with most of the literature on gender and climate change (Bush and Clayton (2022), for example) – but small and insignificant ($p = 0.409$), which we attribute to age of our participants.

Asymmetric treatment effects are common in the information provision literature (Haaland, Roth and Wohlfart, 2023) and while we did not include it in the pre-analysis plan, the fourth and final column of Table 2 reports exploratory estimates for the following linear spline model:

$$\begin{aligned} Y_i^k = \nu_0 &+ \nu_1 T_i^H + \nu_2(Belief_i^H - Actual_i^H) + \nu_3 T_i^H(Belief_i^H - Actual_i^H) \\ &+ \nu_4 \max(T_i^H(Belief_i^H - Actual_i^H), 0) + \nu_5 T_i^E + \nu_6(Belief_i^E - Actual_i^E) \\ &+ \nu_7 T_i^E(Belief_i^E - Actual_i^E) + \nu_8 \max(T_i^E(Belief_i^E - Actual_i^E), 0) + \nu\mathbf{X_i} + \epsilon_i. \end{aligned} \tag{3}$$

In particular, if "good news" about COVID-19 death rates – that is, $T_i^H(Belief_i^H - Actual_i^H) > 0$ does not crowd in environmental concerns, but the combination of "bad news" and a finite pool of worries crowds them out, we would expect $\nu_3 > 0$ and $\nu_4 = -\nu_3 < 0$, but no such pattern is observed. There is still no evidence of a response, asymmetric or otherwise, to news about unemployment, while for mortality rates, we find $\nu_3 \approx 0$ but $\nu_4 = 0.006$ ($p = 0.042$), which implies that bad news has no effect, and good news increases concern for the environment.

5 Conclusion

This paper reports on what we believe to be two encouraging pieces of evidence on the environmental beliefs of young American adults: most believe that the environment should be prioritized – when asked to compare to other political and social belief, most respondents in our studies rank environment as one of the most important. This belief is robust with respect to variations in pandemic experience. College students sent home when the pandemic first hits in March 2020 do not change their views on the importance of the environment or their reported pro-environmental behaviors when asked two months later. Young adults participating in the online study about one year into the pandemic and presented with information about the pandemic economic and health costs on average report the same views on the priority of the environment as those who do not receive the same information (and similar to the college students in our first study). While our main outcome of interest – stated beliefs about the importance of the environment – did not require individual trade-offs, we nevertheless believe that self-reported attitudes and beliefs are important to understanding the evolution of broader environmental norms.

Beyond welcome efforts to replicate these findings with different samples, we see several possible extensions. First, while the first of the studies we discussed hinted that continued prioritization of the environment was reflected in individual behavior, more evidence is needed. Second, there are no analogous studies for older Americans or, for that matter, residents of other countries. Third, it remains to show whether our results should be viewed as a challenge to the finite pool of worries hypothesis or as an affirmation of the "specialness" of environment and climate-related concerns. That is, it is at least possible that individuals' pools are indeed finite, but that worries were re-allocated after the pandemic such that climate concerns were unaffected perhaps because younger adults implicitly understand the connection between threats such as the pandemic and climate. In this case a new threat may be deemed simply a new manifestation of the ongoing risk. It also remains to show what the political consequences of this moral bandwidth might be.

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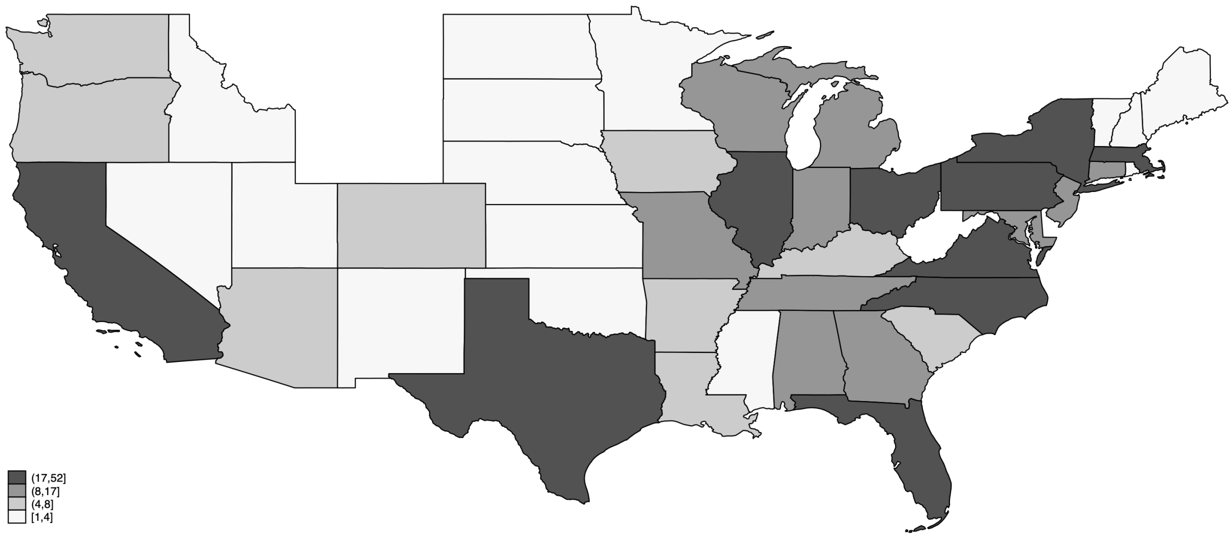
A Online Appendix

Table A.1: Summary Statistics and Comparison of Waves: College Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Observed twice			Observed once			P-value of diff
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	
Female	268	0.68	0.47	267	0.59	0.49	0.04
Foreign	261	0.07	0.25	258	0.12	0.32	0.06
Liberal (1-5)	241	4.10	0.80	232	3.96	0.87	0.07
Household income	245	5.71	2.45	250	5.76	2.68	0.82
February status	282	0.16	0.36	283	0.10	0.30	0.04
Graduate school	222	0.73	0.45	208	0.83	0.38	0.01
Interested (1-4) in:							
Fine and performing arts	272	2.48	0.92	268	2.39	0.90	0.23
Foreign languages	272	3.07	0.82	269	3.14	0.82	0.36
Literature, history, philosophy	272	2.81	0.89	269	2.88	0.81	0.30
Economics and political science	271	2.95	0.94	268	2.96	0.90	0.93
Psychology	270	2.97	0.76	266	3.00	0.77	0.74
Life sciences	270	2.83	0.86	266	2.91	0.89	0.27
Physical sciences, mathematics	271	2.70	1.03	269	2.77	0.97	0.43
Environmental studies	270	3.08	0.79	267	2.95	0.77	0.05
International and global studies	271	2.86	0.76	267	2.90	0.83	0.61

Notes: Comparison of student characteristics of those who completed two surveys (observed twice) and one survey (before or after).

Figure A.1: Geographic Coverage in mTurk Data



Notes: The map shows the number of observations by state (excluding Alaska).

Table A.2: Balance Table and Summary Statistics (mTurk Data)

Variable	N	(1) 1 Mean/SD	N	(2) 2 Mean/SD	N	(3) 3 Mean/SD	N	(4) 4 Mean/SD	N	(5) Total Mean/SD	F-test for joint orthogonality
PriorityEnviro (1-5)	138	4.043 (0.935)	139	4.129 (0.947)	140	4.093 (1.038)	138	4.116 (0.952)	555	4.095 (0.967)	0.210
DonationNature (=1)	138	0.522 (0.501)	139	0.446 (0.499)	140	0.450 (0.499)	138	0.464 (0.501)	555	0.470 (0.500)	0.681
DonationFeeding (=1)	138	0.457 (0.500)	139	0.453 (0.500)	140	0.479 (0.501)	138	0.442 (0.498)	555	0.458 (0.499)	0.131
Female (=1)	138	0.529 (0.501)	138	0.558 (0.498)	139	0.561 (0.498)	138	0.616 (0.488)	553	0.566 (0.496)	0.738
Liberal (1-5)	130	3.585 (1.112)	137	3.686 (1.027)	133	3.677 (1.145)	138	3.833 (1.036)	538	3.697 (1.081)	1.222
COVIDknow (=1)	138	0.725 (0.448)	139	0.763 (0.427)	140	0.750 (0.435)	138	0.804 (0.398)	555	0.760 (0.427)	0.837
COVIDConcern (1-4)	138	3.217 (0.732)	139	3.266 (0.795)	139	3.295 (0.747)	138	3.391 (0.729)	554	3.292 (0.752)	1.313

*Notes: F-test is for joint orthogonality of each variable across all treatment arms. *** p<0.01, ** p<0.05, * p<0.1. Column (5) shows summary statistics for the entire sample.*