When alternative hypotheses shape your beliefs: Context effects in probability judgments

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Author Note

All data and analyses reported in this paper can be found at this repository at the Open Science Framework:

https://osf.io/3h7v4/?view_only=81ebdb4f9fb645cd86f7cd16d8f27e00. A draft of this paper was posted on PsyArXiv. Correspondence concerning this article should be addressed to Xiaohong Cai (E-mail: cai_xh@ku.edu).

Abstract

When people are asked to estimate the probability of an event occurring, they sometimes make different subjective probability judgments for different descriptions of the same event. This implies the evidence or support recruited to make subjective probability judgments is based on the descriptions of the events (hypotheses) instead of the events themselves, as captured by Tversky and Koehler's (1994) support theory. However, is the support assigned to a hypothesis invariant, as is often assumed? Here, across three studies where participants were asked to estimate the probability that an event would occur given a set of relevant statistics. We show that the support people recruit about the target hypothesis depends on the other hypotheses under consideration. Results reveal that the presence of a dud—a hypothesis objectively dominated by the target hypothesis—boosts the subjective probability assigned to the target hypothesis, and the presence of a ressembler—a hypothesis that is objectively similar to the target hypothesis—detracts more from the subjective probability assigned to the target hypothesis than the competing hypothesis. These context effects invalidate the support invariance assumption in support theory and more generally suggest that a similar process drives the construction of preference may also underlie belief.

Keywords: judgment, context effect, attraction effect, dud-alternative effect, similarity effect

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Introduction

People constantly forecast the probabilities of events, from the mundane (i.e., what is the probability that I'll hit traffic coming back from work?) to very unusual events (i.e., what is the likelihood of a Dogecoin crash?). In some situations, expert forecasters may provide subjective probabilities that are well calibrated with objective probabilities (Mandel & Barnes, 2014; Murphy & Winkler, 1977) though not always (Keren, 1987; Moore et al., 2015; Mellers et al., 2014). One of the reasons for this disconnect between subjective probability judgments and objective probabilities is that people assign different subjective probabilities to the same event. For instance, Johnson, Hershey, Meszaros, and Kunreuther (1993) identified a group of MBA executive students who were scheduled to travel to Bangkok, Thailand and asked how much they would pay for terrorism insurance for their flight. One group of students was asked how much they would pay to insure each leg of the trip. They were willing to pay almost twice as much as another group who were asked about insuring the round trip. This result, which is one of many, demonstrates that subjective probability judgments depend on the *description of the event* rather than the *event* itself. When people assign different probabilities to different descriptions of the same event this creates a violation of a description invariance principle, a cornerstone principle of many theories of subjective probabilities (Brenner et al., 2002).

Support theory seeks to explain these violations of description invariance in subjective probability judgments (Tversky & Koehler, 1994; Rottenstreich & Tversky, 1997). According to this theory, each description of an event or *hypothesis* elicits a fixed strength of evidence or *support* s for the hypothesis occurring (Rottenstreich & Tversky, 1997; Tversky & Koehler, 1994). The subjective probability judgment P that the hypothesis A is realized is equal to the ratio of the support for that hypothesis relative to the total support

for all hypotheses under consideration. If one considers only the alternative hypothesis B :

$$P_{\{A,B\}}(A) = \frac{s(A)}{s(A) + s(B)}. \quad (1)$$

As support is based on the description of the event, support theory predicts violations of description invariance. For instance, support theory allows the support of a packed hypothesis (e.g., terrorism insurance for a round trip flight) to be less than the sum of the support given to mutually exclusive and exhaustive hypotheses (e.g., terrorism insurance for each leg of a flight). By doing so, support theory is able to account for these subadditive probability judgments (Dougherty & Hunter, 2003; Koehler et al., 1997; Rottenstreich & Tversky, 1997; Tversky & Koehler, 1994)(though see Bearden et al., 2007; Sloman et al., 2004).

Crucially, although support theory relaxes the constraint of description invariance for subjective probability judgments, it still maintains an invariance property: the support for a hypothesis is independent to other hypotheses being considered. It is alluring to assume that support, a latent psychological variable, has a one-to-one mapping relationship to the external world (a hypothesis). Such a property permits scaling subjective probabilities directly from support judgments (e.g., Fox, 1999; Koehler, 1996; Koehler et al., 1997; Tversky & Koehler, 1994). It also facilitates the development of more complete psychological models about reasoning and decision making in the face of uncertainty (Dougherty et al., 1999; Fox & Rottenstreich, 2003; Koehler et al., 2003; Thomas et al., 2008). The property also enables support theory to help make sense of basic subjective probability judgment phenomena, such as the role of working memory in judgment (Dougherty & Sprenger, 2006); the role of typicality in judgment (Sloman et al., 2004); how decisions are made under uncertainty (Fox & Tversky, 1998); and the pervasiveness of ignorance prior in judgment (Fox & Levav, 2004).

However, there is evidence to suggest that support invariance may not hold. Consider the dud-alternative effect: when people were given a list of possible cities that might be on the equator, they estimated the chances that *Nairobi, Kenya* is on the equator as *greater*

when the list contained the “duds” *Cincinnati, Ohio* and *Moscow, Russia* than when it did not (Windschitl & Chambers, 2004). Such a pattern where the subjective probability assigned to an event increases when additional competing hypotheses are being considered suggests that the support assigned to a hypothesis is not invariant.

An effect similar to the dud-alternative effect, the attraction effect, has been reported in the decision making domain where adding an additional option dominated by an existing option, increases the probability of choosing the dominating option (Huber et al., 1982; Berkowitsch et al., 2014). We call this additional option/hypothesis the *dud*, the option/hypothesis dominating the dud the *target*, and the competing option/hypothesis in the original set which is neither dominates nor is dominated by the dud the *competitor*. Such an increase in choice share of the target option when adding a dud option into the original choice set violates a regularity principle. In the domain of choice, the regularity principle states that adding options to the choice set should not increase the probability of choosing an option from the original set. This property is assumed by a large class of probabilistic choice models (for a review see Rieskamp et al., 2006). Analogously, the dud-alternative effect, which is similar to the attraction effect, can create a regularity violation in terms of belief. In the domain of belief, the regularity principle states that adding hypotheses to the hypothesis set ($\mathbb{Y}$) should not increase the subjective probability assigned to a hypothesis (A) from the original hypothesis set ($\mathbb{X}$). Stated formally

$$P_{\mathbb{X}}(A) \geq P_{\mathbb{Y}}(A) \tag{2}$$

for A in $\mathbb{X} \subseteq \mathbb{Y}$. As we prove in the Supplementary Material (Section S1.1), support theory implies regularity, and so does any model that uses support theory as a framework (e.g., Brenner, 2003; Idson et al., 2001; Koehler et al., 2003; Narens, 2009; Fox & Rottenstreich, 2003).

The attraction effect can also be seen via a violation of independence. In this case, the choice share of an option (A or B) in a three-option choice set is higher when the option (A or B) is a target than when it is a competitor ($P_{\{A,B,C_A\}}(A) \geq P_{\{A,B,C_B\}}(A)$ and

$P_{\{A,B,C_A\}}(B) \leq P_{\{A,B,C_B\}}(B)$, where C_A is an option dominated by the option A and C_B is an option dominated by the option B) (Trueblood, 2012; Trueblood et al., 2013, 2014; Wedell, 1991; Choplin & Hummel, 2005; Gluth et al., 2017; Bhatia, 2014). This ratio of the choice share of two options is not independent from the duds ($\frac{P_{\{A,B,C_A\}}(A)}{P_{\{A,B,C_A\}}(B)} \neq \frac{P_{\{A,B,C_B\}}(A)}{P_{\{A,B,C_B\}}(B)}$) demonstrating that the utility of an option is not invariant to the options in the choice set. Support theory also implies a similar independence property: the ordinal relationship between the subjective probabilities assigned to hypothesis A and hypothesis B —not necessarily their ratio—is independent of additional competing hypothesis C (for a formal proof see Supplementary Material Section S1.2). Stated formally,

$$\begin{cases} P_{\{A,B\}}(A) > P_{\{A,B\}}(B) \iff P_{\{A,B,C\}}(A) > P_{\{A,B,C\}}(B) \\ P_{\{A,B\}}(A) = P_{\{A,B\}}(B) \iff P_{\{A,B,C\}}(A) = P_{\{A,B,C\}}(B) \end{cases} \quad (3)$$

It is an open question whether subjective probability judgments violate this independence principle.

Although we suspect that subjective probability judgments violate the invariance property of support, there is some empirical evidence backing this property. This evidence comes from testing a more restrictive version of the independence principle known as the product rule (Fox, 1999). According to the product rule the ratio of the support for two hypotheses is independent of other competing hypotheses.¹ However, there is also evidence that the comparability or similarity of hypotheses (e.g., a particular set of basketball team winning a game) on one dimension (e.g., offensive ability of the teams) can create violations in the product rule by leading people to put more weight on another dimension (e.g., defensive ability of the teams) when they make their judgments. This effect of comparability can in turn produce a violation of the invariance property of support (Pleskac, 2012).

¹ The product rule states that $\frac{P_{\{A,B\}}(A)}{P_{\{A,B\}}(B)} \times \frac{P_{\{B,D\}}(B)}{P_{\{B,D\}}(D)} = \frac{P_{\{A,C\}}(A)}{P_{\{A,C\}}(C)} \times \frac{P_{\{C,D\}}(C)}{P_{\{C,D\}}(D)}$. Substituting Equation 1 into this expression shows that both sides of the equation equal $s(A)/s(D)$ demonstrating that the ratio of the support given to two hypotheses (A, D) is independent of other competing hypotheses (B, C).

The direct and indirect indicators of possible violations of regularity and the order-based independence property in belief call for further investigation of the invariance property of support. We should emphasize that regularity and independence are logically distinct. One can violate regularity, but not independence (and vice versa). Thus, establishing these two violations can expose two different problems for any theory of subjective probability judgments that adheres to the regularity and independence principles, including support theory.

One may question whether the violations of regularity and independence observed in decision making domains beget a foregone conclusion that there will be similar violations in the domain of belief. Two observations seem pertinent here. First, as we have established to-date the leading descriptive theory of probability judgments—support theory—implies regularity and independence. Second, the current choice theories that predict violations of regularity and independence are limited to the domain of choice (Bhatia, 2013; Roe et al., 2001; Trueblood, 2012; Turner et al., 2018; Usher & McClelland, 2004; Trueblood et al., 2014). Critically, none of the theories have been extended to the domain of belief. In sum, the current theories of belief predict that subjective probability judgments should adhere to the regularity and independence principles, and the theories that do predict regularity and independence violations are limited to the choice domain. Thus, it seems anything but a foregone conclusion that there will be violations of regularity and independence in subjective probability judgments.

In our first study, we sought to replicate and further explore the degree to which the dud-alternative effect is present in subjective probability judgments. We expanded the investigation of the dud-alternative effect in two ways. First, we investigated the dud-alternative effect with a different task. Windschitl & Chambers (2004) largely used vignette-based questions to study the dud-alternative effect, primarily relied on an intuitive definition of a dud (e.g., U.S. children aged 7–10 eating pizza vs. grilled fish). We addressed this issue by using a task—the predict-victor task—where participants were

asked to judge the probabilities of hypotheses occurring using different numeric attributes of the hypotheses (Pleskac, 2012). Specifically, during the task participants were asked to judge the probability of a bicyclist beating one or two other opponent(s) in a race. To make the judgments, participants were informed of the bicyclists' sprinting and climbing abilities, which were also used to determine the objective probabilities that the racers would win. Thus, by clearly specifying the statistics of bicyclists' sprinting and climbing abilities we were able to define a dud hypothesis as a hypothesis dominated by the target hypothesis but not by the competitor hypothesis. The second way we expanded the investigation was by using the dud-alternative effect to examine not only the violation of regularity, but also the violation of independence. To do so, we asked participants to report their subjective probability judgments of targets and competitors. Doing so allowed us to examine the relationship between the subjective probabilities assigned to a target and a competitor when a dud was absent and present.

We also used this study as an opportunity to investigate the possible moderating effect of the response scale on context effects. Windschitl & Chambers (2004) found that the dud-alternative effect was more pronounced when participants used a scale to estimate their probabilities anchored by the phrases "Extremely Unlikely" and "Extremely Likely", henceforth *verbal scale*, as compared to estimating their probabilities on a scale anchored by "0%" and "100%", henceforth *numeric scale*. This phenomenon is consistent with the hypothesis that a verbal scale may prompt greater reliance on System 1 heuristic reasoning relative to a numeric scale (Windschitl & Weber, 1999). We sought to replicate the scale type effect with the predict-victor task and explore the extent to which the scale type impacted subjective probability judgments.

Anticipating the results, we replicated Windschitl & Chambers (2004) and found that the presence of a dud hypothesis produced a higher subjective probability assigned to a target hypothesis than when the dud was absent, but this increase was only observed when participants entered their judgments using a verbal scale. Moreover, we also found

violations of independence, but in both scales. We suspected this apparent dissociation between violations in regularity and violations in independence had more to do with how participants used the response scales, specifically the midpoint. Therefore, in a second study we broadened the scope of the question, asking if the similarity effect (Tversky, 1972)—another context effect in the domain of choice—was present in subjective probability judgments. In the similarity effect, the presence of a *resembler*, an option similar to but not dominated by the target, decreases the attractiveness of the target compared to the competitor and hence disproportionately detracts from the share of the choice proportion for the target as compared to the competitor. This disproportionate effect on the target is another instance of the violation of independence. Our second study established the presence of a similarity effect in subjective probability judgments and, like the first study, showed that the violation of independence was constant across the types of scales used. We also show that the key difference between verbal and numeric scales is how people used the scale, particularly the midpoint.

In a final study, we addressed a potential limitation with asking participants to predict the outcome of a bicycle race. In this case, the violations of support invariance we observed in the earlier two studies could be the result of participants inferring that competitors in the race might adjust their performance based on who they are competing against. Although our training emphasized this was not the case, it could be that this inference still impacted beliefs. Thus, in the third study we adapted the predict-victor task to incorporate a race between wooden cars down an elevated track. Here we replicated the dud-alternative effect and the similarity effect, though we did find that the similarity effect was smaller and that there was more heterogeneity in the effect. We were also able to further establish the critical role response scale usage plays in these context effects.

Study 1: Dud-alternative Effect

The aim of this study was to investigate the dud-alternative effect first reported by Windschitl & Chambers (2004). We expanded the scope of the inquiry to not only examine the dud hypothesis’s impact on the subjective probability judgments and the subjective probability judgment’s ability to adhere to both the regularity principle and the independence principle. We also investigated the influences of changes in the response scale used to report judgments—verbal or numeric—on how the dud was processed and ultimately on the reported subjective probabilities.

Methods

Participants. We recruited 102 college students from the psychology undergraduate participant pool at Michigan State University to participate (age: $M = 19$) in the experiment. Participants received 2 credits per hour. The study was approved by the Michigan State University Institutional Review Board.

Materials and Design. During the task, participants were shown two or three bicyclists’ climbing and sprinting abilities in a table (rows corresponding to attributes and columns to hypotheses), and asked the subjective probability that the bicyclist in the farthest right column won the race. A bicyclist’s sprinting ability is how fast the bicyclist was able to complete a lap on a recent criterion race. Participants were told the fastest time ever was 35 s, the average lap time was 85 s, and the slowest time was 135 s. A bicyclist’s climbing ability is how far up a steep mountain the bicyclist goes in 20 minutes. Participants were told the best climber reached 8.9 km up the mountain, the average climber reached 5.6 km, and the worst climber reached 2.4 km. The position of sprinting and climbing abilities in the table was counterbalanced.

For the task, we generated 10 triplets of bicyclists to be used for the experimental trials (see Supplementary Material Section S2.1 for the stimuli used in the experimental trials). Each of these triplets had a target, a competitor and a dud. Targets had higher

scores on one of the abilities compared to the competitors (i.e., the advantageous ability) and less on the other ability (i.e., the disadvantageous ability). Duds had the same scores as targets' scores of the advantageous bicycling ability but had lower scores on the other ability making the duds dominated by the target.² We also generated 10 triplets of bicyclists with random scores for filler trials.

Procedure. Figure 1 plots the schematic diagram of the procedure performed in this study. The study proceeded in two stages. In the training phase, participants were familiarized with bicyclists' sprinting and climbing abilities. During this stage, participants were shown two bicyclists and asked to predict who would win a two-person race. Climbing and sprinting statistics for each bicyclist were randomly generated from within the range of the values used in the experimental trials. For the first 10 trials, participants were shown only the sprinting abilities; on the next 10 trials, they were shown only the climbing abilities. On the final 30 trials, they were shown both the sprinting and the climbing abilities. After each trial, the computer simulated the race, informed participants of the race's outcome, and reported the frequency out of 100 times that the chosen bicyclist would have won. The computer simulated a race for each pair or triplet of bicyclists based on the probability pr of a bicyclist x winning a race. The probability pr was a logistic function

$$pr(x) = \frac{\exp(\text{fit}(x)/\sigma)}{\sum_{y \in \Omega} \exp(\text{fit}(y)/\sigma)}, \quad (4)$$

where Ω is the set of bicyclists being considered. The function $\text{fit}(w)$ denotes the total fitness of a bicyclist (w) determined by an additive combination the bicyclist's standardized sprinting and climbing ability,

$$\text{fit}(w) = \text{Sprinting}_{\text{standard}}(w) + \text{Climbing}_{\text{standard}}(w). \quad (5)$$

The parameter σ was set to 4. Thus, in the predict-victor task the actual probability was consistent with the regularity and the independence principles assumed in support theory.

² In the experiment we verified that participants assigned lower subjective probabilities to duds than targets (See Supplementary Material Section S2.5).

Figure 1. A schematic diagram of the procedure performed in Studies 1 and 2.

Thus, the participants had the opportunity to learn that the objective probability of a bicyclist winning was consistent with the regularity and the independence principles.

In the experimental phase, participants made 180 judgments about the probability that the bicyclist in the farthest right column of the table would win the race. For 60 of those trials, participants judged the probability that one of the three bicyclists from each of the 20 triplets (10 experimental and 10 filler) would win the race. For the remaining 120 trials, participants judged the probability that each of the bicyclists from the triplet would beat one of the other bicyclists for each of the possible pairs. All pair-trials and triplet-trials were mixed randomly and divided into four blocks. As participants made multiple judgments about the same bicyclist pair or triplet, we worked to minimize the chance that participants might recall how they responded to a particular pair or triplet by ensuring that a pair or triple did not appear more than once in a block. No feedback was given during the experimental trials. Participants entered their judgments using a semicircular scale at the bottom of the screen that stretched from “0%” to “100%” in the numeric condition and from “Extremely Unlikely” to “Extremely Likely” in the verbal condition. Participants clicked a mouse along the semi-circle to select a probability. Half of the participants were randomly assigned to the numeric condition and the other half to the verbal condition. In the analysis, we mapped the responses on the verbal scale to the same 0 to 100 scale as the numeric scale.

Analysis

We used a Bayesian approach to analyze data. We fitted the data using the Bayesian hierarchical linear regression using the Stan-based package, *rstanarm*, in R for regression analyses. We entered “participant” as a random effect to account for individual variation beyond condition-dependent effects. In general, we report the mean of the posterior distribution of the parameter (b) or the mean of the posterior predictive distribution (M) and two-sided 95% equal-tail credible intervals (CI) around each value. For the results of

simple regression analyses, we report the difference of the means of the posterior predictive distribution (M_{diff}) of the parameters or statistics of interest and two-sided 95% equal-tail credible intervals (CI). As probability judgments were measured on a bounded scale between 0% and 100%, we transformed these values into a log odds scale ($\log \frac{p}{1-p}$), with subjective probability judgments of 0 and 1.0 set to .0001 and .9998, respectively. Regression coefficients for these analyses are reported in terms of this log-odds transformation. We applied similar analyses to Studies 2 and 3.

Figure 2. Subjective probability that a bicyclist would win in tests of the violations of regularity and independence in Study 1. A smaller dot represents the average subjective probability assigned to the targets or the competitors per participant. The larger dots are the posterior predictive means. Error bars are the 95% predicted credible intervals.

Results

Regularity. As we discussed earlier, support theory implies the regularity and the independence principles. We examined whether subjective probability judgments follow the two assumptions separately.³ If subjective probability judgments follow regularity, then the presence of duds should not increase the probabilities assigned to targets. Alternatively, if the presence of duds increases the probabilities assigned to targets, it indicates the dud-alternative effect in subjective probability judgments. Figure 2 plots the subjective probabilities for both the target and the competitor with and without the dud for verbal and numeric scales. To test the regularity violation on the target hypothesis, we ran a Bayesian hierarchical linear regression where the probability judgment of the target was regressed onto (a) dud presence or absence, (b) scale type (verbal; numeric), and (c) the interaction between the two variables. See Table S7 in the Supplementary Material Section for the complete regression results. Figure 2 shows that the presence of the dud increased the subjective probability assigned to the target compared to when it was not present ($M_{\text{diff}} = .10$, CI [.07, .14]) with a verbal scale, but not with a numeric scale ($M_{\text{diff}} = .01$, CI [-.03, .04]). This difference between verbal and numeric scales is credible as indicated by an interaction between scale type and the presence or absence of dud ($b = -.17$, CI [-.26, -.08]). These findings suggest that the presence of duds increases the subjective probabilities assigned to the targeted hypotheses and hence violates the regularity principle with a verbal scale, but not a numeric scale.

It is also useful to examine how the subjective probabilities assigned to the

³ We examined other aspects of subjective probability: the degree to which the subjective probabilities aligned with the objective probabilities (see Supplementary Material Section S2.2), the degree to which the subjective probabilities assigned to two (binary complementarity) and three mutually exclusive and exhaustive events (trinary complementarity) summed to one (see Supplementary Material Section S2.3), and the degree to which participants made consistent subjective probability judgments across repeated trials (see Supplementary Material Section S2.4).

competitor were impacted by the presence of duds. To test the regularity violation on the competitor hypothesis, we ran a Bayesian hierarchical linear regression where the probability judgment of the competitor was regressed onto (a) dud presence or absence, (b) scale type (verbal; numeric), and (c) the interaction between the two variables. See Table S8 in the Supplementary Material Section for the complete regression results. Figure 2 shows that when using the verbal scale, the subjective probabilities assigned to the competitor when the dud was present were similar to when it was not present ($M_{\text{diff}} = 0$, CI $[-.03, .04]$). In comparison, when using the numeric scale, the presence of the dud decreased the subjective probabilities assigned to the competitor relative to when the dud was not present ($M_{\text{diff}} = -.04$, CI $[-.08, -.01]$). However, this difference between verbal and numeric scales in terms of the effect of the dud on the competitor is not credible ($b = -.09$, CI $[-.18, .01]$). Nevertheless, this effect on subjective probabilities assigned to the competitor is intriguing for two reasons. The first reason is that it suggests that the relationship between the subjective probabilities assigned to the target and the competitor may be impacted by the presence of the dud indicating a possible violation of independence. We address the independence assumption next. The second reason is that it suggests that perhaps the different scales prompted different uses of the response scale. Using the scale differently between conditions could possibly confound the explanation that the different scales prompt different modes of thinking. We consider the possibility of a confound further in the summary of this study and address it more directly in Studies 2 and 3.

Independence. If subjective probability judgments follow independence, then the ordinal relationship between the subjective probabilities assigned to the target and the competitor should remain the same regardless of whether the duds are present or not. To test for independence violations, we ran a Bayesian hierarchical linear regression regressing the probabilities assigned to targets and competitors onto (a) dud present or absent, (b) scale type (verbal; numeric), (c) hypothesis type (target; competitor), and (d) the interaction between the variables. See Table S9 in the Supplementary Material Section for

the complete regression results. Consistent with a violation of independence, there was a credible interaction between whether the dud was present or absent and the hypothesis type (target or competitor), $b = -0.18$, CI $[-0.27, -0.09]$. Without the dud, subjective probabilities assigned to the targets and the competitors were similar ($M_{\text{diff}} = -.01$, CI $[-.03, .02]$). However, when a dud was present, subjective probabilities assigned to the targets were higher than those assigned to the competitors ($M_{\text{diff}} = .07$, CI $[.04, .09]$). Notably, the three-way interaction between dud presence or absence, hypothesis type, and scale type was not credible, $b = 0.09$, CI $[-0.04, 0.22]$. The lack of the three-way interaction suggests that the violation of independence is not affected by the scale type.

Summary

We found the dud-alternative effect where the presence of a dud increased the subjective probability given to a target hypothesis in the verbal scale condition and increased the ratio of the subjective probability of a target hypothesis to the subjective probability of a competitor hypothesis in both verbal and numeric scales. Replicating Windschitl & Chambers (2004), the subjective probability for a target hypothesis was even higher when a dud was present than absent, implying a violation of regularity. However, this violation only occurred when subjective probabilities were reported using a verbal scale and not a numeric scale.

One interpretation of the effect of the verbal scale is that when people estimate subjective probabilities on a numeric scale they engage in more deliberative processing (System 2) and this reduces the effect of the dud (Windschitl & Chambers, 2004). Yet, it seems possible that how the response scale might prompt two systems of thinking is confounded with how the scale is used. Consider first that the violation of independence was present in both verbal and numeric conditions. Then consider what becomes apparent if we look closer at the violation of independence in both scale conditions. When the dud was absent, participants in both the verbal and the numeric conditions assigned similar

probabilities to the target ($M_{\text{diff}} = 0$, CI [-.05, .04]) or the competitor ($M_{\text{diff}} = .01$, CI [-.04, .06]). When the dud was present, the probabilities assigned to the target or the competitor were higher in the verbal condition than in the numeric condition. Notably, the difference of the probabilities assigned to the target was similar to the difference of the probabilities assigned to the competitor ($M_{\text{diff}} = -.02$, CI [-.07, .04]). Taken together, one possible reason that we did not detect an increase in the subjective probability assigned to the target in the numeric condition is that when there were three bicyclists/hypotheses some other factors shifted all the subjective probabilities down in the numeric condition. This shift resulted in a lower subjective probability on the competitor but was offset by the impact of the dud on the target. We suggest that the confound is in how the response scale is used. Participants in the numeric condition used the half-way point in the scale to describe two equally likely hypotheses and a point about one-third of the way up the scale for equally likely hypotheses. However, with the verbal scale—where there is more ambiguity in how to interpret the scale—participants treated the half-way point as the point for expressing a belief that events are equally likely for both two and three hypotheses. We explore this confound more in Studies 2 and 3.

Study 2: Similarity Effect

To further examine the violation of independence we found in Study 1, in this study, we asked whether the similarity effect occurs in subjective probability judgments. The similarity effect has been found in the choice domain (Tversky, 1972; Trueblood et al., 2013; Tsetsos et al., 2011) where a ressembler that is similar to the target (but not dominated by or dominating the target) decreases the attractiveness of the target compared to the competitors. In contrast to the dud-alternative effect, if the similarity effect exists in subjective probability judgments, then the addition of the ressembler should lower the subjective probability assigned to the target relative to the competitor creating another violation of the independence principle. We again used the predict-victor task, but

now the additional hypothesis was the ressembler hypothesis which was similar to the target hypothesis but was neither dominating nor dominated by the target hypothesis. Again half of the participants completed the task using a verbal scale and the other a numeric scale. The working hypothesis from Windschitl & Chambers (2004) was that the verbal scale would prompt more System 1 heuristic reasoning and thus would show a greater impact of the ressembler. But, from the previous study we suspected that at least part of the scale type effect could be due to differences in how participants used the scales. In particular, we were curious to what extent participants change (or not) how they interpret the midpoint of the verbal scale. To study this, we asked participants to make probability judgments among identical sets of two or three bicyclists so that we could compare the subjective probabilities given to hypotheses when they were equally likely.

Method

Participants. We recruited 90 participants (age: $M = 24$, 37 females, 53 males.) for the experiment from the community around the Max Planck Institute for Human Development in Berlin, Germany. Participants were paid an hourly wage of 12 € per hour. The study was approved by the Max Planck Institute for Human Development Institutional Review Board. Thirty two participants were randomly assigned to estimate probabilities with a numeric scale that stretched from “0%” to “100%”, 29 participants were assigned to estimate with a verbal scale that stretched from “Extremely Unlikely” to “Extremely Likely”, and 29 participants were assigned to an exploratory condition where participants were asked to choose the bicyclist who would win. The choice condition was an exploratory condition and we report the results in the Supplementary Material.⁴

We determined our sample size with the following logic. In Study 1, we had

⁴ We did not observe the similarity effect in choice condition where participants were asked to choose the bicyclist who would win the race. The analysis for the choice data is in the Supplementary Material Section S3.7.

approximately $N = 50$ participants per scale type condition. Within each condition there were ten triplets of bicyclists that were used to examine the dud-alternative effect creating 500 sets of observations which contributed to our estimate of the dud-alternative effect. As we could afford to have a total of about $N = 90$ participants with $N = 30$ in each condition we increased the number of experimental triplets to twenty to create a total of 600 sets of observations to contribute to our estimate of the similarity effect.

Materials, Design and Procedure. The experimental design was similar to Study 1. We again used the predict-victor task centered around a bicycle race, but generated 20 triplets to test for the similarity effect (see Supplementary Material Section S3.1 for the stimuli). Each of these triplets had a target, a competitor, and a ressembler. The ressemblers were neither dominating nor dominated by the targets, but rather had higher scores on one ability and lower on the other ability relative to the targets.⁵ We generated 3 triplets to test how the midpoints of the scales were used. Each of these triplets contains three identical bicyclists. We also randomly generated 20 triplets for the filler trials.

The procedure was the same as in Study 1 (See Figure 1 for a schematic diagram of the procedure). During the experimental phase, participants made 215 judgments about the probability that the bicyclist in the farthest right column of the table would win the race. For 129 of those trials, participants made judgments about the 43 triplets (20 experimental, 3 identical-hypothesis, and 20 filler), such that each bicyclist in the triplet would have their probability of winning the race judged once. For the other 86 trials, pairs were generated from each of the 43 triplets and participants judged the probability that one of the two bicyclists would win the race. In the experimental trials, the chosen pairs of bicyclists from each of the 20 experimental triplets were the targets and the competitors. In the identical-hypothesis trials and the filler trials, the pairs of bicyclists from each of the

⁵ Participants assigned different subjective probabilities to ressemblers and targets, but the difference was small (See Supplementary Material Section S3.4).

23 triplets were random. All pair-trials and triplet-trials were mixed randomly and divided into five blocks. Neither a pair nor a triplet of bicyclists appeared more than once per block. No feedback was given during the experimental task.

Results

Figure 3. Subjective probability that a bicyclist would win in tests for the violations of regularity and independence in Study 2. A smaller dot represents the average subjective probability assigned to the targets or the competitors per participant. The larger dots are the posterior predictive means. Error bars are the 95% predicted credible intervals.

Independence. If subjective probability judgments follow independence, then when ressemblers were present, the subjective probabilities assigned to the targets should not decrease more than the subjective probabilities assigned to the competitors. Figure 3 plots the probability judgments for both the target and the competitor with and without the ressembler for verbal and numeric scales. To test the independence violation, we ran a

Bayesian hierarchical linear regression regressing the probabilities assigned to targets and competitors onto (a) ressembler present or absent, (b) scale type (verbal; numeric), (c) hypothesis type (target; competitor), and (d) the interaction between the variables. See Table S15 in the Supplementary Material Section for the complete regression results. Consistent with a violation of independence, there was a credible interaction between the presence or absence of ressemblers and hypothesis type, $b = -.10$, CI $[-.18, -.02]$. Without ressemblers, the subjective probabilities assigned to the targets were similar to those assigned to the competitors ($M_{\text{diff}} = 0$, CI $[-.02, .02]$). However, when the ressemblers were present, the subjective probabilities assigned to targets were credibly lower than those assigned to the competitors ($M_{\text{diff}} = -.04$, CI $[-.06, -.02]$). Notably, the three-way interaction between the presence or absence of duds, hypothesis type and scale type was not credible, $b = .06$, CI $[-.06, .17]$. The lack of the three-way interaction suggests that the violation of independence is not affected by the scale type.⁶

Use of the Verbal and Numeric Scales. Recall that in Study 1, although we replicated Windschitl & Chambers (2004)’s finding that the regularity violation appeared using a verbal scale but not a numeric scale, we found the independence violation using both scales. This set of results led us to suspect that how the scale might prompt two systems thinking may be confounded with how the scale, especially the midpoint, is used. Following up on these speculations, we examined the degree to which participants might have changed how they used the scale on different numbers of hypotheses by asking participants to estimate the probabilities when the bicyclists had identical abilities. If participants have same midpoint interpretation on both scales, then participants should assign the same subjective probabilities for one of those two or three identical bicyclists

⁶ We examined other aspects of subjective probability: the degree to which the subjective probabilities aligned with the objective probabilities (see Supplementary Material Section S3.2) and the degree to which the subjective probabilities assigned to two (binary complementarity) and three mutually exclusive and exhaustive events (trinary complementarity) summed to one (see Supplementary Material Section S3.3).

Figure 4. The subjective probabilities in the identical-hypothesis trials. Each smaller dot represents the subjective probability assigned to a bicyclist by a participant in an identical-hypothesis trial. The larger dots are the posterior predictive means. Error bars are the 95% predicted credible intervals.

regardless of the scale type. Figure 4 plots the distribution of the subjective probabilities for one of two or three identical bicyclists for verbal and numeric scales. It shows that the verbal and the numeric estimates for one of two identical bicyclists were similar ($b = -.05$, CI $[-.26, .16]$), with both around $1/2$ ($M_{\text{verbal}} = .53$, CI $[.45, .61]$, $M_{\text{numeric}} = .50$, CI $[.42, .58]$). However, the verbal and the numeric estimates for one of three identical bicyclists tended to be different (though not credibly so), $b = -.18$, CI $[-.38, .01]$. The verbal estimates were around $1/2$ ($M = .46$, CI $[.38, .55]$), while the numeric estimates are around $1/3$ ($M = .36$, CI $[.29, .44]$). Together these results suggest that participants used the numeric and the verbal scales differently when there are two vs. three bicyclists.

Summary

We found that a ressembler hypothesis impacted the support recruited to evaluate the hypotheses such that a ressembler seems to decrease the support of the target relative to the support of the competitor. This similarity effect resulted in a violation of independence. Notably, the similarity effect appeared using both verbal and numeric scales. These results are inconsistent with the working hypothesis from Windschitl & Chambers (2004) where use of the numeric scale was expected to prompt more deliberative thinking. We also found that the scale type impacted the equally likely judgments such that the numeric estimates were placed at the halfway point for two identical hypotheses and at the one-third point for three. For verbal estimates, participants tended to always use the halfway point. This difference in how the midpoint of the scale is used helps potentially explain the apparent differences in the violation of regularity from the first study. We hypothesized that the regularity violation would be found for those “midpoint users”, who use the midpoint of a scale to indicate equally likely, but would not be found for “ratio users”, who use the ratio of one to the number of identical hypotheses to indicate equally likely. We explore this hypothesis in Study 3 where we examined the dud-alternative effect and the similarity effect using a within-subjects design and we measured scale usage using identical-hypothesis trials.

Study 3: Dud-alternative Effect and Similarity Effect

We had two main aims for this study. The first aim was to replicate the dud-alternative effect and the similarity effect in subjective probability judgments with a different predict-victor task. In Studies 1 and 2, participants were asked to predict the outcome of a bicycle race. However, it was possible that bicycle competitors in a race might adjust their performance based on who they were competing against. For instance, bicyclists may change tactics if they are competing against other bicyclists who have similar climbing abilities. Thus, one might explain the observed violations of regularity and

independence as resulting from a belief that bicyclists change their racing strategies based on who they compete against. Although the required learning tasks participants completed should have reinforced that bicyclists' performance is independent to other bicyclists', it is possible that these beliefs still influenced participants' judgments. To address this confound, we developed a new version of the predict-victor task where participants were asked to predict a race between unpowered (inanimate) wooden cars.⁷ In this race cars were released on a tilted race track and participants predicted the probability that one of the cars would pass the finish line at the end of the track first. Participants made their judgments based on the amount of weight added to the back of the car and the amount of friction the cars exhibited. Participants could not infer possible racing tactics because the cars, as inanimate objects, are unable to adjust their performance, and thus the possible confound was eliminated.

We also aimed to better explore how the scale usage may have impacted the probability judgments within the dud-alternative effect and in turn impacted the regularity violation we observed in Study 1. Thus, as in Study 2, we asked participants to make several judgments about cars with identical attributes so that we could identify midpoint users, who use the midpoint of a scale to indicate equally likely, and ratio users, who use the ratio of one to the number of identical hypotheses to indicate equally likely. If the regularity violation we observed in Study 1 was due to using the midpoint of the scale to indicate equally likely, then midpoint users should violate regularity and ratio users should follow regularity regardless of what type of response scale they used when testing the dud-alternative effect.

Method

Participants. In total, we recruited 193 participants who passed the attention and comprehension checks (See Table S18 in the Supplementary Material for the attention and

⁷ We thank Gryffin Eason for this invaluable reframing of the task.

comprehension checks). We determined our sample size using the following logic. This study was designed to test the dud-alternative and the similarity effects together. To do so while keeping the study about an hour in duration, we identified 8 triplets of competitors designed for testing each effect. Recall that in Studies 1 and 2, across participants we had approximately 600 sets of observations per effect within each scale condition (e.g., 20 triplets $\times$ 30 participants in Study 2). Thus, we set a goal of at least $N = 75$ participants per scale condition, which allowed us to maintain about 600 sets of observations to examine each effect as in Studies 1 and 2. However, the study was online and we anticipated needing to recruit more participants to offset incompletions or other issues. Consequently, we set a goal of a total of 190 participants or about 20 additional participants per condition. Participants were paid an hourly wage of \$12 per hour plus an accuracy bonus. We analyzed the data from 183 participants who completed the whole experiment (See Supplementary Material Section S4.2 for more details about participants). The study was approved by the University of Kansas Institutional Review Board.

Materials and Design. We adapted the predict-victor task to use inanimate objects to address a possible confound in the bicycle race where participants may have inferred that bicyclists adjust their biking style based on who they are competing against and thus created what looked like a violation of regularity or independence. In this case, we created a predict-victor task centered around predicting a race between wooden cars modeled after the pinewood derby competition often run by the Cub Scouts in the United States. Participants were told that a neighbor’s child had a collection of unpowered wooden cars with simple wheels and axles. In each trial, the child raced two or three cars from the top of a tilted race track. Participants were asked to predict the probability that one of the cars passed the finish line at the end of the race track first.

To make their predictions, participants were provided two pieces of information: how much extra weight was added on the back of the car (weight) and the time taken to pass a friction test (friction). Weight can be added to the back of a car to increase its potential

energy by slightly raising the center of gravity and thus increasing the chance that the car would cross the finish line first. According to the neighborhood racing records, the heaviest extra weight added to the back of a car was 105 g, the average was 45 g and the lowest was 0 g. In terms of friction, participants were told that the neighborhood measured friction by placing every car on a perfectly horizontal 10 foot long track and giving them an identical initial speed. The time it takes a car to roll to the end of that track is a measure of the friction so that the faster the time, the lower the friction, and the greater the chance the car would cross the finish line first. According to the neighborhood club, the longest time a car took to roll the 10 ft track was 8.9 s, the average time was 5.6 s, and the shortest time was 2.4 s. Participants were reminded that weight and friction were independent and that both were equally important to win a race. As with Studies 1 and 2, the probability of a car winning was determined with the logistic expression (Equation 4) using normalized weight and friction values.

We adapted 8 triplets from Study 1 to test the dud-alternative effect. We adapted 8 triplets from Study 2 to test the similarity effect.⁸ We also generated 3 identical-hypothesis triplets to measure scale usage and 10 triplets with random scores for filler trials. See Supplementary Material Section S4.1 for the stimuli used in the experimental trials.

Like Studies 1 and 2, participants entered their judgments by selecting a point along a scale, but due to programming constraints for the online task participants moved a pointer along a straight line to select a probability. The scale stretched from 0% to 100% in the numeric condition and from “Extremely Unlikely” to “Extremely Likely” in the verbal condition. The pointer was set at a random position on the scale at the beginning of each trial.

⁸ Participants assigned lower probabilities to duds than targets and assigned similar probabilities to resemblers and targets (See Supplementary Material Section S4.5).

Figure 5. A schematic diagram of the procedure performed in Study 3.

Procedure. The procedure in this study was similar to Studies 1 and 2. Figure 5 plots the schematic diagram of the procedure performed in this study. The study proceeded in two stages. In the training phase, participants were familiarized with how to use added weight and friction to make predictions and how to use the numeric or verbal scale to make subjective probability judgments. In the first 10 practice trials, participants were shown three cars' weight and friction-test time. They were asked to select which car would win the race. Similar to Studies 1 and 2, after each trial, participants were informed the race's outcome and the frequency out of 100 times that each car would have won. After the 10 practice trials, participants were asked to complete three attention and comprehension checks. Those participants who passed all the checks entered the second phase of training. Participants were given 10 practice trials to familiarize them with the reward system we used in the experimental phase, where participants were rewarded for accuracy using a linear transformation of the quadratic scoring rule⁹ (von Holstein, 1970). At the end of each of these trials, participants were told the outcome of the race and how many points they earned. Points from the training phase did not carry over into the experimental phase. With the quadratic scoring rule, the possible points per trial ranged from 0 to 100 points.

During the experimental phase, participants made 145 judgments about the probability that the car in the farthest right column of the table would win the race. The position of weight and friction-test time in the table was randomized across participants. Similar to Study 2, for 87 of those trials, participants made judgments about the 29 triplets

⁹ In each trial, we simulated a race using the probabilities of cars winning determined by the logistic expression (Equation 4). The reward $r(w)$ that a participant could have in that trial is defined as

$$r(w) = 100[1 - (I_w - pr(w))^2]. \quad (6)$$

where I_w denotes the indicator function

$$I_w = \begin{cases} 1, & \text{if the car } w \text{ won the simulated race} \\ 0, & \text{if the car } w \text{ lost the simulated race} \end{cases}$$

and $pr(w)$ denotes the probability that participant gave.

(16 experimental, 3 identical-hypothesis, and 10 filler), such that each car in the triplet would have their probability of winning the race judged once. For the other 58 trials, pairs were generated from each of the 29 triplets (16 experimental, 3 identical-hypothesis, and 10 filler) and participants judged the probability that one of the two cars would win. Each of a chosen pairs of cars from an experimental triplet were the target and the competitor. Each of a chosen pair from an identical-hypothesis or filler triplet were two randomly selected cars. All pair-trials and all triplet-trials were mixed randomly respectively. Participants were randomly assigned to complete all pair-trials first or all triplet-trials first. Although no feedback on the outcome of the races was given, participants were told how many points they had earned every 20 trials. Participants were randomly assigned to estimate probabilities using a verbal scale or a numeric scale. In the analysis, we mapped the responses on the verbal scale to the same 0 to 100 scale as the numeric scale.

Results

We had two aims in this study. The first aim was to replicate the dud-alternative effect and the similarity effect in subjective probability judgments with a different predict-victor task. The second aim was to examine how scale type and scale usage may have impacted the probability judgments. It is useful to begin with the second aim as it turned out to play an important role in our understanding of the dud-alternative effect and the similarity effect.¹⁰

Scale Usage. As in Study 2, across several trials, we presented participants two or three cars that had identical attributes and we asked participants to judge the probability of one of the cars would win the race. If participants used the scale like a probability scale,

¹⁰ We examined other aspects of subjective probability: the degree to which the subjective probabilities aligned with the objective probabilities (see Supplementary Material Section S4.3) and the degree to which the subjective probabilities assigned to two (binary complementarity) and three mutually exclusive and exhaustive events (trinary complementarity) summed to one (see Supplementary Material Section S4.4).

Figure 6. The subjective probabilities assigned to one of two or three identical cars in Study 3. Each dot indicates the subjective probability that a participant assigned to a car in an identical-hypothesis trial.

we expected that when two identical cars were under consideration, they would select a point at about the half-way point on both the numeric and the verbal scales. When three identical cars were under consideration, we expected participants to select a point at about one-third the way from the bottom anchor (“0%” in numeric and “extremely unlikely” in verbal). However, based on the previous studies, we also expected that some participants would deviate from these expectations and tend to enter a judgment at the half-way point for both two and three-car conditions, especially when they use a verbal scale. Figure 6 plots the judgments as a function of the number of identical cars. As with Study 2, there was a notable tendency to use the half-way point when entering the probability judgment for the three-car condition. This tendency to use the half-way point was particularly

prevalent in the verbal scale. But, the top panel of Figure 6 also shows that there appears to be a fair number of mid-point users in the numeric scale condition.

Figure 7. Percentage of participants classified as midpoint users, ratio users, and others in Study 2 (left column) and Study 3 (right column) in the verbal (top row) and the numeric conditions (bottom row).

To better understand how scale usage changed between the different scale types, we

classified participants into three types of scale users: midpoint users, ratio users and others. If participants' averaged subjective probabilities in the identical-hypothesis trials were between .40 to .60 in both the two-car three-car conditions, then we classified them as midpoint users; if participants' averaged subjective probabilities in the identical-hypothesis trials were between .40 to .60 in the two-car condition and between .20 to .40 in the three-car condition, then we classified them as ratio users; participants not meeting these conditions were classified as others. Figure 6 shows the distributions of subjective probabilities assigned to identical hypotheses for each of these categories. Figure 7 shows the percentage of participants that fell in each category for Study 3 (right column). We included Study 2 (left column) for comparison. We ran a multinomial logistic regression where the scale usage (midpoint users; ratio users; others) was regressed onto scale type (verbal; numeric). The results show that participants who used a verbal scale relative to who used a numeric scale were more likely to be classified as midpoint users relative to ratio users ($b = 1.48$, $CI = [.80, 2.18]$). We did the same analysis using the data from Study 2 and got a similar result ($b = 1.94$, $CI = [.77, 3.20]$).

Altogether the results indicate that the scale type impacted how participants used the scale. Recall, we speculated that how participants used the scale may account for the observed violation of regularity in Study 1. Therefore, in all subsequent analyses we included a variable indicating how they used the scale (ratio users, midpoint users, or others), and all the interactions with the manipulated variables. Note that although we included other users in the regressions, our analyses focused on ratio users and midpoint users. We did this for two reasons. First, we do not have clear predictions on how other users should behave in the dud-alternative effect and the similarity effect. Second, the sample size of other users is too small to make convincing static inference. Next we analyze the results in terms of the dud-alternative effect and then the similarity effect.

Dud-alternative Effect. Like Study 1, we examined whether the dud-alternative effect resulted in violations in the regularity and the independence principles separately.

Figure 8. Subjective probabilities that midpoint users and ratio users assigned to targets and competitors in test of the dud-alternative effect in Study 3. The smaller dot represents the average subjective probability assigned to the targets or the competitors per participant when duds were absent or present. The larger dots are the posterior predictive means. Error bars are the 95% predicted credible intervals.

Regularity. Recall, if subjective probability judgments violate regularity, the subjective probability assigned to the target was higher when the dud was present than when it was not. To test the regularity violation in Study 3, for the experimental trials for testing the dud-alternative effect we conducted a Bayesian hierarchical linear regression regressing the probability judgments assigned to targets onto (a) dud present or absent, (b) scale type (verbal; numeric), and (c) scale usage (midpoint users; ratio users; other users), and (d) all the interactions between these variables. See Table S23 in the Supplementary Material Section for the complete regression results. Figure 8 plots the subjective probabilities assigned to the target when the dud was absent and present when participants used the verbal (top row) and numeric (bottom row) scales. As the left panels of Figure 8 show verbal-midpoint users ($M_{\text{diff}} = .14$, CI [.06, .22]) and numeric-midpoint users ($M_{\text{diff}} = .16$, CI [.03, .28]) showed a credible regularity violation. But as the right panels of Figure 8 shows verbal-ratio users ($M_{\text{diff}} = .09$, CI [-.03, .20]) and numeric-ratio users ($M_{\text{diff}} = .03$, CI [-.06, .11]) showed no regularity violation. The pattern implies that midpoint users were more likely to exhibit the regularity violation. The results also show that, across midpoint users and ratio users, scale type had no credible impact on the regularity violation ($M_{\text{diff}} = -.02$, CI [-.13, .08]).¹¹ Thus, following our speculation from Study 1, the results are consistent with our hypothesis that midpoint users violate regularity but not ratio users when they used a verbal or a numeric scale.

Independence. A violation of independence occurs when the relationship between the subjective probabilities assigned to the target and the competitor changes when the dud was present vs. absent. To test the independence violation in the dud-alternative effect, for the experimental trials for testing the dud-alternative effect, we ran a Bayesian hierarchical linear regression regressing the probability judgments assigned to targets or

¹¹ If we also consider other users, scale type had a marginally credible impact on the regularity violation ($M_{\text{diff}} = -.10$, CI [-.19, 0]). There was a violation of regularity in the verbal condition ($M_{\text{diff}} = .17$, CI [.11, .23]) but not in the numeric condition ($M_{\text{diff}} = .07$, CI [-.01, .15]).

competitors onto (a) dud present or absent, (b) scale type (verbal; numeric), (c) scale usage (midpoint users; ratio users; other users), (d) hypothesis type (target; competitor), and all the interactions between these predictors (See Table S24 in the Supplementary Material Section for the complete regression results.). Focusing on the midpoint users and the ratio users, as Figure 8 shows these participants assigned similar probabilities to the targets and the competitors ($M_{\text{diff}} = -.01$, CI $[-.06, .04]$) when the dud was absent. But, when the dud was present, higher probabilities were assigned to the targets than to the competitors ($M_{\text{diff}} = .14$, CI $[.09, .19]$). Midpoint users did not show a credibly stronger independence violation than ratio users ($M_{\text{diff}} = .11$, CI $[-.04, .26]$). And among those midpoint users and ratio users, the scale type also had no credible impact on the independence violation ($M_{\text{diff}} = .04$, CI $[-.10, .19]$).¹² These results show that, in terms of the dud-alternative effect, both midpoint users and ratio users violated the independence principle when they used verbal and numeric scales.

Similarity Effect and Independence. Recall that according to the similarity effect when the ressembler that is similar to the target (but not dominated by or dominating the target) is added to the hypotheses being considered, it decreases the support of the target compared to the competitors creating an independence violation. To test the independence violation in the similarity effect, for the experimental trials for testing the similarity effect, we ran a Bayesian hierarchical linear regression regressing the probability judgments assigned to targets or competitors onto (a) ressembler present or absent, (b) scale type (verbal; numeric), (c) scale usage (midpoint users; ratio users; other users) and (d) hypothesis type (target; competitor), and all the interactions between these predictors (See Table S25 in the Supplementary Material Section for the complete regression results. Figure 9 plots the subjective probabilities assigned to the targets and the competitors when the ressembler was absent and present when midpoint users (left column) and ratio users

¹² After including other users to the simple analysis, the scale type also has no credible impact on the independence violation ($M_{\text{diff}} = 0$, CI $[-.14, .14]$).

Figure 9. Subjective probabilities that midpoint users and ratio users assigned to targets and competitors in test of the similarity effect in Study 3. The smaller dot represents the average subjective probability assigned to the targets or the competitors per participant when ressemblers were absent or present. The larger dots are the posterior predictive means. Error bars are the 95% predicted credible intervals.

(right column) used a verbal (top row) and a numeric (bottom row) scale. We first focused on midpoint users. As the left panels of Figure 9 showed, verbal-midpoint users and numeric-midpoint users assigned similar probabilities to the targets and the competitors when resemblers were absent ($M_{\text{diff}} = -.04$, CI $[-.12, .04]$; $M_{\text{diff}} = -.02$, CI $[-.14, .09]$) and present ($M_{\text{diff}} = 0$, CI $[-.08, .08]$; $M_{\text{diff}} = 0$, CI $[-.12, .11]$). Consequently, for midpoint users there was no evidence of an independence violation. Focusing on ratio users, as the right panels of Figure 9 showed, verbal-ratio users ($M_{\text{diff}} = -.02$, CI $[-.13, .08]$) and numeric-ratio users ($M_{\text{diff}} = .01$, CI $[-.07, .09]$) assigned similar probabilities to the targets and the competitors when resemblers were absent. But when resemblers were present, verbal-ratio users ($M_{\text{diff}} = -.08$, CI $[-.18, .03]$) and numeric-ratio users ($M_{\text{diff}} = -.09$, CI $[-.16, -.01]$) showed a tendency to assign lower probabilities to the targets compared to the competitors. This pattern of results is consistent with a violation of independence. However, the strength of the independence violation was not strong for ratio users across verbal and numeric scales ($M_{\text{diff}} = -.07$, CI $[-.17, .02]$).¹³

Given the weaker similarity effect, we further explored the effect at the item level. To do so, we reran the Bayesian hierarchical linear regression for the similarity effect, but now included the triplet index (1 to 8) as a predictor.¹⁴¹⁵ By looking at the similarity effect at

¹³ We examined whether scale usage impacted on the independence violation in Study 2. See Table S16 in the Supplementary Material Section for the complete regression results. The results show that both midpoint users and ratio users assigned similar probabilities to the targets and the competitors when the resemblers were absent ($M_{\text{diff}} = .01$, CI $[-.04, .05]$; $M_{\text{diff}} = 0$, CI $[-.03, .04]$). They tended to assign lower probabilities to the targets than the competitors when the resemblers were present ($M_{\text{diff}} = -.04$, CI $[-.08, 0]$; $M_{\text{diff}} = -.02$, CI $[-.06, .01]$). The strength of the similarity effect between midpoint users and ratio users was not credibly different ($M_{\text{diff}} = .02$, CI $[-.06, .09]$).

¹⁴ The complete regression tables for the item level analyses could be found in the Open Science Framework: [link](#).

¹⁵ We also analyzed the dud-alternative effect at the item level and found no systematic differences across items. See Supplementary Section S4.6.3

the individual triplet level, two clusters of triplets became apparent. In one cluster (Triplet 1-4) participants assigned lower probabilities to targets compared to the competitors when the resemblers were absent ($M_{\text{diff}} = -.32$, CI $[-.37, -.27]$). In a second cluster (Triplet 5-8), participants assigned higher probabilities to targets compared to the competitors when the resemblers were absent ($M_{\text{diff}} = .28$, CI $[.22, .33]$). This difference between clusters is associated with the presence of the similarity effect and the violation of independence.

Table 1

The results of the simple regression analyses of testing the similarity effect at item level in Study 3.

Triplets	Scale Usage	Scale Type	Targ		Comp		Targ - Comp	
			M_{diff}	95% CI	M_{diff}	95% CI	M_{diff}	95% CI
1-4	Mid	Verbal	-.11	[-.20, -.01]	.08	[0, .16]	-.19	[-.31, -.07]
		Numeric	.08	[-.05, .21]	.06	[-.07, .19]	.02	[-.16, .21]
	Ratio	Verbal	.03	[-.09, .16]	.10	[-.02, .22]	-.07	[-.24, .11]
		Numeric	.04	[-.05, .14]	.03	[-.05, .11]	.01	[-.11, .14]
5-8	Mid	Verbal	.24	[.15, .32]	.12	[.03, .20]	.13	[.01, .24]
		Numeric	.08	[-.06, .22]	.13	[.01, .26]	-.06	[-.24, .13]
	Ratio	Verbal	.22	[.09, .35]	.04	[-.09, .17]	.18	[0, .36]
		Numeric	.27	[.18, .36]	.09	[0, .17]	.18	[.06, .31]

Notes. The table shows the difference of the posterior predictive distribution of targets (Targ), competitors (Comp), or the difference of targets and competitors (Targ - Comp) when the resemblers were absent and present. Mid stands for midpoint user. Ratio stands for ratio user.

Table 1 summarizes the posterior estimates of the difference in the mean probability judgments assigned to the target when the ressembler was absent or present, the corresponding values for the competitor, and the interaction between hypothesis type and absence vs. presence of ressembler for these clusters of trials. Figure 10 plots the subjective

Figure 10. Subjective probabilities that midpoint users (1st and 2nd columns) and ratio users (3rd and 4th columns) assigned to targets (Targ) and competitors (Comp) in tests of the similarity effect in Triplet 1-4 or Triplet 5-8. The smaller dot represents the average probability assigned to the targets or the competitors per participant when the resemblers were absent or present. The larger dots are the posterior predictive means. Error bars are the 95% predicted credible intervals.

probabilities assigned to the targets and the competitors in Triplet 1-4 and in Triplet 5-8 when the resemblers were absent and present for the midpoint users (Mid) and the ratio users (Ratio) when they used the verbal or numeric scales. As the top row of the figure shows, no similarity effect occurred in Triplet 1-4 and for most of the participants the probabilities assigned to targets and competitors when the resemblers were absent were

similar to when the resemblers were present. In comparison, in Triplet 5-8, the similarity effect tended to occur in Triplet 5-8. These results indicate that the similarity effect was more likely to occur when the probabilities assigned to targets were higher than the competitors when the resemblers were absent.

Summary

We replicated the dud-alternative effect using a new predict-victor task. We found the regularity and independence violations in the dud-alternative effect when participants used the numeric and the verbal scales. However, midpoint users showed the regularity violation but ratio users did not. We believe that the regularity violation was mostly driven by scale usage. More specifically, participants used midpoints to indicate equally likely when the duds were presented, hence, creating the violation of regularity.

We found a weak similarity effect and a weak independence violation in this study. Exploring the effect further we found some item-level differences in the similarity effect: We found some indications that the similarity effect were more likely to occur when the probabilities assigned to targets were higher than to the competitors when the resemblers were absent. One of the possible explanations for not observing the similarity effect when the probabilities assigned to the targets were lower than the competitors when the resemblers were absent is the flooring effect. In those situation, the support for the targets was already low. Therefore, the support could not be significantly lower when the resemblers were present.

Discussion

In our study, we found the dud-alternative effect and the similarity effect in subjective probability judgments: Adding an additional dud hypothesis can boost the subjective probability assigned to the target hypothesis while adding an additional resembler hypothesis can decrease the subjective probability assigned to the target more deeply relative to the competitor hypothesis. We also found that the ability to detect these

effects in probability judgments depends on how participants used the response scale. More broadly, these context effects invalidate the support invariance assumption of support theory, and are revealing in terms of the cognitive processes that drive belief and subjective probability judgments. We turn to these topics now.

The support invariance principle is a key component of support theory (Tversky & Koehler, 1994; Rottenstreich & Tversky, 1997). The philosophy under this principle is that the evidence or support that our mind generates for a hypothesis under consideration can be represented with a single fixed numeric value. This stable mapping relationship aids in the computability of subjective probability from the support assigned to the hypotheses as doing so only requires knowing the support for each hypothesis (e.g., Fox, 1999; Koehler, 1996; Koehler et al., 1997; Tversky & Koehler, 1994). The presence of the dud-alternative effect and the similarity effect implies that the interdependencies between the hypotheses under consideration must also be accounted for. One way that they could be accounted for is to model the joint distribution of support generated for all the hypotheses under consideration, such as has been done in different ways in the choice literature (Meyer & Kahn, 1991; McFadden, 2001). However, some of these adjustments are rather ad hoc and do not explain why effects occur, making it difficult to predict when effects will occur (Rieskamp et al., 2006).

A different approach is to develop a generative model of support and subjective probability judgments. The hypothesis that a verbal scale prompts more System 1 thinking and a numeric scale prompts more System 2 thinking and thus more context effects might intuitively be expected with a verbal scale is a step in this direction (Windschitl & Chambers, 2004; Windschitl & Weber, 1999). However, it would seem that this two systems explanation might not be able to account for the violation of independence we found across the three studies and the violation of regularity we found in the numeric condition in Study 3. We suspect that the regularity violation we observed could be accounted for by how people use the response scales. As we showed in Studies 2 and 3 there is some indication that

people treat the response scales differently. Some participants (midpoint users) tend to use the half-way point of the scale when reporting subjective probability judgments that are equally likely, regardless of the number of hypotheses. This tendency was most prominent when participants entered their response on a scale with verbal anchors (“Extremely Unlikely” to “Extremely Likely”). Other participants (ratio users) shifted where they made subjective probability judgments that are equally likely to correspond to a position that is $1/n$ of the way up the scale, where n is the number of hypotheses being considered. In Study 3, we showed that midpoint users violated regularity (regardless if they used a verbal scale or even a numeric scale). In contrast, ratio users showed no credible regularity violation. These results suggest that specific regularity violation that can arise via the dud-alternative effect may be largely driven by how people enter their beliefs on a response scale. There is at least two implications from this conclusion. First, it would seem fair to say that evidence for a violation of regularity is confounded with how a response scale is used meaning that a successful theory of probability judgments may not have to explain how a violation of regularity may arise from beliefs, but how it arises in the response process. Second, returning to the claim that System 1 thinking may lead to context effects, the effect of response scale on the violation of regularity would seem to weaken this claim.

A different explanation for the context effects is that they may arise via the process by which people sequentially sample and accumulate support for the hypotheses under consideration. A similar process seems to explain context effects observed in the decision making domain (Berkowitsch et al., 2014; Cataldo & Cohen, 2020; Evans et al., 2019; Trueblood et al., 2014; Roe et al., 2001; Rieskamp et al., 2006; Turner et al., 2018). According to these models, preference between choice options is constructed by sequentially evaluating attributes of the choice options, comparing the evaluations across options, and accumulating these evaluations to form a preference for a particular option (Bhatia, 2013; Bussemeyer & Townsend, 1993; Roe et al., 2001; Usher & McClelland, 2001, 2004; Trueblood et al., 2014). Each model posits a different processing architecture and as a

result the precise explanation of each context effect varies. However, it seems an exciting area of future investigation is how these models might be adapted to explain belief formulation and the context effects reported here and in Pleskac (2012). Curiously, we found the similarity effect in subjective probability judgment but not in choice with the same stimuli (see Supplementary Material Section S3.7). This result indicates that the cognitive mechanism between choice and probability judgment might be not entirely the same, but perhaps just similar. Nevertheless, a dynamic account of subjective probability judgment could help advance our understanding of belief formation and forecasting.

Conclusion

Tversky and Koehler (1994) summarized the contribution of support theory as establishing that, "...subjective probability, or degree of belief, is nonextensional and hence nonmeasurable in the sense that hypothesis partitions of the space can yield different judgments. Like the measured length of a coastline, which increases as a map becomes more detailed, the perceived likelihood of an event increases as its description becomes more detailed" (p. 565). Analogously, just as a given coastline might seem long or short depending on the alternatives it is compared against, our studies have shown that the support people recruit for a particular hypothesis occurring depends on the alternative hypotheses being considered. Obviously, the relative nature of subjective probability judgments naturally implies that the perceived likelihood of an event will depend upon the hypotheses it is being compared against. But, this systematic change in support implies the perceived likelihood of an event is systematically impacted even further by the descriptions of alternative events. The inclusion of dud hypotheses can strengthen the support assigned to a focal hypothesis and thus increase the perceived likelihood of it occurring, while ressembler hypotheses appear to weaken the support assigned to hypotheses reducing their perceived likelihood. Isolating the roll context effects like these play in intuitive judgments of uncertainty may help us further understand how these

judgments are made and perhaps improve the quality of these judgments.

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Declaration of Competing Interest

None.

CRedit authorship contribution statement

Xiaohong Cai: Conceptualization, Investigation, Formal analysis, Writing - Original Draft, Writing - Reviewing & Editing. Timothy J. Pleskac: Investigation, Formal analysis, Writing - Reviewing & Editing, Supervision.

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