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Review

One year on: What we have learned about the psychological effects of COVID-19 social restrictions: A meta-analysis

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Judith A. Feeney² and Jeffrey A. Simpson³

Abstract

This article reports on the first meta-analysis of studies on the association between government-imposed social restrictions and mental health outcomes published during the initial year of the COVID-19 pandemic. Thirty-three studies (N = 131,844) were included. Social restrictions were significantly associated with increased mental health symptoms overall ($d = .41$ [CI 95% .17–.65]), including depression ($d = .83$ [CI 95% .30–1.37]), stress ($d = .21$ [CI 95% .01–.42]) and loneliness ($d = .30$ [CI 95% .07–.52]), but not anxiety ($d = .26$ [CI 95% –.04–.56]). Subgroup analyses demonstrated that the strictness and length of restrictions had divergent effects on mental health outcomes, but there are concerns regarding study quality. The findings provide critical insights for future research on the effects of COVID-19 social restrictions.

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Edited by **Gery C. Karantzas** and **Jeffrey A. Simpson**

For complete overview about the section, refer [Separation, Social Isolation, and Loss \(2022\)](#)

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COVID-19, Lockdown, Mental health, Loneliness, Meta-analysis.

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As the COVID-19 pandemic spread across the world, governments of many nations instituted a variety of social restrictions to “flatten the curve” and contain the exponential rise of case numbers and deaths from the virus. In many countries, social restrictions entailed strict lockdowns of regions, cities, and even entire countries, as well as quarantine measures for those who had (or were suspected to have) contracted the virus. However, humans have fundamental needs for social connection and emotional bonding [1,2]. The attainment of these needs is significantly compromised during times of social restriction when individuals are forced to isolate from family and other close members of their social network [3]. Indeed, numerous studies into social isolation highlight the many negative psychological outcomes associated with strict and enduring social isolation, which include, but are not limited to, depression, anxiety, loneliness, and post-traumatic stress [4–6].

Numerous commentaries, rapid reviews, and position papers published in the early stages of the pandemic raised concerns about the possible negative effects on mental health of the social isolation associated with these social restrictions [3,7]. This was followed by a large number of cross-sectional studies investigating the mental health effects of social restrictions and quarantine measures. Most of this work was motivated by the urgent need to generate evidence to determine whether pandemic-related social restrictions negatively impacted mental health.

One year on, government-mandated social restrictions continue to be enforced in many parts of the world due to problems with the timely roll-out of COVID-19 vaccines [8,9], vaccine hesitancy [9,10], and the high transmissibility of the Delta (B.1.617.2) and Omicron variants [11,12]. To this end, the need for a solid evidence-base to guide mental health policies in times of social restrictions is timely and necessary.

Although there has been a rapid output of research, this fast response has come with potential costs. As noted recently [13], one such cost involves concerns about study quality. Moreover, research has generated

Table 1

Search terms.

| Key Concept | Social isolation during lockdown | Context | Mental health outcomes |
|---|--|--|--|
| Free text terms | <ul style="list-style-type: none"> - "Quarantine" - Lockdown - "Social distancing" - "Physical distancing" - "Social restrict" - "Social isolation" - "Stay at home order" - Confinement | <ul style="list-style-type: none"> - COVID*-19 - "Coronavirus" | <ul style="list-style-type: none"> - "Mental health" - "Mental illness" - "Mental disorder" - "Emotional problems" - "Psychological adjustment" - Depression - Anxiety - Stress - mood - Grief - bereavement - Trauma - Loneliness - "Posttraumatic stress" - "Psychological impact" - "Suicide**" - "Self-harm" - "Psychological distress" - "Psychiatric disorder" - "Psychosis" - "Mania" - "Bipolar disorder" - Maladjustment |
| Controlled vocabulary terms/subject terms | <ul style="list-style-type: none"> - MM "quarantine" - MM "physical distancing" - MM social isolation - DE "social isolation" | <ul style="list-style-type: none"> - MM "pandemics" OR MM "epidemics" - MM "COVID-19" | <ul style="list-style-type: none"> - MM "mental health" - MM Major Depression - MM "emotional trauma" - MM "grief" - MM "hopeless" - MM "loneliness" - MM "pessimism" - MM "sadness" - MM "emotional disturbance" - MM "stress" - MM "depression (emotion)" - MM "traumatic loss" - MM "posttraumatic stress disorder" - MM "panic disorder" - MM anxiety - DE "Mental health" - DE "distress" |

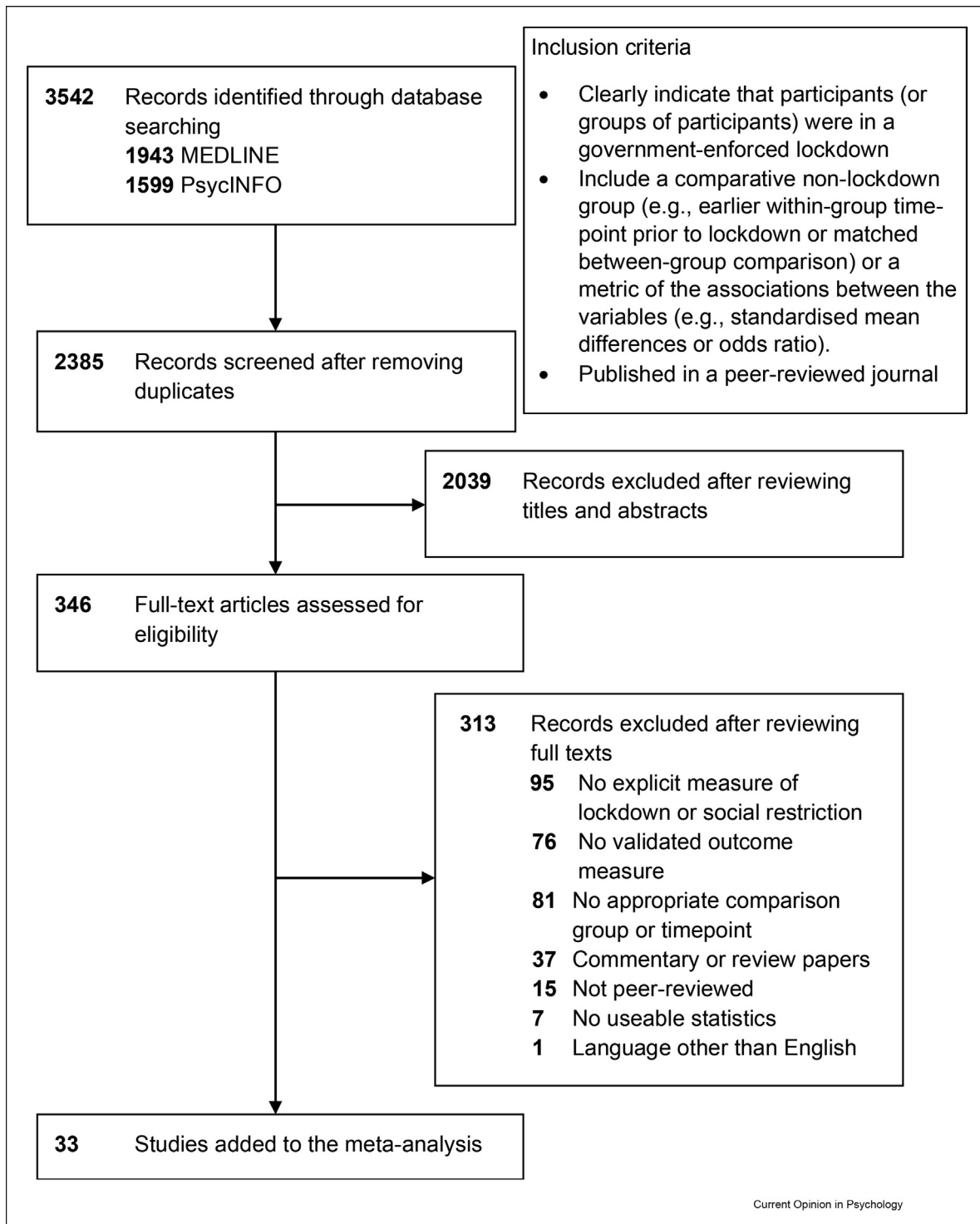
contradictory findings regarding how social restrictions tend to be associated with certain mental health outcomes. The issue of study quality, coupled with the contradictory findings reported across studies, makes it difficult to interpret the effects of COVID-19 social restrictions on the overall mental health of people.

When we synthesize the published research investigating the effects of social restrictions on mental health outcomes during the first full year of the COVID-19 pandemic, what do we find? Is the strictness of social restrictions associated with an increase in mental health symptoms? Synthesizing the current evidence and determining its quality is essential to guard against the possible harmful effects of overly dramatic or inaccurate reporting on the COVID-19 pandemic, including mental health outcomes [13].

Methods

This meta-analysis was conducted following PRISMA guidelines [14]. We conducted parallel systematic searches of MEDLINE and PsycINFO for all studies that investigated the relation between social isolation and mental health outcomes published in peer-reviewed journals between March 2020 and March 11, 2021 (to capture the first full year of the pandemic, World Health Organisation [WHO] [15]). Language was restricted to English. The reference lists of the included studies were manually searched. Key search terms reflected the main concepts: social restriction measures enforced by governments in response to COVID-19 and mental health outcomes. The full list of search terms appears in Table 1 and the criteria for study inclusion appear in Figure 1. After duplicate articles were removed, two reviewers (LK & DR)

Figure 1



PRISMA study selection flowchart.

independently screened titles, abstracts, and full texts of articles identified in the search using Covidence software. Disagreements regarding inclusion were

discussed with the senior reviewer (GK) until consensus was reached. We did not contact authors to seek missing data.

Data analysis

Statistical analyses were conducted using Comprehensive Meta-Analysis version 3.3.070 CMA Biostat Inc., Englewood, NJ [16]. The effect size metric varied across the studies. Thus, to allow for cross-study comparisons, all reported effect sizes were converted to a common metric – Cohen’s d – in CMA. Five meta-analyses were conducted. The first estimated the effect size of social restriction on overall mental health symptoms. The other four meta-analyses estimated the effect size of social restriction (separately) on mental health outcomes for which there were multiple studies – namely – depression, anxiety, stress, and loneliness. A random-effects model with restricted maximum likelihood estimation was used for each meta-analysis to account for heterogeneity between studies. Heterogeneity between studies was assessed with the Q statistic [17] and the I^2 [18]. Publication bias was assessed by visual inspection of the funnel plot and Egger’s regression intercept [19].

Subgroup moderator analyses were conducted when there were multiple studies present for each subgroup. These moderator analyses were based on the type of social restriction (low, moderate, or strict), length of exposure to social restrictions (less than 2 weeks, 2–4 weeks, more than one month), WHO region classification (Americas, Europe, or Western Pacific), age (under 18 years, 18–30 years, 31–59 years, 60+ years), and whether or not the sample had participants who reported pre-existing physical or mental health vulnerabilities. Additionally, we conducted a “study design” subgroup moderator analysis, which compared cross-sectional, retrospective reporting, and longitudinal studies. All subgroup moderator analyses were completed using random-effects models and z -tests to determine the significance of the difference in point estimates observed for each subgroup.

Study quality

The quality of each study was evaluated according to the National Institutes of Health’s Quality Assessment Tool for Observational Cohort and Cross-Sectional Studies [20] (see [supplemental material Table S1](#)). To assess the impact of any poor quality, sensitivity analyses were conducted. To establish the stability of the overall effect estimates, a one-study-removed sensitivity analysis was undertaken in CMA to identify any outlier studies [16].

Results

In all, 3542 records were identified from both databases (see [Figure 1](#)), 1157 duplicate records were removed, and 2038 more studies were excluded because they were ineligible. This resulted in 346 full-text articles retrieved for further screening. Of these, 33 articles were eligible for inclusion in the meta-analyses (see [Table 2](#)).

The intra-class correlation between reviewers was high (0.96, 95% CI 0.89–0.98) for study quality assessment, and studies were generally rated poor ($n = 14$) or fair ($n = 16$) in quality (see [Table 3](#)) on the four-point scale, which ranged from *poor* to *excellent*. The most common risk to study quality was not having a clearly defined or reliable measure of social restrictions; in many cases, global assumptions were made regarding participants’ experiences and compliance with social restrictions. Additionally, some studies did not allow a sufficient timeframe to witness an association between exposure to social isolation due to lockdown and mental health outcomes (i.e., studies that were cross-sectional, or collected data on the day social restrictions commenced). Finally, although confounding variables were measured in some studies (e.g., whether people were isolating alone, satisfaction with social restriction measures, having or knowing someone who had COVID-19), many studies did not measure confounding variables, such as pre-existing mental health conditions, previous history of experiencing loneliness, or enduring vulnerability and resiliency factors.

The overall pooled point estimate of the effect of social restrictions on overall mental health symptoms (i.e., all outcomes combined) was .41 (see [Table 4](#)). When mental health symptoms were broken down by the four outcomes for which there were multiple studies (i.e., depression [$k = 27$], anxiety [$k = 19$], stress [$k = 9$] and loneliness [$k = 6$]), the pooled point estimates for each outcome varied between $d = 0.21$ and 0.83 (see [Table 4](#); forest plots are presented in the supplementary material [[Figures S1–S4](#)]). Specifically, people who experienced social restrictions reported significantly higher levels of depression, stress, and loneliness. However, no significant association was found between social restrictions and anxiety. Substantial heterogeneity was detected in all meta-analyses of the pooled estimates.

Subgrouping of studies by different characteristics further revealed several significant effects on the point estimates (effect sizes and z -tests available in supplementary material [[Tables S2–S3](#)]). Depression was significantly higher in people exposed to strict compared to moderate social restrictions ($Z = 2.07$, $p = 0.04$), whereas anxiety was significantly higher for those exposed to low compared to moderate social restrictions ($Z = 3.88$, $p = 0.0001$). In terms of length of exposure to social restrictions (less than 2 weeks, 2–4 weeks, or 1+ month), stress was significantly higher for people experiencing shorter social restrictions (i.e., less than 2 weeks) compared to those experiencing longer restrictions (i.e., 2–4 weeks [$Z = 2.11$, $p = .03$] and 1+ month [$Z = 2.09$, $p = .04$]). Participants from the European region reported significantly more depression than did those from other regions (i.e., Americas [$Z = 2.16$, $p = .03$] and Western Pacific [$Z = 2.16$,

Table 2

Study characteristics of primary studies included in meta-analysis.

| Study | Country (<i>WHO Region</i>) | <i>N</i> | Age Mean (SD) | Data collection period | Length of exposure to social restrictions ^a (days) | Study design | Social restriction type ^b | Comparison | Outcome measure |
|------------------------------------|-----------------------------------|----------|----------------------------|--|---|-----------------|--------------------------------------|---|--|
| Altieri & Santangelo (2021) [27] | Italy (<i>Europe</i>) | 84 | 63.6 (10.9) | April 21 st – May 3 rd 2020 | 49 | Cross-sectional | Strict | Retrospective pre-lockdown scores | Anxiety (HADS) Depression (HADS) |
| Brailovskaia & Margraf (2020) [28] | Germany (<i>Europe</i>) | 436 | 27.01 (6.41) | March 20 th – March 28 th 2020 | 4 | Longitudinal | Strict | Pre-lockdown scores (October 2019) | Depression (DASS21) Anxiety (DASS21) Stress (DASS21) |
| Campos et al. (2020) [29] | Brazil (<i>Americas</i>) | 12,101 | Not reported | March 18 th – June 25 th 2020 | 7 | Cross-sectional | Low | Socialisation unchanged or better than usual (versus lower) | Depression (DASS21) Anxiety (DASS21) Stress (DASS21) Psychological Distress (IES-R) |
| Castellini et al. (2020) [30] | Italy (<i>Europe</i>) | 130 | 34.03 (14.03) | April 22 nd – May 3 rd 2020 | 42 | Longitudinal | Strict | Pre-lockdown scores (December 2019–January 2020) | Depression (BSI) Anxiety (BSI) |
| Daly, Sutin & Robinson (2020) [31] | USA (<i>Americas</i>) | 5428 | 48.4 (16.3) | April 2020 | 0 | Cross-sectional | Moderate | Non-lockdown sample | Depression (PHQ-2) |
| Davide et al. (2020) [32] | Italy (<i>Europe</i>) | 30 | 43.17 (14.87) | April 16 th – April 17 th 2020 | 42 | Longitudinal | Strict | Pre-lockdown scores (January–February 2020) | OCD symptoms (Y-BOCS) |
| Elmer et al. (2020) [33] | Switzerland (<i>Europe</i>) | 212 | Not reported | April 2020 | 14 | Longitudinal | Low | Correlation between feeling socially isolated and outcomes | Depression (CES –DS) Stress (CES – SS) |
| Fiorillo et al. (2020) [34] | Italy (<i>Europe</i>) | 20,720 | 40.4 (14.3) | March–May 2020 | 21 | Cross-sectional | Strict | Non-lockdown sample | Depression (DASS21) Anxiety (DASS21) Stress (DASS21) |
| Forte et al. (2020) [35] | Italy (<i>Europe</i>) | 2991 | 30.0 (11.5) | March 18 th – March 31 st 2020 | 10 | Cross-sectional | Strict | Retrospective pre-lockdown scores | Depression (SCL-90) Anxiety (SCL-90) Stress (SCL-90) |
| Giannopolou et al. (2020) [36] | Greece (<i>Europe</i>) | 442 | Not reported (Adolescents) | April 16 th – April 30 th 2020 | 21 | Cross-sectional | Low | Retrospective pre-lockdown scores | Depression (PHQ-9) Anxiety (GAD-7) |
| Groarke et al. (2020) [37] | United Kingdom (<i>Europe</i>) | 1964 | 37.11 (12.86) | March 23 rd – April 24 th 2020 | 0 | Cross-sectional | Strict | Non-lockdown sample | Loneliness (Three-Item Loneliness Scale) |
| Harris & Sandal (2020) [38] | Norway (<i>Europe</i>) | 4008 | Not reported | March 20 th – March 27 th 2020 | 7 | Cross-sectional | Unclear | Non-lockdown sample | Psychological Distress [depression & anxiety] (HSCL-10) |
| Hawes et al. (2021) [39] | New York, USA (<i>Americas</i>) | 283 | 17.49 (1.42) | March 27 th – May 15 th 2020 | 7 | Longitudinal | Strict | Pre-lockdown scores (December 2014–July 2019) | Depression (CDI) Anxiety (SCARED) |

(continued on next page)

Table 2. (continued)

| Study | Country (WHO Region) | N | Age Mean (SD) | Data collection period | Length of exposure to social restrictions ^a (days) | Study design | Social restriction type ^b | Comparison | Outcome measure |
|--------------------------------------|-------------------------------|--------|---------------|--|---|-----------------|--------------------------------------|--|--|
| Holloway et al. (2021) [40] | International | 8209 | Not reported | April 16 th – May 4 th 2020 | Unclear | Cross-sectional | Unclear | Non-lockdown sample | Anxiety (single item) Loneliness (single item) |
| Jia et al. (2020) [41] | United Kingdom (Europe) | 3097 | 44.0 (15.0) | April 3 rd – April 30 th | 11 | Cross-sectional | Strict | Prospective matched UK-normative sample (non-lockdown) | Depression (PHQ-9) Anxiety (GAD-7) Stress (PSS-4) |
| Krendl & Perry (2020) [42] | USA (Americas) | 87 | 75.20 (6.86) | April 21 st – May 21 st 2020 | 28 | Longitudinal | Moderate | Pre-lockdown scores (June–November 2019) | Depression (PHQ-9) Loneliness (UCLA Loneliness Scale) |
| Li et al. (2020) [43] | China (Western Pacific) | 555 | 19.6 (3.4) | January 13 th – January 15 th 2020 | 15 | Longitudinal | Strict | Pre-lockdown scores (December 2019) | Psychological Distress ([Depression & Anxiety] PHQ-4) |
| Lopez-Morales et al. (2020) [44] | Argentina (Americas) | 204 | 32.56 (4.71) | March 22 nd – May 10 th 2020 | T2: 14 T3: 47 | Longitudinal | Moderate | Start of lockdown score (2–5 days into lockdown) | Depression (BDI-II) Anxiety (STAI) |
| Lu et al. (2020) [45] | China (Western Pacific) | 1849 | 30.62 (9.44) | March 6 th – March 12 th 2020 | 43 | Cross-sectional | Strict | Non-lockdown sample | Depression (CED) |
| Magson et al. (2020) [46] | Australia (Western Pacific) | 248 | 14.4 (0.5) | May 5 th – May 14 th 2020 | 60 | Longitudinal | Strict | Pre-lockdown scores (2019) | Depression (SCAS) Anxiety (SMFQ – CV) |
| Marroquin et al. (2020) [47] | USA (Americas) | 118 | 39.2 (11.5) | March 26 th – March 28 th 2020 | 7 | Longitudinal | Moderate | Pre-lockdown scores (2019) | Depression (CES –DS) Anxiety (GAD-7) |
| Ozamiz-Etxebarria et al. (2020) [48] | Spain (Europe) | 1112 | 33.80 (16.65) | April 2 nd – April 12 th 2020 | 20 | Longitudinal | Strict | Start of lockdown scores (–3 to 4 days into lockdown) | Depression (DASS21) |
| Ruggieri et al. (2020) [49] | Italy (Europe) | 113 | 32.05 (8.01) | March 25 th – April 14 th 2020 | 14 | Longitudinal | Strict | Pre-lockdown scores (March 7 th – 9 th 2020; 2 days before lockdown) | Depression (DASS21) Anxiety (DASS21) Stress (DASS21) Loneliness (Three-Item Loneliness Scale) |
| Shi et al. (2020) [50] | China (Western Pacific) | 59,679 | 35.97 (8.22) | February 28 th – March 11 th 2020 | 36 | Cross-sectional | Strict | Non-lockdown sample | Depression (PHQ-9) Anxiety (GAD-7) Stress (ASDS) |
| Sibley et al. (2020) [51] | New Zealand (Western Pacific) | 1003 | 51.6 (13.2) | March 26 th – April 12 th 2020 | 1 | Longitudinal | Strict | Pre-lockdown scores (October–December 2019) | Psychological distress (K-6) |
| Tang, F. et al. (2020) [52] | China (Western Pacific) | 1160 | Not reported | February 5 th – February 7 th 2020 | 13 | Cross-sectional | Strict | Non-lockdown sample | Depression (CES –DS) Anxiety (GAD-7) |
| Tang, W. et al. (2020) [53] | China (Western Pacific) | 256 | 19.81 (1.55) | February 20 th – February 27 th 2020 | 28 | Cross-sectional | Strict | Non-lockdown sample | Depression (PHQ-9) PTSD (PCL-C) |

| | | | | | | | | | |
|----------------------------------|--------------------------------------|------|---------------|---|---------|-----------------|----------|---|---|
| Tull et al. (2020) [54] | USA (<i>Americas</i>) | 500 | 40.0 (11.6) | March 27 th – April 5 th 2020 | unclear | Cross-sectional | Moderate | Non-lockdown sample | Depression (DASS21) Health anxiety (SHAI) Loneliness (UCLA loneliness Scale) |
| White & Van der Boor (2020) [55] | United Kingdom (<i>Europe</i>) | 600 | 36.75 (13.44) | March 31 st – April 3 rd 2020 | 7 | Cross-sectional | Strict | Retrospective pre-lockdown scores | Depression (HADS) anxiety (HADS) |
| Wilson et al. (2020) [56] | USA (<i>Americas</i>) | 848 | 48.02 (16.30) | March 30 th – April 5 th 2020 | unclear | Longitudinal | Unclear | Non-lockdown sample | Depression (PHQ-9) anxiety (GAD-7) |
| Wong et al. (2020) [57] | Hong Kong (<i>Western Pacific</i>) | 583 | 70.9 (6.1) | March 24 th – April 15 th 2020 | 0 | Longitudinal | Strict | Pre-lockdown scores (October–December 2019) | Depression (PHQ-9) Anxiety (GAD-7) Loneliness (De Jong Gierveld loneliness scale) |
| Xin et al. (2020) [58] | China (<i>Western Pacific</i>) | 515 | 19.9 (1.6) | February 1 st – February 20 th 2020 | 3 | Cross-sectional | Strict | Non-lockdown sample | Depression (PHQ-9) |
| Zhu et al. (2020) [59] | China (<i>Western Pacific</i>) | 2279 | Not reported | February 12 th – March 17 th 2020 | 14 | Cross-sectional | Strict | Non-lockdown sample | Depression (PHQ-9) Anxiety (GAD-7) |

Note. ASDS, *Acute Stress Disorder Scale*; BDI-II, Beck Depression Inventory-II; CDI, Children's Depression Inventory; CES – DS, Center for Epidemiologic Studies–Depression Scale; CES – SS, Center for Epidemiologic Studies–Stress Scale; DASS21, Depression, Anxiety, Stress Scale – 21; GAD-7, Generalized Anxiety Disorder- 7; HADS, Hamilton Anxiety and Depression Scale; HSCL-10, Hopkins Symptom Checklist; IES-R, Impact of Events Scale – Revised; K-6, Kessler-6; PCL-C, PTSD Check List- Civilian Version; PHQ-9, The Patient Health Questionnaire- 9; PHQ-2, The Patient Health Questionnaire- 2; PSS-4, Perceived Stress Scale- 4; SCARED, Screen for Child Anxiety-Related Disorders; SCAS, Spence Children's Anxiety Scale; SCL-90, Symptom Checklist- 90; SHAI, Short Health Anxiety Inventory; SMFQ – CV, Short Mood and Feelings Questionnaire-child version; STAI, State-Trait Anxiety Inventory; Y-BOCS, Yale-Brown Obsessive Compulsive Scale.

^a At the start of data collection time.

^b Type of Social Restriction: Strict = Stay at home order enforced, closure of non-essential businesses, all public venues closed, social gatherings banned, travel banned; Moderate = Stay at home order not enforced, closure of non-essential businesses, plus restrictions on public gatherings, domestic quarantine for close contacts of confirmed cases, restriction on travel; Low = Stay at home order not enforced, closure of non-essential businesses.

Table 3

Study quality ratings.

| Study | Q1 | Q2 | Q3 | Q4 | Q5 | Q6 | Q7 | Q8 | Q9 | Q10 | Q11 | Q12 | Q13 | Q14 | Rating |
|--------------------------------------|----|----|----|----|----|----|----|----|----|-----|-----|-----|-----|-----|--------|
| Altieri & Santangelo (2021) [27] | 1 | 1 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | NA | 0 | 1 | Poor |
| Brailovskaia & Margraf (2020) [28] | 1 | 1 | 1 | 1 | 1 | 1 | 0 | 1 | 0 | 0 | 1 | NA | 0 | 0.5 | Fair |
| Campos et al. (2020) [29] | 1 | 1 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0.5 | Poor |
| Castellini et al. (2020) [30] | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 0 | 0 | 1 | NA | 0 | 1 | Good |
| Daly, Sutin & Robinson (2020) [31] | 1 | 1 | 1 | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 1 | NA | 1 | 0.5 | Fair |
| Davide et al. (2020) [32] | 1 | 1 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 1 | Poor |
| Elmer et al. (2020) [33] | 1 | 1 | 1 | 0 | 1 | 0 | 0 | 1 | 0 | 1 | 1 | NA | 0 | 0.5 | Fair |
| Fiorillo et al. (2020) [34] | 1 | 1 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 1 | Poor |
| Forte et al. (2020) [35] | 1 | 1 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0 | Poor |
| Giannopolou et al. (2020) [36] | 1 | 1 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0 | Poor |
| Groarke et al. (2020) [37] | 1 | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | NA | NA | 1 | Poor |
| Harris & Sandal (2020) [38] | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | NA | NA | 0.5 | Poor |
| Hawes et al. (2021) [39] | 1 | 1 | 1 | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 1 | NA | 1 | 0.5 | Fair |
| Holloway et al. (2021) [40] | 1 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | NA | NA | 0.5 | Poor |
| Jia et al. (2020) [41] | 1 | 1 | 0 | 0 | 1 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 1 | Fair |
| Krendl & Perry (2020) [42] | 1 | 1 | 0 | 0 | 1 | 1 | 1 | 1 | 0 | 1 | 1 | NA | NA | 0 | Fair |
| Li et al. (2020) [43] | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 1 | 0 | 1 | 1 | NA | 0 | 0.5 | Fair |
| Lopez-Morales et al. (2020) [44] | 1 | 1 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 1 | 1 | NA | 1 | 1 | Good |
| Lu et al. (2020) [45] | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 1 | Fair |
| Magson et al. (2020) [46] | 1 | 1 | 1 | 1 | 0 | 1 | 1 | 1 | 0 | 1 | 1 | NA | 0 | 1 | Good |
| Marroquin et al. (2020) [47] | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 1 | 1 | NA | 1 | 0.5 | Good |
| Ozamiz-Etxebarria et al. (2020) [48] | 1 | 1 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0 | Poor |
| Ruggieri et al. (2020) [49] | 1 | 1 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 1 | 1 | NA | 0 | 0 | Poor |
| Shi et al. (2020) [50] | 1 | 1 | 1 | 1 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0.5 | Fair |
| Sibley et al. (2020) [51] | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 1 | Fair |
| Tang, F. et al. (2020) [52] | 0 | 1 | 1 | 0 | 1 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 1 | Fair |
| Tang, W. et al. (2020) [53] | 1 | 1 | 1 | 1 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0.5 | Fair |
| Tull et al. (2020) [54] | 1 | 1 | 0 | 1 | 1 | 1 | 0 | 1 | 0 | 0 | 1 | NA | NA | 1 | Fair |
| White & Van der Boor (2020) [55] | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | NA | NA | 0 | Poor |
| Wilson et al. (2020) [56] | 1 | 1 | 0 | 1 | 1 | 1 | 0 | 1 | 0 | 0 | 1 | NA | NA | 0.5 | Fair |
| Wong et al. (2020) [57] | 1 | 1 | 1 | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 1 | NA | 1 | 1 | Good |
| Xin et al. (2020) [58] | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | NA | NA | 0.5 | Poor |
| Zhu et al. (2020) [59] | 1 | 1 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 1 | NA | NA | 0 | Poor |

Table 4

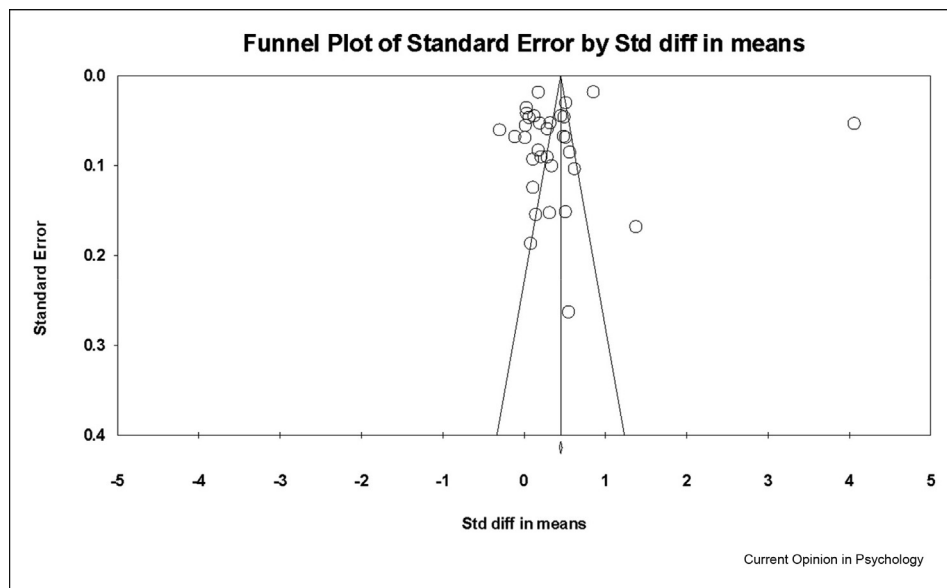
Results of random-effects meta-analyses.

| Outcome | Number of Studies | Cohen's <i>d</i> (95% CIs) | Q statistic (df) [<i>I</i> ²] | p-value |
|--------------------------------|-------------------|----------------------------|--|---------|
| Overall mental health symptoms | 33 | .41 (.17–.65) | 6154.43 (32) [99.48] | <.0001 |
| Depression | 27 | .83 (.30–1.37) | 21668.73 (26) [99.88] | <.0001 |
| Anxiety | 19 | .26 (–.04–.56) | 4013.08 (18) [99.55] | = .098 |
| Stress | 9 | .21 (.01–.42) | 687.01 (8) [98.84] | = .044 |
| Loneliness | 6 | .30 (.07–.52) | 53.40 (5) [90.64] | = .009 |

$p = .03$). Additionally, outcomes of depression were significantly higher among people who reported no pre-existing physical or mental health conditions than those who did ($Z = 2.02$, $p = .04$), and among people under age 18 ($Z = 4.60$, $p = .01$) or between 31 and 59 ($Z = 2.20$, $p = .03$) compared with older adults (aged 60+ years). All other subgroup moderator analyses either showed no significant effect on the overall point

estimates, or there were too few studies to conduct the moderator analysis (see supplementary material [Tables S2–S3]). The “study design” subgroup moderator analyses indicated that the magnitude of the effect size for all outcomes did not differ significantly as a function of study design (i.e., cross-sectional, retrospective reporting, or longitudinal designs; see supplementary material [Tables S2–S3]).

Figure 2



Funnel plot of all studies.

Visual inspection of the funnel plot indicated no potential publication bias (see Figure 2). Confirming this, Egger's regression intercept was not significant (intercept = -1.40 ; SE = 4.34 ; $p = 0.75$). Although there is no evidence of publication bias, the overall point estimates should be interpreted with caution due to the high heterogeneity of the data.

Although the study quality subgroup moderator analyses revealed no significant impact of poor study quality on the point estimates (see supplementary material [Table S2]), visual inspection of the forest plots identified one study [34] as a potential outlier. However, removing this study from the meta-analyses did not significantly change the overall pooled estimates or 95% confidence intervals for any of the outcomes (see supplementary material [Table S2]). In fact, the point estimate remained significant and 95% CIs overlapped in all iterations of the one-study-removed analysis (see supplementary material [Figures S5-S8]), indicating that no study had an undue influence on the point estimates.

Discussion

Our quantitative synthesis of 33 studies demonstrates that, overall, mental health symptoms were significantly worse when people were exposed to mandated social restrictions and quarantine measures. Our subgroup analyses suggest that the strictness and length of social restrictions have divergent effects on depression, anxiety and stress (but not loneliness). For depression, strict restrictions are associated with higher symptoms; for anxiety, low social restrictions are associated with higher symptoms; for stress, shorter social restrictions are

associated with greater stress symptomatology. Furthermore, subgroup analyses that examined the moderating effects of age and having a pre-existing physical or mental health condition revealed significant differences between subgroups, but only for depression.

One limitation common to many of the published studies included in this meta-analysis is the failure to assess important individual difference variables, personal vulnerabilities, and contextual factors that could clarify our understanding of the associations between social restrictions and mental health outcomes [3,21]. As part of our meta-analysis, subgroup analyses of such factors was limited to only two variables – both individual difference factors – age and having an existing physical or mental health condition. Nevertheless, there is a need for integrative research that takes into account the inherent complexities and confluence of a variety of factors that can affect mental health symptoms. These include, but are not limited to, the multiple contextual stressors brought on by the pandemic such as financial/job insecurity, health anxieties and concerns regarding virus contagion, socio-economic and cultural differences, media reporting of the pandemic, and the role of individual difference variables, including enduring vulnerabilities (e.g., negative affectivity, difficulties regulating emotions), resiliency factors (e.g., problem-focused coping styles, trait optimism), and interpersonal factors (e.g., the quality of familial relationships and the strength of social networks). Moreover, such integrative approaches should consider how facets of mental health and wellbeing are inter-related. In the context of COVID-19 social restrictions, it is important to consider how loneliness may act as an explanatory variable in understanding

the association between social restrictions and mental health outcomes such as depression and anxiety. Various reviews of the effects of loneliness find moderate associations between loneliness and such facets of mental health [22]. Furthermore, during the COVID-19 crisis, loneliness has been especially prevalent among helpline callers, particularly during periods of strict social restrictions [23]. Thus, research that adopts a multifactorial, integrative perspective to the study of COVID-19 social restriction measures is likely to be best positioned to provide informative explanations regarding the effects of these restrictions on individuals' mental health [3,21].

Although there were no significant differences in the findings as a function of study quality, 91% of the studies had poor-to-fair quality. This highlights another limitation of existing research and raises concerns regarding the confidence that can be placed in the broader findings on relations between COVID-19 social restrictions and mental health effects [13]. The biggest study quality issue involved the validity of assessments of participants' exposure to social restrictions. Most studies made global assumptions about people's experiences and compliance with social restrictions. Even longitudinal studies, many of which compared pre-social-restriction assessments of mental health with assessments conducted during social restriction, did not model time as a continuous variable. However, modelling time as continuous would have allowed participants' mental health symptoms at the time of data collection to be mapped onto the number of days each participant had experienced a certain amount of social restriction. Moreover, given the wide-ranging period during which data collection occurred in particular studies, even some of the longitudinal studies do not provide a clear picture of the effects of lockdown on mental health. Thus, future research must model time as a continuous variable, implement better assessments of the duration and adherence to social restrictions, and use these findings to inform public health policies.

Future studies should also collect multiple assessments of mental health symptoms during and after social restrictions, to model mental health trajectories. Doing so may clarify when mental health symptoms are at their highest and help determine recovery time. For example, our findings suggest that stress may be highest during the earlier stages of social restrictions, as people struggle to adjust to the changes. The modelling of linear and non-linear patterns of change may provide critical insights into the timing of public health interventions to address mental health issues within different populations. The leveraging of digital technologies for the monitoring of symptoms, coupled with the delivery of self-directed e-therapies and the provision of telehealth, can offer cost-effective solutions with wide reach and penetrability for addressing population-level mental health issues when social restrictions preclude access to in-person mental health support and services. The

coupling of frequent assessments of mental health symptoms along with self-directed digital interventions can provide an opportunity to track the mental health of individuals and trigger push notifications to engage in tailored programs or activities when subclinical or clinical levels of mental health symptoms are logged.

Given that social restrictions were found to be associated with increases in loneliness, and that those who experience a lack of social contact often report feeling lonely [24], it may be important to promote digital technologies and interventions that provide opportunities for lonely individuals to have social contact with others in virtual environments. For instance, in addition to encouraging people to connect with family and friends online, access to interventions that help people meaningfully (re)develop group memberships and social identities may be especially important (see Haslam et al. [60], this issue). At the very least, providing moderated forums that help those experiencing loneliness during social restrictions to connect with others may be useful, given that contact, even amongst strangers, is important for enhancing well-being [25,26].

In closing, the current research represents the first quantitative synthesis of the effects of COVID-19 social restrictions on mental health outcomes during the first full year of the pandemic. The findings clarify the state of the field regarding whether social restrictions are, in fact, associated with the psychological wellbeing of individuals. These findings offer novel insights that can: (a) guide and hopefully enhance the quality of future mental health research, and (b) inform policy to support the psychological wellbeing of citizens experiencing the strain of social restrictions.

Conflict of interest statement

Nothing declared.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.copsyc.2022.101315>.

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- * of special interest
- ** of outstanding interest

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This rapid review draws on studies that investigated past pandemics to provide initial insights into the effects of social restrictions on mental health that could be applied to the COVID-19 pandemic.

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