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Cross-temporal relations of conditional risk perception measures with protective actions against COVID-19

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ABSTRACT

Two decades ago a research team clarified that cross-sectional associations of risk perceptions and protective behavior can only test an “accuracy” hypothesis: e.g., people with higher risk perceptions at T_i should also exhibit low protective behavior and/or high risky behavior at T_i. They argued that these associations are too often interpreted wrongly as testing two other hypotheses, only testable longitudinally: the “behavioral motivation” hypothesis, that high risk perception at T_i increases protective behavior at T_{i+1}, and the “risk reappraisal” hypothesis, that protective behavior at T_i reduces risk perception at T_{i+1}. Further, this team argued that risk perception measures should be conditional (e.g., personal risk perception if one’s behavior does not change). Yet these theses have garnered relatively little empirical testing. An online longitudinal panel study of U.S. residents’ COVID-19 views across six survey waves over 14 months in 2020–2021 tested these hypotheses for six behaviors (hand washing, mask wearing, avoiding travel to infected areas, avoiding large public gatherings, vaccination, and [for five waves] social isolation at home). Accuracy and behavioral motivation hypotheses were supported for both behaviors and intentions, excluding a few waves (particularly in February–April 2020, when the pandemic was new in the U.S.) and behaviors. The risk reappraisal hypothesis was contradicted—protective behavior at one wave increased risk perception later—perhaps reflecting continuing uncertainty about efficacy of COVID-19 protective behaviors and/or that dynamic infectious diseases may yield different patterns than chronic diseases dominating such hypothesis-testing. These findings raise intriguing questions for both perception-behavior theory and behavior change practice.

1. Introduction

Seemingly paradoxical relationships can occur between risk perception and behavior, as when high risk perception may be associated with low rather than expected high preparedness (e.g., Wachinger et al., 2013 on natural hazards). Risk perception was associated with behaviors for adapting to, but not mitigating, air pollution in Beijing (Tan and Xu, 2019), but higher perceptions of severity and higher negative emotion in an early-2020 Chinese sample were associated with more protective behavior against the coronavirus (Ning et al., 2020). Across multiple topics and disciplines the association of risk perception and behavior is not straightforward.

Our focus is improving measurement of the association of risk perception with behavior, following advice by Neil D. Weinstein, an early prominent risk perception researcher, and his colleagues, to apply longitudinal research designs (cf. Siegrist, 2013) and conditional risk perception measures that account for whether the respondent has or is taking protective action (Brewer et al., 2004). Their proposals focused only on actual behavior. However, because some researchers’ empirical tests of their hypotheses included or emphasized behavioral intentions over behavior, and the role of behavioral intentions is also of theoretical and practical interest, we also address this question left unaddressed by Brewer et al. (2004). Causal relationships cannot be identified in the cross-sectional (i.e., self-reports of perceived risk and behaviors occur in the same survey) studies dominating perception-behavior literatures. Barriers to identifying potential causal relationships are reduced, but not eliminated, when longitudinal or experimental studies are used instead. Longitudinal designs have drawbacks: e.g., Gerrard et al. (1996) noted that potentially confounding variables must be controlled carefully; the best interval to capture behavior change is unknown; shifts in public knowledge can obscure perception-behavior relationships (cf. Brewer et al., 2007a); and stabilizing objective risk and precautionary behavior

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may eliminate observable variance in behavior change (cf. Weinstein and Nicolich, 1993; Brewer et al., 2007a). But experimental studies, the main alternative, also can be problematic, particularly for the efficacy of and reactance to experimental manipulations.

We use a longitudinal panel study of Americans' views and self-reported behaviors on COVID-19 to test all three perception-behavior hypotheses (Brewer et al., 2004). Most previous studies focused on only one perception-behavior hypothesis, used only one or no conditional risk perception measure, tested only one behavior, or did not account for longitudinal sensitivity of perception-behavior associations (Supporting Information, Table 1). Our study provides the most extensive and granular data yet for testing the association between risk perception and protective behavior.

2. Background

2.1. Weinstein-Brewer theses

In several papers (Brewer et al., 2004; Weinstein, 1988, 1993; Weinstein and Nicolich, 1993; Weinstein et al., 1998, 2007), Weinstein and colleagues posited two critical theses about relations of risk perception and risk-related behavior: 1) differences between accuracy, risk reappraisal, and behavioral motivation hypotheses about such relationships are often unrecognized, and 2) questions about risk perception should indicate whether or not protective measures are or will be taken, as "conditional" risk perception measures. The Weinstein-Brewer team was not alone in raising such issues in perception-behavior relationships, although with much mutual citation. For example, behavioral motivation was considered in reviews by Janz and Becker (1984) and Harrison et al. (1992), on cancer screening (McCaul et al., 1996), and the first paper to name the "motivational hypothesis" (Gerrard et al., 1996). The risk reappraisal hypothesis was addressed for cancer and smoking (Gibbons et al., 1991) and mammography (Aiken et al., 1995), while Van der Velde et al. (1996) found conditional measures of risk perception had stronger links with sexually transmitted disease behavioral intentions. However, Brewer et al. (2004) offered a more systematic assessment of requirements to properly test various kinds of perception-behavior relationships than earlier publications did.

Table 1
Summary of study hypotheses and research questions, and findings.

	Hypotheses and research questions being tested	Results
H1	In each wave people with higher risk perceptions will report lower protective behavior (Accuracy hypothesis).	Supported
H2	Higher risk perceptions in one wave will be associated with higher behavioral intentions in the next wave (Behavioral motivation hypothesis).	Supported
H3	Higher behavioral intentions in one wave will reduce risk perceptions in the next wave (Risk reappraisal hypothesis).	Not supported
H4	Risk perception-behavior associations will be similar for all protective behaviors.	Supported (except for avoiding travel and mask-wearing behavior)
H5	Personal risk perceptions that account for taken or intended protective actions (RPA) will be lower than personal risk perceptions that presume -no protective action (RPNA).	Supported
RQ1	Do perception-perception, behavior-behavior, and/or perception-behavior relationships change over time in magnitude or direction?	• RPNA was more persistent across time and explained more variance than did RPA. • Intention for protective behavior was generally high with temporal variations across actions • Perception-behavior association patterns changed over time, but inconsistently.
RQ2	Will RPNA-behavior correlations be higher, lower, or the same as RPA-behavior correlations?	Associations of the protective behaviors assessed here with risk perception were generally similar between RPNA and RPA.

The first thesis argued that among survey designs, only longitudinal rather than cross-sectional designs could properly assess two of these hypotheses. For example, cross-sectional risk perception measurement cannot distinguish between precautionary behavior associated with lowering perceptions and risky behavior raising perceptions. As adoption of self-protective actions likely occurs early in a threat's occurrence, over time any sample of non-adopters will be dominated by those thinking they cannot change their behavior (Weinstein and Nicolich, 1993).

Brewer et al. (2004; cf. Weinstein et al., 1998 on accuracy and behavioral motivation hypotheses) identified three hypotheses about perception-behavior relationships:

- **Accuracy**: *People perceive higher risk if they engage more in risky behaviors or situations, or enact less protective behavior.* This hypothesis is testable cross-sectionally. As protective action need not seem to reduce risk, Brewer et al. (2004) noted that an accuracy test could yield positive, negative, or zero correlations given initial and post-action risk perceptions (e.g., those who act may still see more risk than non-actors). Accuracy was confirmed for Lyme disease and vaccination (Brewer et al., 2004), and the Lyme-vaccinated exhibited lower perceived risk than did the unvaccinated (Brewer et al., 2007b).
- **Risk reappraisal**: *Increasing self-protective behaviors is associated with lower subsequent perceived risk as such behavior, if deemed effective, should reduce one's objective risks.* This can only be measured longitudinally, comparing behavior at Time 1 to risk perception at Time 2. This reappraisal depends upon belief in response efficacy (i.e., that the behavior will reduce, or has reduced, objective risk). As the behavior must precede measurement of perceived risk, it differs from accuracy. This is not always recognized even when citing Brewer et al. (2004); e.g., Dibonaventura (2007) used a cross-sectional research design to "test" reappraisal. Reappraisal was confirmed for Lyme disease and vaccination (Brewer et al., 2004).
- **Behavioral Motivation**: *Perceptions of high personal risk are associated with later behavior to reduce that perceived risk.* This can only be measured longitudinally, comparing risk perceptions at Time 1 to protective behavior at Time 2 (versus the reverse for reappraisal), and as with risk reappraisal depends upon the action's perceived risk-reduction efficacy. Yet it is also often "tested" with cross-sectional data, which can only test accuracy. Behavioral motivation was confirmed for vaccination (Brewer et al., 2004, 2007b; Weinstein et al., 2007).

In proposing these hypotheses, Brewer et al. (2004) cautioned about accounting for prior behavior (see Van der Pligt, 1996 on this being a rare control, and Stephan et al. 2011 for an example): e.g., controlling for prior-year vaccinations in probing vaccination intentions confounds earlier behavior with effects of prior risk perceptions on earlier behavior, "statistically removing the effects of the independent variable one wishes to test" (Brewer et al., 2004, p. 129; cf. McAuley et al., 2007 on confounding habits).

The second thesis was that too many studies, cross-sectional or prospective, failed to control for prior behavior or behavioral intentions in measuring risk perceptions (e.g., Weinstein et al., 1998). Without such control, "the observed perceived risk-behavior association will underestimate the true association between perceived risk without behavior and the behavior itself" (Brewer et al., 2007c, p. 138).

Conditional risk perception measures—indicating the level of risk contingent upon action or inaction—are needed. In a partial example, asking about perceived risk at Time 1 for vaccine inaction—“Let’s say that you do not get the Lyme vaccine. What do you think the chance would be that some time in the future you would get Lyme disease? Do you think that it’s likely or that it’s unlikely that you would get Lyme disease in the future?” was followed at Time 2 by either the identical wording, or by omitting the initial sentence for those who reported having been inoculated against Lyme disease (Brewer et al., 2004). We treat this as a “partial” application of conditional risk perception measures given its split-sample approach; later studies using both conditions do so for all respondents (as we do). Conditional non-action questions may depend upon perceived efficacy of the behavior. Brewer et al. (2007c) noted that mammography changes risk severity, while vaccination changes its likelihood. Thus conditioning’s salience depends upon whether severity, likelihood, or other types of risk perceptions are measured (e.g., Walpole and Wilson, 2021), although we note that all empirical studies to date appear to have used only such cognitive types of risk perception, rather than expanding into alternatives not covered by Brewer et al. (2004), such as affective measures.

Our review of empirical literature on the Brewer theses (Supporting Information, Table 1) found variability in conclusions across and within studies. Five studies found results consistent or mostly consistent with the accuracy hypothesis; six studies found results consistent or mostly consistent with the risk reappraisal hypothesis, but one study yielded uncertain results; and two studies found results consistent with the behavioral motivation hypothesis, but three studies yielded mixed results at best. Only five of 18 studies used no conditional risk perception measures, but only eight used both. Almost all such studies concerned chronic health issues (cancer to partner violence), excluding infectious diseases of interest here, and the few studies available prevent testing whether findings’ variability reflects different topics.

2.2. This study

A new study of Brewer et al. (2004) theses has many justifications, including that only Brewer et al. (2004) and Hay et al. (2007) examined all three perception-behavior hypotheses, limiting evidence of their applicability to the same behaviors and sample.

H1. . In each wave people with higher risk perceptions will report lower protective behavior or behavioral intentions (accuracy).

H2. . Higher risk perceptions in one wave will be associated with higher behavior or behavioral intentions in the next wave (behavioral motivation).

H3. Higher behavior or behavioral intentions in one wave will reduce risk perceptions in the next wave (risk reappraisal).

Including multiple protective behaviors against COVID-19 better tests cross-behavior consistency than do prior studies; we assume that *any* behavior will support Brewer et al. (2004) hypotheses.

H4. Risk perception-behavior/intention associations will be similar for all protective behaviors.

Largely unaddressed is whether such associations vary over time, given few studies with even three waves. Grevenstein et al. (2015, p. 382) found few positive associations between risk perception and frequency of use of tobacco, alcohol, and cannabis, but some temporal variation “might imply age specific development processes” (Grevenstein et al., 2015, p. 382), plausible given that their sample was surveyed over 10 years, with a mean starting age of 14. However, Grevenstein et al. (2015) linked frequency of personal behavior with opinions about *other* people’s risk (versus one’s own risk). COVID-19 experience could alter one or both assessments: i.e., people learn over time how risky the pandemic is for them, and how effective protective behaviors might be, but changes in the virus (new variants) and its spread (e.g., shutdown severity; public compliance) also alter what experience means. Thus mean associations of the same risk perception or behavior/intention measure across time may change. For example, early in the U.S. pandemic (e.g., our Wave 1, late February 2020) particularly unexpected associations may arise for such a novel threat, with later associations perhaps more stable and expected.

RQ1. Do perception-perception, behavior-behavior, and/or perception-behavior relationships change over time in magnitude or direction?

Both action and no-action risk perception measures were rarely used to test the three perception-behavior hypotheses, but mostly whether risk perception was higher for the no-action than action measure. This is expected if people see any risk-reduction efficacy at all to target behaviors.

H5. *Personal risk perceptions that account for taken or intended protective actions (RPA) will be lower than personal risk perceptions that presume no protective action (RPNA).*

The conditional item specifying no action (RPNA) is our focus, as it precludes overlap of the predictor (perception) and outcome (behavioral intentions/reported behavior), and thus most cleanly tests Brewer et al. (2004) hypotheses. When both measures were used (Supporting Information, Table 1), RPNA items usually exhibited stronger behavioral associations, although RPA-behavior correlations might be higher as RPA brings behavior into the perception measure.

RQ2. *Will RPNA-behavior correlations be higher, lower, or the same as RPA-behavior correlations?*

We summarize these hypotheses and research questions in Table 1.

3. Methods

3.1. Sampling

Data came from a six-wave longitudinal panel study over almost 14 months—February 28–29, 2020 (Wave 1, n = 2004), April 27–May 6, 2020 (Wave 2, n = 1613), August 5–13, 2020 (Wave 3, n = 1184), October 12–22, 2020 (Wave 4, n = 1026), January 22–February 11, 2021 (Wave 5, n = 866), and March 25–April 13, 2021 (Wave 6, n = 1019)—recruited from the Prolific online panel of American adults. All people answering a given wave were invited to the next, excluding Wave 6 where everyone answering Wave 1 was invited to test for attrition effects. This study was reviewed by the Decision Research Institutional Review Board (IRB) for adherence to ethical research standards, and deemed exempt. Participant consent was indicated by survey participation after completing informed consent.

3.2. Measures

Age, gender, education (treated as ordinal), income, race/ethnicity, and political ideology were collected in Wave 1. Two conditional personal risk perception items (fully labeled scale; 1 *no risk*, 6 *very high risk*) included no-action or RPNA [Risk Perception No-Action] (“How much risk does the coronavirus pose to you or your family, *if you or your family* ***don’t*** *do anything new* to protect yourself against the coronavirus?„) and action or RPA [Risk Perception Action] (“Now, how much risk does the coronavirus pose to you or your family*, if you or your family do anything new* to protect yourself against the coronavirus?„; original emphases). The reason for this distinction, argued Brewer et al. (2004), is that risk perception questions that do not specify such behavioral conditions may elicit identical answers from people varying widely in knowledge, intentions, and other factors that shape such responses.

Two critical issues about these risk perception questions must be noted: their cognitive emphasis, and the generality of their references to “new” behavior. The generic phrasing we used here (“how much risk does the coronavirus pose … ?“) is common in research in risk analysis, but it is as cognitively focused as more specific concepts—e.g., judged likelihood of hazard exposure, judged susceptibility to suffering consequences if exposed, judged severity of consequences—prominent in recent risk perception taxonomies (e.g., Walpole and Wilson, 2021). Conditional risk perception research, and research testing Brewer et al. (2004) hypotheses, also has emphasized cognitively phrased measures. Non-cognitive options exist, most prominently affective measures—e.g., anticipatory emotions; anticipated emotions (e.g., Sheeran et al., 2014; Walpole and Wilson, 2021 included concern, worry, and fear in their affect subscale)—but also experiential measures (e.g., Ferrer et al., 2016), although there appear to be some unexplored overlaps between these latter two types. While as we note below we included other risk perception types, including affective ones, given that the accuracy, behavioral motivation, and risk reappraisal hypotheses have been tested only with cognitive risk perception measures, for consistency and brevity we maintain that focus here.

The generality of "new" behavior in our risk perception items refers to the fact that most prior conditional risk perception research, on conventional (mostly non-infectious) health issues, has specified a behavior in the conditional risk perception question: e.g., "if you don't get vaccinated" in Brewer et al.'s (2004) example for Lyme disease. However, there are some exceptions to that specificity (Dibonaventura, 2007; Kim et al., 2007; also see Van der Velde et al., 1996, in a study that preceded the Brewer et al., 2004, paper), and there were good reasons for not doing that here. First, unlike diseases and health conditions which are driven primarily by a single risky behavior (e.g., smoking for lung cancer; heavy alcohol consumption for alcohol use disorder), the COVID-19 pandemic had more than one transmission route, although aerosols turned out to be the most pertinent, and that was scientifically unclear—and even less clear in pronouncements by the CDC and WHO—for an extended period. Furthermore, there were a variety of means by which exposure to potentially infectious aerosols could be reduced, such as mask wearing, social distancing, avoidance of large public gatherings, and isolation at home, among others. Thus implementing the specific-behavior conditional risk perception option here would at minimum have required 12 risk perception items (2 (RPNA, RPA) X 6 (six behaviors used here: see below)). But our project overall was not focused solely on testing the Brewer et al. (2004) hypotheses, and included eight other risk perception items (e.g., implementing affective, severity, and likelihood types of risk perception) in one or more waves, not to mention other dependent variables than behavior and multiple moderator and mediator variables for other, non-Brewer, analyses. If the lack of specification means that respondents might have had all kinds of "new" behavior in their respective minds when answering these general conditional risk perception questions, the resulting statistical noise should attenuate the associations observed. If those associations are strong, therefore, this should underline the robustness of those findings.

Protective behaviors are separate from the risk perception items detailed above. We used the scale, inspired by the precaution adoption process model (e.g., Weinstein and Sandman, 2002), "My household … 1) has never considered taking this action, 2) is considering it, 3) decided against taking this action, 4) decided to take this action, 5) has taken this action, 6) has taken this action and will continue to take this action as needed." One question about use of this ordinal scale is whether it unduly collapses intentions and behavior, given that Brewer et al.'s (2004) hypotheses and empirical study focused only on behavior (in that case, for Lyme disease vaccination). Several authors citing Brewer et al. (2004) who tested one or more of their hypotheses empirically (Supporting Information) did include intentions (e.g., Dibonaventura, 2007; Drinkwater, 2014 on the decision to fly; Harding and Helweg-Larsen, 2008; Kim et al., 2007, 2022; Renner et al., 2008; Sheeran et al., 2014; Waters et al., 2019). In fact, Sheeran and colleagues found more consistency in their meta-analysis between risk perceptions and intentions than for the risk perception-behavior association. This usage may or may not be consistent with Brewer et al.'s (2004) aims—they did not make any explicit claim about whether their hypotheses did not or could not apply to intentions—but scholarly findings suggest that the hypotheses are able in at least some cases to apply to intentions as well as behaviors. We also note that unlike most health behavior research testing one or more Brewer hypotheses, we were dealing with a disease that varied in incidence across space and time (unlike, say, lung cancer or diabetes), and for which there were multiple potentially-protective behaviors of varying usage and perceived efficacy over time. Focusing only on reported COVID-protective behavior drastically reduces the number of cases that reflect the condition (here, enacting the behavior, rather than intending to implement it), even before we account for results from Wave 1, so early in the U.S. pandemic that confirmed SARS-CoV-2 infections were <50, making some behaviors quite infrequent.

Considering all of these factors, we ran analyses for both reported behavior alone, and for behaviors and intentions combined, to address questions about how far the Brewer et al. (2004) hypotheses should be applied. We found that behavior-only results differed little from the behavior + intention results, so we report the former in Supporting Information, and focus on the latter in our main text.

We assessed these intentions and behaviors for each of six actions: "wash hands with soap and warm water many times a day"; "wear a face mask when going out in public"; "avoid travel to infected areas in China or other countries, including U.S. areas where people have been infected"; "avoid large public gatherings (including formal organized events such as concerts, sports events, or fairs, or informal gatherings like going to the mall, school, work or other places where lots of people happen to be"; "getting vaccinated when a coronavirus vaccine becomes available"; and "stay at home and isolate household from outside contacts" (the latter in Waves 2–6 only). We omitted one action included in this survey that some people reported taking—avoiding Asians—because to include this spurious "protective" action would make nonsense of the presumption that (e.g.) risk reappraisal depends upon valid belief that the previously taken action would be effective at self-protection (Brewer et al., 2004).

3.3. Analyses

We recoded behavioral responses as binary, with intention (decided to take) and behavioral reports (has taken; has taken… and will continue) coded "1," and all other responses (never considered; considering; decided against) coded "0." For the behavior-only analyses the coding was "1" for behavioral reports, and "0" for all other responses, including intentions. For vaccination specifically, we omitted all behavior responses in Waves 1–4, as vaccination was not available until Waves 5–6.

The R lavaan package allowed path modeling of variables' relations within and across waves, for more economical testing of Brewer et al.'s (2004) hypotheses in one model by comparing cross-lagged effects and cross-sectional associations. The false discovery rate method, applied to all analyses, controlled for potentially higher error rates due to testing multiple parameters (Benjamini and Hochberg, 1995; Glickman et al., 2014). We weighted descriptive statistics for risk perceptions and behavioral intentions using the *anesrake* package in R software to reflect distributions of gender (male, female), age (18–44, 45–64, 65+), education (high school or less, some college, bachelor's degree or more), and race/ethnicity (non-Hispanic white and other groups) as estimated in the U.S. Census 2020 Current Population Survey.

We ran separate models for RPNA's and RPA's respective associations with behavior, for each of the six protective actions. We controlled for demographic variables' effects (gender, age, race/ethnicity, education, income, and political ideology). Demographic covariates had few, weak, and inconsistent effects on specific behaviors with risk perception included (Supporting Information, Table 2). For example, despite strong associations between education and income in some data sets, the

Table 2
Sample (wave 1) versus U.S. Adult demographics.

Demographic Categories	N	Sample (%)	2020 U.S. Census (%)
Gender			
Male	997	50.4	48.4
Female	980	49.6	51.6
Age			
18-44	1540	77.0	45.9
45-64	385	19.3	32.4
65+	74	3.7	21.7
Race/Ethnicity			
Non-Hispanic White	1442	72.1	61.6
Non-Hispanic Black	153	7.6	12.4
Native American	13	0.6	1.1
Hispanic	121	6.0	18.7
Asian/Pacific Islander	202	10.1	6.2
Other	70	3.5	2.9
Education			
High school or less	269	13.4	37.6
Some college	631	31.8	27.6
Bachelor's degree or more	1095	54.7	34.8
Income			
Under $15,000	215	10.8	9.4
Under $100,000	1630	81.6	66.5
Party			
Democrats	993	49.6	NA
Republican	308	15.4	NA
Independent or undeclared	660	34.9	NA
Ideology			
Slightly to extremely liberal	1229	61.4	NA
Moderate	380	19.0	NA
Slightly to extremely conservative	392	19.5	NA

NA = not available.

correlation here was low (r = 0.27, $p < .001$), and running RPNA-behavior analyses with and without either variable differed non-substantively with our results using both, except that the RPNA_4 → Wash_5 association became significant at adjusted $p < .05$ without income.

Residual covariance of the risk perception and behavioral report at each wave after Wave 1 allowed us to probe the cross-sectional (vertical) association between the variances of those variables that are not accounted for by other predictors (i.e., risk perception and behavioral reports in the prior wave).

Data and scripts used for these particular analyses are provided in Supporting Information. The full data set for the entire research project will be archived at the Interuniversity Consortium for Political and Social Research once the project is completed.

4. Results

4.1. Sample

Respondents to Wave 1 were, compared to 2020 U.S. Census estimates for U.S. adults 18+ (Table 2)—using Current Population Survey Educational Attainment tables for adults 18+ because official 2020 Census results were not yet available at this writing—slightly less female, substantially younger, more non-Hispanic white, and far better educated. Household income was on average poorer than for the U.S. overall. This pattern is not unexpected for online samples, with the age of the sample meaning that many more than in earlier generations have college educations but have not yet had the time to turn those credentials into high incomes. Half reported being Democrats, a third independent or undeclared political partisanship, and the rest Republican; almost two-thirds reported slightly to extremely liberal political ideology, a fifth reported conservative ideology.

We compared risk perception and behavior results, and demographics of those dropping out at some point (n = 1241, including 271 returning in Wave 6) to those finishing all surveys (n = 764; 38.1%) to assess attrition effects. Most W1 and W6 risk perception and behavior results differed non-significantly between dropouts and finishers. On demographics, gender and political party exhibited no difference in attrition; finishers were more likely to have college educations (χ^2 (10, n = 2001) = 28.93, $p < .01$) and report non-Hispanic white ethnicity (χ^2 (5, n = 2001) = 23.62, $p < .001$), and less likely to be young (χ^2 (10, n = 1999) = 113.58, $p < .001$). Given these results, we find no substantive attrition effect.

We also assessed whether missing data (i.e., non-response to survey items) occurred in patterns. Note that lavaan only includes cases that answered all six waves (or all five waves for self-isolation), so attrition does not count as missing values. Missing values found in our risk perception and behavior variables across all six waves ranged from 0.0% to 1.45%, with the only values over 1.0% occurring in Wave 6. These values were likely missing at random, given non-significant results in the Missing Completely At Random test (MCAR; Little, 1988), with χ^2 ranging from 4.39 to 42.56. When missing data are MCAR, results using the full set of cases will have lower statistical power without biasing observed results (Jakobsen et al., 2017). No demographic characteristics showed significant associations with missing data in chi-square tests. Although multiple imputation is often deemed appropriate to deal with missing data for its unbiasedness and efficiency, conclusions as to the criterion for when imputation should be used differ (e.g., Jakobsen et al., 2017 suggested 5% as the threshold for imputation). In a longitudinal study where *distinctness*—independence of missingness mechanism and data generation—is less likely to be met, multiple imputation may not improve estimation quality, and even potentially weaken analyses. For example, one criterion for multiple imputation—ignorability—is hard to meet when data come from the same panel, and complete case analysis yields the same confidence intervals as multiple imputation when data are missing MCAR (Sidi and Harel, 2018; e.g., p. 170). Altogether, we believe that the impact of the loss of cases due to non-response is likely inconsequential here, so we used listwise deletion in the lavaan package for the following analyses. We also examined statistical power, using semPower in R to calculate power for each wave with RMSEA (Jobst et al., 2021). All types of protective behaviors achieved sufficient power (1 - beta >0.99) at 0.05 alpha with the current number of complete-case observations (n = 744). The threshold sample size for a model with RMSEA = 0.05, alpha = .05, with our current perception-behavior association predictors is 300, yielding power = 0.84. Given our data (containing more variables than used in current analyses) and the study's multiple aims (beyond just the Brewer et al., 2004, hypothesis tests), high power for the sample was unavoidable, warranting caution about effect sizes (below). G* Power (Faul et al., 2007) is unsuitable for calculating power for a SEM model (versus for *t* tests, ANOVA, or regressions), but as the path analysis is conceptually a combination of a series of regressions, we used this for backup power analyses for the small effect sizes ($f^2 = 0.03$ or 0.11) discussed below, at alpha = 0.05, n = 744 (complete cases across waves), with 8 predictors (e.g., "Wash_2" is regressed onto "RPNA_1", "Wash_1", and six demographic variables). These have the power of .94 or > 0.99, respectively, suggesting we have sufficient (large) power to detect effects.

4.2. Conditional risk perceptions

Personal-no action (RPNA) yielded consistently higher mean values at each wave, for both raw and weighted (for representativeness) means, than personal-action (RPA) responses (Table 3, top), confirming H5. Unweighted mean differences were 0.41 in Wave 1; $t\,(2001) = 26.41$, $p < .001$, but increased to ~ 0.83-0.88 (all $p < .001$), presumably reflecting greater frequency of already-taken behavior to be accounted for in RPA at these later waves.

We assessed relations within (over time) and between risk perception measures, to answer RQ1 on perception-perception links (Supporting Information, Fig. 1). Associations between conditional measures fell

Table 3
Risk perceptions and percentages of behavioral intentions/reports across six longitudinal panel surveys (mean and standard deviation; raw top, and weighted bottom, per row).

Risk Perceptions	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
	M (SD)	*M (SD)*	*M (SD)*	*M (SD)*	*M (SD)*	*M (SD)*
NO action (1–6)	2.79 (1.10)	3.79 (1.13)	3.97 (1.35)	3.90 (1.37)	4.05 (1.37)	3.77 (1.38)
	2.77 (1.08)	3.68 (1.36)	3.89 (1.44)	3.78 (1.45)	4.04 (1.41)	3.69 (1.44)
Action (1–6)	2.38 (0.97)	2.91 (1.08)	3.10 (1.15)	3.05 (1.15)	3.21 (1.20)	2.89 (1.14)
	2.38 (0.98)	2.85 (1.10)	3.05 (1.19)	3.01 (1.20)	3.17 (1.21)	2.84 (1.20)
Behavioral Intentions/Reports						
Hand washing	93.1%	97.0%	96.8%	96.8%	97.0%	95.9%
	88.6%	97.8%	95.5%	97.5%	97.1%	95.9%
Mask wearing	9.4%	84.4%	97.2%	96.5%	96.7%	96.5%
	10.7%	86.1%	97.0%	96.0%	97.1%	96.6%
Avoiding travel to infected areas	54.9%	72.9%	73.1%	71.7%	74.0%	72.9%
	49.8%	69.6%	68.3%	63.7%	69.6%	67.8%
Avoiding large public gatherings	25.3%	95.8%	91.7%	91.3%	92.1%	90.0%
	26.4%	94.4%	91.9%	89.0%	92.7%	88.5%
Getting vaccinated	23.4%	43.1%	45.1%	44.1%	64.2%	73.3%
	20.4%	40.9%	42.5%	43.9%	64.1%	69.4%
Isolating at home	NA	90.3%	82.1%	79.3%	81.7%	81.9%
		88.2%	80.9%	81.4%	81.0%	79.9%

NA = not applicable. Weighting adjusts for demographic differences between the sample and census data (see Methods-Analyses).

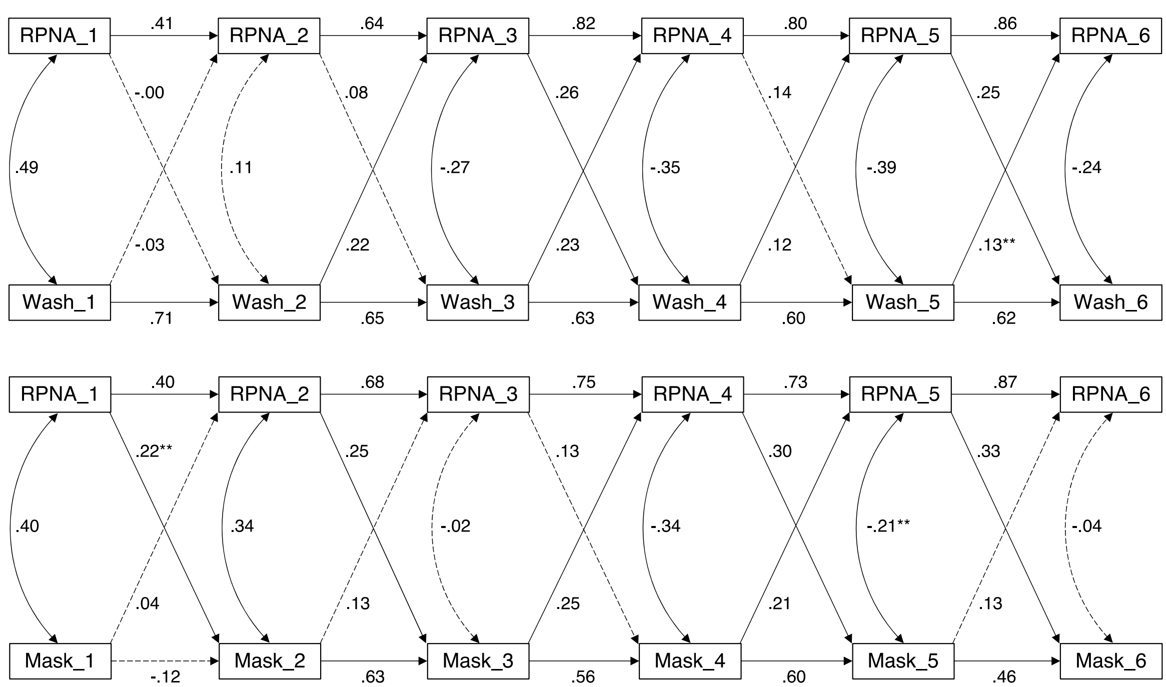

Fig. 1. COVID-19 risk perception assuming no action is taken (RPNA), and hand-washing (Wash) and mask wearing (Mask), for Waves 1–6. Vertical curved arrows (accuracy hypotheses) indicate cross-sectional associations; arrows pointing down and right (behavioral motivation hypotheses) and arrows pointing up and right (risk reappraisal hypotheses) indicate cross-temporal associations. Dashed lines reflect non-significant paths. $p < .001$ if significant, excluding * $p < .05$, ** $p < .01$.

over the first three waves, then largely stabilized. Within conditional perceptions, RPNA's associations across waves grew, while RPA's declined. Initially highly uncertain risk judgments reflect COVID-19's novelty, risk judgments excluding new protective behavior became more certain, and it was harder to decide over time how effective any new action might be. RPNA affected later RPA more strongly, controlling for prior RPA, than RPA affected later RPNA controlling for prior RPNA.

4.3. Behavioral reports

Table 3 (bottom) shows sample proportions reporting intentions *or* actions, so numbers here exceed those for self-reported behavior only in other COVID studies. Reported hand-washing was always high, although our definition—e.g., it did not specify 20 s or more of washing per session—and/or social desirability bias might have aided over-reporting of truly protective hand-washing. Mask-wearing was minimal (9%) at W1, 84% at W2, and about 97% subsequently. Avoiding travel to infected areas was endorsed by half in W1, rising to about three-quarters later. Avoiding large public gatherings was also low to start (25%), but in the 90% range later. Intentions to get a vaccine once it became available came from a quarter of the sample in February 2020, rose to about 45% for the next three waves, and then in the final two waves (when vaccines were available for at least some people), combined intentions/reported behavior rose to two-thirds and three-quarters, respectively. Home isolation or self-quarantine, not elicited until W2, was highest at that wave at 90%, but remained stable at about 82% in later waves.

4.4. Accuracy

Our interpretation of this Brewer et al. (2004) hypothesis (H1) assumes that people who report more protective behavior at a given time will report lower risk perceptions at that same time, producing negative associations (see earlier note that Brewer et al., 2004, argue that the relationship could also be positive or null instead, to be discussed later). Figs. 1–3 show relationships within and across time for RPNA perceptions and behavior across all six behaviors (Supporting Information compares RPNA with RPA results, with and without demographic covariates). Results for vertical arrows, embodying the accuracy hypothesis, show general support for negative relationships (we use unstandardized covariance estimates because behavior variables are categorical, with those for W2-6 being estimates of residual covariances). Quantitatively, 14 of 35 associations (the denominator comprises five behaviors with six waves, plus isolation over five waves) were negative and statistically significant per H1. Negative associations non-significant at $p < .05$ included eight—wearing masks (W3, W6), avoiding travel (W5, W6), avoiding large public gatherings (W3, W6), and vaccination (W3, W6). The remainder entailed partly unexpected (see below) positive relationships, including hand-washing W1 (W2 is positive but non-significant); mask-wearing W1-2; avoiding travel W1 (W2-3 non-significant); avoiding gatherings W1 (W2 non-significant); W1-2 vaccination; and W2 isolation (positive non-significant association W3).

Defining temporal changes in accuracy associations (RQ1) by changing signs, attenuation occurred over the first three waves: positive significant associations at W1 become non-significant associations at W2 and usually negative and significant associations later. For changes within sign, negative accuracy associations strengthen from W3 to W4-5, then weaken slightly in W6, for hand-washing; mask-wearing, avoiding large gatherings, and vaccination strengthened in W4-5, then became weaker and non-significant; and isolation strengthened from W3 to W4-5, then attenuated. Avoiding travel is an exception, as a significant negative association at W4 becomes non-significant and weaker in later waves.

4.5. Behavioral motivation

This Brewer et al. (2004) hypothesis (H2) posits higher risk perception prompts greater behavioral action and intentions later, supported here for arrows pointing down and right (Figs. 1–3), representing risk perception (e.g., RPNA_2) effects on behavior in the next wave (e.g., Wash_3) while controlling for the behavior's prior-wave effects (e.g., Wash_2 → Wash_3). With five associations per behavior (except for isolation), 17 of 29 associations show expected results, corrected for multiple tests. Positive non-significant associations include hand-washing (W2-3, W4-5), wearing masks (W3-4), avoiding travel (W2-3, W3-4), avoiding gatherings (W1-2, W2-3), vaccination (W2-3, W3-4), and isolation (W2-3). Unexpected negative associations, all non-significant, occurred only for hand-washing (W1-2) and vaccination (W1-2). Cohen's f^2 measuring local effect size for individual predictors suggest that the average risk perception effect at T_i (e.g., RPNA_1) on behavior at T_{i+1} (e.g., Wash_2) is 0.028 across protective behaviors, ranging from 0.003 (Avoiding travel) to 0.072 (Mask wearing), which can be interpreted as a small effect size (Selya et al., 2012).

To put motivational effects into context, Table 4 provides odds ratios (ORs) to represent increases' size, again controlling for behavior-behavior effects across waves. Few W1-3 odds ratios were statistically significant (~20% W1-2, 17% W2-3), 50% of W3-4, and almost all of W4-5 and W5-6 ORs. RPNA exhibited more significant ORs than did RPA. Weakest effects were on avoiding travel (16%–17% increases), with the strongest for mask wearing (25%–45%) and avoiding gatherings (16%–49%).

As for behavioral motivation differences across time (RQ1), path diagram patterns were mixed: perception-behavior associations grew

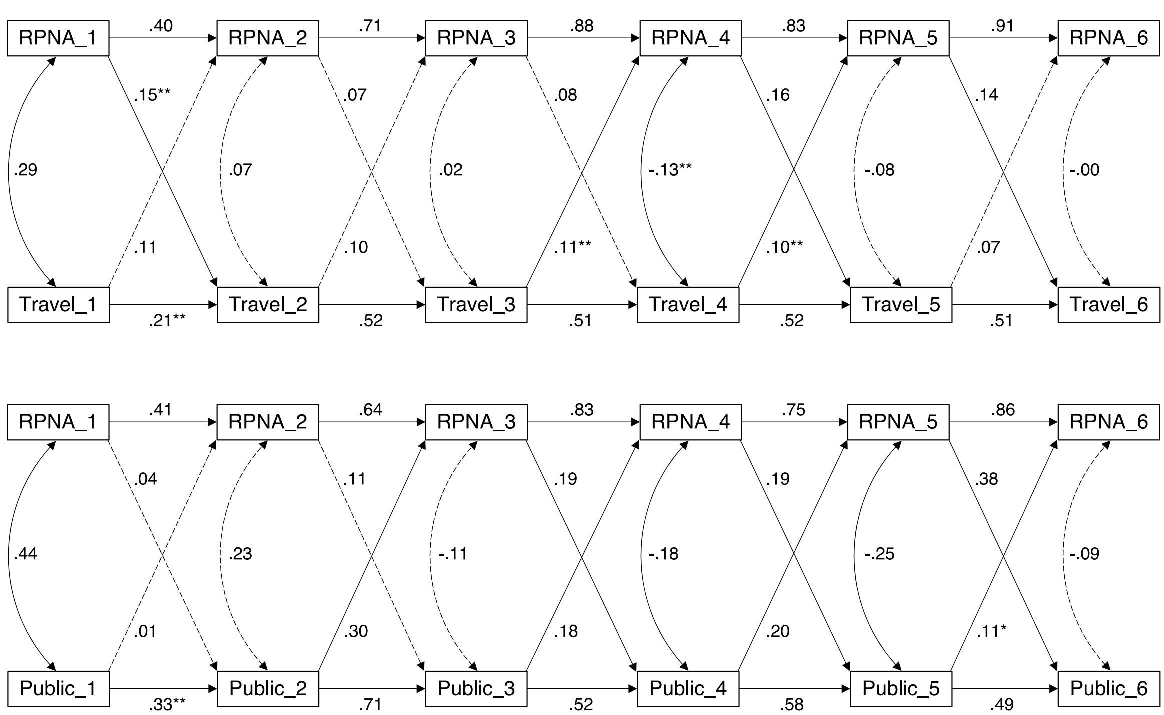

Fig. 2. COVID-19 risk perception assuming no action is taken (RPNA), and avoiding travel to infected areas (Travel) and avoiding large public gatherings (Public), for Waves 1–6. Vertical curved arrows (accuracy hypotheses) indicate cross-sectional associations; arrows pointing down and right (behavioral motivation hypotheses) and arrows pointing up and right (risk reappraisal hypotheses) indicate cross-temporal associations. Dashed lines reflect non-significant paths. $p < .001$ if significant, excluding * $p < .05$, ** $p < .01$.

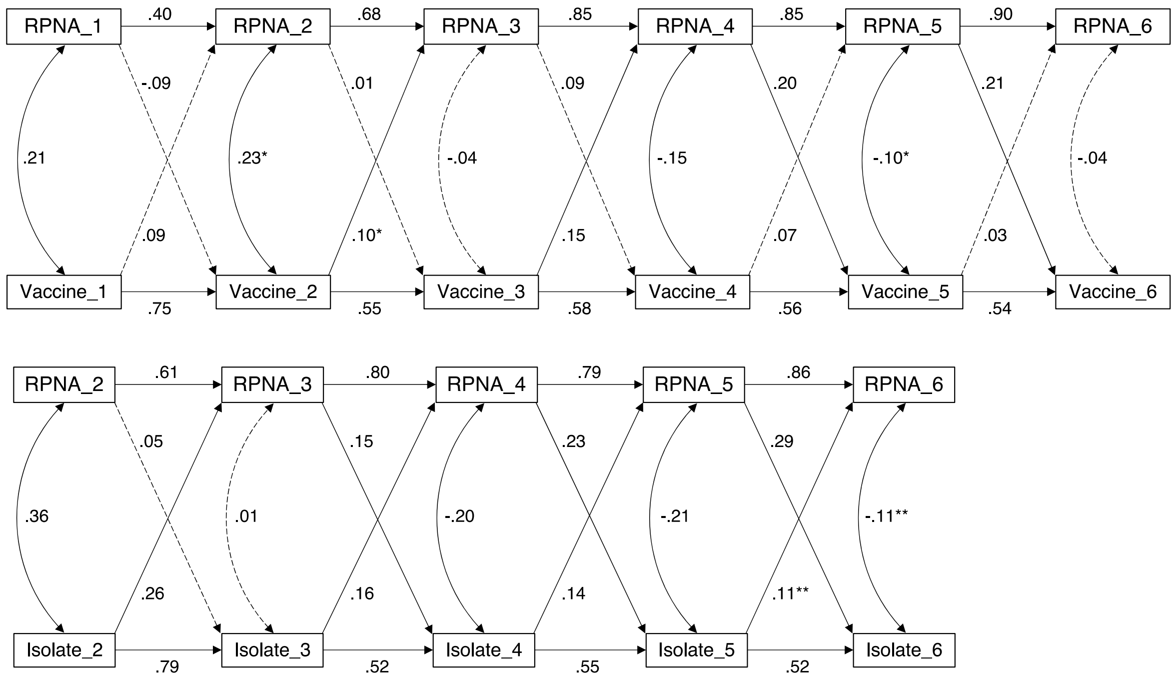

Fig. 3. COVID-19 risk perception assuming no action is taken (RPNA), and getting vaccinated when a vaccine is available (Vaccine, Waves 1–6) and isolating oneself at home (Isolate, Waves 2–5). Vertical curved arrows (accuracy hypotheses) indicate cross-sectional associations; arrows pointing down and right (behavioral motivation hypotheses) and arrows pointing up and right (risk reappraisal hypotheses) indicate cross-temporal associations. Dashed lines reflect non-significant paths. $p < .001$ if significant, excluding * $p < .05$, ** $p < .01$.

Table 4
Odds ratios for behavioral motivation effects (taking action in the following wave given prior-wave risk perceptions).

	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5	
Risk Perception	No-Action	Action	No-Action	Action	No-Action	Action	No-Action	Action	No-Action	Action
Hand washing	1.00 (0.85, 1.17)	1.01 (0.93, 1.30)	1.08 (0.96, 1.21)	1.18 (1.03, 1.35)	1.30 (1.17, 1.44)	1.37 (1.23, 1.53)	1.15 (1.02, 1.29)	1.14 (1.00, 1.30)	1.29 (1.17, 1.43)	1.37 (1.23, 1.53)
Mask wearing	1.25 (1.10, 1.42)	1.13 (1.01, 1.27)	1.28 (1.14, 1.45)	1.10 (0.94, 1.29)	1.14 (0.99, 1.32)	1.01 (0.86, 1.17)	134 (1.24, 1.45)	1.39 (1.24, 1.56)	1.39 (1.28, 1.51)	1.45 (1.33, 1.58)
Avoiding travel	1.16 (1.06, 1.28)	1.12 (1.00, 1.24)	1.07 (0.98, 1.16)	1.06 (0.96, 1.17)	1.08 (1.00, 1.18)	1.08 (0.98, 1.18)	1.17 (1.08, 1.26)	1.17 (1.07, 1.29)	1.16 (1.07, 1.25)	1.06 (0.97, 1.17)
Avoiding gatherings	1.04 (0.90, 1.19)	.95 (0.82, 1.10)	1.12 (1.00, 1.25)	1.21 (1.06, 1.39)	1.21 (1.11, 1.32)	1.16 (1.03, 1.31)	1.21 (1.12, 1.31)	1.19 (1.07, 1.31)	1.47 (1.36, 1.58)	1.49 (1.36, 1.63)
Getting vaccinated	0.91 (0.82, 1.01)	0.93 (0.83, 1.05)	1.01 (0.93, 1.09)	0.91 (0.82, 1.01)	1.09 (1.01, 1.17)	1.06 (0.97, 1.16)	1.23 (1.13, 1.33)	1.18 (1.07, 1.30)	1.24 (1.15, 1.33)	1.12 (1.03, 1.22)
Isolating at home	NA	NA	1.05 (0.96, 1.14)	1.03 (0.92, 1.16)	1.16 (1.08, 1.24)	1.13 (1.03, 1.24)	1.26 (1.17, 1.35)	1.25 (1.15, 1.36)	1.34 (1.25, 1.43)	1.40 (1.29, 1.52)

Odds ratios (ORs) exponentiated coefficients in the path diagrams: Wave X presents ORs of the perception's effect on reported behavior in Wave X + 1 controlling for behavior in Wave X. Parentheses contain 95% confidence interval (CI) for the odds ratio. Shading represents results not significant at $p < .05$, adjusted for the false discovery rate for multiple comparisons. All results controlled for demographic covariates (Methods).

across waves for mask-wearing, avoiding gatherings, vaccination, and isolation, but were up-and-down for hand-washing and avoiding travel. Odds ratios were highest for W5-6 effects for all behaviors, and with few exceptions (hand washing) ORs increased over time.

4.6. *Risk reappraisal*

This hypothesis posits that current protective behavior actions or intentions reduce later risk perceptions, again controlling for effects of prior-wave risk perceptions. Arrows pointing up and right indicate statistically significant associations are positive (18 of 29), contradicting H3. Non-significant positive associations occurred for W1–W2 for all behaviors but hand washing (negative non-significant) and home isolation (not in W1). Other positive non-significant associations included mask-wearing (W2-3, W5-6), avoiding travel (W2-3, W5-6), and vaccination (W4-5, W5-6). We omit a table paralleling Table 3: one can directly infer reappraisal effects from figures: e.g., a one-unit change in W2 behavior (0 → 1) will increase W3 perception by .24 units if the path has a regression coefficient of 0.24. The average behavior effect at T_i (e.g., Mask_1) on risk perception at T_{i+1} (e.g., RPNA_2) is slightly larger (Cohen's $f^2 = 0.107$) than the behavioral motivation effect, but still construed as a small effect (Selya et al., 2012). As for temporal changes in behavior-risk perception associations (RQ1), path diagram strength decreased for hand-washing, mask-wearing, vaccination, and isolation, was up-and-down for gatherings, and was stable for avoiding travel. Effects peaked at W3-4 for all behaviors except gatherings and isolation.

4.7. Similarity over behaviors, time, and risk perception measures

We had proposed similar associations between risk perceptions and behaviors across protective actions (H4); asked whether behavior-behavior relations would change temporally in either magnitude or direction (RQ1); and asked whether RPNA-behavior and RPA-behavior associations would differ (RQ2). In addressing these topics we focus only on associations significant at $p < .05$ in Figs. 1–3.

On cross-behavior similarities, for accuracy travel avoidance had many fewer significant associations; hand washing (4:1) and isolation (3:1; this ratio is equivalent to hand washing's, as isolation intentions were elicited in one less wave) were more skewed between negative and positive associations than were other actions (2:1 or 1:1); and these associations' magnitude (within sign) was greater for hand-washing. On behavioral motivation, hand washing- and vaccination had the fewest (2) and mask wearing the most (4) statistically significant associations; all actions had positive associations as hypothesized; and avoiding gatherings and mask-wearing had stronger associations while avoiding travel and vaccination had the weakest. On risk reappraisal, the fewest significant associations occurred for avoiding travel and vaccination; all significant associations were positive, contrary to H3; and the weakest associations were for avoiding travel and vaccination. Avoiding travel and mask-wearing were thus overall the most divergent from each of the other behaviors, although observed differences were moderate.

In addressing RQ1 on temporal patterns in associations of behavioral reports between survey waves, most behavioral associations (17 of 29) explained about 25% of variance cross-wave (i.e., between-wave *r*s = 0.40-0.60), but with variation across behaviors. Isolation, vaccination, and hand-washing exhibited in that order a drop in the association from W1-2 (W2-3 for isolation; about 50% of variance explained) to subsequent waves. By contrast, sharp increases in this association occurred particularly for mask-wearing (effectively zero variance explained in W1-2), but also for avoiding travel (5% variance explained W1-2) and gatherings (9% variance explained W1-2). Lower associations across time for vaccination and hand-washing may reflect the hypothetical nature of a COVID-19 vaccine early in the pandemic (plus perhaps increasing politicization of vaccination hesitancy over time), and the aforementioned possible social desirability bias of hand-washing responses eroded by emerging evidence that viral transmission via surfaces was very low. It is less clear why home isolation would initially evoke a high association. Mask-wearing was discouraged early by U.S. public health officials to avoid anticipated supply-chain gaps for health workers, so this may explain poor association across the first two waves. As for avoiding travel or large public gatherings, we assume that the low incidence of U.S. coronavirus infections at W1 meant few people considered these steps then, whereas by W2 a pandemic had been officially declared.

Finally, observed (statistically significant) associations of risk perceptions and protective behaviors were similar for the two perception measures (RQ2). RPNA had stronger associations on accuracy, but the gap was generally small, skewed towards the few cases where associations were positive, not negative (e.g., gatherings, vaccination, isolation). No obvious differences appeared on behavioral motivations and risk reappraisal (despite the latter's slightly larger ranges for RPNA on gatherings and vaccination).

5. Discussion

5.1. Major findings

A longitudinal panel study of U.S. responses to the COVID-19 pandemic tested hypotheses and measurement issues raised by Brewer et al. (2004). Although we report in the main text results for data that combined behavior and intentions, our Supporting Information shows that focusing on behavior only yielded only minor changes. As summarized in Table 1, the findings strongly supported the behavioral motivation hypothesis, that later protective behavior is associated with earlier high-risk perceptions (H2). The accuracy hypothesis (H1), that at any one time people with lower protective behavior exhibit higher risk perceptions than those who have acted to protect themselves, was mostly supported, although ambiguity in the original hypothesis warrants discussion (below). The risk reappraisal hypothesis (H3), that prior protective action reduces later risk perceptions, was strongly refuted. Despite general similarity of these findings across protective behaviors, some variation occurred in statistical significance, the association's sign, and/or its magnitude, both at particular waves and across time. Avoiding travel and mask-wearing were general outliers, although other actions (e.g., hand-washing; isolation; vaccination) were modest outliers on specific analyses. Our results supported the hypothesis (H5) from a few prior empirical studies that a conditional risk perception measure that excludes the prospect of protective action will yield higher risk perceptions than a conditional measure that accounts explicitly for such action. Analyses regarding RQ1 found associations between the two perception measures attenuated over time, with no-action perceptions more persistent across waves and explaining more variance in perceptions of risk if new protections were adopted than the reverse. Reported intentions or implementation of a given behavior across time (RQ1) were generally high, although low initially for mask-wearing and avoidance of public gatherings; temporal trends included decreases for hand-washing, vaccination, and isolation, and increases for mask-wearing and avoiding large public gatherings. Regarding perception-behavior associations over time (RQ1), accuracy exhibited a mix of up-and-down patterns, behavioral motivation was more dominated by strengthening associations across time, and risk reappraisal exhibited a mix of declining and up-and-down associations over time. Overall, patterns of association between measures in the first and/or second wave of data collection (W1-2 for all behaviors except isolation, elicited first in W2-3) were markedly different from those for later waves. Finally, correlations between protective behavior and risk perceptions were largely similar regardless of the perception measure used, but a bit stronger for no-action perceptions on accuracy and risk reappraisal.

5.2. Implications

There were few differences between the behavior-only and the behavior + intention results reported in the main text, suggesting that it might be useful to continue including both in future studies to determine whether the apparent applicability of the Brewer et al. (2004) hypotheses to intentions as well as actual behavior generalizes to other topics than COVID-19 and to other samples than the U.S. one used here. The behavior-only focus might still be warranted in many studies, but including both will further clarify when intentions and behaviors diverge or converge in their associations with risk perceptions.

Our results add to the scanty literature favoring behavioral motivation (H2): higher risk perception now fosters more protective action later. As this hypothesis is at the core of the risk perception paradox (Wachinger et al., 2013)—why do people *not* always adopt precautions when they see a large threat?—our strong confirmation of this hypothesis across multiple behaviors in a longitudinal design implies that part of the paradox might stem from using cross-sectional designs inappropriate for testing the hypothesis (Brewer et al., 2004). More attention to longitudinal panel designs might reduce the paradox's frequency.

Our results reinforced dominant empirical findings (Supporting Information) favoring the accuracy hypothesis (H1): at a given time, people reporting higher perceived risk also report lower protective action than people with lower risk perceptions. Brewer et al. (2004) noted that positive or null associations were also possible, as when higher risk perceptions persist among those taking protective actions if these actions are not seen as removing risk, consistent with the few cases where we saw positive associations (i.e., higher risk perceptions persisted). Yet further theorizing on why accuracy-hypothesis associations might differ in sign would bolster scholars' ability to properly test the hypothesis.

We found results inconsistent with most empirical studies to date (Supporting Information, Table 1) on risk reappraisal (H3), with action at time T yielding higher perceived risk at time T + 1. The rising burden of COVID-19 cases and deaths, and of SARS-CoV-2 viral variants, plus shifting protective-behavior recommendations, policies, and fellow citizens' behaviors, in the U.S. over the survey's 14 months might have shaped this unexpected response due to continuing uncertainty about whether one's actions were indeed protective. Another speculation is that the risk reappraisal hypothesis might work well for the largely chronic and familiar health conditions covered by extant studies, but not for COVID-19 or other hazards that are novel and/or exhibit highly variable incidence, deserving further study.

Risk perception-behavior associations were largely similar across protective behaviors (H4), with avoiding travel and mask-wearing as outliers. Generalization is unwarranted pending more separate analysis of each behavior, as Johnson (2019a) found regarding Zika protective actions that the common practice in natural hazards research of using a count of the number of actions taken as one's dependent variable obscured sometimes large inter-behavior differences. "Avoiding travel" to infected areas may reflect what Johnson (2019b) called symbolic hazard avoidance: it is easy to deny travel intentions if you never intended going there anyway, and this action exhibited the weakest, and fewest statistically significant, associations in our perception-behavior hypothesis-testing. That mask-wearing exhibits the strongest and most often significant associations of all behaviors might reflect its emergence as the most visible sign of both the pandemic and of politicization of protective COVID-19 behavior in the U.S.

Our longitudinal panel design, with many more waves than most such studies, allowed us to discern temporal variation in perception-behavior associations otherwise unobservable. Differences between W1-2 and later waves might reflect the hypotheticality of these actions or of local infections at this early stage of the U.S. pandemic, underlining the inability of cross-sectional research designs to yield stable inferences, at least for dynamic phenomena. Generalizability of other temporal differences is unclear pending further longitudinal panel research on temporal dynamics in risk responses.

Risk perceptions measured with the no-action conditional item (RPNA) were higher than those measured with the action conditional item (RPA), consistent with H5 and the limited literature, but excluding a few cases RPNA had no stronger or otherwise different associations with behavior (RQ2). Conceptually RPNA measures seem appropriate for most future risk perception-behavior research, to avoid respondents potentially interpreting the question to include behavior. Implications also arise for proposals to standardize measures of personal risk perception (e.g., Walpole and Wilson, 2021), which do not currently account for any protective behaviors the respondent might have in mind in answering proposed questions about exposure, susceptibility, severity, and affective responses. Yet future research using both types, if study goals or survey length allow, will both yield more evidence on their relative efficacy, and allow the comparison to implicitly measure the perceived efficacy of protective behaviors.

A further note on measuring risk perceptions concerns our RPNA item referring to judged risk if no new protective behaviors are enacted. Most conditional risk perception items (see Dibonaventura, 2007, and Kim et al., 2007; for exceptions) name a specific behavior (e.g., "if you don't stop smoking" to assess perceived lung cancer risk), but to a large extent these items arise almost by default, because for most of the health conditions studied there is a dominant precipitating factor. There was no such dominance for COVID-19, when even for the ultimately-determined most important transmission route—airborne aerosols containing infectious viral particles—there were multiple actions that people could take to reduce their potential exposure (e.g., self-isolation, masking, physical distancing, avoiding large public gatherings, increasing ventilation), and other transmission routes that were less dominant were still potentially infectious, and requiring yet other actions (e.g., hand washing). A behavior-specific approach for infectious diseases would be useful to probe in later research, but was infeasible here given the large number of risk perception measures (eight others besides the two listed here), protective behaviors, and non-Brewer-related items in our survey instrument. A further question has to do with the lack of grounding provided to respondents by our unspecific term referring to "new" behaviors, meaning that different people might have something completely different in mind in answering the question. The resulting statistical noise should have attenuated the associations we observed. That we were able to confirm strongly the accuracy and behavioral motivation hypotheses, and that the disconfirmation of the risk reappraisal hypothesis also was strong, suggests to us that any diversity in our sample in the interpretation of "new" made our findings more robust, not less so. That said, ideally more specification would occur in future testing of Brewer et al. (2004) hypotheses. Finally, we emphasize again that we used only cognitive risk perception measures here to be consistent with the Brewer et al. (2004) analyses, but that exploring perception-behavior/intention associations using affective and other risk perception measures may be useful extensions.

A single study cannot be definitive on practical implications, but we suggest that cross-sectional studies' inability to identify causal effects undermines their practical import. We found small effect sizes, so caution is further warranted about speculating on practical implications. However, spread over a large population like that of the U.S., or accumulated over time, even small effects might have strong substantive impacts. We must emphasize the contingency of such speculation: as Oswald et al. (2015, p. 565) said on another topic,

> Small standardized effect sizes can have trivial, moderate or major. Consequences, and the same can be said for large effect sizes. However, the fact that small effects can be consequential in principle does not mean that the small effects observed… are necessarily consequential.

With this caution, our behavioral motivation findings imply that public health agency officials should seek to achieve adequate risk perceptions to increase protective behavior among target populations, despite resistance (e.g., from politicization of COVID-19 risk perceptions [e.g., Calvillo et al., 2020], or low behavioral compliance, such as from younger adults at lower, but not zero, objective COVID-19 risk [e.g., Gadarian et al., 2020]), with the caveat long-understood that also emphasizing the efficacy of the recommended behavior, or the inefficacy of the non-recommended behavior, may be critical for converting potential into actual protective behavior (e.g., Maloney et al., 2011; Witte, 1992a). Our unexpected findings for COVID-19 on the risk reappraisal hypothesis might indicate that the positive association between protective behavior now and greater risk perception later can amplify behavioral motivation effects, because that greater risk perception could lead to even more protective behavior even later, if response efficacy is also emphasized. However, as we stressed earlier, this positive association is so far unique to the COVID-19 case, and our speculation that it might stem either from the dynamics of the U.S. experience of the pandemic or from peculiarities of infectious diseases versus more familiar and stable diseases or health conditions should be tested before considering generalizing this finding into practical applications.

5.3. Limitations

Our opportunity sample of Americans precludes generalization of descriptive statistics to U.S. adults overall, and generalization of model results to non-U.S. populations. Moreover, our skewed sample in age distribution might raise concerns about the risk perception-behavior associations of younger respondents being hard to generalize because elders are on average more at risk from COVID-19. However, research suggests a weak age effect on disease severity when controlling for important age-related risk factors (e.g., diabetes, coronary heart disease/cerebrovascular disease, etc.; Starke et al., 2020). Furthermore, representative samples are not characteristic of perception-behavior studies to date, and we controlled for demographics (i.e., gender, age, race/ethnicity, income, education, and political ideology) to offset our skewed sample, so our modeling of longitudinal perception-behavior associations is defensible.

In the six prior studies using at least one conditional risk perception measure to test at least one Brewer et al. (2004) hypothesis (Supporting Information, Table 1), the risk perception measure usually specified a protective behavior (e.g., how much risk would you have if you stopped alcohol drinking, or continued smoking, or did not participate in regular physical activity?). Given our number of protective behaviors, our risk perception measures were not behavior-specific (e.g., "How much risk does the coronavirus pose to you or your family, *if you or your family don't isolate yourself at home*"). This phrasing might attenuate associations between risk perception and behavior, so testing such alternative phrasings against our more general language should be addressed in future research.

6. Conclusions

Brewer et al. (2004) theses about risk perception measures, and perception-behavior associations, have so far had small effect on health behavior research designs. Given the centrality of both risk perceptions and risky versus protective behavior to the field, this oversight is puzzling but correctable. Not only may proper (longitudinal) survey design substitute valid tests of behavioral motivation for the dominant cross-sectional design, but our rejection of risk reappraisal for COVID-19 suggests that the assumed negative association between protective behavior now and risk perceptions later may be an artifact of the hitherto narrow focus of empirical research on chronic, familiar health conditions. Brewer et al. (2004) theses about measuring risk perception could have equally productive outcomes for what appears to be a burgeoning interest in improving practice in this area (e.g., Walpole and Wilson, 2021).

We hope, therefore, that our colleagues will take this opportunity to adopt conditional risk perception measures and longitudinal panel (plus experimental) research designs to further advance causal understanding of relations between risk perceptions and behaviors, and their temporal variation.

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Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The revision adds data and scripts limited to the specific analyses reported in this manuscript as Supporting Information

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2023.115867.

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