Promoting Farsighted Decisions via Episodic Future Thinking: A Meta-Analysis

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Episodic future thinking (EFT) denotes our capacity to imagine prospective events. It has been suggested to promote farsighted decisions that entail a trade-off between short-term vs. long-term gains. Here, we meta-analyze the evidence for the impact of EFT on such intertemporal choices that have monetary or health-relevant consequences. Across 174 effect sizes from 48 articles, a three-level model yielded a medium-sized effect of $g = .44$, 95% CI [.33, .55]. Notably, this analysis included a substantial number of unpublished experiments, and the effect remained significant following further adjustments for remaining publication bias. We exploited the observed heterogeneity to determine critical core components that moderate the impact of EFT. Specifically, the effect was stronger when the imagined events were positive, more vivid, and related to the delayed choice. We further obtained evidence for the contribution of the episodicity and future-orientedness of EFT. These results indicate that the impact of EFT cannot simply be accounted for by other modes of prospection (e.g., semantic future thinking). Of note, EFT had a greater impact in samples characterized by choice impulsivity (e.g., in obesity), suggesting that EFT can ameliorate maladaptive decision making. It may accordingly constitute a beneficial intervention for individuals who tend to make myopic decisions. Our analyses moreover indicated that the effect is unlikely to merely reflect demand characteristics. This meta-analysis highlights the potential of EFT in promoting long-term goals, a finding that extends from the laboratory to real-life decisions.

episodic future thinking | delay discounting | choice impulsivity | self-control | intertemporal choice

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Introduction

Many of our decisions entail a trade-off between short-term gains and long-term consequences. They range from the trivial (e.g., having a second helping vs leaving room for dessert) to the highly consequential (e.g., splurging on a new car vs. saving for retirement). In the laboratory, these intertemporal choices are typically examined by having participants choose between small and sooner rewards (e.g., Euro 5 now) vs

larger but more delayed rewards (e.g., Euro 10 in 2 months). A rich line of research has documented that, everything else being equal, people are more likely to forego the larger reward with increasing delay until its delivery (Amasino et al., 2019; Berns et al., 2007; Green & Myerson, 2004; Peters & Büchel, 2011). That is, we tend to assign a smaller subjective value to the otherwise identical reward when we would have to wait longer for it. This phenomenon is most commonly referred to as delay discounting (DD; Green & Myerson, 2004; Mazur, 1987).

Although DD is a ubiquitous feature of intertemporal choices, individuals vary with the extent to which they discount delayed rewards. Steeper discounters tend to make poorer health-relevant decisions (Amlung et al., 2019; Peters & Büchel, 2011; Story et al., 2014). For example, exaggerated monetary DD has been associated with detrimental behaviors such as physical inactivity (Story et al., 2014), overeating (Appelhans et al., 2011; Rollins et al., 2010), and substance abuse (Bickel et al., 1999; Kollins, 2003; MacKillop et al., 2011). These behaviors implicitly constitute intertemporal choices between immediately available rewards (i.e., indulging in the moment) vs. temporally protracted rewards (i.e., being in good health) (Bulley & Schacter, 2020; Epstein et al., 2003; Hollis-Hansen et al., 2019). DD generally characterizes choice impulsivity and may constitute a critical trans-disease process (Amlung et al., 2019; Hamilton et al., 2015).

The link between DD and maladaptive behaviors has stimulated research on interventions that may prove effective in nudging individuals towards more farsighted decisions (Peters & Büchel, 2011; Rung & Madden, 2018b). These include mindfulness-based approaches (Hendrickson & Rasmussen, 2013; Morrison et al., 2014), framing (Dshe-muchadse et al., 2013; Read et al., 2005) and other contextual effects (Dai & Fishbach, 2013; Green et al., 2005), as well as pharmacological interventions (de Wit et al., 2002). We here examine the impact of episodic future thinking (EFT): the imagination of episodes that may take place in one's personal future (Atance & O'Neill, 2001; Schacter et al., 2017a).

EFT exhibits similar phenomenological properties as episodic memory (D'Argembeau & van der Linden, 2004) and follows a similar life-span developmental trajectory (Ad-dis et al., 2008; Busby & Suddendorf, 2005). Moreover, lesions to the medial temporal lobes, long known to severely

impair episodic memory, also have a detrimental impact on EFT (Hassabis et al., 2007; Race et al., 2011a). Indeed, evidence from neuroimaging indicates that these two capacities are more broadly supported by the same core network of brain regions (Benoit & Schacter, 2015). These commonalities have been taken to suggest that EFT draws on memories of the past that get recombined into simulations of prospective events (Addis, 2020; Schacter et al., 2017b).

While there are other modes of future-oriented cognition (e.g., predictions, intentions) (Szpunar et al., 2014), EFT is unique in the sense that it allows us to mentally experience possible future events (Atance & O'Neill, 2001; Szpunar et al., 2014). This simulated experience also conveys our anticipated prospective emotions (Boyer, 2008; Benoit et al., 2018, 2014). EFT allows humans to not only consider the immediate outcomes of their actions but also their extended consequences (Atance & O'Neill, 2001; Boyer, 2008; Suddendorf & Corballis, 2007). It renders an otherwise intangible future more concrete (Lee et al., 2020a; Thorstad & Wolff, 2018) and can guide our intertemporal decisions (Benoit et al., 2018; Boyer, 2008; Bulley et al., 2016).

The Current Meta-Analysis. Over the last decade since the initial reports (Benoit et al., 2011; Peters & Büchel, 2010), there has been rapidly accumulating evidence that EFT indeed attenuates DD of monetary rewards. A previous survey had already summarized the early studies as part of a broader attempt to identify various methods of influencing DD (Rung & Madden, 2018b). However, the current work goes well beyond that survey with respect to the impact of EFT. First, with a fivefold increase in included studies, this meta-analysis is based on a substantially broader empirical basis. Second, it seeks to take account for publication bias by including a large number of unpublished data sets (16% of the included studies) and by adopting several statistical detection and adjustment procedures. Third, due to the implementation of a three-level meta-analytical model (see below), we were able to include all effect sizes from each study. Fourth, while the previous meta-analysis focused on the discounting of monetary rewards, we critically assess the generalizability to health-relevant choices (e.g., smoking, alcohol, food). Finally, we exploit the heterogeneity of the used experimental procedures to quantitatively elucidate *how* EFT fosters farsighted decisions.

EFT is a complex capacity, and we are still lacking a clear understanding of the core components that render it effective in reducing discounting. The behavioral procedures typically ask participants to imagine specific prospective episodes (i.e., the EFT condition) or to engage in one of various other tasks (i.e., the control condition), before they have to make an intertemporal choice (that either affects their monetary pay off or has possible health-relevant consequences). Across studies, however, there is a great heterogeneity in the nature of both the EFT and control conditions.

We make use of that heterogeneity to determine the crit-

ical core features of EFT that lead to more farsighted decisions. We accomplish this by conducting a number of targeted moderator analyses. These analyses focus on the (a) *valence* of the imagined event, its (b) *vividness*, (c) *episodicity*, and (d) *future-orientedness*. We also examine the impact of the (e) *content specificity* of the imagined event (i.e., whether the imagination entails the consumption of the delayed reward or its possible consequences). By assessing the contribution of these critical core features, we also gauge whether the observed effect is likely to result from EFT or whether it may be accounted for by other modes of future-oriented cognition that do not entail the vivid mental experience of a prospective event (e.g., semantic future thinking; Suddendorf & Corballis, 2007; Szpunar et al., 2014). Finally, we assess whether EFT is more effective in people who are characterized by greater (f) *choice impulsivity* (e.g., individuals with obesity). In the following, we introduce these candidate core components.

Candidate Core Components of Episodic Future Thinking.

Valence. When we are faced with an intertemporal choice, we do not experience the positive emotional impact that a delayed reward would hold (e.g., Rick & Loewenstein, 2008). EFT conveys this affective experience by allowing us to mentally visit that future moment (Benoit et al., 2018; Schacter et al., 2015). This positive experience has been hypothesized to add value to the delayed reward and to act as a break on impulsive tendencies (Benoit et al., 2011; Boyer, 2008; Frederick et al., 2002; Loewenstein & Lerner, 2003; Rick & Loewenstein, 2008). In contrast, imagining negative episodes may even have the opposite effect of further devaluing the future (Bulley et al., 2016; Liu et al., 2013).

However, the few studies that have directly examined the effect of valence have provided mixed evidence (Bulley et al., 2019; Calluso et al., 2019; Liu et al., 2013; Zhang et al., 2018). We here used the heterogeneity of the episodes' valence across all studies to systematically examine whether EFT more strongly promotes farsighted decisions when the imagined events are positive rather than neutral or negative.

Vividness. EFT may have a stronger impact on one's future-oriented decisions when the prospective events are imagined more vividly (Bromberg et al., 2015; Ciaramelli et al., 2019; Peters & Büchel, 2010). Related to the previous point, more vividly imagined events elicit stronger affective responses (D'Argembeau & Van der Linden, 2012; Holmes & Mathews, 2010). In addition, vividly imagined events are also deemed more likely or plausible to actually occur (Gregory et al., 1982; Sherman et al., 1985; Szpunar & Schacter, 2013; J. Q. Wu et al., 2015). Vivid imaginings might partially attenuate discounting by rendering the future more certain (Bulley et al., 2016). To date, there is only limited evidence that participants who imagine future episodes more vividly are also more likely to choose delayed rewards (Bromberg et al., 2015; Ciaramelli et al., 2019; Peters & Büchel, 2010). To gauge *vividness* as a potential moderator, we focused on the studies that have reported such ratings for both the EFT

and control condition. Specifically, we determined the effect sizes of the difference between conditions and included them as a moderator.

Episodicity. Though intertemporal choices inherently entail a trade-off with the future, there is evidence that people who are apparently stuck in the present do not necessarily show atypical patterns of DD. Specifically, amnesic patients with damage to the medial temporal lobes are also severely impaired in simulating future episodes (McCormick et al., 2018; Race et al., 2011b). Yet, per se, they do not discount future rewards more strongly than neurotypical persons (De Luca et al., 2018; Kwan et al., 2012, 2013). Critically though, unlike neurotypicals, amnesic individuals do not show reduced delay discounting when given the opportunity to engage in EFT before making their choices (Lebreton et al., 2013; Palombo et al., 2015). This is particularly the case when they have more extensive lesions in the medial temporal lobes (Kwan et al., 2015).

The neuropsychological data indicate that the ability to construct *episodic* imaginings of the future contributes to the impact of EFT. To further test this hypothesis regarding the episodicity of future thoughts, we specifically examined the studies that compared EFT to a nonepisodic, yet also future-oriented control condition (i.e., semantic future thinking; Atance & O'Neill, 2001; Suddendorf & Corballis, 2007).

Future-Orientedness. EFT makes prospective events more salient and changes people's time perspective further into the future (Kurth-Nelson et al., 2012; Lin & Epstein, 2014). This shift towards a greater future-orientedness may lead to more farsighted decisions (Sheffer et al., 2016; Shevorykin et al., 2019).

Supporting this account, there is some general evidence that more future-oriented individuals discount monetary rewards less steeply (Daugherty & Brase, 2010; Joireman et al., 2008; Steinberg et al., 2009) and that they make better health-relevant choices (e.g., Hall et al., 2015).

There is also more specific evidence that episodic *future* thinking reduces discounting and promotes health-relevant choices - even compared with other forms of episodic thinking (e.g., recall of recent or remote past events; Daniel et al., 2016; Dassen et al., 2016; Stein et al., 2018; c.f. Ciaramelli et al., 2019). We systematically examined these studies to test the hypothesis that a temporal shift toward the future is a contributing core component of EFT.

Content Specificity. The studies on EFT and intertemporal choice differ with respect to the degree that the future imaginings were *content-specific*, that is, that they directly incorporated the future payoffs. Some studies did match the EFT with the outcome. They instructed participants to specifically imagine the impact of choosing the delayed monetary option (Benoit et al., 2011; Palombo et al., 2015) or the health-relevant consequences (Chiou & W.-H. Wu, 2017; Dassen et al., 2016). Others required their participants more

generally to imagine any future event of their choosing (e.g., Cheng et al., 2012; Wu et al., 2017).

Content-specific EFT may particularly motivate farsighted decisions by explicitly binding the delayed option to one's personal future goals (Boyer, 2008; Ernst et al., 2018). However, the few studies that have directly compared the influence of content-specific vs. generic EFT have yielded inconclusive results (Dassen et al., 2016; Hollis-Hansen et al., 2019; O'Donnell et al., 2017). We here made use of the methodological variance in the literature to further examine this account across all included studies.

Choice Impulsivity. An individual tendency for steeper delay discounting has been linked to maladaptive outcomes and diverse clinical disorders (Amlung et al., 2016, 2017, 2019; Paret et al., 2017). It is particularly important to gauge whether people who are characterized by choice impulsivity (e.g., individuals with obesity or with substance use disorder) may benefit from EFT (Peters & Büchel, 2011). Indeed, given their generally greater disregard for the future, it seems possible that their choices may in fact be more malleable to the influence of directed EFT.

Further Procedural Moderators.

In addition to the theoretically motivated moderators, we also examined a number of additional moderators that are of more procedural interest. Specifically, we tested whether there are systematic differences in the observed effect sizes (g) for between- vs. within-subject designs and for studies conducted (h) *in person* (e.g., the physical laboratory) vs. online (e.g., Amazon Mechanical Turk [MTurk]). Finally, in an attempt to gauge the impact of publication bias (Rothstein et al., 2005), we included a moderator analysis based on the (i) *publications status* of the experiment. This was possible due to the identification of a large number of unpublished experiments (16% of the included effect sizes).

Summary. This meta-analysis provides a comprehensive quantitative review of the impact of EFT on intertemporal choices. It examines the influence on delay discounting of monetary rewards and, for the first time, on health-relevant behavior. The targeted moderator analyses critically inform our understanding of the core components of EFT that are involved in nudging choices toward more farsighted decisions.

Method

Our meta-analysis was guided by the *Preferred Reporting Items for Systematic Reviews and Meta-Analyses* (PRISMA) checklist (Moher et al., 2009; Fig. 1). The full coding sheet is included in the online supplemental material. All data and R analysis scripts are publicly available at the Open Science Framework (https://osf.io/9rejff/?view_only=fee62b3cadd4405b8aafdac75015f60b).

Literature Search. We sought to identify all studies that had investigated the impact of EFT on intertemporal choices involving either monetary or health-relevant decisions. We searched PubMed and the Web of Science data bases for articles published before February 2020 using the following search terms:

- PubMed: (("episodic future thinking") OR ("episodic simulation") OR ("episodic prospection") OR ("mental time travel") OR ("prospective thinking") OR ("counterfactual thinking") OR (foresight)) AND (discount*)
- Web of Science: (TS=("episodic future thinking" OR "episodic prospection" OR "episodic simulation" OR "mental time travel" OR foresight) AND TS=(discount*))> OR "prospective thinking"> OR "counterfactual thinking")

In addition, we retrieved all 364 unique references cited in the identified articles on EFT and health-relevant decisions. We were further concerned that our database search had missed relevant work from a prolific lab in the field, and examined all of their 314 published articles (adding a total of $n = 3$ articles). The keywords of these articles differed from our previous searches in their focus on clinical and translational aspects (i.e., obesity, ecological momentary intervention, or health behavior). We also reviewed the reference section of recent surveys of the literature (Bulley et al., 2016; Rung & Madden, 2018b), which added seven relevant citations. We also added 10 further references that we were personally aware of.

To mitigate the likely influence of publication bias, we moreover aimed to retrieve pertinent studies that had not been published in peer-reviewed journals. We first searched the data bases ProQuest and (the German-language alternative) Thesius for otherwise unpublished Master and PhD theses:

- ProQuest: (episodic AND future AND thinking NEAR discount OR episodic AND simulation NEAR discount OR episodic AND prospection NEAR discount OR mental AND time AND travel NEAR discount OR prospective AND thinking NEAR discount OR counterfactual AND thinking NEAR discount OR foresight NEAR discount)
- Thesius: (delay AND discounting AND Zukunft) as well as (delay AND discounting AND episodisch)

In addition, to identify further unpublished experiments, we emailed all 25 corresponding or senior authors of pertinent publications for which we could identify current contact information ($n = 21$ authors; on February 18, 2020). A total of $n = 10$ authors replied to our request and, as a result, we were able to additionally include $n = 2$ studies ($n = 8$ effect sizes). In total, our literature search thus yielded 1,625 hits after removing duplicates (Fig. 1).

Inclusion and exclusion criteria.

We included all studies that (a) entailed a condition that induced EFT before the intertemporal choices, (2) compared this condition to a non-EFT control condition, (3) included either monetary or health-relevant intertemporal choices, (4) were written up in English. We did not include data of amnesic individuals, given their well-documented deficiency in EFT (McCormick et al., 2018; Race et al., 2011b).

Sarah A. Rösch and Davide F. Stramaccia independently screened the titles and abstracts. When there was any doubt about an article's eligibility, it was retained for further review. This approach led to the selection of 234 potentially relevant articles for full-text reading. Based on the above criteria, we eventually retained a total of 48 articles that contained a combined 58 studies (Fig. 1; for a complete list, see Appendix).

Data Extraction. Data were extracted and coded by Sarah A. Rösch and a trained research assistant (MSc level), including information required for the calculation of effect sizes (i.e., sample sizes, means and standard deviations, t -, F - or p -values). We calculated the standard deviations from their respective standard errors when only the latter were available.

Forty-three percent of the reports only presented relevant statistics as graphs. In these cases, we used the WebPlotDigitizer (Rohatgi, 2019) to extract the data to the second decimal with perfect inter-rater reliability (intraclass correlation coefficient = 1.00). For studies that did not provide sufficient information to compute effect sizes, we emailed the authors. Of all studies that had met the inclusion criteria, we only had to exclude a single study due to missing data.

Moderator Coding. The moderators were coded by Sarah A. Rösch and a trained research assistant (MSc level), who discussed and resolved discrepancies throughout the coding process. The coding criteria are detailed in Table 1. The moderator coding for each effect size is listed in the Appendix.

Choice Domain assessed whether a given effect size was derived from monetary vs. health-relevant choices (e.g., nicotine or calorie intake).

Valence of the imagined episode was determined based on the experimenters' description. If the methods did not provide the information, our coding was based on ratings by the participants. We here took the valence label of the anchor that was closest to the average rating of the episodes. For the case where also no ratings were available, we discerned the valence from the provided examples.

Vividness was coded for those studies that had provided such ratings for both the EFT and control condition ($n = 15$ studies). Specifically, we determined the effect sizes of the difference between these conditions and included them as a moderator. We considered all studies that had reported vividness scores as well as studies that reported any com-

Fig. 1. PRISMA Flowchart Illustrating Study Selection.

posite scores that included vividness (e.g., a score composed of vividness and frequency; Bulley & Gullo, 2017; Hollis-Hansen et al., 2019). Of note, only two studies reported vividness ratings from the EFT task proper Bulley & Gullo, 2017; Hollis-Hansen et al., 2019. The remaining 13 studies reported ratings that were assessed during an earlier stage when participants generated episodic cues that were then to be used during the EFT task.

Episodicity refers to studies that used a control condition that was matched in all aspects to the EFT condition but in the requirement to imagine a specific event. These control conditions constitute semantic future thinking (e.g., Benoit et al., 2011). For example, participants may be required to consider the general positive future benefits of quitting smoking rather than to imagine any specific pertinent future episode (Chiou & W.-H. Wu, 2017). The other level of the moderator included all other control conditions.

Future-orientedness, by contrast, coded whether the control condition was matched on all aspects but the temporal orientation (e.g., episodic recent thinking; O'Donnell et al., 2017). The other level of the moderator included all other control conditions.

Content-specificity coded whether the imagined episodes in the EFT condition entailed the experience of the possible future reward. Thus, for monetary choices, all episodes were classified as content-specific that somehow involved imagining spending the money (e.g., Benoit et al., 2011; Palombo et al., 2015), whereas the other studies were classified as content-unspecific. For health-relevant choices, the reward is related to the achievement of a health-related goal, such as losing weight or refraining from smoking or substance use. Thus, if the imagined episodes were directly linked to these goals (e.g., positive events that would

occur if participants successfully quit smoking; Chiou & W.-H. Wu, 2017) or related behaviors, (e.g., buying water instead of juice to lose weight; Hollis-Hansen et al., 2019), they were classified as content-specific. If the imagined episodes were unrelated to the goal, they were classified as content-unspecific.

Choice impulsivity was categorized based on recent reviews (Hamilton et al., 2015) and meta-analyses (Amlung et al., 2016, 2019; MacKillop et al., 2011). A sample was classified as choice impulsive, if it comprised individuals with overweight or obesity (i.e., Body Mass Index (kg/m²) 25 or 30), self-reported smokers, or individuals with drug-dependences or pathological gambling as diagnosed according to the Diagnostic and statistical manual of mental disorders (apa, 2000).

Design For studies that applied a mixed design (e.g., Kakoschke et al., 2018), that is, a between- and a within-subjects manipulation, we retained each effect size of the between- and the within-subjects comparison. This was possible due to the correction for correlated samples in our three-level model (see below).

Study Site coded whether a study was conducted online (e.g., on MTurk) or in person on site. The latter category also included a number of studies that assessed health-relevant choices in more naturalistic settings – either in the field (i.e., a food court; $n = 1$) or by directly assessing pertinent behavior (e.g., caloric intake at ad libitum food buffets, $n = 6$). We performed a separate analysis only on these studies to assess the impact of EFT on health-relevant choices with particularly high ecological validity.

Publication status differentiates between studies that had been published in peer-reviewed journals and those that

we had only identified either via the dissertation data-bases or via our direct call for unpublished studies.

Statistical Analysis. We computed a series of meta-analyses in R 3.6.1 (R Core Team, 2019) with the package metafor 2.4 (Viechtbauer, 2010) to assess the impact of EFT on monetary and health-relevant intertemporal choices.

Deriving Effect Sizes. We computed effect sizes as the bias-corrected Hedges' g (using the `escalc` function; measure option set to SMD). For *between-subjects* designs, this represents the bias-corrected standardized mean difference between the EFT and control groups. For *within-subjects* designs, we derived its equivalent (Morris & DeShon, 2002), the standardized mean change using raw score standardization (Becker, 1988; using the `escalc` function; measure option set to SMCR). d -values were converted via the Hedges' g function of the `esc` package (Lüdtke, 2019).

Determining the effect sizes for within-subject designs requires the correlation between EFT and control conditions. This correlation has rarely been reported in the surveyed literature. As recommended (Borenstein et al., 2009), based on pertinent data from our and other labs (Martínez-Loredo et al., 2017; Strickland et al., 2019; Weafer et al., 2013), we estimated the coefficient as $r = .6$. (Note that additional sensitivity analyses, using alternative correlation coefficients of $r = .2$ and $r = .8$, yielded qualitatively identical results, see online supplemental materials).

Some outcome measures were reverse-coded, that is, greater numbers reflected less patient choices (e.g., k -values as in Mellis et al., 2019). In these cases, we multiplied the reported value by -1 to ensure that positive effect sizes consistently represented more farsighted decisions. All effect sizes are reported in the Appendix.

Data Synthesis. Seventy-seven percent of the reports reported multiple effect sizes, including multiple studies per report (applicable for 17% of the reports), outcomes for both monetary and health-relevant choices, different dependent variables for the same outcome (e.g., k and percentage of immediate choices), and different groups (e.g., two EFT groups vs. the same control group).

The data entail two types of dependencies: *correlated effects* (i.e., effect sizes derived from overlapping samples) and *hierarchical effects* (i.e., effect sizes nested within studies; Tanner-Smith & Tipton, 2014). These dependencies violate assumptions underlying traditional meta-analytic approaches (Lipsey & Wilson, 2001) and could artificially narrow confidence intervals and shrink standard errors, and, as a consequence, increase type I error (Hox, 2010; Viechtbauer, 2010).

We used two methods to account for both types of dependencies while retaining all provided information and without losing statistical power (see Cheung, 2014, 2019). First, due to the hierarchical dependency, we adopted a three-level

random-effects meta-analytic model. This model accounts for the variance in the observed effect sizes (level 1), variance between effect sizes within a report (level 2), and variance between reports (level 3; Cheung, 2014). This approach allowed us to estimate the random variation at level 2 to account for dependence (i.e., clustering of effect sizes) among effect sizes from the same article (Konstantopoulos, 2011). Model parameters in the three-level model were estimated using restricted maximum likelihood estimation (REML; Cheung, 2014; Viechtbauer, 2010) with the Knapp & Hartung (2003) method for calculating regression coefficients and confidence intervals (Assink & Wibbelink, 2016).

Second, due to the correlated dependencies, we performed statistical tests on cluster-robust standard errors and confidence intervals (Hedges et al., 2010) that we generated for the estimates from the three-level meta-analytic model (via the `robust` function of the metafor package; Viechtbauer, 2010). Individual coefficients were tested using a t -distribution and all model coefficients were tested using an F -distribution (i.e., omnibus test; excluding the intercept).

Assessment of Heterogeneity. We evaluate the heterogeneity of the reported models by reporting total I^2 -values along with the components for the second (I^2_{Level2} ; variance within the same studies) and third level (I^2_{Level3} ; variance between studies). These values indicate the percentage of variability that is based on differences between studies rather than sampling error. Heterogeneity provides information regarding the influence of study-level characteristics (e.g., valence of the imagination) on the observed effect size. We classified heterogeneity following the Cochrane Handbook (Higgins & Green, 2011), with I^2 -values above 50% as substantial and above 75% as considerable.

We determined the significance of the heterogeneity at each level using the Q -statistic. This approach entailed separate one-tailed log-likelihood-ratio tests in which the deviance of the full model was compared to the deviance of the model excluding one of the variance parameters (Assink & Wibbelink, 2016). A significant Q -value at either of the levels suggests systematic differences beyond what would be expected from sampling error alone, potentially due to significant moderating factors accounting for heterogeneity.

Moderator Analysis. An important aim of our analysis was the assessment of the critical core components that influence the impact of EFT on intertemporal choices. We took advantage of the heterogeneity of the reported effect sizes and conducted a series of moderator analyses. These were targeted at hypothesized candidate components and at further procedural factors. The potential moderators varied either between and within studies (e.g., monetary vs. health-relevant choices) or between studies (e.g., publication status).

We dummy-coded all categorical moderators with more than two levels (e.g., for the moderator valence) before entering them into meta-regression equations. Due to the limited

number of studies that contributed to the levels of the moderators, we focused on a series of single-moderator analyses. We also report a model that included all moderators with significant univariate effects to reduce multicollinearity (Hox, 2010). Moderator analyses need a large amount of observations and low heterogeneity to achieve high power (Hedges & Pigott, 2004). Null effects in tests of moderation should be interpreted cautiously.

Model Comparisons. We informed our moderator analyses by assessing whether each model provided a better fit to the data than a more parsimonious intercept-only model. These model comparisons were based on the Akaike Information Criterion (AIC; Akaike, 1973) with small-sample correction. The AIC values were transformed to conditional probabilities for each model, that is, AIC weights (AICw; Wagenmakers & Farrell, 2004). This was done using the AIC (from the metafor package) and the Weights (from the MuMIn package) functions in R.

However, AIC is not suitable for comparing models that differ in their fixed effects structure and that have been estimated via REML. Therefore, we based these comparisons on models that were refitted using the Maximum Likelihood method, again with the Knapp & Hartung (2003) correction.

To obtain a measure of statistical significance, we further compared the single-moderator models using Likelihood Ratio Tests. These outperform omnibus moderator tests in terms of statistical accuracy (i.e., Type I error rates; López-López et al., 2017).

Outlier Analyses. The sensitivity of meta-analyses is also vulnerable to outliers and influential data points. We used four diagnostic tests that can identify such data points in three-level meta-analytical models (i.e., when using rma.mv in metafor) (Viechtbauer, 2010; Viechtbauer & Cheung, 2019).

These included (a) Cook's distances (via the cooks.distance.rma.mv function), (b) the change of the estimated coefficient in standard deviations upon excluding a specific study (via the dfbetas.rma.mv function), (c) the diagonal elements of the hat matrix (via the hatvalues.rma.mv function), and (d) the standardized residuals (via the rstudent function of the stats package; (R Core Team, 2019)).

We defined influential outliers as effect sizes (a) with cook's distance exceeding the critical χ^2 -value threshold ($\chi^2 [p, 1-\alpha]$), (b) whose exclusion would result in a change > 1 in standard deviations of the estimated coefficient change, (c) with hat values (i.e. diagonal elements of the hat matrix) greater than three times the number of model coefficients divided by the number of studies, and (d) standardized residual values exceeding 3. We evaluated the sensitivity of our analyses by re-estimating our meta-analytical model following the exclusion of the one effect size that this procedure had flagged as an influential outlier.

Assessment of Publication Bias. Meta-analyses are prone to provide exaggerated effect size estimates due to publication bias, that is, the selective reporting of studies yielding significant effects (Carter et al., 2019; Renkewitz & Keiner, 2019). To minimize this bias, we sought to identify studies without a peer-reviewed publication record. Notably, 16% of our effect sizes stem from such unpublished sources. In addition, we (a) examined the degree to which our results remain bias-inflated and (b) attempted to adjust for such effects. To accomplish these goals, we used several complementary methods, given that none of them is without their particular drawbacks (Carter et al., 2019; Renkewitz & Keiner, 2019) or capable to remedy all questionable research practices (Simonsohn et al., 2014).

The first set of analyses was directly based on the three-level model: First, we used publication status as a moderator to examine whether there is a systematic difference in the reported effect sizes. Second, we visually inspected contour-enhanced funnel plots for asymmetries that are likely indicative of publication bias: lack of non-significant findings to the left side of the meta-analytically estimated effect size and an overrepresentation of low-powered findings to its right side. Visual inspection, however, is inherently subjective in its interpretation. Moreover, because the plots do not consider the data's three-level-structure, clusters of data points and ensuing asymmetry may be mistaken as bias (Lau et al., 2006).

Third, to formally assess funnel plot asymmetry, we conducted Egger's test (with $p < .1$ as the critical value; Egger et al., 1997). This test evaluates whether the precision of the effect (i.e., the standard error) is associated with the size of the effect. We adapted it to the three-level meta-analytic model by adding random effects for between- and within-study variance to account for dependency and by including the precision of the effect as the informative moderator (Fernández-Castilla et al., 2019). A statistically significant slope indicates bias by suggesting that effect sizes are systematically different for high- vs. low precision studies. Fourth, we similarly interrogated the funnel plot using the Macaskill regression test that examines whether effect sizes can be regressed on sample sizes (Macaskill et al., 2001). Though this test has fairly low power, it typically outperforms Egger's test in controlling Type I error (Kromrey & Rendina-Gobioff, 2006; Macaskill et al., 2001).

A second set of analyses required that the included effect sizes are all independent (Assink & Wibbelink, 2016). Thus, they could not be based on our three-level model. For the following analyses, we ensure independence by aggregating all dependent effect sizes (via the agg function of the MAD package; del).

The first two methods examine the distribution of p -values. They are based on the rationale that the distribution of significant p -values contains more low ($p \leq .01$) than high ($p > .04$) p -values for a true effect that is not influenced by publication bias.

First, we applied *p*-curve (Simonsohn et al., 2014) via the *pcurve* function of the *dmatar* package (Harrer et al., 2019). We estimated the true effect (effect. estimation option set to TRUE) with the search space set between $g = .00$ and $g = 1.$. We report results for the “full” (for all *p*-values < .05) and “half” *p*-curve (for all *p*-values < .025). The half *p*-curve is more sensitive to stronger cases of *p*-hacking (i.e., the exploitation of researcher’s degrees of freedom to produce a significant *p*-value and subsequent selective reporting of this significant result), though at the cost of reduced statistical power (Simonsohn et al., 2015). A *p*-curve attempts to correct for publication bias by computing the probabilities of observing the significant reported effect sizes contingent on the meta-analytical effect size being the true underlying one. In the case of highly heterogeneous data, the estimates provided by *p*-curve should be considered estimates of the average true effect sizes of only the statistically significant set of studies (van Aert et al., 2016).

Second, we implemented *p*-uniform* (van Aert & van Assen, 2020) via the *puni_star* function of the *puniform* package (van Aert, 2019). This method, in contrast to *p*-curve, also takes nonsignificant effects into account and allows for a direct estimation and test of (full) between-study variance. It attempts to safeguard against the overestimation of the effect size that could arise from including significant effects only and, more generally, as a consequence of between-study variance (Carter et al., 2019; van Aert et al., 2016).

Finally, we created a funnel plot of the aggregated effect sizes and applied the trim-and-fill method (Duval & Tweedie, 2000). Trim-and-fill seeks to restore the symmetry of the funnel plot by imputing presumably missing effect size estimates. These are then entered into a new random-effects meta-analytical model. We note, however, the current debate regarding the validity of this method (Carter et al., 2019; Renkewitz & Keiner, 2019; van Aert et al., 2016), and suggest to interpret the results with caution (Sutton et al., 2000).

Results

Descriptive Statistics. The current meta-analysis included 48 articles that reported 58 independent studies and comprised a total of 174 effect sizes. They were published between 2010 and 202. The Appendix lists each effect size along with its sampling variance, sample size, moderator coding as well as the comparison that it was based on. Overall, the sample size was $N = 3,882$, with individual samples ranging from $n = 8$ (Cooper, 2013) to $n = 200$ (Bulley et al., 2019). Most articles examined monetary intertemporal choices only ($n = 32$, 67% of effect sizes), some included both monetary and health-relevant choices ($n = 13$, 27%), and a few were focused on health-relevant choices only ($n = 3$, 6%).

Overall Effect. Overall, we obtained a significant, medium-sized effect, $g = .439$, $SE = .053$, $t = 8.324$, $p < .001$, 95%

confidence interval (CI) = [.333, .545], with substantial overall heterogeneity, $Q_E(173) = 684.42$, $p < .001$, $I^2 = 79.21$, 95% CI [72.48, 85.23] (Fig. 2). Of our influence measures, only the standardized residuals indicated $n = 1$ effect size as a potential outlier. Removing this effect size (Snider et al., 2016) did not substantially affect our overall effect size estimate, $g = .430$, $SE = .052$, $t = 8.26$, $p < .001$, 95% confidence interval (CI) = [.325, .534], and retained substantial overall heterogeneity, $Q_E(173) = 663.97$, $p < .001$, $I^2 = 78.51$, 95% CI [71.53, 84.77].

Variance of the Overall Effect Size. We next sought to examine the distribution of the variance across the three levels of our meta-analytical model. The choice of this model was corroborated by an intraclass correlation coefficient of $\rho = .483$, indicating that the underlying true effects within reports were strongly correlated. Moreover, the variance was significant at both the within-study level (estimate = .083, $p < .001$) and the between-study level (estimate = .077, $p < .001$).

Follow-up analyses indicated that variance was distributed across the sampling, within-study, and between-study levels as 2.79%; 4.94%, 95% CI [26.47, 61.89] and 38.27%, 95% CI [16.34%, 78.47%], respectively. The low percentage of variance attributed to the samples further motivates the exploration of potential moderators (Hunter & Schmidt, 1990).

Moderator Analyses. An important goal of the present meta-analysis was the examination of possible core components of EFT that influence intertemporal choices. These moderator analyses were clearly further motivated by the substantial heterogeneity and low contribution of *samples* to the effect-size variance. Specifically, we report a series of moderator analyses that examined theoretically motivated candidate core components (Fig. 3) as well as additional, more procedural factors of the used study designs (Fig. 4). First, however, we examine whether EFT is differentially effective in influencing monetary vs. health-relevant intertemporal choices. All moderator results are further detailed in Table 2.

Choice Domain. The model for the moderator *choice domain* (monetary vs health-relevant), $AIC_w = 33.3\%$, $\chi^2(2) = .71$, $p = .401$, did not show a better fit to the data than the intercept-only model. Accordingly, the effect of choice domain was not significant, $F(1, 46) = .98$, $p = .328$, $Q_E(172) = 681.75$, $p < .001$. This analysis thus provides no evidence that EFT has a stronger impact on either kind of intertemporal choice. Indeed, consistent with our hypothesis, we obtained significant effect sizes for both monetary, $g = .462$, $t(46) = 7.83$, $p < .001$, and health-relevant choices, $g = .385$, $t(46) = 5.05$, $p < .001$.

Theoretically-Motivated Moderators.

(i) **Valence.** We had hypothesized that EFT would have the

Fig. 2. Forest plot on Aggregated Effect Sizes. Symbols indicate the respective effect size (Hedges' g). Larger symbols indicate smaller sampling variance (i.e., larger sample sizes). * indicate unpublished studies. Bars denote 95% Confidence Intervals.

strongest impact on intertemporal choices when the simulated episodes are positive, and possibly the weakest (or even reversed) impact when they are negative. Indeed, the model including the valence moderator showed a superior fit to the data, carrying approximately 100% of the cumulative model weight. It was significantly better than the intercept-only model, $\chi^2(3) = 29.21, p < .001$.

Consistent with these results, the main effect of valence was significant, $F(3, 44) = 3.02, p = .04, Q_E(170) = 555.73, p < .001$. As predicted, positive episodes yielded the largest and significant effect, $g = .52, t(44) = 8.79, p < .001$, followed by positive-to-neutral episodes, $g = .438, t(44) = 4.37, p < .001$, with only a non-significant trend for neutral episodes, $g = .184, t(44) = 1.83, p = .074$. Indeed, negative episodes were associated with the smallest, and non-significant effect, $g = -.179, t(44) = -.52, p = .607$.

Our predictions were further largely corroborated by the comparisons between the levels of the moderator. The effect for positive episodes was significantly larger than for neutral episodes, $\beta = .336, t(44) = 2.96, p = .005$, though only non-significantly larger than for negative episodes, $\beta = .699, t(44) = 1.94, p = .059$. Positive-to-neutral events yielded non-significant trends for a larger effect compared with neutral, β

$= .254, t(44) = 1.8, p = .08$, and negative episodes, $\beta = .617, t(44) = 1.71, p = .094$. For all other direct comparisons, t was smaller than 1.29, p greater than .2 (Fig. 3A).

(ii) Vividness. We next tested the hypothesis that imagined events should be particularly impactful when they are experienced as more vivid. We determined the difference in vividness between the EFT and respective control condition and used the effect sizes as a moderator. (Note that we could not compare this model with the intercept-only model, because it was only based on the subsample of studies that had provided vividness ratings.)

As predicted, the moderator analysis yielded the significant main effect of vividness, $F(1, 13) = 8.43, p = .012, Q_E(58) = 126.51, p < .001$. Across studies, a vividness increase of one standard deviation was associated with an increase of $\beta = .180, t(13) = 2.91, p = .012$, in the standardized mean difference (Fig. 3B).

(iii) Episodicity. We also examined whether the episodic nature of the future simulations constitutes a core component of EFT with respect to its influence on intertemporal choices. We performed a moderator analysis with two groups: One group included effect sizes that compared an EFT condition

Fig. 3. Effect Sizes (Hedges' g) of the Theoretically-Motivated Moderators. Each dot shows the observed effect size for a single study for the respective moderator level. Studies with larger samples (i.e., greater precision) are displayed with larger dots. Black squares indicate the mean, whiskers the standard error. The shape illustrates the distribution of effect sizes for the respective moderator level. Panel A: Valence. Panel B: Vividness. Panel C: Episodicity. Panel D: Future-orientedness. Panel E: Content specificity. Panel F: Choice impulsivity.

with control conditions that were closely matched in all aspects but the episodicity (i.e., studies requiring semantic future thinking). The other group included the effect sizes for comparisons with all other control conditions. The corresponding moderator model did not show a significantly better fit to the data than the intercept-only model, $AIC_w = 29.5\%$, $\chi^2(1) = .35$, $p = .552$, and there was accordingly no main effect of the moderator, $F(1, 46) = .77$, $p = .384$, $Q_E(172) = 678.38$, $p < .001$. Critically, however, we corroborated that we did not only obtain a significant effect for the more dis-

parate control conditions, $g = .433$, $t(44) = 8.10$, $p < .001$, but also for those that only differed from EFT in their lack of episodicity, $g = .553$, $t(44) = 4.15$, $p < .001$, $\beta = .12$, $t(45) = .88$, $p = .383$ (Fig. 3C).

(iv) Future-Orientedness. Analogously to the previous analysis, we sought to determine the contribution of the temporal orientation. We split the effect sizes based on whether the control condition was closely matched in all aspects (including the episodicity) but the future-orientedness

(e.g., episodic recent thinking) or whether the control conditions were more disparate. This model did not fit the data better than the intercept-only model, $AICw = 26.7\%$, $\chi^2(1) = .08$, $p = .779$ and there was no main effect of the moderator, $F(1, 46) = .07$, $p = .789$, $Q_E(172) = 663.1$, $p < .001$. Importantly though, we observed not only a significant effect for the more varied non-episodic control conditions, $g = .45$, $t(46) = 6.51$, $p < .001$, but also for those that were matched in all facets but the future-orientedness of the imagination, $g = .430$, $t(46) = 6.57$, $p < .001$, $\beta = -.02$, $t(45) = .27$, $p = .789$ (Fig. 3D).

(v) Content Specificity. We next tested the hypothesis that EFT exerts a stronger influence on intertemporal choices if the simulation directly incorporates the future pay-off (e.g., the enjoyment of the later reward). Indeed, the model for *content specificity* (content-specific vs general EFT) showed a good fit to the data, carrying 9.5% of the cumulative model weight, and outperformed the intercept-only model, $\chi^2(1) = 6.61$, $p = .01$. The moderator was significant, $F(1, 46) = 9.21$, $p = .004$, $Q_E(172) = 657.19$, $p < .001$, reflecting that the effect was stronger for content-specific, $g = .707$, $t(46) = 6.95$, $p < .001$, $\beta = .317$, $t(45) = 3.03$, $p = .004$ than general future simulations, $g = .391$, $t(44) = 7.46$, $p < .001$ (Fig. 3E).

(vi) Choice Impulsivity. We finally tested the hypothesis that the impact of EFT would be larger for individuals characterized by choice impulsivity than for neurotypical individuals. If the former tend to put less weight on the future consequences of their actions, they may particularly benefit from such an intervention. The moderator model did fit the data significantly better than the intercept-only model, $AICw = 76.8\%$, $\chi^2(1) = 4.49$, $p = .034$, and choice impulsivity had a significant main effect on the effect size, $F(1, 46) = 6.23$, $p = .016$, $Q_E(172) = 639.37$, $p < .001$. Even though neurotypical samples do benefit from EFT, $g = .367$, $t(44) = 6.44$, $p < .001$, the effect was significantly larger for individuals characterized by choice impulsivity, $g = .604$, $t(45) = 8.02$, $p < .001$, $\beta_1 = .237$, $t(45) = 2.5$, $p = .016$ (Fig. 3F).

However, we observed the effect of choice impulsivity across monetary and health-relevant choices (e.g., alcohol consumption). The latter choices were clearly more relevant for the impulsive groups (e.g., people with alcohol substance abuse) than for their respective controls. We wanted to establish whether this moderator influences both, pertinent health-relevant and also general monetary choices.

For health-relevant choices only, *choice impulsivity* was again a significant moderator, $F(1, 14) = 12.58$, $p = .003$, $Q_E(69) = 189.43$, $p < .001$. Samples characterized by choice impulsivity did make more health-beneficial choices following EFT, $g = .572$, $t(14) = 6.64$, $p < .001$, $\beta_1 = .445$, $t(14) = 3.55$, $p = .003$, whereas the neurotypical samples did not show such an effect, $g = .128$, $t(14) = 1.24$, $p = .22$.

For monetary choices only, *choice impulsivity* constituted just a marginal, non-significant moderator, $F(1, 43) = 3.67$, $p = .062$, $Q_E(101) = 424.17$, $p < .001$. This reflected

a non-significant trend for a greater effect in samples characterized by choice impulsivity, $g = .619$, $t(45) = 6.8$, $p < .001$, $\beta_1 = .21$, $t(42) = 1.91$, $p = .062$. For the monetary choices, the neurotypical individuals also showed a significant effect, $g = .41$, $t(42) = 6.32$, $p < .001$.

Procedural Moderators. In the following, we test possible moderators that reflect major procedural differences rather than theoretically relevant components of EFT.

(vii) Design. We tested whether effect sizes would differ for studies that had used between- vs. within-subject manipulations. The model for design, $AICw = 72.2\%$, $\chi^2(1) = 4.0$, $p = .045$ showed a significantly better fit to the data than the intercept-only model, though the moderator was just not significant, $F(1, 46) = 3.8$, $p = .057$, $Q_E(172) = 621.71$, $p < .001$. There was a non-significant trend for effect sizes from between-subjects designs, $g = .509$, $t(46) = 7.26$, $p < .001$ to exceed those from within-subjects designs, $g = .319$, $t(46) = 4.89$, $p < .001$, $\beta_1 = .19$, $t(45) = 1.95$, $p = .057$ (Fig. 4A).

(viii) Study Site. The model comparing experiments conducted in person vs. online did not fit the data better than the intercept-only model, $AICw = 28.4\%$, $\chi^2(1) = .24$, $p = .623$, and was not a significant moderator, $F(1, 46) = 1.02$, $p = .278$, $Q_E(172) = 678.74$, $p < .001$. Both in-person experiments, $g = .451$, $t(46) = 7.50$, $p < .001$, and experiments conducted online, $g = .377$, $t(46) = 8.27$, $p < .001$ yielded significant effects (Fig. 4B).

The on-site studies included a number of experiments on health-relevant choices that seemed particularly ecologically valid ($n = 7$), either because they were conducted in the field or measured choices with immediate consequences (e.g., caloric intake at an ad libitum buffet). We performed an additional exploratory analysis on this subset of studies, which also yielded a significant effect, $\beta = .603$, $t(45) = 4.83$, $p < .001$.

Publication Status. The model that differentiated between published and unpublished studies did not fit the data better than the intercept-only model, $AICw = 38.5\%$, $\chi^2(1) = 1.16$, $p = .282$ – consistently, publication status was also not a significant moderator, $F(1, 46) = .55$, $p = .463$, $Q_E(172) = 639.16$, $p < .001$ (Fig. 4C). These data do not provide evidence for a selective reporting of results based on the magnitude of the effect size. However, on their own, only published, $g = .457$, $t(45) = 9.37$, $p < .001$, but not unpublished studies, $g = .307$, $t(45) = 1.53$, $p = .132$, did show a significant effect.

Multiple Moderator Model. We finally fit a multiple moderator model that included all significant moderators (except for the vividness moderator that was fit on only a subset of the data). As summarized in Table 3, the omnibus test was significant, $F(5, 42) = 3.62$, $p = .008$, $Q_E(166) = 538.22$, $p < .001$, suggesting that at least one of the regression coefficients of the moderators deviated significantly from zero.

Fig. 4. Effect Sizes (Hedges' g) of the Procedural Moderators. Each dot shows the observed effect size for a single study for the respective moderator level. Studies with larger samples (i.e., greater precision) are displayed with larger dots. Black squares indicate the mean, whiskers the standard error. The shape illustrates the distribution of effect sizes for the respective moderator level. Panel A: Design, Panel B: Study site, Panel C: Publication status.

Accounting for multicollinearity (Hox, 2010), these findings revealed that content-specific (vs. general) EFT had a significant unique moderating effect, $\beta = .243$, $t(42) = 2.12$, $p = .040$, as had positive (vs. neutral) episodes, $\beta = .31$, $t(42) = 2.56$, $p = .014$.

Assessment of Publication Bias. We sought to minimize the impact of publication bias on our meta-analytical results by including studies that had not been published in peer-reviewed journals. The analysis provided no evidence for a moderating effect of publication status. However, we used various complementary methods to further assess potential publication bias. (See online supplemental materials for bias corrections separately for monetary and health-related choices.)

Assessment on the Three-Level Model. The first set of analyses was based on a funnel plot of our main analytical approach, that is, the three-level model. Egger's regression test provided evidence for significant asymmetry, $F(1, 46) = 27.64$, $p < .001$, indicating publication bias. By contrast, this was not the case for the Macaskill regression test, $F(1, 46) = .64$, $p = .43$.

Assessment on Aggregated Effect Sizes. The second set of analyses was based on aggregated effect sizes. Visual inspection of the funnel plot suggested publication bias (Fig. 5A). We followed-up on this impression using various formal statistical tests:

p-Curve. If there is evidential value in the pattern of p -values, the corresponding p -curve should be right-skewed (i.e., toward values $\leq .01$; Fig. 5B). Indeed, both the tests for the half (i.e., $p < .025$; $p_{Half} < .001$) and full p -curve (i.e., $p < .05$, $p_{Full} < .001$) were significant. Moreover, the p -curve was also not flat (tests for flatness: $p_{Full} = 1.0$, $p_{Binomial} = 1.0$). The power estimate of the examined studies was high (87%, 95% CI [76.7% - 93.1%]) and the true effect size was estimated as $g = .677$. However, given the significant heterogeneity ($> 50\%$), this estimate may not be considered trust-

worthy (Harrer et al., 2019; van Aert et al., 2016).

p-uniform*. P -uniform* indicated significant between-study variance, $\tau^2 = .076$, 95% CI [.035, .150], $L = 35.42$, $p < .001$, though the publication bias test was not significant, $L = 2.42$, $p = .298$. This approach does not provide evidence for the presence of publication bias. p -uniform* estimated the effect size as $g = .427$ [.268, .579], $L = 16.25$, $p < .001$.

Trim-and-fill. The trim-and-fill procedure (Duval & Tweedie, 2000) evens out the symmetry of the funnel plot by imputing additional values and trimming the values of extreme observations. It estimated that 13 studies were missing to achieve symmetry (Fig. 5A). However, following adjustment for this putative bias, the newly estimated meta-analytic effect size remained significant at $g = .299$, 95% CI [.166, .431].

Summary. The methods did not all converge with respect to the question whether our results are still affected by publication bias following the inclusion of the identified unpublished results. Egger's test and trim-and-fill did indicate publication bias, whereas this was not the case for the moderator analysis, the Macaskill regression test, as well as p -curve and p -uniform*. Critically, however, the impact of EFT on intertemporal choices remained significant following all attempts to adjust for publication bias.

Discussion

Our pervasive tendency to devalue future rewards often leads to myopic decisions. We forego larger amounts of money in favor of smaller short term gains or seek the instant pleasures of rich desserts at the cost of our long-term well-being. We here performed the first comprehensive and quantitative analysis of the hypothesis that episodic future thinking can positively influence such intertemporal choices. Specifically, in a three-level model, we meta-analyzed the rich literature that has accumulated over the last decade with a total of 174 effect sizes. Our results indicate that EFT has a statistically significant, medium-sized effect on both monetary and health-

Fig. 5. Assessing Publication Bias on Aggregated Effect Sizes. Panel A: Funnel plot of effect size estimates and standard errors for aggregated effect sizes. Solid black dots represent effect sizes included in the analysis and empty white dots represent studies imputed via trim-and-fill (Duval & Tweedie, 2000). The dashed line shows the trim-and-fill adjusted effect size. The left funnel is centered at 0, which is the value under the null hypothesis of no effect, whereas the right funnel is centered at the model estimate under the null hypothesis that the meta-analytic model estimate is true. Contours represent 95% confidence intervals. The dashed red line shows the regression line from Egger's test. Panel B: Observed p -curve of all articles, including 30 statistically significant ($p < .05$) results, of which 29 had a p -value $< .025$. An additional 18 results were excluded from the p -curve because they had p -values $> .05$.

relevant intertemporal choices. Notably, 16% of our effect sizes stemmed from unpublished articles and the effect stayed significant following further attempts to adjust for remaining publication bias. Overall, this meta-analysis corroborates that EFT promotes farsighted decisions.

The meta-analytical model also yielded significant and large heterogeneity in the effect sizes, hinting at meaningful experimental differences that do not reflect simple sampling error. We exploited the heterogeneity in a number of moderator analyses. These assessed critical core components of EFT that may be instrumental in influencing intertemporal choices as well as more procedural study differences.

Core Components of Episodic Future Thoughts.

Valence. The impact of EFT varied considerably with the valence of the prospective episode. As hypothesized, we obtained a graded pattern, with the largest beneficial effect for positive events, followed by positive-to-neutral events. Neutral episodes only yielded a trend and negative episodes were also not significant.

On the one hand, EFT acts as an “affective forecast” (Gilbert & Wilson, 2007) that conveys the anticipated positive affect that the delayed reward would hold. This *anticipated affect* may add to the valuation of the delayed reward and effectively attenuate its discounting (Benoit et al., 2018; Berns et al., 2007; Boyer, 2008; Bulley et al., 2016; Bulley & Schacter, 2020). EFT can make the reward feel worth the wait. On the other hand, EFT further conveys utility to the wait itself by also eliciting *anticipatory affect*. For example, there is evidence that people opt to defer events that they deem particularly pleasurable, such as a promised kiss from a movie star – presumably because they also cherish the prospect in and of itself (Loewenstein, 1987).

EFT encourages individuals to organize their behavior in accordance with anticipated and anticipatory positive emo-

tions (Baumgartner et al., 2008; Lempert et al., 2016). However, given that the meta-analysis did not yield a significant effect for the few studies instructing for negative EFT, the data did not support the account that imagining a negative future tethers individuals to the immediate present (Liu et al., 2013).

Vividness. The experiments in which participants imagined the events more vividly also yielded the greatest impact of EFT on intertemporal choices. However, this analysis was based on only 31% of the articles (34% of effect sizes) that had reported vividness ratings for the EFT and control conditions. Though inferences from across-sample correlations are susceptible to ecological fallacy, this finding is consistent with a few studies that had reported such associations across subjects (Ciaramelli et al., 2019; Peters & Büchel, 2010).

As an additional caveat, we note that the majority of the studies had assessed the vividness of the episodes not during the actual EFT task but during a preceding stage when participants generated the episodes that they would then imagine. Future studies may consider including ratings immediately after the respective EFT trials, both to ensure a valid measurement and to enable the assessment of within-subject associations between vividness and choice behavior.

There are a number of ways in which vividness could influence the impact of EFT. First, all of the included studies required participants to imagine positive future episodes. A greater vividness may have boosted the effect by eliciting a stronger positive affect (D'Argembeau & Van der Linden, 2012; Holmes & Mathews, 2010; Pearson et al., 2015; Schubert et al., 2020). Second, imagining an event with greater vividness makes it seem more certain to occur (Gregory et al., 1982; Sherman et al., 1985; Szpunar & Schacter, 2013; J. Q. Wu et al., 2015). As a consequence, the delayed reward would seem more likely to manifest and accordingly constitute a safer bet (Bulley et al., 2016). Finally, more vivid EFT

imbues the future option with more concrete episodic details, and leads to a construal at a level that typically characterizes greater immediacy (D'Argembeau & van der Linden, 2004; Lempert & Phelps, 2016; Trope & Liberman, 2010). EFT may effectively make the future appear closer in time.

Content Specificity. The analysis revealed that the content of the future imagining is a key feature determining the impact of EFT on DD. This moderator provided a unique effect that was not accounted for by either the valence or vividness of the simulation. Specifically, the influence of EFT was greater when the imagined content was closely tied to the nature of the delayed choice option. This could either entail the future moment of spending the monetary reward (e.g., Benoit et al., 2011; Palombo et al., 2015) or the direct consequences of avoiding behavior that would be detrimental to one's health (e.g., positive life events after successfully quitting smoking; Chiou & W.-H. Wu, 2017).

The content specificity of EFT seems particularly important for the latter kind of choices, where the adverse long-term consequences of impulsive decisions can seem fairly intangible (e.g., protracted health issues). More generally, EFT has been argued to foster farsighted decisions by supporting a model-based choice process that involves the mental construction of the context in which the delayed reward would be delivered (Kurth-Nelson et al., 2012). This process requires a correspondence between the specific content of the imagination and the nature of the delayed choice option.

Episodicity. Our analysis also yielded a significant effect for the small subsample of studies whose control conditions were tightly matched in all aspects but the imaginings' episodicity. Indeed, the moderator analysis did not provide evidence that their effect size differed from studies that used looser control conditions. This pattern indicates that the episodic nature of EFT contributes to its effect beyond what is provided by semantic future thinking (Atance & O'Neill, 2001; Szpunar et al., 2014).

This finding is consistent with reports that amnesic patients do not additionally profit from cues prompting them to engage in EFT (Lebreton et al., 2013; Palombo et al., 2015) even though they can attenuate DD via a more semantic mechanism that is also directed toward the future (Palombo et al., 2016). This is particularly the case when lesions in the medial temporal lobes are more extensive and when there is little residual EFT capacity (Kwan et al., 2015). EFT does not merely influence intertemporal choices by inducing a generic future orientation (Lin & Epstein, 2014). Instead, the data indicate that the autonoetic experience of a future episode constitutes a critical component (Benoit et al., 2018).

Future-Orientedness. Complementary to the previous section, we also observed that EFT nudges toward farsighted choices when the control condition is matched on all aspects but the temporal direction. That is, EFT yielded an effect even when compared with other episodic conditions that were directed toward the distant (Banes, 2016; Ciaramelli et al.,

2019) or recent past (Daniel et al., 2013; Rung & Madden, 2019) or towards the present (Yi, 2020). Moreover, the moderator analysis did not indicate that the effect sizes were smaller than when the control conditions also diverged in other aspects from the EFT conditions.

These findings dovetail with the observed effect on content specificity, in the sense that an orientation towards the future also aligns the imagined episode closer with the potential moment of receiving the later reward. The temporal direction may constitute a core component of EFT that shifts attention toward a delayed choice option (Lin & Epstein, 2014) and provides motivational incentives for the pursuit of long-term goals (Dreves & Blackhart, 2019).

Interim conclusion: The Case for Episodic Future Thinking. Episodic future thinking has been hypothesized to promote farsighted decisions by mediating the mental experience of what it would feel like to receive a delayed reward. It would render the future more tangible and convey the affective experience associated with the delayed reward (Boyer, 2008; Benoit et al., 2019). Our analysis corroborated that conditions entailing EFT indeed promote more farsighted decisions. However, prior work had provided little evidence whether imagining the specific episode of receiving the delayed reward adds anything to intertemporal decisions in comparison to other modes of future-oriented cognition. For example, simply constructing a representation of a general future state of affairs (i.e., semantic future thinking; Atance & O'Neill, 2001; Szpunar et al., 2014; Suddendorf & Corballis, 2007) may be sufficient in inducing more farsighted decisions (Kwan et al., 2015; Palombo et al., 2016).

The foregoing moderator analyses systematically deconstructed the contribution of individual core components that are integral to EFT. First, merely adding the requirement to imagine a specific episode (i.e., the episodicity) to an otherwise comparable control condition rendered decisions more farsighted. Second, this was particularly the case when the episodes were imagined more vividly. These findings indicate that the impact of EFT is unlikely to be simply accounted for by a stronger general consideration of the future as may be induced by semantic modes of future-oriented cognition. Third, EFT yielded a significant effect when compared with other conditions that were also episodic in nature but lacked the temporal alignment with the delayed reward. Fourth, EFT was more effective when the future simulation was content specific. These two latter points suggest that mentally experiencing the future benefit of the delayed reward contributes to the effect of EFT. Finally, the impact of EFT was greatest when it entailed the simulation of a positive event, highlighting the contribution of the experienced anticipated affect.

Together, these moderator analyses indicate that the impact of EFT is unlikely to be accounted for by other modes of future-oriented cognition. Other processes, for example, semantic future thinking, may also nudge towards more farsighted decisions (Kwan et al., 2015; Palombo et al., 2016).

However, the analyses suggest that subtracting a core component that is integral to EFT would reduce the effectiveness of such interventions. We take this pattern to support the hypothesis that EFT influences intertemporal choices by allowing us to mentally experience the benefits of the future that arise from foregoing the smaller temptations of the present (Boyer, 2008; Benoit et al., 2019).

Intertemporal Choice and Impulsivity: Clinical Implications. We also established that the effect of EFT was particularly strong in groups characterized by choice impulsivity (Hamilton et al., 2015). These included individuals with obesity and substance dependences (e.g., nicotine addiction). The effect in these groups surpassed the one in neurotypical individuals. This pattern is consistent with a previous observation in neurotypical people, albeit in a small sample, that EFT yields a greater benefit for those who are usually less prone to consider the future consequences of their actions (Benoit et al., 2011).

The effect of choice impulsivity was particularly pronounced for health-relevant decisions. This may simply reflect the more limited relevance of these decisions for the respective neurotypical control samples. For example, reducing calories intake would clearly have a greater importance for individuals with obesity than for people with normal weight. In fact, neurotypicals make, by definition, less impulsive choices, leaving less room for EFT to nudge them toward more farsighted decisions. It is thus particularly noteworthy that we observed a similar trend for monetary choices that are of comparable relevance for either group.

Choice impulsivity has been proposed to constitute a critical trans-disease process (Amlung et al., 2019; Hamilton et al., 2015). Its contribution to various psychological disorders highlights the potential of creating interventions that may render individuals less impulsive (Levens et al., 2019; Peters & Büchel, 2011; Rung & Madden, 018b). Accordingly, a number of behavioral trainings are currently being trialed for reducing impulsivity (Scholten et al., 2019; Ince et al., 2021). Our results corroborate that EFT constitutes a candidate mechanism for altering maladaptive, impulsive behavior and suggest that it may be suitable for inclusion in future trials (e.g., Patel & Amlung, 2020). Specifically, our analysis highlights the effectiveness of positive future imaginings. Targeted attempts to foster such EFT may particularly benefit individuals who tend to have a less positive imagination of the future as is often the case in depression (Gamble et al., 2019, 2021; see also Ji et al., 2017; Renner et al., 2017). A similar problem may arise for other groups of people that are deficient in EFT, such as long-term opiate users (Mercuri et al., 2015).

Procedural Moderators. We further conducted a number of moderator analyses targeted at salient study differences. There was no difference in effect sizes for studies that had been conducted *in person* (e.g., the laboratory) vs. *online*

(e.g. via MTurk). This result is promising, given the potential of online experiments to include more diverse and more representative samples (Paolacci & Chandler, 2014). However, we caution that not all core components may be equally amenable to online experiments. For example, given the typically shorter time frame, it seems more difficult to implement *within-subject* designs with subtle manipulations such as episodic vs. semantic future thinking. Of note, we also obtained a significant effect when focusing only on those studies that had examined health-relevant choices in more naturalistic settings (e.g., dietary choices in a food court). This finding highlights the translational potential of EFT.

Notably, the model including between- vs. within-subject design could account better for the data than the model with only the intercept. Between-subject designs trended to yield greater effect sizes than within-subject designs. This is noteworthy, given that the former generally tend to be less powerful (Lakens, 2013). That finding highlights the difficulties inherent to within-subject designs (Greenwald, 1976) where the effect of EFT may carry over into the control condition. This is particularly the case when both conditions repeat the identical intertemporal choices.

Caveats.

Influence of Demand Characteristics? To ascertain that EFT constitutes a viable option for affecting intertemporal choices, it is critical to understand whether the observed effects may simply reflect demand characteristics (Orme, 1962). Indeed, it has been shown that, under certain circumstances, participants can deduce the experimenter's intention from the instructions (Rung & Madden, 018a; Stein et al., 2018). This is particularly the case when only the experimental condition requires considering the future (Rung & Madden, 018a). Accounting for such insight, however, did not alter the results (Stein et al., 2018).

Notably, a recent study compared EFT with a control condition that was carefully constructed to induce a greater demand effect (Rung & Madden, 2019). Yet, the EFT condition did show a stronger beneficial impact on intertemporal choices. EFT has also been shown to alter choices in ecologically more valid settings that are presumably less susceptible to demand effects. For example, such studies assessed, as dependent measures, calories intake in a food court (O'Neill et al., 2016) and real-life weight loss (O'Neill et al., 2016). Indeed, as described above, we obtained a significant effect of EFT when we just focused on these studies with particular high ecological validity. Moreover, our moderator analysis indicates that EFT reduces discounting even when compared with semantic future thinking, a condition that is presumably well matched in terms of demand characteristics (e.g., Benoit et al., 2011; for a discussion, see Rung & Madden, 018a).

To conclude, in situations where the hypothesis is likely to be derived and the stakes for the participants are low, it may be beneficial to assess awareness for the hypothesis or to include scales for socially-desirable behavior that can serve

as covariates (Rung & Madden, 2018b). However, based on our moderator analyses and the aforementioned studies (Stein et al., 2018; Rung & Madden, 2019), we propose that the impact of EFT is unlikely reducible to demand characteristics.

EFT may not always be adaptive. Though exaggerated discounting is clearly maladaptive (Amlung et al., 2019), there are several reasons why it is not always beneficial to nudge decisions via EFT. First, given the inherent uncertainty of the future, choices focusing on the immediate present are not always myopic (Hayden, 2016). It is, for example, sometimes better to choose a smaller but certain reward over a larger one that may not actually materialize (Bulley et al., 2016).

Second, our analysis indicates that the impact of EFT is also based on an “affective forecast” of the delayed choice option (Benoit et al., 2019; Boyer, 2008). However, these forecasts are often not very accurate. For example, there is a tendency to overestimate the intensity and duration of positive reward-related emotions (Bulley et al., 2017; Wilson & Gilbert, 2005), suggesting that we may, at times, overvalue the future.

Third, the analysis corroborates that EFT fosters farsighted choices that can immediately be acted upon (i.e., by foregoing an immediate reward). However, long lasting behavior change may additionally require the identification of possible obstacles (Oettingen & Gollwitzer, 2018; Oettingen & Reininger, 2016). This research indeed indicates that merely fantasizing about a positive future event can make it less likely that people then work toward achieving that goal.

Finally, not all people may equally profit from EFT. Although our data show that individuals with greater choice impulsivity benefit from this mechanism, this may not be the case for people with emotional disorders such as anxiety or depression. They often find it more difficult to conjure positive imaginings of the future (Gamble et al., 2019), suggesting that they would also struggle in creating a positive mental image of the delayed reward (but see also Ji et al., 2017; Renner et al., 2017).

Publication Bias. A major strength of this meta-analysis is its analytical approach. In our three-level model, we comprehensively included all effect sizes, avoiding selection bias (Cheung, 2019), and accounted for dependency with robust variance estimation (Hedges et al., 2010). In addition, we made efforts to account for publication bias. We carefully searched for unpublished experiments, resulting in 16

We further used various methods to adjust for remaining publication bias, given that there are issues with virtually any one of them (Carter et al., 2019; Renkewitz & Keiner, 2019). Though they were somewhat inconclusive with regards to the question whether the study pool is still affected by bias, all adjustments revealed significant effect size estimates (ranging from $g = .299$ following trim-and-fill to $g = .677$ according to p -curve). In the absence of any preregistered, large-

scale replication studies, we suggest that this meta-analysis currently provides the best available evidence for the impact of EFT on intertemporal choices.

Conclusions. The current meta-analysis provides a comprehensive and quantitative synthesis of EFT and its influence on farsighted decisions. Our moderator analyses highlight critical candidate core components of EFT and suggest avenues for future research aimed at delineating their interactions and unique contributions. By showing that EFT is particularly effective in nudging individuals characterized by choice impulsivity, we corroborate that it may be a helpful mechanism for optimizing future-oriented decisions. We further envision that EFT may have a similar impact in other domains that inherently entail a tradeoff with the future, including societal challenges such as promoting more sustainable behavior (Bø & Wolff, 2020; Lee et al., 2020b; Williams & Benoit, 2021).

Context. Following the seminal contribution by Peters & Büchel, 2010 and our publication (Benoit et al., 2011), we were excited to witness the accumulation of research into the impact of EFT on intertemporal choice. A particular important development was the generalization of this work from the delay discounting of monetary rewards to the examination of health-relevant choices (Daniel et al., 2013). Given the progress of this field and its translational potential (e.g., Stein et al., 2016), we found it prudent to take stock of the extant literature. In particular, we saw the opportunity to exploit the heterogeneity of the experimental protocols across studies to identify critical core components of EFT. We suggest that the current meta-analysis does not only corroborate the impact of EFT on intertemporal choice, but also informs future research into the mechanisms by which EFT promotes more farsighted decisions.

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MAAd: Meta-analysis with mean differences.

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Table 1. Moderator Coding Criteria

Table 1

Moderator Coding Criteria

Moderator and levels		Criteria	Example
Choice domain			
Monetary		Any measure of monetary delay discounting (e.g., AUC, k -values)	"[...] Degree of delay discounting was quantified as the adjusted immediate amount that would appear on the seventh trial if one was programmed to occur (i.e., the indifference point). [...]" (Rung & Madden, 2019)
Health-relevant		Either food (e.g., energy intake), smoking (e.g., demand intensity), or alcohol-related measures (e.g., demand intensity)	"[...] to select eight foods they would hypothetically like to consume from a grid of 16 food images (eight healthy; eight unhealthy) [...]" (Kakoschke et al., 2018)
Valence			
Positive		Assignment was based on description in methods if available. Otherwise, assignment was based on ratings or on provided examples. Only conditions that were explicitly deemed positive were assigned to this level. When it was ambiguous whether all episodes were likely positive (i.e., some may have been neutral), they were assigned to the positive-to-neutral level.	"[...] Parents and children in the EFT condition generated audio recordings of positive events they were looking forward to and could vividly imagine. [...]" (Sze et al., 2015)
Positive-to-neutral			"[...] to promote the vivid simulation of events alternative to the perceptual present, we encouraged participants to remember or imagine something positive or neutral in nature, which they would feel comfortable re-/pre-/experiencing in detail. [...]" (Ciaramelli et al., 2019)

Table 1 continued

Moderator and levels		Criteria	Example
Neutral			“[...] participants were instructed to imagine, as vividly as possible, and to list a series of personal events with a positive, negative or neutral emotional valence that were likely to happen around those time intervals. [...]” (Calluso et al., 2019)
	Negative		“[...] The neutral events were everyday activities [...]” (Bulley et al., 2019) „[...] a negative event was defined as an experience they would prefer to avoid [...]” (Zhang et al., 2018)
Vividness		Either vividness or imagery score or any composite score of these scores. The standardized mean difference or standardized mean change between the EFT and control group was derived as a continuous moderator.	“[...] Participants also rated the frequency and vividness of the episodic associations evoked by each event on a scale of 1–6 (1 for not at all to 6 for very high) [...] (Daniel et al., 2013b)
Episodicity			
Control: others		All other control conditions.	“ [...] participants first completed the intertemporal task for 96 trials in control condition, in which they did not image future events [...]” (Liu et al., 2013)
Control: Semantic future		The control task also required participants to consider the future but in a non-episodic fashion.	“[...] participants under the SFT condition were instructed to write about only three events related to aspects of their ideal selves (e.g., physical, social, moral, and psychological) that would occur within 1 year. Therefore, participants in the SFT group generated representations

Table 1 continued

Moderator and levels		Criteria	Example
			of only their ideal selves. They did not engage in prospective imagery to mentally pre-experience the life events of the ideal self. [...]”(Wu et al., 2017)
Future-orientedness			
Control: others		All other control conditions.	“[...] The No Cue version of the task was administered at the beginning of the assessment visit and served as the control discounting task for each manipulation. [...]” (Mellis et al., 2019)
Control: non-future, episodic		The control task also entailed simulations of episode that, however, did not take place in the possible future.	“[...] The ERT groups were asked to list positive and enjoyable events that took place within the last 60 h [...]” (O’Donnell et al., 2019)
Content specificity			
Content-specific		Any episode that involved spending the reward (i.e., the money)	“[...] Participants either imagined specific episodes of spending money (e.g., £35 in 180 days at a pub) [...]” (Benoit et al., 2011)
		Any episode that included behaviors instrumental to achieve a health-related goal or that entailed the consequences of its achievement	“[...] Participants were instructed to write about three positive life events that would occur in 1 year’s time given the condition “if you successfully quit smoking now.” [...]” (Chiou & Wu, 2017)

Table 1 continued

Moderator and levels	Criteria	Example
Non content-specific	Any general type of EFT unrelated to the reward	
Choice impulsivity		
Individuals with choice impulsivity	Individuals with overweight or obesity and those with an addiction to nicotine or other substances	"[...] Participants who reported drinking in the past 6 months and reported at least 3 of the 7 dependence criteria outlined in the DSM-IV-TR were eligible [...]" (Snider et al., 2016)
Neurotypical individuals	All other samples	"[...] Exclusion criteria included medications or conditions associated with changes in appetite, working memory or inhibitory control (e.g. methylphenidate and attention-deficit hyperactivity disorder), current tobacco use, diabetes, current or past history of a psychiatric disorder (e.g. anxiety or depression), current dieting and pregnancy. [...]" (Lin & Epstein, 2014)
Design		
Between-subjects	Effect size estimates from independent groups	"[...] Participants were randomly assigned to an episodic-future thinking ($n = 14$) or control-episodic-thinking ($n = 12$) condition [...]" (Daniel et al., 2013a, p. 201)
Within-subjects	Effect size estimates from repeated measures	"[...] At least 1 month after participants completed the standard discounting task, they performed a second intertemporal choice task that included personal future event cues. [...]" (Kwan et al., 2015)

Table 1 continued

Moderator and levels	Criteria	Example
Publication status		
Unpublished	Dissertations, unpublished data, in-prep manuscripts	
Published	Peer-reviewed articles	
Study site		
On-site	Any study conducted in person (mostly in-lab)	"[...]The design and procedure of the fMRI discounting task [...]" (Sasse et al., 2015)
Online	Any study not conducted in person (e.g., via crowdsourcing platforms such as MTurk).	"[...] We recruited participants ($N=117$) from Amazon Mechanical Turk, a crowdsourcing platform allowing individuals to complete brief Human Intelligence Tasks (HITs) to earn monetary compensation. [...]" (Stein et al., 2018)

Note. AUC = area under the curve; EFT = episodic future thinking; DSM-IV-TR = *Diagnostic and Statistical Manual of Mental Disorders–Fourth Edition–Text Revised*; MTurk = Amazon Mechanical Turk.

Table 2. Moderator Analyses

Table 2

Moderator Analyses

Moderator and level	ES	s	g	95% CI	SE	F-value ^a	p-value ^a	t-value ^b	p-value ^b	Level 2 variance	Level 3 variance	Q _E (df)	p-value	I ²
Choice domain						F(1, 46) = 0.98	.328			.081	.082	681.75 (172)	< .001	79.49 [72.73, 85.49]
Monetary	103	54	0.462	[0.343, 0.58]	0.06			7.83	< .001					
Health-relevant	71	18	0.385	[0.231, 0.538]	0.08			5.05	< .001					
Intelligence ^c						F(3, 44) = 3.02	.04			.061	.064	555.73 (170)	< .001	74.64 [65.88 82.49]
Negative	8	4	-0.178	[-0.877, 0.518]	0.33			-0.518	.607					
Neutral	26	10	0.184	[-0.018, 0.385]	0.1			1.83	.074					
Positive-to-neutral	11	7	0.438	[0.236, 0.639]	0.1			4.37	< .001					
Positive	129	42	0.52	[0.4, 0.639]	0.06			8.79	< .001					
Vividness ^d	60	15				F(1, 13) = 8.43	.012	2.91	.012	.013	.069	126.51 (58)	< .001	62.99 [34.55, 83.55]

Table 2 continued

Moderator and level	ES	s	g	95% CI	SE	F-value ^a	p-value ^a	t-value ^b	p-value ^b	Level 2 variance	Level 3 variance	Q _L (df)	p-value	I ²
Control: others	168	56	0.433	[0.324, 0.542]	0.054			8.0	< .001					
Control: Semantic future	6	4	0.553	[0.285, 0.821]	0.133			4.15	< .001					
Future-orientedness						F(1, 46) = 9.21	.789			.084	.076	663.1 (172)	< .001	79.22 [72.34; 85.4]
Control: others	90	35	0.43	[0.307, 0.553]	0.061			6.51	< .001					
Control: non future, episodic	84	28	0.45	[0.312, 0.587]	0.068			6.57	< .001					
Content specificity						F(1, 46) = 9.21	.004			.084	.064	657.19 (172)	< .001	77.9 [70.89, 84.23]
Non content-specific	141	53	0.391	[0.285, 0.821]	0.13			7.46	< .001					
Content-specific	33	12	0.707	[0.502, 0.912]	0.11			6.95	< .001					
Choice impulsivity						F(1, 46) = 6.23	.016			.086	.059	639.37 (172)	< .001	77.51 [70.36, 84.03]
Neurotypical	108	44	0.367	[0.246, 0.487]	0.060			6.12	< .001					

Table 2 continued

Moderator and level	ES	s	g	95% CI	SE	F-value ^a	p-value ^a	t-value ^b	p-value ^b	Level 2 variance	Level 3 variance	Q _L (df)	p-value	I ²
Choice impulsivity	66	15	0.604	[0.452, 0.755]	0.075			8.02	< .001					
Design						F(1, 46) = 3.8	.057			.088	.055	621.71 (172)	< .001	77.19 [69.97, 83.75]
Between-subject	116	33	0.509	[0.368, 0.651]	0.07			7.26	< .001					
Within-subjects	58	27	0.319	[0.188, 0.45]	0.065			4.89	< .001					
Study site						F(1, 46) = 1.2	.278			.082	.081	678.74 (172)	< .001	79.56 [77.84, 85.54]
On-site	144	50	0.451	[0.33, 0.572]	0.06			7.50	< .001					
Online	30	8	0.377	[0.285, 0.469]	0.046			8.27	< .001					
Publication status						F(1, 46) = 0.55	.463			.085	.071	639.16 (172)	< .001	78.72 [71.71, 85.02]
Unpublished	27	11	0.307	[-0.096, 0.71]	0.2			1.53	0.132					

Table 2 continued

Moderator and level	ES	s	g	95% CI	SE	F-value ^a	p-value ^a	t-value ^b	p-value ^b	Level 2 variance	Level 3 variance	Q _L (df)	p-value	I ²
Published	147	49	0.457	[0.359, 0.556]	0.049			9.37	< .001					

Note. ES = number of effect size estimates; s = number of studies; F-value = test of moderator significance; g = Hedges' standardized difference on each level of the moderator; 95% CI correspond to the CIs of Hedges' g on each level, p corresponds to the p-value for the omnibus test for the F-value or to the significance on the level of the moderator, respectively. The number of effect size estimates and studies often do not add up as expected because some studies provided multiple effect size estimates and/or did not provide data for the level of a moderator.

^a Omnibus test. ^b Test for the coefficient. ^c Follow-up-analyses showed that positive episodes have a significantly stronger beneficial effect as compared to neutral episodes, $\beta = 0.336$, $t(44) = 2.96$, $p = .005$, and a marginally more beneficial effect relative to negative episodes, $\beta = 0.699$, $t(44) = 1.94$, $p = .059$. Positive-to-neutral episodes were only associated with a marginally greater effect than neutral, $\beta = 0.254$, $t(44) = 1.8$, $p = .08$, and negative episodes, $\beta = 0.617$, $t(44) = 1.71$, $p = .094$. None of the other direct comparisons was significant or showed a trend, all $t(44) < 1.29$, all $p > .2$. ^d Test for coefficient of an increase in 1 SD in vividness. Other differences between the moderator levels were not significant.

Table 3. Results for the Multiple Moderator Model**Table 3***Results of the Multiple Moderator Model*

Moderator variables	β (SE)	95% CI	<i>p</i> -value	<i>t</i> -value
Intercept	0.16 (0.10)	[-0.04, 0.35]	.113	1.62
Negative episode (vs. neutral)	-0.37 (0.29)	[-0.95, 0.22]	.206	-1.28
Positive-to-neutral episode (vs. neutral)	0.24 (0.15)	[-0.06, 0.54]	.120	1.59
Positive episode (vs. neutral)	0.31 (0.12)	[0.07, 0.55]	.014	2.56
Content-specific episodes (vs. non content-specific)	0.24 (0.12)	[0.01, 0.48]	.040	2.12
Neurotypical sample (vs. sample characterized by choice impulsivity)	0.07 (0.10)	[-0.14, 0.28]	.517	0.07
Omnibus test	$F(5, 42) = 3.63, p = .008$			
Variance within studies	0.061			
Variance between studies	0.060			
Number of ESs	174			

Note. CI = Confidence Interval, ES = Effect Size.