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Rewards of Kindness? A Meta-Analysis of the Link Between Prosociality and Well-Being

Bryant P. H. Hui
University of Cambridge and University of Hong Kong

Jacky C. K. Ng
Hong Kong Shue Yan University

Erica Berzaghi
Anglia Ruskin University

Lauren A. Cunningham-Amos and Aleksandr Kogan
University of Cambridge

In recent decades, numerous studies have suggested a positive relationship between prosociality and well-being. What remains less clear are (a) what the magnitude of this relationship is, and (b) what the moderators that influence it are. To address these questions, we conducted a meta-analysis to examine the strength of the prosociality to well-being link under different operationalizations, and how a set of theoretical, demographic, and methodological variables moderate the link. While the results revealed a modest overall mean effect size ($r = .13$, $K = 201$, $N = 198{,}213$) between prosociality and well-being, this masked the substantial variability in the effect as a function of numerous moderators. In particular, the effect of prosociality on eudaimonic well-being was stronger than that on hedonic well-being. Prosociality was most strongly related to psychological functioning—showing a more modest relationship with psychological malfunctioning and physical health. Using prosociality scales was more strongly associated with well-being than using measures of volunteering/helping frequency or status. In addition, informal helping (vs. formal helping) was linked to more well-being benefits. Demographically, younger givers exhibited higher levels of well-being other than physical health, while older and retired givers reported better physical health only. Female givers showed stronger relationships between prosociality and eudaimonic well-being, psychological malfunctioning, and physical health. Methodologically, the magnitude of the link was stronger in studies using primary (vs. secondary) data and with higher methodological rigor (i.e., measurement reliability and validity). We discussed all of these results and implications and suggested directions for future research.

Public Significance Statement

The present meta-analysis suggests a small and significant association between prosocial behavior and well-being. It also provides researchers with important insights into what theoretical (i.e., types of prosociality and well-being), demographical (i.e., age, gender, and retirement status), and methodological factors (i.e., primary vs. secondary data collection and methodological rigor) may strengthen or weaken the link between prosociality and well-being.

Keywords: prosocial behavior, well-being, mental health, physical health, meta-analysis

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Bryant P. H. Hui, Department of Psychology, University of Cambridge, and Department of Sociology, Faculty of Social Sciences, University of Hong Kong; Jacky C. K. Ng, Department of Counselling and Psychology, Hong Kong Shue Yan University; Erica Berzaghi, Department of Student Services, Anglia Ruskin University; Lauren A. Cunningham-Amos and Aleksandr Kogan, Department of Psychology, University of Cambridge.

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Correspondence concerning this article should be addressed to Bryant P. H. Hui, Department of Sociology, Faculty of Social Sciences, University of Hong Kong, 9/F, Jockey Club Tower, Pokfulam Road, Hong Kong SAR, China. E-mail: bryanthph@gmail.com

> If you want others to be happy, practice compassion. If you want to be happy, practice compassion.
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> —Dalai Lama

Prosocial behavior—acting kindly, cooperatively, and with compassion toward others—is perhaps most remarkable in its pervasiveness. For example, more than a quarter of Americans aged over 16 took part in volunteering activities between 2011 and 2015 (Bureau of Labor Statistics, U.S. Department of Labor, 2016). Across 23 countries, between 7 and 67% of their respective population were volunteers (Plagnol & Huppert, 2010), highlighting both the prevalence and variability in volunteering practices. Prosociality is not only highly common, but also a vital linchpin of society—altruism, cooperation, trust, and compassion are all necessary ingredients of a harmonious and well-functioning society (see, e.g., Konrath, 2014; Penner, Dovidio, Piliavin, & Schroeder, 2005; Wilson, 2000, for reviews). Unsurprisingly, the topic has attracted extensive attention from different disciplines, including anthropology, economics, evolutionary biology, and psychology (e.g., Andreoni, 1989; Dunn, Aknin, & Norton, 2008; Fehr & Fischbacher, 2003; Henrich et al., 2006). In its broadest sense, prosociality refers to a constellation of voluntary acts that are motivated by a concern for the welfare or benefit of others (Keltner, Kogan, Piff, & Saturn, 2014; Midlarsky & Kahana, 1994) and can come in many forms, such as prosocial spending or donation of money (Dunn et al., 2008), formal and informal volunteering (Li & Ferraro, 2005; Wilson, 2000), and blood or internal organ donation (Brethel-Haurwitz & Marsh, 2014; Steele et al., 2008).

Within the large body of research on prosociality, there is a growing interest in the link between acting prosocially and one's own well-being (e.g., Aknin et al., 2013; Borgonovi, 2008; Musick & Waggoner, 2007). At the core of these studies is the question of whether acting prosocially can benefit not only the recipient, but also the giver. The strong interest in understanding how prosociality is related to well-being has generated a substantial empirical base of studies. By using various indicators, many of these studies have now demonstrated that acting prosocially is positively associated with or beneficial to mental and physical health (see Curry et al., 2018; Midlarsky & Kahana, 2007; Oman, 2007; Post, 2005, for reviews). This research base is diverse, spanning numerous populations, approaches, metrics, and potential moderators. Therefore, we believe the time is ripe for a meta-analysis of existing literature with the aim of extracting the overarching principles that govern the prosociality to well-being link, with a careful focus on the *for whom* and *when* questions. Our work was organized around several key objectives under this broader goal. First, we aimed to establish the magnitude of the prosociality to well-being effects (different effects for different types of well-being). Second, we modeled how a variety of different factors influenced this relationship. Factors included in our study were: (a) types of prosocial behavior, (b) types of well-being outcomes, (c) demographic variables, and (d) methodological factors.

The Link Between Prosociality and Well-Being: Theories and Empirical Data

Well-being is an umbrella term used to describe the optimal psychological experience and functioning of humans (Deci & Ryan, 2008). Contemporary psychologists have made a dual, yet overlapping, distinction between two views of well-being—hedonism and eudaimonism. In general, the hedonic view focuses on how one feels about his or her life (Ryan & Deci, 2001). One dominant approach in hedonism is to assess subjective well-being, which includes life satisfaction, the presence of positive affect, and the absence of negative affect (Diener, Suh, Lucas, & Smith, 1999). Instead of just focusing on subjective feelings, the eudaimonic view on happiness or well-being is also concerned with actualizing human potential (Deci & Ryan, 2008). Following this conceptualization, well-being is a process of realizing one's daimon, or true self, that is, acting congruously with deeply held values and fulfilling virtuous potentials (Deci & Ryan, 2008; Ryan & Deci, 2001). In a more general sense, well-being can be defined as "feeling hopeful, happy, and good about oneself, as well as energetic and connected to others" (Post, 2005, p. 68). Hence, it can be broadly treated as mental and physical health (e.g., McKee-Ryan, Song, Wanberg, & Kinicki, 2005; Ware, Kosinski, & Keller, 1994). Researchers working on understanding the prosociality to well-being link have studied the relationship across the wide spectrum of well-being perspectives and definitions and provided a rich corpus of work to derive the (a) general principles about well-being and (b) more specific and nuanced comparisons across different well-being subdefinitions.

In line with the general belief exemplified by the words of wisdom from Dalai Lama, scholars have developed various theories to explain why prosociality might be linked to well-being. In a model of helping, Midlarsky (1991) proposed five mechanisms through which prosocial behavior may benefit the help-givers, especially for older adults, based on the fact that acting prosocially can (1) increase self-evaluations and perceived competence, (2) distract help-givers from focusing on their own troubles and stress, (3) help realize the meaning and value of life, (4) increase positive moods, and (5) facilitate social integration. In a similar vein to (2) and (3), the response shift theory suggests that the process of engaging in prosocial behavior facilitates psychological adaption via the shift of internal standards, values, and the conceptualization of well-being (Schwartz & Sendor, 1999; Sprangers & Schwartz, 1999) and the disengagement from self-focused mental problems, such as anxiety and depression (Schwartz, Meisenhelder, Ma, & Reed, 2003). In line with (4) that is pertinent to mood improvement, the negative-state relief model posits that helping actions reduce negative moods (Cialdini, Baumann, & Kenrick, 1981; Cialdini & Kenrick, 1976), while the theory of warm-glow giving focuses on the sense of joy and satisfaction from doing good for others (Andreoni, 1989).

In recent years, Dunn, Aknin, and Norton (2014) used self-determination theory (SDT; Deci & Ryan, 2000) to understand why giving is beneficial to emotional well-being, where the basic psychological needs for competence and relatedness appear to share theoretical overlap with (1) and (5) in Midlarsky's model, respectively. In particular, individuals garner more subjective well-being when their prosocial spending has a positive impact on a recipient, which satisfies their fundamental need for competence (Aknin, Dunn, Whillans, Grant, & Norton, 2013). They also experience higher positive affect after prosocial spending on a stronger social tie (vs. a weaker social tie), because the need for relatedness is satisfied (Aknin, Sandstrom, Dunn, & Norton, 2011). There is also another study showing that people experience more well-being only if they have autonomous

motivation for a prosocial act, where the need for autonomy is satisfied (Weinstein & Ryan, 2010).

While the above theories mainly focus on psychological well-being, Danner, Friesen, and Carter (2007) proposed a model on prosocial behavior and physical health. According to their model, positive emotions—future-oriented positive ones in particular—arise from helping others. These emotions then motivate people to contribute their time and energy to prosocial behaviors more frequently. Such choices, together with the positive emotions, strengthen the cardiovascular and physiological immune responses; thus, improving health and increasing longevity.

The scholarly inquiry into the link between prosociality and well-being has attracted plentiful empirical investigations—be it cross-sectional, experimental, longitudinal, diary, and experience-sampling. Numerous correlational studies have examined the link between prosociality and well-being using a variety of indicators, but the findings have been mixed. A number of studies have demonstrated a significant positive association between prosociality and well-being. For example, long-term volunteering has been shown to predict mental health benefits (i.e., reduced levels of depression) among all age groups in the United States (Musick & Wilson, 2003). Middle-aged adults in the United States who volunteered formally had better self-reported health and happiness (Borgonovi, 2008). Other work has documented that volunteering was positively related to longevity in the United States (Luoh & Herzog, 2002) and Israel (Shmotkin, Blumstein, & Modan, 2003). As for Asian countries, one study that sampled Japanese undergraduate students showed that subjective happiness could be increased by counting one's own acts of kindness during the week (Otake, Shimai, Tanaka-Matsumi, Otsui, & Fredrickson, 2006). Older Chinese volunteers reported better physical health, higher self-efficacy, greater life satisfaction, and less psychological distress (Wu, Tang, & Yan, 2005). Moreover, at a global level, the association between volunteering and well-being has been established in a 23-European country study (Plagnol & Huppert, 2010). On a larger scale, Aknin et al. (2013) showed a positive link between prosocial spending and happiness in a study across 136 countries—even after controlling for income level and other relevant variables.

However, there are also numerous studies showing a weak, nonsignificant or even negative association between prosociality and well-being. For instance, one study found that trait prosociality had a negative correlation of −.06 with life satisfaction (Gebauer, Riketta, Broemer, & Maio, 2008). Komninos (2009) showed that the score of a prosocial behavior inventory had a nonsignificant correlation of −.03 with negative affect. The association between volunteering time and happiness was also weak—.08 (Dulin, Gavala, Stephens, Kostick, & McDonald, 2012). Other studies found that both volunteerism and altruistic activities in the past 6 months were weakly and negatively correlated with depression at around −.06 (Gilster, 2012; Morris & Kanfer, 1983). As for the studies on giving money, charitable contributions were found to have a correlation of .05 with psychological well-being (Choi & Kim, 2011). Collectively, the broad spectrum of effects in the above empirical studies highlights that there is still substantial uncertainty regarding the magnitude of the prosociality to well-being effect, giving us reason to suspect that it can range from nonexistent to moderate. Thus, our first aim in the present meta-analysis was to establish a clear effect size to guide future theoretical and empirical work.

It is also noteworthy that, there has been an increasing number of diverse experimental studies with an aim to establish a causal link flowing from prosocial actions to enhanced well-being, especially in recent years. For example, research studies have used both one-off experiments (e.g., Donnelly, Lamberton, Reczek, & Norton, 2017; Martela & Ryan, 2016b) and multiple-time point experiments (e.g., Kerr, O'Donovan, & Pepping, 2015; Ko, Margolis, Revord, & Lyubomirsky, 2019; Trew & Alden, 2015) to examine the effects of kindness on well-being indicators. While certain researchers are interested in how well-being is influenced by giving time or acts of kindness (e.g., Buchanan & Bardi, 2010; Ouweneel, Le Blanc, & Schaufeli, 2014), other researchers have examined the effect of prosociality in the context of giving money or prosocial spending (e.g., Aknin et al., 2013; Anik, Aknin, Norton, Dunn, & Quoidbach, 2013; Dunn et al., 2008) and economic games (e.g., Konow & Earley, 2008).

Intriguingly, there is also evidence that feeling good promotes prosociality (e.g., Isen & Levin, 1972). Meanwhile, some researchers have argued that there may be reciprocal effects between well-being and prosociality. For example, using a panel design, Thoits and Hewitt (2001) successfully demonstrated that volunteer work made people feel happier, gave them greater life satisfaction, self-esteem, and physical health, and that people who were happier tended to do more volunteer work. In a section of their meta-analysis, Lyubomirsky, King, and Diener (2005), provided evidence that positive affect heightened generosity. However, they also stressed that there might be a positive feedback loop where helping elevates mood, and pleasant moods foster helping. Nevertheless, the effect of prosociality on well-being has received more empirical support than the other way round to date. Granted, the issues with respect to the direction of causality or reciprocity are important, but the focus of the present meta-analysis was on the magnitude of the link between prosociality and well-being as well as the effects of experimental manipulations of prosociality on well-being variables.

Previous Reviews and Meta-Analyses

Despite the burgeoning state of research related to prosociality and well-being—over 55,750 hits were obtained from five electronic bibliographic databases—reviews and meta-analyses examining the effect of prosociality on well-being are quite scarce. In a recent review, Konrath (2014) systematically summarized the studies on giving time and money, and their links to well-being. There is also another review on older adults suggesting that volunteering among seniors is associated with reduced depression, better health, fewer functional limitations, and lower mortality (Anderson et al., 2014). Yet, these reviews did not rely on any statistical meta-analytic techniques, leaving many empirical questions open.

There are, however, a few meta-analyses available. In one recent meta-analysis on mortality, Okun, Yeung, and Brown (2013) found that volunteering reduced mortality risk of older adults by 24% on average. The earliest meta-analysis on prosociality and well-being was probably conducted by Wheeler, Gorey, and Greenblatt (1998), who found a significant association between volunteering and quality of life among seniors (mean $r = .25$). Furthermore, they showed that nearly eight out of 10 older adults who offered formal help scored higher on measures of quality of life than those who did not volunteer. Nonetheless, there are several limitations to this meta-analysis. First, the authors only examined 29 independent studies. Second, all the

participants were from North America (the United States and Canada). Third, the study focused exclusively on older volunteers (mean age = 71). Fourth, perhaps because of the limited number of independent studies, only the direct (vs. indirect) type of volunteer services was found to be significantly moderating the effect of volunteering on well-being. More recently, a meta-analysis on 27 experimental studies was conducted by Curry et al. (2018), which revealed that the overall effect of kindness on actors' well-being was small-to-medium ($\delta = 0.28$) and found no significant moderators. While their findings advance our understanding of the well-being benefits of kindness, the analysis covered mostly research studies from North America and Europe. Hence, it is prudent to note that there are still many experimental studies in other countries, let alone other study designs that are not included. For instance, there are studies in Australia (e.g., Kerr et al., 2015), India (e.g., Aknin et al., 2013, Study 2b), and China (e.g., Guo, Wu, & Li, 2018). It is possible that, because of the lack of variations in such a limited number of studies and participants, Curry et al.'s (2018) effort of moderation examination was unsuccessful. Indeed, over the past two decades, there has been a proliferation of empirical studies on different kinds of prosociality and well-being indicators across a wide variety of different demographic characteristics, nations, and research designs. Thus, this is now an opportunity to substantially expand the research into the link between prosociality and well-being beyond previous meta-analytic efforts and overcome the above limitations.

Moderators of Prosociality's Effect on Well-Being

One potential reason for the diverse main effects described above is that there are important factors that moderate the relationship between prosociality and well-being, including various theoretical, demographical, and methodological factors. In the present work, we focused on two classes of theoretical moderators: (a) types of prosociality (i.e., formal helping vs. informal helping; charitable donation or prosocial spending, volunteering/helping frequency, volunteering/helping or not (i.e., yes or no), membership in voluntary associations, and prosociality scale), and (b) types of well-being (i.e., eudaimonic well-being vs. hedonic well-being; and psychological functioning, psychological malfunctioning, and physical health). Methodological variables comprised study quality, data collection (i.e., primary vs. secondary), and research design (i.e., cross-sectional, longitudinal, diary or experience sampling, experimental, and volunteering program). Demographic variables included age, gender, and retirement (i.e., retired vs. nonretired).

Theoretical Moderators

Types of prosociality. Based on its degree of formality, helping can be conceptualized as formal and informal helping. Formal helping is either for the betterment of the community or for a specific group of people who are in need, usually planned, and carried out in the context of organizations. On the other hand, informal helping refers to spontaneous daily helping acts, in the form of private and unorganized assistance toward nonrelatives (Konrath, 2014; Wilson & Musick, 1997). While formal helping and informal helping are moderately correlated, $r = .42$ (Krause, Herzog, & Baker, 1992), studies have reported that they predict different well-being indicators. For instance, Plagnol and Huppert (2010) found that, compared with formal helping, informal helping was generally associated with higher self-reported health after controlling for sociodemographic variables across 23 European countries. Furthermore, informal helping was more strongly correlated with both hedonic indicators (i.e., happiness, life satisfaction, and positive affect) and eudaimonic indicators (i.e., accomplishment and worthwhile). In contrast, Li and Ferraro (2005) found that formal helping had a beneficial effect on older people's depression in the United States, whereas informal helping did not. Despite the mixed empirical evidence, these findings highlight the importance of distinguishing between formal and informal helping and their effects on well-being. Thus, in the present study, we proposed testing the interaction effect of these two types of helping acts on the prosociality to well-being link and predicted that informal helping might better determine well-being benefits than formal helping.

While the formality of prosociality is possibly the most fruitful distinction previously found in understanding the moderators of the prosociality to well-being link, we reasoned that the type of measurement used to operationalize prosociality would also likely be important. To start with, there are different approaches to measuring donations of money and time (Konrath, 2014). In terms of giving money, researchers have measured the amount of money that participants spent on others or donated to a charity in a certain period of time (e.g., Choi & Kim, 2011; Dunn et al., 2008), as well as whether participants donated money to a charity in a specific timeframe using dichotomous responses (e.g., Aknin et al., 2013, Study 1). As for focusing on giving time, researchers have focused on participants' frequency of volunteering/helping in terms of the amount of time (e.g., hours per week, or per month) or the frequency within a given time-frame (e.g., never, rarely, sometimes, or frequently; e.g., Dulin et al., 2012; Harris & Thoresen, 2005). In some studies, respondents answered yes/no questions to indicate whether they had volunteered/helped others over a certain period of time (e.g., Brown, Hoye, & Nicholson, 2012). Other studies have measured time commitment in terms of associations with a volunteer organization (e.g., Rietschlin, 1998). Apart from these, various psychometrically established scales were adopted in a number of studies to measure prosociality, such as the Self-Reported Altruism Scale (e.g., "I have given directions to a stranger" and "I have helped an acquaintance to move households"; Rushton, Chrisjohn, & Fekken, 1981), the Prosocial Personality Battery (e.g., "I have allowed someone to go ahead of me in a line" and "I have offered to help a handicapped or elderly stranger across a street"; Penner, Fritzsche, Craiger, & Freifeld, 1995), and the Prosocialness Scale of Adults (e.g., "I share the things that I have with my friends" and "I spend time with those friends who feel lonely"; Caprara, Steca, Zelli, & Capanna, 2005). The above measurements represent the wide variety of approaches to (a) conceptualizing and (b) measuring prosociality. We reasoned that such different operationalizations of prosociality could lead to different magnitudes of the correlation with well-being. Hence, we modeled them by testing the moderation effect of the five types of prosociality measures reviewed above (i.e., charitable donation or prosocial spending, volunteering/helping frequency, volunteering/helping status (yes/no), membership in voluntary associations, and prosociality scale).

Types of well-being. Well-being can be conceptualized as eudaimonism and hedonism. We reasoned that prosocial behavior is one of the ways to help actualize the virtuous potential for humans, such as helping them to understand the meaning of life (Van Tongeren,

Green, Davis, Hook, & Hulsey, 2016), rather than just producing positive feelings. Therefore, people may enjoy more eudaimonic benefits than hedonic benefits from acting prosocially. The existing empirical literature has rarely compared the effect of prosociality on these two types of well-being directly. In one of the few studies that did the comparison, researchers adopted a structural equation model (path analysis) to demonstrate that volunteering status (i.e., yes or no) predicted eudaimonic well-being, but not hedonic well-being, though the number of volunteering hours made no difference (Son & Wilson, 2012). While there is a lack of studies comparing the effects of prosociality on eudaimonic and hedonic well-being directly, many studies have looked at one or the other; thus, with the help of meta-analytic techniques, we could examine the differences in the magnitude of the relationship between prosociality and eudaimonic and hedonic well-being.

Past research on the effects of prosociality has also investigated well-being in various ways, with some focusing on psychological functioning (e.g., self-esteem, self-efficacy, life satisfaction, happiness, and affect), some on psychological malfunctioning (e.g., depression, stress, hassle, and anxiety), and others on physical health (e.g., self-reported health, chronic health condition, exercise, and mortality). Although there are good theoretical bases for prosociality predicting well-being, the effects might vary across different aspects or categories of well-being. We reasoned that prosocial behavior predicted relatively greater psychological functioning, as such behavior can produce immediate positive emotion or reduce negative moods (Andreoni, 1989; Cialdini & Kenrick, 1976), thereby helping the individual to return to a better psychological state. Supporting our proposition, Aquino, Russell, Cutrona, and Altmaier (1996) reported that the correlations between volunteering hours and life satisfaction, depression, and physical health were .16, −.13, and .09, respectively. Similarly, in another study, Syu, Yu, Chen, and Chung (2013) found that the correlations between volunteering frequency and the subscales of subjective well-being ranged from .17 to .25, while the correlations between volunteering frequency and the subscales of depression ranged from −.04 to −.14.

There are, in fact, many different operationalizations of well-being (e.g., affect, subjective happiness). Unfortunately, not enough studies on each operationalization were available for us to perform separate meta-analyses. Therefore, to examine whether the effect of prosociality is different across various operationalizations of well-being, we focused on the moderation effects of two sets of theoretical categorizations, namely eudaimonic versus hedonic well-being; and psychological functioning, psychological malfunctioning, and physical health.

Demographic Moderators

Age. As people age, their social relationships, roles, and outlooks on life change, and in turn, their prosocial behavior changes (Wilson, 2000). For example, younger volunteers tend to focus on outcomes related to interpersonal relationships, whereas older volunteers tend to be concerned about service and community obligations (Omoto, Snyder, & Martino, 2000; Prouteau & Wolff, 2008). Perhaps driven by different factors, the well-being benefits of prosocial behavior vary as well at different ages. For instance, one study showed that the association between volunteering and depression was stronger for people aged 65 or above than those aged below 65 (Musick & Wilson, 2003). Similarly, other research has documented that older volunteers experienced greater improvements in life satisfaction and perceived health than their younger adult counterparts did (Van Willigen, 2000). Given these findings, the moderating effect of age on the magnitude of prosociality to well-being link is central, and thus included as a moderator in the present meta-analysis. Moreover, with the age variation in different studies, we could also investigate the possible moderating effect of age on eudaimonic well-being or hedonic well-being, which has been rarely documented.

Gender. There are more female (27.8%) than male volunteers (21.8%) across all age groups, educational levels, and other demographic characteristics in the United States (Bureau of Labor Statistics, U.S. Department of Labor, 2016). In addition, many studies have investigated the gender difference in helping behavior (Eagly & Steffen, 1986; Wilson, 2000, for reviews), highlighting the potential importance of gender in prosociality. Yet, when examining the effect of prosociality on well-being, many empirical studies only included gender as a covariate rather than as a moderator (Konrath, 2014). The different effects of prosociality on well-being for males and females are, thus, still unclear. Based on sex-typed social norms research (Witt & Wood, 2010; Wood, Christensen, Hebl, & Rothgerber, 1997), we reasoned that women, who are expected to be caring and intimate with others, would get more reward of good feelings and positive self-concept from prosocial behavior for acting in valued and norm-congruent ways. In one of the few studies investigating this topic, it is found that in the case of female adolescents only, general helping behavior was positively correlated with social relations, and family helping was correlated with better physical health (Schwartz, Keyl, Marcum, & Bode, 2009). Still, little work was done to investigate the gender differences in prosocial effects among adult samples.

Retirement. According to both role theory (Adelmann, 1994) and social integration theory (Durkheim, 1951), taking up meaningful social roles and having supportive social networks should foster well-being (Berkman, Glass, Brissette, & Seeman, 2000). As most retired people may have lost their productive role and probably their social networks, they may feel less useful to society. Baker, Cahalin, Gerst, and Burr (2005) found that, those who were made redundant or retired felt useful to others after engaging in productive activities that could improve well-being (i.e., life satisfaction, happiness, and reduced depressive symptoms). Volunteering and informal help were two examples of productive activities. Prosocial behavior, especially volunteering activities, is one of the few options for social engagement and helps to offset role loss from retirement. Thus, there are reasons to believe that retired people would enjoy a greater well-being boost from prosocial acts than nonretired people. We noticed that the well-being effects of prosociality on retired and nonretired people are rarely compared. In one of the few studies that did the comparison, Musick and Wilson (2003) found that volunteering lowered depression levels for those over 65, but not for those below 65. Still, the results could also be because of age difference. In view of this, we tested meta-analytically the moderating effect of retirement by including abundant retired and nonretired samples in the present research.

Methodological Moderators

Study quality. In the present meta-analysis, the quality of a study refers to the methodological rigor across the selected studies, which may moderate the strength of the effect sizes. Realizing the importance of addressing the quality of an individual study, researchers from other fields have developed quality assessment criteria and checklists (e.g., Coren & Fisher, 2006; Scottish Intercollegiate Guidelines Network, 2011). Yet, specific elements for assessing the quality of a study could vary from topic to topic (Valentine, 2009). Because the most commonly used effect size between prosociality and well-being is the correlation, *r*, and the main assessment tools are self-reported items or scales, we followed Cheng, Cheung, Chio, and Chan's (2013) practice and took into account two main sources of methodological issues: psychometric properties of instruments and sampling representativeness. In this study, however, we modified the way that quality evidence was weighted in each dimension, depending on the availability of the relevant evidence. If the psychometric properties of the two instruments were poor, they might have resulted in incorrect statistical inferences. For assessing the psychometric properties of instruments, we considered whether the validity of prosociality or well-being measures had been established, and whether the metric for the measurement reliability had been stated (Valentine, 2009). As for the issue of sampling representativeness, probability samples were preferable to nonprobability or convenience samples in terms of external validity, as results generated from probability samples were more likely to represent the population well. Taken together, study quality was determined by the validity and reliability of the measures, as well as the sample representativeness.

Data collection. Data analysis can occur at two levels: primary and secondary data analysis. The former refers to the original analysis in which the data is collected by a researcher who conducts a study for a specific purpose, while the latter is the reanalysis of data by someone other than the researcher to answer a new research question (Glass, 1976). To increase completeness, we have included data from primary research studies, as well as those from secondary analysis that were gathered for other purposes. For example, the Survey of Midlife Development in the United States and the European Social Survey were secondary data that were not originally designed and collected for answering questions related to prosociality and well-being. We reasoned that the effect size obtained from such secondary data sets might be weakened because of the less-than-optimal measures or nonspecific research design. Thus, one possibility is that the heterogeneity of effect sizes might be attributed to the type of data used—primary or secondary. We tested this possibility by including primary versus secondary data collection as a potential moderator.

Research design. Depending on the research questions, budget, time, ethics, availability of data, and other factors, researchers use different research designs for their studies. For the same research question, variations in research designs may result in different levels of validity and study findings (e.g., Schulz, Chalmers, Hayes, & Altman, 1995; Wilson & Lipsey, 2001). Thus, multimethod measurement is recommended in the psychology field (Eid & Diener, 2006). We believed that meta-analysis could help integrate empirical studies with different research designs and assess the validity and statistical significance of a proposed relationship across studies with the metaregression technique, thereby examining the possible heterogeneity of effect sizes because of different research designs. In the body of prosociality and well-being studies, we identified five major types of research designs: cross-sectional, longitudinal, diary or experience sampling, experimental, and volunteering program. A cross-sectional design collects data from a sample at only one specific time point. A longitudinal design measures the same variables at multiple time points, usually with a time interval of months, a year, or more than 1 year (e.g., Whillans, Dunn, Sandstrom, Dickerson, & Madden, 2016), while a diary (e.g., Weinstein & Ryan, 2010, Study 1) or experience sampling (e.g., Hui & Kogan, 2018) design has a shorter data collection period, usually 1 or 2 weeks, and a shorter time interval, usually daily or over several hours. Experimental studies are those involving random assignment and manipulation of prosociality—either one-off or multiple-time point (e.g., Alden & Trew, 2013). Finally, a volunteering program design refers to organized programs that people can join to participate in formal volunteering, such as Senior Companion and Foster Grandparent programs (Dulin, 2000) and Family Friends Program (Kuehne & Sears, 1993). There is no control group or random assignment in this program design, and various measures are used to tap participants' prosociality, including scales, length of volunteering time, and frequency of participation. The magnitude of the effect sizes in studies with different research designs is quite diverse. For example, some studies using a program design have reported most effect sizes of .30 or above (e.g., Kuehne & Sears, 1993; Yuen, 2003; Yuen, Huang, Burik, & Smith, 2008), while studies utilizing a longitudinal design have reported all effect sizes below .20 (e.g., Harris & Thoresen, 2005; McDougle, Handy, Konrath, & Walk, 2014; Shmotkin et al., 2003). In light of this, we included research design as a potential moderator in our analyses. Because experimental design and volunteering program design involve some kind of intervention, while other designs are just observational, we expected that studies with an experimental or a volunteering program design might have a stronger effect size for the link between prosociality and well-being than others.

Overview of the Present Meta-Analysis

Our goal with the present meta-analysis was twofold: (a) to establish the effect size of prosociality to different types of well-being, and (b) to examine how these relationships are moderated by a number of theoretical, demographic, and methodological variables. To do so, we structured our analyses into three broad components. First, we examined the relationship between prosociality and well-being. Specifically, we synthesized the results from empirical studies that had used a diverse set of methodologies and outcome types. In line with most of the past research on the topic (Curry et al., 2018; Wheeler et al., 1998), we expected a positive relationship between prosociality and well-being. Using meta-analytic techniques, we aimed to establish the weighted average effect size of the proposed link between prosociality and well-being.

After demonstrating the strength of the relationship, we meta-analytically tested a number of moderators that could possibly explain the heterogeneity of effect sizes derived from different independent studies. Using univariate models, we examined whether the theoretical moderators (i.e., types of prosociality), demographic moderators (i.e., retirement, age, and gender), and

methodological moderators (i.e., study quality, data collection, and research design) moderated the proposed link between prosociality and well-being independently. Then, we put the significant moderators from the univariate models into the multivariate models and tested for independent effects.

Finally, we tested whether prosociality had different effects on (a) eudaimonic well-being compared with hedonic well-being, and (b) psychological functioning, psychological malfunctioning, and physical health. These two hypotheses are rarely considered in primary studies but are testable meta-analytically. In addition, because the existing literature has elucidated the possible theoretical differences in well-being, we meta-analyzed the relationship between prosociality and well-being separately for eudaimonic well-being, hedonic well-being, psychological functioning, psychological malfunctioning, and physical health in both of the above univariate and multivariate models.

Method

Literature Search

To maximize the number of potentially relevant studies in our meta-analysis, we used a comprehensive searching strategy, which included electronic bibliographic database searches and manual methods (Lipsey & Wilson, 2001; Reed & Baxter, 2009).

We first conducted a search of five electronic bibliographic databases, namely Social Sciences Citation Index, PsycINFO, PsycARTICLES, PubMed, and ProQuest Dissertations and Theses. To search all potentially pertinent articles that examined the relationship between prosociality and well-being measures, we paired keywords of prosociality with those of well-being (see Table 1 for all keywords used). Wildcards were used when it was possible to include more pertinent studies. For example, a search using the term "happ*" may locate articles that involve "happy," "happiness," and "happily." In addition, we used both American and British spellings (e.g., behavior and behaviour). The search strategy was to include all the studies with at least one keyword of prosociality and at least one keyword of well-being. The initial database searches were conducted in April, 2014, with follow-up searches on December 14, 2016 and September 30, 2019. Thus, our list of papers is inclusive of studies published up to the time point of the last search. The searches together yielded a total of over 55,750 hits.

Apart from searching the electronic databases, we adopted manual methods for the greatest number of potential studies. Specifically, we looked for the references of all relevant research, review, and meta-analysis articles from our database searches (e.g., Aknin et al., 2013; Curry et al., 2018; Gottlieb & Gillespie, 2008; Konrath, 2014; Okun et al., 2013; Post, 2005; Wheeler et al., 1998). We also contacted researchers who, according to our electronic database searches, had at least three articles on prosociality and well-being published over the past decade (2009–2018). We solicited them to provide unpublished data, articles, in-press articles, and references related to the topic. We hoped that the inclusion of unpublished data (i.e., those that did not produce statistically significant results) could help address the file drawer problem and increase the validity of our meta-analysis (Lipsey & Wilson, 2001). In addition, we posted a call for published and unpublished data to the listserv of the Society for Personality and Social Psychology, the Altruism, Morality, and Social Solidarity Section of the American Sociological Association, and the Well-Being Institute.

Studies Included

After obtaining all potential articles using different searching methods detailed above, the first author screened all the titles and abstracts to evaluate whether they were empirical studies describing the link between prosociality and well-being. If a selection decision could not be made based on the information presented in the title and abstract, the full text of the article was further screened.

There were five criteria for the selection of studies in this meta-analysis: (1) Studies had to involve adult participants (i.e., aged 18 years and above). If a study involved participants of less than 18 years old who could not be separated from an adult sample, it would be discarded. (2) Studies were limited to those written in English. (3) Each study had to have at least one variable of prosociality and at least one variable of well-being. (4) Zero-order correlation/s (effect size/s) between variables in (3) had to be available. Studies were retained only if they reported effect size/s or sufficient information for the estimation of the effect size/s, or if the effect size/s could be obtained from the authors of the study via e-mail. (5) Studies that examined extraordinary altruism, such as kidney donation (e.g., Massey et al., 2010) and religious prosociality, were excluded, as the motives of extraordinary altruism are not comparable with those of ordinary prosocial behavior.

These selection criteria netted us 126 relevant articles. Among them, 110 are journal articles, and 16 are dissertations or theses. Because some articles involved more than one study with a unique sample, the present meta-analysis included 201 independent samples comprising a total of 198, 213 participants. Effect size/s from each independent study was extracted for analyses. Table S1 in online supplemental materials presents the main characteristics of independent samples included in the present meta-analysis.

Table 1
Keywords Used in the Five Electronic Bibliographic Databases

Prosociality	Well-being
Genero*, cooperat*, prosocial*, altruis*, kind*, prosocial behavior, prosocial behaviour, prosocial act*, altruistic act*, help*, volunteer*, donat*, give money, giving, assist*, benevolen*, charit*, altruistic spending, prosocial spending, dictator game, and empath*	Well-being, happ*, life satisfaction, satisfaction with life, quality of life, positive affect*, feeling good, positive feeling, positive emotion*, health*, pleasure, illness, depress*, and mental illness

Note. To broaden the scope of pertinent studies, wildcards (*) were used to locate word variants.

Coding Procedures and Reliability

To make sure that the data coding was consistent and reliable, we had two groups of coders to go through all articles. The first group comprised the 1st author who coded all of the articles, whereas the second group consisted of the 2nd, 3rd, and 4th authors, each of whom coded one-third of all articles. The four coders were trained for the use of the coding scheme and of the accompanying coding manual. After we were familiarized with the coding materials, we started practicing coding by selecting and coding several diverse articles. This made it possible for us to compare our coding approaches and discuss the difficulties encountered and any disagreements on coding. Following the training, the coders coded 10% of the independent studies on their own. Then, the data were checked by a third person who was blind to the coders' identities and research hypotheses. Any discrepancies in coding were resolved through discussion between the two groups of coders before we continued to code the rest of the articles.

By having two groups of coders reviewing all articles, we were able to identify most of the mistakes in the coding process (e.g., Cheng et al., 2013; Schmitt, Branscombe, Postmes, & Garcia, 2014). Upon finishing the coding, interrater reliability was assessed using Cohen's κ to ensure the accuracy and consistency of the coding data (Cohen, 1960). The reliabilities of all variables ranged from .85 to 1. Before moving onto analyses, a meeting was held with the entire meta-analysis team, during which we reviewed all discrepancies and made arbitration decisions on each one.

Effect Size Coding and Computation

We identified five types of prosociality measures: (1) frequency of volunteering/helping, (2) volunteering/helping or not, (3) prosociality scales, (4) amount of charitable donation or prosocial spending, and (5) affiliation to or membership of any voluntary associations or not. For experimental studies, we first identified the manipulated prosociality variable (e.g., acts of kindness vs. control condition, prosocial purchase vs. personal purchase, and prosocial spending recall vs. personal spending recall). Studies involving acts of kindness were coded as (2) volunteering/helping or not, while studies on prosocial purchase or recall were coded as (4) amount of charitable donation or prosocial spending. Having located the prosociality variables, we extracted effect sizes involving different aspects of well-being, such as self-esteem, self-efficacy, social connectedness, happiness, positive affect, negative affect, life satisfaction, vitality, subjective well-being, purpose in life, self-actualization, hopelessness, self-reported mental health, depression, stress, mortality, self-rated health, chronic medical conditions, and so on.

Because the present work aimed at examining the strength of the relationship between prosociality and well-being, we chose to use the Pearson correlation coefficient (r) effect sizes. If the effect size (r) was not reported in the study, coders would extract other statistical information, such as mean, standard deviation, t-value, and degrees of freedom for calculating the effect size (r). We also approached corresponding authors of the studies in which information was inadequate for subsequent coding. The data included in the present meta-analysis were confined to those obtained by November 17, 2019. When multiple prosociality or well-being measures were used in a study, apart from coding them one by one, we created a single composite effect size. Instead of averaging across the multiple effect sizes (Cooper, 1998; Lipsey & Wilson, 2001), we used Borenstein, Hedges, Higgins, and Rothstein's (2009) formula to combine effect sizes and variances, as the former approach assumes the correlation between outcome variables to be 1.0, and thus overestimates the variance and underestimates the precision. The Borenstein et al.'s approach, however, requires all correlations between outcome variables. Unfortunately, over 55% of the multiple-effect size studies in the present meta-analysis do not have outcome-outcome correlations—some do not even have all needed correlations in a single study (e.g., Aquino et al., 1996; Boenigk & Mayr, 2016). To solve the issue of unknown correlation, we extracted all available outcome correlations and applied the average correlation, which was .33, to Borenstein et al.'s formula.

In addition, effect sizes that are in the opposite direction to others in nature—including those between prosociality and negative affect, anxiety, stress, depression, psychological distress, number of chronic medical conditions, functional limitations, amount of pain experienced, and the like—were reversed so that they were in the same direction as the effect size of prosociality and other well-being variables, indicating the higher the levels of prosociality, the higher the levels of well-being.

Moderator Coding

Theoretical moderators.

Types of prosociality. Apart from coding the five measures of prosociality variables and the manipulated prosociality variable mentioned above, we also assessed the formality of prosociality—whether the prosocial behavior/helping was formal (coded as 0), informal (coded as 1), or mixed (coded as 2). The variable was coded as formal (0) if the prosocial or helping acts were done through a volunteer organization. Examples of this code included items "do you currently volunteer with any formally organized group," and "the number of hours serving in the Senior Companion and Foster Grandparent programs." The variable was coded as informal (1) if the prosocial or helping acts were spontaneous and unofficial. Examples of this included "how much instrumental support was provided to friends and neighbors over the past year," and "unpaid help to older adults." We treated charitable donations as formal, and the unplanned act of giving money to someone in need as informal. To minimize missing data because of binary classification, the variable was coded as mixed (2) if a measure involved items tapping both formal helping (e.g., "I have given money to a charity") and informal helping (e.g., "I have helped carry a stranger's belongings"; e.g., Gebauer et al., 2008).

Types of well-being. While there are many different categories of well-being indicators, it was not feasible to meta-analyze each of them, as it would greatly reduce the number of studies in a regression model, and in turn undermine the power of the model. Therefore, we created two sets of effect sizes based on a broad definition of well-being, while maintaining maximum sample size. The first and more philosophical set is eudaimonic well-being (coded as 0) and hedonic well-being (coded as 1). Examples of eudaimonic well-being variables included self-actualization, purpose of life, self-acceptance, personal growth, and flourishing, while examples of hedonic well-being variables included self-esteem, life satisfaction, happiness, social well-being, affect, and

psychological distress. The second and more general set is psychological functioning (coded as 0), psychological malfunctioning (coded as 1), and physical health (coded as 2). Psychological functioning is similar to hedonic well-being in a way that it encompasses all markers of positive well-being, while the markers of negative well-being or psychological distress (e.g., depression, stress, hassle, and anxiety) are labeled as psychological malfunctioning. Physical health included measures of subjective and objective health (e.g., self-reported health, chronic health condition, number of days not feeling well, exercise, and mortality). In the case where there was more than one type of well-being variable per independent study, the issue of dependence arose when we carried out moderation analyses. To tackle this, we randomly selected only one composite effect size of a type of well-being variable per study, so as to make sure that only a single effect size was generated per independent sample (Lipsey & Wilson, 2001).

Demographic moderators. In terms of the demographic moderators or age, gender, and retirement, we coded the mean age of participants (ranging from 18.10 to 83.79) and the percentage of females in each study (ranging from 0 to 100%). The retirement status of participants was coded as 0 (nonretired), 1 (retired), and 2 (a mix of retired and nonretired), thereby minimizing the loss of studies because of binary classification. If the information was not available, we would attempt to obtain it from the corresponding author. The variable was treated as missing if the author failed to provide the data.

Methodological moderators.

Study quality. Based on a modified version of the practice of Cheng et al. (2013), study quality was assessed by three codes of measurement reliability, measurement validity, and sample representativeness. To assess measurement reliability of the prosociality and well-being variables, a score of 1 was assigned to variables which reported an acceptable internal reliability of .70 or above (e.g., Cortina, 1993; Nunnally, 1978), a score of 0.5 was assigned to variables that reported an internal reliability of less than .70, and a score of 0 was assigned to variables which did not report any internal reliability. For experimental studies, a score of 1 was assigned to an independent variable (i.e., prosociality variable) when manipulation check was reported, while a score of 0 was assigned to an independent variable when manipulation check was not reported. The aggregated score of measure reliability for a study ranged from 0 to 2. To assess the measurement validity of the prosociality and well-being variables, a score of 1 was assigned to variables for which the validation source of the instrument was known, while a score of 0 was assigned to variables for which validation evidence was unknown. The aggregated score of measure validity for a study ranged from 0 to 2. To assess sample representativeness of an independent study, studies using probability sampling method were coded as 0, whereas studies using nonprobability or convenience sampling methods were coded as 1.

Data collection. Data in the independent studies of the present meta-analysis can be categorized into primary and secondary data. Primary data collected by the researchers who conducted the study was coded as 0, while secondary data collected by someone other than the researchers was coded as 1. To circumvent the issue of independence, we only included one study from each secondary dataset once. If two studies used the same secondary dataset with different sample sizes, apart from considering the availability of the relevant demographic data and statistical analyses, we selected only the study which was published first.

Research design. Different research paradigms were used in the selected studies. To capture this, we initially developed a code with nine types of research design including (1) cross-sectional design; (2) longitudinal design, with prosociality and well-being variables in different waves analyzed or well-being variable controlled in the previous wave; (3) longitudinal design, with prosociality and well-being variables analyzed in the same wave; (4) diary or experience sampling design, with prosociality and well-being variables in different time points analyzed or well-being variable controlled in the previous time point; (5) diary or experience sampling design, with average prosociality and well-being scores used; (6) one-off experimental design; (7) multiple-time point experimental design (daily or weekly), with post well-being score used; (8) multiple-time point experimental design (daily or weekly), with average well-being score used; and (9) volunteering program. Because of an extremely uneven number of studies in each category (ranging from 1 to 101), we collapsed (2) and (3) into longitudinal design, (4) and (5) into diary or experience sampling design, and (6), (7), and (8) into experimental design. Thus, our revised code for assessing research design was: cross-sectional (coded as 0), longitudinal (coded as 1), diary or experience sampling (coded as 2), experimental (coded as 3), and volunteering program (coded as 4).

Meta-Analytic Procedures

After extracting data from the articles, we conducted all analyses using the metaphor package (Viechtbauer, 2010) in the R statistical environment (R Core Team, 2013). There are two common models within meta-analyses: fixed-effects and random-effects models. The fixed-effects model assumes that the effect size heterogeneity is because of sample errors only, whereas the random-effects model assumes that such effect size heterogeneity is because of sample errors and other sources of random variability (Lipsey & Wilson, 2001). Because the independent samples are expected to be different in many ways—for example, prosociality and well-being were measured differently—the application of the random-effects model was more appropriate than that of the fixed-effects model. The random variance in the random-effects model was determined by maximum likelihood.

Before the analyses, the effect sizes were weighted to give more weight to reliable effect size calculations (Hedges & Olkin, 1985). This weighted mean effect size was computed by weighting each effect size (r) by the inverse of its variance, which could be calculated when the sample size of the independent study was available (Lipsey & Wilson, 2001). To examine the strength and directionality of the relationship between prosociality and well-being, we first conducted the analyses of the weighted mean effect size for each independent study. To correct for problems in standard error formulation that are common for this kind of effect size (Rosenthal, 1994), the original effect sizes (r) were under Fisher's Z transformation before analyses and then converted back to r for ease of interpretation. Cohen (1988) suggested correlations of .10, .30, and .50 to be small, medium, and large effects, respectively; and we used this conventional rule to interpret the magnitude of the effect sizes. Along with the weighted mean effect size esti-

mates, 95% confidence interval (CI) were reported to indicate the range within which the population means was likely to be (Lipsey & Wilson, 2001). If the CI does not include zero, the weighted mean effect size is statistically significant.

Next, we assessed whether the effect sizes from all selected studies varied with different study characteristics by using three indicators of between-studies variability in effect sizes: Q statistic, τ, and I^2. Q statistic is the most commonly used index for heterogeneity. However, it only informs us of the existence of heterogeneity and does not indicate the degree of such heterogeneity. Complementary to Q statistic, we also used τ and I^2. Tau provides an estimation of the standard deviation of the true effect sizes. I^2 tests whether the proportion of total variation in the true effect size estimates is because of heterogeneity rather than sample error. According to Higgins and Thompson (2002), percentages of around 25% ($I^2 = 25$), 50% ($I^2 = 50$), and 75% ($I^2 = 75$) indicate low, medium, and high heterogeneity, respectively. An I^2 of more than 50% indicates that it is worthwhile to search for study characteristics that can account for variation in effect sizes, rather than random sampling error.

Metaregression was used to test the moderation effect of the proposed variables. Using the univariate model, we reported the effect of each moderator independently. With the multivariate model, we then examined the effect of a particular moderator in a multiple regression-analog analysis after controlling for the effect of other moderators. We noted that multicollinearity might arise when there were many predictors in a multiple regression-analog model. Therefore, we only tested significant predictors in the univariate model with the multivariate model to minimize the possibility of multicollinearity.

Results

Overall Effect Size

We first examined the overall relationship between prosociality and well-being at the independent study level, and found that the weighted mean effect size across 201 studies ($N = 198{,}213$) was positive, $r = .13$, 95% CI [.12, .15], $z = 16.53$, $p < .001$. This effect size is generally considered small. However, the test of homogeneity was statistically significant, $Q(200) = 1654.72$, $p < .001$, indicating the effect sizes were heterogeneous. Moreover, τ equaled .10, and the value of I^2 was 95.12%, suggesting that a high degree of heterogeneity in the true effect size estimates was because of a nonsample error.

Moderators of the Overall Effect

The considerable heterogeneity in effect sizes among the samples supported our examination of potential moderators of the overall effect. We first used univariate models to examine the effect of each categorical and continuous moderator separately (see Table 2).

For categorical variables, we found that prosociality measures (i.e., volunteering/helping frequency, volunteering/helping or not, prosociality scale, charitable donation/prosocial spending, and membership in voluntary associations), the formality of prosociality (i.e., formal helping, informal helping, and mixed), and data collection (i.e., primary vs. secondary) moderated the overall relationship between prosociality and well-being independently, while research design (i.e., cross-sectional, longitudinal, diary/experience sampling, experimental, and volunteering program), sample representativeness (i.e., nonprobability sampling vs. probability sampling), and retirement (i.e., retired, nonretired, and mixed) were not significant moderators.

Specifically, concerning prosociality measures, the weighted mean effect size was the strongest ($r = .20$) when prosociality scales were used to operationalize prosociality, and the weakest ($r = .10$) when volunteering/helping frequency was asked. The effect sizes that were operationalized by volunteering/helping (yes/no), charitable donation or prosocial spending, and membership in voluntary associations were $r = .14$, $r = .14$, and $r = .12$, respectively. Pairwise comparisons among the weighted mean effect sizes for five classes showed that the weighted mean effect size for studies using prosociality scales was significantly larger than that for those using volunteering/helping frequency, and volunteering/helping (yes/no). No differences were found among other classes. For the formality of prosociality, the weighted mean effect size of formal helping was weaker ($r = .11$) when compared to that of informal helping ($r = .15$) and mixed helping ($r = .21$). No differences were found between informal helping and mixed helping. In terms of data collection, the weighted mean effect size for primary data was significantly larger ($r = .17$) than that for secondary data ($r = .09$).

Regarding continuous moderators, measurement reliability, measurement validity, and age, but not female percentage, moderated the link between prosociality and well-being independently. That is, the higher the measurement reliability and validity, as well as the younger the help-givers, the stronger was the weighted mean effect size. As an illustration, in studies with measurement reliability below the average, the weighted mean effect size was $r = .11$, whereas in studies with measurement reliability above the average, the effect size was $r = .17$. Similarly, in studies with measurement validity below the average, the weighted mean effect size was $r = .11$; in contrast, in studies with measurement validity above the average, the effect size was $r = .15$. Inversely, in studies with age of participants below the average, the weighted mean effect size was $r = .17$, whereas in studies with age of participants above the average, the effect size was $r = .11$.

To examine the simultaneous impact of variables, we ran a weighted regression-analog model by including all significant moderators in the above univariate moderation analysis. As shown in Table 3, while controlling for the influence of other variables, the only significant moderator in the model was data collection, $F = 13.73$, $p < .001$. The weighted mean effect size for studies using secondary data (vs. primary data) was found to be significantly weaker, $B = -.11$, 95% CI [$-.17$, $-.05$], $t = -3.71$, $p < .001$. The whole model explained 32% of the variance in the positive link between prosociality and well-being.

To sum up, in explaining the variations of the link between prosociality and well-being, prosociality measures, formality of prosociality, data collection, measurement reliability, measurement validity, and age of participants were found to be significant moderators in the univariate analysis, among which data collection played the strongest moderating role in the multivariate analysis (see Table 4 for a summary of results).

Table 2
Univariate Moderation Tests of Categorical and Continuous Variables for the Overall Relationship Between Prosociality and Well-Being

Moderator	r/B[a]	95% CI	z	k	Q_M	R^2
Categorical variables						
Prosociality measures				201	14.63**	.11
Volunteering/helping frequency	.10	[.07, .13]	7.25***	56		
Volunteering/helping or not	.14	[.11, .16]	10.89***	75		
Prosociality scale	.20	[.15, .24]	9.24***	27		
CD/PS	.14	[.10, .18]	6.59***	35		
MVA	.12	[.05, .19]	3.43***	8		
Formality of prosociality				187	12.99***	.08
Formal helping	.11	[.09, .13]	11.58***	111		
Informal helping	.15	[.12, .18]	9.51***	61		
Mixed	.21	[.15, .26]	7.44***	15		
Data collection				201	31.60***	.18
Primary	.17	[.15, .19]	16.93***	130		
Secondary	.09	[.06, .11]	7.77***	71		
Research design				201	4.84	.02
Cross-sectional	.13	[.11, .15]	12.52***	101		
Longitudinal	.10	[.06, .14]	4.87***	27		
Diary/experience sampling	.10	[.00, .20]	1.96*	7		
Experimental	.16	[.12, .19]	8.93***	55		
Volunteering program	.15	[.07, .22]	3.72***	11		
Sample representativeness				201	0.10	.00
Nonprobability sampling	.14	[.12, .15]	14.53***	154		
Probability sampling	.13	[.10, .16]	7.89***	47		
Retirement				174	2.86	.02
Yes	.18	[.12, .23]	6.56***	20		
No	.13	[.11, .16]	10.27***	88		
Mixed	.13	[.10, .15]	9.57***	66		
Continuous variables						
Measure reliability	.04	[.02, .06]	4.04***	201	16.28***	.12
Measure validity	.04	[.02, .06]	3.37***	201	11.34***	.08
Age	−.001	[−.002, −.000]	−2.60**	160	6.78**	.05
Female percentage	.04	[−.06, .15]	0.81	186	0.66	.01

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; r = correlation coefficient representing the weighted mean effect size; B = unstandardized coefficient; 95% CI = 95% confidence interval; z = z test statistic; k = number of independent studies; Q_M = Q statistic for test of moderators; R^2= amount of heterogeneity accounted for.
[a] r is used for categorical moderators, while B is used for continuous moderators.
* $p < .05$. ** $p < .01$. *** $p < .001$.

Eudaimonic Versus Hedonic Well-Being

To examine the moderation effect of different well-being types (i.e., eudaimonic vs. hedonic), we only included independent studies ($k = 197$) with either one or both types of well-being coded. If more than one type of well-being was coded in a single study, we randomly selected either one to ensure the independence of the sample. The weighted mean effect size of the relationship between prosociality and both eudaimonic and hedonic well-being was very similar to that of the overall effect, $r = .14$, 95% CI [.12, .16], $z = 15.78$, $p < .001$. The indicators for heterogeneity also supported the examination of the moderation effect, $Q(196) = 1701.50$, $p < .001$, $\tau = .11$, and $I^2 = 95.09\%$. We analyzed whether eudaimonic versus hedonic well-being moderated the link. The results showed that this coding (i.e., eudaimonic vs. hedonic) significantly moderated the weighted mean effect size for the link, $Q_M(1) = 12.08$, $p < .001$, $R^2 = .06$. The weighted mean effect sizes for the prosociality-eudaimonic well-being link and prosociality-hedonic well-being link were $r = .22$, 95% CI [.17, .26], $k = 25$, and $r = .13$, 95% CI [.11, .15], $k = 172$, respectively. The robustness of the results was further confirmed by a contemporary technique, three-level meta-analysis, which offers an alternative approach to address the issue of dependence among correlations within a single study (e.g., Konstantopoulos, 2011).[1]

Eudaimonic well-being. We examined eudaimonic and hedonic well-being separately. In all studies pertaining to eudaimonic well-being ($k = 49$), the weighted mean effect size of the relationship between prosociality and eudaimonic well-being was $r = .17$, 95% CI [.12, .21], $z = 7.87$, $p < .001$. The indicators for heterogeneity supported the examination of the moderation effect, $Q(48) = 441.62$, $p < .001$, $\tau = .14$, and $I^2 = 96.65\%$. Results of univariate moderation tests of categorical and continuous variables are shown in Table 5. We found that prosociality measures, the

[1] The omnibus test of the three-level model showed that eudaimonic versus hedonic well-being moderated the link, $F(1, 751) = 18.74$, $p < .001$. The weighted mean effect sizes for prosociality-eudaimonic well-being link and prosociality-hedonic well-being link were $r = .17$ and $r = .14$, respectively.

Table 3
Weighted Multiple Regression-Analog Model of Prosociality and Well-Being

Moderator	*B*	95% CI	*t*	F_M
Intercept	.22	[.12, .32]	4.27***	
Prosociality measures[a]				0.80
Volunteering/helping or not	.04	[−.00, .08]	1.78	
Prosociality scale	.03	[−.09, .15]	0.50	
CD/PS	.03	[−.06, .12]	0.70	
MVA	.02	[−.05, .09]	0.58	
Formality of prosociality[b]				2.53
Informal helping	−.03	[−.11, .04]	−0.98	
Mixed	.07	[−.02, .17]	1.57	
Data collection[c]				13.73***
Secondary	−.11	[−.17, −.05]	−3.71***	
Age	−.00	[−.00, .00]	−0.67	0.45
Measurement reliability	.02	[−.02, .06]	1.12	1.25
Measurement validity	−.06	[−.12, .00]	−1.90	3.61
$F(df1, df2)$		3.97 (10, 141)***		
$Q_E(df)$		951.19 (141)***		
R^2		.32		
k		152		

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; *B* = unstandardized coefficient; 95% CI = 95% confidence interval; *t* = *t* statistic for each moderator; *F* = omnibus test for the significance of whole model; F_M = omnibus test for a single moderator while controlling for the influence of others; $Q_E = Q$ statistic for test of residual heterogeneity; R^2 = amount of heterogeneity accounted for; *k* = number of independent studies in the model.
[a] Volunteering/helping frequency as the reference level. [b] Formal helping as the reference level. [c] Primary as the reference level.
*** $p < .001$.

formality of prosociality, data collection, measurement reliability, measurement validity, age, and percentage of female participants independently moderated the link between prosociality and eudaimonic well-being.

As far as prosociality measures are concerned, pairwise comparisons among the weighted mean effect sizes for the five classes showed that the weighted mean effect size for studies using prosociality scale ($r = .42$) or measuring charitable donation/prosocial spending ($r = .39$) was significantly larger than that for those using volunteering/helping frequency ($r = .11$), volunteering/helping (yes/no; $r = .19$), and membership in voluntary associations ($r = .10$). There was also a significant difference in the effect size between studies using volunteering/helping frequency and volunteering/helping or not. No differences were found among other classes. Regarding the formality of prosociality, the weighted mean effect size of formal helping ($r = .12$) or informal helping ($r = .20$) was significantly weaker than that of mixed helping ($r = .36$). No differences were found between formal helping and informal helping. As for data collection, the weighted mean effect size for primary data was significantly larger ($r = .28$) than that for secondary data ($r = .11$). It is noteworthy that the number of studies in some categories (e.g., charitable donation/prosocial spending and mixed helping) was small, thus, those results should be interpreted with caution.

All continuous predictors were found to be significant in the univariate moderation analysis (see Table 5). That is, the higher the measurement reliability, measurement validity, percentage of female participants, as well as the younger the help-givers, the stronger was the link between prosociality and eudaimonic well-being. As a demonstration, in studies with measurement reliability below the average, the weighted mean effect size was $r = .12$, while in studies with measurement reliability above the average, the weighted mean effect size was $r = .29$. Similarly, in studies with measurement validity below the average, the weighted mean effect size was $r = .11$; in contrast, in studies with measurement validity above the average, the weighted mean effect size was $r = .28$. Regarding gender effect, in studies with the percentage of female participants below the average, the weighted mean effect size was $r = .13$, whereas in studies with the percentage of female participants above the average, the effect size was $r = .23$. Inversely, in studies with age of participants below the average, the weighted mean effect size was $r = .32$, whereas in studies with age of participants above the average, the effect size was $r = .10$.

We then ran a weighted multiple regression-analog model for eudaimonic well-being. Results in Table 6 indicated that prosociality measures, measurement reliability, measurement validity, and age of participants significantly moderated the effect of prosociality on eudaimonic well-being, after controlling for the influence

Table 4
Summary Table of Univariate and Multivariate Moderation Models for All Well-Being Outcomes

	Overall well-being ($K = 201$)		Eudaimonic well-being ($k = 49$)		Hedonic well-being ($k = 197$)		Psychological functioning ($k = 188$)		Psychological malfunctioning ($k = 74$)		Physical health ($k = 83$)	
Moderator	UM	MM[a]	UM	MM[a]	UM	MM[a]	UM	MM[a]	UM	MM[a]	UM	MM[a]
Prosociality measures	✓	×	✓	✓	✓	×	✓	×	×		×	
Formality of prosociality	✓	×	✓	×	✓	✓	✓	×	×		×	
Age	✓	×	✓	✓	×		✓	×	×		✓	×
Female percentage	×		✓	×	×		×		✓	×	✓	✓
Retirement	×		×		×		×		×		✓	×
Measurement reliability	✓	×	✓	✓	✓	×	✓	×	×		×	
Measurement validity	✓	×	✓	✓	✓	×	✓	×	×		×	
Sample representativeness	×		×		×		×		×		×	
Data collection	✓	✓	✓	×	✓	✓	✓	✓	✓	×	×	
Research design	×		×		×		×		✓	✓	×	

Note. UM = univariate moderation; MM = multivariate moderation. ✓ = significant moderator; × = nonsignificant moderator.
[a] The multivariate moderation model only includes significant variables in the univariate moderation models.

Table 5
Univariate Moderation Tests of Categorical and Continuous Variables for the Relationship Between Prosociality and Eudaimonic Well-Being

Moderator	r/B[a]	95% CI	z	k	Q_M	R^2
Categorical variables						
Prosociality measures				49	86.4***	.80
Volunteering/helping frequency	.11	[.09, .14]	8.73***	29		
Volunteering/helping or not	.19	[.11, .27]	4.92***	7		
Prosociality scale	.42	[.36, .49]	12.54***	6		
CD/PS	.39	[.22, .57]	4.41***	2		
MVA	.10	[.04, .16]	−0.33**	5		
Formality of prosociality				47	28.21***	.55
Formal helping	.12	[.09, .15]	7.43***	35		
Informal helping	.20	[.11, .30]	4.06***	7		
Mixed	.36	[.27, .45]	8.05***	5		
Data collection				49	23.01***	.42
Primary	.28	[.22, .34]	9.33***	19		
Secondary	.11	[.07, .15]	5.56***	30		
Research design				49	3.42	.10
Cross-sectional	.16	[.12, .21]	7.35***	37		
Longitudinal	.12	[−.06, .31]	1.31	2		
Diary/experience sampling	.41	[.14, .69]	2.93**	1		
Experimental	.14	[−.00, .28]	1.92	6		
Volunteering program	.15	[−.03, .33]	1.67	3		
Sample representativeness				49	0.33	.00
Nonprobability sampling	.16	[.11, .21]	6.73***	38		
Probability sampling	.19	[.10, .28]	4.14***	11		
Retirement				47	2.82	.02
Yes	.36	[.12, .60]	2.98**	2		
No	.15	[.10, .20]	6.15***	32		
Mixed	.16	[.09, .24]	4.16***	13		
Continuous variables						
Measurement reliability	.10	[.06, .15]	5.04***	49	25.38***	.51
Measurement validity	.14	[.10, .18]	6.99***	49	48.85***	.62
Age	−.01	[−.01, −.01]	−7.51***	43	56.46***	.67
Female percentage	.52	[.08, .95]	2.34*	46	5.47*	.14

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; r = correlation coefficient representing the weighted mean effect size; B = unstandardized coefficient; 95% CI = 95% confidence interval; z = z test statistic; k = number of independent studies; Q_M = Q statistic for test of moderators; R^2 = amount of heterogeneity accounted for.
[a] r is used for categorical moderators, while B is used for continuous moderators.
* $p < .05$. ** $p < .01$. *** $p < .001$.

of other variables. All variables together explained 91% of the variance in the link between prosociality and eudaimonic well-being.

Hedonic well-being. Based on all studies on hedonic well-being ($k = 197$), the weighted mean effect size of the link between prosociality and hedonic well-being was $r = .14$, 95% CI [.12, .15], $z = 15.66$, $p < .001$. The indicators of heterogeneity suggested investigating moderation effect, $Q(196) = 1635.98$, $p < .001$, $\tau = .11$, and $I^2 = 95.09\%$. Results from univariate moderation tests of categorical and continuous variables can be found in Table 7, which summarizes that prosociality measures, the formality of prosociality, data collection, measurement reliability, and measurement validity were all independent moderators for the link between prosociality and hedonic well-being.

In particular, for prosociality measures, the weighted mean effect size was the strongest ($r = .20$) when prosociality scales were used to operationalize prosociality. It was significantly stronger than the effect sizes when operationalizing prosociality by asking about volunteering/helping frequency ($r = .10$) and whether volunteering/helping or not ($r = .14$). There was also a difference in effect size between volunteering/helping frequency and whether volunteering/helping or not. No differences were found among other classes. Concerning formality of prosociality, the weighted mean effect size of formal helping was weaker ($r = .11$) than that of informal helping ($r = .15$) and mixed helping ($r = .23$). There was also a significant difference in the effect size between informal helping and mixed helping. With regard to data collection, the weighted mean effect size for primary data ($r = .18$) was significantly larger than that for secondary data ($r = .09$).

As for the continuous moderators, we found that the higher the measurement reliability and measurement validity, the stronger was the link between prosociality and hedonic well-being. That is, for example, in studies with measurement reliability below the average, the weighted mean effect size was $r = .10$, whereas in studies with measurement reliability above the average, the effect size was $r = .18$. Likewise, in studies with measurement validity below the average, the weighted mean effect size was $r = .11$, whereas in studies with measurement validity above the average, the effect size was $r = .16$.

Table 6
Weighted Multiple Regression-Analog Model of Prosociality and Eudaimonic Well-Being

Moderator	*B*	95% CI	*t*	F_M
Intercept	.99	[.50, 1.47]	4.17***	
Prosociality measures[a]				3.14*
Volunteering/helping or not	−.19	[−.50, .11]	−1.28	
Prosociality scale	−.07	[−.34, .20]	−0.52	
CD/PS	−.29	[−.70, .13]	−1.42	
MVA	.12	[−.04, .21]	3.04**	
Formality of prosociality[b]				0.03
Informal helping	−.02	[−.27, .23]	−0.16	
Data collection[c]				3.19
Secondary	−.20	[−.43, .03]	−1.79	
Age	−.01	[−.02, −.01]	−4.39***	19.29***
Female percentage	−.09	[−.49, .30]	−0.48	0.23
Measurement reliability	−.14	[−.28, −.00]	−2.08*	4.34*
Measurement validity	.14	[.00, 0.29]	2.10*	4.40*
$F(df1, df2)$		11.13 (10, 30)***		
$Q_E(df)$		115.25 (30)***		
R^2		.91		
k		41		

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; *B* = unstandardized coefficient; 95% CI = 95% confidence interval; *t* = *t* statistic for each moderator; *F* = omnibus test for the significance of whole model; F_M = omnibus test for a single moderator while controlling for the influence of others; $Q_E = Q$ statistic for test of residual heterogeneity; R^2 = amount of heterogeneity accounted for; *k* = number of independent studies in the model.
[a] Volunteering/helping frequency as the reference level. [b] Formal helping as the reference level. [c] Primary as the reference level.
* $p < .05$. ** $p < .01$. *** $p < .001$.

Finally, Table 8 shows the results of a weighted multiple regression-analog model for hedonic well-being. Taking into account other variables, data collection was found to have moderated the link between prosociality and hedonic well-being, $F = 13.89$, $p < .001$, such that using secondary data (vs. primary data) significantly weakened the weighted mean effect size of the link, $B = -.09$, 95% CI [−.14, −.04], $t = -3.73$, $p < .001$. The formality of prosociality was also a significant moderator in the multivariate model, $F = 3.09$, $p = .048$, such that the weighted mean effect size for mixed helping was stronger, $B = .09$, 95% CI [.00, .17], $t = 2.03$, $p = .044$. A total of 34% of the variance was explained in the prosociality-hedonic well-being link.

In summary, the above results showed that there was a significant difference in the effect of prosociality on eudaimonic well-being and hedonic well-being. As summarized in Table 4, univariate moderators, including prosociality measures, the formality of prosociality, data collection, measurement reliability, and measurement validity, were found to be significant for the effect of prosociality on both eudaimonic well-being and hedonic well-being. The substantial differences were that only the link between prosociality and eudaimonic well-being was found to be moderated by age and percentage of female participants, such that studies with younger help-givers (vs. older help-givers), as well as studies with more females reported higher levels of eudaimonic well-being. In the multivariate models, while data collection and the formality of prosociality were the two significant moderators for the link between prosociality and hedonic well-being, there were several significant moderators for the link between prosociality and eudaimonic well-being, including prosociality measures, age, measurement reliability, and measure validity. However, because of the limited number of studies in the model, the results were not very conclusive, and thus, should be interpreted cautiously.

Psychological Functioning, Psychological Malfunctioning, and Physical Health

We next examined whether categorizing well-being as psychological functioning, psychological malfunctioning, and physical health made a substantial impact. These categories were derived by calculating the average composite score for each type of well-being in a single study. When there was more than one type of well-being in a study, we randomly selected only one single type. We had altogether 201 studies, and the weighted mean effect size was $r = .14$, 95% CI [.12, .16], $z = 13.73$, $p < .001$. To investigate the possibility of moderation, we looked at the indicators for heterogeneity: $Q(200) = 1990.96$, $p < .001$, $\tau = .13$, and $I^2 = 96.16\%$, which indicated a high degree of variability among effect sizes. Thus, we tested the moderation effect of this coding, and found that the types of well-being (psychological functioning vs. psychological malfunctioning vs. physical health) significantly moderated the weighted mean effect size for the link, $Q_M(2) = 12.38$, $p = .002$, $R^2 = .07$. The weighted mean effect sizes for psychological functioning, psychological malfunctioning, and physical health were $r = .17$, 95% CI [.14, .19], $z = 13.07$, $p < .001$, $k = 126$, $r = .11$, 95% CI [.07, .15], $z = 5.19$, $p < .001$, $k = 39$, and $r = .09$, 95% CI [.04, .13], $z = 3.80$, $p < .001$, $k = 36$, respectively. Pairwise comparisons for the three types of well-being showed that the weighted mean effect size for psychological functioning was significantly larger than that of psychological malfunctioning ($p = .023$), and physical health ($p = .002$), while there was no significant difference in the weighted mean effect sizes between psychological malfunctioning and physical health ($p = .409$). The robustness of the results was further confirmed by the three-level meta-analysis.[2]

Psychological functioning. We analyzed the three types of well-being separately. Studies on any of the three types of well-being or more were included. For studies on psychological functioning ($k = 188$), the weighted mean effect size of the link between prosociality and psychological functioning was $r = .14$, 95% CI [.13, .16], $z = 16.19$, $p < .001$. The indicators for heterogeneity were $Q(187) = 1628.55$, $p < .001$, $\tau = .11$, and $I^2 = 95.02\%$, revealing the possibility of moderation effect. Results of univariate moderation analyses of categorical and continuous variables are shown in Table 9. We found that prosociality measures, the formality of prosociality, data collection, measurement reliability, measurement validity, and age of participants independently moderated the link between prosociality and psychological functioning.

[2] The omnibus test of the three-level model showed that well-being types (psychological functioning versus psychological malfunctioning versus physical health moderated the link, $F(2, 915) = 8.17$, $p < .001$). The weighted mean effect size for psychological functioning, $r = .14$, was significantly larger than that of psychological malfunctioning, $r = .12$, $t = 2.78$, $p = .006$, and physical health, $r = .12$, $t = 3.45$, $p < .001$, while there was no significant difference in the weighted mean effect sizes between psychological malfunctioning and physical health, $t = 0.34$, $p = .735$.

Table 7
Univariate Moderation Tests of Categorical and Continuous Variables for the Relationship Between Prosociality and Hedonic Well-Being

Moderator	r/B[a]	95% CI	z	k	Q_M	R^2
Categorical variables						
Prosociality measures				197	14.27**	.11
Volunteering/helping frequency	0.1	[.07, .13]	6.67***	55		
Volunteering/helping or not	0.14	[.12, .17]	10.62***	75		
Prosociality scale	0.2	[.15, .24]	8.75***	27		
CD/PS	0.14	[.09, .18]	5.92***	32		
MVA	0.13	[.06, .21]	3.57***	8		
Formality of prosociality				183	14.16***	.10
Formal helping	0.11	[.09, .13]	11.31***	110		
Informal helping	0.15	[.12, .18]	9.12***	59		
Mixed	0.23	[.17, .29]	7.42***	14		
Data collection				197	31.31***	.19
Primary	0.18	[.15, .20]	16.25***	128		
Secondary	0.09	[.06, .11]	7.13***	69		
Research design				196	4.27	.02
Cross-sectional	0.14	[.11, .16]	12.09***	101		
Longitudinal	0.1	[.06, .15]	4.54***	25		
Diary/experience sampling	0.09	[−.02, .19]	1.66	6		
Experimental	0.16	[.12, .19]	8.34***	53		
Volunteering program	0.15	[.07, .23]	3.6***	11		
Sample representativeness				197	0.02	.00
Nonprobability sampling	0.14	[.12, .16]	13.71***	151		
Probability sampling	0.13	[.10, .17]	7.56***	46		
Retirement				170	3.47	.02
Yes	0.19	[.13, .24]	6.47***	20		
No	0.13	[.10, .15]	9.35***	88		
Mixed	0.13	[.11, .16]	9.22***	62		
Continuous variables						
Measurement reliability	0.04	[.02, .06]	4.16***	197	17.27***	.13
Measurement validity	0.04	[.01, .06]	2.95**	197	8.71**	.07
Age	−.001	[−.002, .00]	−1.81	156	3.29	.03
Female percentage	0.03	[−.09, .14]	0.44	182	0.2	.01

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; r = correlation coefficient representing the weighted mean effect size; B = unstandardized coefficient; 95% CI = 95% confidence interval; z = z test statistic; k = number of independent studies; Q_M = Q statistic for test of moderators; R^2 = amount of heterogeneity accounted for.
[a] r is used for categorical moderators, while B is used for continuous moderators.
** $p < .01$. *** $p < .001$.

In particular, for prosociality measures, the weighted mean effect size pertaining to prosociality scales ($r = .23$) was significantly stronger than those involving volunteering/helping frequency ($r = .12$), whether volunteering/helping or not ($r = .14$), charitable donation or prosocial spending ($r = .14$), and membership in voluntary associations ($r = .13$). No differences were found among other classes. In regards to the formality of prosociality, the weighted mean effect size in relation to informal helping ($r = .15$) or mixed helping ($r = .24$) was significantly stronger than that in relation to formal helping ($r = .12$). There was also a significant difference in the effect size between informal helping and mixed helping. Concerning data collection, the weighted mean effect size for studies on primary data ($r = .18$) was significantly larger than that for studies on secondary data ($r = .09$).

For the continuous predictors, the higher the measurement reliability and validity, as well as the younger the help-givers, the stronger was the link between prosociality and psychological functioning. As an illustration, the weighted mean effect size for studies with measurement reliability below the average was $r = .11$, whereas that for studies with measurement reliability above the average was $r = .18$. Likewise, the weighted mean effect size for studies with measurement validity below the average was $r = .11$, whereas that for studies with measurement validity above the average was $r = .16$. Conversely, the weighted mean effect size in studies with age of participants below the average was $r = .18$, whereas that in studies with age of participants above the average was $r = .12$.

In a similar fashion, we ran a weighted multiple regression-analog model for psychological functioning (see Table 10). After including all significant univariate moderators, we found that only data collection played a significant moderating role for the effect of prosociality on psychological functioning, $F = 9.50$, $p = .003$. The weighted mean effect size for studies using secondary data (vs. primary data) was found to be significantly weaker, $B = -.10$, 95% CI [−.16, −.04], $t = -3.08$, $p < .001$. Overall, the multivariate model explained 41% of the variance in the link between prosociality and psychological functioning.

Psychological malfunctioning. For psychological malfunctioning ($k = 74$), the weighted mean effect size of the link (reversed) between prosociality and psychological malfunctioning

Table 8
Weighted Multiple Regression-Analog Model of Prosociality and Hedonic Well-Being

Moderator	*B*	95% CI	*t*	F_M
Intercept	.17	[.11, .23]	5.62***	
Prosociality measures[a]				0.79
Volunteering/helping or not	.03	[−.01, .07]	1.52	
Prosociality scale	−.00	[−.10, .10]	−0.00	
CD/PS	−.01	[−.07, .07]	−0.05	
MVA	.04	[−.04, .11]	0.96	
Formality of prosociality[b]				3.09*
Informal helping	−.02	[−.08, .04]	−0.69	
Mixed	.09	[.00, .17]	2.03*	
Data collection[c]				13.89***
Secondary	−.09	[−.14, −.04]	−3.73***	
Measurement reliability	.02	[−.01, .06]	1.25	1.55
Measurement validity	−.03	[−.08, .01]	−1.38	1.91
F(*df*1, *df*2)		4.74 (9, 173)***		
Q_E(*df*)		991.58 (173)***		
R^2		.34		
k		183		

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; *B* = unstandardized coefficient; 95% CI = 95% confidence interval; *t* = *t* statistic for each moderator; *F* = omnibus test for the significance of whole model; F_M = omnibus test for a single moderator while controlling for the influence of others; Q_E = *Q* statistic for test of residual heterogeneity; R^2 = amount of heterogeneity accounted for; *k* = number of independent studies in the model.
[a] Volunteering/helping frequency as the reference level. [b] Formal helping as the reference level. [c] Primary as the reference level.
* $p < .05$. *** $p < .001$.

was $r = .10$, 95% CI [.07, .14], $z = 5.98$, $p < .001$. The indicators for heterogeneity supported the examination of the moderation effect, $Q(73) = 835.74$, $p < .001$, $\tau = .14$, and $I^2 = 97.14\%$. As shown in Table 11, univariate moderation analyses of categorical and continuous variables indicated that data collection, research design, and female percentage independently moderated the link between prosociality and psychological malfunctioning. Looking into data collection, the weighted mean effect size for studies using primary data ($r = .17$) was significantly larger than that for studies using secondary data ($r = .07$). As for research design, the weighted mean effect size in experimental studies ($r = .32$) was significantly larger than that in cross-sectional studies ($r = .08$), and longitudinal studies ($r = .09$). No differences were found among other classes. As an illustration of gender effect, in studies with the female percentage below the average, the weighted mean effect size was $r = .07$, whereas in studies with the female percentage above the average, the effect size was $r = .15$.

We then analyzed the effect of each significant univariate moderator in a multivariate regression-analog model for the prosociality-psychological malfunctioning link. As presented in Table 12, research design still played a moderating role after controlling for the effects of other moderators, $F = 3.61$, $p = .011$, such that experimental studies resulted in a greater weighted mean effect size, $B = .29$, 95% CI [.14, .45], $t = 3.72$, $p < .001$. The whole set of moderators accounted for 37% of the variance in the effect sizes.

Physical health. For physical health ($k = 83$), the weighted mean effect size of the link between prosociality and physical health was $r = .09$, 95% CI [.07, .10], $z = 15.48$, $p < .001$. The indicators for heterogeneity supported the examination of the moderation effect, $Q(82) = 313.57$, $p < .001$, $\tau = .04$, and $I^2 = 75.17\%$. Statistics from Table 13 show that retirement, age, and percentage of female participants in each study moderated the link independently.

More specifically, the weighted mean effect size in studies with retired help-givers ($r = .13$) was stronger than that in studies with nonretired help-givers ($r = .07$) or a mix of retired and nonretired help-givers ($r = .09$). No differences in effect size were found between the nonretired and mixed help-givers.

For the continuous predictors, the older the help-givers, as well as the higher the percentage of female participants, the stronger was the link between prosociality and physical health. As an illustration, in studies with participant age below the average, the weighted mean effect size was $r = .08$, whereas in studies with participant age above the average, the effect size was $r = .10$. Also, the weighted mean effect size in studies with the female percentage below the average was $r = .08$, as compared with the effect size of $r = .10$ in studies with the female percentage above the average.

Finally, the results of weighted multiple regression-analog models for physical health are presented in Table 14. After controlling for the influence of other variables, female percentage still positively predicted the effect of prosociality on physical health, $B = .25$, 95% CI [.06, .45], $t = 2.62$, $p = .011$. The multivariate model accounted for 34% of the variance.

In short, the above results revealed significant differences in the effect size between the prosociality-psychological functioning link and the prosociality-psychological malfunctioning link or the prosociality-physical health link. The results in relation to psychological functioning were in line with those for overall well-being, hedonic well-being, and eudaimonic well-being. Substantial evidence showed that prosociality measures, the formality of prosociality, measurement reliability, measurement validity, and data collection moderated the link between prosociality and psychological functioning (see Table 4 for comparison). Age was a significant univariate moderator in models pertaining to psychological functioning and physical health, but the predictions were in opposite directions—younger help-givers reported higher levels of psychological functioning, while older help-givers reported higher levels of physical health. Consistent with the result from the eudaimonic well-being model, female percentage significantly moderated the prosociality links pertaining to psychological malfunctioning and physical health. That is, prosociality had higher levels of recuperative/beneficial effect on psychological malfunctioning/physical health for female help-givers. Across all models, retirement revealed a significant moderation effect in the model concerning physical health only, such that retired or mixed (retired and nonretired) help-givers reported higher levels of physical health from prosociality than nonretired help-givers did.

Publication Bias

Despite efforts to include all available unpublished data, the results of our meta-analysis might still be confounded by publication bias. We adopted three traditional and recent approaches to examine any possible publication bias. We conducted publication bias analyses on all data and subsets of various well-being types. Given the significant difference in the weighted mean effect size

Table 9
Univariate Moderation Tests of Categorical and Continuous Variables for the Relationship Between Prosociality and Psychological Functioning

Moderator	r/B[a]	95% CI	z	k	Q_M	R^2
Categorical variables						
Prosociality measures				188	18.77***	.18
Volunteering/helping frequency	.12	[.09, .14]	8.07***	53		
Volunteering/helping or not	.14	[.11, .16]	10.29***	72		
Prosociality scale	.23	[.19, .28]	9.91***	23		
CD/PS	.14	[.09, .18]	6.15***	32		
MVA	.13	[.06, .20]	3.62***	8		
Formality of prosociality				178	17.45***	.14
Formal helping	.12	[.10, .13]	11.86***	108		
Informal helping	.15	[.12, .19]	9.51***	57		
Mixed	.24	[.18, .30]	7.92***	13		
Data collection				188	32.38***	.21
Primary	.18	[.16, .20]	16.85***	122		
Secondary	.09	[.07, .11]	7.7***	66		
Research design				188	2.65	.01
Cross-sectional	.15	[.13, .17]	13.03***	97		
Longitudinal	.11	[.06, .16]	4.38***	21		
Diary/experience sampling	.10	[−.00, .21]	1.9	6		
Experimental	.15	[.11, .19]	7.89***	53		
Volunteering program	.15	[.07, .23]	3.52***	11		
Sample representativeness				188	0.07	.00
Nonprobability sampling	.14	[.12, .16]	14.02***	145		
Probability sampling	.15	[.11, .18]	8.11***	43		
Retirement				162	3.21	.02
Yes	.19	[.13, .26]	6.34***	18		
No	.14	[.11, .16]	10.13***	86		
Mixed	.14	[.11, .17]	9.41***	58		
Continuous variables						
Measurement reliability	.04	[.02, .06]	4.32***	188	18.64***	.16
Measurement validity	.04	[.02, .07]	3.87***	188	14.95***	.13
Age	−.001	[−.002, −.000]	−2.32*	151	5.39*	.05
Female percentage	.03	[−.10, .16]	0.67	175	0.18	.01

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; r = correlation coefficient representing the weighted mean effect size; B = unstandardized coefficient; 95% CI = 95% confidence interval; z = z test statistic; k = number of independent studies; Q_M = Q statistic for test of moderators; R^2 = amount of heterogeneity accounted for.
[a] r is used for categorical moderators, while B is used for continuous moderators.
* $p < .05$. *** $p < .001$.

between primary and secondary data across most of the models, we also conducted publication bias analyses on these two subsets to examine whether any potential bias had affected the results.

First, we utilized funnel plots to examine effect sizes against precision (in our case, Fisher's z transformed correlation Coefficients × Standard Errors). As shown in Figures 1 to 4, the distributions in the funnel plots seemed to be roughly asymmetric. We then used a traditional and relatively more quantitative trim-and-fill approach to assess symmetry in the funnel plots and adjust for any bias (Duval & Tweedie, 2000). Results in Table 15 showed that no studies were added to the estimated mean effect sizes, except for the subset of hedonic well-being (with 29 effect sizes added). No mean effect sizes were adjusted, except for that in the subset of hedonic well-being which was decreased by .03. Taken together, these findings indicated minimal evidence of publication bias.

Second, we further examined the symmetry of funnel plots using the Egger's regression intercept (Egger, Davey Smith, Schneider, & Minder, 1997). As shown in Table 15, the Egger's regression intercepts were significant for six of the eight effect sizes, but not so in the subsets of physical health ($z = 0.61$, $p = .541$) and secondary data ($z = 0.59$, $p = .561$). Contrary to the findings from the trim-and-fill approach, the Egger's regression approach indicated possible bias in the data.

Lastly, we used the more recent approach of selection methods to assess and adjust for publication bias (Vevea & Woods, 2005), whereby we specified four different weight functions and then estimated the adjusted mean effect size under each of them. We ran models for the four weight functions, namely *moderate one-tailed selection* (i.e., assuming almost all significant findings in the expected direction and half of the nonsignificant findings in the opposite direction were included), *severe one-tailed selection* (i.e., assuming almost all nonsignificant findings in the opposite direction were included), *moderate two-tailed selection* (i.e., assuming 60% of the nonsignificant findings in either direction were included), and *severe two-tailed selection* (i.e., assuming 25% of the nonsignificant findings in either direction were included). The results are summarized in Table 15. The adjusted mean effect sizes estimated by the models of moderate one-tailed selection, moder-

Table 10
Weighted Multiple Regression-Analog Model of Prosociality and Psychological Functioning

Moderator	*B*	95% CI	*t*	F_M
Intercept	.17	[.06, .27]	3.21**	
Prosociality measures[a]				0.25
Volunteering/helping or not	.00	[−.04, .06]	0.49	
Prosociality scale	−.01	[−.13, .10]	−0.23	
CD/PS	−.03	[−.12, .07]	−0.53	
MVA	.02	[−.05, .09]	0.57	
Formality of prosociality[b]				2.48
Informal helping	−.00	[−.08, .07]	−0.10	
Mixed	.11	[.01, .22]	2.10*	
Data collection[c]				9.50**
Secondary	−.10	[−.16, −.04]	−3.08***	
Age	.00	[−.00, .00]	0.47	0.22
Measurement reliability	.03	[−.02, .07]	1.20	1.44
Measurement validity	−.03	[−.09, .03]	−1.11	1.22
$F(df1, df2)$		3.86 (10, 134)***		
$Q_E(df)$		782.83 (134)***		
R^2		.38		
k		145		

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; *B* = unstandardized coefficient; 95% CI = 95% confidence interval; *t* = *t* statistic for each moderator; *F* = omnibus test for the significance of whole model; F_M = omnibus test for a single moderator while controlling for the influence of others; $Q_E = Q$ statistic for test of residual heterogeneity; R^2 = amount of heterogeneity accounted for; *k* = number of independent studies in the model.
[a] Volunteering/helping frequency as the reference level. [b] Formal helping as the reference level. [c] Primary as the reference level.
* $p < .05$. ** $p < .01$. *** $p < .001$.

ate two-tailed selection, and severe two-tailed selection were similar to the unadjusted mean effect sizes from the main analyses—the largest difference was .016 in average, without changes in the effect direction. However, the model of severe one-tailed selection estimated much smaller effect sizes than the unadjusted ones. Among which, a change of direction was observed in the subset concerning psychological malfunctioning.

By and large, the three approaches suggested different degrees of potential publication bias in our data. By reporting multiple publication bias analyses, we sought to present a real current state of the empirical data in the field and interpret our results cautiously.

Discussion

Our analyses aimed to tackle two core questions: (a) is prosociality related to well-being, and (b) what are the factors that moderate this link? We organized our discussion below into core conclusions to answer these two motivating questions.

Prosociality Generally Has a Weak, Positive Association With Well-Being

Using empirical studies from the past several decades, this is the first meta-analysis to provide a summary estimate of the hypothesized relationship between prosociality and well-being on such a large scale. Our results were consistent with the hypothesis that, in general, higher levels of prosociality are related to higher levels of well-being. However, the link was generally weak-to-moderate in magnitude—comparable with a recent meta-analysis based on experimental studies (Curry et al., 2018). Ultimately, this outcome can provide ammunition to those who are skeptical of the impact of prosociality on the well-being of the giver (Dovidio, Piliavin, Schroeder, & Penner, 2006) and those who champion the link as the bedrock of human nature (e.g., Andreoni, 1989; Cialdini & Kenrick, 1976; Danner et al., 2007; Krause et al., 1992; Midlarsky, 1991; Midlarsky & Kahana, 2007; Schwartz & Sendor, 1999).

A major issue emerged from our work is whether the modest effect size of $r = .13$ between prosociality and well-being is of societal concern. There are several reasons to answer in the affirmative and understand why the relationship was not stronger. First, the modest effect size was not surprising; after all, well-being is multidetermined, and prosocial action is merely one of the many sources that may contribute to it (Lyubomirsky et al., 2005). Indeed, there are theoretical and empirical reasons to suspect that other social actions have a stronger influence on well-being than prosociality, such as successful goal striving (Klug & Maier, 2015) and leisure engagement (Kuykendall, Tay, & Ng, 2015). However, a modest effect size between prosociality and well-being can still have a significant impact at a societal level. The pervasiveness of volunteering has been well-documented in America and European countries. Research from other fields has suggested that small effect sizes could still translate into substantial societal significance when many people or the same persons were affected repeatedly (Greenwald, Banaji, & Nosek, 2015), and when only a little and inexpensive effort is needed to possibly make a change, which can be cumulative over time (Coe, 2002; Funder & Ozer, 2019). Another point worth noting is that, as we discussed below, the modest overall effect size hid the substantial heterogeneity across different theoretical, demographic, and methodological moderators. In some cases, the effect size was substantially larger than the overall average. Thus, we suggest that while there may not be a strong association between prosociality and well-being, there may be a valuable path toward well-being enhancement and sustainability.

There Is Significant Moderation of the Prosociality and Well-Being Link

We found that the effect size of the relationship between prosociality and well-being was modest, and that effect had a high level of heterogeneity. This was suggestive that some other variables might moderate the relationship. In fact, we found numerous theoretical, demographic, and methodological moderators affecting the basic relationship between prosociality and well-being across our models.

Theoretical moderators.

Givers experienced more eudaimonic well-being than hedonic well-being. Apart from demonstrating the magnitude of the relationship between prosociality and well-being in general, we also examined whether the effect of prosociality was different for various types of well-being. Although the lack of unified definition and limitations of operationalization concerning eudaimonic well-being might cause problems when drawing a line between eudaimonic well-being and hedonic well-being (see Kashdan, Biswas-Diener, & King, 2008), we previously illustrated that eudaimonic well-being, which pertains to actualizing one's human potential, is

Table 11
Univariate Moderation Tests of Categorical and Continuous Variables for the Relationship Between Prosociality and Psychological Malfunctioning

Moderator	r/B[b]	95% CI	z	k	Q_M	R^2
Categorical variables						
Prosociality measures[a]				74	3.64	.05
Volunteering/helping frequency	.07	[.03, .12]	3.08**	35		
Volunteering/helping or not	.14	[.08, .20]	5.25***	30		
Prosociality scale	.09	[−.02, .21]	1.63	7		
MVA	.11	[−.09, .30]	1.08	2		
Formality of prosociality				69	0.12	.00
Formal helping	.11	[.07, .13]	5.29	55		
Informal helping	.09	[−.01, .20]	1.83	11		
Mixed	.13	[−.06, .32]	1.36	3		
Data collection				74	7.18**	.09
Primary	.17	[.11, .22]	5.78***	29		
Secondary	.07	[.03, .11]	3.54***	45		
Research design				73	16.46**	.26
Cross-sectional	.08	[.05, .12]	4.57***	51		
Longitudinal	.09	[.01, .16]	2.17*	10		
Diary/experience sampling	.08	[−.20, .36]	0.54	1		
Experimental	.32	[.21, .43]	5.68***	8		
Volunteering program	.12	[−.07, .31]	1.22	3		
Sample representativeness				74	0.00	.00
Nonprobability sampling	.10	[.07, .14]	5.53***	62		
Probability sampling	.10	[.01, .19]	2.27**	12		
Retirement				69	3.79	.04
Yes	.20	[.09, .31]	3.69***	9		
No	.08	[.03, .13]	3.16**	32		
Mixed	.10	[.05, .16]	3.65***	28		
Continuous variables						
Measurement reliability	.04	[−.02, .09]	1.27	74	1.62	.03
Measurement validity	−.04	[−.16, .09]	−0.55	74	0.30	.00
Age	−.002	[−.004, .001]	−1.21	66	1.46	.03
Female percentage	.24	[.01, .48]	2.06*	69	4.25*	.07

Note. MVA = membership in voluntary associations; r = correlation coefficient representing the weighted mean effect size; B = unstandardized coefficient; 95% CI = 95% confidence interval; z = z test statistic; k = number of independent studies; $Q_M = Q$ statistic for test of moderators; R^2 = amount of heterogeneity accounted for.
[a] No charitable donation or prosocial spending class in prosociality measures. [b] r is used for categorical moderators, while B is used for continuous moderators.
* $p < .05$. ** $p < .01$. *** $p < .001$.

distinct from hedonic well-being, which emphasizes one's subjective feelings about his or her life. In other words, eudaimonic well-being is not an outcome, but a process of realizing one's daimon or true nature (Deci & Ryan, 2008). We also suggested meta-analysis is probably one of the best research tools to meta-analyze the markers of eudaimonic and hedonic well-being and compare whether prosociality contributes to the markers of eudaimonic and hedonic well-being differently, based on the data in the entire field. Our results revealed that prosociality linked differently to eudaimonic and hedonic well-being ($p < .001$), such that givers extracted more eudaimonic than hedonic well-being from prosociality.

Past research has shown that people's conceptions of well-being and different components of well-being, including both eudaimonic and hedonic ones, vary across different life stages (see Diener & Lucas, 2000; Ryff, 1989, 1991). Following this reasoning, we expected that eudaimonic or hedonic well-being benefits from prosocial behavior vary as a function of certain fundamental individual differences, such as age and gender. Consistent with our reasoning, we found that the prosociality-eudaimonic well-being link was moderated by some unique variables of individual differences, such as age and percentage of female participants. In particular, our results showed that studies with younger participants and a higher percentage of female participants had stronger effect sizes of eudaimonic well-being. On the other hand, some moderation patterns—prosociality measures, the formality of prosociality, data collection, and study quality (i.e., measurement reliability and measurement validity)—were found to be similar for both well-being types.

Prosociality was more strongly related to psychological functioning than to psychological malfunctioning and physical health. We categorized well-being into psychological functioning, psychological malfunctioning, and physical health. In line with our predictions, the results revealed that the link between prosociality and psychological functioning was the strongest among the three categories, with the link between the other two categories and prosociality being similar. In other words, it is possible that prosocial behavior is relatively more effective in bringing about better psychological functioning than improving

Table 12
Weighted Multiple Regression-Analog Model of Prosociality and Psychological Malfunctioning

Moderator	*B*	95% CI	*t*	F_M
Intercept	.05	[−.13, .23]	0.60	
Data collection[a]				0.19
Secondary	−.02	[−.11, .07]	−0.44	
Research design[b]				3.61*
Longitudinal	.01	[−.08, .10]	0.15	
Diary/experience sampling	−.01	[−.32, .30]	−0.06	
Experimental	.29	[.14, .45]	3.72***	
Volunteering program	.02	[−.21, .24]	0.14	
Female percentage	.07	[−.18, .32]	0.52	0.27
$F(df1, df2)$		3.62 (6, 61)**		
$Q_E(df)$		510.30 (61)***		
R^2		.37		
k		68		

Note. *B* = unstandardized coefficient; 95% CI = 95% confidence interval; *t* = *t* statistic for each moderator; *F* = omnibus test for the significance of whole model; F_M = omnibus test for a single moderator while controlling for the influence of others; Q_E = *Q* statistic for test of residual heterogeneity; R^2 = amount of heterogeneity accounted for; *k* = number of independent studies in the model.
[a] Primary as the reference level. [b] Cross-sectional as the reference level.
* $p < .05$. ** $p < .01$. *** $p < .001$.

psychological malfunctioning or physical health. While givers can immediately receive a boost of psychological functioning (e.g., a "warm-glow") from helping others, it takes time to distract them from focusing on their own psychological malfunctioning (e.g., anxiety and depression), as these symptoms are characterized by their persistent self-focused nature. Similarly, it is unlikely that givers will have an immediate positive effect on physical health after helping. For any positive effect of prosocial behavior on psychological malfunctioning or physical health, time must be given for observation. That said, considering the weighted effect sizes of the present meta-analysis, we might not be able to capture completely the effect of prosocial behavior on psychological malfunctioning or physical health. More future research using longitudinal designs, with a reasonable time interval between prosociality measures and psychological malfunctioning as well as physical health, is advisable.

Validated prosociality measures showed the strongest effects. We found that the measurement of prosociality played a key role in explaining the heterogeneity in the links between prosociality and overall well-being, hedonic well-being, and psychological functioning. Specifically, the weighted mean effect size from studies using validated prosociality scales was stronger than that from studies using volunteering/helping frequency and volunteering/helping status (yes or no). Indeed, the same result was observed in the link between prosociality and eudaimonic well-being, but we do urge cautious interpretation because the number of studies was relatively small in some categories.

There are several potential reasons for our finding. First, multiple-item scales tend to perform better than single-item measures in covering a theoretical construct (prosociality in our case) and, thus, are more successful in explaining variance in the outcome variables (Gorsuch, 1997). Second, the prosociality scales are more susceptible to social desirability bias. As compared to single-item behavioral measures tapping actual prosocial behavior during a fixed period of time, scales with multiple items are more ambiguous and easier for individuals to put themselves in a positive light because of social desirability. Given that well-being measures also tend to have a social desirability component, an underlying correlation driven by shared social desirability can emerge.

Our finding has several conceptual and methodological implications for operationalizing prosociality. For instance, researchers have to be cautious about whether to use a one-item measure for specific actual prosocial behavior, such as volunteering in a certain period of time, or a valid scale for prosocial behavior in general without a time frame. It is also worth noting that participants may prefer to use the frequency range in a measure as a frame of reference to give their response (Schwarz, 1999). Therefore, instead of using frequency measures of prosocial behavior, it is advisable to ask frequency questions using an open-response format, for example, "How many hours per week do you contribute your time to volunteering activities?" A combination of both behavioral measures and valid scales of prosociality, as well as nonself-reported prosociality indicators, are recommended for future research.

While some recent empirical evidence has suggested that prosociality should be domain general—across different economic games—and temporally stable (e.g., Peysakhovich, Nowak, & Rand, 2014), it is of theoretical interest to understand the distinction between giving money and time, and their impact on givers' well-being (Konrath, 2014). For instance, using SDT as a framework (Deci & Ryan, 2000), it is possible to reason that giving money may fulfill competence, while giving time may fulfill relatedness, and both in turn lead to higher levels of well-being. Although the weighted mean effect size related to charitable donation or prosocial spending always seemed to be larger than that related to volunteering/helping frequency across all models, the difference was only statistically significant in the eudaimonic well-being model (see Table 5). Yet, the number of studies on charitable donation/prosocial spending was small ($k = 2$), and we could not draw any firm conclusions from our metaregression results. Thus, future researchers may want to explore these potential mechanisms in SDT.

Informal (or mixed) helping is linked to more well-being benefits. In contrast to Wheeler et al. (1998) which only examined formal helping, our study is the first meta-analysis to examine the difference between formal and informal helping. In line with our prediction, informal helping (vs. formal helping) was found to moderate the effect of prosociality on overall well-being, hedonic well-being, and psychological functioning. The difference between formal and informal helping in their effects could be explained by SDT. Compared with formal helping, informal helping might fulfill relatively more basic psychological needs for autonomy and relatedness. This is because informal helping is more casual, spontaneous, and freely chosen compared to formal helping, where helpers/givers have to follow certain rules, regulations, or decisions of a formal organization. Daily helping acts, as well as private and unorganized assistance are relatively easier ways for helpers/givers to establish social relationships in which they feel close to and accepted by important others. Using SDT, Weinstein and Ryan (2010) have demonstrated that autonomous prosocial acts fostered well-being, which is in line with our reasoning that

Table 13
Univariate Moderation Tests of Categorical and Continuous Variables for the Relationship Between Prosociality and Physical Health

Moderator	r/B^b	95% CI	z	k	Q_M	R^2
Categorical variables						
Prosociality measures				83	6.71	.09
Volunteering/helping frequency	.08	[.06, .09]	12.12***	33		
Volunteering/helping or not	.10	[.08, .12]	4.56***	36		
Prosociality scale	.03	[−.04, .10]	1.86	5		
CD/PS	.10	[.03, .16]	8.21**	4		
MVA	.08	[.03, .12]	3.61***	5		
Formality of prosociality				81	0.61	.00
Formal helping	.09	[.07, .10]	15.02***	74		
Informal helping	.11	[.05, .16]	3.55***	4		
Mixed	.10	[.02, .18]	2.53*	3		
Data collection				83	1.98	.03
Primary	.11	[.08, .14]	6.79***	23		
Secondary	.08	[.07, .09]	14.17***	60		
Research design[a]				82	2.75	.01
Cross-sectional	.08	[.07, .10]	14.21***	64		
Longitudinal	.08	[.05, .11]	5.49***	11		
Experimental	.25	[.05, .45]	2.44*	2		
Volunteering program	.10	[.01, .18]	2.27*	5		
Sample representativeness				83	0.63	.00
Nonprobability sampling	.08	[.07, .10]	13.75***	64		
Probability sampling	.10	[.07, .12]	7.17***	19		
Retirement				77	9.52**	.17
Yes	.13	[.10, .17]	7.38***	10		
No	.07	[.06, .09]	8.81***	25		
Mixed	.09	[.07, .10]	11.73***	42		
Categorical variables						
Measure reliability	.01	[−.02, .04]	0.57	83	0.33	.01
Measure validity	.003	[−.024, .031]	0.22	83	0.05	.00
Age	.001	[.000, .002]	2.19*	73	4.78*	.11
Female percentage	.20	[.08, .32]	3.23**	81	10.45**	.18

Note. CD/PS = charitable donation or prosocial spending; MVA = membership in voluntary associations; r = correlation coefficient representing the weighted mean effect size; B = unstandardized coefficient; 95% CI = 95% confidence interval; z = z test statistic; k = number of independent studies; Q_M = Q statistic for test of moderators; R^2 = amount of heterogeneity accounted for.
[a] No diary or experience sampling class in research design. [b] r is used for categorical moderators, while B is used for continuous moderators.
* $p < .05$. ** $p < .01$. *** $p < .001$.

autonomy and relatedness could explain the stronger linkage between informal helping and well-being. The smaller association between formal helping (vs. informal helping) and well-being outcomes was also in line with the proposed moderators in the positive-activity model (Lyubomirsky & Layous, 2013). This may be because formal helping such as regular volunteering can become stale, monotonous, and burdensome, and hence, fail to contribute to positive emotions and well-being benefits; whereas informal helping, which is novel and varied, is more likely to be associated with stronger well-being benefits (Fritz & Lyubomirsky, 2018).

With that in mind, we unexpectedly found that there was also a significant difference between formal helping and mixed helping. In some cases, the effect of mixed helping was even stronger than that of informal helping on eudaimonic well-being, hedonic well-being, and psychological functioning. While this finding may be imperative, given the small number of studies in the mixed helping category (k ranging from 5 to 15), it is still premature to conclude on the stronger relationship between mixed helping and well-being. Thus, more research efforts are needed to probe this potential mechanism.

Demographic moderators.

Younger givers reported higher levels of well-being other than physical health. The moderation analysis showed that younger givers reported higher levels of overall well-being, eudaimonic well-being, and psychological functioning, with hedonic well-being and psychological malfunctioning in the same direction, but not showing significance. On the other hand, older givers reported higher levels of physical health. Furthermore, the moderation of age was so robust in the eudaimonic well-being model that it was still significant in the presence of other univariate moderators. Focusing on this intriguing age effect on the prosociality-eudaimonic well-being link, we suggested that while eudaimonic well-being is vital throughout life, young adults and midaged adults might experience it more significantly because they are still in their life developmental phases and undergoing rapid and widespread changes that represent growth opportunities. One of the important developmental tasks for emerging adults is to find their

Table 14
Weighted Multiple Regression-Analog Model of Prosociality and Physical Health

Moderator	*B*	95% CI	*t*	F_M
Intercept	−.06	[−.28, .16]	−0.54	
Retirement[a]				0.35
No	−.03	[−.10, .04]	−0.83	
Mixed	−.02	[−.07, .03]	−0.65	
Age	.00	[−.00, .00]	0.40	0.16
Female percentage	.25	[.06, .45]	2.62*	6.86*
$F(df1, df2)$	3.73 (4, 63)**			
$Q_E(df)$	203.45 (63)***			
R^2	.34			
k	68			

Note. *B* = unstandardized coefficient; 95% CI = 95% confidence interval; *t* = *t* statistic for each moderator; *F* = omnibus test for the significance of whole model; F_M = omnibus test for a single moderator while controlling for the influence of others; Q_E = *Q* statistic for test of residual heterogeneity; R^2 = amount of heterogeneity accounted for; *k* = number of independent studies in the model.
[a] Yes as the reference level.
* $p < .05$. ** $p < .01$. *** $p < .001$.

meaning and purpose in life—a measure of eudaimonic well-being (Eccles, Templeton, Barber, & Stone, 2003). Supporting this notion, Ryff (1991) found that young adults and midaged adults experienced more personal growth than older adults. It is possible that being prosocial not only gives young adults and midaged adults some sort of general well-being, but also offers them a chance to reconceptualize the self and to enhance their understanding of the purpose and meaning of life.

It is also noteworthy that the average age of participants in the present study moderated the prosociality-physical health link, but in the opposite way. If we apply the above similar reasoning that different kinds of well-being are valued in different life developmental stages, physical health appears to be a more important concern when people age. Our work provides strong "meta-empirical" support to previous review research on the elderly that suggested prosocial behavior (e.g., volunteering) was associated with better self-reported health, fewer functional limitations, and lower mortality (Anderson et al., 2014). Our data are also in line with another meta-analytic study that showed organizational volunteering reduced mortality risk among late-middle-aged and older adults (Okun et al., 2013). While the evidence available is highly consistent, more experimental studies are needed to identify the role of prosociality on physical health, especially among the elderly.

Female givers received more benefits of eudaimonic well-being, psychological malfunctioning, and physical health. Our result demonstrated that female givers enjoyed more eudaimonic benefits, which was consistent with our general prediction. The finding also lent support to a previous study that showed that volunteer work significantly predicted positive well-being, such as feeling useful, hopeful, and purposeful in women, but not in men (Waddell & Jacobs-Lawson, 2010). The authors of that study interpreted that volunteering might enhance a sense of worth in women, while men might be less affected and could derive positive well-being from other sources. In a similar vein, a qualitative study by Warburton and McLaughlin (2006) also suggested that prosocial acts contributed to women's identities and gave their lives meaning—a facet of eudaimonic well-being.

Numerous empirical studies have suggested that volunteering may derive physical health benefits (see Oman, 2007 for review). Yet, meta-analytic investigations into the health benefit of helping others are rare (Okun et al., 2013). Strikingly, based on our meta-analytic results, the estimated effect size between prosociality and physical health was quite small, $r = .09$. This relationship, much like those discussed above, was heavily moderated. We found that gender was a robust moderator in the multivariate model concerning physical health, even after controlling for other univariate moderators. Specifically, the more female participants in a study, the stronger the relationship was between prosociality and physical health.

Konrath (2014) has suggested that females tend to reap fewer potential benefits from prosocial action as helping is stereotypically a gendered act, and women are obliged or externally motivated to help. However, we found that prosociality actually had stronger recuperative/beneficial effects on psychological malfunctioning/physical health for female givers. In other words, our results did not support Konrath's thesis, but echoed the work by Schwartz and colleagues (2009) which found general helping and family helping be positively associated with social relations and physical health in females, but not males, respectively.

Retired givers reported better physical health only. Retirement only moderated the model concerning physical health, such that the estimated effect size of the relationship between prosociality and physical health for those retired was almost double the size for those nonretired (i.e., $r = .13$ vs. $r = .07$). While our meta-analytic results contributed to the literature by supporting that prosociality is one of the candidates for enhancing physical health among older retired people, it is also worthwhile to highlight some nonsignificant results for further investigation. Although the moderator of retirement did not reach a significant level of .05 for other well-being outcomes, all results were in the predicted direction, with the retired reporting more well-being benefits compared to those still working. Both the role theory (Adelmann, 1994) and social integration theory (Durkheim, 1951) suggest that the elderly may benefit more from acting prosocially by engaging in useful social roles and taking part in supportive social networks, but our statistical analyses did not seem to lend support to this argument. If that is really the case, perhaps a better line of distinction should be drawn between the employed and the unemployed to remove the potential confounds of age and retirement status. Future research looking into employment status may help strengthen the argument significantly.

Methodological moderators.

Primary data analysis had a bigger effect than secondary data analysis. Consistent with our prediction, we found that the weighted mean effect size of the link between prosociality and well-being was stronger in studies using primary data than those using secondary data across all models, except for the model concerning physical health where the result was not significant, but still in the predicted direction. There are several possibilities as to why secondary data sets are less ideal (Hox & Boeije, 2005). First, secondary data sets usually contain more items than primary data sets. Thus, the risk of participants becoming bored or fatigued is higher. Second, we could not control the item sequences in secondary data sets, yet numerous studies have shown that preceding items may affect responses of subsequent ones (e.g., Schwarz &

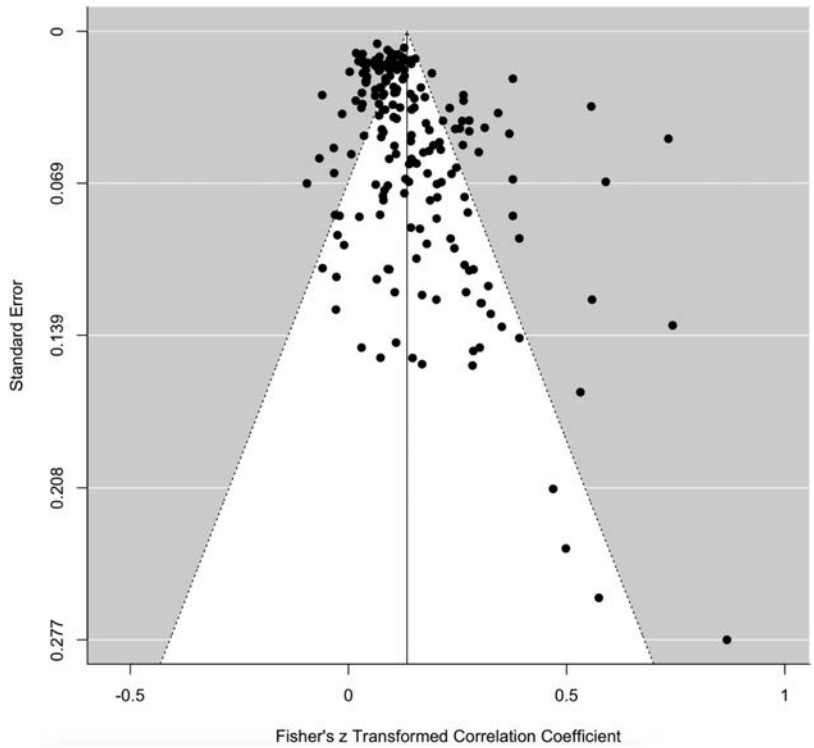

Figure 1. Funnel plot of effect sizes (Fisher's *z* transformed correlation coefficients) for the overall prosociality well-being relationship.

Strack, 1991; Sudman, Bradburn, & Schwarz, 1996). That being said, secondary data sets do have two strengths that make them appealing data sources. In general, they are readily available, inexpensive, and comprising large and diverse samples. Besides, multinational data sets allow researchers to conduct rich cross-cultural comparisons. These advantages help to boost the potential ecological validity of the conclusions (e.g., Aknin et al., 2013, Study 1). Thus, we do not recommend avoiding secondary data sets. Instead, we suggest researchers take special care in considering the potential pitfalls of a particular dataset and pay close attention to question ordering and length.

Inconclusive but useful findings on the role of study design. Study design only played a moderating role in the model concerning psychological malfunctioning, such that the weighted mean effect size for studies with an experimental design was larger than that for those with a cross-sectional or longitudinal design. However, our results were based on a limited number of studies in some categories, so they should be interpreted cautiously. Although studies using experimental and volunteering program designs always showed—not statistically—a larger effect size than others, this only weakly supported our speculation that experimental and volunteering program designs with intervention might have a stronger effect size than other observational designs. Despite the nonsignificant results, the primary effect sizes calculated for each study design could still be useful for future research on prosociality and well-being, especially in light of the growing emphasis on power analysis where estimating effect size is required. While our weighted mean effect size for experimental studies was comparable to that found in Curry et al. (2018), future research can refer to and use our estimated effect sizes for other research designs and for different well-being variables in their power analysis.

Higher-quality studies showed a stronger effect. The quality of meta-analytic results largely depends on the quality of constituent independent studies (e.g., Moher et al., 1998). In the present study, we found that two of the study quality components—measurement reliability and measurement validity—always moderated the prosociality to well-being link, except for models concerning psychological malfunctioning and physical health. In contrast, sample representativeness (i.e., probability sampling vs. nonprobability sampling) had no effect across all models. While perfectly measured studies are an ideal goal, empirical studies would inevitably vary considerably in a range of methodological quality metrics, such as using measures of different validity and reliability, different sampling methods, and different sample sizes. It is important to examine the effect of study quality on an effect size of a relationship or have it as a control variable in multivariate models, although the components to be included in quality assessment may vary in different fields and different topics. Only a limited number of meta-analyses in social psychology have assessed the quality of the studies (e.g., Cheng et al., 2013; Cheng, Lau, & Chan, 2014), hence we recommend that researchers consider this issue when conducting meta-analyses, as it may alter the interpretation of the results.

Caveats and Future Directions

To date, our meta-analysis is the single largest analysis of research on prosociality and well-being to have included both experimental and nonexperimental studies, and there are several

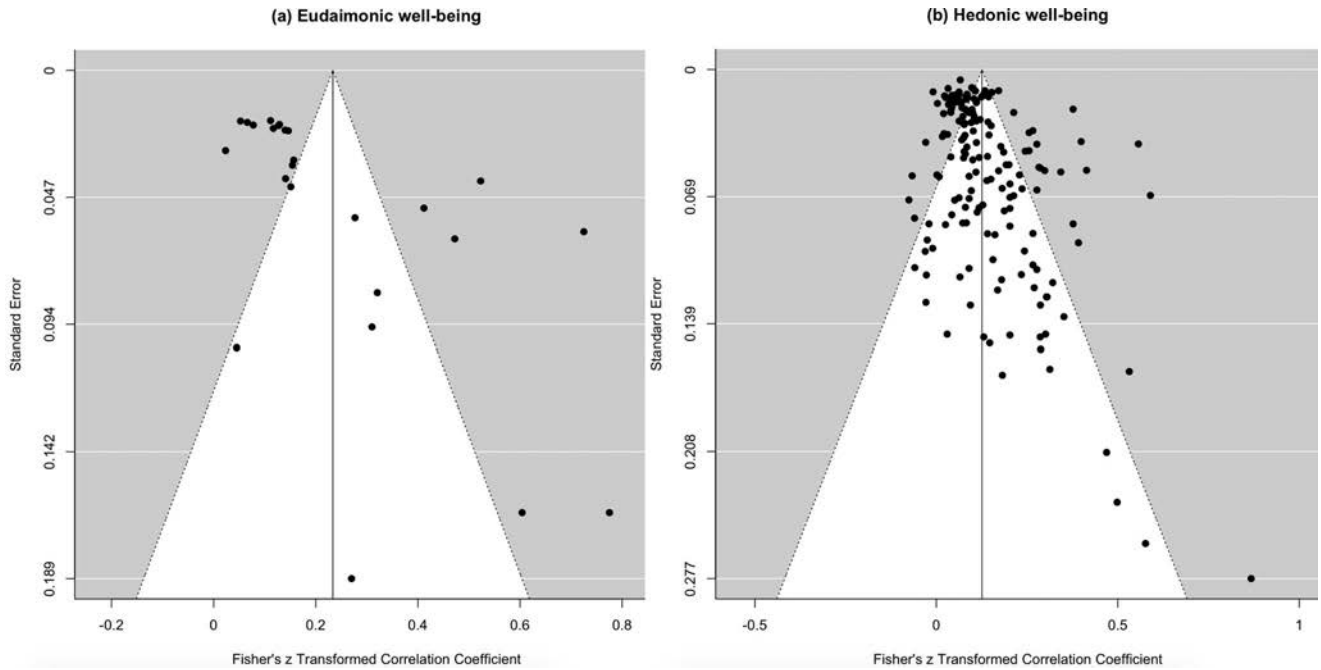

Figure 2. Funnel plots of effect sizes (Fisher's *z* transformed correlation coefficients) for (a) prosociality-eudaimonic well-being relationship and (b) prosociality-hedonic well-being relationship.

steps that future researchers can take to further build upon our findings. First, although a considerable number of studies have proposed that prosociality leads to well-being, other work suggests that happiness or feeling good also heightens prosociality (Isen & Levin, 1972; Lyubomirsky et al., 2005). Moreover, some researchers have suggested that prosociality and well-being may act in a positive feedback loop and mutually reinforce one another (e.g., Aknin, Dunn, & Norton, 2012; Konrath, 2014; Lyubomirsky et al., 2005; Thoits & Hewitt, 2001). Therefore, there are intriguing and unresolved competing hypotheses as to whether people's prosocial behavior leads to better well-being, or high well-being encourages acts of kindness. Building on the present meta-analysis, future meta-analyses will be in prime position to meta-analyze the experimental studies along the three possible directions above and compare the effect sizes estimated by the competing camps of studies.

We also note that there are a number of theoretically important moderators that we could not meta-analyze (also see Lyubomirsky & Layous, 2013). For example, ethnicity is a potential moderator of the prosociality to the well-being effect (McIntosh & Danigelis, 1995). Yet, in many studies, the ethnicity of participants was not reported. Relatedly, the social class of an individual may influence his or her prosocial behavior, such that lower-class individuals have been proven to be more prosocial than their upper-class

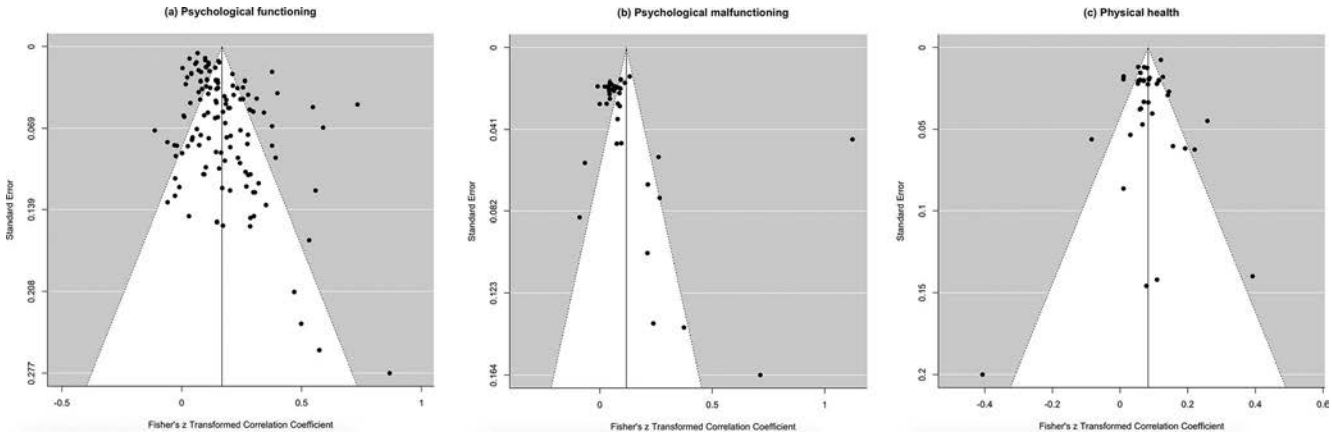

Figure 3. Funnel plots of effect sizes (Fisher's *z* transformed correlation coefficients) for (a) prosociality-psychological functioning relationship, (b) prosociality-psychological malfunctioning relationship, and (c) prosociality-physical health relationship.

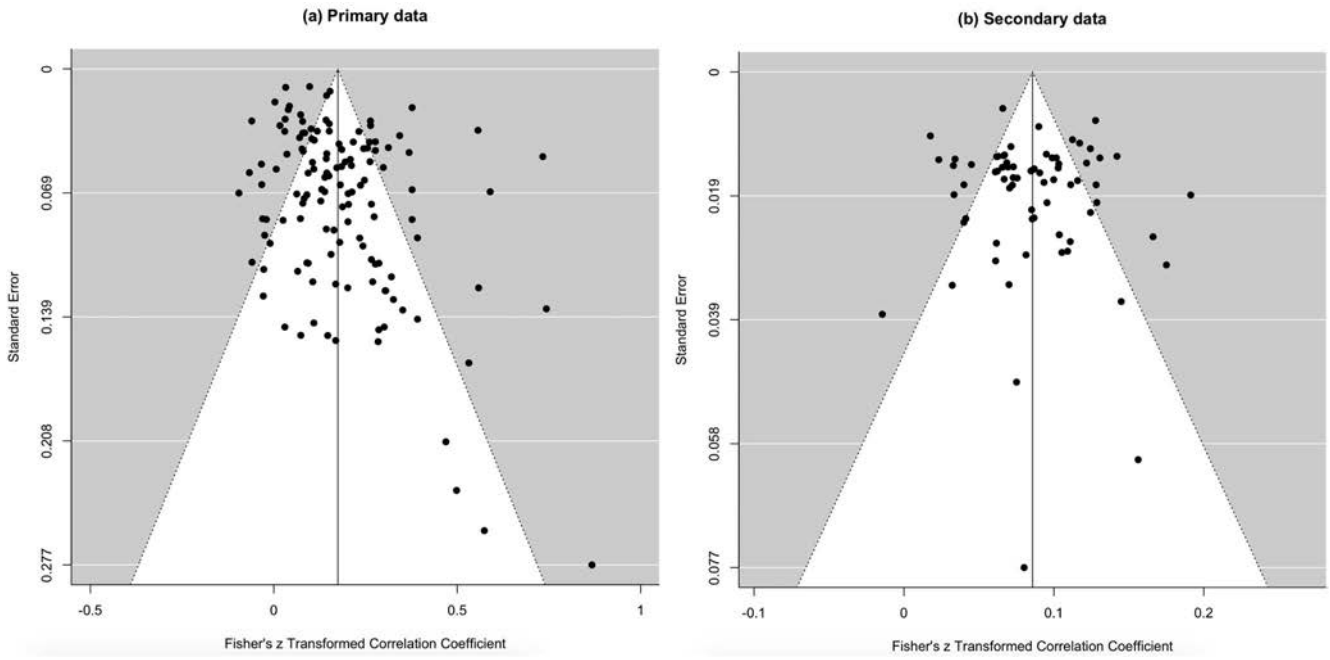

Figure 4. Funnel plots of effect sizes (Fisher's *z* transformed correlation coefficients) for the overall prosociality-well-being relationship (a) using primary data and (b) using secondary data.

counterparts (Piff, Kraus, Côté, Cheng, & Keltner, 2010). Following a similar logic, social class may be a significant moderator for the link between prosociality and well-being, as lower-class (vs. upper-class) help-givers may gain higher levels of well-being benefits in relatively more hostile environments. In addition, the relationship between prosociality and well-being can also be moderated by different types of supports (e.g., instrumental vs. emotional; Brown, Nesse, Vinokur, & Smith, 2003; Morelli, Lee, Arnn, & Zaki, 2015), motives of the help-giving (Clary et al., 1998), and the perceived impact of helping from help-givers (e.g., Aknin et al., 2013; Grant & Sonnentag, 2010). Unfortunately, there are currently a limited number of studies on these potential moderators, which do not constitute a sample large enough for meaningful meta-analysis.

Another fascinating question that remains is whether prosociality always predicts greater well-being linearly. Some previous work has suggested that the relation between prosociality and well-being is nonlinear, and high levels of prosociality may be detrimental to well-being (Post, 2005). For example, a study demonstrated that over 100 annual hours of volunteer work and paid

Table 15
Results of Trim and Fill, Egger's Regression, and Selection Methods for Publication Bias

	Trim-and-fill						Egger's regression		Selection methods			
	Unadjusted		Adjusted									
Variable	*k*	$Z\,\bar{r}$	Direction	*k* added	Adj. $Z\,\bar{r}$	Change	*z*	*p*	Moderate one-tailed	Severe one-tailed	Moderate two-tailed	Severe two-tailed
Overall well-being	201	.13	Left	0	.13	0	6.11	<.001	.12	.07	.13	.12
Well-being subset 1												
Eudaimonic well-being	25	.23	Left	0	.23	0	3.61	<.001	.20	.13	.22	.21
Hedonic well-being	172	.13	Left	29	.10	−.03	5.20	<.001	.11	.07	.12	.11
Well-being subset 2												
Psychological functioning	126	.17	Left	0	.17	0	3.12	.002	.14	.09	.16	.15
Psychological malfunctioning	39	.11	Left	0	.11	0	2.97	.003	.07	−.05	.11	.11
Physical health	36	.08	Left	0	.08	0	0.61	.541	*Err*	.07	*Err*	*Err*
Data collection subset												
Primary data	130	.17	Left	0	.17	0	3.17	.002	.15	.15	.16	.15
Secondary data	71	.09	Left	0	.09	0	0.59	.561	*Err*	*Err*	*Err*	*Err*

Note. $Z\,\bar{r}$ = unadjusted mean effect size; Direction = the side which the estimated number of missing studies is on; Adj. $Z\,\bar{r}$ = adjusted mean effect size after including imputed studies; Change = change in mean effect size; *z* = regression statistic for funnel plot asymmetry (nonsignificant *p* indicates the lack of evidence of asymmetry); *Err* = error occurred when applying selection methods to physical health and secondary data subsets, which is probably because of the small variances of effect sizes in the subsets.

work self-reported at a previous wave predicted poor health and death at the next wave (Luoh & Herzog, 2002). Similarly, Windsor, Anstey, and Rodgers (2008) found that the highest scores of well-being (i.e., life satisfaction and positive affect) were reported by those who engaged in 100–800 hr of volunteering activities per year. Musick, Herzog, and House (1999) found that volunteering had a protective effect on mortality only for those who worked in one volunteer organization or volunteered for 40 hr or less over the past year. While more empirical work has to be done to probe this possible nonlinear relationship, it is presently not feasible to synthesize and meta-analyze these results with such a small number of studies reporting nonlinear effect sizes. Another way is to compare the within-study differences in effect sizes as a function of a categorical prosociality predictor variable (e.g., Okun et al., 2013). However, it is difficult to make a statistical judgment on how many studies with a significant result can be concluded as linear or nonlinear, and some prosociality variables are continuous. To date, we are unaware of any better practice to meta-analyze the nonlinear effect sizes. This gives rise to the need for more advanced statistical methods.

Last but not least, it is noteworthy that few studies included in the present meta-analysis were preregistered (e.g., Fritz, 2019; O'Brien & Kassirer, 2019) or even in the format of a registered replication report (RRR; e.g., Aknin, Dunn, Proulx, Lok, & Norton, 2020). Given the high degree of variations in research designs, independent variables, dependent variables, sample size, and so forth, publishing preregistered studies and RRRs is probably one of the promising ways to minimize the file drawer problem, and in turn, reduce the possibility of publication bias in future meta-analyses. In our meta-analysis, publication bias has become a concern in the severe one-tailed selection (i.e., assuming almost all nonsignificant findings were included), with many of the effect sizes dropping by over 43%. In the RRR (Aknin et al., 2020), the effect sizes in Study 1 were .16 and .18, which are comparable to the effect size of .14 pertaining to charitable donation or prosocial spending in our meta-analysis. They also reported five much smaller effect sizes ranging from .01 to .09 in studies 2 and 3, resulting in a smaller overall effect size in the RRR, which are similar to the much-reduced effect sizes in our severe one-tailed selection. In nonpreregistered studies or non-RRRs which address the same prosociality-well-being link, these small or nonsignificant effect sizes were probably unreported—a practice that creates the file drawer problem and a source of publication bias. Given the significance of publication bias in a meta-analysis, we recommend the practice of preregistration and RRR for a more unbiased and precise estimate of an effect size in future meta-analyses (Simons, Holcombe, & Spellman, 2014).

Conclusion

While the link between prosociality and well-being was initially counterintuitive, the flurry of empirical findings over the past decade—and the attached press coverage—have made it almost a given. Yet, after a close inspection of all relevant literature, we have found that there is only a modest relationship between them. Furthermore, we have discovered that the link between prosociality and well-being depends heavily on a variety of theoretical, demographical, and methodological factors. It is our hope that this work contributes to more nuanced views of the effects of prosociality.

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