Age differences in prosociality across the adult lifespan:
Insights from self-reports, experimental paradigms, and meta-analyses

Altersunterschiede in Prosozialität über die erwachsene Lebensspanne hinweg:

Erkenntnisse aus Selbstberichten, experimentellen Paradigmen und Meta-Analysen

DISSERTATION

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Conflict of interest

The author declares no competing interests.

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Summary

Human prosociality, encompassing generosity, cooperation, and volunteering, holds a vital role in our daily lives. Over the last decades, the question of whether prosociality undergoes changes over the adult lifespan has gained increased research attention. Earlier studies suggested increased prosociality in older compared to younger individuals. However, recent meta-analyses revealed that this age effect might be heterogeneous and modest. Moreover, the contributing factors and mechanisms behind these age-related variations remain to be identified. To unravel age-related differences in prosociality, the first study of this dissertation employed a meta-analytical approach to summarize existing findings and provide insight into their heterogeneity by exploring linear and quadratic age effects on self-reported and behavioral prosociality. Additionally, two empirical research studies investigated whether these age-related differences in prosociality were observed in real life, assessed through ecological momentary assessment (Study 2), and in a controlled laboratory setting by applying a modified dictator game (Study 3). Throughout these three studies, potential underlying behavioral and computational mechanisms were explored. The outcome of the meta-analysis (Study 1) revealed small linear age effects on prosociality and significant age group differences between younger and older adults, with higher levels of prosociality in older adults. Explorative evidence emerged in favor of a quadratic age effect on behavioral prosociality, indicating the highest levels in midlife. Additionally, heightened prosocial behavior among middle-aged adults was observed compared to younger adults, whereas no significant differences in prosocial behavior were noted between middle-aged and older adults. Situational and contextual features, such as the setting of the study and specific paradigm characteristics, moderated the age-prosociality relationship, highlighting the importance of the (social) context when studying prosociality. For Study 2, no significant age effect on real-life prosocial behavior was observed. However, evidence for a significant linear and quadratic age effect on experiencing empathy in real life emerged, indicating a midlife peak. Additionally, across all age groups, the link between an opportunity to empathize and age significantly predicted real-life prosocial behavior. This effect, indicating higher levels of prosocial behavior when there was a situation possibly evoking empathy, was most pronounced in midlife. Study 3 presented age differences in how older and younger adults integrate values related to monetary gains for self and others to make a potential prosocial decision. Younger individuals effectively combined both values in a multiplicative fashion, enhancing decision-making efficiency. Older adults showed an additive effect of values for self and other and displayed increased decision-making efficiency when considering the values separately. However, among older adults, individuals with better inhibitory control were better able to integrate information about both values in their decisions. Taken together, the findings of this dissertation offer new insights into the multi-faceted nature of prosociality across adulthood and the mechanisms that help explain these age-related disparities. While this dissertation observed increasing prosociality across the adult lifespan, it also questions the assumption that older adults are inherently more prosocial. The studies highlight midlife as a potential peak period in social development but also emphasize the importance of the (social) context and that different operationalizations might capture distinct facets of prosociality. This underpins the need for a comprehensive framework to understand age effects of prosociality better and guide potential interventions.

Zusammenfassung

Menschliche Prosozialität beinhaltet Verhaltensweisen wie Großzügigkeit, Kooperation und freiwilliges Engagement und spielt eine entscheidende Rolle in unserem täglichen Leben. In den letzten Jahrzehnten hat die Frage, ob sich Prosozialität über die erwachsene Lebensspanne hinweg verändert, zunehmende Bedeutung in der Forschung erfahren. Frühere Studien zeigten eine erhöhte Prosozialität bei älteren im Vergleich zu jüngeren Erwachsenen. Meta-Analysen zeigten jedoch, dass dieser Alterseffekt heterogen und geringfügig sein könnte. Zusätzlich sind die Faktoren und Mechanismen, die zu diesen altersbedingten Veränderungen beitragen, noch wenig verstanden. Um die altersbedingten Unterschiede in Prosozialität besser zu charakterisieren, wurde in der ersten Studie dieser Dissertation ein meta-analytischer Ansatz verfolgt, um vorhandene Forschungsergebnisse systematisch zusammenzufassen und Einblicke in die zugrundeliegende Heterogenität zu erhalten. Hierfür wurden lineare und quadratische Alterseffekte auf selbstberichtete und verhaltensbezogene Prosozialität untersucht. Zusätzlich untersuchten zwei empirische Studien, ob diese altersbedingten Unterschiede in prosozialem Verhalten auch im realen Leben durch „ecological momentary assessment“ (wiederholte Selbstberichte im Alltag; Studie 2) und in einer kontrollierten Laboruntersuchung mittels eines modifizierten Diktator-Spiels (Studie 3) beobachtbar sind. Im Rahmen dieser drei Studien wurden zudem potenzielle zugrundeliegende Verhaltens- und komputationale Mechanismen untersucht. Die Ergebnisse der Meta-Analyse (Studie 1) zeigten einen geringfügigen linearen Anstieg von Prosozialität über das erwachsene Alter hinweg und signifikante Unterschiede zwischen jüngeren und älteren Erwachsenen, wobei ältere Erwachsene prosozialer waren. Zusätzlich zeigte eine explorative Analyse einen quadratischen Effekt von Alter auf prosoziales Verhalten, mit den höchsten Werten im mittleren Erwachsenenalter. Darüber hinaus verhielten sich mittelalte Erwachsene prosozialer im Vergleich zu jüngeren Erwachsenen, während keine signifikanten Unterschiede zwischen mittelalten und älteren Erwachsenen gefunden wurden. Situative und kontextuelle Merkmale, wie beispielsweise das Setting der Studie und bestimmte Merkmale des Paradigmas, moderierten den Zusammenhang zwischen Alter und Prosozialität und heben damit die Bedeutung des (sozialen) Kontextes bei der Untersuchung von Prosozialität hervor. Studie 2 konnte keinen signifikanten Zusammenhang zwischen Alter und prosozialem Verhalten im realen Leben finden. Es zeigte sich jedoch ein signifikanter linearer und quadratischer Alterseffekt auf das Erleben von Empathie im realen Leben, mit den höchsten Werten im mittelern Erwachsenenalter. Zudem zeigte sich, dass der Zusammenhang zwischen der Möglichkeit, in einer Situation Empathie zu empfinden, und dem Alter das Ausmaß an prosozialem Verhalten im realen Leben vorhersagte. Dieser Effekt, d.h. ein höheres Maß an prosozialem Verhalten in Situationen, die Empathie auslösen, war am stärksten im mittleren

Erwachsenenalter ausgeprägt. In Studie 3 hingegen wurden Altersunterschiede in der Art und Weise beobachtet, wie ältere und jüngere Erwachsene die Werte potenzieller Gewinne für sich selbst versus für eine andere Person berücksichtigen, um eine mögliche prosoziale Entscheidung zu treffen. Jüngere Erwachsene kombinierten beide Werte auf multiplikative Weise, was zu einer erhöhten Entscheidungseffizienz führte. Ältere Erwachsene zeigten hingegen einen additiven Effekt der Werte für sich selbst und die andere Person auf ihre Entscheidungen und waren effizienter in ihrer Entscheidungsfindung, wenn sie die Werte separat betrachteten. Eine stärkere inhibitorische Kontrolle ermöglichte es älteren Erwachsenen, Informationen beider Werte in ihre Entscheidungsprozesse einzubeziehen. Die Ergebnisse dieser Dissertation liefern wertvolle Erkenntnisse zur vielschichtigen Natur der Prosozialität über die erwachsene Lebensspanne hinweg sowie zu den Mechanismen, die diese altersbedingten Unterschiede erklären können. Obwohl die Ergebnisse eine Zunahme an Prosozialität mit dem Alter stützen, hinterfragen sie auch die Annahme, dass ältere Erwachsene grundsätzlich prosozialer sind. Die einzelnen Studien setzen die Lebensmitte als möglichen Höhepunkt der sozialen Entwicklung in den Fokus, betonen aber auch die Bedeutung des (sozialen) Kontexts sowie die Tatsache, dass unterschiedliche Operationalisierungen möglicherweise unterschiedliche Facetten der Prosozialität erfassen. Dies hebt die Notwendigkeit einer umfassenden Übersichtsarbeit hervor, um Alterseffekte von Prosozialität besser verstehen und mögliche Interventionen erarbeiten zu können.

1 General introduction

Consider yourself at a local community event organized by a charitable organization urgently seeking volunteers for an upcoming project. In this situation, the decision you make impacts not only you but also others. To make an informed choice, you must weigh various factors. Understanding the broader social context is crucial, as is assessing the potential positive and negative outcomes of the different choice options (acting prosocially vs. selfishly). This example highlights the importance of prosociality in a thriving society (Nowak, 2006). Prosociality not only enriches social interactions (Brief & Motowidlo, 1986; Dovidio et al., 2017; N. Eisenberg et al., 2006, 2014; N. Eisenberg & Miller, 1987; Penner et al., 2005) but also significantly influences individuals' mental and physical well-being (Brown et al., 2003; Hui et al., 2020; Konrath et al., 2012; O'Reilly et al., 2008). However, the decision to act prosocially continues to be a fascinating puzzle, considering that individuals across cultures are often willing to bear the costs of prosocial behavior rather than prioritizing self-interest (e.g., Henrich et al., 2005). Recently, research has begun investigating how these (pro)social decisions and corresponding behaviors change across the adult lifespan. With aging populations, shifts in prosociality can have significant social and economic implications (e.g., Harper, 2014). Thus, a critical question arises: How does prosociality develop and evolve throughout adulthood, and which factors facilitate or hinder this process?

Aging has been associated with declines in various areas, such as cognitive abilities (S.-C. Li et al., 2001), physical mobility (Cleaver et al., 2009), and decision-making skills (Mata et al., 2011). However, research also indicates age-related increases, especially in social and emotional aspects (Blanchflower & Oswald, 2017; N. C. Ebner & Fischer, 2014; Holt-Lunstad et al., 2010; Scheibe & Carstensen, 2010). Several studies have highlighted increased prosociality across adulthood (e.g., Bailey et al., 2013; Beadle et al., 2015; Sparrow et al., 2021; Van Lange et al., 1997). Nonetheless, the effect sizes from meta-analyses examining age effects on prosociality tend to be modest, and there is notable variability in the relationship between adult age and prosociality (Bagaïni et al., 2023; Sparrow et al., 2021). Additionally, the factors contributing to age-related differences in prosociality remain unclear (Mayr & Freund, 2020). Therefore, an inclusive theoretical and empirical approach is necessary to gain a better comprehension of prosociality in the context of adult development and aging (Bailey et al., 2021).

The studies in this dissertation explored how prosociality varies across different age groups in adulthood. We conducted a systematic meta-analysis, paying particular attention to unpublished data and focusing on the period of midlife (Study 1). Additionally, we conducted two empirical studies (Study 2 and Study 3) to measure prosocial behavior in different settings (real-life vs. laboratory) and using different measurement methods (experience sampling vs. behavioral paradigm) involving various age groups (younger/middle-aged/older adults vs. younger/older adults). As the factors influencing age-related differences in prosociality are not fully understood, this dissertation also investigated the role of potential moderators and computational mechanisms underlying prosocial decision-making. Study 1 specifically examined how different aspects of the task design (i.e., paradigm, incentive, interaction, setting) influence prosociality. Study 2 focused on how individual differences in socio-emotional abilities (i.e., empathy, compassion, theory of mind) modulate potential age-related differences. In Study 3, we applied a drift-diffusion modeling framework to investigate various subcomponents of the decision-making process (i.e., drift rate, initial bias, boundary separation). Moreover, Study 3 also explored the moderating impact of socio-emotional abilities (i.e., empathy, compassion, theory of mind), as well as cognitive abilities (i.e., intelligence, inhibitory control). In summary, this dissertation aimed to enhance our understanding of the relationship between aging and prosociality in adulthood, shedding light on the underlying computational processes and potentially other mechanisms involved.

1.1 Human prosociality

Definition and conceptualization

The study of prosociality has gained considerable interest in recent decades (e.g., Bailey et al., 2021; Böckler et al., 2016; N. Eisenberg et al., 2006; N. Eisenberg & Lermon, 1983; Nitschke et al., 2022; Thielmann et al., 2020; Thielmann & Pfattheicher, 2022; Van Lange et al., 1997). However, the field has faced conceptual ambiguity (Pfattheicher et al., 2022). Prosociality is commonly defined as inherent inclinations, motivations, and actions aimed at benefiting others or society as a whole (Brief & Motowidlo, 1986; Dovidio et al., 2017; N. Eisenberg et al., 2006; Penner et al., 2005). Furthermore, it can manifest in various forms, including cooperation, donations, volunteering, helping others, and reciprocal support (Schroeder et al., 2015).

A recent systematic review (Pfattheicher et al., 2022) suggests distinguishing three key dimensions of prosociality, each representing distinct conceptualizations: i) intentions and motives, ii) societal context, and iii) costs and benefits. Intentional prosociality encompasses any behavior aimed at prompting welfare, perceived by the actor as prosocial. This perspective underscores the intention to enhance others' well-being, consciously or unconsciously. From a societal standpoint, the approval of others is central, defining prosociality as normative. Thus, a behavior is considered prosocial as long as society values and approves it. In contrast, other definitions emphasize the costs and benefits associated with the specific behavior, known as the consequentialist perspective. According to this definition, prosocial acts result in others' welfare, regardless of whether the act was deliberate. The key element here is the personal cost for the actor, which can take the form of monetary losses or the sacrifice of resources like time or energy.

Prosocial behavior in a value-based decision framework

From the consequentialist standpoint and in line with other definitions (e.g., Andreoni, 1989; Contreras-Huerta, 2023), behaving prosocially involves personal costs, comparable to other decision-making processes. While conventional decision-making studies have primarily focused on individual choices based on personal values and preferences, many crucial decisions in our daily lives occur within social contexts, affecting not only ourselves but also those around us (Fehr & Camerer, 2007; Rilling & Sanfey, 2011; Sanfey, 2007). Recently, scholars have proposed describing the process of social decision-making as a value-based choice (Berkman et al., 2017; Gold & Shadlen, 2007; Hutcherson et al., 2015; Mayr & Freund, 2020; Rangel et al., 2008). This implies that when individuals are presented with various choice options (e.g., acting generously vs. selfishly), they evaluate the subjective value of each option by weighing potential positive and negative consequences. Consequently, engaging in prosocial behavior can be viewed as a cost-benefit analysis. To select the most valuable option, individuals engage in a dynamic process of integrating these subjective values (see also Pärnamets et al., 2020; Tusche & Bas, 2021).

Which computational processes are involved in weighing the trade-offs between acting prosocially and potential personal costs? The drift-diffusion model (DDM; Ratcliff, 1978; Ratcliff et al., 2016) provides a computational framework that decomposes the decision-making process into distinct subcomponents, allowing for the analysis of various latent cognitive processes (Forstmann et al., 2011; Voss et al., 2015). In DDMs, participants' reaction times and choice behavior are utilized to understand how a decision-maker accumulates noisy information until one of two response thresholds, representing the different choice options, is reached, and a decision is made (Busemeyer & Townsend, 1993; Hunt et al., 2012; Krajbich et al., 2010; Ratcliff & McKoon, 2008). In value-based decision-making, the noisy information accumulated is based on the relative subjective values associated with the available choice options (e.g., Rangel et al., 2008). This evidence accumulation process has key features representing distinct elements of the decision-making process: the drift rate, initial bias, and boundary separation. The drift rate signifies how quickly evidence is accumulated, thus reflecting the efficiency of the decision-making process. The initial bias refers to the decision-maker's initial preference towards a particular choice option before engaging in the decision-making process. The boundary separation indicates the quantity of information that must be accumulated before reaching a decision threshold (Voss et al., 2004).

How do the different subcomponents of the decision-making process affect prosocial behavior? Recent research employing (neuro-)computational models like DDMs has been helpful in better understanding the precise mechanisms behind social behavior (e.g., Iotzov, Saulin, et al., 2022; Iotzov, Weiß, et al., 2022; Saulin et al., 2022; Weiß et al., 2023). However, only a few studies focused on the underlying computational processes in a value-based decision framework of prosocial choice behavior. One such model, centered on altruistic choices (Hutcherson et al., 2015), found that both self and other values (representing monetary gains for the self and another person) are separately represented in the brain and information about both values is linearly weighted to reach a prosocial decision. Other implications of the model are: i) the decision to behave generously is made more slowly (as more evidence accumulation is needed), and this effect gets smaller the more generous an individual is, ii) generous decisions increase with the value of other, decrease with the value of self, but also increase with less strict decision-criteria, and iii) generous choices can sometimes be unintentional decision errors due to noise in the choice process. Another study by Tusche and Hutcherson (2018), using the same neurocomputational modeling approach, found that people exhibit more altruistic behavior when considering consequences for others or adhering to social norms. The study sheds light on the specific mechanisms behind altruistic behavior. It demonstrates that relevant choice features (self and other values in terms of monetary gains and losses) are encoded separately in the brain. By manipulating regulatory goals (emphasizing ethics, partner considerations, or natural responses), the authors could highlight the importance of the context of the decision situation: i) a higher weight was placed on values for oneself in the natural condition, ii) this effect decreased in the focus on ethics and partner condition, and iii) an increasing weight was placed on values for others in both the focus on ethics and partner condition. Taken together, the studies offer valuable new insights: First, the social context in which (pro)social decisions are made seem to be important when studying prosociality. Second, not all differences in prosocial behavior might be attributable to underlying preferences but also the amount of noise in the decision-making process.

In summary, adopting a value-based perspective on prosocial behavior presents several advantages. When combined with formal (neuro-)computational models, this approach holds promise for generating and testing predictions about the prosocial decision-making process (see also Tusche & Bas, 2021), as it allows to investigate the precise underlying cognitive mechanisms (Forstmann et al., 2011). Notably, evidence from the non-social domain suggests that age influences cost-benefit trade-offs and information integration processes (Bagaïni et al., 2023; De Dieuleveult et al., 2017; Devine et al., 2021; Ruel et al., 2021) which might be attributable to lower drift rates, and, consequently, reduced decision efficiency (Bocheva et al., 2018). However, evidence from the social domain with respect to prosocial development is currently lacking, even though this methodological approach would offer great potential to precisely explain why observable (prosocial) behavior undergoes changes across adulthood.

Prosocial behavior in a dual-system framework

Within the field of prosociality, a significant debate revolves around whether prosociality is an inherent inclination or if individuals need to overcome selfish tendencies before engaging in prosocial actions. This debate originates from the dual-system perspective, a concept put forth by researchers in various domains (Evans, 2008; Frankish, 2010), including social decision-making (e.g., Liu & Hao, 2011; Moll et al., 2006; Moore & Loewenstein, 2004; Schulz et al., 2014). The dual-system perspective assumes that decisions result from two distinct modes of cognitive functioning: an automatic (system 1, fast and intuitive) and a deliberative process (system 2, slow and controlled) (Epstein, 1994; Kahneman, 2003; Loewenstein et al., 2001; Reyna, 2004; Sloman, 1996; Stanovich & West, 2002). Both systems play essential roles in decision-making, and optimal choices emerge when they interact and operate in parallel (e.g., Damasio, 1994). Thus, concerning prosocial behavior, a key research question is whether prosociality derives from intuition (i.e., system 1) or controlled reflection (i.e., system 2). Notably, research addressing this question has yielded contradictory findings and conclusions, as outlined in the following paragraphs.

In the context of prosocial decision-making, it is assumed that the deliberative system 2, which operates on moral and ethical principles, dominates the automatic system 1, which is influenced by immediate rewards and self-interest (Moll et al., 2006; Moore & Loewenstein, 2004). Thus, behaving selfishly should align with the default reaction, especially when acting prosocially requires personal sacrifices, such as giving up time, money, or energy. Overcoming this inclination towards self-centeredness needs additional deliberate effort to engage in prosocial behavior (DeWall et al., 2008; Steinbeis et al., 2012). Interestingly, recent studies could not consistently confirm these assumptions when using response time as a measure of automatic and controlled answering: while there are studies indicating evidence in favor of the assumption that prosociality requires additional deliberation via system 2 (Lohse, 2016; Lohse et al., 2014; Piovesan & Wengström, 2009), there are also studies illustrating that prosociality might be rapid and intuitive, and driven by system 1 (Rand, 2017; Rand et al., 2012, 2014).

In line with the latter, in recent years, more research suggested that engaging in prosocial actions arises intuitively via system 1 preferences (e.g., Inaba et al., 2018; Ponti &

Rodriguez-Lara, 2015; for a review, see Zaki & Mitchell, 2013). This implies that humans have an intrinsic inclination towards prosocial behavior. However, additional deliberation becomes necessary to restrain these prosocial tendencies when opting for self-serving actions. In line with this, meta-analytic evidence illustrated that intuition (system 1) promotes cooperative behavior, while deliberate thinking (system 2) tends to favor actions that maximize personal gains (Rand, 2016).

However, recent studies challenge the dual-system perspective as the explanatory mechanism behind acting prosocially (or selfishly). Instead, they argue that there is no general underlying default inclination across all human beings but that individuals have different default preferences determining their choice behavior under time pressure (F. Chen & Krajbich, 2018; Mischkowski & Glöckner, 2016; Yamagishi et al., 2017). Other research in the field of decision-making suggests that a reduction in cognitive capacities (i.e., when individuals are stressed, tired, or distracted) affects choice precision rather than underlying preferences (Olschewski et al., 2018), especially in value-based decisions (Hutcherson et al., 2015; Milosavljevic et al., 2010). Furthermore, another study illustrates that individuals may have initial attentional biases (e.g., a tendency to look first at their own outcome), which drive their underlying social preference, and which may then predict changes in their choice behavior under time pressure (Teoh et al., 2020).

In conclusion, dual-system models have strongly influenced prosociality research. However, it is still an open debate whether intuition or deliberation drives prosociality, also with respect to the research field of adult age differences in prosociality. Applying these perspectives to the adult development, one possibility would be that age-related declines in deliberative capacities result in increased antisocial tendencies in older age. Alternatively, following the intuitive-prosociality model, cognitive decline in older age would weaken additional deliberation and lead to increased prosociality in the absence of motivational changes (see also Mayr & Freund, 2020).

Measurement of prosociality

The studies outlined in this dissertation used two principle measures for prosociality: self-reported questionnaires and behavioral paradigms, especially so-called economic games (Böckler et al., 2016, 2018; Thielmann et al., 2021; Wilhelm et al., 2018). Furthermore, the studies distinguished the setting in which these self-reported versus behavioral measures were applied, including experimental settings in the laboratory, online, and real life.

Self-reported vs. behavioral prosociality. In recent decades, various standardized measures have been developed to assess prosociality (e.g., Bailey et al., 2021; Böckler et al., 2016; Peysakhovich et al., 2014). However, it is crucial to recognize that these different measures may represent different aspects of the multi-faceted construct of prosociality (Tusche & Bas, 2021). One way to address this potential issue is by distinguishing between self-reported and behavioral measurements, as studies have shown they present separate yet interconnected aspects of prosociality (Böckler et al., 2018). Therefore, it is recommended to consider them as two distinct dimensions of the same underlying construct at a methodological level (Dang et al., 2020; Tusche & Bas, 2021; Wilhelm et al., 2018).

Psychologists frequently employ standardized and validated self-reported measures to assess prosociality. Examples include the altruism or prosocialness scale (Caprara et al., 2005; Rushton et al., 1981). These questionnaires evaluate behaviors and emotions associated with various actions such as sharing, supporting, helping, caring, or experiencing empathic concern. These assessments assume a broad-based trait of prosociality.

On the other hand, Wilhelm and colleagues (2018) suggest grouping paradigms that assess behavioral prosociality through game-theoretical approaches, aiming to manipulate and observe participants' behavior in controlled experimental settings under the category of game-theoretical conflicts (GTCs). These GTCs represent fundamental dimensions of behavioral prosociality (Wilhelm et al., 2018) and are used to investigate strategies for prompting everyday forms of prosocial behavior (Haesevoets et al., 2022). Examples of GTCs encompass tasks related to resource allocations, demonstrating how participants distribute resources between themselves and another person, such as social value orientation (defined as stable preferences for outcomes for self and other (Van Lange et al., 1997)), donation paradigms, or various economic games. Economic games, a traditional tool of behavioral economics (Camerer, 2003), function as social decision-making tasks, simulating real-life scenarios in controlled experimental settings to study social interactions across various situations (Baumard et al., 2013; Camerer, 2003). The use of economic games offers several advantages (Thielmann et al., 2021), such as i) direct observation of the variable of interest (prosocial behavior), ii) established theoretical frameworks (e.g., Game Theory (Luce & Raiffa, 1957; von Neumann & Morgenstern, 1944), Interdependence Theory (Kelley & Thibaut, 1978; Thibaut & Kelley, 1959), iii) a toolkit to explore prosocial behavior across diverse interdependent scenarios, and iv) the inclusion of outcomes, often involving monetary incentives. Based on structural characteristics, economic games can be categorized as sequential resource-allocation games and social dilemmas. In sequential resource-allocation games, one player divides an endowment with others (Thielmann et al., 2021), for instance, the dictator game and the ultimatum game. In the dictator game (Forsythe et al., 1994; Kahneman et al., 1986), the dictator allocates tokens to a recipient without their input, maintaining complete control over the distribution of resources. The ultimatum game (Güth et al., 1982), an adaption of the dictator game, involves a proposer allocating tokens to a responder. Unlike the dictator game, the responder can accept or reject the offer, leading to the proposed division of resources when the offer is accepted and no gain for both when the offer is rejected. In contrast, social dilemmas arise when it is collectively disadvantageous for all to be selfish compared to everyone acting prosocially. However, individually, one benefits most from selfishness when others are prosocial. Classic examples are the prisoner's dilemma and the public good game (Thielmann et al., 2021). In the prisoner's dilemma (e.g., Kollock, 1998), two individuals choose to cooperate or defect, with outcomes dependent on their combined choices. Wherein cooperation yields positive results, mutual betrayal yields negative outcomes, and unilateral betrayal with the other cooperating is highly favorable for the betrayer. The public good game (Samuelson, 1954) involves multiple participants contributing tokens to a shared account, which will be equally distributed afterward. Thus, while selfish players tend to contribute nothing, the best outcome for all participants occurs when everyone contributes their total endowment.

Laboratory vs. online vs. real-life setting. Controlled laboratory experiments are a prominent method in social science, widely recognized as the gold standard for establishing causal understanding (Falk & Heckman, 2009; Rubin, 2008). This approach is highly effective in providing controlled settings, allowing researchers to isolate specific variables and directly test theoretical hypotheses while minimizing the influence of confounding factors (Webster & Sell, 2014). Recently, online labor markets such as Amazon Mechanical Turk and Prolific Academic have gained prominence, offering a streamlined and efficient mean of collecting data from diverse and extensive participant pools (Buhrmester et al., 2018). These platforms leverage the internet to connect participants with remote study opportunities and compensate them with modest payments. This makes them especially well-suited for conducting incentivized behavioral experiments, such as economic games. The benefits of online experiments encompass reduced logistical concerns for researchers, such as easy participant recruitment and lower participation costs compared to traditional laboratory settings (Rand, 2012). Despite criticism of both laboratory and online studies for their potential lack of real-life applicability (Levitt & List, 2007), evidence supporting their external validity (e.g., Franzen & Pointner, 2013; Potters & Stoop, 2016; Walter et al., 2019) and reliability has been reported (Dandurand et al., 2008; Rand, 2012).

Alternatively, additional methods that provide valuable complementary insights into everyday contexts encompass lab-in-the-field experiments and real-life assessments such as ecological momentary assessment. Lab-in-the-field experiments combine elements of both laboratory and natural field experiments, employing standardized and validated tasks from laboratory settings within real-world contexts and thus using naturalistic environments (Gneezy & Imas, 2017). This methodology effectively bridges the gap between controlled laboratory experiments and real-world field studies, broadening the applicability of laboratory experiments to diverse subject groups and environments (e.g., Rieger & Mata, 2015). Furthermore, they can be employed post hoc to assess the external validity of a laboratory experiment (Gangadharan et al., 2022). On the other hand, smartphone-based ecological momentary assessment involves intermittently signaling participants throughout the day in their natural surroundings, most often over several days. Upon these cues, participants promptly report on their immediate experience (Schwartz & Stone, 1998). Ecological momentary assessment offers multiple benefits, including heightened ecological validity, reduced recall bias, and the capability to capture within-person dynamics and immediate, nearly real-time, emotional responses to events (Csikszentmihalyi, 2011; Gregorova et al., 2022).

1.2 Prosociality across adulthood

Theories on prosociality in older age

For a considerable period, older adults have often been characterized as incapable, reliant, and burdensome to society (Midlarsky, Kahana, & Belser, 2015). However, recent progress in developmental and social psychology has challenged this negative perception and has significantly contributed to a more nuanced understanding of age-related differences in social behaviors, particularly in the domain of prosociality (e.g., Bagaïni et al., 2023; Bailey et al., 2021; Freund & Blanchard-Fields, 2014; Henry et al., 2013; Hubbard et al., 2016; Reiter et al., 2017; Sparrow et al., 2021; Stietz et al., 2021; Sze et al., 2012). The resulting advances in scientific theories on human aging and social development are primarily observable in lifespan theories of motivation and social value orientation (e.g., Brandtstädter et al., 2010; Carstensen et al., 1999; Van Lange et al., 1997), but also recently by applying the value-based decision framework to the realm of prosocial decision-making in older age (Mayr & Freund, 2020). In the following, it will be explored how each perspective offers unique insights into the origins of age-related differences in prosociality and why prosociality can be considered a normative aspect of the aging process (e.g., Bailey et al., 2021; Carstensen & Fried, 2012; Mayr & Freund, 2020; Midlarsky & Kahana, 2007; Okun & Michel, 2006).

The contributory model of successful aging (Midlarsky & Kahana, 1994) suggests that older adults are inclined to act prosocially due to heightened empathic concern, strengthened religious beliefs, and a heightened sense of duty towards ethical norms. It posits that aging is associated with reduced meaningful social roles and social integration. To compensate for this loss, older adults tend to invest more time in prosocial actions. Consequently, increased prosociality is driven by an intrinsic motivation to contribute to future generations and society, regardless of the potential personal costs or external rewards (Midlarsky, Kahana, & Belser, 2015). Empirical evidence from a naturalistic study has demonstrated an age-related linear increase in donation behavior (Midlarsky & Hannah, 1989). The authors emphasize the significance of aligning personal costs with the resources of older individuals, enabling them to act consistent with their contributory roles and intrinsic motivation.

The socioemotional selectivity theory (Carstensen et al., 1999) is grounded in the assumption that perception of time significantly shapes goal priorities, thereby influencing age-related differences in social preferences and prosocial behavior (Carstensen, 2021). According to the theory, when time is perceived as limitless (a perspective typically held by younger individuals), behavior is driven by knowledge-based goals. Conversely, as individuals grow older and perceive time as increasingly limited, behavior becomes motivated by present-focused emotional goals. This qualitative shift in emotional experience is proposed as a natural part of the aging process. This results in greater emphasis on investing in close social interactions and emotional regulation goals, including pursuing prosocial goals (Carstensen et al., 1999). Several studies have empirically supported the socioemotional selectivity theory (e.g., Carstensen et al., 2000, 2011; Fung et al., 1999, 2020; Fung & Carstensen, 2004).

The dual-process model of developmental regulation (Brandtstädter & Greve, 1994; Brandtstädter & Renner, 1992; Brandtstädter & Rothermund, 2002) aligns with Carstensen and colleagues (1999) in positing that a perceived limitation of time leads to shifts in goal selection and pursuit. This model suggests that resilience in later life (reflected in life satisfaction, self-esteem, perceived control, and life quality in general) arises from the interplay of two adaptive processes: i) assimilative activities, which involve intentional efforts to align behavior or actual situations with personal goals, and ii) accommodative processes, which involve adjusting personal goals in response to constraints and situational changes. When time is seen as limited, accommodative processes play a crucial role in shifting individuals away from individualistic, future-oriented goals and towards intrinsic goals rooted in ethics, moral values, and sources of meaning, such as altruism. Evidence supporting this theory can be found in different studies indicating that different processes of developmental regulation are associated with age (e.g., Brandtstädter et al., 2010; Haase et al., 2013)

The *prosocial-growth hypothesis* (Van Lange et al., 1997) suggests that individual differences in Social Value Orientation (SVO) stem from the relative value assigned to potential outcomes for self and others. Prosocial individuals maximize joint outcomes, individualist individuals maximize outcomes for themselves, and competitive individuals maximize the relative differences between both outcomes. The prosocial-growth hypothesis found supporting evidence in a study that illustrated an increased proportion of prosocial individuals and a decreased proportion of individualists and competitors in older age (Van Lange et al., 1997). It is argued that the heightened prosociality observed in older age can be attributed to three factors (Y. Liu et al., 2022; Matsumoto et al., 2016; Van Lange et al., 1997): First, individuals may accumulate a positive understanding of the benefits of acting prosocially throughout their lifespan, based on their more expansive range of past experiences. Second, as individuals progress through different stages of life, their social roles and levels of interdependence change. This means that life events like becoming a parent or grandparent can lead to changes in prosociality. Last, cohort effects, which arise from demographic shifts across generations (such as differences in childhood experiences between rural and urban environments), are put forth as a potential explanation for variations in prosocial behavior associated with age. However, a recently published study failed to replicate the association between SVO and age (Y. Liu et al., 2022). Other authors argue that due to its relative stability, SVO can be seen as an underlying preference for prosocial behavior (e.g., Yamagishi et al., 2017), in line with the intuitive-prosociality model (see Zaki & Mitchell, 2013).

Other explanations support the idea that the age-related increase in prosociality across adulthood might be the result of older adults having more external resources, such as time, money, or social capital (e.g., Bekkers & Wiepking, 2011; Wiepking & James, 2013). This, in turn, lowers the threshold for acting prosocially, suggesting that older adults exhibit higher levels of prosociality because they have the means to do so. Even though older adults may have accumulated higher wealth (e.g., Cheung & Lucas, 2015; Huggett, 1996), the explanation lacks consistent empirical evidence. While there is cross-cultural evidence for a positive relationship between wealth and prosociality (Vanags et al., 2023), there are also studies indicating the opposite: poorer people being more prosocial than wealthy people (e.g., Bekkers et al., 2022; Cutler et al., 2021; Piff et al., 2010; Piff & Robinson, 2017). Interestingly, across various countries, the linear increase in prosociality throughout adulthood seems not to be affected by wealth (Cutler et al., 2021).

Recently, Shane and colleagues (2021) integrated the *motivational theory of lifespan development* (Heckhausen, 2001; Heckhausen et al., 2010) with the *expectancy-value theory* (Atkinson, 1957; Beckmann & Heckhausen, 2018; Eccles & Wigfield, 2002; Wigfield et al., 2015) to formulate a new framework explaining the development of prosociality across the adult lifespan. The framework sheds light on how individuals adapt and pursue goals as they age, emphasizing that commitment or disengagement from a goal is contingent on one's expectations of their capabilities, combined with the value placed on that goal, affecting their levels of prosociality. The theory suggests that in terms of lifespan development, control capacities responsible for goal selection and pursuit peak in midlife. Additionally, *Erikson's theory of psychosocial development* (Erikson, 1950) highlights the period of midlife by introducing the concept of generativity, where individuals strive to contribute to the well-being of future generations, which starts to increase in midlife. In line with other lifespan theories of motivation, it is assumed that individuals become less individualistic through generativity and more focused on contributing to society (Morselli & Passini, 2015). Aligning with the theories, Shane and colleagues (2021) found evidence that both prosociality and generativity show highest levels in midlife, a period characterized by the highest levels of expectancy and values attributed to acting prosocially. In line with this, perceived control, control striving, and agreeableness found to be significant predictors for prosociality and generativity.

These diverse theoretical accounts aim to increase the understanding of adult age differences in prosociality, with some of them arguing for a linear increase with the highest levels of prosociality in older age. In contrast, others propose the period of midlife as peak time. Even though age trajectories of prosociality might not be fully understood, these diverse theoretical hypotheses do not need to be mutually exclusive. A recent attempt to integrate them has been made by Mayr and Freund (2020) by broadening *the value-based decision framework* to the research field of the adult development of prosociality. Based on the value-based decision framework (e.g., Pärnamets et al., 2020), it is assumed that prosocial behavior results from a cost-benefit calculation. A central element of Mayr and Freund (2020) includes identifying potential critical drivers of prosocial behavior. According to the framework, these variables (so-called distal factors) directly affect the immediate positive and negative consequences (i.e., the cost-benefit trade-off) associated with the specific choice options when the decision is made. These distal factors can be in form of motivational orientations and preferences (e.g., Brandtstädter et al., 2010; Carstensen et al., 1999; Erikson, 1950; Midlarsky & Kahana, 1994; Van Lange et al., 1997; Yamagishi et al., 2017), as well as resources and constraints (e.g., Bekkers & Wiepking, 2011; Heckhausen et al., 2010; Shane et al., 2021; Wiepking & James, 2013), all of which are proposed to change across adulthood. However, currently, it is difficult to disentangle the different theoretical assumptions and how they influence the development of prosociality across adulthood due to a lack of empirical evidence. Mayr and Freund (2020) propose to adequately operationalize the key elements of each theory to test their specific influence on the cost-benefit calculation of prosocial decision-making within the value-based decision framework. Additionally, it seems promising to further combine the value-based decision framework (Mayr & Freund, 2020) with drift-diffusion modeling (Ratcliff, 1978; Ratcliff et al., 2016). Recent work illustrated that value-based decision-making, especially how cost-benefit trade-offs are approached and information integration is processed, was affected by adult age (e.g., Bagaïni et al., 2023; De Dieuleveult et al., 2017; Mata & Nunes, 2010; Ruel et al., 2021). DDM approaches can help explain such age-related differences by providing insights into the underlying computational mechanisms and how age affects them. For example, Bocheva and colleagues (2018) illustrated reduced decision efficiency (i.e., lower drift rates) in older age when information integration was necessary. Furthermore, a recently published meta-analysis illustrated reduced drift rates in older adults in perceptual and memory tasks and higher boundary separation in different non-social paradigms (Theisen et al., 2021). These results highlight that DDMs can identify differences in the decision-making processes during aging and thus be promising to better characterize age-related differences in prosocial decision-making.

Currently, studies combining the value-based decision framework with drift-diffusion modeling in prosocial development are lacking. Therefore, the third study of this dissertation utilized this interdisciplinary approach to examine age-related differences in prosocial decision-making between younger and older individuals. We aimed to identify critical drivers of prosocial behavior, specifically focusing on motivational and resource-related variables, in alignment with the value-based decision framework. Additionally, we employed drift-diffusion modeling to uncover the underlying mechanisms of the decision-making process. Studies 1 and 2 aimed to unravel age-related patterns of prosociality by examining both linear and quadratic effects of age on prosociality, as well as potential moderators. In the following sections, a summary of current empirical evidence regarding the effects of age on prosociality across adulthood, and factors that may moderate these effects, particularly in the context of the studies in this dissertation, will be provided.

Empirical evidence of age effects on prosociality

Social development has traditionally been studied in childhood research (e.g., N. Eisenberg & Lermon, 1983; Knight & Dubro, 1984; Thompson & Hoffman, 1980). However, recent decades have seen a growing interest in exploring various facets of social development in old age and throughout adulthood (e.g., Henry et al., 2013; Reiter et al., 2017; Stietz et al., 2021). It is now widely accepted, both in academic and lay psychology (Gopnik, 2021), that as we age, we tend to become more prosocial. However, does prosociality follow a linear trajectory throughout the adult lifespan, or might there be a potential quadratic age pattern of prosociality? In the following paragraphs, the current state of research on adult age and prosociality will be summarized.

Experimental laboratory studies frequently exhibit heightened levels of prosociality in older age, as measured through various methodologies. Several studies illustrate an age-related increase in prosocial tendencies across adulthood, as assessed using validated self-reported questionnaires (e.g., Bailey et al., 2008; Sparrow et al., 2019; Sparrow & Spaniol, 2018). Furthermore, research demonstrates that prosocial behavior also increases across adulthood in various economic games, including the ultimatum game (e.g., Bailey et al., 2013; Fernandes et al., 2019), dictator game (e.g., Beadle et al., 2015; Kettner & Waichman, 2016; Matsumoto et al., 2016; Rosi et al., 2019), prisoner's dilemma, and public good game (Matsumoto et al., 2016). This pattern is also observed in analyses involving SVO (Matsumoto et al., 2016; Van Lange et al., 1997), as well as charity donations or volunteering efforts (Bailey et al., 2020; Bjälkebring et al., 2016; Cutler et al., 2021; Freund & Blanchard-Fields, 2014). Results from meta-analyses, focusing on specific paradigms or the subcomponent of altruism, support the positive relationship between adult age and prosocial behavior (Bagaïni et al., 2023; Engel, 2011; Sparrow et al., 2021). Contrarily, other experimental investigations in the laboratory present a different perspective. These studies did not detect the age-related effect in both self-reported prosociality and behavioral measures of prosociality (e.g., Beadle et al., 2012; Borges et al., 2017; Bruine de Bruin & Ulqinaku, 2020; Cavallini et al., 2021; Roalf et al., 2012).

Interestingly, when investigating prosociality in real-life or lab-in-the-field studies, the findings become more diverse, revealing significant variability. In certain studies conducted within naturalistic lab-in-the-field environments - where standardized, validated behavioral paradigms are employed (Gneezy & Imas, 2017) - there is support for the positive association between age and prosocial behavior, particularly evident in economic games such as the public good game, dictator game, or prisoner's dilemma (Grimalda et al., 2016; Rieger & Mata, 2015; Romano et al., 2021). Nevertheless, other research in naturalistic settings presents no discernible link between age and prosociality, neither in donation paradigms (Best & Freund, 2021; Tognetti et al., 2013) nor in public good games (Rieger & Mata, 2015; Tognetti et al., 2013), and in some instances, even a negative correlation emerges, especially when evaluating prosocial behavior using SVO assessments (Ehlert et al., 2021).

However, there are also studies supporting the assumption that midlife might be a peak time for prosociality and that the positive association between prosociality and age becomes negative in older individuals (see Wiepking & James, 2013 for a review). Recent longitudinal studies illustrate that self-reported prosociality (assessed in national surveys) is most frequent in midlife, supporting evidence for an inverted U-shaped pattern of age on prosociality (Henning et al., 2023; Shane et al., 2021). This is also observed in an online study (Best & Freund, 2021) using a single prosociality factor (combination of self-reported and behavioral prosociality, see Hubbard et al., 2016): the quadratic age term significantly predicts prosociality, with highest levels in midlife. Similar findings are also reported in (lab-in-the-)field studies, illustrating that the relationship between age and donation behavior becomes negative at the age of 75 years (Midlarsky & Hannah, 1989) and that middle-aged adults show the highest contributions to the public good compared to younger and older individuals (Rieger & Mata, 2015). However, studies could not find a quadratic effect of age on prosociality, neither in an experimental setting with the help of a donation paradigm (Sze et al., 2012) nor in a dictator game (Rieger & Mata, 2015). Moreover, a recently published global study found evidence for a U-shaped age pattern in a hypothetical donation paradigm, with the lowest levels of prosociality in midlife (Cutler et al., 2021).

Taken together, while there is growing evidence pointing towards a positive relationship between age and prosociality (Bagaïni et al., 2023; Engel, 2011; Sparrow et al., 2021), this linear association has not been consistently confirmed (e.g., Best & Freund, 2021; Ehlert et al., 2021; Roalf et al., 2012). Moreover, results from longitudinal and global studies propose the idea that prosociality might instead follow a quadratic age pattern, even though the direction of this effect needs to be further investigated (Cutler et al., 2021; Henning et al., 2023; Shane et al., 2021).

Factors moderating age effects on prosociality

In their framework, Mayr and Freund (2020) especially highlight an important role of motivational and resource-related factors, which can be explained within the context of different theoretical accounts on (pro)social development (e.g., Bekkers & Wiepking, 2011; Carstensen et al., 1999; Erikson, 1950; Midlarsky et al., 2015; Van Lange et al., 1997; Wiepking & Bekkers, 2012). Other research suggests that contextual features, like who is receiving help or the form of the assistance, may significantly impact the relationship between age and prosocial behavior (Bailey et al., 2021; Penner. et al., 2005). Some even argue that prosocial behavior, in general, is reliant on the specific situation (e.g., Columbus et al., 2019; Thielmann et al., 2020). In the studies in this dissertation, particular emphasis was placed on investigating the moderating role of motivational and resource-related variables, such as empathy, compassion, Theory of Mind (ToM), intelligence, and inhibitory control (Study 2 and Study 3). Additionally, especially in Study 1, the moderating role of situational and contextual aspects was analyzed, including the study's environment, the specific measurement, and elements associated with the particular paradigm, such as the type of incentive and form of interaction applied.

Empathy & theory of mind. Empathy and ToM are vital for understanding others (Batson, 2009; Preston & De Waal, 2002). However, there is no universal consensual definition of empathy, leading to varying conceptualizations. Some studies describe empathy as a multi-faceted construct (Blanke & Riediger, 2019; Davis, 1983; Perry & Shamay-Tsoory, 2013; Zaki, 2017), dividing it into the subcomponents of emotion sharing, perspective taking, and compassionate concern (Depow et al., 2021; Zaki, 2017). Other conceptualizations grounded in neuroscience distinguish a socio-cognitive route, encompassing processes facilitating perspective taking (e.g., ToM), from a socio-affective route, including affect sharing (empathy) as well as feelings of caring for others' suffering (compassion) (Bloom, 2017; Jordan et al., 2016; Kanske et al., 2015; Singer & Klimecki, 2014). The so-called empathy-altruism hypothesis posits that empathy, in terms of affect sharing, produces altruistic (i.e., prosocial) motivation.

This link between empathy and prosociality has been explored and observed in various studies (Batson, 2010, 2014; Batson et al., 1981; De Waal, 2008; N. Eisenberg & Fabes, 1990; N. Eisenberg & Miller, 1987). Similarly, ToM, the cognitive counterpart of empathy, appears to be positively associated with prosociality (e.g., Caputi et al., 2012; Lehmann et al., 2022; Underwood & Moore, 1982). Furthermore, empirical evidence suggests empathy and ToM may undergo developmental changes throughout adulthood. While empathy and compassion tend to remain stable or increase with age, ToM is often reported to decline in older age (Reiter et al., 2017; Stietz et al., 2019, 2021). However, despite the influence of empathy and ToM on prosociality and age-related differences in these processes, it remains uncertain whether the empathy-altruism hypothesis also accounts for the increase in prosociality observed across adulthood. Some studies support a positive association between adult age and empathy/ToM as well as prosociality (e.g., Beadle et al., 2015; Cavallini et al., 2021; Sze et al., 2012), while others do not find this effect (Bailey et al., 2020; Cho et al., 2022). In the current dissertation, two out of three studies (Study 2 and Study 3) examined how empathy, compassion, and ToM might influence age-related differences in real-life and laboratory-observed prosocial behavior. While Study 2 applied the definition of empathy as an umbrella term (subsuming affect sharing, compassion, and perspective taking), Study 3 differentiated socio-affective (empathy and compassion) from socio-cognitive (ToM) processes.

Inhibition & intelligence. Two cognitive abilities that have been shown to be associated with age and hypothesized to moderate the effect of age on prosociality are inhibitory control and intelligence. Inhibitory control is one of the core cognitive control functions and describes the ability to inhibit impulses or habitual responses to facilitate goal pursuit (Wolff et al., 2016). This cognitive control function may reflect, for example, in suppressing impulsive thoughts and actions to achieve a prosocial goal (Carlson et al., 1998; N. Eisenberg et al., 2010; Gailliot, 2010; Rothbart, 2011) and is known to decline with age (Braver et al., 2001; Kane et al., 1994). The majority of existing research that links inhibitory control and prosocial behavior has predominantly centered on the childhood period (e.g., Blake et al., 2015; Hao, 2017; Liu et al., 2016; Yavuz et al., 2022). This body of work suggests that children with stronger inhibitory control tend to demonstrate higher levels of prosocial behavior (e.g., Aguilar-Pardo et al., 2013; Paulus et al., 2015; Steinbeis et al., 2012). In the case of adulthood, there is an ongoing debate regarding whether prosociality comes intuitively (Inaba et al., 2018; Ponti & Rodriguez-Lara, 2015; Zaki & Mitchell, 2013) or whether individuals need to overcome dominant, self-centered impulses before engaging in prosocial actions (DeWall et al., 2008; Steinbeis et al., 2012). However, to date, no studies have directly explored the influence of inhibitory control on the relationship between adult age and prosociality. Another cognitive ability that may potentially moderate age effects on prosociality is intelligence. General human intelligence can be divided into fluid and crystallized abilities, with fluid abilities being more biologically determined and crystallized abilities being based on experience and knowledge (Cattell, 1971; J. L. Horn, 1968, 1970). Previous findings suggest an age-related decline in fluid abilities but an experience-related increase in crystallized abilities in older compared to younger adults (S.-C. Li et al., 2004). Concerning intelligence, studies have shown that the age-related increase in prosocial behavior appears to be more pronounced in older adults with lower general intelligence (Rosi et al., 2019). Similar findings have been observed in other studies, indicating a negative relationship between intelligence and prosocial behavior in adult age (Cappelen et al., 2016; C.-C. Chen et al., 2013; Ogawa et al., 2020). Study 3 specifically investigated whether inhibitory control and intelligence can be considered critical drivers of prosociality across the adult lifespan. To this end, we tested whether both variables moderate how younger and older individuals integrate values for themselves (i.e., potential gains for the self) and others (i.e., potential gains for others) in a social decision-making task.

Contextual & situational features. Prosociality can vary as a function of context, culture, and socialization (Bailey et al., 2021; N. Eisenberg et al., 2014; Penner et al., 2005), implying that potential situational and contextual features might shape prosociality. While evidence suggests that age significantly affects prosociality on an intraindividual level, this effect indeed appears to be moderated by situational features and the social context (Bailey et al., 2021), for example specific workplace environments (Wieck et al., 2021). Furthermore, Sparrow et al. (2021) found a less robust association between age and prosociality in older-old samples (aged > 70.5 years) compared to young-old samples (aged < 70.5 years), implying the potential role of diminishing resources (e.g., finances, social networks) in the Fourth Age (aged 70 years and older, Baltes, 1998). Additionally, Raposo et al. (2021) demonstrated that older adults exhibited increased health-promoting behavior when benefitting a charity but not necessarily when aiding loved ones. Moreover, older adults selectively offer emotional support based on the situational context (Wieck et al., 2021). Studies on interdependent cooperation and volunteering empathize the significance of intergenerational effects (Romano et al., 2021), distinct life stages like retirement (Bjälkebring et al., 2021), and sociocultural norms (Chi et al., 2021) when examining prosociality throughout the adult lifespan. The moderating role of situational and contextual variables was primarily investigated in Study 1 of this dissertation. As Study 1 represents a systematic meta-analysis, we focused on whether different types of measures, the form of incentive and interaction applied in the included studies, or the setting in which the studies were conducted moderate age-differences in prosociality.

1.3 Research objectives and hypotheses

The objective of this dissertation was to disentangle age-related patterns of prosociality by summarizing existing findings and exploring the effect of age on behavioral and self-reported prosociality, as well as prosocial decision-making in different adult aged samples and with the help of different measurement modalities. Moreover, to unravel potential mechanisms driving the heterogeneity in the reported effects of age on prosociality, the moderating roles of different variables were examined, and drift-diffusion modeling was applied to capture latent cognitive processes of the (pro)social decision-making process.

Specifically, the first study involves systematic meta-analyses of self-reported and behavioral prosociality spanning the entire adult lifespan. In this comprehensive investigation, we aimed to analyze potential moderators, emphasizing situational and contextual factors. The second study focused on whether age-related differences in prosocial behavior were observable in a real-life assessment. Using open-source experience sampling data, we investigated age group differences in everyday prosocial behavior across the adult lifespan. Moreover, we explored whether individual differences in empathy, compassion, and ToM modulate potential age-related differences. The third study aimed to test age group differences in prosocial behavior assessed in a controlled laboratory setting. By applying a value-based decision framework coupled with computational modeling (DDM), we compared younger and older adults in a (pro)social decision-making paradigm and investigated the precise underlying computational processes. Additionally, we explored whether cognitive abilities (intelligence and inhibition), as well as empathy, compassion, and ToM moderated age group differences in prosocial decision-making.

Study 1: Age differences in prosociality across the adult lifespan: A meta-analysis

Studies examining adult age differences in prosociality are frequent, and evidence has been proved in previous meta-analyses (Bagaïni et al., 2023; Engel, 2011; Sparrow et al., 2021), revealing a small overall effect of age on prosocial behavior. Within Study 1, our objective was to provide an updated overview of age effects on prosociality by addressing and overcoming methodological limitations observed in previous research. To achieve this, we expanded the range of included measures and differentiated between self-reported and behavioral prosociality, as recommended on methodological grounds (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Dang et al., 2020; Tusche & Bas, 2021; Wilhelm et al., 2018). Furthermore, we placed particular emphasis on the inclusion of unpublished literature to prevent potential biases and systematic differences in interpretation (L'Abbe et al., 1987; Mosteller & Colditz, 1996). Additionally, we strived to investigate the entire adult lifespan, with a particular focus on the understudied and often underestimated period of midlife (Bailey et al., 2021). Based on this comprehensive approach, we aimed to integrate findings from published and unpublished data, examining age-related increases and quadratic age effects on self-reported and behavioral prosociality with the help of random-effect and multi-level models. Moreover, an additional aim was to approximate the question of underlying mechanisms by examining the role of various moderators in distinct meta-regression models, with a focus on contextual and situational features.

The systematic meta-analysis adhered to the PRISMA guidelines (Page et al., 2021) and was preregistered in the International Prospective Register for Systematic Reviews (PROSPERO; CRD42021223917, https://www.crd.york.ac.uk/prospero/display_record.php?ID =CRD42021223917, Pollerhoff et al., 2021). Based on previous research and theoretical perspectives supporting the prosocial-growth hypothesis with increasing age (e.g., Bagaïni et al., 2023; Mayr & Freund, 2020; Sparrow et al., 2021; Van Lange et al., 1997), we hypothesized that age would be positively associated with prosociality. We did not put forth specific hypotheses for the analysis of quadratic age effects on prosociality, as it followed an explorative approach which was not part of the preregistration.

Study 2: Investigating adult age differences in real-life empathy, prosociality, and well-being by using experience sampling.

Considering the diverse findings in lifespan developmental research on prosocial behavior and adult age (e.g., Bailey et al., 2020; Beadle et al., 2012, 2015; Cavallini et al., 2021; Roalf et al., 2012), it is still an open question whether age effects on prosocial behavior are observable in everyday life. Ecological momentary assessment (EMA) represents a valuable method for capturing real-life processes. It offers advantages such as enhanced ecological validity, reduced recall bias, and the ability to measure within-person variability in short-term changes (Bielak et al., 2014; Csikszentmihalyi, 2011; Neubauer et al., 2020). Even though these advantages are especially relevant for lifespan developmental research, only a limited number of studies have investigated prosociality in everyday life using EMA, and most did not address adult age differences (e.g., Depow et al., 2021; Grühn et al., 2008; Nezlek et al., 2001).

Study 2 aimed to adopt a real-life approach utilizing EMA to explore daily prosocial behavior. To achieve this and leverage the advantages of EMA, we used an open-source experience sampling dataset from Depow and colleagues (2021), which includes measures of prosocial behavior, empathy, and well-being in daily life. In a representative sample of adults in the United States, categorized into four adult age groups spanning from 18 to 55 years and older, we examined both linear and quadratic age effects on daily prosociality with the help of mixed-effect modeling. We separated between- and within-subject effects to explore interactions between daily empathy, prosocial behavior, and age.

Given the considerable heterogeneity observed in age-related research on prosocial behavior and the need for studies examining the construct in real-life settings across the adult lifespan, our hypotheses regarding age differences in daily prosocial behavior were formulated as undirected. This means we anticipated age-related differences but left the direction of the effect unspecified. Furthermore, based on a previous study (Cavallini et al., 2021), we hypothesized a weaker association between empathy and prosocial behavior among older individuals.

Study 3: Adult age differences in the integration of values for self and other

The value-based decision framework (Mayr & Freund, 2020) suggests that prosocial behavior can be understood as the result of a cost-benefit calculation, which involves integrating one's own and others' values (Berkman et al., 2017; Hutcherson et al., 2015). DDMs (Ratcliff, 1978; Ratcliff et al., 2016) can be employed to computationally describe these cost-benefit calculations and provide a quantitative approach to grasp the cognitive processes underlying prosocial decision-making. Previous studies have indicated that DDMs effectively capture essential subcomponents of the decision-making process, also in the field of prosociality (Hutcherson et al., 2015; Saulin et al., 2022; Tusche & Hutcherson, 2018).

In Study 3, we aimed to examine whether the commonly observed age-related differences in prosocial behavior can be found in a controlled laboratory setting. To achieve this, we used a modified dictator game, based on Hutcherson et al (2015), to compare the prosocial decision-making behavior of younger (18-30 years) and older adults (65-78 years). In this paradigm, participants are confronted with choice options representing varying payoffs for oneself and another person. A decision can be made by integrating the values for oneself and the other person or by considering them separately, allowing to examine value-integration processes. Mixed-effect models were employed to analyze age group differences in choice behavior and reaction times. Hierarchical drift-diffusion modeling was applied to estimate the three key DDM parameters (drift rate, initial bias, and boundary separation). Furthermore, we explored whether intelligence, inhibitory control, empathy, compassion, and ToM could account for any potential age-related differences in prosocial decision-making, as they are discussed to moderate age differences in prosocial behavior (Mayr & Freund, 2020).

Drawing from the growing evidence that indicates higher prosociality in older age (Engel, 2011; Pollerhoff et al., 2021; Sparrow et al., 2021), we expected to find these age-related differences in older compared to younger adults. Evidence from the non-social domain illustrated age group differences in value-integration processes (e.g., Bagaïni et al., 2023; De Dieuleveult et al., 2017; Ruel et al., 2021). Thus, we hypothesized that older individuals show reduced value-integration compared to younger adults and that these differences might be further explained by the key characteristics of the DDM (drift rate, initial bias, and boundary separation). Furthermore, we hypothesized that individual differences in cognitive (intelligence and inhibition) and socio-emotional abilities (empathy, compassion, ToM) affect potential age-related differences in prosocial-decision making.

2 Manuscripts and publications

For this dissertation, three studies were conducted to achieve the formulated goals by investigating how prosociality differs as a function of adult age and factors that potentially moderate these age differences. All supplementary materials can be found on the CD attached to this dissertation or via the Open Science Framework (see hyperlinks below for every study).

Study 1: Pollerhoff, L., Reindel, D.F., Kanske, P., Li, S.-C., & Reiter, A.M.F. (in preparation). Age differences in prosociality across the adult lifespan: A meta-analysis.

Material: https://osf.io/hs4y3/?view_only=571ef98e60a2441da346750957c4975d

Study 2: Pollerhoff, L., Stietz, J., Depow, G.J., Inzlicht, M., Kanske, P., Li, S.-C., & Reiter, A.M.F. (2022). Investigating adult age differences in real-life empathy, prosociality, and well-being using experience sampling. *Scientific Reports, 12*(1), 1-15. https://doi.org/10.1038/s41598-022-06620-x

Material: https://osf.io/d8ghq/

Study 3: Pollerhoff, L.*, Saulin, A.*, Kurtz, M., Stietz, J., Peng, X.-R., Hein, G., Tusche, A., Kanske, P., Li, S.-C., & Reiter, A. M. F. (submitted). Adult age differences in the integration of values for self and other.

* Equal contribution

Preprint via PsyArXiv: https://doi.org/10.31234/osf.io/ejg67.

Material: https://osf.io/zu4p3/?view_only=8b5cf6703f3b4d04a7122308c31445f4

2.1 Age differences in prosociality across the adult lifespan: A meta-analysis

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Abstract

Lifespan developmental theories and empirical research suggest a positive effect of adult age on prosociality. However, this effect has not been found consistently across studies, and many studies exclude the socially active phase of midlife from their analyses. In the current study, we summarize studies examining adult age effects on prosociality by combining 120 (independent) samples (*n* = 103,829) in a lifespan meta-analysis approach. We investigated linear and quadratic age effects on prosociality across the adult lifespan and conducted age group comparisons of younger versus middle-old versus older adults. We considered prosociality in terms of behavioral measures (game-theoretical conflicts) and self-report. The meta-analysis revealed small linear age effects on prosociality, as well as significant differences between younger and older adults, supporting the hypothesis that humans show a slight increase in both behavioral and self-reported prosociality as they get older. Furthermore, we leveraged open access to a large number of datasets (64 independent samples) to directly analyze (predominantly unpublished) quadratic age effects on prosociality. We indeed found evidence for a quadratic effect of adult age on behavioral prosociality. In age group comparisons, we observed higher prosociality in middle-old as compared to younger adults (on both the behavioral and self-reported level) and no difference between middle-old and older adults (on the behavioral level). These findings suggest a potential peak of behavioral prosociality already in midlife. The current meta-analysis offers new perspectives on age trajectories of prosociality, revealing the period of midlife as a potentially important phase of pronounced prosociality.

Introduction

Many political, societal, and economic decisions are made by people who are, compared to the age structure of the world population, relatively old (Ritchi & Roser, 2019). As an example, according to a Forbes ranking of the *world's most powerful people*, all contemporary influential international leaders are middle-aged to senior (Forbes Media LLC., 2018). Prosociality, defined as social thoughts and actions that are intended to benefit others, is not only linked to individual well-being across age groups (Hui et al., 2020). It is widely believed that a healthy society as a whole depends on its members' prosociality (N. C. Ebner et al., 2017), as important decisions made by powerful people often affect a society as a whole and not only oneself. Thus, understanding the development of prosociality in an age group that constitutes a substantial part of the total population and a majority of the economically active population has high societal relevance in the face of an aging world population (McNicoll, 2002).

Human prosociality is defined as all types of actions benefitting another person or society as a whole (Brief & Motowidlo, 1986; Dovidio et al., 2017; N. Eisenberg et al., 2006; Penner et al., 2005), often with the consequence of a personal cost for oneself (Andreoni, 1989). Prosociality can manifest in different forms, like cooperating, sharing, volunteering, or reciprocal support, and is linked to various biological, motivational, cognitive, and social processes, like empathy and trust (N. C. Ebner et al., 2017; N. Eisenberg et al., 2006). Different ways to measure prosociality in a standardized way have been introduced over the last two decades when the construct was increasingly studied across disciplines (e.g., Bailey et al., 2021; Böckler et al., 2016; Peysakhovich et al., 2014). Studies in behavioral economics most often used economic games (EG) to assess socio-economic decision-making in controlled experimental settings, encompassing paradigms like the public good game (PGG) and prisoner's dilemma (PD), as well as the dictator (DG) and ultimatum game (UG) (Baumard et al., 2013; Peysakhovich et al., 2014; Thielmann et al., 2021). There are further indicators of behavioral prosociality, like social value orientation (SVO) or donation behavior, which capture how individuals divide specific resources like money between themselves and another person or charity (Wilhelm et al., 2018). Besides these so-called *game-theoretical conflicts* (GTC) (Wilhelm et al., 2018) that assess behavioral prosociality, other popular methods frequently used to derive human trait prosociality are standardized and validated self-reported measures (SRM, e.g., the altruism scale by Rushton et al., 1981). Even though GTC and SRM are used to measure the same construct of prosociality, there is evidence that the two approaches might reflect partially different constructs. It is discussed that they should be differentiated on a methodological level (Dang et al., 2020; Tusche & Bas, 2021; Wilhelm et al., 2018).

In the field of developmental psychology, social development has traditionally mainly been studied during childhood (N. Eisenberg & Lermon, 1983; Honig et al., 1992; Knight & Dubro, 1984; Rushton, 1975; Thompson & Hoffman, 1980). However, over the last decades, the question of whether social cognitive and affective processes, as well as prosociality, may change during adulthood has increasingly attracted research attention (Henry et al., 2013; Mayr & Freund, 2020; Pollerhoff et al., 2022; Reiter et al., 2017; Sparrow et al., 2021; Stietz et al., 2019, 2021). Some economic accounts tended to see older adults mainly as a drain on societal resources or as net consumers of resources provided by younger adults (compare Midlarsky et al., 2015). In contrast, in psychological theories, prosocial behavior is mainly seen as a normative aspect of getting older (Bailey et al., 2021; Carstensen & Fried, 2012; Mayr & Freund, 2020; Midlarsky & Kahana, 2007; Okun & Michel, 2006). One influential theory promotes the *prosocial-growth hypothesis* (Van Lange et al., 1997), which postulates that a higher prosociality in older age is attributed to three different aspects (Y. Liu et al., 2022; Matsumoto et al., 2016; Van Lange et al., 1997): First, over the course of their lifespan, individuals might learn positive consequences of behaving prosocially based on their richer repertoire of past experiences. Second, lifespan development is associated with varying social roles, especially with respect to interdependence. Thus, situational change (like becoming a (grand)parent) might lead to different levels of prosociality. Third, cohort effects reflecting demographical changes across different generations (e.g., growing up in rural vs. more urban areas) have been claimed as one reason for age differences in prosociality. There are further theories arguing that the positive effect of age on human prosociality stems from a shift in motivation, such as more focus on intrinsic goals related to ethical and moral values (Brandtstädter et al., 2010), emotion-regulation goals (Carstensen et al., 1999), or the well-being of the next generation, also known as generativity (Erikson, 1950). There are further approaches that postulate an increase in external resources (e.g., more money or time in older age) to be the driving mechanisms of prosocial development (Bekkers & Wiepking, 2011; Wiepking & James, 2013). In fact, these different assumptions are not necessarily mutually exclusive. A recent theory, the *value-based decision framework* (Mayr & Freund, 2020), combines these different theoretical assumptions by proposing an integration of both motivational aspects (e.g., generativity, materialism) and resources (e.g., health, wealth, theory of mind) into a cost-benefit calculation akin to other (non-social) decision-making processes (cf. Hutcherson et al., 2015).

In the face of this myriad of theories emphasizing a putative age-related increase of prosociality during the human lifespan, it seems puzzling that such an age effect could not be consistently replicated in some of the more recent studies (e.g., Best & Freund, 2021; Borges et al., 2017; Cavallini et al., 2021; Liu et al., 2022; Pollerhoff et al., 2022; Roalf et al., 2012), suggesting substantial heterogeneity in published findings. Such heterogeneity might stem from variations in the setting in which prosociality is tested but may also reflect problems associated with publication bias or smaller sample sizes in traditional developmental psychology studies (Frank et al., 2017). Indeed, some of the more recent studies were not classical laboratory studies but real-life (Best & Freund, 2021 (Study 3)) or online studies (Y. Liu et al., 2022), with much larger samples than traditional developmental studies (n > 200), or other special characteristics like non-typical age group comparisons (mid- to old life) (Cavallini et al., 2021), or real-life outcomes (Best & Freund, 2021 (Study 2 + 3)).

A further limitation of current accounts (and published effects) is that most of them have focused on comparing prosociality in younger versus older age groups or have suggested linear increases in prosociality over the lifespan in samples in which not all different age periods of the adult lifespan were always sufficiently represented. However, there are several reasons to hypothesize that prosociality undergoes changes already during midlife. On a theoretical level, the motivational theory of life span development (Heckhausen et al., 2010, 2019) focuses on adaptive capacities with respect to goal selection and pursuit throughout development. It suggests that these adaptive capacities are driven by self-regulated motivational processes (like motivation for prosocial behavior). These motives are expected to peak in midlife (Heckhausen, 2001; Heckhausen et al., 2010), which might contribute to a higher degree of prosocial behavior in midlife. Moreover, there is evidence that the period of midlife is associated with a high expression of several personality traits that are suggested to be closely related to human prosociality. First, midlife has classically been suggested to be a prime period of generativity, i.e., the concern and caring about the next generations (Erikson, 1950; Schoklitsch & Baumann, 2012; Wojciechowska, 2017). Indeed, cross-sectional and longitudinal studies show a peak of generativity in midlife. (e.g., Keyes et al., 1998; McAdams et al., 1993; Nelson & Bergeman, 2021). Second, agreeableness, a trait representing individual differences towards interpersonal relationships (Graziano & Tobin, 2009) and empirically linked to prosociality (e.g., Ben-Ner et al., 2004; Koole et al., 2001; Pothos et al., 2011), has been demonstrated to be highest and most stable in midlife (Roberts & Mroczek, 2008; Specht et al., 2011). Moreover, normative personality development across adulthood is believed to prepare individuals for age-specific developmental tasks and corresponding adult roles (see Hutteman et al., 2014; Staudinger & Kunzmann, 2005). Indeed, developmental roles in midlife are typically associated with prosocial behaviors in the domain of caregiving and helping, including parenting as well as caring for older relatives (Fingerman et al., 2011; Grundy & Henretta, 2006). Additionally, introspection and reflection, two abilities that might be important contributors to take others' perspectives and show prosocial behavior towards them, are increased in midlife (Neugarten, 1968). Taken together, midlife might be a peak time and turning point in social development. However, this life period is unfortunately often overlooked and understudied in lifespan psychology (Lachman et al., 2015), perhaps in part due to practical difficulties in recruiting a sufficient amount of middle-aged adults for studies. Indeed, it has recently been emphasized that it is highly relevant to also consider non-linear age effects on prosociality (Bailey et al., 2021). Although there are only a few published studies analyzing quadratic effects of social development yet, there is some evidence for transformation already in midlife (Cutler et al., 2021; Hannikainen et al., 2018; O'Brien et al., 2013; Pollerhoff et al., 2022; Rieger & Mata, 2015).

The present meta-analysis. The current study takes a meta-analytic approach (k = 120, n = 103,829) to address the development of prosociality across the adult lifespan. Specifically, we consider linear (k = 89, n = 100,613) and quadratic adult age effects (k = 27, n = 82,990) on prosociality across the entire adult life span. In further meta-analyses including overlapping studies with the latter, we summarize studies comparing younger (YA) versus middle-old adults (MA) (k = 55, n_{YA} = 35,925, n_{MA} = 36,892), YA versus older adults (OA) (k = 50, n_{YA} = 32,996, n_{OA} = 18,028), and MA versus OA (k = 31, n_{MA} = 35,130, n_{OA} = 16,816) regarding their levels of prosociality. This resulted in a total of five different meta-analyses. We differentiated between behavioral and self-reported prosociality in these five specific analyses (Dang et al., 2020; Tusche & Bas, 2021; Wilhelm et al., 2018). Note that due to a limited number of studies (k = 2) for which data on self-reported measures was available, quadratic age effects and age group differences between MA and OA could only be considered for behavioral prosociality.

Moreover, we aimed to approximate the question of underlying mechanisms by considering the role of several moderators. A theoretically important moderator of age differences is whether monetary incentives/resources may have an effect on prosocial behavior. Evidence of whether these forms of incentive differentially influence prosociality is mixed (Bühren & Kundt, 2015; Camerer et al., 1999; Engel, 2011; Fantino et al., 2007; Fetchenhauer & Dunning, 2009; Gillis & Hettler, 2007, 2007; Lönnqvist et al., 2011; Thielmann et al., 2016; L. Zhang & Ortmann, 2014). However, wealth as a resource is believed to affect the cost-benefit evaluation (Mayr & Freund, 2020) of prosocial decision-making. Given that OA might have accumulated more financial resources than YA, reduced age effects on prosociality when adjusting for the payoff paid in the experiment (incentive) are hypothesized. One form of incentive applied in previous studies is that participants get a real outcome, meaning they receive a payoff contingent on their behavior, implicating true/actual consequences (behavior-contingent incentive). Otherwise, some studies only create hypothetical decision situations in which participants know that their payoff is independent of their behavior in the task (see also Thielmann et al., 2016, 2020). Thus, we considered the incentive structure of a study to be a moderator of the relationship between age and prosociality. Further moderators included were the measure of prosociality used in the studies, the type of interaction applied to the GTC (one-shot vs. repeated), the setting of the study (lab vs. lab in field vs. online), demographical characteristics of a study's population (sample size, nationality of the sample, year of publication/data collection, sex composition, and mean age), as well as the publication status (unpublished vs. published effect). Further moderator analyses, especially with respect to special characteristics of different GTCs and cognitive functioning, were initially planned (see Table S1 *data extraction (selection and coding)*), but too little data was available, or reporting in the published articles was not transparent enough to extract the relevant information.

Importantly, the meta-analyses conducted in this study extend previous related meta-analyses (Engel, 2011; Sparrow et al., 2021) with regard to the following aspects:

i) We focus on a wide range of validated and standardized measures of prosociality rather than restricting the analysis to one specific economic game (e.g., Engel, 2011) or the construct of altruism (e.g., Sparrow et al., 2021) only. Moreover, based on Wilhelm et al. (2018), we differentiated between behavioral and self-reported operationalization (see also Dang et al., 2020; Tusche & Bas, 2021).

ii) We included as many unpublished effects as possible. As meta-analyses based on published effects/data may lead to misleading interpretations and systematic differences (L'Abbe et al., 1987; Mosteller & Colditz, 1996), we expanded our literature search with a special focus on unpublished effects (so-called *grey literature*). We leverage the fact that many studies investigating prosociality in adults do not focus on age differences per se, but participants' age is collected as part of a general demographic characterization of the sample. This enables the inclusion of a particularly high number of unpublished effects (57 independent samples, corresponding to 46 papers) into the meta-analysis.

iii) A special focus was placed on the inclusion of middle-aged samples. As mentioned above, MA are often understudied, and midlife might hence be underestimated in its influence. In the current meta-analysis, we consider the whole adult lifespan to examine linear and quadratic age effects on prosociality. We follow this up with age group comparisons of MA versus YA and MA versus OA.

Taken together with these meta-analyses we aim to integrate findings of an age-related increase or quadratic effects of adult age on self-reported and behavioral prosociality across published and unpublished studies.

Methods

The goal of the current set of meta-analyses is to determine adult age effects on prosociality as measured i) via behavior in an experiment or ii) self-report. All conducted meta-analyses are aligned with the PRISMA 2020 guidelines (Page et al., 2021) and preregistered on PROSPERO (Pollerhoff et al., 2021; https://www.crd.york.ac.uk/prospero/display_record.php ?ID=CRD42021223917). In this preregistration, research questions, as well as a-priori inclusion criteria and methodological information regarding the literature screening and data analyses were specified. Discrepancies between the preregistration and our actual meta-analysis are explicitly pointed out in Table S1. All data, materials, and code are publicly available via the Open Science Framework at: https://osf.io/hs4y3/?view_only=571ef98e60a2441da346 750957c4975d.

Selection criteria. The following a-priori inclusion criteria were defined to detect eligible studies:

(1) Studies had to compare at least two different adult age groups (i.e., YA vs. MA, YA vs. OA, MA vs. OA). If they measured age on a continuum (linear term), the age range had to be between 18 and 101, and ± 2 SD of the mean age had to cover at least two age groups. The age groups were defined as follows: (a) YA: between 18 and 35 years, (b)

MA: between 36 and 59 years, and (c) OA: 60 years and older, in line with Sparrow et al. (2021).

(2) The study participants were not explicitly recruited as part of a clinical population or a pharmacological intervention study. However, healthy control samples of studies with a clinical focus were considered for inclusion.

(3) Participants underwent a behavioral task in which they made an actual prosocial decision (behavioral prosociality) or filled in a validated self-reported measurement of prosociality (self-reported prosociality). Studies using real-life altruism, like frequency of blood donation or money spent on charities in the last month/year measured with unvalidated questions, were excluded. Self-reported questionnaires (e.g., Caprara et al., 2005; Rushton et al., 1981) already cover real-life altruism but in a validated and operationalized form (e.g., questions corresponding to charity donations, helping behavior, or civic engagement). Furthermore, studies covering real-life altruism have been reviewed elsewhere (e.g., Bekkers & Wiepking, 2011; Serrat et al., 2019).

(4) Only studies written in English were considered for inclusion, as it is the universal language of science.

(5) Sufficient data had to be available to derive relevant information, especially effect sizes and sample characteristics (either in the article, supplementary material, available data, or provided upon request).

We included all studies matching the defined inclusion criteria. The data could be published or unpublished, including in the form of a dissertation. No restrictions regarding the publication year were defined. Studies that explicitly reported that their sample consisted of one homogenous group were excluded (e.g., mostly or entirely consistent of students (Adres et al., 2016), volunteers of one organization (Clary & Orenstein, 1991), mothers from college students (Barry et al., 2008), sport club members (Navarro et al., 2020), or participants that were particularly screened for one specific feature, for example, scoring high on narcissism (Böckler et al., 2017)).

Search strategy and identification of relevant studies. To locate and collect data relevant to the current meta-analysis, we based our search strategy on multiple approaches (see Figure 1 for a corresponding PRISMA flow chart). First, we extensively searched for studies assessing adult age differences in prosociality, primarily by an electronic search via three different databases, comprising *Web of Science* (Thomson Reuters), *Psych INFO* (American Psychological Association), and *PubMed* (National Library of Medicine). This systematic computerized literature search was conducted in November 2020 to detect potentially relevant English-language published peer-reviewed articles and dissertations by using a priori defined key search parameters reflecting i) prosociality and related constructs and ii) the adult lifespan (see supplementary p. 8 *key search parameters*). This search resulted in 14,113 records after duplicates were removed.

Next, three reviewers (LP, DFR, and TDAB) screened all articles for eligibility and identified 60 articles reporting one or multiple relevant studies corresponding to at least one effect size (see Figure 1, green path). Out of these 60 articles, 38 did not report all relevant information necessary for analyses (especially effect sizes and sample characteristics; 22 articles (23 studies/samples) reported all relevant information). For these 38 articles, we either located openly available datasets (5 datasets for 5 articles) to extract and calculate missing information or, if no open data were available, contacted corresponding authors (28 authors for 31 articles). For two articles, we were not able to find a valid e-mail address, leading to the exclusion of these studies. A reminder e-mail was sent if no answer was received within three to four weeks. Overall, we received feedback from 14 authors (50%), which led to the inclusion of a further 13 articles (15 studies/samples). In sum, we were able to include 40 articles (43 studies/samples) via this classic search through the different databases (see Figure 1, green path).

Importantly, many studies on prosociality that are not specifically focused on age effects on prosociality did assess participants' age for the sake of a basic demographical characterization of the sample. These studies did not focus on, and indeed often not explicitly reported, age effects on prosociality but are interesting for our meta-analysis at hand, as they provide more unbiased estimates of an effect (as publication bias most likely plays less of a role for effects that are not at the core of a research question). Thus, crucially, we also included studies identified through our systematic literature research that reported an eligible prosocial measure and age range (e.g., as part of the sample description), even if age effects on prosociality were not explicitly covered (k = 117, see Figure 1, orange path). We first searched for openly available datasets corresponding to these articles and identified 13 datasets for 13 articles. The evaluation of these 13 openly available datasets according to our inclusion criteria led to the inclusion of six additional articles/studies (the other 7 datasets had to be excluded due to different reasons; see Figure 1, orange path). If no open data were found, we requested information on age effects that had not been published within the original articles via e-mail (92 authors for 101 articles). We did not find valid e-mail addresses of the corresponding authors of two articles, and one article was already included via the request of the professional list server (see next passage). We received feedback on our request from 28 authors (30.4%). This enabled us to include 19 additional articles (22 studies/samples). Taken together, this search procedure, which focused on unpublished effects, successfully resulted in the inclusion of 25 articles (29 studies/samples, see Figure 1, orange path).

In March 2021, a call for further (published and/or unpublished) data was sent out via the mailing list of *The German Psychological Society* (DGPS). We received 27 (unpublished and published) datasets/papers, from which an additional 15 papers (16 studies/samples) matched our inclusion criteria. Additionally, in January 2023, we sent out a call for data via the mailing lists of the *Social Dilemma Community* (SDC) and the *Economic Science Association* (ESA). In total, both calls resulted in 15 datasets/papers which were sent to us. From these 15 records, we were able to include an additional 6 papers (6 studies/samples; see Figure 1 blue path for exclusion criteria). Moreover, we took advantage of the newly developed *Cooperation Databank* in January 2023 (CoDa; Spadaro et al., 2022). To detect a broad range of records, we screened all papers detected via the following filter settings: mean age 20-100, highest age 40-100, lowest age 17-100. This resulted in 45 records, from which we had to exclude 31 (see Figure 1 blue path for exclusion criteria). We were able to include two articles due to openly available datasets. Another 12 articles did report an eligible measure and age range but no openly available datasets, which is why these authors were contacted via mail. We received responses from three authors. In total, we were able to include 5 additional papers (10 studies/samples). See Figure 1, blue path, for an overview of these search strategies.

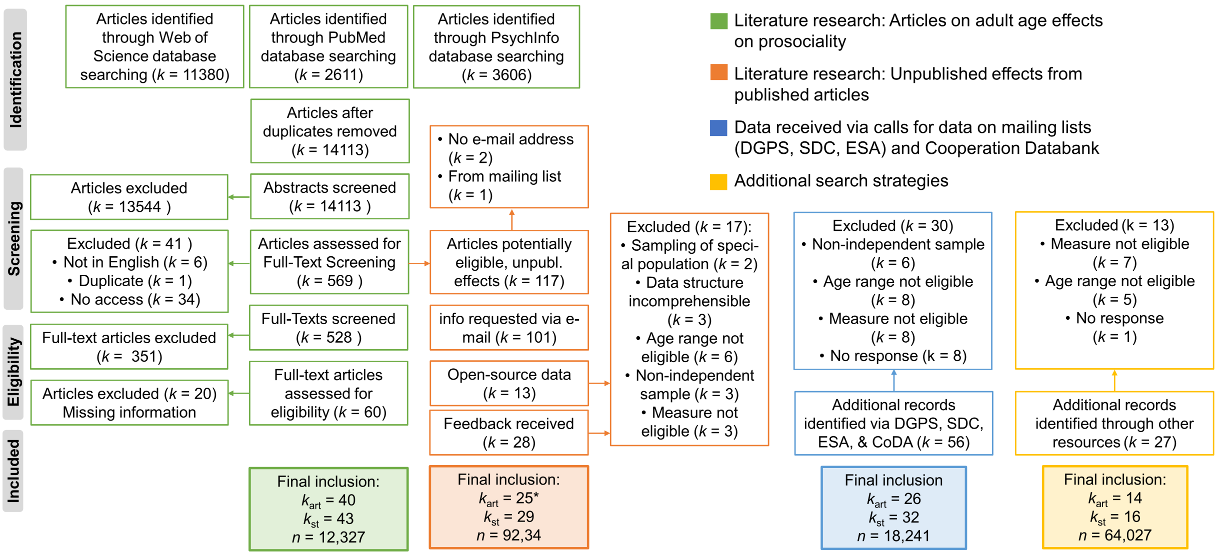
Furthermore, we applied different additional search methods (see Figure 1, yellow path): (1) Authors of all papers suitable for inclusion, identified until this step, were contacted via e-mail and asked for additional (published and/or unpublished) manuscripts and datasets. We received an additional 16 papers/datasets from different authors, of which 10 papers/datasets met our inclusion criteria. (2) For all meta-analyses and literature reviews capturing topics relevant to our research question at hand, which we had identified through the initial systematic literature search, we conducted a backward-reference search (k = 87). This led to the inclusion of one additional paper. (3) A backward-reference search was further conducted with respect to the bibliographies of all included papers. This did not lead to the inclusion of any further papers. (4) All articles included in a special issue from the journal *Psychology and Aging* published during the period of literature research (*Prosociality in Adult Development and Aging*; Bailey et al., 2021) were screened for inclusion. This resulted in two additional included articles (three studies/samples). (5) A backward- and forward-reference search was performed for one meta-analysis of this special issue (Sparrow et al., 2021). Only the forward-reference search led to the inclusion of one additional paper (two samples). Taken together, these additional search approaches (see Figure 1, yellow path) led to the discovery of 27 papers/data sets, of which 13 papers could not be considered for inclusion due to non-eligibility, which led to an additional inclusion of 14 papers (16 studies/samples).

Thus, the current meta-analyses reported here comprised a total of 105 papers, corresponding to 120 studies/samples with a total of *n* = 103,829 subjects. See Tables S2 and S4 for an overview of all included papers and studies.

Inter-rater-reliability. During the whole screening process, each paper was screened by at least two independent reviewers out of a pool of three reviewers (LP, DFR, TDAB) who first conducted a title- and abstract-based screening. Subsequently, the full texts of potentially eligible papers were screened, again by a minimum of two of the same reviewer pool (LP, DFR, TDAB). Afterward, discrepancies between the reviewers were solved through discussion or through a fourth independent reviewer (AMFR). To determine the level of agreement between the reviewers after completion of the screening processes, Cohen's Kappa was calculated. The interrater reliability at the full-text screening level was substantial (Kappa = 0.68, McHugh, 2012).

Figure 1

Flow chart representing the literature search and screening process (according to PRISMA guidelines, Page et al. (2021))



Note. k_{art} = final sum of included papers, k_{st} = final sum of included studies (independent samples). The green path represents the systematic search via different databases, covering papers on age-related differences in prosociality. The orange path represents studies identified through the systematic literature search via different databases, which did not report age-related differences in prosociality (because the research question of these studies did not cover age-related differences) but included an eligible age range (e.g., as part of the demographic characterization of the sample) and measure. That is, the orange path

corresponds to unpublished age effects derived from published articles. The blue path represents the search via the mailing list of the DGPS, SDC, and ESA, as well as the search via the CoDa. The yellow path represents the following additional search strategies: i) contacting authors from all included papers, asking about related (including unpublished) studies, ii) backward-reference search of all meta-analyses and literature reviews identified during the systematic literature search, iii) backward-reference search with respect to bibliographies of all included papers, iv) screening of a special issue from the journal Psychology and Aging, v) backward- and forward-reference search for one meta-analysis of this special issue (Sparrow et al., 2021).
*25 instead of 24 because one study corresponds to two papers.

Treatment of age and coding of effect sizes. Lifespan studies differ with respect to whether they treat age continuously or categorically (i.e., as age groups, e.g., YA, MA, and OA). Thus, we calculated separate meta-analyses, including either studies that reported age as a continuous variable (MA_{cont}) or analyses of age groups (MA_{groups}). In cases where raw data were available (55 papers, 64 studies/samples) and where we were able to calculate and extract the relevant information ourselves, papers were included in both MA_{cont} and MA_{groups} by determining effect sizes appropriate for the data. To calculate effect sizes, we adopted the following approach:

(1) Continuous treatment of the variable age, linear age effects ($MA_{cont\text{-}lin}$): To be included in this analysis, ± 2 SD of the mean age reported in the paper had to cover at least two different adult age groups (YA: 18 and 35 years, MA: 36 and 59 years, and OA: 60 years and older, following a related meta-analysis (Sparrow et al., 2021)). We used different correlation coefficients as the measure of effect size for $MA_{cont\text{-}lin}$. If the two variables (age and prosocial measure) were continuous and normally distributed, Pearson's product-moment correlation coefficient was used (e.g., Borenstein, 2009; Fisher, 1915). Continuous but not normally distributed or ordinal variables were analyzed with Spearman's Rank correlation coefficient (e.g., De Winter et al., 2016). For relationships of one continuous and one naturally dichotomized variable, point-biserial correlation coefficients were calculated, and for the association of one continuous and one artificially dichotomized variable (e.g., Jacobs & Viechtbauer, 2017), biserial correlation coefficients were employed. A meta-regression was carried out as a sensitivity analysis to check whether different types of correlation coefficients showed different results. This did not indicate significant differences with respect to the different measures (see Table S8).

(2) Continuous treatment of the variable age, quadratic age effects ($MA_{cont\text{-}quad}$, explorative analysis): Some previous findings suggest that the effect of adult age on

prosociality might follow a quadratic rather than a linear function (e.g., Cutler et al., 2021; Rieger & Mata, 2015). Note however that quadratic effects reported in the primary literature are difficult to meta-analyze as they most frequently stem from regression models with varying numbers of predictors limiting comparability of the regression weights for age. Thus, we made use of the high number of raw data we had available to calculate comparable effect sizes from uniform regression models in order to meta-analyze quadratic effects on prosociality. For $MA_{cont\text{-}quad}$, all age groups (YA, MA, and OA, classification as above) had to be represented in the specific sample/study, with at least 20 participants per age group, to ensure that we covered the entire adult lifespan. We calculated generalized linear models, including both age as a linear and quadratic term, with standardized variables. The standardized beta-coefficients of the quadratic age term were used as the measures of effect size. The standardized beta-coefficient of the corresponding linear age term was further included as a covariate in the model. Based on whether the outcome variable of the regression model was continuous or binary, linear versus logistic regression models were calculated to receive the standardized beta-coefficients. A sensitivity analysis in terms of a meta-regression model was carried out to ensure that outputs from the different regression models (linear vs. logistic) did not lead to systematically different results. The meta-regression model did not indicate a significant difference based on whether a logistic or linear model was calculated (see Table S10).

(3) Categorical treatment of the variable age, i.e., age in groups (MA_{qroups}): The groups were classified into YA (18 and 35 years), MA (36 and 59 years), and OA (60 years and older), following the meta-analysis by Sparrow et al. (2021). At least two age groups had to be present in the raw data to meet our inclusion criteria, with at least 20 participants per age group. Note that in the literature, original studies following a categorical treatment of age differ with respect to which age range is considered as YA, MA, or OA. In case no raw data were available, and the age group classification from the original studies differed from the one defined above, sensitivity analyses were carried out. Whenever age ranges for YA, MA, and OA were defined in different ways than defined above, meta-analyses with and without these specific studies (k = 11) were analyzed (see Table S7.1). If no qualitative change was observed by excluding these studies, the studies remained included. For MA_{groups}, the bias-corrected standardized mean difference (SMD) *Hedge's g* was used (Hedges & Olkin, 1985). Whenever the SMD was not directly reported, we extracted or calculated the Mean and SD of the prosocial measure per age group to derive Hedge's g. Three different meta-analyses were conducted to compare effect sizes by comparing i) YA versus MA, ii) YA versus OA, iii) MA versus OA.

Across all meta-analyses ($MA_{cont\text{-}lin}$, $MA_{cont\text{-}quad,}$ and MA_{groups}), we excluded participants aged younger than 18 years if raw data were available. Otherwise, if we had to contact the corresponding authors due to missing information, we requested the specific effects only for participants aged 18 years and older. Despite these endeavors, for *k* = 2 studies, we were not able to derive effect sizes without participants < 18 years due to a lack of available information (the minimum age for these studies was 17 years). Meta-analyses with and without these studies were carried out as sensitivity analyses (see Table S7.1). The studies remained included if no qualitative change was observed in the different models. All effect sizes were coded such that higher (positive) values represent a positive relationship between age and prosociality or so that positive values represent higher prosociality in the older age group. Some studies employed multiple measures of prosociality and thus reported more than one effect size for the association of age with prosociality within the same sample. To avoid loss of information, we opted for the following approach in handling multiple non-independent effect sizes within one study/sample: Whenever more than one effect size was reported or could be calculated within the same sample of a study, we calculated aggregated effect sizes using the *aggregate* function from the metafor package (Viechtbauer, 2010), but differentiating between $MA_{cont\text{-}lin}$, $MA_{cont\text{-}quad}$, and MA_{groups}, and further between behavioral and self-reported measures. We approximated the degree of correlation among the sampling errors of the different effect sizes *rho* via calculating correlations among the various measures (Viechtbauer, 2010). Due to a relatively high number of raw data sets available, we were able to calculate the inter-correlations of measures for most of the studies. Whenever more than two effect sizes within one study were reported, we used the median of the calculated inter-correlations to define *rho*. In case no raw data were available to approximate the sampling error of the effect sizes within one study, the median of all known correlations included in the corresponding meta-analysis was used as an approximation to define *rho* (see Viechtbauer, 2010). This procedure ensured that each study/sample only entered the specific meta-analysis with one effect size estimate. Multi-level meta-analyses, including individual effect sizes nested within studies, were further carried out for every random-effect model as sensitivity analysis. If no qualitative change in results occurred (random effect vs. multi-level meta-analyses), the random effect meta-analyses will be reported (see Table S7.2 for sensitivity analyses).

Dimensions of prosociality. Previous findings (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Dang et al., 2020; Tusche & Bas, 2021; Wilhelm et al., 2018), suggested trait/self-reported prosociality and behavioral prosociality as two fairly independent dimensions of prosociality. We here adopted this structure in our analyses: all meta-analyses ($MA_{cont\text{-}lin}$, $MA_{cont\text{-}quad}$, and MA_{groups}) were distinguished into two independent meta-analyses considering age effects on i) behavioral prosociality (i.e., assessed by an experimental task/GTC; MA_{behav}) and ii) self-reported prosociality (i.e., assessed by a validated questionnaire; MA_{SRM}). We did not meta-analyze MA_{SRM} for MA_{groups} (MA vs. OA) and MA_{SRM} for $MA_{cont\text{-}quad}$ due to the limited number of included studies (*k* = 2; see Figure 2 for an overview).

Coding of study characteristics. For an overview of all variables coded in the screening process and the current meta-analysis, see Table S6. Due to limited information reported in the articles and thus too little data available, we were not able to analyze all a-priori-defined moderators as initially planned, especially with respect to cognitive functioning and specific characteristics of the GTCs (see Table S1). One moderator (whether participants played an active or passive role in the GTC) could not be analyzed in a meta-regression model, even though enough data could be extracted from the papers due to the model diagnostic procedure. All papers in which participants played a passive role had to be excluded based on the leave-one-out diagnostic procedure as they were detected to be outliers and overly influential. In the following, we will only report the information extracted for the variables we were able to meta-analyze (either in random-effect, multi-level, or meta-regression models):

***Age*.** Age was one key variable of interest for the current meta-analysis. Irrespective of how age was treated in the analysis ($MA_{cont\text{-}lin}$ vs. $MA_{cont\text{-}quad}$ vs. MA_{groups}), we coded age range, mean age, and its SD (either for the whole sample or separately for every age group). We further investigated the potential moderation by the mean age (of the whole sample or corresponding age groups) as previous research demonstrated a drop in prosociality in the highest age, indicating that the positive relationship between age and prosociality becomes negative in the oldest old (for a review see Wiepking & James, 2013). Moreover, in a previous meta-analysis, it was suggested that the mean age of OA significantly impacted the association between age and altruism, but in a negative way (Sparrow et al., 2021).

***Prosocial measure*.** The second key variable of interest was the measure of prosociality used in the study. Following recent evidence that behavioral and self-report of prosociality are fairly independent (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Dang et al., 2020; Tusche & Bas, 2021; Wilhelm et al., 2018), we differentiated them into two separate meta-analyses (MA_{behav} for behavioral prosociality including behavioral tasks (GTC) like EG, SVO, and donation paradigms (DP) vs. MA_{SRM} for self-reported trait prosociality). In both cases, the exact type and description of the measure was extracted (e.g., DG, UG, SVO for MA_{behav} and e.g., altruism scale, prosocialness scale for MA_{SRM}, see Tables S3 and S5). Even though all of them assess the construct of prosociality, they do differ with respect to structural characteristics, which in turn means that different psychological processes are afforded and expressed with respect to the specific behavior elicited by different games (Thielmann et al., 2021). The recently published *affordance-based framework* (Thielmann et al., 2020) assumes that interdependent situations, as elicited in GTC, activate certain personality traits, either associated with prosocial or selfish behavior, across four broad situational affordances: possibility for exploitation, and reciprocity, temporal conflict, and dependence under uncertainty. Depending on the structural characteristics of the applied game, one or more specific situational affordances will model the behavior and activate specific psychological processes. This means that the different GTCs included in the current meta-analysis can be assigned to the four different affordances and thus might shed light on underlying psychological processes. Given the fact that many pre-defined moderators associated with specific game characteristics (see Table S1 and S6) could not be analyzed in the current meta-analysis, we decided to include the measure as a moderator variable (moderator *measure*). See Table S3 and S5 for the specific measures used per study.

Applied incentive. In the case of MA_{behav}, we coded whether the behavioral measure involved real payouts (e.g., money or other resources), i.e., incentives contingent on participants' behavior, or depending on the game structure contingent on the behavior of the partner (including when participants were made believe they would receive real payouts contingent on their decision-making), or whether the task included a hypothetical decision without any task-contingent payout. Even though results are mixed, studies could illustrate different levels of prosociality depending on whether they used a behavior-contingent incentive structure or hypothetical decisions (Bühren & Kundt, 2015; Camerer et al., 1999; Engel, 2011; Fantino et al., 2007; Fetchenhauer & Dunning, 2009; Gillis & Hettler, 2007; Lönnqvist et al., 2011; Thielmann et al., 2016; L. Zhang & Ortmann, 2014). To investigate whether real payout contingent on the behavior in the game versus hypothetical decisions impacts the relationship between age and prosociality, we used it as a moderator in a meta-regression model (moderator *incentive*).

Interaction. Additionally, for studies included in MA_{behav} we coded whether the game involved a one-shot interaction versus repeated interactions. The latter includes the possibility of reciprocity, one typical affordance in EG, meaning a player can adjust his behavior according to the previous behavior of its partner. Therefore, the participants play repeatedly together in sequential interactions (see Thielmann et al., 2021). In one-shot interactions, the individuals interact only once in one trial, not for several rounds. It is well established that people behave differently in one-shot versus repeated interaction scenarios (Engel, 2011; Fehr & Gintis, 2007; Gintis, 2000; Gintis et al., 2003). We therefore test whether age effects on prosociality differ, depending on whether participants played in repeated reciprocal or one-shot interactions (moderator *interaction*).

Study setting. Further, for every study included in MA_{behav}, we examined whether the study was conducted in the lab, online, or in a (lab-in-the) field setting. Laboratory experiments are seen as the gold standard in social sciences (e.g., Rubin, 2008), but recently, online labor markets such as Amazon Mechanical Turk or Prolific Academic have become a viable option for data collection in large and often heterogeneous samples (Buhrmester et al., 2018). Even though lab and online studies are oftentimes criticized for their lack of generalizability to real life (Levitt & List, 2007), there is evidence for external validity (e.g., Franzen & Pointner, 2013; Potters & Stoop, 2016; Walter et al., 2019). However, another method that could lead to important complementary insights regarding daily life represent lab-in-the-field experiments, a combination of lab and natural field experiments, by using standardized and validated tasks from laboratory experiments but in a naturalistic setting (Gneezy & Imas, 2017). Given the fact that online participation is associated with higher anonymity and less expectations of social desirability (e.g., Joinson, 1999), this might increase selfish behavior, as higher anonymity in the lab is associated with reduced prosociality (e.g., Krysowski & Tremewan, 2021). Therefore, to account for the potential effect of different settings with respect to the association of age and prosociality, we included setting as a moderator variable in MA_{behav} (moderator *setting*).

Publication status. We coded whether the effect was published or unpublished. Publication bias is discussed as a substantial threat with respect to the validity of meta-analyses, leading to an overestimation of published effects and false-positive results (Lane & Dunlap, 1978). Even though different methods are established to assess publication bias in meta-analyses (for an overview, see Rothstein et al., 2006), their limitations are discussed (Thornton, 2000). Demonstrating our success in including as many unpublished effects as possible, we use publication status as a moderator variable (moderator *publication status*). Whereas published effects include peer-reviewed published studies or dissertations reporting specific age effects on prosociality (even though age was only included as a covariate in a regression model), unpublished effects include both completely unpublished datasets or datasets with an eligible age range and prosociality measure that were published but did not investigate age effects in their initial publication. In the case of $MA_{cont\text{-}quad}$, only studies specifically reporting an analysis of quadratic age effects are coded as published effects (e.g., Cutler et al., 2021; Rieger & Mata, 2015). All other studies are coded as unpublished effects.

Additional study characteristics. We further coded information about the sample characteristics of the included studies, namely, sample size, sex composition (i.e., percentage female), nationality of the sample, country of the institution, and year of publication/data collection (called *year*). As was done in previous meta-analyses on this topic (Engel, 2011; Sparrow et al., 2021), we account for a potential confounding effect of sex composition. Additionally, in order to avoid cohort effects, we adjusted for the nationality of the sample, as recommended and done by previous global sample analyses (e.g., Cutler et al., 2021). In order to be included, studies had to specifically report the nationality of the included sample. This led to the fact that we did not have enough data to represent individual countries, which is why we post-hoc classified the countries into continents (i.e., Asia, Europe, Africa, North America, Oceania). We further adjusted for sample size and year of publication/data collection.

Analytic strategies. All analyses were carried out using RStudio (R Core Team, 2020) and the metafor package (version 3.1.34) (Viechtbauer, 2010). In general, separate random-effect (RE) models were fitted to the data based on the treatment of age ($MA_{cont\text{-}lin}$ vs. $MA_{cont\text{-}quad}$ vs. MA_{groups}) and the measure of prosociality employed (MA_{behav} vs. MA_{SRM}) in the original study if a minimum of k = 5 was available (see Jackson & Turner, 2017). Multi-level meta-analyses with individual effect sizes nested within studies were carried out as sensitivity analyses (see Table S7.2) and only reported if the results significantly differed from the RE model. In this case, we follow a conservative approach and report the non-significant model.

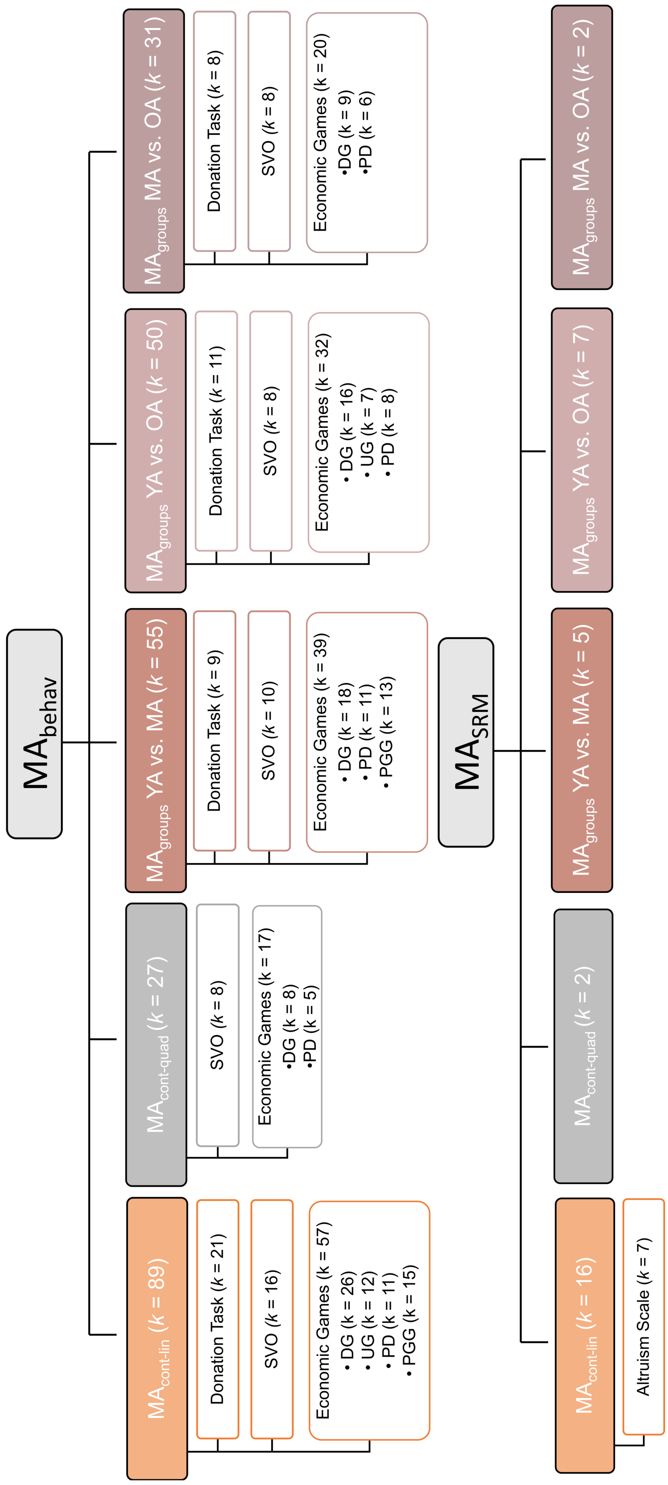
Across all analyses, we proceeded in the following way: In the first step, one main analysis (separate model for $MA_{cont\text{-}lin}$ vs. $MA_{cont\text{-}quad}$ vs. MA_{groups}), including all possible studies (differentiating between MA_{behav} vs. MA_{SRM}) was carried out to estimate the average (meta-analyzed) effect size (see Figure 2 for an overview). For each meta-analysis, results are expressed in terms of k (number of studies included for RE models, or number of effect sizes included for multi-level models), $\hat{\mu}$ (estimated average outcome), CI (95% confidence interval for $\hat{\mu}$), and p-value. Further, a restricted maximum-likelihood estimation was used to estimate the amount of heterogeneity to report $\boxed{\hat{\tau}^2}$ (estimated amount of heterogeneity in the true outcomes; Viechtbauer, 2005). Additionally, we report the results of the Q-test for heterogeneity (Cochran, 1954), as well as I^2 (percentage of the total variability in the observed outcomes due to between-study heterogeneity; Higgins & Thompson, 2002). Whenever the value of I^2 equals 0, the RE model can be reduced to a fixed-effect model due to the absence of heterogeneity. A 95% prediction interval (PI) was reported whenever heterogeneity was detected ($\boxed{\hat{\tau}^2}$ > 0), irrespective of the Q-test (Riley et al., 2011). The PI indicates the 95% probability that the effect size of a new, randomly selected study originating from the same population as the studies already included will be covered by the range of this interval. A leave-one-out diagnostic procedure, as recommended by Viechtbauer (2010) and Viechtbauer and Cheung (2010), was applied to detect potential outliers and/or overly influential studies in the context of the specific model, with the help of studentized residuals and Cook's distances. If no qualitative change in results was observed by excluding the overly influential and/or outlier studies, the studies remained included (see Table S7.1 for such sensitivity analyses). This diagnostic procedure has also been used in previous recent meta-analyses (e.g., Habeck & Schultz, 2015; Nouwen et al., 2019; van Valkengoed & Steg, 2019). Furthermore, to investigate the potential effect of publication bias, we carried out the rank correlation test (Begg & Mazumdar, 1994), the regression test (Sterne & Egger, 2005) and created funnel plots to examine all meta-analyses for indication of publication bias (see supplementary for funnel plots). If a model did not converge, we changed the number of iterations via the *control* argument of the metafor package (Viechtbauer, 2010).

In the next step, meta-regressions were carried out for every analysis with at least k = 10 studies included (Deeks et al., 2022) to test the potential influence of different covariates of interest, namely sample size, year, sex composition (i.e., percentage female), mean age, nationality, publication status, setting, measure, interaction, and incentive. An omnibus test was carried out for all coefficients included in the specific model (Q_M test). Beside the result of the Q_M test, we report the estimated regression coefficients (i.e., β), as well as the corresponding SE. Either to indicate how much the average estimated outcome (correlation coefficient, Hedge's g, standardized β-coefficients) changes with a 1-unit increase of the moderator, or to illustrate how much the estimated average outcome differs between the factor levels. The pseudo-R^2-value is reported as an estimate of the amount of heterogeneity for which the specific moderator accounted (Raudenbush, 2009). Further, we computed adjusted effects based on the specific meta-regression model, whereas the predicted effect is either estimated on the average of the moderator variable (e.g., average sample size) or on the mean of the corresponding dummy variables. The latter represents an estimate of a population of studies with the same relative frequencies as the levels of the categorical moderator (Viechtbauer, 2022). Outliers and/or overly influential studies were identified via studentized residuals or Cook's distances (Viechtbauer & Cheung, 2010), and sensitivity analyses were carried out. If the exclusion of these specific studies did not change the results qualitatively, they remained included. Whenever sensitivity analyses resulted in qualitatively different results, the full and reduced model is reported in the referring meta-regression tables in the supplementary.

We further set up measure-specific sub-meta-analyses to test whether age effects on prosociality vary as a function of different measures (i.e., GTCs or questionnaires) of prosociality applied in the original studies. For details and model results, see supplementary Table S14-16.

Figure 2

Diagram representing the number of studies included in the different meta-analyses



Note. RE models were only carried out (and with respect to sub-meta-analyses are only depicted in the figure) if a minimum of k = 5 was available. k represents the number of independent papers/studies/articles, not the number of effect sizes. For sub meta-analyses, see supplementary Table S14-S16.

Results

In the following, we report on the results of the meta-analyses (RE/multi-level models) covering lifespan studies with a continuous ($MA_{cont\text{-}lin}$) and quadratic treatment of age ($MA_{cont\text{-}quad}$), as well as a categorical treatment of age (MA_{groups}) whilst differentiating between behavioral (MA_{behav}) and self-reported prosociality (MA_{SRM}) respectively. Note that a limited number of studies available for MA_{SRM} precluded separate meta-analyses for $MA_{cont\text{-}quad}$ (k = 2) and MA_{groups} (MA vs. OA, k = 2). Descriptive information and a summary of study characteristics corresponding to the different meta-analyses are reported in Table 1. See Figure 2 for an overview of all analyses. Measure-specific sub-analyses with respect to $MA_{cont\text{-}lin}$, $MA_{cont\text{-}quad}$, and MA_{groups} are reported in the supplementary Tables S14-16.

Linear age effects of behavioral prosociality ($MA_{cont\text{-}lin}$ – MA_{behav}). The $MA_{cont\text{-}lin}$-MA_{behav} RE model (k = 89, n = 100,613) revealed a small but significant effect ($\hat{\mu}$ = 0.04, p < 0.001) of adult age on behavioral prosociality. This result (see Table 2 and Figure 3) indicates meta-analytical evidence for a small linear increase in behavioral prosociality across the adult lifespan. No funnel plot asymmetry was observed, suggesting no indication of a potential publication bias (ps > 0.14; see Figure S1).

The Q-test was significant ($Q(88)$ = 375.22, p < 0.001), providing evidence for substantial heterogeneity in the distribution of these effect sizes. To explore some of this detected variability, meta-regression models were estimated, including pre-defined moderators (see Table 3 for a list of moderators). Only the moderator incentive significantly impacted the linear relationship between age and behavioral prosociality ($QM(1)$ = 4.12, p = 0.04, R^2 = 10.81%, k = 74). Real payoff, i.e., behavior-contingent incentive, was associated with a significant effect size and thus with an age-related increase in behavioral prosociality. This indicates that in studies using behavior-contingent incentives, higher age was associated with higher behavioral prosociality ($\hat{\mu}$ = 0.04, p < 0.001, 95% CI [0.02, 0.07], k = 57). This age-related increase in behavioral prosociality was not observed in studies using hypothetical decisions ($\hat{\mu}$ = -0.001, p = 0.96, 95% CI [-0.04, 0.04], k = 17). The adjusted effect of age in the model, including incentive, remained significant, with a similar estimate of $\hat{\mu}$ =0.03 (p > 0.01, 95% CI [0.01, 0.05], 95% PI [-0.09, 0.16]). None of the other moderators were significant (all ps > 0.05, see Table S8).

Table 1

Descriptive information and study characteristics of the different meta-analyses

	$MA_{cont\text{-}lin}$ – MA_{behav}	$MA_{cont\text{-}lin}$ – MA_{SRM}	$MA_{cont\text{-}quad}$ – MA_{behav}^{α}
k_{art} (k_{st})	72 (89)	14 (16)	27 (27)
No. of effect sizes	116	19	37
Year (*Md*)	2002-2022 (2018)	2006-2021 (2019)	2002-2022 (2019)
Unpubl. Effects (%)	57.30%	56.25%	88.89%
N	100,613	7,064	82,990
M Age (*SD*)	38.97 (12.83)	42.19 (13.04)	42.99 (14.67)
Age range	18-100	17-91	18-99
Female (%)	49.28%	57.94%	47.60%

	MA_{groups} – MA_{behav}					
	YA vs. MA		YA vs. OA		MA vs. OA	
k_{art} (k_{st})	50 (55)		49 (50)		31 (31)	
No. of effect sizes	74		67		43	
Year (*Md*)	2002-2022 (2018)		2002-2022 (2019)		2002-2022 (2019)	
Unpubl. Effects (%)	52.73%		28.00%		45.16%	
N	35,925	36,892	32,996	18,028	35,130	16,816
M Age (*SD*)	26.61 (4.42)	45.72 (6.68)	25.49 (4.02)	68.35 (5.48)	46.61 (6.67)	66.60 (5.08)
Age range	18-39	26-59	17-45	55-100	35-59	60-99
Female (%)	47.60%	53.66%	51.78%	50.90%	51.80%	46.88%

	MA_{groups} – MA_{SRM}			
	YA vs. MA		YA vs. OA	
k_{art} (k_{st})	5 (5)		6 (7)	
No. of effect sizes	8		9	
Year (*Md*)	2020-2021 (2021)		2008-2021 (2018)	
Unpubl. Effects (%)	40%		14.29%	
N	1,395	1,284	914	794
M Age (*SD*)	27.60 (6.51)	45.58 (6.51)	23.74 (3.52)	70.41 (5.60)
Age range	18-35	36-59	18-35	60-97
Female (%)	61.47%	58.4%	62.62%	54.29%

Note. k_{art} = final sum of included papers, k_{st} = final sum of included studies (independent samples). unpubl. effects = unpublished effects. If more effect sizes than studies were available (caused by different measures within one study/sample), these effect sizes were aggregated.

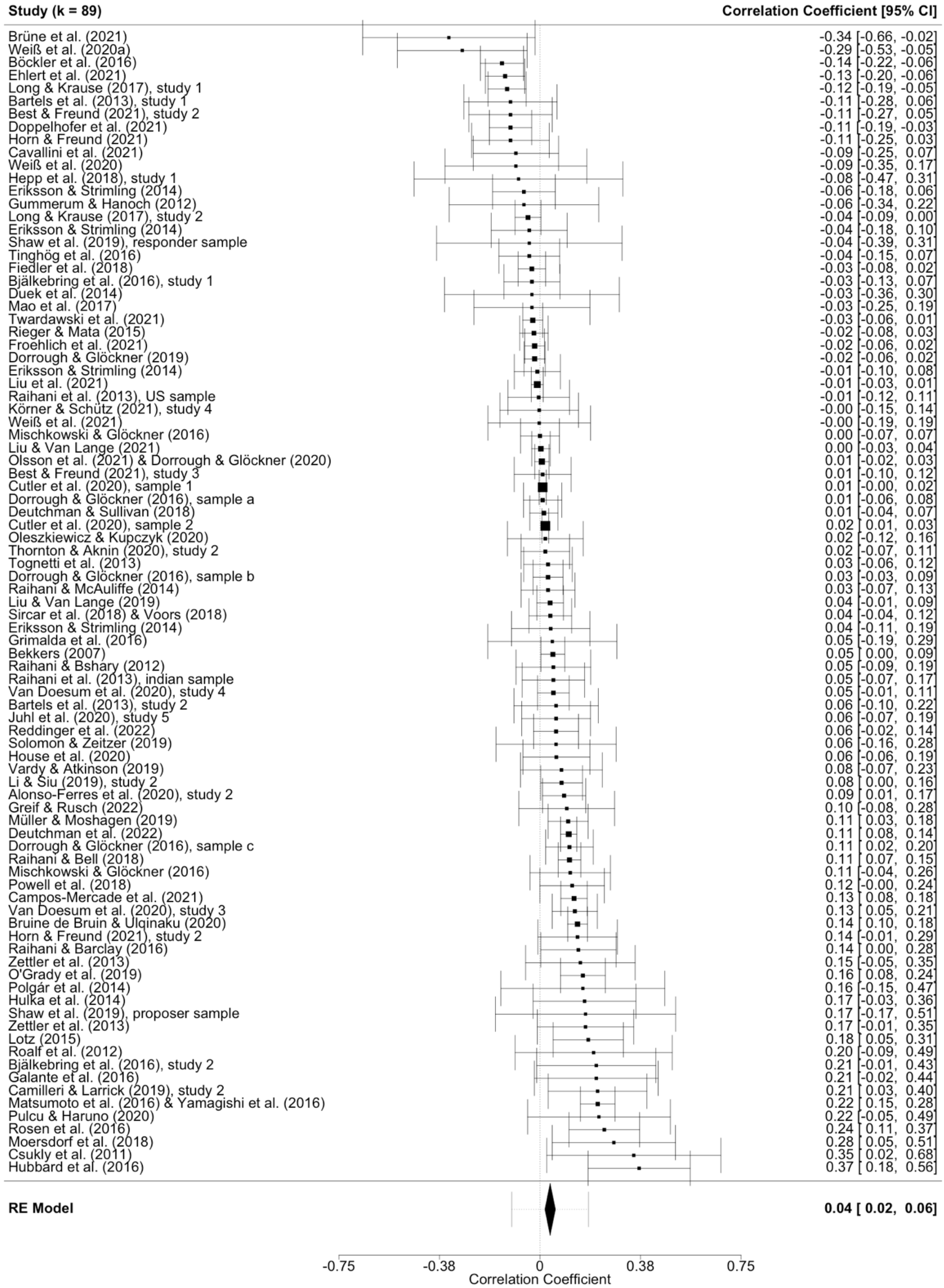
$^{\alpha}$ Note that in this table, the originally identified set of studies (k = 22) is reported. Over the course of the analysis, k = 4 studies were excluded as they were identified as overly influential/outliers (see Table S7.1 for sensitivity analysis) based on our pre-defined criteria of studentized residuals and Cook's distances.

Linear age effects of self-reported prosociality ($MA_{cont\text{-}lin}$ – MA_{SRM}). The $MA_{cont\text{-}lin}$-MA_{SRM} RE model (k = 16, n = 7,064) revealed a significant average effect size of age on self-reported prosociality ($\hat{\mu}$ = 0.09, p = 0.01). This finding mirrors the significant linear increase of prosociality across the adult lifespan we had detected with respect to behavioral prosociality (see Table 2 and Figure 4). No funnel plot asymmetry was observed, indicating no evidence of publication bias in this model (ps > 0.31; see Figure S2).

A Q-test indicated significant heterogeneity among effects, $Q(15) = 111.21$, $p < 0.001$. To explore this significant amount of heterogeneity within our set of studies and to identify possible underlying factors, different meta-regressions were carried out (see Table 3 for a list of moderators). One significant moderation effect was observed for sex contribution (i.e., percentage female; β = -0.01, p = 0.003, R^2 = 39.05%, k = 16). A higher percentage of females included in an original study was significantly associated with lower effect sizes, i.e., a weaker association of age with self-reported prosociality (see Figure S3). The adjusted effect was still significant and qualitatively only slightly different from the model without this specific moderator ($\hat{\mu}$ = 0.08, p > 0.01, 95% CI [0.03, 0.14], 95% PI [-0.11, 0.28]). Further, the moderator measure had a significant effect on the linear association between age and self-reported prosociality ($Q_M(3)$ = 28.04, p < 0.001, R^2 = 92.29%, k = 12). Only in studies using the HEXACO-PI-R-100 to operationalize prosociality (Lee & Ashton, 2018) the average estimated correlation coefficient of age with prosociality was observed to be significant ($\hat{\mu}$ = 0.23, p < 0.01, 95% CI [0.30, 0.15], k = 2). None of the other assessments showed a significant linear increase of self-reported prosociality across the adult lifespan when computing their estimated average effect size (all ps > 0.11). Note that the adjusted effect of age on self-reported prosociality across the different measures remained significant, even when adjusting for the effect of the moderator measure ($\hat{\mu}$ = 0.07, p < 0.01, 95% CI [0.03, 0.10], 95% PI [0.004, 0.13]). None of the other moderators was significant (all ps > 0.05; see Table S9).

Figure 3

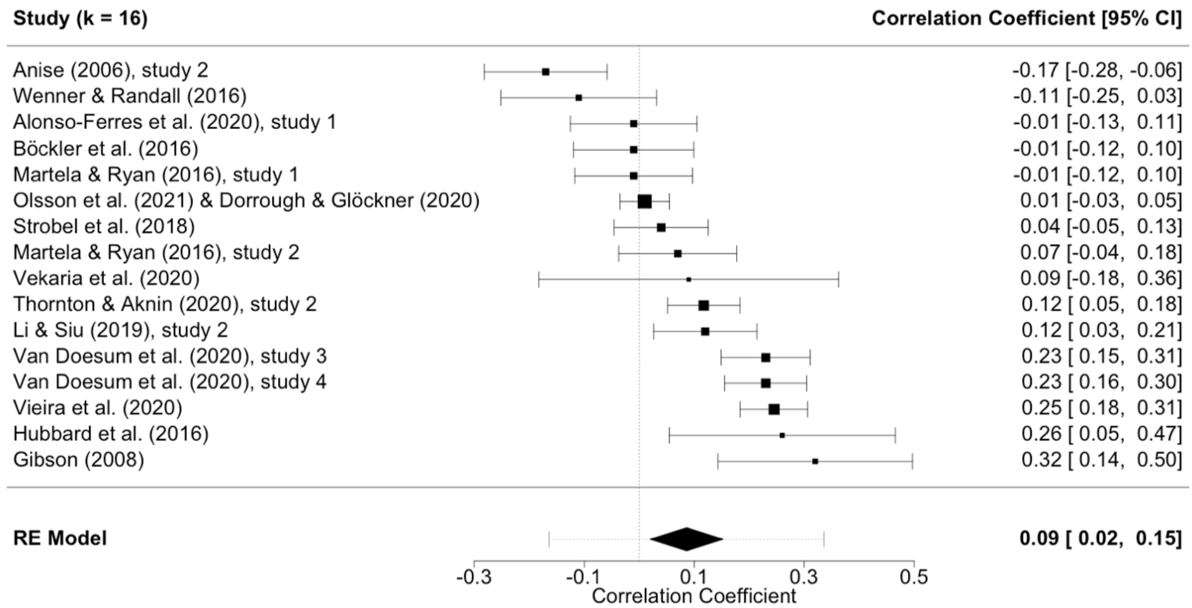
Forest plot for studies covering linear age effects ($MA_{cont\text{-}lin}$) on behavioral prosociality (MA_{behav})



Note. k = 89 studies, n = 100,613. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Positive effect sizes indicate a positive relationship between age and behavioral prosociality. The RE model on the bottom represents the average effect size resulting from a RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted error bars.

Figure 4

Forest plot for studies covering linear age effects ($MA_{cont\text{-}lin}$) on self-reported prosociality (MA_{SRM})



Note. k = 16 studies, n = 7,064. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Positive effect sizes indicate a positive relationship between age and self-reported prosociality. The RE model on the bottom represents the average effect size resulting from a RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars.

Table 2

Age-differences in behavioral and self-reported prosociality across all included studies

Meta-Analysis	*k*	$\hat{\mu}$ (SE)	*z*-val	*p*	95% CI	95% PI	*Q*	t^2	I^2
$MA_{cont\text{-}lin} - MA_{behav}$ (RE)	89	0.04 (0.01)	3.79	< 0.01***	[0.02, 0.06]	[-0.11, 0.18]	375.22***	0.01	85.74%
$MA_{cont\text{-}lin} - MA_{SRM}$ (RE)	16	0.09 (0.03)	2.52	0.02	[0.02, 0.15]	[-0.16, 0.34]	111.20*	0.02	87.87%
$MA_{cont\text{-}quad} - MA_{behav}{}^{\alpha}$ (RE)	23	-0.14 (0.05)	-2.50	0.01	[-0.24, -0.03]	[-0.24, -0.03]	8.18	0	0.00%
MA_{groups}- MA_{behav}: YA vs. MA (RE)	55	0.09 (0.03)	3.42	< 0.01***	[0.04, 0.14]	[-0.21, 0.39]	246.83***	0.02	87.13
MA_{groups}- MA_{SRM}: YA vs. MA (RE)	5	0.23 (0.10)	2.30	0.02	[0.03, 0.42]	[-0.18, 0.64]	25.62***	0.03	78.05%
MA_{groups}- MA_{behav}: YA vs. OA (RE)	50	0.20 (0.05)	3.94	<0.01***	[0.10, 0.31]	[-0.45, 0.86]	285.21***	0.11	95.45%
MA_{groups}- MA_{SRM}: YA vs. OA (RE)	7	0.48 (0.24)	2.02	0.04	[0.01, 0.95]	[-0.77, 1.73]	63.25***	0.35	93.84%
MA_{groups}- MA_{behav}: MA vs. OA (multi-level)	43	0.05 (0.03)	1.80	0.07	[-0.004, 0.10]	[-0.20, 0.29]	187.50***		78.33%

Note. k = number of included independent studies; $\hat{\mu}$ = effect size estimate from RE model; *SE* = standard error; z-val = z-value; *p* = p-value; CI = confidence interval; PI = prediction inveral; *Q* = Cochran's Q statistic, t^2 = estimated amount of heterogeneity in the true outcomes; I^2 = percentage of total variability in the observed outcomes due to heterogeneity.

$^{\alpha}$ Model representing the adjusted effect (adjusting for the linear standardized beta coefficient/lower order term). The Q-value represents the residual heterogeneity.

*** p < 0.001

Quadratic age effects of behavioral prosociality ($MA_{cont\text{-}quad}$ – MA_{behav}). Four studies were detected to be outliers/overly influential based on studentized residuals and/or Cook's distances and were excluded from the model (see supplementary Tables S7.1 and S7.2 for sensitivity analyses). The reduced RE model $MA_{cont\text{-}quad}$-MA_{behav} *(k* = 23, *n* = 50,613) probing quadratic age effects on behavioral prosociality yielded a significant quadratic effect ($\hat{\mu}$ = -0.14, *p* = 0.01, see Table 2 and Figure 5), adjusted for the linear age term. This negative standardized quadratic beta coefficient indicates an inverted U-shaped pattern with respect to the quadratic relationship of age and behavioral prosociality, describing higher values of behavioral prosociality already in midlife. No funnel plot asymmetry was observed, indicating no evidence for publication bias in this model (*p*s > 0.23, see Figure S4).

We note that Figure 5 indicates one study showing a particularly strong negative quadratic age effects whilst other studies reported lower negative effects, or even positive However, no evidence for a significant amount of residual heterogeneity among effects was detected, $Q(21) = 8.18$, $p = 0.99$. Based on the assumption that due to statistical power, the Q-test sometimes fails to detect heterogeneity (Lipsey & Wilson, 2001), further meta-regressions were conducted (see table 3 for a list of moderators). However, no further moderators were observed to significantly impact the quadratic effect of age on behavioral prosociality (all *p*s > 0.11; see Table S10). No meta-analysis with respect to MA_{SRM} was carried out due to the limited number of studies (*k* = 2).

Table 3

An overview of the moderators tested per model

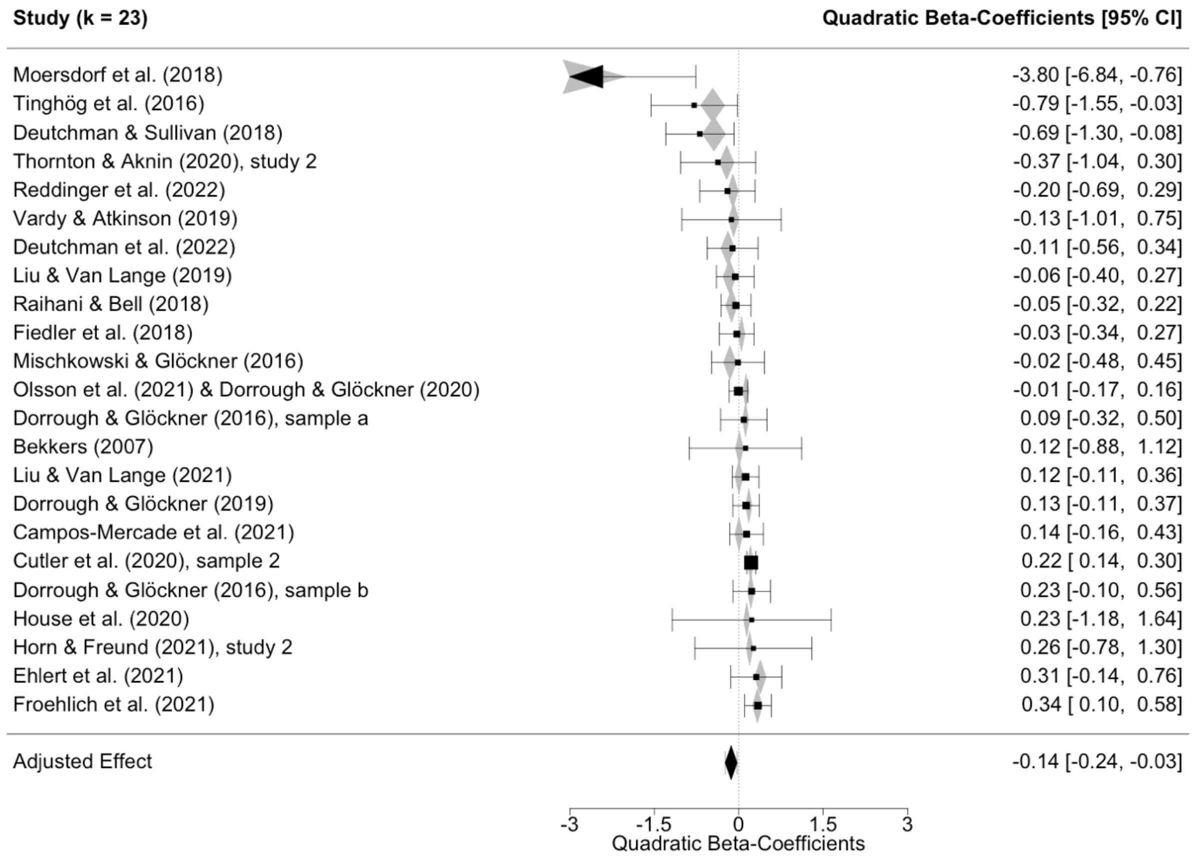
Meta-Analysis	*N*		Year	Sex		Age		P. status	Setting	Measure	Incentive	Interaction	Continent
$MA_{cont\text{-}lin} - MA_{behav}$	X		X	X		X		X	X	X	X	X	X
$MA_{cont\text{-}lin} - MA_{SRM}$	X		X	X		X		X	X	X			X
$MA_{cont\text{-}quad} - MA_{behav}$	X		X	X		X				X	X	X	X
Meta-Analysis	*n* YA	*n* MA	Year	Sex YA	Sex MA	Age YA	Age MA	P. status	Setting	Measure	Incentive	Interaction	Continent
$MA_{groups} - MA_{behav}$: YA vs. MA	X	X	X	X	X	X	X	X	X	X	X	X	X
Meta-Analysis	*n* YA	*n* OA	Year	Sex YA	Sex OA	Age YA	Age OA	P. status	Setting	Measure	Incentive	Interaction	Continent
$MA_{groups} - MA_{behav}$: YA vs. OA	X	X	X	X	X	X	X	X	X	X	X	X	X
Meta-Analysis	*n* MA	*n* OA	Year	Sex MA	Sex OA	Age MA	Age OA	P. status	Setting	Measure	Incentive	Interaction	Continent
$MA_{groups} - MA_{behav}$: MA vs. OA	X	X	X	X	X	X	X	X	X	X	X	X	X

Note. Meta-regressions were only carried out for analyses with at least $k = 10$ studies included. Both $MA_{groups} - MA_{SRM}$ comparing YA vs. MA and YA vs. OA had $k < 10$ studies; n = sample size; year = year of publication/data collection; sex = percentage females; age = mean age; p. status = publication status, published vs. unpublished effect; setting = lab vs. lab in field vs. online study; incentive = hypothetical vs. behavior-contingent incentive; interaction = one-shot vs.

repeated interaction; continent = continent with respect to the nationality of the sample; More information on specific meta-regression models are in the corresponding tables in the supplementary and the specific results section.

Figure 5

Forest plot for studies covering quadratic age effects ($MA_{cont\text{-}quad}$) on behavioral prosociality (MA_{behav})



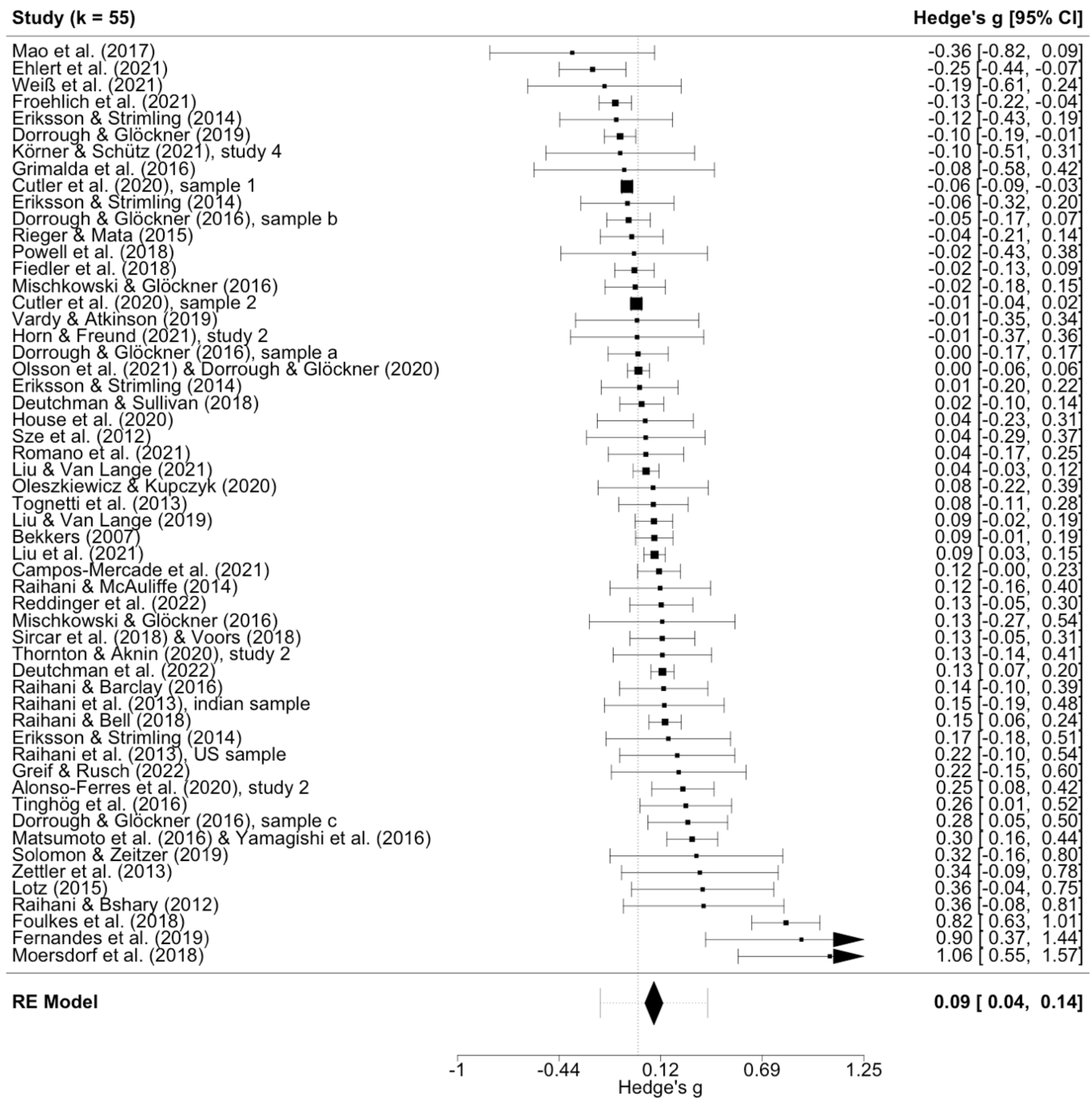
Note. k = 23 studies, n = 50,613. Adjusted effect w/o Cutler et al. (2021, sample 1), Liu et al. (2021), Rieger & Mata (2015), and Sircar et al. (2018). The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Negative effect sizes indicate an inverted U-shaped relationship between age and behavioral prosociality. The RE model on the bottom represents the average effect size resulting from a RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars.

Age group comparison of behavioral prosociality in YA versus MA (MA_{groups} – MA_{behav}). With respect to the MA_{groups}-MA_{behav} RE model comparing YA and MA (k = 55 studies, n_{YA} = 35,925, n_{MA} = 36,892), we observed significant age differences in behavioral prosociality ($\hat{\mu}$ = 0.09, p < 0.001). Again, these results (see Table 2 and Figure 6) point towards meta-analytical evidence for age-related differences in behavioral prosociality. More specifically, they indicate that MA show higher behavioral prosociality compared to YA. There was an indication for funnel plot asymmetry with respect to a significant regression test (p = 0.03, see figure S5), but due to the high number of unpublished effects included (53% unpublished effects), the risk of publication bias was estimated to be low.

Given a significant Q-test suggesting heterogeneity among the effects ($Q(54) = 246.83$, $p < 0.001$), we aimed to explore this variance in further meta-regressions (see Table 3 for a list of moderators). We found a significant moderation effect regarding the mean age of the MA groups (β = -0.04, p < 0.001, R^2 = 54.27%, k = 55), such that higher mean age in MA was significantly associated with lower effect sizes (see Figure S6). That is interestingly, effects for the difference between YA and MA were particularly pronounced in studies that included participants that were, on average, closer to the lower bound of the middle-age range. The adjusted effect of age on behavioral prosociality in the model that included the mean age of the MA sample was still significant and similar to the RE model without any covariates ($\hat{\mu}$ = 0.09, p < 0.001, 95% CI [0.05, 0.13], 95% PI [-0.11, 0.29]). Another significant moderation effect was found for interaction ($Q_M(1) = 5.12$, p = 0.02, R^2 = 0.60%, k = 37), indicating that the average effect size differed across the type of interaction (one-shot vs. repeated) used in the behavioral paradigm. Analyzing the estimated average effect size for the different factor levels showed significant age differences between YA and MA whenever one-shot interactions were applied ($\hat{\mu}$ =0.09, p < 0.01, 95% CI [0.04, 0.14], k = 30). No significant age differences were found for repeated interactions ($\hat{\mu}$ =-0.03, p = 0.54, 95% CI [-0.12, 0.06], k = 7). This indicates that whenever prosociality is measured with one single interaction between two individuals (i.e., no possibility of reciprocity), age differences are more pronounced. However, the overall difference between YA and MA in terms of behavioral prosociality was still significant, even when adjusting the effect for the moderator interaction ($\hat{\mu}$ =0.07, p < 0.01, 95% CI [0.02, 0.11], 95% PI [-0.12, 0.25]). We did not find any other significant moderation effects (all ps > 0.05, see Table S11).

Figure 6

Forest plot for studies covering age group differences (MA_{groups}) in YA vs. MA regarding their behavioral prosociality (MA_{behav})

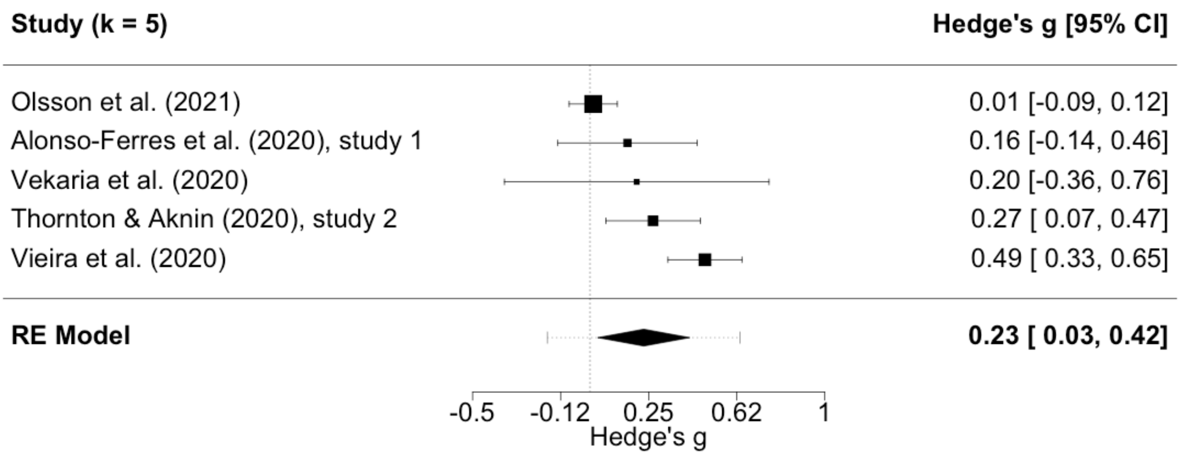


Note. k = 55 studies, n_{YA} = 35,925, n_{MA} = 36,892. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Positive effect sizes indicate higher prosociality in MA as compared to YA. The RE model on the bottom represents the average effect size resulted from the RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars.

Age group comparison of self-reported prosociality in YA versus MA ($\mathrm{MA_{groups}}$ – $\mathrm{MA_{SRM}}$). The RE model probing differences between YA and MA ($\mathrm{MA_{groups}}$) with respect to SRM ($\mathrm{MA_{SRM}}$) (k = 5, n_{YA} = 1,395, n_{MA} = 1,284) revealed a significant group effect ($\hat{\mu}$ = 0.23, p = 0.02). This constitutes meta-analytical evidence for age-related differences in YA and MA with respect to prosociality, as measured with self-reported questionnaires (see Table 2 and Figure 7). MA were significantly more prosocial than YA. Of note, this mirrors the group differences between YA and MA observed on behavioral measures of prosociality. There was no indication for funnel plot asymmetry (ps > 0.96, see Figure S7). Further significant heterogeneity among the effects was observed ($Q(4)$ = 25.62, p < 0.001).

Figure 7

Forest plot for studies covering age group differences ($\mathrm{MA_{groups}}$) in YA vs. MA regarding their self-reported prosociality ($\mathrm{MA_{SRM}}$)



Note. k = 5 studies, n_{YA} = 1,395, n_{MA} = 1,284. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% Cis. Positive effect sizes indicate higher prosociality in MA as compared to YA. The RE model on the bottom represents the average effect size resulted from the RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars.

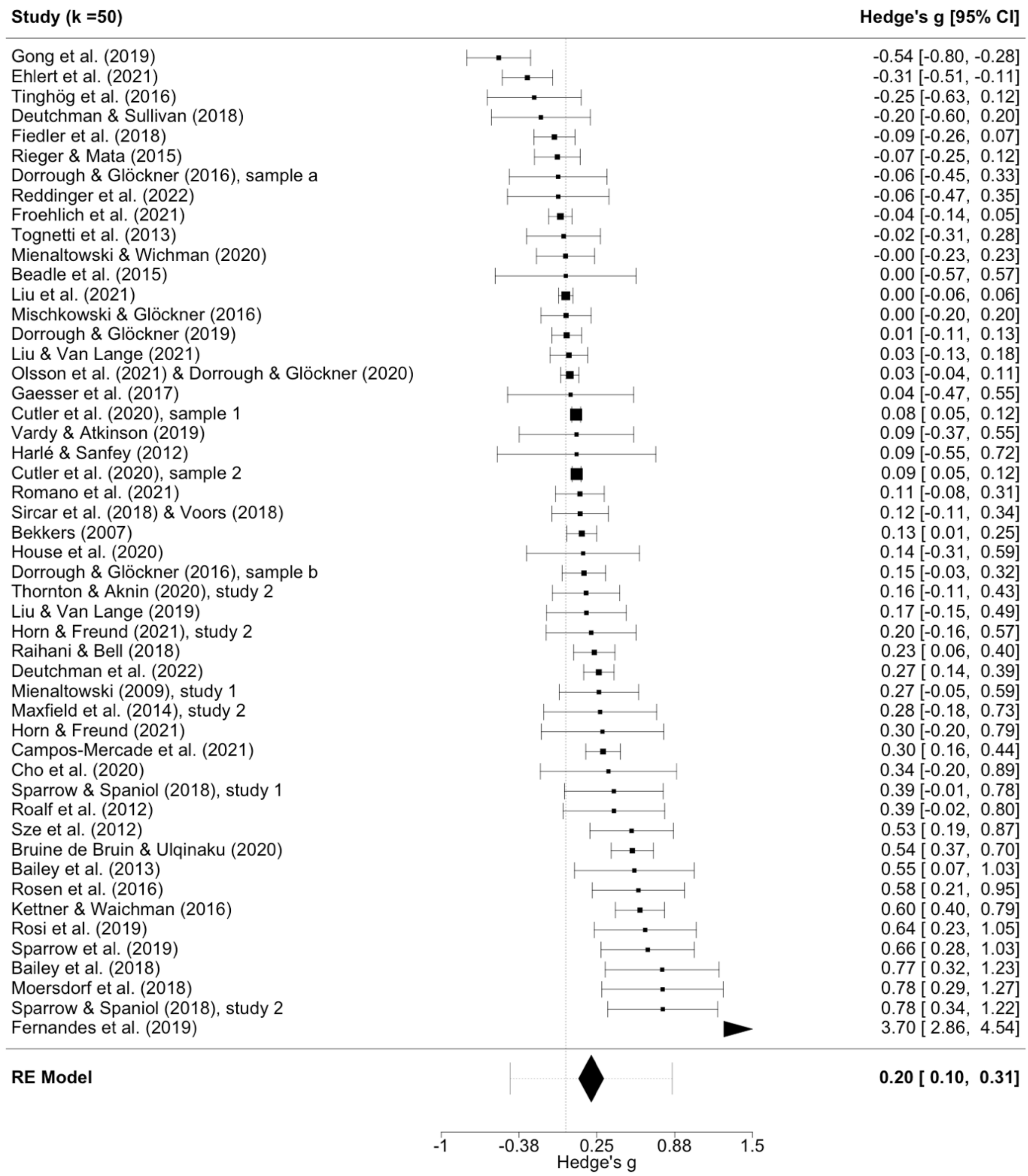
Age group comparison of behavioral prosociality in YA versus OA ($\mathrm{MA_{groups}}$ – $\mathrm{MA_{behav}}$). The RE model (k = 50, n_{YA} = 32,996, n_{OA} = 18,028) revealed significant age-group differences between YA and OA regarding behavioral prosociality ($\hat{\mu}$ = 0.20, p < 0.001). Again, this mirrors meta-analytical evidence for age-related differences in behavioral prosociality, with OA behaving significantly more prosocial than YA (see Table 2 and Figure 8). Evidence for funnel plot asymmetry was indicated by the regression and rank correlation test (p < 0.03, see

Figure S8). However, with almost 30% unpublished effects included, the risk of a potential publication bias was estimated to be low.

With respect to a highly significant Q-test ($Q(49) = 285.21$, $p < 0.001$) indicating heterogeneity of the effect across studies, further meta-regressions were carried out (see Table 3 for a list of moderators). We found that the mean age of YA significantly negatively predicted the age group differences between YA and OA ($\beta = -0.04$, $p = 0.004$, $R^2 = 29.05\%$, $k = 50$). A higher average age of the YA groups was associated with lower effect sizes, which means that the age group differences between YA and OA with respect to behavioral prosociality were significantly stronger the younger the YA groups were on average (see Figure S9). However, the age group difference between YA and OA regarding behavioral prosociality was still significant, even when adjusting for the effect of the mean age of the YA group ($\hat{\mu} = 0.21$, $p < 0.01$, 95% CI [0.12, 0.30], 95% PI [-0.34, 0.76]). Further, we observed a significant moderation effect of the mean age of OA ($\beta = 0.04$, $p < 0.01$, $R^2 = 14.17\%$, $k = 47$). A higher average age in OA was significantly positively associated with higher effect sizes. More specifically, the older the OA group on average, the stronger the age group differences in behavioral prosociality, with OA being more prosocial than YA (see Figure S10). The adjusted effect was also observed to be significant ($\hat{\mu} = 0.16$, $p < 0.01$, 95% CI [0.09, 0.23], 95% PI [-0.25, 0.57]). Further, we observe that the setting significantly moderated age group differences between YA and OA ($Q_M(2) = 7.54$, $p = 0.02$, $R^2 = 14.54\%$, $k = 47$). This indicates that the average effect size might be different depending on the setting in which a study was conducted. Different estimated average effect sizes were observed when analyzing them separately. Age differences between YA and OA were observed for lab studies ($\hat{\mu} = 0.37$, $p < 0.01$, 95% CI [0.20, 0.54], $k = 18$), but neither for lab in field ($\hat{\mu} = -0.001$, $p = 0.99$, 95% CI [-0.25, 0.25], $k = 7$), nor for online studies ($\hat{\mu} = 0.12$, $p = 0.08$, 95% CI [-0.01, 0.25], $k = 22$). However, the overall age group differences between YA and OA with respect to behavioral prosociality did not change after adjusting for the moderator setting ($\hat{\mu} = 0.20$, $p < 0.01$, 95% CI [0.10, 0.29], 95% PI [-0.40, 0.79]). Finally, a significant moderation effect was found for interaction ($Q_M(1) = 6.99$, $p < 0.01$, $R^2 = 38.83\%$, $k = 30$), indicating different average effect sizes for different types of interactions (one-shot vs. repeated) used in the behavioral paradigm. When analyzing the estimated average effect size for one-shot versus repeated interactions, significant age differences between YA and OA were observed whenever one-shot interactions were applied ($\hat{\mu} = 0.19$, $p < 0.01$, 95% CI [0.11, 0.27], $k = 23$). No significant age differences were found for repeated interactions ($\hat{\mu} = -0.01$, $p = 0.89$, 95% CI [-0.13, 0.11], $k = 7$). Again, this shows that whenever prosociality is measured with respect to one single interaction between two individuals (i.e., without the possibility of reciprocity), differences in behavioral prosociality between YA and OA are more pronounced compared to the application of repeated interactions. However, the differences between YA and OA regarding overall behavioral prosociality remained significant after adjusting for the moderator interaction ($\hat{\mu}$ =0.14, $p < 0.01$, 95% CI [0.07, 0.21], 95% PI [-0.13, 0.41]). None of the other moderators were significant (all *ps* > 0.07, see Table S12).

Figure 8

Forest plot for studies covering age group differences (MA_{groups}) in YA vs. OA regarding their behavioral prosociality (MA_{behav})

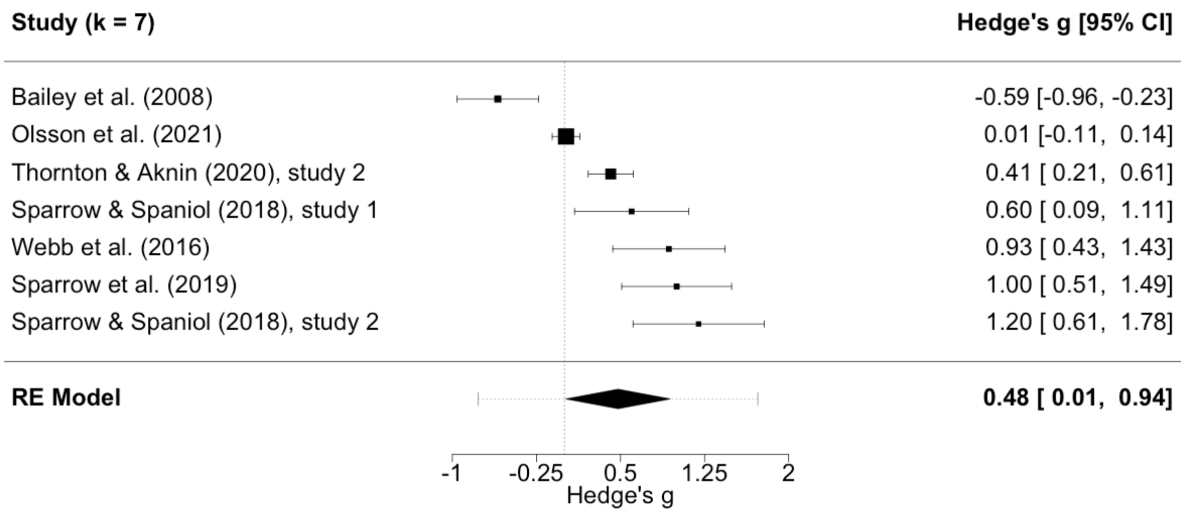


Note. k = 50, n_{YA} = 32,996, n_{OA} = 18,028. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Positive effect sizes indicate higher prosociality in OA as compared to YA. The RE model on the bottom represents the average effect size resulted from the RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars.

Age group comparison of self-reported prosociality in YA versus OA (MA_{groups} – MA_{SRM}). The RE model investigating MA_{groups} and MA_{SRM} in YA versus OA (k = 7, n_{YA} = 914, n_{OA} = 794) yielded a significant effect ($\hat{\mu}$ = 0.48, p = 0.04). This indicated that YA and OA also differed significantly with respect to self-reported prosociality. Specifically, OA were more prosocial than YA (see Table 2 and Figure 9). No evidence for funnel plot asymmetry was observed, indicating that there was no evidence for publication bias in this model (ps > 0.08, see Figure S11). The significant Q-test, $Q(6)$ = 63.248, p < 0.001, provided evidence for substantial heterogeneity in the distribution of these effect sizes.

Figure 9

Forest plot for studies covering age group differences (MA_{groups}) in YA vs. OA regarding their self-reported prosociality (MA_{SRM})



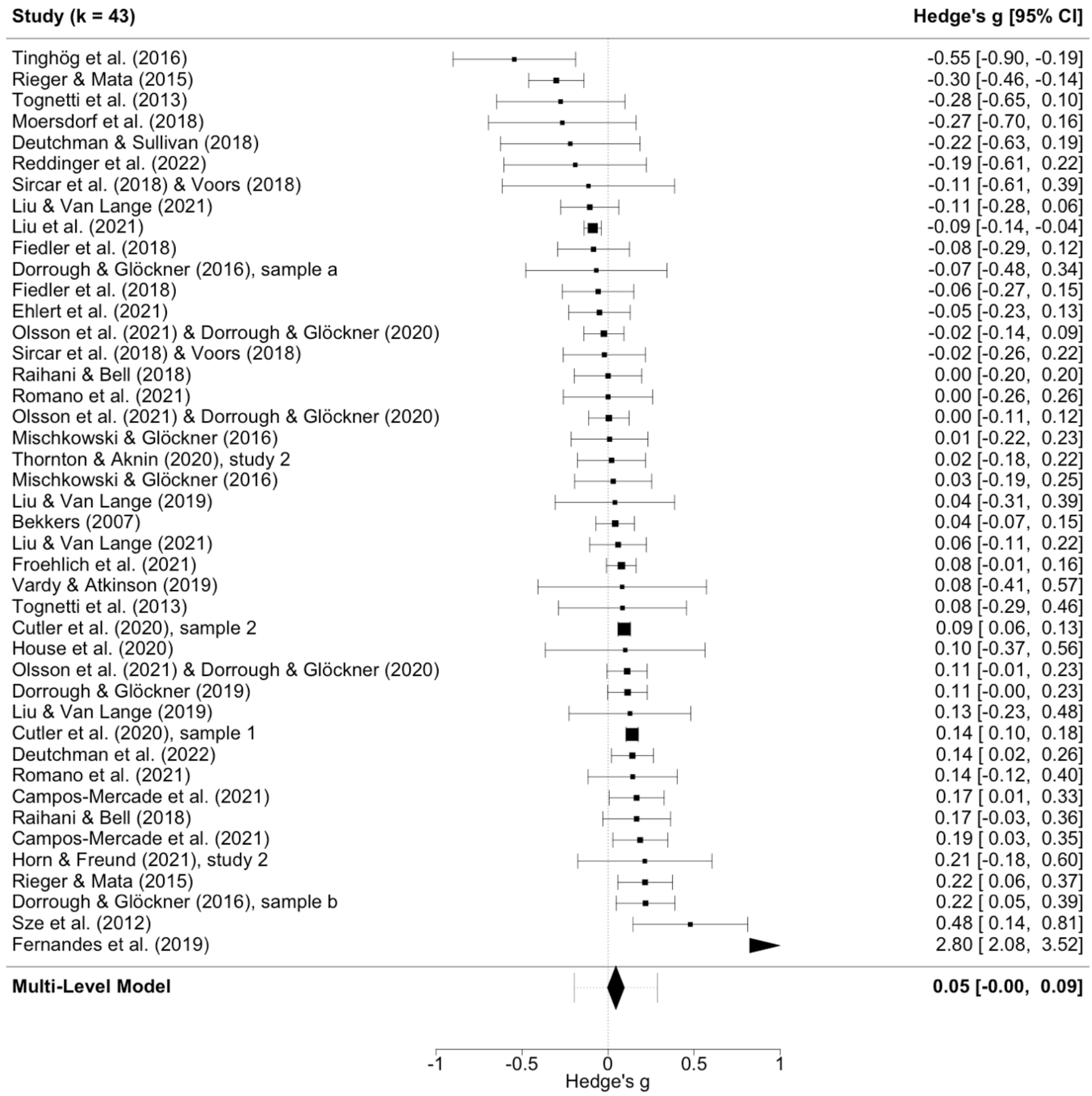
Note. k = 7 Studies, n_{YA} = 914, n_{OA} = 794. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Positive effect sizes indicate higher prosociality in OA as compared to YA. The RE model on the bottom represents the average effect size resulted from the RE meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars.

Age group comparison of behavioral prosociality in MA versus OA (MA_{groups} – MA_{behav}). Whilst the RE model was significant ($\hat{\mu}$ = 0.05, p = 0.04, k = 31), when we accounted for dependency of effect sizes by multi-level modeling, including individual effect sizes nested within studies, the MA_{groups}-MA_{behav} multi-level model (k = 43, n_{MA} = 35,130, n_{OA} = 18,816), comparing MA and OA, failed to reach significance ($\hat{\mu}$ = 0.05, p = 0.07, see Table S7.2). While there was no indication of publication bias in this model when analyzed with the rank-correlation test (p = 0.37, see Figure S12), the regression test showed evidence for funnel plot asymmetry (p = 0.01, see Figure S12). However, with almost half of the included effects being unpublished (45%), the risk of a potential publication bias was estimated to be low.

The Q-test indicated significant heterogeneity (Q(42) = 187.50, p < 0.001). Therefore, further meta-regressions were conducted to explore variability (see table 3 for a list of moderators). We only observed a significant moderation effect of interaction ($Q_M(1)$ = 4.71, p = 0.03, k = 27). This effect showed that the average effect size was significantly different for studies applying one-shot versus repeated interactions in their GTC. By analyzing the estimated average effect size for one-shot versus repeated interaction, we see significant age differences between MA and OA, with OA being more prosocial than MA, only for GTCs using a one-shot design ($\hat{\mu}$ =0.07, p = 0.01, 95% CI [0.02, 0.13], k = 16), but not for repeated interactions ($\hat{\mu}$ =-0.02, p = 0.58, 95% CI [-0.07, 0.04], k = 11). This highlights that whenever a study included a one-shot interaction, age group differences between MA and OA are more pronounced. Notwithstanding, the overall age group differences between MA and OA remained non-significant even after adjusting for the moderator interaction ($\hat{\mu}$ =0.04, p = 0.08, 95% CI [-0.004, 0.08], 95% PI [-0.06, 0.14]). None of the other a-priori defined moderators was a significant predictor (see Table S13, all ps > 0.09). No group-specific meta-analysis comparing MA versus OA with respect to MA_{SRM} was carried out due to the limited number of studies (k = 2).

Figure 10

Forest plot for studies covering age group differences (MA_{groups}) in MA vs. OA regarding behavioral prosociality (MA_{behav})



Note. k = 43, n_{MA} = 35,130, n_{OA} = 18,816. The plot illustrates the distribution of the different effect sizes, indicated by the position of the black square on the x-axis. The vertical line (0) represents no effect. Error bars indicate 95% CIs. Positive effect sizes indicate higher prosociality in OA as compared to MA. The multi-level model on the bottom represents the average effect size resulted from the multi-level meta-analysis, as well as its 95% CI in square brackets and 95% PI by the dotted errors bars. And PI.

Discussion

It is almost a psychological truism that social interactions continue to play an important role across lifespan development. Over the last decades, the adult development of the social mind has come into focus as a prominent research theme (Bailey et al., 2021; Henry et al., 2013; Holt-Lunstad et al., 2010; Luo et al., 2012; Midlarsky & Kahana, 2007; Steptoe et al., 2013). This has demonstrated that the process of lifespan development is not only defined by loss and decrease (like cognitive functioning or physical health) (e.g., Diehr et al., 2013; Lamar et al., 2003) but also by increases, often related to the social mind and life satisfaction (e.g., Blanchflower & Oswald, 2017; Frijters & Beatton, 2012; Reiter et al., 2017; Stietz et al., 2021). In this context, the notion of increasing prosociality when individuals become older has become popular over the last decade (Bailey et al., 2013, 2018; Beadle et al., 2015; Freund & Blanchard-Fields, 2014; Lockwood et al., 2021; Van Lange et al., 1997). In this meta-analysis, we adopt a lifespan approach covering the whole adult lifespan by investigating adult age differences in prosociality across five partially independent meta-analyses. Importantly, we considered linear and quadratic age effects on prosociality across the entire adult life span, as well as including studies comparing different age groups (YA vs. MA, YA vs. OA, and MA vs. OA). Following evidence that behavioral and self-reported prosociality might be partially independent constructs (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Wilhelm et al., 2018), we further considered two independent ways to measure prosociality, namely on a behavioral and a self-reported (trait) level. We found slightly increased behavioral as well as self-reported prosociality with older age. These effects could be backed up by significant age group differences between YA and OA. Further, we observed meta-analytic evidence that the effect of age on behavioral prosociality might be described by a quadratic function. These findings were also described by follow-up meta-analyses capturing age group comparisons: While we did find significant differences between YA and MA in behavioral prosociality, no significant age group differences between MA and OA in behavioral prosociality emerged. Taken together, these results indicate that behavioral prosociality already reaches a peak in midlife (here defined as the age period between 36 and 59 years), whereas there was no evidence of a significant in- or decrease afterward.

Linear age effects on behavioral and self-reported prosociality. Results from two meta-analyses (including behavior and self-report of more than 100.000 participants), which integrated published and unpublished effects of adult age (measured continuously across the adult lifespan from 18 to 100 years), revealed meta-analytical evidence for a (small) linear increase of behavioral and self-reported prosociality across adulthood. In line with these linear effects, an increase in prosociality was also supported by two follow-up meta-analyses (on both the behavioral and self-reported level, including partially overlapping studies as the meta-analysis on linear age effects) of studies reporting age group differences in around 34,000 younger and 19,000 older individuals: They consistently showed higher prosociality in OA as compared to YA. These findings are in accordance with lifespan developmental theories (e.g., Brandtstädter et al., 2010; Carstensen et al., 1999; Liu et al., 2022; Van Lange et al., 1997) and a recently published meta-analysis on aging and altruism (Sparrow et al., 2021), which demonstrated higher degrees of altruism in human aging. Moderator analyses revealed that an age difference in behavioral prosociality was more evident the younger the YA and the older the OA included in a study were on average. This demonstrates that the larger the gap between the two age groups, the stronger the effect a specific study showed.

Of note, the positive effect of age on prosociality (evident as a linear age effect and within age group differences) was found both on prosocial behavior as measured in experimental tasks (prominently including EG) but also on self-reported prosociality (measured via validated questionnaires). The convergence of these age effects across both measurement modalities is interesting in light of findings that prosocial behavior and self-reported prosociality might not be highly correlated within an individual (e.g., Böckler, Tusche, Schmidt, et al., 2018) but could support the assumption of a *cooperative phenotype* when getting older (Peysakhovich et al., 2014). Even though they are not correlated on an intra-individual level, they independently increase with age. The latter pattern has been shown for longitudinal increases of both types of prosociality as a function of a training program (Böckler, Tusche, Schmidt, et al., 2018).

We deem it important to emphasize the tiny to small average estimated effect size found for both linear associations of age and prosociality ($\hat{\mu}$ = 0.04 in MA_{behav} and $\hat{\mu}$ = 0.09 in MA_{SRM}, corresponding to 0.16% and 0.81% of explained variance in prosociality). This might be surprising in light of the popularity of the effect, but low effect sizes are not uncommon in (social) psychology and inter-individual difference research, particularly if studies adopt practices to avoid the inflation of effect sizes (e.g., replication, preregistration) (Cutler et al., 2021; Lovakov & Agadullina, 2021; Schäfer & Schwarz, 2019; Wilson & Wixted, 2018). It is however, important to note that many published studies in the field of lifespan developmental (social) psychology to date would have been underpowered to detect effects in the range we identified through this meta-analysis (K. R. Blake & Gangestad, 2020; Frank et al., 2017; W. Li et al., 2022; Maxwell, 2004). Another potential explanation for these small effects might be that they are influenced by the huge amount of heterogeneity detected (for discussion of moderator analyses, see below). Other reasons might lie in the moderate reliability of measurements, putting a lower bound on effect sizes, particularly for behavioral task measures (Enkavi et al., 2019; Frey et al., 2017; Tusche & Bas, 2021). Indeed, behavioral measures have been shown to display lower reliability than self-report measures when assessing prosociality (Böckler, Tusche, Schmidt, et al., 2018), but also in other contexts (Enkavi et al., 2019). Measurement reliability is a conditio-sine-qua-non to detect inter-individual differences (Zech et al., 2022), which might lower the effect sizes found for EG in our meta-analyses. Above and beyond the field of neuro-economics, this has raised questions about the suitability of psychological tasks as trait-like measures of individual differences (e.g., Enkavi et al., 2019; Frey et al., 2017; Reiter et al., 2021). Besides measurement issues, the higher age effects on self-reported prosociality observed in this meta-analysis might also reflect tendencies of social desirability that differ between age groups (Stone et al., 1999). In terms of social desirability, self-reported measures might reflect a stronger underlying motivation to portray oneself as moral, generous, and helpful than tasks (Böckler et al., 2016, 2018).

A noticeable strength of our meta-analysis is the huge number (more than 50%) of unpublished effects included in the model of linear age effects. This might picture a more realistic view of small age-related differences in prosociality. These effects are either derived from studies that did not focus on age effects in their initial publication but collected necessary demographic information and an eligible prosocial measure or from completely unpublished data sets (which led to partially overlapping data as in MA_{behav} YA vs. OA). However, even though we consider this a particular strength of our analysis, it is worth mentioning that publication status was not found to significantly moderate our meta-analytical findings.

Quadratic age effects in behavioral prosociality. In an explorative meta-analysis model across almost 51,000 participants and mostly (22 out of 23 studies) unpublished effects, we observed that the relationship of adult age with behavioral prosociality could be described with a significant quadratic term. This is further described by partially overlapping meta-analyses of group comparisons, contrasting YA versus MA (around 36,000 YA and 37,000 MA) and MA versus OA (around 35,000 MA and 17,000 OA): whilst significant age group differences between YA and MA emerged, with higher behavioral and self-reported prosociality in midlife, the meta-analyzed differences between MA and OA with respect to behavioral prosociality were smaller and not significant (when analyzed in a multi-level model). Together, these findings might point towards the direction of non-linearity of age effects on prosocial behavior. They may indicate a peak of prosociality already before old age, as well as stability until later in life, even though we caution that we cannot conclude similarity from a non-significant difference between MA and OA. Moreover, we note that in the meta-analysis on quadratic effects, one included data set (Moersdorf et al., 2018) showed a particularly strong effect size compared to other studies included in this analysis, which demonstrated smaller effects and even effects in the opposite direction. However, according to the leave-one-out diagnostic procedure (Viechtbauer, 2010; Viechtbauer & Cheung, 2010), this study was not identified as overly influential or outlier and the formal test of heterogeneity in effects was not significant. Notwithstanding, we suggest interpreting this effect with caution and as a pointer for future research to consider prosociality more directly in midlife. Of note, while the quadratic age effect and the age group differences between MA and OA could only be tested for behavioral prosociality (due to the limited number of studies using SRM in MA vs. OA), the age group differences between YA and MA could be tested, and were significant, for both, behavior, and self-report.

Such a quadratic pattern of age on prosociality could be interpreted within the framework of the motivational theory of lifespan development (Heckhausen, 2001; Heckhausen et al., 2010, 2019) in the sense that higher primary control capacities (domain-general, to influence the environment) in midlife lead to a highly effective implementation of prosocial behavior. As midlife is highly associated with agreeableness and generativity (Lachman, 2004; Roberts & DelVecchio, 2000; Roberts & Mroczek, 2008; Specht et al., 2011; Wojciechowska, 2017), as well as caregiving (Fingerman et al., 2011; Grundy & Henretta, 2006), it seems plausible to consider midlife as a peak time for prosociality. Further, the mean age of the middle-old sample moderated the age difference in behavioral prosociality, such that studies including MA on average closer to the lower bound of the age range definition (36-59 years) showed stronger age group differences. Cohort effects might explain this rather puzzling moderation effect. Different levels of prosociality and social behavior per se have indeed been suggested for different birth cohorts (e.g., *millennials*, where, given the publication date and age ranges of the studies, the groups of YA and MA are both partially located) (Koczanski & Rosen, 2019; Sandeen, 2008). Future longitudinal designs are warranted to disentangle developmental from cohort effects when studying the lifespan development of prosociality.

It is noteworthy that only three of the 27 studies initially identified for $MA_{cont\text{-}quad}$ reported non-linear age effects of prosociality in their published articles (Cutler et al., 2021 (both samples/studies); Rieger & Mata, 2015). Also, with respect to MA_{groups} (YA vs. MA, and MA vs. OA, partially overlapping data as in $MA_{cont\text{-}quad}$), only two studies (out of 55 and 31 studies, respectively) directly examined and reported age-group differences, including midlife (Fernandes et al., 2019; Sze et al., 2012). All other effects used in this meta-analysis were derived from open data or from data that was made available by the authors upon request. This means that a huge number of included effects were unpublished and thus potentially reporting a more realistic effect independent of publication bias. It also showcases that the importance of the period of midlife is oftentimes neglected in contemporary research on social lifespan development. It has also been noted in neighboring fields of lifespan development, e.g., in the emotional domain, that assumptions about adult development are often made without investigations of a midlife sample (Lachman et al., 2015). Given that middle-aged people play a crucial role in the lives of both younger and older people at home, at work, and in society at large, this shortcoming of many previous studies in the field of socio-emotional development is lamentable (Lachman et al., 2015). It calls for more detailed research with an expanded focus on the inclusion of the middle-aged population, with particular attention to non-linear links between age and prosociality (compare Pollerhoff et al., 2022).

Drivers of prosocial development across adulthood. Different theories try to explain the motives and drivers of the phenomena of increased prosociality with higher age. Proposed mechanisms include varying social roles, instrumental learning of prosociality, and cohort effects (Y. Liu et al., 2022; Van Lange et al., 1997), the facing of shorter time horizons with increasing age (Brandtstädter et al., 2010; Carstensen et al., 1999), or increased external resources in older adults (Bekkers & Wiepking, 2011; Wiepking & James, 2013). The value-based decision framework (Mayr & Freund, 2020) combines such resource and motive-related aspects as part of a decision-typical cost-benefit analysis to determine whether one should act prosocially in a given situation.

Interestingly, in this context, the moderator incentive significantly impacted the linear relationship between age and behavioral prosociality. Studies, which, by experimental design, included (monetary) payoff contingent on the participants' (or partners') behavior, revealed a stronger linear age effect, as compared to studies applying hypothetical decisions with no real payoff. One possible interpretation might be that age effects only become apparent in scenarios where prosocial behavior really matters in terms of costs. Another possible interpretation might be indirectly derived from theories arguing that an age-related increase in prosociality could be explained by an accumulation of external resources (Bekkers & Wiepking, 2011; Wiepking & Bekkers, 2012), for example, wealth (Huggett, 1996). This, in turn, would then mean that one factor potentially contributing to older individuals' relatively higher degrees of prosociality might be they have enough external resources to afford it. For younger individuals who are typically included in psychological research, such as students or trainees, one prominent motive for participating in research projects might indeed be gaining money due to more limited financial resources. Indeed, student samples have been shown to act particularly selfishly in EG (e.g., Belot et al., 2015; Cappelen et al., 2015; Falk et al., 2013; Kettner & Waichman, 2016). Such a difference in external resources between younger and older participants might lead to more pronounced age differences in experimental designs where money is actually paid out, whereas these differences are less prominent in scenarios where the payoff is only hypothetical. This, in turn, perhaps limits the external validity of these games. Prosociality in real life is not always associated with monetary loss but can also entail other costs like time (consider the case where you help a friend organize her birthday party). Notwithstanding, there is evidence arguing against the hypothesis that the accumulation of wealth alone is responsible for age-related differences in prosociality (for an overview, see Mayr & Freund, 2020). In the value-based decision framework (Mayr & Freund, 2020), financial resources are only seen as one possible distal factor contributing to the cost-benefit calculation in the process of behaving prosocially. Also, in the current meta-analysis, incentive only accounted for 13% of heterogeneity, and when adjusting for this moderator, age differences were still significant. Moreover, the fact that we find age-related increases also in self-reported prosociality points towards the direction that it is not only the availability of monetary resources that drives more prosociality with increasing age: In self-report questionnaires like the Altruism Scale, individuals are asked to rate items that are not associated with monetary costs (e.g., helping someone, spending time for someone). Notwithstanding, to completely understand the influence of monetary resources on the relationship of age and prosociality, it is indispensable to control for wealth or actual income in future studies to avoid confounding effects of monetary resources. Bekkers and colleagues (2022) illustrated decreased generosity with increased household income in a longitudinal Dutch data set. Cutler and colleagues (2021) showed that a linear increase in donation behavior with age across adulthood was not confounded by wealth or actual income. Both studies might illustrate that the influence of wealth and/or actual income on actual prosociality might be more complex. Therefore, future work should also investigate whether age effects stay robust even when measured in non-financial domains (e.g., time, food, and health-promoting products), a domain which does not necessarily increase with age as it is assumed for wealth. However, this is a relatively young research area, which just got more research attention over recent years (e.g., Best & Freund, 2021), leading to the fact that we were only able to include k = 9 studies investigating non-financial domains.

When comparing different age groups, we further observed significant moderation effects for the type of interaction applied to the design of the GTC, with stronger age-group differences in one-shot, compared to repeated interactions, across all three group comparisons (YA vs. MA, YA vs. OA, MA vs. OA). This effect indicated that compared to the older sample included (MA or OA), the younger age group (YA or MA) was less prosocial, particularly in one-shot settings. Compared to one-shot scenarios, repeated interactions, for which we did not find significant age differences, offer an opportunity for reciprocity. Prosocial behavior, as shown in GTCs with repeated interactions, can be viewed as a conditional concern for the welfare of others (e.g., Perugini et al., 2003; Thielmann et al., 2020). Furthermore, reciprocity can be seen as an internalized social norm which can be found in most human societies (Gouldner, 1960). Our finding of age differences, specifically in one-shot scenarios, might speak for enhanced intrinsic, unconditional prosociality in MA and OA (compared to

YA). On the contrary, in scenarios where strategic instrumental reciprocity plays out, age groups do not differ significantly.

Additionally, we found setting (i.e., online vs. lab studies) to significantly moderate age group differences in behavioral prosociality, but only in YA versus OA. It is interesting that no age differences were apparent when considering (lab-in-the) field studies only, which typically operate in a more naturalistic and potentially less artificial and anonymous context than lab or online studies. This moderation effect may suggest that the prosocial behavior of individuals in different age groups is differentially influenced by various factors such as social desirability and the similarity of the experimental setting to real-life situations. Another possible explanation might lie in the fact that samples that are included in laboratory and online studies are different from the ones included in (lab-in-the) field studies. On one hand, there is evidence that study populations from online marketplaces like Amazon Mechanical Turk are sufficiently similar to samples from laboratory studies (Paolacci et al., 2010; Ross et al., 2010). Further, both settings are designed to measure a construct in a highly anonymized and standardized setting, producing comparable results (Dandurand et al., 2008; Segen et al., 2021). While this has the advantage of reduced bias, it goes along with the disadvantage of not providing a naturalistic social context (Levitt & List, 2007). (Lab-in-the) field studies, on the other hand, are often times applied in more rural areas, where the participants are residents of a specific village (e.g., Sircar et al., 2018; Tognetti et al., 2013; Vardy & Atkinson, 2019). Thus, participants often times directly interact with each other and, more importantly, often know each other as they are, for example, neighbors. This, in turn, creates a strikingly different social context compared to highly anonymized laboratory or online studies. Thus, it is worth highlighting that carefully designed (lab-in-the) field studies might picture a more realistic view of a specific behavior in a naturalistic environment (e.g., Gneezy & Imas, 2017), and thus higher external validity. In line with this, a recently published study measuring self-reported prosociality in real life via smartphone-based ecological momentary assessment also did not find age-related differences in prosociality across the adult lifespan (Pollerhoff et al., 2022).

Altogether, one main limitation in the current meta-analysis is the fact that several pre-registered moderators of interest could not be examined due to a limited number of studies investigating them, or indeed, often times a lack of information in the articles. One moderator of great interest would be cognitive functioning. Given the fact that already younger individuals make errors in the prosocial decision-making process (see Hutcherson et al., 2015), it would be interesting to examine whether this might happen more often with increasing age. Better cognitive abilities have been shown to be associated with an increase in selfish behavior (Yamagishi et al., 2014) and mediated age differences in socioeconomic decision-making (Rosi et al., 2019). Cognitive resources are seen as distal factors and, thus, potentially critical moderators in the prosocial cost-benefit calculation in the value-based decision framework (Mayr & Freund, 2020). Further, considering variables that represent the special characteristics of the different behavioral tasks would have been helpful to interpret the current findings in terms of an affordance-based framework of prosocial behavior (Thielmann et al., 2020). The idea of this framework is that prosocial behavior is not solely driven by internal factors such as personality or values but is also influenced by external cues in the environment that suggest opportunities to help others. How these cues influence decision-making might differ as a function of age. Initially, we had indeed pre-registered to investigate the moderating role of such cues, name, deception, feedback, role played in a game, group size, and information about the interaction partner. Due to a lack of information in the published articles, we were, not able to extract enough data to robustly estimate meta-regression models. This highlights the importance of consistent reporting standards in (psychological) science and the improvement of open science in general, including proper data management and code books. Such a more transparent reporting approach might have allowed us to better understand study specifics and be able to include them as moderators in our analysis.

In future studies, we and others suggest that it would be fruitful to focus on the contributory role of the so-called distal factors to prosocial behavior as well as age differences in prosociality (Mayr & Freund, 2020). Distal factors include resources and constraints like (socio-)cognitive capacities, financial resources, or remaining time to live, as well as motives like generativity or cultural norms. They are believed to affect the more immediate costs and benefits associated with a specific prosocial action (proximal factors).

In this context, one potential driver of prosociality might be empathy (for reviews see Batson, 2010; Davis, 2015; N. Eisenberg & Miller, 1987; Telle & Pfister, 2016): The role of empathy in contributing to prosociality and age differences therein was previously studied in the lab (e.g., Beadle et al., 2015; Cavallini et al., 2021; Sze et al., 2012), but also in real life based on ecological momentary assessment data (EMA) (Depow et al., 2021). Depow and colleagues (2021) illustrated that several aspects of empathy promoted real-life helping behavior. Interestingly, within the same dataset, small quadratic effects of age on daily empathy were observed, reminiscent of the pattern identified for prosociality in the current meta-analysis: a tendency to act more prosocially in the context of a situation eliciting empathy (as compared to when there was none) was found to be slightly pronounced in midlife (for a discussion see Pollerhoff et al., 2022). Moreover, an age-related linear increase in behavioral prosociality across younger, middle-aged, and older participants was also found to be partially mediated by empathic concern in a previous lab study (Sze et al., 2012).

Limitations and future research. To the best of our knowledge, the current meta-analysis is the first study that investigates age differences of behavioral and self-reported prosociality across the whole range of adult age following a lifespan approach. There are, however, several limitations that should be noted and lead to important implications for future research. First, we exclusively included cross-sectional studies, which are susceptible to, for example, cohort effects of birth and age. Longitudinal data are important to get a better and deeper understanding of the development of prosociality (Mayr & Freund, 2020). Bekkers and colleagues (2022), for example, could demonstrate that in the Netherlands, where they tracked the same cohort over time as it aged (in a period from 1995-2015), age-related differences in generosity were mostly explained by secularization and loss of prosocial values, i.e., societal change. Even though there are other first results speaking for longitudinal changes within the social mind, including prosociality (Roberts et al., 2014) and empathy (Oh et al., 2020), additional research is needed to more thoroughly investigate age trajectories of prosociality, with a special focus on midlife and non-linear associations. Second, besides longitudinal data, real-life observations or smartphone-based EMA are promising tools to assess prosocial behavior in daily life. EMA is a relatively new method to capture thoughts, feelings, and behaviors as well as their changes in real life, repeatedly within one person, with high external validity and reduced recall bias (Bielak et al., 2014; Mehl & Conner, 2012; Neubauer et al., 2020). In the current meta-analysis, only one study used EMA combined with a validated SRM to measure age effects on prosociality (Vekaria et al., 2020) and was thus considered for inclusion. Due to this reduced number of real-life studies included, we are not able to draw conclusions about age differences of prosociality in everyday life based on this meta-analysis. Over the last decade, due to the widespread availability of smartphones in all age groups, EMA and other real-life assessments have been used increasingly in the field of lifespan developmental psychology, also with respect to socio-emotional development (e.g., Grühn et al., 2008; Hülür et al., 2016; Nikitin & Freund, 2021; Pollerhoff et al., 2022; Raposo et al., 2021; Rauers et al., 2013; Zhaoyang et al., 2018). This might allow future meta-analyses to make inferences on age differences in daily prosociality, as well as to compare effect sizes derived from such real-life studies with lab studies.

Conclusion. The present meta-analysis offers a new view on age trajectories of prosociality by illustrating that the relationship between adult age and prosociality might be described both by a linear and a quadratic function. We suggest that higher prosociality not only emerges late in life but also that the period of midlife (here defined as the age period between 36 and 59 years) is a phase of pronounced prosociality. We conclude that future studies should consider non-linear trajectories of socio-emotional development and place special focus on investigating the whole adult lifespan and on mechanisms driving these age differences.

Authors' contributions

L.P. and A.M.F.R designed the research. L.P., D.F.R., and A.M.F.R. did the literature search and screening. L.P. analyzed the data with input from A.M.F.R. L.P. and A.M.F.R. interpreted the data. L.P. wrote the manuscript with input from A.M.F.R. All authors provided critical revisions and approved the final version of the manuscript.

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2.2 Investigating adult age differences in real-life empathy, prosociality, and well-being using experience sampling

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Abstract

While the importance of social affect and cognition is indisputable throughout the adult lifespan, findings of how empathy and prosociality develop and interact across adulthood are mixed and real-life data are scarce. Research using ecological momentary assessment recently demonstrated that adults commonly experience empathy in daily life. Furthermore, experiencing empathy was linked to higher prosocial behavior and subjective well-being. However, to date, it is not clear whether there are adult age differences in daily empathy and daily prosociality and whether age moderates the relationship between empathy and prosociality across adulthood. Here, we analyzed experience-sampling data collected from participants across the adult lifespan to study age effects on empathy, prosocial behavior, and well-being under real-life circumstances. Linear and quadratic age effects were found for the experience of empathy, with increased empathy across the three younger age groups (18 to 45 years) and a slight decrease in the oldest group (55 years and older). Neither prosocial behavior nor well-being showed significant age-related differences. We discuss these findings with respect to (partially discrepant) results derived from lab-based and traditional survey studies. We conclude that studies linking in-lab experiments with real-life experience-sampling may be a promising venue for future lifespan studies.

Introduction

Throughout the lifespan, satisfying social interactions are key for well-being (e.g., Ishii-Kuntz, 1990) as well as for mental and physical health (e.g., Steptoe et al., 2015). Experiencing empathy in response to someone's suffering, a feeling that is believed to trigger prosocial behavior, is an important ingredient for establishing and maintaining relationships with other people (Batson & Powell, 2003; Grühn et al., 2008).4/30/24 10:49:00 AM Whilst it is certainly indisputable that social functioning remains important throughout the adult lifespan (Cacioppo & Cacioppo, 2018; Holt-Lunstad et al., 2010), findings on how empathy and prosociality develop and interact over the course of adulthood are still mixed.

Research on the lifespan development of empathy is often built on the distinction of affective components (affect sharing, empathic concern, compassion) from cognitive components (perspective taking) of empathy (Beadle & de la Vega, 2019; Stietz et al., 2019). In two recent laboratory studies using a naturalistic paradigm to dissociate both affective and cognitive aspects of understanding others (Reiter et al., 2017; Stietz et al., 2021), we did not observe age-related differences in affect sharing. However, and in line with our expectations, empathic concern was found to be enhanced whereas perspective taking ability decreased in older compared to younger adults. Whilst this pattern of findings contributes to an emerging prevalent view of reduced cognitive but preserved or increased affective empathy in older vs. younger adults (Beadle & de la Vega, 2019 for a recent review), there is substantial heterogeneity in the literature, particularly when considering adult age effects across the lifespan.

With respect to experimental studies, there is some evidence for an age-related increase in task-based (i.e., less naturalistic behavioral measure conducted in the lab) empathic concern (Sze et al., 2012), and affect sharing (Richter & Kunzmann, 2011) in some studies. However, other comparable studies find no evidence for significant age-related differences with respect to task-based empathic concern (Bailey et al., 2020; Beadle et al., 2015), and affect sharing (Wieck & Kunzmann, 2015). The same inconsistencies are observed in terms of perspective taking when measuring the construct in behavioral tasks. Overall, meta-analytic evidence points towards a decrease of perspective taking ability (Henry et al., 2013) but, there are other experimental studies showing no evidence for age differences in perspective taking (Girardi et al., 2018; Grainger et al., 2018; Lecce et al., 2019; Slessor et al., 2007). With respect to studies that measure empathic concern, affect sharing, or perspective taking with self-report, results have suggested linear and inversed U-shaped relationships between age and self-reported empathic concern (O'Brien et al., 2013; Sze et al., 2012) and perspective taking (O'Brien et al., 2013), as well as age-related declines in self-reported affect sharing (Phillips et al., 2002; Schieman & Gundy, 2000).

The understanding of developmental change and stability with respect to prosociality has become an important research topic (Phillips et al., 2002; Schieman & Gundy, 2000). Similar to the heterogeneity of findings in the domain of empathy, previous research on adult age differences in prosocial behavior has yielded mixed results. Experimental age-comparative laboratory studies have often revealed a higher degree of prosocial behavior in older compared to younger adults (Bailey et al., 2013, 2020; Beadle et al., 2015; Charness & Villeval, 2007; Lockwood et al., 2021), and have shown a linear increase in prosociality when examined across the adult lifespan (Bjälkebring et al., 2016; Freund & Blanchard-Fields, 2014; Sze et al., 2012). There are, however, other studies which do not find such age-related increases in prosocial behaviors neither in experimental tasks or self-reported prosocial measures when comparing younger and older adults (Beadle et al., 2012; Roalf et al., 2012), when comparing middle- and older age groups (Cavallini et al., 2021), nor when looking at age correlations across adulthood (Borges et al., 2017; Bruine de Bruin & Ulqinaku, 2020).

Social psychology and neuroscience research suggests that feeling empathy with a person usually results in greater prosocial behavior (for reviews see Batson, 2010; Davis, 2015; N. Eisenberg & Miller, 1987; Telle & Pfister, 2016). Some previous studies have asked whether this link might be moderated by adult age. Experimental studies in the laboratory demonstrate enhanced prosocial behavior after an empathy induction in older adults (compared to younger adults) (Beadle et al., 2015) as well as age-related linear increases in prosocial behavior across younger, middle aged, and older adults that were partially mediated by empathic concern (Sze et al., 2012). Age was also found to positively moderate the association of self-reported prosocial behavior and empathic concern, but only in participants younger than 75 years (Cavallini et al., 2021). However, another study did not find age-related differences regarding the link between empathy and prosocial behavior when comparing younger and older adults (Bailey et al., 2020).

The question of whether subjective well-being, used as an umbrella term for feelings of happiness, a sense of purpose in life, and life satisfaction (Steptoe et al., 2015), changes over the course of the adult lifespan is a research topic that has also attracted much attention over the last decades (e.g., Hicks et al., 2012; Kunzmann et al., 2000). Most prominently, an influential lifespan theory of affective development ("socio-emotional selectivity theory", Carstensen et al., 1999) postulates higher socio-emotional well-being in older adults. It is argued that when individuals perceive their remaining lifetime as limited, they tend to prioritize present goals like optimizing socio-emotional well-being (Charles & Carstensen, 2010). This is thought to be related to a "positivity effect", i.e., a motivational shift to positive over negative information processing (Erbey et al., 2020; Mather & Carstensen, 2005; Ziaei et al., 2015), even though there are studies that do not find evidence for age-related differences regarding this processing bias for positive stimuli (Grühn et al., 2005; Ziaei et al., 2017). Large-scale international surveys across several countries often reveal a U-shaped association between age and evaluative well-being, illustrating higher well-being in younger and older adult age (e.g., Frijters & Beatton, 2012; Orben et al., 2020). Studies using ecological momentary assessment or other daily measures also show a U-shaped relationship of adult age and life satisfaction (Stone et al., 2010), or a curvilinear relationship of age and negative emotional experience (Carstensen et al., 2000). Notwithstanding, there is a current debate about the putative U-shaped pattern, including critiques with respect to its robustness and generalization (Galambos et al., 2020; Laaksonen, 2016; N. Li, 2016). Prosocial behavior and well-being have been suggested to be associated with each other, and some studies have suggested that their link is moderated by age. In a recently published large-scale daily diary study (Chi et al., 2021), younger adults' prosocial behavior was associated with both costs and benefits, in that they experienced both greater negative affect and more positive experiences at the same time, while these associations were both attenuated in older adults. This was interpreted as a decreasing influence of prosociality on well-being over the course of the life span. In contrast, earlier age-comparative studies have pointed towards more beneficial effects of volunteering on life satisfaction in older compared to younger adults (Van Willigen, 2000).

Considering the lifespan developmental findings (and their considerable heterogeneity) reviewed above, an intriguing open research question is whether there are adult age differences in empathy, prosociality, and well-being as they occur in daily life. Ecological momentary assessment (EMA) is a method which is well-suited to capture these processes in real life. EMA is a relatively new method, with advantages like increased ecological validity, and decreased recall bias, while measuring within-person variability and change over a short time period (Csikszentmihalyi, 2011). These advantages may be particularly important for lifespan developmental studies, as recall bias (Neubauer et al., 2020) and within intra-individual variability (Bielak et al., 2014) have been shown to be subject to age effects in different behavioral contexts. Unfortunately, and to the cost of external validity, to date only a small list of studies (Depow et al., 2021; Grühn et al., 2008; Nezlek et al., 2001) examined empathy and prosociality using EMA, most of them with no focus on adult age differences. With the current study we aimed to fill this gap by leveraging the advantages of EMA and investigating daily empathy, prosociality, and well-being under real-life circumstances, repeatedly per day within person. To this end we analyzed smartphone based experience-sampling data recently published by Depow and colleagues (2021). The primary goal of Depow and colleagues' study (2021) was to analyze the perception of empathy in everyday lives and the prediction of prosocial behavior and subjective well-being (defined as feelings of happiness and purpose of life). They used quota-sampling to ensure their sample was representative of the U.S. adult population on key demographics, including age. However, age effects were not part of their original analysis. Given the growing interest in age-related differences regarding components of the social mind (Bailey et al., 2021) and the lack of real-life data in this field, we used the data acquired and provided openly by Depow et al. (2021) to examine age effects with regards to daily empathy, prosocial behavior, and well-being. Separating contributions of within- from between-subject variability, we aimed to investigate whether age influences daily empathy, prosocial behavior, and well-being, as well as their interactions. Of note, in the current study, empathy was defined as an umbrella term, subsuming the three different subcomponents emotion sharing, compassion, and perspective taking. Prosocial behavior was defined as an opportunity to help someone.

Based on the heterogeneity of age-related findings regarding the constructs of empathy and prosocial behavior as reviewed above, and considering inconsistencies in the definition of empathy, we had undirected hypotheses in terms of age differences in daily empathy and prosocial behavior. In line with previous results (Frijters & Beatton, 2012; Orben et al., 2020; Stone et al., 2010), we expected a U-shaped relationship of adult age with well-being (measured in terms of happiness and sense of purpose), with higher well-being in younger and older adults. Based on a positivity bias postulated for older adults in the lifespan theories of emotional aging (Erbey et al., 2020; Mather & Carstensen, 2005; Ziaei et al., 2015), we expected older adults to experience more empathy in contexts with positive valence (i.e., situations where the target emotion was positive). Based on a previous lab-based study (Cavallini et al., 2021), we hypothesized an attenuated relationship of empathy and prosocial behavior in the older age range. Furthermore, based on the literature (Chi et al., 2021) we assumed a greater relationship of empathy and well-being with increasing age and expected the same age-related pattern with respect to the link between prosocial behavior and well-being.

Methods

The current study is based on the publicly available dataset from Depow and colleagues (2021) who used ecological momentary assessment to increase ecological validity and to explore within-person differences. In the current study, we focused on analyzing adult age-related differences on different aspects of daily empathy, daily prosocial behavior, and daily subjective well-being. In the following, we reiterate the methodological approach used by Depow and colleagues (2021).

Participants. Quota-sampling was used for a nearly representative U.S. sample, in cooperation with the survey company Qualtrics (Qualtrics®, 2002; www.qualtrics.comwww.qualtrics.com). Overall, 3486 participants filled in a demographic questionnaire, including informed consent about participating in a "Daily Interactions' study". In a next step, 841 quota-sampled participants were invited via email to participate in the study. Altogether, 375 completed the baseline survey (trait questionnaire measures, instruction to download the app, glossary of the terms), whereas 285 participants completed the full experience sampling for one week, seven times per day. Participants were excluded if they missed more than 7 surveys in total, which resulted in a sample of 246 participants. Three more participants were excluded from the current analysis due to missing information regarding their age. In the dataset from Depow and colleagues (2021) age was originally measured in six age groups. The groups were divided into (1) 18 to 24 years (n = 14), (2) 25 to 34 years (n = 57), (3) 35 to 44 years (n = 59), (4) 45 to 54 years (n = 51), (5) 55 to 64 years (n = 42), and (6) 65 years and older (n = 20). Due to comparably small sample sizes in the youngest and oldest age groups Depow and colleagues (2021) merged the two youngest and the two oldest age groups for a more homogenous sample size across the groups, an approach which we adopted for the current analysis. Thus, the final sample consisted of 243 participants (18–34: n = 71, 45 female, 3 "other"; 35–44: n = 59, 28 females; 45–54: n = 51, 34 female; 55 and older: n = 62, 29 female). The age groups differed significantly with respect to their gender and education distribution, but not in surveys answered, income, and religiosity (see Table 4 for descriptive and interferential statistics). Participants answered a total of 7141 surveys, which we consider a dataset characterized by a high degree of ecological validity. Due to a bug in the experience sampling procedure in the original study by Depow et al. (2021), a very small number of participants (n = 58 out of a total of n = 243 participants), received prompts to fill out the survey more often, and sometimes within a shorter time interval (i.e., < 15 min). These cases could be problematic, since the experience sampling was done within a time interval that was shorter than 15 min, which may not have capture participants' responses to independent events/experiences. Thus, all surveys that were prompted < 15 min after a previous survey were excluded from the analyses (a total of n = 110 surveys from a total of n = 7251 surveys, corresponding to only 1,5% of all data). Also due to this issue, we were able to include eight surveys per day for a minority of participants (n = 18), instead of the intended maximum of seven surveys per day, all of which by time windows ≥ 15 min. For information on how often participants reported an empathy opportunity, having been the target of empathy, and prosocial behavior, as well as reported well-being, see Table 5. Further, we observed differences in how often participants reported an empathy opportunity, an opportunity to be the target of empathy, and prosocial behavior, as well as the reported well-being scores (i.e., questions asked on the first level of the survey) as a function of the different survey days. While these differences are mainly driven by the first day of the survey compared to later points in time, they are apparent across all age groups (compare Table 5) and do not differ significantly between the different age groups (all adj. ps > 0.236).

All participants provided informed consent regarding their participation in the project and were told they were free to cease their participation at any point. All procedures were approved by the University of Toronto Research Ethics Board to ensure they adhered to relevant ethical guidelines for human data collection and usage (Protocol No. 36941).

Table 4

Descriptive and interferential statistics of the final sample characteristics

	18-34 years	35-44 years	45-54 years	55 + years	Test Statistic
Surveys answered	29.46 ± 11.73	29.49 ± 11.12	30.06 ± 12.00	28.65 ± 12.50	$F = 0.09$, $p = 0.78$
Gender (m/f/o)	23/45/3	31/28/0	17/34/0	33/29/0	chi² = 16.46, $p = 0.012$
Education (Highschool/GESD or less/ some college/ college graduate/ graduate degree)	23/14/20/14/0	12/19/16/8/4	18/11/9/8/5	5/17/20/10/10	chi² = 28.63, $p = 0.004$
Income (under 25000/ 25000-50000/ 50000-100000/over 100000)	22/24/21/4	13/23/16/7	18/16/15/2	15/23/16/8	chi² = 7.11, $p = 0.626$
Religiosity (not at all/ slightly/ religious/ strongly/ extremely)	22/17/14/10/5	20/13/15/4/7	11/12/11/12/2	16/17/14/8/5	chi² = 10.40, $p = 0.581$

Table 5

Proportion of how often participants reported an empathy opportunity, an opportunity to be the target of empathy, and acting prosocially, as well as the mean value of well-being relative to all surveys answered per day, shown as a function of survey day and age group.

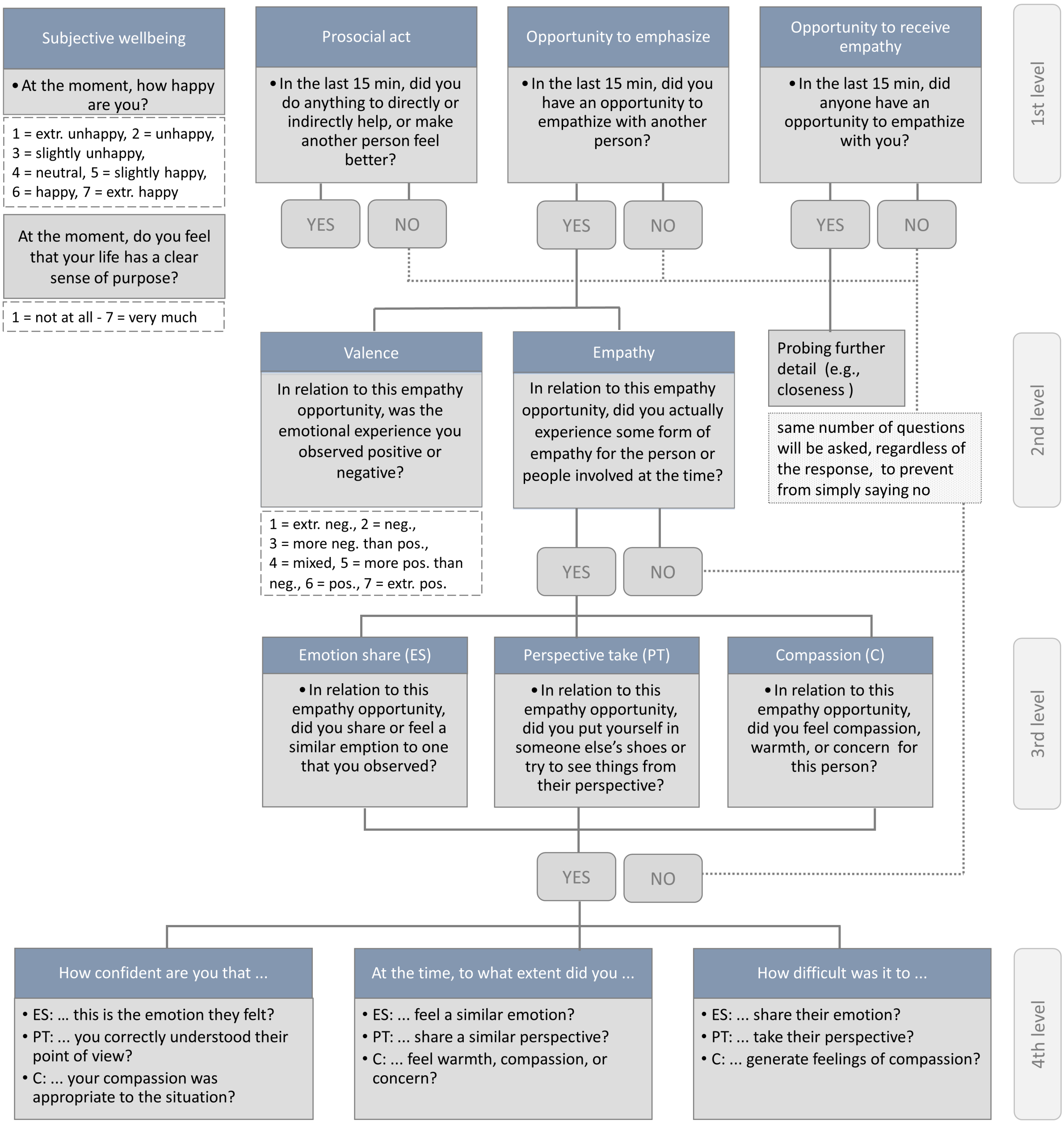
	18-34 years	35-44 years	45-54 years	55 + years
Cases of Empathy Opportunities (in %)				
Day 1	37.17	27.79	24.95	35.93
Day 2	25.18	20.15	17.45	21.64
Day 3	19.95	21.52	14.07	23.67
Day 4	23.72	13.62	16.93	23.01
Day 5	20.34	16.91	11.89	18.63
Day 6	15.81	12.74	10.83	16.66
Day 7	16.58	13.32	14.06	15.22
Cases of Target Opportunities (in %)				
Day 1	20.09	16.10	15.08	19.88
Day 2	16.06	12.78	8.76	10.95
Day 3	13.06	12.46	11.50	19.63
Day 4	15.63	9.56	11.02	8.25
Day 5	16.64	8.46	9.68	9.16
Day 6	11.80	10.43	8.19	13.08
Day 7	13.56	6.11	9.02	13.78
Cases of Prosocial Behavior (in %)				
Day 1	38.94	27.39	29.56	35.41
Day 2	29.19	22.63	20.65	21.98
Day 3	22.43	22.47	16.63	20.87
Day 4	19.96	17.88	14.31	23.92
Day 5	17.72	18.54	13.13	20.43
Day 6	21.08	14.10	10.62	19.52
Day 7	21.24	15.16	14.40	17.69
Well-Being (Mean)				
Day 1	4.94	5.09	4.98	5.24
Day 2	4.99	4.92	5.04	5.34
Day 3	5.05	5.00	4.95	5.23
Day 4	4.91	5.03	4.97	5.16
Day 5	4.84	4.92	4.90	5.23
Day 6	4.79	4.95	5.13	5.26
Day 7	4.89	5.28	5.19	5.11

Procedure. ***Baseline survey.*** Participants first underwent a baseline survey to collect demographic information and trait measures (compare Depow et al., 2021). The baseline survey included a glossary of the important terms (empathy opportunity, emotion and sharing, perspective taking, compassion, prosocial behavior), to ensure that all participants would understand the concepts in the same way. For detailed information about the materials see Depow and colleagues (2021).

Experience-sampling survey. After the baseline survey, participants underwent seven short surveys per day, sent between 10 am and 10 pm for one week, delivered by Metricwire (MetricWire®, 2013). Surveys were sent semi randomly, within a 90-min window, with a minimum 15 min gap between surveys. Surveys expired 20 min after the prompt. The daily survey consisted of four levels that were built on each other (see Figure 11). On the first level, participants were questioned about their current subjective well-being, and if they had an opportunity to empathize (empathy opportunity), to be the target of empathy, or to act prosocially in the last 15 min. If an empathy opportunity had occurred in the last 15 min, further details relating to this opportunity were acquired. On the second level, participants were asked if they experienced actual feelings of empathy for the person or people involved. If they responded "yes", they were subsequently asked to indicate whether they experienced any (or several) of empathy's subcategories: emotion sharing, perspective taking, and compassion. Subsequently, extent, difficulty, and confidence were probed for every subcategory they indicated. The length of the survey was always the same, irrespective of the answer provided. In the original study, a multitude of questions were asked, which are not relevant for the current research question. For further details see Depow and colleagues (2021).

Figure 11

Daily survey design, visualizing the different survey levels and related questions.



Note. Only the questions relevant for the current study are depicted here. For further details, and a full study protocol, see Depow and colleagues (2021). Note that empathy (level 2) was assessed as an umbrella term, thus, questions about the subcomponents (level 3) emotion share, perspective taking, and compassion were only rated if the participant indicated to have actual feelings of empathy on level 2 of the survey. For each reported subcomponent on level 3, participants were asked about confidence, extent, and difficulty on level 4 of the survey.

Statistical analysis. All data were analyzed using R (Version 4.0.3, R Core Team, 2020) with RStudio (RStudio Team, 2020). We adopted the statistical approach described in Depow and colleagues (2021) but, additionally examined the effect of age group and interactions with age group on our variables of interest. Age group entered all models as a continuous variable, based on the assumption of ordinality and continuity (e.g., Hastie et al., 1989; Sze et al., 2012). Age differences in sample characteristics (compare Table 4) were examined with Chi^2-tests for categorical variables and ANOVA for continuous variables using the R package "stats" (R Core Team, 2020) and "rstatix" (Kassambara, 2021).

Age differences regarding the different aspects of daily empathy, prosocial behavior, and well-being were analyzed with mixed-effect models, using the *mixed* function from the "afex" package (Singmann et al., 2021). Binary outcomes (experience of empathy (yes/no), engaging in prosocial behavior (yes/no)) were analyzed with generalized mixed-effects models and p-values were calculated with likelihood-ratio tests. Well-being as a continuous outcome (global well-being score resulting from happiness and sense of purpose) was analyzed by using mixed-effects models, with p-values calculated based on the Satterthwaite method (Singmann et al., 2021). For each outcome variable, we constructed two models as follows: i) including age as a linear predictor ii) including age both, as a linear and a quadratic predictor. Model selection was conducted based on a loglikelihood ratio test (R function *anova)*. In the results section, the results of the better fitting model are reported, respectively. All models were nested within participant and survey day as random intercepts. In case of convergence warnings, the number of iterations were increased, and the optimizer changed to "bobyqa", followed by reducing the maximum random effect structure by survey day. We controlled for gender, income, religiosity, and education by including them as covariates, each in a separate model. Controlling for these four different covariates did not change the significance level of our predictors of main interest (i.e., age). Thus, in the results section only the models without covariates are reported.

In order to analyze between- and within-subject effects regarding the influence of empathy and age on prosocial behavior and well-being, and prosocial behavior and age on subjective well-being, predictors were centered in different ways. Continuous variables were participant-centered for within-subject effects, and grand-mean centered for between-subject effects. Binary variables were dummy-coded (1 = yes, 0 = no) for within-subject effects and for between-subject effects the grand-mean centered average proportion of yes responses of a participant was used (for further details see Depow et al., 2021). Model statistics, including effect sizes "r" for fixed effects in mixed-effect models derived from R^2 (Edwards et al., 2008) were calculated with the *summaryh* function from the "hausekeep" package (Lin, 2019). The *p-adjust* function from the "stats" package (R Core Team, 2020) was used to correct p-values for multiple testing with the false discovery procedure (Benjamini & Hochberg, 1995). Data are publicly available at: https://osf.io/y3ud7/, and scripts are publicly available at: https://osf.io/fdmtg/.

Results

In the current study we reanalyzed open-source data from Depow and colleagues (2021), using EMA in a demographically representative sample across the adult lifespan (final sample n = 243, 136 females, 104 males, and 3 others) to measure daily empathy, prosociality, and well-being. In the main experience sampling survey participants underwent seven surveys per day for one week, including four levels, building on each other (see Figure 11). The first level measured daily well-being and prosocial behavior, as well as opportunities to empathize with someone or any situations where the participant could be the target of empathy. In case they had reported an empathy opportunity before, on the next (2nd) level participants answered questions about their actual feelings of empathy. On the third level the subcategories emotion sharing, perspective taking, and compassion were investigated. On the fourth level the extent, difficulty, and confidence for every indicated subcomponent were probed. Depow and colleagues (2021) collected information on participants' age by asking about participants' age group; participants did not provide information on their exact age. Age was binned into four ordered age groups, based on the original classification from Depow and colleagues (2021): (1) 18 to 34 years (n = 71, 45 females, 3 "other), (2) 35 to 44 years (n = 59, 28 female), (3) 45 to 54 years (n = 51, 34 female), and (4) 55 years and older (n = 62, 29 female). Thus, age group entered all models as a continuous variable, based on the assumption of ordinality and continuity (e.g., Hastie et al., 1989; Sze et al., 2012). Outcome variables derived from all four levels of the survey (see Figure 11) were analyzed. All outcome variables were analyzed by using two (generalized) mixed-effect models, one including age group as linear predictor and one including age group both as a linear (age group) and a quadratic predictor (age group^2). We report p-values corrected based on the false-discovery rate.

Adult age differences in everyday empathy. ***Empathy opportunities.*** Regarding the first level of the survey (see Figure 11), i.e., the frequency of empathy opportunities, and the frequency of being the target of empathy, no age-related differences were found (empathy opportunities: b = 0.01, SE = 0.08, z = 0.16, p = 0.872, adj. p = 0.872, r = 0.00; target of empathy: b = 0.02, SE = 0.10, z = 0.21, p = 0.834, adj. p = 0.872, r = 0.01). Reassuringly, this null effect of age suggests that potential age differences in all subsequent analyses are unlikely to be affected by baseline differences in how often different age groups experienced an opportunity to empathize in the first place.

***Experiencing empathy**.* With respect to the actual feelings of empathy (2nd level of the survey), significant linear as well as quadratic effects of age were observed (linear: $b = 0.37$, $SE = 0.15$, $z = 2.44$, $p = 0.015$, $r = 0.10$; quadratic: $b = -0.41$, $SE = 0.18$, $z = -2.24$, $p = 0.025$, $r = -0.11$). Daily feelings of empathy increased across the first three age groups, from 18 to 44 years, but, as the significant quadratic trend suggests, show a tendency to decrease beyond these ages, in those 55 years and older (see Figure 12A).

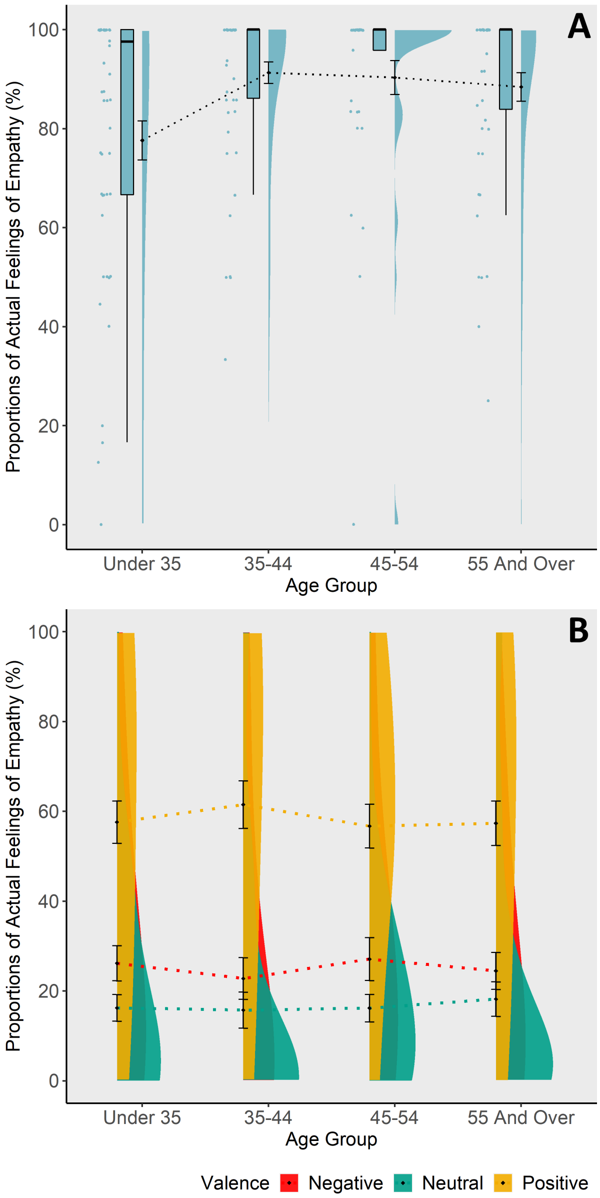
In lifespan developmental theories, a positivity bias for older as compared to younger adults has been suggested (Erbey et al., 2020; Mather & Carstensen, 2005; Ziaei et al., 2015). Here, we tested this hypothesis with respect to the role of emotional valence (i.e., positive, neutral, or negative) of the empathy opportunity on actual feelings of empathy. That is, we tested for an interaction of age group with valence of the empathy opportunity. As reported in Depow and colleagues (2021), subjects generally showed higher actual feelings of empathy after positive empathy opportunities; however, contrary to our expectation this effect was not moderated by age (Chi^2 (2) = 0.27, $p = 0.872$, see Figure 12B).

Subcategories of empathy. Regarding the subcomponents of empathy (3rd level of the survey, Figure 11), no significant age-related differences (neither linear, nor quadratic) were found for emotion sharing ($b = -0.17$, $SE = 0.13$, $z = -1.31$, $p = 0.189$, adj. $p = 0.567$, $r = -0.05$), perspective taking ($b = 0.01$, $SE = 0.14$, $z = 0.06$, $p = 0.952$, adj. $p = 0.952$, $r = 0.00$), or compassion ($b = 0.18$, SE = 0.35, $z = 0.52$, $p = 0.601$, adj. $p = 0.902$, $r = 0.05$). Note that ratings of the subcomponents of empathy were only provided by those participants who indicated actual feelings of empathy on the second level of the survey. Thus, while we observe age-related differences in the tendency to experience actual feelings of empathy in general (2nd level), among the proportion of participants who experienced actual feelings of empathy, no age differences with regard to the subcomponents were observed. Further, after adjusting p-values, no age-related differences were observed in the extent, difficulty, and confidence (4th level) of the different subcomponents: emotion sharing, perspective taking, and compassion (all bs < 0.04, all SEs < 0.08, all ts < 0.96, all adj. ps > 0.081, all rs < 0.20; see supplementary table S1).

Depow and colleagues (2021) showed that the co-occurrence of the three subcomponents of empathy (i.e., emotion sharing, perspective taking, and compassion) was very high. Thus, the different components of empathy seem to mainly co-occur in everyday life. Analyzing age effects on the co-occurrence of the empathy subcomponents using a chi^2-test, we did not find any significant effects of age (all chi^2s < 5.76, all ps > 0.124; see supplementary figures S1-4).

Figure 12

Daily empathy



Note. The y-axis shows the proportions of answering 'yes' when asked about the actual feelings of empathy relative to the total amount of answered surveys. (A) Adult age differences regarding actual feelings of empathy. Significant quadratic association of actual feelings of empathy and age. (B) Interaction between valence (negative, neutral, or positive target emotion) of the empathy opportunity and age on reported feelings of daily empathy. Age did not moderate the link between the valence of the situation and actual feelings of empathy.

Adult age differences in prosociality. Contrary to our expectations, we did not find significant age-related differences with respect to daily prosocial behavior (b = 0.007, SE = 0.08, z = 0.08, p = 0.934, r = 0.00, see Figure 13A). Depow and colleagues (2021) found significant associations between different aspects of everyday empathy (e.g., empathy opportunity, actual feelings of empathy, subcomponents) and prosocial behavior. In the current analysis most of those effects were not significantly moderated by participants' age (all adj. *p*s > 0.227, see Table 6). Only the interaction term of reported empathy opportunities and age as quadratic and linear term, respectively, significantly predicted prosocial behavior as a within-subject effect (linear: b = -0.13, SE = 0.05, z = -2.80, p = 0.005, adj. p = 0.024, r = -0.04, quadratic: b = 0.15, SE = 0.06, z = 2.72, p = 0.006, adj. p = 0.024, r = 0.04). Across all age groups the experience of an empathy opportunity was associated with higher prosocial behavior, but this effect was more pronounced in the middle-aged groups than in the youngest and oldest groups (see Figure 13B).

Table 6

Within- and between person effects of different interactions regarding different aspects of daily empathy and age predicting daily prosocial behavior

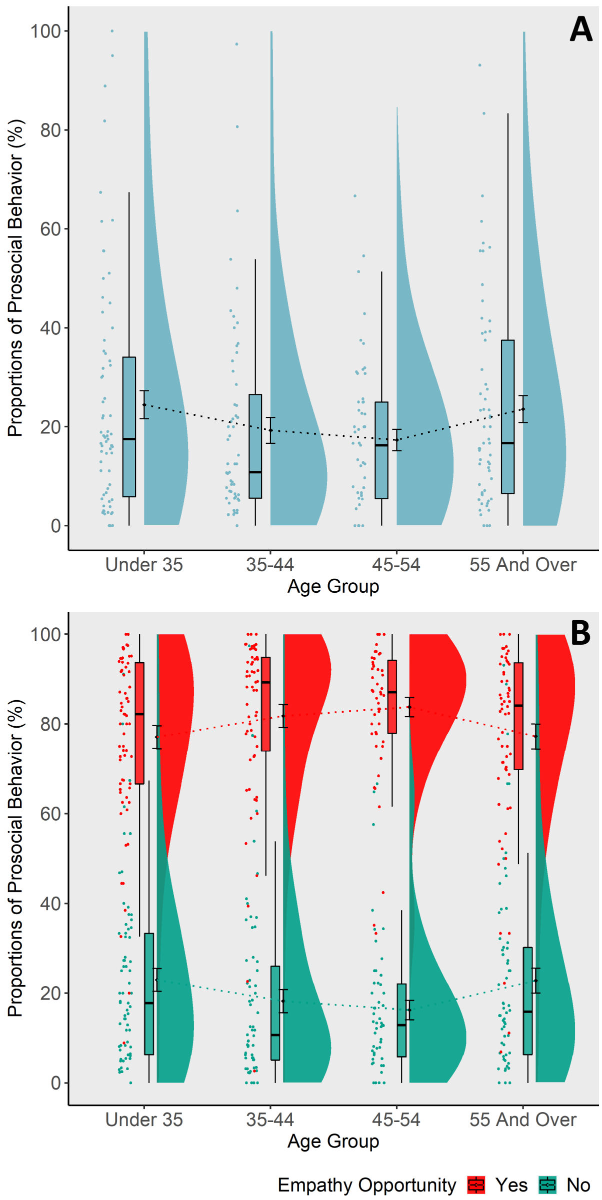
	Within-subject effects				Between-subject effects			
Interaction term predicting prosocial behavior	*t* or *z*-score	Adj. *p*-value	Estimate (*SE*)	Effect size (*r*)	*t* or *z*-score	Adj. *p*-value	Estimate (*SE*)	Effect size (*r*)
Empathy opportunity × age group	-2.80	0.024*	0.05	-0.04	-1.90	0.406	0.20	-0.10
Empathy opportunity × age group^2	2.72	0.024*	0.06	0.04				
Target of empathy × age group	1.12	0.351	0.04	0.01	-0.94	0.819	0.32	-0.08
Empathy × age group	1.13	0.351	0.09	0.03	0.03	0.978	0.31	0.00
Emotion share × age group	-1.43	0.308	0.11	-0.04	0.81	0.819	0.33	0.07
Perspective take × age group	-1.72	0.227	0.11	-0.05	0.52	0.844	0.32	0.05
Compassion × age group	0.42	0.674	0.15	0.02	0.25	0.940	0.41	0.03
Interaction term predicting prosocial behavior	Chi^2	Adj. *p*-value			Chi^2	Adj. *p*-value		
Valence$^{\alpha}$ × age group$^{\beta}$	0.20	0.674			0.53	0.819		

Note. Prosocial behavior was included in all models as binary outcome variable. Statistics obtained from mixed models, nested within participant and survey day. Each interaction ran in a separate model, with age as linear and quadratic term separately. Model selection was conducted based on a loglikelihood ratio test. *P* values were adjusted to control the false discovery rate.

*$p < 0.05$. [α]Positive, negative, or neutral target emotion. [β]Reduced random effect structure due to convergence warnings, only nested within participant.

Figure 13

Daily prosocial behavior



Note. The y-axis shows the proportions of answering 'yes' when asked about acting prosocially relative to the total amount of answered surveys. (A) Adult age differences in prosocial behavior. No significant association between age and prosocial behavior was found. (B) Within-subject effect of empathy opportunity x age on prosocial behavior. All age groups showed more prosocial behavior after an empathy opportunity. A quadratic effect of age group indicated that this effect was more pronounced in the middle-aged groups than in the younger and older age group.

Adult age differences in subjective well-being. Overall, and contrary to our a-priori hypothesis, we did not find significant adult age effects on daily subjective well-being (b = 0.11, SE = 0.08, $t(233)$ = 1.48, p = 0.141, r = 0.10, see Figure 14A). In Depow and colleagues' (2021) analyses, different aspects of daily empathy (e.g., empathy opportunity and actual feelings of empathy) were associated with subjective well-being. Thus, in a next step, we analyzed whether such an association of different aspects of daily empathy and well-being, as reported on the group-level, was moderated by age. No significant interaction with age was revealed (all adj. ps > 0.154, see Table 7). We found a significant interaction effect between age and prosocial behavior on subjective well-being, but only as a within-subject effect (b = 0.03, SE = 0.01, $t(6315)$ = 2.62, p = 0.009, r = 0.03; see Table 7). As can be seen from Figure 14B, within-person, across all age groups, higher well-being was associated with acting prosocially before. However, the difference in well-being as a function of an opportunity to act prosocially was slightly more pronounced in the younger age groups (see Figure 14B).

Table 7

Within- and between person effects of different interactions regarding different aspects of daily empathy and age predicting daily well-being

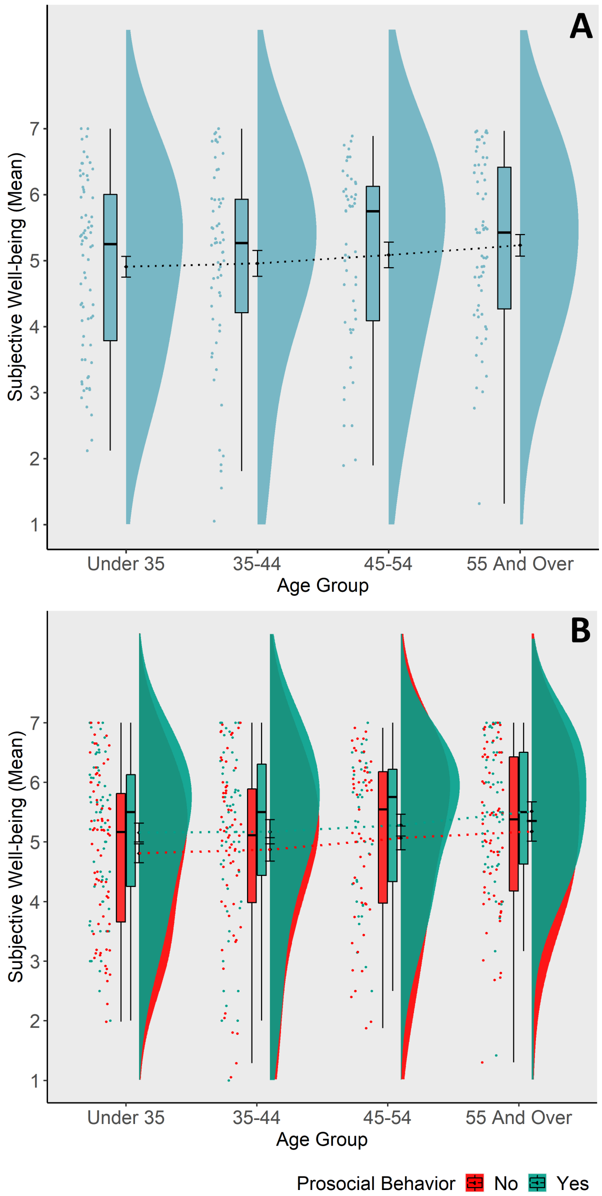
	Within-subject effects				Between-subject effects			
Interaction term predicting well-being	*t* or *z*-score	Adj. *p*-value	Estimate (*SE*)	Effect size (*r*)	*t* or *z*-score	Adj. *p*-value	Estimate (*SE*)	Effect size (*r*)
Empathy opportunity × age group $^{\alpha}$	0.43	0.667	0.01	0.01	-2.03	0.154	0.35	0.13
Target of empathy × age group $^{\alpha}$	0.52	0.667	0.01	0.01	-2.26	0.154	0.40	0.15
Empathy × age group $^{\alpha}$	0.74	0.643	0.04	0.02	1.30	0.341	0.30	0.09
Emotion share × age group	-1.17	0.569	0.04	0.04	-0.13	0.931	0.30	0.01
Perspective take × age group	-1.35	0.569	0.04	0.04	1.02	0.435	0.31	0.07
Compassion × age group	-0.96	0.593	0.05	0.03	-0.09	0.931	0.39	0.01
Interaction term predicting well-being	*F*-value	Adj. *p*-value			*F*-value	Adj. *p*-value		
Valence$^{\beta}$ × age group	1.73	.569			2.86	.212		
Interaction term predicting well-being	*t* or *z*-score	*p*-value	Estimate (*SE*)	Effect size (*r*)	*t* or *z*-score	*p*-value	Estimate (*SE*)	Effect size (*r*)
Prosocial Act × age group	2.62	0.009**	0.01	0.03	-1.29	0.198	0.34	0.08

Note. Well-being was included in all models as continuous outcome variable. Statistics obtained from mixed models, nested within participant and survey day. Each interaction ran in a separate model, with age as linear and quadratic term separately. Model selection was conducted based on a loglikelihood ratio test. P-values were adjusted to control the false discovery rate.

** p < 0.01. [α]Between- subjects effect models include religiosity as covariate. [β]Positive, negative, or neutral target emotion.

Figure 14

Daily well-being



Note. (A) Adult age differences in subjective well-being. No significant association between age and well-being. (B) Within-subject effect of prosocial behavior x age on well-being. Positive association between well-being and acting prosocially, irrespective of participants' age. Stronger association of a prosocial act with well-being in younger adults.

Discussion

In this study, we used experience sampling data (Depow et al., 2021) to test adult age differences with respect to empathy, prosociality, and well-being. Based on a representative sample, we were able to yield several new insights into the lifespan development of these important social functions, as well as their interactions in daily life.

With respect to daily empathy, we found a (small) linear effect of age on empathy across the lifespan, which is in line with postulated stable or even increased socio-affective processes in older adults, as measured in laboratory studies (e.g., Reiter et al., 2017; Stietz et al., 2021; Sze et al., 2012). Interestingly, in this ecologically valid description of a near-representative sample we found an inverted U-shaped relationship of age and empathy. This pattern suggests that daily empathy increased from 18 years on, with a peak in midlife, but decreased slightly in the oldest group (55 years and older). Notably, the empathy level of the oldest group was higher than the one of the youngest. This pattern is in line with the findings from O'Brien and colleagues (2013), with respect to their quadratic associations in self-reported trait empathic concern and perspective taking. Further, there is cross-sectional and longitudinal evidence that moving from early to middle adulthood is associated with more stable dispositional traits like agreeableness (McAdams & Olson, 2010) and that the period of middle adulthood is also linked to highest values of generativity (Wojciechowska, 2017), potentially rendering empathy a particularly relevant skill in this period of life.

When comparing the results of this experience-sampling study to previous research on the lifespan development of empathy, it is important to note that the definition of the construct empathy varies across studies, which has been criticized recently (Hall & Schwartz, 2019), reminiscent to a discussion about so-called "Jingle-Jangle fallacies" (Kelley, 1927; Marsh, 1994). Such an inconsistency in the definition of empathy also affects studies investigating age differences in empathy. In the current study, empathy was regarded as an umbrella term spanning the subcomponents emotion sharing, compassion, and perspective taking; consequently, participants only provided ratings for these subcomponents when they had indicated feelings of empathy before. Based on this operationalization, a high co-occurrence between emotion sharing, perspective taking, and compassion was reported, and no age-related differences regarding the three subcomponents nor their co-occurrence were found. Previous ageing studies differentiated (emotional) empathy in terms of affect sharing, from compassion, and perspective taking (e.g., Reiter et al., 2017; Stietz et al., 2021), by assessing them independently from each other, which revealed differential age-related findings for each construct (Beadle & de la Vega, 2019; Stietz et al., 2019 for review).

The majority of studies on empathy thus far focused, by design, on empathy exclusively in the context of negative stimuli and most often with strangers in a lab (e.g., EmpaToM-

Paradigm (Kanske et al., 2015), as used in Reiter et al. (2017)). A commendable recent exception by Ziaei and colleagues (2021) included positive and negative stimuli when measuring empathy in a behavioral task in the lab, demonstrated that older adults responded significantly slower to negative than to positive stimuli. Interpreting this result, the authors argued there is a greater difficulty in processing negative emotions in older adults. The dataset used for the current analyses offered the opportunity to examine age effects in interaction with the valence of a real-life situation. Indeed, Depow and colleagues (2021) showed that empathy was reported more frequently following situations with a positive valence. In contrast to a hypothesized positivity bias (Ziaei et al., 2021), this effect was not significantly enhanced with increasing age. This is in line with a previous study conducted in the lab (Ziaei et al., 2017) which could not find behavioral age-related differences in working memory performance as a function of emotional valence. However, the experience of affective empathy, particularly compassion (Klimecki et al., 2014), be it in an emotionally positive or negative context, might be rewarding in itself or subsequently lead to satisfying social interactions (if, e.g., followed by prosocial behavior as demonstrated in the current dataset). In this regard, experiencing enhanced empathy in both positive and negative domains is in line with the lifespan goals suggested by emotional selectivity theory (Carstensen et al., 1999), namely an enhanced pursuit of emotionally satisfying interactions in midlife and older age.

In this experience-sampling study, no adult age differences in prosocial behavior were observed. This is in contrast to most laboratory studies using economic decision-making tasks (e.g., dictator game, public good game), or experimental helping/donating paradigms, which showed pronounced prosocial behavior in older adults (e.g., Bailey et al., 2013, 2020; Beadle et al., 2015). Also studies using self-report measures in terms of validated questionnaires, showed greater prosociality with advancing age, by comparing younger vs. older adults (Gaesser et al., 2017), across younger and middle-old adults (Gibson et al., 2008), or across the whole adult lifespan (T. Li & Siu, 2019; Rushton et al., 1989). While these inconsistencies in results may be argued to be due to these studies not measuring behavior in real life, discrepancies also occurred when comparing the current results with studies that measured real-life prosociality. Cavallini et al. (2021) reported a significant negative association between age and self-reported real-life prosocial activities, arguing that real-life prosocial behavior is more cognitively and physically demanding. An important difference between the study by Cavallini and colleagues (2021) and the current study is the higher age on average, as they only included participants aged 55 years and older, but no younger age group. Thus, our comparably young sample here might not be ideally suited to detect potential effects of physical and cognitive demands on an aging effect in prosocial behavior. Further, in Cavallini and colleagues' study (2021) prosocial behavior was not measured using EMA (e.g., in the last 15 min), but based on a self-reported questionnaire which asked about the recalled frequency of acting prosocially in a pre-defined set of prosocial scenarios during the last 12 months. It is possible that different types of prosocial acts are assessed by self-reported questionnaires compared to EMA measures. Cavallini et al's (2021) method has potentially captured acts that are more memorable over a longer time frame and thus might have been more resource-intense (e.g., charitable giving). In the current study, prosociality is defined more broadly (as anything including direct or indirect help, to make another person feel better). Thus, daily acts (including smaller acts) of prosociality (e.g., holding the door open) would be included in the EMA-based self-report. Consequently, it is plausible that EMA-based self-report captures acts that might have been forgotten otherwise. Due to such memory effects, or recall biases, which are more susceptible in global retrospective reports (Shiffman et al., 2008), a difference in the reference time frame of self-report (12 months in Cavallini et al's study (2021) vs. 15 min in the current study) could be particularly relevant for lifespan studies, as there is evidence for an increased memory-experience gap in older individuals (Neubauer et al., 2020).

We expected an attenuated relationship of empathy and prosocial behavior with higher age (Cavallini et al., 2021) which was only partially confirmed. Age did not modulate the associations between actual feelings of empathy, nor of any of its subcomponents with prosocial behavior. However, the interaction between the opportunity to empathize, and age, both as a linear and a quadratic term on prosocial behavior was statistically significant as a within-person effect. Meaning all age groups showed more prosocial behavior when there was an empathy opportunity. However, this stronger tendency to act prosocially if there was a situation eliciting empathy (as compared to when there was none) was found to be slightly more pronounced in the middle-aged group of the sample. This is different compared to the results by Cavallini and colleagues (2021), which showed that with increasing age (> 75 years), daily prosocial behavior was less driven by empathic concern. When comparing these results it should be noted that the participants in Cavallini and colleagues (2021) were considerably older, and the self-reported measure reflected memories of prosocial acts within the last 12 months. Taken together, it leaves an open research question on what drives prosocial behavior in older adults' everyday life.

In the current study, we did not observe the often-found U-shaped age-related pattern of self-reported well-being previously found in larger studies (e.g., Orben et al., 2020; Stone et al., 2010). Subjective well-being was assessed by two different questions, merged into one universal well-being score, covering hedonic well-being (i.e., experienced happiness) and eudaimonic well-being (i.e., purpose of life). Even though eudaimonic well-being gets more attention in the current literature (Dolan et al., 2017; Steptoe et al., 2015), most studies that have found a U-shaped pattern of subjective well-being emphasized a hedonic operationalization of well-being (e.g., Frijters & Beatton, 2012; Galambos et al., 2020). However, beyond the measurement level, it is noteworthy to discuss the recent debate about the robustness and generalization of the putative U-shaped pattern with regard to the association of adult age and well-being (Galambos et al., 2020; Laaksonen, 2016; N. Li, 2016). A recently published study (Galambos et al., 2020) challenged this often as typically assumed U-shaped pattern, by revealing inconsistent findings with respect to the association of age and well-being, both in cross-sectional and longitudinal studies. Additionally, it is argued that age effects on well-being could be an epiphenomenon of other variables, reflecting the current life circumstances associated with ageing (e.g., income, education), fixed effects like personality traits, or selection effects which might differ as a function of age group (Frijters & Beatton, 2012; Lelkes, 2008; N. Li, 2016).

We confirmed a hypothesized moderation effect of age on the link between daily prosocial behavior and subjective well-being. Across all age groups, subjective well-being within a person was higher when a prosocial act was performed 15 min before. However, this trend was attenuated with increasing age. This is in line with Chi and colleagues' (2021) recently published observations about a decreased influence of prosocial behavior on well-being in older compared to younger adults. Interestingly, an earlier study (Van Willigen, 2000) observed the opposite when looking at the long-term impact (i.e., over 3 years of time) of volunteering in a longitudinal survey, namely enhanced well-being as a result of volunteering in older compared to younger adults. Given shrinking time horizons for older versus younger adults, which have been associated with different (socio-emotional) life goals (Carstensen et al., 1999), it would be an interesting venue for future studies to differentiate short- and long-term consequences of prosocial behaviors and age differences therein.

It is interesting to note that across many of the measured variables, inconsistencies (and indeed, often times null effects of age) with previously reported age-related results were observed. More specifically, compared to published reports on effects of adult age on prosocial behaviors, well-being, and empathy, we observed null effects of age on prosociality and well-being, and a small quadratic effect on empathy. In the following, we speculate about where these discrepancies may arise from.

First, such discrepancies may reflect different motivational aspects elicited by different study designs (e.g., in-lab vs. real-life, experimental vs. self-reported (Böckler et al., 2016)). On one hand, there is evidence for external validity of commonly used tasks in the domain of prosociality (Franzen & Pointner, 2013; Peysakhovich et al., 2014). On the other hand, a previous study showed that lab-induced prosocial behavior differed drastically from real-life prosocial behavior in a natural-field dictator game. The authors interpreted these findings as inflated prosocial behavior elicited by the lab context (Winking & Mizer, 2013). Further, observability has been shown to be associated with higher prosocial behavior (Bradley et al., 2018), underpinning the assumption that lab-induced prosocial behavior might be different in nature from prosocial behavior in real life. Further, different studies identified differential drivers of empathy. The likelihood to engage in empathy-eliciting situations increased with monetary incentives, and also when the target of empathy was familiar (Ferguson et al., 2020). It has also been found that increased perceived closeness leads to better perspective taking abilities in older adults (X. Zhang et al., 2013, 2018) and both, our own age and the social interaction partner's age affect these capacities (Reiter, Diaconescu, et al., 2021).

Second, it has been shown that the sampling strategy influences the degree of prosociality. Indeed, in many age-comparative studies, the younger age group might consist predominantly of students (e.g., Reiter et al., 2017; Stietz et al., 2021), who have been demonstrated to display systematically less prosocial behavior in in-lab studies. This remains true when controlling for age in this population (Carpenter et al., 2008). A strength of the current study is that the sample was quota-sampled on six key demographic variables (i.e., sex, ethnicity, education, geographic region, income, and age), even though representativeness might not be given with respect to other demographics (e.g., marital status, current living situation), as the sample is not random. Moreover, there was a reduced number of participants sampled from the youngest (18–24 years) and oldest (65 years and older) age group, which is why the two youngest and two oldest groups were merged for all analyses. Thus, conclusions about differences with respect to these groups, and comparability with ageing studies, which typically include a wider range of older adults, are limited.

A methodological strength of the dataset analyzed here is the experience-sampling method used to acquire data. EMA studies help to provide closer and deeper insights and to broaden our understanding of real differences across the lifespan by investigating the frequency, intensity, and complexity of different measures. Advantages lie in increased ecological validity, decreased recall bias, and the possibility to measure variability and change over a short time. Despite its many advantages, repeatedly responding to the survey could also have an influence on whether people notice empathy opportunities (for a more detailed discussion of representativeness, potential training, and fatigue-effects see Depow et al., 2021). Experience sampling also shares with other self-report measures its susceptibility to biases, like social desirability. More objective, implicit measures of empathy (e.g., physiological reactions towards others' suffering) have been used in the lab and have also revealed age differences (Y.-C. Chen et al., 2014; Sze et al., 2012). A limitation of the current study is that we exclusively relied on explicit EMA reports and did not include other, complementary measures of empathy. A recent study (Hildebrandt et al., 2021) adopted a combined EMA-fMRI approach in young adults to show that affective components of empathy ratings assessed via EMA were associated with behavioral measures from an experimental empathy paradigm, but not with neural activation. Daily cognitive components of empathy ratings assessed using EMA were correlated with neural activation in the medial prefrontal cortex, but not significantly related to perspective taking performance in an experimental task. To gain a more comprehensive insight into the adult lifespan development of the social mind, such multi-modal designs combining EMA, experimental, and physiological measures (Gadassi et al., 2021; Grosse Rueschkamp et al., 2019) are a promising venue for future lifespan studies.

Conclusion. Factors that contribute to, or result from, successful social interactions like empathy, prosocial behavior, and well-being are important throughout life. This analysis of cross-sectional experience-sampling data suggests that most of these constructs show no significant age-related differences when measured repeatedly via self-report in daily life. One exception was empathy. Here we observed a weak inverted U-shaped effect of age on empathy over the course of the lifespan. Future studies should take into account methodological differences stemming from varying study-designs and construct definitions, which might be one source of inconsistencies in age-related results in the literature. Multivariate studies combining standardized in-lab experiments with real-life experience-sampling data are a promising venue for future lifespan studies on the social mind.

Authors' contributions

L.P. and A.M.F.R. conceived the study. G.J.D., and M.I. developed the survey and collected data. L.P. and A.M.F.R. analyzed the data. L.P., J.S., P.K., S-C.L., and A.M.F.R. interpreted the data. L.P. wrote the manuscript. All authors provided critical revisions and approved the final version of the manuscript.

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2.3 Adult age differences in the integration of values for self and other

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Abstract

Previous research suggests that older adults may display more prosocial behavior than younger adults. However, recent meta-analyses indicate that effects are heterogeneous, may be small, and are influenced by how prosociality is measured. Further, the precise cognitive and computational factors contributing to age-related differences in prosocial behavior remain largely unknown. In this study, we utilized a modified dictator game to combine a value-based decision framework with Bayesian hierarchical drift-diffusion modeling to investigate prosocial decision-making in a sample of younger (n = 63) and older adults (n = 48). We observed differences in how older and younger individuals incorporate information corresponding to potential gains for themselves (self) and another person (other) to reach a (potentially prosocial) decision. Younger adults integrated values for benefits for themselves and others in the decision-making process and demonstrated increased decision-making efficiency by effectively integrating both sources of information. In contrast, OA showed improved decision-making efficiency when solely considering values for self and others separately. Interestingly, individual differences in the capacity of inhibitory control in older adults moderated the observed age effects: older adults with stronger inhibitory control abilities made decisions based on the integrated information of benefits for themselves and others, showing a behavioral pattern similar to that of younger adults. Together, these findings offer new insights into the behavioral and computational mechanisms influencing age effects in prosocial decision-making.

Introduction

In 2050, the global population will comprise an equal number of elderly and younger individuals and thus, aging global populations present significant health, social, and economic changes in the coming decades (e.g., Harper, 2014). This change is linked to challenges as well as opportunities. Prosocial behavior, for example, is a vital component for the functioning of societies (Nowak, 2006) and has a profound impact on individuals' mental and physical well-being across their lifespan (Brown et al., 2003; Hui et al., 2020; Konrath et al., 2012; O'Reilly et al., 2008). Past research has provided evidence for higher prosociality in older as compared to younger adults (e.g., Cutler et al., 2021; Engel, 2011; Pollerhoff et al., 2023; Sparrow et al., 2021). However, recent meta-analyses indicated that this effect might be small and have revealed significant heterogeneity in the association of adult age and prosociality, depending on how prosociality is measured (Bagaïni et al., 2023; Pollerhoff et al., 2023; Sparrow et al., 2021). Importantly, cognitive mechanisms underlying age-related differences in prosocial behavior, as well as factors moderating them, remain yet to be identified (Mayr & Freund, 2020).

Prosociality incorporates various behaviors such as cooperating, helping, or sharing (N. C. Ebner et al., 2017; N. Eisenberg et al., 2006), all of which involve a cost to the self in order to benefit others (Andreoni, 1989; Contreras-Huerta, 2023). Thus, a decision to engage in prosocial acts can be cast as the result of a cost-benefit calculation based on integrating values for oneself and others (Berkman et al., 2017; Hutcherson et al., 2015; Mayr & Freund, 2020). Interestingly, studies in the non-social domain suggest differences between younger and older adults in tasks that require integrating information from different sources or cost-benefit trade-offs (Bagaïni et al., 2023; De Dieuleveult et al., 2017; Devine et al., 2021; Ruel et al., 2021), potentially due to aging-related changes in available cognitive (control) resources (H.-Y. Chen et al., 2021; Ruel et al., 2021). A decline in cognitive resources in older age might affect the integration of all available information relevant to the decision-making process (Mata & Nunes, 2010) and computationally, lower cognitive abilities have been related to lower efficiency in the decision process (e.g., Schmiedek et al., 2007; Schubert et al., 2015). In line with this notion, it has been proposed that older adults may rely on processes that require lower cognitive demands during decision-making, aiming to minimize cognitive effort (Gigerenzer, 2003; Sanfey & Hastie, 1999), for example, by analyzing and/or integrating less information (Mata et al., 2007, 2010; Shah & Oppenheimer, 2008).

To explicitly test the factors underlying potential age-related differences in (pro)social decision-making, drift-diffusion models (DDMs; Ratcliff, 1978; Ratcliff et al., 2016) are a promising tool to computationally describe trade-offs like the decision to help someone at one's own cost (e.g., Iotzov, Saulin, et al., 2022; Iotzov, Weiß, et al., 2022; Saulin et al., 2022; Weiß et al., 2023). That is, DDMs provide a quantitative framework to delineate cognitive subcomponents involved in cost-benefit calculations. In detail, they assume that a noisy signal representing the relative value of (most often) two options, like helping someone or not, is integrated at each moment, and a decision is made when the accumulated evidence reaches the boundary of the corresponding choice option (Busemeyer & Townsend, 1993; Hunt et al., 2012; Krajbich et al., 2010). This process can be characterized in terms of three parameters that each capture a different aspect of the decision process: Firstly, the *drift rate*, a parameter which quantifies the efficiency of the decision process. The larger this parameter, the more efficiently the decision is made. Secondly, the *initial bias*, a parameter which is a marker for a participant's a-priori bias towards one decision option before accumulation of evidence has started. This reflects that if one of the two response options (like helping someone or not) has a higher expected value for an individual, participants shift the starting point towards this favored option. And thirdly, the *boundary separation* which is a measure for how much evidence needs to be accumulated to reach a decision. The larger this parameter, the larger a participant's response caution favoring accuracy over speed (Ratcliff et al., 2016). Past work has demonstrated that DDMs can be successfully applied to delineate cognitive subcomponents that drive decisions in the social domain (Chen & Krajbich, 2018; Hutcherson et al., 2015; Hutcherson & Tusche, 2022). Thus, this approach is a valuable tool to gain insights into the cognitive mechanisms underlying differences in prosocial decision-making between younger and older adults (Tusche & Bas, 2021).

Previous studies leveraging DDM to study age-related differences in decision-making observed lower drift rates when the task required the integration of different information, i.e., a reduced decision-making efficiency in older but not younger adults when combining different sources of information (Bocheva et al., 2018). More generally, older adults exhibited lower drift rates in the perceptual and memory domain (Ratcliff et al., 2011; Theisen et al., 2021). Moreover, on a range of non-social tasks older adults task-independently adopted higher boundary separations as compared to younger adults (i.e., they apply more cautious response criteria than younger adults, Theisen et al., 2021).

Building on these findings in the non-social domain, we aimed to provide insights into the precise mechanisms underlying age effects in the cost-benefit trade-off of social decision-making. More specifically, we asked whether older and younger adults differ when integrating values for themselves and others to reach a social decision and whether we can relate these differences to interpretable cognitive subcomponents like decision-making efficiency, evidence accumulation, and initial response bias for self-serving or other-serving options. To test this, we exposed younger and older adults to a widely used task to measure prosociality (modified dictator game, Hutcherson et al., 2015). In this paradigm, the decision-maker is exposed to a set of choice options with varying payoffs for oneself and another person, which can be integrated or considered independently to make a decision. This allowed us to examine age group differences in value-integration processes during prosocial decision-making by modeling their choice behavior and reaction times using state of the art drift diffusion modeling (Vandekerckhove et al., 2011; Wiecki et al., 2013).

Further, we were interested in factors that might influence the cost-benefit trade-off of prosocial decision-making, and age differences therein. In general, there is increasing evidence that socio-emotional and socio-cognitive abilities like empathy, compassion, and Theory of Mind predict prosociality (Batson, 2014; Caputi et al., 2012; De Waal, 2008; Depow et al., 2021; N. Eisenberg & Fabes, 1990; N. Eisenberg & Miller, 1987; Lehmann et al., 2022; Schulreich et al., 2022; Tusche et al., 2016; Underwood & Moore, 1982). However, studies testing whether the influence of empathy and Theory of Mind on prosocial behavior differs as a function of age showed mixed results (e.g., Bailey et al., 2020; Beadle et al., 2015; Cavallini et al., 2021; Cho et al., 2022; Pollerhoff et al., 2022; Sze et al., 2012). Moreover, general cognitive abilities have been suggested to play a moderating role in age-related differences in prosocial behavior (Mayr & Freund, 2020). For instance, older age is associated with reduced inhibition and cognitive functioning per se (Braver et al., 2001; Deary et al., 2009; Kane et al., 1994; Lövdén et al., 2020), both of which are negatively associated with giving behavior in adult age (Cappelen et al., 2016; C.-C. Chen et al., 2013; Inaba et al., 2018; Ogawa et al., 2020; Rosi et al., 2019; Sütterlin et al., 2011; Yamagishi et al., 2016, 2017). To investigate the role of such potential social and cognitive moderators of age differences in social decision-making, we additionally assessed social, general cognitive and inhibition capacity in our participants.

Based on previous studies in the non-social domain, we hypothesized that older adults use the integrated information about their own and the other person's payoff to a lesser extent than younger adults. This would be reflected in a smaller interaction effect of self and other payoff in older as compared to younger adults. Furthermore, we hypothesized that age-related differences in prosocial decision-making may be attributed to the efficiency of the prosocial decision-making process (i.e., different drift rates in older adults than younger adults, especially for other-serving decisions), and/or the initial bias towards other-serving decisions (i.e., different initial bias in older adults than younger adults). Additionally, response caution may be increased for older adults (i.e., larger boundary separation in older adults than younger adults). Lastly, we explored to what extent individual differences in cognitive and social abilities may modulate these potential age-related differences.

Methods

The current study was part of a bigger project consisting of three testing sessions. Here, we focused on the behavioral data from a social choice task (a modified dictator game based on Hutcherson et al., 2015), the EmpaToM task (Kanske et al., 2015), a paradigm to assess socio-affective and socio-cognitive processes, and a cognitive functioning battery. In a separate session participants completed an inhibitory control battery (based on Wolff et al., 2016). Other data gathered within this larger project have been (or will be) reported elsewhere (Stietz et al., 2021)

Participants. A total of 126 participants participated in the relevant test sessions of the study: 70 younger adults (YA), age range 18-30 years, and 56 older adults (OA), age range = 65-78 years. This sample largely overlapped with a previously published study on a different experimental task completed within the same sessions (Stietz et al., 2021). For recruitment and exclusion criteria see Supplemental Methods. Sample characteristics of the final sample are reported in Table 8. We did not observe significant age group differences concerning years of education, relationship status, or residence.

Table 8

Descriptive and inferential statistics with respect to sample characteristics, inhibition scores, cognitive functioning, and the EmpaTom measures.

Sample Characteristics	*n* (YA / OA)	Frequency (YA / OA)	Test statistic
Relationship status (partner/single)	62/44	13|31 / 27|35	χ^2 = 1.59, *p* = 0.21
Residence (alone/with others)	61/48	14|47 / 14|34	χ^2 = 2.10, *p* = 0.35
Sample Characteristics	*n* (YA/OA)	M (*SD*) (YA/OA)	Test statistic
Years of education	62/41	16.71 (2.28) / 16.84 (3.15)	*t* = 0.39, *p* = 0.69
Inhibition			
Go-Nogo (ms)	63/47	388 (88) / 461 (105)	*t* = 4.06, *p* < 0.01
Stop Signal (ms)*	60/46	184 (55) / 210 (60)	*t* = -2.27, *p* = 0.03
Stroop (ms)	63/45	71 (49) / 128 (93)	*t* = 3.18, *p* < 0.01
Inhibition composite score (ms)	60/44	0.27 (0.63) / -0.35 (0.70)	*t* = 4.92, *p* < 0.01
Cognitive Functioning			
IDP accuracy	63/47	0.96 (0.05) / 0.96 (0.05)	*t* = 0.27, *p* = 0.79
IDP RT (ms)	63/47	2113 (333) / 3109 (457)	*t* = 12.82 *p* < 0.01
SAW accuracy	63/47	0.71 (0.07) / 0.81 (0.09)	*t* = 7.42, *p* < 0.01
SAW RT (ms)	63/47	3642 (1022) / 3779 (745)	*t* = 1.29, *p* = 0.20
TMT A (sec)	63/47	22.41 (7.09) / 40.82 (17.32)	*t* = 9.33, *p* < 0.01
TMT B (sec)	63/47	44.81 (13.85) / 82.13 (27.84)	*t* = 9.75, *p* < 0.01
DSb	63/47	7.56 (2.26) / 6.28 (1.41)	*t* = 2.59, *p* = 0.01
Composite of fluid abilities	63/47	0.42 (0.41) / -0.57 (0.50)	*t* = 10.26, *p* < 0.01
Composite of verbal abilities	63/47	-0.20 (0.66) / 0.26 (0.62)	*t* = 4.52, *p* < 0.01
Social emotions and understanding (EmpaToM)			
Empathy	63/48	-0.61 (0.41) / -0.66 (0.59)	*t* = 0.98, *p* = 0.33
Compassion	63/48	3.19 (0.93) / 3.78 (1.25)	*t* = 5.88, *p* < 0.01
Theory of Mind	63/48	0.71 (0.18) / 0.50 (0.20)	*t* = 7.07, *p < 0.01*

Note. YA = Younger adults, OA = Older adults, RT = Reaction time, IDP = Identical Pictures test, SAW = Spot A Word test, TMT A = Trail Making test A, TMT B = Trail Making test B, Dsb = Digit Span backward test. Test statistics represent results from a non-parametric robust yuen test (see Mair & Wilcox, 2019) due to violations with respect to the assumption of normality and/or homoscedasticity.

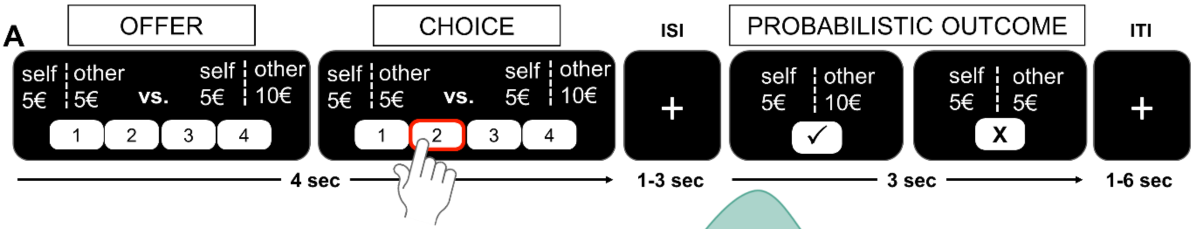
*Results from an independent t-test are reported as the assumptions of normality and homoscedasticity are fulfilled.

Materials and procedure. Participants provided written informed consent before the sessions. Participants filled in a questionnaire on socio-demographic characteristics(see Table 8). They were compensated with 8.50€ per hour for their participation and a bonus of a maximum of 11€ based on their decisions in the social choice task. Ethical approval was granted by the Technische Universität Dresden ethics committee in accordance with the Helsinki declaration (EK 486 112 015).

Social choice task: Modified dictator game. Two participants of the same age group and self-reported gender completed a social choice task (modified dictator game, DG; adapted from Hutcherson et al. 2015). To emphasize the social context, participants were first asked to consider the other participant (whom they saw at the beginning of the session) as their partner in the following game. Next, each participant completed nine practice trials of the computerized task to ensure task comprehension, followed by the 180 trials of the main DG. In each trial, participants chose between a proposed offer with varying monetary distributions for themselves ("€self" value) and their partner ("€other" value), or a constant default distribution of money (5€ for both) (see Figure 15A). The location of the two offers on the screen (left vs. right) was randomized across participants. The proposed offer varied from 1€ to 10€ and was drawn from one of the nine offer types shown in Figure 15B. Every offer type was shown 20 times, randomly intermixed across the experiment. As in Hutcherson et al. (2015), to avoid habituation and repetition effects, the values of €self and €other were randomly jittered by plus or minus 0 – 0.40 € (10€ were always jittered downward). Participants had up to four seconds to choose between the default distribution and the proposed offer on a four-point scale of "strong yes" (accepting the varying proposal) to "strong no" (rejecting the proposed offer in favor of the default). Failures to respond within four seconds yielded the automatic acceptance of the default and the on-screen feedback "too slow." To reduce social desirability effects, we further implemented the chosen options probabilistically (as in Hutcherson et al., 2015): with an 80% probability the chosen option was logged after the trial, while the non-chosen option was implemented in the remaining 20% of the time. Thus, in the case of a self-serving choice, a partner could not be certain whether this was indeed a selfish choice by the player, or a probabilistic outcome implemented by the computer algorithm. Participants were informed that at the end of the task, one trial would be randomly chosen and implemented (determining the bonus pay at the end of the session).

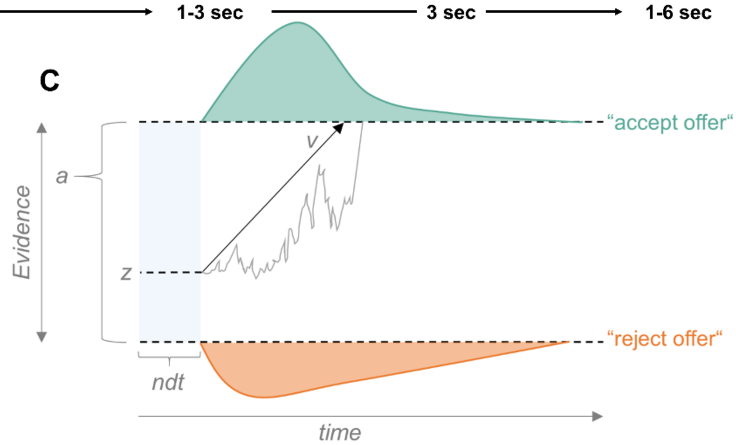
Figure 15

Modified dictator game to assess the integration of values for self and others and schematic illustration of the drift-diffusion model



B

No.	€Self	€Other	Condition
1.	2.50	7.50	other-serving
2.	2.50	10.00	other-serving
3.	5.00	1.00	self-serving
4.	5.00	10.00	rational
5.	7.50	1.00	self-serving
6.	7.50	2.50	self-serving
7.	10.00	1.00	self-serving
8.	10.00	2.50	self-serving
9.	10.00	5.00	rational



Note. (A) Illustration of the social choice task (modified dictator game, DG, based on Hutcherson et al., 2015). In each trial, participants accepted or rejected a varying monetary offer that affected payoffs for themselves (self) and another player (other) versus a constant default distribution of money (5€ for both players) using a 4-point scale (1 = strong no, 2 = weak no, 3 = weak yes, 4 = strong yes - towards the varying monetary offer; scale direction counterbalanced across participants). Outcomes were implemented in a probabilistic manner such that choices were implemented in 80% of trials or reversed in 20% of trials. (B) Nine proposed offer types, representing different monetary distributions between the participant (€self) and its partner (€other) and post-hoc classification of the offer types into self-serving, other-serving, and rational condition. Every offer type was shown 20 times (using a random jitter of up to 0.40€ to reduce habituation), randomly intermixed across the task. (C) The drift-diffusion model is used to conceptualize latent variables underlying the decision process, which are defined by the noisy accumulation of information (squiggly line). The non-decision time (ndt) controls for both sensory and motor-related processes. The *v*-parameter (drift rate) represents the speed of the accumulation process. The *a*-parameter (boundary separation) describes the distance between the two boundaries. The *z*-parameter indicates the initial bias of the accumulation process with respect to the two boundaries. A decision is made the moment one of the two boundaries is crossed (i.e., enough evidence accumulated). In the example trial (Figure 15A), choosing "1" or "2" would correspond to rejecting the offer (lower boundary), whereas choosing "3" or "4" would correspond to accepting the offer (upper boundary). Upper boundary = accepting the offer (i.e., rejecting the default distribution), lower boundary = rejecting the offer (i.e., accepting the default distribution).

Inhibitory control battery. To investigate whether inhibitory control moderates the relationship between adult age and the way values for oneself and others are integrated, we used a previously validated, computerized task battery designed to assess (amongst other constructs) inhibition performance (adopted from Wolff et al., 2016). The three inhibition tasks included a Go-Nogo, Stroop, and Stop Signal task (see Table 9 for details). Participants were instructed to react as fast an accurate as possible in all tasks. For the Go-Nogo and Stroop tasks, we calculated inverse efficacy scores (IESs) (Townsend & Ashby, 2014). For the Stop Signal task, we computed the stop signal reaction time (SSRT) using the quantile method (Congdon et al., 2012). Z-scores of all task measures were used to build a composite inhibition score for each participant by calculating the mean of these z-scores. Z-scores were estimated based on the following approach: For models including the whole sample, z-scores were calculated based on the whole sample (YA and OA) to reflect differences between YA and OA. For models including only one age group, z-scores based on the specific age group were calculated (YA or OA) to reflect intra-group differences in one of the age groups. See Table 8 for descriptive and inferential statistics of task-specific and composite inhibition scores.

Table 9

Inhibition tasks (adapted from Wolff et al. (2016))

Task (adapted from)	Task description	Outcome measure
Go-Nogo	Two vertically or horizontally arranged dots (500ms) were presented. In “go” trials (vertically arranged dots), participants should press the response key. In “nogo” trials (horizontally arranged dots), participants should not react. In total the task consisted of 280 “go” and 40 “nogo” trials. At least five “go” trials were in between a “nogo” trial.	IES[1] (average RTs for correct “go” trials are divided by the proportion of correct “nogo” trials)
Stroop (Bush et al., 1998; Krönke et al., 2015)	A stimulus was presented (1000ms) illustrating a row of either one, two, three, or four identical digits ranging from 1 to 4. The number and denotations of the digit could either be congruent (80 trials) or incongruent (80 trials). Instruction indicated to ignore denotations but respond in accordance with the number of presented digits by pressing one of four corresponding response keys.	IES[2]-difference (IES from congruent vs. IES from incongruent trials)
Stop Signal (Congdon et al., 2012; Rubia et al., 2003)	Participants saw an arrow that either pointed to the left or right side. In total, 200 “go” and 40 “stop” trials were included. In “go” trials (arrow presented for 1000ms), participants should press the left or right response key corresponding to the direction of the arrow. In “stop” trials (upward-pointing instead of sideward-pointing arrow), participants were instructed to withhold the response after a variable stop-signal delay (SSD). At least five “go” trials were presented between “stop” trials.	SSRT (quantile method[3] based on Congdon et al., 2012)

Note. Each trial starts with a fixation cross (750ms). IES = inverse efficiency score; RT = reaction time; SSRT = stop signal reaction time.

[1]IES, derived by dividing the average RT for accurate “go” trials by the ratio of correct response in “nogo” trials (indicated in milliseconds).

[2]First, IES were calculate separately for congruent and incongruent trials by dividing the corresponding average RT by the corresponding accuracy rate. After that, the difference between IES in congruent and incongruent trials was calculated for each participant.

[3]RTs from correct go trials are sorted in ascending order, and quantiles of RTs related to ERs on “stop” trials are selected. The mean SSD is then subtracted from the quantile RT to calculate SSRT (see also Congdon et al., 2012 and Wolff et al., 2016).

Cognitive functioning battery (fluid vs. verbal abilities). We characterized differences between YA and OA in cognitive functioning by adopting a previously used task battery (Reiter et al., 2017) that included the following tests: Trail Making tests A and B (TMT A and B; Reitan, 1955), the Identical Pictures test (IDP; Ekstrom et al., 1976), the Digit Span backward (DSb; Wechsler, 1997), and the Spot A Word test (SAW; Lindenberger et al., 1993). Matching the procedure of Reiter et al. (2017) and Stietz et al. (2021), mean composite scores were calculated based on z-scored test values to derive measures of fluid and verbal abilities. As in the inhibition task battery, two different approaches were followed for calculating z-scores and corresponding composite scores: For models including the whole sample, z-scores were estimated based on the entire sample (incl. YA and OA). For models including only one age group, z-scores were calculated with the values of this specific age group (YA or OA). To derive the average composite of fluid abilities, we assessed processing speed with the IDP, working memory with the DSb, visual attention with the TMT A, and complex attention with the TMT B. SAW scores (performance and RT), were used for the average composite score of verbal capacities. Previous findings from a larger population-based lifespan sample (S.-C. Li et al., 2004) suggested an age-related decline in fluid capacities, but an experience-related increase in crystallized abilities in OA compared to YA. In line with those findings (see S.-C. Li et al., 2004), YA showed better cognitive abilities concerning our composite score of fluid abilities compared to OA (YA: $M = 0.42$, OA: $M = -0.57$, $t = 10.26$, $p < 0.01$), whereas OA outperformed YA regarding the composite score of verbal abilities as assessed with the SAW (OA: $M = 0.26$, YA: $M = -0.20$, $t = 4.52$, p < 0.01). Descriptive and interferential statistics for the different cognitive tasks and composite scores can be found in Table 8.

EmpaToM paradigm (empathy, compassion, and theory of mind). The EmpaToM (Kanske et al., 2015; Tholen et al., 2020), a video-based social understanding task, was designed to measure empathy, compassion, and Theory of Mind (ToM) within the same individual and task. Every trial consisted of a ~15-second video clip in which a male or female narrator reported an autobiographic experience whose content could be either neutral or negatively emotional. Afterward, participants indicated on two different rating scales: i) how they felt after the specific video, that is, the current valence of their emotion (empathy measure), and ii) how much compassion they felt for the individual in the video (compassion measure). Last, participants responded to a multiple-choice question, which either required ToM (“[name of the narrator] thinks that...”) or factual reasoning (“It is correct that…”) as a control measure. Matching previous studies (Hildebrandt et al., 2021; Reiter et al., 2017; Stietz et al., 2021), the variables were operationalized as follows: i) behavioral empathy was defined by mean valence ratings in emotional minus neutral trials, ii) behavioral compassion was operationalized by mean compassion ratings across emotional and neutral trials and iii) behavioral ToM was analyzed by utilizing the accuracy in ToM-trials. For further details, see Stietz et al. (2021).

Statistical analyses. Statistical analyses were performed using Matlab (2022b; MATLAB, 2020), R (Version 4.1.2; R Core Team, 2018) with RStudio, and Python (Version 3.6.13). Age differences concerning sample characteristics, inhibition, cognitive functioning, empathy, compassion, and ToM (see Table 8) were examined with a non-parametric robust yuen test (see Mair & Wilcox, 2019) or χ^2-test. When the data fulfilled the assumptions of homoscedasticity and normality, an independent t-test was used (indicated by an asterisk in Table 8). The two primary outcome variables of interest for the DG were choice behavior and reaction time (RT). RTs were analyzed for responses given on all 180 choice trials. Rare trials with RTs < 200ms were removed (M = 1.91, SD = 3.13 of removed trials across all participants). In the following, RTs are indicated in seconds.

To model the effect of the different offer types as defined by our experiment, trials showing the different offers (Figure 15B) were classified into a condition variable (self-serving, other-serving, and rational condition). Trials in the other-serving condition included offer types where their acceptance yielded costs to participants to benefit their partner (compared to the default option of 5€ for both players). In other words, a choice was considered other-serving when participants accepted offers with €self ≤ 5€ but €other > 5€. The self-serving condition mirrored the other-serving condition. Specifically, choices were classified as self-serving when participants accepted offers that benefitted themselves at the cost to their partner (i.e., accepting offers with €self ≥ 5€ but €other < 5€). Two offer types (accepting €self5:€other10 and €self10:€other5) were categorized as rational condition since they don't disadvantage either player and thus their acceptance can neither be classified as prosocial nor selfish behavior. Refer to Figure 15B for classifying the nine different offer types into the condition variable. This classification allowed us to focus on the decision process in the other-serving condition as defined by prior studies on altruistic choice (Hutcherson et al., 2015; Tusche & Hutcherson, 2018).

See the paragraphs below for detailed information on the analyses of choice behavior, RTs and modeling. All scripts for statistical analysis are available via: https://osf.io/zu4p3/?view_only=8b5cf6703f3b4d04a7122308c31445f4.

Mixed model regression analyses of choice behavior and RTs in the DG. We ran two separate mixed-effect models to examine age effects (YA, OA) regarding the trial-by-trial choices and RTs in the DG, using the *mixed* function from the "afex" package in R (Version 1.1-0; Singmann & Kellen, 2019). For all mixed models, the F test statistic and p-values based on the Satterthwaite approximation are reported (Singmann & Kellen, 2019). When simple slopes were extracted from the mixed models, 95% confidence intervals were calculated using the *emtrends* function from the "emmeans" package in R (Version 1.7.3; Lenth et al., 2019).

Choice behavior (as derived from ratings on the four-point Likert scale) was included as a continuous dependent variable (DV) ranging from 1 = *strong no* to 4 = *strong yes* so that higher values indicated a stronger preference for the proposed offer compared to the default distribution (5€ per person). RTs were also treated as continuous DV. In both models (choice behavior vs. RT as DV), age group, amount distributed to oneself (€self), and amount distributed to the other person (€other) were included as fixed effects. In addition to the main effects, we also tested for 2-way and 3-way interactions. To account for potential time-related effects, we included a continuous trial variable (1 to 180) as a covariate in the fixed effects. Before fitting the random effects (including random intercept and random slopes), a model with and without the time covariate was compared via the loglikelihood ratio test (R function *anova*), and the model with the lower AIC was selected. Based on a conservative approach, the maximal random effect structure was fitted in the next step (Barr et al., 2013): Participant ID was included as a random intercept; the model also allowed for random slopes for the interaction of €self × €other and correlations among the random effect parameters. Whenever convergence warnings occurred, the number of iterations was increased, and the optimizer was changed to "bobyqa".

Moderation analyses concerning inhibitory control, a composite of fluid abilities, and a composite of verbal abilities, as well as empathy, compassion, and ToM were further investigated in separate post-hoc models, separately for choice behavior and RTs. In separate models for YA and OA we tested whether trial-by-trial choice behavior and RTs were predicted by a 3-way interaction of the moderator variable with €self × €other (e.g., whether the interaction term of inhibition × €self × €other predicted choice behavior in OA). Thus, a moderation effect was defined by a significant 3-way interaction either predicting choice behavior or RTs in YA or OA. We adjusted the p-values derived from models including the composite scores of fluid and verbal abilities (derived from the same cognition battery), as well as p-values derived from the models including empathy, compassion, and ToM (derived from one task, the EmpaToM task), as moderators to control for the false-discovery rate (FDR): (*p-adjust* function from the "stats" package (Version 4.1.2; R Core Team, 2022).

Hierarchical drift-diffusion modeling. Bayesian hierarchical drift-diffusion modeling (HDDM) (Vandekerckhove et al., 2011; Wiecki et al., 2013) was applied to model the choice and RT data from the modified DG. HDDM allows the exploitation of between- and within-subject variability in relatively small sample sizes with the help of Bayesian estimation methods. Diffusion modeling of choice and RT data was carried out with the Python package HDDM (Version 0.8.0, Wiecki et al., 2013).

For the HDDM analysis, binary outcomes were used that were computed based on participants' choice behavior on the four-point Likert scale (as measured in the DG). To achieve this, we recoded choices into a binary outcome format (accept proposed offer vs. reject proposed offer) following previous work (Hutcherson et al., 2015; Hutcherson & Tusche, 2022; Tusche & Hutcherson, 2018). A participant's choice of "strong no" or "weak no" indicated the rejection of the proposed offer (and acceptance of the default distribution) and was coded as zero. Choosing "weak yes" or "strong yes" defined the acceptance of the proposed offer coded as one.

The HDDM framework uses both trial-by-trial choices and RTs to estimate latent variables characterizing the prosocial decision process (*v*, *z*, and *a*-parameters). The *v*-parameter (drift rate) captures the speed of the accumulation process, with which the participant decides to accept or reject the offer. Thus, the v-parameter represents the efficiency of the prosocial decision-making process itself (Forstmann et al., 2016). The *z*-parameter describes the initial choice bias with which a participant enters the choice process, i.e., the extent to which a participant favors one choice option over the other (accept vs. reject) before entering the evidence accumulation process (F. Chen & Krajbich, 2018). Lastly, the *a*-parameter (boundary separation) reflects the amount of relative evidence to be accumulated to choose one of the two options. A large *a*-parameter indicates more cautious response behavior, whereas a small *a*-parameter indicates response behavior favoring speed over caution (e.g., Hedge et al., 2019; Ratcliff et al., 2006).

According to the theoretical reasoning of the drift-diffusion model, the integration of potential values associated with €self and €other during the decision process should be reflected by the modulation of the *v*-parameter (Forstmann et al., 2016; Saulin et al., 2022). The categorical effect of offer type, presented by the condition variable (other-serving vs. self-serving vs. rational), should potentially influence the modulation of the *z*-parameter and/or the *a*-parameter. These two parameters reflect the pre-decisional characteristics of the decision-making process. It is important to note that the combined information of €self and €other constitutes the condition. Therefore, including the condition as an additional potential regressor on the v-parameter would introduce collinearity and lead to model instability.

How exactly do older and younger adults make decisions in the DG? To address this question and test which components of the social decision process – such as concerns for benefits for themselves (€self) or others (€other) – best accounted for the observed choices and RTs, we estimated 12 models separately for each age group (see Table S9 for an overview). In the simplest model, all parameters were fixed across conditions. Progressively more complex models included only self-regard (€self), only other-regard (€other), or their combination. In the full model, the drift rate (*v)* was modulated by the interaction of values of €self and €other (reflecting the trade-off between self- and other-regard), and condition-related changes (other-serving vs. self-serving vs. rational) of the boundary separation (*a)* and initial bias (*z)*. Considering evidence that older adults behaved more prosocially than younger adults, the other-serving condition was used as the intercept of the regression model. Thus, the intercept of the condition effect on the *a*- and *z*-parameters can be interpreted as the absolute initial bias towards accepting the other-serving offer. In turn, the estimated coefficients of the self-serving and the rational condition can be understood as a decrease or increase in initial bias towards accepting the proposed offer *relative* to the acceptance of the other-serving condition.

Besides the three main parameters, we further estimated the non-decision time ndt and allowed for trial-by-trial variations of the initial bias (*sz*), the drift rate (*sv*), and the non-decision time (*sndt*) fixed across conditions. As no mixed designs (between- and within-subject effects) are currently possible to implement in HDDM, we fitted all 12 models separately for YA and OA as recommended by the authors of the toolbox (Sandhu, 2020; https://groups.google.com/g/hddm-users/c/JD1kfl_12q8/m/b6f_yNCoAAAJ). Default values for priors and hyperpriors, as provided by HDDM, were used for the estimation process (see Wiecki et al., 2013). All models (i.e., the included parameters) were visually inspected for convergence with respect to traces, autocorrelations, and marginal posteriors (see Figures S5 and S6). Due to the visual appearance of convergence problems in the estimation process of the a-parameter (especially with respect to OAs' choices and RTs), we increased iterations to 4000 and burn-ins to 2000 (for recommendation see Iftach, 2023; https://groups.google.com/g/hddm-users/c/cQaJ4gWc9uY/m/R0YUnfYLBwAJ). Further, we increased the possibility of potential outliers to 10% to improve model fit (as recommended, e.g., Voss et al., 2013). This approach satisfyingly improved convergence for nearly all parameters, (see Figure S2). Convergence for the *a*-parameter was still suboptimal, which is why the results with regard to the *a*-parameters should be interpreted with caution.

Model convergence was further checked with the Gelman-Rubin statistic (all values < 1.01, Gelman and Rubin, 1992). In the next step, posterior predictive checks were performed. In detail, we compared our observed data with 500 simulated datasets based on the posterior of our model (Wiecki et al., 2013). If the empirical data fell within the 95% probability interval of the simulated data, the model was considered to describe our data well. We extracted the parameters of interest (*v*, *z*, and *a*-parameter) from the winning models (YA and OA) for further analyses. Statistical analyses were performed on the mean group posteriors, and we considered an effect significant if the 95% density interval excluded zero.

Linear models were calculated to see whether the pre-defined moderators (inhibitory control, cognitive functioning, empathy, compassion, and ToM) predicted the mean group posteriors of the drift rate (*v*-parameter), initial bias (*z*-parameter), and boundary separation (*a*-parameter), separately for YA and OA (function *lm*, from the “stats” package (Version 4.1.2; R Core Team, 2022), and *summaryh* function from the “hauselin/hausekeep” package in R (Version 0.0.0.9003; Lin, 2019).

Data availability. All data and scripts to conduct the reported analyses and reproduce the figures are available at: https://osf.io/zu4p3/?view_only=8b5cf6703f3b4d04a7122308c 31445f4.

Results

Generous versus selfish choices. YA chose generously (i.e., accepted an other-serving offer that would benefit the other at the cost of themselves) in 17% of the other-serving condition trials and chose selfishly (i.e., accepting a self-serving offer at the cost of the partner) in 33% of the self-serving condition trials of the trials. OA made generous decisions in 25% of the other-serving condition trials and opted for selfish choices in 38% of the self-serving condition trials. There were no significant age group differences in accepting self-serving versus other-serving offers (age group × condition interaction: $F(15261.09) = 0.02$, $p = 0.89$). However, we found age group differences such that OA generally favored offers (compared to the constant default distribution of 5€ per person), independent of if the offer was self-serving or other-serving (main effect age group: $F(108.97) = 4.91$, $p = 0.03$).

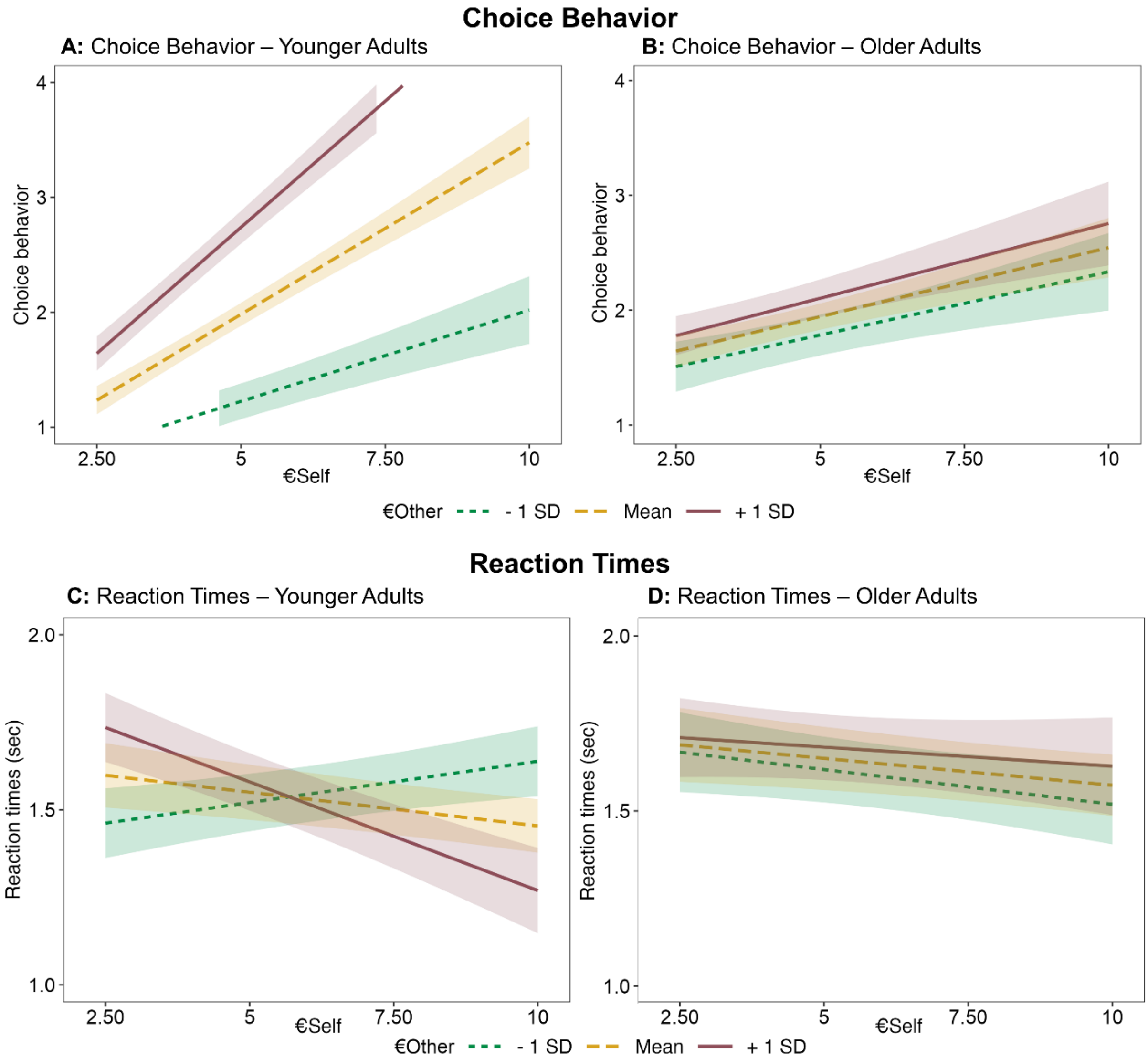
Age group differences in integrating values for oneself and other during social decision-making. ***Choice behavior as a value integration process.*** We were particularly interested in whether adult age impacted (pro)social choice behavior with respect to the degree to which outcomes for oneself (€self) and others (€other) guide behaviors. We calculated a mixed effect regression model with trial-by-trial choices as DVs, and the main effects and interactions of the following predictor variables: age group, payoffs to oneself (€self), and payoffs to the partner (€other). We observed the expected main effects of age group, €self, and €other (all *p*s < 0.002, see Table S1) (compare Hutcherson et al., 2015), as well as significant age × €self and age × €other interactions (all *p*s < 0.001, see Table S1) on a decision to accept an offer. Critically, we additionally found a significant 3-way interaction of age group × €self × €other ($F(109.02) = 30.80$, $p < 0.01$, see Table S1). The latter indicates age group differences with respect to the integration of €self and €other (see Figure 16A and 2B). As a post hoc test, the data was split into two models, separately for each age group (YA and OA), and including the main effects, as well as the 2-way interaction of €self × €other. We found significant main effects of €self and €other in both YA and OA (all *p*s < 0.02, see Table

S2). Interestingly, however, while YAs' choices were modulated by an interaction of €self × €other (F(61.99) = 62.71, p < 0.001), this was not observed in OAs' choice behavior (interaction term €self × €other : F(47.10) = 0.57, p = 0.46). As Figure 16A illustrates, in YA, the tendency to choose the proposed offer as a function of €self was modulated by €other: Indeed, low payoffs for the other person decreased the tendency to accept an offer with increasing €self in YA (simple slope €other – 1SD: β = 0.16, 95% interval = [0.11, 0.21]; simple slope €other + 1SD: β = 0.44, 95% interval = [0.39, 0.49], contrast €other -1SD vs. + 1SD: estimate = -0.28, p < 0.001, see Figure 16A). In OA, the probability of accepting the proposed offer increased with higher values of €self, irrespective of €other (simple slope €other – 1SD: β = 0.11, 95% interval = [0.06, 0.16]; simple slope €other + 1SD: β = 0.13, 95% interval = [0.07, 0.19], contrast €other -1SD vs. + 1SD: estimate = -0.02, p = 0.73, see Figure 16B). That is, in OA, whilst there was evidence for an additive effect of €self and €other (i.e., significant main effects of €self and €other, there was no significant evidence for an integration of both values (in the sense of a multiplicative effect).

Reaction times. Next, we examined age-related differences in the trade-off between self- and other-regard (i.e., interaction €self × €other) captured in RTs in YA versus OA. Again, we calculated a mixed effect model, consisting of trial-by-trial RTs as DV, and the same predictors of age group, value for self (€self), and value for other (€other), including their main effects and interactions. Strikingly, the RT results mirrored the pattern we observed for choice behavior: the 3-way interaction of age group × €self × €other was significant (F(108.94) = 18.30, p < 0.001, see Table S3 and Figure 16C and 2D). Again, YAs' RTs were modulated by a significant 2-way interaction of €self × €other (F(61.96) = 27.37, p < 0.001, see Table S4), while this was not apparent in OAs' RTs (F(46.95) = 0.44, p = 0.51, see Table S4). The significant 2-way interaction of €self × €other in YA illustrates that YA reacted quickly when values for €self and €other were either both high or both low, and YA reacted slowest when there was a conflict between €self and €other, i.e., when one of them was high, and the other was low, or vice versa (simple slope €other – 1SD: β = 0.02, 95% interval = [0.01, 0.04]; simple slope €other + 1SD: β = -0.06, 95% interval = [-0.08, -0.04], contrast €other -1SD vs. + 1SD: estimate = 0.09, p < 0.001, see Figure 16C). OA on the other hand reacted faster the higher €self was in a trial's offer, and this was not significantly influenced by €other (simple slope €other – 1SD: β = -0.02, 95% interval = [-0.04, -0.002]; simple slope €other + 1SD: β = -0.01, 95% interval = [-0.03, -0.01], contrast €other -1SD vs. + 1SD: estimate = -0.01, p = 0.79, see Figure 16D).

Figure 16

Adult age group differences in choice behavior and RTs with respect to the interaction of €self × €other



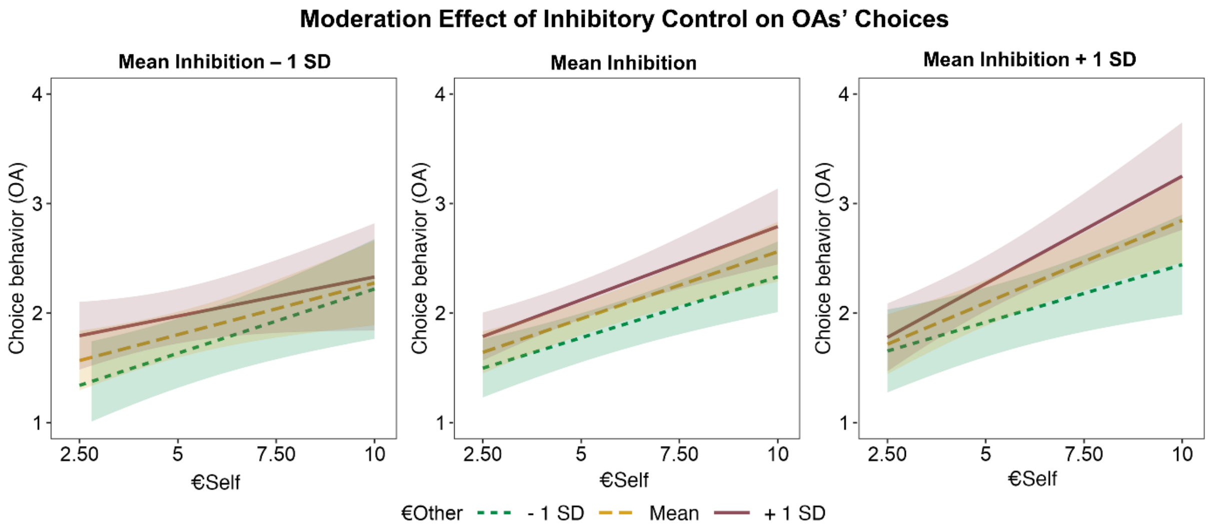
Note. We identified significant 3-way interactions of age group × €self × €other (both for choices and RTs). The shaded areas around the lines represent the 95% confidence intervals. Note that there was no condition where both values (i.e., €self and €other) fell below 5€ (A) YAs' choices were modulated by an interaction of €self × €other. Higher values increased the probability of choosing the proposed offer. (B) OAs' tendency to accept an offer as a function of own benefits was not significantly modified by payoffs for the other: The probability of choosing the proposed offer increased with higher values of €self (regardless of €other). (C) YAs' RTs were modulated by an interaction of €self × €other. YA showed faster RTs when both values (€self and €other) were either high or low, and slower RTs when values diverged (i.e., one was high and the other low). (D) OAs' RTs were only modulated by €self, irrespective of €other. Faster RTs were observed for higher values of €self (regardless of €other).

Moderation effect of inhibitory control on choice behavior. In the main manuscript, moderation effects in terms of significant 3-way interactions for choice behavior (i.e., moderator × €self × €other) are reported. Results of other moderator analyses are provided in the supplemental results (see pp.4-13). See Figure S4 and S5 for histograms of the distribution of the different moderator variables.

In YA, we did not observe significant main or interaction effects involving inhibitory control (all *p*s > 0.25, see Table S5). Interestingly, we found a significant moderation effect of inhibitory control on OAs' choices (i.e., significant 3-way interaction of inhibition × €self × €other, $F(42.04) = 6.27$, $p = 0.02$, see Figure 17 and Table S5). As Figure 17 suggests, the OAs with higher inhibitory control were better able to integrate payoffs for themselves and others. That is, in the OA subgroup with high inhibitory capacities (see Figure 17, right panel, mean inhibitory control + 1 SD), slopes for €self differed as a function of €other ($\beta = 0.01$, $SE = 0.01$, 95% confidence interval [0.002, 0.022]). In other words, as can be seen in Figure 17 (right panel), the OA subgroup with high inhibitory control abilities looked more similar to YA. OA with medium inhibitory control abilities (see Figure 17, middle panel, mean inhibitory control) showed a positive effect of €self ($\beta = 0.12$, $SE = 0.02$, 95% confidence interval [0.08, 0.17]) and €other ($\beta = 0.05$, $SE = 0.02$, 95% confidence interval [0.01, 0.09]), but no interaction of €self × €other ($\beta = 0.003$, $SE = 0.004$, 95% confidence interval [-0.004, 0.011]). Interestingly, in OA with low inhibitory capacities (see Figure 17, left panel, mean inhibitory control – 1SD), there was a positive effect of €self ($\beta = 0.10$, $SE = 0.03$, 95% confidence interval [0.03, 0.16]), but not €other ($\beta = 0.04$, $SE = 0.03$, 95% confidence interval [-0.02, 0.09]), and the positive effect of €self was not significantly modulated by €other ($\beta = -0.01$, $SE = 0.01$, 95% confidence interval [-0.02, 0.004]).

Figure 17

Significant moderation effect of inhibitory control on OAs' choices behavior (mean inhibitory control – 1SD vs. mean inhibitory control vs. mean inhibitory control + 1SD)



Note. Significant interaction of €self x €other x inhibition ability. Note that we split up the sample in low vs. medium vs. high inhibitory control for illustration purposes only. Inhibition ability entered the model as a continuous predictor. In OA with high inhibition abilities (right panel, mean inhibitory control + 1 SD), choices to accept the proposed offer were impacted by a multiplicative effect of both €self and €other. In OA with medium inhibition abilities (middle panel, mean inhibitory control), choices to accept the proposed offer were impacted by values for €self and €other, but not by the interaction of €self x €other. OA with low inhibition abilities (left panel, mean inhibitory control – 1SD) were only influenced by the values for self (€self). The shaded areas around the lines represent the 95% confidence intervals.

Hierarchical drift-diffusion model. We conducted hierarchical drift-diffusion modeling analyses to determine which components of the social decision-making process were differentially influenced by condition, and the trade-off between self-regard (€self) and other-regard (€other) in the two age groups.

We estimated the three main DDM parameters for every participant (*v*-, *z*-, and *a*-parameter) within the same model, but separately for YA and OA. Across 12 models per age group, the winning model with the lowest DIC value was the most complex model that was subsequently chosen to test our hypotheses (see Table S9). This model allowed the *a*-(boundary separation) and *z*-parameter (initial bias) to vary by condition (other-serving vs. self-serving vs. rational). The *v*-parameter (drift rate) was assumed to be modulated by payoffs for themselves (€self), for the partner (€other), and their interaction (€self × €other). We further modeled the non-decision time *ndt* as a fixed parameter across conditions (YA: M = 0.98, SD = 0.21; OA: M = 0.92, SD = 0.29). By comparing our observed data with 500 simulated datasets, we could illustrate that the winning model fits the data well, both in YA and OA (see Table S14-S15).

Here, we were interested in whether age group differences in the integration of values for €self and €other can be explained by age group differences in drift rate (*v*-parameter), initial bias (*z*-parameter), and/or the amount of relative evidence accumulated (*a*-parameter). As our research question focuses on prosocial decision behavior, the other-serving condition was included as the intercept of the regression model. This modeling approach enables conclusions regarding the initial bias or the amount of relative evidence towards accepting an other-serving offer, indicating a prosocial decision. As a result, the regression coefficients for the self-serving and rational condition can be interpreted relative to the other-serving condition.

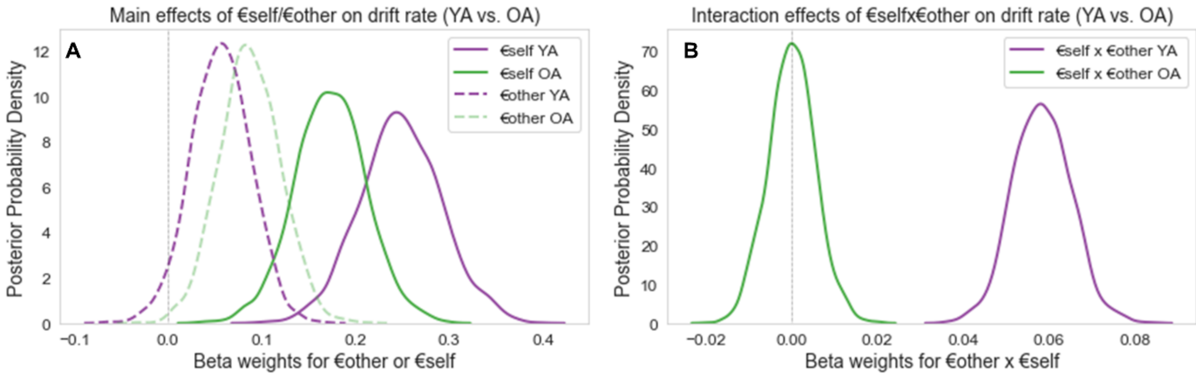
Drift rate (v). In YA, we observed meaningful main effects of €self and €other on drift rate (*v*-parameter). The drift rate increased in proportion to the degree of €self (probability($v_{YA\text{-}€self} > 0$) = 1.0) and €other (probability($v_{YA\text{-}€other} > 0$) = 0.96) (see Figure 18A). Matching main effects of €self and €other were also present in OA (probability($v_{OA\text{-}€self} > 0$) = 1.0; probability($v_{OA\text{-}€other} > 0$) = 1.0; see Figure 18A). Notably, comparisons of main effects between YA and OA did not reveal differences across age groups (probability($v_{YA\text{-}€self} > v_{OA\text{-}€self}$) = 0.91; probability($v_{YA\text{-}€other} > v_{OA\text{-}€other}$) = 0.75).

Next, we investigated the influence of the integration of values for €self and €other (i.e., the interaction term €self × €other) on drift rates in YA and OA. YAs' drift rates were positively impacted by the interaction of €self × €other (probability($v_{YA\text{-}€selfx€other} > 0$) = 1.0; see Figure 18B). Interestingly, no such interaction effect on OAs' drift rates was observed (probability($v_{OA\text{-}€selfx€other} > 0$) = 0.51, see Figure 18B). Direct comparisons across age groups found that the influence of the interaction of €self × €other on drift rates was meaningfully more pronounced in YA compared to OA (probability($v_{\text{YA-€selfx€other}} > v_{\text{OA-€selfx€other}}$) = 1.0; see Figure 18B).

Taken together, consistent with previous findings, values of €self and €other were linked to drift rates *v* (the speed of information accumulation) and, thus, the efficiency of the social decision-making process. This was true for both age groups, YA and OA. However, drift rates in YA – but not OA – were also associated with the interaction of both payoffs (€self × €other), indicating differences in the complex trade-off and integration of information of benefits for €self and €other across age groups.

Figure 18

Bayesian posterior densities of drift rates (v-parameter)



Note. Bayesian posterior densities of drift rates (*v*-parameter) estimated from hierarchical regression drift-diffusion models and how they varied as a function of payoff values €self and €other. For an effect to be meaningful, 95% of the distribution has to be on the left or right of zero (dotted vertical grey line). (A) Main effects of €self and €other on the drift rate, separately for YA (purple) and OA (green). All main effects indicated a positive impact of €self and €other on drift rates in YA and OA. (B) €self × €other interaction effect on the drift rates in YA and OA. Only YA (purple) demonstrated a positive interaction effect of €self × €other on drift rates.

Initial bias (z). Next, we tested whether the initial bias (*z*-parameter) differed as a function of condition (other-serving vs. self-serving vs. rational). Estimates of the initial bias (*z)* can range from 0 to 1, with values > 0.50 representing a bias towards the upper boundary (acceptance of the offer type) and values < 0.50 reflecting an initial bias towards the lower boundary (rejection of the offer type).

In YA, we observed different initial biases for the three different conditions (other-serving vs. self-serving vs. rational, see Figures 19A and 5B). There was no initial bias for the intercept, i.e., other-serving condition ($z_{\text{YA-other-serving}}$ = 0.49, probability($z_{\text{YA-other-serving}}$ < 0.50) = 0.77). However, compared to the other-serving condition, YA showed a meaningfully increased initial bias in the rational condition towards the acceptance of the offer (β-$z_{\text{YA-rational}}$ = 0.14, probability(β-$z_{\text{YA-rational}}$ > 0) = 1.0, $z_{\text{YA-rational}}$ = 0.63). Further, they showed a meaningfully decreased initial bias for the self-serving condition (i.e., towards the rejection of the offer type (β-$z_{\text{YA-self-serving}}$ = -0.03, probability(β-$z_{\text{YA-self-serving}}$ < 0) = 0.96, $z_{\text{YA-self-serving}}$ = 0.46), compared to the other-serving condition.

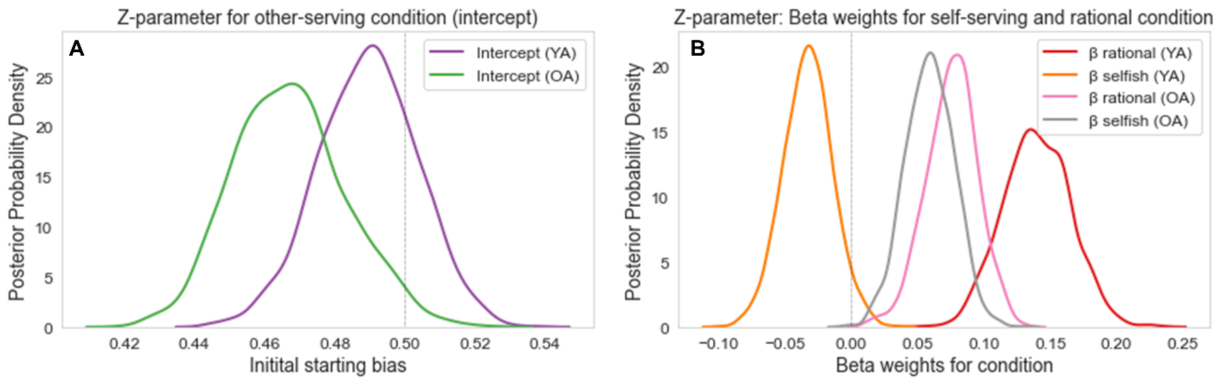
In OA, we also observed meaningful differences in initial biases depending on the condition (see Figures 19A and 5B). OA did show an initial bias towards the lower boundary, i.e., the rejection of the offer type for the other-serving condition (intercept; $z_{\text{OA-other-serving}}$ = 0.47, probability($z_{\text{OA-other-serving}}$ < 0.5) = 0.98). Further, they showed higher initial biases in both the rational (β-$z_{OA\text{-rational}}$ = 0.08, $z_{OA\text{-rational}}$ = 0.55, probability(β-$z_{OA\text{-rational}}$ > 0) = 1.0) and self-serving condition (β-$z_{OA\text{-self-serving}}$ = 0.06, $z_{OA\text{-self-serving}}$ = 0.53, probability(β-$z_{OA\text{-self-serving}}$ > 0) = 1.0), compared to the other-serving condition.

We did not observe age group differences between YA and OA with respect to their initial bias in the other-serving condition (i.e., the intercept; probability($z_{YA\text{-other-serving}}$ > $z_{OA\text{-other-serving}}$) = 0.86). However, the beta weights for the initial bias in the self-serving condition were significantly higher in OA compared to YA (probability(β-$z_{OA\text{-self-serving}}$ > β-$z_{YA\text{-self-serving}}$) = 1.0). Also, the beta weights for the initial bias in the rational condition varied meaningfully between age groups. That is, both age groups did show increased initial biases for the rational compared to the other-serving condition, but this effect was more pronounced in YA compared to OA (probability(β-$z_{YA\text{-rational}}$ > β-$z_{OA\text{-rational}}$) = 0.98).

This means that OA had a larger initial bias towards accepting self-serving offers than YA. YA, on the other hand, had a larger bias towards accepting rational offers than OA.

Figure 19

Posterior density plots with respect to the initial bias (z-parameter)



Note. Posterior density plots concerning the initial bias (*z*-parameter). (A) Initial bias in YA (purple) and OA (green) of the condition intercept (other-serving condition). The grey dotted vertical line (x = 0.50) indicates no choice bias. Values > 0.50 represent a bias towards the upper boundary (accepting the offer type). Values < 0.50 represent a bias towards the lower boundary (rejecting the offer type). (B) Beta weights correspond to the within-subject effects of the self-serving and rational condition on the z-parameter in YA (red and orange) and OA (pink and grey). Beta weights illustrate how the initial bias changes in self-serving and rational conditions with respect to the intercept (other-serving condition).

Boundary separation (a). Finally, we investigated whether the amount of relative evidence that participants required to cross a decision boundary (*a*-parameter, cf. Figure 15C) and, thus, reach a decision differed depending on the condition (other-serving vs. self-serving vs. rational). Due to convergence problems in the initial model fitting procedure, in the estimation process of the a-parameters in OA, the following results must be interpreted cautiously.

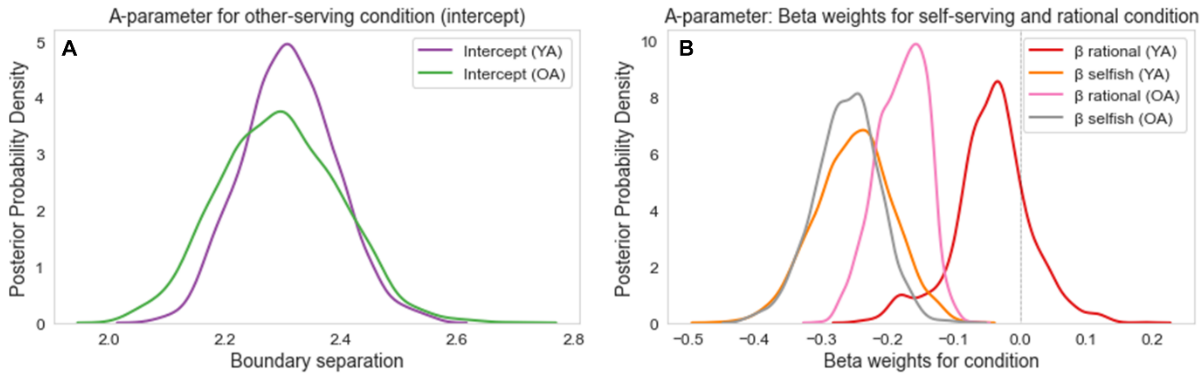
In YA, the boundary separation in the other-serving condition (intercept) was $a_{\text{YA-other-serving}} = 2.31$ (see Figure 20A). Compared to this intercept, the boundary separation decreased in the self-serving condition ($\beta\text{-}a_{\text{YA-self-serving}} = -0.25$, $a_{\text{YA-self-serving}} = 2.06$, probability($\beta\text{-}a_{\text{YA-self-serving}} < 0$) = 1.0, see Figure 20B). This indicates that less evidence had to be accumulated in the self-serving (compared to the other-serving) condition to reach a decision. No meaningful change in boundary separation was observed for the rational condition compared to the intercept, i.e., other-serving condition ($\beta\text{-}a_{\text{YA-rational}} = -0.04$, $a_{\text{YA-rational}} = 2.27$, probability($\beta\text{-}a_{\text{YA-rational}} < 0$) = 0.81, see Figure 20B).

OA showed a boundary separation of $a_{\text{OA-other-serving}} = 2.29$ (see Figures 20A) in the other-serving condition (intercept). However, the boundary separation decreased in both the self-serving ($\beta\text{-}a_{\text{OA-self-serving}} = -0.26$, $a_{\text{OA-self-serving}} = 2.03$, probability($\beta\text{-}a_{\text{OA-self-serving}} < 0$) = 1.0) and rational condition ($\beta\text{-}a_{\text{OA-rational}} = -0.18$, $a_{\text{OA-rational}} = 2.12$, probability($\beta\text{-}a_{\text{OA-rational}} < 0$) = 1.0), as compared to the other-serving condition (see Figures 20B). This suggests that OA had to accumulate less evidence to reach a decision in the self-serving and rational condition, as compared to the other-serving condition.

No age group difference concerning the other-serving condition in boundary separation was observed (probability($a_{\text{YA-other-serving}} > a_{\text{OA-other-serving}}$) = 0.55). Moreover, we did not find age differences in boundary separation when considering the beta weights for the self-serving condition in YA and OA (probability($\beta\text{-}a_{\text{YA-self-serving}} > \beta\text{-}a_{\text{OA-self-serving}}$) = 0.56). However, the beta-weights for the rational condition varied meaningfully between age groups (probability($\beta\text{-}a_{\text{YA-rational}} > \beta\text{-}a_{\text{OA-rational}}$) = 0.98). Thus, in the rational (relative to the other-serving) condition OA showed a larger decrease in boundary separation than YA.

Figure 20

Posterior density plots with respect to the boundary separation (a-parameter)



Note. Posterior density plots concerning the boundary separation (*a*-parameter). (A) Boundary separation in YA (purple) and OA (green) with respect to the intercept, i.e., other-serving condition. (B) Beta weights corresponding to the within-subject effects of the self-serving and rational condition on boundary separation in YA (red and orange) and OA (pink and grey). Beta weights illustrate how boundary separation changes in self-serving and rational conditions relative to the intercept (other-serving condition).

Linear models that were conducted to examine whether the pre-defined moderators (inhibitory control, cognitive functioning, empathy, compassion, and ToM) predicted the mean group posteriors of the *v*-, *z*-, and *a*-parameter are included in the supplementary results (pp. 19, Tables S10-S12). In brief, we found significant associations of the a-parameter and ToM-abilities in YA, and correlations of boundary separation with cognitive abilities (fluid and verbal) in OA (for more details see supplementary results section).

Discussion

Here, we adopted a value-based decision-making framework and applied hierarchical drift-diffusion modeling to investigate age group differences in decision-making for self and others. Other than previously suggested, OA did not display higher prosociality per se in our study. Harvesting our design, we however found differences in how OA and YA solve the cost-benefit trade-off of (pro)social decision-making. We found that YA, but not OA, integrated values for self and others in a multiplicative fashion: the tendency to accept an offer based on their own payoff was modulated by the others' payoff only for YA. In line with this, computational modeling showed that the integrated information of values for self and others (€self, €other) positively influenced the efficiency of YA's decision-making process. Contrary, in OA payoffs for self and others affected decision efficiency in an additive, but not an interacting, fashion. Interestingly, OA with better inhibitory control abilities demonstrated improved integration of the different values for €self and €other, showing a behavioral pattern more similar to YA. These findings provide novel mechanistic insights into *how* age might change the social decision process and unravel inhibitory control as an important moderator of age-related differences in prosocial decision-making.

Previous meta-analytic evidence suggests a small age-related increase in self-reported and behavioral prosociality across adulthood, but also substantial heterogeneity in findings (Pollerhoff et al., 2023). In the current study, we did not observe increased prosociality per se in OA versus YA. Even though the prevailing view in the literature is that older people are more prosocial than young people, the present results are in line with a recently published study using a similar behavioral paradigm (Falco et al., 2023), that observed less aversion towards advantageous inequity in OA compared to YA. Moreover, additional recent studies demonstrated a negative association between age and prosociality, i.e., decreased prosocial behavior in older age (Ehlert et al., 2021; Gong et al., 2019), painting a more mixed picture of prosociality in older age than has traditionally been suggested.

The design of our task allowed us to pinpoint age group differences in the integration of values for self and others. In detail, YA demonstrated a tendency to maximize their own profits, but this tendency was modulated by others' payoffs, i.e., was reduced with decreasing benefits for others. This was paralleled by an observation that YAs' RTs slowed down when payoffs for self and other were in conflict, this means when one's own gain was high, but the others was low. This suggests that, in line with previous work (Fehr & Schmidt, 1999), younger participants did not simply focus on maximizing their own monetary outcomes but also cared about the other's well-being. Interestingly, OAs' tendency to choose based on their own potential monetary gain was not significantly affected by the other's benefits.

We used drift-diffusion modeling to gain further insight into the underlying processes of these age group differences in social value integration. Mirroring choice data and RT results, modeling indicated that for YA, an interaction of values for self and others was associated with a more efficient decision-making process, characterized by a more rapid accumulation of evidence towards accepting an offer. Previously, a formal model of evidence accumulation in prosocial decision-making (Hutcherson et al., 2015), based on data in YA, tested an additive effect of monetary gain for the self and the other on the decision-making process. Our study extends this model by showing that YA show a multiplicative effect of values for oneself and another person. That is, the effect of potential own gains on behavior is not fixed but varies based on what another person could gain. For OA, decision-making efficiency was influenced by self or other monetary gain in an additive fashion, i.e., taking into account separate information, as originally suggested.

Moreover, even contrary to what we expected based on the literature, drift-diffusion modeling demonstrated that OA showed an initial bias towards rejecting other-serving offers (i.e., offers that benefitted partners at a cost to themselves). Moreover, their initial bias suggested a tendency to accept self-serving offers (i.e., offers that benefitted themselves at the partner's cost). Interestingly, YA showed a different pattern of initial bias than OA. They did not express a clear bias towards accepting or rejecting other-serving offers (different from OA) and instead displayed a bias towards rejecting self-serving offers.

Hence, the current results are inconsistent with some typical assumptions about prosociality in younger and older age groups. Firstly, they challenge the conception of younger individuals, particularly emerging adults, as inherently selfish (Arnett, 2007), as they did not exhibit a bias favoring self-serving offers. Secondly, they do not align with a view of older individuals as inherently prosocial (Hubbard et al., 2016), as they did neither show a bias towards other-serving offers in OA, nor did our choice data indicate more generous choice behavior in OA during the DG in general. Interestingly, there is evidence arguing that an initial bias reflects personal predispositions (F. Chen & Krajbich, 2018), but can also be distorted by social information (e.g., about the interaction partner; Reiter et al., 2021; Toelch et al., 2018), something we cannot disentangle with the current design.

In sum, raw choice data analyses, RTs, and computational modeling results show converging evidence that in the realm of social decision-making, the integration of information from various sources differs in OA compared to YA. Work outside of social contexts has previously reported age differences between YA and OA in certain tasks that involved combining information from different sources (e.g., perception, Bocheva et al., 2018; Costello & Bloesch, 2017) or making decisions based on cost-benefit analyses (Bagaïni et al., 2023; De Dieuleveult et al., 2017; Ruel et al., 2021). In these studies, older individuals were typically shown to be impaired in a capacity to effectively integrate all relevant information (Maddox et al., 2010; Mata & Nunes, 2010). It is concluded that in order to reduce the cognitive demands of a specific task (Gigerenzer, 2003; Sanfey & Hastie, 1999), older individuals adapted their decision-making behavior by analyzing and integrating less information (Mata et al., 2007, 2010; Shah & Oppenheimer, 2008) and showing less efficient decision-making when information needs to be integrated (Bocheva et al., 2018) or when more complex memory processes are required (Ratcliff et al., 2011; Theisen et al., 2021). According to the research-rational approach (Ruel et al., 2021), the employment of simpler and less demanding decision approaches (such as reduced information integration), present in children and older individuals, can be seen as a deliberative and effective decision-making strategy optimizing the use of available mental resources. Thus, the present results extend these previous findings to the realm of social decision-making by demonstrating relatively less consideration of integrated information in OA as compared to YA. More generally, our results argue in favor of conceptualizing prosocial behavior as a cost-benefit computation. For aging studies, they suggest experimental designs akin to ours in which value for self and other are parametrically manipulated and traded off against each other. Given that OA showed a general tendency to favor offers (both self-serving and other-serving offers), we speculate that other tasks (e.g., one-shot tasks) could yield misleading results, depending on response framing (e.g., if saying "yes" is associated with helping or giving in a study, compare Schneider, 2016).

The present study also showed that older individuals with better inhibitory control abilities demonstrated a greater capacity to effectively integrate information from various sources. Inhibitory control enables flexible decision-making in response to changing task demands (Barkley, 1997; Nigg, 2000) and studies consistently indicated a decline in inhibitory control abilities with older age (Braver et al., 2001; Campbell et al., 2020; Kane et al., 1994; Rey-Mermet & Gade, 2018). Most of the existing research linking inhibitory control and prosocial behavior mainly focused on the period of childhood (e.g., Blake et al., 2015; Hao, 2017; Liu et al., 2016; Yavuz et al., 2022), indicating that children with better inhibitory control tended to exhibit higher levels of prosocial behavior (e.g., Aguilar-Pardo et al., 2013; Paulus et al., 2015; Steinbeis et al., 2012). In adults, it is a lively debate whether prosociality is intuitive (Inaba et al., 2018; Ponti & Rodriguez-Lara, 2015; Zaki & Mitchell, 2013), or whether before acting prosocially, individuals must overcome prepotent, selfish impulses (DeWall et al., 2008; Steinbeis et al., 2012). Additional lifespan research is thus needed to gain more insights into the relationship between inhibitory control, prosocial decision-making, and its underlying mechanisms.

It is a limitation of our study that we compared only YA and OA, leaving out the socially important phase of midlife. This might limit our understanding of the adult development of a relationship between socio-emotional factors and prosocial behavior. There are reasons to hypothesize that social aspects, such as prosociality and empathy, undergo changes during midlife. The period of midlife may be a particularly prosocial and empathetic phase as suggested by theoretical approaches, research on personality traits, as well as typical developmental tasks including caregiving (Fingerman et al., 2011; Grundy & Henretta, 2006; Lachman, 2004; Pollerhoff et al., 2022, 2023; B. W. Roberts & DelVecchio, 2000; B. W. Roberts & Mroczek, 2008; Specht et al., 2011; Wojciechowska, 2017).

Conclusion. In summary, by adopting a value-based decision framework and utilizing drift diffusion modeling, we uncovered age-related differences in how older and younger individuals integrate values for self and others to reach a social decision. Collectively, this study provides valuable insights into the behavioral and computational mechanisms underlying age differences in social decision-making processes. It emphasizes social decision-making as a cost-benefit analyses which is solved differently by younger and older adults. In conclusion, these findings underscore the significance of exploring the underlying (computational) mechanisms of prosocial behavior and the broader social decision-making process to develop a comprehensive understanding of how this construct evolves throughout adulthood.

Authors' contributions

A.M.F.R., S.C.L. and P.K. developed the study concept, J.S., M.K., A.M.F.R., S.C.L., P.K. contributed to the study design. L.P., J.S., M.K. collected the data, L.P. conducted the data preparation and L.P. and A.S. analyzed the data. L.P., A.S. and A.M.F.R. interpreted the data and wrote the manuscript. M.K, J.S., X.R.P., G.H., A.T., P.K., S.C.L. provided critical revisions. All authors approved the final version of the manuscript.

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3 General discussion

3.1 Summary

The main aim of this dissertation was to examine potential age-related variations in prosociality across adulthood in three studies. Specifically, the objective was two-fold. First, the dissertation aimed to unravel potential age-related trends in prosociality throughout adulthood. To this end, in Study 1, we conducted a systematic and comprehensive meta-analysis, summarizing published and unpublished studies on adult age and prosociality, providing an up-to-date overview of the field. Additionally, in two empirical studies, we investigated adult age group differences in prosocial behavior using EMA in real life (Study 2) and a game-theoretical paradigm in a controlled, experimental laboratory setting (Study 3). Second, to address that the factors contributing to age-related differences in prosociality remain insufficiently understood (Mayr & Freund, 2020), we explored the influence of potential moderators on the association of age and prosociality. These include situational features, as well as cognitive and socio-emotional abilities. Additionally, given the lack of studies exploring the computational mechanisms underlying prosocial decision-making, we examined whether potential age differences in prosociality could be attributed to differences in the precise cognitive processes involved in the decision-making process.

Age differences in prosociality across the adult lifespan: A meta-analysis.

In Study 1, we conducted a thorough meta-analysis encompassing the entire adult lifespan to investigate how age is associated with prosociality. The pre-registered analysis incorporated various validated established measures, distinguishing between self-reported and behavioral prosociality, including published and unpublished literature, focusing on the understudied midlife population. The analysis encompassed data from 120 independent samples, involving 103,829 participants. We examined age effects in prosociality through five partially independent meta-analyses, considering both linear and quadratic age effects, as well as comparisons of younger adults, middle-aged adults, and older adults. The results indicated a modest yet statistically significant increase in prosociality with advancing age, both in self-reported and behavioral prosociality. Age group comparisons further supported this trend, revealing higher levels of behavioral and self-reported prosociality in older compared to younger individuals. Interestingly, an explorative meta-analysis unveiled a quadratic age effect on behavioral prosociality, suggesting an inverted U-shaped pattern. This pattern indicated that the highest levels of prosocial behavior were observed in midlife. Age group comparisons partially supported this, showing greater behavioral prosociality in middle-aged compared to younger adults but no significant difference between middle-aged and older adults. Notably, the study identified significant moderating effects of situational and contextual factors. Specifically, linear age effects were more prominent in studies incorporating monetary incentives based on participants' or their partners' behavior, and no significant age effects were observed in lab-in-the-field studies (i.e., the application of a standardized and validated task from laboratory settings within real-world contexts). Additionally, age group differences in behavioral prosociality were more pronounced in studies using one-shot interactions compared to repeated interactions. In summary, the study sheds new light on how prosociality might change with age, suggesting that heightened prosociality may not be limited to later stages of life but may be particularly pronounced during midlife. Moreover, the results underscore the importance of considering contextual and situational factors when evaluating the influence of prosociality across the adult lifespan.

Investigating adult age differences in real-life empathy, prosociality, and well-being using experience sampling

Study 2 investigated behavioral prosociality and empathy across different adult age groups in everyday life. By analyzing publicly available experience sampling data (as published by Depow et al., 2021) involving participants aged 18 to 55 years and older (N = 243, categorized into four different age groups), we found that daily prosocial behavior showed no significant differences across age groups. However, the experience of empathy (i.e., the actual emotional experience of some form of empathy, corresponding to affect sharing, compassion, and perspective taking) exhibited both linear and quadratic age effects. This experience of empathy increased from 18 to 45 years, stabilizing or slightly decreasing in older age groups. We did not observe significant interactions between age and the experience of empathy or its subcomponents (emotion sharing, perspective taking, and compassion) in predicting daily prosocial behavior. Nonetheless, there was a significant effect for the link between perceived empathy opportunities (i.e., the opportunity to empathize with another person) and age on daily prosocial behavior. This indicated both linear and quadratic effects on daily prosocial behavior, with all age groups displaying higher levels of prosocial behavior in situations potentially eliciting empathy, but most prominently in midlife. Overall, these findings suggest that most aspects examined in this study, particularly daily prosocial behavior, did not exhibit significant differences across adult age groups in real life. However, intriguingly, an inverted U-shaped pattern emerged in how age influenced the experience of empathy and how the interaction between age and empathy opportunities influenced prosocial behavior in everyday life. These results challenge findings from classical laboratory and survey studies regarding age-related increase of prosociality by indicating potential non-linear social development across adulthood, with midlife being a significant phase.

Adult age differences in the integration of values for self and other

Study 3 investigated age-related differences in prosocial decision-making in a controlled laboratory setting. The study involved both younger (*N* = 63, aged between 18 and 30 years) and older adults (*N* = 48, aged between 65 and 78 years) and utilized a modified dictator game (based on Hutcherson et al., 2015). In this task, participants decided between varying monetary amounts between themselves (self) and a designated game partner (other). By applying a value-based decision framework and Bayesian drift-diffusion modeling, we examined how age groups differed in their prosocial decision-making, as well as the underlying computational processes and potential moderators (cognitive and socio-emotional abilities). The results revealed no significant age group differences in prosociality based on choice behavior. However, younger adults, but not older adults, integrated values for self and other in their decision-making process, influencing their choices and reaction times. This integration of information positively impacted decision-making efficiency in younger individuals. Conversely, older adults considered values for self and other separately in their decisions, with these values having an additive effect on decision efficiency. Interestingly, older adults with better inhibitory control exhibited a pattern more akin to younger adults, demonstrating improved integration of the values for self and other. No significant moderation effects were observed for intelligence or socio-emotional abilities in both younger and older adults regarding prosocial-decision making (i.e., the integration of values for self and other). These findings highlight the importance of exploring the underlying computational processes in prosocial decision-making to gain insights into precise cognitive mechanisms. Furthermore, cognitive control processes, especially inhibitory control, might be a significant factor in understanding how age-related variations influence prosocial decision-making throughout adulthood.

3.2 Implications

Unveiling age patterns: Linear vs. quadratic effects of age on prosociality

In the past decade, there has been extensive research focusing on the development of prosociality throughout adulthood (e.g., Bailey et al., 2013, 2021; Beadle et al., 2015; Sparrow et al., 2021; Sze et al., 2012). Yet, individual studies have yielded conflicting results, casting uncertainty on the consistency of age-related effects on prosociality. In light of this, the first aim of this dissertation was to disentangle these findings.

Linear age effects. The findings of Study 1 revealed a small linear association between age and prosociality, both self-reported and behaviorally. These results are consistent with various lifespan developmental theories of motivation and preferences (e.g., Carstensen et al., 1999; Liu et al., 2022; Mayr & Freund, 2020; Van Lange et al., 1997), as well as recent meta-analyses in the field (Bagaïni et al., 2023; Sparrow et al., 2021). Interestingly, this age-related increase in behavioral prosociality was not observed in the two empirical studies, neither in a real-life assessment of prosocial behavior across adulthood (Study 2) nor in a comparison of younger and older individuals using a validated behavioral prosocial measure in a controlled laboratory setting (Study 3).

In summary, the findings suggest either non-significant or small effects of adult age on prosociality. The small linear age effect observed in Study 1 (r = 0.04 and r = 0.09) is comparable with previous meta-analytic research on prosociality (Bagaïni et al., 2023; Sparrow et al., 2021), which, per definition (e.g., Schober et al., 2018), also reported weak correlations (r = 0.11 and r = 0.24, respectively). It is worth noting that such small effect sizes are common in the field of psychology, both in individual studies and meta-analyses (e.g., Cutler et al., 2021; Defoe et al., 2015; Hui et al., 2020; Vanags et al., 2023). Additionally, it is important to acknowledge that the interpretation of correlation coefficients (i.e., “weak”, “moderate”, “strong”) is subject to debate, with recognized arbitrariness and inconsistency in defining these cut-offs, emphasizing the need for careful consideration (Schober et al., 2018). Furthermore, significant levels of heterogeneity were observed in Study 1 and existing meta-analyses on prosociality and adult age (Bagaïni et al., 2023 and Sparrow et al., 2021), which may influence the observed effects. In line with the value-based decision framework (Mayr & Freund, 2020), identifying factors associated with adult age differences in prosociality appears crucial for a comprehensive understanding of these effects.

The inconsistencies between the outcomes of Study 1 and Study 2 may be attributed to the absence of real-life studies utilizing EMA in the meta-analysis (Study 1). Therefore, conclusions regarding age-related effects on real-life prosociality cannot be drawn based on the meta-analysis. While some argue for the comparability of EMA and self-reported measures, it is more appropriate to differentiate between them due to the capacity of EMA to capture a broader spectrum of prosocial acts. Specifically, EMA measurements can encompass smaller gestures, like holding a door open, which participants might have forgotten or omitted in retrospective self-reports. Such memory effects or recall biases tend to be more prominent in retrospective self-reports (Shiffman et al., 2008), and it is proposed that due to a salience memory heuristic, potentially less noticeable events are overlooked, whereas prominent instances are overemphasized (Stone et al, 2005). This so-called *memory-experience gap*, where the overall assessment of emotional experiences tends to be more intense than the average of individual emotions (Miron-Shatz et al., 2009), is suggested to be increased in older individuals (Neubauer et al., 2020). Thus, relying on retrospective self-reports may hinder an accurate understanding of individual real-life behaviors, potentially missing the specific dynamics of everyday life (see Shiffman et al., 2008). The issue becomes particularly critical when considering developmental changes across the adult lifespan, as it could lead to skewed findings. Another noteworthy difference between Study 1 and Study 2 is the broader age range in the former. In Study 1, linear age effects on behavioral and self-reported prosociality were investigated across a wide span of ages, covering the entire adult lifespan (18-100 years and 17-91 years, respectively). Conversely, in Study 2, participants' ages were recorded in categories rather than exact ages. There were very few participants who fell into the category of 65 years and older, which is why this group was combined with the 55-64 years category. As a result, Study 2 had a narrower range of ages and consequently included a relatively young old sample compared to Study 1. According to different lifespan theories proposing a linear increase of prosociality across adulthood as a consequence of normative development (e.g., Brandtstädter et al., 2010; Carstensen et al., 1999; Midlarsky et al., 2015; Van Lange et al., 1997), this age range might not be sensitive enough to detect an age-related increase of prosociality. These theories posit that with higher age, individuals become intrinsically motivated to engage in prosocial behavior due to a shift in emotional experience and the perception of remaining lifetime as limited. Thus, with a narrower age range, as in Study 2, it is plausible that individuals still prioritize more individualistic goals. However, in Study 2, we did not specifically examine key attributes behind these different lifespan developmental theories. This again underscores the importance of accurately operationalizing these concepts and testing their distinct impact on the age-prosociality relationship (Mayr & Freund, 2020).

The differences in the outcomes between Study 1 and Study 3 may seem unexpected, especially since the meta-analysis model in Study 1, which compared younger and older adults in their levels of prosocial behavior, indicated the highest effect size. However, Study 3 introduced a more complex perspective by combining the value-based decision framework with drift-diffusion modeling. In Study 3, there was no evidence for increased prosocial behavior in older compared to younger adults. Additionally, older adults did not display an initial bias towards other-serving offers. These findings challenge established lifespan developmental theories (e.g., Carstensen et al., 1999; Liu et al., 2022; Mayr & Freund, 2020; Van Lange et al., 1997), but also the intuitive-prosociality model (Zaki & Mitchell, 2013) and the picture of older adults being purely altruistic (Hubbard et al., 2016). However, it is worth noting that the intuitive-prosociality model (Zaki & Mitchell, 2013) addresses explicitly scenarios where self- and other-serving options are directly compared. Something which was, for example, not possible in Study 2, as data only indicated whether a person acted prosocially or not without providing insight into the specific criteria or reasoning behind their actions.

Similarly, this applies to the meta-analyses in Study 1. The design of the modified dictator game in Study 3 allowed for a direct comparison of younger and older individuals in their ability to process value integration by presenting them with self- and other-serving choice options. Interestingly, Study 3 highlighted that combining information to make a decision posed a specific challenge to older individuals, impacting their decision efficiency (especially in those with reduced inhibitory control). These findings align with previous research in the field of decision-making, which has shown a tendency towards reduced information integration processes (e.g., Maddox et al., 2010; Mata & Nunes, 2010) and increased choice inconsistency in older adults (Bielak et al., 2014; Bunce et al., 2004; Hultsch et al., 2002). Taken together, the findings from Study 3 raise several questions: Are older adults really inherently prosocial? Do they really exhibit increased prosociality compared to younger adults, as proposed by several theoretical frameworks (e.g., Brandtstädter et al., 2010; Carstensen et al., 1999; Mayr & Freund, 2020; Van Lange et al., 1997)? Alternatively, could these observed age-related differences reflect choice inconsistency? Addressing these questions requires further research, specifically by focusing on the precise underlying (computational) mechanisms of the prosocial decision-making process, particularly investigating cost-benefit trade-offs and value-integration processes across the adult lifespan.

Quadratic age effects. In addition to the small linear impact of age on prosociality, Study 1 revealed a quadratic effect of adult age on behavioral prosociality in an explorative meta-analysis. This inverted U-shaped pattern indicates that the highest level of behavioral prosociality occurs during midlife rather than in old age. To some extent, Study 2 supported these findings by demonstrating a significant quadratic effect of age on the experience of empathy in daily life and a significant quadratic effect of age and daily empathy opportunities on daily prosocial behavior. Again, midlife emerged as the period where these effects were most prominent. Conversely, Study 2 did not identify a significant quadratic age effect on real-life prosocial behavior. In Study 3, examining quadratic age effects on prosocial behavior was not feasible, as the primary focus was to distinguish between younger and older cohorts. In summary, these results offer a new perspective on the developmental trajectories of prosociality and related social constructs like empathy. They suggest that age-related effects may not strictly adhere to a linear pattern and that midlife may represent a pivotal phase in social development across adulthood.

Earlier theoretical approaches, meta-analyses, and empirical studies (e.g., Bagaïni et al., 2023; Bailey et al., 2020; Beadle et al., 2015; Carstensen et al., 1999; Kettner & Waichman, 2016; Rosi et al., 2019; Sparrow et al., 2019, 2021; Sparrow & Spaniol, 2018) that propose a linear development of prosociality across adulthood often focus predominantly on comparisons between younger and older adults. Thus, these approaches frequently lack a comprehensive lifespan perspective due to the exclusion of middle-aged adults in their analyses, potentially hindering a thorough exploration of the phenomenon at hand. The omission of the midlife period highlights a common gap in prior research, not only in prosocial development but also in related fields such as the emotional domain (Lachman et al., 2015). Recently, there has been growing recognition of the need for more comprehensive investigations that include middle-aged adults to gain a deeper understanding of the relationship between adult age and prosociality (Bailey et al., 2021). However, research exploring non-linear age effects on prosociality remains limited. Notably, a few studies have supported the idea of peak prosociality in midlife (Best & Freund, 2021; Henning et al., 2023; Rieger & Mata, 2015; Shane et al., 2021). Additionally, studies focusing on the adult development of other closely related social constructs, such as empathy and ToM, have also found similar results, indicating evidence for an inverted U-shaped pattern across the adult lifespan (e.g., Brunsdon et al., 2019; O'Brien et al., 2013; Wacker et al., 2017).

Shane and colleagues (2021) recently introduced a novel framework that combines the motivational theory of lifespan development (Heckhausen et al., 2010, 2019) with the expectancy-value theory (Atkinson, 1957; Beckmann & Heckhausen, 2018; Eccles & Wigfield, 2002; Wigfield et al., 2015) to provide insights into the motivational mechanisms driving prosocial development across adulthood. According to this framework, the expectancy of realizing prosocial intentions, encompassing perceived control and control strivings, together with the value assigned to a specific behavior (e.g., concern for others), drives prosociality. The study demonstrates that midlife, a period characterized by heightened domain-general control capacities and expectancies (Heckhausen, 2001; Heckhausen et al., 2010, 2019), represents a peak period for prosociality but also generativity, a concept closely related to prosociality. Notably, individual differences in perceived control had the strongest influence on prosociality in middle adulthood, aligning with the motivational theory of lifespan development (Heckhausen et al., 2010, 2019) and emphasizing the significance of variations in this peak period for individuals' prosociality. The authors also highlight the role of agreeableness, which can increase the value one assigns to acting prosocially, thus promoting prosociality across the adult lifespan (Shane et al., 2021). This trait, which has been linked to prosociality in prior research (Thielmann et al., 2020), is shown to be highest and most stable in midlife (B. W. Roberts & Mroczek, 2008; Specht et al., 2011). Additionally, it can be assumed that other traits, such as honesty-humility, which involves treating others fairly and sincerely (Ashton & Lee, 2007) and is strongly associated with prosocial behavior (Thielmann et al., 2020), may also contribute to the value assigned to acting prosocially. Notably, honesty-humility also peaks in midlife (Milojev & Sibley, 2014). Furthermore, considering normative personality development throughout adulthood, which provides individuals with the necessary skills and attributes for age-specific developmental tasks and roles (Hutteman et al., 2014; Staudinger & Kunzmann, 2005), it is evident that in midlife, adults heavily depend on their social skills and emotional regulation to effectively manage various roles (Infurna et al., 2020). Some of these roles involve prosocial actions like caregiving and helping (Fingerman et al., 2011; Grundy & Henretta, 2006). This underscores that prosocial trajectories are highly influenced by individual differences and may vary across different life domains, each with its distinct developmental course (Shane et al., 2021).

Factors influencing the age-prosociality relationship across adulthood

The current dissertation has identified linear and quadratic age patterns in prosocial development. While further research is needed to disentangle the specific age trajectories, we have shown that additional factors may influence the age-prosociality relationship. One set of factors based on different lifespan developmental assumptions (e.g., Bekkers & Wiepking, 2011; Carstensen et al., 1999; Erikson, 1950; Midlarsky et al., 2015; Van Lange et al., 1997; Wiepking & Bekkers, 2012), pertains to motivation and resource-related variables (see also Mayr & Freund, 2020). These frameworks posit that older age is linked to an emotional shift, increasing intrinsic motivation for prosocial actions. However, another perspective suggests that individual differences in prosociality arise from specific situational factors influencing affordances within interdependent contexts (Thielmann et al., 2020). In alignment with this, other researchers emphasize that situational and contextual factors may be crucial in understanding age-related differences in prosociality (Bailey et al., 2021; Penner et al., 2005). Therefore, the second aim of this dissertation was to examine potential factors, specifically motivational and resource-related variables (such as empathy, compassion, ToM, intelligence, inhibition) and situational and contextual factors (such as study environment, measurement approach, study design characteristics), that might moderate the link between adult age and prosociality.

Socio-emotional abilities. The role of empathy in influencing both prosociality and the association of prosociality with age has been the subject of investigations in laboratory settings (e.g., Beadle et al., 2015; Cavallini et al., 2021; Sze et al., 2012) and real-life scenarios using EMA (Depow et al., 2021). In Studies 2 and 3, we hypothesized that socio-emotional abilities (namely, empathy, compassion, and ToM) moderate age-related differences in prosocial behavior.

In Study 2, we found that daily experience of empathy did not significantly moderate the link between adult age and real-life prosocial behavior. However, the interaction between daily empathy opportunities and adult age significantly affected prosocial behavior in everyday life. In Study 3, no significant moderating effect of empathy, compassion, or ToM was found concerning the integration of values for self and others in both younger and older adults. However, evidence did indicate a significant effect of individual differences in ToM and compassion on how strongly values for oneself influenced decision-making, but only in the context of the reaction times of younger adults. Specifically, younger adults with high levels of compassion or ToM exhibited a significantly reduced impact of self-values on their reaction times. Standing in contrast to the theoretical expectations that contributing to the well-being of others in older age fosters a sense of meaning and social connection (Mayr & Freund, 2020), the studies in this dissertation did not show a significant moderating effect of socio-emotional abilities on real-life prosocial behavior or prosocial decision-making. These unexpected findings challenge the assumption that the experience of empathy, compassion, or ToM plays a contributing role in the age-prosociality relationship. Lifespan theories of motivation propose that older individuals are inherently motivated by emotionally meaningful experiences, such as spending quality time with loved ones. Thus, perceiving oneself as compassionate or empathetic would be considered particularly important in achieving these emotionally driven goals, such as acting prosocially (Beadle & de la Vega, 2019). In the following, potential explanations for the findings of Study 2 and Study 3 will be outlined.

Notably, Study 2 and Study 3 used different empathy definitions and operationalizations. Study 2 defined empathy as a multi-faceted construct (Blanke & Riediger, 2019; Davis, 1983; Perry & Shamay-Tsoory, 2013; Zaki, 2017) encompassing emotion sharing, compassion, and perspective taking and adopted an unvalidated self-reported measure with the help of EMA in real-life. Study 3 differentiated a socio-cognitive route involving ToM from a socio-affective route involving affect sharing and compassion (Bloom, 2017; Jordan et al., 2016; Kanske et al., 2015; Singer & Klimecki, 2014) and used a standardized and validated behavioral paradigm in a controlled laboratory setting. This variability in defining and thus measuring empathy has faced criticism in recent years (Hall & Schwartz, 2019), resembling a discourse about what is known as *jingle-jangle fallacies* (Kelley, 1927; Marsh, 1994).

Considering the results of Study 2, the presence of a situational opportunity to empathize with another person appeared to be enough to elicit prosocial behavior across all age groups. However, this was not the case in Study 3, where the introduction of the experiment already emphasized the social aspects of the task with the following (translated from the German version): "In this experiment, we aim to investigate how individuals succeed in empathizing with others and understanding their thoughts and feelings. For this purpose, we will show you a selection of short videos. In these videos, individuals recount from their lives. These videos have been chosen to include particularly emotionally impactful events, as well as everyday occurrences". This setup immediately made participants aware of the social context, eliminating their need to deduce it themselves.

Additionally, Study 3's behavioral paradigm, the EmpaToM (Kanske et al., 2015), primarily focused on negative and neutral states without including positive emotions. However, previous research indicates that individuals in a positive mood tend to engage in behaviors likely to reinforce that mood. Therefore, individuals in a positive emotional state may be more inclined to exhibit certain behaviors, expecting these actions to contribute to sustaining their positive mood. Among such behaviors, acting prosocially stands out as it can potentially improve one's mood (see Telle & Pfister, 2016 for a review). In line with this, Study 2, which encompassed a range of emotional situations, including negative, neutral, and positive ones, demonstrated that participants reported a heightened experience of empathy following a positive situation, regardless of their age group. However, it is worth noting that, contrary to the assumption that positive emotional stimuli are crucial when studying the relationship between empathy and prosocial behavior, a recent study by Lehmann and colleagues (2022) did find a significant association between the experience of empathy (measured with the EmpaToM, Kanske et al., 2015) and helping behavior. It is important to mention that this study operationalized prosocial behavior through the hypothetical question, "Would you be willing to help this person?" (related to the person in the narrative video). This definition does not consider associated costs or benefits for participants, potentially not fully capturing prosocial behavior. Therefore, the specific study design, and notably, the definition and operationalization of prosociality and empathy appear to be critical factors when examining their moderating role. This once again highlights the issue of jingle-jangle fallacies (Kelley, 1927; Marsh, 1994).

Cognitive abilities. Different cognitive abilities tend to decline as individuals age, including processing speed, intelligence, and executive functions (Diamond, 2013; S.-C. Li et al., 2004; Lindenberger et al., 1993). Since functions such as working memory, processing speed, and inhibitory control appear to be at least partially linked to various decision-making processes throughout adulthood (e.g., Biella et al., 2020; Henninger et al., 2010; Y. Li et al., 2013; Ramchandran et al., 2020; Toplak et al., 2010), a decline of these abilities with age could potentially influence the process of prosocial decision-making. Previous research has primarily focused on the link between inhibitory control and prosocial behavior in childhood (e.g., Aguilar-Pardo et al., 2013; Paulus et al., 2015; Steinbeis et al., 2012). This research indicates that children with stronger inhibitory control tend to exhibit higher levels of prosocial behavior. However, in the case of adults, an ongoing debate persists regarding underlying preferences. While some argue that prosociality is intuitive (Inaba et al., 2018; Ponti & Rodriguez-Lara, 2015; Zaki & Mitchell, 2013), others posit that it requires overcoming selfish impulses (DeWall et al., 2008; Steinbeis et al., 2012).

We were able to test this assumption in Study 3. The results of Study 3 did not show any significant moderating effect of intelligence, neither fluid nor verbal. However, results indicated that older adults with enhanced inhibitory control exhibit a higher capacity for effectively integrating information from diverse sources (i.e., monetary values offered for the self and other), enabling them to make decisions that consider all information, comparable to the decision behavior of younger adults. Conversely, older adults with weaker inhibitory control abilities tended to make decisions by considering the values for self and other separately (in an additive fashion) or by only considering potential personal gains (i.e., only the monetary value offered for the self). While this could suggest that increased inhibitory control may lead to the suppression of selfish inclinations (i.e., acting selfish as default mode), it challenges the intuitive-prosociality model (i.e., acting prosocial as default mode; Zaki & Mitchell, 2013), predicting that cognitive decline weakens deliberative processes (i.e., reduced inhibitory control) and increases prosociality in older individuals without any underlying motivational shifts. Given the observed significant influence of inhibitory control on value-integration processes in Study 3, neither dual-system perspective adequately accounts for the observed behavior. First, older adults with weak inhibitory control did not show higher levels of prosocial behavior. Second, inhibitory control did impact value-integration processes, and, thus the decision-making process, but these decisions were not necessarily more prosocial or selfish. An alternative explanation from the value-based decision framework (Mayr & Freund, 2020) proposes that the impact of cognitive control, (including inhibitory control), is mediated through the positive and negative outcomes associated with available choices, which subsequently shape cost-benefit trade-offs. In this context, the improved inhibitory control in older individuals may not directly lead to more generous behavior. Instead, it enables additional deliberation, resulting in more valuable decisions facilitated by better value-integration processes, allowing them to navigate cost-benefit trade-offs better.

Moreover, the results parallel insights from decision-making research outside the social context, where older individuals often displayed reduced cognitive abilities for integrating pertinent information (Maddox et al., 2010; Mata & Nunes, 2010). Consequently, to mitigate cognitive demands (Gigerenzer, 2003; Sanfey & Hastie, 1999), they adapt their decision-making strategy by incorporating and analyzing less information (Mata et al., 2007, 2010; Shah & Oppenheimer, 2008). This simpler, resource-efficient decision-making approach is viewed as a deliberative strategy to optimize mental resources (Ruel et al., 2021), but as the findings from Study 3 suggest, may not necessarily lead to more prosocial choices.

Situational and contextual features. Given evidence indicating that age is associated with prosociality at an individual level, this influence might also be modulated by situational and contextual factors (Bailey et al., 2021). In Study 1, a particular focus was placed on how these factors influence the link between adult age and prosociality. The most important moderators observed were, i) the form of (monetary) incentives applied (i.e., payoff contingent to behavior in the task vs. payoff independent of behavior in the task), ii) the form of interaction applied (i.e., one-shot interaction, one interaction in one trial, vs. repeated interactions across various trials), and iii) the setting in which the study was conducted (i.e., online vs. laboratory vs. lab-in-the-field).

The type of incentive used in the studies significantly affected the relationship between age and prosocial behavior. When studies linked monetary rewards to participants' task performance, a stronger age-related effect on prosocial behavior was observed. Scenarios with hypothetical rewards unrelated to task performance showed weaker age effects. One possible explanation is that age-related effects become stronger when prosocial behavior includes higher costs. This aligns with theories suggesting that increased prosocial behavior in older age may be linked to the accumulation of external resources (Bekkers & Wiepking, 2011; Wiepking & Bekkers, 2012), such as wealth (e.g., Cheung & Lucas, 2015; Huggett, 1996), which enables older adults to afford acting prosocially. Contrary to this, several studies have indicated that increased wealth does not necessarily lead to higher levels of giving (e.g., Bekkers et al., 2022; Cutler et al., 2021; Piff et al., 2010; Piff & Robinson, 2017). In line with this, Study 3, which introduced costs regarding potential monetary gains and losses, did not find that older adults primarily base their decisions on the potential monetary benefit for their partner (i.e., monetary values offered for others). Instead, we observed the opposite behavior among older adults with low cognitive control abilities, who primarily based their decisions on their own potential gain (i.e., monetary values offered for the self). In contrast, older adults with better inhibitory control abilities integrated information from both sources to make a decision. In another study employing a comparable design and paradigm, it was also observed that older individuals exhibited a higher tendency to prioritize choice options that maximized their own potential gains and were less aversive towards advantageous inequity compared to younger individuals (Falco et al., 2023). These findings suggest that the interplay of associated costs and benefits influences age-related effects on prosociality. However, financial resources alone might not fully explain age-related differences in prosociality. Instead, these resources should be seen as just one of the potential factors contributing to the cost-benefit calculation in the decision to behave prosocially (see also Mayr & Freund, 2020).

Another notable moderation effect was observed based on the type of interaction employed within the specific game-theoretical paradigm. Stronger distinctions between age groups became apparent in one-shot interactions compared to repeated interactions. This indicated that, particularly in one-shot settings, younger age groups were less inclined towards prosocial behavior than their older counterparts. Repeated interactions, unlike one-shot scenarios, provide room for reciprocity and were not associated with significant age differences. This finding aligns with the idea that various situations offer different opportunities for expressing one's motives, goals, values, and preferences. Consequently, depending on the situational context, distinct personality traits or latent tendencies may become activated to guide prosocial behavior (Thielmann et al., 2020). Prosocial behavior featuring repeated interaction can be seen as an expression of conditional concern for others' well-being (e.g., Perugini et al., 2003; Thielmann et al., 2020), and reciprocity can be viewed as an internalized social norm present in most human societies (Gouldner, 1960). Hence, the age group differences in one-shot interactions might suggest heightened intrinsic and unconditional prosocial tendencies in middle-aged and older adults. This would underpin the assumption that increased prosociality can be considered a normative aspect of the aging process, which is characterized by a shift in emotional experiences leading to increased intrinsic motivation to promote the well-being of others or society (e.g., Brandtstädter et al., 2010; Carstensen et al., 1999; Midlarsky et al., 2015; Midlarsky & Kahana, 1994), irrespective of reciprocity. Younger individuals, on the other hand, tend to prioritize individualistic, future-orientated, and instrumental goals, which involve seeking factual information and personal gains, social comparisons, and pursuing identity development (Brandtstädter & Greve, 1994; Brandtstädter & Renner, 1992; Brandtstädter & Rothermund, 2002; Carstensen et al., 1999). This, in turn, may indicate that situational factors and the (social) context, such as external rewards and reciprocity, potentially have a stronger influence on younger individuals and their levels of prosociality in interdependent situations. However, based on the current findings we cannot make final conclusions about whether different situational affordances further explain age differences in prosociality across the adult lifespan, as we did not find a significant moderation effect of the different measures on the association between age and prosocial behavior. To answer the question of whether different situational affordances across interdependent situations affect prosocial development across adulthood, the application of the affordance-based framework (Thielmann et al., 2020) seems promising.

Furthermore, we observed that setting significantly moderated age group differences in prosocial behavior. Intriguingly, when exclusively considering lab-in-the-field studies, which tend to provide a more naturalistic and less artificial context than laboratory or online studies, no age differences were evident. Interestingly, this moderation effect also aligns with the results from our two empirical studies 2 and 3. While we did not observe age-related differences in prosocial behavior when measuring the construct in real life with EMA (Study 2), we did observe age group differences in how potential prosocial decisions were made when measuring the construct in a controlled, experimental laboratory setting (Study 3). The moderation effect of setting on the association between age and prosocial behavior in the meta-analysis, in line with the results from Study 2 and Study 3, suggests the possibility that factors like social desirability and the resemblance of the experimental setting to real-life situations may differentially impact prosocial behavior in various age groups. Lab-in-the-field studies typically occur in local settings, with participants being residents of a specific village, interacting directly with each other, often with neighbours (e.g., Rieger & Mata, 2015; Sircar et al., 2018; Tognetti et al., 2013; Vardy & Atkinson, 2019). This setup is more akin to real-life situations and resembles the conditions encountered when using EMA. EMA and lab-in-the-field settings establish a markedly different social environment compared to the highly anonymous nature of laboratory or online studies. For example, in EMA and lab-in-the-field studies, one might assume that prosocial actions are often directed towards socially close individuals, like friends or family members, rather than strangers, which contrasts with the common practice in laboratory studies. Interestingly, even in the laboratory, it has been observed that age-related differences in generosity are no longer evident when assessing generosity towards socially close individuals (Pornpattananangkul et al., 2019). This may explain why age effects on prosociality are found in laboratory settings (usually assessing prosociality towards strangers), but not in real life. Taken together, these findings underscore the significance of considering the (social) context, such as the social distance to the interaction partner, when studying prosociality. Furthermore, EMA and lab-in-the-field studies may offer more authentic insights into specific behaviors within their natural settings, enhancing their external validity, and potential explaining inconsistencies in findings.

Another possible explanation for inconsistencies in the findings on age differences in prosociality might lie in the different operationalizations and definitions applied to assess the construct, underpinning the importance of situational and contextual features. Study 2 adopts a comprehensive approach to evaluate prosocial behavior, encompassing every action that contributed to another person's well-being, including everyday acts like holding a door open, helping an older person with their transactions at the cash register, assisting a stranger with their belongings, or offering a seat on public transportation. These more minor gestures may not necessarily involve a significant cost-benefit analysis for the acting individual, as they might occur spontaneously. Experimental designs assessing this kind of prosocial behavior might apply operationalizations, such as donating participation fees (e.g., Freund & Blanchard-Fields, 2014) or engaging in short additional tasks to support a study, like folding brochures (e.g., Bailey et al., 2020) or answering additional questions (Solomon & Zeitzer, 2019). In contrast, in Study 3, the modified dictator game defines prosocial behavior as sacrificing money for another person, which involves more pronounced costs and benefits. Unlike some studies, this task allowed for a direct comparison between younger and older individuals' ability to weigh values for self- and other-serving options. Thus, to make a valuable decision, one needs to consider potential costs and benefits before deciding to behave prosocially

(Mayr & Freund, 2020), which is why we characterize it as prosocial decision-making. However, not every study design applying paradigms aiming to assess prosocial decision-making specifically examine how immediate positive and negative consequences associated with self- and other-serving choice options are integrated to make a decision, potentially leading to different outcomes. For example, most economic games included in the meta-analysis (Study 1) did not systematically vary values for oneself and others in terms of self- and other-serving options, hindering the process of weighing these values against each other and, thus, the examination of cost-benefit trade-offs. Instead, prosociality is often defined in terms of helping or allocating money (e.g., “Would you be willing to help this person?”, “How much money do you want to allocate to this charity?”), lacking insights into a contrasting self-serving option. Given the results from study 3, highlighting different value-integration processes in older compared to younger individuals, the specific response framing in social decision-making tasks seems to be important to examine cost-benefit trade-offs in older compared to younger adults specifically and to better differentiate it from spontaneous prosocial behavior. One might argue that while spontaneous prosocial behavior may be driven by underlying preferences (cf. intuitive-prosociality model, Zaki & Mitchell, 2013), prosocial decision-making, including a thorough cost-benefit calculation, requires careful deliberation (cf. value-based decision framework, Mayr & Freund, 2020). For understanding behaviors that benefit society, especially in the context of global population aging (Harper, 2014), both aspects are crucial. Firstly, investigating spontaneous prosocial acts is essential for unraveling underlying preferences and determining whether intuitive or controlled processes across the adult lifespan primarily drive prosociality. Secondly, examining how older individuals navigate cost-benefit calculations when making prosocial decisions provides valuable insights into the decision-making processes of a demographic that wields substantial influence in political, societal, and economic domains.

3.3 Open science and replicability

In recent years, there has been a growing emphasis on conducting research that prioritizes transparency and robustness, partly in response to challenges like the replicability crisis (Errington et al., 2021; Ioannidis, 2005; Open Science Collaboration, 2015; Vazire, 2018). Embracing reproducible and open science practices not only improves the reliability of research outcomes but also allows for the reuse of methodologies, data, code, and software from previous studies (Chan et al., 2014; Diaba-Nuhoho & Amponsah-Offeh, 2021; Downs, 2021; Ioannidis et al., 2014). This approach enhances the transparency, dependability, and accessibility of scientific findings (Barba, 2018; Claerbout & Karrenbach, 1992; Nosek et al., 2022; Parsons et al., 2022; Wolf, 2017). However, the integration of reproducible research and open science practices remains limited across various academic disciplines (Blanco et al., 2019; Grant et al., 2013; Hardwicke et al., 2022., 2020; Page & Moher, 2017).

Throughout this dissertation, we took advantage of the principles of open science and reproducibility. This was especially important due to the disruption caused by the COVID-19 pandemic, which led to adjustments in our original research plans and data collection approaches. An excellent illustration of effective open-source practices can be found in the work of Depow and colleagues (2021). They demonstrated how existing data can be seamlessly analyzed using accessible coding books, scripts, and transparent reporting, which enabled us to conduct Study 2. In Study 1, we also ensured transparency and robust methodology by registering the study in PROSPERO (Schiavo, 2019), an international register of systematic review protocols, and adhering to the PRISMA guidelines (Page et al., 2021). However, the absence of data, scripts, and coding books from corresponding studies presented challenges. If authors had made these resources available on open-source platforms, it would have provided several benefits. First, we could have included a larger number of studies, particularly grey literature, as some were excluded due to a lack of response from corresponding authors after requesting specific effects or data. Second, the data analysis process would have been smoother for all involved parties, as we could have calculated the requested effects independently. Third, comprehensive datasets accompanying the individual studies would have allowed us to see whether additional potential covariates were assessed that were not reported in the manuscripts (e.g., cognitive functioning), to analyze these in meta-regression models. This underscores the importance of uniform reporting standards in psychological science and emphasizes the significance of open science practices, including rigorous data management, and well-structured scripts and codebooks.

Several recent articles have addressed integrating reproducible research and open science practices in research institutes, offering practical implementation guidelines (e.g., Frankowski, 2023; Kohrs et al., 2023; Leising et al., 2022). Additionally, there is a growing trend, especially in the context of meta-analyses, towards developing research platforms that enable users to access studies focused on specific variable connections. Examples include the cooperation databank (Spadaro et al., 2022) and MetaLab (Tsuji et al., 2017). These platforms offer features such as visualizing study outcomes, conducting meta-analyses and meta-regressions, assessing publication bias, and performing statistical power analyses.

3.4 Limitations and future directions

The studies in this dissertation offer new insights into prosocial development across adulthood. However, it is crucial to acknowledge their limitations to inform future research.

In Study 2 and Study 3, the sample sizes were constrained, and age categories were used, potentially limiting the representation of the entire adult lifespan. For instance, in Study 2, due to small participant numbers in the youngest and oldest categories, the two youngest and two oldest groups were combined. This might skew the results towards a more youthful elderly population, potentially confounding age effects. For example, previous research indicated that empathic concern was positively associated with self-reported prosociality in individuals under 75 years old (Cavallini et al., 2021). Similarly, it has been shown that the relationship between charitable giving and age shifts negatively in the oldest old (Wiepking & James, 2013), and age effects on altruism are reduced in studies with relatively older-old age cohorts compared to young-old samples (Sparrow et al., 2021). Furthermore, in Study 3, age was treated categorically, and only younger and older adults were included. This approach excluded middle-aged participants, making it impossible to explore potential non-linear relationships between age and prosocial decision-making. Given that the results from Study 1 and Study 2 suggested that midlife might be a significant period in social development (see also Shane et al., 2021), it would be beneficial for future studies to incorporate age as a continuous variable. Additionally, efforts should be made to include a balanced representation of participants across each age decade, even when age is examined in groups, as is typical in development psychology. Overall, addressing these limitations will contribute to a more comprehensive understanding of age trajectories of prosocial development across the entire adult lifespan.

Moreover, it is worth noting that, like many previous studies, both Study 2 and Study 3 included participants from *Western, educated, industrial, rich, and democratic* (WEIRD) populations. These samples do not offer a representative snapshot of the global population (Henrich et al., 2010b, 2010a), which poses a challenge when applying these findings broadly. Cultural disparities play a significant role in human development (S.-C. Li, 2003), underscoring the limitation in terms of generalizability. Additionally, it is essential to highlight that none of the three studies conducted power analyses, potentially leading to publication bias (e.g., Kühberger et al., 2014). Future investigations could address these shortcomings by reinvestigating the link between prosociality and adult age in real-world scenarios and laboratory settings, incorporating non-WEIRD samples, and conducting thorough power analyses.

Furthermore, all three studies relied on cross-sectional data, providing insights into prosocial behavior at specific time points and on the association with age. This means that establishing a causal relationship between these variables was not possible based on the data. Moreover, cross-sectional studies can be influenced by cohort effects related to birth and age differences (see Bekkers et al., 2022). Employing large-scale longitudinal studies would allow for a comprehensive examination of developmental trajectories, including investigations into generational variations in prosocial development. Recent longitudinal studies illustrated different developmental paths for prosociality and generativity across the adult lifespan (Henning et al., 2023; Shane et al., 2021). Longitudinal data could further facilitate deriving practical implications and the long-term effectiveness of strategies and training interventions, as noted by Bailey et al. (2021). A comprehensive approach could involve a combination of longitudinal studies spanning at least a decade of adulthood to measure prosociality alongside cross-sectional samples representing the entire adult lifespan and diverse cultural contexts.

Additionally, it is essential to recognize that while EMA offers significant advantages (e.g., Csikszentmihalyi, 2011), it has its limitations, particularly in terms of standardization. To enhance comparability with other research, there is a need for greater standardization within EMA studies. Currently, there is limited consensus on factors like timing intervals for questions, handling missing data, and the instructions (see Gregorova et al., 2022). Moreover, EMA scales often lack validated psychometric properties, a concern echoed in Study 2. To ensure the reliability of measurements for social dimensions like prosociality or empathy, future studies should incorporate items from validated and standardized questionnaires. For example, Vekaria and colleagues (2020) utilized daily experience sampling to assess every day helping behavior, drawing on items from the Self-Report Altruism Scale (Rushton et al., 1981).

It is also worth noting that the task-based measures used to assess prosocial behavior in this dissertation have recently faced scrutiny, particularly regarding their psychometric properties, especially reliability (Tusche & Bas, 2021). Behavioral measures have demonstrated lower reliability than self-reported measures in evaluating prosocial behavior (Böckler, Tusche, Schmidt, et al., 2018), which aligns with findings in various other contexts (Enkavi et al., 2019). This raises questions about the suitability of psychological tasks as indicators of stable individual differences (Enkavi et al., 2019; Frey et al., 2017; Hedge et al., 2018; Reiter, Atiya, et al., 2021), as reliable measurements are crucial for identifying individual variations (Zech, Gable, et al., 2022). This is significant because constraints in reliability limit the observable correlation between two variables, potentially explaining why many task-based measures show only modest or no correlation with real-life outcomes (I. W. Eisenberg et al., 2019). Some of this lower reliability can be attributed to variation that is not accounted for (Liljequist et al., 2019). Therefore, a valuable direction for future research involves delving into and potentially modeling fluctuations in the constructs of interest. This could help reduce unexplained variance in studies focusing on individual differences, such as adult age differences in prosocial behavior. In turn, this could enhance reliability, observed correlations, as well as statistical power (Zech, Gable, et al., 2022; Zech, Waltmann, et al., 2022). Consequently, a significant avenue for future research involves gaining deeper insights into the factors driving fluctuations in prosocial behavior observed across the adult lifespan (cf. Mayr & Freund, 2020).

In this dissertation and across all three studies, various potential factors that could influence the relationship between prosocial behavior and adult age were explored. These factors were primarily categorized into motivational and resource-related aspects, as well as situational and contextual factors. Another important category, derived from the value-based decision framework (Mayr & Freund, 2020), pertains to proximal factors. These immediate considerations play a role in shaping decision-making, encompassing the assessment of rewards and challenges associated with a specific choice. This involves weighing the significance of benefitting others through altruistic acts or evaluating the level of personal sacrifice in allocating resources. Mayr and Freund (2020) additionally stress the importance of distal factors, such as working memory, ToM, health, generativity, cultural norms, and financial resources for understanding age-related variations in prosociality due to their stability and gradual changes throughout adulthood. However, they suggest that distal factors do not directly enter the cost-benefit calculation of the prosocial decision-making process. Instead, they influence the more immediate proximal factors, which then enter the value-based decision process. Future studies could investigate how motivational and resource-related factors influence the more immediate proximal factors. It would be beneficial to incorporate a comprehensive range of key variables outlined by various theoretical frameworks. This would allow for a thorough examination of how these factors specifically impact cost-benefit trade-offs across the entire adult lifespan in the context of (pro)social decision-making (e.g., assessed with economic games) and spontaneous prosocial behavior (e.g., assessed with EMA). These variables may encompass future time perspective and goal priority (e.g., Lang & Carstensen, 2002) and perspectives, accommodative flexibility, and regulation processes (e.g., Brandtstädter et al., 2010), underlying social preferences, levels of interdependence, and socialization (e.g., Van Lange et al., 1997), perceived control, control strivings, and potential values like agreeableness, honesty-humility, affect sharing, compassion, or perspective taking (e.g., Shane et al., 2021). Additionally, socio-demographic factors like actual income and wealth (e.g., Bekkers et al., 2022; Cutler et al., 2021), as well as cultural disparities encompassing factors like accessible resources and societal norms (e.g., House et al., 2013; Tognetti et al., 2013; Vardy & Atkinson, 2019) should be taken into account. Overall, this approach could lead to a deeper understanding of how these different levels of influence collectively contribute to age-related differences in prosociality across adulthood.

Neuroimaging studies on aging shed light on the neurobiological underpinnings of social processes and decision-making in later stages of adulthood (Beadle & de la Vega, 2019; Lighthall, 2020). Previous cognitive neuroscience research has already shown that specific brain networks, are associated with the anterior insula, temporoparietal junction, and the ventromedial prefrontal cortex, influence individual differences in prosociality (Hutcherson et al., 2015; Tusche & Hutcherson, 2018). A meta-analysis has implicated the ventromedial prefrontal cortex in prosocial decision-making (Cutler & Campbell-Meiklejohn, 2019). However, there is limited evidence regarding the development of these neural networks across adulthood. Additionally, integrating insights and analytical approaches from neuroscience, psychology, and behavioral economics is still relatively uncommon (Tusche & Bas, 2021). Therefore, a promising avenue for future research involves using neurocomputational models that incorporate behavioral and brain data, such as fMRI, similar to the approach taken by Hutcherson et al. (2015) and Tusche and Hutcherson (2018). In terms of value-based decision-making and the associated cost-benefit considerations, these formal neurocomputational models have the potential to demonstrate how different values are encoded and potentially integrated in the brain to make the most valuable choice. This can facilitate the development of comprehensive frameworks, as they combine responses across various levels of analysis, differentiate between competing explanatory mechanisms, and thereby enable precise predictions of behavioral outcomes (I. D. Roberts & Hutcherson, 2019).

Understanding the factors and processes influencing variations in prosociality is crucial for developing more effective strategies to promote prosocial behavior. For instance, research indicates that enhancing emotional or cognitive capacities can be a viable approach to promote prosociality (Tusche et al., 2016). A recent large-scale intervention study (Böckler, Tusche, Schmidt, et al., 2018) supported this idea, showing that interventions focused on improving social affective processes significantly increased prosociality. In addition to its relevance in crisis management scenarios like climate change (Alston, 2015; Marx & Weber, 2012) and conflicts such as the Ukraine war (Politi et al., 2023), insights from intervention grounded in neural understanding can offer valuable guidance to policymakers and the general public in fostering global cooperation.

3.5 Conclusion

The primary goal of this dissertation was to advance our understanding of prosociality in the context of adult development and aging, by integrating different lifespan developmental perspectives. We introduced a new perspective regarding age trajectories of prosociality, revealing that both linear and quadratic functions can characterize the link between adult age and prosociality. This uncovers a noteworthy pattern: increased prosociality may not only emerge in old age but may also exhibit a distinct peak earlier in midlife. We further examined potential factors that may moderate the association between age and prosociality, as well as computational mechanisms underlying the (pro)social decision-making process, to provide a basis for a more comprehensive understanding of what may drive the observed age effects. While confirming the role of situational and contextual factors for prosocial behavior, the results also suggest that inhibitory control might be an important moderator and that decision-efficiency might further explain age-related differences. Given the inconsistency in the findings regarding age effects on prosociality and the different operationalizations and definitions of prosocial behavior across the three studies, we propose that distinguishing between prosocial decision-making (i.e., including cost-benefit trade-offs), and spontaneous prosocial behavior (e.g., offering a seat in public transportation) may contribute to a clearer picture of age effects on prosociality. These aspects may exhibit distinct developmental trajectories across various life domains.

To conclude, age differences in prosociality may be observed more likely in some contexts than in others and this may further depend on interindividual differences such as cognitive functioning. Whether a given study finds an age effect or not may be explained by both the configuration of situational characteristics of the study and interindividual characteristics of the samples. Overall, the different findings presented within this dissertation underscore the complexity and diversity of age effects on prosociality and emphasize the need for a comprehensive framework which may then inform targets for intervention.

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5 Appendix

Publication list

Pollerhoff, L., Stietz, J., Depow, G.J., Inzlicht, M., Kanske, P., Li, S.-C., & Reiter, A.M.F. (2022). Investigating adult age differences in real-life empathy, prosociality, and well-being using experience sampling. *Scientific Reports, 12*(1), 1-15. https://doi.org/10.1038/s41598-022-06620-x

Stietz, J., Pollerhoff, L., Kurtz, M., Li, S.-C., Reiter, A. M. F., & Kanske, P. (2021). The ageing of the social mind: Replicating the preservation of socio-affective and the decline of socio-cognitive processes in old age. *Royal Society Open Science*, *8*(8), Article 210641. https://doi.org/10.1098/rsos.210641

Pollerhoff, L.*, Saulin, A*., Kurtz, M., Stietz, J., Peng, X.-R., Hein, G., Tusche, A., Kanske, P., Li, S.-C., & Reiter, A. M. F. (submitted). Adult age differences in the integration of values for self and other.

*Equal contribution

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Curriculum Vitae

Affidavit

I hereby confirm that my thesis entitled “Age differences in prosociality across the adult lifespan: Insights from self-reports, experimental paradigms, and meta-analyses” is the result of my own work. I did not receive any help or support from commercial consultants. All sources and / or materials applied are listed and specified in the thesis.
Furthermore, I confirm that this thesis has not yet been submitted as part of another examination process neither in identical nor in similar form.

Lena Katharina Pollerhoff	09/11/2023	Würzburg	
Doctoral Researcher’s Name	Date	Place	Signature

Eidesstaatliche Erklärung

Hiermit erkläre ich an Eides statt, die Dissertation „Altersunterschiede in Prosozialität über die erwachsene Lebensspanne hinweg: Erkenntnisse aus Selbstbeurteilungen, experimentellen Paradigmen und Meta-Analysen“ eigenständig, d.h. insbesondere selbstständig und ohne Hilfe eines kommerziellen Promotionsberaters, angefertigt und keine anderen als die von mir angegebenen Quellen und Hilfsmittel verwendet zu haben.
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Individual author contributions

Statement of individual author contributions and of legal second publication rights to manuscripts included in this dissertation

Pollerhoff, L., Reindel, D.F., Kanske, P., Li, S.-C., & Reiter, A.M.F. (in preparation). Age differences in prosociality across the adult lifespan: A meta-analysis.					
Participated in	**Author Initials,** Responsibility decreasing from left to right				
Study Design Methods Development	LP AMFR LP AMFR				
Data Collection	LP DFR				
Data Analysis and Interpretation	LP	AMFR			
Manuscript Writing Writing of Introduction Writing of Materials & Methods Writing of Discussion Writing of First Draft	 LP AMFR LP LP AMFR LP	 AMFR AMFR	 DFR PK SCL		

Explanations (if applicable):

Pollerhoff, L., Stietz, J., Depow, G. J., Inzlicht, M., Kanske, P., Li, S.-C., & Reiter, A. M. F. (2022). Investigating adult age differences in real-life empathy, prosociality, and well-being using experience sampling. *Scientific Reports*, *12*(1), 1–15. https://doi.org/10.1038/s41598-022-06620-x					
Participated in	**Author Initials,** Responsibility decreasing from left to right				
Study Design Methods Development	GJD MI GJD MI				
Data Collection	GJD MI				
Data Analysis and Interpretation	LP	AMFR	PK	JS	
Manuscript Writing Writing of Introduction Writing of Materials & Methods Writing of Discussion Writing of First Draft	 LP AMFR LP LP AMFR LP	 AMFR AMFR	 GJD MI	 SCL PK JS	

Explanations (if applicable): we re-analyzed open-source data from GJD and MI.

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Lena Katharina Pollerhoff	09/11/2023	Würzburg	
Doctoral Researcher’s Name	Date	Place	Signature
Prof. Dr. Andrea Reiter	09/11/2023	Würzburg	
Primary Supervisor’s Name	Date	Place	Signature

Statement of individual author contributions to figures/tables of manuscripts included in the dissertation

Pollerhoff, L., Reindel, D.F., Kanske, P., Li, S.-C., & Reiter, A.M.F. (in preparation). Age differences in prosociality across the adult lifespan: A meta-analysis.					
Figure	**Author Initials,** Responsibility decreasing from left to right				
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Explanations (if applicable):

Pollerhoff, L., Stietz, J., Depow, G. J., Inzlicht, M., Kanske, P., Li, S.-C., & Reiter, A. M. F. (2022). Investigating adult age differences in real-life empathy, prosociality, and well-being using experience sampling. *Scientific Reports*, *12*(1), 1–15. https://doi.org/10.1038/s41598-022-06620-x					
Figure	**Author Initials,** Responsibility decreasing from left to right				
11	LP	AMFR			
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Pollerhoff, L.*, Saulin, A.*, Kurtz, M., Stietz, J., Peng, X.-R., Hein, G., Tusche, A., Kanske, P., Li, S.-C., & Reiter, A. M. F. (submitted). Adult age differences in the integration of values for self and other.					
Figure	**Author Initials,** Responsibility decreasing from left to right				
15	LP	AMFR	AT		
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19	LP	AS	AMFR		
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S1-S4	LP	AMFR			
S5-S6	LP	AS	AMFR		
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8	LP	AMFR			
9	LP	AMFR	MK		
S1-S13	LP	AMFR			
S14-S15	LP	AS	AMFR		

Explanations (if applicable): *Equal contribution

I also confirm my primary supervisor's acceptance.

Lena Katharina Pollerhoff 09/11/2023 Würzburg

Doctoral Researcher's Name Date Place Signature