American Economic Journal: Microeconomics 2017, 9(1): 123–151
https://doi.org/10.1257/mic.20150172

Making Case-Based Decision Theory Directly Observable[†]

By Han Bleichrodt, Martin Filko, Amit Kothiyal, and Peter P. Wakker*

Case-based decision theory (CBDT) provided a new way of revealing preferences, with decisions under uncertainty determined by similarities with cases in memory. This paper introduces a method to measure CBDT that requires no commitment to parametric families and that relates directly to decisions. Thus, CBDT becomes directly observable and can be used in prescriptive applications. Two experiments on real estate investments demonstrate the feasibility of our method. Our implementation of real incentives not only avoids the income effect, but also avoids interactions between different memories. We confirm CBDT's predictions except for one violation of separability of cases in memory. (*JEL* D12, D81, R30)

Case-based decision theory (CBDT) was introduced as an alternative to Savage's (1954) classical state-space model for decision under uncertainty. In Savage's model, a state space captures uncertainty, where only one of its elements is true, but the decision maker does not know for sure which one. The decision maker has to choose between different acts. Their outcome depends on the true state of nature and, hence, is uncertain. Formally, acts map states to outcomes and preferences are expressed over acts. Savage's state-space model is commonly used to study expected utility and to incorporate ambiguity, especially in the version of Anscombe and Aumann (1963). However, Savage's model is too restrictive in many decision problems. Often, not only the probabilities of the states of nature are unknown, as under ambiguity, but, more fundamentally, even the states themselves cannot be specified. CBDT provides a framework to analyze such decision problems. Its aim is not to replace expected utility or ambiguity theories but, instead, to be applicable to other, less structured situations.

In CBDT, preferences are determined by cases in the decision maker's memory and their similarity with the decision at hand. Similarity is measured by weights and these weights are the main new subjective parameters of CBDT. CBDT has several advantages over Savage's model. No counterfactual events or outcomes need to be considered in the real choice situation (Gilboa and Schmeidler 2001, 43, 93–95—henceforth, GS). CBDT naturally fits with our everyday thinking. The primary

*Bleichrodt: Erasmus School of Economics, Erasmus University Rotterdam, Rotterdam, the Netherlands (e-mail: bleichrodt@ese.eur.nl); Filko: Institute for Financial Policy at the Ministry of Finance, Bratislava, the Slovak Republic; Kothiyal: Max Planck Institute for Human Development, Berlin, Germany (e-mail: kothiyal@mpib-berlin.mpg.de); Wakker: Erasmus School of Economics, Erasmus University Rotterdam, P.O. Box 1738, Rotterdam, 3000 DR, the Netherlands (e-mail: wakker@ese.eur.nl). We are sad that Martin Filko passed away on March 28, 2016. Wilbert van den Hout made helpful comments.
[†]Go to https://doi.org/10.1257/mic.20150172 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

achievement of GS was to connect case-based models of information processing, widely used in computer science[1] and other fields,[2] with economic decision making.

Gilboa and Schmeidler (1995, and many follow-ups) and Eichberger and Guerdjikova (2013) gave preference foundations of CBDT, demonstrating its theoretical soundness. There is, however, a dearth of empirical applications. Those that exist use introspective judgments (Lovallo, Clarke, and Camerer 2012) or parametric fittings.[3] Such fittings require a commitment to particular parametric families of similarity weights, usually based on a particular distance function, and to particular error theories. As often pointed out (Tversky and Kahneman 1992 Section 2.3; Farias, Jagabathula, and Shah 2013), this commitment may introduce distortions when the parametric assumptions conflict with people's preferences. This possibility is particularly relevant for new concepts such as CBDT's similarity weights, because to date, little is known about their properties (Guerdjikova 2008; Pape and Kurtz 2013 footnote 13). Similarity judgments may even violate basic properties of distance measures, such as symmetry and the triangular inequality (Tversky and Gati 1982).

We introduce a nonparametric method to measure similarity weights that avoids all parametric assumptions and that reveals these weights whatever they are. Our method is similar to de Finetti's (1931) betting odds system for measuring subjective probabilities, a well-known nonparametric measurement method in classical decision theory.[4] Nonparametric measurements have several other advantages besides avoiding parametric assumptions. They are elementary and can be carried out using only pencil and paper. Further, the empirical meaning of concepts is directly related to preferences, so that violations can directly be traced back to the underlying choices, showing where the model should be improved. We thus make CBDT directly observable (and falsifiable) in full generality. A final advantage of nonparametric measurements is that they can be used in prescriptive and interactive sessions to determine optimal decisions (Keeney and Raiffa 1976), as for instance in policy recommendations.

We apply our method in two experiments with gambles that mimic real estate investments, a domain that is particularly prone to case-based reasoning (Gayer, Gilboa, and Lieberman 2007). In particular, it is unclear how a relevant Savage state space could be defined for the decisions considered.[5]

A complication in experimentally testing CBDT is that CBDT deviates not only from Savage's state-space model but also, and more fundamentally, from classical revealed preference. This complicates the use of real incentives, and we had to design a case-based analog of the popular random incentive system to incentivize the experiments. The random incentive system avoids income effects (interactions between different outcomes received), but the measurement of CBDT requires a

[1] See Riesbeck and Schank (1989); Aha, Marling, and Watson (2005); and Hüllermeier (2007).

[2] See Dubois et al. (1999); Hertwig et al. (2004); Stewart, Chater, and Brown (2006); and Greco, Matarazzo, and Słowiński (2008).

[3] See Gayer, Gilboa, and Lieberman (2007) and Golosnoy and Okhrin (2008). Also see Guerdjikova (2008, 112) and Pape and Kurtz (2013). These studies all based similarity weights on Euclidean distance and found support for CBDT.

[4] The decision-theoretic term nonparametric should not be confused with the statistical term, as discussed by Gilboa, Lieberman, and Schmeidler (2011) in the context of CBDT.

[5] This also explains why we cannot compare the performance of CBDT with the performance of a traditional analysis based on classical expected utility.

procedure that also avoids interactions between the memories used in different decision problems. Designing such an incentive mechanism is difficult because people cannot deliberately forget information. We propose an incentive-compatible mechanism for CBDT that is based on Bardsley's (2000) conditional information system. Our mechanism avoids income effects and interactions between different memories and, thus, allows us to consider various memories, also if they overlap.

Our experimental findings confirm all predictions of CBDT that we tested except one: in one test we found interactions between different cases in a memory. These interactions violate case-separability and they confirm GS's (see their section 19.2) suggestion that generalizations of case-separability are desirable (Pęski 2011; Eichberger and Guerdjikova 2013). Our findings on real estate investments are plausible. They underscore that CBDT complements classical expected utility and classical revealed preference and gives new insights into decision making.

I. Case-Based Decision Theory Defined

This section first defines CBDT in general, and then introduces simplifying notation targeted to our experiment.

A. *Definitions*

We use a general version of CBDT based on act similarity (GS, 51). Other versions are discussed in Section VI. CBDT adds memories to the revealed-preference choice sets. Because of CBDT's increased complexity, it requires more complex stimuli in experiments. This is a necessary price for capturing richer phenomena with unknown state spaces and giving more insights into decision making.

A *memory* M contains cases. A *case* is a triple $(q,e,r) \in P \times A \times R$, where $q \in P$ is a *problem* encountered in the past, $e \in A$ is the *act* chosen in that problem, and $r \in R$ is the *result* in this case. The pair (q,e) is a *circumstance*. If a particular act e is chosen in a given real problem, then there is no need to specify the other acts that could have been, but were not chosen (they were all less preferred than e). In contrast with Savage's classical paradigm, there then is no need either to specify hypothetical or counterfactual events and/or outcomes/results (events and outcomes/results that could have happened but did not happen) in CBDT. All results have really been experienced.

Let p denote the actual problem faced. In CBDT, the act (= choice option) a that is chosen from the *choice set* D of available acts maximizes

$$U(a) = \sum_{(q_j,e_j,r_j)\in M} s((p,a),(q_j,e_j))u(r_j). \tag{1}$$

Here, u is a *utility function* mapping results to the reals, and s is a nonnegative *similarity function*.[6] The similarity weights determine the exchange rate between utility units under different circumstances. The similarity function, which is the new

[6]Nonnegativity is not essential because only differences between decision weights matter for decision making, as we will see later.

subjective parameter introduced by CBDT, describes the similarity of each pair of circumstances (p, a) and (q_j, e_j). The total sum of the similarity weights in equation (1) may be different for different acts. There may be many similar cases in memory for one act, and only few for another.

A result with utility 0 is called *neutral*. For simplicity, we assume that it is unique. Whereas in most applications of CBDT the neutral result needs to be known, our method to measure the similarity weights—the main topic of this paper—does not require knowledge of the neutral result. We achieve this by a design where the terms corresponding to the neutral result drop from the equations.

In our experiment we assume *linear utility*:

$$u(r) = r - \theta, \tag{2}$$

where θ is the neutral result, which can be different from 0.

B. *Discussion*

CBDT departs fundamentally from classical revealed preference theory. Preference conditions in the latter relate choices from different sets of choice alternatives (acts in our case) to each other. For instance, Savage's (1954) sure-thing principle relates a choice between two acts that have a common outcome to a choice between two other acts that have another common outcome, and transitivity relates a choice from $\{a, b\}$ and a choice from $\{b, c\}$ to a choice from $\{a, c\}$. Under CBDT, choices from the same set of acts can be compared under different informational conditions (GS, section 4.2, 94; Gilboa, Schmeidler, and Wakker 2002, 485—henceforth, GSW), and similarity weights obtain their empirical meaning from these comparisons. The informational conditions are formalized through a memory that contains past cases similar to the actual decision problem. CBDT can combine variations in memory with variations in sets of choice alternatives, leading to generalized versions. To clarify the novelty of CBDT, this paper keeps the set of choice alternatives fixed and only varies memory.

The additive form of equation (1) implies that the contribution of each case is not affected by the other cases in memory (*separability*; GS, 66, A2, "combination"). It also implies that the utility exchange rate between two cases (the ratio of their similarity weights) is not affected by adding other cases to memory, a condition tested in our experiments.

The neutral result is not neutral in the sense of classical revealed preference theories, such as prospect theory, where it refers to value. Instead, in CBDT neutrality refers to information and variations in memory. A neutral result gives information that does not favor or disfavor any act. Further discussions of neutral results include GS (133, 148 ff.).

C. *Simplifying Notation to Prepare for the Measurement Method*

From now on, we assume one fixed actual problem p with one fixed set of available acts $\{a, b\}$. This ensures that classical revealed preference techniques, which use varying sets of available choice options, play no role in our measurements. Instead, we focus on the new feature of CBDT, the dependence on memory, and we use varying memories. To clarify this novelty, we simplify all other aspects and notation as much as possible.

We call b the *basic act* and a the *alternative act*. This terminology does not reflect a substantive difference between a and b and only serves to improve readability. We suppress p and $\{a, b\}$ henceforth. We denote the preference relation over acts by $\succcurlyeq_M$; $\succ_M$ and $\sim_M$ denote strict preference and indifference. Following the notational conventions in CBDT (GS), we denote the dependencies on memory, our main interest, through subscripts. Because the decomposition of circumstances $c = (q, e)$ in memory into the problem q and the act e plays no role in what follows, we use the notation c_j for circumstance (q_j, e_j) (discussed by Guerdjikova 2008, 109).

For each circumstance c_j, we define the *decision weight* as the difference between the similarity weights of a and b, the two acts under consideration:

$$(3) \quad d_j = s\big((p,a),(q_j,e_j)\big) - s\big((p,b),(q_j,e_j)\big) = s\big((p,a),c_j\big) - s\big((p,b),c_j\big).$$

Decision weights can be interpreted as similarity weights under the scaling convention $s((p,b),c_j) = 0$ for all j.[7] In equation (3), positive decision weights support a over b and negative decision weights support b over a. We call circumstance c_j *favorable* if $d_j > 0$, *neutral* if $d_j = 0$, and *unfavorable* if $d_j < 0$. These terms indicate how the circumstances support a rather than b (GSW, 487). If c_j is favorable, then improving its result strengthens the preference for a; and so on. By equation (1), the preference between acts a and b is determined by the sign of

$$\sum_{(q_j,e_j,r_j)\in M}\Big(s\big((p,a),(q_j,e_j)\big) - s\big((p,b),(q_j,e_j)\big)\Big)u(r_j),$$

that is,

$$(4) \qquad a \begin{array}{c}\succ_M\\ \sim_M\\ \prec_M\end{array} b \Leftrightarrow \sum_{(c_j,r_j)\in M} d_j u(r_j) \begin{array}{c}>\\ =\\ <\end{array} 0.$$

Decision weights comprise all the relevant information about similarity weights. That is, the level 0 of the similarity weights can be freely chosen for each circumstance c_j. Further, decision weights are unique up to a common positive factor. This invariance is similar to ratio scales except that decision weights can be of either sign or zero, whereas ratio scales are usually only positive. We summarize the uniqueness results for CBDT.

THEOREM 1: *Under equation* (4) (*and* (1)), *preferences are not affected if*:[8]

(*i*) *u is multiplied by a positive factor*;

[7] This scaling convention relaxes the nonsubstantive requirement that the similarity weights should be nonnegative.

[8] See GSW, theorem 1.

(*ii*) *all similarity weights are multiplied by a common positive factor*;

(*iii*) *for each circumstance* c_j *in memory, a constant* μ_j *is added to the similarity weight* $s(a,c_j)$ *for each act* $a \in D$.

Under minimal richness assumptions (a continuum for u and nondegenerate preferences), Theorem 1 describes all uniqueness properties. The similarity weights are joint interval (cardinal) scales[9] and u is a ratio scale. Thus, in general, the neutral result with utility 0 is empirically meaningful and cannot be chosen arbitrarily. This follows because the sum of the similarity weights may not be constant and can depend on the act and memory under consideration (GS, 40, 43).

II. Theoretical Derivation of the Measurement Method

This section shows how the parameters of CBDT, utility, and the similarity weights can be measured nonparametrically from variations in memory. The basic idea is as follows: If improving the result of circumstance 1 by α leads to indifference in a given choice situation, and improving, instead, the result of circumstance 2 by β also leads to indifference, then the ratio of the similarity weights of these two circumstances is β:α. That such an elementary procedure, reminiscent of de Finetti's (1931) betting odds procedure, can be used to measure CBDT's similarity weights has not been known before.

We will show how to observe the signs of decision weights and how to distinguish the pair of decision weights 1, 2 from the pair -1, -2 even though they have the same ratio $1/2 = (-1)/(-2)$. This sign dependence complicates the mathematical and statistical analysis of similarity weights in the same way as it complicates the analysis of sign-dependent ratios in general (such as cost-effectiveness ratios; see Groot Koerkamp et al. 2007).

The variations of memory that we consider are as follows: To measure the ratio of decision weights d_0 and d_1, we vary the results r_0 and r_1 of two circumstances c_0 and c_1 in memory. This part of memory, concerning c_0 and c_1, is *explicit* and is denoted M_e. The remaining part of memory, $M \backslash M_e$ is implicit, and is denoted M_p. It is called the *prior memory*. In our measurements, all terms generated by cases in M_p drop from the equations and can be ignored. In the experiments, M_p will be the memory of the subjects when they enter the lab, which we obviously cannot control, and M_e concerns the extra cases that we add. The results r_j of the circumstances c_j in M_e will be varied in the experiments. By $M_{r_i r_j r_k}$ we denote the memory $M_p \cup M_e$ with $M_e = \{(c_i,r_i), (c_j,r_j), (c_k,r_k)\}$, and similarly $M_{r_i r_j}$ denotes $M_p \cup M_e$ with $M_e = \{(c_i,r_i), (c_j,r_j)\}$. Thus, if M_e is $\{(c_0,r_0), (c_1,r_1)\}$, then $\sim_{M_{\mathbf{r}'_0 t_1}}$ denotes an indifference relationship resulting when in M_e, r_0 was replaced by $\mathbf{r'_0}$ and r_1 by t_1. That is, it concerns an indifference $\sim_M$ with $M = M_{\mathbf{r}'_0 t_1} = M_p \cup \{(c_0,\mathbf{r'_0}), (c_1,t_1)\}$.

[9] See Guerdjikova (2008, 109–110). In many applications of CBDT, further scaling conventions are imposed on the similarity weights that determine their 0 level, such as being 0 whenever the acts involved in the two circumstances are different (Gilboa and Schmeidler 1995, theorems 1, 2; Gilboa and Schmeidler 1996, 1997; Blonski 1999; Jahnke, Chwolka, and Simons 2005, 17, 23). Then the similarity weights are ratio scales. If similarity weights are assumed nonnegative, then the constants that can be added are constrained.

We take a pair of *touchstone* results t_0 and t_1, and touchstone cases (c_0, t_0) and (c_1, t_1) for which the acts a and b are not indifferent. We then consider two changes of the results in these cases, $t_0 \rightarrow \mathbf{r_0'}$ and $t_1 \rightarrow \mathbf{r_1'}$, each of which (in isolation) leads to indifference between a and b. We sometimes write the results that have been changed to produce indifference in bold to clarify how we use Theorem 2 below. Gilboa and Schmeidler usually analyzed CBDT under the assumption that such results $\mathbf{r_0'}$ and $\mathbf{r_1'}$ exist. To ensure this existence, they used various diversity axioms or solvability/continuity axioms (for the latter, see Gilboa and Schmeidler 1995, 635–636 Axiom A2′, or GSW, 489: solvability). We follow this approach in the following theorem, where we also assume that $\mathbf{r_0'}$ and $\mathbf{r_1'}$ exist.

GSW present two ways to measure utility that we discuss in Appendix A. The central topic of our paper is the measurement of similarity weights, and in our method we assume that utilities are known. They may have been obtained using the procedure described in Appendix A, or they may be linear. In our experiments, we accordingly assume linear utility. This reduces the number of experimental tasks because utility does not need to be measured. Many authors have argued for the reasonableness of linear utility for moderate money amounts (as in our experiments) and have confirmed it empirically.[10] Linear utility was also assumed by Gilboa and Schmeidler in several papers (Gilboa and Schmeidler 1995, 613, and Axiom A4, and 635 and Axiom A4′; Gilboa and Schmeidler 1997, 50, and Axiom A3). We included two tests of linear utility in our experiments and these supported our assumption. In the next theorem, $\nsim$ denotes the negation of indifference.

THEOREM 2: *Assume equation* (4) (*and* (1)) *with linear utility* (*equation* (2)) *and assume that the memory M is a disjoint union $M_p \cup M_e$. The choice set D is $\{a, b\}$* (*for problem p*). *Assume*

$$a \nsim_{M_{t_0 t_1}} b, \; a \sim_{M_{\mathbf{r_0'} t_1}} b, \text{ and } a \sim_{M_{t_0 \mathbf{r_1'}}} b \tag{5}$$

for some $\mathbf{r_0'}$ and $\mathbf{r_1'}$. Then,

$$\frac{d_1}{d_0} = \frac{\mathbf{r_0'} - t_0}{\mathbf{r_1'} - t_1} \tag{6}$$

with both ratios well-defined and nonzero.

Theorem 2 shows that, starting from memory $M_{t_0 t_1}$, a change from t_0 to $\mathbf{r_0'}$, which is weighted by d_0, has the same nonzero effect as a change from t_1 to $\mathbf{r_1'}$ weighted by d_1. This leads to equation (6). Regarding the sign of a decision weight, i.e., the favorability of the corresponding circumstance, we have the following result.

[10] See de Finetti (1993); Pigou, Friedman, and Georgescu-Roegen (1936); Luce (2000, 86); Rabin (2000); Birnbaum (2008, 469); and Epper, Fehr-Duda, and Bruhin (2011). If expected utility, while extensively falsified empirically, is nevertheless assumed, then risk aversion for small stakes as found, for instance, by Holt and Laury (2002) could be taken as evidence against linear utility. However, under the empirically more valid prospect theory, such risk aversion can be explained by probability weighting and it is consistent with linear utility. The assumption of linear utility in our experiment also implies that our nonparametric measurement only concerns the similarity weights, and not utility. The measurement of general utility described in Appendix A is parameter-free, leading to a completely parameter-free measurement of CBDT.

THEOREM 3: *Under the assumptions of Theorem* 2, *we have*:

(7) $\{\exists r_0$ with: $[r_0 > r_0'$ and $a \succ_{M_{r_0 t_1}} b]$ or $[r_0 < r_0'$ and $a \prec_{M_{r_0 t_1}} b]\} \Rightarrow \mathbf{d_0} > \mathbf{0}$;

(8) $\mathbf{d_0} > \mathbf{0} \Rightarrow$ for all r_0: $[r_0 > r_0' \Rightarrow a \succ_{M_{r_0 t_1}} b]$ and $[r_0 < r_0' \Rightarrow a \prec_{M_{r_0 t_1}} b]$;

(9) $\{\exists r_0$ with: $[r_0 > r_0'$ and $a \prec_{M_{r_0 t_1}} b]$ or $[r_0 < r_0'$ and $a \succ_{M_{r_0 t_1}} b]\} \Rightarrow \mathbf{d_0} < \mathbf{0}$;

(10) $\mathbf{d}_0 < \mathbf{0} \Rightarrow$ for all r_0: $[r_0 > r_0' \Rightarrow a \prec_{M_{r_0 t_1}} b]$ and $[r_0 < r_0' \Rightarrow a \succ_{M_{r_0 t_1}} b]$.

The same results hold with subscripts 1 *and* 0 *interchanged* (*replacing* $\succ_{M_{r_0 t_1}}$ *by* $\succ_{M_{t_0 r_1}}$, *for instance*).

Theorem B1 in Appendix B generalizes Theorem 3 to general utility. Because we can observe utility (equations (A1) and (A2)), Theorem 2 (together with Theorem B1) gives all empirically relevant and identifiable information about d_0 and d_1 and their underlying similarity weights. This follows from the uniqueness results in Theorem 1, the subsequent discussion there, and the discussion following equation (4). These results show that only the degree to which circumstances in memory are more favorable for a than for b is relevant to the choice between acts. To derive equation (6), we do not need to know the neutral result, i.e., the level of utility, because this level drops from the equation.

In our experiments, we found negative weights d_0. We therefore normalized equation (6) by dividing by $|d_0| = -d_0$, so that larger d_1 weights always correspond with larger normalized weights. For easy reference, we display the following rewriting of equation (6):

$$(11) \qquad \frac{d_1}{-d_0} = \frac{t_0 - \mathbf{r_0'}}{\mathbf{r_1'} - t_1}.$$

III. A CBDT Version of the Random Incentive System

The implementation of incentives in experiments on CBDT raises a subtle issue, not present in traditional decision experiments based on classical revealed preference. In traditional experiments, subjects are usually asked to make many choices to obtain as much information as possible within the constraints of the experiments. Each of these choices has to be taken in isolation to avoid income effects resulting from interactions between outcomes.[11] Illuminating early discussions are in Ramsey (1931, 169–174) and Savage (1954, 29). The random incentive system (RIS) was introduced to implement incentive compatibility.[12] Subjects are asked to make many choices, but only one randomly selected choice is played out for real.

[11] If repeated purchases and multiple consumptions are relevant for the actual decision problem, then they have to be explicitly modeled that way. Then a choice option should describe combinations of purchases and of consumption bundles. A choice option is chosen once by definition.

[12] It was suggested by Savage (1954, 29).

Under some plausible assumptions, it is in subjects' best interest to consider each choice in isolation (Starmer and Sugden 1991). The choices in the experiment are then partly hypothetical and only partly real. They are only real conditional on the probability of being selected for real at the end of the experiment. Yet this procedure has been commonly accepted as sufficiently realistic in experimental economics. GS (94) explain that elicitations (as well as axiomatizations) in CBDT, as in every other preference theory, cannot escape from using such hypothetical choices.[13] CBDT's avoidance of hypothetical choices relative to expected utility concern the real choice situation, as will be the case in our experiment.

To measure CBDT, we have to consider choices under different memories. As in the RIS, we can play out one randomly selected choice for real at the end of the experiment, avoiding income effects. However, under CBDT we also need to neutralize a "memory effect." Our measurement procedure only works if the new memory *replaces* previous memories in each choice, rather than being added to them. Simply asking subjects to forget or ignore the information provided at previous choices may be possible in hypothetical choices with cooperative subjects (Gilboa and Schmeidler 1995, 621 points I and II). However, it does not work in the common and realistic setting of experimental economics with real incentives and self-interested subjects, which is also the setting of our experiments.

To control for memory effects, we told subjects that they had to make many decisions, each with a different piece of information (= memory). One of these pieces was true, and the others were made up by us (although they might be partly true). This was clear in the experiments because different memories were usually mutually incompatible. During the experiments, subjects did not know the true information. At the end of the experiments, the decision that was implemented for real involved the true information, which was revealed to the subjects only then. Bardsley (2000) used a similar way of "lying without lying" in a public good game. We adapted his idea to CBDT.

It can be argued that the choices in our experiment were partly hypothetical and only partly real, in agreement with GS's (94) explanation. Conditional on the probability of being selected for real at the end of the experiment, the information provided was true and was actually experienced by the subject, as was the outcome that would result later. As previously discussed, such conditional realism occurs in every implementation of the random incentive system in experimental economics, and is commonly accepted as proper implementation of real incentives as opposed to hypothetical choice (GS, 94). Therefore our incentives satisfy all the requirements of being real rather than hypothetical that experimental economics dictates.

We explained to our subjects that, to have their best choice implemented in the real decision, they should take each piece of information (memory) provided at a choice as true for that choice. The memories provided before and after that choice were irrelevant to the choice under consideration (Bardsley 2000, 227) and should be ignored. Rational subjects will follow this advice. Because some of the

[13] We thank an anonymous referee for pointing out the desirability to cite GS so as to avoid potential misunderstandings here.

Figure 1. Map of the Netherlands

alternative memories might be partly true, subjects did not know what was true or not in those alternative memories. Subjects may have speculated that the made-up information gave clues about true or untrue information, or they may have perceived a meta-lottery over pieces of information. The risks of such misunderstandings are similar to those in the traditional RIS, and more generally, to any spillover or learning effect in experiments. To our best knowledge, they do not lead to systematic biases and are part of the noise in our data. In our design, isolated treatment is in a subject's best interest as it is in the traditional RIS.

IV. Experiment 1 to Measure Similarity Weights and to Test CBDT

This section shows how we measured the decision weights in our first experiment.

A. *Stimuli*

The stimuli were based on the development of real estate prices in the provinces (states) of the Netherlands. Figure 1 depicts a map of the Netherlands indicating the

15 provinces. Our notation will make the text easily accessible to readers unfamiliar with Dutch geography. Knowledge of Figure 1 is not needed. Subjects had to choose between two acts a and b, with:

(12) Basic act b (*gambling on a house in South Holland*)
yields €$(3 + 10x)$
with x the % increase in the average price of a
single-family house in South Holland in the coming month.

(13) Alternative act a (*gambling on an apartment in North Brabant*)
yields €$(3 + 10y)$
with y the % increase in the average price of an
apartment in North Brabant in the coming month.

Figure 2 illustrates how the experimental questions were presented.

In CBDT, we have to specify not only the acts to choose from (the lower part of Figure 2), but also the cases in memory (the upper part of Figure 2). The cases in memory that we manipulated concerned past investments in related but different types of dwelling in various provinces in the Netherlands. This led to the following three circumstances (i.e., problems where the acts chosen concerned investments in the real estate mentioned):

c_0: house in North Holland;

c_1: house in Limburg;

c_2: apartment in Utrecht.

We provided results r_0, r_1, and/or r_2 for (some of) these circumstances, leading to cases (c_j, r_j) in memory.[14] We manipulated the results r_j and, hence, the cases in memory, but kept the acts a, b fixed. In regression terminology, the results r_j are the independent variables and the choice between a and b made by the subjects is the dependent variable. Acts were referred to as "gambles" (see Figure 2).

In this paper (but not in the experiment for the subjects), we number the circumstances c_0–c_2 according to our prior expectation (which was confirmed by the data) of their favorability for a against b. Higher decision weights correspond to higher subscripts. In particular, c_0 will be perceived as most similar to b, and favorable results under c_0 will reduce the preference for a over b.[15]

[14] We gave subjects the annual appreciation taken over the last three years. CBDT allows for flexible descriptions of cases in memory and they do not all need to have actually been experienced (GS, 47). The information that we provide may concern acts and results experienced by others. Here it can be interpreted as the results of investments made by others. As GS (101) explain, it can then be "translated" into cases of own choices.

[15] Our subjects, like almost everyone else, are probably not finance specialists who believe in a reversal (good past performance implies bad future performance) for real estate prices. The decision weight d_0 of c_0 will be negative. A favorable outcome for c_2 will rather support a. In the following analyses, the acts b and c_0 will serve as a benchmark to compare the other acts. They will be used for normalization in the algebra.

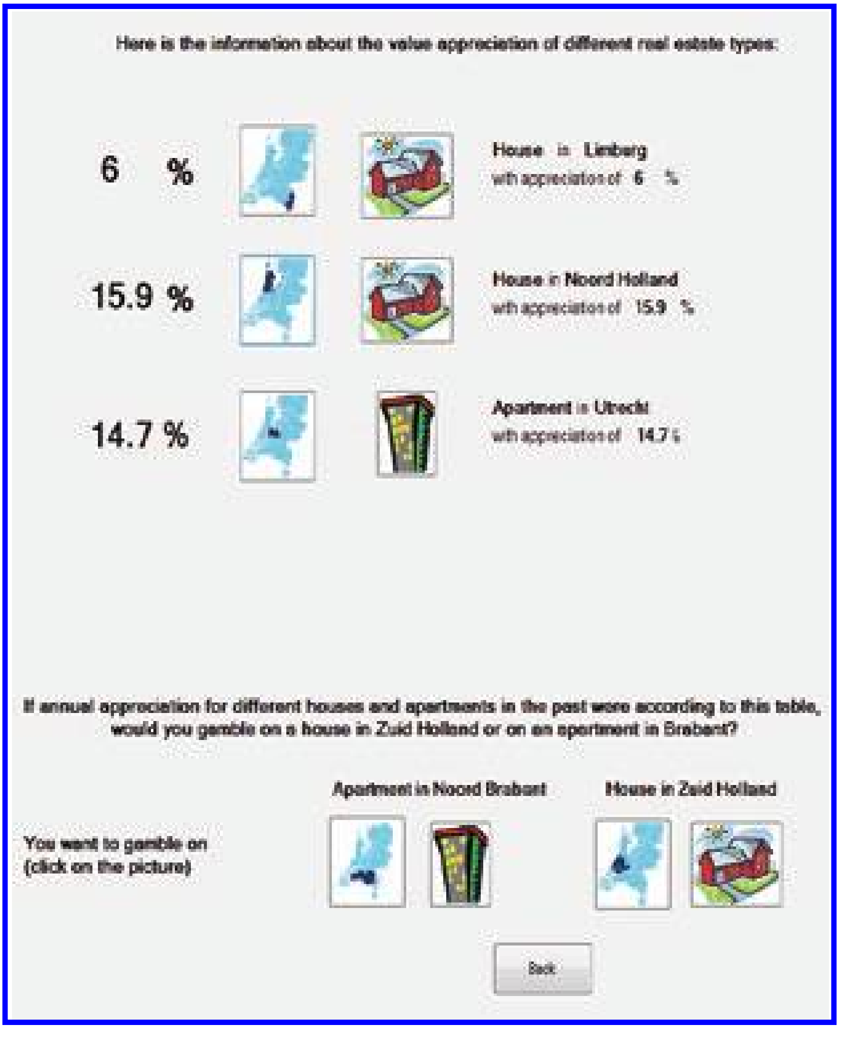

Figure 2. Stimuli in Experiment 1

Notes: The lower half of the figure presents the set of acts ("gamble") to choose from (left act: a; right act: b). The upper half presents the cases in memory that we added (M_e).

Our subjects will have more information than we provided in the experiment: they entered the lab with some prior knowledge about real estate prices. This is their prior memory, M_p, which is subject-dependent and unobservable for us.[16] The explicit memory M_e refers to the additional cases provided in the experiment. In this experiment, M_e was $\{(c_0, r_0), (c_1, r_1), (c_2, r_2)\}$ or a subset thereof. The memory M is $M_p \cup M_e$ with M_e and M_p playing similar roles as in Section II. The prior memory influences the subject's decisions, but not the correctness of our measurements. As

[16] We use indexes $j > 4$ for cases in M_p because the indexes $j = 3, 4$ will be used in Experiment 2.

explained in Section II, the terms related to M_p drop from the equations.[17] Thus biases in retrieving cases from memory, such as the availability heuristic (Lovallo, Clarke, and Camerer 2012), do not affect the correctness of our measurements of decision weights.

We expected that $d_2 > d_1 > d_0$ with d_0 usually negative. Because we assume linear utility, we have $u(r_j) = r_j - \theta$, with θ the subject-dependent and unobserved neutral result, which we do not need to know for our analysis. A plausible neutral result in our experiment may be the risk-free interest rate (Golosnoy and Okhrin 2008; Bernard and Ghossoub 2010; Barberis 2013, 179). Given that all payments were made one month after the experiment, discounting will not affect linearity of utility.

The true results t_0, t_1, and t_2 of the three circumstances c_0, c_1, and c_2 were 0.159, 0.06, and 0.147, respectively. They will serve the role of touchstone results of Section II, and we therefore use the same symbol t. Their values are depicted in Figure 2. Thus, over the past three years, the true annual rise in the price of a house in North Holland was 15.9%, 6% for a house in Limburg, and 14.7% for an apartment in Utrecht.

Indifferences were determined through an iteration procedure illustrated in Table 1. The table displays the answers of subject 35. We first asked the subject to choose between a and b for $M_{t_0 r_1}$ with $r_1 = 100\%$ (first row).[18] Unsurprisingly, she chose a. We then decreased r_1 to 0% (row 2) and she switched to b. Decreasing r_1 decreased preference for a, indicating a positive decision weight $d_1 > 0$. By varying r_1 depending on the subject's choices, we zoomed in on the result $\mathbf{r_1^1}$ for which the subject was indifferent between a and b. The recorded indifference result $\mathbf{r_1^1}$ was the midpoint of the two closest results where preference switched. In the table it is 19.8% (the midpoint of 19.1% and 20.6%).

Table 2 gives an overview of the questions asked in Experiment 1. The second column, with M^1, concerns the measurement of r_1^1 which is shown in Table 1. The empty cell at c_2 in that column indicates that circumstance c_2 was not present in the explicit memories M_e used here. The result t_0 in the second row indicates that the true value $t_0 = 15.9\%$ was always given for circumstance c_0. We write $\mathbf{r_1^1}$ in bold to indicate that the results in circumstance c_1 were varied to reach indifference between a and b, which happened for r_1^1. For each subject, we measured eight (bold-printed) values $\mathbf{r_j^i}$, each corresponding to a column and an M^j, and each obtained through iteration. The order was randomized except that M^3, which depended on M^2, followed randomly after M^2. Throughout, empty cells indicate that the corresponding circumstance was not present in a memory. The results that are not in bold were equal to their true values (except $r_0^2 + 5$ for M^3), and they were kept fixed. The results $\mathbf{r_j^i}$ will play the roles of $\mathbf{r_0'}$ and $\mathbf{r_1'}$ in Section II to obtain decision weights.

In each of the eight iterations, there was one choice situation in which all results, including the bold one, were true. Subjects did not know in which situation this

[17] As explained before, it is unlikely that one of the cases in the added memory was already present in the prior memory. Hence, we assume that M_p and M_e are disjoint.

[18] That is, we asked her to choose between a house in South Holland (act b) and an apartment in North Brabant (act a) given that the annual increase in house prices in North Holland was 15.9% and the annual increase in house prices in Limburg was 100% over the past three years.

TABLE 1—CHOICES FOR $M_{t_0 r_1}$ LEADING TO INDIFFERENCE FOR $r_1 = \mathbf{r_1^1} = 19.8\%$

Iteration	t_0	r_1	Choice
1	15.9%	100%	a
2	15.9%	0%	b
3	15.9%	6%	b
4	15.9%	53%	a
5	15.9%	29.5%	a
6	15.9%	17.7%	b
7	15.9%	23.6%	a
8	15.9%	20.6%	a
9	15.9%	19.1%	b

TABLE 2—OVERVIEW OF INDIFFERENCES OBTAINED BY QUESTIONS IN EXPERIMENT 1

	M^1	M^2	M^3	M^4	M^5	M^6	M^7	M^8
c_0	t_0	$\mathbf{r_0^2}$	$x + 5$*	$\mathbf{r_0^4}$	t_0	t_0	$\mathbf{r_0^7}$	t_0
c_1	$\mathbf{r_1^1}$	t_1	$\mathbf{r_1^3}$			$\mathbf{r_1^6}$	t_1	t_1
c_2				t_2	$\mathbf{r_2^5}$	t_2	t_2	$\mathbf{r_2^8}$

Notes: Each column gives an extra memory $M_e = M^j$ yielding indifference, and specified by the results below it, referring to the circumstances in the corresponding row. Thus, $M_e = M^1 = M_{t_0\mathbf{r_1^1}}$ and $M_e = M^7 = M_{\mathbf{r_0^7}t_1t_2}$ give indifference between acts a and b.

*We used two different values for x in Experiment 1: $x = r_0^2$ for group 1a and $x = r_1^1$ for group 1b.

happened. In Table 1, the true values (with $r_1 = 6\%$) are in Row 3. Figure 2 displays the choice situation with the true values for M^6.

B. *Similarity Weights in Our Design*

Subject 35 was indifferent between a and b for $\mathbf{r_1^1} = 19.8\%$ in the first choice question. Given memory M_p, the information in favor of a provided by case $(c_1, \mathbf{r_1^1})$ exactly offsets the remaining information, i.e., the information provided by case (c_0, t_0) jointly with M_p. The decision weight d_1 was positive for this subject because she preferred a for results larger than $\mathbf{r_1^1}$ and b for results smaller than $\mathbf{r_1^1}$.

The results $\mathbf{r_1^1}$ and $\mathbf{r_0^2}$ are such that

$$(14) \qquad a \sim_{M_{t_0\mathbf{r_1^1}}} b \text{ for } M_{t_0\mathbf{r_1^1}} = M_p \cup \{(c_0, t_0), (c_1, \mathbf{r_1^1})\} = M^1 \text{ and}$$

$$(15) \qquad b \sim_{M_{\mathbf{r_0^2}t_1}} a \text{ for } M_{\mathbf{r_0^2}t_1} = M_p \cup \{(c_0, \mathbf{r_0^2}), (c_1, t_1)\} = M^2.$$

As another example, result $\mathbf{r_2^8}$ is such that

$$(16) \qquad b \sim_{M_{t_0t_1\mathbf{r_2^8}}} a \text{ for } M_{t_0t_1\mathbf{r_2^8}} = M_p \cup \{(c_0, t_0), (c_1, t_1), (c_2, \mathbf{r_2^8})\} = M^8.$$

By Theorem 2 we have the following implications, where the notation is explained below:

$$(17) \qquad \frac{d_1}{-d_0} = \frac{t_0 - \mathbf{r_0^2}}{\mathbf{r_1^1} - t_1} =: \frac{d_1^1}{d_0^2} \left(M^1 \text{ and } M^2\right);$$

$$(18)\qquad \frac{d_1}{-d_0} = \frac{t_0 - (x + \mathbf{5})}{\mathbf{r_1^1} - r_1^3} =: \frac{d_1^3}{d_0^1}\left(M^1 \text{ and } M^3\right);$$

$$(19)\qquad \frac{d_1}{-d_0} = \frac{t_0 - \mathbf{r_0^7}}{\mathbf{r_1^6} - t_1} =: \frac{d_1^6}{d_0^7}\left(M^6 \text{ and } M^7\right)^{19};$$

$$(20)\qquad \frac{d_2}{-d_0} = \frac{t_0 - \mathbf{r_0^4}}{\mathbf{r_2^5} - t_2} =: \frac{d_2^5}{d_0^4}\left(M^4 \text{ and } M^5\right);$$

$$(21)\qquad \frac{d_2}{-d_0} = \frac{t_0 - \mathbf{r_0^7}}{\mathbf{r_2^8} - t_2} =: \frac{d_2^8}{d_0^7}\left(M^7 \text{ and } M^8\right).$$

In equation (17), $\frac{d_1^1}{d_0^2}$ denotes the statistic that estimates $d_1/(-d_0)$ and that can be derived from M^1 and M^2. Similarly, in equation (18), $\frac{d_1^3}{d_0^1}$ denotes the statistic estimating $d_1/(-d_0)$ that can be derived from M^1 and M^3; and so on.

By equations (7) and (9), the observations in our experiment also reveal the signs of all d_j. Theorem 1 then shows that our measurements reveal all the information that can be obtained about the similarity weights.

C. *Sample and Procedure*

Subjects.—A population of size $N = 53$ (26 female) undergraduate students from Erasmus University Rotterdam with diverse academic backgrounds participated in the experiment. We did not exclude any subject for erratic behavior (although we did treat zero decision weights as missing, as explained in Section IVE). Given the novelty of CBDT, it is unclear whether deviations from the model (such as non-monotonicity in the informational effect of results) can be interpreted as erratic or as valid violations. In general, including all deviating subjects increases the noise in the data and makes tests conservative.

Stimuli.—Table 2 describes the choices faced by the subjects. They were presented as in Figure 2.

Procedure.—The experiment was computer-run in group sessions with three or four subjects per session. After receiving experimental instructions (see Appendix C), subjects answered the experimental questions. Subjects were not allowed to communicate and answered the questions at their own pace. They were asked two training questions to make them familiar with the experimental procedure. Subjects

[19] If the two relevant memories contain two cases, as in equations (17) and (18), and (20), then we can immediately apply Theorem 2. When both memories contain three cases (M^6–M^8), as in equations (19) and (21), we always have one ("common") case that has the same result throughout; e.g., for M^6 and M^7, this is c_2. We then apply Theorem 2 with this common case included in M_p. For example, to obtain equation (19), we take the M_p of Theorem 2 equal to $M_p \cup \{(c_2, t_2)\}$.

indicated their choice by clicking on the button corresponding with their preferred act. On average, the experiment took 20 minutes.

Motivating Subjects.—Each subject received a €3 participation fee at the end of the experiment, plus a performance-contingent payment (equations (12) and (13)) of, on average, €6. If real estate were to depreciate so much that equation (12) or (13) would become negative, despite the extra €3, then subjects did not have to pay. This was unlikely to happen, and indeed did not happen. The performance-contingent payment depended on the choices made and on the real estate prices in the following month. We told subjects that some of their choices involved real (true) data, and that the decision implemented at the end would involve the true data.

To achieve incentive compatibility without deception, we used Bardsley's (2000) conditional information system. There were six choices with true data (in each of M^1, M^2, M^4, M^5, M^6, and M^8), but subjects did not know which or how many choices had real data. At the start of the experiment, each subject received an envelope, which contained one of the memories with true data. The envelope was opened at the end of the experiment and we then implemented the choice the subject had made in the question pertaining to this memory. Implementation meant that we waited until the following month's real estate appreciation had become known, after which the resulting payment was transferred to the subject's bank account. Given that the experimenters are professors at the students' university, that subjects received signed promises, and that all information needed to determine the payment was public, this procedure is trustworthy.

Table 1 shows that our questions were adaptive and an answer to one question might influence the next questions asked. However, the choice implemented for real used the true values. Because subjects' answers could not influence the true values (contained in the envelope that they received before answering questions), it was obvious that strategic behavior was impossible. It was in the subjects' best interests to respond truthfully.

Consistency Checks.—The choices with true data were the same in M^1 and M^2, in M^4 and M^5, and in M^6 and M^8 (see Table 2). We used these three repeated choices to check consistency and to obtain an estimate of the error in subjects' choices.

D. *Predictions of CBDT*

We tested three predictions of CBDT. In each prediction, part (a) is a prediction about the ratio of decision weights and the other parts are predictions about favorability, i.e., about the sign of the decision weights. We will, therefore, refer to Predictions 1a, 2a, and 3a as *ratio* predictions and to the other predictions as *sign predictions*. The sign predictions are based on equations (7) and (9).

The first prediction, which follows from a comparison between equations (17) and (18), tests linearity of utility. If utility is linear, then the two ratios $\frac{d_1^1}{d_0^2}$ and $\frac{d_1^3}{d_0^1}$ are equal. The other conditions of CBDT are less critical in this test because the memories M^1, M^2, and M^3 contain only c_0 and c_1. Prediction 1 would be violated if u in equations (1) and (4) were nonlinear.

PREDICTION 1 OF CBDT (linear utility with respect to d_1): (a) $\frac{d_1^1}{d_0^2} = \frac{d_1^3}{d_0^1}$ (equations (17) and (18)). (b) $\mathbf{r_1^1}$ and $\mathbf{r_1^3}$ imply the same sign (favorability) of d_1.

The next two predictions test CBDT's assumption that cases in memory are separable (GSW, condition A5; GS, condition A2, 66, combination, and many related conditions). They are not affected by the shape of utility in equations (1) and (4), but test the summation operation used there. They would be violated if there were nonadditive interactions, e.g., if a multiplicative term $s(a, c_1)s(a, c_2)r_1 r_2$ were added in equation (4). Prediction 2 follows from a comparison between equations (17) and (19). In memories M^1 and M^2, r_2 is not specified, and in memories M^6 and M^7, we assume $r_2 = t_2$. CBDT's separability condition implies that the tradeoffs between c_1 and c_0 are unaffected by the other cases. Thus, r_2 can be ignored, from which the predicted equality follows.

PREDICTION 2 OF CBDT (Separability with respect to a common r_2): (a) $\frac{d_1^1}{d_0^2} = \frac{d_1^6}{d_0^7}$ (equations (17) and (19)); (b) $\mathbf{r_0^2}$ and $\mathbf{r_0^7}$ imply the same sign of d_0; (c) $\mathbf{r_1^1}$ and $\mathbf{r_1^6}$ imply the same sign of d_1.

Prediction 3 follows from a comparison between equations (20) and (21). Here the common result is r_1 at circumstance c_1. In memories M^4 and M^5, r_1 is not specified. In memories M^7 and M^8, we have $r_1 = t_1$. CBDT''s separability condition implies that r_1 can be ignored, from which the predicted equality follows.

PREDICTION 3 OF CBDT (Separability with respect to a common r_1): (a) $\frac{d_2^5}{d_0^4} = \frac{d_2^8}{d_0^7}$ (equations (20) and (21)); (b) $\mathbf{r_0^4}$ and $\mathbf{r_0^7}$ imply the same sign of d_0; (c) $\mathbf{r_2^5}$ and $\mathbf{r_2^8}$ imply the same sign of d_2.

E. *Analysis*

If a decision weight is (close to) 0, then the subject never changes preference. Unfortunately, such choices may also arise because subjects pay insufficient attention. In both cases, we cannot determine the sign of d_1. In theoretical papers, such difficulties are usually avoided by assuming that all or at least several circumstances are nonneutral. We treated zero values as missing.

Testing ratios is problematic if the denominator can be 0 or can change sign between subjects. In our domain, for example, the ratio $(-1)/(-2)$ cannot be equated with the ratio $1/2$. Fortunately, d_0 was negative for nearly all subjects ($\geq 82\%$ for every measurement of d_0). We could, therefore, test the ratio predictions restricting them to the subjects for whom d_0 was negative. It is plausible that the few positive observations of d_0 were mostly caused by error. The rate of positive weights d_0 was indeed approximately equal to the observed inconsistency rates. Thus, we used the negative d_0s to normalize the other decision weights, avoiding the complexities of analyzing ratios for which the denominator changes sign.

Because the signs of d_1 and d_2 varied more between subjects than those of d_0, we did not use these for normalization purposes. We only compared the absolute strength of d_0 versus d_1 and d_2 by testing whether the absolute values $|d_j/d_0|$ were above or below 1 ($j = 1,2$).

All tests are two-sided. We used sign tests to statistically test ratio predictions.[20] Within-subject (dependent samples) tests were used whenever possible. The rest of this paragraph explains our choice. Sign tests are conservative (have little power) but have the advantage of being applicable to the most general scales. Kolmogorov-Smirnov tests showed that we could not use *t*-tests. Wilcoxon signed-rank tests require comparability of scale differences between subjects. Given the novelty of the sign-dependent ratios of decision weights, little is known about their statistical properties, and the required comparability is questionable. The sign tests had enough power to detect differences in our data. To test the sign predictions of CBDT, we used the usual binomial tests, within subjects whenever possible.

F. *Results: Tests of CBDT*

The three consistency checks gave similar results with an average of 23.8% inconsistencies for all choices, and 15% if zero decision weights were excluded. Such inconsistency rates are common in decision theory (Camerer 1989; Hey and Orme 1994; Abdellaoui 2000; Stott 2006). They are reassuring given that the stimuli for CBDT are more complex than those used in classical decision tasks. Subjects should not only consider the two acts to choose from, but should also consider varying memories containing multiple circumstances (Guerdjikova 2008, 115, l.–3).

Consistency of signs was generally confirmed. The null of random signs could be rejected in favor of consistent signs ($p < 0.01$) in all but one case (d_1^6 in Prediction 2c). In Predictions 1b, 2b, 3b, and 3c the nulls of no sign changes were not rejected (always $p > 0.3$). This null could only be rejected in Prediction 2c ($d_1^1 > d_1^6; p = 0.01$).

For the ratio predictions, Predictions 1a and 3a were not rejected ($p > 0.5$), but Prediction 2a was rejected ($p = 0.007$). That is, linear utility was not rejected, and separability of cases was not rejected when (c_1, t_1) was added to memory, but was rejected when (c_2, t_2) was added to memory.

G. *Results: Explorations Regarding Real Estate Investments*

Our expectations about the signs and the strengths of the decision weights were confirmed by the data. We anticipated that c_2 would be most favorable for a, that c_0 would be most unfavorable for a, and that c_1 would be in between with no clear expectation about the sign of c_1. We indeed observed $d_0 < 0$, $d_2 > 0$, and

[20] The sign test refers to the well-known distribution-free statistical test. It does not refer to testing the signs of decision weights.

$d_2 > d_1\left(\frac{d_2^5}{d_0^4} > \frac{d_1^1}{d_0^2}, p = 0.02; \frac{d_2^8}{d_0^7} > \frac{d_1^6}{d_0^7}, p = 0.05\right)$. The median of the average of $|d_2/d_0|$ divided by $|d_1/d_0|$ was 2.2. As explained, we could not directly compare d_0 with d_1 and d_2, but we could compare the strength of the weights. We found that $|d_1/d_0| < 1 \left(|\frac{d_1^1}{d_0^2}| < 1, |\frac{d_1^3}{d_0^1}| < 1, \text{and } |\frac{d_1^6}{d_0^7}| < 1, \text{all with } p < 0.001\right)$, but $|d_2/d_0| = 1\left(p = 1, \text{both for } |\frac{d_2^5}{d_0^4}| \text{ and } |\frac{d_2^8}{d_0^7}|\right)$. Thus, c_0 and c_2 had similarly strong effects (although in opposite directions) and both had more effect than c_1. The sign of d_1 was positive (although not significant in M^6). Apparently, geographical vicinity (Limburg borders North Brabant) was more important than the difference in the type of dwelling (house versus apartment).

H. *Discussion of Experiment* 1

Only one prediction of CBDT was rejected: separability of cases for d_1 if (c_2, t_2) is added to memory (M^1 versus M^6), in Prediction 2. This violation means that the effect of the price of a house in Limburg depends on the price of an apartment in Utrecht. This is not surprising. In M^1, the only other circumstance in memory is c_0, which is similar to b and favors it. It is then natural to see c_1 as favoring a. In M^6, c_2 is also present in memory. It concerns the same type of dwelling (apartment) as a, making it more questionable whether c_1, which concerns a different type of dwelling (house), should be seen as favoring a. The inclusion of c_2 makes the support of c_1 for a weaker.

The gambles in our experiment mimicked financial investments in the sense that profits were proportional to the market values of real estate. Despite the one violation of CBDT, the measurements of this theory gave useful insights into real estate investment decisions. The preference for a over b was primarily affected by negative results for c_0, a bit less by positive results for c_2,[21] and least by positive results for c_1. For c_1, we learned from the experiment that subjects related it more to a than to b. Using the terminology of GS (78), the similarity of the attribute "geographic" was more important than the similarity of the attribute "dwelling." A limitation to the external validity of these conclusions is, of course, that our experiment mimicked investment decisions, but that our subjects were students in a lab rather than real investors.

V. Experiment 2 to Measure Similarity Weights and to Test CBDT

In Experiment 1, circumstance c_1 was complex to take into account for subjects because type of dwelling and geography had opposite effects. This may have contributed to the observed violation of separability and to inconsistencies in choice. Hence, Experiment 2 used circumstances in memory that were easier to handle. Favorability was always clear, and type of dwelling and geography always had the

[21] Although the absolute values of d_2 and d_0 are similar, the sign of d_2 is more variable, leading to a less pronounced overall effect.

same effect. In addition, there were fewer subjects per session. Both changes reduced noise and increased statistical power. Unfortunately, because this experiment had to be done in the same month as Experiment 1, we could only run a limited number of subjects. We next describe the differences between the two experiments.

A. *Stimuli Measure Similarity Weights*

We used the same acts a and b and the same payment scheme as in the first experiment. The circumstances in memory were:

c_0: house in North Holland;

c_3: apartment in Limburg;

c_4: apartment in Gelderland.

The decision weights d_0, d_3, and d_4 are as in equation (3). Circumstance c_3 more clearly supports a against b than c_1 did in Experiment 1, because, unlike c_1, c_3 concerns the same type of dwelling as a. It is also easier for subjects to compare c_3 and c_4 in the three-circumstance memories (M^{14}, M^{15}, M^{16}), because they only differ geographically and not in type of dwelling. Over the past three years, the real annual price increases of c_0, c_3, and c_4 were 15.9%, 4.1%, and 6.0%, respectively.

Table 3 depicts the design of the second experiment. As in Experiment 1, the results in bold were varied to produce indifference and the results that are not in bold were set equal to their true values.

A difference with Experiment 1 was that in M^{11} we added 5 to r_3^9 rather than to x, to test for linear utility for other differences in results. By Theorem 2, we have the following results, using the same shorthand notation as in Experiment 1:

$$(22) \qquad \frac{d_3}{-d_0} = \frac{t_0 - \mathbf{r_0^{10}}}{\mathbf{r_3^9} - t_3} =: \frac{d_3^9}{d_0^{10}} \ \left(M^9 \text{ and } M^{10}\right);$$

$$(23) \qquad \frac{d_3}{-d_0} = \frac{t_0 - (\mathbf{r_3^9} + \mathbf{5})}{\mathbf{r_3^9} - r_3^{11}} =: \frac{d_3^{11}}{d_0^9} \ \left(M^9 \text{ and } M^{11}\right);$$

$$(24) \qquad \frac{d_3}{-d_0} = \frac{t_0 - \mathbf{r_0^{15}}}{\mathbf{r_3^{14}} - t_3} =: \frac{d_3^{14}}{d_0^{15}} \ \left(M^{14} \text{ and } M^{15}\right);$$

$$(25) \qquad \frac{d_4}{-d_0} = \frac{t_0 - \mathbf{r_0^{12}}}{\mathbf{r_4^{13}} - t_4} =: \frac{d_4^{13}}{d_0^{12}} \ \left(M^{12} \text{ and } M^{13}\right);$$

$$(26) \qquad \frac{d_4}{-d_0} = \frac{t_0 - \mathbf{r_0^{15}}}{\mathbf{r_4^{16}} - t_4} =: \frac{d_4^{16}}{d_0^{15}} \ \left(M^{15} \text{ and } M^{16}\right).$$

Table 3—Overview of Indifferences Obtained by Questions in Experiment 2

	M^9	M^{10}	M^{11}	M^{12}	M^{13}	M^{14}	M^{15}	M^{16}
c_0	t_0	$\mathbf{r_0^{10}}$	r_3^9+5	$\mathbf{r_0^{12}}$	t_0	t_0	$\mathbf{r_0^{15}}$	t_0
c_3	$\mathbf{r_3^9}$	t_3	$\mathbf{r_3^{11}}$			$\mathbf{r_3^{14}}$	t_3	t_3
c_4				t_4	$\mathbf{r_4^{13}}$	t_4	t_4	$\mathbf{r_4^{16}}$

Note: $M^9 = M_{t_0\mathbf{r_3^9}}$; $M^{16} = M_{t_0 t_3 \mathbf{r_4^{16}}}$; and so on.

B. *Sample and Procedure*

Subjects.—A population of size $N = 23$ (12 female) undergraduate students from Erasmus University Rotterdam with diverse academic backgrounds participated in the experiment.

Procedure.—The experiment used individual interviews or sessions involving 2 students. Thus, there were fewer subjects per session than in Experiment 1.

Consistency Checks.—The real choices were the same in M^9 and M^{10}, in M^{12} and M^{13}, and in M^{14} and M^{16}. We used these pairs to test for consistency.

C. *Predictions of CBDT*

We could again derive three predictions of CBDT, one testing for linearity of utility and the other two testing for separability of cases.

PREDICTION 4 OF CBDT (linear utility with respect to d_3): (a) $\frac{d_3^9}{d_0^{10}} = \frac{d_3^{11}}{d_0^9}$ (equations (22) and (23)); (b) $\mathbf{r_3^9}$ and $\mathbf{r_3^{11}}$ imply the same sign of d_3.

PREDICTION 5 OF CBDT (Separability with respect to a common r_4): (a) $\frac{d_3^9}{d_0^{10}} = \frac{d_3^{14}}{d_0^{15}}$ (equations (22) and (24)); (b) $\mathbf{r_0^{10}}$ and $\mathbf{r_0^{15}}$ imply the same sign of d_0; (c) $\mathbf{r_3^9}$ and $\mathbf{r_3^{14}}$ imply the same sign of d_3.

PREDICTION 6 OF CBDT (Separability with respect to a common r_3): (a) $\frac{d_4^{13}}{d_0^{12}} = \frac{d_4^{16}}{d_0^{15}}$ (equations (25)and (26)); (b) $\mathbf{r_0^{12}}$ and $\mathbf{r_0^{15}}$ imply the same sign of d_0; (c) $\mathbf{r_4^{13}}$ and $\mathbf{r_4^{16}}$ imply the same sign of d_4.

D. *Results: Tests of CBDT*

The three consistency tests gave similar results, with an average of 7% inconsistencies overall. This is considerably better than in Experiment 1 and in most other individual choice experiments in the literature.

No prediction of CBDT was rejected. The null of random signs could always be rejected in favor of consistent signs. The sign and ratio predictions of CBDT were never rejected (always $p > 0.15$).

E. *Results: Explorations Regarding Real Estate Investments*

All results in Experiment 2 were as anticipated. We found $d_0 < 0$, $d_3 > 0$, and $d_4 > 0$ $(p < 0.01)$. We found no difference in strength between d_3 and d_4, with $\frac{d_4^{13}}{d_0^{12}} = \frac{d_3^9}{d_0^{10}}$ and $\frac{d_4^{16}}{d_0^{15}} = \frac{d_3^{14}}{d_0^{15}}$ $(p > 0.6)$. We found $|d_3/d_0| < 1$ and $|d_4/d_0| < 1$ $\left(|\frac{d_3^{14}}{d_0^{15}}| < 1, p = 0.02; |\frac{d_4^{16}}{d_0^{15}}| < 1, p = 0.02\right)$, indicating that c_0 provided more support for b than both c_3 and c_4 did for a, although the inequalities were not significant for $\left(|\frac{d_3^9}{d_0^{10}}|\right)$ and $\left(|\frac{d_4^{13}}{d_0^{12}}|\right)$.

F. *Discussion of Experiment* 2

No prediction of CBDT was rejected in the second experiment. This was not due to a lack of power. Even though the number of subjects was less than half the number in Experiment 1, we did observe significant differences. In Experiment 2, we made a special effort to have circumstances in memory with clear predictions, and to reduce noise. This reduced inconsistency by a factor 3.

All findings are plausible. The signs of the decision weights were significantly different from 0, and the strengths (irrespective of direction) of c_3 and c_4 were smaller than that of c_0, with no other significant differences. The equality of d_3 and d_4 suggests that Limburg and Gelderland are equally more similar to North Brabant than to South Holland. The preference of a over b was most affected by negative results for c_0 and in equal measure by positive results for c_3 and c_4.

G. *A Comparison between Experiment* 2 *and Experiment* 1

We compared all ratios d_j/d_0 of Experiment 2 $(j = 3,4)$ with those of Experiment 1 $(j = 1,2)$ and found the following significant differences:

$$d_3 > d_1\left(\frac{d_3^9}{d_0^{10}} > \frac{d_1^1}{d_0^2}, p = 0.02;\ \frac{d_3^{14}}{d_0^{15}} > \frac{d_1^6}{d_0^7}, p = 0.005\right);$$

$$d_4 > d_1\left(\frac{d_4^{16}}{d_0^{15}} > \frac{d_1^6}{d_0^7}, p = 0.005\right).$$

These differences are again plausible. In c_3 and c_4, the type of dwelling was the same as in a, whereas it was different in c_1, and geographical differences with a were identical or comparable for c_1, c_3, and c_4. All other ratios d_j/d_0 did not differ significantly between the two experiments. Again, this was as expected because the differences in type of dwelling and geography were always the same.

Combining the two experiments, we find that the preference for a over b was most affected by negative results for c_0, then by positive results for c_2, then by positive results for c_3 and c_4, and least by positive results for c_1.

VI. General Discussion

Ossadnik, Wilmsmann, and Niemann (2013); Pape and Kurtz (2013); and Grosskopff, Sarin, and Watson (2015) found that CBDT fitted their data better than some alternative theories of choice under ambiguity. Our tests were also favorable for CBDT because we corroborated nearly all of its predictions. The only exception was separability of different cases in memory in the first experiment, which was violated once: The informational value of a house in Limburg (c_1) was affected by that of an apartment in Utrecht (c_2). As pointed out by Gilboa and Schmeidler (1995, 631; 1997, 52), such a violation is similar to the violations of separability over disjoint events (the sure-thing principle, or independence) found for expected utility, and is equally unsurprising. Other violations of separability are discussed by GS (74 and Ch. 19, second-order induction). If separability is violated, cases in prior memory may interact with the cases that we provide. This complicates measurements of CBDT in a similar way as, for instance, background risks complicate experimental risk measurements.

Although our violation of separability occurred under a change in memory size, it reflects an interaction between cases that will also give violations when memories have the same size. This could, for instance, be tested by adding cases with the neutral result (if known) to the smallest memories. Eichberger and Guerdjikova (2013) developed restrictions of separability to memories of equal size and imposed mixture versions of independence for such cases. We conjecture that the interaction of cases that we found will also lead to violations of those weaker axioms of separability.

Esponda and Vespa (2014) showed that it is difficult for subjects to think hypothetically. CBDT avoids the hypothetical thinking of Savage's model. Further, by providing subjects with envelopes containing the real information, we facilitated their thinking conditional upon this true information. Having information tangibly in their hands indeed made it more realistic, underscoring that only one of the pieces of extra information provided was the true information that would be implemented at the end. In these ways we minimize the problems signaled by Esponda and Vespa (2014).

We kept the choice set fixed and measured how similarity weights depended on the circumstances available in memory. For binary choices between acts, this is all that we can observe. Comparisons of the units of decision weights for different pairs of acts are not empirically meaningful. A complex question is whether choices between more than two acts satisfy transitivity for a given CBDT model. Gilboa and Schmeidler developed axioms, primarily diversity axioms that can test/ensure transitivity (GS, 66). We have concentrated on the simplest situation to clarify the novelty of CBDT.

The dependence of utility and decision attitudes on the information provided, or on any other variables, has often been studied. Comparative statics investigate how optima depend on other variables. In these studies, however, utility and decision attitudes are still defined and measured in the traditional manner, referring to variations of available choice alternatives. In our study and in CBDT, the very definition of utilities and decision attitudes refers to new observables.

We have used classical statistical tests, such as sign tests. They account for errors in the data and are robust in the sense that they are correct for virtually all possible error distributions. Unlike econometric analyses and regressions, they do not need commitment to specific error theories.

Finally, we discuss the implications of our results for some alternative versions of CBDT. Special cases of the act similarity version in equation (1) arise if similarity s depends only on the problems p and q (GS, 35; Gilboa and Schmeidler 1995, 610), if s is 0 whenever $e \neq a$ (GS, 38; Gilboa and Schmeidler 1995, 610), or if each q appears at most once in M (GS, 37–38; Gilboa and Schmeidler 1995). Because these are special cases of equation (1), our measurements remain valid. Further generalizations occur when circumstances in memory are not decomposed into problems and acts (to which our analysis applies with no modification), and when similarity can also depend on the result (GS, 52). The latter version is so general that its parameters cannot be measured unless we can use the repetitions approach, where each circumstance c can occur any finite number of times in memory (GS, ch. 3). The datasets in our experiments are not rich enough to apply the repetitions approach.

A final alternative version of CBDT arises if the similarity weights are normalized, leading to an average rather than a (weighted) sum of utility. This approach is appropriate if we decide infinitely often, using the present choice merely to find the long-term highest average. Our paper and experiments consider one-off choices. Then maximizing the sum in equation (1), and not the average, is appropriate (GS, 74, 158 ff.; Pape and Kurtz 2013, 63).

VII. Summary and Conclusion

This paper has introduced a nonparametric method to measure similarity weights, the main new subjective parameters of case-based decision theory (GS, 35). Our method directly shows the relation between the weights and decisions, without imposing any restrictions on either of these two. It makes CBDT directly observable and, thus, directly falsifiable. We assumed linear utility, which is reasonable for the moderate amounts used in our experiments and which was not rejected in our statistical tests. An extension to nonlinear utility is in Appendix A.

Our measurement method works as follows. If a preference for a over b can be turned into an indifference by increasing the result under c_i by €α, and also by increasing, instead, the result under c_j by €β, then the ratio d_i/d_j of decision weights is equal to β/a. It is the case-based analog of de Finetti's betting odds for nonparametrically measuring subjective probabilities.

Decision weights are differences of similarity weights and only their ratios and signs can be observed. CBDT generally requires information about several variables (the neutral utility level and the cases in memory prior to the experiment), which may be hard to obtain. Our method does not need this information because these variables drop from the equations.

We developed a case-based analog of the random incentive system. Thus, we could vary memories in an incentive-compatible manner without requiring subjects to forget previously provided information. We resolved the statistical complication of testing ratios with changing signs by separating sign and ratio predictions. Two experiments showed that our method is implementable. Case-based decision theory was generally supported by our data, although generalizations of separability of cases in memory may be desirable.

Case-based decision theory is a viable alternative to classical revealed preference. We implemented it in an experiment on real estate investments, in which no objective probabilities were available and in which no Savage state space could be specified. Yet CBDT could readily be applied. In our experiments, the findings of CBDT agree with intuitive judgments when they are available (based on Dutch geographic conditions), and give new insights when they are not. Our measurement method is easy to implement and transparently shows the empirical meaning of similarity weights, the main new parameters of CBDT.

Appendix

A. *Extension to Unknown (Nonlinear) Utility*

GSW proposed two methods for measuring utility. One (their section 3) adopts the repetitions approach, where circumstances can be repeated and thus weighted differently.[22] Since we do not have such data available in this study, we do not discuss it further. For the second method, we consider variations of results of two fixed circumstances in memory, an assumption made in the rest of this section. We assume $M_e = \{(c_0, t_0), (c_1, t_1)\}$, so that $M_{\alpha_0,\sigma_1} = \{(c_0, \alpha_0), (c_1, \sigma_1)\}$, and so on. Under some nondegeneracy assumptions, indifferences

$$\text{(A1)} \qquad a \sim_{M_{\alpha_0\sigma_1}} b, a \sim_{M_{\beta_0\tau_1}} b, a \sim_{M_{\gamma_0\sigma_1}} b, a \sim_{M_{\delta_0\tau_1}} b,$$

imply

$$\text{(A2)} \qquad u(\alpha_0) - u(\beta_0) = u(\gamma_0) - u(\delta_0).$$

This can be derived from equation (1), as was demonstrated by GSW (equation (6)). The indifferences in equation (A1) are such that all unknowns in the equations, such as decision/similarity weights and the terms referring to M_p, drop. Under usual richness assumptions, equalities of utility differences suffice to measure utility u up to level and unit. To completely measure utility, we should also determine its level (i.e., where utility is 0), which can be inferred by verifying the neutrality condition. However, this is not necessary for the measurement of similarity weights.

[22] Another distribution-based way of weighting cases unequally is proposed in similarity based forecasting (Lovallo, Clarke, and Camerer 2012).

B. *Proof*

THEOREM B1: *If we drop the assumption of linear utility in Theorem* 2, *then we get*, *instead of equation* (6),

$$\text{(B1)} \qquad \frac{d_1}{d_0} = \frac{u(\mathbf{r_0'}) - u(t_0)}{u(\mathbf{r_1'}) - u(t_1)}.$$

The results in equations (7)–(10) *remain valid if all values* r_j *and* r_j' *in the inequalities are replaced by their utility values. If utility is strictly increasing, they remain valid without any change.*

PROOF:

The first indifference in equation (5) implies

$$s(a, c_0)u(\mathbf{r_0'}) + s(a, c_1)u(t_1) + \sum\nolimits_{(c,r)\in M_p} s(a,c)u(r) =$$

$$s(b, c_0)u(\mathbf{r_0'}) + s(b, c_1)u(t_1) + \sum\nolimits_{(c,r)\in M_p} s(b,c)u(r).$$

Substituting equation (3) and writing $K = \sum_{(c,r)\in M_p} s(a,c)u(r) - \sum_{(c,r)\in M_p} s(b,c)u(r)$, we get

$$\text{(B2)} \qquad d_0 u(\mathbf{r_0'}) + d_1 u(t_1) + K = 0.$$

The second indifference in equation (5) similarly implies

$$\text{(B3)} \qquad d_0 u(t_0) + d_1 u(\mathbf{r_1'}) + K = 0.$$

Subtracting equation (B3) from equation (B2) gives

$$\text{(B4)} \qquad d_0(u(\mathbf{r_0'}) - u(t_0)) - d_1(u(\mathbf{r_1'}) - u(t_1)) = 0.$$

This implies equation (B1), where both numerators and denominators are nonzero because of equation (5). The results on the signs of d_0 and d_1 follow from equation (1). ∎

C. *Instructions for Subjects*

You are participating in an experiment on decision making. During the experiment, we will provide you with information about the development of prices of some real estate types in various regions of the Netherlands over the last three years. Based on the information presented in each of the questions, you have to choose between two types of property to gamble on: either a single-family house (in Dutch: eengezinswoning) in the province of Zuid-Holland[23] or an apartment (in Dutch: appartement) in the province of Noord Brabant.

You have just chosen a computer. Next to it, you will find an envelope. This envelope will be opened by the experimenter at the end of the experiment. It contains true information about the development of real estate prices in the Netherlands between 2005 and 2008 (from the Dutch Cadaster Index). It also offers you a choice between two types of property to gamble on. You will face this particular choice at some point during the experiment. You will receive a payment that is based on your choice during the experiment and on the actual development of real estate prices (houses or apartments) in the month of the experiment (March 2009). Please note that neither you nor the experimenters can know at this moment how the housing market will evolve in this month and, therefore, what your payment will be in a month.

Because we are interested in your choices in many situations, we will ask you several questions. In each question, you receive a piece of information and you will be asked to make a what-would-you-gamble-on-if choice, based on this piece of information. Often this information is hypothetical and made up by us. But, as mentioned before, one of the questions asked during the experiment is the one contained in the envelope and it is based on real data.

For each question, only the piece of information provided there is relevant, and you best make your choice assuming that this is the only piece of information you have. After all, if this question turns out to be the one contained in your envelope, then all the pieces of information provided in the other questions are not true and are irrelevant for the payment you will receive. That is, it is best for you, at each question, to forget all information provided in previous questions, and to focus only on the information provided at that question.

Your payment is determined as follows. In addition to a show-up fee of €3, you will get another €3 + 10 times the monthly appreciation rate (in percentage) of the property you chose in the question contained in the envelope. In case the property value will decrease, we will subtract it from the additional €3, but not from the original show-up fee. You will always receive at least the show-up fee. Please bear in mind that despite the slowdown of the economy, real estate prices in the Netherlands have still increased during the last months.

Example: For example, assume that you choose an apartment in Zuid-Holland in the real question. If the prices of apartments in Zuid-Holland go up by 0.6% in March, you will receive €(3 + 0.6% × 10) = €3 + €6 = €9, plus the original show-up fee of €3. If the prices go down by 0.2%, you will receive €(3 − 0.2% × 10) = €3 − €2 = €1 for your investment, plus the original show-up fee of €3. If the prices go down by 0.3% or more, you will receive €0 for your investment, but you still keep the show-up fee of €3.

Once the Cadaster makes the information about the real estate prices in March public (in April 2009), we will inform you by e-mail and either deposit the money in your bank account, or you can collect it at the office of the experimenter, L3-121.

[23] Here, as throughout the instructions, we used the Dutch names of provinces.

REFERENCES

Abdellaoui, Mohammed. 2000. "Parameter-Free Elicitation of Utility and Probability Weighting Functions." *Management Science* 46 (11): 1497–1512.

Aha, David W., Cindy Marling, and Ian Watson, ed. 2005. *Knowledge Engineering Review* 20 (3). Cambridge: Cambridge University Press.

Anscombe, F. J., and R. J. Aumann. 1963. "A Definition of Subjective Probability." *Annals of Mathematical Statistics* 34 (1): 199–205.

Barberis, Nicholas C. 2013. "Thirty Years of Prospect Theory in Economics: A Review and Assessment." *Journal of Economic Perspectives* 27 (1): 173–96.

Bardsley, Nicholas. 2000. "Control without deception: Individual behaviour in free-riding experiments revisited." *Experimental Economics* 3 (3): 215–40.

Bernard, Carole, and Mario Ghossoub. 2010. "Static portfolio choice under Cumulative Prospect Theory." *Mathematics and Financial Economics* 2 (4): 277–306.

Birnbaum, Michael H. 2008. "New paradoxes of risky decision making." *Psychological Review* 115 (2): 463–501.

Bleichrodt, Han, Martin Filko, Amit Kothiyal, and Peter P. Wakker. 2017. "Making Case-Based Decision Theory Directly Observable: Dataset." *American Economic Journal: Microeconomics*. https://doi.org/10.1257/mic.20150172.

Blonski, Matthias. 1999. "Social learning with case-based decisions." *Journal of Economic Behavior and Organization* 38 (1): 59–77.

Camerer, Colin F. 1989. "An experimental test of several generalized utility theories." *Journal of Risk and Uncertainty* 2 (1): 61–104.

de Finetti, Bruno. 1931. "Sul Significato Soggettivo della Probabilità." *Fundamenta Mathematicae* 17 (1): 298–329. Mara Khale, trans. 1993. "On the Subjective Meaning of Probability." In *Probabilità e Induzione*, edited by Paola Monari and Daniela Cocchi, 291–321. Bologna: Clueb.

Dubois, Didier, Lluís Godo, Henri Prade, and Adriana Zapico. 1999. "On the Possibilistic Decision Model: From Decision under Uncertainty to Case-Based Decision." *International Journal of Uncertainty, Fuzziness and Knowledge-Based Systems* 7 (6): 631–70.

Eichberger, Jürgen, and Ani Guerdjikova. 2013. "Ambiguity, data and preferences for information—A case-based approach." *Journal of Economic Theory* 148 (4): 1433–62.

Epper, Thomas, Helga Fehr-Duda, and Adrian Bruhin. 2011. "Viewing the future through a warped lens: Why uncertainty generates hyperbolic discounting." *Journal of Risk and Uncertainty* 43 (3): 163–203.

Esponda, Ignacio, and Emanuel Vespa. 2014. "Hypothetical Thinking and Information Extraction in the Laboratory." *American Economic Journal: Microeconomics* 6 (4): 180–202.

Farias, Vivek F., Srikanth Jagabathula, and Devavrat Shah. 2013. "A Nonparametric Approach to Modeling Choice with Limited Data." *Management Science* 59 (2): 305–22.

Gayer, Gabrielle, Itzhak Gilboa, and Offer Lieberman. 2007. "Rule-Based and Case-Based Reasoning in Housing Prices." *B.E. Journal of Theoretical Economics* 7 (1).

Gilboa, Itzhak, Offer Lieberman, and David Schmeidler. 2011. "A similarity-based approach to prediction." *Journal of Econometrics* 162 (1): 124–31.

Gilboa, Itzhak, and David Schmeidler. 1995. "Case-Based Decision Theory." *Quarterly Journal of Economics* 110 (3): 605–39.

Gilboa, Itzhak, and David Schmeidler. 1996. "Case-Based Optimization." *Games and Economic Behavior* 15 (1): 1–26.

Gilboa, Itzhak, and David Schmeidler. 1997. "Act similarity in case-based decision theory." *Economic Theory* 9 (1): 47–61.

Gilboa, Itzhak, and David Schmeidler. 2001. *A Theory of Case-Based Decisions.* Cambridge: Cambridge University Press.

Gilboa, Itzhak, David Schmeidler, and Peter P. Wakker. 2002. "Utility in Case-Based Decision Theory." *Journal of Economic Theory* 105 (2): 483–502.

Golosnoy, Vasyl, and Yarema Okhrin. 2008. "General uncertainty in portfolio selection: A case-based decision approach." *Journal of Economic Behavior and Organization* 67 (3–4): 718–34.

Greco, Salvatore, Benedetto Matarazzo, and Roman Słowiński. 2008. "Case-Based Reasoning Using Gradual Rules Induced from Dominance-Based Rough Approximations." In *Rough Sets and Knowledge Technology.* Lecture Notes in Computer Science, edited by Guoyin Wang, Tianrui Li, Jerzy W. Grzymala-Busse, et al., 268–75. Heidelberg: Springer.

Groot Koerkamp, Bas, M. G. Myriam Hunink, Theo Stijnen, James K. Hammitt, Karen M. Kuntz, and Milton C. Weinstein. 2007. "Limitations of Acceptability Curves for Presenting Uncertainty in Cost-Effectiveness Analysis." *Medical Decision Making* 27 (2): 101–11.

Grosskopf, Brit, Rajiv Sarin, and Elizabeth Watson. 2015. "An experiment on case-based decision making." *Theory and Decision* 79 (4): 639–66.

Guerdjikova, Ani. 2008. "Case-based learning with different similarity functions." *Games and Economic Behavior* 63 (1): 107–32.

Hertwig, Ralph, Greg Barron, Elke U. Weber, and Ido Erev. 2004. "Decisions from Experience and the Effect of Rare Events in Risky Choice." *Psychological Science* 15 (8): 534–39.

Hey, John D., and Chris Orme. 1994. "Investigating Generalizations of Expected Utility Theory Using Experimental Data." *Econometrica* 62 (6): 1291–1326.

Holt, Charles A., and Susan K. Laury. 2002. "Risk Aversion and Incentive Effects." *American Economic Review* 92 (5): 1644–55.

Hüllermeier, Eyke. 2007. *Case-Based Approximate Reasoning.* Vol. 44, *Theory and Decision Library*. Heidelberg: Springer.

Jahnke, Hermann, Anne Chwolka, and Dirk Simons. 2005. "Coordinating Service-Sensitive Demand and Capacity by Adaptive Decision Making: An Application of Case-Based Decision Theory." *Decision Sciences* 36 (1): 1–32.

Keeney, Ralph L., and Howard Raiffa. 1976. *Decisions with Multiple Objectives: Preferences and Value Tradeoffs.* Hoboken, NJ: John Wiley and Sons.

Lovallo, Dan, Carmina Clarke, and Colin Camerer. 2012. "Robust analogizing and the outside view: Two empirical tests of case-based decision making." *Strategic Management Journal* 33 (5): 496–512.

Luce, R. Duncan. 2000. *Utility of Gains and Losses: Measurement-Theoretical and Experimental Approaches.* Scientific Psychology Series. Mahwah, NJ: Lawrence Erlbaum Associates Publishers.

Ossadnik, Wolfgang, Dirk Wilmsmann, and Benedikt Niemann. 2013. "Experimental evidence on case-based decision theory." *Theory and Decision* 75 (2): 211–32.

Pape, Andreas Duus, and Kenneth J. Kurtz. 2013. "Evaluating case-based decision theory: Predicting empirical patterns of human classification learning." *Games and Economic Behavior* 82: 52–65.

Pęski, Marcin. 2011. "Prior symmetry, similarity-based reasoning, and endogenous categorization." *Journal of Economic Theory* 146 (1): 111–40.

Pigou, A. C., Milton Friedman, and N. Georgescu-Roegen. 1936. "Marginal Utility of Money and Elasticities of Demand." *Quarterly Journal of Economics* 50 (3): 532–39.

Rabin, Matthew. 2000. "Risk Aversion and Expected-utility Theory: A Calibration Theorem." *Econometrica* 68 (5): 1281–92.

Ramsey, Frank Plumpton. 1931. "Truth and Probability." In *The Foundations of Mathematics and other Logical Essays,* edited by R. B. Braithwaite, 156–98. New York: Routledge and Kegan Paul.

Riesbeck, Christopher K., and Roger C. Schank. 1989. *Inside Case-Based Reasoning*. Hillsdale, NJ: Lawrence Erlbaum Associates Publishers.

Savage, Leonard J. 1954. *The Foundations of Statistics.* Hoboken, NJ: John Wiley and Sons.

Starmer, Chris, and Robert Sugden. 1991. "Does the Random-Lottery Incentive System Elicit True Preferences? An Experimental Investigation." *American Economic Review* 81 (4): 971–78.

Stewart, Neil, Nick Chater, and Gordon D. A. Brown. 2006. "Decision by Sampling." *Cognitive Psychology* 53 (1): 1–26.

Stott, Henry P. 2006. "Cumulative prospect theory's functional menagerie." *Journal of Risk and Uncertainty* 32 (2): 101–30.

Tversky, Amos, and Itamar Gati. 1982. "Similarity, separability and the triangle inequality." *Psychological Review* 89 (2): 123–54.

Tversky, Amos, and Daniel Kahneman. 1992. "Advances in prospect theory: Cumulative representation of uncertainty." *Journal of Risk and Uncertainty* 5 (4): 297–323.