Judgment and Decision Making, Vol. 5, No. 4, July 2010, pp. 272-284

Precise models deserve precise measures: A methodological dissection

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Abstract

The recognition heuristic (RH) — which predicts non-compensatory reliance on recognition in comparative judgments — has attracted much research and some disagreement, at times. Most studies have dealt with whether or under which conditions the RH is truly used in paired-comparisons. However, even though the RH is a precise descriptive model, there has been less attention concerning the precision of the methods applied to measure RH-use. In the current work, I provide an overview of different measures of RH-use tailored to the paradigm of natural recognition which has emerged as a preferred way of studying the RH. The measures are compared with respect to different criteria — with particular emphasis on how well they uncover true use of the RH. To this end, both simulations and a re-analysis of empirical data are presented. The results indicate that the adherence rate — which has been pervasively applied to measure RH-use — is a severely biased measure. As an alternative, a recently developed formal measurement model emerges as the recommended candidate for assessment of RH-use.

Keywords: recognition heuristic, methodology, simulation, adherence rate, signal detection theory, multinomial processing tree model.

1 Introduction

In the past decade since it was baptized, the recognition heuristic (RH; Goldstein & Gigerenzer, 1999, 2002) has inspired much innovative research. It has been studied extensively from a normative and descriptive point of view and provoked some controversial debate at times. Many other interesting investigations notwithstanding, the majority of empirical studies has dealt with the descriptive question of whether and to what extent the recognition cue is considered in isolation — that is, how often the RH is actually *used*. Whereas some have aimed to show that this is rarely the case altogether (e.g., Bröder & Eichler, 2006; Newell & Shanks, 2004; Oppenheimer, 2003; Richter & Späth, 2006), others have concentrated on the bounding conditions or determinants of RH-use (e.g., Hilbig, Scholl, & Pohl, 2010; Newell & Fernandez, 2006; Pachur & Hertwig, 2006; Pohl, 2006), possible individual differences (Hilbig, 2008a; Pachur, Bröder, & Marewski, 2008), and tests of alternative cognitive process models (Glöckner & Bröder, in press; Hilbig & Pohl, 2009; Marewski, Gaissmaier, Schooler, Goldstein, & Gigerenzer, 2010).

Clearly, the RH is a precise model which makes exact predictions about choices and underlying processes. However, to gain insight about whether and under which conditions these predictions are actually correct, measurement must also be precise. Although many agree that it is a promising and fruitful research strategy to uncover the situational and individual determinants of fast-and-frugal heuristics (Bröder, in press), it is, as yet, much less clear *how* to study and measure RH-use. What may, at first glance, appear to be a rather trivial question, turns out to represent a substantial challenge and, in my view, source of much of the controversy surrounding the RH.

So far, emphasis has been put on which paradigms and materials are appropriate for studying the RH. Indeed, Pachur et al. (2008) provided an extensive discussion of such questions. They suggested no less than eight critical methodological necessities which an adequate investigation or test of the RH should, in their view, comprise.[1] Also, they reviewed the extant literature and argued that many previously published studies yield drawbacks with respect to these eight points (Pachur et al., 2008, Table 1). However, even if their list of studies with problematic features had not been somewhat incomplete,[2] it does bear the dilemma that the proposed necessities, if taken

*I thank Jon Baron and an anonymous reviewer for many helpful comments and suggestions. Also, thanks are due to Andreas Glöckner and Arndt Bröder for providing the raw data of their experiment. Address: Benjamin E. Hilbig, Psychology III, University of Mannheim, Schloss Ehrenhof Ost, D-68131 Mannheim, Germany. Email: hilbig@psychologie.uni-mannheim.de.

[1] These necessities include using naturally recognized objects, not providing any cues, excluding criterion knowledge, requiring inferences from memory (not from given information), sufficiently high recognition validity, and not making cues available for unknown objects.

[2] For example, the problem of induced cue knowledge also pertains to Goldstein and Gigerenzer (2002, Exp. 2); likewise, the caveat of low recognition validity also applies to Pachur and Hertwig (2006).

seriously, leave a rather small niche for empirical investigations of the RH, and, worse yet, severe problems when attempting to measure RH-use. I will sketch this problem in what follows.

As a central point, Pachur et al. (2008) argue that the RH is more likely to be used when objects are naturally recognized and cues must be retrieved from memory. This is in line with the assumption that inferences from memory are more often based on simple heuristics, an assumption that has received support in the past (Bröder & Newell, 2008). The central argument favoring naturally recognized objects is that the RH hinges on decision makers acquiring the recognition-criterion-relation through experience and thus learning to trust on recognition when appropriate. Those who — like myself — buy into such arguments, which rule out teaching participants artificial objects or providing them with cues, are faced with a severe obstacle: how to *measure* use of the RH when there is no control over participants' cue knowledge?

Assume a participant is faced with the judgment which of two cities is larger and recognizes one but not the other. If she provides the judgment that the recognized object has the higher criterion value, a choice in line with the RH is produced. However, such cases of *adherence* cannot imply that recognition was considered in isolation and thus do not provide information about use of the RH. More generally, a participant may have adhered to the prediction of the cue in question by actually considering some entirely different piece of information that points in the same direction (Hilbig, in press). In the case of comparing a recognized with an unrecognized city, for example, a decision maker may have chosen the recognized city based on the knowledge that this city has an international airport, a large university, or the like. Thus, so long as there is no control over participants' further knowledge in specific paired-comparisons, adherence to the prediction of the RH is non-diagnostic. Or, as Bröder and Schiffer (2003) put it, "... simple counting of choices compatible with a model tells us almost nothing about the underlying strategy" (p 197).

The best remedy for this caveat is, of course, to unconfound recognition and further knowledge: If participants are taught certain objects and cue patterns — as is typically done when studying other fast-and-frugal heuristics (e.g., Bröder & Schiffer, 2006) and alternative approaches (Glöckner & Betsch, 2008) — the experimenter has full control and can investigate whether additional cues alter the degree to which participants adhere to the RH (Bröder & Eichler, 2006). Indeed, unconfounding different cues is vital when considering the adherence to simple one-cue strategies (Hilbig, 2008b). Moreover, full experimental control over cue patterns allows for the application of sophisticated methods for strategy classification: Bröder and Schiffer (2003) proposed to bridge the gap between theories of multi-attribute decision making and empirically observed choices by means of a formal measurement model. This Bayesian approach provides information about the decision strategy that most likely generated a data vector. Recently, this approach has been extended to considering choice outcomes, response latencies, and confidence ratings (Glöckner, 2009; Jekel, Nicklisch, & Glöckner, 2010). However, both these elegant approaches necessitate teaching or providing all cue patterns for a set of artificial objects, so as to discriminate between different strategies. Clearly, this is at odds with the central methodological recommendations of Pachur et al. (2008) who call for using naturally recognized objects without teaching or providing any further information.

Overall, in the paradigm most favored by Pachur and colleagues (see also Pachur & Hertwig, 2006), only three pieces of information are available on which researchers must base the assessment whether the RH was used: (i) which objects were presented in a given trial (including their true position on the criterion dimension), (ii) which of these objects is recognized by the participant, and (iii) which object is chosen, that is, which is judged to have the higher criterion value. How, based on these pieces of information, can we measure RH-use? So far, three classes of measures have been applied, viz. the adherence rate, enhanced measures based on adherence rates, and a formal measurement model. In what follows, I will introduce these measures, briefly discuss their theoretical advantages and limitations, and present simulations and a re-analysis of existing empirical data to evaluate them.

2 Measures of RH-use

In the quest for an optimal measure of RH-use, I will focus on three criteria. First, the measure must be applicable to data generated in the paradigm of natural recognition outlined above. Unlike elegant maximum-likelihood strategy-classification methods (Bröder & Schiffer, 2003; Glöckner, 2009), it must not afford full experimental control over objects and cue patterns — since proponents of the RH have called for natural recognition and knowledge (Pachur et al., 2008). All measures described in what follows comply with this requirement. Second, measures should provide a readily interpretable statistic that would optimally denote the probability of using the RH and thus also allow for direct interpretation of, say, differences between experimental conditions. This holds only for some of the measures discussed below; however, the desired information can also be gained from those measures which do not immediately provide it — at least if one is willing to make some additional assumptions. Third, and most importantly, an appropriate measure should of course be able to reliably uncover the true probability of RH use (or proportion of RH-users in a sample) without strong bias. At a minimum, a useful measure must provide estimates that are a monotonic function of the true probability of RH-use; otherwise one cannot even interpret differences in estimated values conclusively as "more" or "less". This third point (unbiased estimation) will be the central criterion against which the different measures are appraised.

Before the different measures are described in more detail, two important theoretical points should be stressed: First, none of these measures specifies an alternative process to the RH. That is, they do not entail any assumptions about what exactly decision makers are doing when they do not use the RH. Consequently, these measures cannot inform us about which alternative strategies decision makers rely on whenever they do not use the RH. Plausible candidates may be different weighted additive models, equal weights strategies, other heuristics, or mere guessing (Bröder & Schiffer, 2003; Glöckner, 2009). On the one hand, it is unfortunate that the available measures are uninformative concerning alternative processes. On the other hand, this can also be an advantage because the results do not depend on which alternative strategies are tested. For example, in comparing different models, Marewski et al. (2010) come to the conclusion that no model outperforms the RH in explaining choice data, whereas Glöckner and Bröder (2010) arrive at the exact opposite; this apparent incompatibility is — at least in part — driven by the fact that very different alternative models were investigated in each of these works.

A second important point concerns recognition memory. Essentially, all measures rely on participants' reports of which objects they do or do not recognize. Like in the RH theory, recognition is treated as "a binary, all-or-none distinction" and does thus "not address comparisons between items in memory, but rather the difference between items in and out of memory" (Goldstein & Gigerenzer, 2002, p. 77). The RH and the measures of RH-use considered herein operate on recognition judgments as the output of what is usually termed "recognition" in memory research. Admittedly, considering recognition to be binary is an oversimplification (Newell & Fernandez, 2006). However, as yet, measures of RH-use that explicitly model recognition memory processes are not available — though promising starting points based on threshold-models of recognition memory have recently been developed (Erdfelder, Küpper-Tetzel, & Mattern, 2010).

2.1 Adherence rates

The vast majority of studies on the RH have trusted in the adherence rate as a measure of RH-use. For each participant, the number of cases in which the RH could be applied (cases in which exactly one object is recognized) is computed. Then, the proportion of these cases in which the participant followed the prediction of the RH is assessed, thus representing the adherence (or accordance) rate. As an advantage, the adherence rate can be understood as a proportion, ranging from 0 to 1. Thus, both on the individual and on the aggregate level (taking the mean across all participants), the adherence rate can be interpreted as the probability of RH-use. As discussed above, this also avails direct interpretability of differences between experimental conditions. On the individual level, one could classify participants as RH-users if they have an adherence rate of 1 — or close to 1 if one allows for strategy execution errors. However, in the latter case, one must select some value close to 1 arbitrarily, given that the error probability is unknown.

More problematically, as hinted in the introduction, the adherence rate will rarely provide an unbiased estimate of RH-use. Indeed, a consistent non-user of the RH could produce an adherence rate of 1, if she always considered additional cues which point toward the recognized option. So, the central disadvantage of the adherence rate is the confound between recognition and further knowledge. As an effect, the adherence rate will mostly be biased towards the RH, that is, it will typically overestimate the probability of RH-use. In fact, it will overestimate the use of any one-cue heuristic if there is no control over other cues and knowledge (Hilbig, in press). The simulation reported below will shed further light on the severity of this limitation.

2.2 Measures derived from Signal Detection Theory

To gain more insight about RH-use, Pachur and Hertwig (2006) proposed to view the comparative judgment task from the perspective of Signal Detection Theory (SDT; for an introduction see Macmillan & Creelman, 2005). Specifically, given that one object is recognized and the other is not, choice of the recognized object can either represent a correct or a false inference with respect to the judgment criterion (see Pohl, 2006). Thus, following recognition when this is correct would represent a *hit* in terms of SDT. By contrast, if choice of the recognized object implies a false inference, this would be denoted a *false alarm*. Thus, the SDT parameters d' and c can be computed individually for each participant (Pachur, Mata, & Schooler, 2009, Appendix A):

$$d' = z(H) - z(FA) \tag{1}$$

and

$$c = -\frac{z(H) + z(FA)}{2} \tag{2}$$

where $z(H)$ is the z-transformed hit rate (probability of following the recognition cue, given that this is correct) and $z(FA)$ denotes the z-transformed false alarm rate (probability of following the recognition cue, given that this is false). The former, d', denotes a participant's ability to discriminate cases in which recognition yields a correct versus false inference. The latter, c, is the response bias or the tendency to follow the recognition cue (independent of one's ability).

Clearly, both d' and c provide information beyond the mere adherence rate. For example, a participant with a large d' cannot have considered recognition in isolation. Unlike the adherence rate, however, neither d' nor c can readily be interpreted as the probability of RH-use. As d' is the difference between the z-transformed hit and false alarm rates, it allows for only one clear numerical prediction: a true user of the RH cannot show any discrimination (as she always follows recognition and ignores all further information), that is, she must score $d' = 0$ or close to zero if strategy execution errors are assumed. However, the size of d' is difficult to interpret: How much more often did a participants use the RH if she scores $d' = .50$ versus $d' = 1.2$? The same principally holds for c.

So, to obtain an overall probability of RH-use from these measures, one must make some assumptions which value true users of the RH will achieve. Specifically, as stated above, a true RH-user must score d' = 0. Thus, one can compute for how many participants this holds. However, with an unknown rate of strategy execution error, it is hard to determine which interval around zero would be appropriate to still classify a participant as a RH-user. For c, the limitation is even greater: clearly, a RH-user must have a tendency to follow recognition (and thus $c < 0$, using Formula 2). However, how strongly below zero must c be for a user of the RH?

2.3 The discrimination index

A measure similar to Pachur and Hertwig's (2006) d' is the discrimination index (DI), an individual proxy indicating whether a participant may be a user of the RH (Hilbig & Pohl, 2008). Formally, the DI is computed as the difference in adherence rates in all cases in which recognition implies a correct versus a false judgment, given that it discriminates between choice options, that is:

$$DI = (H) - (FA) \tag{3}$$

where (H) is the hit rate and (FA) denotes the false alarm rate in accordance with Pachur and Hertwig (see above). As such, the basic logic is the same as for d': Any true user of the RH must score DI = 0, as she cannot discriminate whether the RH yields a correct vs. false judgment on a given trial. However, the DI differs in two respects from d' as proposed by Pachur and colleagues: First, on a theoretical level, the DI does not refer to SDT. As such, it is not based on any of the according theoretical assumptions. For example, it remains unclear what the underlying dimension or decision axis from SDT (i.e., signal strength) would be in the case of comparing pairs of cities with respect to their population. Secondly, and more practically, the DI and d' differ in that the DI does not comprise z-transformation of hit and false alarm rates.

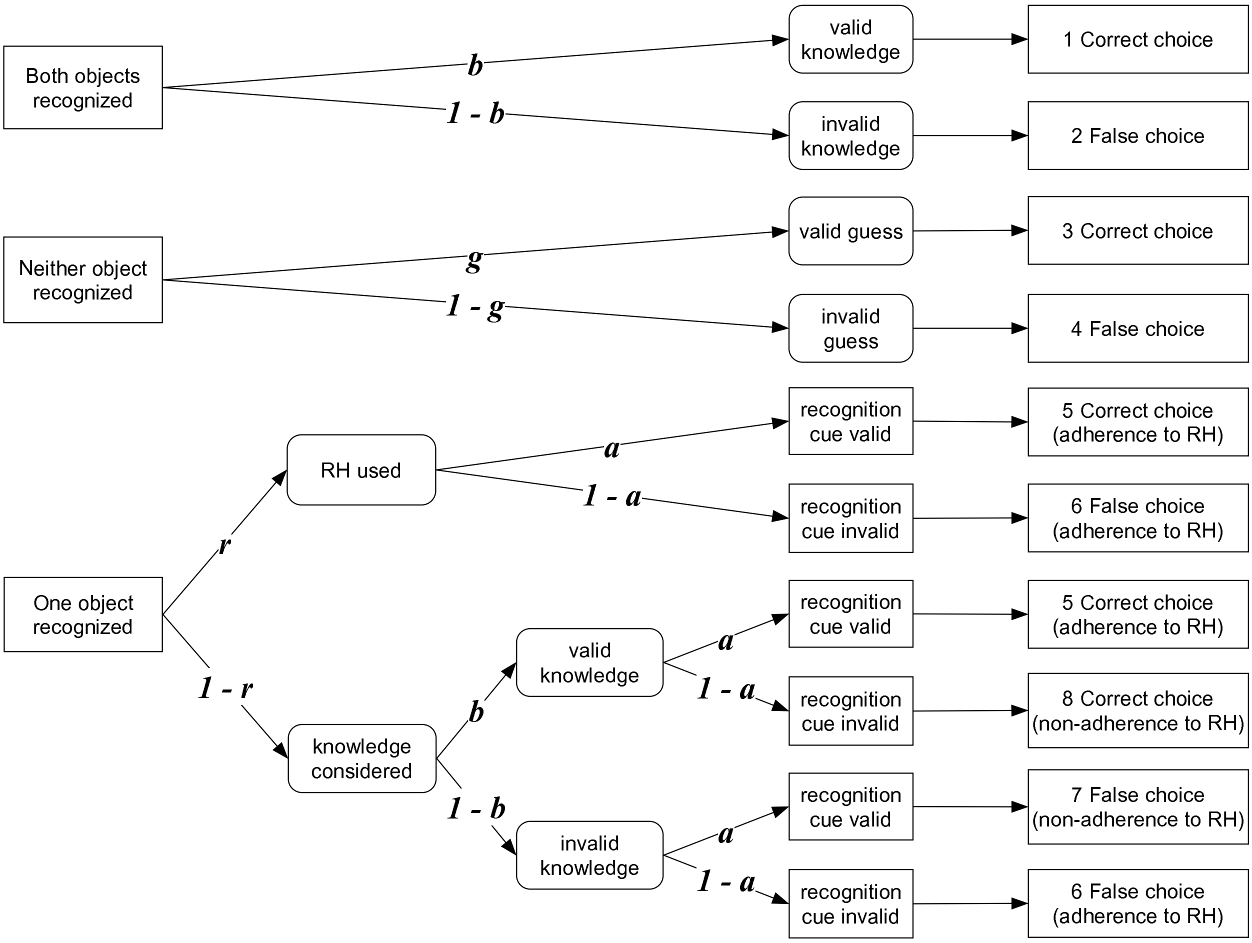
Just like the measures derived from Signal Detection Theory, the DI cannot be interpreted as the probability of RH-use. Instead, as holds for d', this probability must be approximated by classifying those participants as RH-users who score DI = 0 (or, again, close to zero when allowing for strategy execution errors). So, in this respect, the DI shares the disadvantages of d' and c.

2.4 The r-model

In a recent attempt to overcome the limitations of existing measures of RH-use, we developed a formal measurement model for comparative judgments (Hilbig, Erdfelder, & Pohl, 2010). This multinomial processing tree model (Batchelder & Riefer, 1999; Erdfelder et al., 2009), named r-model, comprises a parameter which specifically denotes the probability of RH-use without suffering from the confound between recognition and knowledge. As is displayed in Figure 1, the aggregate frequencies of eight observable outcome categories are explained through four latent parameters representing processes or states. The parameters *a* and *b* exactly mirror what Goldstein and Gigerenzer (2002) call the recognition and knowledge validity, respectively: The former denotes the probability with which a recognized object has a higher criterion value than an unrecognized object. The latter denotes the probability of retrieving and considering valid knowledge. The parameter *g* merely denotes the probability of guessing correctly. Most importantly, the parameter *r* stands for the probability of using the RH, that is, following recognition while ignoring all further information and knowledge. By contrast, with probability 1–*r* one's judgment is not based on recognition alone (though, as hinted above, the model does not make any assumptions about which alternative process may be at work).

As is typically the case for parameters in multinomial models (Erdfelder et al., 2009), *r* denotes a probability and thus represents a readily interpretable measure of RH-use in much the same way as the adherence rate. Additionally, and unlike any of the other measures introduced above, the r-model allows for goodness-of-fit tests. Specifically, since there are five free outcome cat-

Figure 1: The r-model depicted as processing trees depending on whether both objects are recognized (topmost tree), neither is recognized (middle tree), or exactly one is recognized (bottom tree). The parameter *a* represents the recognition validity (probability of the recognized object representing the correct choice), *b* stands for the knowledge validity (probability of valid knowledge), *g* is the probability of a correct guess and, most importantly, *r* denotes the probability of applying the RH (following the recognition cue while ignoring any knowledge beyond recognition).



egories and four free parameters, the overall model fit can be tested by means of the log-likelihood statistic G^2 (χ^2-goodness-of-fit test with df = 5 - 4 = 1). From a practical perspective, researches are thus provided with a test that, if significant (and given reasonable statistical power), would imply not to interpret the parameters of the r-model substantively. A first set of analyses (8 experiments with 400 participants in total), revealed very good fit of the r-model. In addition, experimental validation of the *r* parameter was obtained: Most importantly, *r* was substantially larger in an experimental condition in which participants were instructed to "use" the RH — as compared to a control condition without any additional instruction. The *r* parameter could thus be shown to reflect the judgment process it stands for, namely RH-use (Hilbig et al., 2010).

3 Measure evaluation through simulation

How do these different measures perform? Apart from the theoretical and practical advantages and limitations outlined above, comparisons of the measures' ability to uncover the probability of RH-use (or the proportion of RH users) seemed in order. Therefore, several simulations were run to evaluate how well the measures perform when the ground truth is known.

In the simulation, twenty objects (e.g., cities) were used. For each object, the cue values of two cues, the recognition cue and an additional knowledge cue, were simulated. Specifically, the probability of a positive cue value for both the recognition and the knowledge cue fol-

Table 1: Mean absolute deviation, sum of squared differences, and maximally observed deviation from perfect estimation for each of the measures and all four simulations. (AR = adherence rate.)

		Measures				
		AR	*d'*	*c*	DI	*r*
Simulation 1	Mean absolute deviation	.30	.49	.13	.01	.02
(perfect conditions and typical cue validities)	Sum of squared differences	1.4	3.63	.42	< .01	< .01
	Maximally observed deviation	.61	.97	.47	.03	.05
Simulation 2	Mean absolute deviation	.29	.32	.20	.18	.05
(+ strategy execution error)	Sum of squared differences	1.31	1.87	.70	.56	.04
	Maximally observed deviation	.58	.83	.48	.39	.11
Simulation 3	Mean absolute deviation	.30	.34	.21	.27	.05
(+ extreme validities)	Sum of squared differences	1.37	2.04	.75	1.19	.04
	Maximally observed deviation	.59	.85	.48	.60	.11
Simulation 4	Mean absolute deviation	.33	.34	.20	.19	.08
(forcing a positive correlation between recognition and knowledge cue patterns)	Sum of squared differences	1.65	2.06	.72	.55	.11
	Maximally observed deviation	.65	.83	.48	.40	.18

lowed a sigmoid function[3] (see also Schooler & Hertwig, 2005, Figure 5). Note that the values of the two cues were drawn independently, thus allowing for any correlation between the two cue patterns. Additionally, to manipulate differences between cue patterns and between individuals, random noise was added: For each individual (and separately for the two cues) the probability of random noise was drawn from a normal distribution with given mean and standard deviation (for the exact values see simulations reported below). The cue value of each object was then reversed with the probability of random noise. Cue patterns with below-chance-level validity were discarded.

Next, the twenty objects were exhaustively paired, resulting in 190 comparative judgments (e.g., which city is more populous?). For each single pair, it was determined whether recognition was positive for neither, both, or exactly one of the objects. If neither was recognized, one of the objects was randomly chosen. If both were recognized, the object to which the knowledge cue pointed was selected (if the knowledge cue did not discriminate between the two objects, one of the two was randomly chosen). The only difference between users and non-users of the RH occurred whenever exactly one object was recognized, i.e., a case in which the RH could be applied: here, users followed the recognition cue in all cases (always chose the recognized object). Non-users, by contrast, followed the recognition cue if and only if the knowledge cue was positive for the recognized object, but chose the unrecognized object otherwise. The value of the knowledge cue for an unrecognized object was always ignored, implementing the assumption that one cannot retrieve knowledge for an unknown object.

Eleven data sets were thus created, each with 1,000 simulated individuals and the following true proportions of RH-users: .01, .10, .20, .30, .40, .50, .60, .70, .80, .90, .99. Each of these data sets was analyzed with the methods described above. The mean adherence rate across participants was computed as a measure of the overall probability of RH-use. Likewise, the r-model was applied to the aggregated outcome frequencies and the estimate of

[3]The effective probabilities of a positive cue value were 0.97, 0.95, 0.93, 0.90, 0.86, 0.81, 0.75, 0.67, 0.59, 0.50, 0.41, 0.33, 0.25, 0.19, 0.14, 0.10, 0.07, 0.05, 0.04, and 0.03 for objects 1 to 20, respectively.

r was obtained for each data set — again indicating the overall probability of RH-use. As described above, d', c, and DI could not be used to estimate the overall probability of RH-use. Instead, the proportion of RH-users was estimated from these measures: for d' and the DI, a value of zero was sufficient to be classified as a RH-user. For c, any value smaller than zero was sufficient.

3.1 Simulation 1: optimal conditions and typical cue validities

The first simulation was run implementing optimal conditions for identification of RH-use versus non-use. First, in this simulation, there was no strategy execution error; thus, the overall probability of RH-use and the proportion of RH-users in the sample are equivalent. Therefore, all measures can be compared against the same criterion, viz. the true underlying proportion of RH-users in each data set. Secondly, the random noise probabilities when drawing the cue patterns were chosen to result in a mean recognition validity of .75 and mean knowledge validity of .65 (thus mirroring typical data sets, Hilbig et al., 2010); specifically, the individual probability of random noise was drawn from a normal distribution with M = .10, SD = .05, and M = .20, SD = .05 for the recognition and the knowledge cue, respectively. In the following simulations 2 to 4 these constraints will be manipulated to assess the robustness of the measures investigated.

The results of this first simulation are shown in the top left panel of Figure 2 which plots the estimated probability of RH-use (proportion of RH-users) against the true underlying proportion of users. Optimal estimates would lie on the diagonal (dashed black line).

Table 1 additionally provides, for each measure, the mean absolute deviation, sum of squared differences, and maximally observed deviation from the true criterion across the eleven simulated data sets. As can be seen, the adherences rate substantially and consistently overestimated the probability of RH-use by up to .61 and with a mean absolute deviation of .30. Thus, even under optimal conditions, the adherence rate performed poorly and, as Figure 2 clearly demonstrates, severely overestimated use of the RH.

Surprisingly, the d' measure also performed poorly, as it practically predicted no RH-use at all. As the severe underestimation provided by this measure (see Figure 2) indicates, the criterion of classifying only those decision makers as RH-user who score $d' = 0$ is too strict. This is especially interesting in light of the very satisfying performance of the DI which used the same classification criterion (DI = 0) and, as introduced above, is almost tantamount to d', except for the lack of z-transformation. The DI, however, was almost perfectly related to the true criterion (with a mean absolute deviation of .01), and actually outperformed all other measures in the set (see Table 1).

The performance of c, by contrast, was relatively poor as indicated by a maximally observed deviation of .47. Interestingly, for true criterion values between .40 and .90, this measure performed very well and comparable to the DI. However, especially in case of lower true proportions of RH-users, c yielded severe overestimation of RH-use. Worse yet, the proportion of estimated RH-users obtained from c was not a monotonic function of the true underlying proportion of RH-users (see Figure 2). So, conclusive interpretation of differences in c as more versus less RH-use is not warranted — even under optimal conditions.

Finally, the r parameter estimated with the r-model showed very good performance (mean absolute deviation of .02) which was highly comparable to the DI. Indeed, the very small differences between the two should not be overemphasized. Rather, under the perfect conditions and typical cue validities implemented in this simulation, both measures provided very accurate estimation of RH-use or the proportion of RH-users.

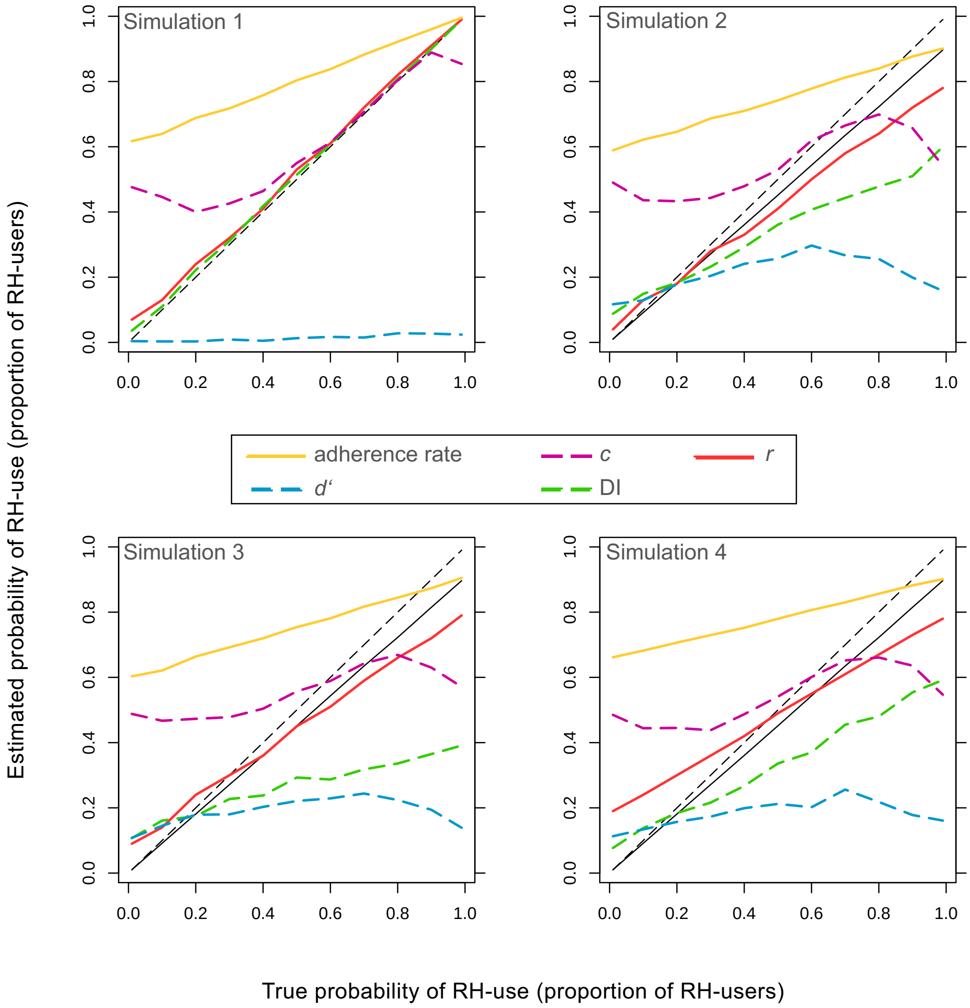
3.2 Simulation 2: Strategy execution error

The assumptions implemented in the above reported simulation are, admittedly, not entirely realistic. Most importantly, simulated participants' strategy execution was perfect, that is, no errors occurred. In real empirical data, however, it is unlikely that this would hold (e.g., Glöckner, 2009; Rieskamp, 2008). Therefore, in the next simulation, an individual error probability was set for each participant, randomly drawn from a normal distribution with M = .10 and SD = .05. On each trial, after the choice had been determined, this choice was switched with the probability of an error. As a consequence, even a true RH-user would now, on some trials, choose the unrecognized object.

Note that under these conditions the true underlying proportion of RH-users and the overall probability of RH-use are no longer the same. Therefore, the adherence rate and the r parameter were evaluated against the actually resulting overall probability of RH-use (solid black line in Figure 2), whereas d', c, and the DI were again compared to the underlying proportion of RH-users (dashed black line). Additionally, because the classification criterion of d' and the DI is unrealistic when strategy execution errors must be expected, both were allowed a more lenient criterion. For the DI, any simulated participant scoring within $-.05 \leq DI \leq .05$ was classified as a RH-user. While the DI has a possible range from -1 to 1, d' can practically take values anywhere between -3 and 3. Thus, the classification criterion was three times as large as for the DI, specifically $-.15 \leq d' \leq .15$.[4] The results

[4]Note that, when using the same classification criterion both for the

Figure 2: Simulation results under optimal conditions and typical cue validities (top left), adding strategy execution errors (top right), adding extremely high recognition and low knowledge validity (bottom left), and forcing the recognition and knowledge cue patterns to correlate positively (bottom, right). The adherence rate (yellow) and *r* parameter (red) are compared against the overall probability of RH use (solid black line). The DI (dashed green), d' (dashed blue) and c (dashed purple) are compared against the proportion of RH-users in each sample (dashed black line).



of this simulation are provided in Table 1 and displayed in the top right panel of Figure 2. As could be expected, most measures suffered from the addition of strategy execution errors. However, they were affected differentially: The adherence rate did not perform notably worse, but merely maintained its consistent and severe overestimation of RH-use. The d' measure, though again performing worst of all, actually improved. Obviously, this is due to the more lenient classification criterion implemented. However, the estimated proportion of RH-users derived from d' was non-monotonically related to the underlying true proportion (see Figure 2) which severely limits the interpretability of this measure. In any case, d' was clearly outperformed by all other measures — even the simple adherence rate.

All other measures were now negatively affected. Both c and the DI performed notably worse, with estimates diverging from the true proportion of RH-users by as much as .48 and .39, respectively. Under the current conditions, the fit statistics provided only weak evidence for the superiority of the DI over c. However, Figure 2 (top, right) does indicate that c was again a non-monotonic function of the true underlying proportion of RH-users. As is the case for d', this is a drawback which strongly limits interpretability of c. While the DI also performed notably worse than under optimal conditions, it did at least retain its monotonic relation to the true to-be-estimated criterion.

DI and d', the latter performed much more poorly.

The r parameter estimated from the r-model, too, no longer performed optimally. Indeed, it now produced estimates diverging from the true probability of RH-use by as much as .11. On the other hand, the fit statistics unequivocally indicated that r was now the best-performing measure in the set (see Table 1). Its mean absolute deviation of .05 is less than a third of the according statistic for the second-best measure, the DI.

3.3 Simulation 3: Extreme validities

So far, the cue validities implemented in the simulations were intermediate in size and reflected the typically observed difference between the recognition and knowledge validity. However, it may occur that the recognition validity is much larger than the knowledge validity and quite extreme in absolute terms (Hilbig & Richter, in press). As a result, there will be much fewer cases in which the RH actually yields a false prediction. This fact in turn should affect measures placing particular emphasis on such cases (especially the DI). To manipulate the cue validities, the random noise probabilities were changed: For the recognition cue, there was no longer any random noise; for the knowledge cue, the random noise probability was drawn from a normal distribution with M = .25 and SD = .05. Consequently, the mean recognition validity increased to .90, while the mean knowledge validity dropped to .55. Otherwise this simulation was exactly the same as the previous one (including strategy execution errors).

The results are shown in the lower left panel of Figure 2 and fit statistics are again found in Table 1. As could be expected, the resulting decrease in performance was most obvious for the DI, which now actually performed worse than the c measure in terms of fit statistics. Clearly, the extremely large recognition validity led to increasingly severe underestimation of the true underlying proportion of RH-users by the DI. The performance of d' and c, by contrast, was not as strongly affected but merely remained generally poor. Also, both were again non-linearly related to the underlying criterion, thus hampering interpretability. On a more positive note, the r parameter was not affected by the extreme validities. In fact, it performed exactly as in the previous simulation with a very satisfying mean absolute deviation of .05.

3.4 Simulation 4: Cue inter-correlation

In a final simulation, another potential caveat for strategy classification other than extreme validities was sought. Specifically, the recognition and knowledge cue patterns were now forced to correlate positively (r $\geq$ .3). To implement this restriction, a naïve method was used which simply computed the correlation of the two cue patterns and redrew cue values if the condition of r $\geq$ .3 was not fulfilled. However, as a consequence, the cue validities were also affected. Therefore, the random noise probabilities were adjusted to render the current simulation comparable to the first two: The probabilities were drawn from normal distributions with M = .30, SD = .05 and M = .05, SD = .05 for the recognition and knowledge cue, respectively, resulting in a mean recognition validity of .75 and mean knowledge validity of .64. This simulation was thus exactly the same as Simulation 2 (including strategy execution errors), apart from the addition of positive cue-pattern correlations which will again render strategy identification more difficult because less diagnostic cases occur when cues are correlated (Glöckner, 2009). In other words, the knowledge cue was substantially less likely to argue against a recognized object.

The results are depicted in the lower right panel of Figure 2 (see also Table 1). Whereas the performance of most measures only worsened slightly compared to Simulation 2, the r parameter now showed less satisfactory fit statistics. The effect of introducing cue-pattern correlations on the r estimate is clearly visible by comparing the upper and lower right panels of Figure 2: The r parameter now tended to overestimate RH-use when the true underlying proportion of RH-users was small. This is plausible given that the positive cue-pattern correlation will increase the probability of a RH-non-user following the recognition cue — simply because the knowledge cue is less likely to argue against it. However, these findings notwithstanding, the r parameter was still the best-performing measure in the set and its mean absolute deviation of .08 can still be considered satisfactory.

3.5 Summary and discussion of simulation results

Several measures for assessing the probability of RH-use or, alternatively, the proportion of RH-users in a sample were compared in a set of simulations. As a starting point, optimal conditions for strategy identification were implemented, namely no strategy execution errors, typical cue validities, and independently drawn cue patterns. The results of this simulation revealed that both the adherences rate and Pachur and Hertwig's (2006) d' performed poorly. That is, even assuming optimal conditions, these measures should not be applied to assess RH-use. By contrast, c performed more acceptably in terms of fit and especially for larger underlying proportions of RH-users. However, at lower levels, c showed a varying tendency to overestimate RH-use and, worse yet, was a non-monotonic function of the to-be-estimated criterion which is a severe drawback. Neither of these problems were apparent for the DI (Hilbig & Pohl, 2008) which provided highly accurate estimates of the proportion on RH-users in the simulated samples. Likewise, the *r* parameter as estimated from the multinomial processing tree model proposed by Hilbig, Erdfelder, and Pohl (2010) showed almost perfect performance.

In the following simulations, the implemented constraints ensuring optimal conditions for strategy identification were relaxed. Specifically, strategy execution errors were introduced, extreme validities were implemented, and positive cue-pattern correlations were enforced. Overall, those measures originally performing well (DI and *r*) did suffer from these obstacles. In particular, the DI strongly underestimated higher proportions of RH-users in a sample when an extremely large recognition validity (.90) and very low knowledge validity (.55) were implemented. The *r* parameter, by contrast, provided adequate estimates under these circumstances but performed less well when positive cue-pattern correlations were enforced. On the whole, however, the *r* parameter provided the best estimates of RH-use which held even under conditions clearly hampering optimal strategy classification.

4 Measure evaluation through empirical data

Simulations bear advantages and limitations. One of the latter is that the behavior of actual decision makers can, at best, only be approximated. In a second step, I thus sought to evaluate the different measures of RH-use through empirical data. However, as outlined in the introduction, the paradigm of natural recognition (without any control over participants' cue knowledge) cannot provide any useful comparison against which to evaluate these measures. Instead, it is much more informative to apply these measures to data in which the cue patterns are known and RH-use can be assessed using the strategy-classification method of Bröder and Schiffer (2003). The combination of this method with diagnostic tasks yields vastly more control and allows for more conclusive classification of participants to strategies.

Specifically, the data of Glöckner and Bröder (in press) were analyzed because the authors implemented a paradigm in which participants were provided with additional information beyond recognition: Participants were shown recognized and unrecognized US-cities and were additionally given information about these, namely three additional cues. Based on the artificially created cue patterns, participants' choice data were analyzed with the Bröder/Schiffer-method. As reported by Glöckner and Bröder (in press, Figure 1), a proportion of up to 36.25% of their sample were accordingly classified as users of non-compensatory strategies such as the RH.

The question then was how the measures of RH-use investigated herein would perform as compared to the Bröder/Schiffer-method. Importantly, all these measures ignore information about the cue patters in specific trials. So, from Glöckner and Bröder's data, I kept only the three pieces of information necessary for computing the measures of RH-use: (i) which objects were compared on each trial, (ii) which objects participants reported to recognize, and (iii) actual choices. For those measures which afford some fixed criterion to classify participants as RH-users, the following were used: A participant with a DI within the 95%-confidence-interval of zero (± .11) was classified as a RH-user (cf. Hilbig & Pohl, 2008). The same criterion (± .07) was used for d'. For c, participants with values smaller than the upper bound of the 95%-confidence-interval of zero (.11) were considered RH-users. The remaining measures, viz. the adherence rate and the *r* parameter, again estimated the overall probability of RH-use.

Results were mostly consistent with what might be expected from the simulations reported above. The mean adherences rate in the sample was .71 (SD = .14), thereby severely overestimating RH-use as compared to the results of the Bröder/Schiffer-method. Also, *d'* showed the same strong underestimation which was already visible in the simulations, proposing that only 6% of participants were RH-users. Overall, c and the DI yielded more accurate estimates, implying proportions of RH-users in the sample of .52 and .59, respectively. Clearly, both performed better than the adherence rate and d', but neither provided an estimate which was satisfyingly close to what was expected from the maximum-likelihood strategy classification. Finally, the r-model (which fit the empirical data well, $G^2(1) = .12$, $p = .74$) estimated the overall probability of RH-use to be $r = .40$ (SE = .01) which is close to the conclusion drawn from the Bröder/Schiffer-method, namely that about 36% of participants were most likely to have used the RH.

In sum, once more, the r-model provided the best estimate of RH-use — though, unlike in the simulations, "best" here does not refer to the known underlying truth but rather to the results obtained from a well-established and widely-used method for strategy classification. However, one may argue that this method need not uncover the actual judgment processes — especially if only choices are considered (Glöckner, 2009). Therefore, from the current analysis, it might be more adequate to conclude that the r-model provides the estimate of RH-use closest to what is implied by Bröder and Schiffer's (2003) maximum-likelihood strategy-classification method (and no more). Importantly, though, the r-model achieves this without considering any information about cue patterns in the different trials.

Table 2: Results concerning desirable criteria for measurement tools of RH-use.

	Measures				
	AR	d’	*c*	DI	*r*
Directly interpretable estimate of RH-use	yes	no	no	no	yes
Adequate estimate of RH-use (under optimal conditions)	no	no	no	yes	yes
Adequately robust (under non-optimal conditions)	*	*	*	yes**	yes***
Estimate monotonically related to RH-use	yes	no	no	yes	yes
Parallel results to maximum-likelihood strategy classification in empirical data	no	no	yes	no	yes
Goodness-of-fit tests	no	no	no	no	yes

* It makes little sense to interpret the robustness of measures which performed poorly even under optimal conditions.

** The DI is least robust if the recognition validity is extremely high and much larger than the knowledge validity.

*** The r-estimate is least robust if recognition and knowledge cue patterns correlate positively.

5 Discussion

Concerning the recognition heuristic (RH; Goldstein & Gigerenzer, 2002), most of the recent investigations have concluded that it neither represents a general description of comparative judgments nor appears to be refutable altogether (Hilbig, in press) — very much like the take-the-best heuristic (Bröder & Newell, 2008). Consequently, it is an important quest to uncover the conditions and individual differences which foster or hamper application of simple one-cue strategies, such as the RH. However, mutual progress in this domain would necessitate some consensus as to the paradigms and measures appropriate for investigating use of this strategy. So far, there has been some work concerning suitable paradigms and it is my impression that using naturally recognized objects without teaching (or providing) any further cue knowledge or information has emerged as one preferred method (Pachur et al., 2008) — especially given that the potential dangers of participants possessing criterion knowledge need not be too severe (Hilbig, Pohl, & Bröder, 2009).

However, such a paradigm in which there is no control over participants’ knowledge beyond recognition renders measurement of RH-use very difficult. Clearly, choices in line with a single-cue strategy provide little information about its actual use, if other cues (the values of which are unknown) may imply the same choice (Bröder & Eichler, 2006; Bröder & Schiffer, 2003; Hilbig, 2008b, in press). In this article, I have therefore considered different measures and evaluated them with respect to their ability of uncovering true use of the RH. Specifically, apart from the adherence rate (proportion of choices in line with the RH), Pachur and Hertwig’s (2006) d' and c (Pachur et al., 2009), the discrimination index (DI; Hilbig & Pohl, 2008), and the parameter *r* from the r-model (Hilbig et al., 2010) were compared.

Table 2 summarizes the main results with respect to several desirable criteria. Firstly, only the adherence rate and r-model provide a directly interpretable estimate of RH-use; d', c and DI, by contrast, necessitate further assumptions as to the values RH-users would show (as a necessary but not sufficient condition, cf. Hilbig & Pohl, 2008). Secondly, DI and *r* provide adequate estimates of RH-use under optimal conditions, whereas c, the adherence rate, and d' perform less convincingly: While the adherences rate consistently and severely overestimated RH-use, the exact opposite was the case for d'. Furthermore, d' and c were mostly non-monotonically related to the true proportion of RH-users which hampers the interpretability of differences in these measures. Overall, only *r* was satisfactorily robust against less optimal conditions for strategy identification — though situations bearing a substantial positive correlation between recognition and knowledge cue patterns do pose difficulties for this measure, too.

Additionally, I asked which measures would produce results similar to choice-based maximum-likelihood strategy-classification (Bröder & Schiffer, 2003; Glöckner, 2009) in Glöckner and Bröder’s (in press) empirical data. The most comparable estimates were provided by c and, even more so, the *r* parameter. Finally, as an additional benefit, the r-model allows for goodness-of-fit tests and comprises many of the other advantageous features of multinomial processing tree models (Erdfelder et al., 2009) — including, for example, model comparisons with respect to goodness-of-fit *and* complexity (Myung, 2000). Also, in light of recently developed free and platform-independent software for analysis of multinomial models (Moshagen, 2010), the r-model is no more difficult to apply than any of the other measures.

In sum, for those studying comparative judgments between naturally recognized objects (without teaching or providing further cues), the r-model will yield the best measure of RH-use currently available. However, there are also situations in which this measurement tool will not be helpful and I consider it important to point to these cases: Firstly, the r-model cannot be applied to preferential choice, that is, situations in which there is no conclusive criterion which choice option represents a correct versus false judgment. In fact, this limitation applies to all measures discussed herein except for the adherence rate. Secondly, the r-model is designed for exhaustive paired-comparisons as it affords cases in which both objects are recognized *and* cases in which only one is recognized. At least, a representative sample of each of these sets of cases is necessary. This limitation does not hold for any of the other measures, each of which can be applied to only those cases in which exactly one object is recognized. On the other hand, I am aware of few empirical investigations which actually were limited to such cases.

Beyond some recommendations for measuring RH-use, what methodological conclusions can be drawn? As the extremely poor performance of the adherence rate (which is the measure most often applied so far) indicates, more careful consideration of our measurement tools seems advisable. Precisely formulated process models of judgment and decision making deserve precise (and process-pure) measures. So long as measurement is vague, exact description on the theoretical level will not avail us. With good reason, Gigerenzer and colleagues have called for precise theories (Gigerenzer, 1996, 2009; Gigerenzer, Krauss, & Vitouch, 2004). However, it does not suffice — though it is necessary — to build precise theories. If we do not add a call for using the most precise measurement tools available, we may too often fall prey to premature conclusions. For the recognition heuristic theory, I hope to have provided some insight which measures are more or less likely to enhance our understanding.

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