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The effect of time ambiguity on choice depends on delay and amount magnitude

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Abstract

Time ambiguity—that is, having partially/fully incomplete information about *when* an outcome will occur—is common in everyday life. A recent study showed that participants preferred options with time-exact delays over options with time-ambiguous delays, a phenomenon they called time-ambiguity aversion. However, the empirical robustness and boundaries of this phenomenon remain unexplored. We conducted three online studies: Study 2 ($n = 118$) was a replication of Study 1 ($n = 76$) using preregistered analyses; Study 3 ($n = 202$; preregistered) was a follow-up study suggested during review. In Studies 1 and 2, participants completed hypothetical choices between €5 today versus later-but-larger (LL) rewards that systematically varied in their amount, delay, and time-ambiguity level (e.g., for a 180 day delay, time ambiguity varied from 179 to 181 to 0–360 days). Effects of time ambiguity on choice were best encoded in an absolute, dose-dependent manner and depended on delays and amounts: Increasing time ambiguity led to more time-*exact* LL choices at shorter delays but more time-*ambiguous* LL choices at longer delays. Additionally, time-ambiguity ranges including today were chosen more frequently than ranges excluding today, akin to the present bias in intertemporal choice. Lastly, evidence suggested that more time ambiguity was preferred for smaller LL amounts yet disliked for larger LL amounts. Study 3 demonstrated that time-risk and time-ambiguity preferences are differentiable by giving participants choices involving hypothetical time-exact, time-ambiguous, and time-risky options. Taken together, our results extend the nascent literature on time ambiguity by showing that (i) time-ambiguity preferences are distinguishable from both time-risk and delay preferences and (ii) time ambiguity is not generally aversive, but its impact depends on delay and amount magnitude.

KEYWORDS
ambiguity, decision-making, delay discounting, intertemporal choice, uncertainty

1 | INTRODUCTION

Rewards have been shown to lose subjective value as a function of how long one has to wait for them, a phenomenon called *delay discounting* (Samuelson, 1937). Delay discounting is commonly investigated using intertemporal choice paradigms, in which participants choose between monetary sooner-smaller (SS) versus later-larger (LL) rewards with exact delays (e.g., €5 today vs. €20 in 4 weeks).

Choices in these paradigms are associated with problematic real-world behaviors, such that people who choose more SS rewards are also more likely to, for example, use or abuse (illegal) substances, have credit card debts, or suffer from obesity or ADHD (e.g., Bickel et al., 2007; Reimers et al., 2009; Reynolds, 2006; Scheres et al., 2010).

However, everyday-life intertemporal choices rarely have delays that are as exactly known as those used in the laboratory paradigms. For example, when a smoker chooses between smoking a cigarette (a SS reward) versus refraining from smoking for long-term health benefits (a LL reward), it is unclear when exactly the long-term health benefits would be obtained. Having only partial or fully incomplete information about when something will happen was introduced as *time ambiguity* by Ikink et al. (2019). Accordingly, for the purpose of this paper, we will define ambiguity as partially or completely unknown information about a choice-relevant *attribute* of an option. This attribute could thus be its probability of occurring (as in the probability-ambiguity literature, where ambiguity is typically defined as partially or completely unknown probabilities; see, e.g., Baillon et al., 2018) but also its delay before receipt, as in the current paper.

To date, few studies have investigated the impact of time ambiguity on choice, although Onay and Öncüler (2007) and Dai et al. (2019) have shown that timing *risk* (i.e., a delay of either 1 or 11 months, each with 50% chance) was disliked compared with sure-timing options (i.e., a delay of 6 months). In the only currently existing time-*ambiguous* intertemporal choice paradigm (Ikink et al., 2019), delay information about the SS, LL, or both rewards was either exact (0 week range: e.g., in 15 weeks), of low time ambiguity (4 week range: e.g., in 13 to 17 weeks), or of high time ambiguity (8 week range: e.g., in 11 to 19 weeks). On average, participants preferred options with time-exact delays over those with time-ambiguous delays, displaying what we call time-ambiguity *aversion*.[1]

Note that in order to speak of time-ambiguity preferences at either the psychological or choice level, the use of identical delay midpoints (i.e., 15 weeks) is critical: Given identical delay midpoints, a decision-maker that is time-ambiguity *neutral* at the psychological level should treat time-ambiguous and time-exact options the same at the choice level, if they assume that time-ambiguous delays have *on average* a delivery time that is identical to the midpoint of the range. That is, such a decision-maker would first replace the time-ambiguity range with a single delay point within the range, namely, its midpoint; then, they would discount the reward based on that delay. For such a decision-maker, it does not matter which discounting function is used, because if only the midpoint is used for discounting (and this midpoint is identical to the delay of the time-exact option), all discounting functions would predict indifference between time-ambiguous and time-exact options at the choice level. Accordingly, treating time-ambiguous and time-exact options with identical delay midpoints differently indicates either time-ambiguity *aversion* or *seeking* (i.e., depending on whether time-exact or time-ambiguous options were preferred at the choice level, respectively). Another decision-maker, however, might first hyperbolically discount the reward at each possible delay within the range and, afterwards, average across these discounted subjective values (although this is computationally speaking more costly than the abovementioned strategy). Such a decision-maker—which one could call time-ambiguity neutral at the psychological level, because time ambiguity is simply dealt with by averaging across all possible discounted delays—would be expected to consistently prefer time-ambiguous over time-exact options at the choice level. Further, this choice preference would be expected to become weaker with increasing delays due to the shape of the discounting function (i.e., stronger absolute value decreases per time unit across shorter than longer delays).[2] Taken together, preferring time-exact over time-ambiguous options at the choice level cannot be explained by hyperbolic discounting (as this would lead to either indifference toward time ambiguity, as in the first case, or time-ambiguity seeking, as in the second case) and indicates that something else must be at play, which we refer to as a decision-maker's time-ambiguity preference—in this case time-ambiguity aversion.

We also want to point out that in the probability literature, a differentiation between risk (known probabilities) versus ambiguity (partially/fully unknown probabilities) is commonly made (Baillon et al., 2018; Ellsberg, 1961; Tymula et al., 2012), such that the term probability-ambiguity preferences is reserved to those aspects of the preferences that remain after accounting for risk preferences. If we were to strictly apply this same distinction to time ambiguity, what Ikink et al. (2019) called time-ambiguity preferences thus would encompass both time-risk and time-ambiguity preferences (as in Ikink et al., only delay preferences were taken into account, not time-risk preferences—which may also play a role in how people deal with time ambiguity). However, like Ikink et al., we are specifically interested in how time-ambiguous compared with time-exact delays impact choice and will refer to this as time-ambiguity effects in the rest of our paper. Please note, though, that due to a suggestion during the review process, we conducted an additional study to differentiate between time-risk and time-ambiguity preferences (see Study 3 below).

The boundaries of the time-ambiguity aversion effect remain unknown, for example, whether the effect of time ambiguity on choice depends on the extent of time ambiguity, delay, or reward magnitude. For example, Onay and Öncüler (2007) and Dai et al. (2019) showed that timing *risk* was treated differently depending on

[1]Note that this term describes an overt choice effect without assuming a psychological mechanism. Throughout the paper, certain other terms/phrases will be used in a similar manner, that is, without assuming a psychological mechanism (unless specified otherwise). Thus, to be precise, *time-ambiguity aversion* and *disliking time ambiguity* describe a preference for time-exact over time-ambiguous options at the choice level; *time-ambiguity neutrality* or *indifference* describes not showing a preference for either time-ambiguous or time-exact options at the choice level; *time-ambiguity seeking* and *preferring* or *liking time ambiguity* describe a preference for time-ambiguous over time-exact options at the choice level, while *time-ambiguity preference* refers to all of these possible choice-derived preferences.

[2]This prediction holds for all convex discounting functions (i.e., including the exponential discount function), provided that (i) the value decreases are considered in absolute terms (as only in absolute [but not relative] terms, value decreases become smaller over time for all convex discount functions) and (ii) each possible delay is assumed to be equally likely to occur. Although we use the latter assumption for all our time-ambiguity predictions, as it is a common assumption in the ambiguity literature, it may not hold in real life (see, e.g., Baillon et al., 2018).

FIGURE 1 Overview of task stimuli, with (a) a time-exact and (b) a time-ambiguous later-larger (LL) option in the word version and (c) a time-exact and (d) time-ambiguous LL option in the timeline version. In the timeline version, the delay of each LL option was always indicated by a red stripe on the timeline (e.g., 90 days in c). However, for time-ambiguous options, the precise location of the red stripe on the timeline was hidden behind a box, thereby creating a range of possible delivery dates (e.g., between 90 and 270 days in d). Display version (word/timeline) was varied between subjects; position of SS and LL rewards was randomized within subject. Note that in reality, the timelines did not cover the whole width of the screen. The timeline stimuli are available at https://osf.io/rhau2/files/.

the probabilities of the earlier versus later delay occurring. Thus, the present research's main aim was to investigate the relevant conditions of time-ambiguity aversion in a set of two studies (Studies 1 and 2), by systematically varying time-ambiguity levels, delay midpoints, and outcome magnitudes of the LL, while keeping the SS fixed. Since Ikink et al. (2019) found evidence that such intertemporal choices may also depend on the visual presentation or display of the trials (see also Data S1, Appendix A), we presented all trials in either a verbal–numerical or visuospatial time-representation format (see Figure 1).

Thus, we initially conducted two online studies, with Study 2 serving as a direct replication of Study 1 using preregistered analyses (see https://osf.io/sx79a/). We first analyzed the two datasets separately but also combined them to test the reliability of the observed effects. Note that our hypotheses are not based on assuming specific mechanisms by which time ambiguity may be resolved but instead draw on previous findings in the probability-ambiguity literature, as well as on trying to translate existing delay effects that have been commonly found in the intertemporal choice literature to potential time-ambiguity effects.

> **Hypothesis 1.** Based on Ikink et al. (2019) and the existing literature on ambiguity aversion in the probability domain, we expected to find a time-ambiguity aversion effect, such that the LL option would be more often chosen when it was time exact compared with when it was time ambiguous.

More specifically, we expected that the effect of time ambiguity on choice would be better described by a dose–response relationship (i.e., the extent of time ambiguity matters) than a discrete relationship (i.e., only presence vs. absence of time ambiguity matters).[3]

> **Hypothesis 2.** Furthermore, we hypothesized that when the midpoint of a time-ambiguous delay is further in the future, the time-ambiguity aversion effect would become weaker.

This expectation is consistent with findings from Onay et al. (2013), who showed that people are typically less averse to probability ambiguity in the future compared with now. Furthermore, Hypothesis 2 is also consistent with one basic implication of hyperbolic discounting models, namely, that delays have a smaller aversive impact on the value function and thus choice over time (Kirby & Herrnstein, 1995; Mazur, 1987). Therefore, the aversive effect of time ambiguity—another time manipulation—might also become weaker given longer delays.[4]

[3]Please note that Ikink et al. (2019) found no dose–response relationship. The authors speculated that this may have been caused by only having two time-ambiguity levels in their design. As the current studies included more variation in time-ambiguity levels, we expected that the extent of time ambiguity would matter.

[4]Note that the hyperbolic (or any other convex) discounting model does *not* predict time-ambiguity aversion but seeking (which, in absolute terms, would become weaker over time). However, we refer to discounting functions because they predict smaller absolute value decreases per time unit across longer than shorter delays. Thus, if the time-ambiguity effect depends on delay in *some* way, for example, is scaled by delay (which seems reasonable given that time ambiguity is a time manipulation), the time-ambiguity effect would become smaller for each additional time unit, making it a nonconstant effect—at least in absolute terms.

Hypothesis 3. Similarly, we expected that for larger LL amounts, the effect of time ambiguity would decrease: Decision-makers become more patient when the difference between LL and SS rewards increases, even despite longer delays (e.g., Benzion et al., 1989; Kirby & Marakovic, 1995), suggesting that amount effects are stronger than delay effects.

Thus, the impact of time ambiguity might (similar to the impact of delay) become weaker given larger LL amounts.

Note that Hypotheses 2 and 3 also fit with diminishing sensitivity to time ambiguity given larger delays/amounts, based on the Weber–Fechner law in psychophysics (Takahashi et al., 2012).

Hypothesis 4. Based on other work demonstrating a present bias (i.e., immediate SS rewards are often overvalued compared with when both rewards are in the future, as if immediate rewards are "special"; Figner et al., 2010; Laibson, 1997; Prelec & Loewenstein, 1991), we additionally expected that a time-ambiguous LL range that includes a possible today delivery would have a weaker aversive effect compared with a similarly time-ambiguous LL range without possible today delivery (e.g., 0–20 days compared with 20–40 days).

Hypothesis 5. Lastly, we expected that the display type would influence overall patience levels and possibly also time-ambiguity effects.

First, we expected overall more LL choices when the delay and time-ambiguity information was presented visuospatially on a timeline (i.e., the timeline version) compared with when the same information was presented verbally–numerically (i.e., the word version; see also Figure 1). This expectation was based on a possible anchoring effect in the timeline version that would be absent in the word version (Furnham & Boo, 2011): The always visible maximum delay of 360 days in the timeline version might serve as an anchor against which the other delays would be compared (which, therefore, could appear to be relatively short). Alternatively, one could argue that a timeline is a more concrete representation of time than a verbal description. As more concrete delays (e.g., in the form of a date; see Read et al., 2005) have been shown to increase patience, the timeline display might lead to greater patience than the verbal display. Finally, the amounts might stand out more in the timeline than the verbal display due to, for example, their larger font size (see also Figure 1), leading to a greater focus on amounts and thus potentially increased patience. With respect to the time-ambiguity effects, we expected that they might be stronger in the timeline than the word version, given the greater salience and concreteness of time ambiguity in the timeline compared with the verbal presentation format (for more background about Hypothesis 5, see Data S1, Appendix A).

As suggested during the review process, we additionally conducted a third online study: Study 3 (preregistered under https://osf.io/3gjze).

Hypothesis 6. In this follow-up study, we investigated whether the effects of time ambiguity on choice can be empirically differentiated from those of time risk.

2 | METHODS OF STUDIES 1 AND 2

2.1 | Participants and procedure

Participants were anonymously recruited via our university's online research participation system (i.e., convenience sampling from our university's student population). After providing informed consent, participants received instructions about the online intertemporal choice task and performed several practice trials before starting the task (consisting of 156 binary choice trials). After completing the task, participants answered several questions about how they made their choices. In Study 2, participants additionally filled in the future self-continuity scale (Ersner-Hershfield et al., 2009) and a numeracy scale (Lipkus et al., 2001). As preregistered, these measures were not included in our analysis but collected for future exploratory purposes. The whole study took roughly 1 h per participant, and participants received course credit for their participation. The study was approved by the local ethics committee.

In Study 1, 88 participants finished the online experiment. However, the final sample size was smaller due to the exclusion of participants based on our modeling procedure. Indeed, as part of this work, we attempted to develop and fit computational choice models to capture individuals' behavior (see Data S1, Appendix B for more details). Using our preregistered modeling exclusion criteria (which were rather strict, because the data quality of online experiments can be somewhat lower compared with lab-based experiments; see, e.g., Gould et al., 2015), we removed data of 12 participants (14%): Three participants did not show enough variation in their choices (which complicates parameter estimation; criterion: >5 times the SS/LL option chosen), five returned no model fits, and four were outliers in their estimated parameter values. As preregistered, we excluded these participants from both our computational choice models and our statistical mixed-effects choice models, as they possibly completed the choice task differently than the majority of participants (which we could model). Of the remaining 76 participants (15 males; $M_{age} = 20.08$, $SD = 2.22$), 38 completed the timeline version, and 38 the word version.

In Study 2, 145 participants finished the online experiment, and 27 participants (19%) were excluded based on our preregistered exclusion criteria for the modeling: three due to insufficient variation, 10 because they returned no model fit, 13 because their parameter values were outliers, and one because choices were not predicted above chance level (more details in Data S1, Appendix B). The final sample of Study 2 therefore included 118 participants (19 males;

M_{age} = 19.82, SD = 2.18) of which 57 completed the timeline version and 61 the word version.

The final combined sample (from Studies 1 and 2 together) thus included 194 participants, with 95 completing the timeline version and 99 completing the word version. Since a rather large number of participants were excluded in Studies 1 and 2 due to our strict criteria, we repeated the main choice analyses with all participants as a robustness check. We found only minor changes compared with the results reported here, and most importantly, all our main conclusions remain unchanged (details in Data S1, Tables B–D).

2.2 | Online intertemporal choice task

Participants completed an online intertemporal choice task with 156 trials, grouped in three different blocks (with the possibility to take a short break between blocks). Both the included trials per block and the order in which the three blocks were completed were the same across all participants. However, the trials within each block were presented in random order and varied in their amounts, delays, and time-ambiguity level. The task was programmed in Unipark Questback, with separate versions for the timeline and word displays, as this was a between-subject variable (see Figure 1). In each trial, participants chose between a fixed sooner-smaller (SS) option of €5 today and a later-larger (LL) option that varied in amount, delay midpoint, and time-ambiguity level. Thus, only the LL reward could be time ambiguous. In the timeline version, exact LL delays were always indicated by a red stripe on a timeline ranging from 0 to 360 days, and this red stripe was sometimes hidden behind a box, creating time-ambiguous options (as in Ikink et al., 2019). In the word version, exact LL delays were indicated by words (e.g., "90 days"), and time-ambiguous options were indicated by a range (e.g., "60 to 120 days"; see also Figure 1). The two choice options were presented simultaneously on a computer screen, with one option at the top of the screen and the other at the bottom (position was randomized). Participants made their choice via a mouse click. As hypothetical (compared with real) rewards have been shown to result in similar discount rates (Bickel et al., 2009; Matusiewicz et al., 2013; but see Hinvest & Anderson, 2010) and brain activation in reward regions (Bickel et al., 2009; Bray et al., 2010), we decided to use hypothetical rewards. No response times were recorded.

The monetary amount of the LL option was either €5.20, €10.50, €16.80, or €25.30, and the delay midpoint was 1, 10, 30, 90, or 180 days for trials with and without time ambiguity. The time-ambiguity level of the LL option ranged from 0 (exact trials) to 360 days and was always added in such a way that the delay midpoints of time-ambiguous and time-exact trials were identical, making them comparable (i.e., 8–12 days vs. 10 days; 60–120 days vs. 90 days). For time-ambiguous options, participants were told that there was always a precise delay specified somewhere within the given range (word version) or a red stripe present somewhere behind the box (timeline version), but that it was unknown to the participants. We added exact time trials with delay midpoints of 270 and 360 days to have more exact trials. For an overview of all trials, see Data S1, Table E.

One of our research questions was how decision-makers might represent time ambiguity: One option is that the mere presence versus absence of time ambiguity affects participants' choices (in Ikink et al., 2019, it did not matter whether the time-ambiguity range was 4 or 8 weeks). Another option is that the effect of time ambiguity on choice follows a dose–response relation such that higher degrees of time ambiguity show a stronger effect on choice. If so, then either the absolute level of time ambiguity might matter (e.g., a time-ambiguity range of 20 days always has the same effect) or the relative level of time ambiguity might matter (e.g., a time-ambiguity range of 20 days has a stronger effect when the delay midpoint is 30 days compared with when the delay midpoint is 180 days).

To test between these three possibilities, we compared in our statistical analysis three different ways of coding for time ambiguity: (i) presence/absence of time ambiguity, (ii) absolute time ambiguity (in days), or (iii) relative time ambiguity (representing time ambiguity in % relative to the delay midpoint). Coding time ambiguity as presence/absence means that we ignore the extent of time ambiguity, while absolute and relative time-ambiguity levels both incorporate the extent of time ambiguity, though in different ways. For example, 12 days of absolute time ambiguity given a delay midpoint of 30 days (in the task: 24 to 36 days) can be translated into a relative (%) time-ambiguity level: $12/30 * 100 = 40\%$. Vice versa, a 200% relative time-ambiguity level for a delay midpoint of 180 days translates into 360 days of absolute time ambiguity and for a delay midpoint of 10 days into 20 days of absolute time ambiguity. The design included systematic variation in both absolute and relative levels to avoid an a priori bias for one specific way of coding (e.g., for absolute: 0, 2, 10, 20, and 60 days; for relative: 0%, 20%, 40%, 100%, and 200%; see also Data S1, Table E). We expected that coding for the degree of time ambiguity would result in a better model fit compared with only coding for its presence/absence. Further, we expected that relative time ambiguity might provide a better fit than absolute time ambiguity, because choice attributes are often evaluated relative to some standard or reference point (Loewenstein & Prelec, 1992; Rangel & Clithero, 2012).

It is important to note that the time-ambiguity levels and delay midpoints could not be combined in a fully factorial design (i.e., when using a delay midpoint of 1 day, an absolute time-ambiguity level of 10 days is not possible nor a relative time-ambiguity level of, say, 20%, as the smallest unit was a full day). We therefore had 39 unique trials based on delay midpoint and time-ambiguity level (7 exact and 32 time-ambiguous trials, varying in absolute/relative time-ambiguity level; Data S1, Table E), crossed with four amounts, resulting in 156 trials. The data from one specific exact timeline trial (€5.00 today or €5.20 in 180 days) had to be removed, as the choice patterns were nonsensical and suggested an error in how the trial was programmed (in this trial, the majority of participants would be expected to choose the SS, yet the majority chose the LL; see Data S1, Figure A). Although we feel this is the most appropriate way to deal with this trial, for completeness sake, we also report the results including this trial in Data S1, Tables B and C.

2.3 | Data analysis

The analyses in Studies 1 and 2 and the combined sample were identical (except that they used different datasets). After having analyzed the data of Study 1, we collected the data of Study 2 and preregistered the analyses, such that Study 2 could serve as an independent replication of Study 1's results (see https://osf.io/sx79a/). The choices were analyzed using mixed-effects models in a Bayesian framework using R (R Core Team, 2018). Both data and code are available online (https://osf.io/rhau2/files/).

The general analysis strategy was as follows: To answer the question which form of time-ambiguity coding (i.e., presence/absence, absolute, or relative) would best fit the observed choice patterns, we first ran three separate models (each incorporating one variant of time-ambiguity coding) and compared their statistical model fit using the Widely Applicable Information Criterion (WAIC; Watanabe, 2010), as implemented in the package brms (Bürkner, 2017). Once the best-fitting model was determined (based on lowest WAIC) and thus the best way of time-ambiguity coding, we looked into the effects of individual predictors within the best-fitting model in order to answer the other research questions. All follow-up analyses then also used only that way of time-ambiguity coding. We believed this method to be most objective, as the individual model results could not bias our model selection.

In each of the models, the dependent variable was binary choice (0 for SS and 1 for LL). Fixed effects were (a) version (timeline vs. word; categorical), (b) LL amount (continuous), (c) delay midpoint (continuous), and (d) one of the three ways of coding for time-ambiguity level: (i) time ambiguity present versus absent (categorical), (ii) absolute time-ambiguity level (in days; continuous), or (iii) relative time-ambiguity level (in %; continuous). All predictors were varied within-subject except version (which was varied between-subject), and we also included all possible two-, three-, and four-way interactions between the predictors. The pseudo-R code for the models ran thus looked like this:

choice (SS or LL) $\sim$ 1 + display version (word/timeline) * LL amount (standardized) * LL delay midpoint (standardized) * LL time ambiguity[5] + (1 + LL amount * LL delay * LL time ambiguity|participant number).

Only for the best-fitting model, we then ran follow-up models: For significant interactions including version (timeline/word), we split up the data per version; for interactions including continuous predictors, we either split up the continuous delay factor (resulting in small and large delays, i.e., 1/10/30 days vs. 90/180 days) or ran a follow-up model per amount. All continuous predictors were standardized; contrasts of categorical variables were sum-to-zero coded.

To model the repeated-measures structure of the data and to avoid inflated Type I errors, we used a maximum random effect structure in all models as recommended by Barr et al. (2013). Thus, each model included a participant-specific random intercept, random slopes for all the within-subject predictors (including main and interaction effects), and all possible random correlations. We calculated posterior credible intervals using the *brm* function of the R-package brms (Bürkner, 2017), which provides an interface to Stan (Carpenter et al., 2017). We used brms' weakly informative default priors, and each model was fit using six chains with 2000 iterations (500 warm-ups). The Bayesian models were inspected for convergence by checking Rhat (Rhats should be >0.9 and <1.1) and visually inspecting the traceplots of all parameters. We deemed an effect statistically significant when the corresponding 95% posterior credible interval (CI) did not include 0. We called an effect a trend when the 90% CI did not include 0.

[5]LL time ambiguity was thus coded either as categorical predictor (present/absent) or as continuous predictor in either relative (%) or absolute (in days) terms. In the first case, we used −1/+1 sum-to-zero coding; in the latter two cases, the predictors were standardized.

3 | RESULTS OF STUDIES 1 AND 2 AND THE COMBINED SAMPLE

3.1 | Interpretation of results across studies

We present the results of Study 1 ($n = 76$), replication Study 2 ($n = 118$), and the combined sample ($n = 194$) together, as we believe this provides the clearest overview of the effects we found. As described in our preregistration, we deemed an effect reliable when it was significant in both separate studies and the combined sample. If an effect was nonsignificant in both studies and the combined sample, we deemed it absent. As research on time ambiguity is yet scarce and the effects of time ambiguity on choice are not well understood, we thought it was important to avoid inflating not only Type 1 but also Type 2 errors. Therefore, we did not immediately discard effects that were observed in one study but not the other. Instead, we pooled the data of Studies 1 and 2 and analyzed this combined dataset to check the effects for reliability by increasing statistical power. If we observed a significant effect in one study and the combined sample, we interpreted that as evidence that the effect is present but not as reliable. If an effect was significant only in one study but not the combined sample, we deemed it a false positive. Finally, if we found a significant effect in the combined sample but not in Study 1 or 2, we interpreted it as a somewhat unreliable effect (i.e., one that could be observed only with a large sample size).

3.2 | Determining the best way of coding for time ambiguity

To answer our research question about the best representation of time ambiguity (absence/presence, absolute, or relative), we compared the statistical model fits (WAIC) of the three main choice models. These three models were identical except for the way of time-ambiguity coding: presence/absence of time ambiguity, absolute time ambiguity (in days), or relative time ambiguity (in %, relative to the delay midpoint). Consistent across Studies 1 and 2 and the combined sample, we found that coding for only the presence/absence of time ambiguity provided the worst model fit, followed by relative time ambiguity. Absolute time ambiguity provided the best model fit (see Table 1 and Data S1, Table B for the same results when including all trials/participants). This suggests that the effect of time ambiguity was—as we expected—better represented by a dose–response relationship than a discrete relationship. However, contrasting with our expectations, the absolute representation was a better fit than the relative representation. While the difference between absolute and relative time ambiguity seemed small and inconclusive in Study 1, this difference was replicated and larger in Study 2 and the combined sample, giving us enough confidence to conclude that absolute time ambiguity provided the best fit. Accordingly, we used the model with absolute time ambiguity (in days) for the remainder of the analyses.

TABLE 1 Overview of statistical model fit (WAIC) and its estimated standard error (SE) across the three main models and studies.

	Study 1 ($n = 76$)		Study 2 ($n = 118$)		Combined ($n = 194$)	
Time-ambiguity encoding	WAIC	SE	WAIC	SE	WAIC	SE
Presence/absence of time ambiguity	5017	133	7573	171	12,564	216
Relative time ambiguity (in %)	4897	133	7315	167	12,198	212
Absolute time ambiguity (in days)	4858	130	7166	164	11,995	209

Note: Lower WAIC indicates better model fit.

TABLE 2 Estimated coefficients (*B*s) and 95% posterior credible intervals (CIs) for all the effects in the absolute time-ambiguity (main) model across Studies 1 and 2 and the combined sample.

		Study 1 ($n = 76$)		Study 2 ($n = 118$)		Combined ($n = 194$)	
Effects	Overview per effect (across the studies)[a]	*B*	95% CI	*B*	95% CI	*B*	95% CI
Display (word/timeline)	s; s; s	1.363	[0.023, 2.764]	1.535	[0.403, 2.661]	1.436	[0.572, 2.292]
Amount	s; s; s	4.334	[3.591, 5.106]	5.352	[4.521, 6.184]	4.906	[4.326, 5.514]
Delay (midpoint)	s; s; s	−4.532	[−5.562, −3.571]	−3.861	[−4.624, −3.171]	−4.086	[−4.710, −3.496]
Time ambiguity (absolute; in days)	ns; ns; ns	−0.206	[−0.530, 0.119]	0.067	[−0.214, 0.362]	−0.002	[−0.217, 0.219]
Display × amount	t; s; s	0.658	[0.073, 1.268]	0.939	[0.106, 1.764]	0.832	[0.273, 1.399]
Display × delay	ns; t; s	0.547	[−0.377, 1.539]	0.625	[0.031, 1.242]	0.588	[0.014, 1.161]
Display × time ambiguity	ns; ns; ns	0.180	[−0.124, 0.475]	−0.037	[−0.272, 0.200]	0.024	[−0.161, 0.215]
Time ambiguity × amount	s; ns; s	−0.439	[−0.711, −0.170]	−0.176	[−0.493, 0.170]	−0.325	[−0.557, −0.083]
Time ambiguity × delay	s; s; s	0.798	[0.510, 1.102]	0.366	[0.034, 0.668]	0.545	[0.290, 0.780]
Time ambiguity × delay	ns; ns; ns	0.063	[−0.441, 0.575]	−0.006	[−0.558, 0.567]	−0.006	[−0.407, 0.401]
Time ambiguity × amount × display	ns; ns; ns	−0.071	[−0.296, 0.162]	0.048	[−0.198, 0.870]	−0.004	[−0.174, 0.173]
Time ambiguity × delay × display	s; ns; s	−0.327	[−0.608, −0.063]	−0.217	[−0.495, 0.045]	−0.271	[−0.478, −0.063]
Amount × delay × display	t; ns; t	0.421	[0.035, 0.825]	0.308	[−0.247, 0.870]	0.365	[0.049, 0.681]
Time ambiguity × amount × delay	ns; s; s	0.147	[−0.112, 0.405]	−0.425	[−0.810, −0.097]	−0.239	[−0.491, −0.010]
Time ambiguity × amount × delay × display	ns; s; s	−0.100	[−0.327, 0.124]	−0.266	[−0.541, −0.009]	−0.207	[−0.390, −0.029]

Abbreviations: ns, nonsignificant; s, significant; t, trend.
[a]For trend effects, 90% posterior CIs instead of 95% CIs are reported.

3.3 | Main choice model

We investigated the main effects of version (timeline/word), amount, delay midpoint, and absolute time ambiguity, as well as all possible (two-, three-, and four-way) interactions. For an overview of all the statistical results per study, see Table 2 (and Data S1, Table C for similar results when including all trials/participants). As expected, we found significant effects of delay and amount in Studies 1 and 2 and the combined sample, showing that (i) when the delay of the LL option increased, fewer LL choices were made and (ii) when the amounts of

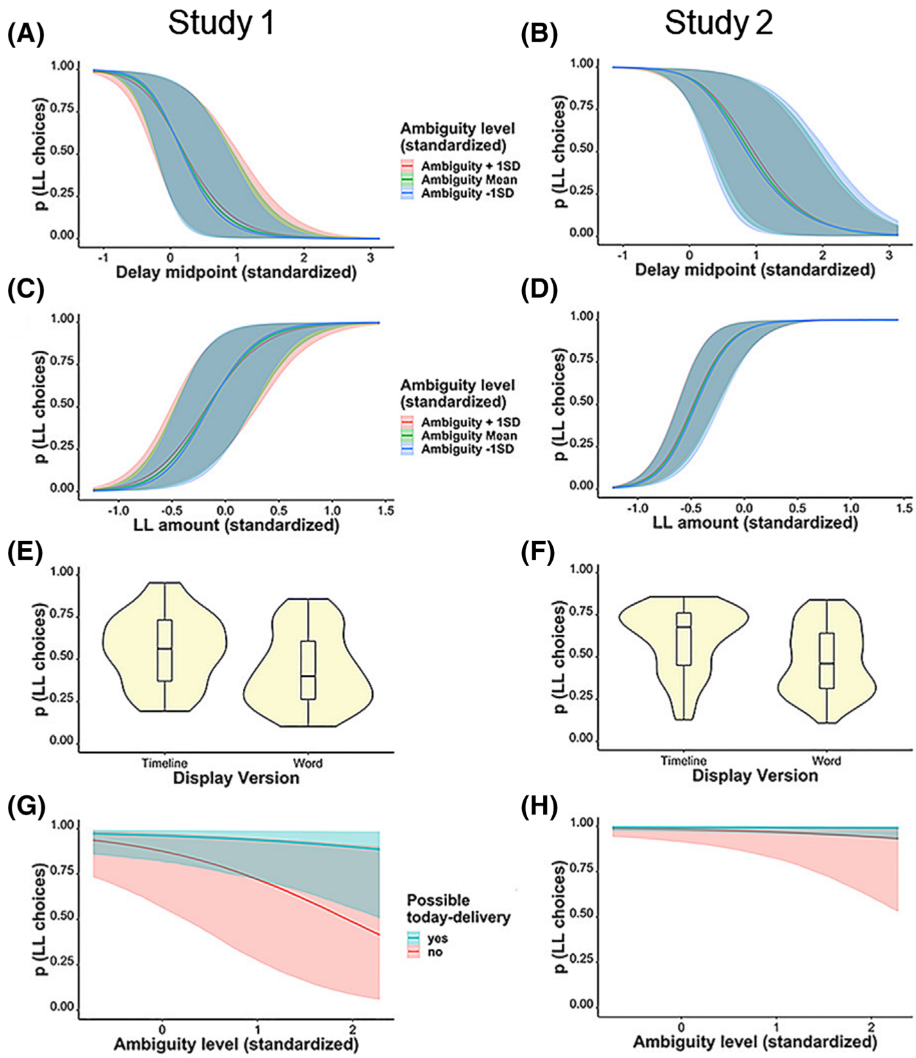

FIGURE 2 Effects of interest in Study 1 ($n = 76$) and Study 2 ($n = 118$). We show mainly estimated marginal effect plots as these take the nonorthogonal design and other estimated effects into account. (a and b) Estimated marginal effect plots of the interaction between time-ambiguity level and delay midpoint (significant in both studies and the combined sample). Follow-up analyses showed that across shorter delays (until 30 days), more time ambiguity was disliked (trend in Study 1 and significant in the combined sample), whereas at larger delays (90/180 days), more time ambiguity was liked (trend in Study 2 and significant in the combined sample). (c and d) Estimated marginal effect plots of the interaction between time-ambiguity level and LL amount (significant in Study 1 and the combined sample; not in Study 2). The main effect of LL amount is visible (i.e., more LL choices with larger LL amounts; significant in both studies and the combined sample), with slight time-ambiguity liking for smaller LL amounts and slight time-ambiguity disliking for larger LL amounts (again, only significant and visible in plot c/Study 1—not plot d/Study 2). However, time ambiguity had no significant effect for any single LL amount, consistent with the visual impression that time ambiguity showed only subtle effects. (e and f) Combined violin and boxplots based on the raw data, showing the main effect of display version (significant in both studies and the combined sample): Participants chose the LL option more often in the timeline compared with the word version. (g and h) Estimated marginal effect plots of the interaction between time-ambiguity level and possible today delivery (PTD) (trend effect in both studies and significant in the combined sample). The main effects of PTD and time ambiguity are visible: Fewer LL choices were made when no today delivery was possible (significant in both studies and the combined sample) and when the time ambiguity level increased (significant in Study 1 and the combined sample and trend in Study 2). The interaction shows that only options without PTD became more aversive given an increase of time ambiguity; options with PTD remained equally attractive.

the LL option increased, more LL choices were made. These effects suggest that participants understood the task, paid attention, and made reasonable choices (despite the use of hypothetical rewards and an online task paradigm). Below, we report the other results, loosely grouped by our hypotheses.

3.3.1 | Time-ambiguity effects

First, we tested our main hypothesis predicting a time-ambiguity aversion effect, indicating that people would less often choose the LL option when the level of time ambiguity increases (Hypothesis 1). However, the results across both independent studies and the combined sample consistently indicated that time ambiguity did not influence LL choice as main effect (Table 2). Instead, there was a clear, reliable Time ambiguity × Delay interaction (found in Studies 1 and 2 and the combined sample), indicating that for relatively short delays (1/10/30 days), increasing time ambiguity led to time-exact options being chosen more often (trend in Study 1; similar direction but nonsignificant in Study 2; and significant in the combined sample), whereas for longer delays (90/180 days), increasing time ambiguity led to time-ambiguous options being chosen more often (nonsignificant in Study 1; trend in Study 2; and significant in the combined sample; Figure 2a,b and Data S1, Table F). Thus, neither our hypothesis that time ambiguity would be aversive in general (Hypothesis 1) nor that this effect would decrease with longer delays (Hypothesis 2) was confirmed. Instead, we found that time ambiguity was disliked only across shorter delays, while it became liked at longer delays. Such a crossover effect (i.e., the effect of time ambiguity switched as a function of delay midpoint) is consistent with the absence of a main effect of time ambiguity.

Our third expectation was that the effect of time ambiguity would decrease for larger LL amounts (Hypothesis 3). While there was indeed a significant Time ambiguity × Amount interaction in Study 1 and the combined sample (but not in Study 2; see Table 2), the results were not in line with our expectation: Inspecting Figure 2c suggests that for smaller LL amounts, more time ambiguity was liked, whereas for larger LL amounts, more time ambiguity was disliked. However, follow-up analyses per amount resulted in no significant time-ambiguity effects for any of the amounts (Data S1, Table G), suggesting a somewhat weaker moderation effect. To conclude, we found no support for our hypothesis that with larger amounts, the time-ambiguity effect would decrease.

Lastly, we found an unexpected three-way interaction showing that the time ambiguity by delay interaction (i.e., the crossover effect) differed depending on amount magnitude. This effect was found in Study 2 and the combined sample, but not in Study 1 (Table 2). Follow-up models per amount level suggested that the crossover effect became smaller (up to nonsignificant) for larger LL amounts (i.e., smaller coefficients; Data S1, Table H). This may indicate that larger LL amounts had relatively more impact on choice, thereby decreasing the crossover effect.

3.3.2 | Display effects

Consistent with our expectation that display format influences people's overall patience and/or time-ambiguity effects (Hypothesis 5), participants made overall more patient choices in the timeline version compared with the word version (Study 1: $M_{p(\text{LLchoice})\ \text{Timeline}} = 0.561$, $M_{p(LL\text{choice})\ \text{Word}} = 0.439$; Study 2: $M_{p(\text{LLchoice})\ \text{Timeline}} = 0.606$, $M_{p(LL\text{choice})\ \text{Word}} = 0.480$; Table 2 and Figure 2e,f). This effect was found in both studies and the combined sample, indicating a reliable effect. Furthermore, both the amount and delay effect differed across the two versions: First, Display × Amount was a trend effect in Study 1 and became significant in Study 2 and the combined sample (Table 2). Follow-up models per version indicated that the amount effect was stronger in the timeline than the word version (Data S1, Table I). Second, there was a significant Display × Delay interaction in the combined sample and a trend effect in Study 2 (nonsignificant but similar direction in Study 1; see Table 2). Here, follow-up models indicated that the delay effect was weaker in the timeline than word version, although this effect required a larger sample to be detected reliably (Data S1, Table J). Possibly, the stronger amount (and weaker delay) effect in the timeline compared with word version can partially explain the increased patience in the timeline version. However, time-ambiguity effects were not clearly stronger in the timeline version (which we had expected given the visually more salient box to indicate the time-ambiguity range), although we did find a significant three- and four-way interactions involving both time ambiguity and display (in Study 1 and the combined sample and Study 2 and the combined sample, respectively; Table 2; more details in Data S1, Appendix C and Tables K and L). To conclude, we found support for our hypothesis that participants would choose more patiently in the timeline than the word version, but time-ambiguity effects were not stronger in the timeline than the word version.

For an overview of the proportion of LL choices per delay midpoint, time-ambiguity level, amount, and display version in the combined sample ($N = 194$), see Data S1, Table M.

3.4 | Possible today-delivery (PTD) model

One of our research questions asked whether the effect of time ambiguity on choice might differ as a function of whether the time-ambiguity range includes a possible today delivery (PTD) versus not, such that a possible today delivery may lead to a relatively higher acceptance of time-ambiguous options (i.e., reduced time-ambiguity aversion). To address this question, we used a subset of the data, namely, only time-ambiguous trials that either included a possible today delivery or had a similar time-ambiguity level but *no* possible today delivery (see Table 3). We tested the same effects as in the main choice model but also included a possible today-delivery predictor (PTD: yes/no) and its interaction with time ambiguity. For this PTD model, we were specifically interested in the PTD effect, the time-ambiguity effect, and their interaction, so we do not further

TABLE 3 Trials included in the restricted dataset to test the possible today-delivery (PTD) hypothesis.

Possible today delivery	No possible today delivery
0 to 2 days	9 to 11, 29 to 31, 89 to 91, and 179 to 181 days
0 to 20 days	20 to 40, 80 to 100, and 170 to 190 days
0 to 60 days	60 to 120 and 150 to 210 days
0 to 180 days	90 to 270 days
Total: 4 * 4 (amount) = 16 trials	Total: 10 * 4 (amount) = 40 trials

Note: Here, absolute time-ambiguity levels are similar, but delay midpoints differ.

report or interpret any other results of this model. Please note, though, that all the reliable findings in the main choice model were also observed in all the models that are reported in this section (see Table 4 and Data S1, Table D for similar results with all participants).

As hypothesized, we found a reliable PTD effect in both studies and the combined sample, indicating that people were more likely to choose a time-ambiguous LL option when the time-ambiguity range included a possible today delivery compared with when it did not (Study 1: $M_{\text{p(LLchoice) PTD}} = 0.676$, $M_{\text{p(LLchoice) no-PTD}} = 0.454$; Study 2: $M_{\text{p(LLchoice) PTD}} = 0.704$, $M_{\text{p(LLchoice) no-PTD}} = 0.496$; Table 4). Furthermore, in both Studies 1 and 2, we found a trend for an interaction between PTD and time ambiguity, which became significant in the combined sample. This indicates a not very reliable but still somewhat consistent effect (Table 4 and Figure 2g,h). The interaction showed that for trials without PTDs, an increase in time ambiguity led to fewer LL choices, whereas for trials with PTDs, this effect was much smaller or even absent. Thus, options with a PTD were generally more attractive compared with options without a PTD, and their apparent attractiveness was barely reduced when the time-ambiguity range increased (i.e., from 0–2 to 0–180 days)—unlike that of options without a PTD.

Furthermore, in this restricted dataset, we found a significant main effect of time ambiguity in Study 1 and the combined sample, whereas in Study 2, it was a trend effect (Table 4). Indeed, an increase in time-ambiguity level resulted in fewer LL choices, which is the time-ambiguity aversion effect we had originally expected. As time ambiguity was not significant in the main choice model, we wanted to check whether the change in significance was perhaps due to using only a subset of all trials. We therefore repeated the main choice model in the restricted dataset. However, like in the full dataset, time ambiguity was consistently nonsignificant (across both studies and the combined sample; see Data S1, Appendix D). This indicates that the difference in datasets cannot explain the difference in significance.

Instead, it seemed that time ambiguity became significant when accounting for the possible today-delivery effect: A significant main effect indicating time-ambiguity aversion was found in the restricted dataset (i) when PTD and its interaction with time ambiguity was added (Table 4) and (ii) when PTD was added as main effect only (in Study 1 and the combined sample, but not in Study 2; see Data S1, Appendix D). Furthermore, in a third model, we removed all PTD trials ($n = 20$) from the full dataset and reran the main choice model. Here, we again found some support for a time-ambiguity aversion effect (time ambiguity significant in Study 1 and trends in Study 2 and the combined sample; see Data S1, Appendix D). Together, these analyses suggest that accounting for the presence of PTD trials is relevant for being able to detect a time-ambiguity aversion effect.

Lastly, in the PTD model, time-ambiguity levels for PTD and no-PTD trials were matched, but delay midpoints were not. In other words, delay midpoints in the PTD trials were systematically lower than those in the no-PTD trials (a confound; see also Table 3), making it possible that the PTD effect was significant due to the difference in delay midpoints (although delay midpoint was always included in these models and as such controlled for). To further validate the PTD effect, we therefore ran two follow-up models: (1) a model where delay midpoints were matched but time-ambiguity levels were not (i.e., PTD being confounded with time-ambiguity level instead of delay; see Data S1, Table N for the included trials) and (2) a model including all time-ambiguous trials, not trying to match on either delay midpoint or time-ambiguity level. The results were the same across Studies 1 and 2 and the combined sample: PTD was nonsignificant in follow-up model 1 but significant in follow-up model 2, indicating that the difference in delay plays at least some role (more details in Data S1, Appendix D). Lastly, in follow-up model 2, we found again a significant time-ambiguity aversion effect across the three samples (this could not be tested in follow-up model 1; see also Data S1, Appendix D), further confirming that accounting for possible today deliveries impacts the time-ambiguity effect.

Thus, taking all results together, we can conclude that (i) our hypothesis that time ambiguity is considered less aversive given a possible today delivery (PTD) was confirmed, (ii) the PTD effect is found quite consistently, but at least to some extent impacted by the difference in delay midpoint, and (iii) accounting for PTD trials impacted the time-ambiguity effect (i.e., changing it from a nonsignificant to a trend/significant effect).

3.5 | Computational choice models

Similar to Ikink et al. (2019) and as described in our preregistration, we compared in total 38 different computational choice models (24 preregistered models based on Ikink et al. and 14 post hoc models) to explore how time ambiguity might influence subjective value. We only explored variations of the hyperbolic discounting model (Mazur, 1987). However, our results indicated that across all participants, one of the two standard hyperbolic discounting models, that is, a model that did not incorporate time ambiguity at all, provided the best fit among our model candidates (based on BIC comparisons; penalizing for model complexity). This may seem to indicate that the

TABLE 4 Estimated coefficients (*B*s) and 95% posterior credible intervals (CIs) for all the effects in the possible today-delivery (PTD) model across Studies 1 and 2 and the combined sample.

Effects	Overview per effect (across the studies)[a]	Study 1 ($n = 76$)		Study 2 ($n = 118$)		Combined ($n = 194$)	
		B	95% CI	*B*	95% CI	*B*	95% CI
Display (word/timeline)	s; s; s	1.595	[0.431, 2.798]	1.667	[0.453, 2.896]	1.674	[0.836, 2.535]
Amount	s; s; s	4.539	[3.752, 5.442]	5.992	[4.984, 7.152]	5.117	[4.469, 5.822]
Delay	s; s; s	−2.877	[−3.657, −2.165]	−2.952	[−3.708, −2.219]	−2.781	[−3.314, −2.258]
Time ambiguity (absolute; in days)	s; t; s	−0.935	[−1.391, −0.472]	−0.479	[−0.874, −0.063]	−0.601	[−0.942, −0.257]
Display × amount	s; s; s	0.806	[0.072, 1.573]	1.040	[0.074, 2.011]	0.985	[0.380, 1.613]
Display × delay	ns; s; s	0.364	[−0.226, 1.005]	0.964	[0.348, 1.629]	0.759	[0.330, 1.212]
Display × time ambiguity	ns; ns; ns	0.177	[−0.143, 0.519]	0.054	[−0.274, 0.412]	0.077	[−0.156, 0.321]
Time ambiguity × amount	s; ns; ns	−0.493	[−0.831, −0.139]	0.113	[−0.347, 0.645]	−0.210	[−0.528, 0.127]
Time ambiguity × delay	s; s; s	1.256	[0.802, 1.714]	0.881	[0.410, 1.335]	1.016	[0.683, 1.338]
Amount × delay	ns; s; s	−0.353	[−0.850, 0.167]	−0.684	[−1.309, −0.053]	−0.540	[−0.940, −0.110]
Time ambiguity × amount × display	ns; ns; ns	0.000	[−0.286, 0.291]	0.064	[−0.279, 0.428]	0.029	[−0.199, 0.269]
Time ambiguity × delay × display	s; t; s	−0.364	[−0.643, −0.093]	−0.275	[−0.549, −0.028]	−0.345	[−0.567, −0.139]
Amount × delay × display	ns; s; s	0.154	[−0.237, 0.589]	0.842	[0.328, 1.418]	0.591	[0.239, 0.948]
Time ambiguity × amount × delay	ns; ns; ns	0.242	[−0.054, 0.542]	−0.021	[−0.456, 0.372]	−0.007	[−0.280, 0.247]
Time ambiguity × amount × delay × display	ns; ns; t	−0.176	[−0.423, 0.073]	−0.148	[−0.474, 0.142]	−0.181	[−0.351, −0.016]
Possible today delivery (yes/no)	s; s; s	−0.629	[−0.910, −0.349]	−0.590	[−0.832, −0.351]	−0.577	[−0.756, −0.400]
Possible today delivery × time ambiguity	t; t; s	−0.251	[−0.478, −0.016]	−0.202	[−0.402, −0.003]	−0.205	[−0.378, −0.030]

Abbreviations: ns, nonsignificant; s, significant; t, trend.
[a]For trend effects, 90% posterior CIs instead of 95% CIs are reported.

time-ambiguity effects as observed in our mixed-models choice analysis can thus be ignored. However, as we replicated most of our findings, the time-ambiguity effects appear to be reliable and valid, but we could not identify an adequate computational model to account for these effects—at least not at the aggregate level, using alternative-based hyperbolic discounting models like Ikink et al. (2019). Future modeling work could perhaps investigate, for example, variations of prospect theory, attribute-based, or drift diffusion models to more appropriately capture and incorporate the observed time-ambiguity effects across all participants.

Furthermore, when we explored which model fitted best at the single-participant level (i.e., allowing for individual differences instead of searching for one *general* model across all participants), we found that all time-ambiguity models together provided a somewhat better fit compared with the standard hyperbolic discounting models (across all time-ambiguity models: $M_{BIC} = 64.649$, 119 best-fitted participants; across the two standard discounting models: $M_{BIC} = 67.559$, 75 best-fitted participants). This may suggest that time ambiguity did influence subjective value but differently per individual, possibly explaining why no general "best-performing" time-ambiguity model could be detected. For full details of our modeling procedure and results, see Data S1, Appendix B and Table A.

4 | STUDY 3

Although our results so far seem to indicate that time ambiguity influences choice, we cannot rule out that we have captured the joint impact of not only time-ambiguity but also time-risk preferences. Therefore, during the review process, we were asked to conduct an additional study to test whether time ambiguity and time risk have distinguishable empirical effects on overt choice, which we hypothesized to be the case (Hypothesis 6). If so, this would indicate that time ambiguity is a distinct phenomenon—distinguishable from not only existing delay preferences but also existing time-risk preferences.

The online experiment was preregistered (see https://osf.io/3gjze), programmed in Qualtrics, and included seven hypothetical Ellsberg-like preference questions (Ellsberg, 1961). For the precise items used and the descriptive statistics per item, see Data S1, Appendix E and Table O. Data and code are available online (https://osf.io/rhau2/files/). Specifically, in each item, participants were told that there were two bags, each containing 100 balls. Each ball contains a number that indicates a specific delay. From one of the two bags, a ball would be drawn at random, which would determine when they would receive £100. Importantly, the content of the bags differed across the seven items, including the extent to which it was known which numbers were on the balls. For example, a bag could contain 100 balls that all had the number 51 on them (a time-exact bag); a bag could contain 50 balls with the number 1 and 50 balls with the number 100 (a time-risky bag); or a bag could contain 100 balls that all had either the number 1 or 100 on them, but it was unclear how many balls had which number (a time-ambiguous bag). Thus, in each item, participants read the description of the contents of the two bags and were then asked to indicate to what extent they preferred to draw a ball from one or the other bag. They did so by clicking on a rating scale going from −50 (*definitely bag 1*) to 50 (*definitely bag 2*), with an additional label *no clear preference* at score 0.

Out of the total of seven items, three items asked for the preference between a time-risky bag versus a time-ambiguous bag. Two of the other four items asked for the preference between a time-exact versus a time-risky bag; and another two asked for the preference between a time-exact versus a time-ambiguous bag. Based on these latter four items, two difference scores were computed. For these difference scores, we subtracted a participant's indicated preference for an item concerning a time-exact versus time-risky bag from their preference for the corresponding item concerning a time-exact versus time-ambiguous bag (with corresponding item, we mean that the two items were identical, except that one item presented a time-risky and the other a time-ambiguous option). To make sure that all five scores (three scores from the former three items, plus two difference scores from the latter four items) were on a similar scale, we standardized them by dividing each the five preference scores by their standard deviation.

We created two versions of the experiment, counterbalancing whether bag 1 or 2 was always the more time-certain option. Scores were coded such that negative scores always indicated a dislike of time-ambiguous bags over time-risky bags (i.e., time-ambiguity aversion). The online study took around 8 min to complete, and participants received £1.20 for their participation. The study was approved by the local ethics committee.

The sample included 215 participants from Prolific (all UK-based and having a Prolific acceptance rate of at least 80%), but 13 people (6.05%) were excluded: one due to missing data and 12 due to answering at least one check question incorrectly despite having gone through the instructions twice. Specifically, after finishing the instructions, participants were asked to answer three questions to check their understanding of the bag descriptions. If at least one mistake was made, additional instructions were shown (this was needed for 30 out of 215 people; 14%). Afterwards, participants had to again answer three check questions, which were conceptually very similar (but not identical) to the original three. We preregistered to exclude the data of participants that still made at least one mistake in this second set of check questions. Of the remaining 202 participants (78 males; $M_{\text{age}} = 29.54$, range 19–35 years), 101 completed one version of the experiment and 101 the other.

To answer our research question whether people differentiate time ambiguity from time risk, we conducted two simple Bayesian mixed-models analyses. In one analysis, we used directional scores, such that preference scores could range from preferring time-risky options to not differentiating to preferring time-ambiguous options. In the other analysis, we used absolute scores, such that 0 scores indicated neutrality toward time risk versus time ambiguity (i.e., no differentiation), while scores above 0 indicated differentiating between time ambiguity and time risk, regardless of which option was preferred (collapsing across preferences for time-risky and time-ambiguous bags). Given the meaningful 0 point in the data in both analyses (indicating no differentiation), we then simply tested whether the intercept significantly differed from 0. The analysis setup was identical to that of Studies 1 and 2 apart from using more iterations (16,000, with 8000 warmup). As before, an effect was deemed significant if the 95% CI did not include 0.

Results indicated significant intercepts in both models (directional scores: $B = -0.282$; 95% CI [−0.357, −0.205]; absolute scores: $B = 1.376$, 95% CI [1.316, 1.440]). Thus, as expected, time ambiguity was differentiated from time risk (Hypothesis 6), and the directional scores indicated that time-ambiguous options were on average less preferred than time-risky options. In two exploratory models, we also included Item type (direct comparison score or difference score) as fixed effect and random slope and found again that the intercept was significant in both analyses (directional scores: $B = -0.238$; 95% CI [−0.312, −0.162]; absolute scores: $B = 1.383$, 95% CI [1.314, 1.453]). Item type was also significant in both these models (directional scores: $B = 0.182$; 95% CI [0.118, 0.249]; absolute scores: $B = -0.250$, 95% CI [−0.309, −0.191]), showing that time ambiguity was more strongly disliked/differentiated from time risk when using the three direct time-ambiguity versus time-risk preference scores compared with using the two difference scores.

These results indicate that time ambiguity has effects on choice that can be differentiated from those of time risk and, more specifically, that time-ambiguous options were on average less preferred than time-risky ones (although this directional effect may change depending on the used amounts and delay midpoints, as suggested by the results of Studies 1 and 2 and the combined sample). This fits (i) the existing literature on ambiguity (i.e., when the level of uncertainty of a choice-relevant attribute differs between options, people typically prefer the more certain option) and (ii) our hypothesis that time ambiguity is a distinct phenomenon, such that its effects on choice cannot be explained by delay discounting or time-risk preferences alone. Thus, although the results of Studies 1 and 2 and the combined sample may still in part be explained by time-risk preferences in addition to time-ambiguity preferences, Study 3 shows that the effects we found cannot be attributed to time-risk alone: Time ambiguity is empirically differentiable from time risk.

5 | GENERAL DISCUSSION

This paper investigated the effects of time ambiguity on intertemporal choice. Participants made hypothetical choices between a fixed SS option (€5 today) and LL options that varied in amount, delay, and time-ambiguity level, such that we had time-exact delays (e.g., 90 days) and time-ambiguous delays (e.g., 45–135 days). The main results can be summarized as follows: First, we found no main effect of time ambiguity; instead, time-ambiguity effects were moderated by both delay and amount magnitude. Second, when the time-ambiguity range included today—indicating a possible today delivery (PTD)—time-ambiguous options were chosen more frequently compared with when there was no PTD. Third, coding for time-ambiguity effects using absolute time ambiguity (in days) provided a better fit than coding relative time ambiguity (in %) or coding only the presence/absence of time ambiguity. Lastly, we found a clear display effect such that participants chose more patiently in the timeline than the word version.

First and unexpectedly, we found no general time-ambiguity aversion main effect (Hypothesis 1) as previously reported (Ikink et al., 2019). Instead, we observed that the effect of time ambiguity on choice differed as a function of the LL delay midpoints and amounts. This indicates that people do not always simply avoid ambiguity in the time domain but take into account other choice characteristics such as the delay and amount magnitude. This result fits the notion that people generally show some flexibility in how they deal with ambiguous options.

Second, and partly in line with Ikink et al. (2019), we found that higher levels of time ambiguity were more strongly disliked when delays were relatively short. But although we expected this aversion effect to become weaker for longer delays (Hypothesis 2), our results instead suggest that people started to *prefer* time-ambiguous options when delays increased. This crossover effect (from time-ambiguity disliking to liking for longer delays) cannot be explained by the nonlinearity of discount functions: If participants would be time-ambiguity neutral at the psychological level and would resolve time ambiguity by first hyperbolically discounting the reward at each possible delay and then averaging across these discounted values, they would prefer time-ambiguous options at the choice level, and this choice effect would become weaker given longer delays (due to the hyperbolic shape of the discounting function). Alternatively, if time-ambiguity neutral participants would simply take the midpoint of a time-ambiguous range and use that to calculate a discounted value, there would be no effect of time ambiguity on choice nor an interaction with delay.

From a mechanistic viewpoint, there are at least four possible explanations why time-ambiguity disliking might turn into liking for longer delays: First, people might simply care less about time ambiguity when they have to wait for a long time. For example, Abdellaoui et al. (2011) found that participants became more risk tolerant of lotteries that were resolved after a delay compared with immediately. Thus, participants might view time-ambiguous ranges across longer delays like a delayed lottery and become willing to accept more uncertainty by choosing more time-ambiguous options (compared with shorter delays where they prefer to know the delay). Second, longer delays might be perceived as more abstract (high-level construal), whereas shorter delays might be perceived as more concrete (low-level construal; based on construal level theory; Trope & Liberman, 2010), thereby making time ambiguity less aversive across longer delays. On a somewhat similar note, longer delays are often implicitly perceived as more uncertain (also known as the implicit risk hypothesis; see, e.g., Bixter & Luhmann, 2015). Thus, time ambiguity might be perceived as unusual or "odd" at short delays (since short delays usually entail little uncertainty), whereas for longer delays, time ambiguity might seem acceptable since options are less certain already. Thus, time ambiguity might be perceived as incongruent at shorter delays but congruent at longer delays, resulting in the observed change from time-ambiguity disliking to liking. Fourth and last, Ebert and Prelec (2007) showed that decreasing sensitivity to the time dimension results in more discounting across shorter delays and less discounting across longer delays when assuming an exponential discounting model with an additional parameter for time sensitivity. Possibly, adding time ambiguity is a way to decrease time sensitivity (i.e., by making it a less reliable source of information), thus resulting in disliking time ambiguity across shorter delays but liking time ambiguity across longer delays. We believe the last two explanations (time-ambiguity congruency and decreased time sensitivity) to be the most likely, as they most clearly predict the change from disliking to liking, whereas the first (caring less) and second (low-/high-level construal) explanations may predict reduced aversion but not per se liking. Future studies should try to disentangle these explanations. However, the computational models also suggest that there are individual differences in how time ambiguity was dealt with, suggesting multiple plausible strategies and/or behavioral patterns at the participant level.

Regardless, the observed crossover effect seems relevant for research on probability ambiguity (i.e., not having full information about the probability of winning a lottery, comparing, e.g., 50% to 25–75%). Using a single probability midpoint of 50%, quite consistent evidence for probability-ambiguity aversion has been found (see, e.g., Blankenstein et al., 2016; Tymula et al., 2012; van den Bos & Hertwig, 2017). However, based on our results, it could be possible that probability ambiguity might sometimes be preferred, for example, given probability midpoints other than 50%. Indeed, Kocher et al. (2018) showed that for lower probabilities, ambiguity liking was observed, somewhat similar to the time-ambiguity liking at longer delays that we found. Moreover, in everyday life, ambiguity is also not varied around one single (delay/probability) midpoint. Thus, adding more variation in midpoints would likely result in a better understanding of ambiguity effects, as well as increase its ecological validity. Lastly, the crossover effect that we observed in time ambiguity might be specifically important when studying impulsivity-related disorders and development: In impulsivity-related disorders such as addiction, the crossover (or switch point) might occur at a later delay (or not at all), as time-ambiguous delays might be considered specifically aversive. In contrast, the switch point might occur at an earlier delay in adolescents (compared with adults), given that adolescents often show increased probability-ambiguity tolerance (Blankenstein et al., 2016; Tymula et al., 2012; van den Bos & Hertwig, 2017) and might prefer time ambiguity at shorter delays as well.

Third, we did not confirm our hypothesis that for larger LL amounts (i.e., larger relative amount differences), the effect of time-ambiguity aversion would become weaker (Hypothesis 3). Instead, it seemed that more time ambiguity was somewhat preferred for smaller LL amounts and became somewhat aversive for larger LL amounts. Note, though, that this interaction was not found in Study 2, and time ambiguity had no significant effect for any LL amount in the follow-up analyses. Thus, it seems a somewhat weaker moderation than the one with delay midpoint and should perhaps be replicated and/or further investigated using more variation in amounts (and possibly varying not only relative amount differences but also absolute amount magnitudes). Nonetheless, this result suggests that people's willingness to accept time-ambiguous options is influenced by the relative difference between SS and LL amounts.

When taking both moderation results together, they seem to suggest that the effect of time ambiguity depends on what is at stake (like the peanuts effect in risky choice[6]; Markowitz, 1952): Specifically, when stakes are small (here represented by long delay midpoints and/or small relative monetary differences), some uncertainty seems acceptable such that more time ambiguity becomes attractive, whereas for larger stakes (here represented by shorter delay midpoints and/or larger relative monetary differences), certainty is preferred, and as such, more time ambiguity becomes aversive. This interpretation also fits the result that the crossover effect was weaker for larger LL amounts (i.e., when the stakes increased). Future research could perhaps more directly investigate this idea.

Fourth, we confirmed our hypothesis that time ambiguity is less aversive if the time-ambiguity range includes a possible today delivery (PTD) compared with when it does not (Hypothesis 4). This seems somewhat similar to the present bias (Laibson, 1997), also known as the now effect (Figner et al., 2010) or immediacy effect (Prelec & Loewenstein, 1991), where people show more pronounced impatient behavior if the SS is available now (e.g., €5 now or €8 in 2 weeks) compared with when both options are in the future (e.g., €5 in a year or €8 in a year and 2 weeks). Thus, the PTD effect is consistent with the idea that *today* is special: Even when receiving an option today is only *possible*, the option seems to become more attractive. Indeed, if one believes the LL might be delivered today, this is obviously better than the SS given the LL's larger amount. And even if participants would ultimately have to wait for the LL, that might still be fine given their focus on the larger amount. However, we should note that the confound between the PTD and delay midpoint predictor influenced the PTD effect, warranting future research to use delay midpoints that are closer together (i.e., as only reducing the confound is possible, not eliminating it).

Interestingly, coding for time ambiguity using absolute terms (i.e., in days) provided the best statistical model fit, whereas simply encoding presence versus absence of time ambiguity provided the worst fit, with relative time ambiguity coding being in between. Note that all the included effects in the model determine model fit, thus not only the main effect of time ambiguity (which was nonsignificant) but also interaction effects (which were significant). Thus, time ambiguity is better represented by a dose–response than a discrete relationship, that is, the extent of time ambiguity matters. This contrasts with the results of Ikink et al. (2019), who found that the extent of time ambiguity did not matter. However, Ikink et al. used only two time-ambiguity levels, likely explaining the apparent discrepancy in results. Also, a dose–response relationship is in line with previous work on probability ambiguity, where dose–response relationships have been found (Blankenstein et al., 2016; Tymula et al., 2012; van den Bos & Hertwig, 2017).

That absolute encoding provided a better fit than relative encoding could be interpreted as if participants would treat a time-ambiguity range of, for example, 20–40 days, the same way as a time-ambiguity range of, for example, 170–190 days, while relatively speaking the second range is much smaller. Given that many effects are encoded in a relative manner (see, e.g., Loewenstein & Prelec, 1992; Rangel & Clithero, 2012), this seems surprising. However, it is important to point out that our results do not indicate that such ranges were always treated the same. Instead, we found that a time-ambiguity range of 20–40 days was on average disliked, whereas a time-ambiguity range of 170–190 days was on average liked. Such a crossover effect could not have been identified using relative encoding, perhaps explaining its worse fit compared with absolute encoding.

Fifth, with regard to display effects, we confirmed that people chose more patiently when the LL options were presented via a visual timeline (timeline version) compared with via words/numbers (word

[6]The peanuts effect indicates that people are more willing to gamble for small compared with large amounts (i.e., small- vs. large-stakes gambles). Note, however, that this effect concerns absolute monetary magnitudes, which were not varied in the current study (the SS was fixed at €5 today).

version; Hypothesis 5). This fits previous notions that display formats can affect choice by changing the way choice options become represented psychologically (Johnson et al., 2012; Read et al., 2005). We speculate that the display effect may be due to (i) a possible anchoring effect in the timeline but not word version (i.e., in the timeline version, the maximum delay was always visible and may have served as an anchor; Furnham & Boo, 2011), (ii) a difference in concreteness (i.e., compared with the word version, timelines may have made delays more concrete resulting in more patience, like the date/delay effect[7]; Read et al., 2005), or (iii) an attention or saliency difference (i.e., amounts stood out more in the timeline than word version, as amounts were displayed in a [relatively] bigger font and the delay/time-ambiguity information was more complicated in the timeline version). The last explanation might be the most plausible, as it is consistent with the stronger amount effects in the timeline version. Furthermore, it might explain why display affected overall patience but not time-ambiguity effects, whereas the first two explanations should also impact time-ambiguity effects. Thus, by simply presenting timing information via timelines instead of words, it looks like we nudged people toward more patient decisions, which may imply that visuospatial presentation—instead of the often-used verbal–numerical display—could serve as a fruitful choice architecture tool (see also Johnson et al., 2012; Rung & Madden, 2018).

While it is challenging and perhaps premature to translate our results into real life, it is useful to speculate how the main insights and implications from our study might work out in some everyday life examples: For example, it might help decision-makers to not spend their money but set it aside for a (shorter-termed) holiday if the decision-maker knows precisely when the holiday is (e.g., in 10 months from now) instead of having time-ambiguous information (e.g., sometime between 6 and 14 months from now). In contrast, if a decision-maker would be saving for their children's future education, that is, a more long-term goal, it might help to not know when exactly the money is needed (e.g., the decision-maker does not know exactly when the children graduate from high school) instead of having precise timing information (e.g., in 18.32 years from now). Furthermore, over time, this option would become shorter term, such that at some point, the crossover would occur and saving would be easier if knowing precisely in what time frame the children would need it.

Especially when comparing with these more real-life situations, it is important to point out that we used an almost complete within-subject design, with each participant making a large number of choices. Although this is common for intertemporal and other choice paradigms, real life clearly involves less repetition, and this may affect the generalizability of our results. Indeed, using a within-subject design with many similar trials may reduce the impact of time ambiguity over repeated trials. Thus, future work could include fewer trials per participant or vary time ambiguity using a between-subjects design instead. We would speculate that the pure magnitude of the time-ambiguity effect might be bigger in a between-subjects design (where a participant would see either only time-ambiguous or only time-exact trials). However, in terms of statistical power, within-subject designs are typically better suited to detect effects in the presence of substantial individual variation. Second, since we measured time-ambiguity preferences by only taking into account delay preferences but not time-risk preferences in Studies 1 and 2, we cannot rule out that we have measured not only time-ambiguity but also time-risk preferences. Although Study 3 showed that these preferences are empirically differentiable, the study was not designed to quantify how big the influence of time risk may have been. Indeed, it was not the main aim of the current paper to disentangle the two, such that future studies should look further into this.

More generally, this paper mainly aimed to test moderators for the phenomenon of time-ambiguity aversion at the choice level, not to test the mechanisms of the phenomenon. Thus, our findings give an indication of what people ultimately do, but not why. Testing these mechanisms seems very interesting and relevant for future research. Given that this is a new field of study, one could explore many interesting future research directions. For example, how do people resolve time ambiguity? Do people simplify the problem by, for example, assuming that the actual delay will be typically shorter or longer than the delay midpoint or simply devalue an option as soon as it is time ambiguous? Alternatively, do they assume a probability distribution; and if so, what are their beliefs about this probability distribution and where do those beliefs come from? Moreover, how do time perception, discounting preferences, risk preferences, probability-ambiguity preferences, and subjective utility functions play into this? Note that for probability ambiguity, many of these issues have not yet been resolved either, thus exploring these questions in tandem in the probability and time domain would be an interesting avenue for research. Lastly, our research included only gains, whereas it would be interesting for future research to also investigate the effect of time ambiguity on losses.

To conclude, across two different samples, we show that time ambiguity impacts choice in a fashion that is dependent on the delay and amount magnitude. Furthermore, time ambiguity is empirically differentiable from time risk, further establishing time ambiguity as a distinct phenomenon that cannot be explained by discounting or time-risk preferences alone. These findings may help shed new light on the understanding of intertemporal choice in real-life situations, where, so far, the impact of time ambiguity has been ignored.

CONFLICT OF INTEREST STATEMENT

None.

DATA AVAILABILITY STATEMENT

Data, scripts, and materials are available online (https://osf.io/rhau2/files/).

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[7]The date/delay effect indicates that people choose more patiently when delays are presented as a concrete calendar day (i.e., December 10) compared with the more typical (less concrete) length of the delay (i.e., in 6 weeks).

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