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Los Angeles

Moving Through Time: How Past and Future Connections

Impact Consumer Decisions

A dissertation submitted in partial satisfaction of the

requirements for the degree Doctor of Philosophy

in Management

by

Kate Christensen

2021

ABSTRACT OF THE DISSERTATION

Moving Through Time: How Past and Future Connections

Impact Consumer Decisions

by

Kate Christensen

Doctor of Philosophy in Management

University of California, Los Angeles, 2021

Professor Hal Ersner Hershfield, Chair

Consumers often fail to save at the rate they say they want to, and my research looks at this problem from a persuasive messaging standpoint. In Chapter 1, I examine time-based behavioral appeals to help consumers reach their long-term financial goals. I test a novel intervention: instead of going forward to the future, try going back to the present. Our research suggests that where participants start their trip through time affects the financial decisions they make. To test these ideas in the field, I conducted two large-scale field studies with FinTech companies. Our partners used this research in their marketing efforts.

While Chapter 1, goes back to the present, with the goal of connecting the future self to the present self, Chapter 2 goes further back in time: to the past, with the goal of exploring the connecting between the present self and the collective past. In this chapter, I examine the role of

the past in consumers' trading decisions. Specifically, I investigate how heritage – a connection to a shared past – drives consumer valuations in market transactions between buyers and sellers. In an incentive-compatible study, I find that sellers have a lower willingness-to-accept (WTA) for heritage goods when selling to buyers with a shared heritage connection relative to buyers without heritage connection (i.e., a heritage discount). Further, I find that this heritage discount holds after controlling for buyer's usage and cannot be explained by a pre-existing relationship between buyer and seller. This heritage discount leads to a WTA/WTP asymmetry where sellers have a lower WTA for connected buyers, while perceiving that connected buyers have a higher valuation and a high willingness-to-pay (WTP) for their good. Finally, in an incentive-compatible study, I find that heritage loss (the difference in heritage connection between seller and buyer) fully mediates the discount connected buyers receive. an incentive-compatible study using real goods.

Overall, this research contributes to the financial decision-making literature, the sharing literature, and the literature on the endowment effect. My findings have marketing implications for both financial savings products and for consumer goods (e.g., collectibles) that derive product value by connecting consumers to history and to traditions that matter.

The goal of this stream of research is to go beyond exploring atomized connection between the present and the future and the present and the past to reach a broader understanding of how we as human beings connect to ourselves across time. The past and the future are not disconnected realms of memory and imagination. They are inextricably linked. Our imagination of the world to come and our own individual futures builds on our memory of both our individual past and to our psychological connection to a longer, collective past that extends far beyond the self.

The dissertation of Kate Christensen is approved.

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2021

DEDICATION PAGE

This dissertation is dedicated to the people who came before me. To my grandmother, who taught me to read on her living room floor. To my parents, whose chance meeting on a blind date many years ago created everything that came after, and whose love and time and dedication made me the person I am today. To Eliza, my little sister, who has taught me so much and who has always generous with her humor, empathy, and insight.

Most of all, thank you, Sasha, my partner in life, your kindness, creativity, and joy are wonderful to wake up to each morning. Thank you for accepting me, for taking care of me, and for being the strong, authentic, and generous human being that you are. I am lucky to be on this journey with you.

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** **Christensen, Katherine L**. and Broderick L. Turner, "Nudging Judges: An Intervention to Reduce Incarceration Rates in the U.S.," *Marketing Strategy Consortium, Austin*, December 2020

** **Christensen, Katherine L.**, Hal E. Hershfield, Mikella Green, and Gregory Samanez-Larkin, "The Role of Connection in Medication Adherence," *Harvard Kennedy School, Boston,* August 2020

** **Christensen, Katherine L**. and Suzanne B. Shu, "The Role of Heritage in Consumer Valuation,"
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- *Society for Consumer Psychology, Huntington Beach,* March 2020

** **Christensen, Katherine L**., Hal E. Hershfield, and Sam B. Maglio, "Back to the Present: How Mentally Traveling from the Future to the Present Affects Thoughts and Behavior,"
- *Association of Consumer Research, Atlanta, October 2019*
- *University of Southern California, Mather Lab, Los Angeles, November 2019*
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CHAPTER ONE

Back to the Present: How Direction of Mental Time Travel

Affects Thoughts and Behavior

Many consumers fail to save for the future at the rate they say they want to, and the current research looks at this savings behavior problem from a persuasive messaging standpoint. With the goal of helping people to take better care of their future selves, we build on a stream of research that suggests that the way people view their identity over time dramatically affects the savings decisions they make. Past research on similarity judgments between these selves across time has always started in the present and moved forward to the future, yet similarity could theoretically be measured by starting at any point in time. Thus, instead of only asking people, "How similar is the present self to the future self?", our research allows for backward time travel, also asking people, "How similar is the future self to the present self?" Across eleven studies, we find that mentally traveling from the future to the present increases similarity judgments between one's present and future self by causing time to be perceived as moving more quickly. Additionally, mentally traveling from the future to the present increases savings intentions in a laboratory study and consequential saving behavior in two large-scale field experiments.

Intertemporal choices – decisions that involve trade-offs at multiple points in time – are a key part of human experience. Yet, consumers struggle to identify with their selves across time (Parfit 1971). They steeply discount the value of rewards, over-eating and over-spending today, while planning to reform tomorrow. A growing literature has found that increasing the psychological connection between the present and future self can improve decision-making by encouraging greater allocation of resources to the future self (Parfit 1971, 1984; Urminsky 2017). Critically, an underlying premise of this literature is that the decision-maker mentally travels through time linearly, starting in the present and moving toward the future. We turn this research on its head, asking whether the direction of mental time travel can affect the relationships of these selves across time and change the way consumers save.

Consumers live in a perpetual present, yet almost every choice has a temporal component (spend on Amazon today, save for the future tomorrow). Given both the immediacy of the present and the temporal and psychological distance of the future, it is not surprising that consumers discount the enjoyment of their future selves, choosing to accelerate rewards, so they arrive in the present and delay costs, so they slide towards the future (Chen 2013; O'Donoghue and Rabin 1999; Thaler 1981; Thaler and Benartzi 2004). Hyperbolic discounting predicts that when choosing to splurge on an Amazon Alexa today, saving more tomorrow seems like a small price to pay (Laibson 1996; McClure, Laibson, Loewenstein, and Cohen 2004; Shui and Ausubel 2004). Yet, different feelings will arise when tomorrow actually comes to pass, and Mark Twain's classic question, "Why put off till tomorrow, what you can do the day after tomorrow?" may come to mind. As tomorrows turn into todays, consumers, again and again, eschew opportunities to pay off past purchases and save for the future, instead heading out the door to more spending in saying, "Alexa, open Uber."

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While procrastination is easy, maintaining the motivation to resist Amazon's Prime Day is difficult. Further research to help consumers save is needed for two reasons. First, consumers do not currently have the savings they need to comfortably survive large or small-scale disruptions. Four in ten Americans cannot pay a $400 bill out of their savings, and PwC's annual Employee Financial Wellness Survey found that employees ranked financial stress as a bigger stressor than all other life stressors combined (PwC 2020). Second, the future is uncertain. Disruptions – as large as a global pandemic and as small as an overheated engine – are inevitable, and during these disruptions, consumers still have bills to pay. To pay their future rent, future mortgage, future grocery bills, and future medical bills, consumers need money. To retire tomorrow, consumers need to save today.

Despite consumers' best efforts to plan for the future, they are often naive about future choices, overconfident in their future self-control, and they overestimate both their future saving and their future free time (Benartzi and Thaler 2013; DellaVigna and Malmendier 2006; Zauberman and Lynch 2005). Notably, all of this work presumes that when consumers consider these tomorrows, they do so from the standpoint of today. The present research asks the novel question, could mentally starting in the future, and traveling back to the present alter how consumers think about themselves over time and change the financial decisions that they make? In attempting an answer to this question, we first review the literature on mental time travel (or the way that consumers mentally project themselves through time) and then discuss the literature on identity over time.

MENTAL TIME TRAVEL

Mental time travel allows consumers to travel across personal time, remembering their past, experiencing the current present, and simulating an imagined future (Bar 2011; Corballis 2002; Suddendorf and Busby 2003; Vohs and Schmeichel 2003). Future thinking is fundamental to the way consumers pursue long-term goals (like saving for retirement), yet research to date has adopted an arguably limited conceptualization of how consumers mentally travel across time.

Although most research has understood the present moment as both the center of time (the future lies ahead; the past lies behind) and the default point of departure for mental time travel, there are indications that traveling can be both simple (today, tomorrow) and more complex (remembering the past to plan to the future and even traveling to multiple points in time simultaneously). This research suggests that the trips consumers take across time may affect the decisions they make.

RELATIONSHIP BETWEEN CURRENT AND FUTURE SELVES

A growing literature has found that relationships between the various selves that traverse these different time periods can impact real-world decisions. If, for example, a consumer sees their future self as fundamentally similar to their present self, they are more likely to turn down smaller, sooner rewards and wait for larger, later rewards (Bartels and Urminsky 2015; Bartels et al. 2011). Just as people take better care of close loved ones than relative strangers (e.g., Jones and Rachlin 2006; Rachlin and Jones 2008), they take better care of their future selves if the person they will become tomorrow feels psychologically connected to the person they are today. If spending too much today resulted in a close friend losing money they needed for paying their bills, how likely would you be to over-spend?

While empathy for a close friend is commonplace, consumers struggle to identify with the people they will become tomorrow, next year, ten years into the future, and beyond (Parfit 1984; Pronin, Olivola, and Kennedy 2008; Schelling 1984). Yet, increasing the connection between present and future selves can help consumers make better decisions today for their selves across time (Bryan and Hershfield 2012). The higher the similarity a consumer perceives between present and future selves, the more willing they are to save for retirement. Increasing similarity between the present and future selves reduces the intertemporal discount rate, enabling the present self to select more optimal decisions for the future self (Bartels and Urminsky 2015; Bartels et al. 2011).

One implicit assumption underlying this stream of research is that the present self exists in one moment, "now," while the future self exists in another moment, "later" (see also Hershfield and Maglio 2019). This linearity of time is a widely accepted premise in Western thought and a view that was shared by Aristotle. By contrast, developments in physics have suggested that "time is an illusion" and Theravada Buddhists have found that people can "experience the past and foresee the future" (Bunnag 2019; Farriss 1987). Recently, consumer behavior theorists have suggested that seeing the past, present, and future as equally visible - a "bird's eye view" of time - might help consumers make more far-sighted decisions (Mogilner, Hershfield, and Aaker 2018).

Thus, there are indications that, at least in the mind of consumers, tomorrow does not solely follow today, but can, instead, co-exist with today, or even precede it. In light of these descriptive discrepancies, we first wondered in what direction people do mentally travel through time in their daily lives.

In a pilot study, we simply asked Amazon Mechanical Turk participants (N = 250) how they naturally traveled through time. Participants were told, "There are two ways to think about time when we think about what sort of things are going to happen in the future. One way to think about time is to start in the future and mentally travel backward to the present. Another way to think about time is to start right now in the present and mentally travel ahead to the future." We then asked participants to indicate on a 0-100 slider scale, "Which way of thinking about time would you say you do?" On one side of the scale they saw, "I start in the future and mentally travel backward to the present," and on the other side of the scale they saw, "I start in the present and mentally travel ahead to the future." We randomized the prompt and slider scale to control for order effects and offered a middle option (50 on the 0-100 scale) where participants could indicate a response of "Neither. I've never thought about this." For ease of interpretation, we coded the scale such that 100 indicated forward time travel and 0 indicated backward time travel. In line with a standard linear conception of time, participants were heavily skewed toward forward time travel ($M = 77.56$; $SD = 26.93$). Nonetheless, as shown in figure 1, there was variation in these preferences. Namely, some 15% of respondents chose a response indicative of backward time travel.

This pilot study suggests that, although traveling in a forward fashion is the default, starting in the present and moving forward may not be the only way that people naturally travel through time. If time is not solely viewed in the traditional, linear manner common to Western contexts, how might different ways of traveling through time affect the relationships that exist between the present and future selves? And, can changing the way people travel through time change the relationship between their selves across time? Prior research on spatial travel speaks to how such a question may be answered.

TEMPORAL GOING HOME EFFECT

Past research suggests that the starting point of a journey affects how long the journey seems to take (Hu and Maglio 2018; Maglio and Kwok 2016; Maglio and Polman 2014). And, trips to a known destination, "home", seem to take less time than trips to a less known destination, "away" (Raghubir, Morwitz, and Chakravarti 2011): It takes less time to get mentally underway from a destination to home, less time to feel halfway home, and less time to feel almost home than it does to go to a new destination. Thus, researchers have found a "Going Home Effect" in which direction of travel changes perceptions of the time it takes to travel a fixed distance.

While the "Going Home Effect" focused on trips through the streets of a city, we reasoned that mentally traveling through time might share some of the same features as mentally traveling through space owing to a similar mental representation of the two (Kim, Zauberman, and Bettman 2012; Maglio 2020; Maglio, Trope, and Liberman 2013a). Drawing on both the mental time travel literature and the valuable past research on the Going Home Effect, we theorized that people live in the present, their temporal "home," while the future is a less known destination, "away" from home. If this theorizing holds, starting in the future and going "home" to the present might seem to take less time than going "away" to the future. Accordingly, our theory predicts that time should move more quickly when participants travel back to the present than when they travel forward to the future.

Though the present self changes into the future self over time, this progression can seem to elapse at varying rates (e.g., Quoidbach, Gilbert, and Wilson 2013). Thus, time is a key

ingredient in any changes between the present self, which exists now, and the future self, which exists later. If the passage of time separates the current self from the future self, then when time moves more quickly, the current and future selves should feel closer.

Should moving from the future to the present feels faster than moving from the present to the future, this might lead to increased feelings of closeness with the future self. This faster trip might increase similarity judgments between the selves across time. We find some support for this theorizing in the similarity judgment literature, which finds that a lesser-known item (an away) is more similar to a more well-known item (home). For example, a friend is more similar to the self than the self is to a friend (Holyoak and Gordon 1983; Tversky 1977). Taken together, we formally predict:

H1: Traveling from the future to the present will lead to higher similarity judgments between the selves across time.

H2: The effect of mental time travel direction on similarity judgments will be mediated by perceived speed of time, such that traveling from the future to the present will feel faster than traveling from the present to the future.

By increasing the speed of time and increasing similarity judgments between the selves across time, we suggest that traveling from the future to the present will have downstream benefits by helping consumers save more for their future selves. Past research in the financial decision-making literature has found that the length of perceived delays affects how people save, with delay reducing consumers' savings behavior (Kim and Zauberman 2009; Zauberman et al. 2009; Zauberman, Ratner, and Kim 2009). Further research on financial decision-making has found that similarity judgments affect how people save for their future selves (Oettingen, Sevincer, and Gollwitzer 2018), such that consumers who feel a greater sense of similarity save more. Given this past research, moving from the future to the present by way of increasing similarity may also increase investments in that future self. Thus, we predict:

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H3: Traveling from the future to the present will lead to higher savings intentions and a higher likelihood to save than traveling from the present to the future.

THE CURRENT RESEARCH

Across six experiments, one follow-up study, and one large-scale field study, we present research that changes the way consumers mentally travel through time and, by doing so, increases psychological connectedness with future selves. Throughout, we hypothesize that starting in the future and mentally traveling back to the present will not only increase similarity judgments between selves but will also lead to higher savings intentions and behaviors. In study 1a, we examine whether changing the direction people mentally travel through time can impact perceived similarity between current and future selves and replicate this effect using a shorter, non-milestone time horizon (study 1b). Next, we explore the potential mediators of the effect of mental time travel on similarity judgments (study 2a) and address alternative explanations (study 2b). We then examine whether mental time travel can increase intentions to save for the future self (study 3). Finally, we test the effect of mental time travel direction on consequential savings behavior (study 4a), and, using an advertisement for financial products, we provide evidence that mental time travel direction affects longer-term savings intentions (study 4b).

Overall, the current research makes theoretical contributions to the literatures on mental time travel, similarity judgments, identity across time, and the going home effect. Practically, this research contributes to the literature on financial decision-making and highlights an intervention that may help consumers save for their future selves. All laboratory studies were pre-registered in advance of data collection.

STUDY 1A: SIMILARITY JUDGMENTS ACROSS TIME

In study 1a, we investigated the relationship between mental time travel direction and the similarity of the present and future selves. Specifically, we examined whether starting in the future and traveling backward changes similarity judgments between the current self and the future self in ten years. Following prior work, we measure the psychological connection between past and future selves as a similarity judgment (Hershfield et al. 2011). In line with H1, we predicted that participants who traveled from the future to the present would report higher levels of similarity between their present and future selves. After assessing similarity judgment, we further tested to see if these effects would be moderated by the age, gender, or income of participants.

Method

Participants. In study 1a, MTurk participants (N = 1012, average age = 34.1, 53% female) were paid a nominal fee and randomly assigned to travel either from the present toward the future or from the future toward the present. Namely, participants were either asked how similar their present self was to their future self or how similar their future self was to their present self.

Procedure. The experiment employed a between-subjects design. Participants in the "present" condition were asked to mentally travel from the present to the future while those in the "future" condition were asked to mentally travel from the future to the present. All participants saw an initial prompt. Participants in the present condition saw the prompt, "The

year is 2018," while participants in the future condition saw the prompt, "The year is 2028."

Next, all participants were asked to make a similarity judgment. Participants in the present

condition started in the year 2018 and moved forward in time, in that they were asked, "How

similar is 2018 you to 2028 you?" By contrast, participants in the future condition started in the

year 2028 and moved backward in time, and were asked, "How similar is 2028 you to 2018

you?" In both conditions, participants judged similarity on a 7-point scale (1= not at all, 7=very

much). Finally, participants provided their age, gender, and income. For more details, please see

http://aspredicted.org/blind.php?x=mn4nv9.

Results

Similarity Judgment. In line with H1, we identified a small but significant positive

relationship between mental time travel direction and similarity. Specifically, participants in the

future condition indicated higher similarity between their selves across a ten-year time horizon

(M = 3.56, SD = 1.65) than participants in the present condition (M = 3.17, SD = 1.65, F(1,

1007) = 13.78, *p* < .001, *d* = .24). Moreover, when including age, income, and gender in the

model, condition was still a significant predictor or similarity. As a robustness check, we

included age, gender, and income in the model. Addition of demographic variables did not

change the pattern of our results, and there was no interaction between condition and any of the

demographic factors.

Discussion

Study 1a provided initial evidence of a causal relationship between mental time travel direction and similarity judgment. When participants started in the future and traveled back to the present, they indicated higher levels of similarity between their current and future selves than when they started in the present and traveled forward to the future. These effects held after controlling for both age, gender, and income. This suggests that traveling back to the present may increase the similarity between selves over time.

Despite the promising nature of these initial findings, we recognized three potential limitations in study 1a. First, our sample size in this initial study was on the larger side. Given the fact financial transactions are regularly completed at scale, we determined that even a smaller effect could be useful in the savings domain. Thus, we used the smallest effect size of interest (SESOI) approach to test the validity of our theory (Lakens, Scheel, and Isager 2018), and the sample size needed for this study was pre-registered in advance of data collection. Second, study 1 tested future self-continuity using a ten year milestone. While these results were encouraging, we wondered if our intervention would hold for a shorter, non-milestone timeframe. Third, while including a present to future condition helped us control for direction of movement through time, a reader might ask why we did not include a control condition that simply assessed the similarity of the selves across time. We agree that including a control condition could provide a further test of the value of our findings. We address these limitations in study 1b.

STUDY 1B: EXAMINING ALTERNATIVE TIME HORIZONS

The goal of study 1b was to address the limitations in study 1a. We did this in two ways. First, we tested the effectiveness of a future to present intervention on connection to a non-

milestone, shorter time frame than we used in study 1a: a six-year future self. Second, we wanted to ensure that our intervention was statistically distinct from the status quo. Thus, we included a control condition to measure similarity between participants' present and future selves.

Method

Participants. In study 1b, a total of 923 CloudResearch Approved participants on MTurk (average age = 37.9, 61% female), participated in a short study in exchange for a nominal payment. We randomly assigned participants to one of three conditions: a control condition, a present condition, or a future condition. To determine the sample size needed for each cell in this study we first reviewed data from all studies run earlier in our research process (we conducted study 1b after having completed study 1a and many of the later studies reported in this paper). Using the simcomp and multcomp packages, we simulated these data in R (Lakens, Scheel, and Isager 2018). Based on this simulation and the data from studies completed earlier, we determined that a predicted effect size of $d = .25$ would be appropriate to test. To provide a test of our theory with 80% Power, and an alpha of .05, a power analysis indicated a sample size of 301 per cell. Accordingly, we requested 903 participants in three batches, and we received 923 participants. Two of these participants reported ages beyond the human lifespan, and their ages were replaced with the mean age. Results are reported with all participants included, but are robust to a) inclusion of only the first 903 participants and b) exclusion of participants who misreported their age at the time of study.

Procedure. We randomly assigned participants to a future, presentor control condition. The future and present conditions replicated the design of study 1a with one exception:

participants evaluated the similarity of their present and future selves using a shorter (non-milestone) six-year time horizon (comparing 2021 and 2027 selves). Participants in the control condition also compared their 2021 and 2027 selves by answering the question, "How similar are your 2021 and 2027 selves?" and completed the same rating task on a 7-point Likert scale (1 = not at all, 7 = very much). Finally, we asked all participants to provide their demographic data. Methods and analysis plan were pre-registered at https://aspredicted.org/V1T_TPD.

Results

Similarity Judgment. Replicating study 1a and in line with H1, we using a simple t-test found a significant positive relationship between mental time travel direction and similarity in two pairwise comparisons. Consistent with the findings in study 1a, participants in the future condition indicated higher similarity between their selves across a six-year time horizon ($M = 3.75$; SD = 1.65) than those in the present condition ($M = 3.15$; SD = 1.70; $p < .001$, $d = .35$), and higher than those in control ($M = 3.33$; SD = 1.73; $p = .002$, $d = .24$).

The addition of demographic variables did not change the significance of our results. There was a small interaction between condition and age such that the difference between participants in the future condition and participants in both the control (B = -.03; SD = .01; $p = .023$). and present conditions (B = -.02; SD = .01; $p = .038$) was largest for younger participants. The main effect held in a model that included demographic variables and the interaction term when the future condition was compared to control (B = .40; SD = .14; $p = .003$) and when future condition was compared to the present condition (B = .62; SD = .13; $p < .001$). This

slightly reduced effect of our intervention on older consumers is consistent with prior research

that has shown that older consumers already report higher levels of perceived similarity between

present and future selves (Lockenhoff and Rutt 2017). See the appendix for details of the full

model.

Discussion

In study 1b, we replicated our preliminary findings in study 1a, and we presented

evidence that traveling from the future to the present could also affect similarity judgments

across a six-year time horizon. We tested the strength of our intervention relative to a control

condition and found a significant difference in conditions. In study 2a, we provide further

evidence of these effects and investigate mechanism by testing whether speed of time might

mediate the effect of mental time travel direction on similarity judgments.

STUDY 2A: UNDERLYING PROCESS

Study 2a had three aims. First, we sought to replicate the main effect we observed in

study 1. Second, we wanted to examine a mechanism underlying the relationship between mental

time travel direction and similarity judgments. Specifically, we theorized that if the present

represents one's temporal "home," then traveling back to the present might feel faster than

traveling forward to the future (or, what we term a "temporal going home" effect). If, as we

suspect, time moves faster when participants go back to the present, then this increased speed of

time should increase the similarity of the present and future selves (H2). Third, we entertained

the possibility of an additional mechanism based on the feature-matching work of Tversky (1977). The role of feature-matching in similarity judgments generally contends that when people start with a lesser-known concept and compare that to a more known concept, fewer features are known, fewer features are loaded, and as a result, a higher percentage of the loaded features match. In other words, when starting with a lesser-known concept and matching to a more well-known concept, the perceived similarity between the two concepts increases. As applied to similarity between selves across time, we considered that people traveling from the future (a lesser-known concept) to the present (a more well-known concept) might load fewer features in making their comparisons across time, thereby increasing similarity.

Method

Participants. In study 2a, MTurk participants (N=1099, average age = 34.1, 58% female) were randomly assigned to travel either from the present to the future or from the future to the present and were paid a nominal fee.

Procedure. The experiment employed the same between-subjects design as earlier studies. Participants were randomly assigned to the "present" or to the "future" condition. As in study 1, we measured similarity judgments. Participants in the present condition read, "The year is 2019. How similar is 2019 to 2029 you?" while participants in the future condition read, "The year is 2029. How similar is 2029 to 2019 you?" Then, participants answered two questions. To examine speed of time, participants were asked, "When you moved from 2019 (2029) you to 2029 (2019) you, how did time seem to move?" We measured the speed of time on a Likert scale (Time moves slowly, 1 - Time moves fast, 7). In addition to assessing speed of time using this

measure, in two earlier pre-tests we asked a simpler question about the speed of time, "Do you think about how quickly time seems to move, which seemed like a more natural way of phrasing the question. Our results were robust to both ways of phrasing the question, but we ultimately chose to use neutral phrasing to avoid biasing responses in the direction of our hypothesis.

To examine feature-matching, participants were asked, "When you thought about similarity, did you think about how any of the following would change?" and provided all participants with a list of possible changes (e.g., relationships, happiness, career, money, identity).

It seemed possible that participants had either not thought of any of the listed options or had thought about all the listed options. Thus, the study did not force or limit responses to this question. The order of the feature-matching and speed of time questions was randomized (please see http://aspredicted.org/blind.php?x=hh5g7g).

Results

Similarity Judgments. Replicating our findings in study 1, participants in the future condition (M = 3.46, SD = 1.69) reported higher levels of similarity compared to those in the present condition (M = 3.11, SD = 1.69), $F(1, 1097)$ = 11.99, $p < .001$, $d = .21$.

Speed of Time. In line with our predictions (H2), participants in the future condition perceived that time moved faster (M = 5.32, SD = 1.58) compared to those in the present condition (M = 4.89, SD = 1.83), $F(1, 1097)$ = 17.59, $p < .001$, $d = .253$, see figure 2.

Features. However, participants in the future and present conditions did not reveal a difference in terms of the number of features that they considered (M_{Future} = 4.30, SD_{Future} = 2.13,

$M_{Present}$ = 4.15, $SD_{Present}$ = 2.23), F(1, 1097) = 1.31, *p* = .253, failing to provide support for the feature-matching account of similarity (Tversky, 1977). We discuss this result further in the General Discussion.

Mediation. Finally, to examine whether the perceived speed of time mediated the effect of mental time travel direction on similarity judgments (H2), we conducted a mediation analysis to test if traveling from the future to the present would increase similarity judgments by increasing the speed at which time seems to move. Using 10,000 bootstrapped samples of the data, results indicated that speed of time partially mediated the relationship between mental travel direction on similarity judgments, with an estimate of *ab*=.04 and a confidence interval that did not cross zero (95% CI = [.01, .07]; Hayes 2018; Zhao, Lynch, and Chen 2010) providing support for H2.

Discussion

Study 2a provided initial evidence that traveling from the future to the present increased the perceived speed of time and that the speed of time mediated the effect of mental time travel direction on similarity judgments. These data support our theorizing that mental time travel direction makes time seem to move faster and, when time moves faster, participants consider the future self to be more similar to the present self.

While we did not observe any effect of condition on the number of features considered, it is possible that feature-matching occurs very quickly and may not be a conscious process (and thus, our methods would not have picked up on such a difference), a possibility that we return to in the General Discussion.

At this point, we have provided initial evidence of our proposed mechanism—temporal velocity—and its effect on similarity judgments. The data supports our theorizing, but other alternative accounts still remain possible. To rule these alternative explanations out, we ran two additional studies, where we tested the effect of our manipulation on various potential mechanisms. For example, mentally traveling from the future to the present requires consumers to first start their mental time travel trip at some future point. Could starting the trip at a future time point prompt a more vivid and concrete representation of the future, and thus a more vivid and more concrete representation of the future self? If the future self is more vivid, then perhaps the future and present selves seem more similar, and, as a result of this similarity, time seems to pass more quickly. The aim of study 2b, thus, was to test this alternative account.

STUDY 2B: EXAMINING AN ALTERNATIVE MECHANISM

The aim of study 2b was to examine an alternative hypothesis that might explain the pattern of our results. Since previous literature has found that enhancing the concreteness of the future self can increase similarity judgments (Hershfield et al. 2011; Malkoc and Zauberman 2006), we thought that it might be possible that starting in the future and moving back to the present might also increase the concreteness of one's future self. Thus, we examined whether traveling from the future to the present increased the concreteness of a future self. We did so using two distinct methods. First, following past research, we explicitly asked participants to assess the concreteness of their year 2031 self by describing that self and then rating the concreteness of their description on a Likert Scale (Malkoc and Zauberman 2006). Second, since explicitly assessing concreteness could prove difficult for participants, we further analyzed the

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concreteness of participants' writing by using the Linguistic Inquiry and Word Count ("LIWC"), a computerized text analysis method (Pennebaker et al. 2015), and a concreteness dictionary that contains concreteness ratings for 40,000 English lemmas (Brysbaert et al. 2014).

Method

Participants. In study 2b, MTurk participants (N = 607, average age = 31.0, 57% female) were randomly assigned to the same present or future conditions used in prior studies.

Procedure. As in studies 1a and 2a, we asked participants in this study about a ten-year time horizon. Participants started in either the year 2031 or the year 2021 and made a similarity judgment between their year 2031 (2021) and their year 2021 (2031) selves. We then asked a further question to describe their year 2031 future self, "When you moved from 2021 (2031) you to 2031 (2021) you, what was 2031 you like?" After answering this question, participants could proceed to the next screen, where we showed participants their answer again, and we asked them to self-code their responses using a three-item concreteness scale. Participant answered each on a 7-point Likert scale (1 = not detailed / concrete / vivid, 7 = very detailed / concrete / vivid) and display was randomized: "How detailed does your answer seem? How concrete does your answer seem? How vivid does your answer seem?" (α = .787). Finally, we collected demographics. Methods and analysis plan were pre-registered at https://aspredicted.org/M4G_JGD

Results

Similarity. As in earlier studies, participants in the future condition indicated higher levels of similarity (M = 3.20, SD = 1.61) than participants in the present condition (M = 2.81, SD = 1.50), F(1,605) = 9.47, p = .002, d = .25.

Self-Assessments of Concreteness. Participants in the future condition did not rate the concreteness of their 2031 self differently (M = 4.09, SD = 1.40) than did participants in the present condition (M = 4.23, SD = 1.40), F(1, 605) = 1.67, p = .197, d = .10. Thus, using the explicit measure, we found no difference in concreteness between conditions. Breaking down the three-item scale into individual items, we did find a difference in level of detail between conditions. Notably, these differences were in the opposite direction than what one would expect if concreteness was accounting for similarity differences between the future and present conditions: Participants in the future condition rated their description of the 2031 self as lower in detail (M = 3.46, SD = 1.47) than did participants in the present condition (M = 3.76, SD = 1.62, F(1,605) = 5.55, p = .019, d = .19). As a robustness check, we included both level of detail and condition as predictors of similarity judgment in an upgraded model then ran an ANOVA model comparison between the basic model (regressing similarity judgments on condition) and the upgraded model (adding level of detail as a predictor). An ANOVA model comparison found no difference between the basic and upgraded models, F(1, 604) = 2.27, p = .132, and the effect of condition on similarity remained significant in both models.

LIWC Text Analysis of Concreteness. As a further examination, we analyzed responses using the Linguistic Inquiry and Word Count text analysis program (LIWC). The LIWC program detects linguistic patterns in text using a lexicon (Humphreys and Wang, 2018; Tausczik and Pennebaker 2010). Prior work has found that the "cognitive processing" variable from its

algorithm contains an accurate representation of how abstract vs. concrete a passage of text is (for further details, see Stephan, Sedikides, and Wildschut 2012), with higher scores on the cognitive processing variable being associated with more abstract thinking and lower scores being associated with more concrete thinking. As in prior research, we processed the full corpus of participants' future self descriptions through LIWC and tested for differences in abstractness vs. concreteness using the cognitive processes variable. As with the explicit measure of concreteness, we found no differences between the future (M = 12.62, SD = 14.47) and present conditions (M = 14.26, SD = 12.94, F(1, 605) = 2.16, p = .142, d = .12). We also ran one exploratory test to assess whether the future condition led to more insights than the present condition, but we find no difference between conditions using the insight variable in LIWC (M = 2.51, SD = 7.76; M = 2.86, SD = 5.61, F(1, 605) = 2.16, p = .527, d = .05). These results also hold when the data is normalized. For more details on this study please see the pre-registration (https://aspredicted.org/blind.php?x=a6jz6m). These results are a replication of results from an earlier concreteness study, which is available in the appendix.

Brysbaert Text Analysis of Concreteness. In addition to our pre-registered analyses of concreteness, we ran one more test of concreteness using a well-established dictionary of 40 thousand generally known English lemmas that have all been evaluated for concreteness, a number more than five times higher than that included in LIWC (Brysbaert, Warriner, and Kuperman 2014). We processed the full corpus of participants' future self descriptions through this dictionary and tested for differences in both concreteness in two ways. First, we took the average concreteness of the words participants used in their 2031 self descriptions and compared this across conditions. Second, we tested for the sum of the concreteness of all words participants used in their 2031 self descriptions. As with the explicit measure of concreteness and the LIWC

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measure of concreteness, we found no differences in average concreteness of the words used in the future ($M = 2.42$, SD = .31) and present conditions ($M = 2.40$, SD = .32, $F(1, 607) = 1.06$, $p = .303$, $d = .08$), and although we did find a difference in the sum of concreteness for all words used with the future condition ($M = 25.65$, SD = 22.46) using less concrete language overall than the present conditions ($M = 32.28$, SD = 24.41, $F(1, 607) = 12.14$, $p < .001$, $d = .28$), we found that the main effect of travel direction on similarity judgments did not diminish when the sum of concrete language was added to the model, but rather increased (B = .40, $p = .001$ vs. B = .45, $p < .001$). Concreteness cannot explain our findings.

Follow-up Study 1. Since concreteness was not the only possible alternative explanation for our findings, we ran an additional study (N = 1,099) to further examine mediation. Given that our study increased similarity between the selves across time, we tested multiple models of similarity that might possibly explain our findings including the alignable differences model, the transformation model, and the geometric model (Malkoc, Zauberman, and Ulu 2005; Markman and Gentner 1993; Suzuki, Ohnishi, and Shigemasu 1992; Zhang and Markman 1998). As a further test, we also asked if unknowns of the year 2029 self varied between conditions. Finally, we included one question about the speed of time. All options were randomized. In this large-scale test of multiple competing theories, only the speed of time was a significant predictor of similarity judgments, $F(1, 1097) = 8.12$, $p = .004$, $\eta^2 = .01$.

Discussion

In study 2b, we replicated our findings in similarity judgment and examined an alternative explanation for those findings: concreteness. Neither participants' ratings of their own

responses nor a text analysis of those responses revealed evidence that the intervention increased the concreteness of the future self, thus ruling out this alternative explanation. Although we thought that concreteness was the likeliest alternative explanation for our findings, we addressed nine more alternative explanations for our findings in a follow-up study. Only speed of time mediated the effect of mental time travel direction on similarity judgments.

Thus far, across four experiments and a follow-up study, we have provided robust evidence that traveling back to the present increases similarity judgments between selves across time. Taken together, and in line with H2, these studies suggest that going back to the present may increase the perceived speed of time and that, when time moves faster, current and future selves are perceived as being more similar. Neither concreteness nor feature-matching can explain the effect of mental time travel direction on similarity judgments. Of course, other potential mechanisms might also explain our effect.

Because the ultimate goal of this research program is to enhance consumers' financial decision-making, in study 3, we sought to build on past research in intertemporal choice. Namely, prior work has demonstrated that increased perceptions of similarity between current and future selves can boost intentions to save (Bartels et al. 2011; Bartels and Urminsky 2015). In study 3, we thus tested the impact of a mental time travel intervention on saving intentions.

STUDY 3: SAVINGS INTENTIONS

The primary aim of study 3 was to examine a potential application of our intervention: helping consumers save for the future. Given that starting in the future and traveling back to the present increases similarity between the present and future selves, and given that higher

connection to the future self leads to higher intentions to save (Bartels and Rips 2010), we theorized that traveling from the future to the present would increase savings intentions relative to traveling from the present to the future.

Method

Participants. Amazon Mechanical Turk workers (N = 1025, average age = 33.1; 56% female) were randomly assigned to begin in the present and travel forward to the future or to begin in the future and travel back to the present.

Procedure. The experiment used a between-subjects design with participants in both the "present" conditions and "future" conditions asked to make a similarity judgment, and then answer a question about their intentions to save for the future. As in prior studies, participants began the study by completing a similarity judgment task (see study 1 for details of the task). Immediately following the similarity judgment task, participants were asked for their feedback on a savings promotion. In the savings scenario, participants read that their bank was offering a special savings promotion. Given that multiple banks were offering a 5% introductory rate to attract new customers at the time we conducted this study, we chose to offer participants this 5% interest rate as their bank's savings promotion (Financial Panther 2018; Gillman 2020). "This savings account returns 5% a year. After you deposit funds into the account, the money will be locked and unavailable until the year 2028, meaning that 2018 you will be helping out 2028 you," Participants then rated their likelihood to use this savings account on a Likert scale (1-Not At All, 7-Very Much). We note here that this study received slightly more workers than

requested (1,025 instead of 1,000). Pre-registration is available at

https://aspredicted.org/blind.php?x=xm9ww3.

Results

Similarity. As in prior studies, participants in the future condition (M = 3.48, SD = 1.70)

reported higher similarity judgments than participants in the present condition (M = 3.14, SD =

1.79), F(1, 1023) = 10.00, p = .002, d = .24.

Likelihood to Save. In line with our prediction, participants in the future condition

reported a higher likelihood to use the high-interest savings account (M = 4.69, SD = 1.90) than

participants in the present condition (M = 4.45, SD = 1.89), F(1, 1023) = 4.11, p = .037, d = .13),

providing support for H3.

Mediation. As a further test of the relationship between mental time travel direction,

perceived similarity, and savings intentions, we conducted a pre-registered mediation model

using 10,000 bootstrapped samples. This model indicated that similarity judgment mediated the

effect of mental time travel direction on likelihood to save. The bootstrapped unstandardized

indirect effect was .02, and the 95% confidence interval ranged from .00, .06, p=.064. After

adding the indirect effect to the model, the direct effect was not significant, and the total effect of

mental time travel direction on savings intentions was significant with an estimate of .25. The

95% confidence interval ranged from .01, .47.

Discussion

The primary goal of study 3 was to examine whether traveling from the future to the present could increase intentions to save in a controlled lab setting. While the effect size was not large, financial transactions are regularly completed at scale, and a small effect spread over many people on a digital platform could nonetheless have an impact on consumer well-being. As a result, in study 4, we examined whether starting in the future and mentally traveling back to the present would impact consequential financial decisions.

STUDY 4A: CONSEQUENTIAL SAVINGS BEHAVIOR

Study 4a tested the impact of mental time travel direction on consequential savings behavior in a large-scale field study. To evaluate the effectiveness of our intervention in the field, we partnered with a financial technology company (Dreams) based in Sweden. At the beginning of the study period, our partner provided us with access to 6,732 of their customers. The customers in this sample had relatively low savings balances in their app-based mobile savings account (M = $1,011, Mdn = $470).

Our partner offers its customers four primary options to save money for the future. We hypothesized that an intervention designed to encourage customers to travel from the future to the present (versus from the present to the future) would increase deposits in savings products.

Studies 1 and 2 found that this manipulation enhances similarity to the future self, which study 3 established as foundational in predicting allocation for the future self. Indeed, the design of study 3 mirrored the predominant approach in the existing literature, asking only about investment writ large (e.g., Bartels and Rips 2010; Hershfield et al. 2011; Pronin et al. 2008). This prior work has asked the question – *will consumers who feel closer to their future selves*

save more – not – *what investment choice will consumers who feel closer to their future selves make*. In study 4a, we replicate study 3, in a consequential context by measuring consumers' real-world savings behavior in a digital savings app.

Method

Participants. Savers (N = 6732, average age = 30.8; 79% women) were randomly assigned to start in the present and travel forward to the future (n = 3404) or to start in the future and travel back to the present (n = 3328).

Design. The experiment examined savers' choice to save (or not to save) in each of four savings options: a one-year savings plan; a one-time deposit from an external account to an investment account within Dreams; a funds transfer from an internal account to an investment account within Dreams; a "follow-the-market" savings plan, in which saving increases when the stock market rises (for more detail on these savings options, please see the Appendix). All Dreams savers had access to all of the four savings options.

Procedure. In the field experiment, 6,732 Dreams' investors were sent one push message, which was translated into Swedish to ensure that all participants could easily understand it. Participants in the present condition were sent a message asking them to move forward, "The year is 2019. Move forward to 2029. Save for 2029 you!" while participants in the future condition were sent a message asking them to rewind back, "The year is 2029. Rewind back to 2019. Save for 2029 you!"

After sending investors the push message, we observed savings behavior on the Dreams'

platform during a one-week study period. Given that we could not observe open rates on the

push message itself, all data were analyzed using a simple χ^2 Intent-to-Treat analysis.

Results

Saving. Overall, 474 participants in the future condition signed up to save in at least one

of four savings products, versus 413 participants in the present condition, and this difference in

choice to save was statistically significant (χ^2 (1) = 6.55, p = .010). Participants in the future

condition were more likely to save in the one-year savings plan, "saving plan 1" (χ^2 (1) = 6.03, p

= .014), directionally more likely to make a one-time deposit, "saving plan 2" (χ^2 (1) = 2.67, p =

.102), more likely to transfer funds from an internal account to an investment account, "saving

plan 3," (χ^2 (1) = 5.50, p = .019), and more likely to invest in a follow-the market plan, "saving

plan 4" (χ^2 (1) = 6.23, p = .013). For a visualization of investors' overall likelihood to save in

each of the investment options, see figure 3.

Discussion

Our aim in study 4a was to test the effect of mental time travel direction outside of the lab

in a consequential context with real savers. Overall, our findings in the field provide further

evidence to support our findings in our lab experiments.

In a large-scale field study, we found that traveling from the future to the present

increased consumers' likelihood to deposit their money in a long-term savings product.

Study 4a provided initial evidence that mental time travel direction affects savings intentions and behavior in a real-world context. This indicates that traveling from the future to the present may be a valuable tool to help consumers save for the future. To replicate and extend this broad claim, our next study aimed to further examine the effect of time travel direction on increasing consumers' saving intentions in a controlled online study with longer time horizons.

STUDY 4B: ADVERTISING USING MENTAL TIME TRAVEL DIRECTION AFFECTS SAVINGS INTENTIONS ACROSS A 20-YEAR TIMEFRAME

Our goals in study 4b were twofold. First, if our findings in a population of millennial, Swedish investors generalize across national boundaries, we should be able to validate these data in an empirical test using a US-based sample. We thus sought to test the external validity of these findings in study 4b. Second, all prior studies tested the effect of mental time travel direction by using a time horizon of ten years or less. Given that participants across our prior MTurk studies have an average age in their mid to late-thirties, testing a longer time horizon might more accurately reflect the savings timeframe that participants need to keep in mind to save for their retirement needs, and we wanted to verify that our intervention is effective across longer time horizons. Further, nearly all of our prior online studies, with the exception of our field study and study 1b, relied on an MTurk population. For this study, we used a more newly established platform, Prolific Academic, to test the strength of our results in a newer participant pool. Thus, our final study tests intention to invest in a Treasury bill over a twenty-year time horizon.

Method

Participants. Prolific Academic workers based in the US (N = 1000, average age = 32.7; 56% female) were randomly assigned to begin in the present and travel forward to the future or to begin in the future and travel back to the present.

Procedure. As in prior studies, we assigned participants to either a present condition or a future condition. In this study, all participants were asked to make a choice about saving. We told them that, "Hatch Bank is a new bank focused on helping customers. As part of a promotion, they are planning to give one customer $1000. Before this happens, they want to know what customers would like to do with this money. First, you will see Hatch's ad. Then, you will be asked about saving. You will only see the ad once." We then showed them an ad that asked them to start in the year 2040 and rewind back to 2020 or start in the year 2020 and move forward to 2040 (see figure 4). We then asked them to imagine they had received $1000 from Hatch Bank and asked them to choose one of three options (cash: take all the money to spend; guaranteed investment: buy a treasury bill and receive double the money in 20 years; 50/50 gamble: play roulette and get no money or double the money to spend now). As pre-registered (see http://aspredicted.org/blind.php?x=w8qb8s), and in line with H4, we hypothesized that participants in the future condition would be more likely to save for the future by buying a treasury bill than would participants in the present condition.

Results

Likelihood to Invest in a Treasury Bill. As predicted, participants in the future condition reported a higher likelihood to invest in a low-risk 20-year Treasury Bill (χ^2 (1) = 5.20, p = .023),

providing further support to H4. This effect held after age, gender and income were added to the model (B = -.28, SE = .13, z(996) = 2.22, p = .026).

Likelihood to Spend. Although not an *a priori* prediction, we found that participants in the future condition reported a lower likelihood to spend all the money now (χ^2 (1) = 5.80, p = .016), and this effect held when demographics were added to the model (B = -.30, SE = .13, z(996) = 2.32, p = .021).

Discussion

The primary aim of study 4b was to validate that traveling from the future to the present could increase intentions to choose to invest for the future in a highly controlled lab context. We found strong support for the effect of our invention on investment intentions. Participants who saw Hatch Bank's ad in the future condition were more likely to plan to save for the future by buying a Treasury bill, and less likely to plan to spend all money now. Second, we found that the effect of mental time travel direction on financial decision making is not limited to ten-year savings goals or to savers who already have targeted investment goals. The effect of mental time travel direction on investors' intention to invest for the future holds across a 20-year time horizon.

STUDY 4C: MENTAL TIME TRAVEL DIRECTION AFFECTS LIKELIHOOD TO INPUT PERSONAL DATA AND SIGN UP FOR AN INVESTMENT ACCOUNT

The primary goal of study 4c was to test the strength of our intervention in a large-scale field study with United States investors and a clear, pre-registered DV. For this study, we partnered with the college savings app, UNest, which seeks to help parents and families save more easily for important events like sending their kids to college. When investors complete the sign-up process with UNest, $10 is automatically deposited in their account. Crucially, signing up to invest has a high barrier to entry: parents must input their private family data. UNest has contact information for tens of thousands of users who have contacted them about their products or begun the sign-process, but who have not provided completed the onboarding flow and received their first deposit. They asked us to help them convert these users into subscribers who would receive a welcome $10 deposit and begin their investing journey.

Method

Participants. Consumers who had contacted UNest or one of their subsidiaries, but who had not yet finished the sign-up process (N = 26,979) were randomly assigned to begin in the present and travel forward to the future or to begin in the future and travel back to the present. We pre-registered sample size in advance (https://aspredicted.org/blind.php?x=2878kw).

Procedure. As in prior studies, we assigned participants to either a present condition or a future condition. In this study, participants were contacted twice over a one-week period. Depending on their contact preferences, users received an email or a push message on their mobile device with a time travel direction" The Year is 2031 (2021)**.** Rewind back (Move forward) to 2021," All users were then advised, "Save now for college and get $10 on us." Providing additional support for H4, we hypothesized that participants in the future condition would be more likely to input

their personal data, accept a \$10 sign-on bonus, and commit to invest in a new savings app than would participants in the present condition. Since we cannot observe if participants have notifications enabled on their phones and we do not know if they received the email, we will analyze all data using an Intent to Treat analysis.

Results

Likelihood to Complete Sign-Up Process. As predicted, participants in the future condition were more likely to complete the user flow by inputting their personal data than participants in the present to future condition (χ^2 (1) = 9.39, p = .002), providing further support to H4. We note here that although we cannot observe demographic data, all participants were randomly assigned to condition as is standard practice at UNest.

Discussion

In study 4c we planned to provide further evidence that traveling from the future to the present could increase real-world financial behavior. To do this, we pre-registered the sample size and analysis in advance of data collection and used a simple and clear DV: completion of the onboarding user flow. As predicted, participants in the future condition were more likely to complete the flow than participants in the present condition. The mental time travel intervention held in a real-world context with a concrete financial pay-off: \$10.

GENERAL DISCUSSION

People frequently fail to save for the future at the rate they say they want to, and the current work provides a novel strategy to help consumers close this gap: travel back to the present from the future. Across six experiments, one observational study, one follow-up study, and one large-scale field study, our findings demonstrate that mental time travel direction can affect how similar consumers feel to their future selves as well as the actions they take on behalf of those distant selves. Namely, we provide evidence that, relative to traveling from the present forward to the future, traveling from the future back to the present can increase similarity judgments between the selves across time. Moreover, we reveal process evidence that traveling back to the present may increase the speed at which time seems to move, which we term "a temporal going home effect." Perhaps most important, our subsequent studies reveal that such backward mental time travel can improve financial decision-making by increasing intentions to save in a lab context and increasing savings behavior in a large-scale field study with actual investors.

Theoretical Contributions

The present findings have relevance to a number of different literature streams. First, work on future self-continuity has found that when consumers feel psychologically similar to and connected with their future selves, they discount the future less (Bartels and Urminsky 2015; Bartels et al. 2011; Ersner-Hershfield, Wimmer, and Knutson 2009; Oettingen et al. 2018). The current research proposes a potential strategy to increase perceptions of similarity between selves, which can, in turn, lower behaviors that resemble temporal discounting. Namely, we find

that traveling back to the present can increase future self-similarity and that this increased sense of similarity can change financial decision-making by increasing intentions to save and driving consequential saving behavior.

This work also makes contributions to the literature on the Going Home Effect, which finds that perceived trip duration varies as a function of direction (Raghubir et al. 2011). Specifically, the same trip seems to take longer when traveling away from home than when going home. The present work extends this finding in the marketing literature by applying the Going Home Effect, a theory that explains why trips to a physical home can feel faster, to movement across time. We find that, like travel through space, travel through time seems to take longer when traveling away (to the future) than when traveling home (to the present), providing preliminary evidence of a Temporal Going Home Effect.

Further, the present research extends the similarity judgment literature. Similarity judgments, by default, compare a better-known concept, the prototype, to a lesser-known concept, the variant. Crucially, past research has found a similarity judgment asymmetry (Holyoak and Gordon 1983; Tversky 1977). Yet, the concepts tested in these similarity judgments – nations, shapes, and people – are all in existence. To our knowledge, the current research is the first to test this similarity asymmetry across time, where one piece of the comparison, the future self, does not yet exist. Critically, the future self will only exist once the present self ceases to be. Yet, we find that, even under these conditions, the similarity asymmetry holds. This research demonstrates that by reversing the standard comparison – by starting with the future self and comparing it to the present self – we can increase similarity judgments between the selves across time.

This research also contributes to the literature on backward planning that finds that going back in time (from the goal to the present) can lead to more realistic estimations of task completion time (Buehler et al. 1994; Buehler et al. 2010; Halkjelsvik and Jorgensen 2012). Past work in backward planning has found that when a planner starts in the future, at a goal, and moves back to the present, they arrive at a higher time estimate for project completion than when they start in the present and move forward in time (Wiese, Buehler, and Griffin 2016). Thus, a backward planner realizes that they need more time to achieve their goal – and the changes they want to make to a project - than a forward planner would predict. Crucially, the backward planning literature has looked at tasks that are already planned or in process.

The current research suggests that traveling backward can create value even for consumers who do not already have a plan. We find that travelers who go back to the present judge their future self as more similar to their current self than a forward traveler would predict. We explain this finding by demonstrating that time moves faster when traveling back to the present, and that this altered conceptualization of time leads to a feeling that the future self and the present self are not that different after all. Further, we find that when the future self feels psychologically closer to the current self, consumers are more likely to commit to saving and to investments designed to help the future self.

Future Research

Future work could further examine the mechanism of the effect of mental time travel direction on similarity judgments and savings behavior. The current research demonstrates an effect of mental time travel direction on similarity judgments and preliminary support for

mediation by the speed of time. We had also expected to uncover more support for the features

of similarity theory (Tversky 1977). In Tversky's theorizing, comparing a lesser-known concept

to a prototypical concept leads to the loading of fewer features, a higher match percentage

between concepts, and higher similarity judgments. Although these data could not validate

feature-matching empirically, the feature-matching theory does not conflict with the current

findings. For example, it may be the case that loading fewer features when traveling between two

points in time leads to a perception that time moves more quickly. While people might not have a

lay sense of how many features they consider when making a judgment, everyone understands

that time moves quickly sometimes and slowly at other times. The lack of empirical support for

the features of similarity is, then, more likely a limitation of the empirical methods we employed

rather than invalidation of the features of similarity theory.

Nevertheless, it seems at least possible that feature-matching across time may differ in

some critical ways vs. feature-matching across people or nations. While some features about

friends and nations can be considered known, it could be argued that the future is largely

unknown. Thus, future research could explore how similarity judgments of a known present and

unknown future differ from similarity judgments of more prosaic concepts such as two nations or

two shapes.

Practical Implications

This research has significant implications for companies, governments, and individual

decision-makers looking to help reduce the savings shortfall. While this paper has examined the

potential of mental time travel direction to drive behavior change in millennial investors in

Sweden, we suggest that a similar intervention could be tested by financial institutions looking to increase savings behavior. The present research might also apply to Social Security claiming in the United States, where early claiming can reduce retirees' benefits by as much as 30% over the lifespan (Epperson 2015). If changing the direction people travel through time can speed up time, perhaps postponing claiming for an additional six months or a year might make deferral an easier choice for potential retirees. The present research could also apply to consumer and student loan debt. Temporal discounting not only affects the rate at which people save it also impacts how much they choose to borrow (Greenberg and Hershfield 2018). In the case of student loans, a simple mental time travel manipulation before asking students how large a student loan they would like to borrow might decrease the amount students borrow. While this paper has examined the potential of mental time travel direction to drive behavior change in the domain of financial decision-making, traveling back to the present could also affect change in non-financial domains that benefit from higher future self-similarity including eating, education, healthcare, and exercise.

This research started with an observational study that asked consumers how they travel through time. We found natural variability in the way people conceptualize time: While most people travel forward in time, some do travel backward. The rest of this paper has experimentally induced changes in the way consumers travel through time, asking an experimental condition to travel back to the present. Across nine experiments, we find that mental time travel direction can alter the way consumers see time, how close they feel to their future selves, and how they choose to invest for the future.

We have not yet examined how long the effect of mental time travel direction lasts. Can shifting mental time travel direction have an impact beyond the experimental outcome – can it

shift the way consumers conceive of time and the choices they make in their daily lives? How long does the effect of mental time travel direction last, and how can it be strengthened? By delving more deeply into the mechanisms of mental time travel that connect the present to the future or the future to the present, future research can help consumers more effectively take care of their future selves, by connecting them not just to the day-to-day concerns of the present moment, but to themselves across time.

CHAPTER TWO

The Role of Heritage in Consumer Valuation

From our alma mater to our DNA, heritage – a connection to a shared past – generates consumer value. In five studies, this research demonstrates that sellers have a lower willingness-to-accept (WTA) for heritage goods when selling to buyers with a shared heritage connection relative to buyers without heritage connection (i.e., a heritage discount). This heritage discount holds after controlling for buyer's usage, cannot be explained by similarity, and remains even when sellers perceive that this buyer has a high willingness-to-pay (WTP). We find a WTA/WTP asymmetry, and we provide process evidence that the effect of the buyer's identity on the seller's WTA is driven by heritage loss. The current research contributes to the literatures on sharing, sentimental goods, psychological ownership, and the endowment effect. The findings have marketing implications for consumer goods (e.g., collectibles) that derive product value by connecting consumers to history and traditions that matter.

Heritage – a connection to a shared past – generates value for consumers. Furthermore, consumers' interest in connecting to the people who came before them has been growing (Belk 1990). From the popularity of heritage travel on Airbnb to the trendiness of traditional fabrics in England, guochao style in China, and the cottagecore movement in the United States, consumers have increasingly chosen to buy goods that connect them to a shared past (Achim 2019; Bowman 2020; Duveau and Dumenil 2018; Luo 2019; Rapp 2019; Saunders 2018; Turner 2019). By 2021, 100 million at-home ancestry tests will be in-market globally as consumers buy access to their genetic heritage (Regalado 2019).

Connecting to the past can strengthen identity, provide stability, create meaning across time, and increase positive affect (Baumeister 1991; Baumeister et al. 2013; Friedman 1992; Wildschut et al. 2006). In an 8,000 person survey, 57% of Americans said they would give up alcohol for a year to enjoy one heritage trip (Killiam 2019). Closer to home, the heritage goods consumers own and access – objects that connect their owners to history and traditions that matter - offer clear wellbeing benefits (Fujiwara, Cornwall, and Dolan 2014; Sarial-Abi et al. 2017).

This project examines the impact of heritage, and especially a shared past, on how consumers value the goods they own when thinking about selling them to others. We offer two main sets of findings. First, our work demonstrates that there is value in goods that connect us to the shared or archetypal past. We find that a good's connection to that past can increase its valuation in the present. Crucially, while sentimental goods connect a seller to a personal memory (Sedikides et al. 2008; Yang and Galak 2015), heritage connects the owner to a shared history and to the people who came before. This leads to a critical distinction. Heritage

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connection can be shared between buyers and sellers, and this shared heritage connection can exist with or without the benefit of personal memory.

Second, our work contributes to the literature on endowment and psychological ownership, which states that consumers place a higher value on the goods they own than the goods they do not own (Thaler 1980). We find that while a good gains value through association with an individual owner, it also gains value through its connection to a collective past. But surprisingly, sellers high valuations of heritage goods do not always result in a higher willingness to accept (WTA); we show that a seller's WTA for buyers who connect to the good's heritage is lower than the WTA for strangers, even when sellers perceive that connected buyers have higher valuations and a higher willingness to pay (WTP).

In the next section, we start by defining heritage connection, and then relate how the current work on heritage connection builds on previous findings in the literature on psychological ownership, the literature on sharing, and the literature on sentimental goods. We then report results from an incentive compatible exchange that examines how shared heritage connection between buyers and sellers drives product value in market transactions. We provide process evidence in four additional studies. This research, to the best of our knowledge, is the first to quantify the value of measuring connection to the shared past as a source of product value in market transactions between buyers and sellers.

DEFINING HERITAGE CONNECTION AND HERITAGE LOSS

We consider a heritage good to be a good that is connected, whether symbolically, historically, or otherwise, to the shared past of a meaningful group identity for the buyer or

seller. Heritage goods provide value for consumers by connecting them to the past and to people who lived long ago in a "suppression of time" (Baudrillard 1968). This symbolic link to a collective past generates a good's heritage connection for buyers and sellers. Specifically, we define heritage connection as a good's capacity to bridge the gap between the seller (buyer) and the people they care about by helping them remember where they come from, associating them with family or community traditions that matter, giving them a sense of history, and connecting them with the people who came before them. Our research suggests that a good gains value not simply through its connection to an individual owner but also through its symbolic connection to a larger, shared history.

One implication of this shared history is that when a high heritage seller trades with a high heritage buyer that is also connected to their shared past, they lose less of a connection to the good's shared heritage than when they trade with a less connected buyer. To account for this, we define and calculate the heritage loss due to trade as the seller's heritage connection minus the seller's perception of the buyer's heritage connection. We interpret this heritage loss as a transaction disutility that a seller may incur in the sale of a heritage good.

Now that we have introduced the concepts of heritage connection and heritage loss, we proceed with our theoretical development. Overall, the present research extends past research by hypothesizing and providing empirical support for a theory of heritage connection and heritage loss that predicts how products hold or lose value through their association with the shared past.

THEORETICAL DEVELOPMENT

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In what follows, we develop our theory by describing our hypotheses for the main effect of the buyer's connection to the past on seller's WTA, moderation of WTA by size of the seller's heritage connection, and mediation by heritage loss (when the good is sold to a buyer disconnected from the good's heritage). A premise of this research is that heritage – a connection to a shared past and traditions that matter – is a source of product value. Heritage adds value to a good by serving as a conduit that connects the seller to a shared past and traditions. However, the effects of heritage on sellers' willingness to accept (WTA) can operate in a way inconsistent with strategic pricing and standard economic theory. Specifically, we predict that a seller with high heritage connection to a good will demand *less* from a buyer who also shares the seller's high heritage connection (and thus the seller's higher utility and valuation) than from a buyer without such connection. This is surprising since it is often assumed in market transactions that a buyer's higher utility for a good should increase the seller's WTA.

Imagine, for example, an individual who owns a teacup from a small town that connects to their family's history many generations ago. To the owner, the teacup holds high value, and the owner would require a high reimbursement to give it up. When approached by a stranger to sell the item, the owner's willingness to accept will be quite high. If that stranger is also a collector of old teacups, for whom the utility of this teacup is high as a potential part of a larger collection, then a rational seller may decide to raise the WTA even further. However, if the stranger is connected to the same small town, and wants a good that maintains that connection to those who came before, the owner may paradoxically offer a lower WTA even though this buyer's WTP might be quite high. In other words, we suggest that, contrary to what economists might expect, when sellers value a heritage good more, they set lower prices for heritage buyers. Thus, we formally predict:

H1: Sellers have a higher WTA for heritage goods when selling to buyers without heritage connection relative to buyers who share this heritage connection (i.e., the heritage discount).

H2: Sellers' magnitude of heritage connection to a good moderates H1, such that greater heritage connection leads to a larger heritage discount.

Why do we predict that sellers will reduce their WTA when selling to buyers with a shared heritage connection despite these buyers' higher valuation and utility for the heritage good? First, while past research finds that sellers of emotionally laden items have high valuations for their goods (Yang and Galak 2015) these high valuations do not always result in higher selling prices. Sellers of emotionally laden items have a lower WTA for buyers whose future usage of a good they deem appropriate (Brough and Isaac 2012). This stream of research suggests that heritage buyers might, by demonstrating their acceptable usage plans, benefit from a reduced price for heritage goods. However, we note that this prior work assumes knowledge about the buyer's usage. While differences in usage clearly have value, we argue that shared connection to a collective past – with or without differences in appropriateness of usage– is sufficient to generate a discount in market transactions between buyers and sellers. Thus, we expect that H2 will hold even after controlling for the buyer's intended usage.

Second, the literature on psychological ownership has found that extending ownership beyond simple legal ownership of the object (e.g. by taking a photo of the good) may increase the seller's willingness to dispose of goods they care about (Chu 2018; Kellett and Holden 2014; Winterich, Reczek, and Irwin 2017). Since heritage goods connect individuals to a shared past, their psychological ownership is, at least in part, collective (Belk 2010, 2017; Curasi et al. 2004; Epp and Price 2008; Lastovicka and Fernandez 2005; McCracken 1986). Due to the collective nature of this shared past, when heritage goods pass from one member of this group to another,

the former owner will no longer call the heritage good "mine", but they might still call the good "ours" (Lastovicka and Fernandez 2005). Collective psychological ownership can thus extend past the point of the seller's legal ownership of the object, reducing feelings of loss when the good is physically surrendered.

As a result of these two forces, we expect a collective endowment for heritage goods that results in lower pricing for buyers who share this heritage connection and higher pricing for buyers with no such connection (H1). Furthermore, we expect this effect to be moderated by the seller's heritage connection to the good (H2), which may represent the seller's feeling of collective endowment. We test these hypotheses in our studies by explicitly measuring sellers' WTA for heritage goods when from both connected and disconnected buyers. Further, we measure heritage connection (the strength of the link to a collective past through a material good). Crucially, we also expect H1 and H2 to hold after controlling for the buyers' intended usage of the good.

In addition to predicting moderation by a seller's heritage connection, we also hypothesize a mechanism driving the reduction in sellers' WTA for buyers who shares a connection to the past: heritage loss. Heritage loss is defined as the heritage connection of the seller minus the seller's perception of the heritage connection of the buyer. Similar to the effect of loss aversion on the sale of individually owned goods (Kahneman et al. 1991; Knetsch and Sinden 1984; Thaler 1980), we hypothesize that larger feelings of heritage loss will reduce the value of the trade for the seller and thus increase the seller's WTA. In other words, we theorize that heritage loss is a transaction disutility that increases sellers' WTA for goods that connect to a shared past. Formally, we predict:

> **H3:** Sellers' concern with heritage loss (i.e., the difference between a seller's heritage connection and a buyer's perceived heritage connection) mediates H1.

By explicitly connecting to the seller's shared past, we find that the buyer can move from facing a heritage premium to a receiving heritage discount. Crucially, this heritage discount stems from a connection to a collective past and not simply from a personal relationship between buyers and sellers. In the studies that follow, we empirically demonstrate that a seller's WTA depends on the heritage loss when a good changes hands.

Taken together, these hypotheses generate our model of the value of heritage goods (see Figure 5). To produce our model of the value of heritage goods, we first measure the seller's heritage connection to a good. Second, we manipulate the buyer's perceived heritage connection to the same good by highlighting the buyer's ties to the good's heritage. We then measure the effect of these manipulations on the buyer's perceived heritage connection to the good, and we calculate the implied heritage loss when the heritage good transfers from seller to buyer. Finally, we measure the price the seller sets: the seller's willingness to accept (WTA).

OVERVIEW OF EXPERIMENTS

We tested these hypotheses in five main experiments, two additional experiments, and one follow-up study by using real goods that consumers evaluated in an incentive compatible choice task, we then tested actual goods consumers had in their homes, and we further explored the proposed mechanism using hypothetical and real goods in a final set of controlled studies.

First, we tested our theory that increasing the buyer's perceived heritage connection to a good reduced the seller's asking price in an incentive compatible design with real goods. To do so, we showed California residents a commemorative hat symbolizing 250 years of history. Then, we measured participants' actual WTA using the Becker-DeGroot-Marschak test, while

testing for buyer's valuation of the good, buyer's usage of the good, and buyer's similarity to the seller (study 1). We replicated this main effect in study 2 and began to examine the underlying process by asking sellers to provide their WTA for an item they had in their home. We measured a novel determinant of the difference in prices between inheritors and purchasers: the good's heritage connection for the seller. We then quantified the effect of the seller's heritage connection on their WTA. Our main effect holds after controlling for psychological ownership, market value, and demographic variables. Further, we present solid evidence of moderation by the seller's heritage connection (study 2). Next, in a study that controls for social proximity of buyer and seller, we find a WTA/WTP asymmetry such that although sellers perceive that heritage buyers have a high WTP for a good, they will accept a lower price (study 3). Then, we provided process evidence that the effect of buyer's identity on the seller's WTA is driven by heritage loss (study 4). Finally, we used a Becker-DeGroot-Marschak task to run a final additional incentive compatible study at a large American university (study 5), where we found further evidence of heritage loss using real goods that participants chose to sell or keep.

Overall, in five experiments, two pre-tests, and one follow-up study, we provide strong evidence that heritage loss predicts the seller's WTA for goods that transmit meaning across time. Taken together, these studies contribute to a better understanding of how the shared past can affect market transactions in the present. Across all studies, predictions and sample size were pre-registered in advance of the experiments. Participant instructions, stimuli, ancillary analyses, and figures are included in greater detail in the Figures section. Studies were pre-registered on aspredicted.org in advance of data collection.

STUDY 1: SELLING A HERITAGE GOOD FROM MONTEREY

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To investigate the effect of a buyer's perceived heritage connection on the seller's willingness to accept for a heritage good, we designed study 1 to be incentive compatible by using a real object (a hat) and including a Becker-DeGroot-Marschak elicitation method, a well-validated process designed to collect participants' WTA in trading environments (Becker, Degroot, and Marschak 1964). In this study, we used a probabilistic incentive compatible design, where we randomly selected one participant one participant to receive the results of their choice. Our goal in this study was to investigate if the seller would accept a lower payment from a buyer with a higher connection to the good's heritage. Crucially, we examined if this reduction in WTA would hold after controlling for buyer usage intent. Finally, we assessed the role of buyers' valuation of heritage goods in participants' trading decisions.

Method

All participants included in the study were residents of the state of California, and the endowed heritage good evoked California's history. We asked all participants to complete the study as if they already owned the good. We offered participants the opportunity to sell their heritage good, and we manipulated the connection of the buyer to the good's heritage (high vs. low). We predicted that sellers would accept a lower price for a good when selling to a buyer with a high heritage connection compared to a buyer with a low heritage connection. As a robustness check, we included controls for the value of the good to the buyer and the appropriateness of the buyer's usage of the good.

In study 1, we recruited 400 California residents on Amazon's Mechanical Turk (48.3% female, average age = 36.2 years, SD = 11.97) to participate in an incentive compatible experiment. The study ran online during the 250th anniversary of Monterey, a city in California

and the home of California's first constitution, California's first scenic highway, and the first place where Californians raised the American flag. Monterey also has an aquarium, and visitors come to see it. These characteristics of the city of Monterey, and its anniversary celebration, allowed us to design a study with a 250th anniversary hat with heritage buyers (residents of Monterey) or control buyers (visitors to Monterey). For an image of the anniversary hat shown to participants, see Figure 6.

First, we showed participants their anniversary hat, we told them that someone wanted to buy their Monterey 250th anniversary hat and then we offered them an opportunity to sell their hat. In a between subjects design, we randomly assigned participants to sell their good to a heritage buyer connected to the good's history or a control buyer disconnected from the good's history. In the heritage condition, the buyer was a resident of Monterey (heritage buyer). In the control condition, the buyer was a visitor to Monterey (control buyer). To elicit sellers' WTA, we used the Becker-DeGroot-Marschak task, an incentive compatible procedure proven to be an effective measure for assessing WTA (Becker, Degroot, and Marschak 1964; Bohm, Lindén, and Sonnegård 1997; O'Donnell and Evers 2019). We informed all participants that a price would be chosen randomly and if they were selected (and had indicated a willingness to sell at that price), they would give up the anniversary hat and receive the money instead. On the other hand, if they had indicated that they were not willing to sell at that price, they would keep the anniversary hat.

Participants saw a list of prices in $.50 increments, ranging from $0 to $10 for a total of 21 prices. At each price point, participants indicated their choice to keep or sell the anniversary hat. After the study finished, Qualtrics randomization software generated a participant number and an offer price between $0 and $10, and we presented that offer to the winning participant. If the winning participant had chosen to keep the anniversary hat at the offer price, they received

the anniversary hat, and if the participant had chosen to sell at the offer price, they received the offer as a deposit in their Amazon Mechanical Turk account.

Finally, after eliciting participants' WTA, we asked participants how much value they thought the buyer would get from having the hat (Likert Scale: 1- Low Value, 7-High Value), and we assessed the buyer's likelihood to wear the hat in public, their likelihood to wear the hat in private, their likelihood to collect the hat, their likelihood to sell the hat, and their likelihood to give the hat away (all measured on a Likert Scale 1-Not Likely, 7-Very Likely). We control for buyer usage intent in our analysis.

Results

We excluded participants who had inconsistent preferences (accepting an offer of $.50, but not accepting an offer of $1.00), and we analyzed data from 366 California residents (182 participants in the heritage condition, and 184 participants in the control condition).

WTA. For each participant, we calculated the lowest price at which sellers indicated that they would sell the anniversary hat. If participants would not sell at any price up to and including $10.00, we assigned them a conservative WTA of $10.50. We predicted that sellers' WTA from the control buyer would be higher than their WTA from the heritage buyer, and this is what we found ($M_{Control} = \$5.71$, $SD_{Control} = \$3.72$ vs. $M_{Heritage} = \$4.82$, $SD_{Heritage} = \$3.59$). On average, sellers offered heritage buyers a price 16% lower than control buyers ($t(1,364) = 2.32$, $p = .021$, $\eta^2 = .02$), indicating that heritage buyers received a discount (see Figure 7). As a robustness check, we analyzed our data using the non-parametric Wilcoxon rank sum test, and found the same pattern of results, ($t(1,364) = 2.32$, $p = .021$, $\eta^2 = 8.85$). To further ensure that our effect was not solely driven by sellers with very high valuations who refused to sell at any price, we removed all participants who would not sell the Monterey hat at the highest price available

($N_{Control} = 30$, $N_{Heritage} = 22$). After removing 52 participants who would not sell at the highest price available, the effect of heritage held when analyzed using both a standard t-test ($t(1,307) = 2.01$, $p = .045$, $\eta^2 = .01$) and a non-parametric Wilcoxon rank sum test ($p = .046$, $\eta^2 = 8.88$).

Buyer Valuation. Further, we found that heritage buyers were perceived to have higher valuations for the heritage good than control buyers ($M_{Heritage} = 5.22$, $SD_{Heritage} = 1.36$ vs. $M_{Control} = 4.71$, $SD_{Control} = 1.54$, $F(1,364) = 11.14$, $p < .001$, $\eta^2 = .03$). This finding suggests that sellers gave heritage buyers a discount despite understanding that heritage buyers have higher valuations than control buyers. Next, we examined how the buyer's perceived valuation of the anniversary hat affected the seller's WTA. As expected in traditional economic theory, when sellers perceived buyers had a higher valuation for the anniversary hat, they charged more ($\beta = .45$, $p < .001$, $\eta^2 = .03$). Thus, the heritage buyers' higher valuation of the hat predicted that sellers would have a higher WTA for heritage buyers, than for control buyers, but we found the opposite to be the case, and a mediation analysis found that increased valuations competitively mediated the price-reducing effect of buyer's heritage connection on WTA (95% C.I. = .08, .50, Hayes 2012).

Buyer Usage. It seemed possible that heritage buyers might receive lower prices due to differences in the way that sellers imagine these buyers would use the heritage good. We found support for differences in usage between heritage and control buyers, but these differences in usage could not fully explain the difference in WTA. Heritage buyers were more likely to wear the hat in public (B = .56, $p < .001$), more likely to wear it in private (B = .38, $p = .038$), more likely to collect the hat (B = .35, $p = .022$), and less likely to sell it (B = -.53, $p = .004$). There was no difference in heritage buyers' likelihood to give the hat away ($p = .277$). Of these differences, only likelihood to collect and likelihood to sell affected WTA, and both variables

increased sellers' WTA (B = .28, p = .032, B = .27, p = .013). Since heritage buyers were higher

in likelihood to collect and lower in likelihood to sell, buyer usage does not offer a clear

prediction on the price charged to heritage buyers. Thus, the findings in Study 1 suggest that the

price-reducing effect of shared heritage connection between buyers and sellers may exist over

and above any effect of buyer usage. The effect of heritage connection is robust when controls

for buyer usage are added to the model (B = -.85, p = .030, η^2 = .01). Adding demographic

controls do not change these results. For more details, please see the Web Appendix and the pre-

registered models: https://aspredicted.org/blind.php?x=v3pf7j.

Similarity. Since participants might reveal a willingness to give discounts to people like

themselves (Winterich, Mittal, and Ross Jr. 2011), we designed this study to isolate the effect of

heritage connection over and above any effect of similarity. Specifically, although the Monterey

hat symbolizes a heritage connection to California's first highway, first constitution, and the first

place Californians raised the American flag, 99% of California residents do not live in the city of

Monterey. Thus, our goal in this study was to remove similarity as a possible explanation of our

results. To confirm that our manipulation was successful, we ran an additional, two-question

follow-up study, where we re-surveyed participants who had completed study 1 (N = 215, 51.7%

female, average age = 37.3 years, SD = 11.97) and asked them two simple questions: "How

similar are you to a resident of Monterey?" and "How similar are you to a visitor of Monterey?"

Participants answered both questions on a 1-7 Likert Scale (1-Not Similar, 7-Very Similar), and

we randomized the order of questions. Participants indicated that they were more similar to a

Monterey visitor ($M_{Heritage}$ = 4.69, $SD_{Heritage}$ = 1.60) than to a Monterey resident ($M_{Control}$ = 3.72,

$SD_{Control}$ = 1.61, $F(1, 38.7)$, p < .001) suggesting that higher perceived similarity between

California sellers and Monterey residents could not explain sellers' lower WTA for buyers with a higher heritage connection.

In-Group Favoritism. Although similarity between buyer and seller could not explain our effect, there are other valid alternative explanations for our results. Perhaps the sellers simply liked the heritage buyer, the Monterey resident, better than the control buyer, and felt that they belonged to the same group. If this were the case, an in-group preference rather than any link to the past could explain our results. If in-group preference drove our effect, we would expect this favoritism for a preferred in-group to extend beyond the heritage good to other non-heritage items and tasks. To test this alternative explanation, we ran a follow-up study with the same sample of participants, and assessed sellers' willingness to do a favor for "a person who has lived in (visited) Monterey, CA." We told them that participants would "like to hear about any recommendations you have of books to read about California. As a favor, please list any recommendations you have for them." Participants were not required to make any recommendations, and an RA blind to the hypothesis coded and counted the number of recommendations received. Participants provided a similar level of recommendations across conditions ($M_{Heritage} = 1.45$, $SD_{Heritage} = 1.19$) than to a Monterey resident ($M_{Control} = 1.35$, $SD_{Control} = 1.02$, $p = .596$). We found no difference in participants' likelihood to do a favor by recommending at least one book across conditions, $\chi^2(1, N = 133) = .26$, $p = .609$. For more details, please see the pre-registered models: https://aspredicted.org/blind.php?x=v3pf7j.

Discussion

The results of study 1 suggest that shared heritage connection between buyers and sellers impacts market transactions for goods that connect consumers to a shared past. When sellers indicated their WTA for a 250-year anniversary hat from a buyer less connected to the heritage

of the good, sellers charged more. Crucially, we found that this price hike occurred despite the fact that connected buyers received greater value from the object and thus might be objectively willing to pay higher prices (Luce, Bettman, and Payne 2000; McGraw et al. 2016). This effect persisted after controlling for buyer usage. Further, in one follow-up study with participants who completed study 1, we found no evidence of higher levels of perceived similarity between California sellers and heritage buyers. In sum, using real goods and real transactions, study 1 provided a demonstration of the basic effect: heritage buyers are asked to pay less for a good they value more. We found that sellers discounted their WTA for heritage buyers, and that prior theories of buyer usage, buyer value, and buyer similarity could not fully explain sellers' lower WTA.

Reasoning that in-group bias and in-group favoritism might explain these findings, we conducted a second follow-up study to see if sellers gave preferential treatment to heritage buyers. We found no difference in likelihood to do a favor by condition. This indicates that there may be something specific about heritage goods that explains this discount over and above in-group favoritism. We argue the sellers' lower WTA for connected buyers arises due to the additional value yielded by heritage connection. In study 2, we will provide process evidence that begins to explain why sellers might have a lower WTA for buyers with higher valuations.

STUDY 2: PROVIDING EVIDENCE FOR HERITAGE CONNECTION

In study 2 we provide initial process evidence that greater levels of sellers' heritage connection lead to a larger heritage discount for connected buyers. Since study 1 asked about an anniversary item that participants might not have direct familiarity with, we designed the next

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study to elicit buyers' valuations for objects they have high familiarity with: goods they already owned.

Method

In study 2, 547 participants completed a study on Mturk in exchange for payment. As pre-registered, all participants included in the study successfully completed a three-question analogy attention check, and took the survey from a valid US IP address. We used a 2 (heritage: heritage versus no heritage) × 2 (WTA: family versus stranger) mixed design; the first factor was manipulated between subjects, and the second factor was measured within subjects.

Pre-Tests. Before conducting this study, we ran two pre-tests to identify measures that could help to test for the role of shared heritage connection in sellers' WTA. In an initial pre-test, we tested ten exploratory items and reduced these items to a six-item measure of heritage connection (please see Figure 8). In a second pre-test, we ran a confirmatory factor analysis to test these six items for internal validity and found an acceptable Cronbach's alpha, $\alpha = .93$. Finally, we tested for and found acceptable discriminant validity between our measure of collective ownership, heritage connection, and a pre-existing measure of individual psychological ownership. For additional pre-test results, please see Appendix B.

Scenario. Participants were randomly assigned to answer questions about a heritage good or an ordinary good that they had in their home. By a heritage good, we mean an item that connects participants to a shared past. We specifically asked participants in the heritage condition to "think about something you have inherited that you have in your home." By an ordinary good, we mean an item that does not connect participants to a shared past. We asked these participants to "think about something you have purchased that you have in your home." To make the heritage and no heritage conditions as parallel as possible, we gave both conditions

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the same examples of home items ("a ring, a piece of furniture, a plate, a cup") and we asked all participants to think of an item that would normally cost less than $1000. After participants let us know what item they were thinking of, we exposed them to two scenarios.

WTA. In the first scenario, we asked participants to imagine that a close family member, who is not a child, wanted to buy their item, and we asked participants to indicate the minimum amount that they would accept to sell their item to a connected buyer (a close family member). In the second scenario, participants imagined that a stranger wanted to buy the item, and they entered the minimum amount they would charge a disconnected buyer (a stranger) for the same item.

Log Scale. Participants indicated their WTA on a 13-point Likert scale that ranged from $0 to $20,000 and let sellers choose the price that most accurately reflected their valuation. Given that some participants might have very high valuations of their items, we expected a positively skewed distribution for their items, and a pre-test completed prior to this pre-registered study provided support for this hypothesis. To address this skew and make the evaluation of participant responses simpler, we used a log-scale to capture participant valuations of items in their home. Each value in the Likert scale was approximately twice as large as the value directly before it (e.g., $625, $1250, $2500).

We then asked participants to estimate what their item would sell for in a market ("market value"). After collecting both WTA and market value, we included some measures to better understand the mechanism of any difference in prices charged to family versus strangers, psychological ownership measures (three items), and heritage connection measures (six items).

Results

We conducted this pre-registered study with the goal of providing process evidence for the effect of a seller's heritage connection to a good on their WTA for both disconnected and connected buyers. To effectively test this hypothesis, our model also includes controls for the value of the item and the seller's psychological ownership of the item.

WTA. First, we found a main effect of condition such that heritage sellers charged disconnected buyers more for their items than control sellers charged for the items they had purchased (8.06 vs. 5.89, $F(1, 545) = 120.5$, $p < .001$, $\eta^2 = .18$), and this difference in WTA remained significant after controlling for the item's market value and psychological ownership ($B = 1.876$, $SE = .150$, $t(543) = 12.50$, $p < .001$, $\eta^2 = .18$).

Heritage Connection. As predicted, individuals also attributed higher heritage connection to goods they inherited than other individuals did to goods they purchased (5.23 vs. 3.04, $F(1, 545) = 305.6$, $p < .001$, $\eta^2 = .36$). (See Figure 9) As the seller's heritage connection to a good increased ($\alpha = .95$), the price sellers charged to disconnected buyers increased ($B = .556$, $SE = .043$, $t(543) = 12.82$, $p < .001$, $\eta^2 = .14$).

Heritage Discount. Further, we found support for moderation (H2). Specifically, we found that the greater the seller's heritage connection, the larger the heritage discount that connected buyers received ($B = 1.405$, $SE = .175$, $t(545) = 8.04$, $p < .001$, $\eta^2 = .08$). As a seller's heritage connection to a good increased, the price sellers charged to disconnected buyers over and above that charged to connected buyers increased, and this effect of heritage connection holds after controlling for ownership and market value ($B = .374$, $SE = .052$, $t(543) = 7.23$, $p < .001$, $\eta^2 = .09$). For pre-registered analyses and further details on this study please see the Web Appendix and https://aspredicted.org/blind.php?x=vg3zw8.

Discussion

Our primary aim in study 2 was to provide initial process evidence that greater heritage connection on the part of the seller leads to a larger heritage discount. To test this hypothesis, we first pre-tested a six-item measure of heritage connection and then used this measure to examine the impact of sellers' heritage connection on WTA in study 2. Specifically, we found sellers charged a price premium when selling heritage goods to disconnected versus connected buyers, and psychological ownership could not explain these findings. Further, we present strong support for our theorizing in H2: the magnitude of the seller's heritage connection to a good moderates H1, such that a greater heritage connection leads to a larger heritage discount for connected buyers.

While the results of study 2 lend preliminary support to our hypotheses, the studies rely on asking participants to identify items that they already own. A stronger test of our hypothesis would be to hold constant the item itself and manipulate heritage connection.

This will be the approach in study 3, thus keeping the item constant while manipulating its connection to the buyers' past. In addition to providing further evidence of sellers' lower WTA from heritage buyers, study 3 will investigate a potential alternative explanation of this effect: rational pricing. Yes, heritage sellers may have a lower WTA for heritage buyers, but that does not necessarily mean that WTA is too low. At this point, it seems entirely possible that connected heritage buyers might expect to pay less for a heritage good. If the seller's goal is to complete the transaction in a market, and buyers have a lower WTP, sellers could rationally reduce their WTA to meet demand. In the next study, we examine whether sellers' reduced WTA for connected buyers stems from rational expectations of participants in markets for consumer heritage goods.

In study 3, we examine a potential explanation for our findings: rational expectations of supply and demand. Studies 1 and 2 indicated that sellers provided a heritage discount to connected buyers. A parsimonious explanation of this finding is that sellers have low expectations of buyer demand and price accordingly. In this study, we rule out this potential explanation of the heritage discount and we present evidence that the reverse is true: sellers perceive connected buyers to have higher valuations and a higher WTP for heritage goods. We examine this WTA/WTP asymmetry in study 3 and find that it holds when the buyers' relationship to the seller is held constant across conditions, and when there is no difference in the appropriateness of buyers' usage. Further, we control for any effect of in-group bias by specifying in each case that the buyer is not directly connected to the seller, but rather a friend of a friend.

Method

In study 3, a Cloud Research approved pool of MTurk participants (N = 400, average age = 39.1 years; 47.3% female) participated in an online study in exchange for a nominal payment. Qualtrics randomized participants into a mixed design study using a heritage good: a set of chimes. Participants had an option to sell or not to sell a set of chimes to one buyer (heritage versus no heritage) and then, after indicating their WTA, sellers saw information about both buyers and assessed these buyers' value of the heritage good and their WTP.

Scenario. Participants read a hypothetical scenario about a heritage good that connected the owner to the sounds of the past, "You have a set of chimes, and these 1920s chimes were the first thing your great-great-grandmother bought when she moved into her first home in the

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Shenandoah Valley. These chimes make a great sound when the wind blows since they are designed to make a sound similar to the sounds of the Shenandoah River. When you hear the sound of the river in the chimes you and your family often think of the people who have lived there before you and listened to the same sounds you're hearing now."

Next, we informed participants that due to an upcoming move they had considered selling their chimes and had posted about this on social media. To control for proximity of buyer to seller across conditions, we told participants a "friend of a friend" contacted them about their post. In the connected buyer condition, we told participants, "They are interested in the chimes because they spent time on the banks of the Shenandoah River when they were young, and the sound of the Shenandoah River helps them feel connected to all of the people of the Shenandoah Valley who came before them." In the disconnected buyer condition, we told participants, "They are interested in the chimes because they are very well made, solidly constructed, and because Shenandoah chimes like these should last a long time. They like the high quality of the tone, and the chimes will make a first-rate sound for them to listen to." To control for future preservation of the chimes, we assured all participants that this friend of a friend, "would take care of your chimes in the same way you would".

WTA and WTP. After reading the prompt, sellers indicated their WTA from the heritage (control) buyer using a Becker-DeGroot-Marschak test ($0 - $90). Participants who would not sell at $90 were assigned a conservative WTA of $91. Then, they evaluated the appropriateness of the buyer's planned usage of the item. After completing the BDM task, all participants, regardless of condition, assessed both heritage and control buyers' valuation of the chimes on a 1-7 Likert scale (1-Not at all, 7-Very much), and estimated how much each type of buyer would be willing to pay for the chimes on a slider scale with endpoints determined from different

participants' responses in a pre-test (range: $20 to $200). Value and WTP measures were completed within subject and the order of evaluation was randomized by Qualtrics' survey platform.

Results

The results of study 3 demonstrate that seller perceptions about the buyer's WTP cannot explain the heritage discount. Instead, we find that sellers accept less for their chimes from buyers they perceive as willing to pay more.

According to our pre-registered plan, we did not include participants who had inconsistent preferences (accepting an offer of $60, but not accepting an offer of $72). We provide results for all participants who cleared this threshold (N = 383).

WTA. We found strong evidence of H1: sellers had a lower WTA for connected buyers ($M_{Heritage}$ = $46.44, $SD_{Heritage}$ = $27.15) than for control buyers ($M_{Control}$ = $54.51, $SD_{Control}$ = $26.45, $t(380)$ = 2.94, p = .003, η^2 = .02).

Value. Despite the fact that sellers had a lower WTA for connected buyers, they indicated that connected buyers valued the chimes more ($M_{Heritage}$ = 6.31, $SD_{Heritage}$ = 1.34) than control buyers ($M_{Control}$ = 5.16, $SD_{Control}$ = .84, $F(764)$ = 202.4, p < .001, η^2 = .21).

WTP. Sellers perceived that heritage buyers also had higher WTP for the chimes ($M_{Heritage}$ = $100.79, $SD_{Heritage}$ = $51.58) than did buyers in the control condition ($M_{Control}$ = $86.84, $SD_{Control}$ = $49.78, $F(764)$ = 14.5, p < .001, η^2 = .02). All effects held after controlling for demographic variables, and these results reveal an asymmetry between WTA and WTP in which WTA is lower and WTP is perceived as higher for heritage buyers (please see Figure 10). For further details please see the Web Appendix and http://aspredicted.org/blind.php?x=x94ar7.

Discussion

In study 3 we sought to determine if heritage buyers were perceived to have a lower WTP for a heritage good. If so, this reduced expectation for buyer WTP could explain the heritage discount we observed in studies 1 and 2. We did not find support for this explanation of our findings. Instead, study 3 suggests that the reverse might be true: sellers perceived connected buyers as having a higher WTP for their chimes than control buyers. Yet despite judging connected buyers' WTP to be high, sellers indicated a low WTA for these high WTP buyers, a WTA/WTP asymmetry.

Thus far, we have preliminary evidence to support a heritage discount, and we have found a WTA/WTP asymmetry, suggesting that the discount that connected buyers receive cannot be explained by sellers' rational expectations of demand or even more recent theories of individual ownership. Instead, this study suggests that collective ownership and a connection to a shared past might drive this WTA/WTP asymmetry. We will examine how connection to a shared past might mediate the effect of buyer's identity on the seller's WTA in the next study, and we will look at this mediation through the lens of heritage loss.

STUDY 4: MEDIATION BY HERITAGE LOSS

In study 4, we investigate the effect of heritage loss on the seller's WTA. To do this, we hold the seller's heritage connection to the good constant and manipulate the buyer's perceived heritage connection in a between subjects design. We hypothesized that sellers of a heritage good (a watch) would sell it for less to buyers with a shared heritage connection than they would to buyers in a control condition, and that this effect would hold after controlling for buyers' usage. Our findings support this theorizing, and we provide evidence that the seller's heritage loss (the

difference between the seller's heritage connection and the buyer's perceived heritage connection; see Figure 11) mediates the effect of condition on sellers' WTA for their watch.

Method

In study 4, MTurk participants who had passed four attention check questions in a paid pre-screener designed to filter out bots and non-English speakers (N = 400, average age = 37.7 years; 55.5% female) were invited to participate in the study (please see Web Appendix for pre-screening questions). Since we pre-screened all participants prior to launching the study, we did not pre-register any additional exclusions for this study.

Scenario. We asked participants to imagine that they had been going through storage and looking at things they hadn't looked at in years. As they looked through their storage, they found an old pocket watch left in a package marked, "for the future." Participants were told that as they looked more closely they would see, "The name of your great grandfather is inscribed on the watch." After participants read this scenario, they wrote about what the watch meant to them. Finally, they rated their heritage connection to the watch.

WTA. After participants answered questions about their watch, a functional good that they could use to tell time, we told them that a connected (control) buyer would like to purchase their watch, "A person who works in the same line of work as your great grandfather [in an office] and is connected to your great grandfather's contributions to that line of work [enjoys looking at pocket watches and antiques and curios of all kinds], has been looking for a watch like the one you have. They want the watch so that they can connect to the past and take out the watch and remember the people who came before them [take it out of their pocket to look at the time and so that they can enjoy listening to time tick away] as they go through their daily work." Participants indicated their WTA on a BDM task by accepting or rejecting 16 offers from $48-$1049 (offer

prices were evenly spaced in $91 increments to avoid anchoring sellers to even-numbered prices or salient price points such as $1000). Participants who would not sell at any price were assigned a conservative WTA of $1050.

Heritage Loss. Sellers assessed their heritage connection to the watch and then rated the buyer's heritage connection to the watch. Finally, we asked participants to indicate how appropriate they thought the buyers' usage of their watch would be on a 1-7 Likert scale. (1-Not Appropriate, 7-Very Appropriate).

Results

The findings in study 4 provide further support for our theorizing that shared heritage connection matters and that heritage loss increases sellers' WTA.

Seller's Heritage Connection. As planned, sellers' own heritage connection to their watch did not vary between conditions ($M_{Heritage}$ = 6.29, $SD_{Heritage}$ = 1.73; $M_{Control}$ = 6.35, $SD_{Control}$ = 1.91, $F(398)$ = .13, p = .888).

Buyer's Heritage Connection. Sellers rated connected buyers as having a higher heritage connection to the watch ($M_{Heritage}$ = 5.46, $SD_{Heritage}$ = 1.05) than control buyers ($M_{Control}$ = 3.41, $SD_{Control}$ = 1.49, $F(398)$ = 252.7, p < .001, η^2 = .39).

WTA. We replicate our support for H1: sellers had a lower WTA for their watch when selling to connected buyers ($M_{Heritage}$ = $768.21, $SD_{Heritage}$ = $295.55) than to control buyers ($M_{Control}$ = $848.96, $SD_{Control}$ = $348.28, $t(386)$ = 2.50, p = .013, η^2 = .02). Connected buyers received a WTA $81 lower than control buyers for the same watch.

Appropriateness of Buyer's Usage. We found no difference between conditions in appropriateness of buyer's usage, with participants rating both control ($M_{Control}$ = 6.35, $SD_{Control}$ =

1.91), and heritage buyers' usage of the watch as highly appropriate ($M_{Control}$ = 6.29, $SD_{Control}$ = 1.73, $t(395)$ = .37, p = .714).

Heritage Loss. We calculate heritage loss as the seller's heritage connection – the buyer's perceived connection to the good's heritage. Sellers incurred higher levels of heritage loss when the watch's buyer was a control vs. a connected buyer ($M_{Heritage}$ = .54, $SD_{Heritage}$ = 1.30; $M_{Control}$ = 2.58, $SD_{Control}$ = 1.89, $t(386)$ = 12.57, p = .013, η^2 = .02).

Mediation by Heritage Loss. Having found a main effect of buyer identity on sellers' WTA, we tested for mediation. Specifically, we tested to see if the seller's heritage loss due to trade (the seller's heritage connection – the buyer's heritage connection) mediated the effect of condition on WTA (H3). We found that it did. Selling the watch to a control buyer, increased heritage loss, and this lost heritage increased the seller's WTA for their good, $169. 93 (95% CI = $127.25, $218.5, Hayes 2012). Overall, this study suggests that sellers have a higher WTA for disconnected versus connected buyers with an indirect pathway through heritage loss.

We note here that although both the total effect and the indirect effect of buyer's perceived connection to the good on WTA are negative (WTA goes down when buyer's heritage connection goes up), the direct effect of a buyer's perceived connection is positive. Thus, our study provides evidence of competitive mediation. This competitive mediation builds on our findings in earlier studies, where we provided evidence that connected buyers received more value from a heritage good (study 1) and had a higher WTP (study 3). Thus, this is consistent with our earlier findings that sellers perceive connected buyers have high valuations and yet they charge them less, because of these buyers' connection to a shared past. For further details on this study and all analyses please see the Web Appendix and http://aspredicted.org/blind.php?x=qa5yq9.

Discussion

Overall, study 4 demonstrates that the heritage discount holds when the heritage good is held constant, and that heritage loss explains this discount. First, we provided evidence that connected buyers were perceived by sellers to have a higher connection to the heritage good. Second, sellers accepted lower offers from connected buyers. Third, we showed that sellers perceived that less of their good's shared heritage connection would be lost in a potential trade with connected buyers (i.e., lower heritage loss). Finally, we contributed process evidence that this effect of buyer's identity on seller's WTA was mediated by heritage loss. Taken together the findings in study 4 provide convincing evidence that the buyer's heritage connection can reduce the seller's heritage loss in a sale and may lower the seller's WTA.

In sum, our results from studies 1-4 indicate that the sale of a heritage good to a disconnected buyer leads to a loss of heritage connection: the larger the loss in heritage, the greater the seller's WTA. On the other hand, connected buyers received a price discount despite the fact they would likely receive greater utility from the seller's heritage good, and thus might be willing to pay a price premium. While the results of the hypothetical scenario in study 4 seem encouraging, in study 5 we will investigate the effect of heritage loss on seller's WTA in a fully incentive compatible experiment using real goods.

STUDY 5: SELLING A HERITAGE GOOD

In the final study, our primary aim was to investigate the effect of a buyer's perceived heritage connection on the seller's WTA for a heritage good. Thus, we designed this study to be fully incentive compatible by using real goods and including a Becker-DeGroot-Marschak

elicitation method. To distribute heritage goods uniformly between participants, we endowed participants with a new heritage good that invoked their connection to a shared past and gave them the opportunity to sell that good.

Method

In study 5, 400 individuals were recruited from the campus of a large American research university (73.8% female, average age = 22.1 years, SD = 5.44). All participants had agreed to participate in studies on campus, and they completed the study digitally. At the start of the study, we informed participants that they would receive a magnet that celebrated 100 years of the university's history, and we showed them the magnet. We told participants that this commemorative item would be theirs to keep.

After we endowed participants with a commemorative magnet, that celebrated 100 years of the university's history. We informed participants that someone wanted to buy their magnet, randomly assigned to one of two buyer conditions, and assessed their WTA. In the heritage condition, the buyer was a student from the university (heritage buyer). In the control condition, the buyer was a stranger with no connection to the university (control buyer). To elicit participants' WTA, we used an incentive compatible procedure, the Becker-DeGroot-Marschak task, a well-validated process designed to collect participants' WTA in trading environments (Becker, Degroot, and Marschak 1964; Bohm, Lindén, and Sonnegård 1997; O'Donnell and Evers 2019). Participants were told that a price would be chosen randomly and if they had indicated a willingness to sell at that price, they would give up the magnet and receive the cash instead, while if they had indicated that they were not willing to sell at that price, they would keep the magnet.

Participants saw a list of prices in $.25 increments, ranging from $0 to $6 for a total of 25 prices. At each price point, participants indicated their choice to keep or sell the commemorative magnet. They then indicated the heritage value the commemorative magnet had for them and the heritage value they perceived that the magnet would have for the buyer. Next, Qualtrics randomization software generated an offer price between $0 and $6, and presented that offer to each participant. If the participant had chosen to keep the magnet at the offer price, they kept their magnet. If the participant had chosen to sell at the offer price, they received the offer as a deposit in their university account.

Results

We received data from 400 unique participants. After removing participants who had inconsistent preferences (accepting an offer of $.50, but not accepting an offer of $1), and one participant who was unable to give informed consent, we analyzed data from 387 participants. For each participant, we calculated the lowest price at which they indicated that they would sell the magnet. If participants would not sell at any price up to and including $6.00, we assigned them a conservative WTA of $6.25.

WTA. As predicted, participants' WTA from a stranger with no university connection ($2.63) was higher than their WTA from a university student ($2.20, $F(1,385) = 6.05$, $p = .014$). Sellers indicated that buyers with no university connection had a lower heritage connection to the magnet than the buyers who shared the university's traditions (3.04 vs. 4.61, $F(1, 386) = 170.0$, $p < .001$), meaning that selling to a stranger lead to a greater loss in heritage connection than selling to a member of the university community. For each point in lost heritage connection, the seller charged the buyer an additional $.42, a 16% heritage premium, ($F(1,386) = 59.29$, $p < .001$). Given that we had found a pre-registered effect of condition on WTA and a pre-registered

effect of heritage loss we proceeded to run an additional, mediation analysis. For details, please see https://aspredicted.org/blind.php?x=m2t4qq

Mediation. We found that the loss in heritage connection significantly mediated the effect of condition on the seller's reservation price. Using 10,000 bootstrapped samples, we observed an unstandardized indirect effect of a shared past on WTA through a reduction in heritage loss (-$.82), representing a discount of 34% off the seller's average WTA ($2.41), and a 95% confidence interval that did not include zero (-$1.07, -$.57). After controlling for heritage loss, the effect of buyer's identity on reservation price was no longer fully significant (95% C.I. = -.01, .78), suggesting full mediation by heritage loss.

Discussion

The results of Study 5 provided further evidence that heritage value affects market transactions for goods that connect buyers and sellers to a shared past. When a 100-year commemorative magnet was sold to a buyer less connected to the traditions of the university and less connected to the heritage of the good, sellers had a higher WTA. Crucially, we found that this price hike occurred despite the fact that connected buyers likely received greater utility from the object and thus might be objectively willing to pay higher prices (Luce, Bettman, and Payne 2000; McGraw et al. 2016). This replicates our findings in studies 1-4. In study 5, heritage loss – the difference between the seller's heritage connection to the good and the buyer's heritage connection to the good – mediated the effect of condition on pricing. In sum, using real goods and real transactions, we found that the heritage loss from a transaction affected the seller's WTA.

GENERAL DISCUSSION

In a set of five main experiments (as well as two preliminary experiments and one follow-up experiment), we provide strong evidence that heritage connection affects pricing decisions. In an incentive compatible design with a real good, we found preliminary evidence for the heritage discount by providing evidence that sellers of heritage goods charge connected buyers less for the same good even when they perceive connected buyers' valuations to be high. Thus, the buyer's perceived heritage connection reduced the seller's WTA for a heritage good. We confirmed that the effect of shared heritage connection on the seller's WTA was not driven by low buyer valuations (study 1). The effect held after controlling for appropriateness of usage and could not be explained by the similarity of buyer and seller (study 1). We presented process evidence of the effect of heritage connection on WTA in a study that used real possessions in participants' homes and showed moderation of the heritage discount by the magnitude of the seller's heritage connection to a good (study 2). Furthermore, the effect of shared heritage connection held after controlling for psychological ownership (study 2), and reduced sellers' WTA despite connected buyers' higher perceived WTP (study 3). The effect of heritage connection was robust even without a personal connection between buyer and seller (study 3). We demonstrated that the seller's heritage loss due to trade mediated the difference in WTA from connected versus disconnected buyers (study 4). Finally, we replicated this mediation in an incentive compatible study using real goods (study 5). The effect of heritage on WTA held after controlling for both market value and demographic variables (studies 1-5). Overall, this research provides strong evidence buyers' and sellers' heritage connection affects product disposition, and this work demonstrates that collective ownership of a shared past affects market transactions in the present.

This research contributes to the literature on the endowment effect and psychological ownership. Prior research has found that ownership duration can affect the seller's willingness to accept for a good they own (Ariely, Huber, and Wertenbroch 2005), and even previously owned objects demonstrate an effect of ownership (Strahilevitz and Loewenstein 1998). Our research provides strong evidence that a heritage good's symbolic ability to connect its buyers and sellers to the collective past reduces the seller's WTA and may also create a WTA/WTP asymmetry.

This paper also contributes to the literature on sentimental goods. Past research has found that these products can provide wellbeing benefits (Wildschut et al. 2006), and highly attached owners of sentimental goods demonstrate heightened sensitivity to the future usage of their goods (Brough and Isaac 2012). We extend these findings in the domain of heritage, finding that even after controlling for future usage, the effect of heritage connection as a generator of product value holds.

This paper also contributes to the sharing literature (Belk 2010, 2014, 2017). We respond to this literature's call for greater research on how the aggregate extended self – "us" – affects consumer practices. In our empirical work, we first provide evidence that the shared past creates value for consumers. Then, we empirically demonstrate that the heritage loss when a good is sold to a buyer who does connect to the shared past predicts both the seller's likelihood to trade and their WTA. Further, we demonstrate how collective value, shared between buyers and sellers, drives pricing: sellers indicated a lower WTA for connected buyers even when they perceived that connected buyers had higher valuations and a higher WTP.

These experiments also provide process evidence for the effect of shared heritage connection on pricing decisions. First, sellers' WTA increased when their own heritage connection to a good was high. According to the laws of supply and demand, sellers' WTA

should also increase, or at the very least, remain flat when they perceive buyers have high heritage connection to a good. We found the opposite, indicating that sellers blend market pricing with communal sharing (Fiske 1991). It is not simply the buyer's valuation, but also the heritage loss due to trade that determines the seller's pricing decision. Critically, as the buyer's heritage connection to a good goes up, the seller's WTA for that good goes down.

Practical Implications

These findings can be used in two ways. First, this research has potential applications to markets that involve resale: the $33 trillion U.S. housing market and the $200 billion collectibles market (Fenech 2018; Lloyd 2019; Thredup 2019). Many of these goods connect owners to a shared past. For example, to get a discount on an older house, prospective buyers might emphasize their experience living in a house from the same time period or their goal of staying connected to the past while enjoying the house.

Second, marketers of new products might appeal to consumers' desire to connect to their heritage across time. For example, marketers might sell a subscription to one clothing item that starts by invoking the shared past - using black and white imagery and heritage stitching - and promises to preserve that shared past for its customers by offering "a t-shirt that never gets old," "sustainable clothes with a forever discount," where one heritage t-shirt is endlessly replaced by the same t-shirt, trading between a branded community and the consumers' selves across time (Shatzman 2018). In this way, marketers may be able to create a virtuous cycle that maintains the buyer's heritage connection and the seller's profit.

In this article, we have explored how heritage can impact valuations in the present. Future research could look at the antecedents and moderators of heritage connection. For example, how do the digital world and technology shape the desire for heritage goods? If the antique suppresses time (Baudrillard 1968), perhaps the desire for goods that evoke the shared past will only grow in the fast-moving digital world. It might be the case that uncertainty about the future drives a search for the security and comfort of the past and increases the desire to preserve heritage goods. Alternatively, a feeling of stability across time might decrease the price premium sellers charge for heritage goods.

Another dimension that future research could explore more fully is the distinction between hedonic and utilitarian goods. Mixed research has found that consumers may value hedonic goods more highly in forfeiture than in acquisition (Wertenbroch and Dhar 2000; Chan 2015), although, this asymmetry does not hold in all cases (Shu and Peck 2011). More recently, work on disposition has found that identity-linked products are less likely to be thrown in the trash (Trudel, Argo, and Meng 2016). In the current work, we have not fully examined the differences between hedonic and utilitarian goods. We have tested more functional goods such as a hat or a magnet or a watch that owners might consider more utilitarian, and we have tested less functional goods that owners might consider more hedonic (e.g., the chimes). The impact of hedonic (versus utilitarian) attributes on sellers' disposition was not a dimension we had a clear hypothesis for or tried to directly test, but future research could look more closely at whether the heritage discount differs by type of good.

Additional research on this topic could explore how the value of a shared past depends on external factors. First, as algorithms and artificial intelligence govern a growing part of our lived experience, does this technological shift change how we define the shared past? On the one hand,

the rise of the robot might lead to a stronger desire for only the most known and trusted others

and a more narrowly defined history. On the other hand, conscious exposure to artificial life

could broaden heritage categories from an ethnic group or a brand community to all of humanity.

Conclusion

In sum, heritage matters. Access to a collective past changes how we value our goods and

how we sell them. While heritage goods cannot give the seller or the buyer the ability to travel

back to a past they have never witnessed or to travel forward into a future they will never see, it

can expand time, allowing its owner to imagine a past before their birth and a future after their

death through a connection to a heritage good. We hope that this work increases understanding

of the value of the shared past that underlies how consumers choose to keep and sell goods,

while also contributing to researchers' understanding of how consumers value goods across time.

FIGURES

FIGURE 1: NATURAL VARIATION IN MENTAL TIME TRAVEL DIRECTION
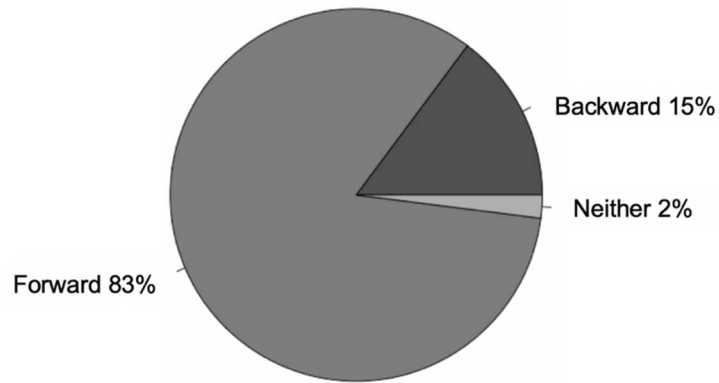
Note: Responses between 0 and 49 coded as "backward time travel," responses at 50 coded as "neither," and responses between 51 and 100 coded as "forward time travel."

FIGURE 2: TRAVELING BACK TO THE PRESENT INCREASES
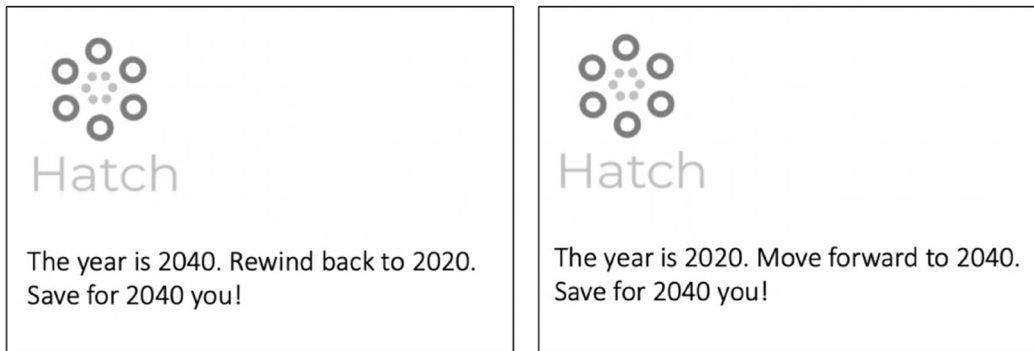SIMILARITY OF FUTURE SELF AND SPEED OF TIME
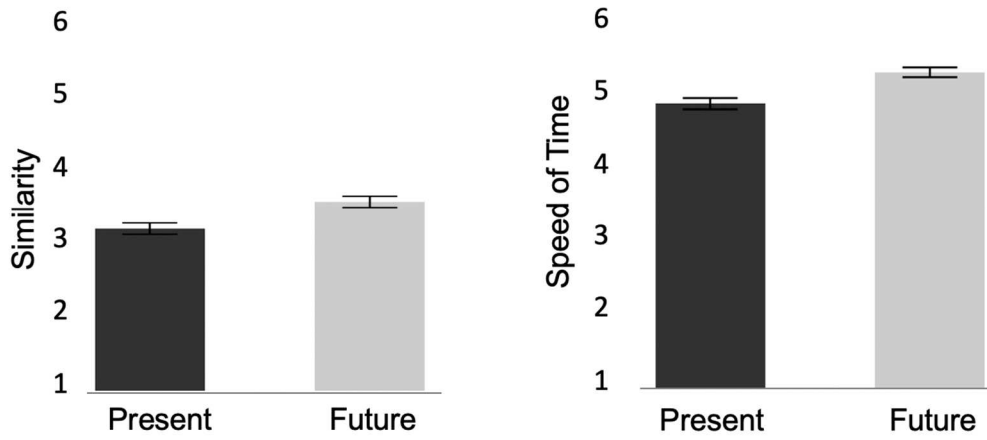
Note: In all figures, error bars represent +/- 1 SE of the mean. Participants answered questions on a 1-7 Likert scale.

FIGURE 3: TRAVELING BACK TO THE PRESENT LEADS TO
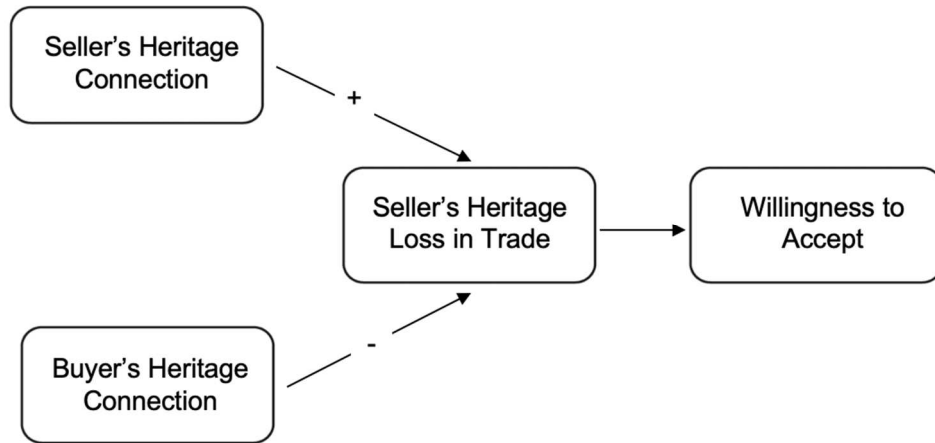HIGHER LIKELIHOOD TO SAVE
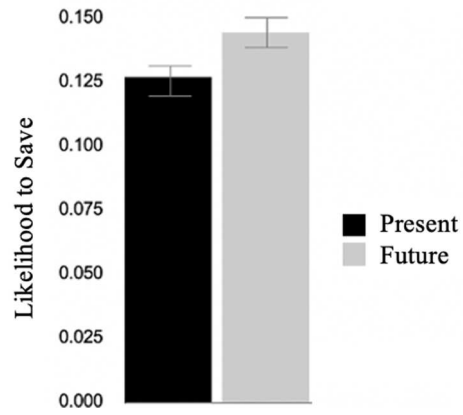
Note: In all figures, error bars represent +/- 1 SE of the mean.

FIGURE 4: FUTURE AND PRESENT CONDITION PROMPTS FOR SAVINGS AD

Note: Future condition on left. Present condition on right.

FIGURE 5: THE VALUE OF HERITAGE GOODS FOR SELLERS

Note: Seller's heritage connection increases the potential for heritage loss in trade, while the buyer's heritage connection decreases the seller's heritage loss. The buyer's heritage connection is the connection perceived by the seller.

FIGURE 6: ANNIVERSARY HAT FROM MONTEREY

FIGURE 7: HERITAGE CONNECTION MEASURES

1. I feel like this _____ connects me to people I care about.
2. I feel like this _____ helps me remember where I come from.
3. I feel like this _____ is about more than just me.
4. I feel like this _____ gives me a sense of history
5. I feel like this _____ connects me to family or community traditions.
6. I feel like this _____ connects me to people who came before me.

Note: For all items the Likert ranged from 1-Strongly disagree to 7-Strongly agree.

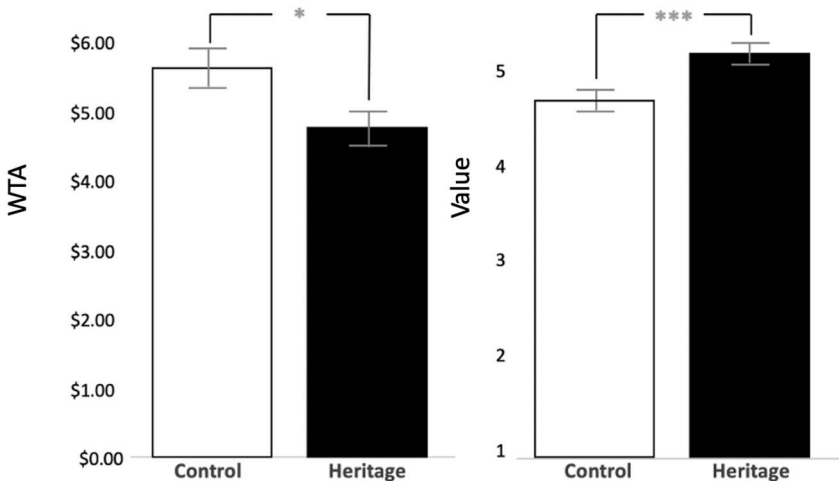FIGURE 8: WTA AND VALUE FOR HERITAGE VERSUS CONTROL BUYERS

FIGURE 9: SELLERS GIVE CONNECTED BUYERS A
DISCOUNT ON HERITAGE GOODS

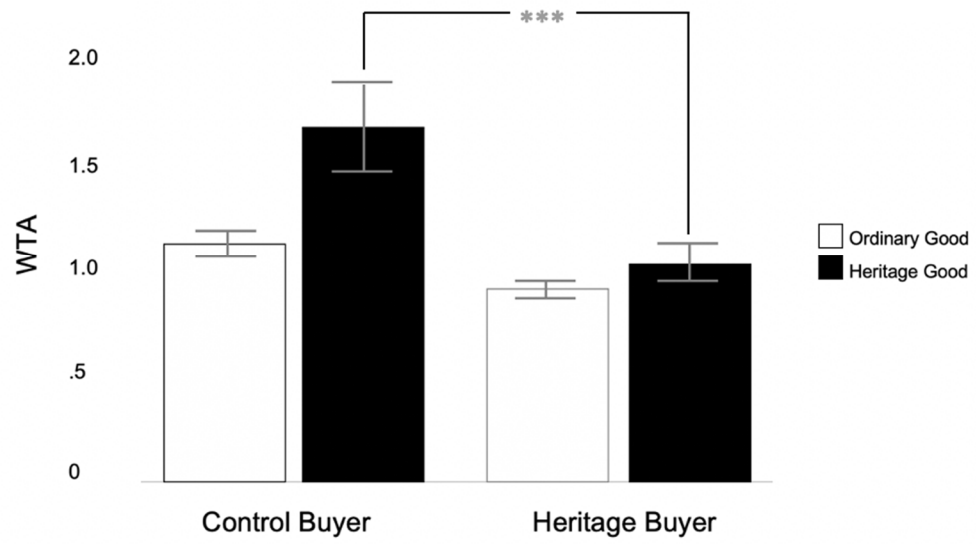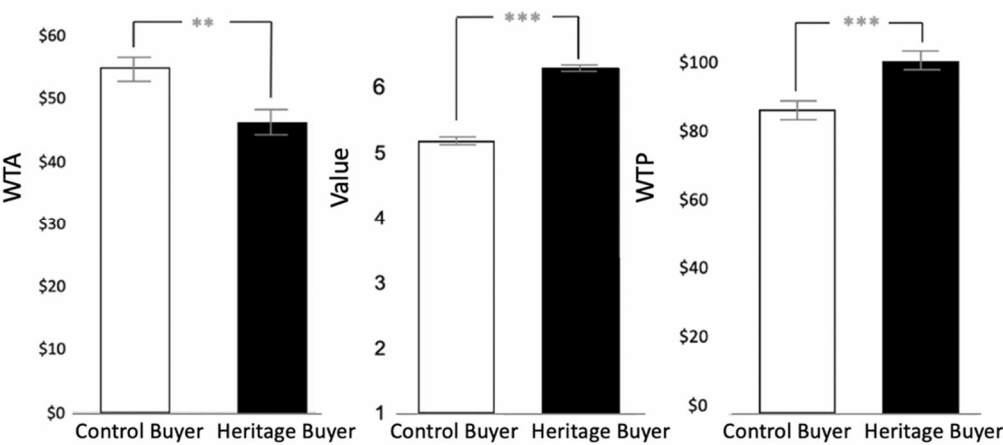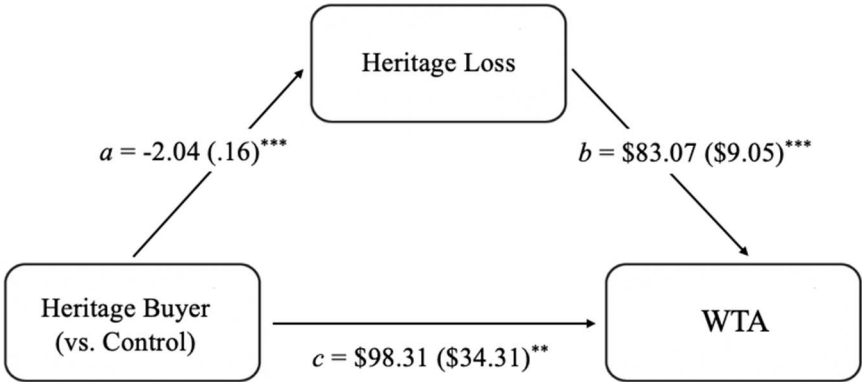FIGURE 10: ASYMMETRY IN WTA/WTP FOR HERITAGE GOODS

FIGURE 11: HERITAGE LOSS INCREASES SELLERS WTA AND MEDIATES THE EFFECT OF
CONDITION ON WTA

Appendix A Chapter 1

Included in this supplement:

A. RESULTS OF STUDY 1A

B. RESULTS OF STUDY 1B

C. RESULTS OF STUDY 2A

D. RESULTS OF STUDY 2B

E. RESULTS OF STUDY 3

F. RESULTS OF STUDY 4A

G. RESULTS OF STUDY 4B

H. STIMULI AND RESULTS OF FOLLOW-UP TO STUDY 2B

A. RESULTS OF STUDY 1A SIMILARITY JUDGMENTS

       To test the effect of mental time travel direction on similarity judgment, we include means, standard deviation of the main effect, and a regression table that includes demographic variables. Given the effect of age on similarity judgments, we tested for interaction between age and condition, but this was not significant ($p = .903$).

Measure	Present	Future	Test
Similarity	$M = 3.17$, SD = 1.65	$M = 3.56$, SD = 1.65	$F(1, 1010) = 14.08, p < .001, d = .24$

	Dependent variable:	
	Similarity Judgments	
	(1)	(2)
Future vs. Present	0.386^{***}	0.372^{***}
	(0.104)	(0.103)
Age		0.010^{*}
		(0.005)
Gender		-0.054
		(0.104)
Income		0.034^{***}
		(0.012)
Constant	3.365^{***}	3.367^{***}
	(0.052)	(0.052)
Observations		
R^2	0.014	0.029
Adjusted R^2	0.013	0.025
Residual Std. Error	1.650 (df = 1010)	1.640 (df = 1004)
F Statistic	14.084^{***} (df = 1; 1010)	7.463^{***} (df = 4; 1004)

Note: Age mean-centered, Gender +.5 female, -.5 male, Future +.5, Present -.5, $^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

B. RESULTS OF STUDY 1B SIMILARITY JUDGMENTS

To test the effect of mental time travel direction on similarity judgment using a shorter time horizon with three conditions, we include means, standard deviation of the main effect, and a regression table that includes demographic variables.

Measure	Control	Future	Test
Similarity	$M = 3.33$, SD = 1.73	$M = 3.75$, SD = 1.65	$F(1, 920) = 9.70$, $p = .002$, $\eta^2 = .01$

Measure	Present	Future	Test
Similarity	$M = 3.15$, SD = 1.70	$M = 3.75$, SD = 1.65	$F(1, 920) = 11.08$, $p < .001$, $\eta^2 = .01$

	Dependent variable:	
	Similarity Judgments	
	(1)	(2)
Control	-0.426**	-0.401**
	(0.137)	(0.136)
Present	-0.605***	0.611***
	(0.136)	(0.135)
Age		0.023***
		(0.005)
Gender		-0.066
		(0.114)
Income		-0.002
		(0.012)
Control * Age		
Present * Age		
Constant	3.754***	3.754***
	(0.167)	(0.165)
Observations	923	923
R^2	0.022	0.050
Adjusted R^2	0.020	0.044

Note: Age mean-centered, Gender +.5 female, -.5 male, Control,

Present dummy-coded, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

C. RESULTS OF STUDY 2A MEDIATION BY SPEED OF TIME

The regression table below provides the full table with results from our study of the effect of mental time travel direction on similarity judgments and the speed of time.

Measure	Present	Future	Test
Similarity	$M = 3.11$, SD = 1.69	$M = 3.46$, SD = 1.69	$F(1, 1097) = 11.99, p < .001, d = .21$
Time	$M = 4.89$, SD = 1.83	$M = 5.32$, SD = 1.58	$F(1, 1097) = 17.59, p < .001, d = .25$
Features	$M = 4.15$, SD = 2.23	$M = 4.30$, SD = 2.13	$F(1, 1097) = 1.31, p = .253, d = .07$

	Dependent variable:			
	Similarity Judgments		Temporal Velocity	
	(1)	(2)	(3)	(4)
Future vs. Present	0.353***	0.346***	0.432***	0.425***
	(0.102)	(0.102)	(0.103)	(0.103)
Age		0.014**		0.015**
		(0.005)		(0.005)
Gender		-0.135		-0.139
		(0.103)		(0.104)
Constant	3.285***	3.296***	5.107***	5.118***
	(0.051)	(0.051)	(0.051)	(0.052)
Observations	1,099	1,099	1,099	1,099
R^2	0.011	0.021	0.016	0.026
Adjusted R^2	0.010	0.018	0.015	0.023
Residual Std. Error	1.689	1.739	1.892	1.699
F Statistic	11.990*** (df = 1; 1097)	7.638*** (df = 4; 1095)	17.590** (df = 1; 1097)	9.805*** (df = 4; 1095)

*Note: Gender +.5 female, -.5 male, Age mean-centered, Future +.5, Present -.5, *p < 0.05, **p < 0.01, ***p < 0.001*

D. RESULTS OF STUDY 2B ALTERNATIVE EXPLANATION

The regression table below provides the full table with results from our study of the effect of mental time travel direction on similarity judgments and concreteness using both self-report and text analysis of responses.

Measure	Present	Future	Test
Similarity	$M = 2.98$, SD = 1.56	$M = 3.70$, SD = 1.66	$F(1, 607) = 24.63, p < .001, d = .25$
Concrete Aggregate	$M = 5.62$, SD = 1.41	$M = 5.45$, SD = 1.40	$F(1, 607) = 1.69, p = .197, d = .10$
Concreteness	$M = 4.78$, SD = 1.66	$M = 4.70$, SD = 1.67	$F(1, 607) = .36, p = .546, d = .05$
Vivid	$M = 4.16$, SD = 1.68	$M = 4.10$, SD = 1.76	$F(1, 607) = 1.69, p = .686, d = .03$
Detailed	$M = 3.76$, SD = 1.62	$M = 3.46$, SD = 1.47	$F(1, 607) = 1.69, p = .019, d = .19$
LIWC	$M = 14.26 (12.94)$	$M = 12.62 (14.47)$	$F(1, 607) = 1.19, p = .276, d = .12$
Brysbaert	$M = 2.40 (.32)$	$M = 2.42 (.31)$	$F(1, 607) = .95, p = .330, d = .08$

	Dependent variable:		
	similarity	detailed	similarity
	(1)	(2)	(3)
Future vs. Present	0.389***	-0.295**	0.397***
	(0.126)	(0.125)	(0.126)
Detailed			0.035
			(0.041)
Age			0.020***
			(0.006)
Gender			-0.221
			(0.253)
Constant	3.006***	3.608***	2.816***
	(0.063)	(0.063)	(0.175)
Observations	607	607	607
R²	0.015	0.009	0.041
Adjusted R²	0.014	0.007	0.035
Residual Std. Error	1.556 (df = 605)	1.544 (df = 605)	1.540 (df = 602)
F Statistic	9.472*** (df = 1; 605)	5.552** (df = 1; 605)	6.454*** (df = 4; 602)

Note: *p < 0.05, **p < 0.01, ***p < 0.001, *Future +.5, Present -.5, Gender +.5 female, -.5 male, Age mean-centered*

E. RESULTS OF STUDY 3 SAVINGS INTENTIONS

The regression table below provides the full table with results from our study of the effect of mental time travel direction on savings intentions.

Measure	Present	Future	Test
Similarity	$M = 3.14$, SD = 1.79	$M = 3.48$, SD = 1.70	$F(1, 1023) = 10.00, p = .002, d = .20$
Investment	$M = 4.45$, SD = 1.89	$M = 4.69$, SD = 1.90	$F(1, 1023) = 4.35, p = .037, d = .13$

	Dependent variable:			
	Similarity Judgments		Investment	
	(1)	(2)	(3)	(4)
Future vs. Present	0.346**	0.344**	0.246*	0.245*
	(0.109)	(0.109)	(0.118)	(0.116)
Age		0.019***		-0.038***
		(0.005)		(0.005)
Gender		-0.019		0.092
		(0.111)		(0.118)
Income		0.004		0.004
		(0.006)		(0.006)
Constant	3.308***	3.309***	4.571***	4.565***
	(0.055)	(0.055)	(0.059)	(0.058)
Observations	1,025	1,025	1,025	1,025
R^2	0.010	0.024	0.004	0.051
Adjusted R^2	0.009	0.020	0.003	0.047
Residual Std. Error	1.749	1.739	1.892	1.850
F Statistic	9.998*** (df = 1; 1023)	6.288*** (4; 1020)	4.347** (df = 1; 1023)	13.610*** (df = 4; 1020)

Note: Age mean-centered, Gender +.5 female, -.5 male
*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

F. RESULTS OF STUDY 4A CONSEQUENTIAL SAVINGS BEHAVIOR

The table below provides likelihood to invest for variables from our field study with 6732 investors on the Dreams platform (3404 participants in the present condition and 3328 participants in the future condition). Based on Mturk ratings in follow-up study 2, the first four variables listed in the columns above were named, "Saving 1", "Saving 2", "Saving 3", "Saving 4".

Likelihood to Invest

Investment choice	Present	Future
Saving plan 1	5 (0.1%)	16 (0.5%)
Saving plan 2	332 (9.8%)	365 (11.0%)
Saving plan 3	82 (2.4%)	112 (3.4%)
Saving plan 4	14 (0.4%)	30 (0.9%)

The table below provides a logistic regression for these variables.

	Dependent variable:			
	Saving 1	Saving 2	Saving 3	Saving 4
	(1)	(2)	(3)	(4)
Future vs. Present	1.194*	0.125	0.341*	0.789*
	(0.513)	(0.081)	(0.148)	(0.325)
Pre-Savings	-0.000	0.000***	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Pre-Fund	-14.800	0.284*	0.391	0.384
	(841.815)	(0.149)	(0.256)	(0.531)
Gender	0.267	-0.095	-0.079	0.112
	(0.515)	(0.101)	(0.185)	(0.363)
Age	-0.029	0.033***	0.010	-0.002
	(0.028)	(0.004)	(0.008)	(0.017)
Constant	-5.609***	-3.355***	-4.163***	-5.508***
	(0.922)	(0.144)	(0.268)	(0.581)
Observations	6,724	6,724	6,724	6,724
Log Likelihood	-136.793	-2,193.722	-864.453	-261.578
R^2	0.142	0.015	0.261	0.001

Note: Pre-Fund is (1,0), 8 missing ages replaced with mean. $^*p < 0.05$, $^*p < 0.01$, $^*p < 0.001$

G. RESULTS OF STUDY 4B ADVERTISING

We tested the effectiveness of our intervention on low risk savings behavior on Mturk with 1000 participants assigned to the present or future condition. Means, regression table, and supplemental figure 1 provided below.

Means, Saving Intentions

Choice	Present	Future
Saving	$M = .45$, SD = .50	$M = .52$, SD = .50
Gamble	$M = .04$, SD = .19	$M = .04$, SD = .20
Spend	$M = .51$, SD = .50	$M = .46$, SD = .50

	Dependent variable:			
	low risk saving		gamble	
	(1)	(2)	(3)	(4)
Future vs. Present	0.288^{**}	0.283^{**}	0.117	0.090
	(0.127)	(0.127)	(0.323)	(0.326)
Age		-0.007		-0.046^{**}
		(0.006)		(0.020)
Gender		0.035		-0.696^{**}
		(0.130)		(0.339)
Income		0.019		0.093^{*}
		(0.019)		(0.049)
Constant	-0.059	0.059	-3.179^{***}	-2.344^{***}
	(0.063)	(0.186)	(0.162)	(0.544)
Observations	1,000	1,000	1,000	1,000
R^2	0.005	0.007	0.000	0.030

Note: Gender +.5, -.5 $*p < .05$, $**p < .01$, $***p<0.001$

H. FOLLOW-UP STUDY TO STUDY 2B

In this study, we surveyed 1,099 participants on Mturk and paid them a nominal fee. We randomized participants into a future or present condition and asked them to make a similarity judgment (see study 1). To begin to explore process, we then asked participants what they thought about, "When you thought about similarity, did you think about any of the following (select all that apply)?" Since our study increased similarity between the year 2019 and year 2029 selves, we tested multiple models of similarity including the alignable differences model, the transformation model, and the geometric model (Malkoc, Zauberman, and Ulu 2005; Markman and Gentner 1993; Suzuki, Ohnishi, and Shigemasu 1992; Zhang and Markman 1998). As a further test, we also asked if unknowns of the year 2029 self varied between conditions. Finally, we included one question about the speed of time. All options were randomized. In this large-scale test of multiple competing theories, only the speed of time was a significant predictor of similarity judgments.

Participants could select any or all of the ten options below or they could simply click ahead to the next question:

How time moves so quickly?
How many problems that 2019 me is dealing with will be fixed by 2029 me?
How my relationships would change?
How successful I would be in 2029?
How I would improve between 2019 and 2029?
What I would have to change to transform 2019 me into 2029 me?
All of the things I don't know about 2029 me?
How my identity would change?
By 2029 will my worst life problem be solved?
How much better my life would be in 2029?

Any items in this list that the participant selected then appeared on the next page and the participant was asked them, "How much did you think about these questions?"

	A little 1	2	3	4	A lot 5
How my identity would change?	○	○	○	○	○
How time moves so quickly?	○	○	○	○	○
How much better my life would be in 2029?	○	○	○	○	○
How I would improve between 2019 and 2029?	○	○	○	○	○

Thus, all items on the ten-item list were assigned a number between 0 and 6. If the participant had not thought about the item at all, that item was rated as a 0, all other items received the 1-5

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ratings assigned by participants on the second screen. Due to this two-part process, we analyzed the data using a logistic regression (did the participant think about it at all) and using a linear regression to assess total thought on each item by condition. Both the logistic and linear regressions revealed significant differences between the future and present conditions for time. No other measure revealed significant differences between the future and present conditions in both logistic and linear regressions.

Likelihood (%) and Effect Size for All Variables

Measure	% of Participants Selected		Effect of Condition (OLS)
	Present	Future	
Speed Time	237 (43.2%)	280 (50.9%)	$F(1, 1097) = 8.12, p = .004, d = .17$
Fixed	188 (34.2%)	192 (34.9%)	$F(1, 1097) = .05, p = .829, d = .01$
Relationships	212 (38.6%)	238 (43.2%)	$F(1, 1097) = .95, p = .331, d = .06$
Successful	268 (48.8%)	283 (51.5%)	$F(1, 1097) = 1.29, p = .255, d = .07$
Improve	315 (57.4%)	327 (59.5%)	$F(1, 1097) = .21, p = .649, d = .03$
Transform	117 (21.3%)	141 (25.6%)	$F(1, 1097) = 4.03, p = .045, d = .12$
Unknown	117 (45.3%)	141 (40.5%)	$F(1, 1097) = 2.03, p = .154, d = .09$
Better Life	233 (42.4%)	253 (46.0%)	$F(1, 1097) = 2.17, p = .141, d = .09$
Solved	114 (20.8%)	123 (22.4%)	$F(1, 1097) = 1.02, p = .314, d = .06$
Identity	197 (35.9%)	205 (37.2%)	$F(1, 1097) = .08, p = .001, d = .02$

	Dependent variable:	
	Temporal Velocity	Temporal Velocity
	logistic	*OLS*
	(1)	(2)
Future vs. Present	0.311[*]	0.370[**]
	(0.121)	(0.130)
Constant	-0.275[***]	1.707[***]
	(0.086)	(0.092)
Observations	1,099	1,099
R^2		0.007
Adjusted R^2		0.006
Log Likelihood	-756.539	
Akaike Inf. Crit.	1,517.078	
Residual Std. Error		2.150 (df = 1097)
F Statistic		8.118[***] (df = 1; 1097)

$^*p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table B1: Study 1 Selling a Heritage Good from Monterey

	Dependent variable:					
	WTA					
	(1)	(2)	(3)	(4)	(5)	(6)
Heritage	-0.877*		-1.135**		-0.986*	-0.890*
	(0.378)		(0.376)		(0.381)	(0.379)
Value		0.441***	0.507***		0.466**	0.474***
		(0.127)	(0.128)		(0.142)	(0.140)
Wear in Public				-0.011	-0.067	-0.070
				(0.152)	(0.152)	(0.151)
Wear in Private				-0.135	-0.140	-0.150
				(0.124)	(0.122)	(0.122)
Sell				0.292*	0.228*	0.226*
				(0.113)	(0.112)	(0.111)
Collect				0.329*	0.195	0.190
				(0.141)	(0.145)	(0.144)
Give Away				-0.093	-0.082	-0.081
				(0.130)	(0.128)	(0.126)
Age						-0.034*
						(0.015)
Gender						-0.321+
						(0.186)
Constant	5.671***	5.235***	5.799***	5.235***	5.725***	6.918***
	(0.266)	(0.187)	(0.263)	(0.188)	(0.264)	(0.613)
Observations	366	366	366	366	366	366
R^2	0.015	0.032	0.056	0.036	0.077	0.100
Adjusted R^2	0.012	0.029	0.051	0.023	0.059	0.078
F Statistic	5.394**	12.044***	10.714***	2.722**	4.266***	4.415***

Note: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$. Study 1 is a between-participant study with heritage and control conditions. Value, age, and all usage variables are mean-centered. Gender coded -1 is male, +1 female. Heritage is dummy coded (1,0). Standard errors are presented in parentheses. Thirty-four participants were removed due to inconsistent preferences.

Table B2: Study 2 Pre-Test 1 Heritage Connection Exploratory Factor Analysis

Effect of Heritage vs. Control on Seller's WTA

Buyer	Test
Disconnected Buyer	$F(1, 308) = .54$, $p = .004$, $\eta^2 = .03$
Connected Buyer	$F(1, 308) = 8.31$, $p = .909$, $\eta^2 = .00$
Connected - Disconnected	$F(1, 308) = 12.07$, $p < .001$, $\eta^2 = .04$

Exploratory Items – Item Total Correlations (α=.855)

Variable	Item Total	Alpha Without	N
I would be satisfied with replacement.	-.633	.920	310
Connects me to people I care about.	.722	.829	310
Mine and no one else's.	.322	.857	310
Helps me understand who I am.	.691	.832	310
Remember where I come from	.806	.821	310
About more than just me.	.800	.822	310
Gives me a sense of history.	.813	.820	310
Connects me to traditions.	.825	.819	310
Belongs to my family and not only mine.	.504	.846	310
Connects me to people no longer living.	.702	.829	310
Not something I should sell.	.669	.833	310

Heritage Connection Measures (α=.931)

Variable	Alpha Without	N
Connects me to people I care about.	.927	310
Remember where I come from	.855	310
About more than just me.	.865	310
Gives me a sense of history.	.853	310
Connects me to traditions.	.856	310
Connects me to people no longer living.	.873	310

Table B3: Study 2 Pre-Test 2 Heritage Connection, Confirmatory Factor Analysis

Factor Loadings

	Factor 1	Factor 2
Heritage 1	.79	
Heritage 2	.86	0.441***
Heritage 3	.86	
Heritage 4	.89	
Heritage 5	.89	
Heritage 6	.92	
Ownership 1		.81
Ownership 2		.93
Ownership 3		.85
SS Loadings	4.68	2.52
Prop Variance	.52	.28
Cum Variance	.52	.80

Correlation Matrix

Variable	Herit1	Herit2	Herit3	Herit4	Herit5	Herit6	Own1	Own2	Own3
Herit1	1.00	.76	.76	.72	.78	.75	.44	.35	.27
Herit2	.76	1.00	.80	.82	.81	.80	.43	.33	.28
Herit3	.76	.80	1.00	.84	.80	.82	.42	.40	.33
Herit4	.72	.82	.84	1.00	.84	.88	.45	.39	.32
Herit5	.78	.81	.80	.84	1.00	.88	.41	.38	.30
Herit6	.75	.80	.82	.88	.88	1.00	.38	.33	.27
Own1	.44	.43	.42	.45	.41	.38	1.00	.80	.73
Own2	.35	.33	.40	.39	.38	.33	.80	1.00	.81
Own3	.27	.28	.33	.32	.30	.27	.73	.81	1.00

Pre-Registered Confirmatory Factor Analysis. We validated the measures of heritage using a Factor Analysis with a two factor model with Varimax rotation, and a *Scree plot analyzing both ownership and heritage found two optimal coordinates with two Eigenvalues greater than the mean.* We determined that the goodness-of-fit measures were acceptable in the two factor model, CFI = 0.98, RMSEA =0.08, SRMSR = .02, and χ^2 = 47.36, df=26, p=.006.

Table B4: Providing Evidence for Heritage Connection

	Dependent variable:		
	WTA$_S$	WTA$_F$	WTA$_S$-WTA$_F$
	(1)	(2)	(3)
Value	0.676***	0.709***	-0.033
	(0.040)	(0.052)	(0.048)
Ownership	0.183**	0.367***	-0.184*
	(0.071)	(0.092)	(0.084)
Heritage	0.556***	0.183**	0.373***
	(0.043)	(0.056)	(0.052)
Age	-0.010	-0.026**	0.016*
	(0.007)	(0.008)	(0.008)
Gender	-0.045	-0.048	0.003
	(0.076)	(0.099)	(0.090)
Constant	6.966***	5.366***	1.599***
	(0.074)	(0.097)	(0.089)
	547	547	547
Observations	547	547	547
R^2	0.542	0.354	0.095
Adjusted R^2	0.538	0.348	0.087
Residual Std. Error (df = 541)	1.734	2.251	2.063
F Statistic (df = 5; 541)	127.941***	59.385***	11.412***

Note: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$. In Study 2, participants indicated their WTA for a good they had in their home in a mixed design with type of good (heritage vs. control) randomized between subjects. Within subject, we assessed their WTA from a disconnected buyer (WTA$_S$), a connected buyer (WTA$_F$), and the difference in WTA for the two buyers (WTA$_S$- WTA$_F$). Value, ownership, heritage, and age are mean-centered. Gender is coded such that -1 is male and 1 is female. Standard errors are presented in parentheses.

Table B5: Study 3 WTA/WTP Asymmetry

	Dependent variable:		
	WTA	Value	WTP
	(1)	(2)	(3)
Connected vs.	-7.227***	1.149***	13.948***
Disconnected Buyer	(2.705)	(0.081)	(3.643)
Age	0.373***	0.003	0.071
	(0.106)	(0.003)	(0.143)
Gender	0.345	0.028	4.736*
	(1.368)	(0.041)	(1.849)
Income	0.614	-0.014	1.200*
	(0.391)	(0.012)	(0.530)
Constant	54.104***	5.163***	87.076***
	(1.909)	(0.057)	(2.577)
Residual Std. Error	383	766	766
F Statistic	0.064	0.212	0.033
Adjusted R^2	0.054	0.208	0.028
Residual Std. Error	26.334 (df = 378)	1.118 (df = 761)	50.408 (df = 761)
F Statistic	6.431***	51.263***	6.537***

Note: ***$p< 0.001$, **$p< 0.01$, *$p< 0.05$. In Study 3, participants indicated their WTA for a set of chimes from a friend of a friend, who was connected to or disconnected from the good's heritage. All participants also assessed the valuation and WTP of both a heritage buyer and a control buyer (order was randomized). Age is mean-centered. Gender is coded such that -1 is male and 1 is female. Heritage is dummy coded (1,0). Standard errors are presented in parentheses.

Table B6: Study 4 Mediation by Heritage Loss

	Heritage Loss	Appropriate	WTA	
	(1)	(2)	(3)	(4)
Connected vs.	-2.024***	-0.075	-69.536*	
Disconnected Buyer	(0.163)	(0.183)	(32.028)	
Heritage Loss				68.952***
				(7.687)
Age	0.004	0.014+	1.079	0.833
	(0.007)	(0.007)	(1.289)	(1.181)
Gender	0.181*	-0.033	18.984	5.097
	(0.082)	(0.092)	(16.146)	(14.884)
Constant	2.557***	6.361***	846.421***	704.829***
	(0.115)	(0.129)	(22.607)	(18.976)
Observations	397	397	397	397
R^2	0.291	0.010	0.018	0.175
Adjusted R^2	0.285	0.002	0.010	0.169
Residual Std. Error (df = 393)	1.626	1.824	318.770	292.162
F Statistic (df = 3; 393)	53.686***	1.327	2.357*	27.753***

Note: ***$p< 0.001$, **$p< 0.01$, *$p< 0.05$. In Study 4, participants indicated their WTA from a heritage vs. control buyer. All participants also assessed their own heritage connection to the good and the buyer's heritage connection. They also assessed the appropriateness of the usage plans of both the heritage and control buyer (order was randomized). Age is mean-centered. Gender is coded such that -1 is male and 1 is female. Heritage is dummy coded (1,0). Standard errors are presented in parentheses.

Table B7: Study 5 Selling a Heritage Good

	Dependent variable:		
	WTA	Heritage Loss	WTA
	(1)	(2)	(3)
Connected vs. Disconnected Buyer	-0.432*	-1.665***	
	(0.176)	(0.130)	
Heritage Loss			0.428***
			(0.054)
Age	0.005	-0.016	0.013
	(0.016)	(0.012)	(0.015)
Gender	-0.209*	0.049	-0.227*
	(0.102)	(0.075)	(0.095)
Constant	2.732***	1.012***	2.437***
	(0.133)	(0.099)	(0.095)
Observations	387	387	387
R^2	0.027	0.308	0.151
Adjusted R^2	0.019	0.302	0.144
Residual Std. Error (df = 383)	1.723	1.274	1.609
F Statistic (df = 3; 383)	3.490**	56.773***	22.631***

Note: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$. In Study 5, participants indicated their WTA from a heritage vs. control buyer. All participants also evaluated both their own heritage connection to the good and the buyer's heritage connection to the good. The effect of heritage condition on both WTA and heritage loss is significant, and heritage loss increases WTA. We provide a mediation analysis in the paper. Age is mean-centered. Gender is coded such that -1 is male and 1 is female. Heritage is dummy coded (1,0). Standard errors are presented in parentheses.

FIGURE B1: Study 1 Monterey Stimuli

Monterey is a city in California that is home to California's first constitution, California's first scenic highway, and is the first place where Californians raised the American flag. Monterey also has the Monterey Bay aquarium, and visitors come to see it.

This year, the city of Monterey, California is celebrating its 250th anniversary. As part of this study, one person will receive a Monterey Bay 250th anniversary hat celebrating the city's bold past and golden future.

Hat Evaluated by Participants

FIGURE B2: Study 4 Watch Stimulus

FIGURE B3: Study 5 100th Anniversary Commemorative Magnet (
Note: Logo Obscured while under review)

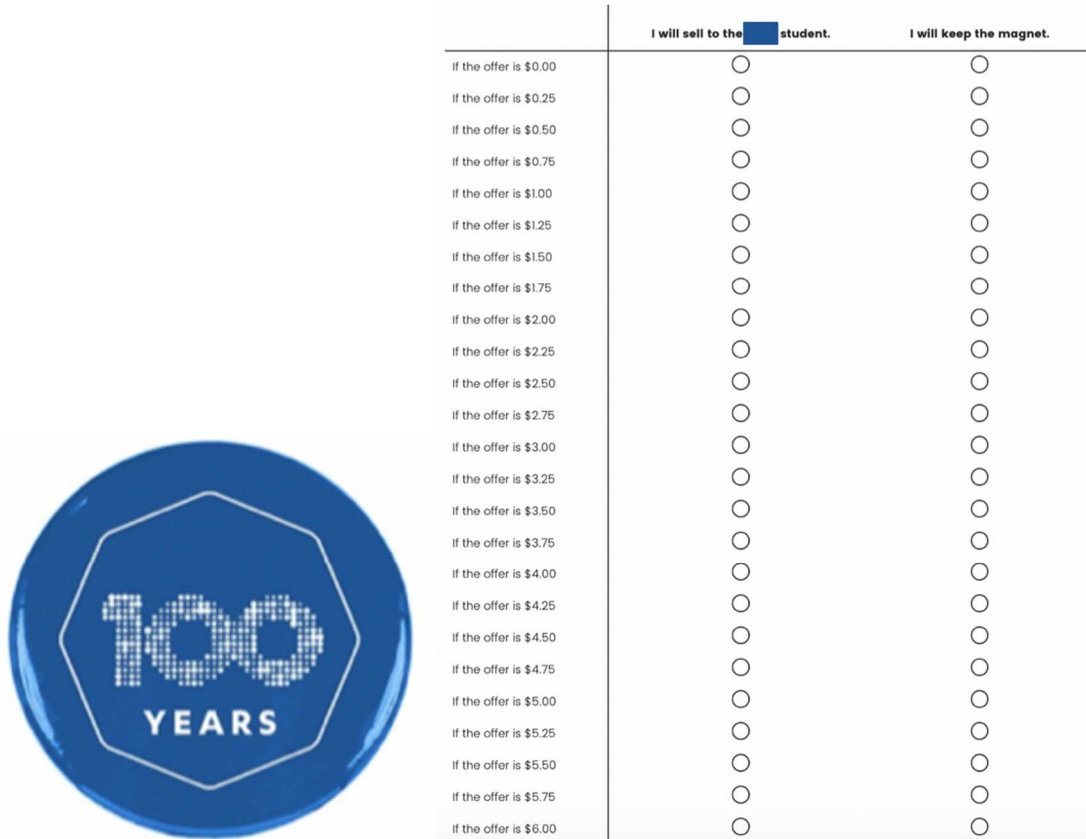|	I will sell to the ▮▮▮ student.	I will keep the magnet.
If the offer is $0.00	○	○
If the offer is $0.25	○	○
If the offer is $0.50	○	○
If the offer is $0.75	○	○
If the offer is $1.00	○	○
If the offer is $1.25	○	○
If the offer is $1.50	○	○
If the offer is $1.75	○	○
If the offer is $2.00	○	○
If the offer is $2.25	○	○
If the offer is $2.50	○	○
If the offer is $2.75	○	○
If the offer is $3.00	○	○
If the offer is $3.25	○	○
If the offer is $3.50	○	○
If the offer is $3.75	○	○
If the offer is $4.00	○	○
If the offer is $4.25	○	○
If the offer is $4.50	○	○
If the offer is $4.75	○	○
If the offer is $5.00	○	○
If the offer is $5.25	○	○
If the offer is $5.50	○	○
If the offer is $5.75	○	○
If the offer is $6.00	○	○

FIGURE B4: Attention Check Questions

These **analogy questions** appeared at the end of the survey for participants in study 2. These participants were paid regardless of response. If they missed more than one of these questions they were excluded from the analysis.

We now have a very quick task before you finish the survey.

Please click on the >> button below to proceed.

Participants selected answers to an analogy question presented in multiple-choice format.

TOE: FOOT ::

thimble : finger
crutch : leg
finger : hand
belt : waist
pillow : head

GLOVE: FINGER ::

armor : sword
crown : cane
torso : waist
earring : jar
stocking : leg

SMELL: NOSE ::

mournful : tear
hear : ear
pungent : apple
ball : eye
vein : tongue

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