Summary of Paper

"Tradeoffs and Comparison Complexity" examines the role of tradeoffs in informing comparison complexity. The authors propose a theory of comparison complexity and flesh out its details in the context of multiattribute, lottery, and intertemporal choice. The authors model a decision maker (DM) who faces a set of alternatives and receives a noisy signal concerning their value, which is informed by the ease of comparison between alternatives, τ_{xy} for alternatives x and y. The authors ground their theory in two reasonably simple principles: comparisons are easier when alternatives are similar and are maximally easy when one alternative dominates the other. Importantly, the authors take the comparability of alternatives to be an increasing transformation of the value-dissimilarity ratio, $\frac{|v_x - v_y|}{d(x,y)}$, where the numerator is the difference in the value of alternatives x and y and the denominator is their dissimilarity.

In elaborating on their theory in the context of their three aforementioned settings, the authors provide more structure on the components of the value-dissimilarity ratio and the probability of making a given choice, as well as provide axiomatic foundations for their characterizations. Through this setup, the authors offer a tractable model with parameters identifiable from binary choice data. They then extend this setup to multinomial choice. The model presented by the authors has a number of interesting implications, including that comparison complexity leads to noisy but unbiased choice under binary menus, but produces systematic biases in larger menus due to differences in the ease of comparison of any two options against the rest of the menu. The authors argue that the principles embedded in their model can rationalize context effects, preference reversals, probability weighting, and hyperbolic discounting.

The authors put the predictions of their model to the test in a series of experiments that span the three settings they consider. First, as a form of validation, the authors show that their proposed complexity measures are predictive of of choice noise and errors. Second, the authors demonstrate that they can eliminate common preference reversals by reducing comparison complexity. Finally, they demonstrate that they can flip the standard results from probability weighting and hyperbolic discounting by eliciting valuations in terms of probability and time equivalents, and hence switching the dynamics of what alternatives are easily comparable. Moreover, the authors conduct an exercise of benchmarking their model against existing models, showing that it performs as well or better in terms of explaining the data's variation, without sacrificing parsimony, as measured by a restrictiveness exercise.

The authors then apply their model to study strategic obfuscation in markets. In examining the proliferation of forms of similar products varying qualities such as quantity and superficial features, they contrast the implications of their model against an alternative explanation of heterogenous consumer preferences. They begin by demonstrating that strategic obfuscation aimed at inducing comparison complexity can produce the same product proliferation result as heterogenous preferences. They then turn to a discussion of differentiating the two explanations, such as through the use of multinomial choice data.

Comments

Overall, the paper is very interesting and offers a cohesive explanation for a number of important behavioral phenomena. It has strengths in the tractability of the model, the intuitive logic grounding the claims, and the compelling experimental evidence.

I'm somewhat confused by the construction of the signal s_{xy}:

$$s_{xy} = \operatorname{sgn}(v_x - v_y) + \frac{1}{\sqrt{\tau_{xy}}}\epsilon_{xy},$$
$$\epsilon_{xy} \sim N(0,1)$$

It seems like this is saying that there are two components of the signal outside of the noise, the sign of the value difference, which provides a discrete jump, and the ease of the comparison, which scales the noise. It seems somewhat strange that the change to the signal would be so discrete in the sign difference and that all of the information about the value difference beyond sign would be embedded within ease of comparability scaling the noise. Perhaps this is sensible; the expression for $\rho(x, y)$ that it yields is attractive, but it would be nice to have more justification for this construction.

In the discussion of the simplification property in the multiple-attribute setting, it may be worth making it more clear that decreasing the number of attributes only weakly simplifies the comparison complexity. This is alluded to in the footnote, though the language in the text and example is such that it could be misread as strict. An example in the same setup as used in the text where it would be weak is

$$x = (10, 10, 10, 10) \qquad x = (10, 10, 10, 10)$$
$$y = (5, 15, 5, 15) \qquad y' = (10, 10, 0, 20)$$

It seems there are intuitive conditions under which it would be strict, for which it may be beneficial to lay out the logic underpinning them. I think this is not so important, given how it appears throughout the paper, but just a small point.

While nicely designed, there is always a question of external validity with this type of endeavor. These experiments are highly stylized and generally quite different than the types of choices people face in the world, at least on some key experiential dimensions. For example, the authors use multiple price lists, which some researchers have expressed concern towards given the strange performance (e.g., multiple switching) many subjects exhibit in them. I think this is ultimately fine, particularly given norms of the field and the strength of the evidence, but, in an ideal world, connecting these results to more naturalistic settings would be great. I can see the discussion of the strategic obfuscation in the market setting as a theoretical step in this direction.

I think it would be reasonable to spend more time justifying some of the functional form assumptions throughout the paper. This could be useful even in some component pieces, such as the $d_{L1}(x, y)$ function for the multi-attribute domain – to have the coefficients on the attribute differences for distance be the coefficients on the feature in the utility function has intuitive appeal, but is of course practically a strong assumption on how people process the information. Is this expression sufficiently logical that we need not consider other options; alternatively, are the results robust to other reasonable distant metrics, etc.?

This is a truly tiny thing to highlight, but in the 5th footnote, the final closing ")" is missing in the expression: $H(r) = (\Phi^{-1}(G(r))^2$.