1   No effects of COVID-19 related social media use on well-being

2   Abstract

3 In times of crisis such as the Corona pandemic, it is important that citizens stay informed

4 about recent events, the latest political decisions, or mandatory protection measures. As a

5 result, to attain relevant information many people use various types of media, and

6 increasingly social media. However, because social media are particularly engaging, some

7 find it hard to disconnect and cannot stop 'doomscrolling.' In this preregistered study, I

8 investigate whether using social media for COVID-19 related topics might put personal

9 well-being at risk. To this end, I analyze data from the Austrian Corona Panel Project,

10 which consists of 24 waves with overall 3,018 participants. The data were analzed using

11 random effects cross lagged panel models, controlling for several stable and varying

12 covariates. Results showed that COVID-19 related social media use did not meaningfully

13 affect several types of well-being, including life satisfaction, positive affect, and negative

14 effect. This pertains to both passive and active social media use, and all the prominent

15 channels such as Facebook, WhatsApp, or YouTube. As a result, this study suggests fears

16 that social media use during times of crisis might be detrimental for well-being can be put

17 to rest.

18   *Keywords:* COVID, well-being, affect, life satisfaction, social media use, news use,

19 random effects within between model, panel study, longitudinal

²⁰   No effects of COVID-19 related social media use on well-being

²¹   During the COVID-19 pandemic, several events unfolded in quick succession. How

²² dangerous is the virus? Is it spreading in my region? How is it transmitted, and how can I

²³ protect myself? Because for many it was (and still is) a matter of life or death, citizens had

²⁴ to stay informed regarding the latest developments. Governments around the world

²⁵ implemented safety measures, from wearing masks or keeping physical distance, to

²⁶ complete lockdowns. In this extraordinary situation, people hence used media excessively,

²⁷ and especially social media were at an all time high (Statista, 2021). A new phenomenon

²⁸ "termed doomscrolling" emerged: People could not stop using social media to attain

²⁹ COVID-19 related news.

³⁰   Several people reported that they were glued to their screens and could not pursue

³¹ other relevant activities such as working, taking a break, or even care-work. Increasingly, it

³² was asked whether such an increase in social media use could still be considered useful and

³³ adaptive, or whether it created an additional and new psychological danger for the users'

³⁴ mental health (Klein, 2021). A study with 6,233 people from Germany found that

³⁵ "[f]requency, duration and diversity of media exposure were positively associated with more

³⁶ symptoms of depression and unspecific and COVID-19 specific anxiety" (Bendau et al.,

³⁷ 2021).

³⁸   As a result, with this study I want to build on this research and investigate the

³⁹ question whether COVID-19 related social media use during the pandemic affected the

⁴⁰ users' well-being. To this end I analyze a large-scale panel study from the Austrian Corona

⁴¹ Panel Project (Kittel et al., 2020). The panel consists of 24 waves and an overall sample

⁴² size of 3018, with at least 1,500 participants per wave, and it is representative of the

⁴³ Austrian population. The panel study collected a large number of variables. Because we

⁴⁴ can therefore control for many confounding third variables, both stable and varying,

⁴⁵ together with the longitudinal design this creates a unique opportunity to analyze causality.

Defining Well-being and Media Use

The underlying theories that guided the selection of variables for this study are the two-continua model of mental health (Greenspoon & Saklofske, 2001) and the hierarchical taxonomy of computer-mediated medation (Meier & Reinecke, 2020). According to the two-continua model of mental health, mental health consists of two dimensions, psychopathology and well-being. Because the aim of this study is better to understand typical users and everyday contexts, my focus will be on well-being. Well-being can be differentiated into subjective and psychological well-being (Diener, Lucas, & Oishi, 2018). Whereas subjective well-being emphasizes hedonic aspects such as a happiness and joy, psychological well-being focuses on eudaimonic aspects including fulfillment and meaning. Subjective well-being is primarily about achieving positive affect and avoiding negative affect. In my view, life satisfaction is a meta concept above both psychological and subjective well-being, representing a meta-appraisal of one's life. Notably, life satisfaction is very stable and fluctuates only little, whereas it's the exact opposite for affect (Dienlin & Johannes, 2020). To capture well-being in this study, I will thus build on life satisfaction, positive affect, and negative affect. Together, this should provide an encompassing perspective on media effects.

The hierarchical taxonomy of computer-mediated communication differentiates six levels of how people engage with digital technology: first, the device (e.g., smartphone); second, the type of application (e.g., social networking site); third, the branded application (e.g., Twitter); fourth, the feature (e.g., status post); fifth, the interaction (e.g., one-to-many); sixth, the message (e.g., content) (Meier & Reinecke, 2020). Whereas the first four levels are focused on the channel, the last two on the type of communication. To measure social media use for consumption of specific news, I here employ both the channel and the communication perspective. First, I will analyze how using the most prominent branded applications affect well-being, and whether this effect changes across applications. The branded applications investigated here are Facebook, Twitter, Instagram, WhatsApp,

73 and YouTube. But because adopting only this position would be both too narrow and too

74 general, I will secondly also investigate how different types of interaction affect well-being.

75 Specifically, I will differentiate between active and passive use. I will distinguish (a) *reading*

76 COVID-19 related social media use (passive), *posting* content regarding COVID (active),

77 and *liking and sharing* COVID-19 related posts by others (both active and passive). Worth

78 noting, this study is not about *general* social media during times of COVID, but on social

79 media use focused on COVID-19 related news and interactions. For example, posting

80 about the pandemic or retweeting COVID-19 related posts.

Effects of Social Media on Well-Being

82 In their study on the relations between media use and mental health during the

83 pandemic, Bendau et al. (2021) found that people who used social media as a primary

84 source of information reported on average "significantly more unspecific anxiety and

85 depression [] and significantly more specific COVID-19 related anxiety symptoms" (p. 288).

86 Hence, this might hint at potential negative effects of social media as news use—but note

87 that this finding comes from a between-person relation stemming from a cross-sectional

88 data. Hence, we don't know whether the differences in mental health and well-being are

89 due to social media use or other third variables, such as age or education.

90 In general, so far there is only little research on news-related social media use and

91 well-being. On the other hand, the question of whether and how *general* social media

92 use—or other, more specific types such as active or passive use—affect well-being is

93 well-researched. A meta review (i.e., an analysis of meta-analyses), found that the relation

94 between social media use and well-being is likely in the negative spectrum—but very small,

95 potentially too small to matter (Meier & Reinecke, 2020). This is well aligned with several

96 new studies that employed the most advanced methods (Kerasteš & Štulhofer, 2020;

97 Orben, Dienlin, & Przybylski, 2019; Przybylski, Nguyen, Law, & Weinstein, 2021; Schemer,

98 Masur, Geiß, Müller, & Schäfer, 2021). For example, Beyens, Pouwels, Driel, Keijsers, and

Valkenburg (2020) reported that although for some users (roughly one quarter) the effects were negative, for almost the same amount of users the effects were positive, while for the majority they were neutral. At all events, in general most effects are somewhere between trivial and small.

What determines a trivial or a small effect is a difficult question (see below). If we refer to standardized effect sizes, according to Cohen (Cohen, 1992) small effect sizes start at $r = .10$. However, most meta-analyses find effect sizes below that threshold (Ferguson et al., 2021; Huang, 2017; Meier & Reinecke, 2020). As a result, I think it's most convincing to expect trivial to small effects also in the case of COVID-19 related social media use.

From a theoretical perspective, why could COVID-19 related social media use be detrimental? Above everything, one can reasonably assume a *direct* negative effect on well-being, especially on positive or negative effect. Dangers, inequalities, corruption – these were the headlines across many countries worldwide. If one learns about such things, the initial reaction might be shock, fear, or dismay. Repeatedly consuming such news might be depressing, perhaps even changing some general perspective on life. So, just like being hit by a hammer hurts and we don't need any "mediating mechanism," this could be the case here as well.

That said, because not all news were negative, because many people showed solidarity and compassion, there were also positive and potentially uplifting news. However, in light of a worldwide pandemic with millions of deaths, the negative direct effect seems more plausible.

There could be also indirect effects. When doomscrolling, users are captivated to such an extent that they cannot stop using social media. For example, during the pandemic social media uses was at an all-time high in the US (Statista, 2021). As has been expressed by many before, it is most likely that moderate social media use is not detrimental (Orben, 2020). However, overuse is more critical, and several studies have showed more pronounced negative effects for extreme users (Przybylski & Weinstein, 2017).

So if a society collectively overuses social media, there is potential for negative effects. Overuse likely impairs well-being if it replaces other meaningful or functional activities such as meeting others, working, actively relaxing, or exercising. If overuse replaces such activities it's reasonable to assume that it's also detrimental.

On the other hand, one can make the case that overuse can be beneficial *in times of a pandemic*, even if it's mainly COVID-19 related. Exchanging COVID-19 related messages with friends via WhatsApp might replace the in-person contact one would have otherwise, but which is logically impossible at that time. At times where meaningful and functional activities are explicitly or implicitly prohibited, using social media to exchange about COVID-19 might not be the worst idea. In fact, given that nowadays a large number of experts, scientists, and politicians converse directly on social media, one can get first-hand information on the current developments. On the other hand, there is of course also much disinformation, and "bingeing" on COVID-19 fake news might also pose risks for impaired well-being.

Together, the strongest argument seems to be that the effects of social media on well-being are, on average, generally small at best. Because this study looks at only *one part* of social media use—namely, COVID-19 related interactions—it is even more focused, and the overall potential of the effects should diminish even further. Whether or not using social media for COVID-19 related aspects is detrimental during a pandemic is also not entirely clear. Therefore, I expect to find that the effects of COVID-19 related news use on well-being will be not be meaningful or practically relevant.

Hypothesis: The within-person effects of all types of COVID-19 related social media use on all types of well-being indicators—while controlling for several stable and varying covariates such as sociodemographic variables and psychological dispositions—will be trivial.

₁₅₁   **Current Study**

₁₅₂ **Smallest Effect Size of Interest**

₁₅₃   Testing this hypothesis, however, is not trivial. First, in contrast to most hypothesis

₁₅₄ typically posited in the social sciences, it explicates an effect size, a so-called smallest effect

₁₅₅ size of interest (SESOI). Effectively testing this hypothesis hence necessitates to define

₁₅₆ what's considered a "trivial effect size" and what's not. Above I referred to standardized

₁₅₇ effect sizes. However, standardized effect sizes should only be a first step toward evaluating

₁₅₈ an effect's relevance (Baguley, 2009). Standardized effect sizes are determined by a

₁₅₉ sample's variance.[1] However, this is problematic: The question of whether or not social

₁₆₀ media use affects me personally in a relevant way should not depend on the variance in the

₁₆₁ sample in which my data were collected. Instead, it should depend on absolute criteria.

₁₆₂ What could be a minimally interesting, a nontrivial effect? I suggest the following SESOI

₁₆₃ for this research question:

₁₆₄   If a heavy user of COVID-19 related social media news stops using social media

₁₆₅ altogether, this should have a noticeable impact on their overall well-being.

₁₆₆   What would this mean practically and how could it be operationalized? In this study,

₁₆₇ COVID-19 related social media use was measured on a 5-point scale, ranging from 1 =

₁₆₈ *never* to 5 = *several times a day.* Thus, the example from above would imply that a

₁₆₉ change of five units in social media use should correspond to a noticeable change in

₁₇₀ well-being. But what's a noticeable change in well-being? According to Norman, Sloan,

₁₇₁ and Wyrwich (2003), people can reliably differentiate between *seven* levels of satisfaction

₁₇₂ with health. So we could state that a five unit change in social media use should result in a

₁₇₃ one unit change in satisfaction. Statistically, in a regression, b estimates the change in the

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[1] Consider the effect size Cohen's d: The mean's of the two groups that are to be compared are subtracted from one another and then divided by the sample's standard deviation (Cohen, 1992). Hence, if there is more deviation/variance in a sample, the effect size decreases, even if the difference of the group's means stays the same.

dependent variable if the independent variable increases by one point. Transferred to our example, we would hence expect a b of $1 / 5 = .20$.

In this study, life satisfaction was measured with 11 units, hence more than people can reliably detect. Hence, we would expect a $11 / 7 * .20$ change, that is a b of at least .31. For affect, which was measured on a 5-point scale, our SESOI would be $5 / 7 * .2 = .14$. In order not to exaggerate precision, these numbers will be rounded. Plus, because we are agnostic as to whether the effects are positively or negatively nontrivial, this leads to an indifference region ranging from $b = -.30$ to $b = .30$ (and $b = -.15$ to $b = .15$ for affect).

Causality

The hypothesis explicitly states a causal effect. In non-experimental designs, causality can be approximated using longitudinal designs. Using longitudinal designs alone, however, is not sufficient for correct causal statements. In addition, it is crucial also to control for third variables, and importantly also for *varying* third variables.

For example, imagine that a person suddenly starts using social media much more than usual, and then at the same time also become less satisfied with their lives. After some time, use recedes again, whereas life satisfaction returns to prior levels. If this happened to several people at the same time, in a longitudinal study we could then find a causal effect of social media use on life satisfaction. However, it could also be the case that during the study there was a major exogenous event (say, a pandemic) and a large part of the working population lost their jobs. Hence, the causal effect reported above was confounded, because in reality it was the pandemic that caused both social media use to rise and their life satisfaction to plummet.

Thus, only if we can control for *all* potential varying third variables, we can correctly estimate causality without any bias (Rohrer, 2018). Obviously, we can never be entirely sure to have included all varying covariates, which makes absolute statements regarding causality impossible. However, when controlling for many varying variables, we can be

much more certain that we measured causality correctly. The aim should still be to collect
as many relevant varying and nonvarying third variables as possible, while knowing that
absolute certainty cannot be reached.

When looking for suitable control candidates, ideally we find variables that affect
both media use and well-being, because controlling for these factors will isolate the actual
effect of social media use on well-being. We can also control for variables that affect *only*
well-being. However, in doing so nothing is gained or lost because the effects of social media
use would remain exactly the same (Kline, 2016). Crucially, when determining the general
causal effect of social media use we should *not* control for mediating variables (Rohrer,
2018). Doing so would bias and distort our assessment of the role of social media use.

In this study, I will hence control for the following variables, which either have been
shown or a likely to affect both social media use and well-being. (I'll additionally include
variables that likely affect only well-being, also to obtain a comparison benchmark for
social media effects): Gender, age, education, Austria country of birth, Austria country of
birth of parents, text-based news consumption, video-based news consumption, residency
Vienna, household size, health, living space, access to garden, access to balcony,
employment, work hours per week, being in home-office, household income, outdoor
activities, satisfaction with democracy, disposition to take risks, and locus of control. I will
not control for variables such as trust in institutions or media, because these variables are
likely influenced by social media use.

Next to including covariates, it is now increasingly understood that causal effects
need to be analyzed from an internal, within-person perspective. If a specific person
changes their media diet, we need to measure how this affects *them.* Between person
comparison from cross-sectional data, where participants are interviewed only once, cannot
provide such data. In this study, I will hence differentiate between within-person effects
and between-person relations.

One precondition of causality is temporal order. The cause needs to precede the

²²⁷ outcome. To this end, finding the right interval when causes and effects are measured is

²²⁸ crucial. For example, if we want to measure the effect of alcohol consumption on driving

²²⁹ performance, it makes a big difference if driving performance is measure one minute, one

²³⁰ hour, one day, or one week after consumption. Finding the right interval is difficult. If

²³¹ variables are more stable, longer intervals are needed, and if they fluctuate a lot, shorter

²³² intervals are necessary. In the case of well-being, to analyze affect we need shorter intervals,

²³³ while for life satisfaction longer ones. Still, finding the right interval is challenging, because

²³⁴ especially short intervals are practically hard to implement and require experience

²³⁵ sampling measures (also known as in situ measurement or ambulant assessment).

²³⁶ In this study, I will adopt an intermediate perspective. I will analyze if, when a

²³⁷ person changes their social media diet, are there simultaneous changes in well-being?

²³⁸ When additionally controlling for both stable and varying covariates, we can then be more

²³⁹ sure that the effect is indeed causal. This approach was implemented already by several

²⁴⁰ other studies and is considered one of the best practices to analyze causality.

Method

²⁴² This section describes the preregistration and how I determined the sample size, data

²⁴³ exclusions, the analyses, and all measures in the study.

Preregistration

²⁴⁵ The hypotheses, the sample, the measures, the analyses, and the inference criteria

²⁴⁶ (SESOI, p-value) were preregistered on the Open Science Framework. The (anonymous)

²⁴⁷ preregistration can be accessed here:

²⁴⁸ https://osf.io/87b24/?view_only=b2289b6fec214fa88ee75a18d45c18f3. Because in this

²⁴⁹ study I analyze data from an already existing large scale data set, the Austrian Corona

²⁵⁰ Panel Project, all of these steps were preregistered prior to accessing the data. The

²⁵¹ preregistration was designed on the basis of the panel documentation online (Kittel et al.,

²⁵² 2020). In some cases the analyses could not be executed as originally planned, because

253 some properties of the variables only became apparent upon data analysis. The most

254 relevant deviations are reported below, and a complete list of all changes can be found

255 online.

Sample

257   The data come from the Austrian Corona Panel Project (Kittel et al., 2021). The

258 study was conducted between March 2020 and October 2021. The contains 26 waves, and

259 at the time of writing, the first 24 waves were available for download. Each wave consists of

260 at least 1,500 respondents. The overall sample size was $N = 3018$, and 72432 observations

261 were collected. Panel mortality was compensated through a continuous reacquistion of new

262 participants. All respondents needed to have access to the internet (via computer or mobile

263 devices such as smartphones or tablets). They were sampled from a pre-existing online

264 access panel provided by Marketagent, Austria. Respondents were asked and incentivized

265 with 180 credit points to participate in each wave of the panel.

266   The sample was representative of the Austrian population in terms of age, gender,

267 region/state, municipality size, and educational level. In order to participate in the study,

268 the respondents needed to be Austrian residents and had to be at least 14 years old. The

269 average age was 42 years, 49 percent were male, 13.60 percent had a University degree, and

270 5.07 percent were currently unemployed.

271   Because the data were analyzed post hoc, no sample size planning on the basis of a

272 priori power analyses was conducted. The sample is very large, and it is hence

273 well-equipped reliably to detect also small effects, which is why no exact post hoc power

274 analysis were conducted. In addition, because such large samples easily generate significant

275 p-values even for very small effects, this study builds on a smallest effect size of interest.

276 To test the hypotheses, I will use the interval testing approach as proposed by Dienes

277 (2014). On the basis of the SESOI, I will define a null region. For well-being, the null

278 region will be between $b = -.30$ and $b = .30$. For example, if the 95% confidence interval

279 lies completely within the null-region (e.g., $b = .15$, [95% CI: .05, .25]), the hypothesis that

280 the effect is only trivial is be supported. If the effects interval and the null region overlap

281 (e.g., $b = .25$, [95% CI: .15, .35]), the hypothesis is not supported and the results are

282 considered inconclusive, whereas a meaningful negative effect is rejected. If the confidence

283 falls completely outside the null-region (e.g., $b = .4$, [95% CI: .35, .45]), the hypothesis is

284 rejected and the existence of a meaningful positive effect is supported.

285   Respondents who answered less than 50% of all questions were removed. The

286 remaining missing responses were imputed using predictive mean matching.

Data Analysis

288   The hypothesis was analyzed using mixed effects models, namely random effect

289 within-between models (REWB)(Bell, Fairbrother, & Jones, 2019). Three models were run,

290 one for each dependent variable. All predictors—that is, social media activities and social

291 media channels, within and between-person factors, stably and varying covariates—were

292 included in each of the three models. The data were hierarchical, and responses were

293 nested in participants and waves. Nesting in participants allowed to separate within-person

294 effects from between-person relations. Nesting in waves allowed to control for general

295 developments, such as general decrease in well-being in the population, for example due to

296 lockdown measures (hence, there was no need additionally to control for specific phases or

297 measures of the lockdown).

298   The factorial validity of the scales were tested with confirmatory factor analyses

299 (CFA). If Mardia's test showed that the assumption of multivariate normality was violated,

300 the more robust Satorra-Bentler scaled and mean-adjusted test statistic (MLM) was used

301 as estimator. To avoid overfitting, scales were tested on more liberal fit criteria (CFI > .90,

302 TLI > .90, RMSEA <. .10, SRMR < .10) (Kline, 2016). Because REWB-models cannot

303 model scales as latent variables, to increase precision factor scores exported from the CFAs

304 were used. For more information on the analyses, see companion website.

Measures

In what follows, I briefly list all variables that were collected. For a complete list of all items and item characteristics, see companion website.

Well-being. Life satisfaction was measured with the item "Taken everything together, how satisfied are you currently with your life?" The response options ranged from 0 (*extremely unsatisfied*) to 10 (*extremely satisfied*). The variable's average across all waves was $M = 6.59$, ranging from $M_{\min} = 6.39$ to $M_{\max} = 6.79$. The average standard deviation was $SD = 1.68$.

To capture positive affect, respondents were asked how often in the last week they felt (a) calm and relaxed, (b) happy, and (c) full of energy. The response options were 1 (*never*), 2 (*on some days*), 3 (*several times per week*), 4 (almost every day), and 5 (daily). The variable's average across all waves was $M = 3.15$, ranging from $M_{\min} = 3.05$ to $M_{\max} = 3.29$. The average standard deviation was $SD = 0.57$.

For negative affect, respondents were asked how often in the last week they felt (a) lonely, (b) aggravated, (c) so depressed, that nothing could lift you up, (d) very nervous, (e) anxious, and (h) glum and sad. The response options were 1 (*never*), 2 (*on some days*), 3 (*several times per week*), 4 (almost every day), and 5 (daily). The variable's average across all waves was $M = 1.73$, ranging from $M_{\min} = 1.66$ to $M_{\max} = 1.81$. The average standard deviation was $SD = 0.39$.

All three items were measured on each wave.

COVID-19 related social media use. The COVID-19 related social media use focused on interaction was measured with three variables (a) reading, (b) liking and sharing, and (c) posting. The general introductory question was "How often during the last week have you engaged in the following activities on social media?" The three items read as follows: "Reading the posts of others with content on the Coronavirus"; "When seeing Posts on the Coronavirus, I clicked 'like,' 'share' or 'retweet' "; "I myself wrote posts on the Coronavirus on Social Media." Answer options were 1 (*several times per day*), 2 (*daily*), 3

(*several times per week*), 4 (*weekly*), 5 (*never*). The items were inverted for the analyses. The variable's average across all waves was $M = 6.59$, ranging from $M_\text{min} = 6.39$ to $M_\text{max} = 6.79$. The average standard deviation was $SD = 1.68$.

The COVID-19 related social media use focused on channels was measured with five variables. The general introductory question was "How often in the last week have you followed information related to the Corona-crisis in the following social media?" The five items were (a) Facebook, (b) Twitter, (c) Instagram, (d) Youtube, (e) WhatsApp. Again, the answer options were 1 (*several times per day*), 2 (*daily*), 3 (*several times per week*), 4 (*weekly*), 5 (*never*). Again, the items were inverted for the analyses.

All items were measured every second wave. However, freshly recruited respondents always answered these questions.

Control variables. The effects of COVID-19 related social media use were controlled for the following stable variables: (a) gender (answer options: female, male, diverse), (b) age, (c) education (10 ordinal options), (d) Austria country of birth (yes/no), (e) Austria parents' country of birth (no parent, one parent, both parents). We controlled also for the following varying covariates: (a) text-based media news consumption, (b) video-based media news consumption, (c) residency is Vienna, (d) household size, (e) Self-reported physical health, (f) Living space (in squaremeter), (g) access to balcony, (h) access to garden, (i) Employment status, (j) Work hours per week, (k) Home office, (l) household income, (m) outdoor activities, (o) satisfaction with democracy, (p) disposition to take risks, (q) locus of control.

Results

Preregistered Analyses

When looking at the variables from a descriptive perspective, we see that all well-being measures did not change substantially across the different phases of the pandemic. COVID-19 related media use, however, increased during the beginning of the

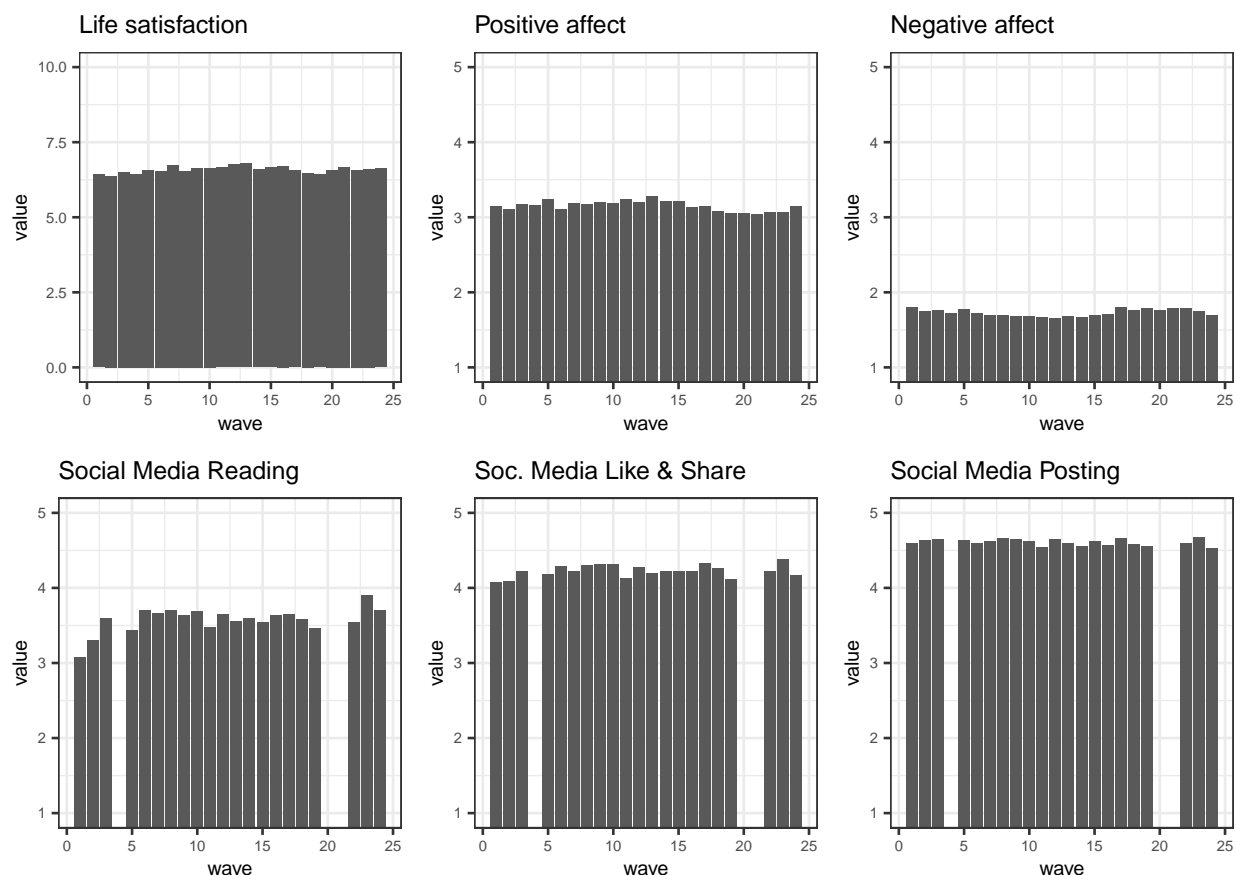358 study and remained stable after approximately six waves.

Figure 1. Development of well-being and media use measures across the pandemic. Values obtained from mixed effect models, with participants and waves as grouping factor and without additional predictors.

359   The study's hypothesis was that the effects of all types of social media use on

360 well-being will be trivial. Regarding the different types of *communication*, that is reading

361 vs. sharing vs. posting, all within-person effects fell completely within the a-priori defined

362 SESOIs. For example, respondents who used social media more frequently than usual to

363 read about COVID-19 related topics did not show a simultaneous change in life satisfaction

364 (b = -0.03 [95% CI -0.09, 0.02]). Only one effect did not include zero. If people posted

365 more about COVID-19 related aspects than they usually did, life satisfaction increased (b

366 = 0.11 [95% CI 0.01, 0.2]). However, because the effect did not fall outside our null region,

367 it's likely not large enough to be considered meaningful. As a result, the hypothesis was

368 supported for all types of COVID-19 related social media communication.

369   Regarding between-person relations, about which no hypotheses were formulated,

370 only two effects didn't include zero. First, respondents who across all waves used social

371 media more frequently than others to read about COVID-19 reported higher levels of

372 positive affect than others ($b = 0.05$ [95% CI 0.01, 0.09]). However, note that this effect

373 still fell completely within the null-region. Hence, although positive the effect was

374 considered too small to matter practically. Second, respondents who across all waves used

375 social media more frequently than others to read about COVID-19 reported lower levels of

376 negative affect than others ($b = 0.01$ [95% CI -0.02, 0.04]). The effect was partially outside

377 the null region, hence the effect might be large enough to be practically relevant, and the

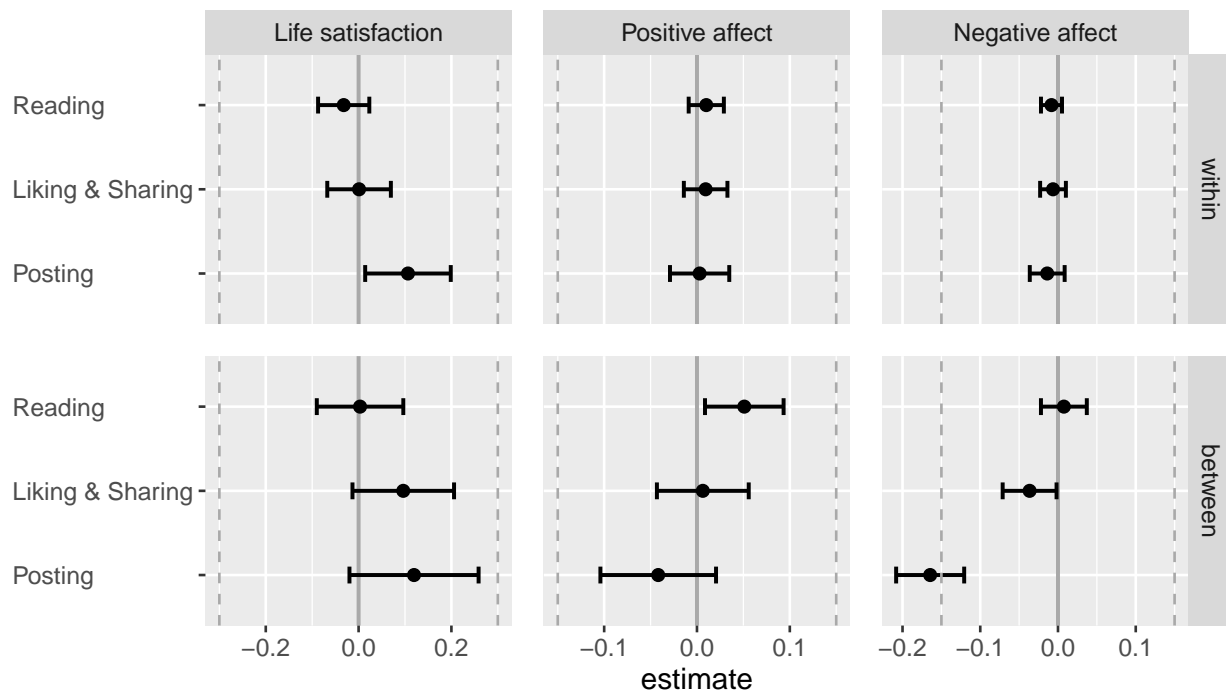378 effect that it's trivial cannot be rejected.

Figure 2. The effects of various types of social media use on three indicators of well-being. Effects are controlled for a large number of covariates (see text). The SESOI was 0.30 for life satisfaction and .15 for affects; hence, no effect is considered meaningful theoretically.

379   Regarding the COVID-19 related use of social media *channels*, the results were

380 virtually the same. Changes in the frequency of using different social media channels to

381 attain information regarding COVID-19 were unrelated to meaningful changes in

382 well-being. For example, respondents who used Facebook more frequently than usual to

383 learn about COVID-19 did not show a simultaneous change in well-being ($b = 0.03$ [95%

384 CI -0.03, 0.1]). Only one effect was substantially larger than zero. Respondents who used

385 Instagram more frequently than usual to attain COVID-19 related news reported slightly

386 lowers simultaneous levels of life satisfaction then usual ($b = $ -0.08 [95% CI -0.15, $>$ -0.01]).

387 However, this effect was still completely within the null region, hence not large enough to

388 be considered meaningful. In sum, the hypothesis was supported for the COVID-19 related

389 use of all types of social media channels.

390   In terms of between-person relations—which, again, weren't included in the

391 hypotheses—no relations crossed or fell outside the null region. However, some relations

392 did not included zero. For example, respondents who across all waves used Instagram more

393 frequently than others for COVID-19 related reasons reported lower levels of life

394 satisfaction ($b = $ -0.11 [95% CI -0.19, -0.03]). Respondents who were more active on

395 WhatsApp compared to others reported slightly lower levels of positive affect ($b = $ -0.03

396 [95% CI -0.07, $>$ -0.01]). Respondents who compared to others were more active on

397 Twitter and YouTube reported lover levels of negative affect (Twitter: $b = $ -0.04 [95% CI

398 -0.07, $>$ -0.01]; YouTube $b = $ -0.06 [95% CI -0.08, -0.03] . However, please note that all

399 these affect not considered large enough to be practically relevant.

400 **Exploratory Analyses**

401   [Report results of some control variables.]

402   **Discussion**

403   Using a panel study with a representative sample of the Austrian population that

404 consistent of 24 waves, this study analyzed the effects of COVID-19 related social media
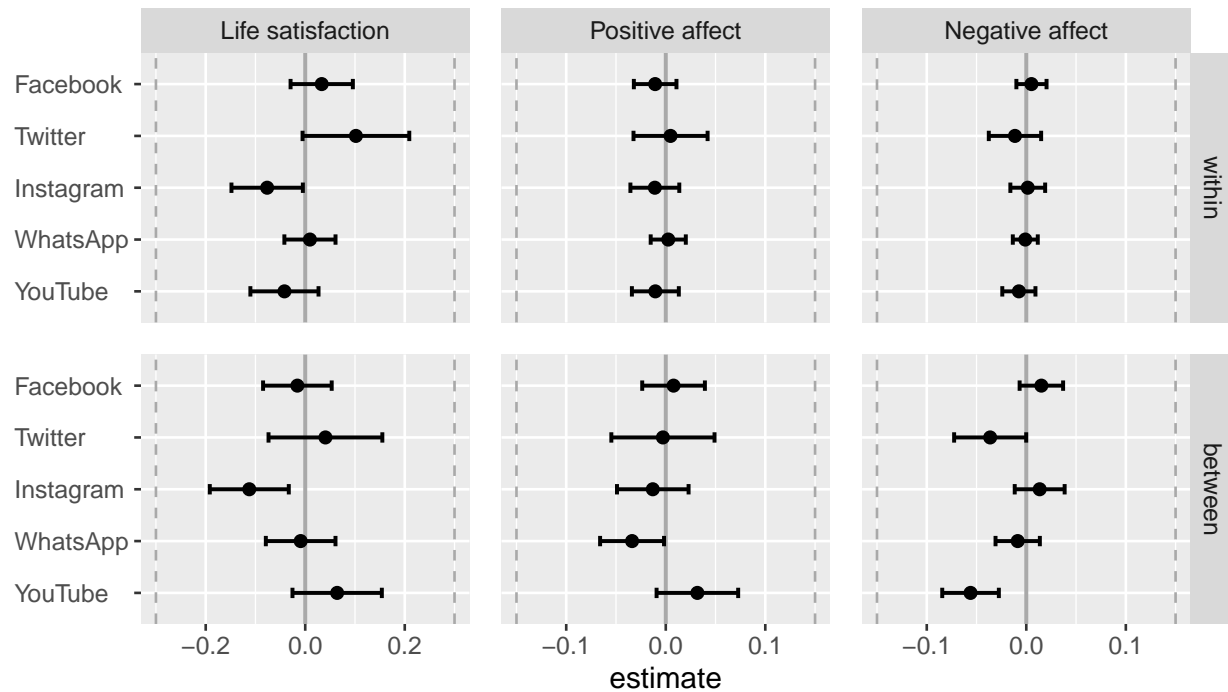
Figure 3. The effects of using various social media applications on three indicators of well-being. Effects are controlled for a large number of covariates (see text). The SESOI was 0.30 for life satisfaction and .15 for affects; hence, no effect is considered meaningful theoretically.

use on well-being. A random effect model that separated between person relations from within-person effects and that controlled for several third variables showed that within-person effects were trivial. People who used social media more than usual to learn about COVID-19 did not show changes in their well-being. As a result, the results imply that COVID-19 related social media use is irrelevant for people's well-being. Other factors among the third variables that were measured revealed much larger effects, implying that the relevance of specific types of social media use for well-being is limited. Popular fears that "doomscrolling" or overusing social media during times of crises might not be justified.

The study is not aligned with a recent cross-sectional study by Bendau et al. (2021) that showed negative relations between social media and well-being. However, Bendau et al. (2021) analyzed cross-sectional data on a between-person level, while not controlling for third variables, which does not allow to make causal inferences. At the same time, this

study is well-aligned with recent studies and meta-analyses from related research questions, which found that the effects of various types of social media use on several well-being indicators is small at best, often too small to matter (Ferguson et al., 2021; Meier & Reinecke, 2020; Orben, 2020).

Limitations

The current study analyzed whether changes in media use were related with changes in well-being, while controlling for several potential confounds. Together, this allows for a good perspective on potential causality. That said, causality necessitates temporal order, and the cause needs to precede the consequence. Regarding media use, such effects often happen immediately or shortly after use, necessitating intervals in the hours, minutes, or even seconds. Only experience sampling studies that ask users in the very moment can produce such knowledge. However, even then we don't know for certain if we actually measured the right interval. Hence, to document how effects unfold it needs future research employing different study designs with different time lags.

The predefined SESOI of $b = .30$ was potentially too large. Media use is only one aspect of multiple factors that simultaneously affect well-being. Is it realistic that extremely changing only *one* of these aspect (e.g., by completely stopping the use of social media) should already manifest in a detectable change in well-being? Or would it make more sense to say, if you regularly start doing *two* activities (e.g. regularly exercising and establishing a reading habit) together should then show in improvements to well-being? In other words, if the beneficial effect of a particular activity is large enough, if people implement *two* of them then they should actually feel a difference. Practically, this would imply a SESOI half as large as I have defined here, that is $b = |.15|$ for well-being and $b = |.075|$. In this case, this would not make a difference, as even with these more liberal thresholds all but one effect would still be completely in the null region. However, at all events future research needs to start a discussion on what effect sizes are considered trivial,

443 and this study is one of the first to provide some concrete guidelines.

444 Both media use and well-being were measured using self-reports. Measuring

445 well-being with self-reports is adequate, because it by definition requires introspection.

446 However, it would be preferable to measure social media use objectively, because people

447 cannot reliably estimate their use. That said, objective measures often cannot capture the

448 content or the motivation of the use, and only very complicated tools that record the

449 content that was used (such as the Screenome project) might produce such data. However,

450 also these procedure introduce other problems, for example related to privacy. Hence, for

451 this type of research question it seems necessary still to use self-reported measures.

452 The generalizability of the results are not large, because the data were collected in a

453 single country. The results are hence potentially limited to the more Western sphere, and

454 might not apply to other cultures, especially if they have a different media landscape or

455 offer alternative social media. That said, because this is a large study, representative of a

456 country's entire population, and because several waves were collected across a large time

457 span, the results should be at least as generalizable as other typical empirical studies

458 collected in the social sciences.

459 Social media use was measured with an ordinal variable, however in the analyses it

460 was treated as a numerical one. If treated as an ordinal one, it would have been necessary

461 to analyze four different contrasts for each media measure, which plus the differentiation

462 between between and within factor would have produced eight different measures, we

463 would have made the model exceedingly complex.

Conclusion

465 In this study, COVID-19 related social media use did not causally affect several

466 indicators of well-being, including life satisfaction, positive affect, and negative affect.

467 However, factors other than social media use did affect well-being, such as income levels or

468 access to a balcony. If it's the aim to improve well-being, it might hence be fruitful not to

focus on social media but to address other, potentially more pressing societal problems

related to inequality.

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Competing Interests

I declare no competing interests.

Supplementary Material

All the stimuli, presentation materials, analysis scripts, and a reproducible version of the manuscript can be found on the companion website (https://xmtra.github.io/Austrian_Corona_Panel_Project/index.html).

Data Accessibility Statement

The data are shared on AUSSDA, see https://doi.org/10.11587/28KQNS, and can be used for scientific purposes only.