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IRVINE

Using Brief Internet Interventions to Challenge Loneliness at Scale

DISSERTATION

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DOCTOR OF PHILOSOPHY

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by

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ABSTRACT OF THE DISSERTATION

Using Brief Internet Interventions to Challenge Loneliness at Scale

by

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Loneliness is an enduring global health issue. Effective evidence-based loneliness interventions exist but lack sufficient scale to reach many who might benefit from them. Single-session interventions (SSIs), which aim to condense the core elements of evidence-based treatments into brief and broadly acceptable self-guided experiences, offer an opportunity to make robust guidance for overcoming loneliness much more accessible. This dissertation's objective was to inform the development of effective and broadly-appealing SSIs for loneliness. Study 1 (n = 908) showed that neither a 20-minute SSI version (p = 0.22) nor a 1-hour 3-session version of a self-guided online loneliness intervention (p = 0.23) changed loneliness over four weeks more than an active control SSI did. However, participants reported finding both loneliness interventions more appealing and valuable than the control (ps < 0.02). While some participants reported that the loneliness interventions had a lasting positive impact, the interventions did not appear to be more useful on average than an active control among lonely people aged 16 and older with internet access. Study 2 explored if popular online content was more effective at reducing distress than typical researcher-created SSI content among a sample

of mostly crowdworkers struggling with psychological distress. The study (n = 916) showed that a popular online content-centered mental health SSI did not affect participants' distress over four weeks differently than a mental health SSI with researcher-created content (p = 0.09) or finding content independently on the web (p = 0.42). On the contrary, participants assigned to the researcher-created SSI reported greater improvement than those assigned to the popular online content-based SSI in depressive symptoms (b = -0.44, p = 0.03) and loneliness (b = 0.29, p = 0.04), on average. Future work should aim to improve SSIs' effectiveness, identify populations for which SSIs are most effective, and implement SSIs in accessible and appealing ways.

CHAPTER 1

Loneliness and Opportunities to Intervene

INTRODUCTION

Social bonds are at the core of human life, shaping our perceptions of ourselves and the world (R. F. Baumeister & Leary, 1995; Hippel, 2018). For most of us, our relationships are our greatest sources of joy and guidance, but when our need for social bonds is unmet, we pay the painful emotional price of loneliness (Hawkley & Cacioppo, 2010; Perlman & Peplau, 1984).

Beyond just a personal struggle, loneliness is now recognized as a global health issue with the potential to profoundly impair mental and physical health across the lifespan (Beller & Wagner, 2020; J. T. Cacioppo et al., 2002; Manoli et al., 2022). As such, leaders in science and politics have been calling for interdisciplinary efforts to build a less lonely world (J. T. Cacioppo & Patrick, 2008; V. H. Murthy, 2020). In May of 2023, US Surgeon General Vivek Murthy released an advisory on loneliness in the USA advocating for a thorough re-prioritization of connection and belonging in American life (Murthy & The Office of the Surgeon General, 2023).

Defining Loneliness

Because social life is often challenging, everyone faces loneliness from time to time (J. T. Cacioppo & Patrick, 2008). We all intuitively relate to the basic experience of loneliness, but, like many emotional experiences, loneliness is also ephemeral and difficult to describe in a way that fully captures it as it is experienced by different people and across situations (Scherer, 2005). To understand loneliness, it is useful to consider it from several intersecting viewpoints.

Loneliness and Isolation

Loneliness's subjective nature differentiates it from social isolation, an objective lack of social contacts (J. T. Cacioppo & Hawkley, 2009; Perlman & Peplau, 1984; Russell et al., 2012). Indeed, loneliness and social isolation are only weakly correlated (Coyle & Dugan, 2012). One can feel lonely when surrounded by friends if one still craves more or different contact, while another might feel satisfied despite being objectively isolated if their need for interpersonal connection is lower (J. T. Cacioppo & Patrick, 2008; Perlman & Peplau, 1984; Svendsen, 2017). Factors like developmental stage and social context shape one's proclivity to feeling lonely at a given time (Matthews et al., 2022). For example, college students may be prone to loneliness because they are at an age when social life is a top priority, they recently lost the anchor of family life, and there is a cultural expectation for their college years to be socially rich.

The Adaptive Function of Loneliness

An evolutionary lens helps to explain why loneliness is such a universal part of the human experience. Interpersonal bonds enabled humans' remarkable success throughout our evolutionary history – a human alone in nature is fragile but humans working together in groups often dominate ecosystems (Hippel, 2018). This importance of belonging to a strong social group in our environment of evolutionary adaptiveness over many thousand years may explain our present-day attunement to the presence and quality of our social connections. According to the evolutionary theory of loneliness, loneliness evolved as a sharp warning signal indicating one's interpersonal relationships are in peril and motivating one to prioritize high-quality connections (J. T. Cacioppo & Cacioppo, 2018). Yet, the modern world's resource abundance and safety may have decreased loneliness's adaptive value relative to its psychological cost.

Dimensions of Loneliness

Loneliness is often categorized along two dimensions. First, the scale of the desired connection (intimate – communal), and second, the duration of the loneliness (transient – chronic) (J. E. Young, 1982). Distinguishing between these kinds may help to determine which interventions are appropriate for whom.

Connection scale can be separated as follows: intimate, including individuals with whom one experiences a self-other overlap, as in best friends or spouses; relational, including friendships and family relationships; and collective, regarding one's valued social identities or "active network" (e.g., group, school, team, or national identity) that enable connection to others at a distance in the collective space (Dunbar et al., 1995; Hawkley et al., 2005, 2012). Each level is independently valuable; someone in a happy marriage might feel lonely if they lack friends or sense that they do not belong in their community.

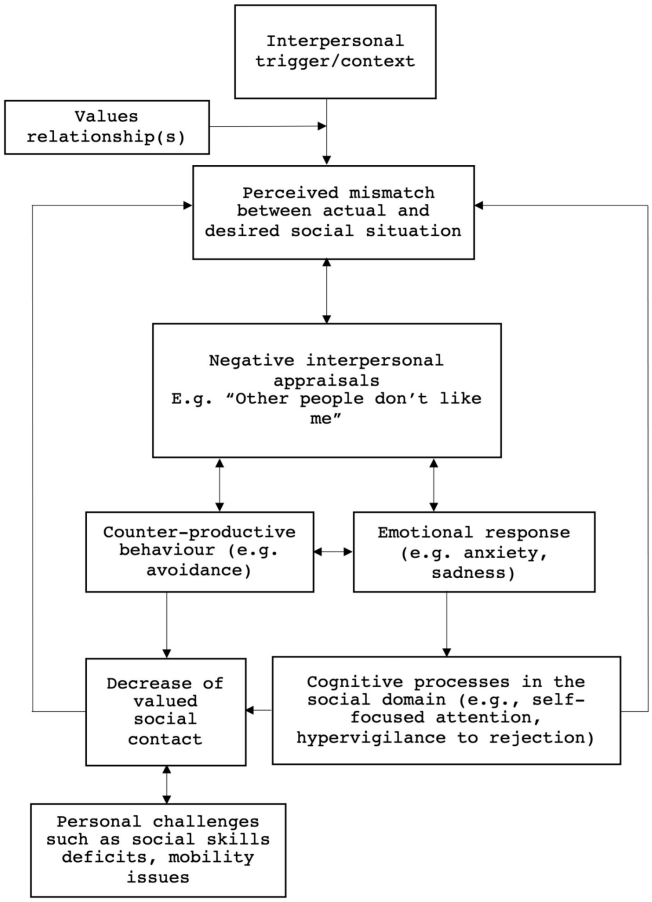
Duration is another dimension of loneliness. A popular taxonomy of loneliness chronicity breaks it into transient (brief and most common), situational (prolonged disruption lasting between six months and two years), and chronic (more than two years) (J. E. Young, 1982). Each of these temporal categories is associated with a distinct cognitive and behavioral pattern and predicts different long-term outcomes (Martín-María et al., 2020).

Processes Triggering and Reinforcing Loneliness

While moderate and short-term loneliness motivates one to prioritize valuable social relationships, loneliness at more chronic and serious levels can skew one's social cognitions and behaviors, making it more difficult to form and maintain high-quality relationships (J. T. Cacioppo & Patrick, 2008). Lonely people show heightened sensitivity to social threat and a tendency toward avoidance in the face of social challenges; as a result, they miss out on the

opportunities that these social challenges might offer (Bangee et al., 2014; S. Cacioppo et al., 2016; Nowland, Robinson, et al., 2018). Finally, loneliness can create a desperation for connection that could lead one to pursue weak or unhealthy relationships (Lynch, 1977). This cycle of maladaptive social perceptions, emotions, and behaviors can easily come to outweigh loneliness's adaptive utility (Käll, Shafran, et al., 2020). Integrating this research, Käll et al. (2020) developed a cognitive-behavioral model of loneliness describing how loneliness arises and becomes chronic, shown in Figure 1 (Käll, Shafran, et al., 2020).

Figure 1. A cognitive-behavioral model of chronic loneliness. From Käll et al. (2020). Reprinted with permission from Dr. Anton Käll, Ph.D.



Stigma

While younger generations today express less stigma than older generations have regarding mental illness in general, this pattern does not appear to extend to loneliness; young

people blame lonely individuals for experiencing loneliness more than older people do (Barreto et al., 2022; Pescosolido et al., 2021). Beliefs that lonely people and those around them hold about loneliness have the power to exacerbate or ameliorate loneliness (Barreto et al., 2022). Even as society grows more understanding of mental health struggles, the deeply-embedded stigma that loneliness is not only a feeling but an immutable mark of communal rejection persists (Pescosolido et al., 2021). For lonely people, the belief that their loneliness reflects a personal inadequacy could contribute to a fear of negative evaluation and feelings of social anxiety (J. T. Cacioppo et al., 2010). Moreover, when others believe lonely people are social burdens whose ostracism could be contagious, they might hesitate to extend a helping hand. The fact that loneliness is generally not considered a mental illness might actually make people consider it to be more of a personal failure and, as such, possibly less treatable.

Measuring loneliness

Loneliness is usually evaluated using self-report measures due to its subjectivity and the lack of reliable environmental or behavioral measures (Perlman & Peplau, 1984). Loneliness measures differ in three ways (Mund et al., 2022). First, they differ in their dimensionality: (i.e., unidimensional vs. multidimensional). Some suggest that loneliness contains between two and five factors such as isolation, relational connectedness, and collective connectedness (J. T. Cacioppo et al., 2006). Others argue for unidimensionality, noting high correlations between factors (Dodeen, 2015). Second, measures differ in the number of items they contain: single-item measures have shown utility, but they also capture more (potentially unreliable) state variance than multiple-item measures (Mund et al., 2022). Third, measures differ in whether they are direct or indirect (i.e., directly asking “Do you feel lonely”). Differences in the directness of loneliness measure items have been shown to cause variation in response patterns; for example,

men report less loneliness than women when using direct measures, but this gender difference disappears or reverses when using indirect measures (Maes et al., 2019).

Some researchers have argued that existing loneliness measures do not fully capture the experience of loneliness. For example, some claim that loneliness measures should include items evaluating how much one feels that others need them (Gordy et al., 2021). Yet, despite varying measurement approaches, popular loneliness measures correlate with one another strongly and self-ratings of loneliness correlate with informant ratings (Mund et al., 2022).

Loneliness and Well-Being

Loneliness is a transdiagnostic clinical phenomenon and a core determinant of health; it is associated with poorer physical health, cognitive health, and brain function (Hawkley & Cacioppo, 2003; Käll, Shafran, et al., 2020; Lim et al., 2020; Rodriguez et al., 2022; Teo et al., 2013). A meta-analysis found both loneliness and social isolation predicted mortality as much or more than other known mortality risk factors like smoking and physical exercise. (Holt-Lunstad, 2017; Holt-Lunstad et al., 2015). Loneliness is also strongly associated with mental disorders in general and with depression and social anxiety in particular (Beutel et al., 2017; Meltzer et al., 2013), though it is not unitary with either depression or social anxiety (Fung et al., 2017).

Despite a general consensus among scientists that loneliness is an important biopsychosocial risk factor, there are a few caveats to consider with this research (J. T. Cacioppo et al., 2010). Most notably, much of the research available on the subject is cross-sectional, preventing inferences about causality. However, there is evidence that loneliness is a common antecedent to emerging mental health problems (Lim et al., 2016), as well as causal evidence of loneliness reducing well-being from studies that experimentally manipulated social support (House et al., 1988; Uchino, 2006). Researchers generally infer a bi-directional relationship

between loneliness and well-being outcomes; e.g., loneliness can cause depression but depression can also push one towards thoughts and behaviors that exacerbate loneliness (Lim et al., 2020; National Academies of Sciences et al., 2020). Another caveat is that most research on loneliness and well-being does not distinguish between acute and chronic loneliness. This is important because chronic loneliness seems to impair functioning and well-being more profoundly than acute loneliness does (Lim et al., 2020; Martín-María et al., 2021; Zhong et al., 2016). Finally, although loneliness is this study's focus, it is not the only aspect of social life relevant to well-being. For example, social strain from a stressful relationship could impair well-being without one feeling lonely (Holt-Lunstad et al., 2015).

Multiple pathways can help to explain the pattern of decreased well-being in lonely individuals (J. T. Cacioppo et al., 2002; Hawkley & Cacioppo, 2003). One pathway is health behaviors (i.e., smoking, diet, and exercise). Differences in health behaviors in lonely versus non-lonely individuals have not been clear according to the extant research, although social isolation appears to be more clearly related to several health behaviors than loneliness is (J. T. Cacioppo et al., 2002; Kobayashi & Steptoe, 2018; Page, 1990). Another pathway is chronic stress. Lonelier people tend to have more active threat surveillance mechanisms and this chronic perception of social threat could cause persistent activation of a neurobiological stress response with downstream health impacts (Lam et al., 2021; McEwen, 2017). Moreover, lonely people lack the stress-buffering benefits of emotional and instrumental social support (Aneshensel & Stone, 1982; Kaveladze et al., 2020; Tsuboi et al., 2016).

Loneliness Risk Factors

Loneliness arises from an array of social, genetic, health, cognitive, and behavioral risk factors (Boomsma et al., 2005; Lim et al., 2020; Spithoven et al., 2019). Genetic and

environmental factors are jointly predictive of loneliness (each explains roughly 50% of its variance); moreover, one's environment determines how genes that contribute to loneliness are expressed (Mund et al., 2020; Spithoven et al., 2019). Some research has concluded "environmental influences [are] unique to each individual and no common environmental contributions to adult loneliness [are] discernible" (Boomsma et al., 2005). Other work has identified slight differences in loneliness across demographic factors; being under 25 or over 65, living alone, being of lower socio-economic status, and migrating to a culturally different country all predict higher loneliness (Barreto et al., 2022; Lim et al., 2020). Elements of built environments and population density also afford or diminish opportunities for social interaction (Bower et al., 2023; Hammoud et al., 2021).

Loneliness is intertwined with many other aspects of social life, including social support, social strain, and social integration (J. T. Cacioppo & Patrick, 2008; Jones & Moore, 1987). Furthermore, both perceived and objective social connection (objective meaning a count of how many connections one has) are relevant to loneliness (Barger et al., 2014). Parigi & Henson argue for integrating subjective and objective measures of connection, proposing that each strong connection helps to prevent loneliness while having too many weak connections (i.e., those that are "actively maintained yet fail to achieve a requisite amount of salience for the individual") worsens feelings of loneliness (Parigi & Henson, 2014). Supporting this reasoning, having more social contacts than one considers ideal is linked to higher loneliness (Russell et al., 2012).

Loneliness is shaped by cognitive-behavioral risk factors. For example, one's appraisals determine if a stressful social situation will lead to feelings of loneliness or not (Käll, 2021). Patterns of thought and action also determine one's likelihood of participating in social interactions that could help to overcome loneliness in the long term; a tendency to avoid

challenging social situations is maladaptive while an optimistic mindset toward social situations should be broadly helpful (J. T. Cacioppo & Patrick, 2008). The avoidance and negative appraisals that are central to anxiety and depression therefore partially explain these disorders' links to loneliness (McHugh Power et al., 2020; National Academies of Sciences et al., 2020).

Prevalence

Estimates of loneliness's prevalence vary widely due to measurement and sampling differences. For example, in a 2018 study of 20,000 Americans, roughly half reported sometimes or always feeling that no one knew them very well (Bruce et al., 2019). Yet, studies using other measures found a lower prevalence of loneliness at a problematic level (around 2% to 20%) (Beutel et al., 2017; Matthews et al., 2022; Surkalim et al., 2022). Prevalence estimates also need to consider demographic characteristics like age; one study found a peak in emotional loneliness in both younger and older people but stability in social loneliness (lack of belonging to a group) in early and middle adulthood (Manoli et al., 2022). Furthermore, it is difficult to interpret cross-cultural and cross-generational differences in self-report measures because people compare themselves to similar or salient others rather than a global average; this "reference group effect" typically results in cultural differences that might be apparent with more objective measures being minimized (Credé et al., 2010; Heine et al., 2002). Overall, future studies need to define and measure loneliness more consistently and better consider methodological weaknesses.

Geographic Variation in Loneliness

A systematic review and meta-analysis found consistent geographic differences in the prevalence of loneliness at a problematic level, with Northern European nations showing the lowest prevalence (2.9%) and Eastern European nations showing the highest (7.5%) (Surkalim et al., 2022). In addition, "individualistic" countries have lower levels of loneliness than

"collectivistic" ones and loneliness predicts well-being more in collective cultures than in independent ones (Beller & Wagner, 2020; Lykes & Kemmelmeier, 2014).

The reasons for these cross-national differences are not well understood, but one potential factor is the variance in social trust across nations. Interpersonal trust is associated with loneliness, and some have suggested that this link scales to the societal level (Rotenberg, 1994; Surkalim et al., 2022; Svendsen, 2017). Consistent with this hypothesis, the percentage of people who believe most people can be trusted is highest in Northern European countries, while Eastern European countries generally rank much lower (Ortiz-Ospina & Roser, 2016; You, 2012). However, other variables, such as the excellent quality of state institutions in Northern European countries, might partially explain both factors.

Addressing Popular Narratives about Loneliness' Prevalence

Narrative 1: Modern Loneliness Epidemic. While imperfect, prevalence estimates can help to evaluate some popular narratives about loneliness. One such narrative is that loneliness has been increasing since the 1970s (Lynch, 1977; Putnam, 2001). Although objective social isolation has increased over the past few decades (i.e., more people are living alone and discussion networks are smaller), global prevalence data do not support the idea that loneliness increased in that time period (Hawkley et al., 2019; McPherson et al., 2006; Ortiz-Ospina, 2019b, 2019a; Surkalim et al., 2022). However, an international cross-temporal meta-analysis that included 124,855 adults (18-29) found a linear increase in loneliness of 0.56 standard deviations on the UCLA loneliness scale between 1976 and 2019 (Buecker et al., 2021).

One of the main reasons cited for the supposed loneliness epidemic is a decreasing emphasis on community caused by capitalism-fueled individualism. However, this claim is weakened by the aforementioned data showing that "individualistic" countries have lower levels

of loneliness than "collectivistic" ones (Lykes & Kemmelmeier, 2014). Another explanation for supposed recent increases in loneliness has been that of a "loneliness pandemic" resulting from the COVID-19 pandemic's lengthy social distancing mandates. A meta-analysis concluded that mean levels of loneliness increased only slightly during the pandemic, although there was heterogeneity in this pattern, with greater increases in loneliness observed in young and older people, women, and single people (Ernst et al., 2022).

Overall, most data do not support an explosion in loneliness over the past half-century worthy of the title "epidemic." However, given self-report data's weaknesses in depicting differences across time, it is hard to know the extent to which the available self-report data correspond to true loneliness levels over time (Credé et al., 2010; Spector, 1994).

Narrative 2: Social Media is Increasing Teen Loneliness. A survey of over one million 15- and 16-year-old students found loneliness levels increased worldwide between 2012 and 2018, as did many other self-report and behavioral indices of teen mental health issues (Twenge, Cooper, et al., 2019; Twenge, Spitzberg, et al., 2019). Interestingly, the large meta-analysis of emerging adults noted above that found a linear increase in loneliness between 1976 and 2019 found that loneliness was relatively stable between 2012 and 2019, a break from the trend of a linear increase from 1976-2011 (Buecker et al., 2021). This difference in findings could reflect variance in loneliness measures used across studies or it could suggest that the increase in loneliness was limited to teens and not older emerging adults.

The reports of a large-scale increase in teen loneliness, alongside the hypothesis that this increase was due to the sudden ubiquity of digital technologies and social media that also occurred around 2012, garnered widespread attention (M. E. Morris, 2018; Tufekci, 2013;

Twenge et al., 2022). The supposed link between social media and loneliness in teens remains a subject of scientific debate (Orben & Przybylski, 2019; Twenge et al., 2021, 2022).

Participation in online social life can have both positive and negative impacts on social connectedness (Odgers et al., 2020). In this view, using the internet to forge and reinforce connections can ameliorate loneliness while using the web to escape the social world can worsen it (Nowland, Necka, et al., 2018; O'Day & Heimberg, 2021). Yet, such conclusions may underestimate the value of both social (e.g., online communities and groupchats) and non-social (e.g., compelling online videos) online experiences online for offering users psychological benefits (Kaveladze, Morris, et al., 2022; Kaveladze & Schueller, 2020).

Narrative 3: Loneliness is the Human Condition. A final narrative is that loneliness is a fundamental human problem. This narrative claims that humans constantly seek a state of connection, "where two are inextricably one," that is just out of reach (Mayers & Svartberg, 2001; Svendsen, 2017). Thomas Wolfe expressed this view in his 1930 essay, "God's lonely man," writing, "Loneliness, far from being a rare and curious phenomenon, is the central and inevitable fact of human existence" (Adams, 1946). Contrary to this narrative, empirical work shows that most people report feeling little to no loneliness most of the time (Barreto et al., 2021; Matthews et al., 2022).

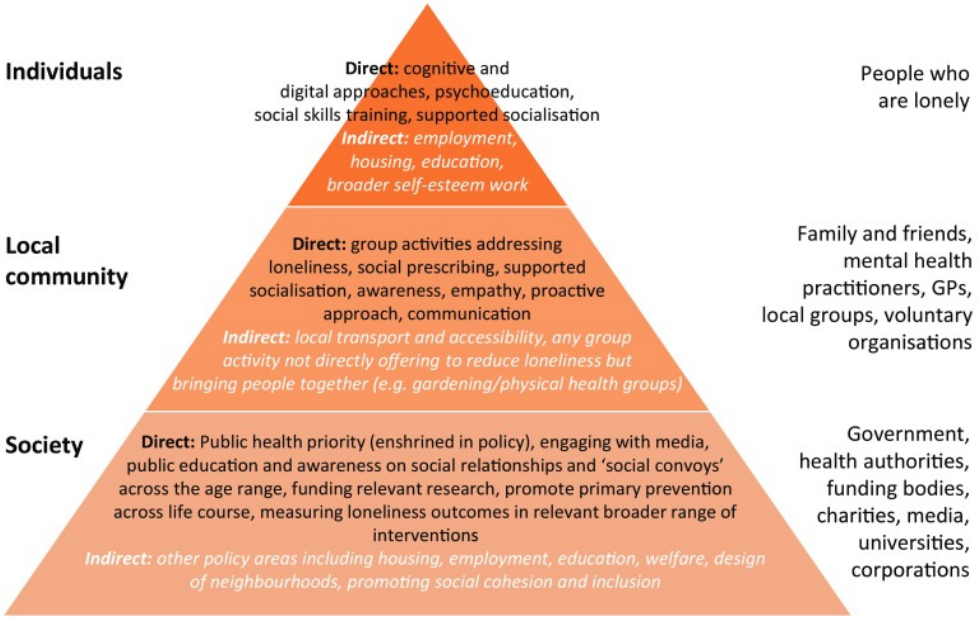
Although claims of modern loneliness explosions are probably overblown, loneliness remains a serious problem worth prioritizing on a population scale. Likewise, regardless of the internet's net impact on loneliness in the global population, it is important to identify ways that the social internet can better support users' well-being. Finally, even if loneliness is a certain part of the human experience, it is worth exploring ways to improve our relationship with it.

Addressing Loneliness

The science of loneliness interventions is young, and although some approaches have emerged as more promising than others, the evidence is not yet strong enough to make specific intervention recommendations (Ma et al., 2020; Mann et al., 2017). The present study, which employs a person-level approach optimized for scalability, should be considered as one of many feasible solutions at the individual, local community, and society levels. Here, I present research on loneliness interventions at the levels of person, relationship, group- and society (see Figure 2) (Mann et al., 2017). Finally, I note other approaches to overcoming loneliness that have received less attention from empirical research but could still provide useful directions for future inquiry.

Loneliness emerges from a complex combination of person-specific factors and manifests in varied ways, so researchers have emphasized the need for tailored loneliness interventions accounting for developmental and social factors (Hickin et al., 2021; Lim et al., 2020; Victor et al., 2018). Even at the individual level, confronting loneliness may require intervention against many risk factors at once (Mrazek et al., 2021).

Figure 2. Levels of Responsibility for Interventions for Loneliness. From Mann et al. (2017), reprinted with permission from Dr. Farhana Mann, Ph.D.



Individual Approaches

Evidence-based psychological interventions for loneliness are generally effective across the lifespan (Hedge's g = 0.43 compared to controls) (Eccles & Qualter, 2021; Hickin et al., 2021; T. Osborn et al., 2021). Yet, at present, no loneliness intervention has a sufficiently robust evidence base to be the clear gold standard (Mann et al., 2017). Moreover, existing interventions tend to have modest impacts compared with interventions for other problems like CBT for depression (Cuijpers et al., 2013; Mann et al., 2017). Two psychotherapeutic approaches have stood out as particularly promising for loneliness: addressing maladaptive social cognitions and improving social skills (Hickin et al., 2021; Masi et al., 2011). Social cognitions are the thoughts and beliefs regarding social life that one holds about oneself and others. Improving maladaptive social cognitions could reduce feelings of loneliness directly as well as indirectly by promoting more helpful thinking and encouraging prosocial behaviors, leading to improved social relationships down the line. Social skills enable one to communicate effectively and to build and strengthen connections. The two are closely connected: improving social cognitions can boost one's confidence in applying social skills while improving social skills can lead to more optimistic social cognitions.

The best-supported psychological loneliness intervention, cognitive-behavioral therapy (CBT) for loneliness, addresses both social cognitions and social skills as well as maladaptive behaviors. CBT for loneliness targets cognitive biases and behavioral patterns to improve self-efficacy, encourage social behaviors, increase social connections, and relax vigilance to social threat (Käll, Backlund, et al., 2020). There is also a wealth of evidence suggesting CBT is effective for disorders that often overlap with loneliness, such as social anxiety (Mayo-Wilson et al., 2014) and depression (Cuijpers et al., 2013). Despite their effectiveness, loneliness interventions' lack of scalability remains a barrier. (Fischer & Hartle, 2022).

It is also worth mentioning individual approaches that have shown promise despite being less intuitively related to interpersonal connection. First, improving general psychological well-being is one way to indirectly decrease feelings of loneliness. Thus, transdiagnostic treatments for mood and sleep can be useful loneliness interventions (Craske, 2012; Harvey et al., 2021). The fact that CBT addresses transdiagnostic processes such as negative thoughts and safety behaviors may be one reason that it is effective for loneliness. Another indirect strategy for decreasing loneliness is immersion in natural spaces (Hammoud et al., 2021). A randomized controlled trial with a sample of parents in the United States found that being assigned to visit a park over three months improved loneliness; it did so regardless of whether one was assigned to walk alone or with other people, suggesting it may have been the impact of time in nature rather than social contact that helped (Razani et al., 2018). Finally, a systematic review found that meditation practice is linked to lower loneliness, perhaps because it helps people to regulate difficult emotions (Saini et al., 2021).

Social Group Approaches

Social group approaches to loneliness aim to help individuals form and improve social relationships rather than focus on the person-level experience of loneliness. When successful, social group interventions may alleviate loneliness in a more self-sustaining and enjoyable way than the independent struggle often required of person-level psychotherapeutic approaches.

One social group intervention is to form groups explicitly for the exchange of social support. Such groups can vary in size and exist both in-person and online. Research evaluating social group interventions in medical contexts – sometimes called social prescribing programs – has shown some support for their usefulness (Hogan et al., 2002; Hou et al., 2022). At the same time, creating thriving social groups from strangers is often difficult and such groups may be a

source of social strain in addition to support (Baglioni et al., 2011). The challenge of creating supportive groups from scratch may be one reason that social group interventions have not yet shown much value in reducing loneliness (Masi et al., 2011).

Social group interventions show the most promise when they demonstrate an understanding of the needs and motivations of the populations they serve. One such success story is Men's Shed, an international organization that creates community-based handiwork/woodshop groups, mainly for older men, driven by the motto, "Men don't talk face to face, we talk shoulder to shoulder" (*Men's Shed*, 2023; V. H. Murthy, 2020). Thus, efforts to better fit users' needs may help to make social group interventions more viable. In addition, a wide range of extremely active support groups can be found online. In my 2019 survey of users of online mental health support communities, I found that the sense of belonging such spaces provided was one of their foremost benefits (Kaveladze & Schueller, 2020).

Society-Level Approaches

While individual- and relationship-level loneliness interventions are effective, they do not address the broader cultural and systemic forces that shape one's social life. A more radical approach advocates for a society-wide re-prioritization of values to put social connection first. In Vivek Murthy's words, "If we truly want to be healthy, happy, and fulfilled as a society, we have to restructure our lives around people" (Leland, 2022).

To Murthy, solving loneliness in America will require an alignment of cultural values, communal trust, familial closeness, and personal insight (V. H. Murthy, 2020). In his "National Strategy to Advance Social Connection," Murthy advocates for concrete changes at a national scale across several spheres of life, including community infrastructure, public policy, healthcare, and digital environments (Murthy & The Office of the Surgeon General, 2023). He

also proposed changes in workplaces to shift company cultures toward supporting and incentivizing kindness, generosity, and sharing among employees (V. Murthy, 2017). Other work has described further potential society-level changes; for example, the entertainment industry could make efforts to combat loneliness's stigma through media (Janoušková et al., 2017).

Other Approaches

We can also look beyond the empirical evidence on loneliness interventions for guidance. Many loneliness interventions have not yet been scientifically tested and other insights may simply not be conducive to testing with existing methods.

One approach to overcoming loneliness with a long tradition among philosophers (but less attention from psychologists) is to embrace the positives of solitude, reframing isolation as an opportunity for reflection, deep work, relaxation, and growth (Svendsen, 2017). Nietzsche called the ability to endure solitude "the most universal deficiency in our kind" (Nietzsche, 1881). According to these views, solitude is a source of freedom and virtue that, "rehabilitates the soul, corrects morals, renews affections, erases blemishes, purges faults, (and) reconciles God and man" (Petrarch, 1356). Or, as Drake asserted 666 years after Petrarch, "I've been losing friends and finding peace. Honestly that sounds like a fair trade to me" (Drake, 2022). From a well-being standpoint, this mindset towards solitude may reduce internalized stigma regarding isolation and help one to find joy in personal pursuits (Larson, 1997; Svendsen, 2017).

Lars Svendsen integrates these philosophies with empirical research on loneliness's health impacts. He concludes that a balance of connection and solitude is the ideal; a proper philosophical stance can transform loneliness into a healthy and meaningful solitude, but too much solitude will inevitably lead to loneliness (Svendsen, 2017).

Another perspective posits that human connection is irreplaceable for overcoming loneliness. James Lynch emphasized the importance of “dialog," which he defined as “reciprocal communication between two or more living creatures” (Lynch, 1977). For Lynch, “no material substitute – no simple thing – can fill the human need for dialog." This notion of dialog’s inherent aliveness was the basis for Lynch’s belief that human connection is fundamentally mysterious. Nearly 50 years later, Lynch’s claim might be subject to skepticism from advocates of using chatbots in digital health interventions to form “digital therapeutic alliances” with users (D’Alfonso et al., 2020; Henson et al., 2019; Kaveladze & Schueller, 2023). Yet, as of 2023, most people still prefer to connect with humans than artificial agents (R. R. Morris et al., 2018).

The Potential of Internet-Delivered Loneliness Interventions

Besides enabling widespread dissemination, several affordances of the internet could make it especially useful for loneliness interventions. These affordances offer benefits over in-person settings in terms of appeal, availability, personalization, and diversity of content (Andersson & Titov, 2014; Evans et al., 2017; Schueller et al., 2013).

One affordance is that the internet offers a relatively non-threatening medium to seek help compared to offline interventions. Seeking in-person support for loneliness may be particularly unappealing for lonely people who feel intimidated by difficult or stigmatizing social situations (Barreto et al., 2021; S. Cacioppo et al., 2016; Caplan, 2003; Käll, 2021). Online support dodges this barrier, offering a more approachable first step toward improvement.

Another affordance of the internet for intervening against loneliness is its facilitation of social connection. Online social spaces such as group chats with friends and hobby-based forum discussion threads can be genuine sources of community, as well as opportunities to improve social skills and confidence (Hunter et al., 2022; Mehta & Atreja, 2015). They are especially

useful because lonely people tend to prefer online interactions to in-person ones and find it easier to express their "true selves" online (Bargh et al., 2002; Indian & Grieve, 2014; McKenna et al., 2002; Morahan-Martin & Schumacher, 2003; Walther, 1996). Finally, belonging to an online community might translate into greater generalized prosociality (Nowland, Necka, et al., 2018).

A third affordance is the internet's vast repository of content. Online, one can find a wealth of information and inspiration in a range of styles across cultural and language boundaries on virtually any question. The web's interactivity also enables collaborative knowledge-building in global online communities of peers with shared experiences (Gilmour et al., 2019; Kaveladze & Schueller, 2020; Merchant et al., 2022; Schueller et al., 2013; Zhu & Stephens, 2019). One can even pay "crowdworkers" from sites like Amazon Mechanical Turk or Prolific for their input. Putting aside the value of face-to-face emotional support, one might be more likely to find effective and personalized solutions to one's issues by exploring the web than solely relying on one's limited in-person community.

Digital Mental Health Interventions

The global demand for mental health support is not being met by existing interventions – there are not enough mental health providers to provide individual interventions to everyone who could benefit from help. Given this need-to-access gap, prominent calls to "reboot" psychotherapy have emphasized that innovative solutions may be required to improve mental health interventions' reach (Kazdin & Blase, 2011).

Answering these calls, the field of digital mental health interventions (DMHIs) has proliferated over the past decade. DMHIs in the forms of online platforms, smartphone apps, and wearables have demonstrated an ability to widely disseminate efficacious support for various mental health issues such as depression and anxiety. Unlike face-to-face behavioral

interventions, DMHIs require less time (or no time) from care providers and can integrate into daily life to support habits and provide just-in-time support (Schueller et al., 2013). Some DMHIs, such as Headspace and Calm, have gained considerable popularity as consumer products, while others have been integrated into medical settings as components of standard treatments (East & Havard, 2015; Mani et al., 2015; Torous & Roberts, 2017).

Several DMHIs have demonstrated robust efficacy. For example, guided internet CBT (iCBT) is as effective as in-person CBT for many mental health issues and requires up to 85% less time from therapists (Andersson et al., 2019; Carlbring et al., 2018; Hedman et al., 2012). Unguided iCBT is also effective, although it generally produces weaker effects with lower adherence than guided iCBT (H. Baumeister et al., 2014; Karyotaki et al., 2017).

Low effective engagement, which refers to sufficient engagement with a tool to achieve intended outcomes, has been the foremost barrier to DMHIs' widespread uptake and impact (Kaveladze, Wasil, et al., 2022; Yardley et al., 2016). Most DMHIs intend for users to interact with them regularly, but most users tend to lose interest after only a few days, never achieving effective engagement (Wasil, Gillespie, et al., 2020). As a result, most existing DMHIs are much less effective when implemented in real-world settings (Wasil et al., 2021).

Loneliness Interventions

Only a few DMHIs targeting loneliness exist and few studies have investigated their efficacy. One study was a pilot randomized controlled trial of *Nod*, a self-guided smartphone app for reducing loneliness in college students that uses "social challenges" and other exercises to help build connections, support cognitive reframing, and foster a growth mindset (Bruehlman-Senecal et al., 2020). The trial found that using *Nod* did not reduce loneliness or other mental health and adjustment outcomes in a sample of first-year college students but it did buffer against

increases in loneliness among students with high baseline depression scores (Bruehlman-Senecal et al., 2020). Engagement with *Nod* was generally low in the trial, although lonelier students engaged with the platform more.

Another study examined the efficacy of a nine-week therapist-supported iCBT for loneliness. The program outperformed a wait list control, producing large improvements in loneliness and quality of life that persisted two years later (Käll, Backlund, et al., 2020). The intervention was based on the authors' cognitive-behavioral model of loneliness (Käll, Shafran, et al., 2020). As with *Nod*, engagement with the nine-week intervention was a challenge; study participants completed only 5.5 out of nine modules on average (Käll, 2021).

Why Create an SSI for Loneliness? – Rationale for the Dissertation

Online single-session interventions (SSIs) offer one solution to DMHIs' engagement problem: making interventions so short that long-term engagement is not required. SSIs are brief mental health interventions that aim to condense the core elements of evidence-based treatments into brief and broadly acceptable self-guided experiences. SSIs' low-intensity format is a departure from traditional models of evidence-based psychotherapy, but they may be well-suited to deliver efficient support to underserved populations who might not access support otherwise.

SSIs have been shown to produce small-to-moderate improvements in several mental health outcomes across diverse youth and adult populations (Ayers et al., 2015; Baglioni et al., 2011; Bunge et al., 2016; Lokman et al., 2017; Miu & Yeager, 2015; T. Osborn et al., 2021; Schleider, Dobias, Sung, & Mullarkey, 2020; Schleider et al., 2022; Schleider & Weisz, 2017b). However, other studies of SSIs have shown null results (Lorenzo-Luaces & Howard, 2022; Mullarkey et al., 2022).

SSI acceptability tends to be high among adolescent and adult populations, even with simple user experience design compared to popular contemporary mental health apps (Ahmedani et al., 2015; Schleider, Dobias, Sung, Mumper, et al., 2020; Wasil, Park, et al., 2020). Yet, although SSIs' brevity eliminates the need for long-term engagement, there is still substantial room to improve effective engagement with SSIs (Schleider & Weisz, 2017b). Even in SSIs, dropout is an issue; one study of adolescents found 15% dropped out of a 20-minute SSI when they were paid $20 and 67% dropped out when they were not paid (Cohen & Schleider, 2022).

Online SSI Design

Current internet-mediated SSIs generally present content and interactions using written explanations, images, and worksheets (see https://osf.io/ch2tg/ and https://osf.io/57b3k/ for examples of evidence-based SSIs). SSIs are often created with user-friendly survey-building / "experience management" software like Qualtrics and Typeform that do not require strong programming skills. This approach to SSI development is valuable because it is rapid, iterable, and has low overhead, allowing a team to create an SSI without needing to develop a custom DMHI platform, which might cost between several thousand and several million dollars, depending on one's goals. This ability to cheaply create and modify interventions and modules is also useful for researchers from an open science perspective as it facilitates comparisons across SSIs and SSI elements. For example, the Lab for Scalable Mental Health encourages researchers to copy, distribute and adapt their publicly-available SSI materials.

Because internet-mediated mental health SSIs are fairly new, evidence-based best practices for design have largely not been established. However, a few promising SSI design principles have emerged, mostly drawing from evidence-based in-person interventions. One is

BEST, which has guided the development of several evidence-based SSIs for youth mental health. BEST includes four elements: B: Brain science to normalize concepts in the program, E: Empower youths to a "helper" or "expert" role, S: Saying-is-believing exercises to solidify learning, and T: Testimonials and evidence from valued others (Schleider, Dobias, Sung, & Mullarkey, 2020). More work is needed to formally test the value of design frameworks like BEST, as well as to explore other approaches to SSI design.

Few people today know that SSIs exist, so identifying how to best market and implement them to reach diverse populations is a key task (Becker, 2015; Kodish et al., 2023). As an example of a tailored implementation of SSIs into existing help-seeking structures and communities, three 5-8-minute SSIs for teenagers were embedded in the social media site *Tumblr* such that participants who searched for a given mental health-related topic were routed to an appropriate SSI. This implementation showed promising effects on user mental health outcomes and resulted in over 6,000 people accessing SSIs through Tumblr in a year (Dobias et al., 2022). In another implementation, individuals experiencing a mental health crisis were randomized to receive either a typical crisis response (988 crisis hotline provision) or a one-minute online "enhanced crisis response" SSI. Participants randomized to the SSI reported a greater decrease in hopelessness and were more likely to use supportive resources offered (Cohen et al., 2023).

Sudden Gains and Insight Experiences as a Promising Direction for SSI Design

In a therapy context, psychological change can occur gradually, as a linear process of learning and practice, and it can also occur in a sudden transformative breakthrough. Sudden gains, which describe a large and rapid improvement in a mental health outcome, are common in

therapy; in one study, over half of the participants undergoing a CBT trial experienced a sudden gain. (Tang et al., 2002; Tang & DeRubeis, 1999). Despite their brevity, simple user experience, and lack of therapeutic alliance, existing SSIs are capable of producing immediate large-scale improvements in mental health symptoms (Schleider, Dobias, Sung, & Mullarkey, 2020; Tang & DeRubeis, 1999). Moreover, greater immediate gains after completing an SSI predicts greater long-term improvement in mental health outcomes (Schleider, Abel, et al., 2019).

I speculate that one mechanism of sudden gains in SSIs is the insight experience (Tulver et al., 2023). An insight experience, or "Aha! Moment," is an event that produces a profound shift in one's thinking and behavior (Laukkonen et al., 2022). A meaningful insight experience in a mental health SSI could show a hopeful path forward and provide the motivation to pursue it.

Yet, it is important to distinguish genuinely useful insight moments from ones that are evocative but misleading. False insight experiences can motivate one to dedicate time and resources to unhelpful pursuits (Danek & Wiley, 2017; Laukkonen et al., 2022). Some work has suggested that self-reported instantaneous improvements in well-being may, at times, indicate a new set of convictions more than genuine improvement in well-being (J. D. Fisher et al., 1989).

Given that SSIs have only a short time to produce an impact in users' lives, improving the probability that one will have a meaningful insight experience is a promising design goal. Research can help to identify how to best design SSIs to spark impactful insight experiences. While awe-inspiring or psychedelic experiences often lead to insights that can have lasting impacts, both are intense and visceral experiences (especially compared to a text-based SSI) (Gail Jones et al., 2022; Timmermann et al., 2021). Insights do not necessarily have to come from emotionally-salient stimuli, but for an SSI to produce insight experiences reliably, it may

need to deliver a compelling experience.

Risks Associated with SSIs

Even though online SSIs are generally intended to provide a “targeted, light touch," they still carry risks (Schleider & Weisz, 2017a). Iatrogenic effects have resulted from in-person SSIs in the past, and online SSIs lack the risk-monitoring procedures that would be expected in a face-to-face intervention (Schleider & Weisz, 2017a). Users should be well-informed beforehand about what to expect from SSIs in terms of their content and effectiveness (Zhang & Chen, 2020). Also, providing access to trained human supporters (as in a stepped care model) or references to other mental health resources may help to reduce risk (Kozlov et al., 2021). Finally, supporting users’ agency in how they choose to use an SSI may help to avoid risks resulting from poor person-intervention fit (Kaveladze, Young, et al., 2022; Mold et al., 1991).

CHAPTER 2

Study 1. Evaluating a Single-Session Intervention for Loneliness

ABSTRACT

An evidence-based online SSI for loneliness could offer a substantial improvement over existing loneliness interventions in reach and appeal. I conducted an online experiment in a sample of people aged 16 and older who were struggling with loneliness (n = 908), comparing a 20-minute SSI for reducing loneliness based on cognitive-behavioral therapy (n = 294) to a 3-session 1-hour version of the same intervention (n = 307) and an active control SSI that was based on supportive therapy and had a similar user experience to the other two interventions (n = 307). Loneliness decreased significantly between baseline and week eight across all three conditions (b = -5.80, $p < 0.01$). Loneliness did not decrease between baseline and eight-week follow-up significantly more in those assigned to either the loneliness SSI (b = -1.21, p = 0.22) or the 3-session version of it (b = -1.17, p = 0.23) than in those assigned to the control SSI. Participants found all three interventions acceptable, with mean scores on the Program Feedback Scale (PFS) for each above 3.95. However, both loneliness interventions were higher than the control SSI on the PFS, Credibility / Expectancy Questionnaire, star rating, and rate of producing an insight experience (ps < 0.01). Finally, the 20-minute loneliness SSI (31.6%) had far lower attrition than the 1-hour 3-session loneliness intervention (85.1%) but had nearly identical effectiveness and acceptability, supporting the idea that shorter SSIs can be as useful as longer ones in some cases. Overall, results indicated that neither the three-week version nor the SSI version of the intervention was more effective for reducing loneliness than an active control SSI. Future work should examine sub-populations for which a loneliness SSI could be more effective and explore new strategies for intervening against loneliness at scale.

INTRODUCTION

Loneliness SSIs exist for various purposes and populations but, to my knowledge, no evidence-based online SSI exists for loneliness, nor has previous work on SSIs included loneliness as a measured outcome. Given loneliness's global prevalence and transdiagnostic significance, an effective and scalable loneliness SSI could be impactful.

My team and I adapted Käll et al.'s nine-week iCBT for loneliness into a 20-minute online self-guided text-based SSI and a 60-minute three-session version of the SSI (Käll, Jägholm, et al., 2020). My goal was to improve the appeal and reach of Käll's iCBT for loneliness without sacrificing too much of its effectiveness. I then conducted an online experiment to compare change in loneliness over eight weeks across the 20-minute loneliness SSI, the three-part version of it, and an active control SSI.

METHODS

Participants

I recruited a sample of English-speaking participants aged 16+ with access to the internet and a computer, tablet, or smartphone from several social media platforms and the University of California, Irvine Social Ecology student subject pool. To advertise the study on social media, I followed other studies in using paid and unpaid advertisements on several social media sites, (Sanchez et al., 2020; Shroff et al., 2021). Figure 3 shows a recruitment advertisement.

To ensure a sufficient number of participants who struggle with loneliness for analysis, if one met criteria for "struggling with loneliness" (see the "Screening Measure" section below), I invited them to participate in the study and offered compensation of a $10 Amazon electronic gift card that I emailed them once they completed the final follow-up eight weeks later. Those

who participated through the UC Irvine subject pool received one hour of participation credit for completing the first part of the study regardless of whether they were eligible for the $10 gift card. Participants who did not meet the criteria were permitted to complete the study without compensation or to access the loneliness interventions directly without participating in the study.

Ethics and Pre-registration

The procedures were approved by the University of California, Irvine human subjects institutional review board (Protocol #1253). The review board granted waivers of participant consent and parental consent. All participants reviewed an electronic information sheet and agreed to participate. Before launching the study, I pilot-tested intervention material with labmates, friends, and family aged 18-82 to ensure the intervention material was generally acceptable. The study was registered on clinicaltrials.gov (ID: NCT05687162) and pre-registered on the open science framework (https://osf.io/4hkda/).

Figure 3. A Study Recruitment Advertisement

Procedure

After participants agreed to participate in the study and completed baseline measures, they were randomly assigned to complete either the loneliness SSI, the three-week loneliness intervention, or a control SSI, with an equal chance of being assigned to each condition. They then completed post-test measures. Four and eight weeks later, they were invited via email to complete follow-up questionnaires. Table 1 shows the timepoints at which each measure was collected. After completing their intervention, participants were offered browser and downloadable versions of the intervention and a list of online mental health resources created by the Fox Lab (Fox Lab et al., 2020). After participants completed the 8-week follow-up, they were given access to both browser and downloadable versions of all three conditions. I used the online survey and experience management software Qualtrics to collect data, randomly allocate participants to conditions, automate sending follow-up emails, and deliver all SSI content.

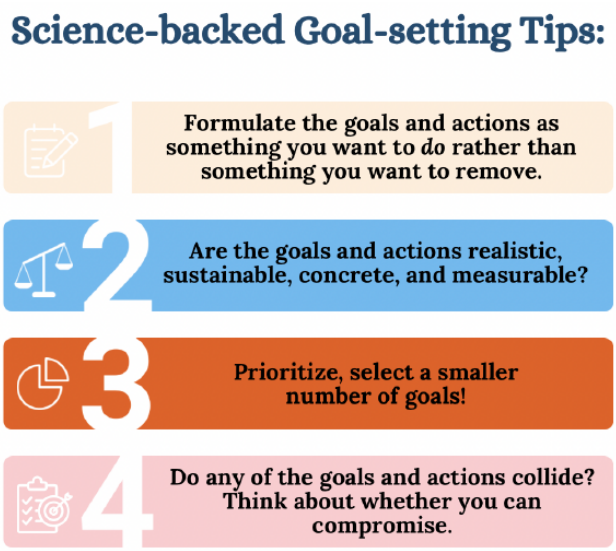
Interventions

My team and I created the 25-minute loneliness SSI and the three-week loneliness intervention by adapting Käll et al.'s nine-week therapist-supported iCBT for loneliness, which focuses on person-level psychological factors contributing to the maintenance of loneliness (more information at https://www.iterapi.se/sites/solus/) (Käll, Shafran, et al., 2020). The SSI was a combination of text and images, including several infographics that my teammates made, as shown in Figure 4. We conducted several rounds of user testing as we developed the SSI, including with digital mental health intervention experts and Anton Käll, who developed the iCBT for loneliness we adapted. Both the SSI and three-week intervention are text-based and self-guided. The three-week intervention includes three 20-minute self-guided sessions (participants complete the first session when they start the study and the next two sessions one and two weeks later).

Following Käll et al.'s iCBT, the loneliness interventions are theoretically rooted in behavioral activation, cognitive restructuring, and behavioral experimentation. They include content relating to understanding loneliness, setting goals and values, testing thoughts and behaviors, planning social behaviors, gaining social skills and confidence, and overcoming barriers to progress. Following Käll et al.'s suggestion, the interventions prioritize an active approach to behavior change, focusing on motivating active behavior change over more passive strategies like cognitive restructuring (Käll, 2021). The SSIs' design and content were also influenced by the Lab for Scalable Mental Health's SSIs, which implement behavioral activation, CBT, and growth mindset interventions with lightweight but appealing presentation (Schleider et al., 2022; Schleider, Mullarkey, Mumper, et al., 2019).

In the control condition, participants received a supportive therapy SSI called "The Sharing Feelings Project" of roughly equivalent style to the loneliness SSI (Schleider & Weisz, 2019). I slightly modified the SSI to make it more applicable to both teens and adults, as it was originally designed for teens only (link to original SSI: https://osf.tvio/u4axs/, license: http://creativecommons.org/licenses/by-nc-sa/4.0/) (Schleider & Weisz, 2019). The SSI "encourages participants to express emotions to close others," but "does not teach specific skills." (Schleider et al., 2022; Schleider & Weisz, 2018). Previous work found studies with waitlist controls produced stronger effect sizes (Hedge's $g = 0.41$) than those using active controls ($g = 0.14$) (Schleider & Weisz, 2017b). Studies using this control found it improved mental health outcomes in adolescents slightly less on average than a growth mindset SSI and a behavioral activation SSI (Schleider et al., 2022; Schleider & Weisz, 2018). Browser and PDF versions of all three SSIs are available at https://osf.io/qzgb6/.

Figure 4. An Infographic From the Overcoming Loneliness SSI



Measures

I measured variables before the intervention (baseline), just after the intervention (proximal), and four weeks and eight weeks later (long-term). Table 1 shows the timepoint(s) at which each variable was collected and Table 2 organizes these variables by dimension.

Screening Measure

To screen participants for loneliness, I used the validated three-item version of the UCLA loneliness scale (ULS-3) (Hughes et al., 2004). The measure includes questions such as "how often do you feel left out" with responses ranging from 1 (hardly ever) to 3 (often), producing a total score between 3 and 9. Researchers have labeled people who score 3-5 as "not lonely" and those who score 6-9 as "lonely" (Steptoe et al., 2013). Following Käll et al. (2021), I also asked directly if one's loneliness was causing one distress. If a participant's score on the ULS-3 was 6 or higher and they answered that their loneliness was causing them distress, they met criteria as "struggling with loneliness" (i.e., experiencing loneliness at a problematic level). The Cronbach's alpha for the ULS-3 in this study at baseline was $\alpha = 0.64$.

Primary Outcome Measures

Loneliness was the primary outcome for Study 1 and a secondary outcome for Study 2. The loneliness measure was the 20-item UCLA Loneliness scale, version 3 (ULS-20), which was found to be reliable and valid in adult, college student, and adolescent populations across cultures (Mahon et al., 1995; Russell, 1996). The scale asks indirectly about feelings of loneliness (e.g., "I feel isolated from others") on a scale from 1 (never) to four (often), with a minimum score of 20 and maximum of 80. Several studies have categorized scores from 20–34 as reflecting low loneliness, 35–49 as moderate loneliness, 50–64 as moderately high loneliness, and 65–80 as high loneliness (Deckx et al., 2014). Researchers continue to debate the dimensionality of the most popular loneliness measure, the UCLA Loneliness Scale, but most have concluded that it is unidimensional (Buecker et al., 2021; McDanal et al., 2021; Mund et al., 2022). The ULS-20 had $\alpha = 0.91$ at baseline.

Secondary Mental Health Outcomes

I measured distress using the 9-item version of the Depression Anxiety and Stress Scale (DASS-9), which is a widely-used scale that has been validated across many settings among adults and teens (Henry & Crawford, 2005; Kyriazos et al., 2018; Yusoff, 2013). The scale includes questions about one's experiences over the past week, such as "I found it hard to wind down," rated on a scale from 0 (did not apply to me at all) to 3 (applied to me very much, or most of the time), with total scores ranging from 0-27. The scale contains depression, anxiety, and stress subscales. The DASS total score had $\alpha = 0.80$ at baseline.

I evaluated symptoms of social anxiety using the three-item Mini Social Phobia Inventory (Mini-SPIN), which is valid and reliable for adults and teens (Connor et al., 2001; Garcia-Lopez

& Moore, 2015; Weeks et al., 2007). The measure includes items about one's experiences over the past week, such as "being embarrassed or looking stupid are among my worst fears" with responses ranging from 0 (not at all) to four (extremely), with total scores ranging from 0-12. The Mini-SPIN had $\alpha = 0.79$ at baseline.

I measured self-efficacy using the short form of the General Self-Efficacy Scale (GSE-6), which is a reliable and valid six-item self-report measure of self-efficacy (Romppel et al., 2013). Self-efficacy is one's perceived ability to deal with a task or achieve a goal (Bandura, 1982; Bandura et al., 1999). The scale includes items such as "If someone opposes me, I can find means and ways to get what I want," with responses ranging from "not at all true" (1) to "exactly true" (4), with a sum score between 6 and 24. The GSE-6 had $\alpha = 0.82$ at baseline.

I measured hopelessness using the four-item version of the Beck Hopelessness Scale, which has been found reliable among adults and youth (Perczel Forintos et al., 2013; Rhoades et al., 2018). The scale includes items like "my future seems dark to me," each rated from 1 (rarely typical) to 3 (very typical), with total scores from 4-12. The BHS had $\alpha = 0.84$ at baseline.

I measured positive thinking and behaviors using the frequency of actions and thoughts scale (FATS) (Terides et al., 2016), which is a validated measure of adaptive behaviors and cognitions related to therapeutic change. The scale includes 12 items about one's experiences over the past week. Items include, "[Did you] change your thinking to be more realistic and helpful?," and are rated from 0 (not at all) to 4 (every day), with total scores ranging from 0-48. The FATS had $\alpha = 0.84$ at baseline.

Acceptability and Engagement Measures

To measure SSI acceptability, I used the Program Feedback Scale (PFS), which is routinely used to evaluate SSIs (Schleider et al., 2022; Schleider, Mullarkey, & Weisz, 2019). The PFS includes seven items, such as “I agree with the program’s message," rated from 1 (really disagree) to 5 (totally agree) and total scores ranging from 7-35 (Schleider et al., 2022; Schleider, Mullarkey, & Weisz, 2019). In previous work, scores greater than or equal to 3.5/5 on a PFS item have been interpreted as reflecting an “acceptable” rating on that item (Schleider et al., 2022; Schleider, Mullarkey, & Weisz, 2019). The PFS had $\alpha = 0.81$.

To further measure the SSI’s appeal and perceived impact, I included the Credibility and Expectancy Questionnaire (CEQ), which is a validated measure that has been used with adults and adolescents (Devilly & Borkovec, 2000; Nock et al., 2007). The measure includes four items evaluating a treatment rationale’s credibility, with items such as “At this point, how logical does the treatment offered to you seem?” and 2 items evaluating a respondent’s expectation of how much a treatment will help them, with items such as “At this point, how much improvement in your symptoms do you really *feel* will occur?." Item responses range from 1 (not at all) to 9 (very much), with total scores ranging between 6 and 54. I adjusted the scale’s wording to refer to the “program” rather than “treatment.” The CEQ had $\alpha = 0.92$.

As another measure of intervention satisfaction, I used a quantitative “star rating," from 1 to 5 stars where more stars indicated greater positive feedback. I also asked participants for open-ended comments and feedback.

I evaluated if the SSI caused an insight experience with a single item asking, “Did you experience an Aha! Moment related to something you learned in the program?” (yes/no). Before the question, I provided the following explanation: “When an Aha! moment occurs, it is as if a

realization or solution to a problem suddenly pops into your mind, like a lightbulb turning on. You might experience surprise, you might feel relief, and you might feel a light sense of happiness and ease. You can think of this experience as a miniature 'Eureka moment'. You might even feel an internal sense of "Aha!," or you might think to yourself, "Of course!," "That was so obvious." Not experiencing an Aha! moment might feel like nothing much at all." This question and explanation came from previous work on insight experiences (Laukkonen et al., 2022).

Demographic Items

I measured several demographic variables: gender (multiple choice), age (continuous), ethnicity (multiple choice), education level (multiple choice), country (multiple choice), Zip code (among USA participants), ever received help from a mental health professional (yes/no), seeing a mental health professional regularly (at least once every 2 months). I measured socio-economic status using the MacArthur Scale of Subjective Social Status – Youth Version, which asks about the perceived social standing of one's family. This measure was found reliable in an adolescent sample (Goodman et al., 2001). All demographic items could be skipped.

Table 1. Full List of Measures and Assessment Timepoints

Measure (Primary, Secondary, Other)	Baseline	Post-Intervention	Week Four Follow-up	Week Eight Follow-up
UCLA Loneliness Scale (V3 20-item)	X		X	X
Depression Anxiety and Stress Scale – nine-item	X		X	X
Three-item Mini Social Phobia Inventory	X		X	X
Short form of the General Self-Efficacy Scale	X	X	X	X
Beck Hopelessness Scale – four-item	X	X	X	X
Frequency of actions and thoughts scale	X		X	X
Program Feedback Scale		X		
Credibility and Expectancy Questionnaire		X		
Insight experience		X	X	X
Star rating)		X		

MacArthur Scale of Subjective Social Status – Youth Version	X			
UCLA loneliness scale – 3 item (screening)	X			

Table 2. Measures Organized by Dimension

Dimension	Application to project	Study Measurement(s)
Effectiveness	Improvements in symptoms and functioning	**Primary**: ULS-20 (loneliness) **Secondary**: DASS-9 (depression, anxiety, and stress; used in this study as a proxy for psychological distress), Mini-SPIN (social anxiety), GSE-6 (general self-efficacy), Beck Hopelessness Scale, FATS (frequency of adaptive behaviors and thoughts related to therapeutic change)
Engagement	Intervention Satisfaction	**Intervention appeal:** PFS (satisfaction with intervention), CEQ (intervention credibility and expectations of impact), insight experience, number of links added to one's resource guide, frequency of using one's guide, insight experience, star rating (program quality)
Screening	Compensation and inclusion in analyses	ULS-3 (loneliness)

Power

To estimate the main analysis's power, I conducted an ANOVA: repeated-measures between-within interaction power analysis in G*Power 3.1, aiming to detect a Cohen's *d* of 0.18 with 0.80 power and a significance level of 0.05, assuming an r = 0.50 correlation across repeated measures (Faul et al., 2007). The total estimated sample size needed for all three conditions, in order to compare the change in loneliness between two timepoints across two conditions, was 369.

Due to the lack of data on SSIs' impacts on loneliness, I based the targeted effect size based on those found in similar studies of SSIs. One study compared a mood SSI to a supportive therapy control (the same one I adapted to use in this study) and found greater effects in the experimental condition on self-rated anxiety (Cohen's *d* = .28), depression (*d* = .32), and perceived behavioral control (*d* = .29) at 9-month follow-up in a sample of adolescents

experiencing depression or other internalizing issues (n = 96) (Schleider & Weisz, 2018). A larger study compared the ABC project and another SSI to the supportive therapy control in a sample of adolescents around the US with elevated depression symptoms recruited via social media (n = 2,452, ages 13–16); compared with the control, both active SSIs reduced three-month depressive symptoms (d = 0.18), decreased post-intervention and three-month hopelessness (ds = 0.16 – 0.28), and increased post-intervention agency (ds = 0.15–0.31) (Schleider et al., 2022). Loneliness interventions tend to have relatively modest effect sizes (Mann et al., 2017).

Hypotheses

My primary hypothesis was that participants randomized to the loneliness SSI would have greater reductions in self-reported loneliness between baseline and eight-week follow-up than those who were randomized to the control "sharing feelings" SSI. As secondary hypotheses, I predicted greater improvements in distress (DASS-9), social anxiety, self-efficacy, hopelessness, and adaptive behaviors and thoughts in those randomized to the loneliness SSI relative to the control SSI. I also predicted that participants randomized to the three-week loneliness intervention would report greater reductions in loneliness than those randomized to the loneliness SSI. I made this prediction based on the idea that more intervention content would be more helpful and that spreading sessions across time would make the material easier to digest.

Exploratory Hypotheses

I predicted that 1) having an insight experience is associated with greater improvement in loneliness between baseline and week eight 2) greater self-efficacy at baseline is associated with change in loneliness between baseline and week eight, and 3) greater engagement with the SSI platform predicts greater improvement in loneliness between baseline and week eight.

Analysis Plan

Inclusion criteria

As pre-registered, I limited the analyses to only those participants who 1) met criteria for struggling with loneliness, 2) were randomized to a condition (i.e., those who consented to participate and completed the baseline measures), 3) did not indicate that they did not participate seriously at the end of the study, and 4) spent at least three minutes completing the study introduction and baseline measures (i.e., were not clearly using bots or rushing through without paying attention). Also, if the same email address was entered for multiple responses, I kept the response that progressed furthest in the survey; if multiple responses progressed the same amount, I kept the earliest of those responses. I invited participants to the follow-up surveys four and eight weeks after beginning the study but I allowed them to participate in the follow-up surveys at any point before I closed the survey on June 17, 2023, giving every participant a chance to complete the eight-week follow-up.

Two months after beginning data collection, I noticed a number of suspicious responses that seemed to have come from bots or individuals who took the survey multiple times with different emails (i.e., fraudulent responses). I labeled responses as likely fraudulent or real based on an array of suspicious factors suggesting that the same person may have taken the survey multiple times or that some form of automated system was used to complete the survey. These decisions were informed by previous research on dealing with bots in online surveys (Cascalheira, 2023; Peng & Salehi, 2021; Storozuk et al., 2020).

The factors I used to characterize responses are as follows: using Windows NT 6, using Android 9 or below, multiple IP Addresses having the same first eight digits (particularly when

submitted around the same time), reporting that one found the study on Facebook, Instagram, or Twitter after Dec 10, 2022, or Reddit after Jan 13, 2023 (weeks after I stopped advertising on those spaces), an email address containing six numbers in a row and being from Gmail, responses to the two open response questions in the Program Feedback Scale being identical, reporting one found the study on ResearchMatch but their email not being on the list of emails I contacted through ResearchMatch, many participants completing each study session within a short amount of time using similar devices, and spending less than eight minutes on the initial study if one finished the survey. I also emailed suspected bots/scammers and asked them to indicate where they found the study and what they learned, and I labeled responses to those emails as suspicious (but not necessarily fraudulent) if one's email address did not match one's stated name, if participants who took the survey within a few minutes of one another also replied to the emails within a few minutes of one another, or if response emails were highly similar or contained linguistic errors.

Missing data handling

Although I took efforts to avoid missing data (e.g., a financial incentive for completing the study and reminder emails), it is not necessary to impute missing data before performing a longitudinal mixed-model analysis because such models use the available data to estimate the fixed and random effects accounting for the hierarchical structure of the data (Twisk et al., 2013).

To test for faulty randomization I used chi-squared tests with Yates's continuity correction in combination with pairwise fisher tests using a Holm-Bonferroni adjustment for multiple comparisons. If any effects were found, I planned to adjust for them using the

confounding variable as a covariate in the main model. I also planned to use these tests, in addition to a logistic regression model, to check for differential dropout.

Main Analysis

In the main analysis, I tested between-group differences across conditions in change in loneliness from baseline to eight-week follow-up. I used a mixed-effects model predicting loneliness with experimental condition, measurement timepoint (1-3, also called "time"), and the 2-way interaction between condition and time as predictors and a participant identifier as a random intercept. Using the "lme4" package in R, these analyses took this form:

```
lmer(loneliness ~ time*condition + (1|study_id))
```

Secondary analysis

I re-ran the main analysis separately for each secondary outcome collected, although the timepoints of the "time" variable differed across outcomes, as described in Table 1. For all analyses I used the standard $p < 0.05$ criterion for determining statistical significance.

Additional analyses testing exploratory hypotheses

Pre-registered analyses for exploratory hypotheses are listed below.

1. To compare change in loneliness across the presence or lack of an insight experience, I conducted a mixed-effects model with loneliness as the DV, timepoint, presence of an insight experience, and the interaction between the two as predictors and a participant identifier as a random intercept.

2. To test if baseline self-efficacy was associated with change in loneliness between baseline and week eight, I ran a mixed-effects model with loneliness as the DV, timepoint, baseline self-efficacy (as measured by the GSE-6), and the interaction between the two as predictors, and a participant identifier as a random intercept.

3. To test if engagement with the intervention predicted change in loneliness, I ran a mixed-effects model with loneliness as the DV, timepoint, engagement (measured as the number of characters entered across all text entry prompts within interventions), and the interaction between the two as predictors, and participant identifier nested within experimental condition as a random intercept.

Sensitivity analyses

I pre-registered two sensitivity analyses. The first evaluated change in loneliness among the subset of participants who did not meet criteria for struggling with loneliness and thereby participated in the study without being compensated. The second evaluated change in loneliness when including responses marked as fraudulent, which were excluded from the main analysis. Due to a high degree of differential dropout across conditions, I also ran two sensitivity analyses that I did not pre-register using different approaches to handling missing data in the main analysis. One analysis removed all participants who did not complete the eight-week follow-up (i.e., list-wise deletion), and the other removed participants who did not complete their assigned intervention (or the first of three sessions for those assigned to the three-week intervention).

Additional analyses

I pre-registered the following analyses.

1. I duplicated the main analysis comparing the change in loneliness from baseline to week four follow-up rather than to week eight.
2. I duplicated the main analysis including only the subset of participants who completed the week eight follow-up, excluding those who did not complete it.
3. I duplicated the main analysis but with the following covariates included in the mixed-effects model: gender (multiple choice), age (continuous), ethnicity (categorical), education level (categorical), socioeconomic status (continuous), and if participants completed the study on a computer or phone/tablet (binary).
4. I conducted a one-way ANOVA to test between-group differences in loneliness at eight-week follow-up. If the omnibus ANOVA produced a significant F-score, I conducted two planned contrasts to compare the experimental condition to each other condition and computed Cohen's ds for these comparisons.
5. I calculated Cohen's d effect sizes reflecting within-group intervention effects on loneliness for each condition from baseline to four-week and eight-week follow-ups.
6. I ran four logistic regression models with dropout as the outcome and all baseline measures as predictors to predict which participants would drop out before completing the intervention, before completing the post-intervention questions, or before completing the four- and eight-week follow-ups.

Finally, I conducted seven unplanned additional analyses.

1. I used a Tukey-Kramer test to contrast the mean change in loneliness between baseline and eight-week follow-up in the control SSI with the mean change in the same timeframe averaged across the two active conditions.

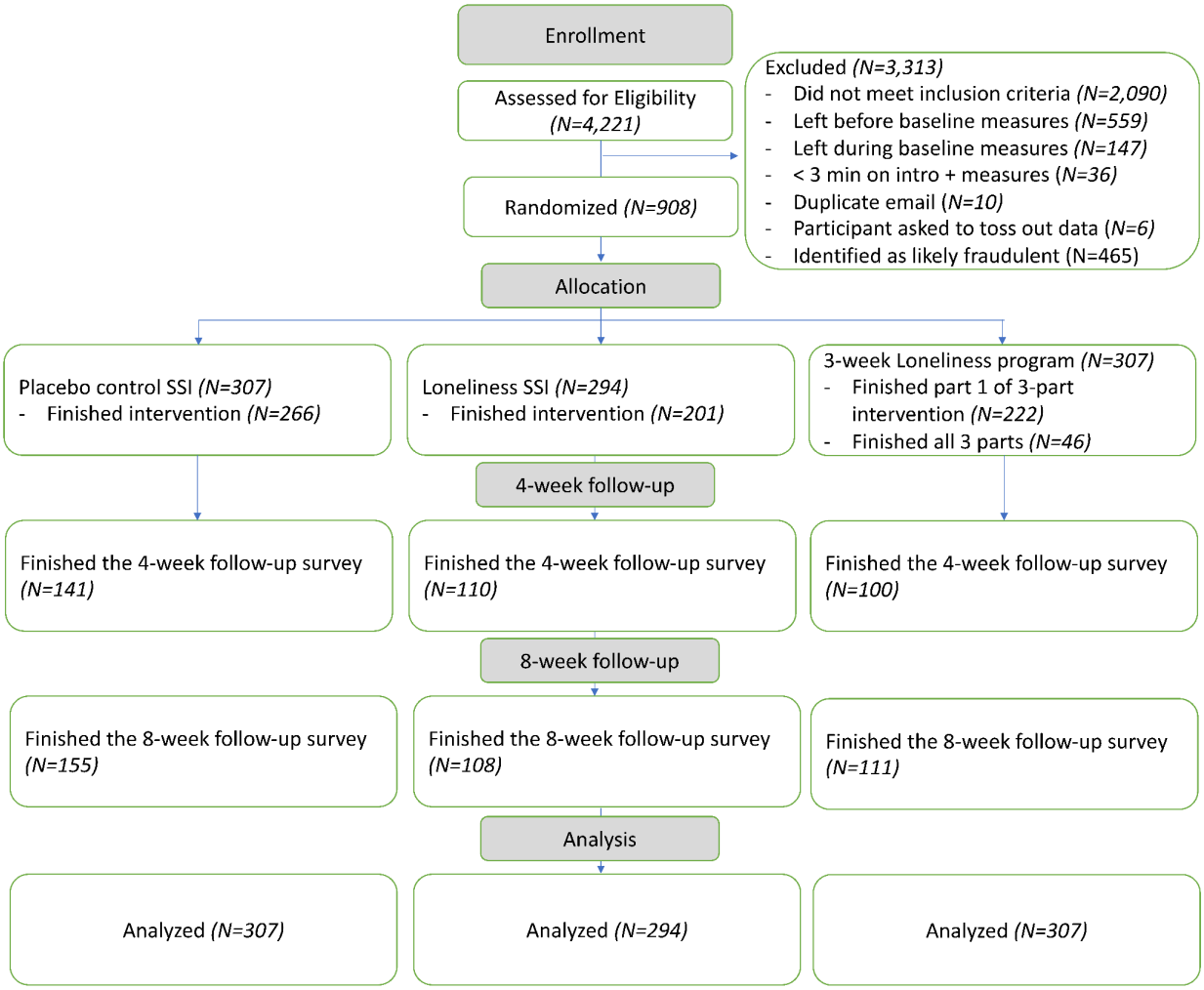
2. To see if having an insight experience was uniquely predictive of change in loneliness, I tested if the CEQ and PFS predicted change in loneliness between baseline and week eight in separate mixed-effects models. I also ran a mixed-effects model with CEQ, PFS, and insight experience as predictors of change in loneliness.
3. To see if insight experiences predicted immediate change in hopelessness and general self-efficacy, I ran two mixed-effects regressions with insight experience predicting change in hopelessness and self-efficacy.
4. To compare effect sizes with previous work, most of which used fixed-effects models, I calculated Cohen's *d* effect sizes of differences across conditions in the change in loneliness from baseline to week eight without accounting for random effects. To calculate the mean and standard deviation of loneliness by condition and timepoint, I imputed participant-level missing loneliness data by creating as many imputed datasets as the percentage of missing data in the outcome, computed the mean and standard deviation for each, and then averaged them across datasets (Honaker et al., 2011).
5. I compared change in loneliness between baseline and week eight across recruitment sources (where participants heard about the study) using a mixed-effects regression.
6. I tested the association between age and loneliness at baseline using a linear regression.
7. I tested how age moderated change in loneliness from baseline to week four using a mixed-effects regression model.

RESULTS

I collected data from November 25, 2022, to June 15, 2023. Figure 5 shows a CONSORT Diagram of the number of participants at each part of the study. I report descriptive statistics at

baseline among the 908 participants who met the criteria for inclusion in analyses. Demographics are summarized in Table 3. Baseline distributions of outcome variables are shown in Figure 6.

Figure 5. CONSORT Diagram



CONSORT diagram showing participant flow through screening, inclusion criteria, randomization, interventions, and follow-up assessments.

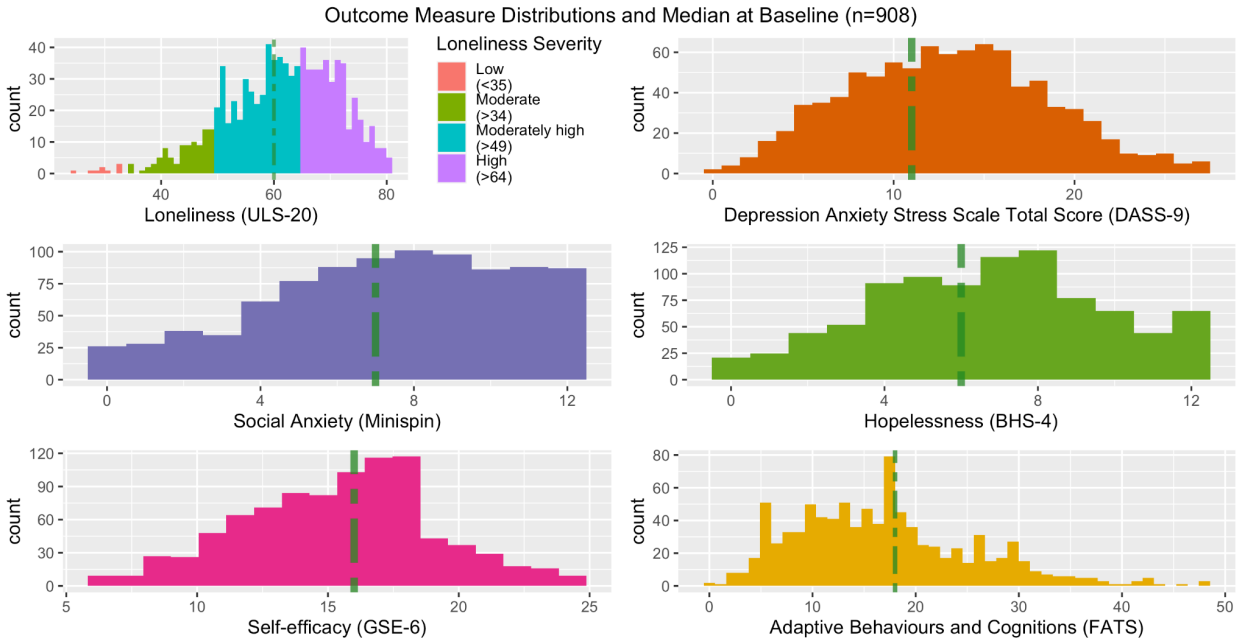
Table 3. Descriptive Statistics, All Measured at Baseline

Characteristic	***n* = 908** *Mean (SD) or n (%)*
Age	29.38 (12.74)
Gender	
Female	432 (48%)

Male	416 (46%)
Non-binary / third gender	27 (3.0%)
Prefer to self-describe	20 (2.2%)
Prefer not to say	10 (1.1%)
Ethnicity	
Caucasian	503 (56%)
Latino/Hispanic	92 (10%)
East Asian	74 (8.2%)
Mixed	62 (6.9%)
African	50 (5.6%)
South Asian	31 (3.4%)
Middle Eastern	18 (2.0%)
Caribbean	17 (1.9%)
American Indian or Alaska Native	6 (0.7%)
Native Hawaiian or Pacific Islander	2 (0.2%)
Other	45 (5.0%)
Country	
United States of America	738 (82%)
Canada	43 (4.8%)
United Kingdom of Great Britain and Northern Ireland	40 (4.5%)
Jamaica	17 (1.9%)
Australia	16 (1.8%)
Other	44 (4.9%)
Employment	
Working full-time	294 (33%)
Student	257 (28%)
Working part-time	157 (17%)
Unemployed and looking for work	83 (9.2%)
Retired	35 (3.9%)
A homemaker or stay-at-home parent	11 (1.2%)
Other	65 (7.2%)
Social Ladder (0-10)	4.58 (2.09)
Receiving Mental Healthcare	272 (30%)
Want Additional Mental Healthcare	586 (65%)
Used Smartphone or Tablet	444 (49%)

Some variation in sample size across demographic variables due to missingness (<2%).

Figure 6. Frequency histograms of outcome variables



Preliminary Analyses

Randomization

The proportion of participants assigned to each condition did not significantly differ (p = 0.83). Similarly, baseline measures did not significantly differ in mean or frequency across conditions (ps > 0.20), indicating good randomization.

Differential Dropout by Condition

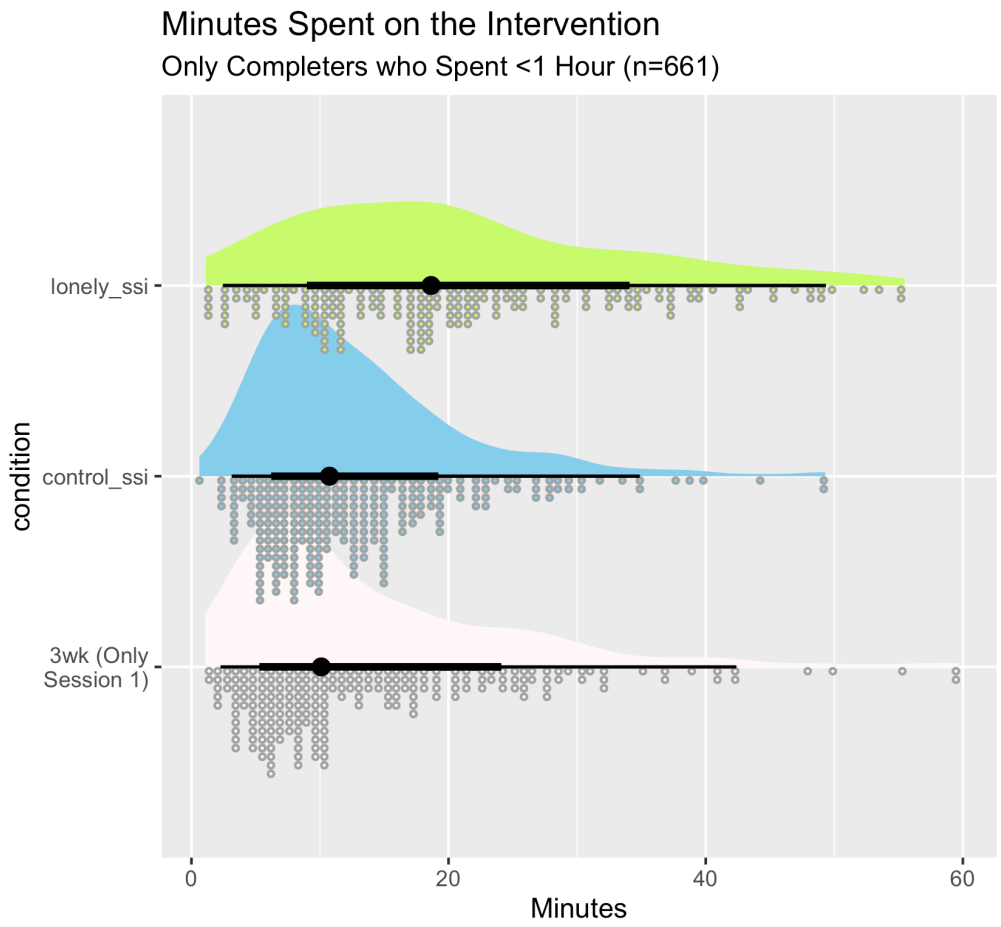
The proportion of participants who completed the intervention differed substantially across conditions – the control SSI had 86.6% completion, the loneliness SSI had 68.4%, and the three-week loneliness intervention (all three sessions) had 14.9% [$X2$(df = 2, n = 908) = 94.86, p < 0.01; all pairwise ps$_{adj}$ < 0.01]. The proportion of completers also differed across conditions when considering only the first session of the three-week intervention (72.3% completion) [$X2$(df = 2, n = 908) = 30.64, p < 0.001; all pairwise ps$_{adj}$ < 0.01]. Conditions differed in the

proportions of participants who completed the eight-week follow-up – loneliness SSI 35.6% completion, three-week intervention 35.8%, control SSI 49.8% [$X2$(df = 2, n = 908) = 16.58, p < 0.01; pairwise $ps_{adj} < 0.01$], except between the three-week intervention and loneliness SSI ps_{adj} = 0.93]. However, among participants who completed their intervention (or the first part of the three-week intervention), the proportion who completed the eight-week follow-up survey did not significantly differ across conditions (p = 0.16).

Dropout by Duration

One potential reason for the large difference observed in completion rates across conditions is the variance in intervention durations, shown in Figure 7 (Goldberg et al., 2021). Among completers, the median number of minutes spent on the intervention was 11.0 (IQR 7.4–16.9) in the control SSI, 19.2 (IQR 11.3–33.3) in the loneliness SSI, 10.2 (IQR 6.3–20.3) in the first session of the three-week loneliness intervention, and 67.2 (IQR 36.6–123.0) across all three sessions of the three-week loneliness intervention. Controlling for experimental condition, the more time that participants spent on an intervention, the less likely they were to complete it (p < 0.01, Nagelkerke pseudo-R^2= 0.07).

Figure 7. Duration of Completed Interventions



Density plots of SSI duration across conditions. The black dot shows the median and the black lines show where 66% and 95% of responses fell, respectively. Lonely_ssi = The Overcoming Loneliness SSI; control_ssi = the Sharing Feelings Project, an active control SSI about expressing feelings that does not teach specific skills; 3wk = the 3-session version of the Overcoming Loneliness SSI.

Predicting Dropout Using Baseline Characteristics

Compared to the participants who dropped out of the study, those who completed the eight-week follow-up survey did not significantly differ in baseline loneliness, self-efficacy, adaptive behaviors and cognitions, rate of receiving professional mental healthcare, or rate of wanting additional mental health support ($ps > 0.08$). However, on average, completers had slightly lower baseline distress [$t(803) = 2.61$; $p = 0.01$; $d = 0.18$; 95% CI, 0.04, 0.31] and slightly higher baseline hopelessness [$t(833) = 2.47$, $p = 0.01$, $d = 0.16$; 95% CI, 0.03, 0.30]. Completers also differed in several demographic characteristics: age [$t(695) = 4.88$, $p < 0.01$, $d =$

0.34; 95% CI, 0.21, 0.48], gender [$X2$(df = 4, n = 905) = 23.25, $p < 0.01$], ethnicity [$X2$(df = 10, n = 900) = 24.45, $p = 0.01$], where one found the study [$X2$(df = 12, n = 897) = 90.99, $p < 0.01$], self-rated socio-economic status [t(902) = -2.88, $p < 0.01$; d = -0.19; 95% CI, -0.33, -0.06], and employment status [$X2$(df = 6, n = 902) = 13.39, $p = 0.04$].

A multi-variable logistic regression including all baseline measures as predictors of whether one would complete the initial survey achieved a Nagelkerke pseudo-R^2 of 0.23 ($p < 0.01$). A model with the same predictors predicting dropout before completing week four also achieved a pseudo-R^2 of 0.23 ($p < 0.01$), and a model predicting dropout before completing week eight achieved a pseudo-R^2 of 0.18 ($p < 0.01$). No mental health measure was significantly predictive of dropout in any of these logistic regression models. Moreover, those who completed the eight-week follow-up did not perceive the intervention to be significantly more acceptable or impactful than those who dropped out did, as measured by the PFS ($p = 0.91$), CEQ ($p = 0.77$), star rating ($p = 0.84$), or frequency of insight experience ($p = 0.15$).

Pre-registered Main Analysis: Change in Loneliness Across Conditions

Loneliness was the pre-registered primary outcome. Loneliness decreased significantly between baseline and week eight across all three conditions (b = -5.80, $p < 0.01$). Contrary to my hypothesis, experimental condition did not significantly moderate how much participants' loneliness changed from baseline to week eight. Neither participants assigned to the loneliness SSI (b = -1.27, $p = 0.20$) nor participants assigned to the three-week loneliness intervention (b = -0.93, $p = 0.34$) reported significantly greater decreases in loneliness than participants assigned to the control SSI condition between baseline and eight-week follow-up. Participants assigned to the loneliness SSI ($b = 0.01$, $p = 0.99$) and the three-week loneliness intervention ($b = 0.64$, p =

0.53) also did not report significantly greater decreases in loneliness between baseline and four-week follow-up compared to those assigned to the control SSI. See Table 6 for the mixed-effects model output.

Loneliness changed significantly between baseline and the eight-week follow-up in the control SSI (d = 0.51; 95% CI, 0.32, 0.71; $p < 0.01$), loneliness SSI (d = 0.68; 95% CI, 0.46, 0.91; $p < 0.01$), and three-week loneliness intervention (d = 0.71; 95% CI, 0.48, 0.93; $p < 0.01$). Yet, the conditions did not significantly differ in mean loneliness at four-week follow-up (p = 0.35) or eight-week follow-up (p = 0.10).

Comparing change from baseline to week eight across conditions without considering random effects, the loneliness SSI reduced loneliness more than the control with Cohen's d = 0.08, the three-week loneliness intervention reduced loneliness more than the control with Cohen's d = 0.07, and the loneliness SSI reduced loneliness more than the three-week loneliness intervention with Cohen's d = 0.01. See Figure 8 for a visualization of change across conditions.

Table 4. Mixed-Effects Model Predicting Loneliness

	Loneliness Mixed-Effects Model			
Predictors	*Estimates*	*CI*	*p*	*df*
(Intercept)	62.11	60.93 – 63.30	**<0.001**	1119.89
condition [3wk]	-1.36	-3.04 – 0.32	0.114	1120.14
condition [lonely_ssi]	-1.04	-2.73 – 0.66	0.232	1120.09
time [Week 4]	-4.70	-6.00 – -3.40	**<0.001**	842.93

time [Week 8]	-5.18	-6.43 – -3.94	**<0.001**	841.59
condition [3wk] × time [Week 4]	0.64	-1.36 – 2.63	0.532	866.03
condition [lonely_ssi] × time [Week 4]	0.01	-1.94 – 1.97	0.988	859.14
condition [3wk] × time [Week 8]	-0.93	-2.85 – 0.99	0.340	866.64
condition [lonely_ssi] × time [Week 8]	-1.27	-3.19 – 0.66	0.196	859.16
Random Effects				
σ^2	34.49			
$\tau_{00\ email\_id}$	77.57			
ICC	0.69			
N_{email_id}	908			
Observations	1637			
Marginal R^2 / Conditional R^2	0.060 / 0.711			

Linear mixed model testing how change in loneliness across time differs across conditions. The dependent variable, loneliness is scored from 20-80. There are three experimental conditions (control, loneliness SSI, and three-week loneliness intervention) and three timepoints (baseline, four-week follow-up, and eight-week follow-up).

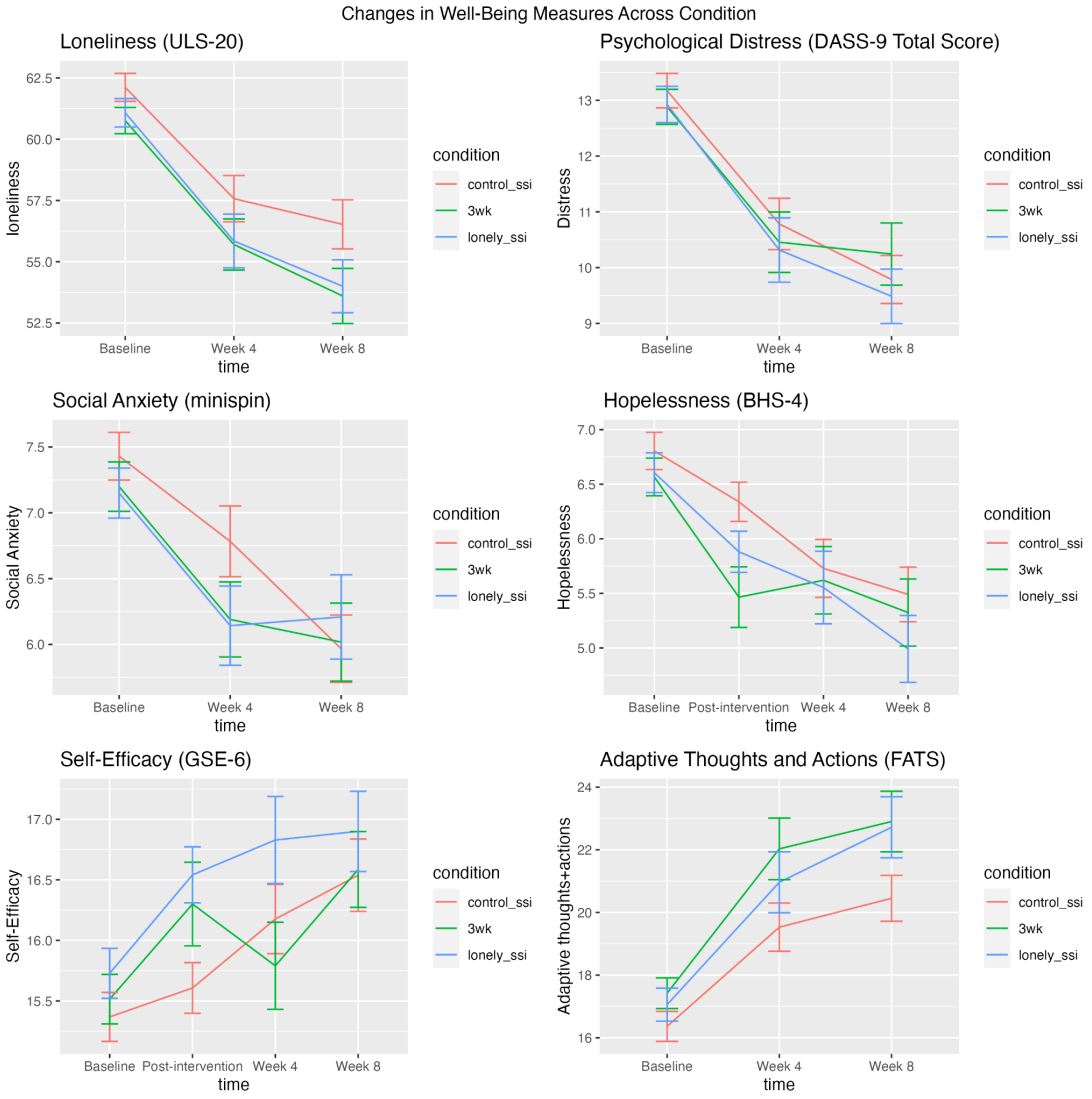
Secondary Analyses

Change Across Conditions in Secondary Well-Being Outcomes (Pre-registered)

Contrary to my hypotheses, neither the loneliness SSI nor the three-week loneliness SSI conditions significantly differed from the control condition in change between baseline and eight-week follow-up in distress (*p*s > 0.17), social anxiety (*p*s > 0.47), hopelessness (*p*s > 0.30), self-efficacy (*p*s > 0.33), or adaptive thoughts and actions (*p*s > 0.32). This was also true for the

post-intervention time-point and four-week follow-up (*p*s > 0.05), except for those assigned to the three-week loneliness intervention, for whom hopelessness declined between baseline and post-intervention more than it did for those in the control (b = -0.64, p = 0.01). Differences by timepoint and condition are shown in Figure 8.

Figure 8. Change in Well-Being Measures



Error bars show +/- one standard error. Baseline n = 908, post-intervention n = 649, week four n = 353, week eight n = 376.

Including Covariates in the Model (Pre-registered)

When gender, age, ethnicity, education level, socioeconomic status, and device type were included as covariates in the main model, compared to those assigned to the control condition, the average decrease in loneliness between baseline and eight-week follow-up was not significantly greater among those assigned to either the loneliness SSI [b = -1.42; 95% CI -3.34, 0.51, p = 0.15] or the three-week intervention [b = -1.04; 95% CI -2.95, 0.87, p = 0.29]. For this analysis, the sample size shrank from n=908 to n= 893 due to missingness in covariate data.

Contrasting the Control SSI Against the Average of the Other Interventions (Not Pre-registered)

Assignment to the loneliness SSI or the three-week loneliness intervention resulted in a 1.1 point greater reduction in loneliness between baseline and week eight on average than assignment to the control SSI, but this difference was not statistically significant [t(857) = -1.33; 95% CI -2.72, 0.52; p = 0.18].

Additional Analyses

Insight Experience and Change in Loneliness (Pre-registered)

Participants who had an insight experience during the intervention they completed had greater reductions in loneliness between baseline and week eight than those who did not have an insight experience [b = -2.76; 95% CI -4.57, -0.96; p < 0.01], but not week four (p = 0.54). They also had greater gains in self-efficacy [b = 0.73; 95% CI 0.28, 1.18; p < 0.01] and reductions in hopelessness [b = -0.41; 95% CI -0.91, -0.12; p = 0.01] at post-intervention, but not at the four-week or eight-week follow-up (ps > 0.09).

Intervention Satisfaction and Change in Loneliness (Not Pre-registered)

Both the PFS mean score [b = -2.38; 95% CI -3.88, -0.89; p = 0.01] and CEQ total score [b = -0.18; 95% CI -0.26, -0.10; p < 0.01] predicted reduction in loneliness between baseline and week eight, suggesting insight experiences are not uniquely predictive of change in loneliness. In a model with PFS mean score, CEQ total score, and insight experience all predicting change in loneliness, only the CEQ total score significantly predicted change in loneliness and only between baseline and week eight [b = -0.18; 95% CI -0.26, -0.10; p < 0.01], all other ps > 0.08.

Baseline Self-Efficacy and Change in Loneliness (Pre-registered)

Higher baseline self-efficacy predicted greater reductions in loneliness between baseline and week eight [b = 0.24; 95% CI 0.01, 0.47; p = 0.04] but not week four (p = 0.10).

Engagement With Intervention and Change in Loneliness (Pre-registered)

Neither the length of responses to written-response intervention exercises (p = 0.65) nor the amount of time one spent on the intervention to which they were assigned (p = 0.61) significantly predicted change in loneliness between baseline and week eight.

Comparing Change in Loneliness Across Recruitment Sources (Not Pre-registered)

Participants came from several recruitment sources, as shown in Figure 9. Sources varied in baseline loneliness and change in loneliness over time, as visualized in Figure 10.

Age and Change in Loneliness

Age did not predict loneliness at baseline (b = 0.04, p = 0.10) and did not predict change in loneliness from baseline to week eight (b = 0.04, p = 0.21).

Figure 9. Participant Recruitment Locations

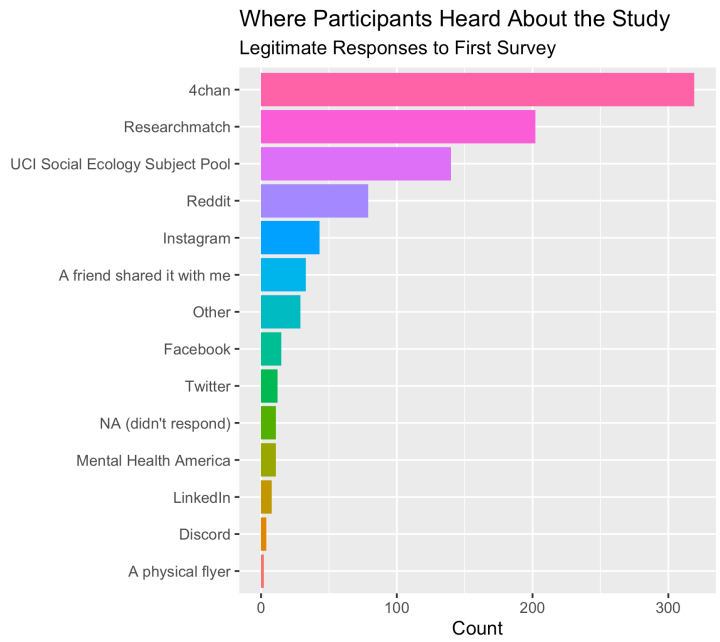
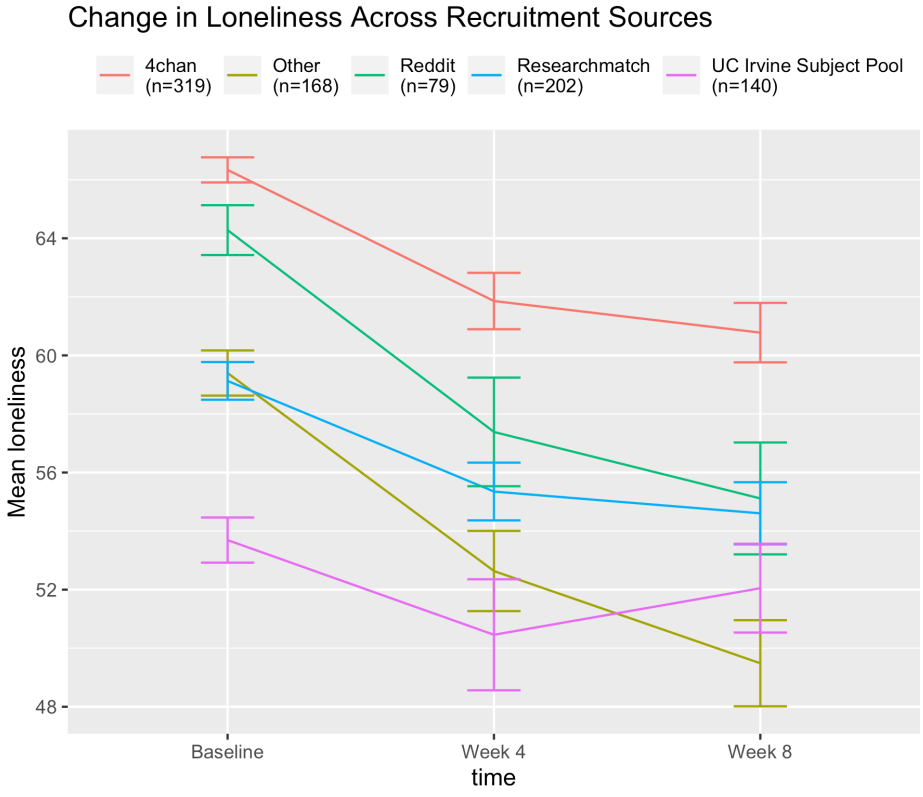


Figure 10. Change in Loneliness Across Recruitment Sources



Average change in loneliness differed across various places that participants heard about the study. The n displayed is the number of participants who completed the baseline measures. Recruitment sources with fewer than 30 observations were binned into the "other" category, these were "A friend shared it with me," "A physical flyer," Discord, Facebook, Instagram, LinkedIn, Mental Health America, and Twitter.

Intervention Appeal and Engagement

According to the PFS, participants found all three interventions acceptable (item-level means were above 3.50/5 for all but one item, see Table 4 for item-level scores). However, the interventions differed in their mean PFS scores [$F_{2,550} = 6.25$, $p < 0.01$], with the control SSI ($\bar{X}$= 3.96) rated lower than both the loneliness SSI ($\bar{X}$= 4.12; $p < 0.01$; d = -0.28; 95% CI, -0.47, -0.09) and the three-week loneliness intervention ($\bar{X}$= 4.16; $p = 0.01$; d = -0.34; 95% CI, -0.57, -0.11), compared using Tukey's Honest Significant Difference.

On the CEQ total score, participants rated both the loneliness SSI ($\bar{X}$= 31.5; $p < 0.01$; d = -0.39; 95% CI, -0.57, -0.20) and three-week loneliness intervention ($\bar{X}$= 32.9; $p < 0.01$; d = -0.51; 95% CI, -0.74, -0.27) higher than the control SSI ($\bar{X}$= 27.1) [$F_{2,550} = 13.39$, $p < 0.01$] – See Table 5 for item-level CEQ scores and Figure 11 for the distribution of CEQ total scores.

The interventions' average star ratings differed [$F_{2,552} = 7.10$, $p < 0.01$], with the control condition ($\bar{X}$= 3.86) lower than both the loneliness SSI ($\bar{X}$= 4.18; $p = 0.01$; d = -0.33; 95% CI, -0.52, -0.15) and the three-week loneliness intervention ($\bar{X}$= 4.14; $p = 0.04$; d = -0.28; 95% CI, -0.51, -0.05).

Participants rated the control condition ($\bar{X}$= 2.44) less relevant to loneliness than the loneliness SSI condition ($\bar{X}$= 2.88; $p < 0.01$; d = -0.42; 95% CI, -0.61, -0.23) and the three-week condition ($\bar{X}$= 2.79; $p = 0.01$; d = -0.33; 95% CI, -0.56, -0.09) [$F_{2,550} = 11.33$, $p < 0.01$], although each SSIs' mean fell between "a moderate amount" and "a lot" in the range of responses.

The conditions differed substantially in the proportion of participants who reported an insight experience during the intervention [$X2$(df = 2, n = 555) = 25.31, $p < 0.01$]. 27.9% of respondents reported an insight experience in the control condition, which was significantly less

than the 51.0% in the loneliness SSI (pairwise $p_{adj} < 0.01$) but not significantly less than the 39.6% in the three-week intervention (pairwise $p_{adj} = 0.08$). The loneliness SSI and three-week intervention also did not significantly differ in the proportion of participants who had an insight experience (pairwise $p_{adj} = 0.08$).

Table 5. PFS Item-Level Means and Standard Deviations by Intervention Completed

Program Feedback Scale Items by Condition			
	control_ssi, N = 258[1]	**3wk**, N = 100[1]	**lonely_ssi**, N = 195[1]
Enjoyed	3.53 (0.89)	3.97 (0.80)	3.92 (0.86)
Understood	4.37 (0.67)	4.50 (0.58)	4.44 (0.65)
Easy to Use	4.43 (0.59)	4.34 (0.71)	4.49 (0.60)
Tried My Hardest	4.13 (0.77)	3.92 (0.87)	3.83 (0.94)
Helpful to Others	3.63 (1.04)	4.05 (0.94)	4.04 (0.91)
Would Recommend to a Friend	3.43 (1.08)	3.92 (1.10)	3.78 (1.11)
Agree with Message	4.19 (0.81)	4.39 (0.62)	4.36 (0.85)
PFS Total Score	3.96 (0.59)	4.16 (0.55)	4.12 (0.58)
[1] Mean (SD)			

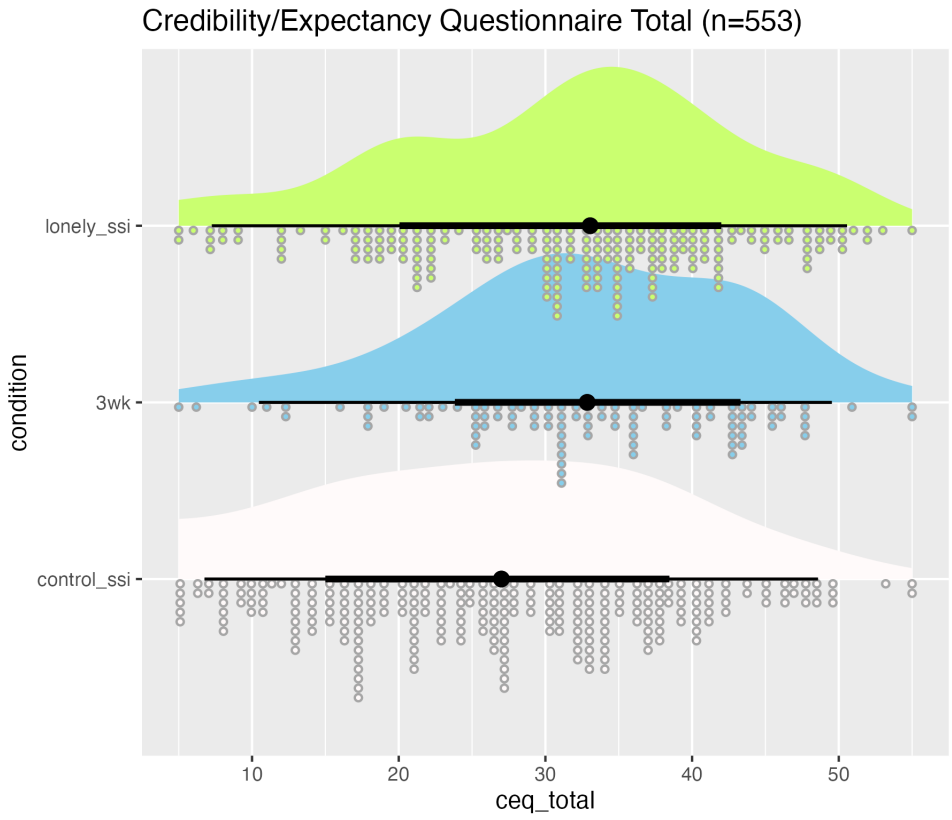
All items rated on a 1-5 scale.

Table 6. CEQ Item-Level Means and Standard Deviations by Intervention Completed

Credibility / Expectancy Questionnaire Items by Condition			
	control_ssi, N = 257[1]	**3wk**, N = 100[1]	**lonely_ssi**, N = 196[1]
Program Seems Logical	6.73 (1.97)	7.11 (1.64)	7.16 (1.72)
Program will Reduce Loneliness	4.34 (2.16)	5.36 (1.94)	5.20 (2.10)
Recommend to a Friend	4.96 (2.50)	5.99 (2.38)	6.02 (2.42)
Improvement Expected Over Next Few Weeks	4.07 (2.34)	5.09 (2.13)	4.77 (2.24)
How Much Improvement *Really* Expected	4.00 (2.25)	4.98 (2.10)	4.66 (2.32)
Percentage Improvement *Really* Expected	29.84 (25.31)	43.32 (26.64)	37.07 (26.12)
CEQ Total Score	27.08 (11.60)	32.86 (10.53)	31.52 (11.25)
[1] Mean (SD)			

All items rated on a 1-10 scale except for percentage.

Figure 11. Distribution of Responses to the Credibility / Expectancy Questionnaire



Sensitivity Analyses

Participants Who Did Not Meet Criteria for Struggling with Loneliness Only (Pre-registered)

Among only participants who did not meet criteria for struggling with loneliness and thereby participated in the study without being compensated (n = 372 at baseline, n = 111 at week four, and n = 118 at week eight), loneliness, averaged across conditions, did not change significantly between baseline and eight-week follow-up [b = -1.15; 95% CI -2.37, 0.08; p = 0.07], but it did change significantly between baseline and four-week follow-up [b = -1.88; 95% CI -3.14, -0.63; $p < 0.01$]. In this subsample, compared to those assigned to the control condition, the decrease in loneliness between baseline and eight-week follow-up was not significantly

greater among those assigned to the loneliness SSI [b = 0.07; 95% CI -2.68 – 2.81; p = 0.96] or the three-week intervention [b = 3.05; 95% CI -0.28, 6.38; p = 0.07].

Including Responses Marked as Fraudulent (Pre-registered)

The sample of participants including those marked as fraudulent (in addition to those marked as real) had n = 1,368 at baseline, n = 737 at week four, and n = 670 at week eight. In this sample, compared to those assigned to the control condition, the decrease in loneliness between baseline and eight-week follow-up did not significantly differ from those assigned to the loneliness SSI (b = -1.76, p = 0.05) or the three-week intervention (b = -1.01, p = 0.26).

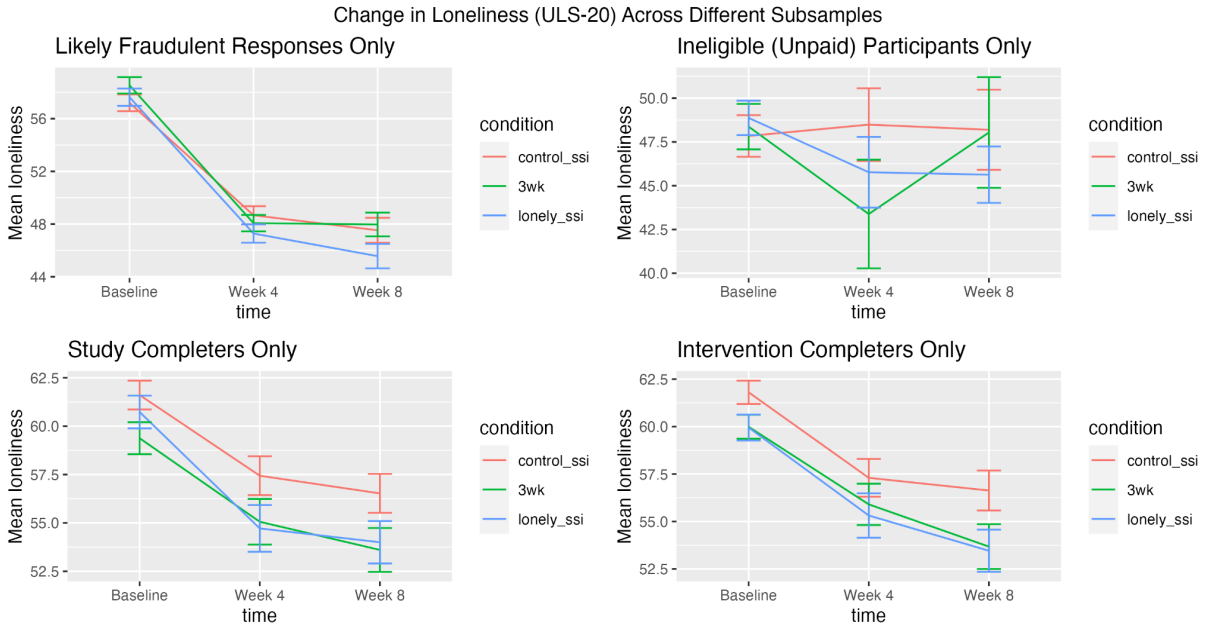
Eight-Week Follow-Up Completers Only (Not Pre-registered)

The sample of only participants who completed the eight-week follow-up included n = 376 at baseline and week eight and n = 292 at week four. Like the main analysis, neither the loneliness SSI (b = -1.65, p = 0.12) nor the three-week loneliness intervention (b = -0.69, p = 0.51) significantly differed from the control SSI in change between baseline and week eight.

Replicating the Main Analysis with Only Intervention Completers (Not Pre-registered)

The subsample of participants who completed their intervention (or the first of three sessions among those assigned to the three-week loneliness intervention) included n = 689 at baseline, n = 325 at week four, and n = 347 at week eight). Like the main analysis, neither the loneliness SSI (b = -1.03, p = 0.32) nor the three-week loneliness intervention (b = -0.67, p = 0.50) significantly differed from the control condition in change between baseline and week eight. See Figure 12 for visualizations of the sensitivity analyses.

Figure 12. Sensitivity Analyses Examining Change in Loneliness in Different Subsamples



Error bars show +/- one standard error. Among responses marked as fraudulent, n = 460 at baseline, n = 369 at week four, and n = 287 at week eight. Ineligible participants did not meet the criteria for struggling with loneliness and were not compensated for participating in the study; among them, n = 372 at baseline, n = 111 at week four, and n = 118 at week eight. Study completers finished the week eight follow-up; n = 376 at baseline, n = 292 at week four, and n = 376 at week eight. Intervention completers finished the SSI or the first session if they were in the three-week loneliness intervention condition; n = 689 at baseline, n = 325 at week four, and n = 347 at week eight.

DISCUSSION

An evidence-based loneliness SSI could greatly improve the accessibility of high-quality support for loneliness. I conducted an online experiment with a sample of people aged 16 and older to test the effectiveness of a 20-minute loneliness SSI against a one-hour three-session version of it and an active control SSI. None of the conditions differed in how much loneliness, distress, social anxiety, self-efficacy, or adaptive thoughts and actions changed from baseline to any follow-up timepoint, except that hopelessness declined more in the three-week loneliness intervention than in the active control post-intervention. Participants were more satisfied with both loneliness interventions than with the active control SSI. Satisfaction was highly similar across the two loneliness interventions and many more participants completed the loneliness SSI

than the three-session loneliness intervention, suggesting that the 20-minute loneliness SSI is a better option than the three-week loneliness intervention in terms of reach and engagement.

Interpreting the Main Analysis

The main analysis did not find a significantly greater reduction in loneliness among participants randomized to either loneliness intervention than among those randomized to the control. It also did not find a significantly greater decrease in loneliness between the three-session and SSI versions of the loneliness intervention. The four sensitivity analyses found similar results, as did the additional analysis adding demographic covariates into the main analysis and the additional analysis contrasting the control against the average of the other two conditions. I conclude that, among people aged 16 and up with access to the internet, the loneliness interventions are not more useful for reducing loneliness than an active control SSI.

Choice of Measure

It is possible that the measure I used was not sensitive enough to detect change in loneliness. As noted in the Overcoming Loneliness SSI, efforts to improve one's social life can be particularly difficult in the early stages as one leaves the safety of avoidance and pursues challenging social situations that might very well go poorly. Positive change in loneliness might tend to take longer than the study's eight-week duration. In that case, more proximal indicators of long-term change might be more informative.

Comparing the Main Analysis to Previous Studies

To compare the effects observed in the main analysis with similar studies, I calculated the magnitude of differences across conditions in change in loneliness without taking random effects into account. Differences across conditions in my study had extremely small magnitudes (d = 0.08 or less). In comparison, one well-powered study comparing the control SSI in this study to

two similarly designed SSIs among a sample of adolescents observed differences in change across conditions of $d = 0.18$ in depression, ds < 0.16 in hopelessness, and ds < 0.31 in agency (Schleider et al., 2022). However, another study comparing an Anxiety SSI to a control SSI in an adult sample found a statistically non-significant difference in change across SSIs of $d = 0.06$ on generalized anxiety and $d = 0.04$ on perceived control (Mullarkey et al., 2022).

The Possibility of Data not Missing at Random

Differing rates of study attrition across experimental conditions is common in research on DMHIs. This differential dropout is problematic because it can cause an overestimation of the effect of the treatment if the missing data are missing not at random (MNAR). Data MNAR means that missingness in the outcome measure can be predicted by the outcome itself (Goldberg et al., 2021). For example, in the main analysis, it could be that people dropped out of the interventions in the study more often when they felt their loneliness was not improving or worsening. Consequently, the outcome data would show that a less effective condition (with more data MNAR) decreased loneliness more than it would have had the missing data been observed. Unfortunately, in study designs like this one, it is not possible to determine whether data are missing at random or to know how much data MNAR may have affected the observed differences across conditions (Potthoff et al., 2006).

In this study, differential dropout across interventions was clear. While data MNAR cannot be ruled out, I believe there are better explanations for the observed differences in dropout rates across conditions. For one, the loneliness SSI took nearly twice as long to complete than the control condition, on average. While the three-week condition's first session was similar in length to the control condition, the prospect of having to complete two more sessions could

have led to dropout as well. The loneliness SSI and three-week intervention also included more writing exercises than the control condition did and previous work has shown that these exercises often cause dropout (Dobias et al., 2022). Finally, PFS and CEQ scores showed that participants found the loneliness SSI (which had higher dropout than the control) more acceptable and believed it would have a greater impact than the control SSI. Thus, it is unlikely that people dropped out of the loneliness SSI more because they felt it was unhelpful (unless the data MNAR was so severe that had the people who dropped out of the loneliness SSI remained, they would have moved the mean satisfaction with the loneliness SSI below that of the control SSI).

Other Findings

Insight Experiences Predicting Change in Loneliness

To my knowledge, previous work on mental health interventions has not measured if an intervention caused an insight experience. I examined if having an insight experience during an intervention predicted lasting improvement in loneliness. The interventions in Study 1 differed substantially in their likelihood of evoking an insight experience. Participants who completed the loneliness SSI had insight experiences nearly twice as often (51.0%) as those assigned to the control SSI (27.9%). The magnitude of this difference was much greater than other measures of SSI satisfaction (e.g., the PFS, CEQ, and star rating). Results also supported my pre-registered hypothesis that having an insight experience during an SSI predicted a greater reduction in loneliness between baseline and week eight, although it did not predict a greater reduction in loneliness between baseline and week four. Finally, results supported my exploratory hypothesis that insight experience predicted greater reductions in hopelessness and increases in self-efficacy just after the intervention (though it did not predict these at four- or eight-week follow-ups).

Having an insight experience was not uniquely predictive of change in loneliness; both the PFS and CEQ predicted change in loneliness as well and when entered as predictors together only the CEQ significantly predicted change in loneliness. Thus, having an insight experience appears to be a meaningful target for mental health SSIs but it does not seem to predict change in loneliness above and beyond other measures of intervention satisfaction.

Baseline Self-Efficacy and Improvement

Participants with higher baseline self-efficacy improved in loneliness slightly more between baseline and week eight but not week four. This could suggest that people with higher self-efficacy respond better to DMHIs. However, it could also mean that people with higher self-efficacy are more able to deal with challenges like loneliness in general.

Differences Across Sub-Populations

This study was unique in recruiting participants from a range of online and in-person communities that differed in their demographic makeup, cultures, and experience with participating in studies. As shown in Figure 10, recruitment source was predictive of baseline loneliness as well as change in loneliness over time. This suggests that SSIs (or particular kinds of SSIs) may be better suited for some populations than for others.

Engagement and Improvement

Participants who engaged more with the interventions, as measured by how much time they spent and the number of characters they wrote in exercises, did not appear to derive more benefit from them. Both of these metrics of engagement are noisy, so they are not sufficient to conclude that it did not matter how much attention one paid to the interventions or that the

interventions were all equally useless. This result could also be interpreted as indicating that the interventions contained too much non-essential content and that engagement would be a clearer predictor of impact if they had less "fluff," leaving only "active ingredients."

Strengths and Limitations of the Study

A strength of this study was that most of the sample was recruited from the kinds of online communities that would be a primary target for implementation of the mental health SSIs being evaluated. This improves the study's external validity. Relatedly, this study had a relatively high proportion of male-identified participants compared to other studies of mental health interventions. This is because the largest recruitment site for the study was the online forum 4chan, which reports on its advertising page that 70% of its users are male. To my knowledge, other studies of mental health interventions have not recruited from 4chan at anywhere near the scale that this study did. The study ads I posted on 4chan received nearly 10 million views and over 10,000 clicks, showing the feasibility of sharing SSIs with large and primarily-male audiences (although I did post the ads at a unique time when buying ads was especially cheap).

A limitation of this study was the prevalence of fraudulent responses. Reviewing the data, it was clear that some kinds of automated systems or organized teams targeted the study to obtain the $10 gift card compensation. While I identified many of these fraudulent responses, there are likely some that I missed. I also noticed some mischievous responding, although it was on a small scale (I noted less than 10 responses). Unfortunately, the nature of online studies with global samples and confidential data makes it infeasible to verify the authenticity of every response or to examine if participants are actually engaging with the intervention content.

Future Directions

Improving Future Research

Chronicity is one dimension of loneliness that I neglected to measure in this study. While the UCLA Loneliness Scale measures severity, it does not measure duration. As I noted in the introduction, chronic loneliness may harm well-being more than acute loneliness does (Lim et al., 2020; Martín-María et al., 2021; Zhong et al., 2016). Similarly, the kind of intervention that would help someone dealing with transient loneliness may differ from the kind that would help someone struggling with chronic loneliness, so future work should measure loneliness chronicity more carefully than I did and explore its role as a moderator of treatment efficacy.

This study found some support for the value of insight experiences as mediators of positive change in SSIs. However, the measure of insight experience used was binary, so it did not capture the experience's intensity or meaningfulness, which might be important predictors of an insight's impact. Future work can use more in-depth measures of insight experience to further explore how such experiences might predict outcomes.

As noted above, this study recruited participants from a range of online communities. Echoing other calls for better person-intervention fit in digital interventions (Mullarkey et al., 2022), I believe future work should explore if tailoring SSIs to specific communities can improve their appeal and impact. Lightweight SSIs like the Overcoming Loneliness SSI can make this tailoring easier than it is in other kinds of DMHIs.

Improving SSIs

While the Overcoming Loneliness SSI was generally well-received, there are many ways that its appeal and impact could potentially be improved. One strategy is to condense the intervention content into a more focused experience because people generally prefer and engage more with DMHIs involving shorter interaction bursts and less cognitive load (Connolly et al., 2021; Dobias et al., 2022; Gandhi et al., 2023). Indeed, several people who reviewed the Overcoming Loneliness SSI advised me that there was too much information in it for users to fully process and apply it, given their limited bandwidth. Instead, they suggested the SSI should aim to create one powerful mindset shift. I am currently conducting a study testing this hypothesis by comparing the 20-minute Overcoming Loneliness SSI to a 10-minute version with a more focused message and less time on exercises and didactics. Preliminary results suggest the 10-minute version is similarly effective and slightly more appealing than the 20-minute version, but follow-up data are not yet available.

Another way to improve loneliness SSIs' effectiveness may be to supplement them with additional follow-up content or other forms of support. In his work on digital loneliness interventions, Anton Käll argues that the changes to thinking and behavioral patterns needed to overcome loneliness require time and therapist guidance (Käll, Backlund, et al., 2020; Käll, Jägholm, et al., 2020). Adding homework or booster sessions (i.e., short follow-ups to review key points) to help users to process and implement changes has also been shown to improve SSIs' long-term impacts (Hecht et al., 2023). Likewise, interactions with a human supporter or an online support community could provide motivation and help users to clarify concepts from the SSI. Yet, as in the three-week loneliness intervention in this study, digital interventions with multiple sessions have high dropout. In addition, including human supporters comes with costs

to scalability. As such, identifying ways to improve self-guided SSIs within a single session remains an ideal solution for wide reach.

Conclusions

This study was the first to examine the effectiveness of an SSI for loneliness. The results showed that the SSI did not reduce loneliness more than a placebo control over eight weeks. Yet, the SSI was also not inferior to a 1-hour 3-session version of the same intervention spread across three weeks. Participants found both loneliness interventions more acceptable and helpful than the control. Future work should explore ways to make loneliness SSIs more impactful and identify populations for whom it could be most effective.

CHAPTER 3

Study 2. Exploring the Use of Popular Online Content in SSIs

ABSTRACT

Although SSIs are highly scalable, their effectiveness and engagingness have considerable room for improvement. In Study 2, I explored if using popular online content (e.g., a psychology lecture on YouTube, an Instagram post from a self-care influencer, or a blog post about one's lived experience with depression) in SSIs rather than researcher-created content could help to boost SSIs' impact and appeal. I ran an online experiment (n = 916) in a sample of people aged 16 and older who were struggling with comorbid depression and anxiety, 70% of whom were crowdworkers. The experiment compared an SSI that centered on a curated library of popular online content relating to mental health (n = 311) to an efficacious evidence-based mental health SSI (n = 291) and to online help-seeking as usual, in which participants searched the web for popular online content relating to their personal struggle (n = 314). Between baseline and four-week follow-up, participants assigned to the curated popular online content SSI did not report a significantly greater decrease in distress than those assigned to the researcher-created SSI (b = 0.81, p = 0.09) or online help-seeking as usual (b = 0.39, p = 0.42). However, participants assigned to the researcher-created SSI reported greater improvement in depressive symptoms (b = -0.44, p = 0.03) and loneliness (b = 0.29, p = 0.04) than those assigned to the curated popular online content condition. Participants found all three interventions generally acceptable; mean scores on the Program Feedback Scale were above 4.08 and did not significantly differ across conditions. Online help-seeking as usual was rated lower than the other two SSIs on the Credibility / Expectancy Questionnaire (ps < 0.05) and star ratings and

likelihood of an insight experience were both higher in the curated popular online content SSI than online help-seeking as usual (ps < 0.01). These results suggest that, among people aged 16+ with access to the internet, popular online content in the context of an SSI is broadly acceptable. However, on average, popular online content (whether selected from a curated library or by browsing the web) does not appear to be more effective for reducing distress than researcher-created content.

INTRODUCTION

In Study 1, a self-guided text-based SSI failed to improve mental health outcomes more than an active control SSI. Other work on evidence-based SSIs has also demonstrated small effects on clinical outcomes that wane markedly over time (Schleider, Dobias, Sung, & Mullarkey, 2020). Existing SSIs are fairly homogenous, using a text-based format with a mix of didactic content and writing exercises to apply concepts to one's life. A crucial unanswered question is how SSIs could be designed differently to be more impactful.

Improving Effective Engagement with SSIs

Effective engagement is necessary for SSIs to achieve their intended impacts (Yardley et al., 2016), so testing ways to improve engagement with SSIs could be a useful research direction (Kelders et al., 2012; Schleider et al., 2022). Two components of engagement in SSIs are particularly promising targets. First, **appeal**: when users find intervention content valuable and compelling, they are more motivated to dedicate attention to it and apply it to their lives. Second, **personalization**: when intervention content is tailored to users' needs and interests, they are more willing to engage with it. These aspects are interconnected; for example, whether one finds a piece of content appealing depends on one's personal preferences. Finding new ways to

improve SSIs' appeal and personalization could help to foster their effective engagement.

As discussed, a strength of many evidence-based SSIs is that they are lightweight, in that they can be developed and iterated on without much cost or programming expertise. However, thus far, SSIs' lightweight design has limited them to fairly simple user experiences without the compelling design features and marketing of highly-funded DMHIs like Headspace and Woebot (Balaskas et al., 2021; Garrido et al., 2019). SSIs ultimately have to compete in an attention economy with other digital interventions (and in some ways with the rest of the internet), so the bar for intervention appeal may be high in real-world settings (Paredes et al., 2014).

Could Popular Online Content Improve Engagement with SSIs?

An ideal solution would be to improve effective engagement with SSIs while keeping them as lightweight as possible. I propose that one such solution is to leverage the wealth of engaging mental health-relevant popular online content available online. People often find support for personal problems and learn helpful concepts and skills through popular online content, which I define broadly as media (text, audio, or visual) uploaded to the internet for a public audience. Although it is rarely evidence-based, popular online content may be more appealing and tailored than the researcher-created content typically used in SSIs.

Here, I focus on popular online content that is potentially relevant to mental health – admittedly, an imprecise category. Some of this content is created with the intention of supporting mental health; for example, psychoeducation lectures on YouTube, self-care advice TikToks, Instagram ADHD infographics, and blog posts written by individuals struggling with Anxiety. Other popular online content is not explicitly about mental health but might support mental health-relevant goals, such as learning concepts related to well-being by listening to a

philosophy podcast or regulating emotions by watching a favorite music video (Moore, 2013).

Popular online content is often appealing and tailored to particular audiences. As such, it may compare favorably to researcher-created intervention content in existing DMHIs, which often fails to appeal to diverse populations (Murphy et al., 2013). Yet, this content also has clear downsides. In short, I believe it is important to test the idea that popular online content can be an appealing, engaging, and safe alternative to researcher-created DMHI content.

Impacts of Popular Online Content on Well-Being

In general, youth and adults who encounter popular online content tend to find some of it useful for their well-being and some of it to be a waste of time or even harmful to their well-being (Rideout et al., 2018). Some work has directly examined how mental health-relevant popular online content impacts well-being. For example, an intervention composed of short researcher-created YouTube videos prevented emotional unbearableness from worsening during the COVID-19 pandemic (Rizvi et al., 2022). In addition, viewing narrative-based films was shown to encourage health behavior change, more so than information-centered non-narrative films and especially when the narrative films featured a character matching one's demographic identity (Murphy et al., 2013). Video interventions were also shown to improve mental health literacy and reduce stigma (Ito-Jaeger et al., 2021; Janoušková et al., 2017). On the other hand, a study found that watching an entire season of "13 Reasons Why," a TV show that prominently featured issues around mental health and suicide, did not affect mental health outcomes, although it did increase conversations and web searches about mental health (Uhls et al., 2021).

More methodologically similar to my study, research on a smartphone app called "PopTherapy" explored the effectiveness of popular online content within a DMHI. The app uses

a machine-learning-based intervention recommender system to push users one of 16 web applications repurposed as micro-interventions at the right time and on an appropriate topic based on their personal preferences. For example, the app might suggest that someone struggling with negative thoughts try a web app that helps to challenge thoughts; or, it could send someone experiencing somatic anxiety symptoms an infographic with stretching and breathing exercises. A study of PopTherapy found most users felt the app helped them to learn how to cope with stress better, though engagement was a challenge – about 80% of participants dropped out of the study within four weeks (Paredes et al., 2014).

Another study with a similar methodology to mine demonstrated the utility of self-discovered popular online content. The study compared "filmed social contact" video interventions, which had previously demonstrated efficacy, to online "self-study," in which participants were asked to browse the web for 30 minutes to find information relevant to particular keywords like "depression." Both conditions significantly increased mental health knowledge to similar extents relative to a control condition (Yamaguchi et al., 2019).

Besides the work reviewed above, there is a paucity of research on the well-being impacts of mental health-relevant popular online content. On one hand, it seems logical that this content could provide valuable information in an engaging format that users could then implement in their lives. On the other hand, many people consume a great deal of engaging and motivational popular online content with actionable guidance on a regular basis and it remains unclear if they truly benefit from that content. Some lay theories hold that consuming self-improvement content can even be destructive to one's well-being, fostering unhelpful perfectionism rather than promoting real-world positive behaviors (Better Ideas, 2020). This underscores the importance of research empirically evaluating the impact of mental health-

related popular online content.

Digital Therapeutic Alliance and Parasocial Relationships in Popular Online Content

An enduring criticism of DMHIs, particularly those without a human supporter (i.e., a therapist or coach) is that they lack therapeutic alliance, which is a bond between a client and care provider involving agreement on the tasks directed toward improvement and therapeutic goals (Bordin, 1979). Greater therapeutic alliance is generally associated with better outcomes from therapy, although some work has suggested this association may be mostly due to client expectations rather than the alliance itself (Del Re et al., 2012; Whelen et al., 2021). Some work has suggested that a "digital therapeutic alliance" (DTA) might be possible between a user and an unsupported digital intervention, in which one feels an alliance with a digital intervention as a whole (D'Alfonso et al., 2020; Henson et al., 2019; Kaveladze & Schueller, 2023). Yet, DMHI users generally do not endorse the idea that they have a "connection" with a digital tool that is anything like the bond they might share with a human supporter (Berry et al., 2018).

From the standpoint of DTA, an interesting element of popular online content is the parasocial relationship (PSR). PSR refers to a one-sided relationship that a viewer perceives with a real or fictional media character (e.g., a TV show character, a YouTuber, or a video game live streamer). PSRs may be unhealthy in their more extreme manifestations, but they often provide inspiration and guidance (Hoffner & Bond, 2022; Wulf et al., 2021; A. F. Young et al., 2013). It could be that the benefits of PSRs overlap with those of therapeutic alliance.

Finding the Right Online Content

Popular online content's quantity and diversity of perspectives are its greatest strengths as a mental health resource. Individuals differ in the kinds of content they find valuable and in the

presentation styles they find engaging – some people resonate with television personality Oprah Winfrey's holistic and deeply caring approach, some with psychologist Jordan Peterson's emphasis on self-improvement and authoritative voice, and some with Reggaeton artist Bad Bunny's heartfelt delivery. Yet, popular online content's diversity also makes it unpredictable in ways with which clinical science has not yet grappled. Compelling popular online content can be harmful, as in content that spreads misinformation or fosters negative and fatalistic attitudes.

Personalizing SSIs by Leveraging Crowdsourcing and User Choice

Personalized DMHIs aim to provide the optimal intervention for an individual at a particular time (Aung et al., 2017). Many intervention personalization efforts have taken a top-down approach, in which an external agent (i.e., a therapist or algorithm) matches one to an appropriate intervention based on personal characteristics (A. J. Fisher & Boswell, 2016; Rohani et al., 2020). In this top-down tradition, work such as the distillation and matching model has contributed to a mechanistic understanding of *what* intervention content works *for whom* (Chorpita et al., 2005).

Another approach to personalization might be called "bottom-up," allowing one to tailor one's own intervention experience. One example of a bottom-up approach is to choose from multiple interventions. A meta-analysis found that receiving a preferred mental health intervention was associated with moderately lower dropout rates and greater therapeutic alliance (Windle et al., 2020). However, other research has found that intervention preference does not predict mental health or engagement outcomes (Linardon et al., 2022; Lindegaard et al., 2020).

Both top-down and bottom-up personalization approaches may improve DMHIs' impact. Both approaches also have potential risks. Top-down personalization risks poor tailoring based

on an incomplete understanding of an individual, particularly when a non-human system does this personalization (Chancellor & De Choudhury, 2020; Kaveladze, Young, et al., 2022). On the other hand, bottom-up personalization might threaten DMHIs' reliability or lead to unintended outcomes; for example, with freedom over the intervention content they consume, users might tend towards flashier content and away from less appealing but more useful content.

The popular online content-driven SSI evaluated in the present study takes a bottom-up approach to personalization by allowing users to choose from a crowdsourced library of content. My SSI is not the first evidence-based program to leverage popular online content to make mental health support more accessible; for example, the online platform CredibleMind provides an aggregated library of over 4,000 articles, videos, podcasts, books, and DMHIs on various mental health topics for each of which they provide a user and expert rating of quality and evidence. My SSI differs slightly from CredibleMind's approach in that it focuses less on the credibility of content and more on its appeal to diverse populations.

Potential Iatrogenic Impacts of Popular Online Content

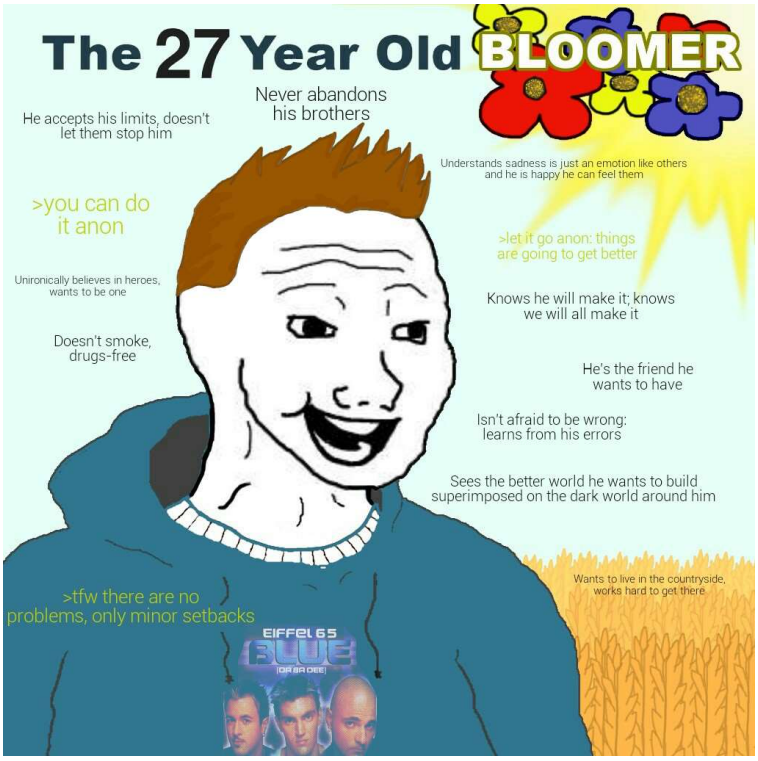
Although I assert that appealing popular online content can be more helpful than less appealing evidence-based content, popular online content also has costs. Popular online content relies on information and stylistic choices from independent content creators, most of whom are not trained mental health practitioners or researchers and are subject to financial incentives or political biases. As a result, much of the popular online content relating to mental health is not evidence-based. One study examining 100 TikToks relating to Attention-Deficit / Hyperactivity Disorder classified roughly half of these videos as misleading (Yeung et al., 2022). Such content could have a range of undesired effects on mental health and other aspects of one's life.

As noted, one concern about popular online content related to mental health is that creators' incentives might be misaligned with users'. One incentive is money – many creators leverage social media sites like Instagram to spread awareness about mental health and provide helpful tips (Latha et al., 2020), but some creators appropriate this mental health discourse to sell products or gain attention (Jennings, 2022). Trying to make money from mental health-related popular online content, or even employing so-called "dark persuasive design patterns" to capitalize on user attention (Southwell et al., 2019), do not inherently produce iatrogenic outcomes. Yet, the impact of monetary incentives should be regarded with caution.

Another concerning incentive is political power; political groups have a long history of co-opting self-improvement rhetoric to rally support for a political project (Bellassai, 2005). For example, in "self-improvement general" (/sig/) discussion threads on the online forum 4chan, posters shared advice, encouragement, and user-made guides for achieving well-being goals such as starting a physical exercise practice or developing a growth mindset. An example of a user-made motivational image found on a /sig/ thread is shown in Figure 13. On one hand, /sig/ was a remarkable instance of a user-led initiative offering tailored information and validation. On the other hand, 4chan's anonymity and lax moderation created conditions in which /sig/ threads sometimes facilitated the spread of Nazi ideology (Elley, 2021).

Even if a piece of popular online content is not iatrogenic in the sense that it risks harm, it could carry the opportunity cost of taking up time and effort that could have been better spent elsewhere. Greater engagement does not necessarily predict greater effectiveness (Kelders et al., 2012). Interventions that include popular online content should take responsibility for that content, ensuring it is truly useful and not harmful.

Figure 13. A Self-Improvement Meme found on a /sig/ thread in the 4chan fitness board



METHODS

Participants

Recruitment mirrored Study 1, except that participants recruited through the crowdwork platform CloudResearch Connect were compensated for completing each part of the study ($0.25 for the screen, $5.00 for the main part, $2.00 for the four-week follow-up, and $2.75 for the eight-week follow-up).

Ethics and Pre-registration

The procedures were approved by the University of California, Irvine human subjects institutional review board (Protocol #1253). The review board granted waivers of participant consent and parental consent. All participants reviewed an electronic information sheet and

agreed to participate. Before launching the study, I pilot-tested intervention material with labmates, friends, and family aged 18-82 to ensure the intervention material was generally acceptable. The study was registered on clinicaltrials.gov (ID: NCT05687162) and pre-registered on the open science framework (https://osf.io/4hkda/).

Procedure

The procedure mirrored Study 1 except that participants recruited through CloudResearch Connect completed the screening measure and information sheet in a separate survey before they were invited to participate in the full study (only if they were eligible to participate).

Interventions

The experimental condition was a popular online content-centered SSI for distress that my team and I created, called the Blu Surfer Program. In the SSI, the user selects the kinds of support they would like to explore (at least three kinds but up to as many as they wished). Next, the user browses a library of crowdsourced and annotated popular online content, filtered based on their chosen kinds of support, and selects the content they want to keep. Finally, the SSI provides the user an annotated list of the content they chose (called a mental health resource guide), which they can keep and build on later.

One comparison condition was the ABC (Action Brings Change) Project, a 20-minute SSI based on behavioral activation found to be efficacious for reducing depression, anxiety, hopelessness, and self-hate and increasing perceived control and agency in youth (link to SSI: https://osf.io/ch2tg/, license: http://creativecommons.org/licenses/by-nc-sa/4.0/) (Schleider,

Burnette, et al., 2020; Schleider et al., 2022; Schleider, Mullarkey, Mumper, et al., 2019). I slightly modified the phrasing in the SSI to make it more relevant to both teens and adults as it was originally designed for teens. I chose to use the ABC Project SSI rather than a less effective control condition (like the control condition used in Study 1) to compare popular online content to researcher-created intervention content with demonstrated effectiveness.

The other comparison condition was "online help-seeking as usual." In this condition, participants were instructed to browse the internet for at least 20 minutes to create an annotated list in Qualtrics of at least seven pieces of popular online content that could help them to overcome personal struggles. Just as in the Blu Surfer SSI, participants receive a copy of their list (i.e., mental health resource guide) to keep and build on later at the end. Browser and PDF versions of all three SSIs are available at https://osf.io/wfxgb/.

Crowdsourcing Popular Online Content

To crowdsource a library of popular online content for the distress SSI, I invited people to contribute links for one or a few pieces of online content, using some variation of the instructions, "If you could send one link to a video, image, article, or other online content to help someone who is struggling with their mental health, what would you send?". Some people contributed anonymously in comments on several Reddit posts I made and others contributed through a psychological science undergraduate class at UC Irvine. My team and I also contributed about 60 pieces ourselves. The library contained 121 pieces of content in total.

Next, my team of four research assistants and I selected pieces to include in the library through an iterative process of independent quality ratings, group discussions, and pilot testing with crowdworkers, labmates, friends, and family. Our content selections were shaped by our

judgments of the pieces' helpfulness, appeal, and accuracy, but we tried to include a range of diverse content representing many different perspectives. For example, we included pieces from several spiritual traditions and many pieces that lacked a clear connection to mental health (i.e., songs and cute animal videos). We also allowed for content that was not related to mental health but might achieve mental health-relevant goals, such as a music video that could inspire awe. We removed content that we deemed potentially harmful for an adolescent and adult audience, inaccurate in ways that might be misleading, or too irrelevant to mental health. We also removed content if its original platform prevented people under 18 from viewing it. An example of an annotated piece of content is shown in Figure 14.

Figure 14. A Piece of Annotated Popular Online Content From the Blu Surfer SSI

Resource 1:
Tags: Understanding mood, strategies for improvement, advice, video, loneliness
Note: *A philosophical perspective on dealing with loneliness by embracing solitude*

Measures

I measured variables before the SSI (baseline), just after the SSI (proximal), and four weeks and eight weeks later (long-term). Table 7 shows which variable was collected at which timepoint(s) and Table 8 organizes these variables by dimension.

Screening Measure

To screen participants for distress, I used the four-item Patient Health Questionnaire (PHQ-4), a validated measure of depression and anxiety (Kroenke et al., 2009). The measure asks “Over the last 2 weeks, how often have you been bothered by [...]?” and includes items such as “Little interest or pleasure in doing things” and “Feeling nervous, anxious or on edge," with response options ranging from 0 (not at all) to 3 (Nearly every day). Scores are rated as normal (0-2), mild (3-5), moderate (6-8), and severe (9-12). The measure includes a depression subscale and anxiety scale, each forming a total score between 0 and 6. I considered participants to be struggling with distress if their combined score across both subscales is greater than or equal to 3 as these scores are suggestive of distress (Kroenke et al., 2009). I pre-registered “struggling with distress” to refer to scores on either subscale being greater than or equal to 3, but I accidentally programmed the inclusion criteria in Qualtrics to require that both scales be equal to or greater than three. The Cronbach’s alpha for the PHQ-4 in this study at baseline was $\alpha = 0.87$.

Primary Outcome Measures

Distress was the primary outcome. I measured distress using the DASS-9, which is a widely-used scale that has been validated across many settings among adults and teens (Henry & Crawford, 2005; Kyriazos et al., 2018; Yusoff, 2013). The scale includes questions about one’s experiences over the past week, such as “I found it hard to wind down," rated on a scale from 0 (did not apply to me at all) to 3 (applied to me very much, or most of the time), with total scores ranging from 0-27. The scale also contains depression, anxiety, and stress subscales. The DASS-9 had $\alpha = 0.83$ at baseline.

Secondary Mental Health Outcomes

I measured loneliness using the validated three-item version of the UCLA loneliness

scale (ULS-3) (Hughes et al., 2004). The measure includes questions such as “how often do you feel left out” with responses ranging from 1 (hardly ever) to 3 (often), producing a total score between 3 and 9. Researchers have grouped people who score 3-5 as “not lonely" and those who score 6-9 as “lonely” (Steptoe et al., 2013). The ULS-3 had $\alpha = 0.84$ at baseline.

I measured self-efficacy using the short form of the GSE-6, which is a reliable and valid six-item self-report measure of self-efficacy (Romppel et al., 2013). Self-efficacy is one’s perceived ability to deal with a task or achieve a goal (Bandura, 1982; Bandura et al., 1999). The scale includes items such as “If someone opposes me, I can find means and ways to get what I want,” with responses ranging from “not at all true” (1) to “exactly true” (4), with a sum score between 6 and 24. The GSE-6 had $\alpha = 0.82$ at baseline.

I measured hopelessness using a short version of the BHS-4, which has been found reliable among both adults and youth (Perczel Forintos et al., 2013; Rhoades et al., 2018). The scale includes four items, such as “my future seems dark to me,” each rated from 1 (rarely typical) to 3 (very typical), with total scores ranging from 4-12. The BHS-4 had $\alpha = 0.89$ at baseline.

I measured meaning and purpose in life using a short-form version of the M&P, which has been found valid and reliable in adults (Salsman et al., 2020). The scale includes four items, such as “My life has meaning," each rated from 1 (not at all) to 5 (very much), with total scores ranging from 4-20. The M&P had $\alpha = 0.84$ at baseline.

I measured positive thinking and behaviors using the FATS (Terides et al., 2016), a validated measure of the frequency of adaptive behaviors and cognitions related to therapeutic change. The scale includes 12 items about one’s experiences over the past week. Items include,

“[Did you] change your thinking to be more realistic and helpful?," and are rated from 0 (not at all) to 4 (every day), with total scores ranging from 0-48. The FATS had $\alpha = 0.89$ at baseline.

Acceptability and Engagement Measures

To measure SSI acceptability, I used the PFS, which is routinely used to evaluate SSIs (Schleider et al., 2022; Schleider, Mullarkey, & Weisz, 2019). The PFS includes seven items, such as “I agree with the program’s message," rated from 1 (really disagree) to 5 (totally agree) and total scores ranging from 7-35 (Schleider et al., 2022; Schleider, Mullarkey, & Weisz, 2019). In previous work, scores greater than or equal to 3.5/5 on a PFS item have been interpreted as reflecting an “acceptable” rating (Schleider et al., 2022; Schleider, Mullarkey, & Weisz, 2019). The PFS had $\alpha = 0.85$ at baseline.

To further measure the SSI’s appeal and perceived impact, I included the CEQ, which is a validated measure that has been used with adults and adolescents (Devilly & Borkovec, 2000; Nock et al., 2007). The measure includes four items evaluating a treatment rationale’s credibility, with items such as “At this point, how logical does the treatment offered to you seem?” and two items evaluating a respondent’s expectation of how much a treatment will help them, with items such as “At this point, how much improvement in your symptoms do you really *feel* will occur?" Item responses range from 1 (not at all) to 9 (very much), with total scores ranging between 6 and 54. I adjusted the scale’s wording to refer to the “program” rather than “treatment” and to refer to change in distress. The CEQ had $\alpha = 0.92$ at baseline.

As another measure of SSI satisfaction, I used a quantitative “star rating," from one to five stars where more stars indicated greater positive feedback. I also asked participants for open-ended comments and feedback.

As a final measure of SSI satisfaction, before sharing the four-week follow-up survey with the CloudResearch Connect participants, I decided to add an additional question to it asking "How much do you feel the program you completed is helping you?" Responses ranged from 1 (none at all) to 5 (a great deal).

I evaluated if the SSI caused an insight experience with a single yes/no item asking, "Did you experience an Aha! Moment related to something you learned in the program?" Before the question, I provided the following explanation: "When an Aha! moment occurs, it is as if a realization or solution to a problem suddenly pops into your mind, like a lightbulb turning on. You might experience surprise, you might feel relief, and you might feel a light sense of happiness and ease. You can think of this experience as a miniature 'Eureka moment'. You might even feel an internal sense of "Aha!," or you might think to yourself, "of course!," "that was so obvious." Not experiencing an Aha! moment might feel like nothing much at all." This question and explanation are drawn from previous work on insight experiences (Laukkonen et al., 2022).

To measure engagement with the popular online content-based SSIs, I measured how many links to online content participants added to their lists. I also asked participants how satisfied they were with the mental health resource guide they created using a scale with responses from 1 (not at all satisfied) to 5 (completely satisfied). In the four- and eight-week follow-up surveys, I asked participants how many times they viewed a piece of content from their list and how many pieces they added to it since creating it.

Demographic Items

I measured several demographic variables: gender (multiple choice), age (continuous),

ethnicity (multiple choice), education level (multiple choice), country (multiple choice), Zip code (among USA participants), ever received help from a mental health professional (yes/no), seeing a mental health professional regularly (at least once every two months). I also measured socio-economic status using the MacArthur Scale of Subjective Social Status – Youth Version, which asks about the perceived social standing of one's family. This measure was found reliable in an adolescent sample (Goodman et al., 2001). I informed participants that all demographic questions were optional to complete.

Table 7. Full List of Measures and Assessment Timepoints

Measure (Primary, Secondary, Other)	**Baseline**	**Post-SSI**	**Four-week follow-up**	**Eight-week follow-up**
Depression Anxiety and Stress Scale – nine-item	X		X	X
UCLA loneliness scale – three item	X		X	X
General Self-Efficacy Scale – Short Form	X	X	X	X
Beck Hopelessness Scale – four-item	X	X	X	X
PROMIS Meaning and Purpose in Life – four-item	X		X	X
Frequency of actions and thoughts scale	X		X	X
Program Feedback Scale		X		
Credibility and Expectancy Questionnaire		X		X
Insight experience		X	X	X
Star rating		X		
MacArthur Scale of Subjective Social Status – Youth Version	X			
Patient Health Questionnaire – four-item	X			

Table 8. Measures Organized by Dimension

Dimension	Application to project	Study Measurement(s)
Effectiveness	Improvements in symptoms and functioning	**Primary**: DASS-9 (depression, anxiety, and stress; used in this study as a proxy for psychological distress) **Secondary**: GSE-6 (general self-efficacy), Beck Hopelessness Scale, Meaning and Purpose in Life measure, FATS (frequency of adaptive behaviors and thoughts related to therapeutic change)
Engagement	SSI Satisfaction	**Appeal:** PFS (satisfaction with SSI), CEQ (SSI credibility and expectations of impact), insight experience, number of links added to one's list of popular online content, frequency of using one's list, insight experience, star rating (program quality)

Screening	Compensation and inclusion in analyses	PHQ-4 (depression/anxiety, used as a proxy for psychological distress)

Power

To estimate the main analysis's power, I conducted an ANOVA: repeated-measures between-within interaction power analysis in G*Power 3.1 (Faul et al., 2007). I aimed to detect a Cohen's *d* of 0.18 with 0.80 power and a significance level of 0.05, expecting an r = 0.70 correlation across repeated measures based on data from sta. That required a sample size of 222. I also wanted to compare satisfaction with the programs across conditions at post-test using an ANOVA, aiming for a Cohen's d of 0.20 and power of 0.80. That required a sample size of 278. As described in the power section of Study 1, I targeted these effect sizes based on research showing active SSIs improved mental health issues more than and were more satisfactory than active controls with effect sizes of $d = 0.17$ and greater (Schleider et al., 2022).

Adjustment to Hypotheses and Analyses Due to Insufficient Data

Only 58 participants completed the week eight follow-up survey by the cut-off date I set for responses to be included in dissertation analyses. As such, I decided to shift all hypotheses and analyses that include data from the eight-week follow-up to instead use the four-week follow-up. For example, the pre-registered main analysis compared change in distress across conditions from baseline to the eight-week follow-up, but now compares baseline to the four-week follow-up.

Hypotheses

My primary hypothesis was that participants who were assigned to the popular online content-based SSI would have greater reductions in distress between baseline and four-week follow-up compared to those assigned to the ABC Project SSI and those assigned to "online help-seeking as usual." I also predicted that participants who were assigned to the popular online content-based SSI would report greater satisfaction with the SSI, be more likely to have insight experiences during the SSI, add more links to their resource guides, and view their resource guides more often than participants assigned to the other two conditions. I further predicted greater improvements between baseline and four-week follow-up in loneliness, self-efficacy, hopelessness, meaning and purpose in life, and adaptive behaviors and thoughts in the popular online content-driven SSI than in the two other conditions.

Exploratory Hypotheses

1. The kinds of content one chooses predict change in distress between baseline and four-week follow-up.
2. Participants are more satisfied with their mental health resource guides when they choose content from an annotated library than when they choose it themselves.
3. Participants are more likely to view their mental health resource guide after four weeks when they choose content from a curated library than when they select it all on their own.
4. Younger participants experience a greater reduction in distress after the popular online content SSIs than older participants between baseline and four-week follow-up. I predicted that younger people would be more likely to learn something new from a piece of popular online content than older people and because younger people, especially "digital natives," might be more comfortable with popular online content in general.

5. The annotated library condition (Blu Surfer) is more likely to lead to an insight experience than online help-seeking as usual.
6. Having an insight experience during an intervention is associated with greater reduction in distress between baseline and four-week follow-up.
7. Baseline general self-efficacy is positively associated with reduction in distress between baseline and four-week follow-up.
8. Greater engagement with a popular online content-based SSI predicts greater reduction in distress between baseline and four-week follow-up.

Analysis Plan

Inclusion Criteria

As pre-registered, I limited the analyses to only participants who 1) met criteria for struggling with distress, 2) were randomized to a condition (i.e., those who consented to participate and completed the baseline measures), and 3) did not indicate that they did not participate seriously at the end of the study. I pre-registered completing the introduction and baseline measures under three minutes as another exclusion criterion but I was not able to determine time spent on that portion due to a survey programming error. If the same email address was entered in multiple responses, I kept the response that progressed furthest in the survey; if multiple responses progressed the same amount, I kept the earliest response. I invited participants to the follow-up surveys four and eight weeks after beginning the study but I allowed them to complete the follow-up surveys at any point before ending data collection (August 20, 2023), giving everyone at least ten days to complete the week eight follow-up.

Given the frequency of suspicious responses in Study 1, I took several measures to be more vigilant about response legitimacy in Study 2. I avoided posting on social media sites where I suspected fraudulent responders had found the study, whenever I noticed a burst of fraudulent responses, I closed the survey and created a new survey, and I recruited many participants using CloudResearch Connect, which takes measures to ensure high-quality data.

Despite these precautionary measures, as in Study 1, I noticed some suspicious responses that seemed to have come from bots or individuals who took the survey multiple times with different emails (i.e., fraudulent responses). I used the same procedures to label responses as likely real or fraudulent as noted in the inclusion criteria section of Study 1.

Missing data handling

Although I took efforts to avoid missing data (e.g., a financial incentive for completing the study and reminder emails), it is not necessary to impute missing data before performing a longitudinal mixed-model analysis because such models use the available data to estimate the fixed and random effects accounting for the hierarchical structure of the data (Twisk et al., 2013).

To test for faulty randomization, I used chi-squared tests with Yates's continuity correction in combination with pairwise fisher tests using a Holm-Bonferroni adjustment for multiple comparisons. I also used t-tests assuming unequal variance and Welch's approximation of degrees of freedom. If any effects were found, I planned to adjust for them using the confounding variable as a covariate in the main model. I also planned to use these tests and a logistic regression model to check for differential dropout.

Main Analysis

In the main analysis, I tested between-group differences across conditions in change in distress from baseline to four-week follow-up. I used a mixed-effects model predicting distress with experimental condition, measurement timepoint (also called "time"), and the interaction between condition and time as predictors and a participant identifier as a random intercept. Using the "lme4" package in R, these analyses took this form:

```
lmer(distress ~ time*condition + (1|study_id))
```

Secondary Analysis

I re-ran the main analysis separately for each secondary outcome collected, although the timepoints of the "time" variable differed across outcomes, as described in Table 7. For all analyses, I used the standard $p < 0.05$ criterion for determining statistical significance.

Additional Analyses Testing Exploratory Hypotheses

1. To test if the kinds of content one chooses predict change in distress over four weeks, I conducted an MLM regression with each kind (defined as in the above analysis) as a binary predictor (except for the reference group), study timepoint as another predictor, and the interaction between selected kind and timepoint as predictors, as well as a participant identifier as a random intercept.

2. To test if participants who completed the Blu Surfer SSI were more satisfied with their resource guide than those who completed online help-seeking as usual, I conducted a t-test comparing mean satisfaction with one's guide across those conditions.

3. To test if participants were more likely to view their mental health resource guides when they chose content from a curated library than when they selected it independently, I conducted a t-test comparing the mean number of times participants viewed their guide across the Blu Surfer and online help-seeking as usual conditions four weeks after creating the guide.

4. To evaluate how age moderated change in distress over four weeks among participants completing popular online content SSIs, I conducted a mixed-effects model with distress as the outcome, age, timepoint, and their interaction as predictors, and a participant identifier as a random intercept.

5. To compare the frequency of insight experiences across the Blu Surfer and online help-seeking as usual conditions, I conducted a chi-squared test.

6. To test how having an insight experience moderated the SSI's impact on change in distress over four weeks, I ran a mixed-effects model with distress as the outcome, insight experience, measurement timepoint, and their interaction as predictors, and a participant identifier nested within experimental condition as a random intercept.

7. To test how baseline self-efficacy moderated the impact of the SSI on change in distress over four weeks, I ran a mixed-effects model with distress as the outcome, baseline self-efficacy, measurement timepoint, and their interaction as predictors, and a participant identifier nested within experimental condition as a random intercept.

8. I tested how engagement predicted change in distress between baseline and four-week follow-up in two ways. First, I ran a mixed-effects model with distress as the outcome, the number of times one had viewed a resource from one's resource guide by week four,

measurement timepoint, and their interaction as predictors, and a participant identifier nested within experimental condition as a random intercept. Second, I ran a mixed-effects model with distress as the outcome, the number of pieces of content added to one's list, measurement timepoint, and their interaction as predictors, and a participant identifier nested within experimental condition as a random intercept. I limited these analyses to participants in the popular online content conditions.

Sensitivity analyses

I pre-registered two sensitivity analyses. The first evaluated change in distress among the subset of participants who did not meet criteria for struggling with distress and thereby participated in the study without being compensated. The second evaluated change in distress when including responses marked as fraudulent, which were excluded from the main analysis. Due to a high degree of differential dropout across conditions, I ran two not pre-registered sensitivity analyses using different approaches to handling missing data in the main analysis. One analysis removed all participants who did not complete the four-week follow-up (i.e., list-wise deletion) and the other removed those who did not complete their assigned SSI.

Pre-registered Additional Analyses

1. I duplicated the main analysis with the subset of participants who completed the week four follow-up.
2. I duplicated the main analysis but with the following covariates included in the mixed-effects model: gender (multiple choice), age (continuous), ethnicity (categorical), education level (categorical), socioeconomic status (continuous), and if participants completed the study on a computer or phone/tablet. However, because the Cloudresearch

Connect data did not include the computer or phone/tablet variable, I left this variable out of the presented analysis to preserve the sample size.

3. I used a one-way ANOVA to test between-group differences in distress at four-week follow-up. If the omnibus ANOVA produced a significant F-score, I planned to conduct two planned contrasts to compare the experimental condition to each other condition and compute Cohen's ds of these comparisons. I ran the same ANOVA on the four-week follow-up and post-intervention for outcomes collected then. I also conducted these ANOVAs on secondary outcomes.
4. I calculated Cohen's *d* effect sizes reflecting within-group intervention effects on distress for each condition from baseline to four-week follow-up.
5. I ran four logistic regression models with dropout as the outcome and all baseline measures as predictors to predict which participants dropped out before completing the SSI, before the post-intervention questions, or before the four-week follow-up.

Lastly, I conducted six not pre-registered additional analyses.

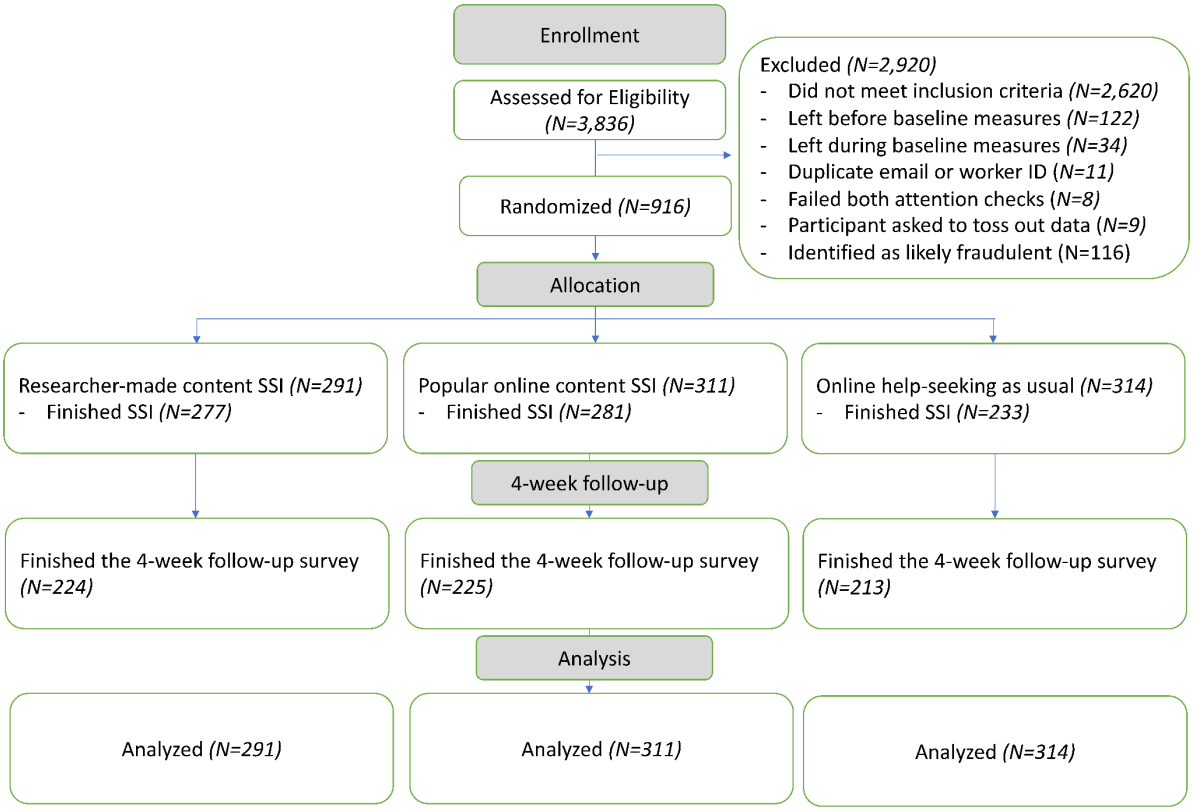
1. To see if insight experiences were uniquely predictive of change in distress, I examined if the CEQ and PFS predicted change in distress between baseline and week four.
2. To account for potential differences across participants recruited from social media and those recruited from CloudResearch Connect (i.e., that crowdworkers were more motivated by compensation and that many of them completed far more surveys and possibly more mental health interventions than participants recruited from social media), I conducted a mixed-effects regression to test if recruitment type (crowdworking platform or social media) moderated change in distress between baseline and week four.

3. I conducted a mixed-effects regression to compare change across the various online spaces where participants found the study.
4. I compared the change between baseline and week four across each subscale of the DASS-9 by conducting a separate mixed-effects regression for each subscale.
5. Much of the existing work on SSIs has used fixed-effects models to examine change across time between conditions, rather than mixed-effects models. To compare the effect sizes from this study with this other work, I calculated Cohen's *d* effect sizes of differences across conditions in the change in distress from baseline to week four, without accounting for random effects. To do so, I imputed participant-level missing distress data by creating as many imputed datasets as the percentage of missing data in the outcome, computing the mean and standard deviation for each condition and timepoint pair, and then averaging them across imputed datasets (Honaker et al., 2011).
6. To test the association between age and distress at baseline, I ran a linear regression.

RESULTS

Data collection for this study ran from April 4, 2023, to July 22, 2023. Figure 15 shows a CONSORT Diagram of the number of participants at each part of the study. I report descriptive statistics at baseline among the 916 participants who met the criteria for inclusion in analyses. Baseline distributions of outcome variables are shown in Figure 16 and sample demographic characteristics are summarized in Table 9.

Figure 15. CONSORT Diagram



CONSORT diagram showing participant flow through screening, inclusion criteria, randomization, interventions, and follow-up assessments. Due to a programming error, approximately 260 participants from the CloudResearch Connect sample were not shown one out of seven follow-up measures at random in the week four follow-up, so there is some additional missingness (< 7%) in each follow-up measure.

Figure 16. Baseline Frequency Histograms of Outcome Variables

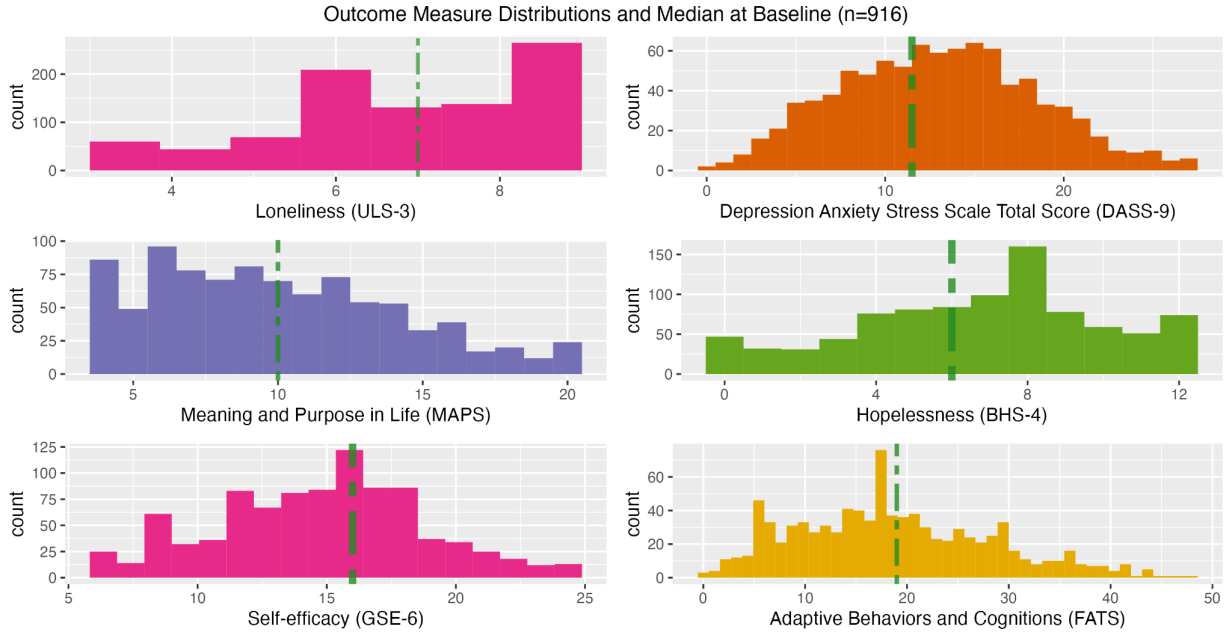


Table 9. Descriptive statistics, All Measured at Baseline

Characteristic	***n* = 916** *Mean (SD) or n (%)*
Age	33.31 (11.17)
Gender	
Female	482 (53%)
Male	394 (43%)
Non-binary / third gender	24 (2.6%)
Prefer to self-describe	8 (0.9%)
Prefer not to say	6 (0.7%)
Ethnicity	
Caucasian	577 (63%)
Latino/Hispanic	88 (9.6%)
African	71 (7.8%)
East Asian	61 (6.7%)
South Asian	35 (3.8%)
Mixed	33 (3.6%)
American Indian or Alaska Native	8 (0.9%)
Caribbean	8 (0.9%)
Middle Eastern	5 (0.5%)
Native Hawaiian or Pacific Islander	3 (0.3%)
Other	23 (2.5%)
Country	
United States of America	813 (89%)
Australia	14 (1.5%)
United Kingdom of Great Britain and Northern Ireland	14 (1.5%)
Canada	13 (1.4%)
Other	62 (6.8%)
Recruitment Source	
CloudResearch Connect	632 (69%)
4chan	100 (11%)
Instagram	83 (9.1%)
Student Subject Pool	51 (5.6%)
ResearchMatch	30 (3.3%)
Other	20 (2.2%)
Employment	
Working full-time	425 (46%)
Working part-time	166 (18%)

Student	118 (13%)
Unemployed and looking for work	92 (10%)
A homemaker or stay-at-home parent	39 (4.3%)
Retired	15 (1.6%)
Other	59 (6.5%)
Social Ladder (0-10)	4.28 (1.96)
Receiving Mental Healthcare	271 (30%)
Want Additional Mental Healthcare	589 (65%)
Used Smartphone or Tablet	150 (53%)

Some variation in sample size across demographic variables due to missingness (<2% for most variables, 69% for "Used Smartphone or Tablet" because CloudResearch Connect does not record that information).

Preliminary Analyses

Randomization

The proportion of participants assigned to each condition did not significantly differ (p = 0.60). Yet, distress (p = 0.03, ds < 0.21), loneliness (p= 0.03, ds < 0.21), socioeconomic status (p < 0.01, ds < 0.27), and ethnicity (p = 0.01, $X2$(df = 20, n = 916) = 36.18) differed across experimental conditions at baseline (before participants were assigned to a condition). Distress was higher in the ABC Project SSI than the Blu Surfer SSI (p = 0.03, d = 0.21), perceived socioeconomic status was higher in the ABC Project SSI than online help-seeking as usual (p < 0.01, d = 0.27), and loneliness was higher in the ABC Project SSI than the Blu Surfer SSI (p = 0.03, d = 0.21); the other group differences at baseline were non-significant.

Dropout by Condition

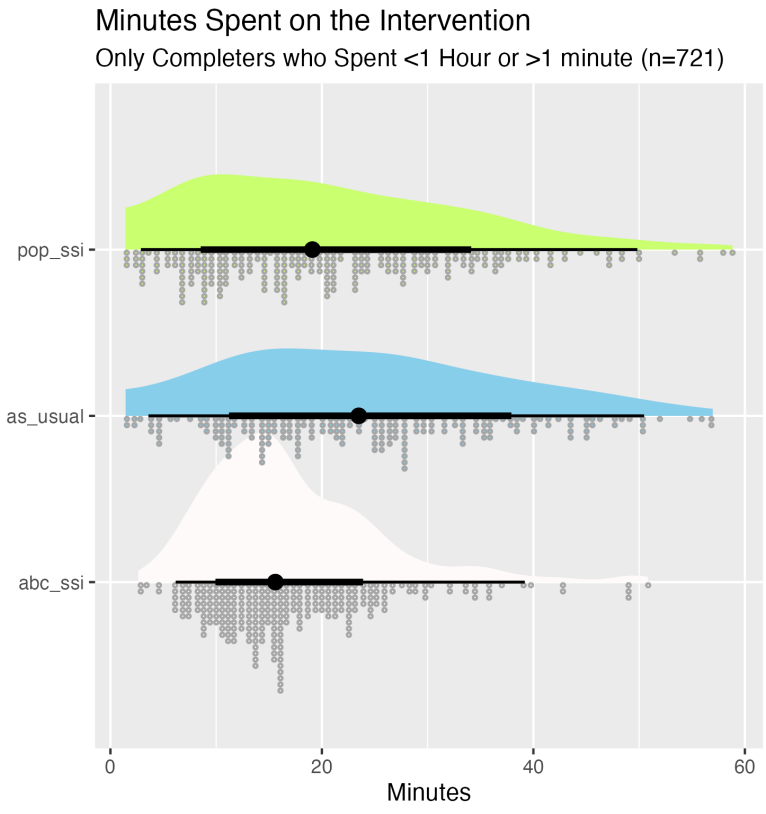
The proportion of participants who completed the intervention differed substantially across conditions – the ABC Project SSI had a 95.2% completion rate, the Blu Surfer SSI had 90.4%, and online help-seeking as usual had 74.1% [$X2$(df = 2, n = 916) = 63.32, p < 0.01; all

pairwise $ps_{adj} < 0.03$]. Completion rates of the four-week follow-up survey did not differ significantly across conditions ($p = 0.05$).

Dropout by Duration

One potential reason for the observed difference in completion rates across conditions is the variance in intervention duration, shown in Figure 17 (Goldberg et al., 2021). Among completers, the median number of minutes spent on the intervention was 15.6 (IQR 11.4–21.6) in the ABC Project SSI, 20.3 (IQR 10.1–33.5) in the Blu Surfer SSI, and 25.5 (IQR 14.6–38.3) in online help-seeking as usual. Controlling for experimental condition, the more time participants spent on an SSI, the less likely they were to complete it ($p < 0.01$, full model Nagelkerke pseudo-$R^2 = 0.20$; time Nagelkerke pseudo-$R^2 = 0.08$).

Figure 17. Duration of Completed Interventions



Density plots of SSI duration across conditions. The black dot shows the median and the black lines show where 66% and 95% of responses fell, respectively. abc_ssi = the Action Brings Change Project, a researcher-created mental health SSI; pop_ssi = the Blu Surfer Program, a mental health SSI centered on popular online content, as_usual = an SSI in which participants browse the web to find pieces of popular online content they find useful.

Predicting Dropout Using Baseline Characteristics

Compared to the participants who dropped out of the study, those who completed the four-week follow-up survey had lower baseline distress [$t(530) = 3.20$; $p < 0.01$; $d = 0.22$; 95% CI, 0.08, 0.37] and higher age [$t(444) = 6.41$, $p < 0.01$, $d = 0.48$; 95% CI, 0.34, 0.63], on average. They also differed in gender [$X2(\text{df} = 4, n = 914) = 19.69$, $p < 0.01$], ethnicity [$X2(\text{df} = 10, n = 912) = 36.36$, $p = 0.01$], where they found the study [$X2(\text{df} = 10, n = 916) = 187.48$, $p < 0.01$], whether they were receiving professional mental healthcare [$X2(\text{df} = 1, n = 913) = 5.72$, $p = 0.02$], and their employment status [$X2(\text{df} = 6, n = 914) = 65.96$, $p < 0.01$]. However, completers did not significantly differ from non-completers in baseline distress, self-efficacy, hopelessness, meaning and purpose in life, adaptive behaviors and cognitions, rate of wanting additional mental health support, or socioeconomic status, (ps > 0.05).

A multi-variable logistic regression including all baseline measures as predictors of whether one would complete the initial survey achieved a Nagelkerke pseudo-R^2 of 0.37, $p < 0.01$. A model with the same predictors predicting dropout before completing week four achieved a pseudo-R^2 of 0.29, $p < 0.01$. No mental health measure was significantly predictive of dropout in either of these multivariable models. Moreover, those who completed the four-week follow-up did not perceive the SSI to be significantly more acceptable or impactful than those who dropped out did, as measured by the PFS ($p = 0.10$), CEQ ($p = 0.81$), star rating ($p = 0.67$), or rate of insight experiences ($p = 0.07$).

Pre-registered Main Analysis: Change in Distress Across Conditions

Participants assigned to the Blu Surfer SSI did not report a significantly different change in distress than participants assigned to the ABC Project SSI (b = 0.81, p = 0.09) or online help-seeking as usual (b = 0.39, p = 0.42) between baseline and four-week follow-up.

Distress changed significantly between baseline and the four-week follow-up in the ABC Project SSI condition (d = 0.50; 95% CI, 0.32, 0.68; p < 0.01), Blu Surfer SSI condition (d = 0.35; 95% CI, 0.18, 0.53; p < 0.01), and online help-seeking as usual (d = 0.44; 95% CI, 0.26, 0.62; p < 0.01). See Table 10 for the mixed-effects model output and the top left of Figure 18 for a visualization of it.

As pre-registered, I re-ran the main analysis but included confounding variables that differed across conditions at baseline. These analyses found highly similar results to the main analysis, with participants assigned to the Blu Surfer SSI not reporting significantly different changes in distress from participants assigned to the ABC Project SSI (b = -0.83, p = 0.08) or online help-seeking as usual (b = -0.31, p = 0.52) between baseline and four-week follow-up.

Comparing change from baseline to week four across conditions without random effects, the ABC Project SSI reduced distress more than the Blu Surfer SSI with Cohen's d = 0.17, the ABC Project SSI reduced distress more than online help-seeking as usual with Cohen's d = 0.09, and online help-seeking as usual reduced distress more than the Blu Surfer SSI with Cohen's d = 0.08. See the top left of Figure 18 for a visualization of change across conditions.

Table 10. Mixed-Effects Model Predicting Distress

	Distress Mixed-Effects Model			
Predictors	*Estimates*	*CI*	*p*	*df*

(Intercept)	12.79	12.16 – 13.43	**<0.001**	1209.97
condition [as_usual]	0.78	-0.11 – 1.68	0.087	1209.98
condition [abc_ssi]	1.16	0.24 – 2.07	**0.013**	1209.95
time [Week 4]	-1.93	-2.60 – -1.27	**<0.001**	715.88
condition [as_usual] × time [Week 4]	-0.39	-1.34 – 0.56	0.417	719.37
condition [abc_ssi] × time [Week 4]	-0.81	-1.75 – 0.13	0.092	707.43
Random Effects				
σ^2	12.85			
$\tau_{00\ pid}$	19.66			
ICC	0.60			
N_{pid}	916			
Observations	1540			
Marginal R^2 / Conditional R^2	0.043 / 0.622			

Linear mixed model testing how change in distress across time differs across conditions. The dependent variable, distress, is scored from 0-27. There are three experimental conditions (Blu Surfer SSI, ABC Project SSI, and online help-seeking as usual) and two timepoints (baseline and four-week follow-up).

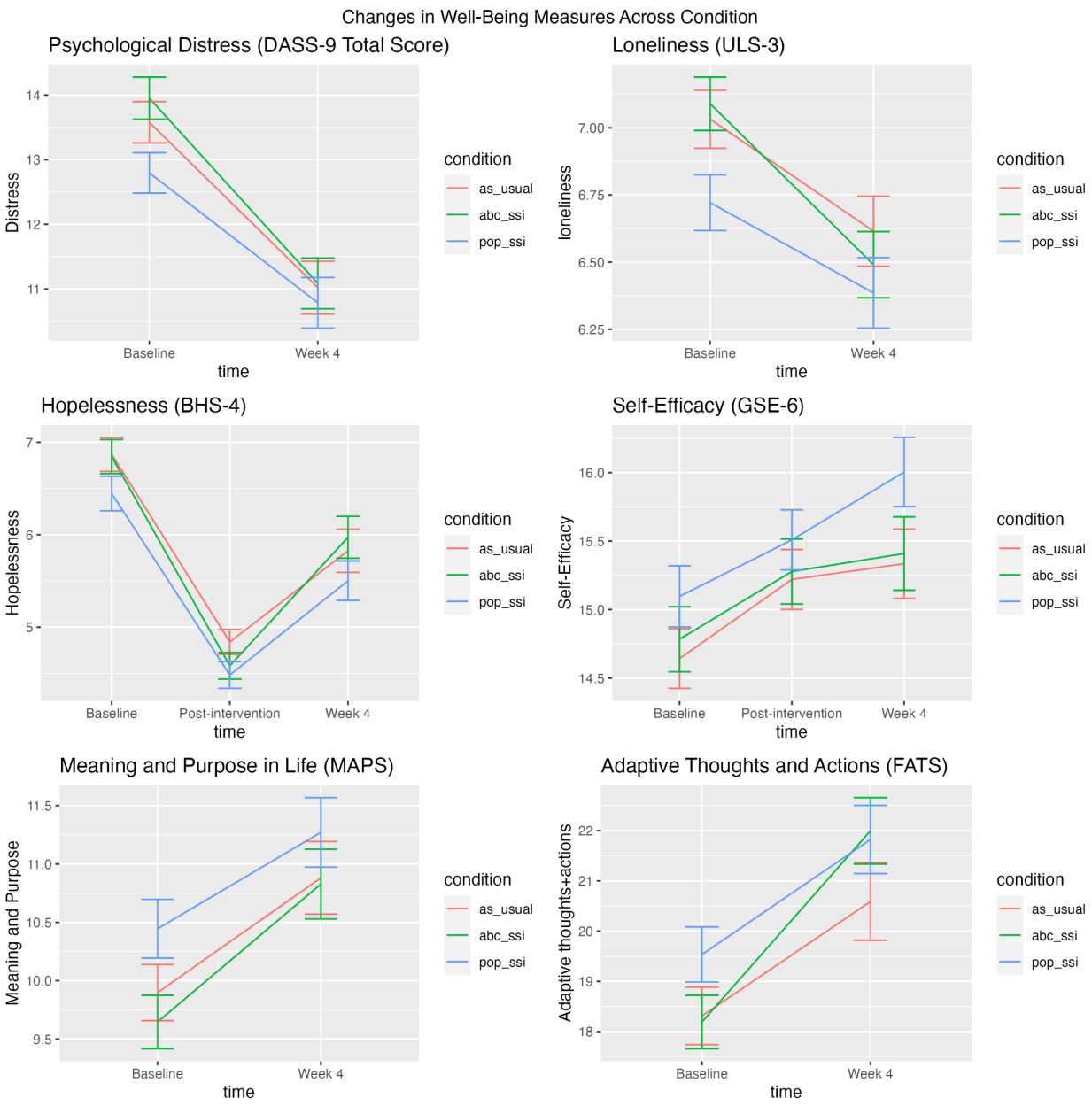
Secondary Analyses:

Change Across Conditions in Secondary Well-Being Outcomes (Pre-registered)

Participants assigned to the Blu Surfer SSI experienced a significantly smaller decrease in loneliness between baseline and four-week follow-up than those in the ABC Project SSI (*b* =

0.29; 95% CI 0.01, 0.56; p = 0.04) but not those in the online help-seeking as usual condition (b = -0.01, p = 0.93). Contrary to my hypotheses, the Blu Surfer SSI did not significantly differ from the other two conditions in change between baseline and four-week follow-up in self-efficacy (ps > 0.29), hopelessness (ps > 0.46), meaning and purpose in life (ps > 0.34), or adaptive thoughts and actions (ps > 0.06). The Blu Surfer SSI also did not significantly differ from the other conditions in change between baseline and directly after completing the SSI in self-efficacy (ps > 0.53) or hopelessness (ps > 0.13). Differences across timepoints and conditions are shown in Figure 18.

Figure 18. Change in Well-Being Measures



Error bars show +/- one standard error. Baseline $n = 916$, week four $n \cong 624$

Including Covariates in the Model (Pre-registered)

When gender, age, ethnicity, education level, and socioeconomic status were included as covariates in the main model, the mean distress of participants assigned to the Blu Surfer condition did not change significantly differently than those assigned to the ABC Project SSI (*b*

= -0.80; 95% CI -1.75, 0.14, $p = 0.10$] or online help-seeking as usual ($b = -0.40$; 95% CI -1.35, 0.55, $p = 0.41$). For this analysis, the sample size shrank from n=916 to n= 910 due to missingness in covariate data.

Intervention Appeal and Engagement

According to the PFS, participants found all three SSIs acceptable (item-level means were above 3.50/5, see Table 11 for item-level scores). The SSIs did not significantly differ in their mean PFS scores ($p = 0.08$).

On the CEQ total score, participants rated the ABC Project SSI ($\bar{X}= 33.1$; $p = 0.04$; $d = 0.21$; 95% CI, 0.04, 0.39) and Blu Surfer SSI ($\bar{X}= 33.5$, $p = 0.01$; $d = -0.26$; 95% CI, 0.08, 0.43) higher than online help-seeking as usual ($\bar{X}= 30.7$) [$F_{2,784} = 4.80$, $p < 0.01$]; however, the ABC Project SSI and Blu Surfer SSI did not significantly differ in mean CEQ scores ($p = 0.89$) – See Table 12 for item-level CEQ scores and Figure 19 for the distribution of CEQ total scores. Lastly, the SSIs' mean star ratings differed [$F_{2,784} = 5.36$, $p < 0.01$], with the Blu Surfer SSI ($\bar{X}= 4.30$) higher than online help-seeking as usual ($\bar{X}= 4.03$; $p < 0.01$; $d = 0.29$; 95% CI, 0.11, 0.47) but not the ABC Project SSI ($\bar{X}= 4.19$; $p = 0.36$) and no significant difference between the ABC Project SSI and online help-seeking as usual ($p = 0.12$).

The conditions differed substantially in the number of insight experiences that participants reported [$X2$(df = 2, n = 787) = 11.73, $p < 0.01$]. 37.5% of respondents reported an insight experience in the Blu Surfer SSI condition, which did not significantly differ from the 31.9% in the ABC Project SSI (pairwise $p_{adj} = 0.07$) but was more than the 23.4% in online help-seeking as usual ($p_{adj} < 0.01$). The ABC Project SSI and online help-seeking as usual did not significantly differ in the proportion of insight experiences ($p_{adj} = 0.07$).

Lastly, four weeks after completing their SSI, most participants felt the SSI was helping them only a little ($\bar{X}$= 2.21, SD= 1.04), and the conditions did not significantly differ in this rating on average (p = 0.67).

Table 11. PFS Item-Level Means and Standard Deviations by Intervention Completed

Program Feedback Scale Items by Condition			
	as_usual, N = 232[1]	**abc_ssi**, N = 276[1]	**pop_ssi**, N = 281[1]
Enjoyed	3.79 (0.92)	3.92 (0.88)	4.02 (0.80)
Understood	4.22 (0.80)	4.38 (0.68)	4.31 (0.63)
Easy to Use	4.28 (0.77)	4.40 (0.63)	4.35 (0.74)
Tried My Hardest	4.38 (0.73)	4.35 (0.78)	4.30 (0.81)
Helpful to Others	3.94 (0.94)	4.06 (0.89)	4.11 (0.88)
Would Recommend to a Friend	3.78 (1.09)	3.87 (1.02)	3.96 (0.94)
Agree with Message	4.21 (0.75)	4.33 (0.70)	4.30 (0.73)
PFS Mean Score	4.08 (0.65)	4.19 (0.58)	4.19 (0.56)

[1] Mean (SD)

All items rated on a 1-5 scale.

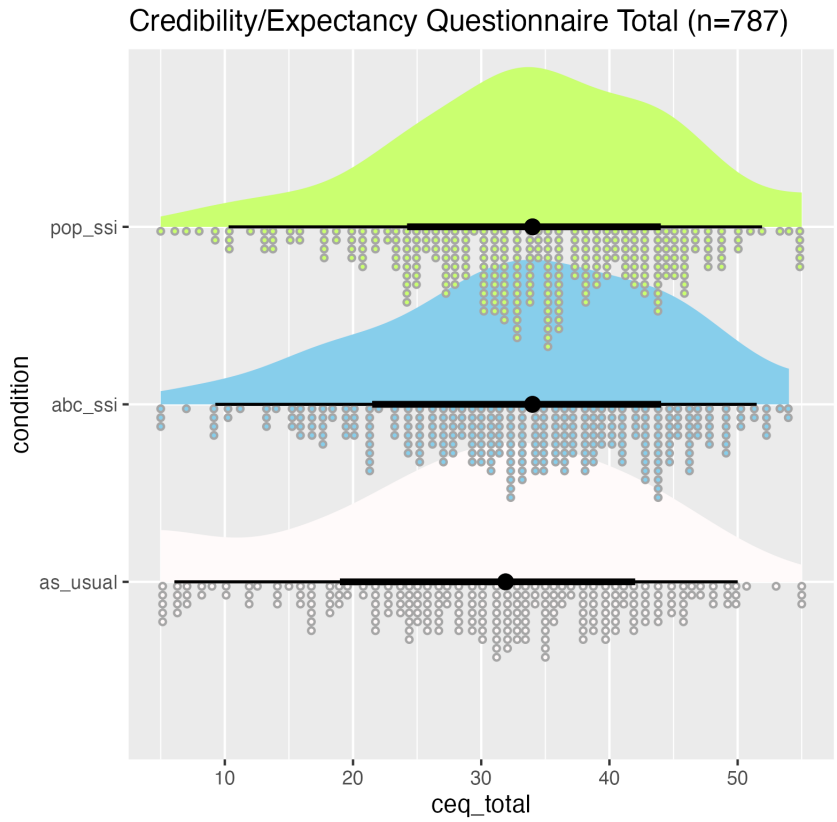
Table 12. CEQ Item-Level Means and Standard Deviations by Intervention Completed

Credibility / Expectancy Questionnaire Items by Condition			
	as_usual, N = 230[1]	**abc_ssi**, N = 276[1]	**pop_ssi**, N = 281[1]
Program Seems Logical	6.38 (2.04)	7.06 (1.69)	6.93 (1.54)
Program will Reduce Loneliness	5.25 (2.20)	5.69 (2.14)	5.76 (1.95)
Recommend to a Friend	5.56 (2.32)	6.01 (2.13)	6.12 (2.17)
Improvement Expected Over Next Few Weeks	5.00 (2.18)	5.18 (2.13)	5.35 (1.95)
How Much Improvement *Really* Expected	4.85 (2.24)	5.20 (2.17)	5.35 (2.06)
Percentage Improvement *Really* Expected	36.56 (26.02)	39.60 (25.25)	40.42 (25.54)
CEQ Total Score	30.70 (11.64)	33.11 (10.93)	33.54 (10.39)

[1] Mean (SD)

All items are rated on a 1-10 scale except for percentage.

Figure 19. Distribution of Responses to the Credibility / Expectancy Questionnaire



Additional Analyses

Insight Experience and Change in Distress (Pre-registered)

Participants who had an insight experience had greater reductions in distress between baseline and week four than those who did not have an insight experience [b = -1.24; 95% CI -2.11, -0.37; $p < 0.01$].

Intervention Satisfaction and Change in Distress (Not Pre-registered)

Both the CEQ total score [b = -0.10; 95% CI -0.13, -0.06; $p < 0.01$] and PFS mean score [b = -0.21; 95% CI -0.30, -0.11; $p < 0.01$] predicted change in distress between baseline and week four, suggesting that insight experience does not uniquely predict change in distress.

Self-efficacy and Change in Distress (Pre-registered)

Baseline self-efficacy did not predict change in distress between baseline and week four ($p = 0.08$).

Engagement With Intervention and Change in Distress (Pre-registered)

Among participants who created a resource guide (i.e., who completed the Blu Surfer SSI or online help-seeking as usual), those who viewed their guide more times in the four weeks after creating it had larger decreases in distress between baseline and four-week follow-up than those who viewed it fewer times [$b = -0.52$; 95% CI -0.82, -0.21; $p < 0.01$], on average.

Among those who created a resource guide, the mean number of resources added to one's guide was 14.20 (SD = 9.25) in the Blu Surfer SSI and 7.63 (SD = 1.34) in online help-seeking as usual. Participants who added more resources to their guide did not have different changes in distress between baseline and four-week follow-up on average than those who added fewer resources [$b = -0.04$; 95% CI -0.11, 0.03; $p = 0.23$].

Kinds of Popular Online Content Chosen and Change in Distress (Pre-registered)

Participants in the Blu Surfer program chose from the following: *motivation* (67.1% chose)*, emotional support* (55.6%)*, guidance and tools* (54.3%)*, understanding mental health* (42.8%)*, meditation / physical practice* (39.8%)*, lighthearted* (37.8%)*, philosophical / spiritual wisdom* (32.6%)*,* and *personal experience* (32.2%)*.* The mean number of kinds of content selected was 3.57 (SD = 1.06). Choosing a particular kind of popular online content was not associated with a greater or smaller change in distress between baseline and four-week follow-up for any kind of content (ps > 0.06).

Satisfaction With Resource Guide Across Conditions (Pre-registered)

As predicted, participants were more satisfied with their mental health resource guides when they chose content from an annotated library (Blu Surfer SSI) than when they choose it themselves (online help-seeking as usual) [$t(481) = 2.03$; $p = 0.04$; $d = 0.18$; 95% CI, 0.01, 0.36].

Age Moderating Change in Distress in Popular Online Content Conditions (Pre-registered)

At baseline, age was negatively related to distress [$b = -0.09$; 95% CI, -0.12, -0.06; $p < 0.01$]. Age also moderated change in distress from baseline to week four such that younger participants tended to experience a greater decrease in distress. This was true when limiting the analysis to only the popular online content conditions [$b = 0.04$; 95% CI, 0.00, 0.09; $p < 0.05$], and also when including all three conditions [$b = 0.04$; 95% CI, 0.00, 0.08; $p = 0.03$].

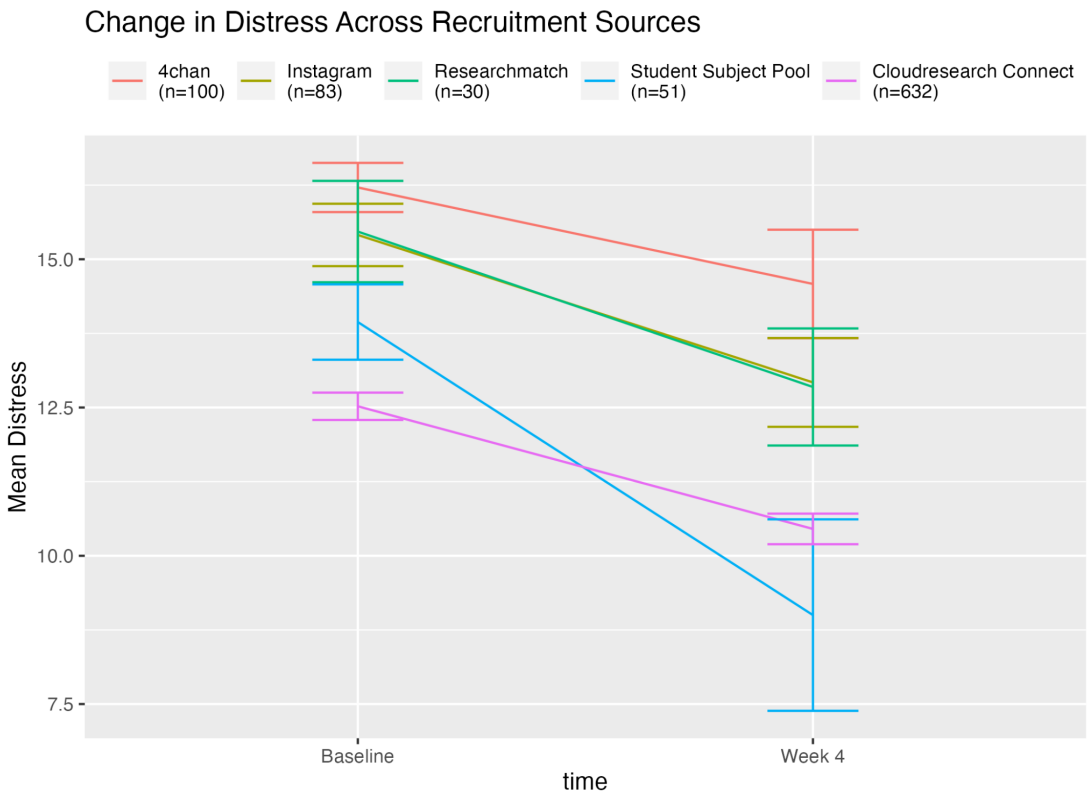
Likelihood of Viewing One's Mental Health Guide Across Conditions (Pre-registered)

Participants reported viewing their mental health guide a median of about twice in the four weeks since they created it, and this did not significantly differ depending on whether one was assigned to the Blu Surfer SSI or online help-seeking as usual ($p = 0.85$). 16% of participants viewed their guide at least 6 times. Not pre-registered additional analyses showed no significant difference across conditions in the probability that one added an additional piece of content to their guide after creating it ($p = 0.46$), but participants in the Blu Surfer SSI were more likely to have kept their guide four weeks after they created it (72.0%) than were participants in online help-seeking as usual (60.2%, $p = 0.02$).

Comparing Change in Distress Across Recruitment Type and Source (Not Pre-registered)

Participants recruited from social media did not change in distress from baseline to week four significantly differently than participants recruited from CloudResearch Connect ($p = 0.21$). Figure 20 visualizes the difference across recruitment sources in change in distress.

Figure 20. Change in Distress Across Recruitment Sources



The sample size displayed is the number of participants who completed the baseline measures.

Change in Depression, Anxiety, and Stress Across Conditions *(Not Pre-registered)*

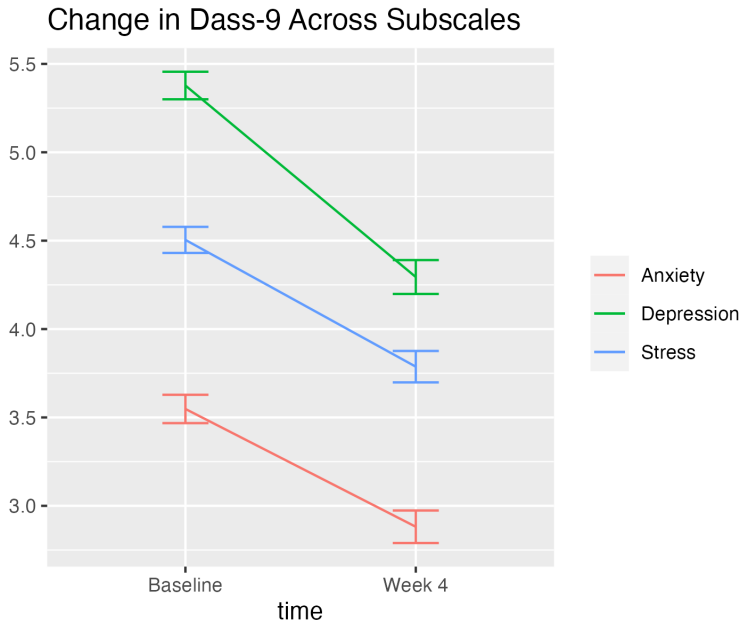
The depression subscale of the DASS-9 decreased between baseline and week four more than the anxiety subscale [$b = -0.42$; 95% CI, -0.67, -0.16; $p < 0.01$] or stress subscale did [$b = -0.37$; 95% CI, -0.62, -0.11; $p < 0.01$]. See Figure 21 for a visualization of this analysis.

Participants assigned to the Blu Surfer SSI reported smaller decreases in depression than participants assigned to the ABC Project SSI [b = 0.44; 95% CI, 0.04, 0.84; p = 0.03] but not online help-seeking as usual (b = 0.17, p = 0.41) between baseline and four-week follow-up.

Participants assigned to the Blu Surfer SSI did not differ from participants assigned to the ABC Project SSI (b = 0.14, p = 0.50) or online help-seeking as usual (b = 0.03, p = 0.89) in the extent of their change in anxiety between baseline and four-week follow-up.

Participants assigned to the Blu Surfer SSI did not differ from participants assigned to the ABC Project SSI (b = 0.08, p = 0.67) or online help-seeking as usual (b = 0.34, p = 0.08) in the extent of their change in stress between baseline and four-week follow-up.

Figure 21. Change Between Baseline and Week Four in Each Subscale of the DASS-9



For each subscale, n = 916 at baseline and n = 679 at week four.

Sensitivity Analyses

Replicating the Main Analysis Including Participants Who Did Not Meet Criteria for Struggling with Distress (Pre-registered)

Among participants who did not meet criteria for struggling with distress and thereby participated in the study without being compensated (n = 399), distress decreased significantly between baseline and four-week follow-up, averaged across conditions [b = -0.74; 95% CI -1.38, -0.09; p = 0.03]. The decrease in distress between baseline and four-week follow-up was not significantly greater in those assigned to the Blu Surfer SSI than those assigned to the ABC Project SSI [b = -0.18; 95% CI -2.84, 2.48, p = 0.89] or to online help-seeking as usual [b = 2.78; 95% CI -0.47 – 6.02, p = 0.09]; see Figure 20 for a visualization of these results.

Replicating the Main Analysis Including Responses Marked as Fraudulent (Pre-registered)

The sample of participants including those marked as fraudulent included n = 1,033 at baseline and n = 786 at week four. In this sample, the change in distress between baseline and four-week follow-up among those assigned to the Blu Surfer SSI did not significantly differ from those assigned to online help-seeking as usual (b = 0.22, p = 0.68) or the ABC Project SSI (b = 1.00, p = 0.06). See the top left quadrant of Figure 20 for a visualization of change in distress across conditions among responses marked as fraudulent.

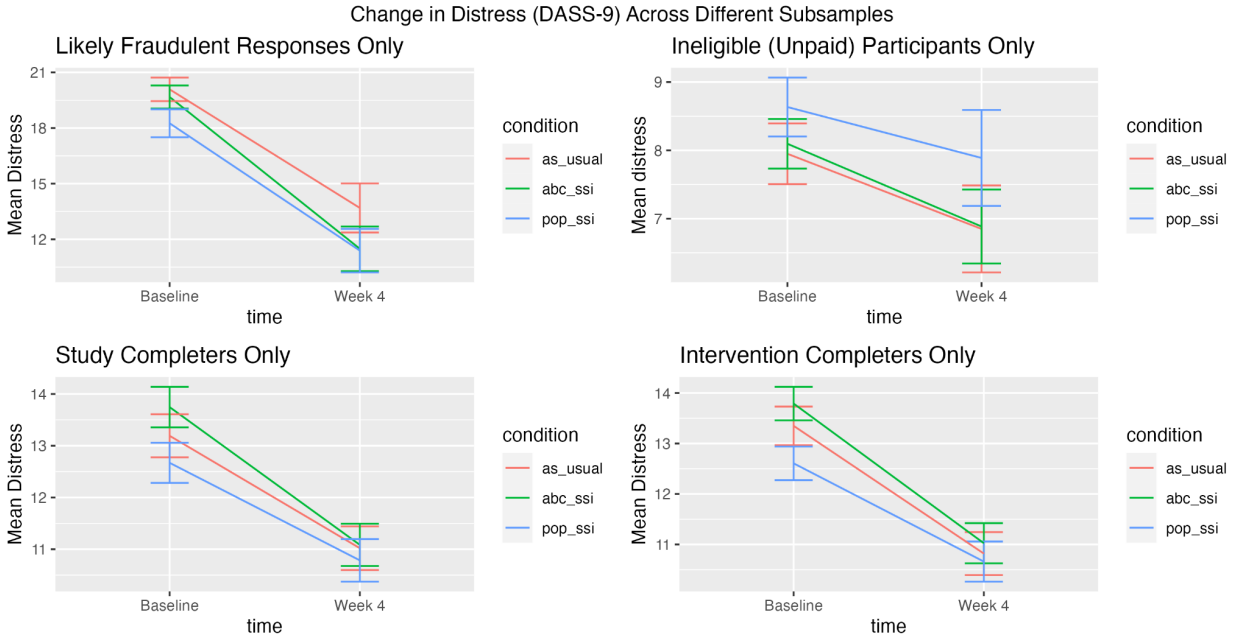
Replicating the Main Analysis with Only Week Four Survey Completers (Not Pre-registered)

The sample of participants who completed the four-week follow-up included n = 625 at baseline and week four). Like the main analysis, neither the ABC Project SSI (b = 0.78, p = 0.12) nor online help-seeking as usual (b = 0.29, p = 0.57) significantly differed from the Blu Surfer SSI in change between baseline and week four.

Replicating the Main Analysis with Only SSI Completers (Not Pre-registered)

The subset who completed their SSI included $n = 791$ at baseline and $n = 649$ at week four. Neither the ABC Project SSI ($b = 0.76$, $p = 0.12$) nor online help-seeking as usual ($b = 0.49$, $p = 0.34$) significantly differed from the Blu Surfer SSI in change between baseline and week four. See Figure 22 for visualizations of all four sensitivity analyses.

Figure 22. Sensitivity Analyses Examining Change in Distress in Different Subsamples



Error bars show +/- one standard error. Among responses marked as fraudulent (see https://osf.io/xhv2d for suspicious factors), $n = 117$ at baseline and $n = 107$ at week four. Ineligible (unpaid) participants did not meet the criteria for struggling with distress; among them, $n = 399$ at baseline and $n = 164$ at week four. Study completers finished the week four follow-up surveys; among them, $n = 625$ at baseline and week four. Intervention completers finished the SSI; among them, $n = 791$ at baseline and $n = 649$ at week four.

DISCUSSION

Improving SSIs' ability to elicit effective engagement can boost their impact and reach. Novel forms of intervention content might offer advantages over current SSI content in terms of engagement and impact. I explored the idea that popular online content (e.g., videos, podcasts,

articles, and infographics) can present the same message or target the same mechanism as researcher-created DMHI content but in more appealing and personalized forms.

I ran an experiment to test how popular online content compares to researcher-created content in online mental health SSIs. The experiment compared three SSIs for psychological distress: the Blu Surfer SSI, an SSI that my team and I created centered on a library of curated popular online content relevant to overcoming psychological distress, the ABC Project, a researcher-created SSI that has demonstrated efficacy in reducing depression and anxiety symptoms among adolescents, and an SSI that replicates online help-seeking as usual. Assignment to the ABC Project SSI led to greater reductions in loneliness and depression than assignment to the Blu Surfer SSI. Otherwise, there were no significant differences across conditions in how SSIs improved the pre-registered mental health outcomes (distress, loneliness, self-efficacy, hopelessness, meaning and purpose in life, and adaptive behaviors and cognitions related to therapeutic change) between baseline and the four-week follow-up. Using popular online content in online mental health SSIs was broadly acceptable to users.

Interpreting the Main Analysis

The main analysis did not find differences across conditions in change in distress between baseline and week 4. My four sensitivity analyses found similar results. Additional analyses revealed that loneliness and depression decreased more among participants assigned to the ABC Project SSI than those assigned to the Blu Surfer SSI This suggests that, among people aged 16+ with internet access, an efficacious SSI with evidence-based researcher-created content may be more useful for reducing loneliness or depression on average than an SSI with a collection of popular online content to choose from.

Baseline Differences in Outcomes

Surprisingly, I observed significant baseline differences across conditions in distress, loneliness, socio-economic status, and ethnicity. Because randomization was automated in Qualtrics, I do not have reason to believe that allocation to conditions was not random.

These baseline differences in outcome have consequences for the interpretation of the main analysis. For example, the fact that the mean baseline DASS-9 total score of participants assigned to the ABC Project SSI was 9% higher than that of participants assigned to the Blu Surfer SSI could have resulted in a stronger regression to the mean in the ABC Project SSI group. This would explain why change in distress trended toward being statistically significantly greater in those assigned to the ABC Project SSI than those assigned to the Blu Surfer SSI.

As pre-registered, I accounted for baseline differences across conditions by including the variables that differed as covariates in the main model. Doing so did not substantially change the model output. Some work has criticized the inclusion of variables that differ at baseline as model covariates on the grounds that covariates should only be selected based on their prognostic strength based on extant research (de Boer et al., 2015; Moher et al., 2010). However, I also ran a pre-registered secondary analysis that included covariates with clearer prognostic strength.

Insight Experiences in Popular Online Content

One reason that I hypothesized that popular online content would be more impactful than researcher-created content was that I thought it would produce insight experiences more reliably, especially when users could choose the content they wanted for themselves. Although the highest rate of insight experiences was in the Blu Surfer condition, the lowest rate was in the online help-seeking as usual condition, so popular online content did not have a uniformly greater chance of producing an insight experience. Moreover, the difference between conditions in the rate of

insight experiences was not very pronounced. I conclude that my hypothesis that popular online content would lead to more insight experiences than researcher-created content was not well supported by the data. Finally, having an insight experience during an SSI predicted a significantly higher decrease in distress between baseline and week four, but not uniquely so among other measures of intervention satisfaction.

Satisfaction and Engagement Across Conditions

Contrary to my predictions, satisfaction with and expected improvement from the SSI one completed did not differ substantially between participants assigned to the ABC Project SSI and those assigned to the Blu Surfer SSI. The results suggest that users do not perceive popular online content as more useful or appealing than researcher-created content.

Participants were, on average, less satisfied with online help-seeking as usual than the other two SSIs. Online help-seeking as usual also had a higher dropout rate than the other two SSIs. This might suggest that SSIs that provide guidance are more appealing than searching for help independently on the web. Yet, online help-seeking as usual was an imperfect proxy for online help-seeking because its approach (i.e., creating a personal guide of online resources) is only one form of online help-seeking, and one that might not resonate with many users (e.g., those who prefer social interactions in online spaces). In addition, significantly more effort was required to find and write the links and names of at least seven online resources in the online help-seeking as usual condition compared to the other two SSIs.

Engagement with Popular Online Content

On average, the Blu Surfer SSI took about 5 minutes longer and the online help-seeking SSI took about 10 minutes longer than the ABC Project SSI. This may partially be because

participants were interested in the popular online content, although it could also be because participants in both popular online content conditions were instructed to spend at least 20 minutes selecting content. Participants in the popular online content conditions reported having viewed their mental health guides about twice, on average, at four-week follow-up (i.e., about once every two weeks). In addition, 11% of respondents reported adding a new piece of content to their guide within four weeks of creating it. Thus, most users did not seem interested in regularly engaging with their resource guides after creating them, though a substantial subset did.

Greater engagement with one's mental health resource guide in the four weeks since one created it was associated with a greater reduction in distress between baseline and week four. This supports the idea that providing take-home resources alongside SSIs could be a viable strategy for boosting impact. Yet, a third variable, such as motivation to change, may partially explain both improvement in distress and engagement with one's resource guide.

Improving the Selection of Popular Online Content for Mental Health

Study participants found the Blu Surfer Program acceptable and were generally satisfied with the resource guides they made with the SSI's help, slightly more so than participants who created a resource guide with the online help-seeking as usual SSI. Yet, other crowdsourcing and popular online content curation strategies might be preferable. For example, libraries of popular online content could be tailored to suit the preferences of particular communities. In addition, algorithmic recommender systems (like those used by Youtube and TikTok) could help people to find popular online content they resonate with (De Choudhury & De, 2014; Rohani et al., 2020).

User research can also help to improve SSIs' effectiveness. My process for selecting content to include in the Blu Surfer SSI's popular online content library involved crowdsourcing

and curating with my team, but it could have benefited from more information about the kinds of content people tend to find most helpful and appealing. This study can offer such information; for example, which genres and specific pieces of content were selected most often. It could also offer insights into which kinds of content offered users the greatest psychological benefits.

Conclusions

In this study, I tested if popular online content, in both SSI and naturalistic contexts, improved distress more than efficacious researcher-created content. The data suggested that although allowing people to choose popular online content relevant to mental health is acceptable as an intervention, an evidence-based researcher-created SSI may, on average, be the better choice for helping to reduce depression or loneliness in a single session. Online help-seeking as usual appeared to be roughly as effective as the other two conditions, although it was clearly less appealing. Future work should examine if particular kinds of popular online content or ways of implementing that content into an SSI could be more effective.

CHAPTER 4

OVERALL DISCUSSION

For many people around the world, loneliness is a terrible and persistent burden. However, high-quality support can help people to reduce loneliness's influence on their lives. SSIs hold promise to empower people with effective tools at an unprecedented scale. However, no evidence-based SSI for loneliness currently exists. Furthermore, current SSIs might have substantial room for improvement in terms of effectiveness and broad appeal. In this dissertation, I tested the effectiveness of an online SSI for loneliness and explored an opportunity to make SSIs more engaging and impactful by leveraging popular online content. Study 1 showed that neither a loneliness SSI nor a three-session hour-long loneliness intervention reduced loneliness more over eight weeks than a control SSI did. The three-week version of the loneliness intervention also had no statistically significant advantage over the SSI version in improving loneliness or other well-being outcomes. Study 2 found that a researcher-created SSI was significantly more effective in reducing depression and loneliness than both an SSI in which users explored a curated library of popular online content and online help-seeking as usual.

Further Interpretation of the Main Analyses from Studies 1 and 2

I interpreted study-specific aspects of the main analyses in the respective discussion sections for Studies 1 and 2. Here, I discuss issues that span both studies.

Examining if SSIs are More Useful for Some Challenges Than Others

Neither of the primary outcomes in Studies 1 and 2 (loneliness and distress, respectively) significantly differed across conditions. One possibility is that these outcomes are particularly difficult to address with SSIs. As I noted in the introduction, existing mental health interventions

might have more modest effects on loneliness than some other well-being outcomes, including depression (Cuijpers et al., 2013; Mann et al., 2017). Work on both traditional CBT and online mental health SSIs has suggested that anxiety, which is central in loneliness (Lim et al., 2016) and composes a third of items on the DASS-9, is harder to improve with psychological interventions than other mental health issues like depression (Leichsenring & Steinert, 2017; Mullarkey et al., 2022; T. L. Osborn et al., 2020). Supporting this argument, depression decreased between baseline and week four significantly more than anxiety and stress did, as shown in Figure 21. However, another meta-analysis found that youth SSIs were more effective for anxiety-related problems than for depression (Schleider & Weisz, 2017b). These results appear to be contradictory, although it could be that SSIs improve anxiety in youths more effectively than they improve anxiety in adults. More work is needed to clarify for which populations, and for which mental health issues, SSIs are most helpful.

SSIs' Impacts Across the Lifespan

One potential reason for the null result in the main analysis is that SSIs are less helpful for improving mental health outcomes in adults than in adolescents. A few other studies have reported no significant difference in depression and anxiety between online mental health SSIs and an active control in a sample of (mostly) adults recruited online (Lorenzo-Luaces & Howard, 2022; Mullarkey et al., 2022). A meta-analysis of SSIs for youth also found that SSIs had greater impacts on young adolescents than on older adolescents (Schleider & Weisz, 2017b).

This pattern of younger people improving more from SSIs may seem to contradict work showing that mental health interventions for adolescents tend to be less effective than they are for children and for adults because these interventions clash with adolescents' desire for agency, status, and respect (Yeager et al., 2018). However, existing mental health SSIs may avoid this

pattern because of their self-guided nature and because they take efforts to reinforce users' agency. For example, the BEST principles for SSI design aim to empower youths to a "helper" or "expert" role (Schleider, Dobias, Sung, & Mullarkey, 2020). In Studies 1 and 2, I aimed to follow this principle by explaining that participating in the study and sharing feedback would help to improve future mental health SSIs and thereby help others in need.

One reason that SSIs could be more beneficial for younger people may be that younger people have had less time to develop fixed maladaptive beliefs and behaviors than older people have. Challenging entrenched negative behavioral and cognitive patterns could require more intensive support than a self-guided SSI can offer (Carhart-Harris et al., 2023). It could also be that the intervention strategies and rhetorical approaches that SSIs employ resonate with more young people (i.e., information that a young person finds paradigm-shifting could seem trite to an older person who has already encountered similar ideas many times). Another reason for this pattern could be that younger people tend to have more severe symptoms at baseline, leaving more room for their symptoms to decrease during the study. Indeed, in Study 2, younger participants had higher distress at baseline than older participants and experienced greater decreases in distress over time. However, this pattern did not extend to loneliness, which trended (non-significantly) in the direction of a positive association with age at baseline, and which did not change over the study's duration to different extents depending on one's age.

Separating Intervention Effects From Other Reasons for Change

All mental health outcomes measured in Studies 1 and 2 improved substantially over time, on average. However, neither study had a wait list control and the differences in improvement across conditions were small, so it is unclear the extent to which the interventions, as opposed to other aspects of the studies, caused that improvement (Herbert & Gaudiano, 2005).

Thus, it could be that all interventions (including the control SSI) had a meaningful and roughly equal positive impact, or it might be that none of the interventions had a positive impact.

One non-intervention factor that could have caused improvement over time is how the study was framed; specifically, the surveys and follow-up emails explained the research mission and thanked participants for their contributions to the study and for helping to improve access to mental healthcare. This framing may have functioned as a sort of gratitude intervention in which participants reflected on others' gratitude to them for their contributions (Hazlett et al., 2021). The study's framing could have also caused demand characteristics, by which participants inferred that the research team wanted participants' symptoms to improve so they reported improvement that was not genuine (Goodwin et al., 2023). Finally, the belief that the interventions reflected cutting-edge science may have boosted participants' expectations of how much they should benefit, creating a placebo effect that made participants believe the SSIs helped them more than they really did (Frisaldi et al., 2017).

Another factor is the impact of completing mental health questionnaires on well-being. Some participants reported that completing the questionnaires was therapeutic because the questions helped them to reflect on their lives. Other work has supported the idea that tracking one's mood could improve self-knowledge and well-being (Schueller et al., 2021).

Finally, regression to the mean among participants with elevated loneliness or distress is almost certainly another major contributor to the improvement observed in all conditions (Barnett et al., 2005). Supporting this possibility, in Study 1, participants who did not meet the criteria for struggling with loneliness (and therefore had lower baseline loneliness) did not experience a significant mean reduction in loneliness over the course of the study. Yet, those

who did not meet the criteria were also not compensated for their participation, which may have affected their reporting in other ways (Wilcox et al., 2012).

Measurement

The SSIs' benefits may not have been captured by the self-report outcome measures I used in this study. In addition to the measures I reported, I included several open-response text entry questions regarding intervention impact (e.g., users' favorite parts of the program, how users felt the program was helpful, and how the program could be improved). Perhaps analyzing this qualitative data would reveal ways that the intervention was helpful that my multiple-choice response measures missed. In addition, I could have assessed other indicators of intervention success. For example, in Study 1, I could have measured if the interventions affected participants' social behaviors or the quality of their relationships.

Despite participants who completed either loneliness intervention in Study 1 being more satisfied with the intervention than those who completed the control SSI, their mental health outcomes did not improve more. This finding is consistent with other work showing weak correlations between intervention satisfaction and mental health outcomes in both psychotherapy and self-guided SSI contexts (Lorenzo-Luaces & Howard, 2022; Viefhaus et al., 2019). On the other hand, at the four-week follow-up in Study 2, most participants reported feeling the SSI they completed had not been helpful, suggesting initial satisfaction with SSIs fades over time. This is consistent with work showing that user perceptions of DMHI credibility and effectiveness decrease over time, with the steepest drop by far occurring between the first DMHI session and the next day (Stalujanis et al., 2021).

Strengths and Limitations of the Studies

A strength of Studies 1 and 2 was that their samples were large enough to detect effect sizes of $d = 0.18$ and above in the main analysis, which is a similar effect to that of previous research on SSIs. Another strength was that both studies included a four-week assessment timepoint between baseline and eight-week follow-up (Lorenzo-Luaces & Howard, 2022), allowing for a better understanding of change patterns than some other studies of SSIs offered (Miu & Yeager, 2015; Mullarkey et al., 2022; Schleider et al., 2022). A final strength was that the studies contained several mental health and engagement measures, including free-response writing items that were not analyzed in this work but could be useful for future inquiry.

A limitation was that the samples in Studies 1 and 2 were quite different, making them hard to compare. One difference was that most participants in Study 1 were recruited from online communities while the vast majority in Study 2 were crowdworkers. In addition, a higher proportion of participants who completed the screener were eligible to participate in Study 1 than Study 2, maybe because my criteria for "struggling with distress" were less commonly met in the populations I sampled than the criteria for "struggling with loneliness" in Study 1. Still, participants' demographic characteristics were fairly consistent across studies.

Another limitation of Studies 1 and 2 was that they lacked wait list control groups, making it difficult to determine what caused the change in the outcomes (Mohr et al., 2009). As noted in the introduction, a meta-analysis of SSIs found substantially greater average effect sizes in studies with wait list controls compared to active controls (Schleider & Weisz, 2017b). Relatedly, although the control condition in Study 1 (The Sharing Feelings Project) provided a decent test of the experimental conditions' effectiveness against an SSI with a similar purpose and style, it may not have been an ideal choice of active control. Had I used a more effective

active control group (e.g., The ABC Project), I would have been able to test how the Overcoming Loneliness SSI compared to the best available SSI.

Future Directions

This dissertation contributes to a growing body of knowledge about online SSIs for mental health. SSIs have demonstrated the potential to scale and improve well-being for some populations and mental health issues; however, the generally small effect sizes shown in extant research and the paucity of evidence of real-world impact have caused some skepticism about SSIs' utility. For SSIs to meet their promise of providing effective support at a global scale, their impact and implementation need to improve. Here, I discuss how I believe the field should direct its efforts in improving and implementing SSIs over the next several years to advance knowledge and increase real-world impact (Beidas et al., 2022). Specifically, I note opportunities for future work toward (1) improving SSIs' effectiveness, (2) identifying which SSIs work for whom, and (3) enabling the implementation of SSIs.

Improving SSIs' Effectiveness

It is not clear that the ways SSIs are currently designed are optimal for their intended impacts. One way to improve SSIs' effectiveness is to make incremental improvements to existing designs with the help of mixed-methods user research. In addition to these incremental improvements, it could be useful to explore entirely new approaches to SSI design. While many existing SSIs employ evidence-based design principles, these SSIs are almost uniformly composed of text-based didactics and writing exercises. This leaves an array of unexplored intervention approaches, ranging from chatbot therapists to popular music videos. Study 2 tested one novel approach (i.e., using popular online content instead of researcher-created content).

Although that approach did not outperform an SSI with researcher-created content, other novel approaches to SSI design could be studied with similar designs. Collaboration across research labs and between researchers and other creatives with their own perspectives can help to make this exploration process more efficient.

As efforts to improve SSIs' effectiveness advance, a few basic questions about how SSIs work and how much they can be expected to help remain unanswered. Pursuing these questions can help to direct the field in more productive directions. First, it is not clear which mechanisms of change SSIs should target. Some existing SSIs, specifically those from the Lab for Scalable Mental Health, are thought to improve mental health by targeting the factors underlying general behavior change (e.g., hope and a growth mindset), regardless of the mental health outcome targeted (Schleider & Beidas, 2022). They also provide users with an "action plan" to help implement new skills and strategies in the future. This combination of a spark of encouragement and a plan for improvement are theorized to lead to cascading benefits. However, there might be other mechanisms by which SSIs produce positive change (or could produce positive change if designed differently). For example, it could be that a mechanism of change for SSIs is to make users feel that others share their struggles. If so, designing SSIs to highlight that message could make them more effective. Another question is what magnitude of improvement users should expect from an SSI. Whether existing SSIs tend to have profound impacts on a few users or small impacts on many users can inform design decisions.

Identifying Which SSIs Work for Whom

In addition to developing SSIs themselves, another way to improve SSIs' impact is to identify the contexts in which particular SSIs are helpful or harmful. It is unlikely that a single

SSI will be sufficiently effective and appealing to achieve the field's desired global reach. Instead, a portfolio of different SSIs would more likely fit diverse users' needs. Knowing which SSIs work for whom could help users to decide which SSI is best for them, as well as inform implementation efforts.

Research can help to determine what kinds of SSIs work for whom by comparing SSIs across different populations and intervention targets. One study that I am currently conducting extends the popular online content-driven approach from Study 2 to loneliness. The study compares a popular online content-driven loneliness SSI to a briefer version of the Overcoming Loneliness SSI and the Sharing Feelings Project control SSI used in Study 1. While not an ideal comparison because it uses samples recruited several months apart, the study will help to determine if a popular online content-based SSI is more or less helpful for reducing distress than. It will also show if the Overcoming Loneliness SSI is more or less useful for participants recruited via social media compared to those recruited via a crowdworking platform. Again, collaboration between research groups to establish priorities and standardize methods (e.g., using the same measures and control conditions) will facilitate meaningful comparisons across studies.

Another aspect of understanding appropriate contexts for SSIs is determining where they belong in the broader landscape of mental health interventions and health systems. Some work has already examined how SSIs can supplement longer-term treatment, showing that SSIs are appropriate for college students already receiving traditional therapy (Taylor et al., 2022). Other studies can use similar designs to investigate how SSI use intersects with use of other mental health supports; for example, a study could empirically test the idea that SSIs improve the likelihood of future positive behaviors and mental health support seeking.

Enabling Implementation

SSIs' ability to reach people at a broader scale than other DMHIs is their greatest strength. However, the fact that SSIs *can* reach many people does not necessarily mean that they will. For SSIs to meet their promise of large-scale dissemination, creative implementation across a range of settings is needed. It is also key that the SSIs disseminated are as helpful as possible for the targeted populations, meaning that implementation efforts should proceed in step with research on SSI effectiveness; for example, this could be done with hybrid effectiveness-implementation studies (Landes et al., 2019).

Existing SSIs like the ones that my team and I created in this dissertation are generally more lightweight and cheaper to develop. However, as noted above, our SSIs do not seem likely to generate global popularity just via word-of-mouth referrals. Implementing SSIs at scale will require substantial resources. The social media platforms, healthcare systems, schools, or other settings that implement SSIs will have to bear these costs, whether they use existing SSIs or create their own (e.g., programming SSIs into a digital platform's user experience, advertising SSIs, or training staff to deliver SSIs). Convincing these stakeholders to invest in SSIs will require evidence that SSIs could contribute to their missions and bottom lines. This underscores the importance of research bridging effectiveness and implementation (Landes et al., 2019).

Social media is one natural target for SSI implementation efforts. Some social media implementations have enabled SSIs to reach thousands of people in need of support (Cohen et al., 2023; Dobias et al., 2021). For continued success in collaborations with online platforms, the field of SSIs will need to align its timelines, incentives, and implementation strategies with the

platforms' priorities (Beidas et al., 2022). This will require a thoughtful balance between flexibility in SSI content to meet platforms' needs and adherence to evidence-based practices.

In addition to formal collaborations with online platforms, SSIs could also spread organically through online community interactions, as the self-improvement meme in Figure 13 did. To enable this kind of dissemination, SSIs need to appeal to the online communities that might share them. Improving this appeal could require efforts within SSIs to make them more engaging as well as efforts to improve the ways SSIs are marketed and packaged, potentially in community-specific ways (Becker, 2015). As mentioned in the introduction, SSIs must compete for users' attention in a limitless landscape of ways to spend one's time. Thus, implementation efforts need to build a widespread reputation for SSIs as trustworthy, appealing, and helpful.

This dissertation's results offer some insight into challenges and opportunities for SSI implementation. As a first challenge, although I advertised the studies broadly (across Studies 1 and 2, 15.6 million people saw the ads I posted on 4chan), there was little indication that people spread the SSIs organically. For example, fewer than 4% of participants said that they heard about the study from a friend. In addition, nine months after Study 1 began recruitment, fewer than 20 people had accessed the Overcoming Loneliness SSI via the browser version that all study completers (n = 588) received. As a second challenge, the rate of participation in the study among people who saw an advertisement for it was low. This is an imperfect metric of dissemination potential because the study required much more effort than an SSI alone would, but it could suggest low public interest in SSIs at present. Finally, the measures of dropout and intervention acceptability offer insight into which interventions might be most successfully implemented. In Study 1, the three-week version of the loneliness SSI was a clear loser in terms of dissemination potential – its high dropout rate was not outweighed by greater acceptability or

effectiveness. In Study 2, it appeared that popular online content was not much more appealing or effective than researcher-created content, so using popular online content in SSIs seems unlikely to improve SSIs' potential for dissemination. Yet, this does not mean that popular online content is not worth further consideration.

Conclusion

SSIs may have the potential to transform the accessibility of mental health resources globally. To ensure that SSIs can reach their full potential, it is important to continue exploring opportunities to improve their impact, appeal, and implementation across different mental health challenges and populations. Although my SSI for loneliness did not reduce loneliness more than an active control in this study, future work should continue to work on developing loneliness SSIs. Similarly, although my attempt to improve the effectiveness of SSIs for distress using popular online content was not successful, other creative approaches to SSI design and implementation might work better.

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