Linking Self-Reported Social Distancing to Real-World Behavior
During the COVID-19 Pandemic

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Abstract

In an effort to combat COVID-19 and future pandemics, researchers have attempted to identify the factors underlying social distancing. Yet, much of this research relies on self-report measures. In two studies, we examine whether self-reported social distancing predicts objective distancing behavior. In Study 1, individuals' self-reported social distancing predicted decreased mobility (assessed via smartphone step-counts) during the COVID-19 pandemic. While participants high in self-reported distancing (+1 *SD*) exhibited a 33% reduction in daily step-count, those low in distancing (-1 *SD*) exhibited only a 3% reduction. Study 2 extended these findings to the group-level. Self-reported social distancing at the U.S. state level accounted for 20% of the variance in states' objective reduction in overall movement and visiting nonessential services (calculated via the GPS coordinates of ~15 million people). Collectively, our results indicate that self-reported social distancing tracks actual social distancing behavior.

Keywords: social distancing, COVID-19, coronavirus, self-report, physical distancing.

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During the COVID-19 Pandemic

During pandemics, public health officials strongly encourage people to distance themselves and limit in-person interactions (e.g., Strohlic & Champine, 2020; World Health Organization, 2020). Indeed, sustained social distancing helped contain the 1918 Influenza Pandemic (e.g., Strohlic & Champine, 2020) and stopped the exponential spread of COVID-19 in 2020 and 2021 (e.g., Anderson et al., 2020; McGrail et al., 2020; Gao et al., 2020). Furthermore, social distancing played a major role in preventing critical care capacities from being overwhelmed during the COVID-19 pandemic (Kissler et al., 2020). Despite its importance, however, variability in social distancing exists. For instance, at the start of the COVID-19 pandemic (mid-March, 2020), while New Yorkers were following shelter-in-place orders, spring breakers in Florida enjoyed packed beaches (Dusenbury, 2020), and people in Chicago attended St. Patrick's Day celebrations (Rahman, 2020).

Therefore, and in an effort to quell COVID-19 and potential future pandemics, researchers have scrambled to elucidate the individual and group-level variables underlying social distancing. For instance, during the COVID-19 pandemic, researchers assessed whether women social distance more than men (Olcaysoy Okten et al., 2020), how boredom proneness and self-control may impair social distancing (Wolff et al., 2020), and whether analytical thinking promotes social distancing (Swami & Barron, 2020), among other predictors. Studies have also examined whether certain messages can heighten people's social distancing, including emphasizing one's duty (Everett et al., 2020), drawing attention to prosocial benefits (Jordan et al., 2020), and eliciting feelings of empathy (and other emotions; Pfattheicher et al., 2020; Heffner et al., 2020). These examples are merely a small portion of this research, however; ~11,000 results were found when

searching for “social distancing” + “COVID-19” on Google Scholar on June 11th, 2020 and selecting 2020 as the publication criteria. All these research efforts ideally should inform policy makers on how to motivate people (and *whom* to motivate) to engage in social distancing during pandemics (Van Bavel et al., 2020).

However, due to the urgency of this line of work and the inability to perform in-person behavioral studies (in part because of social distancing guidelines themselves), much of this research relies on self-report measures.¹ This may be problematic for several reasons. For instance, individuals may over-report their social distancing due to social desirability bias (Fisher, 1993; Maccoby & Maccoby, 1954), wanting to maintain a positive self-impression (Leary & Kowalski, 1990), evaluating themselves more favorably than others (Alicke & Govorun, 2005), and not remembering failures to social distance (Kouchaki & Gino, 2016). Indeed, a rich literature has documented gaps between self-reported judgments and actual behavior, including the intention-behavior gap (Sheeran & Webb, 2016), the attitude-behavior gap (Ajzen & Fishbein, 1977), and the knowledge-behavior gap (Hornik, 1989).

To address these concerns, in two studies, we tested whether self-reported social distancing during the COVID-19 pandemic overlaps with objective behavior. Notably, such results would help validate numerous studies examining social distancing via self-report measures. Further, our findings would contribute to a rich literature documenting the overlap and divergence of self-report and behavioral measures (e.g., intention-behavior gap). To summarize, the COVID-19 pandemic provides a naturalistic test-case of whether people’s self-reported behavior aligns with their actual behavior in the midst of a world-wide emergency in which the behavior in question has serious consequences (infection and death).

¹ We found at least 30 articles utilizing self-reported social distancing measures (see [here](#)). We stopped searching at 30.

In Study 1, we assessed whether self-reported social distancing predicts objective social distancing behavior at the individual-level. Because a core component of social distancing involves staying home (e.g., sheltering in place, avoiding travel), individuals who are social distancing should exhibit a comparative reduction in movement from before COVID-19 to during COVID-19. To test this, we utilized pedometers on participants' smartphones. Specifically, we examined whether participants' self-reported social distancing predicts decreased average daily step-count during the COVID-19. Importantly, because smartphones track users' step-count data over time, U.S. participants' step-count data from before COVID-19 began was easily accessible (e.g., step-count in February, 2020). As such, we were able to use these step-count values to additionally quantify individuals' *change* in movement from before COVID-19 to during COVID-19, in turn providing a proxy for participants' objective degree of social distancing.

Notably, Study 1 provides a conservative test of whether self-reported and objective social distancing overlap. Though researchers have validated smartphone step-counters (Duncan et al., 2018), this data-source remains noisy (Ata et al., 2018). For instance, step-count presumably captures variance alternate to social distancing as well (e.g., exercise), and this likely remains true during a viral pandemic (e.g., such as taking long solitary walks). To account for this error variance, we examined the *change* in participants' average step-count from before COVID-19 to during COVID-19 (this should account for any general individual variance in step-count driven by exercise or an active lifestyle). Additionally, on top of such general-level variance, we also directly accounted for potential third-variables by controlling for participants' actions *during* the pandemic (e.g., jogging or hiking in a socially distanced manner during the pandemic). Given these considerations, in Study 1, we are able to test (1) whether self-reported social distancing overlaps with a proxy of objective social distancing (step-count) at the individual-level, and (2) whether

step-count data qualifies as an easily accessible behavioral measure of social distancing that is already being tracked and stored on millions of people's smartphones.

Social distancing varies not only among individuals, but also among communities (at the group-level). As illustrated by GPS mobility data (e.g., Gatalo et al., 2020; Gollwitzer et al., 2020; VoPham et al., 2020, 2020; Johnson & Thompson, 2020), fluctuating stay-at-home orders, and patchwork business re-openings (Healy et al., 2020), U.S. States and Counties have embraced social distancing at strikingly different degrees. Given this community-based nature of social distancing and of COVID-19 itself (e.g., rapid community spread, ICU bed shortages in specific locations) group-level differences in social distancing are, similarly to person-level differences, critical in understanding and combating COVID-19. In Study 2, we thus tested whether self-reported and objective social distancing overlap at the group-level. To do so, we examined whether the self-reported social distancing of people in U.S. States predicted the extent to which those U.S. States socially distanced as assessed by the GPS coordinates of ~15 million smartphone users. More specifically, we tested whether the self-reported social distancing of inhabitants of U.S. states aligned with a greater reduction in those states' overall movement and visiting nonessential services and retail from before COVID-19 to during COVID-19. Importantly, when doing so we examined both participants' self-reported social distancing in the week prior to participation as well as participants' intentions to social distance in the week following participation. Taken together, Study 2 should help inform whether people's self-reported social distancing at the group-level aligns with actual social distancing during a viral pandemic. And additionally, Study 2 should provide insight into cultural variation within a nation (the United States) during a period of trauma and uncertainty (the COVID-19 pandemic), an analysis level that psychologists have largely left

unexplored (see Harrington & Gelfand, 2014; Nisbett, Bowdle, & Schwarz, 1996; Vandello & Cohen, 1999; Varnum & Kitayama, 2011).

Before continuing we note two additional points.² First, participants' self-reported social distancing may predict reduced step-counts due to a third-variable: COVID-19 severity. That is, participants living in places that are experiencing severe COVID-19 outbreaks may self-report higher social distancing for biased reasons (e.g., social norm pressure, perceiving social distancing as more socially desirable given the circumstances). And, these participants may also exhibit reduced step-counts due to local social distancing restrictions. These two factors, then, would lead to a superficial link between reported social distancing and step-count. To discount this possibility, in Studies 1 and 2, we exploratorily examined whether controlling for COVID-19 severity in participants' locations eliminates any potentially observed links between self-reported social distancing and objective distancing.

Second, in Studies 1 and 2, we report the predictive power of participants' self-reported social distancing in terms of two subtypes: *personal* social distancing (how much participants reported social distancing themselves) and *community* social distancing (how much participants reported that their community was social distancing). Although we had originally planned to examine our results collapsed across these social distancing items, by considering these subtypes, we could examine whether personal and community social distancing differentially predict objective social distancing at the individual- and group-level. For instance, in Study 1, which was at the individual-level, one might expect to see a clearer link between personal social distancing

² These points were either brought to our attention during the review processes or due to results of data analyses (e.g., examining the structure of the self-reported social distancing measures). As such, they were not points directly raised in our original pre-registrations and should be considered exploratory. For a detailed description of how our analyses differed from our pre-registrations, see Supplements.

and objective social distancing. And in Study 2, which was at the group-level, one might expect to see a clearer link between community social distancing and objective social distancing.

Study 1

In Study 1, we examined whether individuals' self-reported social distancing predicted reduced average daily step-count (as assessed by iPhone pedometer data) at the start of the COVID-19 pandemic (April 3-9, 2020) as compared to before COVID-19 struck the U.S. with force (prior to March, 2020). Importantly, in these analyses we controlled for a number of third-variables, including going on solitary jogs or hikes during COVID-19 as well as the COVID-19 severity in participants' locations (COVID-19 cases-per-capita at the U.S. county level).

Nonetheless, given several limitations of Study 1, we also conducted Study S1 (see Supplements) in which we (1) extended the step-count time period to the month of April, 2020 (instead of the single week of April 3-9, 2020), (2) validated participants' reported step-count data by having participants upload screenshots of said data from their smartphones, (3) included additional third-variables, such as whether participants reported an active lifestyle, the extent to which participants reported carrying their phone on their person, and the extent to which participants reported walking indoors, and (4) included iPhone as well as Android users (Study 1 only included iPhone users). Supporting the replicability of the observed findings, the results of Study S1 largely echoed those of Study 1 (see Supplements for detailed Method, Results, and Discussion of Study S1).

Method

Participants. A sensitivity power-analysis indicated that with 258 participants we would have 90% power to find a small effect ($r = .20$; see pre-registration [here](#)).³ We pre-registered to recruit 300 participants on MTurk (to account for participant exclusion). 302 participants were recruited (126 Female; $M_{\text{age}} = 35.26$, $SD_{\text{age}} = 10.83$).⁴ 21 participants were excluded for failing an attention check. We only recruited iPhone users. APA ethical standards were followed and participants completed informed consent.

Self-reported social distancing. We included four self-report social distancing measures. These measures assessed *personal*-directed social distancing (e.g., “I have almost zero in-person social interactions with people I am not living with”) and *community*-directed social distancing (e.g., “People in my community are social distancing”). The measures included general face-valid items (e.g., “I am social distancing”) as well as more specific items (e.g., “I am avoiding small gatherings”).

The first measure, an abbreviated version of a measure developed in political science (Wu & Huber, 2020), provided a definition of social distancing and included 3 items: one personal-directed item, “I am practicing social distancing,” and two community-directed items, “My friends and family are practicing social distancing”, and “People in my community are practicing social distancing.” Likert-scale: 1 = *Strongly disagree* to 5 = *Strongly agree*.

The second measure, adapted from research out of psychology (Jordan et al., 2020), asked participants the degree to which they were avoiding various activities (e.g., going to the grocery store, going to small gatherings; see Appendix; 0 = *I make no effort to avoid this activity*, 50 = *I*

³ Some of the pre-registered analyses are not reported in the main text for brevity’s sake. See Supplements. Several of the presented analyses differ from the pre-registered analyses in specific ways. See Supplements for a thorough discussion of these changes.

⁴ Slight variations between intended and actual recruitment number can happen on MTurk (e.g., someone failed to return the hit despite completing it).

make some effort to avoid this activity, 100 = *I completely avoid this activity*). As in Jordan et al. (2020), we also asked participants whether they would engage in these activities if coronavirus was not a concern (to control for participants' typical activities). All items were personal-directed.

Third, we included a 6-item scale out of our own research group (e.g., 'I have almost zero in-person social interactions with people I am not living with'; 1 = *Not at all true* to 9 = *Very true*). All items were personal-directed.

Fourth, we included 2 face-valid items: one personal-directed item, "How would you rate your personal attempts at social distancing, specifically, in terms of reducing travel and staying home as much as possible?" and one community-directed item, "How would you rate the average person's attempts at social distancing in the state in which you live (i.e., in <participant's region here>), specifically, in terms of reducing travel and staying home as much as possible?" (1 = *terrible* to 7 = *excellent*). See Appendix or [Verbatim Material Files](#) for full measures. See Supplements for more detail.

Step-Count. Participants reported their average daily step-count in the week prior to participation (April 3-April 9, 2020) as indicated by their iPhone Health App (see Duncan et al., 2018 for a validation of assessing step-counts via this App). Participants also reported their average daily step-count in February 2020, in March 2019, and between April 3rd and April 9th, 2019, respectively, to control for average daily step-count taken before COVID-19.⁵ Extensive instructions were provided to participants on how to locate the requested average daily step-counts on their phone (see [Verbatim Materials](#)). Smartphone pedometers and step-counts have been

⁵ We also collected participants' average daily step-count in March, 2020. Given that these step-counts fell between before COVID-19 (early March, 2020 in the U.S.) and during COVID-19 (mid- and late-March 2020 in the U.S.) these results are discussed in the Supplements.

utilized as indirect behavioral measures in prior research (e.g., health behaviors; Larson, Aiello, & Cimiotti, 2004).⁶

COVID-19 severity. We assessed the severity of COVID-19 in the location in which participants completed the survey. To do so, we converted the ZipCode in which participants completed the study to a County FIPS value, and then, using a dataset provided by the New York Times (The New York Times, 2020), calculated the COVID-19 infections per capita in that county on the day that participants completed the study (which was April 9th, 2020; total number of cases in a county on April 9th divided by the population of that county).

Additional measures. For validation purposes, we also assessed participants' perceived control over COVID-19 spread, whether participants reported still traveling for work, and if so, if they qualify as an essential worker, and whether participants are at risk for developing severe COVID-19 symptoms (e.g., diabetes, asthma, heart disease, immunocompromised). We also included a quiz testing individuals' knowledge of COVID-19. For details, see Supplements. See Appendix or [Verbatim Material Files](#) for full measures.

Procedure. Participants were first filtered by whether they had an iPhone (only iPhone users were included in Study 1; Study S1 also included Android users). Participants then completed the self-reported social distancing measures (randomized, clustered together, including the COVID-19 knowledge quiz) and the step-count items (clustered together) in random order.

⁶ An important concern regarding our step-count methodology is that participants may have reported biased daily step-count estimates in an effort to align their self-reported social distancing with their step-count data (an example of response or demand bias). Arguing against this possibility, the order in which participants completed the two measures did not impact the results (see Results of Study 1). Additionally, and perhaps more convincingly, we found consistent results in a supplemental study, Study S1, in which we validated participants' daily step-count data by having participants submit screenshots of said data. Furthermore, in Study S1, we found no systematic differences between participants' self-reported step-count data and the step-count data documented by the screenshots they submitted.

Finally, participants completed the additional measures, an attention check item (see Supplements), and demographics.

Results

Analyses in Studies 1, 2, and S1 were conducted in SPSS, R, and Python. All syntax and code files can be found on OSF [here](#).

Analysis plan. The following results are separated into four sections: (1) Calculation of self-reported social distancing scores, (2) validation analyses, (3) linking self-reported social distancing to lower average daily step-counts during COVID-19 (conducted using Generalized Linear Models [GLMs]), and (4) linking self-reported social distancing to a *reduction* in average daily step-count from before COVID-19 to during COVID-19 (repeated measures; conducted using Generalized Estimating Equations [GEEs]). Finally, we briefly consider a replication study of the presented data (Study S1), and examine whether participants' self-reported social distancing may fall prey to the better-than-average effect (Alicke & Govorun, 2005). Across these analyses, we considered personal- and community-directed self-reported social distancing separately. Additionally, we applied statistical methods that aligned with the format of the dependent outcome; that is, negative binomial models were applied given that average daily step-count qualifies as count data.

Calculating self-reported social distancing. A principal-component analysis revealed that the self-reported social distancing items loaded onto two separate factors, personal-directed social distancing (how much participants reported social distancing themselves; *eigenvalue* = 2.96) and community-directed social distancing (how much participants reported that their community was social distancing; *eigenvalue* = 1.22). The two factors were moderately correlated ($r = .34$). Given these two distinct factors, we calculated separate personal and community-directed social

distancing scores for each participant. The personal social distancing score was calculated by z -scoring participants' scores on the Huber personal social distancing item, the Jordan social distancing scale, our social distancing scale, and the personal face-valid item (see Methods above), respectively, and then averaging across these four z -scores, $\omega_t = .84$. The community social distancing score was calculated by z -scoring participants' scores on the Huber community social distancing items and the community face-valid item, respectively, and then averaging across these two z -scores.⁷ See Supplemental Table S1 for descriptives and internal reliabilities of all the included measures.

Validation of included measures. We examined the validity of participants' self-reported personal social distancing. Supporting said validity, a one-way ANOVA revealed that participants who were traveling for work reported lower personal social distancing, $F(1, 279) = 15.64, p < .001, \eta^2 = .053$.⁸ Additionally, correlational analyses revealed that participants who reported higher personal social distancing were more likely to say that their behavior could impact the spread of COVID-19, $r(279) = .33, p < .001$, and that they were engaging in greater hygiene practices (e.g., washing hands), $r(279) = .61, p < .001$. Finally, self-reported social distancing predicted greater performance on our COVID-19 knowledge quiz, $\omega_t = .71, r(261) = .32, p < .001$ (notably, quiz performance is not a self-report measure, and thus provides additional validity to the included self-reported social distancing measures).⁹

Supporting the validity of the step-count measure, and in line with COVID-19 leading to reduced movement, a paired samples t -test revealed that participants' average step-count reduced

⁷ Consistent results with respect to our key analyses were also found when simply collapsing across all the social distancing measures/items (i.e., ignoring a personal versus community distinction). That is, we still found consistent results when examining our results more closely in line with what we had specified in our pre-registration.

⁸ Although being at risk for experiencing severe symptoms in response to COVID-19 did not predict greater self-reported social distancing, $p = .885$, participants' age did, $r(278) = .21, p < .001$.

⁹ We excluded participants who admitted having looked up answers to the quiz questions.

from before COVID-19 (collapsed across, February, 2020, March, 2019, and April 3-9, 2019), $M = 4647.45$, $SD = 3076.71$, to during COVID-19, April 3-9, 2020, $M = 3916.22$, $SD = 4218.21$, $t(219) = 3.09$, $p = .002$. Additionally, generalized linear models (GLMs)^{10,11} revealed that participants who reported still traveling for work during COVID-19 had higher average daily step-counts April 3-9, 2020, $\chi^2(1, N = 220) = 5.06$, $p = .025$, $\text{Exp}(B) = 1.44$.¹²

Linking self-reported social distancing to step-count. We conducted three generalized linear models (GLMs) each fit on a negative binomial distribution (because step-count qualifies as count data) to test whether participants' self-reported social distancing predicted lower average daily step-count during the COVID-19 pandemic.

Depending on the specific model, participants' self-reported personal or community social distancing was included as the main predictor (z -scored). Model #1 included self-reported *personal* social distancing (the degree to which participants perceived themselves as social distancing) as the main predictor of interest. In Model #2, self-reported *community* social distancing (the degree to which participants perceived their community as social distancing) was the main predictor. In Model #3, we included *both personal and community* social distancing as the main predictors.

Each of the three models also included the following predictors as control variables (all z -scored): (1) participants' average daily step-count before the COVID-19 pandemic (collapsed

¹⁰ Generalized linear models were applied (instead of general linear models) because step-count followed a negative binomial distribution. This is common for step-count data.

¹¹ We had pre-registered that we would conduct correlations rather than generalized linear models. However, generalized linear models are more appropriate for count data given the negative binomial distribution of the data. The observed results remained consistent across the two statistical methods (see Supplements).

¹² Sixty-one participants were excluded from all analyses involving step-count data. These included participants who claimed to have reported inaccurate step-counts ($n = 20$), those whose responses were obviously false ($n = 2$; e.g., 1267543.00 steps), those whose responses qualified as extreme outliers or whose responses were missing ($n = 21$; $+2 SD$ on any of the four reported step-count averages; e.g., 45,000 steps; consistent results were still found when including these participants), 5 participants who reported 0 steps on one of the reported step-count averages, 12 participants for missing COVID data (i.e., COVID cases per capita could not be calculated as their ZipCode showed they were outside the U.S.), and 1 participant who reported an implausible non-integer number of steps (4.10 average steps; see Supplements for more details). This resulted in a final sample of 220 participants.

across average daily step-count in February, 2020, March, 2019, and April 3-9, 2019), (2) the extent to which participants reported going jogging, hiking, or on long walks either alone or with a quarantine partner during April 3-9, 2020, and (3) the severity of COVID-19 in the county in which each participant was located (COVID-19 cases per capita) at the time of study completion (April 9th, 2020). Participants' average daily step-count April 3-9, 2020, $M = 3,916.23$, $SD = 4,218.21$, functioned as the outcome variable.

Personal social distancing (Model 1). Model #1 revealed that participants' self-reported personal social distancing predicted average daily step-count during the COVID-19 pandemic. Participants who reported greater personal social distancing exhibited lower average daily step-counts between April 3-9, 2020, $\chi^2(1, N = 220) = 11.89$, $p = .001$, $\text{Exp}(B) = .794$ (see Table 1; $\text{Exp}(B)$ is equivalent to an Odds Ratio). Regarding effect-size, and relating these findings back to average daily step-counts, for every increase of one SD in self-reported personal social distancing ($SD = 0.85$), individuals' average daily steps were approximately 20.6% lower ($OR \sim .794$). Said another way, because the average number of steps between April 3-9, 2020 was $\sim 3,916$ daily steps, an increase of one SD in reported social distancing would have resulted in an average estimate of $\sim 3,109$ daily steps between April 3-9, 2020 (a decrease of ~ 807 steps per day).^{13,14,15,16}

¹³ Whether participants first completed the self-reported social distancing measures or the step-count measures did not impact the results, $p = .888$.

¹⁴ We indicated in our pre-registration that we would control for traveling for work, being at risk for COVID-19, and qualifying as an essential worker. However, we later realized that, conceptually, these variables may be mechanisms rather than confounds. For instance, someone traveling for work would self-report low social distancing and indeed would have a high step count; their self-reported distancing is an accurate representation of their actual distancing, which is poor. Importantly, the observed link reduced but did not disappear when additionally controlling for these variables (as pre-registered) in the conducted GLM, $\chi^2(1, N = 220) = 8.47$, $p = .004$, $\text{Exp}(B) = .816$.

¹⁵ Further supporting the validity of step-count as a measure of social distancing, the severity of COVID-19 where participants were located predicted decreased step-counts during COVID-19 in Models 1 through 3 in Study 1, $ps < .044$ (see Table 1). This finding was replicated in Study S1, $ps < .047$ (Table S2).

¹⁶ We conducted two further models where we replaced personal social distancing with self-reported hygiene practices (e.g., washing hands) and with general adherence to COVID-19 guidelines (see Supplements). In line with our results, self-reported hygiene and adherence to COVID-19 guidelines also predicted reduced step-count, $\chi^2(1, N = 220) = 9.75$, $p = .002$, $\text{Exp}(B) = .793$, and $\chi^2(1, N = 220) = 11.13$, $p = .001$, $\text{Exp}(B) = .800$. These analyses indicate that our findings may also extend to other self-reported areas of COVID-19 prevention.

Community social distancing (Model 2). Unlike participants' personal social distancing, participants' community social distancing (the degree to which participants judged their community as social distancing) did not predict their step-counts in Model 2, $\chi^2(1, N = 220) = 2.40, p = .121, \text{Exp}(B) = .899$ (Table 1). These results indicate discriminant validity in terms of the observed overlap between *personal* social distancing and reduced step-count in Model 1. That is, if self-reported and objective social distancing truly overlap, one would expect individuals' daily step-count to be predicted by their own self-reported social distancing (their personal social distancing), but not necessarily by the degree of social distancing they perceive in their community.

Personal and community social distancing (Model 3). Finally, when including both social distancing types (personal and community) in the final GLM model (Model 3), personal social distancing still predicted reduced daily step-count, $\chi^2(1, N = 220) = 10.20, p = .001, \text{Exp}(B) = .796$, while community social distancing still did not, $\chi^2(1, N = 220) = 0.01, p = .918, \text{Exp}(B) = .993$ (Table 1). Notably, that the link between community social distancing and step-count appeared to decrease when controlling for personal social distancing (the effect-size changed from $OR = .899$ to $OR = .993$, where an OR of 1 indicates a perfect null effect), suggests that any variance linking community social distancing to reduced step-count is driven by the overlap between community and personal social distancing rather than by community social distancing itself. See Supplemental Table S1 for links between the individual social distancing measures and step-count (i.e., when not collapsing across the included social distancing measures).^{17,18}

Table 1

¹⁷ We observed similar results when relating participants' self-reported social distancing in March, 2020 to their March, 2020 step-count, though these findings were less conclusive (as noted in our pre-registration, this may be because individuals began social distancing at varying times during March, 2020; see Supplements).

¹⁸ Providing further support for these results, personal social distancing did *not* predict reduced step-count before COVID-19; between April 3rd and April 9th, 2019 (the same week one year earlier), $p = .786$, in February, 2020, $p = .916$, or in March, 2019, $p = .417$.

Study 1: Output of GLM Models 1 Through 3. Self-Reported Personal Social Distancing Predicted Lower Average Daily Step-Count Between April 3-9, 2020 in Models 1 and 3. Self-Reported Community Social Distancing Neither Predicted Reduced Average Daily Step-Count in Model 2 nor in Model 3 (Key Findings in Bold).

Predicting Average Daily Step-Count Between April 3-9, 2020				
	<i>Coefficient</i>	Test-Statistic	<i>p</i> value	Effect-Size
Model #1				
Personal Social Distancing	$B = -.231$	$\chi^2 (1, N = 220) = 11.89$	$p = .001$	$\text{Exp}(B) = 0.794, 95\%[0.696, 0.905]$
Step-Count before COVID-19	$B = .742$	$\chi^2 (1, N = 220) = 74.05$	$p < .001$	$\text{Exp}(B) = 2.100, 95\%[1.774, 2.487]$
Jogging/Hiking ¹⁹	$B = .041$	$\chi^2 (1, N = 220) = 0.28$	$p = .594$	$\text{Exp}(B) = 1.042, 95\%[0.897, 1.210]$
COVID-19 Severity	$B = -.139$	$\chi^2 (1, N = 220) = 4.04$	$p = .044$	$\text{Exp}(B) = 0.870, 95\%[0.759, 0.997]$
Model #2				
Community Social Distancing	$B = -.106$	$\chi^2 (1, N = 220) = 2.40$	$p = .121$	$\text{Exp}(B) = 0.899, 95\%[0.786, 1.029]$
Step-Count before COVID-19	$B = .713$	$\chi^2 (1, N = 220) = 67.34$	$p < .001$	$\text{Exp}(B) = 2.039, 95\%[1.720, 2.418]$
Jogging/Hiking	$B = .064$	$\chi^2 (1, N = 220) = 0.65$	$p = .419$	$\text{Exp}(B) = 1.066, 95\%[0.913, 1.244]$
COVID-19 Severity	$B = -.163$	$\chi^2 (1, N = 220) = 5.48$	$p = .019$	$\text{Exp}(B) = 0.850, 95\%[0.741, 0.974]$
Model #3				
Personal Social Distancing	$B = -.228$	$\chi^2 (1, N = 220) = 10.20$	$p = .001$	$\text{Exp}(B) = 0.796, 95\%[0.692, 0.916]$
Community Social Distancing	$B = -.007$	$\chi^2 (1, N = 220) = 0.01$	$p = .918$	$\text{Exp}(B) = 0.993, 95\%[0.864, 1.141]$
Step-Count before COVID-19	$B = .743$	$\chi^2 (1, N = 220) = 73.48$	$p < .001$	$\text{Exp}(B) = 2.102, 95\%[1.774, 2.492]$
Jogging/Hiking	$B = .041$	$\chi^2 (1, N = 220) = 0.29$	$p = .590$	$\text{Exp}(B) = 1.042, 95\%[0.897, 1.212]$
COVID-19 Severity	$B = -.140$	$\chi^2 (1, N = 220) = 4.04$	$p = .044$	$\text{Exp}(B) = 0.870, 95\%[0.759, 0.996]$

Note. B is the log coefficient (ln transformed). $\text{Exp}(B)$ is an odds-ratio (transformed back from log). $\text{Exp}(B)$ below 1 indicates decreased step-count. $\text{Exp}(B)$ above 1 indicates increased step-count. Model 1: A GLM (negative binomial distribution) with personal social distancing, average step-count before COVID-19, jogging/hiking between April 3-9, 2020, and COVID-19 severity in participants' locations as predictors (all z-scored). Model 2: Identical to Model 1

¹⁹ Surprisingly, going jogging/hiking did not predict greater step-count in Study 1. We altered the scale points of this measure and, after doing so, found jogging/hiking to predict higher average step-count as expected in Study S1.

except personal social distancing replaced by community social distancing. Model 3: Identical to Models 1 and 2 except both personal and community social distancing were included as predictors.

Linking self-reported social distancing to change in step-count over time. To establish the robustness of these results, we again examined the link between reported social distancing and step-count but conducted Generalized Estimating Equations (GEEs) instead of GLMs. Though the GLM analyses (described above) revealed that personal social distancing predicted lower step-count during COVID-19 (while controlling for step-count prior to COVID-19), these analyses did not examine whether personal social distancing predicts a *reduction* in daily step-count from before COVID-19 to during COVID-19 (i.e., a change in step-count). A GEE analysis, which is often applied to repeated-measures and time-series analyses (in our case step-count data at Time 1 [April 3 to 9, 2019], Time 2 [March, 2019], Time 3 [February, 2020], and Time 4 [April 3 to 9, 2020]) provides a more rigorous test of whether self-reported social distancing really does predict a reduction in step-count from before COVID-19 to during COVID-19.

The conducted GEEs were identical to the three GLM models we conducted except the repeated measures structure of the data was taken into account (Time Point was included as a within-participants factor). Again, in Model 1 we entered personal social distancing, in Model 2 community social distancing, and in Model 3 both types of social distancing. And again, in each model, jogging/hiking in April 3 to 9 (2020) and COVID-19 cases per capita were entered as continuous control predictors (both *z*-scored). Finally, unlike the GLMs, Time Point was entered as a within-subjects factor (Time 1 [March, 2019], Time 2 [April 3 to 9, 2019], Time 3 [February, 2020], and Time 4 [April 3 to 9, 2020]). As such, the interaction between self-reported social distancing and Time Point functioned as the main predictor of interest. The outcome variable was participants' average daily step-count during each of the four Time Points, respectively (as in the

GLMs, each of the models was fit to a negative binomial distribution given that step-count entails count data).

Personal social distancing (Model 1). Indicating that personal social distancing predicted a reduction in step-count from before COVID-19 to during COVID-19, an interaction between personal social distancing and Time Point was observed, $\chi^2(3, N_{\text{between}} = 219; N_{\text{within}} = 876) = 9.07$, $p = .028$. Importantly, this interaction revealed that participants high in personal social distancing (+1 *SD*) exhibited a reduction in daily step-count from pre-COVID to during COVID-19 (April 3-9, 2020), $\chi^2(1, N_{\text{between}} = 219; N_{\text{within}} = 876) = 23.95$, $p < .001$ (see Table 2; Helmert contrast-coding applied given the multiple before COVID-19 time points). Participants low in personal social distancing (-1 *SD*), on the other hand, exhibited similar step-counts before COVID-19 and during COVID-19, $\chi^2(1, N_{\text{between}} = 219; N_{\text{within}} = 876) = 0.09$, $p = .768$ (see Table 2).²⁰ Regarding effect-size, and relating these findings back to average daily step-counts, for every increase of one *SD* in self-reported personal social distancing ($SD = 0.85$), individuals' average daily steps between April 3-9, 2020 were approximately 14.5% lower (formula: (# of steps lower than the average # of steps between April 3-9, 2020)/(average # of steps in April 3-9, 2020)). And, when comparing change in steps from before COVID-19 to during COVID-19, individuals high in self-reported personal distancing (+ 1 *SD*) exhibited a decrease from 4,834.29 to 3,256.88 average daily steps (a decrease of ~33% percent), while those low in social distancing exhibited a decrease from 4,487.70 to 4,363.97 average daily steps (a decrease of ~3 percent).

Community social distancing (Model 2). Again illustrating the unique predictive validity of personal social distancing, a significant interaction between community social distancing and

²⁰ Consistent results were also observed when conducting a generalized linear mixed-effects model that included participant id as a random intercept. Interaction term between personal social distancing and Time Point: $F(3,866) = 8.43$, $p < .001$ (see Analysis syntax on [OSF](#)).

Time Point predicting step-count was not observed, $p = .497$ (Table 2). That is, participants' perception of the degree of social distancing in their communities did not predict any change in their daily step-counts from before COVID-19 to during COVID-19.

Personal and community social distancing (Model 3). Finally, personal social distancing still interacted with Time Point to predict a reduction in step-count when including community social distancing and the interaction between community social distancing and Time Point in the model, $\chi^2(3, N = 219) = 11.38, p = .010$. And, in this model, the interaction between community social distancing and Time Point remained non-significant, $p = .342$ (see Table 2). Collectively, these analyses provide validity for self-reported personal social distancing as an accurate estimate of individuals' change in objective social distancing from before COVID-19 to during COVID-19.

Table 2

Participants Higher in Self-Reported Personal Social Distancing Exhibited a Reduction in Daily Step-Count from Before COVID-19 to During COVID-19. Those Low in Personal Social Distancing Did Not Exhibit a Step-Count Change. Participants' Self-Reported Community Social Distancing Did Not Predict Differential Changes in Step-Count.

		Average Daily Steps before COVID-19: March, 2019	Average Daily Steps before COVID-19: April 3-9, 2019	Average Daily Steps before COVID-19: February, 2020	Average Daily Steps During COVID-19: April 3-9, 2020
Self-Reported Personal Social Distancing	High Personal Social Distancing (+1 SD)	$M = 4,816.84,$ $SE = 306.51$	$M = 4,727.39,$ $SE = 294.23$	$M = 4,958.63,$ $SE = 350.27$	$M = 3,256.88,$ $SE = 335.78$
	Low Personal Social Distancing (-1 SD)	$M = 4,219.41,$ $SE = 286.78$	$M = 4,451.08,$ $SE = 297.98$	$M = 4,792.62,$ $SE = 324.13$	$M = 4,363.97,$ $SE = 428.09$
Self-Reported Community Social Distancing	High Community Social Distancing (+1 SD)	$M = 4,737.64,$ $SE = 302.72$	$M = 4,986.00,$ $SE = 322.95$	$M = 4,951.43,$ $SE = 392.01$	$M = 3,835.48,$ $SE = 404.41$
	Low Community Social Distancing (-1 SD)	$M = 4,297.03,$ $SE = 292.69$	$M = 4,194.61,$ $SE = 282.61$	$M = 4,797.49,$ $SE = 385.21$	$M = 3,801.04,$ $SE = 382.23$

Replication of findings (Study S1). As noted earlier, it is possible that participants may have reported inaccurate step-count data. Though we excluded participants who reported giving inaccurate step-counts, we conducted a supplemental study (Study S1; $N = 271$) to address this concern more directly. In Study S1, we replicated the findings of Study 1 while (1) having all participants upload screenshots of their step-counts which we verified, (2) changing the step-count time period during COVID-19 to be the full month of April, 2020 (instead of the single week of April 3-9, 2020), (3) additionally controlling for alternate factors, including going on remote walks, hikes, or jogs, exercising indoors, spending time in a private backyard, walking around one's house more than usual during COVID-19, having the 'device' on one's person, and engaging in activities before COVID-19 that would raise step-count, and (4) including not only iPhone but also Android users.²¹ Even when making all these changes, self-reported social distancing still predicted reduced step-count during COVID-19 as well as a decrease in step-count from before COVID-19 to during COVID-19 (see Supplements).

Potential bias in participants' responding. Though participants' self-reported personal social distancing accurately captured variance in their actual social distancing, participants' self-reported responding was not bias-free. Participants' personal social distancing was consistently higher than their community social distancing; participants rated themselves as better social distancers than the average person in their community (in their U.S. State), $t(278) = 12.93$, $p < .001$, $d = .97$. This finding indicates that participants either overestimated their own social distancing performance or underestimated the social distancing performance of others (or both). Either way, these results capture participants' bias in that it is objectively impossible for most

²¹ We also included Fitbit users. Parallel results were not found for Fitbit users potentially because Fitbit users specifically use Fitbit to track exercise (see Supplements: Study S1).

people to be better-than-average at something (the better-than-average effect; Alicke & Govorun, 2005).

Discussion

In Study 1, individuals' reported own degree of social distancing predicted their actual social distancing as assessed by reduced daily step-counts during the COVID-19 pandemic and a decrease in daily step-count from before COVID-19 to during COVID-19 (measured via smartphone step-tracking data). These results were found while controlling for participants' reported tendency to go on socially distanced walks/hikes/jogs and the COVID-19 severity in the participants' location (cases-per-capita in a county on the day participants completed the study). Importantly, supporting the unique predictive validity of these findings, the degree to which participants reported that their *community* was social distancing did not predict their own step-count averages. Indeed, if step-count is a valid indicator of social distancing one would expect people's self-reported personal social distancing—and not necessarily their perceived social distancing of their community—to predict their step-count. Finally, all these findings were replicated in a supplemental study (Study S1) when (1) verifying participants' step-counts via uploaded app screenshots, (2) changing the step-count time period to the month of April, 2020, (3) controlling for numerous additional factors (e.g., exercising indoors), and (4) including not only iPhone but also Android users. Collectively, these findings support the notion that self-reported social distancing measures are valid indicators of people's actual social distancing.

Study 2

Study 1 was limited to the individual-level. Given the community-based nature of viral pandemics (e.g., rapid community spread, ICU bed shortages in specific communities), however, social distancing at the group-level should also play an important role in understanding and

combating pandemics. Indeed, during the current COVID-19 pandemic (as well as the 1918 Influenza Pandemic) stark differences in social distancing were observed at the region and community level (Bosman & Mervosh, 2020; Engle et al., 2020; Gollwitzer et al., 2020; Strohlic & Champine, 2020). And, importantly, these differences resulted in differential subsequent rises in infections and fatalities depending on region (e.g., Gollwitzer et al., 2020; VoPham et al., 2020, 2020; Johnson & Thompson, 2020). Given this apparent importance of group-level differences in social distancing, in Study 2, we tested whether the findings of Study 1 extend to the group-level. Specifically, we examined whether the self-reported social distancing of individuals living in U.S. States during the COVID-19 pandemic predicted the degree of objective social distancing in those states. To do so, we tested whether state residents' self-reported social distancing predicted the degree of reduction in general movement and visiting non-essential services (e.g., barbers, restaurants, clothing stores) from pre- to during COVID-19 in those states as calculated by the daily GPS mobility data of approximately 15 million people (across the U.S.). And, to account for both past judgments (recall) as well as future judgments (intentions), we examined whether participants' self-reported social distancing in the week prior to participation as well as participants' intentions to social distance in the week following participation predicted objective social distancing (in the respective weeks).

Participants. As pre-registered, we aimed to recruit 50 to 115 participants on MTurk in each of the 35 most populous U.S. States (pre-registration [here](#)). 2,922 participants were recruited (1,527 Female; $M_{\text{age}} = 40.41$, $SD_{\text{age}} = 13.10$). 71 participants were excluded for attention check failure, and 8 for completing the study twice (identified via MTurk-ID), resulting in a total sample of 2,843 participants. Participants completed the study between April 10th and 14th, 2020. Recruitment was ended 4 days after posting the study. When the study ended, several of the 35

U.S. states did not include 50 or more participants. Specifically, we were able to recruit 50 participants or more for 29 of the 35 included U.S. States, resulting in a final sample size of 29 U.S. States (see Appendix; $M_{Sample\ Size} \sim 88$ per State; $N = 2566$). A sensitivity power-analysis revealed that with this sample size ($N = 29$) we had 80% power to observe a large effect ($r \sim .5$) at the U.S. State-level. APA ethical standards were followed and participants completed informed consent.

Self-reported social distancing. Unlike in Study 1, in which self-reported social distancing was calculated at the individual-level, self-reported social distancing in Study 2 was calculated at the group-level by collapsing across participants in each of the included states (i.e., creating self-reported distancing estimates for each of the included states). The self-reported social distancing measures participants in each U.S. state completed were as in Study 1 except for several small changes. First, we measured self-reported social distancing in terms of participants' social distancing in the week prior to participation ("In the last week...") and in terms of participants' intention to social distance in the week after participation ("In the next week, I intend..."). All four measures were used to measure self-reported social distancing in the past week. Due to time constraints, only the Jordan et al. (2020) and face-valid measures were used to measure social-distancing intentions. Second, we adapted the Jordan et al. (2020) items because social distancing practices became stricter as COVID-19 progressed into April, 2020. For instance, we changed "going to restaurants" to "getting take out from restaurants" (see Verbatim Files on [OSF](#) for all measures).

Social distancing behavior. U.S. States' objective social distancing behavior was provided by Unacast (a software company that provides location tracking and map services) using ~15 million daily smartphone GPS coordinates across the United States (Unacast, 2020). Unacast

gathers GPS data from thousands of opt-in consent apps. This GPS data is then categorized in terms of stops (a device being stationary over a period of time) and travel events (a device moving). Using this categorization, Unacast provided us with U.S. states' percent reduction in overall movement and visiting non-essential services (e.g., barbers, restaurants, clothing stores) from before COVID-19 struck the U.S. with force (before March 9th) to during COVID-19 (after March 8th).^{22,23} More specifically, social distancing on a specific day in a specific state was calculated as the change in people's overall movement and visiting nonessential services from the same day of the week during the four weeks leading up to March 9 in that state, respectively (to account for variation based on day of the week). For example, a state's level of physical distancing on Monday, March 9 was calculated as the percent reduction in movement and nonessential visits in that state from the average levels of movement and nonessential visits on the four pre-COVID Mondays (February 10, February 17, February 24, and March 2) in that state. So, if a state decreased their movement by 70% on a specific day from their average movement for the same day of the week prior to COVID-19, they would receive a score of 70 on this measure. The two included measures, overall movement and visiting nonessential services, were anonymized in that they were calculated by aggregating GPS coordinates by state (GPS coordinates of specific individuals were not shared with the authors). Importantly, researchers have documented the validity of the utilized Unacast data and have successfully applied this data to examine movement trends during COVID-19 (e.g., Gatalo et al., 2020; Gollwitzer et al., 2020; Johnson & Thompson, 2020; VoPham et al., 2020). Additional information regarding the Unacast data and measures can be found in the Supplements.

²² For what exactly qualified as non-essential retail and services, see Supplements.

²³ Before March 9th was chosen by Unacast (the software company that shared this data with the authors) as pre-COVID.

COVID-19 infections and fatalities. COVID-19 health outcomes (infections and fatalities), which were included as control variables, were quantified in two ways. First, they were calculated as in Study 1 (e.g., infection per capita on the day of participation) but at the state-level rather than county-level (i.e., based on the state participants were located in). Second, as in past research (e.g., Courtemanche et al., 2020; Gollwitzer et al., 2020), we also calculated infections and fatalities in terms of growth rate, quantified as the change in infections and fatalities in a state from the day before (infection and fatality totals were provided by New York Times [2020]). For instance, an increase from 100 cumulative infections on Day 1 in a state to 125 cumulative infections on Day 2 qualifies as a 25% growth rate in that state. The formalization of this calculation is: $(r_t = (I_t - I_{t-1})/I_{t-1} \times 100$, where I is the cumulative number of infections and t is Time in terms of day. We included growth rates as a measure of COVID-19 health outcomes to account for variations in the progression of states' disease spread (e.g., some states may be at the start of an outbreak while others are experiencing advanced viral spread). See Supplements for additional details.²⁴

Procedure. Participants completed the self-reported social distancing measures (randomized, clustered together). Participants then completed the attention check item and demographic items. Objective social-distancing was not provided by participants and instead was calculated via the Unacast GPS mobility dataset as described above.

Results

Analysis plan. The following results are separated into three sections: (1) Calculation of self-reported social distancing and objective social distancing, (2) linking self-reported personal

²⁴ We also examined whether states' self-reported social distancing predicts a time-lagged decrease in those states' infection and fatality growth rates. While we did find this to be the case, these results were not robust to adding temporal and geo-spatial autocorrelation terms to the model, and thus, are discussed in the Supplements.

and community social distancing of U.S. States to objective social distancing in those states (both in terms of self-reported social distancing in the past week and intentions to social distance in the next week; conducted using Linear Mixed-Effects Models [LMEs]), and (3) robustness checks on the observed findings (e.g., including adding COVID-19 rates as a control variable to the conducted models). Across these analyses, and as in Study 1, we examined states' personal- and community-directed self-reported social distancing separately. Additionally, we conducted our analyses for the two types of objective social distancing—states' reduction in general movement and states' reduction in visiting non-essential services—separately. Finally, and as noted earlier, we examined participants' self-reported distancing in the week prior to participation and intentions to social distance in the week after participant, separately.

Calculating the social distancing measures.

Self-reported social distancing in the prior week (past). We first calculated participants' self-reported social distancing in the past week. As in Study 1, we observed two factors underlying participants' self-reported social distancing: personal- (*eigenvalue* = 2.24) and community-directed social-distancing (*eigenvalue* = 1.27). Participants' personal and community self-reported social distancing in the past week was calculated by averaging across the respective personal and community social distancing items (see Study 1; after *z*-scoring each measure; $\omega_t = .92$, and $\omega_c = .71$ across all items, respectively).²⁵ The self-reported personal and community social distancing scores of the 29 included U.S. States were then calculated, respectively, by averaging across the scores of participants within each of these States (see RMarkdown file on [OSF](#) for descriptives; *n* per state ~88; see Figure 1). As in Study 1, we present the results separately for personal- and community-direct social distancing.

²⁵ We had pre-registered that we would exploratorily include states in which we had between 45 and 50 participants. These analyses were not conducted as no states had this number of participants.

Self-reported intentions to social distance in the next week (future). We calculated states' self-reported intentions to social distance in the week after participation. To do so, we z-scored the two future social distancing measures (the adapted Jordan et al. measure and the adapted single face-valid item; $r = .46$), collapsed across these scores, and then averaged across these final values within each of the respective states. Again, intentions to social distance were only assessed for personal-directed (and not community-directed) social distancing (see Figure 1).

Figure 1. A) Self-reported personal social distancing in the week prior to participation. B) Self-reported community social distancing in the week prior to participation. C) Self-reported intentions to social distance in the week after participation. All z-scored.

Objective social distancing. States' objective social distancing in terms of reduction in overall movement and reduction in visiting nonessential services from before COVID-19 to during COVID-19 was provided by the Unacast dataset. Because these values were available for each of the 7 days (one week) prior to participants' study completion and each of the 7 days after

participants' study completion (a total of 14 days), we assigned objective social distancing scores for each of the included 14 days to each participant. Notably, these objective social distancing scores varied as a function of when individual participants completed the study and which state they were in. That is, the objective social distancing for each of the 14 days around participants' completion date depended on if a participant completed the study, for instance, on April 10th or April 14th, 2020, and whether they were in, for instance, Connecticut or Arizona. Taken together, then, objective social distancing in the 7 days prior to participation and 7 days after participation for each participant was calculated based on their participation date and their location (in terms of U.S. State). Finally, to create objective distancing scores at the U.S. state-level, we averaged across participants' objective distancing scores (for each of the 14 days) in each U.S. state to create composite objective social distancing scores for each state for the included 14 days (see Figure 2; see RMarkdown file on [OSF](#) for descriptives per state).

Figure 2. A) Objective social distancing: Reduction in general movement in the week prior to participation. B) Objective social distancing: Reduction in visiting non-essential retail and services in the week prior to participation. C) Objective social distancing: Reduction in visiting non-essential retail and services in the week after participation. D) Objective social distancing: Reduction in visiting non-essential retail and services in the week after participation

Linking self-reported social distancing to objective distancing. We conducted a series of Linear Mixed-Effects Models (LMEs) to examine whether self-reported social distancing in U.S. States predicted objective GPS-based social distancing in those states during the COVID-19 pandemic. We first conducted six LMEs (Models 1 through 6) to examine the predictive power of states' self-reported social distancing in the past week (reported social distancing in the week [7 days] prior to participation; between April 4th and April 14th, 2020) on states' degree of objective social distancing. Depending on the specific LME model, self-reported personal social distancing (Models 1 and 2), self-reported community social distancing (Models 3 and 4), and both personal and community social distancing (Models 5 and 6) in the past week functioned as the predictor

variables (all continuous, z -scored). Objective social-distancing during the same time-period in terms of reduction in general movement (Models 1, 3, and 5) and reduction in visiting non-essential services (Models 2, 4, and 6) functioned as the outcome variables (percent reduction from pre- to during-COVID-19). Time, in terms of day, was included as a within-state fixed-factor (7-levels given the 7 days prior to participation; e.g., if a participant completed the study on April 10th, April 9th was Day 7, the 8th was Day 6, the 7th was Day 5, and so on until Day 1, 7-days before participation, which was April 3rd). State and Time (crossed-factors) were included as random intercepts in the models.

Thereafter, we examined states' self-reported intentions to social distance in the week after participation (Models 7 and 8). The models were identical to Models 1 and 3 except that self-reported personal social distancing in the past week was replaced with self-reported intentions to social distance in the next week (the models with self-reported *community* social distancing were not conducted as such social distancing scores were not assessed for intentions).²⁶ Importantly, in all these models (Models 1 through 8), the outcome variable (states' objective social distancing) were change scores in terms of states' percent reduction in movement and visiting non-essential services from pre-COVID-19 (before March 9th, 2020) to during COVID-19 (after March 9th, 2020). As such, we were able to account for states' movement and visitation before COVID-19 began in our analyses.²⁷

Self-reported social distancing in the past week. As predicted, Models 1 and 2 found that states' self-reported personal social distancing ($N_{participants} = 2,566$; $N_{state} = 29$) in the week prior to participation predicted the degree of actual social distancing in those U.S. states in that week, both

²⁶ We assumed that while participants have insight into their own intentions to social distance in the next week, they likely do not have insight into their communities' intentions to social distance in the next week

²⁷ Before March 9th was chosen by Unacast (the software company that shared this data with the authors) as pre-COVID.

in terms of percent reduction in overall movement ($M = 43.77$, $SD = 6.76$), $B_{\text{movement}} = 2.82$, $p = .022$, and visiting nonessential services ($M = 63.99$, $SD = 5.39$), $B_{\text{visitation}} = 2.02$, $p = .042$ (see Table 3). Similarly, Models 3 and 4 found that states' self-reported community social distancing also significantly predicted objective distancing in terms of predicting a greater reduction in states' general movement, $B_{\text{movement}} = 4.40$, $p < .001$, and visiting of nonessential services, $B_{\text{visitation}} = 3.10$, $p < .001$ (Table 3; Figure 3).^{28,29} Finally, Models 5 and 6 indicated that the link between self-reported social distancing and objective distancing is driven by community rather than personal social distancing responses. While self-reported personal social distancing no longer predicted objective social distancing when both personal and community social distancing were included in the models, $ps > .378$, community social distancing still significantly predicted objective social distancing, $ps < .007$ (Table 3).

²⁸ Additionally, as in Study 1, self-reported hygiene practices also predicted objective social distancing, both in terms of distance, $B_{\text{movement}} = 3.23$, $p = .008$, 95% CI[0.93, 5.53], $R^2_{\text{Marginal}} = .16$, $B_{\text{visitation}} = 2.38$, $p = .015$, 95% CI[0.51, 4.26], $R^2_{\text{Marginal}} = .16$.

²⁹ Our data also descriptively identify which States under self-report (below the slope-line in Figures 2 and 3) and over self-report (above the slope-line in Figures 2 and 3) their social distancing as compared to their true social distancing behavior (as compared to other states). For instance, comparatively, New York and Massachusetts exhibited higher degrees of actual social distancing relative to their self-reported social distancing, whereas Oregon and Tennessee overestimated how well they are actually social distancing.

Table 3*Study 1: Output of LME Models 1 Through 8.*

	<i>Coefficient</i>	<i>p</i> value	95% Confidence Interval
Model #1: Predicting Percent Reduction in General Movement, $R^2_{\text{Marginal}} = .12$			
Personal Social Distancing	$B = 2.82$	$p = .022$	95% CI[0.44, 5.20]
Model #2: Predicting Percent Reduction in Visiting Non-Essential Services, $R^2_{\text{Marginal}} = .11$			
Personal Social Distancing	$B = 2.02$	$p = .042$	95% CI[0.08, 3.96]
Model #3: Predicting Percent Reduction in General Movement, $R^2_{\text{Marginal}} = .29$			
Community Social Distancing	$B = 4.40$	$p < .001$	95% CI[2.42, 6.37]
Model #4: Predicting Percent Reduction in Visiting Non-Essential Services, $R^2_{\text{Marginal}} = .27$			
Community Social Distancing	$B = 3.10$	$p = .001$	95% CI[1.40, 4.80]
Model #5: Predicting Percent Reduction in General Movement, $R^2_{\text{Marginal}} = .30$			
Personal Social Distancing	$B = 0.98$	$p = .378$	95% CI[-1.26, 3.22]
Community Social Distancing	$B = 3.94$	$p = .001$	95% CI[1.69, 6.18]
Model #6: Predicting Percent Reduction in Visiting Non-Essential Services, $R^2_{\text{Marginal}} = .28$			
Personal Social Distancing	$B = 0.73$	$p = .449$	95% CI[-1.22, 2.67]
Community Social Distancing	$B = 2.76$	$p = .007$	95% CI[0.81, 4.70]
Model #7: Predicting Percent Reduction in General Movement, $R^2_{\text{Marginal}} = .07$			
Intentions to Social Distance	$B = 2.43$	$p = .067$	95% CI[-0.18, 5.03]
Model #8: Predicting Percent Reduction in Visiting Non-Essential Services, $R^2_{\text{Marginal}} = .11$			
Intentions to Social Distance	$B = 2.66$	$p = .021$	95% CI[0.43, 4.89]

Note. All self-reported social distancing measures were z-Scored. Descriptive statistics of the outcome variables: Percent reduction in general movement, $M = 43.77$, $SD = 6.76$. Percent reduction in visiting nonessential services, $M = 63.99$, $SD = 5.39$). Ranges: Percent reduction in general movement, min = 34.39, max = 56.25. Percent reduction in visiting nonessential services, min = 55.11, max = 75.62.

Figure 3. Study 2: A) The raw linear trends between U.S. states' self-reported personal social distancing (z-scored) and objective social distancing behavior, as measured via states' percentage reduction in overall movement (left) and travel to nonessential services or retail (right) from pre-COVID-19 to during COVID-19. B) The same raw linear trends but for self-reported community social distancing rather than self-reported personal social distancing (z-scored). Error bands: $\pm 1 SE$.

Given the importance of considering effect-size when examining whether self-reported judgments and objective behavior overlap, we considered the observed effect-sizes in several ways. First, our results indicate that a state $+1 SD$ in self-reported social distancing would exhibit approximately 4.84 (personal) and 7.50 (community) greater percentage points reduction in general movement and visitation from pre- to during COVID-19 as compared to a state $-1 SD$ in self-reported social distancing. Given the overall observed intercepts for states' movement and visitation (~ 53.88 percentage points), this means that the former state would have exhibited a 56.30% (personal) and 57.63% (community) reduction in objective distancing, while the latter state would have exhibited reductions of 51.46% (personal) and 50.13% (community); these differences equate to comparative differences in objective social distancing of 9.4% and 14.86%. When taking the restricted range of states' objective social distancing into account, however ($range_{movement} = [34.39, 56.25]$ and $range_{visitation} = [55.11, 75.62]$), these estimates increased to 23% and 35%. Finally, the observed marginal R^2 values indicate that states' self-reported social distancing overlaps with between 11% and 29% of the variance in states' actual social distancing (averaged: 19.75%). These results of Study 2 indicate that while self-reported and objective social distancing are far from the same thing, they do exhibit substantial overlap at the group-level.

Comparing personal and community social distancing in the past week. As in Study 1, we examined whether self-reported personal versus community social distancing is a better predictor of objective social distancing. Because Study 2 examined the link between self-reported and objective social distancing at the group-level, states' self-reported community social

distancing may be a better predictor of objective social distancing (indeed, Figure 3 and Models 5 and 6 support this possibility). To test this, we compared variance explained (R^2) and goodness-of-fit (AIC) of Models 1 through 4.³⁰ Indeed, community distancing explained more variance than personal distancing for both objective distancing measures (all marginal R^2 : $R^2_{\text{movement}} = .29$ vs. $R^2_{\text{movement}} = .12$, and $R^2_{\text{visitation}} = .27$ vs. $R^2_{\text{visitation}} = .11$, respectively). Community distancing also fit the data better ($\text{AIC}_{\text{movement}} = 1038.84$ vs. $\text{AIC}_{\text{movement}} = 1048.97$, and $\text{AIC}_{\text{visitation}} = 873.65$ vs. $\text{AIC}_{\text{visitation}} = 880.68$, respectively). Additionally, adding self-reported community social distancing to the models including only personal social distancing (see Models 5 and 6) improved model fit, both for distance, $\chi^2 = 11.50, p < .001$, and for visitation, $\chi^2 = 8.08, p < .001$. And, in these models (Models 5 and 6; see Table 3), community social distancing remained a significant predictor of objective distancing, $ps < .007$, while personal social distancing did not, $ps > .378$. Taken together with Study 1, and in line with the different levels of analysis of the two studies (person- vs. group-level), these results indicate that community social distancing is a better predictor of objective distancing at the group-level, while personal social distancing is a better predictor of objective distancing at the person-level.

Self-reported intentions to social distance in the next week. Study 2 also considered participants' self-reported personal intentions to social distance (instead of judgments about past behavior). As was the case for states' self-reported social distancing in the past week, Models 7 and 8 found that states' intentions to social distance in the next week positively predicted the degree of objective social distancing in that state in that next week, albeit a significant link was not observed for general movement, ($M = 41.30, SD = 7.14$), $B_{\text{movement}} = 2.43, p = .067$, but was for visiting nonessential services ($M = 63.15, SD = 6.35$), $B_{\text{visitation}} = 2.66, p = .021$ (Table 3; Figure

³⁰ Intentions to social distance in the next week was assessed only for personal and not community social distancing.

4). A significant result was also found when collapsing across the objective social distancing measures (movement and visitation), $B = 2.54$, $p = .034$, 95% CI[0.21, 4.88]. The observed marginal R^2 values (see Table 3) indicated that states' self-reported intentions to social distance in the next week overlapped with $\sim 9.5\%$ of the variance in states' actual social distancing in that week.

Figure 4. The raw linear trends between U.S. states' self-reported intentions to social distance (z-scored) and objective social distancing behavior, as measured via states' percentage reduction in overall movement (left) and travel to nonessential services or retail (right) from pre-COVID-19 to during COVID-19. Error bands: $\pm 1 SE$

Robustness checks and secondary analyses. We conducted several robustness checks of our results and secondary analyses. First, to examine whether the observed links between self-

reported and objective social distancing vary as a function of Time (Day 1 through Day 7 of the week prior to and after participation), we reran the main conducted LME models (Models 1 through 4 and Models 7 and 8) but included a two-way interaction term between self-reported distancing and Time (Day 1 through Day 7 of each week, entered as a categorical factor) in each model. Suggesting that the observed links between States' self-reported and objective social distancing are relatively stable across time, these two-way interactions did not explain significant variance in objective social distancing in 5 of these 6 models, $ps > .115$. We did, however, find a significant interaction in the case of self-reported community distancing predicting movement, $p < .001$ (see Figures 5 and 6; see RMarkdown file on [OSF](#) for details). Examining this single interaction revealed that self-reported community distancing (in the past week) was slightly more predictive of general movement in the middle of the 7 days as compared to at the endpoints (i.e., Day 1, Day 6, Day 7; see results in RMarkdown). Given that this result was not found for the other included variables, however, this result appears not to represent the general pattern of our data.

Figure 5. Study 2: Objective social distancing of each U.S. State as a function of Time (Day 7 on the x-axis indicates the day before participants completed the study; Day 1 indicates 7 days before the participant completed the study) and as a function of self-reported A) personal social distancing and B) community social distancing in that week (z-scored). Each line represents a U.S. State.

Figure 6. Study 2: Objective social distancing of each U.S. State as a function of Time (Day 7 on the x-axis indicates 7 days after participants completed the study; Day 1 indicates 1 day after the participant completed the study) and as a function of self-reported intentions to social distance in that week (z-scored). Each line represents a U.S. State.

Second, we examined whether the observed links were moderated by the specific self-reported social distancing measure. To test this, we compared goodness-of-fit (AIC) of the main LME models (Models 1 through 4 and Models 7 and 8) but when replacing the self-reported personal and community composite scores with the individual social-distancing measures (e.g., the Jordan et al. measure). Notably, the individual measures largely did not differ in terms of AIC: personal measures, $1053.23 < AIC_{\text{movement}} < 1057.30$, and $884.89 < AIC_{\text{visitation}} < 886.76$; community measures, $1045.03 < AIC_{\text{movement}} < 1049.54$, and $877.16 < AIC_{\text{visitation}} < 882.05$; intention measures, $1330.31 < AIC_{\text{movement}} < 1331.94$, and $1042.63 < AIC_{\text{visitation}} < 1044.70$. Furthermore, adding sub-measure as a within-participants factor to our LME models revealed that the link between self-reported and objective social distancing in all our models (i.e., across personal past, community past, and future predicting distance and visitation) was not moderated

by the sub-measures of the respective type of self-reported social distancing, all two-way interactions: $ps > .344$.

Third, we examined whether our findings remain when controlling for COVID-19 severity. Possibly, participants in locations experiencing more rapid COVID-19 spread would be more likely to self-report social distancing (due to social desirability and demand bias) while, at the same time, actual social distancing increased due to local and state policies. We reran the main LMEs (Models 1 through 4 and Models 7 and 8) to examine whether including COVID-19 infection and fatality growth rates eliminates the observed findings. To do so, we added infection and fatality growth rate in participants' location (U.S. State) as repeated-measure control variables (as was the case for objective social distancing, depending on the specific date participants completed the study; 7-timepoints, Day 1 through Day 7; see Method and Supplements for how growth rate was calculated and why we chose growth rate as a measure of COVID-19 spread). Across all six models, adding infection and fatality growth rates did not significantly change the predictive power of self-reported social distancing on objective distancing, $-0.20 < B_{\text{movement}} < 4.40$, $.068 > ps > .001$; $2.02 < B_{\text{visitation}} < 3.12$, $.042 > ps > .001$. Additionally, these results remained consistent when calculating growth rates using moving averages (to account for single day spikes in infections or fatalities) instead of using single-day scores, $.070 > ps > .001$ (see RMarkdown file on [OSF](#) for details). Furthermore, we did not observe significant interactions between self-reported social distancing and infection and fatality growth rates, $.831 > ps > .133$, except for in a single case, $p = .029$ (the interaction between infection growth rate and self-reported intentions to social distance predicting distance).³¹ Finally, the observed estimates decreased, but only slightly, when calculating COVID-19 severity via cumulative COVID-19 cases-per-capita instead of

³¹ This interaction is not particularly meaningful given that we only found a significant interaction term in 1 of the 12 potential interaction terms.

infection growth rate in the models (though, the intentions to social distance predicting movement was clearly non-significant, $p = .325$; see RMarkdown on [OSF](#)), $.087 > ps > .001$.³² Collectively, and in line with Study 1, these results suggest that states' COVID-19 severity is unlikely to account for the observed links between self-reported and objective social distancing, and further, that these links remain fairly stable across locations experiencing more or less severe viral spread.

Fourth, by collapsing across participants' self-reported social distancing in each U.S. State and using these averaged values in our analyses, we neglected to consider that variance in self-reported social distancing differs depending on the specific state (i.e., some states may have greater variance in participants' responding). Though this error variance was not propagated to additional analyses (and thus likely did not create substantial error; see Cone, Brown-Iannuzzi, Lei, & Dotsch, 2020)³³, we still wished to provide estimates regarding the observed findings that take this error-variance into account. We did so in two ways. First, as outlined by Foster-Johnson and Kromrey (2018), we applied White's correction which accounts for error-variance in micro-to-macro analyses while also retaining statistical power. When doing so, the estimated p -values actually reduced from our original analyses, personal social distancing: $p_{\text{movement}} = .016$, and $p_{\text{visitation}} = .032$, community social distancing: $p_{\text{movement}} < .001$, and $p_{\text{visitation}} < .001$, intentions to social distance: $p_{\text{movement}} = .056$, and $p_{\text{visitation}} = .025$ (see Table 3 to compare to the original models; see RMarkdown file on [OSF](#) for more details).

³² Importantly, Models #3 and #4 (the community social distancing models) remained highly significant, $p < .003$.

³³ Propagation error occurs when uncertain measures are used to calculate further measures or values. For instance, an example of propagation error is when group-based metrics (e.g., group-based CIs based on one sample) are thereafter evaluated by an additional independent sample (as is the case for some reverse-correlation studies; Cone et al. 2020). In the present study, however, group-based values (states' self-reported social distancing) were directly compared to other group-based values (states' objective social distancing). That is, they were not used to calculate a further variable (e.g., evaluated by an independent sample, weighted or multiplied to create a new variable). As such, the presented data do not suffer from propagation error.

Second, we ran 10,000 simulations in which states' self-reported social distancing scores were randomly selected within the 95% confidence intervals of the estimated means in each state. Said another way, the self-reported distancing values, either community or personal, were allowed to vary within the confidence intervals (error) of their average state values in each of the 10,000 simulations. In each simulation, these random social-distancing values (within the CI for each state) were then applied to the main LME models (Models 1 through 4 and Models 7 and 8) to obtain error-corrected coefficients and p -values. Supporting the robustness of our results, the averaged coefficients and p -values (across the 10,000 simulations) did not meaningfully differ from our original models: personal social distancing: $B_{\text{movement}} = 2.77$, $p = .028$, and $B_{\text{visitation}} = 1.98$, $p = .050$, community social distancing: $B_{\text{movement}} = 4.32$, $p < .001$, and $B_{\text{visitation}} = 3.05$, $p = .001$, intentions to social distance: $B_{\text{movement}} = 2.38$, $p = .079$, and $B_{\text{visitation}} = 2.61$, $p = .027$ (see Table 3 to compare to the original models).

General Discussion

Across two studies, we demonstrated a consistent link between self-reported social distancing and objective social distancing. In Study 1, participants' self-reported personal social distancing (e.g., "If I have to leave the house, I make sure to stay at least 6 feet away from other people") predicted lower daily average step-count during COVID-19 (April 3-9, 2020). Additionally, participants who self-reported greater personal social distancing exhibited a greater reduction in average daily step-count from before COVID-19 (March, 2019; April 3-9, 2019; February, 2020) to during COVID-19 (April 3-9, 2020). Specifically, participants high in self-reported social distancing (+1 SD) exhibited a 33% decrease in average daily step-count, while those low in such distancing (-1 SD) only exhibited a 3% reduction. Notably, these links were

observed while controlling for alternate predictors, including going jogging or on long walks in a socially distanced manner and the COVID-19 severity in the participants' locations.

While Study 1 considered the individual-level, Study 2 examined the group-level. Communities have played a major role in how people approach COVID-19 and social distancing. For instance, stay-at-home-orders, enacted at the U.S. state-level, have had a major impact on the spread of COVID-19 (Engle et al., 2020), as may have states' political partisanship (Allcott et al., 2020), and countries' initial responses (Anderson et al., 2020; McGrail et al., 2020). Informing these group-level differences, in Study 2, we observed that U.S. states' self-reported social distancing (both past and future oriented) predicted 20% of the variance in objective social distancing behavior in those states, as assessed by overall movement and visiting nonessential services and retail (quantified via ~15 million daily smartphone GPS-point coordinates across the U.S.).

Collectively, our findings indicate that self-reported social distancing measures do not suffer from self-report biases to the extent that they no longer predict actual behavior. As such, our results tentatively suggest that self-report measures of social distancing may be appropriate when in-person behavioral designs are infeasible. Nonetheless, we believe that behavioral measures should be used when possible; indeed, our article raises two measurement possibilities that entail easily and widely accessible data (step-count and mobility data).

Our results also indicate that researchers and public officials should consider the difference between self-reported *personal* social distancing and self-reported *community* social distancing. In Study 1, the degree participants rated themselves as social distancing predicted reduced step-count, whereas their judgments of their communities' degree of social distancing did not. And, in Study 2, states' self-reported community social distancing was a better predictor of objective social

distancing than their personal social distancing. For one, these results support the validity of our findings. That is, the observed findings align with self-reported personal social distancing predicting one's own objective social distancing, and self-reported community social distancing reflecting the objective distancing of the greater community. For another, they indicate that people's estimations regarding the social distancing performance of their own communities may be surprisingly accurate.

Our article also supports the validity of using smartphone data, including GPS data, as a behavioral measure of social distancing. Numerous recent studies have utilized GPS data as indicative of objective social distancing (Engle et al., 2020; Baker et al., 2020; Allcott et al., 2020; Gollwitzer et al., 2020), and some studies have even used this data as the basis for modeling future COVID-19 cases (Jia et al., 2020; Woody et al., 2020). Further, data scientists, epidemiologists, demographers and representatives of mobile networks have specifically encouraged the use of such data by government and health authorities when shaping public policy (Oliver et al., 2020). Our findings help validate these conclusions by illustrating that GPS data aligns with self-reported social distancing behavior (at least at the group-level).

Our results also may inform social distancing intentions. In Study 2, the intentions of individuals in U.S. States to social distance in the next week predicted those U.S. States' actual future social distancing behavior (though this link was only marginal for general movement). These findings provide the first-step for validating intervention attempts that measure social distancing via self-report (Jordan et al., 2020; Pfattheicher et al., 2020; Heffner et al., 2020). Nonetheless, we believe that a large amount of future validation is necessary for such intervention studies to be considered reliable. Indeed, experimentally manipulated intentions may function very differently than descriptive ones.

Limitations. Our work has limitations. For one, Study 1 relied on participants honestly reporting their step-count. Addressing this concern, we replicated the findings of Study 1 in Study S1 ($N = 271$; see Supplements) while having all participants upload screenshots of their step-counts which we verified. And, in Study S1, we also replicated our results when (1) expanding the step-count time-period to April, 2020 (rather than April 3-9, 2020), (2) additionally controlling for alternate factors, including going on remote walks, hikes, or jogs, exercising indoors, spending time in a private backyard, walking around one's house more than usual, having one's smartphone on one's person, and engaging in activities before COVID-19 that would raise step-count (see Verbatim Materials of Study S1), and (3) when including not only iPhone but also Android users.

A second limitation is that Study 2 was limited in terms of sample size (29 U.S. States). Though not ideal, our study still had 80% power to detect a true correlation of $r \sim .50$ or larger. Further, small sample sizes can be compensated for by using reliable measures (Horwitz & Horwitz, 2012); we included multiple social distancing measures (which exhibited high inter-measure reliability), and objective social distancing was calculated via the GPS coordinates of ~15 million individuals per day. Third, the generalizability of the findings is limited in terms of how representative MTurk participants are of U.S. Adults. Further, the findings may not necessarily extend to alternate cultures, alternate pandemics, and groups that are not based on geographic region (U.S. states).

Finally, it is unknown whether self-reported social distancing, akin to objective social distancing (e.g., Gatalo et al., 2020; Gollwitzer et al., 2020; Johnson & Thompson, 2020; VoPham et al., 2020), predicts real-world COVID-19 health outcomes, such as a subsequent decline in COVID-19 infections and fatalities. To examine this possibility, we conducted supplemental analyses examining whether states' self-reported personal and community social distancing

predicted a time-lagged decrease in COVID-19 infection and fatality growth rates. Though we found support for this possibility, the observed results were not robust to temporal and geo-spatial autocorrelation corrections (which are necessary given the time-series nature of this data). Though this lack of robustness may simply be a function of the small sample size, we deemed these results to be inconclusive and thus solely report them in the supplements (see the Study 2 Results section in the Supplements).

Caveats. We note two final caveats. First, readers should be careful to draw individual-level conclusions solely from the findings of Study 2, which was conducted at the group-level (U.S. States; see the ecological fallacy; Piantadosi, Byar, & Green, 1988). Said another way, self-reported and objective social distancing linking at the group-level does not necessarily mean they are linked at the individual-level (though, Studies 1 and S1 did demonstrate the observed link at the individual-level). That being said, researchers have noted that the ecological fallacy itself produces several fallacies (Schwartz, 1994). For one, the fallacy implies that group-level results are less rigorous than individual-level data and that group-level variables play no role in psychological processes and health outcomes. However, group-level variables have been documented to play a role particularly in disease and disease spread, for instance, in that group tightness and connectivity impacts disease spread (see Eubank et al., 2004; Klov Dahl, 1985). As such, it is possible if not probable that group-level variables play an important role in COVID-19 spread. Indeed, numerous group-level findings relevant to COVID-19 have been documented, including that countries with female leaders experienced smaller outbreaks (Coscieme et al., 2020), that politically conservative counties in the United States exhibit decreased social distancing and subsequent heightened outbreaks (Gollwitzer et al., 2020), and more generally, that social distancing interventions at the group-level (U.S. States) effectively curb infection rates

(Courtemanche et al., 2020). Given these findings, though the results of Study 2 should not be interpreted at the individual level, the results do complement and inform these other group-level COVID-19 findings as well as shed light on group- and community-level differences in social distancing.

As a second caveat, we note that step-count or general movement (as assessed by GPS coordinates) should not be de facto adopted as a direct measure of social distancing behavior. While our results provide support for these measures as indicative of objective social distancing, they are not the *exact same* as social distancing. This is particularly true for the step-count measure in Study 1. For instance, someone hiking in a remote place is social distancing despite having a high step-count. Nonetheless, we observed our findings when controlling for such step-count impacting third-variables in Studies 1 and S1, including but not limited to, exercise while having one's smartphone on one's person, hiking or jogging (including compared to before COVID-19), COVID-19 severity (COVID-19 cases-per-capita in one's location), and before COVID-19 average step-count (which should account for general individual differences in step-count produced by exercise or other variables). In other words, the current results are robust to potential third-variables, and thus, step-count likely did capture variance in individuals' actual social distancing as intended.

Conclusion

In conclusion, we demonstrated that self-reported social distancing is linked to objective social distancing both at the individual-level and the group-level. By demonstrating these associations, our findings provide initial support for the use of self-report measures as a means of assessing social distancing behavior during pandemics. Additionally, the presented studies inform our understanding of social distancing at the individual- and the group-level more generally, and

finally, contribute to a rich tradition of determining the efficacy of self-reported information at tracking real world behavior.

Data Availability

The datasets, analyses files, and verbatim method files of the presented studies are available open-source [here](#).

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