1   Analyzing the effects of COVID-19 related social media use on well-being

Abstract

In times of crisis such as the Corona pandemic citizens need to stay informed about recent events, political decisions, or mandatory protection measures. To this end, many people use various types of media, and increasingly social media. However, because social media are particularly engaging, some find it hard to disconnect. In this preregistered study, I investigated whether using social media for COVID-19 related reasons affected psychological well-being. To answer this question I analyzed data from the Austrian Corona Panel Project, which consists of 3,018 participants. Well-being was measured at all 24 waves, and communication at five specific waves. I ran three random effects within between models, controlling for several stable and varying confounders. Results showed that the effects of COVID-19 related social media use on well-being were very small, arguably too small to matter. Fears that social media use during times of crisis critically impairs well-being are not supported.

Keywords: COVID-19, well-being, social media, news use, panel study.

¹⁶   Analyzing the effects of COVID-19 related social media use on well-being

¹⁷   During the COVID-19 pandemic, numerous events unfolded in quick succession and

¹⁸ several open questions emerged. How dangerous is the virus? Is it spreading in my region?

¹⁹ How is it transmitted, and how can I protect myself? Because for many it was (and at the

²⁰ time of writing still is) a matter of life or death, people aimed to stay informed regarding

²¹ the latest developments. Governments around the world implemented safety measures,

²² such as wearing masks, keeping physical distance, or enforcing lockdowns. In this

²³ extraordinary situation, many people heavily relied on media to attain relevant

²⁴ information, and especially social media were at an all time high (Statista, 2021).

²⁵   Some people actually couldn't stop using social media to learn about COVID-19

²⁶ related news. A new phenomenon emerged, termed "doomscrolling": Users were glued to

²⁷ their screens and found it hard to pursue other relevant activities such as working, taking a

²⁸ break, or looking after their children (Klein, 2021). It was asked whether using social

²⁹ media for COVID-19 related reasons is helpful, or whether it creates an additional burden

³⁰ on mental health (Sandstrom, Buchanan, Aknin, & Lotun, 2021). These concerns seem

³¹ justified: A study with 6,233 people from Germany conducted during the pandemic found

³² that "[f]requency, duration and diversity of media exposure were positively associated with

³³ more symptoms of depression" (Bendau et al., 2021, p. 283).

³⁴   As a result, with this study I want to build on this research and investigate whether

³⁵ or not COVID-19 related social media use affected well-being during the pandemic. To this

³⁶ end, I analyzed a large-scale panel study from the Austrian Corona Panel Project (Kittel et

³⁷ al., 2020). The panel consists of 24 waves and has an overall sample size of 3,018

³⁸ participants. The panel study collected a large number of psychological and demographic

³⁹ variables. I explicitly aimed to investigate the causal effects of COVID-19 related social

⁴⁰ media use on well-being.

Understanding Well-being and Media Use

The underlying theories that guided the selection of variables for my analysis are the two-continua model of mental health (Greenspoon & Saklofske, 2001) and the hierarchical taxonomy of computer-mediated communication (Meier & Reinecke, 2020). According to the two-continua model, mental health consists of (a) psychopathology and (b) 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 happiness and joy, psychological well-being addresses eudaimonic aspects such as fulfillment and meaning. Subjective well-being is primarily about achieving positive affect and avoiding negative affect. One of the most prominent indicators of well-being is life satisfaction. In my view, because it represents a general appraisal of one's life, life satisfaction is best thought of as a meta concept that combines psychological and subjective well-being. Notably, life satisfaction is stable and fluctuates only little, whereas it's the exact opposite for affect (Dienlin & Johannes, 2020). To capture well-being in this study I thus build on life satisfaction, positive affect, and negative affect. Together, this should provide an encompassing perspective on potential 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); and sixth, the message (e.g., content) (Meier & Reinecke, 2020). Whereas the first four levels focus on the communication *channel*, the last two address the communication *type*. To measure social media use for the consumption of COVID-19 related news and topics, I here employ both the channel and the type of communication perspective, which together should offer a nuanced understanding of communication.

First, I investigate how well-being is affected by different types of communication affect, namely active and passive use. Defining what constitutes active and what passive

68 use is not always clear, and different understandings are currently discussed (Ellison, Triệû,

69 Schoenebeck, Brewer, & Israni, 2020; Meier & Reinecke, 2020). Reading is generally

70 considered as passive and writing as active, while there are also specific behaviors such as

71 such liking or sharing content that fall somewhere in-between (Meier & Krause, 2022). In

72 this study, I hence distinguish (a) *reading* (passive), (b) *posting* (active), and (c) *liking and*

73 *sharing* COVID-19 related posts (both active and passive). Second, I analyze how using

74 the most prominent branded applications affects well-being, and whether this effect

75 changes across applications. Branded apps are separate entities with potentially divergent

76 effects. Twitter might have a different effect as compared to WhatsApp because of their

77 respective affordances. For example, Waterloo, Baumgartner, Peter, and Valkenburg (2018)

78 found that it's more adequate to express negative emotions on WhatsApp than on Twitter

79 or on Instagram. The branded applications investigated here are Facebook, Twitter,

80 Instagram, WhatsApp, and YouTube. Worth noting, this study is not about *general* social

81 media use during times of COVID, but on social media use *focused* on COVID-19 related

82 content. Examples of such media use include posting thoughts about the pandemic or

83 retweeting COVID-19 related news.

Theorizing Social Media Effects on Well-Being

85   From a theoretical perspective, how could we explain whether COVID-19 related

86 social media use affects well-being? According to the set-point model of subjective

87 well-being, well-being is surprisingly stable (Lykken, 1999). Although specific events such

88 as marriage or salary can have significant effects, after some time well-being routinely

89 returns to prior levels, which are mostly determined genetically (Sheldon & Lucas, 2014).

90 Only very specific events and factors such as unemployment, disability, or death can cause

91 long-term changes in well-being (Lucas, 2007).

92   Can media use be such a factor? In advance, there doesn't seem to be a clear winner,

93 and it's likely that both positive and negative effects cancel each other out. Empirically,

social media use on average oes not have a strong effect on well-being (Meier & Reinecke, 2020). According to the Differential Susceptibility to Media Effects Model (Valkenburg & Peter, 2013), the effects of media use differ across individuals. Whereas for some media are beneficial, for others they are harmful. On average, however, effects are often small or negligible. But also when focusing on individual users, social media can have both positive and negative effects on well-being (Büchi, 2021). They can impair well-being when causing embarrassment, stress, or disinformation, and they can improve well-being when providing connectedness, information, or entertainment (Büchi, 2021).

Two of the most prominent media effect theories argue against strong negative impacts. First, uses and gratifications theory states that people explicitly and rationally chose specific media because of their respective benefits (Katz, Blumler, & Gurevitch, 1973). If those benefits don't exist, they will spend their time elsewhere. And social media offer copiuos benefits. Most prominently, they help find relevant information, maintain and foster relationships, express one's personality, and entertain oneself (Pelletier, Krallman, Adams, & Hancock, 2020). The second major theory that needs to be listed here is mood management theory (Zillmann, 1988). Users implicitly learn what media help them balance their mood and affect. For example, when bored people use social media to entertain themselves. Using experience sampling of well-being and logs of social media use, a study with 82 participants from Italy found that after episodes of social media use, levels of positive affect increased significantly (Marciano, Driver, Schulz, & Camerini, 2022).

But people can also misjudge media affects and are often overly optimistic (Metzger & Suh, 2017). And precisely because social media have so many positive consequences, one can ask if this is not where the actual problem lies. In other words, social media aren't problematic because they're inherently bad, but rather because they're too good. And as with many other things, there can be too much of a good thing. It is therefore often asked whether social media are addictive, and users sometimes express this fear themselves (Yang, Griffiths, Yan, & Xu, 2021). However, a recently published meta-analysis found

that the two most prominent measures of addiction, the Bergen Facebook Addiction Scale and the Bergen Social Media Addiction Scale, have only small relations to well-being (Duradoni, Innocenti, & Guazzini, 2020). In addition, the general idea of labeling excessive social and new media use as addiction was criticized, arguing that social media represent new regular behaviors that should not be pathologized (Galer, 2018; van Rooij et al., 2018).

Because media effects can differ across situations (Valkenburg & Peter, 2013), I now briefly focus on the effects of COVID-19 related social media use specifically. First, one could assume a *direct* negative effect on well-being, and especially on positive or negative affect, which are more volatile and fluctuating. Dangers, inequalities, corruption—these were the headlines during the pandemic across many countries worldwide. If one learns about such events, the initial reaction might be shock, fear, or dismay. Consuming such news can be depressing (Dörnemann, Boenisch, Schommer, Winkelhorst, & Wingen, 2021), perhaps even changing some general perspectives on life. That said, because not all news was negative, and because many people showed solidarity and compassion, there was also positive and uplifting content, potentially compensating for the negative effects (Dörnemann et al., 2021). A study with 2.057 respondents from Italy reported that during the pandemic virtual community and social connectedness even increased during the pandemic (Guazzini, Pesce, Marotta, & Duradoni, 2022).

There could also be *indirect* effects. When browsing social media for Covid-19 related news, many users reported being captivated to such an extent that they could not stop using social media (Klein, 2021). During the pandemic social media use was at an all-time high in the US (Statista, 2021). Although it is most likely that moderate social media use is not detrimental (Orben, 2020), overuse, however, might be more critical, and several studies have shown more pronounced negative effects for extreme users (Przybylski & Weinstein, 2017). To explain, overuse could impair well-being if it replaces meaningful or functional activities such as meeting others, working, actively relaxing, or exercising. So if a society collectively overuses social media during a pandemic, there might be potential for

negative effects. On the other hand, one can make the case that overuse might even be beneficial, especially in times of a pandemic. Exchanging COVID-19 related messages with friends via WhatsApp might replace the in-person contact one would have otherwise, but which is literally impossible at the time. In situations where meaningful and functional activities are prohibited, using social media to exchange about COVID-19 related topics might not be the worst idea. Besides, given that nowadays a large number of experts, scientists, and politicians converse directly on social media, one can get first-hand high quality information on current developments.

To summarize, from a theoretical perspective it is most likely that the average effects of social media use on well-being are negligible. Building on established theories from Communication, we would not assume that effects are either profoundly negative or strongly positive.

Empirical Studies on Social Media Effects

So far, there is only little empirical research on how well-being is affected by COVID-19 related social media. In their study on the relations between media use and mental health during the pandemic, Bendau et al. (2021) found that people who used social media as a primary source of information reported on average "significantly more unspecific anxiety and depression [] and significantly more specific COVID-19 related anxiety symptoms" (p. 288). Eden, Johnson, Reinecke, and Grady (2020) analyzed the media use of 425 US college students during the first wave of the pandemic, finding both positive and negative relations with well-being. In a sample of 312 respondents collected via Amazon Mechanical Turk, Choi and Choung (2021) reported that people who used media to attain information were more lonely and less satisfied with their lives. Stainback, Hearne, and Trieu (2020) analyzed a large-scale study with 11,537 respondents from the US and found that increased COVID-19 media consumption was related to more psychological distress. A four-wave panel study with 384 young adults from the U.S.

174 analyzed the effects of general digital technology use—objectively measured via screenshots

175 of screen-time applications—on mental health, separating within- and between-person

176 relations (Sewall, Goldstein, & Rosen, 2021). The results showed that digital technology

177 did not have any significant effects on mental health (for a similar study with comparable

178 results, see Bradley & Howard, 2021). Together, the literature is mixed, with a slight focus

179 on the negative effects of social media as news use (see also Dörnemann et al., 2021; Liu &

180 Tong, 2020; Riehm et al., 2020). However, note that all of these findings represent

181 between-person relations stemming from cross-sectional data (see below). We therefore

182 don't know whether the differences in mental health and well-being are due to social media

183 use or due to other third variables, such as age, health, employment, or education.

184   The question of whether and how social media use affects well-being *in general*, on the

185 other hand, is well-researched. This also holds true for the different types of communication

186 such as active or passive use. A meta review (i.e., an analysis of meta-analyses) found that

187 the relation between social media use and well-being is likely in the negative spectrum but

188 very small (Meier & Reinecke, 2020)—potentially too small to matter. What determines

189 whether or not an effect is considered small or trivial? As a starting point, we could refer

190 to standardized effect sizes. According to Cohen (1992), small effect sizes start at $r = .10$.

191 And indeed, several if not most of the current meta-analyses find effect sizes below that

192 threshold (Ferguson et al., 2021; Huang, 2017; Meier & Reinecke, 2020).

193   Finally, also several individual studies employing advanced methods found smalls

194 relations between social media use and well-being (Keresteš & Štulhofer, 2020; Orben,

195 Dienlin, & Przybylski, 2019; Przybylski, Nguyen, Law, & Weinstein, 2021; Schemer, Masur,

196 Geiß, Müller, & Schäfer, 2021). For example, Beyens, Pouwels, van Driel, Keijsers, and

197 Valkenburg (2021) reported that although for some users (roughly one quarter) the effects

198 of social media use on well-being were negative, for almost the same number of users they

199 were positive, while for the rest the effects were neutral. This finding is aligned the

200 Differential Susceptibility to Media Effects Model: Although there is substantial *variation*

of media effects for individual users, the *average* effects reported in the literature are often small (Valkenburg & Peter, 2013).

In conclusion, in light of the theoretical considerations and empirical studies presented above, I expect that COVID-19 related communication on social media doesn't affect well-being in a meaningful or relevant way.

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.

Current Study

Smallest Effect Size of Interest

Testing this hypothesis, however, is not trivial. First, in contrast to most hypotheses typically posited in the social sciences it implicitly contains an effect size, a so-called smallest effect size of interest (SESOI). Effectively testing this hypothesis necessitates defining what's considered a "trivial effect size" and what's not. Above I already referred to standardized effect sizes. However, standardized effect sizes should only be a first step toward evaluating an effect's relevance (Baguley, 2009). Standardized effect sizes are determined by a sample's variance,[1] which is problematic: The question of whether or not social media use affects a particular person in a relevant way should not depend on the variance in the sample in which that person's data were collected. Instead, it should depend on absolute criteria.

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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.

²²² What could be a minimally interesting, nontrivial effect? Because this is a normative

²²³ and ultimately philosophical question, there can never be a clear, single, or unanimous

²²⁴ answer. However, it is still necessary and helpful to try provide such a plausible

²²⁵ benchmark. I therefore suggest the following SESOI for this research question:

²²⁶ SESOI: If a heavy user of COVID-19 related social media news suddenly *stops*

²²⁷ using social media altogether, this should have a *noticeable* impact on their

²²⁸ overall well-being.

²²⁹ What does this mean practically and how can it be operationalized? In this study,

²³⁰ COVID-19 related social media use was measured on a 5-point scale, ranging from 1 =

²³¹ *never* to 5 = *several times a day.* Thus, a change of four units in social media use (e.g., a

²³² complete stop) should correspond to a noticeable change in well-being. But what's a

²³³ noticeable change in well-being? According to Norman, Sloan, and Wyrwich (2003), people

²³⁴ can reliably distinguish *seven* levels of satisfaction with health. So if satisfaction is

²³⁵ measured on a 7-point scale, we would state that a four unit change in social media use

²³⁶ should result in a one unit change in life satisfaction. (For more information, see Methods

²³⁷ section "Inference Criteria.")

Causality

²³⁹ The hypothesis explicitly states a causal effect. In non-experimental studies,

²⁴⁰ longitudinal designs can help investigate causality. Using longitudinal designs alone,

²⁴¹ however, is not sufficient for establishing correct causal statements (Rohrer & Murayama,

²⁴² 2021). In addition, we for example also need to control for confounding third variables, and

²⁴³ importantly also for *varying* third variables.

²⁴⁴ To illustrate, consider the following example. Imagine that a person suddenly starts

²⁴⁵ using social media much more than usual, and then after some time becomes less satisfied

²⁴⁶ with their life. Eventually, use and life satisfaction return to prior levels. If this happens to

²⁴⁷ several people at the same time, in a longitudinal study we could then observe a significant

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), which caused large parts of the working population to loose 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 life satisfaction to go down.

Thus, only when controlling for *all* relevant confounders, we can correctly estimate causality without bias (Rohrer, 2018). Obviously, we can never be entirely sure to have included all confounders, which makes absolute statements regarding causality virtually impossible. In addition, when determining the overall causal effect, we need to make sure *not* to control for mediating variables (Rohrer, 2018), for doing so would bias our assessment of the causal effect. Complicating matters further, it is often unclear if a variable is a mediator or a confounder.[2] However, despite all these caveats, when controlling for relevant variables (that aren't mediators), we can be much more certain that we measured causality correctly. The aim should therefore be to collect as many varying and nonvarying confounders as possible (which I believe is seldom done in our field), while knowing that absolute certainty regarding causality cannot be reached.

When searching for suitable candidates for confounders, we should look for variables that affect both media use and well-being. Controlling for these factors isolates the actual effect of social media use on well-being. We can also control for variables that affect only social media use or well-being. However, in doing so not much is gained or lost, because the effects of social media use would remain virtually the same (Kline, 2016; but see McElreath, 2021).

In this study, I hence plan to control for the following variables, which either have already been shown to affect both social media use and well-being or which are likely to do so, and which also aren't mediators: gender, age, education, Austria country of birth,

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[2] In addition, there also exist colliders, which I don't discuss here and which complicate the issue even further (Rohrer, 2018).

273 Austria country of birth of parents, text-based news consumption, video-based news

274 consumption, residency Vienna, household size, health, living space, access to garden,

275 access to balcony, employment, work hours per week, being in home-office, household

276 income, outdoor activities, satisfaction with democracy, disposition to take risks, and locus

277 of control.[3]

278 Next to including covariates, it's now increasingly understood that causal effects

279 should be analyzed from an internal, within-person perspective (Hamaker, 2014). If a

280 specific person changes their media diet, we need to measure how this behavior affects *their*

281 *own* well-being. Between-person comparisons from cross-sectional data, where participants

282 are interviewed only once, cannot provide such insights. In this study, I hence differentiate

283 between-person relations from within-person effects. And as explicated above, to test the

284 hypothesis I thus consider only the within-person effects.

285 Finally, one precondition of causality is temporal order. The cause needs to precede

286 the effect. Finding the right interval between cause and effect is crucial. For example, if we

287 want to understand the effect of alcohol consumption on driving performance, it makes a

288 big difference if driving performance is measured one minute, one hour, one day, or one

289 week after consumption. If variables are stable, longer intervals are needed; if they

290 fluctuate, shorter intervals. In the case of well-being, we need shorter intervals for affect

291 and longer ones for life satisfaction (Dienlin & Johannes, 2020). Still, choosing the right

292 interval is challenging, because especially short intervals are hard to implement in practice

293 and often require advanced methods such as experience sampling (also known as in situ

294 measurement or ambulant assessment) (Schnauber-Stockmann & Karnowski, 2020).

295 In this study, I hence analyze how using social media during the last week affected

296 their positive and negative affect during the same week. More precisely, if people during

297 the last week used COVID-19 related social media more than the usually do, did they feel

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[3] The dataset includes many other variables that one could also potentially control for, and I invite
interested readers to download the and explore potential interesting relationships.

better or worse during that week than they usually do? Regarding life satisfaction, I implemented a longer interval. Namely, if people during the last week used COVID-19 related social media more than the usually do, were they at the end of the week more or less satisfied with their lives than they usually are? I hence analyze if when a person changes their social media diet, are there (a) *simultaneous* changes in their affect and (b) *subsequent* changes in their life satisfaction? When additionally controlling for both stable and varying confounders, we can then be more sure that the effect is indeed causal. Similar approaches were implemented by other studies (Johannes, Dienlin, Bakhshi, & Przybylski, 2022; Scharkow, Mangold, Stier, & Breuer, 2020), and they are considered a best practice approach toward analyzing causality (Bell, Fairbrother, & Jones, 2019).

Method

In this section I describe the preregistration and how I determined the sample size, data exclusions, the analyses, and all measures in the study.

Preregistration

The hypotheses, the sample, the measures, the analyses, and the inference criteria (SESOI, p-value) were preregistered on the Open Science Framework. The (anonymous) preregistration can be accessed here: https://osf.io/87b24/?view_only=b2289b6fec214fa88ee75a18d45c18f3. Because in this study I analyzed data from an already existing large-scale data set, all of these steps were done prior to accessing the data. The preregistration was designed on the basis of the panel documentation online (Kittel et al., 2020). In some cases I couldn't execute the analyses as I had originally planned, for example because some properties of the variables only became apparent when inspecting the actual data. The most relevant deviations are reported below, and a complete list of all changes can be found in the online companion website (https://XMtRA.github.io/Austrian_Corona_Panel_Project).

Sample

The data come from the Austrian Corona Panel Project (Kittel et al., 2021). The study was conducted between March 2020 and October 2021. It contains 26 waves, and at the time of writing the first 24 waves were available for download. Between March 2020 and July 2020, the intervals were weekly, and afterward the intervals were monthly. Each wave consists of at least 1,500 respondents. The overall sample size was $N = 3{,}018$, and 72,432 observations were collected. Panel mortality was compensated through a continuous acquisition of new participants. All respondents needed to have access to the internet (via computer or mobile devices such as smartphones or tablets). They were sampled from a pre-existing online access panel provided by the company Marketagent, Austria. Respondents were asked and incentivized with 180 credit points to participate in each wave of the panel.

Achieved via quota sampling, the sample matched the Austrian population in terms of age, gender, region/state, municipality size, and educational level. In order to participate in the study, the respondents needed to be Austrian residents and had to be at least 14 years of age. Ethical review and approval was not required for the study in accordance with the local legislation and institutional requirements. The participants provided their written informed consent to participate in this study. The average age was 42 years, 49 percent were male, 14 percent had a University degree, and 5 percent were currently unemployed.

Inference Criteria

Because the data were analyzed post-hoc, no a-priori sample size planning on the basis of power analyses was conducted. The sample is large, and it is hence well-equipped to reliably detect also small effects. In addition, because such large samples easily generate significant p-values even for very small effects, it helps that the hypotheses were tested with a smallest effect size of interest-approach. To this end, I adopted the interval testing approach as proposed by Dienes (2014). On the basis of the SESOI, I then defined a null

349   region. In what follows, I explain how I determined the SESOI and the null region.

350   In this study, life satisfaction was measured on an 11-point scale. If people can

351   reliably differentiate 7 levels as mentioned above, this corresponds to 11 / 7 = 1.57 unit

352   change on an 11-point scale. Hence, a four-point change in media use (e.g., a complete

353   stop) should result in a 1.57-point change in life satisfaction. In a statistical regression

354   analysis, b estimates the change in the dependent variable if the independent variable

355   increases by one point. We would therefore expect a SESOI of $b = 1.57 / 4 = 0.39$. For

356   affect, which was measured on a 5-point scale, our SESOI would be $b = 0.71 / 4 = 0.18$.

357   Because we're agnostic as to whether the effects are positive or negative, the null region

358   includes negative and positive effects. Finally, in order not to exaggerate precision and to

359   be less conservative, these numbers are reduced to nearby thresholds.[4] Together, this leads

360   to a null region ranging from $b = $ -.30 to $b = $ .30 for life satisfaction, and $b = $ -.15 to $b = $

361   .15 for positive and negative affect.

362   Let's briefly illustrate what this means in practice. If the 95% confidence interval falls

363   completely within the null-region (e.g., $b = $ .20, [95% CI: .15, .25]), the hypothesis that the

364   effect is trivial is supported. If the confidence interval and the null region overlap (e.g., $b = $

365   .30, [95% CI: .25, .35]), the hypothesis is not supported and the results are considered

366   inconclusive, while a meaningful negative effect is rejected. If the confidence interval falls

367   completely outside of the null-region (e.g., $b = $ .40, [95% CI: .35, .45]), the hypothesis is

368   rejected and the existence of a meaningful positive effect is supported. For an illustration,

369   see Figure 1).

Data Analysis

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

372   within-between models (REWB)(Bell et al., 2019). Three models were run, one for each

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[4] Note that other researchers also decreased or recommended decreasing thresholds for effect sizes when analyzing within-person or cumulative effects (Beyens et al., 2021; Funder & Ozer, 2019).

373 dependent variable. The data were hierarchical, and responses were separately nested in

374 participants and waves (i.e., participants and waves were implemented as random effects).

375 Nesting in participants allowed to separate between-person relations from within-person

376 effects. Nesting in waves allowed to control for general exogenous developments, such as

377 general decreases in well-being in the population, for example due to lockdown measures.

378 Thus, there was no need additionally to control for specific phases or measures of the

379 lockdown. Predictors were modeled as fixed effects. They included social media

380 communication types and channels, separated into within and between-person factors, as

381 well as stable and varying covariates. All predictors were included simultaneously and in

382 each of the three models.

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

384 (CFA). Because Mardia's test showed that the assumption of multivariate normality was

385 violated, I used the more robust Satorra-Bentler scaled and mean-adjusted test statistic

386 (MLM) as estimator. To avoid overfitting, I tested the scales on more liberal fit criteria

387 (CFI $> .90$, TLI $> .90$, RMSEA $<. .10$, SRMR $< .10$) (Kline, 2016). Finally,

388 REWB-models cannot model latent variables. To increase precision, I therefore exported

389 factor scores from the CFAs for positive and negative affect. Respondents who answered

390 less than 50% of all questions were removed. The remaining missing responses were

391 imputed using predictive mean matching.

392   For more information on the analyses, a complete documentation of the models and

393 results, additional analyses (for example using multiple imputation or no imputation), see

394 companion website.

Measures

396   In what follows, I list all the variables that I analyzed. For the variables' means,

397 range, and variance, see Table 1. For a complete list of all items and item characteristics,

398 see companion website.

Well-being. Life satisfaction was measured with the item "All things considered, how satisfied are you with your life as a whole nowadays?" from the European Social Survey (European Social Survey, 2021). The response options ranged from 0 (*extremely dissatisfied*) to 10 (*extremely satisfied*).

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 (World Health Organization, 1998). The response options were 1 (*never*), 2 (*on some days*), 3 (*several times per week*), 4 (*almost every day*), and 5 (*daily*). The scale showed good factorial fit, $\chi^2(46) = 65.30$, $p = .032$, CFI = 1.00, RMSEA = .02, 90% CI [.01, .03], SRMR = .01. Reliability was high, $\omega = .85$.

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 (World Health Organization, 1998). The response options were 1 (*never*), 2 (*on some days*), 3 (*several times per week*), 4 (*almost every day*), and 5 (*daily*). The scale showed good factorial fit, $\chi^2(331) = 3138.37$, $p < .001$, CFI = .97, RMSEA = .08, 90% CI [.07, .08], SRMR = .03. Reliability was high, $\omega = .89$.

All three variables were measured on each wave.

COVID-19 related social media use. COVID-19 related social media use focused on communication types was measured with the three dimensions of (a) reading, (b) liking and sharing, and (c) posting. The items come from Wagner et al. (2018) and were adapted for the context of this study. The general introductory question was "How often during the last week have you engaged in the following activities on social media?" The three items were "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.

COVID-19 related social media use focused on channels was measured with five variables from Wagner et al. (2018), adapted for this study. The general introductory question was "How often in the last week have you followed information related to the Corona-crisis on the following social media?" The five items were (a) Facebook, (b) Twitter, (c) Instagram, (d) Youtube, and (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.

Social media use was measured for all participants on waves 1, 2, 8, 17, and 23. Freshly recruited respondents always answered all questions on social media use.

Control variables.   The effects of COVID-19 related social media use were controlled for the following stable variables: (a) gender (female, male, diverse), (b) age, (c) education (ten options), (d) Austria country of birth (yes/no), (e) Austria parents' country of birth (no parent, one parent, both parents). I originally planned to implement additional variables as varying covariates. However, because they were not measured often enough or not at the time when social media use was measured, I implemented them as stable variables using their average values across all waves. This includes (a) text-based media news consumption (five degrees), (b) video-based media news consumption (five degrees), (c) residency is Vienna (yes/no), (d) self-reported physical health (five degrees), (e) living space (eleven options), (f) access to balcony (yes/no), (g) access to garden (yes/no), (h) employment (nine options), (i) disposition to take risks (eleven degrees), and (j) locus of control (five degrees). I also controlled for the following varying covariates: (a) five items measuring outdoor activities such as sport or meeting friends (five degrees), and (b) satisfaction with democracy (five degrees). Because it lead to too much attrition in the sample, I did not control for (a) household size, (b) work hours per week, (c) home office, (d) household income.

₄₅₁ **Results**

₄₅₂   First, when looking at the variables from a descriptive perspective (Figure 2), we see

₄₅₃ that all well-being measures did not change substantially across the different waves of data

₄₅₄ collection. COVID-19 related media use, however, decreased slightly at the beginning of

₄₅₅ the study and remained stable after approximately six waves. The initial decrease might be

₄₅₆ explained by the fact that the collection of data began at the end of March 2020, hence

₄₅₇ approximately three months after the pandemic began. It could be that after an initial

₄₅₈ uptick, COVID-19 related social media use was already declining at the time, returning to

₄₅₉ more normal levels.

₄₆₀ **Preregistered Analyses**

₄₆₁   The study's main hypothesis was that the effects of social media use on well-being

₄₆₂ would be trivial. Regarding the effects of different communication *types*—that is, reading

₄₆₃ vs. sharing vs. posting—all within-person effects fell completely within the a-priori defined

₄₆₄ null region (see Figure 3). For example, respondents who used social media more

₄₆₅ frequently than usual to read about COVID-19 related topics did not show a simultaneous

₄₆₆ change in life satisfaction ($b = 0.05$ [95% CI -0.01, 0.1]). All confidence intervals included

₄₆₇ zero; hence, all effects were also statistically non-significant. As a result, the hypothesis

₄₆₈ was supported for all COVID-19 related types of social media communication.

₄₆₉   Regarding between-person relations, about which no hypotheses were formulated,

₄₇₀ only three effects did not include zero. Respondents who across all waves used social media

₄₇₁ more frequently than others to read about COVID-19 related posts reported slightly lower

₄₇₂ levels of positive affect than others ($b = $ -0.03 [95% CI $>$ -0.01, -0.06]). Respondents who

₄₇₃ across all waves used social media more frequently than others to write COVID-19 related

₄₇₄ posts reported higher levels of negative affect than others ($b = 0.06$ [95% CI 0.09, 0.03]).

₄₇₅ Interestingly, respondents who across all waves used social media more frequently than

₄₇₆ others to write COVID-19 related posts also reported slightly higher levels of positive affect

than others ($b = 0.05$ [95% CI 0.09, < 0.01]). However, note that the effect were still completely inside of the null region, hence not large enough to be considered practically relevant.

Note that when comparing the results with and without control variables, the results differed. For example, on the between-person level, one effect stopped being significant if controlled for additional variables. Actively posting on social media was significantly (though not meaningfully) related to decreased life satisfaction. However, when controlling for potential confounders, the effect became virtually zero (see Figure 4).

Regarding the COVID-19 related use of social media *channels*, the results were comparable (see Figure 4). Changes in the frequency of using different social media channels to attain information regarding COVID-19 were unrelated to meaningful changes in well-being. For example, respondents who used Facebook more frequently than usual to learn about COVID-19 did not show a simultaneous change in well-being ($b = $ -0.05 [95% CI -0.11, 0.01]). Only two effects differed substantially from zero. Respondents who used Instagram more frequently than usual to attain COVID-19 related news reported slightly *higher* levels of life satisfaction than usual ($b = 0.09$ [95% CI 0.02, 0.16]). Respondents who used Twitter more frequently than usual to attain COVID-19 related news reported slightly *lower* levels of life satisfaction than usual ($b = $ -0.12 [95% CI -0.23, -0.02]). However, both effects were still completely inside of the null region, hence not large enough to be considered meaningful. In sum, the hypothesis was supported also for the COVID-19 related use of important social media channels.

In terms of between-person relations—which, again, weren't included in the hypotheses—no relations crossed the null region or fell outside of it. Only one relation did not include zero, was hence statistically significant. Respondents who across all waves used YouTube more frequently than others for COVID-19 related reasons reported marginally *higher* levels of life satisfaction ($b = 0.08$ [95% CI < 0.01, 0.16]). However, please note that this effect again was not large enough to be considered practically relevant.

504   Again, note that when comparing the results with and without control variables, the

505   results differed. Especially on the between-person level, altogether five effects stopped being

506   significant if they were controlled for additional variables. For example, using Instagram

507   was significantly (though not meaningfully) related to increased life satisfaction. However,

508   when controlling for additional covariates, the effect became virtually zero (see Figure 4).

Exploratory Analyses

510   In what follows, I briefly report some exploratory analyses that weren't preregistered.

511   First, to contextualize the results reported above and to see if the results included any

512   meaningful effects at all, I also looked at the effect sizes of selected (cherry-picked)

513   covariates. Because each variable had different response options, we would actually need to

514   define a SESOI for each variable, which for reasons of scope I cannot implement here.

515   Therefore, I report the results of the standardized scales, which allows for a better

516   comparison across the differently scaled variables. For what it's worth, as a rough estimate

517   for the SESOI we can build on the typical convention that small effects start at $r = |.10|$.

518   The results showed that several effects fell outside of the SESOI, were hence considered

519   meaningful. This includes for example internal locus of control, health, satisfaction with

520   democracy, or exercising. For an overview, see Figure 5.

521   To find out whether my inferences were robust across legitimate (though arguably

522   inferior) alternative analyses, I reran the analyses also using standardized estimates, mean

523   scores instead of factor scores, and with a data set where missing data were not imputed.

524   The results were virtually the same. For example, all within-person standardized

525   COVID-19 related types of social media use or channels were significantly smaller than $\beta = $

526   $|.05|$, again supporting that effects were tiny. The additional analyses are reported in

527   Figure 3 and Figure 4, and in the companion website.

Discussion

In this study I analyzed the effects of COVID-19 related social media use on well-being. The data come from a panel study with 24 waves and are largely representative of the Austrian population. In a random effects model I separated between person relations from within-person effects and controlled for a large number of both stable and varying covariates, thereby aiming to assess causality. The results showed that within-person effects were trivial. People who used social media more than usual to learn about COVID-19 didn't show meaningful changes in their well-being.

The results imply that COVID-19 related social media use doesn't seem to be particularly relevant for well-being. Other factors among the third variables that were measured revealed larger effects or relations, suggesting that well-being is determined by alternative aspects such as health, satisfaction with democracy, locus of control, or exercising. According to this study, popular fears that "doomscrolling" or overusing social media during times of crises are not supported.

On the one hand, the results are not aligned with several recent studies analyzing similar or closely related research questions. This includes a study by Bendau et al. (2021), which showed negative relations between social media and well-being (but see Bradley & Howard, 2021; or Sewall et al., 2021). However, note that Bendau et al. (2021) analyzed cross-sectional data on a between-person level while not controlling for third variables, which is not optimal for investigating causal effects. On the other hand, the results are well-aligned with many seminal Communication theories such as the uses and gratifications model (Katz et al., 1973). If effects were indeed profoundly negative on average, then people likely wouldn't spend so much time on social media. Likewise, recent studies and meta-analyses analyzing the effects of social media use from a more general perspective or from a somewhat different angle. These studies have found that the effects of various types of social media use on several well-being indicators are small at best, often too small to matter (Ferguson et al., 2021; Meier & Reinecke, 2020; Orben, 2020), which echoes the

555 results obtained here.

556   If anything, two preliminary and subtle trends can be observed. First, in terms of

557 media channels, using Twitter more than usual was related to slightly decreased levels of

558 life satisfaction. Twitter is considered to have more negative affordances and tonality as

559 compared to other networks such as Instagram (Waterloo et al., 2018), which might help

560 explain the results. Instagram, on the other hand, was related to slightly increased levels of

561 life satisfaction. To speculate, the often-criticized positivity bias on Instagram might have

562 been somewhat beneficial in times of the pandemic. The critique that the positivity bias

563 necessarily leads to envy and negative feelings is unrealistic and myopic, because positive

564 content can also inspire and motivate users (Meier, Gilbert, Börner, & Possler, 2020), which

565 could be especially helpful in times of lockdown and home-office. Second, of all the three

566 COVID-19 related social media activities, people who read about the pandemic more than

567 others showed slightly decreased levels of positive affect, and people who actively posted

568 about the pandemic more than others showed slightly increased levels of negative affect.

569 On the other hand, however, people who posted more about COVID-19 also showed slightly

570 higher levels of positive affects, so taken together the results are ambivalent. But future

571 research might elaborate on these specific relations to probe their stability and relevance.

572 **Limitations**

573   The current study analyzed whether changes in media use were related to changes in

574 well-being, while controlling for several potential confounders. Together, this allowed for an

575 improved perspective on assessing causality. That said, causality necessitates temporal

576 order, and the cause needs to precede the effect. Regarding media use, such effects often

577 happen immediately or shortly after use, necessitating intervals in the hours, minutes, or

578 even seconds. In many cases only experience sampling studies asking users in the very

579 moment can produce such knowledge. However, even then we don't know for certain if we

580 actually measured the right interval. Effects depend on the intensity of use or the length of

581 the interval, and to borrow the words from Rohrer and Murayama (2021), there is no such

582 thing as "the" effect of social media use on well-being. Hence, to document how effects

583 unfold, future research needs to employ different study designs probing different time lags.

584 In addition, more thought needs to be invested in what relevant stable and varying factors

585 we should include as control variables, and I hope this study provides a first step into this

586 direction.

587   Although I had already reduced the predefined SESOIs to be less conservative, they

588 were potentially still too large. Media use is only one aspect of several factors that

589 simultaneously affect well-being. Is it really realistic to expect that extremely changing

590 only *one* of these aspects should manifest in a detectable change in well-being? Or would it

591 make more sense to expect that thoroughly committing to say *two* activities (e.g. regularly

592 exercising *and* establishing a reading habit) should then cause a detectable improvement in

593 well-being? Practically, this would imply a SESOI half as large as I have defined here,

594 namely $b = |.15|$ for well-being and $b = |.075|$ for affect. In the case of this study, however,

595 reducing the SESOI would not even make a big difference, as also with these more liberal

596 thresholds all but three effect would still be completely in the null region, and no effect

597 would be outside of the null region. However, at all events I encourage future research to

598 start a thorough conversation on what effect sizes are considered meaningful and what not.

599 Again, with this study I hope to provide some first input and guidelines.

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

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

602 However, it would be preferable to measure social media use objectively, as people cannot

603 reliably estimate their use (Scharkow, 2016). That said, objective measures often cannot

604 capture the content or the motivation of the use, and only very complicated tools recording

605 the actual content (such as the Screenome project) might produce such data.

606 Unfortunately, such procedures introduce other problems, especially related to privacy.

607 Hence, for this type of research question it still seems necessary to use self-reported

608 measures, and in many cases they can still be very informative (Verbeij, Pouwels, Beyens,

609 & Valkenburg, 2021).

610   Because the data were collected in a single country, the generalizability of the results

611 is limited. The results apply primarily to the more Western sphere, and might not hold

612 true in other cultures, especially cultures with a different media landscape or alternative

613 social media channels. That said, because this is a comparatively large study largely

614 representative of an entire country, and because several waves were collected across a large

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

616 collected in the social sciences.

Conclusion

618   In this study, COVID-19 related social media use didn't meaningfully affect several

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

620 However, factors other than social media use were meaningfully related to well-being, such

621 as physical health, exercise, satisfaction with democracy, or believing that one is in control

622 of one's life. If it's our aim to improve well-being, it might hence be more fruitful not to

623 focus so much on social media but to address other, more pertinent societal problems

624 related to health care, regular exercise, or a functioning democratic system.

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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).

Data Accessibility Statement

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

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Table 1

Descriptives of the main variables.

	sd	min	max	mean
Well-being				
Life satisfaction	1.68	6.38	6.81	6.59
Positive affect	0.57	3.05	3.28	3.15
Negative affect	0.39	1.66	1.81	1.73
Social media use				
Read	1.03	2.10	2.92	2.43
Like & share	0.87	1.61	1.92	1.78
Posting	0.63	1.33	1.46	1.39
Social media channel				
Facebook	0.96	2.34	2.68	2.45
Twitter	0.52	1.16	1.72	1.37
Instagram	0.83	1.84	2.66	2.09
WhatsApp	1.23	2.28	2.62	2.45
YouTube	0.88	1.77	2.30	2.00
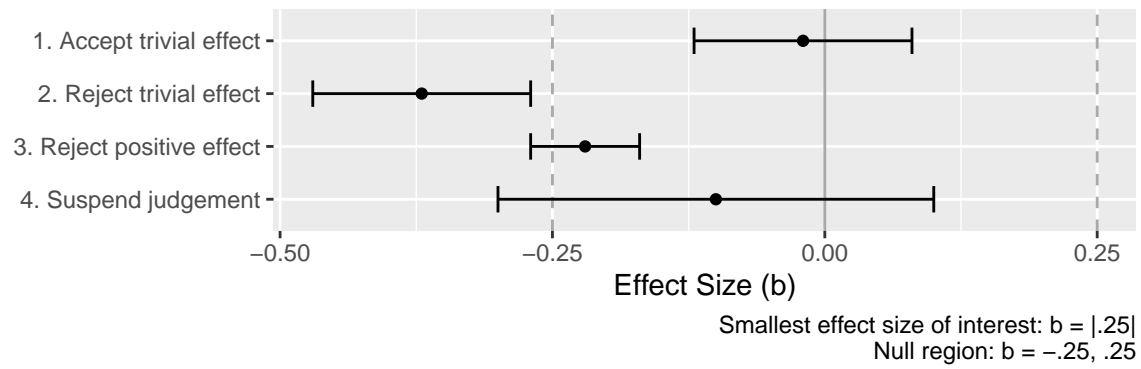
Figure 1. Illustration of how confidence intervals are used to test a null region. Here, a trivial effect of social media use on life satisfaction is defined as ranging from b = -.25 to b = .25
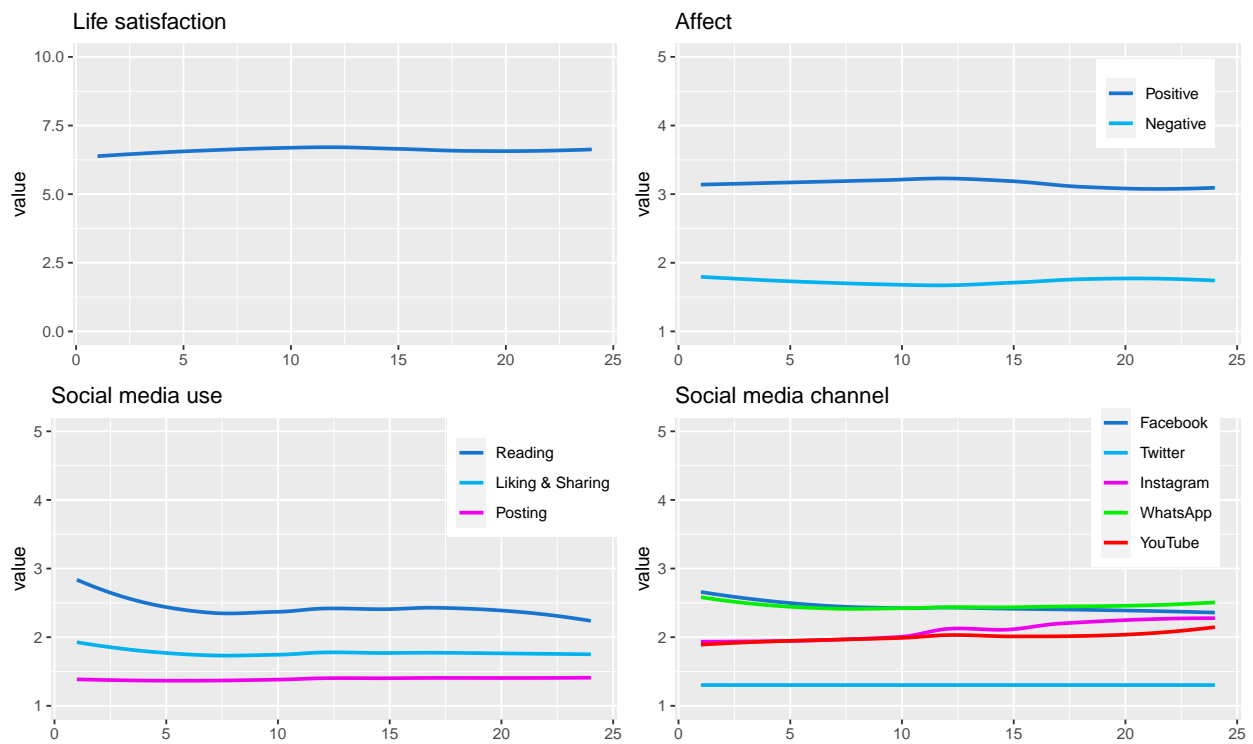
Figure 2. Development of well-being and media use measures across the pandemic. Values obtained from mixed effect models, with participants and waves as grouping factors and without additional predictors.
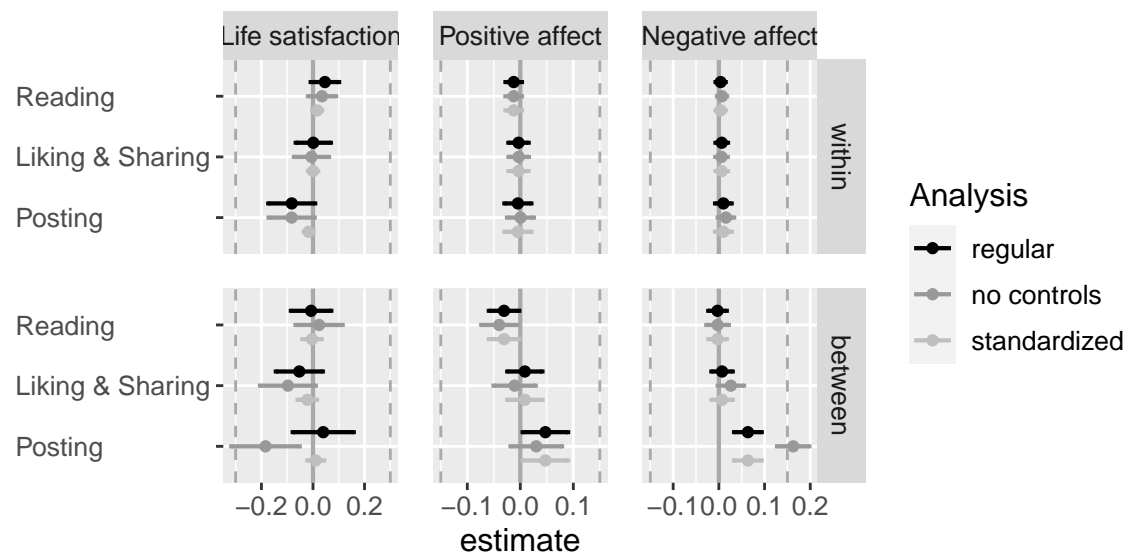
Figure 3. The effects of various types of social media use on three indicators of well-being. The black estimates show the effects controlled for a large number of covariates (see text; preregistered); the grey estimates are without control variables (exploratory). The SESOI was b = |0.30| for life satisfaction and b = |0.15| for affect. Hence, all of the reported effects are not considered meaningful.
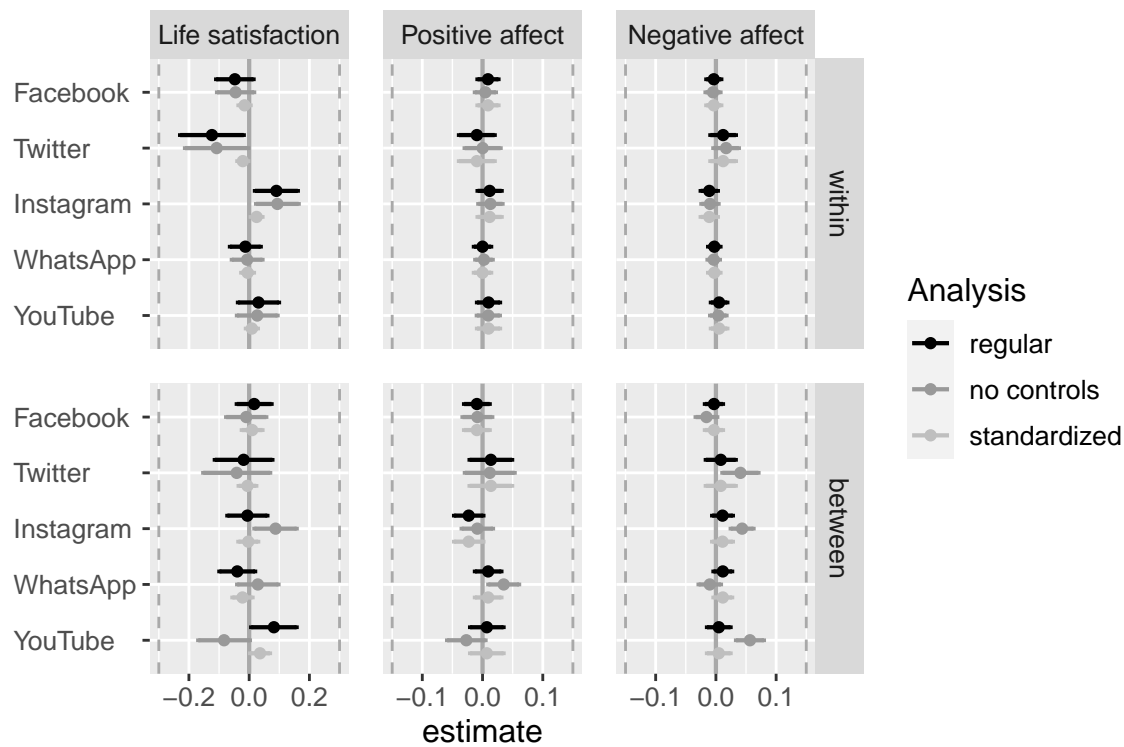
Figure 4. The effects of using various social media applications on three indicators of well-being. The black estimates show the effects controlled for a large number of covariates (see text; preregistered); the grey estimates are without control variables (exploratory). The SESOI was b = |0.30| for life satisfaction and b = |0.15| for affect. Hence, all of the reported effects are not considered meaningful.
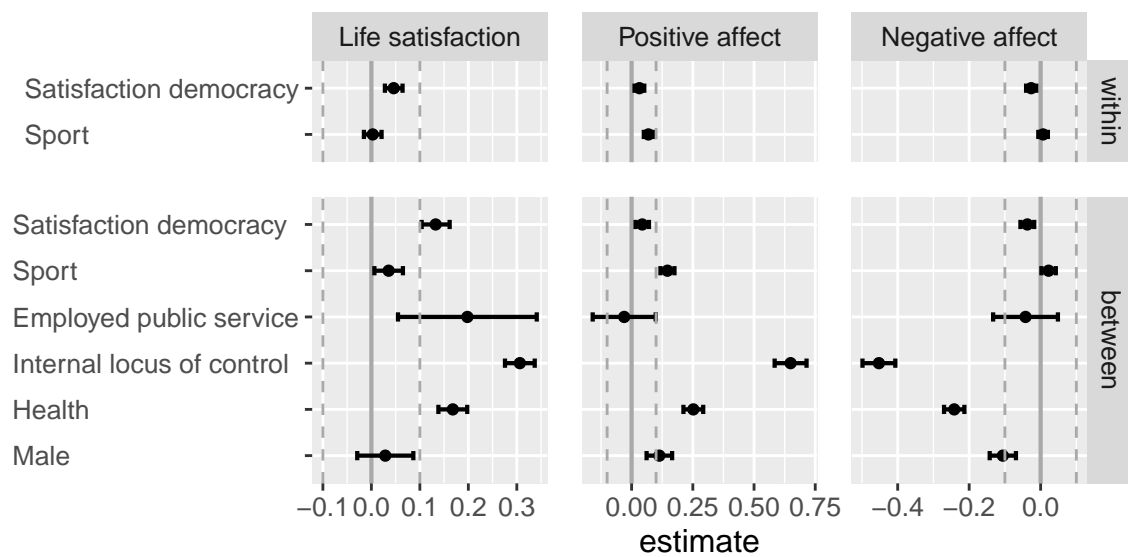
Figure 5. Results of selected covariates. All variables were standardize except 'Male' and 'Employed in public service', because there were measured on a binary scale.