**Psychological Value Theory: A computational cognitive model of charitable giving**

Dale J Cohen1

Monica K Campbell1

Philip T Quinlan2

1Department of Psychology, The University of North Carolina Wilmington, Wilmington, NC

2Department of Psychology, The University of York, York, UK.

Word Count: 19,700

**Author Note**

All authors contributed equally to the work. Correspondence concerning this article should be addressed to Dale J. Cohen, Department of Psychology, University of North Carolina Wilmington, 601 South College Road, Wilmington, NC 28403-5612. E-mail: [cohend@uncw.edu](mailto:cohend@uncw.edu). Dale J. Cohen and Monica K Campbell, Department of Psychology, University of North Carolina Wilmington. Philip Quinlan, Department of Psychology at the University of York UK. We thank the members of the Cohen Cognition and Perception Laboratory for their assistance and support. The current work received no funding. There are no real or potential conflicts of interest that may impact the current research. Experiments 1-4 were approved by the UNCW IRB, protocol# 16-0210. Experiment 5 was approved by the Ethics Committee of the Department of Psychology, University of York IRB, #2242. The data and R code for the analysis of Experiments 1- 5 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>. The experimental presentation code is proprietary and is not open sourced. Only Experiment 5 was pre-registered.

ABSTRACT

Charitable giving involves a complex economic and social decision because the giver expends resources for goods or services they will never receive. Although psychologists have identified numerous factors that influence charitable giving, there currently exists no unifying computational model of charitable choice. Here, we submit one such model, based within the strictures of Psychological Value Theory (PVT). In four experiments, we assess whether charitable giving is driven by the perceived Psychological Value of the recipient. Across all four experiments, we simultaneously predict response choice and response time with high accuracy. In a fifth experiment, we show that PVT predicts charitable giving more accurately than an account based on competence and warmth. PVT accurately predicts which charity a respondent will choose to donate to and separately, whether a respondent will choose to donate at all. PVT models the cognitive processes underlying charitable donations and it provides a computational framework for integrating known influences on charitable giving. For example, we show that in-group preference influences charitable giving by changing the Psychological Values of the options, rather than by bringing about a response bias toward the in-group.

Keywords

Charity; Psychological Values Theory; In-Group Bias; Utility Theory; Stereotype Content Model

**Psychological Value Theory: A computational cognitive model of charitable giving**

Making a charitable donation is predicated on complex economic and social decisions because, unlike consumer purchases, the giver does not appear to accrue any financial benefit from the act of giving. A variety of factors have been called upon to explain charitable giving in ways that sit uneasily with standard economic theorizing. For instance, people may derive satisfaction from their giving (Andreon, 1990), they may feel they are ‘making a difference’ (Duncan, 2004), they may feel morally obliged (Sugden, 1984), they may receive public approbation (Gazer & Konrad, 1996) or they may simply be altruistic (Echazu & Nocetti, 2015). Here we adopt a different approach, we explain gift-giving in terms of *Psychological Value Theory* (PVT). This theory was originally developed to account for judgments made in sacrificial moral dilemmas (Cohen & Ahn, 2016) and one of its key assumptions is that individuals perceive the value of others and that perceived value drives choice. We aim to show the generality of the account by explaining how individuals make charitable decisions. The efficacy of the account is explored in terms of how well the computational model that embodies the theory can explain such decisions.

We begin by framing the current work in terms of more traditional accounts of economic decision-making and therefore we start with a discussion of standard Utility theory (e.g., Barberis, 2013; Smith, 1989; Stigler, 1950). We then go onto discuss alternative ideas couched in terms of PVT.

**Preferential choice and standard Utility theory**

Charitable giving decisions are a type of preferential choice. A preferential choice is any decision in which there is no objectively correct answer. Utility Theory assumes that a latent construct, termed a *util*, determines preferential choice (see e.g., Barberis, 2013; Smith, 1989; Stigler, 1950). Utility Theory equates preferential choice and util (e.g., *value*) by assuming that the consequence of the chosen option has a higher utility than the consequence of any of the alternative, rejected options. This formulation is logically consistent as long as the consequences of the options are believed to provide a benefit to the decision maker.

In giving to charity, the giver chooses to provide an amount of money for an economic good or service that they will likely never receive. As such, charitable giving exemplifies a unique challenge for Utility Theory: the chosen option (i.e., to “give”) appears not to contain any direct benefit (e.g., “utility”) to the giver (e.g., Watson, 2015). Therefore, Utility theorists propose either that the individual forgoes benefit altogether through pure altruism (Andreoni & Miller, 2002) or the “utility” obtained arises *within* the individual from the *act* of giving itself (rather than the goods received) through impure altruism (e.g., a warm glow; Andreoni, Harbaugh, & Vesterlund, 2010).

Whereas pure altruism requires, by definition, the giver receive no benefit from the interaction, impure altruism produces a “private” benefit to the giver. Although there is only one form of true altruism, there are various forms of impure altruism. Khalil (2004) categorizes impure altruism into three rationalistic types (egoistic, egocentric, and altercentric) and three normative types (Kantian, socialization/culturalization, and warm-glow). The rationalistic types describe various ways in which giving produces a benefit in relation to that given. The egoistic altruist will give because contributing to a cooperative environment will maximize the giver’s future expected utility. The egocentric altruist will give because they derive vicarious pleasure from the receiver’s pleasure. The altercentric altruist will give because they have a “moral gene” that allows them to “adopt” the receiver’s utility. The normative types of impure altruism are aimed at explaining giving independent of the degree of benefit received by the giver. The Kantian altruist gives because they experience a “moral utility” (independent of the “pleasure utility”) and there is moral utility in giving. The socialization/culturalization altruist gives to gain approval, etc., from one’s peers. The “warm glow” altruist gives because giving feels good.

Khalil (2004) explains why each form of impure altruism cannot account for why people expend resources for goods or services they will never receive (as in the case of charitable giving behavior). For example, several forms of impure altruism do not explain why the giver gives, rather than enjoys the benefit of a free ride. Others have difficulty explaining why the giver gives to one charity over another. Still others are so vague as to be almost uninformative (e.g., the moral gene hypothesis). Khalil (2004) concludes by stating, “The challenge ahead is to construct an alternative theory” (p. 119).

There are various features of the giver, receiver, and the environment that psychologists have established as influencing charitable giving. For example, people are more willing to donate money when they are happy at the time of donation (Niesta Kayser et al., 2010) or if they are primed to access their emotions when asked to donate (Dickert, et al., 2011). Similarly, people are more willing to donate if the receivers are identified victims rather than statistical victims (Jenni & Loewenstein, 1997; Small & Loewenstein, 2003), members of their in-group (Billig & Tajfel, 1973; Furnham, 1996; Furnham, et al., 2000; Levine et al., 2005; Reed & Aquino, 2003; Simon, et al., 2000; Tajfel, 1970; Tajfel et al., 1971), people who are victims of a similar misfortune (Small & Simonsohn, 2008), and victims of tragedies out of their control (Zagefka et al., 2011). Bekkers and Wiepking (2011) provide an excellent meta-analysis of over 500 papers in which they organize the various features that influence giving into eight factors: the giver’s awareness of the receiver’s need; being solicitated for the donation; various costs and benefits of the donation (similar to Khalil’s rationalistic altruism); altruism (perhaps pure altruism); the reputation of the giver (similar to Khalil’s “socialization/culturalization”); various psychological benefits (including Khalil’s “warm-glow”); pro-social values (more specific variation of Khalil’s Kantian altruism); and the efficacy of the charity. Understanding the features that influence charitable giving is important because they are evidentiary pieces that help identify the social and cognitive processes underlying charitable giving. In the absence of a cohesive model to place them, however, these pieces alone remain difficult to interpret. Indeed, Bekkers and Wiepking (2011) state, “The relative influence of each of the eight mechanisms … is unclear. … We think that identifying systematic patterns in the mix of the mechanisms and interactions among them are important tasks for future research.” (p., 944).

Heeding the calls made by Khalil (2004) and Bekkers and Wiepking (2011), here we submit a computational cognitive theory of charitable giving. Critically, this theory describes the decision process underpinning charitable giving, thus providing a framework for studying precisely how different factors might influence that process.

**Psychological Value Theory**

Generally speaking, with economic transactions, the purchaser of goods is the receiver of those goods, and consequently, derives direct benefit from them. These benefits, however, are not necessarily the object itself. For example, the primary benefit of purchasing a car is not the car itself. Rather, it is a function of a constellation of often psychological factors like convenience and comfort of travel, social status, potential resale value, etc. Without these factors, the car is simply an object of curiosity. Similarly, the purchaser can derive benefit from goods that they purchase, but do not themselves then possess. For example, when a parent purchases food for their child, the parent receives many direct benefits of that purchase (e.g., the continued health and welfare of the child, a positive family dynamic, etc.) even though the food is consumed by their child. Given that a parent does accrue benefits in such cases, purchasing food for one’s child is not considered as an act of charitable giving.

Consideration of the parent/child case above gives rise to the following question: When does an individual derive benefit from goods they purchase, but do not directly receive? In addressing this question, our answer is based in terms of linking accrued benefits to the value that the purchaser attributes to the receiver. That is, the more an individual (Person A) values another (Person B), the more benefit Person A will gain from donating to Person B. As Person B’s value to Person A diminishes, so do the benefits accrued by Person A. Although one may ask for an itemized list of the benefits, we argue that such a list answers a different sort of question (e.g., what are the benefits of owning a car?). What is critical is that charitable giving is well predicted by the value of the receiver in the same way that consumer purchases are well predicted by the value of the goods. To test specific empirical predictions that follow from our person-centered, value-based account, we call upon PVT (for a detailed description, see Cohen et al., 2022).

PVT defines a theoretical construct termed *Psychological Value* (Ψv) as the *perceived* personal worth of a given option (i.e., its subjective value). Because Psychological Value is a perceptual event, it is assumed that its’ underlying representation is noisy (like all perceptions, e.g., Ashby & Lee, 1993). Therefore, in PVT, Psychological Value is captured as a distribution of values, *f*(Ψv), rather than a single value. Such an assumption accords with the similar notions of the representation of sensory evidence in standard signal detection theory (SDT see e.g., Macmillan & Creelman, 2005). Like SDT, each option in the choice is mapped onto its corresponding distribution of Psychological Values. Critically, given two options, the Psychological Value distributions associated with the options are assumed to overlap. The corresponding overlapping region defines the Psychological Values that accrue from *both* options such that the amount of distributional overlap can be interpreted as a measure of the *similarity* of Psychological Value of the two options: The larger the overlapping region, the greater the similarity of Psychological Value of the two options.

It is important to clarify the difference between Psychological Value and the concept of a *util* as discussed in standard economic theory. Perhaps the most fundamental difference is that a util describes the utility of the *consequence* of a choice, whereas Psychological Value is the perceived value of the *stimulus* itself. Because a util is a function of the consequence of the choice, as the magnitude of the consequences change (e.g., a question that involves sacrificing the life of another human vs a question that involves giving $10 to another person) the util will change as well. In contrast, the Psychological Value of a stimulus should predict choices across a variety of cases, with a variety of consequences.

Additionally, whereas the util of an option is considered to vary across individuals (Fishburn, 1981), the perceived Psychological Value of a stimulus is hypothesized to be relatively consistent across individuals (Cohen et al., 2022). For example, whereas I value my own health more than yours, and you value your health more than mine, we both value “my own health” more than that of others. Nonetheless, the distribution of Psychological Values that represent “my own health” should be relatively consistent across individuals. Although there may be some individuals whose distributions of Psychological Values for “my own health” differ significantly from the populations, those individuals would be rare and their deviation may interfere with their fitness.

Although Psychological Value is relatively constant across individuals, it is not immune to context effects. Most context effects, however, are hypothesized to influence most individuals similarly. More formally Cohen and Ahn (2016) defined the Psychological Value of an item (ψ*v*) as

(1)

Here *Pi* refers to perceptible features of events associated with the item and Ci can be construed as contextual weights associated with the item. In fleshing this out Cohen et al. (2022) noted that, “The Ci can vary by situation. For example, the contribution of the flavor of an apple to one’s Psychological Value of that apple may be great when the observer is nutritionally satisfied but may carry little weight when the observer is nutritionally deprived.” (p. 2017). In general terms it is to be accepted that an item’s Psychological Value will vary according to context (such changes will be reflected in the corresponding contextual weights) and these are, in turn, reflected in the distribution of values associated with that item.



**Figure 1. The Robust Random Walk**. The graphs illustrate the RRW decision model. The *y*-axis represents the amount of evidence accumulated in favor of the HVO or the LVO. The *x*-axis represents time. The horizontal dotted lines represent the thresholds for choosing the HVO or LVO, respectively. The evidence begins to accumulate at the *start point*. If the start point is equidistance between the two thresholds, it is unbiased (as illustrated here, and indicated by the grey horizontal line). The top graph illustrates the case where the Psychological Value distribution of the options have a relatively large overlap. Here, the evidence will accumulate slowly towards one of the two options. It will tend towards the HVO threshold, but will frequently and erroneously cross the LVO threshold. The bottom graph illustrates the case where the Psychological Value distribution of the options have a relatively small overlap. Here, the evidence will accumulate quickly towards one of the two options. It will cross the HVO threshold very often and only infrequently erroneously cross the LVO threshold.

Under the assumption that Psychological Value is a perceptual event, Cohen and colleagues (Cohen & Ahn, 2016; Cohen et al., 2022) measured *f*(Ψv) independent of choice using magnitude estimation (Stevens, 1956). Magnitude estimation is a procedure designed and validated as a means to measure more traditional perceptual events (Gescheider, 1988), for example the perceived loudness of a tone (e.g., Marks, 1974). Magnitude estimation involves presenting the participants with a standard stimulus (e.g., a tone of a specific volume) that is assigned a value by the researcher, for example 1000. Then the participant is presented a series of probes (e.g., tones of different volumes), and for each probe, the participant is to assign a value that describes the perception of the probe in relation to the standard. So, if the probe is twice as loud as the standard, the participant would assign a value of 2000. At the time of its development, many researchers thought the procedure was not valid (as discussed by Stevens, 1956), but over time, the procedure has been shown to be both valid and reliable (Stevens, 1975).

In addition to these representational considerations, PVT also describes the underlying cognitive processes that are entrained when participants make preferential judgments. PVT adopts the decisional processes known as sequential sampling procedures (henceforth SSPs) (Busemeyer, Gluth, Rieskamp, & Turner, 2019; Krajbich, 2019; Krajbich, Armel, & Rangel, 2010) because these have been robustly established in many cases of psychological choice (Balakrishnan & Ratcliff, 1996; Cohen & Quinlan, 2016). In general, SSPs are defined on a 2-D Euclidian plane from a starting position between two decision boundaries (see Figure 1). A different boundary is associated with each of the two options. Evidence is continuously sampled at random from two latent distributions, each associated with one of the two options. The favored option, at that time point, is the one associated with the distribution from which the larger value was taken. A step is then taken in the direction of the boundary associated with that option. Evidence gradually accumulates in the direction of one boundary and a decision is made when the accumulated evidence equals the criterion set by that particular boundary. The rate at which information accumulates, termed *drift rate*, is a function of the overlap of the latent psychological distributions (e.g., Link & Heath, 1975; Ratcliff, 1978; Ratcliff & Rouder, 1998).

SSPs predict, in general, that the speed of response (the reaction time: RT) and the response choice are a function of the distributional overlap of the two latent distributions. When the two corresponding distributions have a large overlap, the choice will be difficult because the two distributions driving the SSP will be very similar. This results in a slow, error-prone response. Take, for example, an observer who is asked to identify the longer of two lines. When the line lengths are close, the observer will respond slowly and will have a high probability of erroneously judging the shorter line as longer (because the two lines “look” very similar). In contrast, when the two corresponding distributions have a small overlap, the choice will be easy because the two distributions driving the SSP will be very different. This results in a fast, accurate response. This is analogous to presenting two lines of very different lengths to the observer who must identify the longer line. Here, the observer will be both fast and accurate.

Traditionally, SSPs use the pattern of participants’ RTs and choices to derive estimates of the latent distributions that are driving the sampling process (e.g., Busemeyer, et al., 2019; Krajbich, 2019; Link & Heath, 1975; Ratcliff, 1978; Ratcliff & Rouder, 1998). For example, if the participant responds quickly and accurately, traditional SSPs will estimate latent distributions that have little overlap. When a researcher assumes that “value” is driving a response, these latent distributions are interpreted as representing the “value” of the options (e.g., Busemeyer & Townsend, 1993; see also Busemeyer, Gluth, Reiskamp & Turner, 2019, for a more recent review). However, traditional SSPs theorists do not *test* the assumption that value is driving the responses. For example, the estimated drift rate provides a proxy for the overlap of the Preferential Value (e.g., Krajbich et al., 2010; Rangel & Clithero, 2014; Krajbich, 2019). In this respect, using a sequential sampling procedure to estimate drift rate of a latent “value” construct from preferential choice behavior is theoretically similar to estimating the mean of a latent “value” construct from preferential choice behavior using Utility theory. Whereas Utility theory estimates changes in mean value over a set of options, SSPs estimate the relative difference in value between two choices.

In an attempt to mitigate this limitation, researchers have integrated participants’ ratings of value surrogates into their estimation of drift rate. Experimentally, each value surrogate is most commonly measured along a single dimension such as pleasantness ratings of faces, houses, and paintings (Lebreton, Jorge, Michel, Thirion & Pessiglione, 2009), willingness to pay for snack foods, nonfood consumables (Chib et al., 2009) and movies (Grueschow, Polania, Hare, & Ruff, 2015), and desirability ratings of snack foods (e.g., Colas & Lu, 2017; Gwinn, Leber, & Krajbich, 2019; Krajbich et al., 2010; Krajbich, Hare, Bartling, Morishima, & Fehr, 2015; Lin, O'Doherty & Rangel, 2011). For example, Milosavljevic, Malmaud, Huth, Koch, and Rangel (2010) asked participants to indicate how much they would like to eat each of 50 snack foods on a scale from -2 to 2. Next, participants completed a choice task in which they chose one of two randomly presented snack foods on each trial. The authors used a sequential sampling procedure to estimate drift rate from participants’ RT and response choices. Here, rather than estimate a separate drift rate for each condition, the researchers estimated a single drift rate, scaled it by the difference in value between two options and were able to fit the data reasonably well.

PVT advances this research by defining value, measuring it independent of choice, and using those measurements to predict choice. PVT assumes (and we have some evidence supporting this assumption, Cohen et al., 2022, and internal lab data) that people perceive the Psychological Value of all potential stimuli (objects, animals, people, concepts, etc.). Therefore, rather than collect stimulus specific ratings, Psychological Value is measured consistently across stimuli using the magnitude estimation task. These measurements describe the perceived Psychological Value distribution, *f*(Ψv), associated with each stimulus (rather than just central tendency and therefore there is no need to assume distributional shapes and variance). An individual’s estimate of the Psychological Value of a specific stimulus represents a random draw from the distribution of Psychological Values that represents that stimulus. Because the distribution of Psychological Values representing a stimulus is assumed to be stable across individuals, a large sample of individuals’ estimates of Psychological Values of a stimulus should be a relatively accurate representation of that stimulus’ Psychological Value distribution. Therefore, if the magnitude estimates represent the perceptual qualia of value, then the overlap of these distributions should predict both (i) the probability of choosing the higher valued option, *p*(HVO), and (ii) RT in value-based tasks.

To quantify these predictions, Cohen et al. (2022) developed an SSP that instantiates the assumptions of PVT, termed a Robust Random Walk (*RRW*). Unlike traditional SSPs that estimate latent value distributions from participants’ choices and RTs, the RRW uses the measured Psychological Value distributions to *a* *priori* predict participants’ choices and RTs. To do so, the RRW takes as input the measured overlap of the Psychological Values distributions of options. Inherent in the RRW is the assumption that, when a person is forced to make a preferential choice, they will opt for the option with the highest perceived Psychological Value (termed the “higher valued option” or *HVO*). As such, when people choose the lower valued option (the *LVO*), PVT classifies this as an error. Although classifying the LVO as an error may seem theoretically inconsistent with the concept of *preference*, the classification is consistent with the theoretical underpinnings of the decision model. The RRW predicts similar patterns of responses as traditional SSPs (e.g., large overlaps produce slow, inaccurate responses; small overlaps produce fast, accurate responses). PVT’s predictions, however, are much more constrained than traditional SSPs because the RRW predicts choice and RT from measured Psychological Value distributions (as opposed to derive latent distributions *from* participants choice and RT data; see Table 1 of Cohen et al., 2022). Importantly, PVT’s predictions will fail if (1) its’ definition and measurement of Psychological Value are invalid, (2) the decision mechanism is not well modeled by the RRW, *and/or* (3) the choice is not driven by Psychological Value. As such, if PVT’s predictions are validated, then evidence is provided for the entire system described by PVT (e.g., the features and measurement of Psychological Value, the hypothesized decision process, etc.).

Cohen and Ahn, (2016) tested these predictions. The authors used the magnitude estimation procedure to measure perceived Psychological Value for a variety of items, namely, objects, non-human animals, and humans. Next, they calculated a robust measure of distributional overlap of the value distributions for all pairs of items. A second group of naïve participants were then given a series of trials where, on every trial, they were challenged with a two alternative forced choice (2AFC) in which the decision was to save/or sacrifice one of two options taken from the sample of items used previously. Participant’s choices were noted together with the speed of those choices.

It was shown that, across participants, the probability of choosing the higher valued option, *p*(HVO), scaled as a function of the distributional overlap of the *f*(Ψv)s for the two options. Moreover, decisional speed was also well predicted by the measures of distributional overlap such that decision speed scaled with the distributional overlap of the *f*(Ψv)s. Indeed, distributional overlap of the *f*(Ψv)s accounted for over 90% of the variance in both RT and response choice. These findings not only demonstrate that the perceived Psychological Value of the individuals can be measured accurately, but they also reveal that these measurements accurately predict choice. More critically, this evidence bolsters a central claim of PVT, namely, that people’s preferential choices are driven by an appraisal of the corresponding Psychological Values of the alternative options.

In following up on this work, Cohen, et al. (2022) in their first experiment, took measures of the perceived Psychological Value of (i) individuals in different social roles (e.g., a nun, a judge) and (ii) various numbers of those individuals (e.g., five nuns). The data clearly showed that an individual’s societal role had a large influence on how the individual was valued. For example, participants valued “convicts” lower than they valued “a congressman” (see also Goodwin & Landy, 2014, who showed that the value one places on another person systematically varies with age). Additionally, and contrary to many researchers’ assumptions, quantity had minimal influence on values. So, participants valued one convict about the same as they valued 50 convicts. Cohen et al. (2022) used PVT to predict both RT and response choice. The random walk model, using the Psychological Value of the options as input, predicted performance almost perfectly—accounting for over 90% of the variance in *p*(HVO) and RT. As a consequence, it was concluded that the measures of Psychological Value were accurate, that the random walk accurately described the decision process, and that the perceived Psychological Value of people drives moral judgments. The general conclusion was that the data provided robust support for PVT.

In the present paper, we apply PVT to the topic of charitable giving. If successful, PVT will be the first computational cognitive model of charitable giving with an objectively measured “private” benefit for the giver (i.e., Psychological Value of the receiver) that simultaneously predicts the participants’ response time and choice. In Experiment 1, we test the specific and novel hypothesis that charitable choice is driven by the perceived Psychological Value of the receiver as assessed by the giver. Under this hypothesis, PVT predicts that, when presented with the question of which of two options to donate to, a respondent will choose to make a donation on the basis of an appraisal of the corresponding Psychological Values of the alternatives. Although some may find this demonstration intuitive, we are testing a *specific* quantified process model that instantiates this hypothesis. Furthermore, such a demonstration provides an objective measure of benefit to the giver in charitable giving – something that many have not yet been able to identify (e.g., Khalil, 2004). Third, if the model is accurate, it will demonstrate that the Psychological Value of stimuli predicts choice across questions with very different consequences. Previously it has been demonstrated that these same Psychological Values predict choice when making life or death decisions. Such consequences are vastly different than the consequences in the present experiments, whereby participants are asked to decide to whom they will donate $10. Such a demonstration, while perhaps unsurprising, is counter to most economic theory that states choices are a function of the consequences rather than the stimuli *per se* (Fishburn, 1981).

In Experiment 2, we replicate and extend Experiment 1 by removing the context from the scenarios and demonstrate that PVT predicts charitable giving in individual participants. Demonstrating that the predictions are accurate on an individual participant level speaks to the robust nature of the effect.

In Experiment 3, we demonstrate how PVT can be used as a framework to systematically study how various factors of interest influence the social and cognitive processes associated with charitable giving. Specifically, in Experiment 3 we add an in-group/out-group label to the receivers of the charity and identify how that label influences the perception and decision processes.

In Experiment 4 we extend PVT beyond charitable choice to charitable giving itself. That is, Experiment 4 presents participants the option to donate or keep the money. By doing so, we address whether the Psychological Value of the receiver drives charitable giving or is simply used as the measure of who to give to when presented with a forced choice. This experiment directly addresses the long running debate in the charitable giving literature about the role of impure altruism in charitable giving. That is, it shows how an objectively measured “private” benefit to the receiver can account for charitable giving behavior.

Finally, in Experiment 5, we compare the predictive ability of PVT to that of an account based on competence and warmth. There is considerable evidence that the perceived competence and warmth of Person A relates to how Person B interacts with Person A (see Fiske, Cuddy & Glick, 2007). It is therefore informative to assess how well PVT predicts relative to these two theoretical constructs.

**Experiment 1**

In Experiment 1, we tested the PVT prediction that participants will choose to donate to the option with the greater psychological value. We presented participants with scenarios in which they had to choose to give to one of two options. On each trial, both the speed and nature of the choice were noted. We use the distributional overlaps of Psychological Values of options collected by Cohen et al. (2022) to predict choice in the current set of experiments in line with the hypotheses derived from PVT.

**Method**

**Participants**

Participants included 104 undergraduate students who received partial credit towards a course requirement. Although it would have been preferable to record demographic data, at UNC Wilmington students average age is 22, there are 64% females, and 78% of students are white, 5% are African American, 4% are of one or more other races, and 2% are unknown.  About 7% of students are Hispanic. Credits were assigned based on showing up for the experiment. Participants were permitted to leave without penalty and all responses were anonymous.

Cohen and Ahn (2016) reported extremely large effect sizes (e.g., *r2* > 0.85) with 120 trials per participant. This resulted in an estimated power ≈ 1.0 to detect the effect in an individual participant. Given this level of power, group data is needed to demonstrate consistency in results (rather than increasing power) and provide enough data for the computational model to run well (i.e., provide stable estimates of the dependent variables at each cell). As such, sample size was determined by (a) setting a minimum number of participants (60) to obtain sufficient data for each item for data analysis, (b) estimating the time necessary to collect that number of participants, (c) posting all available experimental slots for the time estimated in “b,” and (d) running all participants who signed up. This resulted in the collection of more than the minimum number of participants because of a higher-than-expected sign-up rate.

**Apparatus and Stimuli**

Participants completed the experiment on a 24-in. LED color monitor Mac with a 72-Hz refresh rate controlled by a Macintosh Mini running an OS X. The resolution of the monitor was 1920 1200 pixels. Participants input responses via an Apple keyboard.

Participants were shown 10 different scenarios eliciting donations. Each scenario followed a basic format involving two items (e.g., a nun and a mother) in need of monetary donations. The scenarios explained that these items would receive donations via separate GoFundMe accounts set up by an unidentified third party. Participants were to choose to donate $10 to one of the two accounts. The reasons for needing donations varied per trial and scenario, as did the people in need of donations. All reasons for needing monetary donations adhered to the following set of criteria, as determined through an intensive, collaborative effort from members of the Cohen Cognition and Perception Laboratory:

1. Each reason for needing a donation was equally plausible for all items in the experiment.

2. Each reason was expressed in a similar range of words (7 to 25 words).

3. Each reason for needing a donation was politically and socially neutral.

4. Each reason for needing a donation was initiated by some circumstance out of the items’ control.

An example of these scenarios is:

*Through circumstances out of their control,*

*Item A and Item B*

*have lost their houses to fires. A third party has created a GoFundMe account for each person to rebuild their houses. You have $10 which you can donate to only one of these GoFundMe accounts.*

*Would you prefer to donate the $10 to help rebuild the house of*

*Item A or Item B?*

The ten different scenarios were consistent in format, varying only by item and reason for needing a donation (see Supplemental Information for a complete listing of the scenarios). For example, instead of “Item A” and “Item B” needing money to help rebuild their houses, a different scenario may explain that they need money to pay for a medical treatment. In each scenario, “Item A” and “Item B” were selected from a list of 22 items. The complete list contained a range of items based on their *f*(Ψv), spanning from items of very low *f*(Ψv) (i.e., a terrorist or a pedophile), to items with very high *f*(Ψv) (i.e., an astronaut or an Olympian). This list of items contained the same human items used in Experiments 2-5 of Cohen et al., (2022), except for three items: a billionaire, an orphan, and a homeless adult. A billionaire was excluded from the list because the item is characterized by having an abundance of money, which could dissuade participants from choosing to donate money to that option. A homeless adult and an orphan were not included in the item list because they were not applicable to at least one of the scenarios (e.g., a homeless person and an orphan (which many people associate with a child) do not own houses, which is a requirement of one of the scenarios). To replace the three removed items, three new items were added: a mother, a father, and a teacher. A mother and a father both are both items with high *f*(Ψv) and therefore provided more values to the upper end of the scale. A teacher was also used in this experiment because it is an item that could plausibly experience all of the circumstances described in the 10 scenarios.

**Procedure**

The procedure followed that used in Experiment 2 of Cohen and Ahn (2016). Participants completed the experiment in a small dark testing room containing a desk, a chair, and a computer. White noise played over speakers in the room to mask any ambient noise.

On each trial participants were presented with a fixation point, followed by the scenario in which the two items were masked for a fixed amount of time, followed by the unmasking of the items. The masked scenario was presented first so that participants would be able to read and comprehend the scenario, without being able to initiate a decision process. This removes the scenario reading time from the participant’s RT and was shown not to influence responses.

More precisely, each trial began with a 500 ms red fixation cross point, followed by the scenario in which the options (i.e., Item A and Item B) were masked. In the beginning of each trial the masked scenarios were presented with each character in the right item, Item A, replaced with a teal “+”, and each character in the left item, Item B, replaced with a teal “=”. A progress bar to the left of the scenario filled at a rate of 300 ms per word in the scenario to graphically show participants how long they had to read each scenario. The rate of 300 ms per word provided participants with sufficient time to reach each scenario fully before being asked to respond.

Once the progress bar completely filled, the two items were unmasked (see Figure 2). With the complete unmasked scenario presented, participants then responded to whom they would donate using either the “d” or “k” keys. As one item was presented on the right side of the screen and the other on the left, the “d” and “k” keys corresponded to “Right Item” and “Left Item.” Participants’ choice and RT were recorded for each trial. RT was measured starting when Item A and Item B were unmasked, and choice was recorded. The unmasked scenario remained on the screen until the participant input their response, at which point the timer stopped and the screen moved to the next trial (see Figure 3).

Text

Description automatically generated

**Item A or Item B?**

**Item A and Item B**

**Figure 2. Unmasked charitable donation scenario**. After the progress bar on the left completely filled, the items were unmasked, and participants responded using either the “d” or “k” keys.

Participants were instructed to read the masked scenario while the progress bar filled and to then respond to the question regarding the scenario at the bottom of the screen once the items unmasked. Participants were told to respond as quickly as they can, but while still providing a thoughtful response. There was no deception in this experiment. Participants were aware that these were hypothetical scenarios, and no donations were to be made.

Graphical user interface

Description automatically generated with medium confidence

**Figure 3. Sequence of screens for each trial**. Each trial consisted of a red fixation cross presented for 500 ms (a), followed by a masked scenario (b). Once the progress bar completely filled, the items were unmasked, and the participant could indicate to which item they would donate (c).

For each trial for every subject, “Item A” and “Item B” were randomly selected without replacement from the list of 22 items such that the two people within a scenario were never the same. Items were randomly selected with replacement across trials; therefore, the same combination of items could appear across different scenarios. Experiment 1 consisted of eight practice trials followed by forty minutes of experimental trials. The practice trials contained the same scenarios and items as the experimental trials. After practice trials, a dialogue box appeared to remind participants of the “d” and “k” key responses. Participants then completed 40 minutes of experimental trials. After completing experimental trials, a dialogue box appeared thanking participants for their participation and informing them that the experiment had finished.

To ensure that participants were familiar with the content of the scenarios, they also responded to two questions after the completion of the experiment. One question asked participants to briefly describe the purpose of GoFundMe accounts and the other was an open-ended question asking them to introspect about the task. The answers to these questions were not analyzed. Participants were presented 8 practice trials followed by experimental trials. The experimental session lasted 45 minutes, with a self-timed break every 13 minutes. Participants completed an average of 83.6 trials (SD = 5.5) in the experimental session.

All procedures were approved by the UNC Wilmington IRB, protocol# 16-0210. The data and R code for the analysis of Experiment 1 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>. The stimuli and instructions can be found in the Supplemental Information. The experimental presentation code is proprietary and is not open sourced. The experiment was not pre-registered. In these studies, we report all measures, manipulations, and exclusions.

**Results and Discussion**

**Psychological Values**

We used the Psychological Values collected by Cohen et al. (2022) to predict our participants’ choice behavior. Although we did not collect Psychological Value estimates in the current experiment, for clarity, we briefly review Cohen et al.’s (2022) procedure here.

In that study, participants were told,

“In this experiment, we ask you to think about how much you value different items. We will call this the item's "personal value."  You can define "personal value" in any way you find appropriate. "Personal value" is not necessarily the same as monetary value.  For example, we may ask the "personal value" of your first report card.  Here, the monetary value may differ dramatically from the ‘personal value.’"

Participants were presented a chimpanzee as the *standard* stimulus and told that it’s personal value equaled 1000. They were to provide a numerical value in relation to the chimpanzee that described their “personal value” of each probe. Further, they were given an example probe of a “fish” and told,

“If you think the fish has a ‘personal value’ 7 times that of the standard, then assign the fish a value of 7000. If you think the fish has a "personal value" one fifth that of the standard, then assign the fish a value of 200 … Remember, you can use any number you feel is appropriate. There are no limits on the values you can use.”

Each of 414 participants provided estimates of a subset of 192 items.

A non-parametric bootstrap procedure was used to estimate the overlap of the Psychological Value distributions. There were 231 potential pairs of options for the 22 items used in the present experiment. The overlaps were combined into successive bins of 0.1 (labeling each bin by the center value of the bin interval, e.g., the bin 0-0.1 is labeled as 0.05), and termed the variable, O0.1.

**Choice Data**

In the present experiment, two participants were removed due to issues with computers during the experiment[[1]](#endnote-1). We optimized the data filtering procedure in the following ways. First, we calculated the coefficient of variation (CV)[[2]](#endnote-2). The CV is a measure response time consistency, with higher CVs indicating large fluctuations in RT across trials. We then removed the participants with largest 5% CVs (6 participants). Ninety-six total participants remained. For each O0.1 bin, we removed individual trials with the slowest and fastest 2.5% of the RTs. All statistical analyses are two tailed tests, unless otherwise specified.

In RT tasks with multiple trials, the participant will tend to get faster at the task as the trials progress. This increasing speed is a function of participants learning the task requirements, motor responses, etc. To identify the influence of trial number on RTs, we fitted a learning function to the data, *log*(RT) = *a* \* exp (*b* \* (trial – 1)) (after Cohen & Ahn, 2016). There was a significant influence of both parameters (*a* = 7.55, *b* = –0.00038), and both *t*’s > 14, *p*’s < .001. To remove the influence of trial number on RTs, the residuals of this function (RTres), served as the RT outcome variable.

**Regression Analysis**

The relation between Overlap and *p*(HVO), averaged across participants, was assessed by fitting the exponential decay function, *p*(HVO) = 0.5 \* (1 – O0.1*b*) + 0.5, and analysis revealed a significant relation between Overlap and *p*(HVO), *b* = 2.61, *t* = 13.05, *p* < .001, *r2* = 0.95 (see Figure 4, top left). We also fit a linear regression to assess the relation between RT and Overlap, RTres = *a* + (*b* \* O0.1) and found that there was a significant linear relation between Overlap and RT, *F*(1, 8) = 304.8, *p* < .001, *r2* = 0.97. Both the intercept (*a* = –0.25) and the slope (*b* = 0.50) were significantly different from 0, *t*’s > 15, *p*’s < .001 (Figure 4, bottom left). Both analyses had an 80% power to detect an effect size of *f2* = 1.0.



**Figure 4. Experiment 1 regression and RRW results.** The p(HVO) and RT fit using separate regressions (left column). The *p*(HVO) (top), RTHVO (bottom circles), and RTLVO (bottom “x”s) fit simultaneously (right column) using the RRW. The black lines/symbols represent responses to the LeftHVO trials, and the darker grey lines/symbols represent responses to the LeftLVO trials. The light gray lines represent 40 individual iterations of the RRW with parameters from the best fit model. The solid line shows the average fit of those 40 lines.

Next, we ran an item analysis to estimate the probability of choosing each individual item relative to every other item used in the experiment. To illustrate this, the item analyses of “a solder,” “an adult,” and “a thief” are shown in Figure 5. In the associated graphs, the overlap of the items is plotted on the *x*-axis, whereby the left-most item is the item that has the least amount of distributional overlap and is more valued than the target item. The right most item has least amount of distributional overlap and is less valued than the comparison item. Therefore, the likelihood of choosing the target item should be least at the left bound, most at the right bound, and gradually increasing in between. All items displayed this predicted pattern.

****

**Figure 5. Item Analyses**. Each graph shows the probability of choosing an individual item (shown in the bottom right corner of each graph) relative to every other item present in the experiment (*y*-axis). On the *x*-axis, the left-most item is the item that has the least amount of distributional overlap and is more valued than the target item. The right most item has least amount of distributional overlap and is less valued than the comparison item.

**RRW Analysis**

To assess whether PVT modeled the data well, we fit the data using the RRW (for details, see Cohen, et al., 2022). Unlike most SSPs, the RRW does not estimate drift rate. Rather, the RRW uses Overlap (as defined) as a direct measure of drift rate. By doing so, the RRW is much more constrained than traditional SSPs. The RRW simultaneously predicts *p*(HOV), the RT to select the higher values option (RTHVO), and the RT to select the lower valued option (RTLVO). The RRW’s only free parameters are non-decision time (*Ter*), response bias parameters (boundary separation and start point), an error parameter (*NSD*), and the Information Accrual Bias (*IAB*). The *IAB* is computed from an exponential function that weights the influence of the sample, *Vi*, by the time since the start of the process (other kinds of weightings have also been discussed, see Ashby et al., 2016). The function is defined relative to two free parameters *dA* and *dB*. Here, we fix *dA* at 0.2 and let *dB* fit to the data. When *dA* is fixed and *dB* is positive, this indicates that perceptually recent information carries more decisional weight than perceptually distant information.

We compared the performance of four models.

* The *Simple Model*, which assumed no response bias and accurate value estimates. This model had four free parameters: boundary position (*b*), non-decision time (*Ter*: this parameter is essentially an intercept term and accounts for the cumulative time of processes not associated with Overlap, such as encoding time, motor response time, etc.), an error parameter *s* that adds noise to the samples (*NSD*), and the recency influence of information as captured by the *dB* parameter.
* The *Value Constant Model*, which assumed no response bias but that the value estimates were all shifted by a constant. This model was identical to the Simple Model, with the exception that there was an added free parameter (i.e., *vc*) that adjusted the separation of the pair-wise overlaps by a constant.
* The *Side Bias Model*, which assessed for a response bias such that participants preferred the item on the left or right side of the display. This model was identical to the Simple Model, with the exception that it added a free parameter to capture the shift in the start point toward the item on the preferred side. This was implemented by dividing the data into two trial types: scenarios in which the LVO was on the left (LeftLVO) and scenarios in which the HVO was on the left (LeftHVO). Within the context of the RRW, a bias toward the “Item on the Right” translates into a positive bias in the LeftLVO trials and an equal but negative bias in the LeftHVO trials. To model this, we effect code the LeftLVO condition as 1 and the LeftHVO condition as -1. This effect coding is multiplied by the *start point* parameter which then varies the start point as a symmetric positive/negative effect around a zero start point for the two trial types.
* The *Value Constant + Side Bias Model* added both a value constant and a response bias. This model was identical to the Simple Model, with the exception that it added both the value constant and response bias parameters.

In addition to the free parameters described in these models, the RRW was run with the *dA* parameter fixed at 0.2, and all other parameters fixed at 0. We used our own Smart Grid Search algorithm to optimize the parameters (for details, see Cohen et al., 2021).

Before fitting the models, we collapsed across participants to get stable estimates of RT and *p*(HVO) per Overlap x Boundary Crossed x Bias Effect Code combination. We had very few LVO choices for some low overlap (high accuracy) trials. To remove unstable estimates, we excluded conditions that had fewer than 40 trials. The RRW is stochastic, hence different runs of the model will eventuate in slightly different fits. To quantify the variability of the fits with a single set of parameters, we ran the best fit model 40 times and calculated the average predicted RT and *p*(HVO) for each Overlap together with the *r2* and BIC of that average predicted fit. Figure 4 (right column) presents the data and model fits. We identified the model with the lowest BIC as the best fit model. If, however, multiple models had essentially equivalent *r2* and BIC values, we favored the simpler model on the basis of parsimony.

Table 1: The number of free parameters, *r2*, and BIC for each RRW model fit to the data in Experiments 1-4. The last column indicates the model with the best overall fit.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Experiment 1 | | | | |
| Model | Free Parameters | *r2* | BIC | Best Fit |
| Simple | 4 | 0.92 | -155 |  |
| Value Constant | 5 | 0.93 | -158 |  |
| Side Bias | 5 | 0.92 | -150 |  |
| Value Constant + Side Bias | 6 | 0.95 | -162 | **✓** |
| Experiment 2 | | | | |
| Model | Free Parameters | *r2* | BIC | Best Fit |
| Simple | 4 | 0.92 | -207 |  |
| Value Constant | 5 | 0.93 | -212 |  |
| Side Bias | 5 | 0.94 | -220 |  |
| Value Constant + Side Bias | 6 | 0.95 | -232 | **✓** |
| Fixed Experiment 1 | 1 | 0.91 | -213 |  |
| Experiment 3 | | | | |
| Model | Free Parameters | *r2* | BIC | Best Fit |
| Value Constant | 5 | 0.86 | -228 |  |
| In-group Start Point Bias | 6 | 0.89 | -245 |  |
| In-group Value Change | 6 | 0.93 | -273 | **✓** |
| In-group Start Point Bias + In‑group Value Change | 7 | 0.91 | -254 |  |
| Experiment 4 | | | | |
| Model | Free Parameters | *r2* | BIC | Best Fit |
| Value Constant | 5 | 0.79 | -115 |  |
| Start Point Bias | 6 | 0.80 | -114 |  |
| Dollar Value Change | 7 | 0.86 | -133 | **✓** |
| Dollar Start Point Bias | 8 | 0.86 | -127 |  |
| Start Point Bias +  Dollar Value Change | 8 | 0.86 | -128 |  |
| Dollar Start Point Bias +  Dollar Value Change | 10 | 0.86 | -122 |  |

Table 1 presents the *r2* and BIC for each model. All models fit the data quite well, but the Value Constant + Side Bias Model was the best fit with an *r2* = 0.95. The values of the six free parameters are presented in Table 2. The boundary value represents 0.5 times the separation of the boundaries. Although the boundary value cannot be interpreted in isolation, changes in boundary values across conditions, stimuli, etc., are meaningful (e.g., a larger boundary in Condition A vs. Condition B indicates that participants have a greater accuracy bias in Condition A relative to Condition B). The positive recency of information effect, *dB*, indicates that as information accumulates over time, participants placed more weight on more recent information than more distant information. This is a common finding in value-based decisions that we analyze using the RRW, nonetheless, the parameter value is still quite small. The value constant parameter, *vc*, equaled -0.04. This indicates that the random walk fit best when the Overlaps of all the pairs of items were reduced by about 0.04. This, in essence, decreases the value similarity of all the pairs of probes. Finally, the start point parameter, *s*, that codes a side bias in the experiment, equaled ‑0.07. This indicates that participants had a very slight bias toward a “left” response (i.e., the start point was about 7% closer to the left item’s boundary).

Table 2: The parameters and *r2* for the best fitting RRW models in Experiments 1 – 3.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Experiment | Ter | NSD | Boundary | dB | S | Vc | Ingroup vc | *r2* |
| Experiment 1 | -0.43 | 4.25 | 51.9 | 0.003 | -0.07 | -0.04 | NA | 0.95 |
| Experiment 2 | -0.23 | 5.25 | 56.7 | 0.0 | -0.06 | -0.05 | NA | 0.95 |
| Experiment 3 | -0.41 | 4.00 | 39.0 | 0.006 | NA | -0.02 | -0.10 | 0.93 |

In sum, the results showed that PVT accounted for 95% of variance in response choice and 97% of variance in RT when analyzed using regressions. Moreover, we tested four versions of the random walk process and discovered that Value Constant + Side Bias Model best fit the data accounted for 95% of variance in the RTHVO, RTLVO, and *p*(HVO) simultaneously. These results show that, as predicted by PVT, the value of the receiver to the giver is the primary driver of choice when presented with charitable scenarios. Further discussion of these results and their implications is included in the General Discussion.

**Experiment 2**

In Experiment 1, we demonstrated that the Psychological Value of the receiver to the giver predicts charitable donation behavior with a high degree of precision, when modeled with PVT. In Experiment 2, we replicate and extend the results of Experiment 1 in the following way. Specifically, our charity scenarios have a formulaic structure: two people are in need because of [*reason X*] and a third party is collecting the donation. The participant is asked who they would donate to. Here, we assess whether PVT can successfully predict the data when the reason for the need is removed. Furthermore, because the scenario is simplified, we were able to collect enough data to examine whether individual participants exhibit the same pattern of behavior as is reflected in the group data.

**Method**

**Participants**

Participants included 93 undergraduate students who received partial credit towards a course requirement. Sample size was determined by the same procedure used in Experiment 1.

**Design and materials**

The apparatus and stimuli were the same as in Experiment 1 with one exception: a single simplified scenario was used throughout. The scenario was as follows:

*Through circumstances out of their control,*

*Item A and Item B*

*areinneedofaid.Athirdpartyiscollectingcharitabledonationsforeachperson.*

*Would you donate to*

*Item A or Item B?*

**Procedure**

The procedure of Experiment 2 was identical to that of Experiment 1. Participants completed an average of 266 trials (SD = 31) in the experimental session.

All procedures were approved by the UNC Wilmington IRB, protocol# 16-0210. The data and R code for the analysis of Experiment 2 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>. The stimuli and instructions can be found in the Supplemental Information. The experimental presentation code is proprietary and is not open sourced. The experiment was not pre-registered. In these studies, we report all measures, manipulations and exclusions.

**Results and Discussion**

Five participants were removed due to issues with computers during the experiment. We filtered the data in Experiment 2 using the same process as Experiment 1. Again, we removed the 5% participants with the largest CVs (5 participants). The final participant sample comprised 83 individuals. For each O0.1 bin, we removed individual trials with the slowest and fastest 2.5% of the RTs. Finally, we removed the influence of learning from the RT data by fitting the same learning function to each participant’s data and the corresponding RT residuals were used in the following analyses.

**Regression Analysis**

We analyzed the data in Experiment 2 using the same process as Experiment 1 (see Figure 6). There was a significant relation between Overlap and *p*(HVO), (*b* = 2.13), *t* = 18.2, *p* < .001, *r2* = 0.98, and there was a statistically significant linear relation between Overlap and RTres, *F*(1, 8) = 328, *p* < .001, *r2* = 0.98. Both the intercept (*a* = –0.14) and the slope (*b* = 0.29) were statistically reliably different from 0, *t*’s > 15, *p*’s < .001. Both analyses had an 80% power to detect an effect size of *f2* = 1.0.



**Figure 6. Experiment 2 regression and RRW results.** The left column contains the *p*(HVO) and RT fit using separate regressions. The right column contains the p(HVO) (top), RTHVO (bottom circles), and RTLVO (bottom “x”s) fit simultaneously using the RRW.

For the RRW, the black lines and symbols represent responses to the LeftHVO trials, and the grey dashed lines and symbols represent responses to the LeftLVO trials. The very thin light gray lines represent 40 individual iterations of the RRW with parameters from the best fit model. The thicker solid line shows the average fit of those 40 lines. The 40 individual iterations are visible on the RT graph, but fall directly on the average fit lines on the p(HVO) graph.

To assess whether individual participant’s data were well predicted by PVT, we fit the same RT and *p*(HVO) functions to each individual participant’s data. Figure 7 (top left and top right) presents the best fit RT and *p*(HVO) functions for each participant. The shade of gray of the line is determined by the *r2* of the fit, whereby an *r 2*= 0 was plotted in white, with the shade of gray transitioning linearly to black when *r2* = 1. The *r2* for each participant’s RT and *p*(HVO) function is presented in the bottom row of Figure 7.



**Figure 7: The data of individual participants in Experiments 2.** The upper two graphs present each participant’s best fit function for RT (left) and *p*(HVO) (right). The shade of grey of the line indicates *r2* value, where darker grey corresponds to higher *r2*. The graphs on the bottom present the *r2* of each participant’s best fit function for RT (left) and *p*(HVO) (right). The *r2* for RT is signed, whereby the *r2* is assigned the same sign as the slope.

To determine whether the parameter values and *r2* values differed significantly from 0, we ran appropriate *t*-tests (80% power to detect an effect size of Cohen’s d = 0.31). The average intercept of the linear relation between RT and Overlap (M = ‑0.14, SD = 0.09) was significantly less than 0, *t*(82) = -14.2, *p* < 0.001. The average slope of the linear relation between RT and Overlap (M = 0.29, SD = 0.19) was significantly greater than zero, *t*(82) = 14.3, *p* < 0.001. Because PVT makes directional predictions, we transformed *r2* to reflect the direction of the slope by multiplying *r2* by -1 if the slope associated with a particular *r2* was negative. We termed this transformed measure, *r2signed*. This transformation weights negative slopes heavily against the average effect size (which is the opposite of our prediction) and would result in an average *r2signed* of 0 if the slopes of the best fit lines were distributed randomly around 0. The average *r2signed* of the linear relation between RT and Overlap (M = 0.57, SD = 0.27) was significantly greater than 0, *t*(82) = 18.9, *p* < 0.001. The average beta of the exponential decay relation between *p*(HVO) and Overlap (M = 2.76, SD = 1.82) was significantly greater than zero, *t*(82) = 13.8, *p* < 0.001. The average *r2* of the exponential decay relation between *p*(HVO) and Overlap (M = 0.75, SD = 0.21) was significantly greater than zero, *t*(82) = 32.9, *p* < 0.001. Consequently, these findings show that individual participant’s data were well predicted by PVT.

**RRW Analysis**

We fit the same models to the data from Experiment 2 as we used Experiment 1, with one addition: the Fixed Value Constant model. This is a simple variant of the Value Constant model from Experiment 1, but now we fixed all parameters to the values estimated in Experiment 1, with the exception of the non-decision time (*Ter*) parameter, that was free to vary.

Table 1 presents the *r2* and BIC for each model. All models fit the data reasonably well, but the Value Constant + Side bias model (BIC = -232; *r2* = 0.95) provided the best fit (see Table 2 for parameter values). Although the fixed parameter model did not provide the best fit, the finding that the data from Experiments 1 and 2 were fit best with the same model demonstrates the robust nature of PVT. This result provides confirming evidence that the value of the receiver to the giver is driving our participants’ decisions. Critically the model provides compelling fits for the data both averaged over participants and for individual participants alike.

**Experiment 3**

In Experiments 1 and 2, we have shown that PVT predicts charitable donation behavior as a function of the perceived Psychological Value of the receiver. However, PVT is far more wide-reaching than this. We do not claim that the Psychological Value of the receiver is the only factor that can influence charitable giving. Rather, we claim that Psychological Value is *the* primary factor that influences preferential choice (of which charitable giving is one type), and that PVT provides a framework for the precise study of how other factors influence the choice of interest.

In Experiment 3, we demonstrate PVT’s ability to identify the source of a feature’s influence on choice by assessing the influence of in-group favoritism in preferential choice tasks. It is well-established that people are more likely to donate to members of in-groups than out-groups (e.g., Ockenfels & Werner, 2014). Evidence for in-group favoritism, in general, is persuasive: It occurs when an in-group/out-group distinction is drawn arbitrarily (Billig & Tajfel, 1973; Tajfel, 1970; Tajfel et al., 1971), or in cases where the distinction is extant, for example, amongst those with shared sexual orientation (Simon et al., 2000), national identity (Reed & Aquino, 2003), group affiliation (Levine, et al., 2015), religious beliefs (Preston & Ritter, 2013), political parties (Misch et al., 2018), and smoking status (Furnham et al., 2000). In such cases, people are demonstrably more likely to donate, more willing to donate, or produce prosocial behavior towards members of their own in-group rather than the out-group. Such in-group favoritism also has been found in the Dictator Game where a robust finding is that generosity towards the other is enhanced when the other is a member of the participants in-group relative to when the other is a member of the out-group (Misch et al., 2018; see also Fehr et al., 2008, for developmental evidence for this).

Such in-group favoritism is more generally discussed in terms of *in-group bias* (Brewer, 1979), but this gives rise to the question of exactly how the in-group specification influences the charitable giving process. Within the framework of PVT (and other decision theories founded in sequential sampling procedures), there are several avenues for factors to influence choice. The primary avenue is through changing the Psychological Value of one or both stimuli (see Figure 8). PVT holds that Psychological Value is perceived, therefore a change in Psychological Value results in a change in the phenomenological experience of the stimulus. Because the perception of Psychological Value is automatic and pre-attentive, any manipulation that changes the Psychological Value of a stimulus will be doing so outside the cognitive control of the viewer. This implies that the manipulation taps into either an evolutionarily important and/or highly learned feature. As an analogy, facial symmetry is positively related to attractiveness (Thornhill & Gangestad, 1999), therefore any manipulation that influences facial symmetry (e.g., the wearing of jewelry, etc.) will also pre-attentively influence the phenomenological experience of attractiveness.



**Figure 8.** This figure illustrates how in-group status might change the Psychological Value of the item it modifies. Imagine a choice between two Items. One is the HVO and one is the LVO, by definition. The top figure illustrates the change that occurs when the in-group status is applied to the LVO item. Here, the Psychological Value of that Item is increased, thus increasing the overlap between itself and the HVO. This greater overlap would produce slower, less accurate responses. The bottom figure illustrates the change that occurs when the in-group status is applied to the HVO item. Here, the Psychological Value of that Item is increased, thus decreasing the overlap between itself and the LVO. This smaller overlap would produce faster, more accurate responses.

Another way in which factors of interest may influence choice is through response bias. A response bias is independent of the Psychological Value of the stimulus. As such, factors that influence response biases do not change the phenomenological experience of the stimulus. Response biases are under cognitive control, and therefore can be controlled through instructional or context manipulations. There are two primary routes for response biases to manifest within PVT, namely, a boundary separation bias and a start point bias. A boundary separation bias refers to a change in the distance between the two boundaries. Changes in boundary separation generally model speed/accuracy tradeoffs. When the boundary separation is small, it takes fewer steps for the accumulated evidence to reach a boundary than when the boundary separation is large. As a result, a small boundary separation translates into a fast, but less accurate response. The reverse is true for a large boundary separation. Boundary separation biases are easily induced through instructional manipulations, such as asking the participant to emphasize speed vs. emphasizing accuracy (Ratcliff et al., 2015). In this way, boundary separation biases influence the choice, but do not influence the phenomenological experience of the Psychological Value of the stimulus.

A start point bias refers to the relative position between the two boundaries that the evidence begins to accumulate (see Figure 9). When the start point is equal distance between the boundaries, it is termed an unbiased start point. When the start point is shifted closer to one boundary than the other, the start point is *biased*. Start point biases model the cases whereby the observer has a preference for a particular option or stimulus class. Here, both the start point and the relative Psychological Values of the two options will influence which boundary is eventually reached and how long such a process will take. Because the start point is closer to the preferred boundary than the non-preferred boundary, for a specific distributional overlap, the evidence has a higher probability of crossing the preferred boundary than the non-preferred boundary. Furthermore, those trials where the preferred boundary is reached will be faster than those trials where the non-preferred boundary is crossed. Thus, factors that influence the position of the start point will influence the eventual response choice. Nevertheless, they do not influence the phenomenological experience of the Psychological Value of the stimulus. That is, the underlying distributions driving the random walk process remain unchanged.

As an example, imagine being presented with a series of choices whereby you will be given a free meal at one of two restaurants that can be either near your house or far away. Further imagine that, even before seeing the options, you are inclined to choose the restaurant that is closer to you. This *pre-choice inclination* is quantitatively modeled in an SSP like PVT as a start point bias to choose the restaurant near your house. Although, in general, you will choose the restaurant with the better food, those restaurants that are near your house will have an advantage. Therefore, a distant restaurant with slightly better food than a nearby restaurant will likely be declined. Indeed, if your start point bias toward nearby restaurants is very strong, even a distant restaurant with much better food than a nearby restaurant may be declined. Critically, the start point bias does not result in you perceiving the mediocre nearby restaurant as having better food than the tasty distant restaurant. Rather, it is a cognitively controlled compromise you make because you prefer to eat close to home.



**Figure 9.** This figure illustrates how in-group status might change the start point of the item it modifies. Imagine a choice between two Items, one is the HVO and one is the LVO. When the respondent has a start point bias towards the in-group item, then the evidence will start to accumulate closer to the item that is modified by the in-group status, than the one that is not modified by the in-group status. The top figure illustrates the start point position when the in-group status is applied to the LVO item. Here, the evidence begins to accumulate closer to the LVO threshold than the HVO threshold. This produces LVO responses that are faster and more frequent than expected by an unbiased start point. Similarly, the HVO responses are slower and less frequent than expected by an unbiased start point. The bottom figure illustrates the equivalent set of conditions when the in-group status is applied to the HVO item.

In our third experiment, we assess whether in-group favoritism is a result of a change in the Psychological Value of the stimuli or a start point bias. In each scenario, we presented participants with a choice to donate money to one of two individuals, each of which labeled with the college that the individual attended. Individuals classified as UNC Wilmington alumni were designated as a member of participants’ in-group, because they were aligned with the same university as the participants. Individuals classified as alumni of another college were therefore designated as out-group members. This allowed us to create three different types of trials: *Compatible*, *Incompatible*, and *Neutral.* First, there are Compatible trials, in which the HVO (higher valued option) belonged to the donor’s in-group and the LVO (lower valued option) belonged to the donor’s out-group. Second, there are the Incompatible trials in which the HVO belonged to the donor’s out-group and the LVO belonged to the donor’s in-group. Finally, there are Neutral trials in which both options belonged to the in-group or both belonged to the out-group. Different predictions follow depending on whether the influence of group identifiers is modelled via a shift in the start position of the random walk, or a change in the relative Psychological Values of the individuals given their group allegiance (e.g., Figures 8 vs 9).

**Method**

**Participants**

Participants included 121 undergraduate students who received partial credit towards a course requirement. Sample size was determined by the same procedure as Experiment 1, with the minimum sample size set at 80 (because ingroup/outgroup effect increased the number cells in the RRW analysis).

**Design and materials**

The apparatus and stimuli were the same as in Experiment 1 with one exception: the options were presented with either an in-group or outgroup qualifier. As all participants were college students attending the University of North Carolina Wilmington, the shared in-group across participants was the school they attend. As such, items in the in-group were identified as “UNC Wilmington alumnus.” Out-group items were described as one of 13 different colleges or universities. UNC Wilmington is ranked #14 in Regional Universities South by the U.S. News & World Report Rankings; the 13 out-groups were schools of comparable academic rankings. Further, “UNC Wilmington” and the 13 out-groups contain a similar number of characters (12 – 15) and therefore were uniform and difficult to distinguish when masked.

Three different types of trials were presented. First, there are Compatible trials, in which the HVO belonged to the donor’s in-group and the LVO belonged to the donor’s out-group. Second, there are the Incompatible trials in which the HVO belonged to the donor’s out-group and the LVO belonged to the donor’s in-group. Finally, there are Neutral trials in which both options belonged to the in-group or both belonged to the out-group.

Participants responded to the same 10 scenarios used in Experiment 1 and chose to donate $10 to one of the two accounts aiding one person. The people and reasons for needing donations varied per trial and scenario, and the colleges which described each item were randomized. As such, both items could be described as an “UNC Wilmington alumnus,” both as an alumnus from one of the thirteen outgroups, or one from each college. An example of a trial in Experiment 2 is:

*Through circumstances out of their control,*

|  |  |  |
| --- | --- | --- |
| *a soldier* |  | *a congressman* |
| UNC Wilmington | *And* | *Simmons College* |
| *Alumnus* |  | *Alumnus* |

*both require physical therapy. A third party has created a GoFundMe account for each person to pay for their physical therapy. You have $10 which you can donate to only one of these GoFundMe accounts.*

*Would you prefer to donate the $10 to help pay for the physical therapy of*

|  |  |  |
| --- | --- | --- |
| *a soldier* |  | *a congressman* |
| UNC Wilmington | *Or* | *Simmons College* |
| *Alumnus* |  | *alumnus?* |

**Procedure**

The procedure of Experiment 3 was identical to that of Experiment 1. Participants completed an average of 134 trials (SD = 12) in the experimental session.

All procedures were approved by the UNC Wilmington IRB, protocol# 16-0210. The data and R code for the analysis of Experiment 3 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>. The stimuli and instructions can be found in the Supplemental Information. The experimental presentation code is proprietary and is not open sourced. The experiment was not pre-registered. In these studies, we report all measures, manipulations, and exclusions.

**Results and Discussion**

Six participants were removed due to issues with computers during the experiment. One participant was removed because they did not understand the task. We filtered the data in the same way as previously described. Again, we removed the 5% participants with the largest CVs (6 participants). The final participant sample comprised 109 individuals. For each O0.1 bin, we removed individual trials with the slowest and fastest 2.5% of the RTs.

As before, we removed the influence of learning from the RT data by fitting the same learning function to the data as before (none of the parameters of this function were statistically significant), and the corresponding RT residuals were used in the following analyses.

**Regression Analysis**

We analyzed the data in Experiment 3 using the same process as before (see Figure 10, left-hand side). There was a significant relation between Overlap and *p*(HVO), (*b* = 1.89), *t* = 14.39, *p* < .001, *r2* = 0.96. There was a statistically significant linear relation between Overlap and RTres, *F*(1, 8) = 279.9, *p* < .001, *r2* = 0.97. Both the intercept (*a* = –0.22) and the slope (*b* = 0.43) were statistically reliable, *t*’s > 14, *p*’s < .001. Both analyses had an 80% power to detect an effect size of *f2* = 1.0.



**Figure 10. Experiment 3 regression and RRW results.** The *p*(HVO) and RT fit using separate regressions (left column). The *p*(HVO) (top), RTHVO (bottom circles), and RTLVO (bottom “x”s) fit simultaneously (right column) using the RRW. The labeling on the *p*(HVO) graph acts as the legend. The thin, light grey lines represent the 40 individual iterations of the RRW with parameters from the best fit model. The thicker, darker lines show the average fit of those 40 lines.

To assess whether the in-group manipulation influenced participants’ response choices, we fit *p*(HVO) = *f*(Overlap) for each of the four conditions: Compatible (UNCHVO vs OtherLVO), Neutral (UNCHVO vs UNCLVO and OtherHVO vs OtherLVO), and Incompatible (OtherHVO vs UNCLVO), PVT predicts that the in-group effect should produce the following ordering of the beta parameter when the exponential decay function is fit: Compatible > Neutral > Incompatible. This is exactly what we found (see Figure 11). In all cases *p*(HVO) was significantly related to Overlap. For the Compatible condition, UNCHVO vs OtherLVO (*b* = 2.9), *t* = 6.0, *p* = .001, *r2* = 0.62. For the two Neutral conditions: OtherHVO vs OtherLVO (*b* = 1.91), *t* = 12.63, *p* < .001, *r2* = 0.94; UNCHVO vs UNCLVO (*b* = 1.98), *t* = 10.38, *p* < .001, *r2* = 0.93. For the Incompatible conditions: OtherHVO vs UNCLVO (*b* = 1.39), *t* = 14.61, *p* < .001, *r2* = 0.97. Although this type of analysis has demonstrated the existence of the in-group effect, it does not identify its source. To identify the source of the in-group effect on charitable giving, we modeled the data using the RRW.



**Figure 11: *p*(HVO) by condition in Experiment 3**. The exponential decay fit for *p*(HVO) for the Compatible condition, Incompatible condition, and the two Neutral conditions. The data show that, in all cases, participants attempted to donate to the HVO. Participants were most effective at this in the Compatible condition (light grey solid line) and least effective in the Incompatible condition (black dashed line). The Neutral conditions (black solid line and grey dashed line) fell in between the other two.

**RRW Analysis**

Experiments 1 and 2 showed that the Value Constant + Side Bias model was the best predictor of the data. The value constant parameter identified a small shift in the pair-wise overlaps of all our stimuli and it is highly likely this shift will generalize to other charity research that uses the current stimuli. Because the parameter is defined as a constant it does not require the data to be parsed into smaller bins. For these reasons, we include it in all the current models.

The start point parameter incorporated in the model was instrumental in assessing the influence of a side bias – the tendency to prefer either the left or right option as presented in the scenario. The effect of the side bias is consistent, but small across the first two datasets. Importantly, the side bias is likely a function of the formatting of our scenarios (for instance, the same bias may not obtain if the options were printed in a column rather than a row). We therefore do not consider it to be central to how charity decisions are made.

Although we do not doubt the presence of the side bias in Experiments 1 and 2, we do not pursue the quantification of it here for several reasons. First, exploring the influence of both a side bias and the in-group manipulation requires the data to be parsed into many more relatively small bins (overlap by side by consistency). The reduced number of observations per bin produce less stable estimates of both response bias and RT. Second, the number of models required to assess both effects would increase multiplicatively, thus further increasing the influence of random effects. Finally, because we randomized the side on which the respective in-group/out-group options were presented, the influence of side bias should be independent of the influence of the in-group manipulation. As a result, eliminating the side bias parameter from consideration does not affect the identification of any effect due to the in-group manipulation. Thus, any putative effects of group membership should generalize, in ways that any putative side bias effects would not.

To identify the source of the in-group effect on charitable giving, we compared the performance of four models:

* The *Value Constant Model* is the same model as Experiment 1.
* The *In-group Start Point Bias Model*, which assessed for a response bias in the data. Such a bias would reflect the fact that participants shifted the start point toward the in-group option (see Figure 9). This model is identical to the *Value Constant* Model, with the exception that it adds a free parameter to capture shifting the start point toward the in-group option. Within the context of the RRW, a bias toward the in-group translates into a positive bias in the Compatible trials and an equal but negative bias in the Incompatible trials. To model this, we effect coded the Compatible trials as 1, the Incompatible trials as -1, and the Neutral trials as a 0. This effect coding is multiplied by the *start point* parameter which then varies the start point as a symmetric positive/negative effect around a zero start point for the trial types.
* The *In-group Value Change Model*, which assessed for a value shift such that the in-group option increased in value by a constant, thus changing the Overlap of the pair. This model is identical to the *Value Constant* Model, with the exception that it adds a free parameter to capture the value shift of the in-group (see Figure 8). Within the context of the RRW, a value increase in the in-group translates into a decrease in the Overlap in the Compatible trials and an equal increase in the overlap in the Incompatible trials. To model this, we effect coded the Compatible trials as 1, the Incompatible trials as -1, and the Neutral trials as a 0. Such an effect will result in a negative parameter value. This effect coding is multiplied by the *value change* parameter.
* The *In-group Start Point Bias + In-group Value Change Model* added both a value change and a start point bias.

In all other ways, we conducted the RRW analysis in Experiment 3 as described previously.

Table 1 presents the *r2* and BIC for each model. All models fit the data quite well, but the In-group Value Change model was the best fit with an *r2* = 0.93 (see Figure 10, right-hand side)[[3]](#endnote-3). The six free parameters in the In-group Value Change model are presented in Table 2. The *Ter, NSD*, *vc*, and *dB* free parameter values are similar to those estimated for the best fit model of Experiment 1. Importantly, the Value Change parameter added in Experiment 3 revealed that the in-group manipulation influenced the value of the probes. Specifically, the in-group manipulation decreased the Overlap by 0.1 when the in-group was the HVO and increased the Overlap by 0.1 when the in-group was the LVO. Because the Value Change parameter simply adjusts the Overlap value, we cannot determine whether, in the data, the effect of the in-group manipulation (i) increases the value of the in-group members (ii) decreases the value of the out-group members, or (iii) simply forces the two groups apart. Nevertheless, we suspect that both influences are at play.

In sum, the results showed that PVT accounted for 96% of variance in response choice and 97% of variance in RT when analyzed using regressions. Moreover, we tested four versions of the random walk process and discovered that the in-group effect was the result of shifting the Psychological Values of the probes rather than a start point bias. Specifically, the In-group Value Change model best fit the data and accounted for 93% of variance in the RTHVO, RTLVO, and *p*(HVO) simultaneously. Further discussion of these results and their implications is included in the General Discussion.

**Experiment 4**

In Experiments 1-3, we have demonstrated that PVT can successfully predict charitable choice based on the perceived Psychological Value of the receiver. The question remains, however, whether the perceived Psychological Value of the receiver drives choice when the giver is offered the option to donate or keep the money for themselves. In Experiment 4, we address this issue by presenting scenarios that request money for a single individual. The participant is given the option to donate to the GoFundMe account or to keep the money. If Psychological Value drives the donation process, then PVT should be able to model both the RT and response choice of the donate decision.

Theoretically, when making the yes/no decision, the respondent sets a threshold. If the Psychological Value of the receiver is perceived to be above the threshold, the respondent would choose to donate. Conversely, if the Psychological Value of the receiver is perceived to be below the threshold, the respondent would choose to keep the money. Similar to the Psychological Value of the option, the threshold is noisy and best described as a distribution. We term this threshold, the Threshold Distribution (abbreviated TD).

In a 2AFC task, the two distributions being compared are identified in the choice. As such, 2AFC tasks markedly constrain the model fits. For tasks that require a yes/no choice, the TD is not objectively identified, and must be inferred from the data. As such, the model fit is less constrained than it is by the 2AFC task. Nevertheless, because a single, consistent TD is assumed to be compared to every Item, the yes/no model remains strictly constrained.

In Experiment 4, we also vary the amount of money the participant is asked to donate: $10, $50, and $100. By varying the amount of money to be donated we will be able to determine how the donation amount influences the decision process. Similar to the in-group manipulation in Experiment 3, the donation amount might influence the position of the start point bias or the placement (i.e., valuation) of the TD. Both options are theoretically viable and might be at play. For example, respondents might have a start point biased toward the “keep the money” option. In this case, the evidence will always start to accumulate closer to the “keep the money” boundary than the “donate” boundary. The degree of bias might be influenced by the amount of money to be donated (a stronger “keep the money” bias for large donations, and a weaker “keep the money” bias for small donations). Alternatively, the respondent may set increasingly more stringent thresholds for donating as the donation amount increases. Here, we would see the value of the TD shift up with the value of the dollar donation amount.

**Methods**

**Participants**

Participants included 146 undergraduate students who received partial credit towards a course requirement. Sample size was determined by the same procedure used in Experiment 1.

**Design and materials**

The apparatus and stimuli were the same as in Experiment 1 with the following exceptions. When presenting a donate/keep choice, the number of different scenarios is equal to the number of items (23) times the number of scenarios. To increase the perceived variability of the presented scenarios we used 9 of the elaborate scenarios of Experiment 1[[4]](#endnote-4). These scenarios were slightly altered to be consistent with a single recipient (see Figure 12). In addition, we added five higher valued Items (people) to further increase the range of Psychological Values of the stimuli list. Those five items are: “your friend,” “your grandfather,” “your mother,” “your cousin,” “your sibling.” Finally, we also added three donation amounts: $10, $50, and $100. We used these amounts because in pilot testing participants reported that they influenced their responses. This resulted in 27 (Items) \* 9 (scenarios) \* 3 (donate amounts) = 729 possible scenarios.

The question at the end of each scenario read, “Would you donate AMOUNT to the GoFundMe account to help pay for REASON of ITEM or keep the money?” Here, AMOUNT was bolded and in yellow text and randomly selected from the dollar donation amounts ($10, $50, and $100). REASON was changed to reflect the GoFundMe request in the scenario. Similar to Experiments 1-3, ITEM was randomly selected from the list of 27 Items.

****

**Figure 12. Example of an unmasked charitable donation scenario used in Experiment 4**. Item A was replaced by a randomly chosen item from the list of 27 Items.

**Procedure**

The procedure of Experiment 4 was identical to that of Experiment 1 with the following exceptions. First, the dollar donation amount was blocked within participant, and the order of blocks was counterbalanced between participants. Each block ran for 15 minutes. Between blocks, a short instruction read, “This portion of the experiment will be identical to the previous portion with one exception: the dollar amount will be changed.” This procedural change enhanced the salience of the dollar donation amount. Second, participants responded using the “l” and “s” keys, which mapped onto “Donate” and “Keep” options. The mapping of the keys to their meaning was counterbalanced between participants. Because of this counterbalancing, we did not use the “d” and “k” keys (they matched the first letters of the response mapping “donate” and “keep”, which might confuse some participants). Participants completed an average of 181 trials (SD = 23) in the experimental session.

All procedures were approved by the UNC Wilmington IRB, protocol# 16-0210. The data and R code for the analysis of Experiment 4 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>. The stimuli and instructions can be found in the Supplemental Information. The experimental presentation code is proprietary and is not open sourced. The experiment was not pre-registered. In these studies, we report all measures, manipulations and exclusions.

**Results and Discussion**

No participants were removed due to issues with computers during the experiment. We filtered the data in the same way as previously described. Again, we removed the 5% participants with the largest CVs (8 participants). One participant was removed automatically because they appeared to reverse the keymapping (i.e., *p*(HVO) = 0.22)[[5]](#endnote-5). The final participant sample comprised 137 individuals. For each O0.1 bin, we removed individual trials with the slowest and fastest 2.5% of the RTs.

As before, we removed the influence of learning from the RT data by fitting the same learning function to the data as before (none of the parameters of this function were statistically significant), and the corresponding RT residuals were used in the following analyses.

**Regression Analysis**

Appendix A described the procedure we used to identify the TD. In the yes/no experiment, Overlap is calculated between every Item and the identified TD. This creates a relatively small set of Overlaps that is concentrated at the upper end of the continuum. As such, calculating regressions on the truncated, collapsed data is relatively uninformative. Nevertheless, for consistency, we do report those regressions. There was a significant relation between Overlap and *p*(HVO), (*b* = 4.22), *t* = 3.18, *p* = .049, *r2* = 0.82. There was a statistically significant linear relation between Overlap and RTres, *F*(1, 2) = 109, *p* < .001, *r2* = 0.98. Both the intercept (*a* = –0.55) and the slope (*b* = 0.68) were statistically reliable, *t*’s > 10, *p*’s < .001.

We ran an item analysis to estimate the probability of choosing to “keep the money” rather than donate to each individual item (see Figure 13). Recall that the overlap of the items is plotted on the *x*-axis, whereby the left-most item is the item that has the least amount of distributional overlap and is more valued than the TD. The right most item has the least amount of distributional overlap and is less valued than the TD. Therefore, the likelihood of keeping the money should be least at the left bound, most at the right bound, and gradually increasing in between. The data conform to this predicted pattern.



Figure 13: **Item Analyses**. The probability of choosing to keep the money as a function of the Psychological Value of the receiver. The x-axis are the Items arranged ordinally from highest Psychological Value (left) to lowest Psychological Value (right).

**RRW Analysis**

To identify the source of the in-group effect on charitable giving, we compared the performance of six models:

* The *Value Constant Model* is the same model as Experiment 1 tailored to the yes/no experiment. Specifically, to identify whether a value constant is required, the data was coded to identify whether the Item was the HVO or LVO relative to the TD, termed the ItemHVO/ItemLVO variable. If the Item was the HVO (and therefore the TD is the LVO), it was assigned a 1, otherwise it was assigned a ‑1. If there is no correction, then responses to the ItemHVO trials will be identical to the ItemLVO trials. If ItemHVO trials are slower and less accurate than ItemLVO, then the TD will be shifted up (the value constant parameter will be negative), and vice versa.
* The *Start Point Bias Model*, which assessed for an overall start point bias in the data. Such a bias would reflect the fact that participants shifted the start point, generally, toward the donate or keep boundary. This model is identical to the *Value Constant* Model, with the exception that it adds a free parameter to capture shifting the start point toward one of the two boundaries.
* The *Dollar Start Point Bias Model*, which assessed for a response bias in the data. Such a bias would reflect the fact that participants shifted the start point toward one or the other boundary separately for each dollar condition. This model is identical to the *Start Point Bias Model*, with the exception that it adds two free parameters to capture any additional shifts in the start points for the $10 and $100 dollar donated amounts. To model this, we created two dummy coded variables: one to code for start point bias shift for the $10 amount (1 if dollar amount = $10, otherwise 0), and an additional start point bias shift for the $100 amount (1 if dollar amount = $100, otherwise 0). As such, the overall start point bias will quantify the start point shift of the $50 amount. The two additional start point parameters will modify the overall start point shift for their respective trial types.
* The *Dollar* *Value Change Model*, which assessed for a value shift such that the donation amounts changed the value of the Item, thus changing the Overlap of the pair. This model is identical to the *Value Constant* Model, with the exception that it adds two free parameters to capture value shifts for the $10 and $100 dollar donated amounts. The same dummy codes used for the Dollar Start Point Bias model were used for this model. However, they were applied to the value parameter, rather than the start point parameter.
* The *Dollar Start Point Bias + Dollar Value Change Model*, which adds both an overall start point parameter and the two value change parameters.
* The *In-group Start Point Bias + In-group Value Change Model* added both a in-group value change parameters and the in-group start point parameters.

Similar to Experiments 1-3, we collapsed across participants to get stable estimates of RT and *p*(HVO) per Overlap x Dollar Donation Amount x ItemHVO/ItemLVO x Correct/Incorrect combination. PVT holds that a correct response in the yes/no experiment occurs when the respondent chooses to donate when the Item is the HVO and keeps the money when the Item is the LVO. In all other ways, we conducted the RRW analysis in Experiment 4 as described previously.



Figure 14: The behavioral data and model fit for Experiment 4. The dots represent the data, and the lines represent the model fit. The top graphs show the performance of the full model, which simultaneously fit all three DVs (p(HVO), RTcorrect, RTincorrect) to each Dollar Donation Amount ($10, $50, $100) x ItemHVO/ItemLVO (HVO or LVO). The labeling on the *p*(HVO) graph acts as the legend. The thin, light grey lines represent the 40 individual iterations of the RRW with parameters from the best fit model. The thicker, darker lines show the average fit of those 40 lines. For ease of viewing the model fits, the bottom three figures break out the data and model fits by Dollar Donation Amount. The black symbols and lines represent the ItemLVO trials, and the grey symbols and lines represent the ItemHVO trials. In the RT graphs, the circles represent correct responses and the “x”s represent incorrect responses.

Table 1 presents the *r2* and BIC for each model. All models fit the data quite well, but the Dollar Value Change model was the best fit with an *r2* = 0.86 (see Figure 14). The seven free parameters in the Dollar Value Change model are presented in Table 3. The value change parameters reveal that all dollar donation amounts resulted in an upward shift of the TD. The $10 donation amount produced a small upward shift of about -0.03 (‑0.1 + 0.07 = ‑0.03). The $50 donation amount produced a larger upward shift of about -0.1 (the overall value change parameter). Finally, the $100 donation amount produced the largest upward shift of about -0.14 (‑0.1 + -0.04 = ‑0.14). This systematic increase in the value of the TD is predicted when the value of the donation amount increases.

Table 3: The parameters and *r2* for the best fitting RRW model in Experiments 4.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Ter | NSD | Boundary | dB | s | vc overall | vc $10 | vc $100 | *r2* |
| -0.85 | 1.25 | 135 | 0.02 | NA | -0.1 | 0.07 | -0.04 | 0.86 |

In sum, the results showed that the Value Change model best fit the data and accounted for 86% of variance in the RTCorrect, RTIncorrect, and *p*(HVO) simultaneously. The identified changes in the value of the TD are consistent with those predicted. Further discussion of these results and their implications is included in the General Discussion.

**Experiment 5**

A previous version of this report drew to a close with Experiment 4. However, following review, we were asked whether *competence* and *warmth* can explain the results as well as Psychological Value. Competence and warmth are two theoretical constructs that have been shown to relate to both judgments of peoples and behaviors towards them (e.g., Cuddy, Fiske, & Glick, 2007; Fiske, 2015; Fiske, Cuddy, & Glick, 2007; Fiske, Cuddy, Glick, & Xu, 2002; Judd, James-Hawkins, Yzerbyt, & Kashima, 2005). In general terms, warmth refers to the perceived intent of another (e.g., friendliness, anger, etc.). Competence refers to the perceived ability of that person to carry out the intended action (e.g., intelligence, planning, etc.).

There are few research articles that relate competence and warmth to performance in the kinds of charitable choice tasks such as ours. Recently, however, Jenkins Karashchuk, Zhu and Hsu (2018) examined the relation between competence and warmth and resource allocation in the Dictator Game (Andreoni, Harbaugh, & Verterlund, 2010). In simple terms, the authors found that the competence and warmth of the receiver boosts (or discounts) the value of the dollar amount given (termed the *Social Perception Weighted* model). Assuming that competence and warmth are two of the many features that contribute to Psychological Value, the results of Jenkins et al. (2018) are consistent with the findings of Experiment 4. Recall, we asked participants whether they would donate the whole of a fixed amount of cash (i.e., $10, $50, or $100) to various receivers. Our results showed that each amount donated shifted the Psychological Value of all the receivers by a constant amount (the Dollar Value Change model). As such, both Jenkins et al. (2018) and our Experiment 4 showed an interaction between the receiver and the dollar amount.

In Experiment 5, therefore, we compared the predictive ability of PVT in Experiments 1, 2, and 4 to an account based on competence and warmth. We exclude Experiment 3 because it’s identical to Experiment 1 with an additional in-group vs. out-group manipulation. Rather than being a quantified model of choice, competence and warmth are two theoretical constructs that are hypothesized to relate to various behaviors. As such, the predictions derived from these constructs are poorly constrained and, at best, directional (rather than point). Therefore, we derived the competence and warmth predictor variable from Experiment 1 (following closely the methods of Jenkins et al., 2018) and used that variable to predict performance in Experiments 2 and 4. We then compared the fit statistics to that of PVT.

Because predictions derived from a competence and warmth account are less constrained than PVTs, PVT is at a predictive disadvantage. Therefore, if PVT predicts charitable choice better than competence and warmth, it will be a convincing demonstration of PVTs theoretical superiority over that offered by competence and warmth. The converse, however, is not necessarily true. That is, superior performance by competence and warmth may simply be a reflection of how easily a less-well specified theory can accommodate patterns of responding relative to a more precisely specified one.

**Method**

**Participants**

Participants included 206 adults recruited through Prolific who received payment for participation. The procedure was a partial replication of the competence and warmth ratings survey described by Jenkins et al. (2018). Jenkins et al. (2018) collected about 50 rating responses per item. In our version, that translates to 150 participants (50 participants \* 3 (27 items/9 at a time) = 150). We attempted to recruit 200 participants, assuming that not all will complete the task with full attention.

**Design and materials**

Participants were asked to rate the 27 items used in Experiment 4 on the 31 attributes used by Jenkins et al. (2018), plus omnibus ratings of competence and warmth. The 27 items are: a celebrity, a congressman, a convict, a father, a gang member, a judge, a mentor, a mother, a nun, a pedophile, a police officer, a rapist, a soldier, a teacher, a terrorist, a thief, an addict, an adult, an assassin, an astronaut, an elderly person, an Olympian, your friend, your grandfather, your mother, your cousin, and your sibling.

We created a survey in Qualtrics to collect ratings data. Each page of the survey contained one of two questions. The question associated with the traits was, “In your opinion, how [trait] is the person below”, where [trait] was replaced by one of the following traits: sincere, tolerant, good-natured, trustworthy, friendly, helpful, moral, understanding, intelligent, efficient, skillful, confident, creative, capable, foresighted, clever, competent, and warm. The question associated with the attributes was, “In your opinion, how capable of each person below of experiencing [attribute]”, where [attribute] was replaced by one of the following attributes: hunger, fear, pain, rage, desire, pleasure, pride, embarrassment, joy, communication, knowing others’ feelings, memory, telling right from wrong, planning, and self-control. Below each of the questions, a random selection of nine of the 27 items were presented. To the right of each item name was a slider that ranged from “Not at all” to “Extremely.”

Finally, we included an attention check. The attention check looked identical to the trait question, and read, “In your opinion, how sadistic is the person below,” with nine sliders below the question. Seven of the nine sliders were item names (your friend, an Olympian, an astronaut, an adult, a teacher, a nun, a mother). These items were chosen because the typical response would generally be low on the scale. The two remaining sliders included instructions that told the participant where to place the slider, either, “respond extremely” or “center this slider.” The remaining seven ‘dummy’ items were to mask the attentional items. Finally, the responses to this trait “sadistic” was, of course, not used in the experiment.

**Procedure**

We mimicked the procedure of Jenkins et al. (2018), with slight modification. Each participant clicked on the link in Prolific and were directed to the Qualtrics survey. They were presented with a consent page, followed by a set of demographic questions (age and gender). Then they were presented with brief instructions, followed by 34 pages each containing 9 slider responses. The order of all questions was randomized between participants and the order of the items within a slider was randomized for every participant x question. When the participants completed the experiment, they were re-directed back to Prolific. The structure of the scale, question, etc., are as described in Jenkins et al. (2018).

All procedures were approved by the Ethics Committee of the Department of Psychology, University of York IRB, #2242. The data and R code for the analysis of Experiment 5 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>. The stimuli and instructions can be found in the Supplemental Information. The experimental presentation code is proprietary and is not open sourced. The experiment was pre-registered, and can be found at: https://doi.org/10.17605/OSF.IO/XREQT. In these studies, we report all measures, manipulations and exclusions.

**Results and Discussion**

First, we filtered participants who failed both attention checks. Because the attention checks were sliders, we identified a failure as a response that was not within 5% of the requested response. Ten participants were removed because they failed both attention checks (with 196 participants remaining).

We used the factor loadings described by Jenkins et al (2018) to derive a measure of competence (*C*) and a measure of warmth (*W*; see Supplemental Information). To do so, for each feature, we calculated the mean rating and then multiplied the mean rating by the factor loading. Finally, we summed the factor loadings. The C and W scores were normalized with a mean = 0 and an SD = 1.



Figure 15: The warmth and competence scores for each of the 27 items used in Experiments 1-4.

Figure 15 presents the competence and warmth values for each item. Both the derived competence construct and the derived warmth construct correlated highly with their omnibus ratings (competence: *r* = 0.7, p < .001; warmth: *r* = 0.98, p < .001).

We derived the combined competence/warmth (*CW*) measure as follows:

, (2)

where *Wi* and *Ci* are the derived Warmth and Competence value for item i, respectively; *aw* and *ac* are constant terms for warmth and competence respectively; *bw* and *bc* are slopes terms for warmth and competence respectively; *bcw* is the slope for the interactive term.

The difference in the CW score between any two items should predict charitable choice between two items:

(3)

where is the probability of choosing item *i* given a choice between items *i* and *j*. When Equation 3 is expanded and simplified, it is possible to maximize the fit of CW to using the following equation:

(4)

To derive the best predicting CW measure from the data, we fit Equation 4 to the choice data of Experiment 1. The regression was significant, *F*(3, 227) = 198.2, *p* < 0.001. Only the warmth parameter (*bw*; *t* = 22.9, p < 0.001) and the interaction term (*bcw*; *t* = -2.1, *p* < 0.03) were statistically reliable. We therefore re-ran the regression, dropping the competence predictor (*bc*). The final regression was significant, *F*(2, 228) = 297.1, *p* < 0.001. Both the warmth parameter (*bw* = 0.24; *t* = 24.3, *p* < 0.001) and the interaction term (*bcw*= -0.03; *t* = -3.6, *p* < 0.001) were significant. Fixing the parameter values *bw* and *bcw*, we computed the final, non-directional CW predictor (*CWij*) for all the pairs of stimuli (see Supplementary Information).

Because *CWij* was derived from the data in Experiment 1, it is advantaged when predicting Experiment 1’s data over PVT: the estimates of Psychological Value were independently derived of the current datasets as described in Cohen and Ahn (2016). Therefore, to determine how well *CW* predicted choice, we assessed how well *CWij* predicts charitable choice in Experiment 2 (nevertheless, for informational purposes, we conducted and report parallel analyses on the data in Experiment 1). To do so, we first calculated the probability of choosing the “correct” response as identified by *CWij*, *p*(rCW). We then predicted *p*(rCW) and RT using the following preregistered regressions for choice,

(5)

and RT,

(6)

However, on examining the data, it was clear that the relation between *p*(rCW) was best captured by a power function,

(7)

Statistical analyses confirmed that the power function outperformed the linear function (see Supplemental Information). To maximize *CWij*’s likelihood of predicting choice, we used Equation 7 to predict the choice data for all subsequent analyses.

Because CW has no choice model comparable to PVT’s RRW, we compare the CW regression fits to the individual PVT preregistered regressions for choice,

(8)

and RT,

(9)

Equation 8 is a two-parameter variant of the typical PVT choice regression equation. The second parameter, *a*, accounts for the value constant correction identified in Experiment 1 and equates the number of parameters in the PVT and CW equations. We compare the model fits using BIC as our primary measure and *r2* as the secondary measure.

Table 4 presents the BIC (smaller is better) and *r2* (larger is better) statistics for the regression fits for entire dataset in Experiments 1 and 2. As is evident in the table, PVT provides a superior fit relative to CW for both choice and RT. We also ran the same regressions for every item (e.g., a mother, a father, etc.). We then calculated paired *t*-tests to determine whether the fit statistic was significantly different for PVT vs CW. Table 5 presents these data for Experiments 1 and 2. As is evident in the table, PVT provides a superior fit relative to CW for both choice and RT, with a large effect size (Cohen’s *D*) in every test. Figure 16 presents boxplots of these fit statistics for Experiment 2 (see Supplementary Information for a similar figure for Experiment 1 and plots of the fits to each item).

Table 4: Fit statistics for the PVT and CW regressions predicting choice and RT for the entire dataset in Experiments 1, 2, and 4.

|  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  | Choice | | | | RT | | | |  |
|  | PVT | | CW | | PVT | | CW | | Superior  Model |
|  | BIC | *r2* | BIC | *r2* | BIC | *r2* | BIC | *r2* |
| Experiment 1 | -327 | 0.52 | -100 | 0.40 | -325 | 0.61 | -285 | 0.53 | PVT |
| Experiment 2 | -470 | 0.71 | -196 | 0.49 | -473 | 0.49 | -449 | 0.44 | PVT |
| Experiment 4 | -44 | 0.93 | -15 | 0.83 | - | - | - | - | PVT |

Table 5: Mean (sd) fit statistics and *t*-tests for the PVT and CW regressions predicting choice and RT by item for Experiments 1 and 2.

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  |  |  |  |  |  |  | Superior  Model |
|  | PVT | CW | *t* | df | *p* | Cohen’s D |
| **Experiment 1** |  |  |  |  |  |  |  |
| Choice |  |  |  |  |  |  |  |
| BIC | -30 (15) | -12 (16) | 6.12 | 21 | < 0.001 | 1.33 | PVT |
| r2 | 0.58 (0.19) | 0.45 (0.18) | 3.71 | 21 | < 0.001 | 0.81 | PVT |
| Reaction Time |  |  |  |  |  |  |  |
| BIC | -38 (11) | -26 (11) | 5.42 | 21 | < 0.001 | 1.18 | PVT |
| r2 | 0.73 (0.2) | 0.57 (0.2) | 4.86 | 21 | < 0.001 | 1.06 | PVT |
|  |  |  |  |  |  |  |  |
| **Experiment 2** |  |  |  |  |  |  |  |
| Choice |  |  |  |  |  |  |  |
| BIC | -46 (16) | -19 (14) | 8.69 | 21 | < 0.001 | 1.89 | PVT |
| r2 | 0.76 (0.15) | 0.54 (0.15) | 5.25 | 21 | < 0.001 | 1.15 | PVT |
| Reaction Time |  |  |  |  |  |  | PVT |
| BIC | -57 (10) | -45 (9) | 6.45 | 21 | < 0.001 | 1.41 | PVT |
| r2 | 0.68 (0.2) | 0.50 (0.22) | 7.47 | 21 | < 0.001 | 1.63 | PVT |



Figure 16: Boxplots of the BIC (lower is better) and r2 (higher is better) by item for Experiment 2. The data reveal that PVT predicts charitable choice more accurately than competence and warmth.

We also assessed how well PVT and CW fit the data in Experiment 4. Because Experiment 4 is a yes/no experiment, rather than choosing between two options, we derived different predictions from CW (recall, there is no stated choice model for CW). RT is a function of the identified threshold, and CW does not have an associated choice model. Therefore, we did not assess the relation between CW and RT. Nevertheless, we could derive a prediction for choice. Specifically, the predicted probability of choosing to donate should be a function of the item’s CW value*, CWi*. This relation should take the form of a logistic function,

, (10)

Where *m* is the minimum, *b* is the slope, and *s* is the scale.

To do the same analysis on PVT, we converted overlap with the reference threshold (identified in Experiment 4) to a distance measure (termed directional overlap, DO) using the following formula,

(11)

where overlapi is Item *i*’s overlap with the reference threshold (identified in Experiment 4) and directioni = -1 if Item *i*’s distribution is lower than the reference distribution, otherwise directioni = 1. DO ranges from -1 (no overlap, with Item I below the reference distribution) to 1 (no overlap, with Item i above the reference distribution). The two distributions overlap completely when DO = 0. We then fit the same function (Equation 10) using DO as the predictor variable and fixed the scale to 1.

Table 4 shows the fit statistics comparing the fit statistics of PVT to CW for Experiment 4. As can be seen, PVT outperforms CW. Figure 17 shows the fits. Similar results were found when the analysis was calculated for each donation amount ($10, $50, $100; see Supplemental Information).



Figure 17: PVT and CW predictions for the probability of donating in Experiment 4. PVT predicts more accurately than CW.

In sum, the results showed that PVT consistently outperformed competence and warmth as a predictor of charitable giving. The superiority of PVT over competence and warmth is especially impressive given the predictions based on a competence and warmth account is far less constrained than the precise point predictions of PVT. Further discussion of these results and their implications is included in the General Discussion.

**General Discussion**

Charitable giving rests on a complex social decision in which the benefits to the giver may not be immediately apparent. To explain charitable giving, we propose that such donations are driven by an assessment of the Psychological Value of the recipient as gauged by the giver. To examine this hypothesis, we adopted the conceptual framework of PVT. The theory rests on the assumption that Psychological Value is a perceptual event, and given that such events are susceptible to noise, Psychological Value is best captured by a distribution rather than a single estimate. Consistent with SDT, when a decision rests on assessing the relative values of two alternatives, the ease and nature of such decisions are gauged relative to the amount of overlap of the distributions of the Psychological Values of the options. Within the context of PVT, the amount of this overlap should predict both the probability of choosing the higher valued option, *p*(HVO), and RT in value-based tasks. Evidence in favor of the PVT account, in general, and our detailed predictions, in particular, is present in the data of all five of the experiments reported here.

In Experiment 1, we assessed performance when, on each trial, the participants were forced to choose to give to one of two options when the scenario described the predicament the options were placed in. We modeled this donation decision using a sequential sampling procedure (i.e., the RRW) developed to instantiate the assumption of PVT. We used the distributional overlap of Psychological Value of the options as a direct estimate of drift rate. Several model variants were compared (see Table 1) and the best fitting accounted for about 90% of the variance simultaneously in *p*(HVO) RTLVO, and RTHVO. The best fitting model – the Value Constant + Side Bias model – contained six free parameters. The *Ter*: parameter accounts for all internal processes that are separate from the decision such as basic input (e.g., encoding) and output (motoric response) processes and defined the distance between the two decision boundaries. The boundary parameter identified half the distance between the boundaries. A third parameter, *NSD*, defined the amount of noise added to the samples taken on each step. Sequential dependencies across trials were modelled by *dB*, the side bias was modelled by *s* and, finally, *vc* altered the separation of the pair-wise overlap of the option distributions by a constant. Minor variants on this general model were then able to account for performance in Experiments 2 and 3.

In Experiment 2 the same general framework was applied, but critically, now the scenarios did not provide any background information as to why the options had been placed in a vulnerable predicament, nor the specific cause of their vulnerability (e.g., their house had burned down). The Value Constant + Side Bias model again best fit with the data. That is, the same model was able to account for decision making regardless of whether or not additional contextual information was contained in the scenarios. Additionally, the model provided convincing fits to data averaged over participants and to individual participants datasets, respectively.

Experiment 3 solidifies our main conclusion. In Experiment 3 we demonstrated that PVT’s framework provides a firm, quantified mechanism for assessing the explanatory influence of contextual features on charitable giving. Specifically, we assessed the nature of in-group designations on charitable giving behavior with PVT’s decision model. Now the same sort of scenarios used in Experiment 1 were re-used but in addition the in-group/out-group allegiance of each of the options was also provided. Again, several variants of the basic model were compared in a bid to see whether the influence of group allegiance could be best accounted for by a change in the Psychological Value attributed to the groups, a response bias towards the in-group or a combination of a change in value and an in-group response bias. Again, the data were well accounted for by PVT and the evidence provided clear support for the idea that in-group favoritism comes about because the difference in value between in-group members and out-group members becomes more pronounced. On these grounds, the notion of an in-group bias may, more generally, reflect less a decisional bias and more a change in the appraisal of the actual psychological values assigned to the respective in-group vs out-group members. (By means of clarification here we are using the term ‘bias’ to refer a very particular construct in the random walk model and this should not be confused with the notion of a cognitive bias qua a deviation from rational normative thought.)

In Experiment 4 we presented participants with single charities, one at a time, and asked them whether they would donate or keep the money. In addition, we varied the dollar donation amount. PVT proposes that the decision rests on a comparison of the Psychological Value distribution of the charity to that of a TD, above which they donate and below which they keep the money. The Dollar Value Change model fit the data well, with the TD getting increasingly more stringent with the increasing donation dollar amount. This experiment demonstrated that the Psychological Value of the receiver drives charitable giving behavior in general as well the choice of which charity to donate to.

Finally, in Experiment 5 we compared PVT’s fits to that of the predictions derived from a formal account of competence and warmth. Competence and warmth are the primary predictor variables in the Stereotype Content Model (e.g., Fiske, et al., 2002; Cuddy, et al., 2007). As expected, the competence and warmth account does provide relatively accurate predictors of charitable giving. Nevertheless, PVT’s predictions are far more accurate (as evidenced by the large effect sizes when comparing model fit statistics). Although we suspect that competence and warmth are two perceptual features that contribute to the perceived Psychological Value of people, PVT’s superior performance revealed in Experiment 5 demonstrates that Psychological Value does not simply reduce to competence and warmth. Psychological Value is a more general construct than competence and warmth. Whereas PVT holds that people can perceive the Psychological Value of any stimulus (e.g., objects, animals, people, concepts, etc., see Cohen & Ahn, 2016), competence and warmth apply to only a small subset of possible stimuli (e.g., people). As a result, PVT will accurately predict your preference when given a choice between two inanimate objects (e.g., a car vs an umbrella) based on Psychological Value of the options. In contrast, it is unclear how one would predict preference for inanimate objects using competence and warmth. Thus, PVT is a more precisely specified model that applies more generally than competence and warmth.

**PVT**

In five experiments, we have provided strong evidence that PVT is a validated computational cognitive model of charitable giving, with the Psychological Value of the receiver serving as the primary driver of the decision process. PVT’s predictions are both precise and accurate. Rather than simply identify a factor that influences charitable giving, PVT is a fully specified computational framework for social and cognitive processes involved in charitable giving. As such, it can serve as a useful and informative computational foundation for the study of charitable giving.

One criterion that determines the empirical content of a theory is the precision of its predictions (Glöckner & Betsch, 2011). Degree of precision is a function of how many alternative patterns of data the model forbids. PVT’s makes point predictions based on the overlap of directly measured Psychological Value distributions together with a computationally specified decision process. The fact that the model makes point predictions, rather than directional predictions, greatly restricts the alternative patterns of data that the model can accommodate. Furthermore, these point predictions are made simultaneously for *p*(HVO), RTHVO, and RTLVO. Because the model links these three variables, they must change together in very specific ways. This linkage further restricts the number of alternative data patterns that the model will accommodate. Despite the high precision of the model, PVT predicts with very high accuracy the considerable amount of empirical data described both here and elsewhere (Cohen & Ahn, 2016, Cohen et al., 2022).

We are mindful that, on the surface, PVT’s predictions may appear to be both trivial and obvious. For example, many readers are likely to be unsurprised to learn that people will choose to donate to a mother over a terrorist. However, when examined carefully, the precision of these findings becomes apparent. That is, PVT predicts the "less than obvious" choices (e.g., nun vs. judge; terrorist vs. pedophile) *just as accurately* as it predicts the obvious choices. Perhaps most importantly, PVT does not simply predict “who” will be chosen more often, it predicts the *precise probability* that a participant will choose person A over person B, the time it will take the participant to choose person A (RTHVO), *and* the time it will take the participant to choose person B (RTLVO). PVT does this for *all the possible combinations of people in the stimuli set*. When taken as a whole, such predictions are neither trivial, nor obvious. The high accuracy of PVT’s predictions could only have been achieved if the psychometric properties of Psychological Value are well specified, accurate, and (because predictions were derived from distributional overlap) contain meaningful parametric information. This speaks to the validity of the theoretical construct (Psychological Value) specifically, and the model as a whole (PVT).

In science, it is, without doubt, useful to be able to compare the predictions of two competing models. Unfortunately, there does not currently exist an alternative quantified model of charitable giving that is comparable in scope and specificity to PVT. Nevertheless, we did compare PVTs predictive ability to that of a formal account based on competence and warmth. Even though the account based on competence and warmth is less constrained than that of PVT, PVT proved to be the superior model. We therefore suggest that future research that explores the role of competence and warmth also assess that of Psychological Value.

A second criterion that determines the empirical content of a theory is the universality of its predictions (Glöckner & Betsch, 2011). A theory has high universality if it can be applied to many, diverse situations. PVT proports to have a high universality because it claims to be applicable to all preferential choices. Despite these claims, to date PVT has only been applied to sacrificial moral dilemmas. Here, we broaden the applied decision types to charitable giving. As part of that application, we test a fundamental prediction of the theory: that the Psychological Value of the *stimuli* drive the choice, rather than the consequences *per se* (as is predicted by economic theory). Specifically, we used the same Psychological Values that predicted choice in sacrificial moral dilemmas as the input in the RRW to predict choice in charitable giving. Critically, the consequences for making a $10 charitable donation are much lower than the consequences in a sacrificial moral dilemma in which the respondent makes a life and death decision. Nevertheless, the same Psychological Value of the stimuli predicted both types of choices.

To date, PVT has successfully predicted moral judgments, charitable giving, and economic decisions. The accuracy with which PVT predicts responses and RTs to these vastly different types of decision classes provides some evidence for its universality. To firmly demonstrate universality, however, PVT must be applied to a wider variety of decision types, such as health decisions, dating decisions, delayed discounting, etc.

**Charitable Giving**

In basic economic terms, our results demonstrate that *people are ends in themselves*. The understanding that people can serve as financial ends provides an objective explanatory mechanism for charitable donating (and gift-giving, in general), where previously no objective mechanism existed (see Khalil, 2004). As described in the introduction, many have hypothesized that acts of giving are the result of a variety of altruistic-like reasons. Many of these altruistic-like reasons (and, of course, true altruism) cannot explain the discerning nature of the giving process (e.g., why give to Charity A rather than Charity B, and why designate the end use of your monetary donation?), or why the giver doesn’t rely on a free ride. Here, we demonstrate that the Psychological Value of the recipient to the giver is the primary driver of the donation process. It explains charitable choice and charitable giving. Furthermore, it explains why there is not free ride – because the recipient is the end in themselves. So, one is not attempting to gain the consequences of the donation, rather one is donating *because* one values the recipient. Furthermore, Experiment 4 revealed that the amount donated is a function of the Psychological Value of the receiver. That is, the giver will be more likely to donate more to those they value more.

Because Psychological Value is a private construct, it can only be directly experienced by the perceiver. As such, some might classify it as a form of impure altruism. Although such labeling does not determine the cognitive process, we hold that Psychological Value of the receiver is a rational, non-altruistic decision. PVT holds that Psychological Values drive choice – whether that choice is to buy a car or to donate to charity. That is, the primary benefit of purchasing a car is not the car itself, it is a function of a constellation of often psychological features (e.g., convenience and comfort in travel). All these factors contribute to the Psychological Value of the car in one way or another. Similarly, the primary benefit of giving to charity is not the giving itself, it is also a function of a constellation of often psychological features (e.g., the welfare of valued individuals/groups and the social rewards they bring). Because the cognitive process is the same in consumer and charitable choices, then it is somewhat arbitrary to classify one as economic and the other as a form of altruism. In our view, they are both preferential choices explained by the same underlying mechanisms.

In Experiment 3, we were able to demonstrate how manipulations of interest in charitable giving can be studied precisely within the framework of PVT. To do so, we studied the mechanism by which in-group specifications influence charitable giving. The data showed that in-group specification changed the perceived Psychological Value of the stimulus, rather than inducing a response bias. The implications of this result are subtle, but important. That is, a start point response bias does not influence the phenomenological experience of the stimulus, is generally under cognitive control, and is relatively easy to manipulate in the experimental setting (Macmillan & Creelman, 2005, Chapt. 2). If in-group favoritism was the result of a start point response bias, that would imply that such a bias is context dependent, easily manipulated, and (perhaps most intriguing) can be induced in the opposite direction (i.e., producing an in-group prejudice). Our findings, however, show that in-group favoritism is the result of a value shift. That is, people perceive the value of the in-group as higher than the out-group. If, as PVT theorizes, value is perceived, then the value of the in-group should be relatively robust over context, difficult to manipulate, and unlikely to be shifted in the opposite direction.

Our model explained in-group favoritism as a shift in Psychological Value, but it does not specify whether the perceived Psychological Value of the in-group increases, the perceived Psychological Value of the out-group decreases, or a combination of both. Of some relevance in this regard are studies that have been carried out on how minimal in-group attachment affects attitudes regarding group membership (Brewer, 1979). Unfortunately, the evidence appears mixed on this issue. Some studies have shown that attitudes towards the out-group suffer (Rabbie et al., 1974), others have shown both an enhancement towards the in-group together with a diminution towards the out-group (Hensley & Duval, 1976). In addition, there is other evidence to suggest that the effects merely reflect an enhancement towards the in-group (Stephenson et al., 1976). Here all we can conclude is that effects of group membership in charitable giving arise because of a heightened difference in the psychological values attributed to in-group and out-group members.

Our previous research on the psychophysics of Psychological Value provides some evidence concerning the mechanism of action that might have influenced the value of the in- and out-groups. Specifically, Cromley and Cohen (2019) showed that people perceive the value of a group as the average of the individual items with some bias towards the maximum item. It may be that the increased value of in-group membership is the result of this pre-attentive and automatic function that perceives the value of a group of items. For example, in the present Experiment 3, university alumni status was used to assign in- and out-group membership. In this case, the value of the university would be automatically “merged” with the value of the alumnus, thus producing a group value equal to the biased average identified by Cromley and Cohen (2019). Because people value their university more than an unknown university, participants’ perceived Psychological Value of the in-group status of the alumnus would be higher than the out-group alumnus. This is exactly what we found. Future research can explore whether this is the mechanism of action for in-group valuation (because PVT makes explicit point predictions assuming the biased average model, Cromley & Cohen, 2019).

Experiment 3 demonstrates how PVT provides a framework for understanding how individual factors influence charitable giving. Specifically, in-group specification influences the perceived Psychological Value of the recipient. Other factors that influence charitable giving identified in the psychological literature likely act in a similar vein (e.g., friends, identified victims, etc.). By revealing that seemingly disparate factors influence charitable giving through the same mechanism (e.g., changing the perceived Psychological Value of the recipient), PVT unifies several lines of research.

PVT can also accommodate findings that do not relate to the qualities of the recipient. For example, over the recent past, debate in the literature has emerged over the locus of prosocial behavior (e.g., one’s general tendency to give regardless of the receiver). Yamagashi et al. (2017) suggests that prosocial behavior is generally intuitive (rather than deliberative). Rand et al. (2014) also suggest that prosocial behavior is a quick, non-deliberative process, but *only* for those who live in “environments that favour cooperation, and … have little prior experience with laboratory experiments” (p. 2). These dual process approaches are countered by Chen and Krajbich (2018) who provide evidence that a single process can account for conflicting prosocial tendencies. Chen and Krajbich (2018) used a traditional SSP approach to describe the data from a dictator’s game and showed that a simple start point bias toward giving can be readily explain the prosocial RT results in the literature. Like Chen and Krajbich (2018), PVT can also account for the tendency toward giving (e.g., prosocial behavior) as a start point bias. However, because PVT specifies the primary controlling variable (Psychological Value), it provides a more detailed explanatory account of the data. In sum, by serving as a unifying framework, PVT advances the scientific study of charitable giving.

**Does Psychological Value = Competence and Warmth?**

In Experiment 5, we compared the ability of PVT to predict choice to that of a model based on the theoretical constructs of competence and warmth. The data revealed that although the model based on competence and warmth predicted charitable giving well, PVT predicted charitable giving better than competence and warmth. To some, the fact that competence and warmth predicted charity may well suggest that Psychological Value is simply an aggregate of competence, warmth, and, perhaps, some other unidentified theoretical construct (in the same way that concrete is a simple aggregate of cement, sand, and rocks). Whereas we agree that competence and warmth likely contribute to the Psychological Value of people, we do not accept that Psychological Value is a simple aggregate of different features of an item. Rather, we claim that Psychological Value is an emergent property that *arises from* the individual features of the item. This emergent property does not depend on any specific features being present and is fundamentally different than the features themselves. For example, emotions are emergent properties of neurons. Yet, (1) no single neuron is sufficient to generate an emotion, (2) no specific neuron is necessary for emotions, and (3) emotions are fundamentally different from neurons. Emergent properties are common in biological entities (e.g., Carmichael, 2016).

As stated in the introduction, the Psychological Value of an item is a function of its’ features (see Equation 1). Nevertheless, we did not intend that a reductionist approach can be used to describe Psychological Value. We therefore clarify the equation below,

(12)

Equation 12 should be read as conveying that Psychological Value is a function of cognitive and perceptual factors but is not simply reduced to those factors.

Critically, Psychological Value is a general construct that applies to all classes of items (e.g., objects, animals, people, concepts, etc., see Cohen & Ahn, 2016). As a result, when presented with a choice between two options, one may not be able to compare the magnitude of specific features across options. For example, imagine a choice between saving the Mona Lisa from destruction or saving a thief from certain death. Because the features of the Mona Lisa (e.g., artistic qualities, historical importance, etc.) are fundamentally different from the features of a thief (e.g., competence, warmth, etc.), one cannot directly compare the magnitude of individual features *across* the options when making a choice. This give rise to an apparent conundrum, namely, “When presented a choice, what do people compare when the features of the options are not directly comparable?” We claim that the individual features of an item give rise to an emergent property that we term Psychological Value. As a result, people can make the choice by directly comparing the magnitude of Psychological Value of each item. This allows people to compare the Psychological Value of incommensurate items (e.g., Cohen & Ahn, 2016).

As an emergent feature (rather than a simple aggregate), Psychological Value is fundamentally different than the features that give rise to it. Therefore, future research may benefit from a three-pronged approach. First, similar to the work conducted here, it is important to study which choices Psychological Value predict well, and which choices it does not. This will provide valuable information about the generalizability of PVT. Second, research should be directed at examining the features of Psychological Value itself (e.g., grouping properties, independence, etc.). This will provide a deeper understanding of this construct and that knowledge will translate into better predictive models. Finally, for those interested in specific item classes (e.g., people), researchers can study the features that influence Psychological Value of specific items or item classes. For example, we suspect that competence and warmth contribute to the Psychological Value of people, whereas other features contribute to the Psychological Value of inanimate objects, etc. In our view, this will provide a deeper understanding of the item class, rather than Psychological Value per se.

**Generalizability**

Here, we have studied how people make charitable giving decisions when the receivers are individuals who hold different social roles. Although many charities are focused on social roles (e.g., the “National Law Enforcement Officers Memorial Fund”), many are not (e.g., the Natural Resources Defense Council, which is a climate change charity). It is important to study how well PVT predicts donations to those charities focused on more abstract issues. There are many ways to approach this topic. Perhaps the most direct approach is to estimate the Psychological Values of abstract concepts such as climate change and use those to predict charitable giving. In our lab, we have successfully collected the perceived Psychological Values of abstract concepts for other purposes, and they predict choice in those instances. Another, less direct but more applied approach, might be to estimate the Psychological Values of concrete stimuli related to these concepts such as a “stable food supply” and use those terms in a scenario that is connected to the charity of interest. In any event, PVT provides a strong computational foundation from which these questions can be addressed.

We also note that our population sample for both our Psychological Value data and the current choice experiment data are college students. We suspect that Psychological Values are relatively stable, nevertheless they may be influenced by social factors such as age, culture, etc. The extent of the influence of social factors is an empirical question that should be explored. Nevertheless, we suspect that the decision process described by PVT reflects a basic cognitive system that is independent of social/cultural factors.

Finally, although we propose that the Psychological Value of the receiver is the primary influence of charitable giving, there remain an indefinite number of other influential factors (see e.g., Bekkers & Wiepking, 2011). For example, is there an influence of familial membership (e.g., your mother vs a mother) above the contribution of Psychological Value and how does the perceived financial need of the receiver influence giving? Although the current set of experiments does not address this question, we do provide the framework for studying factors of interest within the context of PVT. We encourage researchers to do so.

**Concluding remarks**

In sum, PVT provides a computational cognitive model of charitable giving. This model makes precise, point predictions. As such, only very select patterns of data will fit the predictions. We show how PVT provides a quantitative framework for studying the influence of features of interest on charitable giving. Such a framework should be of some help to researchers interested in understanding how features of interest influence the decision processes associated with charitable giving.

PVT was originally applied to modelling decision making in classic sacrificial moral dilemmas (Cohen & Ahn, 2016). The theory was then broadened to account for economic decisions (Cohen et al., 2022). Here we have extended the theory further to show how the same putative processes apparently underpin decisions about charitable giving and this theory has provided a detailed insight into the reasons behind in-group bias.

By extending the application of PVT in this way we claim that the same general-purpose decision-making processes underpin preferential judgments in a range of applications including charitable giving. We therefore feel that, despite arguments to the contrary – that charitable giving implicates special purpose cognitive/neurological mechanisms (Harbaugh et al., 2007; Hare et al., 2010) - the emerging evidence is that the same general purpose mechanisms are theoretically adequate. We do not claim that the current evidence is incontrovertible, but in the absence of alternative computational accounts, we feel that PVT has garnered strong support. Without the level of specification and predictive precision that such models provide, it is impossible to adjudicate between putative competing accounts of cognitive processes.

Open Practices Statement

The data and R code for the analysis of Experiments 1-5 can be downloaded at:  
<https://github.com/ccpluncw/ccpl_data_PVTcharity2021.git>

The analyses of Experiments 1-5 rely on five R packages written by the first author. These packages can be retrieved from Github at the following URLs:

https://github.com/ccpluncw/ccpl\_R\_chutils

https://github.com/ccpluncw/ccpl\_R\_chValues

https://github.com/ccpluncw/ccpl\_R\_chMorals

https://github.com/ccpluncw/ccpl\_R\_RRW

<https://github.com/ccpluncw/ccpl_R_smartGridSearch>

These packages are continuously updated. The most recent analyses for the current manuscript were conducted in June, 2021.

Please cite this article if data is used in any way to produce a publication. These data cannot be used without the first author’s consent for any for profit endeavor.

The experimental presentation code is proprietary and is not open sourced. These experiments were not pre-registered.

**References**

Andreoni, J. (1990). Impure altruism and donations to public goods: a theory of warm-glow giving. *The Economic Journal, 100*, 464–477. doi: <https://doi.org/10.2307/2234133>

Andreoni, J., Harbaugh, W. T., & Vesterlund, L. (2010). Altruism in experiments *Behavioural and experimental economics* (pp. 6-13). London, UK: Palgrave Macmillan.

Andreoni, J., & Miller, J. H. (2002). Giving according to GARP: an experimental test of the consistency of preferences for altruism. *Econometrica, 70*, 737-753. doi: <http://dx.doi.org/10.1111/1468-0262.00302>

Ashby, F. G., Jekel, M., Dickert, S., & Glöckner, A. (2016). Finding the right fit: A comparison of process assumptions underlying popular drift-diffusion models. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 42*, 19821993. doi: <http://dx.doi.org/10.1037/xlm0000279>

Ashby, F. G., & Lee, W. W. (1993). Perceptual variability as a fundamental axiom of perceptual science. *Advances in psychology, 99*, 369-399. doi: <http://dx.doi.org/10.1016/S0166-4115(08)62778-8>

Balakrishnan, J. D., & Ratcliff, R. (1996). Testing models of decision making under confidence ratings in classification. *Journal of Experimental Psychology: Human Perception and Performance, 22*, 615-633. doi: <http://dx.doi.org/10.1037/0096-1523.22.3.615>

Barberis, N. C. (2013). Thirty years of prospect theory in economics: A review and assessment. *Journal of Economic Perspectives, 27*, 173-196. doi: <https://doi.org/10.1257/jep.27.1.173>

Bekkers, R., & Weipking, P. (2011). A literature review of empirical studies of philanthropy: Eight mechanisms that drive charitable giving. *Nonprofit and Voluntary Sector Quarterly, 40*, 924-973. doi: <http://dx.doi.org/10.1> 177/0899764010380927

Billig, M., & Tajfel, H. (1973). Social categorization and similarity in intergroup behaviour. *European Journal of Social Psychology, 3*, 27-52. doi: <https://doi.org/10.1002/ejsp.2420030103>

Brewer, M. B. (1979). In-group bias in the minimal intergroup situation: A cognitive-motivational analysis. *Psychological Bulletin, 86*, 307-324. doi: <https://doi.org/10.1037/0033-2909.86.2.307>

Busemeyer, J. R., Gluth, S., Rieskamp, J., & Turner, B. M. (2019). Cognitive and neural bases of multi-attribute, multi-alternative, value-based decisions. *Trends in Cognitive Sciences, 23*, 251-263. doi: <https://doi.org/10.1016/j.tics.2018.12.003>

Busemeyer, J. R., & Townsend, J. T. (1993). Decision field theory: a dynamic-cognitive approach to decision making in an uncertain environment. *Psychological Review, 100*, 432-459. doi: <http://dx.doi.org/10.1037/0033-295X.100.3.432>

Carmichael, S. T. (2016). Emergent properties of neural repair: elemental biology to therapeutic concepts. *Annals of Neurology, 79*, 895-906. doi: <http://dx.doi.org/10.1002/ana.24653>

Chen, F., & Krajbich, I. (2018). Biased sequential sampling underlies the effects of time pressure and delay in social decision making. *Nature Communications, 9*, 3557. doi: <http://dx.doi.org/10.1038/s41467-018-05994-9>

Chibb, V. S., Rangel, A., Shimojo, S., & O’Doherty, J. P. (2009). Evidence for a common representation of decision values for dissimilar goods in human ventromedial prefrontal cortex. *Journal of Neuroscience, 29*, 12315-12320. doi: <https://doi.org/10.1523/JNEUROSCI.2575-09.2009>

Cohen, D. J., & Ahn, M. (2016). A subjective utilitarian theory of moral judgment. *Journal of Experimental Psychology: General, 145*, 1359-1381. doi: <http://dx.doi.org/10.1037/xge0000210>

Cohen, D. J., Cromley, A. R., Freda, K. E., & White, M. (2022). Psychological value theory: The psychological value of human lives and economic goods. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 2015–2050. doi: <http://dx.doi.org/10.1037/xlm0001047>

Cohen, D. J., & Quinlan, P. T. (2016). How numbers mean: Comparing random walk models of numerical cognition varying both encoding processes and underlying quantity representations. *Cognitive Psychology, 91*, 63-81. doi: <http://dx.doi.org/10.1016/j.cogpsych.2016.10.002>

Colas, J. T., & Lu, J. (2017). Learning where to look for high value improves decision making asymmetrically. *Frontiers in Psychology, 8*, 2000. doi: <https://doi.org/10.3389/fpsyg.2017.02000>

Cromley, A. R., & Cohen, D. J. (2019). Subjective values theory: The psychophysics of psychological value. *PsyArXiv*. doi:<http://dx.doi.org/10.31234/osf.io/wfd5s>

Cuddy, A. J. C., Fiske, S. T., & Glick, P. (2007). The BIAS map: Behaviors from intregroup affect and stereotypes. *Journal of Personality and Social Psychology, 92*, 631-648. doi: <http://dx.doi.org/10.1037/0022-3514.92.4.631>

Dickert, S., Sagara, N., & Slovic, P. (2011). Affective motivations to help others: A two‐stage model of donation decisions. *Journal of Behavioral Decision Making, 24*, 361-376. doi: <http://dx.doi.org/10.1002/bdm.697>

Echazu, L., & Nocetti, D. (2015). Charitable giving: Altruism has no limits. *Journal of Public Economics, 125*, 46-53. doi: <https://doi.org/10.1016/j.jpubeco.2015.03.002>

Fehr, E., Bernhard, H., & Rockenbach, B. (2008). Egalitarianism in young children. *Nature, 454*, 1079-1083. doi: <https://doi.org/10.1038/nature07155>

Fishburn, P. C. (1981). Subjective expected utility: A review of normative theories. *Theory and decision, 13*, 139-199. doi: <http://dx.doi.org/10.1007/BF00134215>

Fiske, S. T. (2015). Intergroup bias: A focus on stereotype content. *Current Opinion in Behavioral Sciences, 3*, 45-50. doi: <http://dx.doi.org/10.1016/j.cobeha.2015.01.010>

Fiske, S. T., Cuddy, A. J. C., & Glick, P. (2007). Universal dimensions of social perception: warmth and competence. *Trends in Cognitive Sciences, 11*. doi: <http://dx.doi.org/10.1016/j.tics.2006.11.005>

Fiske, S. T., Cuddy, A. J. C., Glick, P., & Xu, J. (2002). A model of (often mixed) stereotype content: Competence and warmth respectively follow from perceived status and competition *Journal of Personality and Social Psyhology, 82*, 878-902. doi: <http://dx.doi.org/10.1037//0022-3514.82.6.878>

Furnham, A. (1996). Factors relating to the allocation of medical resources. *Journal of Social Behavior and Personality, 11*, 615-624.

Furnham, A., Simmons, K., & McClelland, A. (2000). Decisions concerning the allocation of scarce medical resources. *Journal of Social Behavior and Personality*, 185-200.

Gescheider, G. A. (1988). Psychophysical scaling. *Annual Review of Psychology, 39*, 169-200.

Glazer, A., & Konrad, K. A. (1996). A signaling explanation for charity. *American Economic Review, 86*, 1019-1028. doi: <https://www.jstor.org/stable/2118317>

Glöckner, A., & Betsch, T. (2011). The empirical content of theories in judgment and decision making: Shortcomongs and remedies. *Judgment and Decision Making, 6*, 711-721. doi: <https://www.proquest.com/scholarly-journals/empirical-content-theories-judgment-decision/docview/1011297243/se-2>

Goodwin, G. P., & Landy, J. F. (2014). Valuing different human lives. *Journal of Experimental Psychology: General, 143*, 778-803. doi: <http://dx.doi.org/10.1037/a0032796>

Grueschow, M., Polania, R., Hare, T. A., & Ruff, C. C. (2015). Automatic versus choice-dependent value representations in the human brain. *Neuron, 85*, 874-885. doi: <http://dx.doi.org/10.1016/j.neuron.2014.12.054>

Gwinn, R., Leber, A. B., & Krajbich, I. (2019). The spillover effects of attentional learning on value-based choice. *Cognition, 182*, 294-306. doi: <http://dx.doi.org/10.1016/j.cognition.2018.10.012>

Harbaugh, W. T., Mayr, U., & Burghart, D. R. (2007). Neural responses to taxation and voluntary giving reveal motives for charitable donations. *Science, 316*, 1622-1625. doi: <http://dx.doi.org/10.1126/science.1140738>

Hare, T. A., Camerer, C. F., Knoepfle, D. T., O’Doherty, J. P., & Rangel, A. (2010). Value computations in ventral medial prefrontal cortex during charitable decision making incorporate input from regions involved in social cognition. *Journal of Neuroscience, 30*, 583-590. doi: <http://dx.doi.org/10.1523/JNEUROSCI.4089-09.2010>

Hensley, V., & Duval, S. (1976). Some perceptual determinants of perceived similarity, liking, and correctness. *Journal of Personality and Social Psychology, 34*, 159-168. doi: <https://doi.org/10.1037/0022-3514.34.2.159>

Jenkins, A. C., Karashchuk, P., Zhu, L., & Hsu, M. (2018). Predicting human behavior toward members of different social groups. *Proceedings of the National Academy of Sciences of the United States of America, 115*, 9696-9701. doi: <https://www.jstor.org/stable/26531753>

Jenni, K., & Loewenstein, G. (1997). Explaining the identifiable victim effect. *Journal of Risk and uncertainty, 14*, 235-257. doi: <https://doi.org/10.1023/A:1007740225484>

Judd, C. M., James-Hawkins, L., Yzerbyt, V., & Kashima, Y. (2005). Fundamental dimensions of social judgment: Understanding the relations between judgments of competence and warmth. *Journal of Personality and Social Psychology, 89*, 899-913. doi: <http://dx.doi.org/10.1037/0022-3514.89.6.899>

Khalil, E. L. (2004). What is altruism? *Journal of Economic Psychology, 25*, 97-123. doi: <https://doi.org/10.1016/S0167-4870(03)00075-8>

Krajbich, I. (2019). Accounting for attention in sequential sampling models of decision making. *Current Opinion in Psychology, 29*, 6-11. doi: <https://doi.org/10.1016/j.copsyc.2018.10.008>

Krajbich, I., Armel, C., & Rangel, A. (2010). Visual fixations and the computation and comparison of value in simple choice. *Nature Neuroscience, 13*, 1292. doi: <https://doi.org/10.1038/nn.2635>

Krajbich, I., Hare, T. A., Bartling, B., Morishima, Y., & Fehr, E. (2015). A common mechanism underlying food choice and social decisions. *PLoS Computational Biology, 11*, e1004371. doi: <https://doi.org/10.1371/journal.pcbi.1004371>

Lebreton, M., Jorge, S., Michel, V., Thiron, B., & Pessiglione, M. (2009). An automatic valuation system in the human brain: evidence from functional neuroimaging. *Neuron, 64*, 431-439. doi: <http://dx.doi.org/10.1016/j.neuron.2009.09.040>

Levine, M., Prosser, A., Evans, D., & Reicher, S. (2005). Identity and Emergency Intervention: How Social Group Membership and Inclusiveness of Group Boundaries Shape Helping Behavior. *Personality and Social Psychology Bulletin, 31*, 443-453. doi: <https://journals.sagepub.com/doi/pdf/10.1177/0146167204271651>

Lin, A., Adolphs, R., & Rangel, A. (2011). Dynamic construction of stimulus values in the ventromedial prefrontal cortex. *PLoS ONE, 6*, e21074. doi: <https://doi.org/10.1371/journal.pone.0021074>

Link, S. W., & Heath, R. A. (1975). A sequential theory of psychological discrimination. *Psychometrika*. doi: <http://dx.doi.org/10.1007/BF02291481>

Macmillan, N. A., & Creelman, C. D. (2005). *Detection theory: A user's guide* (2nd ed.). New York, NY: Lawrence Erlbaum Associates.

Marks, L. E. (1974). On scales of sensation: Prolegomena to any future psychophysics that will be able to come forth as science. *Perception & Psychophysics, 16*, 358-376. doi: <https://doi.org/10.3758/BF03203956>

Milosavljevic, M., Malmaud, J., Huth, A., Koch, C., & Rangel, A. (2010). The drift diffusion model can account for the accuracy and reaction time of value-based choices under high and low time pressure. *Judgment and Decision Making, 5*, 437-449. doi: <https://doi.org/10.1017/S1930297500001285>

Misch, A., Fergusson, G., & Dunham, Y. (2018). Temporal dynamics of partisan identity fusion and prosociality during the 2016 US presidential election. *Self and Identity, 17*, 531-548. doi: <https://doi.org/10.1080/15298868.2018.1430063>

Niesta Kayser, D., Greitemeyer, T., Fischer, P., & Frey, D. (2010). Why mood affects help giving, but not moral courage: Comparing two types of prosocial behaviour. *European Journal of Social Psychology, 40*, 1136-1157. doi: <http://dx.doi.org/10.1002/ejsp.717>

Ockenfels, A., & Werner, P. (2014). Beliefs and ingroup favoritism. *Journal of Economic Behavior and Organization, 108*, 453-462. doi: <http://dx.doi.org/10.1016/j.jebo.2013.12.003>

Preston, J. L., & Ritter, R. S. (2013). Different effects of religion and God on prosociality with the ingroup and outgroup. *Personality and Social Psychology Bulletin, 39*, 1471-1483. doi: <http://dx.doi.org/10.1177/0146167213499937>

Rabbie, J. M., Benoist, F., Oosterbaan, H., & Visser, L. (1974). Differential power and effects of expected competitive and cooperative intergroup interaction on intragroup and outgroup attitudes. *Journal of Personality and Social Psychology, 30*, 46-56. doi: <https://doi.org/10.1037/h0036620>

Rand, D. G., Peysakhovich, A., Kraft-Todd, G., Newman, G. E., Wurzbacher, O., Nowak, M. A., & Greene, J. D. (2014). Social heuristics shape intuitive cooperation. *Nature Communications, 5*, 3677. doi: <http://dx.doi.org/10.1038/ncomms4677>

Rangel, A., & Clithero, J. A. (2014). The computation of stimulus values in simple choice *Neuroeconomics* (pp. 125-148): Academic Press.

Ratcliff, R. (1978). A theory of memory retrieval. *Psychological Review, 85*, 59-108. doi: <http://dx.doi.org/10.1037/0033-295X.85.2.59>

Ratcliff, R., & Rouder, J. N. (1998). Modeling response times for two-choice decisions. *Psychological Science, 9*, 347-356. doi: <https://doi.org/10.1111/1467-9280.00067>

Ratcliff, R., Smith, P. L., & McKoon, G. (2015). Modeling regularities in response time and accuracy data with the diffusion model. *Current Directions in Psychological Science, 24*, 458-470. doi: <http://dx.doi.org/10.1177/0963721415596228>

Reed II, A., & Aquino, K. F. (2003). Moral identity and the expanding circle of moral regard toward out-groups. *Journal of Personality and Social Psychology, 84*, 1270-1286. doi: <http://dx.doi.org/10.1037/0022-3514.84.6.1270>

Simon, B., Stürmer, S., & Steffens, K. (2000). Helping individuals or group members? The role of individual and collective identification in AIDS volunteerism. *Personality and Social Psychology Bulletin, 26*, 5-16. doi: <http://dx.doi.org/10.1177/0146167200266008>

Small, D. A., & Loewenstein, G. (2003). Helping a victim or helping the victim: Altruism and identifiability. *Jourrnal of Risk and uncertainty, 26*, 5-16. doi: <http://dx.doi.org/0.1023/A:1022299422219>

Small, D. A., & Simonsohn, U. (2008). Friends of victims: Personal experience and prosocial behavior. *Journal of Consumer Research, 35*, 532-542. doi: <https://doi.org/10.1086/527268>

Smith, V. L. (1989). Theory, experiment and economics. *Journal of Economic Perspectives, 3*, 151-169.

Stephenson, G. M., Skinner, M., & Brotherton, C. J. (1976). Group participation and intergroup relations: An experimental study of negotiation groups. *European Journal of Social Psychology, 6*, 51-70. doi: <https://doi.org/10.1002/ejsp.2420060105>

Stevens, S. S. (1956). The direct estimation of sensory magnitudes-loudness. *The American Journal of Psychology, 69*, 1-25. doi: <http://www.jstor.org/stable/1418112>

Stevens, S. S. (1975). *Psychophysics: Introduction to its Perceptual, Neural, and Social Prospects*. New York: Wiley.

Stigler, G. J. (1950). The development of utility theory. I. *Journal of Political Economy, 58*, 307-327.

Sugden, R. (1984). Reciprocity: the supply of public goods through voluntary contributions. *The Economic Journal, 94*, 772-787. doi: <https://doi.org/10.2307/2232294>

Tajfel, H. (1970). Experiments in intergroup discrimination. *Scientific American, 223*, 96-103. doi: <https://www.jstor.org/stable/10.2307/24927662>

Tajfel, H., Billig, M., Bundy, R. P., & Flament, C. (1971). Social categorization and intergroup behaviour. *European Journal of Social Psychology, 1*, 149-178. doi: <http://dx.doi.org/10.1002/ejsp.2420010202>

Thornhill, R., & Gangstad, S. W. (1999). Facial attractiveness. *Trends in Cognitive Sciences, 3*, 452-460. doi: <https://doi.org/10.1016/S1364-6613(99)01403-5>

Voss, A., Voss, J., & Lerche, V. (2015). Assessing cognitive processes with diffusion model analyses: A tutorial based on fast-dm-30. *Frontiers in Psychology, 6*. doi: <https://doi.org/10.3389/fpsyg.2015.00336>

Watson, M. V. S. (2015). Mueller and Mises: Integrating the gift and “final distribution” with Praxeology. *The Quarterly Journal of Austrian Economics, 18*, 173-202.

Yamagishi, T., Matsmoto, Y., Kiyonari, T., Takagishi, H., Li, Y., Kanai, R., & Sakagami, M. (2017). Response time in economic games reflects different types of decision conflict for prosocial and proself individuals. *Proceedings of the National Academy of Sciences, 114*, 6394-6399. doi: <https://doi.org/10.1073/pnas.1608877114>

Zagefka, H., Noor, M., Brown, R., de Moura, G. R., & Hopthrow, T. (2011). Donating to disaster victims: Responses to natural and humanly caused events. *European Journal of Social Psychology, 41*, 353-363. doi: <http://dx.doi.org/10.1002/ejsp.781>

**Appendix A**

PVT holds that when presented with a yes/no task, the respondent compares the item to a Threshold Distribution (TD). Whereas one may have a specific hypothesis about the moments of this distribution (based on utility functions, etc.), more often that distribution is unknown. As such, the researcher must infer the TD from the data. There are many ways to do this. In a traditional VSSP, the distributional overlap is inferred for each stimulus/condition from the data while fitting the VSSP model (Voss et al., 2015). This is a minimally constrained process, because it permits a free parameter for each stimulus used. We take a different approach. We have measured Psychological Value distributions for each of our stimuli. We assume that a single TD is compared against every distribution. This greatly increases the constraint of the model. Furthermore, we do not identify the TD while fitting the PVT. Rather, we identify the TD in a separate analytical step and then test that TD in the model. This will undoubtedly produce a less optimal fit than identifying the TD while fitting the PVT model. Nevertheless, the fit will be more robust because it will be less susceptible to the pitfalls of overfitting to the data.

To identify the TD in the current experiment, we made the following assumptions:

1. The TD is gaussian
2. When the data is split only by Overlap and ItemHVO/ItemLVO
   1. The function relating *p*(HVO) to Overlap is an exponential decay function:
   2. The function relating *p*(HVO) to Overlap for ItemHVO and ItemLVO will share a beta coefficient. When Overlap = 1, ItemHVO and ItemLVO will be equal, but opposite distance from the chance line.
   3. The function relating RT to Overlap will be positive and linear.
   4. The RT functions for ItemHVO and ItemLVO will share a slope, but may have different intercepts.

Before estimating the TD, filtered the choice data as described in the text, with the exception that we did not identify the participant who switched keys (that required the TD to be identified first). We then used our Smart Grid Optimization algorithm to identify the TD that best satisfied the assumptions. The algorithm systematically adjusted the mean and SD of the TD until the RT and *p*(HVO) functions produced the best fit to the data.

Footnotes

1. These typically involved the participant notifying the experimenter that the program exited unexpectedly, etc. during the experiment. Because the experimenter was not in the room with the participant when they were running, we could not observe what the participant did prior to the program exiting. [↑](#endnote-ref-1)
2. Because the SD of RTs inherently increase with their mean, SD alone is not a useful measure of RT consistency. [↑](#endnote-ref-2)
3. The same pattern of results was found when the side bias parameter was included in the models. [↑](#endnote-ref-3)
4. We did not include the counseling scenario because we have a separate research program addressing mental health specific charity research. [↑](#endnote-ref-4)
5. The results do not change when the deleted participant remains in the dataset. [↑](#endnote-ref-5)