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Publication Date

2022

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Exploring the impact of priming on young adults' mental health and social media use

By

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DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

DOCTOR OF PHILOSOPHY

in

Communication

in the

OFFICE OF GRADUATE STUDIES

of the

UNIVERSITY OF CALIFORNIA

DAVIS

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2022

Acknowledgments

Abstract

The relationship between social media use and mental health has long puzzled communication and psychology scholars. There have been attempts to explain their relationship (e.g., examining the effects of different social media activities and employing novel measurement strategies). This project adds to the literature by testing priming effects in the context of social media research and advocates for the meaningfulness of measurement strategies by showing how self-reported mental health is subject to survey structure and language. We collected data from participants for three experimental studies (n=571, n=581, and n=656, respectively). Our results demonstrated that presenting the addiction scale first in the survey elicited higher self-reported depression than other conditions placing the depression scale first, but the order of the addiction scale did not alter participants' perceived self-esteem. Notably, results also indicate that the wording of questionnaire items can affect participants' mental health, such that participants in conditions where mental health scales contained mixed wording (i.e., a scale that contains positive and negative descriptions designed to measure the same construct), tended to report lower self-esteem and depression. Furthermore, our results imply that reading an article focusing on the pros of social media resulted in higher self-esteem and lower depression than reading an article highlighting the cons of social media. These findings have implications for future research germane to how social media impacts our mental health.

Keywords: social media, priming, self-esteem, depression

Introduction

Is Facebook a source of happiness or unhappiness? Scholars have paid substantive attention to understanding the relationship between social media use (SMU) and mental health, and ongoing empirical work (e.g., Highfield & Leaver, 2016) and reviews (e.g., Krause et al., 2019) aim to address the mixed relationship between SMU and mental health. Facebook, Instagram, Snapchat, and TikTok, among other popular social media platforms, are gaining active daily users, especially among the younger population (Statista, 2022). More importantly, according to the National Institute of Mental Health (2022), younger adults between the ages of 18-25 reported having the highest prevalence of mental illness in the U.S. (30.60%) compared with that of their counterparts ages 26-49 (25.30%) and those above the age of 50 (14.50%). The National Institute of Mental Health did not survey people before they entered adulthood. Though the causes of mental illness may have derived from different sources, many scholars blame social media (SM) for young adults' decreased mental health (e.g., Fardouly et al., 2018; Lup et al., 2015). Given the conflicting association between SMU and mental health, the prevalence of mental illness in young adults, and the potential ties between mental health and SMU, it is important to understand the cognitive processes and behaviors users have while engaging in an array of SM activities (Valkenburg, 2017).

Notably, scholars have predominantly employed self-reported survey methodologies to assess SMU and mental health owing to the lack of public availability and convenience of other more objective means, such as gathering users' social media use logs (Parry et al., 2021). Self-reported scales have been developed in the past two decades to estimate SMU, but some scholars have criticized self-reported SMU scales for being subjective and failing to

reflect more objective SMU measures (e.g., Ernala et al., 2020; Valkenburg, Beyens, Pouwels, & van Driel, 2021). Therefore, investigating the discrepancy between objective and subjective SM measures is a fruitful research direction for scholars with certain theoretical frameworks (e.g., question order effects; Mcfarland, 1981; priming effects; Mieczkowski et al., 2020).

This work builds on ongoing research focusing on (a) how to accurately record and report SMU and (b) how SMU relates to subjective well-being (hereinafter referred to as SWB). Previous work has predominantly relied on self-reported measures, which consist of a battery of scales that do not report the order in which the items were displayed to participants (see Mieczkowski et al., 2020 for a review). This project adds to the literature by employing no more than two mental health variables in the same instrument, therefore testing the priming effects of (a) addiction and intensity scales (question order effects), (b) depression and self-esteem scales (question order and anchoring effects), and (c) the priming effects of reading an article whose valence either emphasizes the benefits or harm of SM, on users' concurrent self-reported depression or self-esteem levels (context effects).

Social media and its measurement

Addiction scale. Over the past two decades, social scientists have formalized, built, validated, and challenged an array of self-reported scales aiming to quantify SMU for research purposes (e.g., Ernala et al., 2020). For example, a review by Keles et al. (2020) concluded that time-spent/intensity, activity, investment, and addiction were the four most commonly used rationales to study SMU. This paper intends to focus on the time spent/intensity scales and addiction scales, since the former represents a relatively objective

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appraisal, while with the latter, instrument wording tends to prime users. More specifically, "you use social media so much that it has had a negative impact on your job/studies" – is one of the six items included in a scale measuring social media addiction (Andreassen et al., 2017). The goal of the scale was to capture users' addictive behaviors, but its language's emphasis on SM's negative impact might prime users to focus on negative experiences related to their addictive SM behaviors. In a similar vein, given that this scale centers on the addictive aspects of SM, it does not provide reverse-ordered items that state non-addictive SM behaviors (e.g., Rosenberg Self-Esteem Scale; Rosenberg et al., 1965). After reviewing the array of SM addiction scales, we decided to adopt the Bergen Social Media Addiction Scale (BSMAS; Andreassen et al., 2017). More specifically, Andreassen et al. (2012) asserted that there were six major components of addictive behaviors (salience, mood modification, tolerance, withdrawal, conflict, and relapse) and shortened this list for a more general SM addiction scale (Andreassen et al., 2017). The BSMAS has been cited in more than 1,500 other studies and validated in Europe and Asia, where languages other than English were used (e.g., da Veiga et al., 2019; Phanasathit et al., 2015). In summation, empirical evidence suggests that the BSMAS is a valid and reliable scale to measure SM (e.g., Facebook) addiction, but there is a dearth of attention paid to the cognitive processes that take place when participants answer items underscoring problematic SM behaviors.

Intensity scale. As summarized by Keles et al. (2020), another commonly utilized scale is to measure users' time spent/intensity. Similar to the measures of SM addiction, scholarship has focused on how to accurately ask one user to retrospect and report SMU (Suler et al., 2014; Valkenburg et al., 2021). Among a battery of scales, our research team adopted an

instrument developed by Ellison et al. (hereinafter referred to as the EIS; 2007). The EIS consists of 9 items; two items aim to collect (a) users' number of friends within the SM and (b) minutes per day that each user spends on the SM, anchored from 1 (e.g., 10 or fewer friends; 0-14min) to 8 (e.g., >800 friends; >8 hours) and 1 (strongly disagree) to 5 (strongly agree), respectively. The remaining items are a series of Likert-scale designed to measure users' emotional connection and attitudes towards a particular SM platform. Our decision to adopt this scale is threefold. First, this scale indicates more than indices of frequency and duration, such that it incorporates both attitudinal evaluations and actual behaviors. Furthermore, Ernala et al. (2020) investigated how well people recall and report their social media use with aid from Facebook's backend data. After carefully comparing self-reported data with actual server logs, they concluded that Ellison et al. (2007)'s EIM produced fewer errors compared to other scales. More importantly, this particular intensity scale has been cited more than 14,000 times, furthering its reliability and validity in measuring SMU. In summation, empirical evidence suggests that the adoption of the BSMAS and the EIS are the most practical SMU measures, and inquiries about SM's potential impact on users have received the bulk of attention (e.g., Cingel & Olsen, 2018; Hwang & Cho, 2018; Suler et al., 2014; Valkenburg et al., 2022). Measured subjectively, the cognitive process that occurs while completing a questionnaire has received less consideration (e.g., Mussweiler, 1999; Schwarz & Clore, 1983). Empirical work shows that SM users tend to misreport their usage (see Parry et al. 2021 for a review). Therefore the relationship between SMU and subjective mental health is clouded. Before we discuss the theoretical explanations, a review of the locus of the current mental health and SM paradigm should first be thoroughly discussed.

Subjective mental health

In the language of Diener et al. (2018), SWB refers to "the extent to which a person evaluates his or her life." SWB is often conceptualized as having multiple indicators involving cognitive evaluations and affective feelings (e.g., Hampton, 2019; Suler et al., 2014). SWB subsumes desirable constructs (e.g., self-esteem; Kross et al., 2013) and less desirable constructs (e.g., depression; Valkenburg et al., 2022). The rigorous discussion and critique regarding how to define and classify SWB can take place on a more profound theoretical level. However, for clarity and ease, we refer to our dependent variables (i.e., SWB, subjective ill-being) in the paper collectively as *subjective mental health*. Further, a line of research investigates how well-being and ill-being indicators correlate with outcome variables of researchers' interests in differential directions (see Johannes et al., 2021 for a review).

Since the launch of Facebook in 2004, scholars' conclusions about how SM impacts users' lives and mental health have not been unanimous (e.g., Cingel & Olsen, 2018; Krause et al., 2019; Song et al., 2014; Verbeij et al., 2022). Scholars have spent a substantial amount of time examining the possible theoretical explanations for this admixture of results (i.e., positive, negative, and null relationships), and there are many plausible explanations (Burke et al., 2010; Valkenburg, 2021; Valkenburg et al., 2021; Valkenburg et al., 2022). For example, Burke et al. (2010) proposed the idea that treating all SMU as an umbrella behavior without considering different types of use was problematic, and instead, they conceptualize and operationalize passive SMU and active SMU differently. They propose that users will experience detrimental effects of SM while passively browsing as a function of social

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comparison, yet active posting behaviors will lead to no unwanted outcomes (i.e., the passivity hypothesis; Burke et a., 2010). In addition to this theoretical complexity, an array of platforms have emerged since Facebook, some gaining more popularity worldwide, especially among young adults (i.e., TikTok, Instagram, Snapchat; Statista, 2022). More recent work has focused on researching how these newer platforms, and their affordances, affect users' lives (e.g., Pittman & Reich, 2016; Valkenburg et al., 2021; Valkenburg et al., 2022). Furthermore, scholars have proposed, developed, validated, and even challenged the validity of numerous measures of users' mental health.

Depression and self-esteem, the two most representative and studied mental health constructs, are prevalent in the current literature (e.g., Kroenke & Spitzer, 2002; Rosenberg, 1965). It is also worth acknowledging that other mental health constructs have also received ample attention, including body satisfaction (e.g., Cohen et al., 2017), life satisfaction (e.g., Sewall et al., 2020), and happiness (Diener et al., 2018). Notably, it is a common practice to collect both mental health and SMU in the same instrument, and most such instruments are self-reports (e.g., Song et al., 2014; van Rooij et al., 2018; Yang & Robinson, 2018).

Priming and question order effects

Priming is a well-studied psychological and social phenomenon where exposure to one stimulus can influence a respondent's response to a subsequent stimulus, even in the absence of consciousness (Weingarten et al., 2016). The concept of priming was introduced by Lashley (1951). In an effort to understand how humans can effortlessly form sentences and speak up with minimal thinking and preparedness, Lashley (1951) argued that there was a partial activation, or priming, as a theoretical vehicle that aggregates the words together prior

to the sentence formulation. Meyer and Schvaneveldt (1971), who are recognized as pioneering investigators of priming effects, initiated experimental priming research investigating thematic spreading activation. Understood this way, Priming posits that the current situation can prime the activation of certain mental nodes, leading to social perceptions and even behaviors (e.g., Bargh et al., 1996; Bargh, 2014; Hjortskov, 2017; Weingarten et al., 2016). Built upon past priming-related scholarship, carry-over studies have tried to establish linkages between priming and behaviors (Bargh et al., 1996; Steele & Ambady, 2006; Higgins et al., 1977; Williams & Bargh, 2008). For instance, Williams and Bargh (2008) found that physical warmth promoted interpersonal warmth, such that participants who held a hot (vs. cold) cup of coffee reported other discussants having a warmer (vs. colder) personality. Bargh et al. (1996) found that when they primed participants in their laboratory via a stimulus that stereotyped elders, participants walked out of the lab more slowly than those in the control condition, in an unnoticed and unintended manner. Elucidated by the aforementioned work, inquiries pertaining to how priming affects participants' response in a survey have been a heated area (e.g., Lee et al., 2021; Parry et al., 2022), and we designed a set of studies to investigate not only the question order effects, anchoring effects, and context effects, all of which are classified as more nuanced priming effects (Lasorsa, 2003; Thau et al, 2020; Zaval et al., 2014).

Question order effects, a sub-effect of priming in the context of survey methodologies, refer to how participants respond to survey items differently owing to the order in which the questions or options appear in the same questionnaire and its corresponding carry-over effects (Andersen & Hjortskov, 2016; Hjortskov, 2017; Thau et al., 2021). Priming and question

order effects largely depend on the availability and accessibility of participants' mental nodes (Mieczkowski et al., 2020; Williams & Bargh, 2008), and we can easily visualize multiple instances whereby priming effects might have taken place. Prior to completing the experiment, for example, each participant may already have a concrete idea of the potential effects of SM (e.g., the negative impact of SM primed by SM addiction scale; Mieczkowski et al., 2020). Drawing from the literature, we noticed a lack of scholarly attention on the priming effects of SM and mental health research. Work conducted by Mieczkowski et al. (2020) was the only piece that explicitly tested priming effects in SM scales.

Social Media, mental health, priming, and question order effects

Theories have been built to explain the cognitive processes that take place while respondents answer and interpret survey questions, retrieve relevant information in memory, appraise the relevance of the information to the survey's questions and options, summarize the information in the form of attitudes or judgments, and translate the attitudes or judgments to the scale anchored in the survey (Plutzer & Zaller, 1994; Tourangeau et al., 2000). Multiple stages in this cognitive process introduce opportunities for question order effects. From the perspective of Tourangeau et al. (2000), question order effects involve multiple cognitive and behavioral factors: Recency of activation (e.g., Ewoldsen & Rhodes, 2019; Higgins & Eitam, 2014; Plutzer & Zaller, 1994; Rocklage & Fazio, 2018), frequency of activation (Higgins, 1996; Higgins & Eitam, 2014), item and/or option relations (Garbarski et al., 2016; Mieczkowski et al., 2020), and background (Mcfarland, 1981; Schwarz et al., 1991). First, Higgins and Eitam (2014) posit that the recency of activation of mental nodes are a factor. That is, not all related nodes are activated in the same regard, and nodes activated more

recently are more easily activated and consequently have differential downstream effects in primed thoughts or behaviors. In the context of SM and mental health research, SM users who see items measuring addictive SM behaviors or the linkage between SM use and ill-being will cause mental nodes in close proximity with less favorable SM use outcomes to be activated more easily than more favorable SM outcomes, which can subsequently affect participants' evaluations of their momentary states.

Second, frequency of activation is a factor; this is the notion that repetitive and consistent exposure to the same topic and that topic's relevant information can make that topic more accessible and more likely to affect later responses in the same instrument (Higgins, 1996; Higgins & Eitam, 2014). Empirical evidence shows that responding to highly specific questions before general questions can increase user interest in politics (Mcfarland, 1981), understanding of the tax burden (Turner & Krauss, 1978), and satisfaction with public services (Thau et al., 2021). Interestingly, Lasorsa (2003) reported that adding a question testing participants' knowledge of a political topic (i.e., a 'bomber') before presenting participants with scale items measuring their political interest (high in specificity) exibited decreased interest in the topic, compared to those who were not presented with specific topic knowledge questions. Notably, there is less research on the frequency of activation in the context of SM research. Scales developed to measure addictive SM use predominantly focus on items high in specificity, but do not introduce a bomber (e.g., Bergen Social Media Addiction Scale; Andreassen et al. 2017). The inclusion of the bomber question may affect users' momentary appraisal of their subjective mental health. Since the activation of related mental nodes may be interrupted by responding to a bomber question, it is problematic to

claim there are associations between SMU and subjective mental health without considering the structure of the questionnaire.

The third factor influencing participants' responses to a survey is the relationships between the items and/or the options. The order in which the items in the same survey were presented to the participants can result in primed thoughts and attitudes (Garbarski et al., 2016; Mieczkowski et al., 2020). For instance, Garbarski et al. (2016) conducted experimental work and reported question order effects; they found that self-reported health is higher when options are ordered from 'excellent' to 'poor' than when ordered from 'poor' to 'excellent.' Also, when the self-reported health measure appeared first, participants tended to report higher health than when domain-specific health-related items were presented first. This work has implications for order effects on subjective mental health: How does a series of items describing depressive symptoms influence users' affective state? If nodes related to depression are activated before the measurement of other constructs, it can bias associations between SMU and subjective mental health (Mcfarland, 1981).

Another determinant is the respondents' background (Mcfarland, 1981; Schwarz et al., 1991). For example, demographics such as education, occupation, residency, and expertise on a topic can determine question order effects (James & Van Ryzin, 2017). Specifically, Mcfarland (1981) predicted that those with higher education and more knowledge in the particular field are less susceptible to priming effects. However, Englich et al. (2006) argued that differential knowledge did not affect priming effects.

Self-reports are convenient, but they need to be polished to counter the effects of psychological processes, as alluded to in the previous section. For example, in the terms of

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the priming literature (Diener, 1994; Ernala et al., 2020), it is evident that the wording of the instrument will affect participants' responses. Measurement of SM has heavily relied upon self-reported measures (e.g., Cingel & Olsen, 2018). Though there are novel computational methods utilized to collect objective use, subjective self-reports remain the dominant measure in the psychology and communication literature (see Parry et al., 2021). How might the order of the instruments impact the results of subjective mental health? Does the wording of the instrument alter the mindsets of the participants? Mieczkowski et al. (2020) manipulated the scales presented to the participants who later reported their mental health, and they reported that participants exposed to the addiction scale rated higher on depression than those exposed to an intensity scale. Though the effects of priming were salient in their work, Mieczkowski et al. (2020) did not extend their interests across a broader continuum of mental health constructs (e.g., self-esteem).

SMU and priming both have accumulated an abundant amount of research as aforementioned. Yet, there is a lack of research attempting to connect the two, and our previous discussion has made the role of priming in survey answers explicit. This work fills this gap in the literature by manipulating question ordering to test priming effects. Past scholarship, in the context of social media use and mental health, has not paid particular attention to priming effects, and if the design, order, and wording of the questionnaire impact how respondents assess their at-the-moment well-being, we must be cautious about when and where we ask questions, before coming to any conclusions. Notably, distributing a survey comprised of multiple mental health constructs is a common practice in social science research (e.g., Beyens et al., 2016; Przybylski et al., 2013). While the item order is

randomized, it remains plausible that seeing a   depression scale prior to answering questions about stress can prime the activation of mental nodes related to the negative aspects of subjective mental health, which will alter their self-reported stress levels. In summary, our work is novel in that it (a) examines the priming effects in the context of SM and mental health research and (b) offers practical guidance for avoiding priming effects in future survey item construction and development.

Study 1

In study 1, we test differential priming effects of the addiction scale and intensity scale and how they relate to self-esteem and depression. We contribute to the literature by attempting to examine the potential effects of item orders on participants' self-reported assessments of their current mental health. In our between-subject experiment design, we asked our participants to respond to one of the following as the first scale that they encounter: (a) intensity scale, (b) addiction scale, (c) mental health scale, and (d) no SM scale as the control, therefore creating 10 conditions in total.

Fueled by the results of Mieczkowski et al. (2020) we included both self-esteem and depression in our design; these two mental health indicators have been chosen due to their prevalence in the literature and their growing importance in the lives of people in different developmental stages (e.g., Valkenburg, 2021). Self-esteem, according to Baumeister (1993), refers to individuals' evaluation of their worthiness, encompassing beliefs about themselves and emotional states. As meta-analyses (e.g., Krause et al., 2019) consistently reported a mix of relationships between SMU and self-esteem, a person-within-approach seems to explain our confusion such that significant associations only hold for the minority of the population

(Beyens et al., 2021). In other words, self-esteem has been empirically shown to be less subjective to change than other mental health variables (Mieczkowski et al., 2020). If manipulating question order produces changes in relatively stable self-esteem, the same effect may apply to a variety of other mental health variables. Additionally, the inclusion of depression answers the call from a breadth of research that looks into not only SWB (e.g., self-esteem) but also subjective ill-being (e.g., Johannes et al., 2021; Valkenburg et al., 2021). According to the American Psychiatric Association, depression is a mental illness when people show low mood and minimal interest in engagement in activities (2022). After a careful review of the SMU scales, we decided to employ two well-established SM scales in study 1.

Further, we decided to adopt the BSMAS (Andreassen et al., 2017) and the EIS (Ellison et al., 2007), as previously specified, which have been widely adopted in psychology and communication scholarship (e.g., Ernala et al., 2020; Satici & Uysal, 2015). EIS and BSMAS are two representative self-reported measurements adopted by an array of scholarship, yet the conclusions drawn from research investigating the relationship between SM activities and/or SMU have not been unanimous (e.g., Cingel & Olsen, 2018; Krause et al., 2019; Valkenburg et al., 2022). Meta-analyses looking at the relationship between SMU and mental health provide us with a wealth of studies employing self-reported surveys to collect users' subjective mental health and SMU (e.g., Wang et al., 2018). Amid increasing interest and debate about what we know and what we need to know about SMU and mental health, only one study systematically reviewed scholarships employing objective SM measurement (Parry et al., 2021; e.g., back end data, logged entries, screen/smartphone monitor apps).

Specifically, Parry et al. (2021) reported that self-reported data only moderately correlated with logged measures, warranting concerns about the conclusion drawn from studies that only employed self-reported data. Indeed, before drawing conclusions about the impact of SM on mental health, it is essential to acknowledge the limitations of self-reported data (Marengo et al., 2021). The utility of examining the conflicting results has received scholarly attention, and researchers have proposed multiple theoretical explanations, one of which utilizes the measurement bias of self-reported data.

The adoption of survey methodology is not uncommon across disciplines; psychology (e.g., Błachnio & Przepiórka, 2018), sociology (e.g., Smith et al., 2021), economics (e.g., Padhan & Prabheesh, 2021), and communication (e.g., Cingel & Olsen, 2018) have accumulated a rich collection of survey research. Despite the biases of survey methodology, it remains one of the most used methodologies owing to its benefits; the survey is advantageous in such that, in comparison with logged SM data, it is accessible by any scholar and does not require an intimate relationship with external organization hosting all the data (Nardi, 2020; e.g., SM companies). Self-reported data and relevant scales are constantly validated; nevertheless this method is criticized, particularly given the rapid development of technology that records and reports accurate SMU (e.g., Ernala et al., 2020; Junco, 2013; Parry et al., 2021; Verbeij et al., 2022).

Given that recently emerged logged-related technology remains a luxury, the accessibility of survey methodology has still been the most used method and afforded social scientists to assess the SMU rapidly, and it is awash with criticism (e.g., Keles et al., 2020; Malik et al., 2016; van Rooij et al., 2018). Users are likely to be reminded of their attitudes,

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behavior, and past experiences, which tend to affect their responses to questions appearing later (Bargh et al., 1996; Weingarten et al., 2016). For instance, Turner and Krauss (1978) found that if respondents were instructed to offer their appraisal of the federal spending across multiple aspects, they were more inclined to consider their tax burden as "about right" rather than "the tax is too high". In a similar vein, Mcfarland (1981) found that participants tended to express higher interests in politics and religion when they responded to more specific questions before rating on more general questions. More recent research, indeed, has also reported similar findings; Hjortskov (2017) reported that the results of the citizen satisfaction survey were relevant to the prior questions: when asked by positively framed questions pertaining to police services, respondents would rate subsequent local services as more satisfactory. In summary, the line of research regarding the effects of survey item ordering on subsequent responses affirms one overarching theme; general questions are less likely than specific questions to produce the saliency of certain attitudes, behaviors, and experiences (Mcfarland, 1981).

In study 1, we expect to observe differences as a result of the manipulation of the order of the items (e.g., subjective mental health, SMU). According to the priming literature and the aforementioned research about question order effects on survey methodology, we decided to adopt one subjective mental health variable and another scale measuring SMU and manipulate and report the order of these two variables, therefore contributing to the literature that many empirical studies (e.g., Cingel & Olsen, 2018; Yang, 2016) reported SMU followed by mental health scores. We expect our participant's self-esteem to be negatively related to the addiction scale since exposure to items emphasizing problematic and addictive usage of

SM can prime our users to feel worse off regarding their concurrent self-esteem; similarly, we expect to observe a positive correlation between depression and addiction scale. Conversely, we expect that priming will not occur while respondents answer the intensity scale, but the accumulated amount of research has consistently reported an admixture relationship between SM intensity and self-esteem. Thus, we make the following hypotheses and research questions:

H1.1 Participants' self-esteem will be negatively related to the addiction scale.

H1.2 Participants' depression will be positively related to the addiction scale.

RQ1.1 What is the relationship between self-esteem and the intensity scale?

RQ1.2 What is the relationship between depression and the intensity scale?

Furthermore, we expect participants to be primed to contemplate the negative aspects of SM while answering the BSMAS than the EIS, thus reporting lower (higher) levels of self-esteem (depression). Further, we expect that those who complete the mental health scales before answering BSMAS or EIS will not be primed; therefore, they are predicted to have higher (lower) levels of self-esteem (depression). Thus, we made the following hypotheses:

H1.3 Participants who report to the addiction scale first will report lower self-esteem scores than participants who respond to the intensity scale first and

H1.4 Participants who report to the addiction scale first will report higher depression scores than participants who respond to the intensity scale first.

H1.5 Participants who report to the self-esteem scale first will report higher self-esteem scores than participants who respond to the SM scale (i.e., addiction scale, intensity scale) first.

H1.6 Participants who report to the depression scale first will report lower depression scores than participants who respond to the SM scale (i.e., addiction scale, intensity scale) first.

Methods

Participants

We conducted a between-subject experiment online. Overall, 767 young adults between the ages of 18 and 25 completed our experiment. Our final sample consisted of 581 participants after removing (a) participants who did not pay attention to the attention check questions and (b) participants who failed to pass the manipulation check. In regards to their ethnicity, 46.3% identified as Asian/Asian American (n =269), 19.3% identified as Caucasian (n = 112), 16.0% of participants identified as Hispanic/Latino (n = 93), 1.4% of participants identified as Black/African American (n = 8), 0.5% of participants identified as Pacific Islander (n = 3), 0.3% of participants identified as Native American (n = 2), 9.6% of participants identified as Mixed (n = 56), and 2.8% did not answer this question (n = 16). Our sample was split between men (n = 134, 23.1%) and women (n = 399, 68.7%). Notably, we removed those who did not pass the attention check questions (i.e., we instructed our participants to select a fixed choice while reporting their mental health) and those who failed to pass the manipulation check questions (e.g., "Did you answer your social media use questions before, or after, answering questions about how you feel about yourself?").

Table 1.1

Demographics of Our Participants in Study 1

	Percent	n

Age	18	22.5%	131
	19	29.9%	174
	20	19.6%	114
	21	11.4%	66
	22	6.7%	39
	23	0.7%	4
	24	0.5%	3
	25	0.2%	1
	Did not answer	8.4%	49
Sex	Male	23.1%	134
	Female	68.7%	399
	Other	2.4%	14
	Did not answer	5.9%	34
Race/Ethnicity	Asian	46.3%	269
	Caucasian	19.3%	112
	Hispanic/Latino	16.0%	93
	Black/African American	1.4%	8
	Pacific Islander	0.5%	3
	Native American	0.3%	2
	Mixed	9.6%	56
	Self-described	3.8%	22
	Did not answer	2.8%	16

Note. Since we did not assign compulsory demographic questions, some participants did not answer certain questions. Because percentages are rounded to one decimal, the sum index may not exactly be 100%.

Procedure

Following the approval of the university Institutional Review Board, we posted our study URL online for recruitment purposes. Our study was hosted by a survey service company, Qualtrics. When participants entered the survey, they saw the informed consent, the contact details of the principal investigator, and the compensation that they would receive

(one extra credit) for their participation on SONA, a university-wide subject pool where researchers can recruit undergraduate participants (we used the same pool in studies 2 and 3). See table 1.2 for a list of the conditions assigned to participants in study 1. In this between-subject design, each participant was assigned to one of the 10 conditions, encompassing five conditions testing self-esteem as the dependent variable, and other conditions testing the impact of our design on depression. In the first condition, for example, participants answered the EIS (intensity) followed by the self-esteem scale. In our second condition, we replaced the EIS (intensity) with the BSMAS (addiction). In the third and fourth conditions, we reserved the order so that the self-esteem scale preceded the EIS (intensity) and the BSMAS (intensity). We added the fifth condition whereby only self-esteem was assessed and there was no presence of SM scales. The same manipulations applied to conditions 6 to 10, wherein we reconstituted the self-esteem scale with the depression scale.

Table 1.2

Conditions Assigned to Study 1 Participants

Self-esteem		Depression	
Condition 1	EIS (intensity) + self-esteem	Condition 6	EIS (intensity) + depression
Condition 2	BSMAS (addiction) + self-esteem	Condition 7	BSMAS (addiction) + depression
Condition 3	Self-esteem + EIS (intensity)	Condition 8	Depression+ EIS (intensity)
Condition 4	Self-esteem + BSMAS (addiction)	Condition 9	Depression + BSMAS (addiction)
Condition 5	Self-esteem	Condition 10	Depression

Measures

Mental health measures. We measured participants' depression severity through the Patient-Health-Questionnaire-9 scale (PHQ-9, Kroenke & Spitzer, 2002) anchored from 1 (not at all) to 4 (nearly every day). Sample Items included "Thoughts that you would be better off dead, or thoughts of hurting yourself in some way" and "Trouble falling or staying asleep, or sleeping too much," $M = 17.88$, $SD = 6.26$. Cronbach's α = 0.85. We coded depression so that higher numbers indicate higher levels of depression. We assessed users' self-esteem via the Rosenberg Self-Esteem Scale (Rosenberg, 1965). This is a ten-item measure, anchored from 1 (strongly disagree) to 5 (strongly agree). Sample items included "I am able to do things as well as most other people" and "All in all, I am inclined to feel that I am a failure," $M = 3.39$, $SD = 0.70$, Cronbach's $\alpha = 0.91$. We coded self-esteem so that higher numbers indicate higher self-esteem.

Intensity scale. We assessed participants' SM intensity via the scale developed by Ellison and colleagues (2007). This is a nine-item scale; sample items included "You spend a lot of time thinking about social media or planning how to use it" and "You feel an urge to use social media more and more," $M = 3.05$, $SD = 0.88$, Cronbach's $\alpha = 0.81$. See appendix A for a complete list of items.

Addiction scale. This Bergen Social Media Addiction Scale is a six-item scale designed to measure excessive social media use (Andreassen et al., 2017). Sample items included "Social media is part of my everyday activity" and "I feel out of touch when I haven't logged onto social media for a while," $M = 2.64$, $SD = 0.88$, Cronbach's $\alpha = 0.85$. See appendix A for a complete list of items.

Results

Preliminary analyses

We analyzed our data using SPSS 27. Before conducting inferential statistical analyses, we first ran a correlation matrix to observe the correlation between the variables of our interests. We found several significant correlations between our variables (see table 1.3 for the correlations). We did not detect any statistical correlations and, thus, decided to employ a one-way analysis of variance (ANOVA) for further analysis and not to keep any covariates. For all significant results, we report the main effects and post hoc tests in the text, and all other information can be retrieved in respective tables.

Table 1.3

Correlations Between the Variables

	1	2	3	4	5	6	7
1. Age	1						
2. Sex	-0.075	1					
3. Ethnicity	-0.007	0.156**	1				
4. Self-esteem	-0.010	-0.087	-0.016	1			
5. Depression	0.013	0.067	0.096	NA	1		
6. Social media intensity	0.083	0.164*	0.088	-0.006	-0.385**	1	
7. Social media addiction	-0.074	0.115	-0.033	-0.244*	0.304**	NA	1

Note. **denotes that correlation is significant at the 0.01 level; *denotes that correlation is significant at the 0.05 level; Since we did not measure self-esteem and depression simultaneously within any condition, we cannot compute this correlation.

SMU and mental health

Our results indicated that social media addiction (SMA) was negatively related to self-esteem, r (108) = -0.244, *p* = 0.010, lending support for H1.1. Additionally, we reported that SMA was positively related to depression, r (111) = 0.304, *p* = 0.001, supporting H1.2. In regards to answering our research questions, we reported that social media intensity (SMI) was not related to participants' self-esteem, r (108) = -0.006, *p* = 0.954, but negatively related

to depression, r (111) = -0.385, p = 0.001, providing answers to RQ1 and RQ2.

Priming and self-esteem

ANOVA showed no main effect of the question order effects on self-esteem (F (4, 239) = 0.19, p = 0.942, and H1.3 and H1.5 were not supported.

Priming and depression

Our ANOVA showed a main effect of the priming effects of the scale on depression (F (4, 292) = 7.03, p < 0.000), such that those who answered the SMA first scored higher on depression, compared with those assigned to other conditions presenting depression scale first. More specifically, Bonferroni post hoc tests showed no statistically significant difference between participants' depression in conditions 6 and 7 (p = 1.000), rejecting hypothesis 1.4. Furthermore, Bonferroni post hoc revealed that participants who responded to SMA first (condition 7) scored higher on depression than those who reported their depression before reporting SMU (condition 8: p < 0.000; condition 9: p = 0.006) and those in the control (condition 10: p = 0.003); thus, H1.6 was supported.

Table1.4

Mental Health by conditions

	Self-Esteem		Depression
	Mean		*Mean*
Condition 1	3.38 a	Condition 6	19.52 ab
Condition 2	3.37 a	Condition 7	20.52 b
Condition 3	3.45 a	Condition 8	15.69 a
Condition 4	3.41 a	Condition 9	16.79 a
Condition 5	3.31 a	Condition 10	16.60 a

Note. Means with different subscripts indicate a significant mean difference at p < .05. Comparisons are only within the self-esteem (conditions 1 to 5) or within depression (conditions 6 to 10) variables. Exact p-values are reported in the text. Results did not include any covariates since we did not detect any significant relationships.

Discussion

It is evident that the language of the SMU scale, whether it is intended to measure SM addiction or intensity, can influence participants' momentary judgments about their depression, but not self-esteem. We are not able to replicate the findings, as reported by (Mieczkowski et al., 2020), and they reported that presenting the intensity scale or the addiction scale first will elicit higher depression. Our findings can be explained by priming and question order effects (e.g., Garbarski et al., 2016; Hjortskov, 2017; Schwarz & Clore, 1983). When participants were presented with scales designed to measure SMA, mental representations and nodes were activated about the negative impacts of SM; this can make relevant information more salient to their minds, and, subsequently, cause them to make judgments derived from unpleasant experiences; whereas measuring participants' subjective mental health before the occurrence of SMA has no such effect (Mieczkowski et al., 2020). It is plausible that the mounting evidence suggesting a mixed relationship between SMU and depressive symptoms can be accounted for, at least to a certain extent, by the order in which the items are presented by the participants. Albeit the critical importance of question order in this paradigm of research about SMU and depression, we failed to find a study that explicitly reports the order (e.g., da Veiga et al., 2019; Elhai et al., 2016; Jorgensen & Nelson, 2018).

However, we hypothesized that question order effects would take place for those who responded to the self-esteem scale, but we did not detect any statistically significant results. Why did question order effects only affect those who responded to the depression scale and had no such effect on those who rated their self-esteem? According to a review article published by Trzesniewski et al. (2003) - whose primary goal was to investigate the stability of self-esteem - self-esteem is of relatively high stability across age, gender, ethnicity,

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nationality, and year of publication. In other words, self-esteem, in contrast to other mental health constructs, is less subject to environmental changes (Trzesniewski et al., 2003); moreover, the same review article revealed that self-esteem stability reached its peak in young adulthood and started to decline after this age period, and all of our participants are college students between the ages of 18 to 25, and, therefore, have the most stable self-esteem than their counterparts in other age groups. On the other hand, empirical evidence (Tanaka & Huba, 1987) suggested that college students reported relatively less stable depression than older adults. Moreover, Flett et al. (1995), in their two-wave longitudinal study separated by a three-month interval, reported substantial differences in terms of college students' depression scores from wave one to wave two. Therefore, a plausible explanation for why we only observed the question order effects for depression is due to the rather high stability of self-esteem, whereas depression is more prone to environmental factors among young adults.

Results from study 1 offer explicit recommendations for what to do for future SM and subjective mental health research. Theorizing the potential question order effects that can occur amid the group of constructs is worth considering before collecting survey data. Pretesting a questionnaire via randomization to reveal any significant differences resulting from the order is one valid approach to avoid question order effects. Given the priming effects discerned from study 1, what other potential explanatory are similar to or distinct from question order effects? Next, we adopt a different approach and focus on the wording of the scale utilized to measure mental health, which is an additional factor that can elicit priming effects and subsequently affect subjective mental health.

Study 2

The results of study 1 revealed the importance of question order effects, or priming effects, as a function of manipulating which SM scale to use and which scales to measure first. More specifically, we found that when answering questions about SMA (e.g., "You use social media in order to forget about personal problems") was presented first, it tended to have are higher depression score, though we did not find a similar pattern with self-esteem. The presence of both SMU scales and mental health measures in surveys presents a standardized research concern regarding how one predictor affects the other; in an attempt to test or replicate the question order effects in the absence of SMU scales, the first goal of our second study is to test the whether the question order effects that we found in study 1 are replicated in the context of asking two mental health items in the same survey.

Indeed, the wording of the BSMAS emphasizes certain undesirable, excessive, and addictive symptoms of addiction, which can prime the activation and accessibility of mental nodes connected to unwanted outcomes. We have evidence suggesting that exposure to scales designed to measure addictive behaviors can elicit priming effects; however, less is known about the priming effects of answering mental health items on a survey. When respondents are exposed to the depression scale, the items measuring depressive symptoms function as reminders, priming the participants to consider depression and enhancing the availability of mental nodes featuring depression. We therefore hypothesize:

H.2.1 Participants who respond to the depression scale first will have lower levels of self-esteem than those who answer the self-esteem scale first.

Note that the wording of the addiction scale and intensity scale essentially feature excessive addictive behaviors and regular and non-problematic SMU. All items in the

BSMAS center on addictive behaviors – not intentionally acquired by any user, presumably – but EIS even includes certain language that can prime the users to think about the positive aspects and benefits of SM (e.g., "I feel I am part of the Facebook community"). Thus, it can prime the mindset that it can help users feel more connected with others and create a stronger sense of community in cyberspace. It is of the highest importance to acknowledge that both BSMAS and EIS did not include the opposite descriptors, meaning that participants who responded to the addiction scale were not given the chance to indicate that none of the symptoms applied to me, and, likewise, young adults answering the EIS were not reminded of the potential harm of SMU. While it is convenient for the research team not to need to reverse code items (e.g., Maroufizadeh et al., 2019), we intend to test the effects wording further in the context of mental health alone, without necessarily including scales about SMU.

Our study 2 centers not only on the order but also the wording of the mental health scales. The PHQ-9 (Kroenke et al., 2001; Kroenke & Spitzer, 2002) scale adopted in study 1 is a self-administered questionnaire measuring individuals' depressive symptoms, with its sum index indicating respondents' severity of depression. Its sample items such as "trouble falling or staying asleep, or sleeping too much" and "moving or speaking so slowly that other people could have noticed" or the opposite being "so fidgety or restless that you have been moving around a lot more than usual" are clinically describing depressive symptoms, and we intend to test whether or not the same priming or question order effects can impact users' responses to other mental-health measures.

Inquiries concerning how the wording of the materials used in experiments shed some

light on this phenomenon, and we noticed that anchoring effects can be utilized as an explanatory mechanism. Anchoring effects, used to illustrate the effects of priming (Newell & Shanks, 2014), refer to people's tendency to rely on the information that is first presented to them (anchor), and, consequently, people's judgments about their momentary affective states are impacted by the mental nodes activated by such anchor (e.g., Araujo et al., 2017; Englich et al., 2006; Strack & Mussweiler, 1997). In many cases, an anchor is set as a numerical value; for example, Englich et al. (2006) reported that experienced attorneys were inclined to conjure up a longer sentencing decision when asked by the high-anchor-question (i.e., "Do you think that the sentence for the defendant, in this case, will be higher or lower than 3 years?") than those encountered the low-anchor-question (i.e., "Do you think that the sentence for the defendant in this case will be higher or lower than 1 year?"). Note, level of seniority in this study did not alter lawyers' subsequent judgment, and Wilson et al. (1996) reported that even if participants were prewarned about the anchoring effects, they were not less susceptible to their influence. Moreover, numerical indicators do not necessitate that anchoring effects will subtlety take place, and other forms of anchors can function as primes. For example, LeBoeuf and Shafir (2006) illustrated that physical attributes of stimuli (i.e., length, weight, loudness) can be processed as anchors, even though they differ from traditional numerical estimations. Mounting evidence suggests the profound impact of anchoring effects, and we propose that as a form of priming, anchoring itself plays a crucial role in the SMU and mental health paradigm.

While anchors can be pre-designed by experimentalists (see Newell & Shanks, 2014 for a review), there is no assertion that the anchors cannot manifest themselves in the mindset of

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the participants, even if that was not the experimentalists' intent. That is, a self-generated anchor (e.g., instructed to estimate the freezing point of Vodka without giving any reference information) and externally-provided anchor (e.g., showing the participants short vs. long lines before performing priming tasks) can both cloud people's thoughts and behaviors in subsequent priming tasks (Epley & Gilovich, 2005). The notion of a self-generated anchor is striking since we contend that how respondents self-generate their affective states' anchor while completing a survey measuring subjective mental health. Prior to predicting the plausible effects of anchoring, we have decided to take a closer look at some commonly utilized scales measuring ill-being as our starting point.

After reviewing other ill-being scales, we have noticed that scales centering on ill-being constructs often describe symptoms or less evaluatively desirable status quo, and those constructs include loneliness (e.g., "I feel isolated from others"; Roberts et al., 1993), fear of missing out (e.g., "I get worried when I find out my friends are having fun without me"; Beyens et al., 2016), and stress (e.g., "in the last month, how often have you felt that things were going your way"; Cohen et al., 1983). What occurs when we instruct our participants to appraise a number of undesirable symptoms/experiences in detail is unclear. Does asking for more detailed episodes of the less enticing experience advance the outcome variable of our interests? Huang and Cornell (2015) conducted an experimental study and concluded that asking a series of specific incidents related to cyber-bullying items before answering a standardized scale to measure cyberbullying, compared with those who only saw the scale measuring cyberbullying, reported higher cyberbullying scores. Will exposure to a number of questions measuring similar, consistent, and analogous constructs prime the participants to

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self-generate an anchor, which can induce variance in thoughts and behaviors? Does a scale that only focuses on the negative aspects accentuate the unpleasant experiences and make positive nodes less accessible in the context of SMU and mental health? If so, how do we combat such effects and appraise a more precise measure of self-reports? We predict that the wording of the mental health scales affects the magnitude of anchoring effects, such that when participants answer a number of items describing similar events or experiences, they are more prone to set an anchor, whereas it is more difficult to manifest an anchor when a scale contains reverse-coded items. More specifically, a participant who responds to the 9-item PHQ-9 scale of depression may form an anchor as they progress through the scale (Kroenke & Spitzer, 2002). Even if a participant encounters a few symptoms that they have not experienced before, they can be primed to think of them as more pronounced than they otherwise would as a result of the pre-established anchor. Conversely, the presence of certain items that are worded differently from others can induce interpretation, inhibiting the constitution of a self-generated anchor.

Further, as noted earlier, the frequency of activation refers to the effects that repeated and monotonous exposure to the same topic has on subsequent responses (Higgins, 1996; Higgins & Eitam, 2014). If an item has only positively or negatively worded items measuring a construct, will the final measure be subject to the impact of frequency of activation? For example, the PHQ-9 (Kroenke & Spitzer, 2002) consists of nine items measuring the severity of depression, and is the total depression score subject to change when we reverse the wording of certain items, similar to the function of a bomber question adopted by Lasorsa (2003)? We conjecture that adding certain items and transforming a one-sided scale into a

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mixed-wording scale will impact participants' responses. We thus reconcile the arguments from anchoring effects with the concept of frequency of activation and make the following hypothesis:

H.2.2 Participants who respond to the depression scale that has mixed wording will have higher self-esteem and lower depression, compared with those responding to the depression scale that has only negative wording.

H.2.3 Participants who respond to the self-esteem scale that has mixed wording will have lower self-esteem and higher depression, compared with those responding to the self-esteem scale that has only positive wording.

Methods

Participants

We conducted a between-subject experiment online. Overall, 799 young adults between the ages of 18 and 25 completed our experiment. Our final sample consisted of 571 participants after removing (a) participants who did not pay attention to the attention check questions, and (b) participants who failed to pass the manipulation check. In regards to their ethnicity, 51.97% identified as Asian/Asian American (n =295), 21.7% identified as Caucasian (n = 124), 14.1% of participants identified as Hispanic/Latino (n = 86), 1.8% of participants identified as Black/African American (n = 10), 0.7% of participants identified as Pacific Islander (n = 4), 0.2% of participants identified as Native American (n = 1), 3.3% of participants identified as Mixed (n = 19), and 0.6% did not answer this question (n = 32). Our sample consisted of 37.0% males (n = 211) and 63.0% females (n = 358). Note that we

removed those who did not pass the attention check questions (i.e., we instructed our

participants to select a fixed choice while reporting their mental health) and those who failed

to pass the manipulation check questions (i.e., we asked our participants to indicate whether

or not they saw certain items measuring depression in the first block).

Table 2.1

Demographics of Our Participants in Study 2

		Percent	n
Age	18	21.0%	120
	19	28.9%	165
	20	23.5%	134
	21	13.8%	79
	22	7.2%	41
	23	2.1%	12
	24	2.5%	14
	25	1.1%	6
	Did not answer	0%	0
Sex	Male	37.0%	211
	Female	63.0%	358
	Other	0.3%	2
	Did not answer	0%	0
Race/Ethnicity	Asian	51.7%	295
	Caucasian	21.7%	124
	Hispanic/Latino	14.1%	86
	Black/African American	1.8%	10
	Pacific Islander	0.7%	4
	Native American	0.2%	1
	Mixed	3.3%	19
	Self-described	0%	0
	Did not answer	0.6%	32

Note. Since we did not assign compulsory demographic questions, some participants did not

answer certain questions. Because percentages are rounded to one decimal, the sum index may not exactly be 100%.

Procedure

Following the approval by the university Institutional Review Board in 2022, we posted our study URL online for recruitment purposes. Our study was hosted by a survey service company, Qualtrics. When participants entered the survey, they saw the informed consent and the principle investigator's contact details. The survey took about 10 to 15 minutes to complete for participants to complete. Study participants were undergraduate students at a public university in the United States enrolled in psychology courses; participants were compensated for their time with extra credit. Before the exposure to the stimulus, participants answered questions about their demographics. Then, each participant was assigned to one of the eight experimental conditions (see table 2.2). We designed a 2x2x2 factorial experiment and manipulated the following three aspects: first, we altered the order in which the participants responded to either the self-esteem scale first or the depression scale first. Second, we adjusted the wording of the self-esteem scale, such that it had two versions with one scale containing items that need to be reverse-coded, and another version including all positive wording of self-esteem that did not require reverse-coding. We used a similar manipulation for the depression scale, randomly assigning some participants to the original PHQ-9 scale, and others to another version with certain items describing the opposite of depressive symptoms. See table 2.3 for detailed wording of the items.

Table 2.2

Conditions Assigned to Study 2 Participants

Condition 1	SE (mixed)	Depression (mixed)
Condition 2	SE (one-sided)	Depression (one-sided)

Condition 3	SE (mixed)	Depression (one-sided)
Condition 4	SE (one-sided)	Depression (mixed)
Condition 5	Depression (mixed)	SE (mixed)
Condition 6	Depression (one-sided)	SE (one-sided)
Condition 7	Depression (mixed)	SE (one-sided)
Condition 8	Depression (one-sided)	SE (mixed)

Measures

Self-Esteem. We measured users' self-esteem via the Rosenberg Self-Esteem Scale (Rosenberg, 1965). This is a ten-item measure anchored from 1 (strongly disagree) to 5 (strongly agree). Sample items included "At times I think I am no good at all," and "I feel I do not have much to be proud of," $M = 3.53$, $SD = 0.70$. We changed the wording of five items that needed reverse-coding for participants assigned to the other version of the self-esteem scale. Together, we coded self-esteem so that higher numbers indicate higher self-esteem. See Appendix B for a detailed description of the items.

Depression. We measured participants' depression via the Patient-Health-Questionnaire-9 scale (PHQ-9, Kroenke & Spitzer, 2002) anchored from 1 (not at all) to 4 (nearly every day). Sample Items included "Trouble concentrating on things, such as reading the newspaper or watching television" and "Moving or speaking so slowly that other people could have noticed or the opposite being so fidgety or restless that you have been moving around a lot more than usual," $M = 18.86$, $SD = 5.64$. We changed the wording of four items to describe the opposite of depression for participants assigned to the other depression scale condition. We coded depression so that higher numbers indicate higher levels of depression. See appendix B for a detailed description of the items. We did not compute a total Cronbach's α for self-esteem and depression but computed it for each condition. See table 2.3.

Table 2.3

Conditions Assigned to Study 2 Participants

	Cronbach's α	
	Self-Esteem	Depression
SE (mixed) +Depression (mixed)	0.78	0.74
SE (one-sided) + Depression (one-sided)	0.88	0.90
SE (mixed) +Depression (one-sided)	0.86	0.84
SE (one-sided) +Depression (mixed)	0.90	0.73
Depression (mixed) +SE (mixed)	0.81	0.90
Depression (one-sided) +SE (one-sided)	0.89	0.88
Depression (mixed) +SE (one-sided)	0.76	0.90
Depression (one-sided) +SE (mixed)	0.89	0.86

Results

Preliminary analyses

Data were analyzed via SPSS 27. First, we ran a correlation matrix with our variables to determine which to be included as the covariates. We found that sex and age were significantly correlated with depression; thus, we decided to run a MANCOVA. For all significant results, we report the main effects and post hoc tests in the text, and all other information is presented in the respective tables. Because we did not detect any significant interaction results, we did not report the interaction effects in each section (all p's ≥ 0.13).

Table 2.4

Correlations Between the Variables

	1	2	3	4	5
1. Age	1				
2. Sex	0.001	1			
3. Ethnicity	0.076	0.08	1		
4. Self-Esteem	-0.062	-0.008	-0.011	1	
5. Depression	0.088*	0.086*	-0.001	-0.500**	1

Note. **denotes that correlation is significant at the 0.01 level; *denotes that correlation is significant at the 0.05 level.

Question order and mental health

To test H2.1, we conducted a MANCOVA with self-esteem and depression as the dependent variables and controlled for age and sex. The multivariate test showed a main effect of the wording of the depression scale (Wilks' $\lambda = 0.84$, $p = 0.000$, $\eta p^2 = 0.16$) and the wording of self-esteem scale (Wilks' $\lambda = 0.92$, $p = 0.000$, $\eta p^2 = 0.08$), respectively. However, we failed to detect the main effect of the question order. Therefore, H2.1 was not supported.

Wording of depression scale and mental health

The between-subject effects showed a main effect of depression scale wording on depression but not on self-esteem. Bonferroni post-hoc analyses indicated that participants who responded to mixed wording ($p = 0.000$) scored lower on depression compared with those who saw the original PHQ-9 scale, which only contained language explicitly describing depressive symptoms ($p = 0.000$). Thus, H2.2 was partially supported.

Wording of self-esteem scale and mental health

The between-subject effects showed a main effect of the wording of the self-esteem scale on self-esteem but not on depression. Bonferroni post-hoc analyses indicated that participants who responded to mixed wording ($p = 0.000$) scored lower on self-esteem, compared with those who saw the modified Rosenberg's Self-Esteem scale (1965) which contained only positive descriptions of self-esteem ($p = 0.000$). Thus, H2.3 was partially supported.

Table 2.5

Mean of Mental Health by Order, Wording of Depression and Self Esteem

Mental health by order

	Depression	Self-esteem
Self Esteem first	18.71a	3.50a

| Depression first | 19.21$_a$ | 3.54$_a$ |

Mental health by the wording of depression

	Depression	*Self-esteem*
Mixed wording	17.02$_a$	3.50$_a$
One-sided wording	20.90$_b$	3.54$_a$

Mental health by the wording of self-esteem

	Depression	*Self-esteem*
Mixed wording	19.05$_a$	3.35$_a$
One-sided wording	18.87$_a$	3.69$_b$

Note. Means with different subscripts indicate a significant mean difference at $p < .05$. Exact p-values are reported in the text. Results did not include any interactions since there was no presence of interaction effects.

Discussion

In H2.1 we predicted that attention and responses to the depression scale would result in diminished self-esteem, compared with those who had seen items measuring self-esteem. However, we did not find any significant results in this regard. As H2.2 predicted, exposure to a depression scale containing both positively- and negatively-worded descriptors resulted in a better self-reported mental health outcome (i.e., lower depression) than those exposed to a depression scale with only negative wording. Additionally, we hypothesized that responding to the original Rosenberg Self Esteem scale, which has mixed wording, would result in decreased affective states; results revealed an association between scale wording and self-esteem but not depression.

These findings, combined with other empirical studies of subtle priming effects, lead us to center on a plausible explanation: the narrowness of anchoring. The narrowness of anchoring effects refers to a phenomenon wherein the dilution of anchoring effects is not cross-modal but intra-modal (Newell & Shanks, 2014). That is, reading the word dog will

have a robust priming effect in tasks asking participants to write the word dog, yet the robustness of the priming is reduced when participants first hear the word dog instead of reading it (Roediger & McDermott, 1993). Additional research concerning the magnitude of anchor transfer offers support to the notion of the narrowness of anchoring effects; Frederick and Mochon (2012) conducted experimental work and reported that estimating the weight of a raccoon in pounds influenced participants' sequential judgment of the weight of a giraffe in pounds. However, the anchoring effects no longer existed after they changed the response options from pounds (i.e., 7-Likert heaviness scale, estimation in tons). The above-mentioned anchoring effect did not transfer, and limited empirical evidence has had difficulty replicating the results (Oppenheimer et al., 2008). Overall, the effectiveness of anchoring effects is subject to constraints such as modality (e.g., reading vs. hearing), estimation scale (e.g., estimation in tons vs. in pounds), and other plausible factors.

Does this narrowness of transfer apply to responding to multiple mental health constructs? In light of the results of study 2, which found support for the anchoring effects of mixed-wording of items on a mental health scale can result in lower reports of depression in that same scale and that such anchor effects do not impact subsequent scales. The narrowness of the anchoring effect offers an explanation for why the effects of mixed-wording did not transfer across to the other construct. Depression and self-esteem in the current study are only moderately correlated ($r = -0.50$), which is consistent with moderate correlations documented in the literature (Battle, 1987). It is a common practice to conceptualize depression and self-esteem as correlated but distinct constructs, and this helps explain why the anchor transfer occurred in our current work.

Results of study 2 provide researchers interested in understanding the relationship between SM and subjective mental health with a useful and practical suggestion; that is, to create a balance between the negatively worded items (e.g., I wish I could have more respect for myself; original item from Rosenberg, 1965) and positively worded items (e.g., I have enough respect for myself; modified from the original items from Rosenberg, 1965). Depression, loneliness (e.g., Roberts et al., 1993, fear of missing out (e.g., Beyens et al., 2016), and stress (Cohen et al., 1983) are often measured with scales comprised of consistently negatively worded items, which can increase the mean than asking participants to respond to a mix of positively and negatively worded items. Understood this way, it is problematic to use scales comprised solely of negatively worded items, which elicit higher levels of ill-being, to conduct statistical models and assert associations between SM and subjective mental health. In summary, before making conclusions about how SM affects subjective mental health, future work should modify certain scale items to adjust for possible increases or decreases in the mean.

We, of course, admit that anchoring effects and question order effects are only two examples of priming, and caution needs to be taken when drawing conclusions about the applicability of the findings to other incidents of priming. Altogether, the results of study 1 and study 2 indicate it is future work should account for question order effects and anchoring effects, and from a methodological perspective, we intend to take one more step and simulate a more realistic environment where participants are exposed to messages drawing a conclusion about SM (e.g., news articles published by news outlets, research articles published by institutes). In other words, we are interested in examining the pronounced

effects of priming in a more realistic setting.

Study 3

The New York Times (2021) published an article discussing the potentially harmful effects of SM and researchers should consider the presence of similar pieces elaborating on how SM companies design their products to attract users and get them to spend more time on SM. Further, when participants open the online survey, we suspected the wording of the items would impact their response to subsequent items on the same instrument. Regarding the paradoxically conflicting results of the association between SM and mental health conducted by scholars (e.g., Beyens et al., 2021; de Lenne et al., 2020; Krause et al., 2019), it is implausible to report a calculated percentage of the positive and negative content on one's SM feed without accessing its backend data (e.g., Ernala et al., 2020) owing to the person-specific-driven algorithm (Petrescu & Krishen, 2020). Participants likely enter studies with predispositions toward how they think social media might impact their mental health. Participants encounter a variety of content on social media, and our study 3 investigates the extent to which SM users; subjective mental health levels are influenced by a recent article (our stimulus). How does exposure to a message, article, or post, asserting the valence of SM's impact on mental health (i.e., it is either beneficial, harmful, or neutral) affect subjective mental health? As alluded to in our previous discussion of priming and question order effects, it is at the heart of our next inquiry to test the priming effects (Araujo et al., 2017; Englich et al., 2006; Strack & Mussweiler, 1997) in the context of SM and subjective mental health. Context priming, a sub-category of priming, holds its root in thematic priming (Stanovich & West, 1983) and has been expanded into other fields (Zaval et al., 2014).

Context effects, popularized by Stanovich and West (1983), refer to the presence of a stimulus that speeds up processing likely to occur in that given context. It has often been defined as a category of priming effects because it unconsciously impacts people's subsequent behaviors by exposing them to stimuli situated within a context (e.g., Bloom & Hautaluoma, 1987; Stanovich & West, 1983). More specifically, Stanovich and West (1983) demonstrated that words participants read alone, compared with those who did not read any words, were more likely to formulate or complete sentences using the preceding words. Another area of work deals less with the thematic aspect of context priming, showing how context priming can alter participants' concerns about global warming (Zaval et al., 2014). In general, Zaval and colleagues (2014) reported that when the current weather was cold, participants tended to believe that global warming was less severe; conversely, when the weather was hot, participants were more likely to believe in the effects of global warming. In an attempt to uncover why context affected participants' perception of global warming, Zaval and colleagues (2014) instructed participants to finish a scrambled-sentences priming task and randomly assigned participants to three conditions (i.e., heat-prime, cold-prime, or control). They found that those assigned to the heat-prime condition had higher beliefs in and concerns about global warming than those in the cold-prime condition and control group. One can argue that weather is akin to global warming, and it is not surprising that priming effects were detected. Other research investigating contextual primes of weather, interestingly, affected people's overall life satisfaction (Schwarz & Clore, 1983).

If we design a context wherein the valence of SM is highlighted, we may be situating participants in an environment where context effects will cloud their judgments about their

momentary states. Accordingly, we predict that reading an article featuring either the benefit or harm of SM will increase the salience of relevant information when participants are processing information. We thus hypothesize:

H3.1 Young adults who read the article discussing the benefits of SM will report higher self-esteem than those who read the article discussing the harm of SM.

H3.2 Young adults who read the article discussing the benefits of SM will report lower depression than those who read the article discussing the harm of SM.

Methods

Pretest

We conducted a pretest to finalize the stimuli language and ensure corresponding priming effects. A total of 95 undergraduate students - enrolling in an introductory communication course in the U.S. - participated in our pretest and were assigned to one of two conditions. Participants were asked to read a short article. Each article was modified from a real article to meet the needs of the study. Depending on the condition participants were assigned to, participants either read an article emphasizing the harm or benefits of SMU. We presented these articles to our participants and then evaluated their attitudes towards social media in general (i.e., a seven-point Likert scale adapted from Ajzen, 2020). The pretest results indicated that students exposed to the SMU harm condition ($M = 3.92$, $SD = 1.07$) reported less favorable attitudes towards social media compared with those who read the article underscoring the benefits ($M = 4.41$, $SD = 1.22$), $t(93) = -2.04$, $p = 0.020$. We, therefore, decided to use these articles in follow-up experimentation.

Participants

We conducted a between-subject experiment online. Overall, 745 young adults between the ages of 18 and 25 completed our experiment. Our final sample consisted of 656 participants after removing (a) participants who did not pay attention to the attention check questions and (b) participants who failed to pass the manipulation check. In regards to their ethnicity, 51.7% identified as Asian/Asian American (n =339), 22.9% identified as Caucasian (n = 150), 14.8% of participants identified as Hispanic/Latino (n = 97), 1.8% of participants identified as Black/African American (n = 12), 0.2% of participants identified as Native American (n = 1), 0.2% of participants identified as Pacific Islander (n = 1), 3.7% of participants identified as Mixed (n = 24), and 4.9% did not answer this question (n = 32). The majority of participants were female (n = 478, 72.9%).

Table 3.1

Demographics of Our Participants in Study 2

		Percent	n
Age	18	21.1%	139
	19	32.9%	216
	20	20.6%	135
	21	12.0%	79
	22	7.3%	48
	23	3.5%	23
	24	1.7%	11
	25	0.8%	5
	Did not answer	0%	0
Sex	Male	25.5%	167
	Female	72.9%	478
	Other	0.8%	5
	Did not answer	0.9%	6

Race/Ethnicity	Asian	51.7%	339
	Caucasian	22.9%	150
	Hispanic/Latino	14.8%	97
	Black/African American	1.8%	12
	Native American	0.2%	1
	Pacific Islander	0.2%	1
	Mixed	3.7%	24
	Self-described	0%	0
	Did not answer	4.9%	32

Note. Since we did not assign compulsory demographic questions, some participants did not answer every question. Because percentages are rounded to one decimal, the sum index may not exactly be 100%.

Procedure

Approved by IRB and hosted on the survey company Qualtircs, this research was completed by recruiting college students in exchange for extra credit. Upon reading the informed consent about the study and agreeing to participate, participants proceeded to the rest of this questionnaire. This survey took about 5 – 10 minutes to complete. In our between-subject design, participants were randomly assigned to one of the following six conditions. In the first condition, participants read the article underscoring the benefits of SM and, then, answered the self-esteem scale. In the second condition, participants read the article about the harm of SM and answered the self-esteem scale. In the third condition, participants read a neutral article (adapted from an article introducing National Geographic) that does not involve any information about SM and responded to the self-esteem scale. The design of conditions 4, 5, and 6 were similar to that of conditions 1, 2, and 3, except that we replaced the self-esteem scale with the depression scale (see table 3.2). We collected participants'

attitudes towards SM in all conditions and instructed our participants to answer the

manipulation check before exiting the survey; please see table 3.2 for a detailed design of the

conditions and Appendix A for the stimuli.

Table 3.2

Conditions Assigned to Study 3 Participants

Conditions	Stimuli
Condition 1	An article about the benefits of SM + self-esteem
Condition 2	An article about the harm of SM + self-esteem
Condition 3	Read a neutral article + self-esteem
Condition 4	An article about the benefits of SM + depression
Condition 5	An article about the harm of SM + depression
Condition 6	Read a neutral article + depression

Measures

Self-Esteem. We assessed users' self-esteem via the Rosenberg Self-Esteem Scale

(Rosenberg, 1965). This scale is anchored from 1 (strongly disagree) to 5 (strongly agree).

Sample items included "At times I think I am no good at all," and "I feel I do not have much

to be proud of," $M = 3.10$, $SD = 0.69$, Cronbach's $\alpha = 0.89$. We coded self-esteem so that

higher numbers indicate higher self-esteem.

Depression. We assessed participants' depression via the Patient-Health-Questionnaire-

9 scale (PHQ-9, Kroenke & Spitzer, 2002) anchored from 1 (not at all) to 4 (nearly every

day). Sample Items included "Trouble concentrating on things, such as reading the newspaper

or watching television" and "Moving or speaking so slowly that other people could have

noticed or the opposite being so fidgety or restless that you have been moving around a lot

more than usual," $M = 19.88$, $SD = 6.24$, Cronbach's $\alpha = 0.87$.

Attitudes towards social media. We assessed participants' attitudes toward social media

employing the scale suggested by (Ajzen, 2020) - the same we used in our pretest. We informed our participants to rate their attitudes on a 7-point-Likert scale from 1 (bad, unpleasant, harmful) to 7 (good, pleasant, beneficial) and computed an average score representing attitudes toward SM ($M = 4.13$, $SD = 1.18$, Cronbach's $\alpha = 0.87$).

Results

Preliminary analyses

All data were analyzed via SPSS 27. First, we ran a correlation matrix to determine which to include as covariates. Since we measured depression and self-esteem separately, we observed different significant covariates for depression and self-esteem. We found that sex was significantly correlated with depression, and attitudes towards social media and age were significantly associated with self-esteem; thus, sex in the ANCOVA testing depression as the dependent variable.

Table3.3

Correlations Between the Variables

	1	2	3	4	5	6
1. Age	1					
2. Sex	-0.035	1				
3. Ethnicity	0.061	0.062	1			
4. Attitude	-0.039	-0.019	-0.121*	1		
5. Self-esteem	0.210**	-0.040	0.138	0.224**	1	
6. Depression	-0.092	0.187**	-0.051	-0.043	NA	1

Note. **denotes that correlation is significant at the 0.01 level; *denotes that correlation is significant at the 0.05 level.

SM and self-esteem

To test H3.1, we conducted an ANCOVA with self-esteem as the dependent variable while controlling for attitudes towards social media and age. The between-subjects effects

showed a main effect of the valence of the article (F (2, 323) = 6.85, p = 0.001) on

participants' self-esteem, providing support for H3.1. Bonferroni post-hoc analyses indicated

that participants who read the article emphasizing the benefits of SM scored higher on self-

esteem (p = 0.047), compared with those who read the article focusing on the harm of SM.

SM and depression

To test H3.2, we conducted an ANCOVA with depression as the dependent variable and

controlled for sex. The between-subjects effects showed a main effect of valence in the article

(F (2, 322) = 2.82, p = 0.016) on depression, offering support for H3.2. Bonferroni post-hoc

analyses indicated that participants who read the article emphasizing the benefits of SM

scored lower on depression (p = 0.045), compared with those who read the article focusing on

the harm of SM.

Table 3.4

Mental Health by conditions

	Self-Esteem		Depression
	Mean		*Mean*
Condition 1: Positively valenced article	3.33_b	Condition 4: Positively valenced article	18.70_a
Condition 2: Negatively valenced article	3.01_a	Condition 5: Negatively valenced article	20.39_b
Condition 3: Neutral article	3.22_b	Condition 6: Neutral article	20.44_b

Note. Significant mean differences at $p < .05$ are denoted with subscripts. No presence of
different subscript suggests no significant mean difference.

Discussion

We predicted that the valence of an article can stimulate context (priming) effects. We

are intrigued by the inconsistency between study 1 and study 3. Study 1 indicated that self-

esteem was not subject to priming effects but that depression was, whereas study 3 indicated that both depression and self-esteem are subject to context (priming) effects. What can account for this inconsistency? There is a large body of literature documenting the magnitude of priming effects, and we highlight the misattribution and judgment of well-being proposed by Schwarz and Clore (1983); they posited that context valence affected subsequent appraisal of current affective states. In other words, people tend to alter their mode of information processing in differential contexts; analytical reasoning is more likely to occur in the presence of cues indicating unpleasantness (Schwarz & Bless, 2020). However, the manipulations utilized across conditions produced changes as a function of the valence of the article. It is implausible that the article highlighting the pros of SM set a context was so distinctly different from the other article that it elicited less analytical reasoning. What else can account for this inconsistency?

We note that a clear distinction between study 3 and studies 1 and 2 is that study 3 mimics a real-life situation whereby students encounter a massive amount of information about the benefits or harm of SM, and we argue that the pronounced priming effect elicited in study 3 can be attributed to it being in a more natural and realistic setting than studies 1 and 2. We assigned participants to read an article that either highlighted the benefits or harm of SM to place our participants in a context where we could manipulate article valence. As indicated and synthesized by Bargh (2014), a line of research investigating how real-life contexts, rather than a laboratory ambiance, affect participants' affective states will nourish our understanding of priming and produce more useful practical applications. Though we did not find an article that explicitly tested the context (priming) effects in the case of SMU and

subjective mental health, the context (priming) effects of the polling location (e.g., in a church; in a school), for example, and its subsequent primed voting behaviors (e.g., increased votes for religion issues; increased votes for education issues), the number of fast-food restaurants and how this seemingly unrelated number primed users to make faster financial decisions (Devoe et al., 2013), and the pedestrians who were primed to walk slowly surveyed in front of a full-service restaurant than those completed the questionnaire adjacent to a fast-food restaurant (Devoe et al., 2013) offer empirical evidence of how priming functions in the real world. Despite the rich accumulation of research investigating the impact of SM on mental health, we adopted a readable news article prevalently shared to SM. Future inquiries should consider measuring users' attitudes towards social media before and after being exposed to stimuli, given that it is implausible to record the last thing seen by the participants before being part of an experiment. Employing a similar approach and rationale, we created an article whose kind pervades the Internet and were able to detect the priming effects that were even more profound than what we found in study 1.

Overall Discussion

Summary of the results

Regarding the relationship between the SMU (i.e., SMA, SMI), we predicted that only SMA would be associated with worse subjective mental health outcomes and inquired whether the same would hold for intensity. Our results revealed that SMA was associated with worse mental health outcomes (i.e., H1.1: lower self-esteem; H1.2: higher depression), supporting H1.1 and H1.2. We also predicted that exposure to the addiction scale first would

lower participants' self-esteem (H1.3) and heighten their depression (H1.4), and we found

evidence in support of H1.4 and rejected H1.3. Additionally, we predicted that exposure to

self-esteem first (H1.5) and depression first (H1.6), compared with those viewed scales

measuring SMU, yielded higher self-esteem and lower depression, respectively, offering

support for H1.6 but not H1.5.

We hypothesized in our H2.1 that exposure to the depression scale (i.e., priming an ill-

being construct) first would result in lower participant self-esteem compared to participants

who saw items measuring self-esteem (priming a well-being construct) first, yet we found no

significant relationship. Interestingly, as we predicted in H2.2, exposure to a depression scale

that has a mixture of both positive and negative descriptors can elicit a better mental health

outcome (i.e., lower self-reports of depression). Notably, we hypothesized that seeing the

original Rosenberg Self Esteem scale, which contains mixed wording, would lead to worse

mental health outcomes; findings offered support for the predicted relationship between

mixed wording and self-esteem, but not mixed wording and depression.

Furthermore, we predicted that the article emphasizing the benefits of SM would result

in better mental health outcomes, and, conversely, we predicted that the article emphasizing

the harm of SM would prime users to lower their momentary mental health. As predicted in

H3.1 and H3.2, our analyses indicated that article valence impacted participants' self-esteem

and depression.

Table 3.4

Summary of the Hypotheses and Research Questions

Study 1

H1.1. Participants' self-esteem will be negatively related to the   Supported

49

addiction scale.	
H1.2. Participants' depression will be positively related to the addiction scale.	Supported
RQ1.1. What is the relationship between self-esteem and intensity scale?	Answered
RQ1.2. What is the relationship between depression and intensity scale?	Answered
H1.3. Participants who report to the addiction scale first will report lower self-esteem scores than participants who respond to the intensity scale first and	Rejected
H1.4 Participants who report to the addiction scale first will report higher depression scores than participants who respond to the intensity scale first.	Supported
H1.5 Participants who report to the self-esteem scale first will report higher self-esteem scores than participants who respond to the SM scale (i.e., addiction scale, intensity scale) first,	Rejected
H1.6 Participants who report to the depression scale first will report lower depression scores than participants who respond to the SM scale (i.e., addiction scale, intensity scale) first.	Supported

<div align="center">Study 2</div>

H2.1 Participants who respond to the depression scale first will have lower levels of self-esteem than those who answer the self-esteem scale first.	Rejected
H2.2 Participants who respond to the depression scale that has mixed wording will have higher self-esteem and lower depression, compared with those responding to the depression scale that has only negative wording.	Partially supported
H2.3 Participants who respond to the self-esteem scale that has mixed wording will have lower self-esteem and higher depression, compared with those responding to the self-esteem scale that has only positive wording.	Partially supported

<div align="center">Study 3</div>

H3.1 Young adults who read the article discussing the benefits of SM will report higher self-esteem than those who read the article discussing the harm of SM.	Supported
H3.2 Young adults who read the article discussing the benefits of SM will report lower depression than those who read the article discussing the harm of SM.	Supported

Overall, the present work investigating the priming effects taking place in SMU

research employing survey methodology suggests that: (a) the SM scale utilized (e.g.,

addiction, intensity), (b) the order of the items, (c) the inconsistency of the wording within the same scale, (d) and the presence, and valence of the materials exposed to participants before they self-report their mental health can affect their responses. Though McFarland (1981) endorsed the importance of question order in survey research, it has received minimal attention from the communication research community, especially given the prevalence of survey research (see Mieczkowski et al. 2020 for an exception). To our knowledge, this current work is the first of its kind to investigate priming effects in the context of SMU and well-being, including at least one well-being construct (i.e., self-esteem) and one ill-being construct (i.e., depression).

Implications. The growing literature documenting priming effects has its root in thematic priming, and research after Lashley (1951) utilized terms such as unrelated effects and carryover effects until the notion of priming effects was firmly established by Higgin and his colleagues (1977). Our work considers three aspects of priming: question order effects, anchoring effects, and context effects in three experimental settings. Our work contributes to the literature in theoretical and practical manners. First, our evidence supported the priming effects as a function of question order, the wording of the item (anchoring), and the context effects, and our paper is the first of its kind that explicitly manipulated the survey instrument as its experimental stimuli.

It is of course unsurprising to report priming effects amid our three experiments, but we intend to discuss the inconsistency and contradiction that we observed and their theoretical implications. First, in light of the question order effects that we reported in study 1 (i.e., participants who answered the BSMAS first scored higher on depression than those who

answered the BSMAS second), we were not able to replicate this finding in study 2. Note that the narrowness of priming effects indicates that the effect of question order is considered narrow (Bargh, 2014), and while participants engaged in appraising their affective states that had either one-sided or mixed wording, question order effects' transfer was constrained, and the role of wording is more conspicuous. Furthermore, Kuru and Pasek (2016), in their work, attempting to avoid systematic bias in survey research, postulated that reserving an item and phrase it was a common exercise in survey research to avoid acquiescence bias. Their call to advance the items that we utilize in social media research aligns with the goal of study 2, and we argue that the relatively higher average score of the one-sided wording items, in sharp contrast with the mixed-wording items, was influenced by acquiescence bias – predicting that survey respondents tended show high levels of agreeableness in survey research without necessary accessing their memories before answering an item (Hurd, 1999).

The second main contribution of this piece lies in its directive value in survey methodology and instrument construction. Amid the articles investigating the relationship between SMU and subjective mental health that we have reviewed insofar, it is a rarity to notice the order in which the items in the instrument reported in the method section (e.g., Ballantine et al., 2015; Brailovskaia & Margraf, 2016; Cingel & Olsen, 2018; Cohen et al., 2017). Furthermore, among the work that investigates a battery of mental health and SMU, a hurdle emerged when researchers straightforwardly ask participants to respond to the questionnaire because the results can be attributed to the survey structure (e.g., question order) rather than the impact of the independent variables. Does answering multiple items measuring addictive behaviors activate certainly mental nodes that will lower participants'

subsequent response to mental health questions? If the answer is yes, how do we mitigate

such priming effects? A good case in point that illustrates the importance of question order is

this piece conducted by Jarman et al., (2021), wherein Jarman and colleagues investigated the

associations between SMU and subjective mental health. They reported 15 items, yet without

a comprehensive review of the item's order in the original instrument, we cannot rule out the

possibility of priming effects. Note that the absence of the question order accompanied by

loads of items is a common exercise in SM and mental health paradigm. Having theorized the

potential downstream effects of one SM scale on another, Ernala et al., (2020) prepared 10

SM scales and their participants only responded to one scale, avoiding the prospect of

priming effects derived from the wording or the SM question order. Perhaps the question

order and the mixed wording of the items in a questionnaire require deliberate reasoning,

especially in the case when a number of constructs are assessed.

     Hence, summarizing all suggestions from studies 1, 2, and 3, we argue that it is a must

to consider question order effects, anchoring effects, and context effects before distributing

the survey among the target participants. Theorizing how the order of the items would elicit

question order effects via multiple methods (e.g., pretest, randomization), adjusting the

wording within the same scale to observe if one-sided wording - compared with mixed

wording – will alter participants' subjective affective states, and creating contexts whereby

participants existing mindsets about SM (e.g., pre and post measure attitudes towards SM) are

three prevailing suggestions we endorse for future SM and subjective mental health research.

     Here, we argue that it is misleading and inaccurate to employ scales previously

established, ask our participants to complete a questionnaire whose order is not attentively

pretested nor hypothesized, and conjure up sweeping conclusions concerning the association, and even causality, between SM and subjective mental health. Put in the terms of Parry et al. (2021) and Ernala et al. (2020), the well-documented association between self-reported SMU and logged/back-end data was only moderate, not strong. Clearly, there is a battery of refinement that we can adopt to advance our methodology and measurement strategy.

First, given that the objective measures become more attainable and possible for research participants to utilize and the priming effects prevalent in survey research, the meaningfulness of objective data, recording the time and frequency, seems like a better approach than asking participants to self-report (Ernala et al., 2020; Junco, 2013; Parry et al., 2021; Verbeij et al., 2022). SM giants, such as Meta, provide all SM back-end activities records in terms of their frequency available to all users (Angerer, 2018), a huge collection of applications that users can download to track their time on each SM, and smartphone built-in functions (e.g., Screen Time on IOS devises) afford users to record their time spent on each app are plausible measurement strategies available to researchers who share no close affiliation with SM companies. We of course understand that collecting behavioral data is time-consuming, faces objections from the users, raises privacy concerns, and we inevitably have to rely heavily on self-reports. Notably, Verbeij et al. (2022) reported that self-reported data did have comparable predictive validity, compared with objective data, on a between-person, within-person, and person-specific level. Overall, the advancement and utility of digital trace data is a fruitful direction for future inquiries that are free of priming effects, yet pretested self-reports assuring the avoidance of priming effects remain a useful exercise.

Among the vast amount of scales that we can adopt for SM self-reports, Keles et al.

(2020) indicated that intensity and addiction are the two most commonly studied constructs at large. Our results indicated that the placement of the addiction scale before depression elicited significantly higher subjective depressive scores, whereas the intensity scale did not have much effect, as the reserve order. This result implies that asking about mental health items before SMU items might help us to mitigate the question order effects in self-reports. Additionally, the anchoring effects, together with acquiescence bias, posit the caution of employing a scale measuring mental health that only depicts positive or negative wording (e.g., Epley & Gilovich, 2005; Hurd, 1999; Kuru & Pasek, 2016).

Moreover, there is an accumulation of studies pointing out that now seems like a good time to disregard time-based measures and center on specific activities within SM (Krcmar & Cingel, 2019; Meier & Reinecke, 2020; Valkenburg, 2021; Yu, 2016). Indeed, the employment of the intensity scale, in light of the results of study 1, can elicit priming effects, and we largely do not know if such priming can transfer itself to other scales measuring differential SM activities. For example, Verduyn et al., (2020) utilized the dichotomy of active use (e.g., posting a status on Instagram) and passive use (e.g., looking at another user's profile) and argued that passive use resulted in increased levels of social comparison. Though Verduyn and colleagues did not theorize that such a high level of social comparison is a product of priming, can we attribute that increase to priming effects since items measuring passive use inherently activated participants' mental nodes about less favorable experiences on SM? Overall, even if adopting a novel approach to measure nuanced SM activities, whether or not the significant relationship between SM activities and subjective mental health derived from priming effects of the extent of SM activities remains underexplored.

Strengths and Limitations

The usefulness of our current work cannot be understood without realizing its limitations. First, though we tested the priming effects via recruiting participants for three independent studies and strictly screening them so that each participant can only participate in one of the studies, all of our participants are undergraduate students in a public university in California, suggesting that our sample might not be a representation the entire population worldwide despite its diverse demographics. Second, our experimental work was cross-sectional, and self-reported by young adults; priming in social science has at least two categories in light of its longevity: Short-term and long-term priming (Bargh, 2014; Molden, 2014; Wentura & Rothermund, 2014). We only captured the attendant and subjective mental health, which might be subject to change in the long term and cannot be regarded as identical as the utmost long-term priming effects. More importantly, the entirety of our three studies only measured participants' subjective mental health, and we did not examine the objective measure of mental health (e.g., the screen for cognitive impairment in psychiatry designed to measure depression based on the cognitive deficit; Ott et al., 2016).

Note that the change of wording exerted in study 2 did not progress as a measurement for a construct would usually develop, validate, and progress. Ajzen and colleagues, in multiple pieces (e.g., Ajzen, 2019, 2020; Fishbein & Ajzen, 1974), indicated that the construction of a survey withstands exhaustive formative research before the production of each item. In our current work, however, we randomly selected 50 percent of the one-sided scale and reserved their wording; in doing so, we produced a modified scale measuring depression and self-esteem not being constantly validated and tested by additional work. We

nonetheless assert that the change of the wording from one-sided to mixed lowered the average scores for both self-reported depression and self-esteem, and we thus contend that our alteration of the wording is solid and telling effects of anchoring (priming) effects. Future work can consider pretesting by changing different items within the same scale to compute a more balanced and reliable scale.

In summary, to ensure the usefulness and effectiveness of the SM and mental health paradigm, we urge our fellow scholars to certain guidance derived from the results and implications of our current work. It is hitherto of high importance to (a) pay close attention to how the items' order in the same instrument is constructed, (b) employ mixed wording items when possible, and (c) adopt strategies to ensure that our participants' potential primed thoughts are neutralized before self-reports.

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Appendix A

Stimuli/Scales for study 1

<u>SM Intensity</u>

We will now ask you some questions regarding your <u>social media</u> activity. Response categories range from 1 = strongly disagree to 5 = strongly agree, unless otherwise noted.

1. social media is part of my everyday activity

2. I am proud to tell people I'm on social media

3. Social media has become part of my daily routine

4. I feel out of touch when I haven't logged onto social media for a while

5. I feel I am part of the social media community

6. I would be sorry if social media shut down

7. Approximately how many TOTAL social media friends do you have (on your most-used platform)? *

8. In the past week, on average, approximately how much time PER DAY have you spent actively using social media? **

Note. Response categories range from 1 = strongly disagree to 5 = strongly agree, unless otherwise noted. Response to question 7 will be ordinal: 0-100, 100-300, 300-500, 500-800, >800; response to question 8 will be ordinal: < 1h, 1h-2h, 2-4h, 4-8h, >8h

<u>SM Addiction</u>

We will now ask you some questions regarding your <u>social media</u> activity. The six items of BSMAS are measured against 5 standard responses of "very rarely," "rarely," "sometimes," "often," "very often."

1. You spend a lot of time thinking about social media or planning how to use it.
2. You feel an urge to use social media more and more.
3. You have tried to cut down on the use of social media without success.
4. You use social media in order to forget about personal problems.
5. You become restless or troubled if you are prohibited from using social media.
6. You use social media so much that it has had a negative impact on your job/studies.

Appendix B

Stimuli for study 2

Items Utilized to Assess Participants' Self-Esteem	
Mixed wording	One-sided wording (all positive)
1. On the whole, I am satisfied with myself.	1. On the whole, I am satisfied with myself.
2. At times I think I am no good at all. *	2. At times I think I am very good. *
3. I feel that I have a number of good qualities.	3. I feel that I have a number of good qualities.
4. I am able to do things as well as most other people.	4. I am able to do things as well as most other people.
5. I feel I do not have much to be proud of.	5. I feel I have much to be proud of.
6. I certainly feel useless at times. *	6. I certainly feel useful at times. *
7. I feel that I'm a person of worth, at least on an equal plane with others.	7. I feel that I'm a person of worth, at least on an equal plane with others.
8. I wish I could have more respect for myself. *	8. I have enough respect for myself. *
9. All in all, I am inclined to feel that I am a failure. *	9. All in all, I am inclined to feel that I am a success. *
10. I take a positive attitude towards myself.	10. I take a positive attitude towards myself.

Note. * denotes the change of wording. We computed an average score representing each user's self-esteem.

Items Utilized to Assess Participants' Depression	
Mixed wording	One-sided wording (all negative)
1. Many interests or pleasure in doing things. *	1. Little interest or pleasure in doing things. *
2. Feeling upbeat, happy, or hopeful. *	2. Feeling down, depressed, or hopeless. *
3. Trouble falling or staying asleep, or sleeping too much.	3. Trouble falling or staying asleep, or sleeping too much.
4. Feeling energetic or having much energy. *	4. Feeling tired or having little energy. *
5. Poor appetite or overeating.	5. Poor appetite or overeating.
6. Feeling good about yourself or that you are a success or have	

met the expectations of your family. *

7. Trouble concentrating on things, such as reading the newspaper or watching television.

8. Moving or speaking so slowly that other people could have noticed or the opposite being so fidgety or restless that you have been moving around a lot more than usual.

9. Thoughts that you would be better off dead, or of hurting yourself.

6. Feeling bad about yourself or that you are a failure or have let yourself or your family down. *

7. Trouble concentrating on things, such as reading the newspaper or watching television.

8. Moving or speaking so slowly that other people could have noticed or the opposite being so fidgety or restless that you have been moving around a lot more than usual.

9. Thoughts that you would be better off dead, or of hurting yourself.

Note. * denotes the change of wording. We computed a sum index score representing each user's depression.

Appendix C

Stimuli for study 3

<u>The article emphasizes the benefits of SM</u>

A recent investigation by the Wall Street Journal revealed that social media companies were aware of mental health benefits linked to the use of their apps. Internal research by the social media giants found that social media improved individual self-awareness for one in three teenagers, and all teenage users of the apps linked their use to experiences of increased self-esteem, less depression, and a decrease in the sense of loneliness. It isn't the first evidence of social media's benefits. Other research groups have identified social media as avenues for better connections, and reports have linked social media to healthy and prosocial behavior, including a recent spate of volunteering in schools.

<u>The article emphasizes the harm of SM</u>

A recent investigation by the Wall Street Journal revealed that social media companies were aware of mental health risks linked to the use of their apps. Internal research by the social media giants found that social media worsened individual self-awareness for one in three teenagers, and all teenage users of the apps linked their use to experiences of lower self-esteem, increased depression, and a strong sense of loneliness. It isn't the first evidence of social media's harm. Other research groups have identified social media as avenues for cyberbullying, and reports have linked social media to dangerous and antisocial behavior, including a recent spate of school vandalism.

<u>The article does not mention SM</u>

National Geographic is an American monthly magazine published by the National

Geographic Society. Known for its photojournalism, it is one of the most widely read

magazines of all time. The magazine was founded in 1888 as a scholarly journal, nine months

after the establishment of the society. In 1905, it began including pictures, a style for which it

became well-known. Its first color photos appeared in the 1910s. During the Cold War, the

magazine committed itself to present a balanced view of the physical and human geography

of nations beyond the Iron Curtain. In later years, the magazine became outspoken on

environmental issues. Since 2019, controlling interest has been held by The Walt Disney

Company.