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Review Article

A systematic review and meta-analysis of longitudinal cohort studies comparing mental health before versus during the COVID-19 pandemic in 2020

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ABSTRACT

Background: Increases in mental health problems have been observed during the COVID-19 pandemic. The objectives were to examine the extent to which mental health symptoms changed during the pandemic in 2020, whether changes were persistent or short lived, and if changes were symptom specific.
Methods: Systematic review and meta-analysis of longitudinal cohort studies examining changes in mental health among the same group of participants before vs. during the pandemic in 2020.
Results: Sixty-five studies were included. Compared to pre-pandemic outbreak, there was an overall increase in mental health symptoms observed during March-April 2020 (SMC = .102 [95% CI: .026 to .192]) that significantly declined over time and became non-significant (May-July SMC = .067 [95% CI: -.022 to .157]. Compared to measures of anxiety (SMC = 0.13, $p = 0.02$) and general mental health (SMC = -.03, $p = 0.65$), increases in depression and mood disorder symptoms tended to be larger and remained significantly elevated in May-July [0.20, 95% CI: .099 to .302]. In primary analyses increases were most pronounced among samples with physical health conditions and there was no evidence of any change in symptoms among samples with a pre-existing mental health condition.
Limitations: There was a high degree of unexplained heterogeneity observed (I^2s > 90%), indicating that change in mental health was highly variable across samples.
Conclusions: There was a small increase in mental health symptoms soon after the outbreak of the COVID-19 pandemic that decreased and was comparable to pre-pandemic levels by mid-2020 among most population sub-groups and symptom types.

1. Introduction

As of February 2020, the COVID-19 pandemic has been responsible for more than 2.5 million deaths worldwide (World Health Organisation, 2021). Soon after the COVID-19 crisis was declared a pandemic by the World Health Organisation (WHO) on the 11th March, concerns were raised over a potential parallel mental health crisis fuelled by the pandemic and the associated social restrictions imposed by governments to reduce virus transmission (Holmes et al., 2020; Pfefferbaum and North, 2020). The mental health of 'at risk' groups during the pandemic, such as those with pre-existing mental health conditions, has also been highlighted as cause for concern (Pfefferbaum and North, 2020; Yao et al., 2020).

There are considerable methodological challenges to quantifying the impact that the pandemic has had on mental health. During the outbreak of the pandemic studies indicated that many participants perceived their mental health had worsened (Robinson et al., 2021; Wang et al., 2020), with studies of both UK and Chinese adults indicating more frequent self-reported feelings of depression and anxiety (Robinson et al., 2021; Wang et al., 2020). Yet, retrospective reports of change in mental health are prone to substantial bias (Van den Bergh and Walentynowicz, 2016; Ben-Zeev et al., 2009). Other studies have found a greater incidence of mental health problems in cohorts recruited during the pandemic compared to different cohorts who completed measures prior to the pandemic (McGinty et al., 2020; Daly et al., 2021). For example, in US adults both distress (McGinty et al., 2020) and depression (Daly et al.,

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2021) were elevated in samples collected during the early stages of the pandemic compared to samples collected prior to the outbreak of the pandemic. However, differences in how samples were recruited (e.g. greater reliance of online and non-probability based samples during the pandemic) and differences in demographic profiles of pre vs. post-pandemic outbreak cohorts also make inferring change in mental health attributable to the pandemic difficult (Pierce et al., 2020).

A number of longitudinal cohort studies have sampled the same participants before and during the pandemic to examine how mental health has changed. In a large nationally representative sample of UK adults, Daly et al. found that non-specific general mental health symptoms increased in April-June 2020 compared to a pre-pandemic baseline (Daly et al., 2020). Other longitudinal cohort studies have found little change in mental health (van der Velden et al., 2020) or mixed evidence (Shan Wong et al., 2020). Longitudinal studies are uniquely placed to characterise the time course of the mental health burden associated with the COVID-19 pandemic. Although the effects of the pandemic on mental health could be long lasting (Antonis, 2020), a recent multi-wave longitudinal study of US adults found that after an initial increase in distress during the early stages of the pandemic, distress reduced to pre-pandemic levels within a few months (Daly and Robinson, 2020).

There has been no published systematic review and meta-analysis focusing primarily on longitudinal cohort studies with pre vs. during pandemic measurement of mental health among the same population (Prati and Mancini, 2021; Arora et al., 2020; Xiong et al., 2020). Therefore, we systematically reviewed and meta-analyzed studies that examined longitudinal changes in mental health among the same sample of participants before vs. during the pandemic in 2020, in order to quantify the size of the mental health burden associated with the pandemic and whether this was persistent or short-lived. Secondary aims were to examine whether changes have been symptom specific and whether changes differed based on population demographics.

2. Methods

2.1. Eligibility criteria

Participants. To be eligible, studies were required to have sampled the same cohort of participants prior to 11/03/20 (date the WHO declared a pandemic) (Cucinotta and Vanelli, 2020) and at least once after this date. Chinese studies were eligible (but analysed separately) if mental health was assessed prior to and after 23/01/20 because substantial social restriction measures were enforced across China from this point (Yuan et al., 2020). There were no limits on populations sampled and both non-clinical and clinical populations were eligible for inclusion.

Measures of interest. To be eligible, studies were required to have collected data using a validated multi-item measure of mental health symptoms or mental well-being, such as depression (e.g. Patient Health Questionnaire: PHQ9), anxiety (Depression, Anxiety, Stress Scale: DASS), non-specific general mental health related functioning and distress (General Health Questionnaire: GHQ12, Kessler) and well-being (Warwick-Edinburgh Mental Wellbeing Scale). As our focus was on mental health symptoms, ineligible measures included loneliness, stress and physical health related quality of life.

Outcome. Changes in mental health symptoms. Studies that examined continuous changes (i.e. standardised mean change; SMC) in mental health symptoms (e.g. depression, anxiety) were eligible. Studies that examined change to the % of the sample meeting questionnaire specific cut-offs for clinically relevant/likely serious mental health problems were eligible (i.e. Odds Ratio).

Study design features. Studies were required to sample the same participants using the same measure of mental health pre and post-pandemic (repeated cross-sectional studies were not eligible). If only a sub-sample of participants were followed up across survey waves, only data from the sub-sample were eligible. Studies reporting on interventions designed to improve mental health during the pandemic were not eligible. If multiple articles reported on data from the same cohort of participants, the article with the largest number of post-pandemic follow-up data collection points was included. Journal articles and pre-prints were eligible. We deemed the inclusion of pre-prints important as searching for unpublished studies is one way of reducing potential publication bias and to produce timely findings on the impact of COVID-19 on mental health, it was common for eligible studies to be available online as pre-prints.

2.2. Article identification

We searched Pubmed, SCOPUS, Web of Science and PsychInfo from January 2020 to January 11, 2021, using combinations of coronavirus and mental health relevant search terms (see online supplementary materials). One author conducted title and abstract screening and 25% were cross-checked by a second author (no discrepancies). Two authors conducted full-text screening and disagreements in eligibility were resolved by discussion. We searched three databases for unpublished pre-prints; Open Science Framework (inclusive of 30 preprint archives, e.g. PsychArxiv), MedrXiv and the Social Science Research Network, and conducted forward citation tracking (Google Scholar) for all eligible articles. A single author conducted pre-print searches and forward citation tracking; a second author verified eligibility of identified articles.

2.3. Data extraction

The following information was extracted by a single author and checked for accuracy by a second author; country of study, participant group sampled, age group of sample, sampling strategy used (e.g. use of representative sampling vs. convenience), pre and post pandemic dates of data collection, mental health measure, analytic treatment of mental health symptomology change (e.g. use of change in % meeting questionnaire cut-off vs. continuous change in questionnaire score), level of attrition (%), effect size information (authors were contacted if information was missing) and whether the study was reported in a journal article or pre-print. Formal data extraction was conducted from 28/01/21.

2.4. Risk of bias

We reviewed widely used methodological quality scales and risk of bias measures (e.g. Newcastle Ottawa Scale) to develop a list of bias indicators relevant to included studies. See online supplementary materials for full details of risk of bias tools considered and explanations for each risk of bias indicator included in the present study. Indicators were rated by two authors; (i) whether the study reported representative sampling (yes = lower in risk of bias), (ii) whether the study underwent peer review (yes = lower in risk of bias), (iii) relatively low level of attrition (25%) to minimize bias on study results (yes = lower in risk of bias), (iv) whether the study had a relatively large ($N \geq 1000$) sample size (yes = lower in risk of bias), (v) whether the pre-pandemic measure of mental health was collected within the last 12 months of the post-pandemic outbreak measure (yes = lower in risk of bias), (vi), whether survey delivery mode (e.g. online) was consistent across pre and post outbreak waves of data collection (yes = lower in risk of bias), (vii) whether conflicts of interest were reported (no conflicts = lower in risk of bias).

2.5. National COVID-19 data

To examine if heterogeneity of change in mental health was attributable to country level factors, for each eligible study, we identified the number of recorded COVID cases and deaths (by country) per million population for the month that mental health was measured during the

pandemic. We also used data from the Oxford COVID-19 Government Response Tracker to characterize each country's severity of social restrictions, number of health measures and level of economic support in place during the month post-pandemic outbreak mental health was assessed. See supplementary materials for full information.

2.6. Main planned analyses

We examined studies from China separately to studies from other countries because of the earlier timeline of pandemic outbreak and social restrictions in China. For our main analysis on continuous data, we computed standardised mean change (SMC) in pooled SD units (SMC was more appropriate than standardized mean difference because we were examining change among the same population) to account for studies using different questionnaire types and therefore having different scoring scales. For analyses examining questionnaire cut-off data we computed Marginal Odds Ratios to quantify size of change. See online supplementary materials for full statistical information. As studies contributed multiple comparisons to analyses, we conducted a multi-level meta-analysis. Random-effects models were used to attempt to generalize findings beyond the included studies (Cheung et al., 2012). Heterogeneity was assessed using I^2 statistic. We first examined a model that included all effects (across different symptom types) as in subsequent sub-group analyses this allowed us to examine if heterogeneity in effect sizes was explained by symptom type being measured. We also examined whether time (month post-pandemic outbreak measure was collected) predicted change in mental health symptoms using meta-regression. We conducted sub-group analyses on symptom type (depression, anxiety, general mental health functioning (including distress), well-being, psychotic symptoms, other), gender, age (adult vs. child/adolescent), population sampled (general population, university students, mental and physical health conditions) and continent sample was from. For sub-group analyses we compared effects across sample categories (i.e. effects for female samples only vs. male samples only). A minimum of n=5 effects for each sub-group was required for analysis as we anticipated that there would be heterogeneity across samples and any fewer studies may produce unreliable effect size estimates. All analyses were conducted in R. To address influential cases, we examined evidence for outliers, conducted leave one-out-analyses and computed DFBETAS ('difference in beta values'; each parameter estimate with and without influential cases) values for each effect size in the full models (without moderators). Funnel plots were inspected for potential publication bias, Egger's test of asymmetry and a Trim and Fill procedure were used. See online supplementary materials for full details. For SMC, an effect size of 0.2 is typically considered a statistically 'small' effect (Cheung et al., 2012). For Marginal Odds Ratios an effect size of 1.68 is typically considered a statistically 'small' effect (Palmas et al., 2020). Pooled effects with confidence intervals that did not cross zero were considered statistically significant.

2.7. Registration

The study was registered on PROSPERO (CRD42021231256) and we adhered to PRISMA guidelines when conducting this systematic review.

3. Results

3.1. Article identification

After removal of duplicates, title and abstract screening of electronic database search results and identification of eligible articles through other sources, 153 articles were full-text screened. A total of 65 articles were included, with only one eligible study being excluded due to inadequate statistical information and authors not responding to requests for data. See Fig. 1 for study selection flow chart.

3.2. Overview of studies and eligible effects

The 65 articles reported on a total of 201 pre vs. during pandemic mental health comparisons to include in the meta-analyses. The majority of studies sampled European ($N = 31$) or North American ($N = 16$) populations. The majority of comparisons ($n = 177$) examined pre vs. post pandemic outbreak change in overall mental health as a continuous variable (i.e. change in mean questionnaire score) and $n = 24$ examined change in % of sample meeting questionnaire cut-offs for clinically relevant symptomology. See Table S1 in the supplementary materials for individual study information.

3.3. Overall change in mental health symptoms (SMC)

For SMC effects there were 177 pre vs. post pandemic comparisons across 61 studies. Twelve of these comparisons (4 studies) came from Chinese samples, which left a total of 165 comparisons included in the main analysis. Sample sizes of comparisons ranged from $n = 9$ to 11,599. Depression and mood disorder symptoms ($n = 58$), anxiety disorder symptoms ($n = 52$) and general non-specific mental health symptoms ($n = 35$) were the most common symptoms studied. From the 165 comparisons drawn from ~55,015 participants, overall change in mental health symptoms from pre-post pandemic outbreak was significant (SMC = .106 [95% CI: .039 to .172], $z = 3.12$, $p = .002$, $I^2 = 97.8$) and indicative of a heterogeneous and statistically small increase in symptoms. There were no influential cases. There was some evidence of funnel plot asymmetry (see Fig. S1) as indicated by Egger's test being significant, although the Trim and Fill procedure did not impute any studies (see online supplementary materials for influential case and publication bias results).

For studies in China (~ 1,854 participants), change in mental health symptoms was indicative of a small, non-significant increase in symptoms (SMC = .194 [95% CI: -.576 to .964], $z = 0.49$, $p = .622$, $I^2 = 99.5\%$].

3.4. Symptom-level meta-analyses

We examined whether SMC in mental health symptoms was moderated by symptom type. The test of moderation was significant (QM(5) = 19.71, $p < .001$, $I^2 = 97.5\%$) and therefore we conducted separate analysis for each symptom type. There was a significant increase in symptoms of anxiety (SMC = .125 [95% CI: .019 to .230], $z = 2.31$, $p = .021$, $I^2 = 96.2\%$) and depression (SMC = .216 [95% CI: .135 to .296], $z = 5.24$, $p < .001$, $I^2 = 95.0$), with the increase in depression larger than anxiety ($p = .049$). There was a significant decrease in symptoms of psychosis (SMC = -.211 [95% CI: -.376 to -.046], $z = 2.51$, $p = .012$, $I^2 = 73.1\%$).[1] There was no significant change in measures of general mental health (SMD = -.030 [95%CI -.158 to .098], $z = 0.457$, $p = .648$, $I^2 = 98.8\%$), well-being (SMC = .067 [95% CI: -.123 to .256], $z = .690$, $p = .490$, $I^2 = 90.5\%$) or for the mixed 'other' conditions subgroup (SMC = -.041 [95% CI: -.203 to .121], $z = 0.501$, $p = .616$, $I^2 = 89.9\%$). See online supplementary materials Figs. S2–S7 for symptom level forest plots. Because change in symptoms was dependent on symptom type we repeated influential case and publication bias for the depression and mood disorder symptoms meta-analysis and anxiety disorder symptoms meta-analysis separately (as these meta-analyses each had a sufficient number of contributing effects). In line with the primary analyses there was no evidence of influential cases or publication bias (fig. S1 for funnel plot). See online supplementary materials for

[1] Multi-level models remained a better fit of the data separately for anxiety, depression and general mental health measures, but not for psychosis, well-being or other symptoms which is likely due to smaller number of effect sizes. Regardless, treating the data as single or multilevel in these cases did not substantially influence the effect sizes or statistical significance of the models.

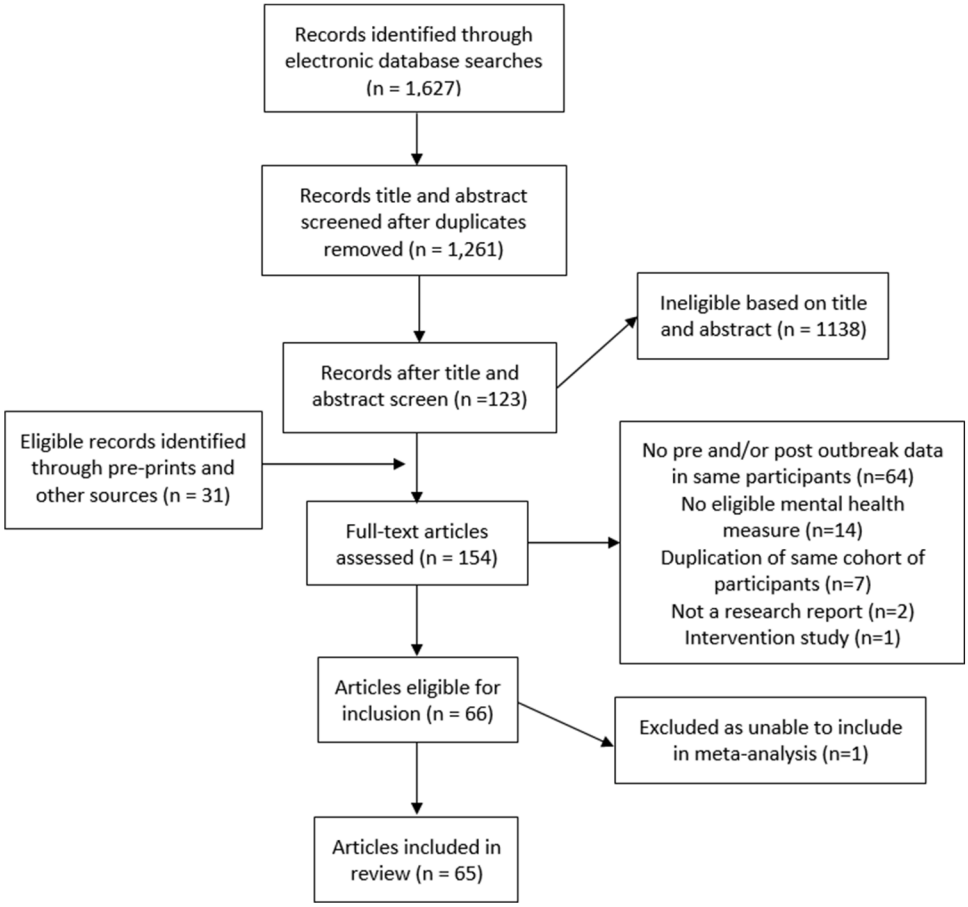

Fig. 1.. Study search and inclusion flow chart
'Not a research report' indicates that a manuscript was ineligible as it did not report on an empirical study. Excluded as unable to include in meta-analysis ($N = 1$) was due to authors not providing the required missing statistical information to compute effect size after having been contacted.

results.

3.5. Time analyses

Change in symptoms from pre-pandemic levels became smaller over each month (monthly change coefficient = -.057 [95%CI: -.100 to -.013], $z = 2.57, p = .010$). To illustrate, among post-pandemic measures of mental health collected in March and April ($n = 98$) the change in mental health was statistically small and significant; SMC = .102 [95% CI: .026 to .192] $z = 2.22$, $p = .026$, I2 = 97.9%). Conversely, for measures collected during May-July ($n = 67$) there was no significant change compared to pre-pandemic mental health symptoms; SMC = .067 [95% CI: -.022 to .157], $z = 1.47, p = .141$, I2 = 97.5%). There was no robust interaction between symptom types and time. See Table 1 for SMCs for each symptom type by time.

3.6. Sub-group analyses

In our primary sub-group analyses (SMC) we found no evidence that change in mental health symptoms differed based on age, gender, or study continent. See Table 2 for number of effects in each sub-group and full results of sub-groups analyses. Change in symptoms tended to be larger among participants with a pre-existing physical health condition compared to the general population. Notably, change in mental health symptoms was non-significant in samples with pre-existing mental health conditions. No country-level data (number of COVID cases/deaths, stringency of government measures or level) explained heterogeneity between samples (ps > .05). See online supplementary materials for country level analysis full results.

3.7. Risk of bias

Due to the small number of studies examining changes in % of sample meeting questionnaire cut-offs, risk of bias analyses were limited to SMC studies in our primary analysis. For SMC studies, the majority of included effects were from published journal articles ($n = 122$), although a substantial minority were from unpublished pre-prints ($n = 43$). Most studies had smaller sample sizes ($N < 1000$), relatively high attrition (> 25%) and collected baseline pre-pandemic data on mental health in the twelve months prior to pandemic outbreak. See Table 2 (column 3) for risk of bias summary information and Table S2 for study-level information. We found limited evidence that the risk of bias indicators predicted size of change in symptoms, except that change in mental health tended to be larger when delivery mode of questionnaire was consistent pre vs. post pandemic. See Table 2 for results of risk of bias analyses in full. In an exploratory analysis limited to a more homogenous collection of studies that were of lower risk of bias results were consistent with the main analyses. See Fig. 2.

3.8. Sub-group and risk of bias analyses for depression and anxiety effects separately

Because change in symptoms was dependent on symptom type we repeated sub-group and risk of bis analyses for the depression and mood disorder symptoms meta-analysis and anxiety disorder symptoms meta-analysis separately. Results were consistent with primary analyses and

Table 1
Standardized mean change in symptoms when mental health was measured earlier vs. later in the pandemic.

Symptom	March-April effect estimate	May – July effect estimate	Symptoms by month time trend analysis
Anxiety disorder symptoms (n = 52)	$n = 29$, SMC = .140 [95% CI: -.024 to .303, I^2 = 97.4%]	$n = 23$, SMC = .051 [95% CI: -.037 to .139, I^2 = 87.9%]	Coefficient = -.067 [95% CI: -.124 to -.009]
Depression and mood disorder symptoms (n = 58)	$n = 32$, SMC = .226 [95% CI: .109 to .343, I^2 = 95.9%]	$n = 26$, SMC = .201 [95% CI: .099 to .302, I^2 = 93.8%]	Coefficient = -.057 [95% CI: -.119 to .005]
General (non-specific) mental health (n = 35)	$n = 20$, SMC = -.013 [95% CI: -.176 to .150, I^2 = 98.5%]	$n = 15$, SMC = -.098 [95% CI: -.292 to .095, I^2 = 99.3%]	Coefficient = -.071 [95% CI: -.119 to .-.022]
Psychotic symptoms (n = 5)	n = 5, SMC = -.211 [95%CI: -.376 to -.046, I^2 = 73.1%]	-	-
Well-being ($n = 7$)	$n = 6$, SMC = .053 [95% CI: -.182 to .288, I^2 = 92.8%]	-	-
Other disorder specific symptoms (n = 8)	$n = 6$, SMC = -.105 [95 CI: -.282 to .073, I^2 = 84.9%]	$n = 2$, SMC = .130 [95% CI: -.180 to .440, I^2 = 88.6%]	Coefficient = .105 [95% CI: -.019 to .229]

n = equals number of comparisons. Cells with - are indicative of insufficient effect sizes for individual analyses. Depression and mood disorder symptoms included depressive symptoms and emotional problems. General (non-specific) mental health measures included mental health related-quality of life, distress, internalizing symptoms. Psychotic symptoms included measures of psychoticism and paranoia. Well-being included measures of overall psychological well-being. Other disorder specific symptoms included eating disorder symptomology, post-traumatic stress disorder symptoms, suicidal ideation. For all measures included by symptom type see Table S1.

are reported in the online supplementary materials (Tables S3 and S4); change in mental health was significant among general population samples and tended to be smaller and non-significant among samples with an existing mental health condition. Although effect estimates tended to be larger for samples with a pre-existing physical health condition compared to other population groups, the number of contributing effects was small ($n = 6$ for depression, $n = 7$ for anxiety) and estimate confidence intervals crossed zero. We found no evidence that any of the risk of bias indicators predicted size of change in symptoms for depression or anxiety symptoms (Tables S3 and S4).

3.9. Change in numbers exceeding questionnaire cut-offs for mental health problems

For change in cut-off effects there were 24 comparisons across 12 studies (~21,825 participants) included. There was a significant effect (single level meta-analysis), with increased odds of exceeding a questionnaire cut-off for mental health problems from pre-post pandemic (Marginal Odds Ratio = 1.31 [95% CI: 1.10 to 1.55], $z = 3.18$, $p = .001$, $I^2 = 93.2\%$). See Fig. 3. There was no evidence of publication bias or influential cases. See online supplementary materials for full results.

4. Discussion

We reviewed sixty-five longitudinal cohort studies comparing mental health prior to and during the COVID-19 pandemic. In our primary meta-analysis (61 studies) of predominantly European and North American studies, there was a statistically small overall increase in mental health symptoms. The increase in mental health symptoms was largest among studies that sampled participants in the early stages of the pandemic (March-April) and severity of mental health symptoms

Table 2
Sub-group analyses for standardized mean change (SMC) in mental health symptoms.

Moderator	Test of sub-group difference	Sub-groups	Effect estimate
Age	QM(1) = 0.001, $p = .978$, I^2 = 97.9%	Adults ($n = 127$)	SMC = .105 [95% CI: .033 to .178], $z = 2.84$, $p = .005$, I^2 = 98.0%
		Child/adolescents ($n = 38$)	SMC = .114 [95% CI: -.030 to .257], $z = 1.56$, p = .120, I^2 = 94.6%%
Gender	QM(1) = 2.73, $p = .098$, I^2 = 93.8%	Males ($n = 13$)	SMC = .086 [95% CI: -.030 to .202], $z = .145$, $p = .147$, I^2 = 89.9%
		Females ($n = 14$)	SMC = .150 [95% CI: .005 to .295], $z = 2.02$, $p = .043$, I^2 = 95.0%
Continent	QM(3) = 0.916, $p = .822$, I^2 = 97.9%	Europe ($n = 100$)	SMC = .093 [95% CI: .002 to .185], $z = 2.00$, $p = .046$, I^2 = 97.8%
		North America (n = 45)	SMC = .132 [95% CI: -.012 to .277], $z = 1.79$, $p = .073$, I^2 = 98.5%
		Other ($n = 15$)	SMC = .160 [95% CI: .085 to .234], $z = 4.20$, p < .001, I^2 = 87.3%
		Mix ($n = 5$)	SMC = .052 [95% CI: -.081 to .184], $z = 0.76$, $p = .446$, I^2 = 72.1%
Population	QM(3) = 8.735, $p = .033$, I^2 = 97.8%	General population ($n = 75$)	SMC = .118 [95% CI: .042 to .193], $z = 3.04$, $p = .002$, I^2 = 97.9%
		University student ($n = 40$)	SMC = .133 [95% CI: -.005 to .272], $z = 1.87$, $p = p$ = .059, I^2 = 95.2%
		Pre-existing mental health condition (n = 25)	SMC = -.017 [95% CI: -.211 to .178], $z = 0.17$, = .867, I^2 = 97.7%
		Pre-existing physical health condition (n = 14)	SMC = .249 [95% CI: .067 to .431], $z = 2.68$, $p = .007$, I^2 = 97.9%
Publication status	QM(1) = 0.053, $p = .818$, I^2 = 97.9%	Journal article (n = 122)	SMC = .101 [95% CI: .021 to .181], $z = 2.48$, $p = .013$, I^2 = 98.1%
		Pre-print ($n = n$ = 43)	SMC = .121 [95% CI: .001 to .241], $z = 1.97$, $p = .049$, I^2 = 96.5%
Representative sampling	QM(1) = 0.114, $p = .735$, I^2 = 97.9%	Reported ($n = 34$)	SMC = .125 [95% CI: .043 to .206], $z = 3.01$, $p = p$ = .003, I^2 = 98.1%
		Not reported or unclear ($n = 131$)	SMC = .101 [95% CI: .020 to .181], $z = 2.43$, $p = .015$, I^2 = 96.2%
Sample size	QM(1) = 0.047, $p = .829$, I^2 = 97.9%	≥1000 ($n = n = 24$)	SMC = .108 [95% CI: .013 to .203], $z = 2.32$, p = .013, I^2 = 98.9%
		<1000 ($n = 141$)	SMC = .108 [95% CI: .031 to .184], z

(continued on next page)

Table 2 (*continued*)

Moderator	Test of sub-group difference	Sub-groups	Effect estimate
			= 2.77, p = .006, I^2 = 94.6%
Pre-pandemic measure[a]	QM(1) = 0.167, p = .683, I^2 = 97.9%	More than 12 months prior to pandemic measure (n = 42)	SMC = .092 [95% CI: -.008 to .193], z = 1.80, p = .072, I^2 = 98.3%
		12 months or less prior to pandemic measure (n = 119)	SMC = .117 [95% CI: .032 to .203], z = 2.70, p = .007, I^2 = 97.3%
Mode of survey	QM(1) = 5.056, p = .025, I^2 = 97.9%	Consistent between pre and during pandemic (n = 82)	SMC = .179 [95% CI: .098 to .260], z = 4.31, p < .001, I^2 = 98.1%
		Inconsistent or unclear (n = 83)	SMC = .034 [95% CI: -.064 to .132], z = 0.685, p = .493, I^2 = 95.9%
Conflicts of interest	QM(1) = 0.111, p = .739, I^2 = 97.9%	Statement included + no reported conflicts (n = 124)	SMC = .112 [95% CI: .041 to .184], z = 3.07, p =.001, I^2 = 97.6%
		No conflict statement or reported conflicts (n = 41)	SMC = .089 [95% CI: -.065 to .243], z = 1.32, p = .257, I^2 = 98.1%
Attrition[b]	QM(1) = 0.045, p = p = .832, I^2 = 97.8%	Level of attrition ≤25% (n = 47)	SMC = .038 [95% CI: -.055 to .131], z = 0.80, p = .424, I^2 = 98.5%
		Level of attrition > 25% (n = 66)	SMC = .103 [95% CI: -.002 to .208], z = .1.93, p = .054, I^2 = 95.4%

[a] Number of comparisons does not add up to n = 165 because for a small number of effects it was unclear whether the pre-pandemic measures was collected more than 12 months prior to the post-pandemic outbreak measure.

[b] We also examined whether attrition was associated with the change in mental health symptoms pre-post pandemic using meta-regression. There was no significant association (coefficient = .092 [95% CI: -.185 to .370] z = .653, p = .514).

decreased significantly over the following months (May-July). This pattern of results may represent an acute and normal response to an unforeseen and distressing traumatic event (Palmas et al., 2020), which was then followed by a period of psychological adaptation and resilience (Daly and Robinson, 2020; Infurna and Luthar, 2018). In line with this interpretation, a large study of US adults found that perceived risk and worries about financial instability, infection and death were highest during the early stages of the pandemic (Robinson and Daly, 2021). However, as more information about the pandemic became available perceived risks decreased and this predicted recovery to pre-pandemic levels of distress (Robinson and Daly, 2021). Similarly, in a large sample of UK adults recruited after the pandemic outbreak, both anxiety and depressive symptoms showed a trajectory of recovery from the beginning of April onwards (Fancourt et al., 2020).

There was a large degree of heterogeneity observed for change in mental health symptoms and sub-groups analyses indicated that symptom type in part explained this variability. Worsening of mental health symptoms was largest in studies examining depression and mood disorders symptoms, and a small increased level of depression symptoms was still observed during May-June. Change in anxiety symptoms was smaller and was not significant in samples measured in May-June. Changes in non-specific general mental health functioning (including distress) and well-being were small and non-significant. The more pronounced change in depressive symptoms observed may reflect the effects of isolation and sadness caused by social restrictions and loss of life during the pandemic (Heinrich and Gullone, 2006). Increases in mental health problems were observed across most population sub-groups sampled (e.g. general population, university students, existing physical health condition), which was the case for our primary meta-analysis that grouped all symptom types and in individual meta-analyses limited to depression and anxiety symptoms separately. However, it is important to note that in some sub-group analyses the number of effects were small and we were therefore not well powered to detect relatively subtle subgroup differences.

Contrary to concerns raised early in the pandemic (Pfefferbaum and North, 2020; Yao et al., 2020), changes in mental health were less pronounced among people with pre-existing mental health conditions and overall there was no statistically significant change in mental symptoms in this group. These findings may in part be explained by

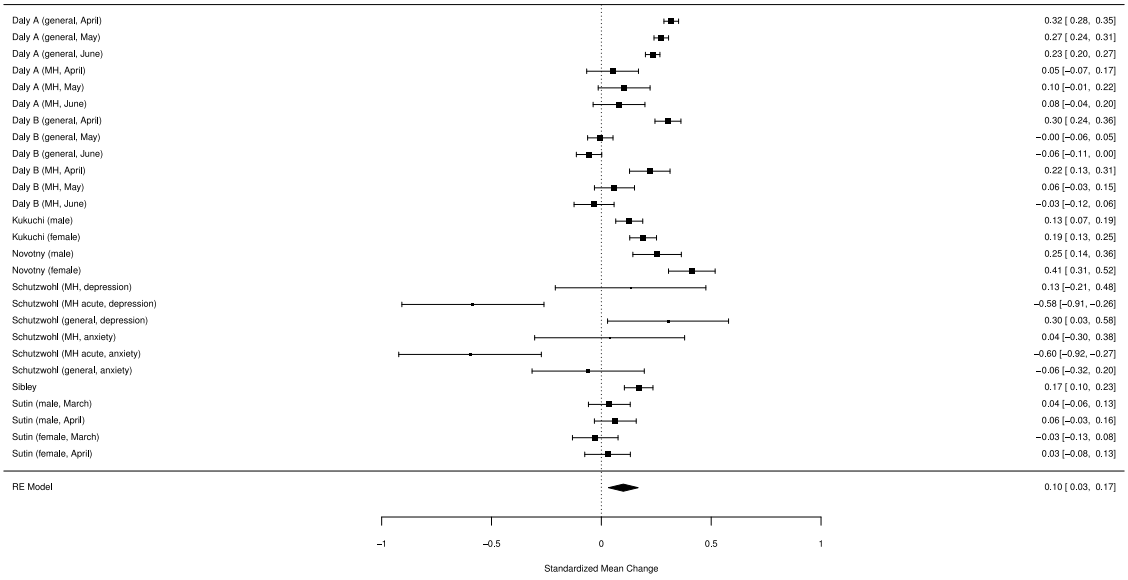

Fig. 2.. Forest plot of effect sizes from studies of depression, general mental health and anxiety symptoms with lower risk of bias.
Analyses of more homogenous collection of studies (depression, anxiety, general mental health measures only) that were of lower risk of bias (used representative sampling, did not report inconsistent mode of survey delivery, reported no conflicts of interest) of ~27,736 participants, SMC = .100 [95% CI: .033 to .166], z = 2.95, p = .003, I^2 = 98.0%). General (general population sample), MH (sample with pre-existing mental health condition).

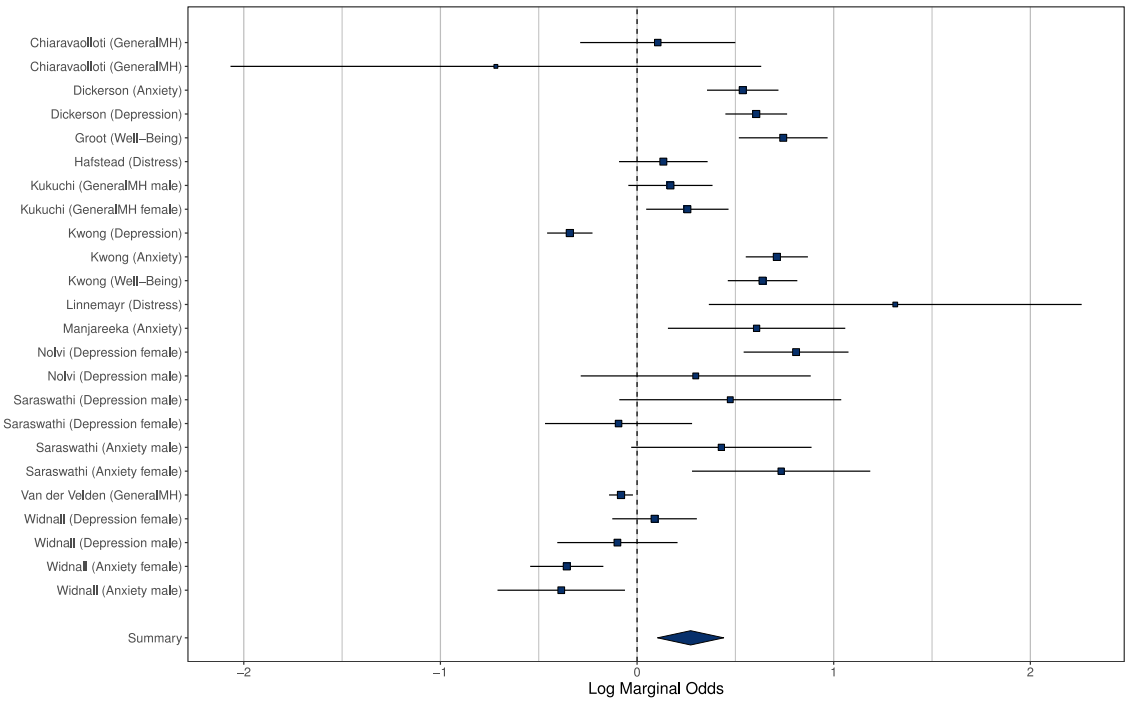

Fig. 3.. Forest plot of effect sizes for exceeding questionnaire cut offs.
General MH (measure of general mental health functioning). Marginal Odds Ratio = 1.31 [95% CI: 1.10 to 1.55], $z = 3.18$, $p = .001$, $I^2 = 93.2\%$).

regression to the mean and naturally occurring recovery of more severe mental health symptoms over time (Streiner, 2001), as well as stay at home restrictions that potentially provided a more structured routine and reduced exposure to external stressors (e.g. large social gatherings) among those with severe mental health conditions (Cheung et al., 2012). A number of countries, such as China, introduced new mental health services in response to the pandemic (Yao et al., 2020) and this may have relieved symptoms among those with and without pre-existing mental health conditions. In contrast, mental health symptoms increased among those with pre-existing physical health conditions, which may reflect the elevated risk and stress posed by COVID-19 to this group.

Increases in mental health symptoms were observed in both North American and European samples, though there were a limited number of studies from other continents and this is a limitation of the present research. We examined whether a range of country-level factors explained heterogeneity in mental health change across samples, including number of country level COVID-19 cases and attributed deaths in the month that mental health was measured, levels of government financial support and social restrictions in place to reduce virus transmission. We found no evidence that country level factors explained heterogeneity in primary analyses or analyses limited to anxiety and depression symptoms separately. However, it is difficult to make firm conclusions because the majority of studies were conducted in the early phase of the pandemic when deaths from COVID-19 were high and restrictive measures had already introduced relatively high levels of restriction. A number of countries also implemented restrictions regionally.

A smaller sub-set of studies examined change in proportion of sampled population exceeding questionnaire cut-offs for clinically relevant mental health symptoms. In line with the main analyses, there was a statistically small increase in likelihood of meeting questionnaire cut-offs. A limitation of these studies is that although questionnaires used have been shown to be valid indicators of clinically relevant mental health disorder symptomology, it was common for response formats to be altered to ask participants to report on shorter time frames (e.g. the last week). Therefore, these studies provide an indication of acute symptom severity rather than clinical diagnostic value (e.g. anxiety disorder diagnosis typically requires symptoms for several months (Kupfer, 2015)).

In contrast to the fear that the COVID-19 pandemic would cause a parallel and longstanding mental health crisis (Holmes et al., 2020; Pfefferbaum and North, 2020), the present findings suggest that overall there has been considerable resilience in mental health. Data on recorded suicides align with this, as there have been stable rates or decreases reported across a number of countries (John et al., 2020). However, there is a need for continued mental health provision, such as online cognitive behavioral therapy (Ho et al., 2020), monitoring of mental health particularly during periods of increased COVID-19 infection and death, and long-term investment in mental health services will also be valuable (Aknin et al., 2021). The increase in depression and mood disorder symptoms that did not return to pre-pandemic levels warrants attention, as even a small upward shift in depressive symptoms may have meaningful cumulative consequences on the population-level.

Strengths of the present review were the inclusion of a wide range of both published and unpublished studies, which should minimize selective reporting of results. Missing data was also very low as we were unable to include only one study due to missing statistical information, and analyses suggested that publication bias was minimal. There are limitations to the studies included. The majority of studies sampled populations in developed countries during the early stages of the pandemic. Given that there have been second waves of the pandemic in many countries during early 2021, it will be important to continue to monitor mental health. Mental health tended to be measured via self-reported questionnaire as opposed to structured diagnostic interviews, which are generally considered gold standard. As is common in systematic reviews and meta-analyses heterogeneity was high and although some of this variability may be explained by differences in questionnaires used, symptom types included and population groups sampled, it will be important to identify further explanatory factors. For example, there are population sub-groups who may be at greater risk of declines in mental health that we were unable to examine and who are likely to be under-represented in studies of the general population. Due to stressful working conditions frontline healthcare workers may be at increased risk and there may be long-term mental health effects among those who

become seriously ill from COVID-19, live in nursing homes or those who suffered financially as a result of the pandemic (Taquet et al., 2021). Further research addressing these populations (e.g. healthcare workers) will be valuable. Participants with pre-existing mental health conditions tended to be grouped in studies and therefore we were unable to examine changes in symptoms among specific patient groups (e.g. schizophrenia). We found little evidence that study outcomes were strongly related to individual risk of bias indicators and in an analysis limited to relatively high-quality studies results were similar to the main analyses. Level of attrition was high across studies and this is a limitation. Although there was no significant evidence of attrition being associated with change in symptoms in meta-regression or sub-group analyses and lower levels of attrition tended to be associated with a smaller as opposed to larger increase in symptoms. Initial title and abstract screening was conducted primarily by a single author due to constraints on time. However, title and abstract screening predominantly involved the exclusion of studies clearly unrelated to mental health and therefore risk of bias is minimal. In line with this, there were no discrepancies in the proportion of title and abstract screened records cross-checked by a second author (25%). A further limitation is that the meta-analysis examining changes in questionnaire cut-off scores relies on combining different questionnaires (with differing cut-off values) and this likely in part explains the large amount of heterogeneity observed. Future work addressing these limitations would be valuable. Further studies that rely on large representative samples with minimal attrition in order to characterise the time course of multiple types of mental health symptom during the pandemic would be valuable and address some of the uncertainty from the present analyses.

5. Conclusion

Among longitudinal cohort studies examining mental health prior to and during the COVID-19 pandemic in 2020 and sampling predominantly European and North American adults, there was a significant but statistically small increase in mental health symptoms. Increases were larger and persistent for depressive symptoms, as opposed to smaller changes in anxiety disorder symptoms and measures of overall mental health functioning. Further monitoring of changes in mental health (particularly depression) and ensuring adequate clinical treatment is available will be of importance. The overall increase in mental health symptoms was most pronounced during the first two months after the WHO declared a pandemic (March, 2020), before decreasing and being comparable to pre-pandemic levels for most symptom types by mid-2020. There was no evidence of a worsening of mental health symptoms among samples of participants with a pre-existing mental health condition.

Data sharing

Study data file and pre-registered analysis protocol are available online https://osf.io/rg24v/. Analysis code and files are available on request.

Role of the funding source

There was no funding source for this study.

Ethical approval

As the study involved no human or animal participants and made use of publicly available information ethical approval was not required

Transparency

ER acts as the guarantor for this work and confirms that the manuscript is an accurate, transparent and honest account of the study, that no important aspects of the study have been omitted and that any discrepancies from the study as planned have been explained.

Articles included in the review and not cited in text

Ahrens et al. (2021), Bignardi, 2020, Brailovskaia and Margraf (2020), Breaux et al. (2021), Castellini et al. (2020), Castellini et al. (2021), Chen et al. (2020), Chiaravalloti et al. (2020), Cohen, 2020, Conti et al. (2020), Creese et al. (2020), , Demir et al. (2020), Dickerson, 2020, Elmer et al. (2020), Ezpeleta et al. (2020), Flentje et al. (2020), Giuntella, 2020, Groot, 2020, Hafstad, 2020, Hajdúk et al. (2020), Hamm et al. (2020), Janssen et al. (2020), Johansson, 2020, Katz (2020), Kikuchi et al. (2020), Kwong et al. (2020), Lee et al. (2020), Linnemayr et al. 1999, Magson et al. (2020), Manjareeka and Pathak (2020), Meda et al. (2021), Munasinghe et al. (2020), Nolvi, 2020, Novotný et al. (2020), Okely et al. (2020), Pan et al. (2020), Papini, 2020, Pasca et al. (2020), Penner et al. (2020), Puhl et al. (2020), Rogers et al. (2021), Ruggieri et al. (2020), Rutherford et al. (2020), Saraswathi et al. (2020), Savage et al. (2020), Schafer et al. (2020), Schützwohl, 2020, Shanahan et al. (2020), Sibley et al. (2020), Sueki and Ueda (2020), Sutin et al. (2020), Tanir et al. (2020), Amaral, 2020, Thombs et al. (2020), van Zyl, 2020, Li et al. (2020), Widnall, 2020, Wright, 2020, Li et al. (2020), Zhang et al. (2020), Zimmermann, 2020

CRediT authorship contribution statement

Eric Robinson: Writing – original draft, Writing – review & editing. **Angelina R. Sutin:** Writing – review & editing. **Michael Daly:** Writing – review & editing. **Andrew Jones:** Data curation, Writing – review & editing.

Declaration of Competing Interest

All authors have completed the ICMJE uniform disclosure form at www.icmje.org/coi_disclosure.pdf . All authors report no conflicts of interest. ER has previously received funding from the American Beverage Association and Unilever for projects unrelated to the present research.

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Supplementary materials

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