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**DANIEL E. BERLYNE (1924-1976):  
TWO DECADES LATER**

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**ABSTRACT**

In this remembrance, Berlyne's seminal ideas about collative motivation and its role in aesthetic phenomena are first briefly reviewed. It is noted that these ideas had not been developed into a formal theory in Berlyne's lifetime. Next, Martindale's negative conclusions about three aspects of the collative-motivation "theory"—a) the relative significance of collative, psychophysical, and ecological variables, b) the Wundt curve, and c) the trade-off between the variables that affect the arousal potential of stimuli—are challenged on conceptual and methodological grounds. It is further claimed that Berlyne's unfortunate fascination with the reticular arousal system sidetracked him from examining the role of sympathetic arousal, whereas the Hullian "baggage" blinded him to the involvement of emotion (with its cognitive, facial-musculature, and sympathetic arousal determinants) in various aesthetic phenomena. A number of pertinent experiments from Konečni's laboratory, which cause problems for both Berlyne's and Martindale's notions, are reviewed, with an emphasis on the importance of context and the type of dependent variable that is used in studying aesthetic phenomena. The article concludes with doubts about (proto)typicality as a key factor in aesthetic preference.

Soon after Daniel Berlyne's death, I was asked by Ernest Hilgard to write a necrology for the *American Journal of Psychology* [1]. In that piece, I presented a summary and a brief evaluation of Berlyne's contributions to experimental psychology and to psychological aesthetics, and also tried to convey personal details of potential interest to Dan's colleagues and the genuine sense of grief that we all felt.

In the meantime, *Empirical Studies of the Arts (ESA)* readers have seen Gerry Cupchik's piece that marked the passage of ten years [2]. On the same occasion, Konečni and Deutsch published a bibliography of Berlyne's writings in *Music Perception* [3]. A small group of colleagues became familiar with the extensive and remarkable interviews with Dan conducted by Roger Myers, a University of Toronto historian of psychology, in 1973 [4]. By the time the present piece appears, the XIVth Congress of the International Association of Empirical Aesthetics (an organization that Dan helped start), including a memorial symposium on Berlyne and the model of collative motivation [5], will have taken place in Prague (August 1-4, 1996).

Twenty years after Dan's death, the feeling of personal loss persists in the psycho-aesthetic community regarding this exceptionally fine human being and scientist of a very high caliber. With that stated, I shall, in the remainder of this remembrance, address mostly non-biographic issues and present a small, highly selective sampling of controversies to which Berlyne's seminal work gave rise.

### BERLYNE'S "THEORY" OF COLLATIVE MOTIVATION

Berlyne's chief objective, and the primary reason for his sustained major influence in psycho-aesthetics, was a multi-pronged attempt to bring aesthetic, including artistic, phenomena into the mainstream of experimental psychology. Theoretical (and, in part, paradigmatic) continuity with the work on infracreatives could apparently be achieved through concepts such as interest, curiosity, and exploratory behavior, which were nevertheless just sufficiently exotic in terms of the standard versions of learning theory to seem promising in the most exalted and elusive domains of human activity—science ("epistemic curiosity") and art. Judgments of beauty could be related to pleasure and to "reward," and all encompassed by the notion of "hedonic value." Objects of aesthetic and artistic interest could be defined in terms of broad stimulus classes, while information-theory notions of uncertainty and redundancy would provide a more sophisticated framework for Birkhoff's old complexity and order—this thinking eventually led to the formulation of the class of "collative" stimulus variables.

Importantly, the various stimulus classes, which at least hypothetically characterize works of art (construed collectively as having "arousal potential"), could affect the fluctuations in arousal that were controlled by the limbic system in the brain, especially the brain-stem reticular formation, while the latter was connected to the primary and secondary reward, and the aversion, systems. The modified (inverted-U) "Wundt curve" seemed neatly to summarize the relationship between the arousal potential and the hedonic value of relevant stimuli.

The collative-motivation model was never a formal theory—for example, à la Hull, whom Berlyne would have presumably held up as a model had he been ready for formally phrased and interrelated propositions. Yet, instead of being

seen for what it objectively was—a heuristically useful, but loosely organized, set of related ideas about psychological principles hypothetically involved in aesthetics and art—Berlyne's approach, most completely expressed in his 1971 book *Aesthetics and Psychobiology* [6], became posthumously reified. Broad strokes have been treated as formal propositions, and casually made quasi-theoretical intuitions subjected to seemingly rigorous, but narrow, experimental scrutiny, often ignoring, significantly, both the context that Berlyne must have taken for granted without spelling it out, and the context that he should have thought of, but apparently did not.

### CHALLENGING THE CHALLENGE: THREE EXAMPLES

In this section I will challenge the significance of the negative conclusions that have been reached about three broad brush strokes in Berlyne's theoretical composition. The issues are: a) The relative significance of collative variables; b) the applicability of the Wundt curve; and c) the trade-off between variables that affect the arousal potential of a stimulus.

The brief discussion will be limited to the work of Colin Martindale, who, as a cherished colleague, the editor of *ESA*, and a genuine admirer of Berlyne's contribution (personal communication), serves here as a perfect foil.

#### The Relative Significance of Collative Variables

Berlyne indeed often stated that collative variables were more important than the psychophysical and ecological ones for dealing with aesthetic issues. He had a hunch, correct I think, that art was somehow sufficiently different from mundane human functions that it might well be an area in which this class of statistical and/or structural variables would figure prominently. As many a theorist proud of an innovation has done, he might even have wanted to draw attention to the uniqueness of his contribution through an overemphasis on collative variables. It seems certain, however, that he did not thereby mean to imply that his "theory" would be falsifiable on this ground, or that he would be borne out in all conceivable situations, for all stimulus materials, subject samples, and so on, especially in terms of the percent-of-variance-accounted criterion used by Martindale and his colleagues [7, 8].

Martindale is well aware that Berlyne simply "proposed tentative hypotheses in need of further testing" [7, p. 53], and he is perhaps right in stating that some researchers may have been stifled by the erroneous belief that Berlyne himself conclusively answered the key questions he had raised. But Martindale, I believe, at least occasionally misjudged both what Berlyne had thought was truly important and, perhaps more significantly, the post-Berlynean research priorities in psycho-aesthetics. To return to the above example, would Berlyne really be

troubled by a finding [7, Experiment 4] that thirty-nine male undergraduates on a 7-point “like a lot—dislike a lot” scale are significantly more swayed by the “color typicality” than the number of turns of random polygons?

Despite its frequent use to manipulate “complexity” in visual psycho-aesthetic research of the synthetic variety, the number of turns is actually a poor indicator of *subjective* complexity, because of the repeatedly changing and alternating sign/supersign criterion as the number of turns increases (which Berlyne well knew). As for ecological stimulus properties, they “involve association with biologically noxious or beneficial conditions” [6, p. 69]; “[M]ost ecologically arousing stimulus patterns in art . . . are ones that have acquired learned associations with events or activities of biological importance” [6, p. 138]. Compare these Berlyne’s definitions, meant, I think, to explain why a trite national anthem moves grown people to tears, and why a Bach chorale or organ fugue creates awe and “chills” [9], to Martindale’s “[C]olor typicality may be viewed as an ecological property, because natural categories seem to be defined in terms of ‘family resemblance’ to a prototype that defines the category (Rosch, 1975)” [7, p. 64; 10].

Where are the associations with significant outcomes here? And does one really increase the big-picture understanding of the relative contribution of ecological and collative variables to the impact of the enormous variety of art “objects” by limited inductive efforts that find that rated “meaningfulness” of drawings and paintings (presumably reflecting the degree to which undergraduates find them representational) accounts for more of the variance on the like-dislike scale than does the rated complexity? [7, Experiments 5-7].

Berlyne’s claim regarding this issue was essentially untestable and thus Martindale’s conclusion of an empirical disproof is logically unsound. The only possible benefit, that of mildly disciplining discourse on the issue in our field, does not seem quite to justify the research effort expended. Had Martindale’s research been originally planned without the (illusory) possibility of disproof as one of its primary objectives, it may well have been an even more important contribution.

## The Wundt Curve

The notorious untestability of inverted-U functions, of which Berlyne was, of course, well aware, in itself indicates that he did not think of his “theory” as more than a general outline, a descriptive model. Paradoxically, the Wundt curve, in proper context, turned out to be Martindale’s nemesis more than Berlyne’s. The latter may well have thought of it (as I have, ever since taking Dan’s graduate seminar in “Aesthetic Behaviour” at the University of Toronto in early 1971) as a general relationship that holds between an infinite number and variety of art objects, from all epochs, and an infinite number of aesthetic judgments of all kinds, by populations of massive size, viewed historically. The Wundt curve may

be an untestable, yet intuitively pleasing and elegant, model of how both individuals and societies react hedonically to works of art, with the peaks of pleasure (by this or that measure) shifting in individual life spans and across historical periods with changing expectations about the appropriateness of the degree of presence of particular psychophysical, ecological, and collative variables in various art forms (and the differentially predictable violations of such expectations).

From this perspective, to obtain or not obtain data that fit an inverted-U function in this experiment or that [7, Experiments 1 and 4] is informative within the narrow boundaries of those experiments, but rather irrelevant for the "theory." Finding a monotonic relationship seldom disproves an inverted-U theory. Even a U-shaped set of results can be interpreted as being a local part of an overarching inverted-U relationship. By at least pretending to be taking Berlyne's Wundt-curve proposal so seriously as to claim to have disproved it experimentally, Martindale's opened his otherwise solid experimental work to methodological assault on a much broader scale.

Among the issues are the sampling and scaling of the independent variables, the mathematical form of the functions that relate various independent variables (present and omitted) to each other and to the variables that may mediate preference—for example, different types of arousal—definitional and operationalizational matters, the relationships among various measures of preference, and so on. All of these issues would have to be taken into account and *systematically* investigated in a research program that aspired to an epistemologically acceptable disproof. Needless to say, such a program would be much vaster than even Martindale's.

## The Trade-Off between Variables

Problems of similar magnitude confront attempts to prove Berlyne wrong on the issue of trade-off between the different variables that contribute to arousal potential. Berlyne indeed contended that the impact of the stimuli from the three classes should be additive, and more or less left it at that. Because this contention was made in the context of the Wundt curve relating combined stimulus strength (arousal potential) to hedonic value, Martindale correctly—in principle, at least—inferred, that there should be a trade-off between the three classes of variables. And the simultaneous factorial variation of stimuli from the different classes—Martindale's research strategy—in principle again, admirably serves the objective of testing the trade-off hypothesis.

The question, however, is: What can one justifiably and conclusively say about the correctness of a theoretical position as incompletely stated as Berlyne's on the basis of *any* result regarding the trade-off hypothesis, confirmatory or disconfirmatory? In my opinion, not much, because Berlyne never specified the

necessary scaling, functional, and measurement issues with sufficient explicitness. Here is a small, off-the-cuff, sample of relevant questions:

1. Is hedonic value an inverted-U function of each of the many individual variables belonging to the three stimulus classes?
2. Are additive effects as likely for combinations of variables from the three different classes as they are for those from the same class?
3. Does the effective range of each variable remain the same regardless of which other variables are present and with what strength they are present?
4. In other words, with regard to question 3, is it possible that certain combinations of variables have additive effects when certain of their respective regions are represented in a compound stimulus array, but various interactive ones in others, and in different overall stimulus constellations?
5. Is additivity perhaps more likely for that relatively small subset of combinations of variables (of the myriad possible combinations) that is found in intact works of art?
6. Are various rating scales (to remain, rather generously, at just this type of measure of hedonic value for the moment), such as, importantly, “pleasingness” and “interestingness,” equally likely to capture both the individual and the hypothesized additive effects of the different variables?
7. What is the relationship between “pleasingness” and “interestingness,” respectively, and the “like a lot—dislike a lot” scale used by Martindale?

Therefore, I am unimpressed when Martindale interprets the following experimental result as supportive of Berlyne’s “theory”: “The more complex a polygon was, the smaller the size that produced maximal preference” [7, p. 75, referring to Experiment 1]. Perhaps this would have made Dan happy, but it should not have. After all, had the finding not been obtained, one could have said:

“Ah, yes, but perhaps the preferred size diminishes with increasing complexity only for stimuli larger than  $80 \times 130$  cm, and then only for the golden-ratio members of a Fibonacci sequence; and, of course, the whole thing may work differently for dot patterns.”

Points of this type can, of course, be made about Martindale’s disconfirmatory conclusions on the basis of his Experiment 4:

“[T]here was a positive relationship between size and preference for prototypical colors but an inverted-U relationship for nonprototypical colors . . . Because typicality is an ecological variable [*meaning* is, but the relationship of meaningfulness to typicality must be extremely dependent on context, as well as on the interaction of context with the subjects’ constructions of what the “meaningfulness” rating scale reflects] and more typicality should lead to more arousal potential [just the opposite can easily be argued in many aesthetic contexts], this is the reverse of what [Berlyne’s] theory predicts” [7, p. 75].

Let the readers make their own conclusions about the justifiability of Martindale's reasoning in these matters of additivity and trade-off.

Berlyne's theoretical model is properly thought of as seminal precisely because it has inspired first-rate researchers, such as Martindale, to test its implications. In this regard, all appears—but only appears—fine. The problem with a many-pronged and many-layered, yet woefully incomplete, theoretical position, such as Berlyne's, is that it seduces researchers into ignoring its fundamental untestability and imputing to it aspects it objectively does not have. In my opinion, far too much precious experimental effort has been expended on the predictably inconclusive tests of the elusive "theory"—an intellectual investment that could have been more profitably spent on advancing the psychological understanding of *art*. Of course, I realize that psycho-aestheticians of good will can have a serious difference of opinion in this matter.

### NEUROPHYSIOLOGY, SYMPATHETIC AROUSAL, EMOTION

Equally honest differences of opinion probably exist about the usefulness of Berlyne's speculations on the neurophysiological basis of the Wundt curve. Since it now appears that his notions were factually wrong even in the early 1970s [11, various chapters], and are certainly outdated now [cf. 9], the point of bringing the matter up may be moot. And, as Martindale has pointed out [8, p. 432], "Berlyne certainly could have been correct [or not correct] about the Wundt curve regardless of anatomical or physiological details." However, some information about the background of this dubious foray of his into neurophysiology is of more than historical interest.

It may well have been Berlyne's desire to secure hard-science status for his work in psycho-aesthetics that gradually overcame his initial resistance to "reductionism" and led him to seek a neurophysiological explanation of the Wundt curve, based on the exciting contemporary work, specifically that on the rat and cat brains by Olds and Delgado, and, even more so, by the Russian work on the orienting reflex. Here is what Berlyne said to Myers in 1973:

"The old Yale [Hullian] party-line—which is similar to the Skinner party-line . . . —was that you should keep physiology out of . . . a theory of behavior . . . Hebb's [1949] book *The organization of behavior* is a protest against this . . . , [but] my previous training [partly Hullian] perhaps produced resistance [to Hebb's reasoning]. I became convinced rather later, when I was at Berkeley writing my book on *Conflict, arousal, and curiosity* and looking up the Russian literature on the orientation mechanism. And I discovered that some amazing things had been done in physiology, the reticular formation and various brain structures governing motivation. I thought that this was stuff you couldn't afford to ignore . . . I [now] think it's



really hopeless to try to build up models or theories in psychology without neurophysiology" [4, pp. 201-202].

The attraction—illusory or not—of observing physiological processes and events that accompany cognition with the high degree of spatial and temporal resolution provided by new techniques, such as positron emission tomography and functional magnetic resonance imaging, seems recently to have been pushing the purely cognitive, largely metaphorical (and even more faddish, if possible), computer-inspired approaches off center stage. My bet is that Dan would have been fond of these latest developments and been seduced, for better or worse, by the new cognitive neuroscience.

However, it would perhaps give him pause to remember that a premature embrace of a particular neurophysiological explanation may lead to too much time being spent on following false scents—in his own case, the reticular arousal system, and the reward and aversion systems. Reticular “arousal” was the conceptual heir to Hullian “drive” and “arousal level” was akin to “drive level” [6, p. 81]. Such preoccupations presumably led Berlyne to pay minimal attention to *sympathetic* arousal [6, p. 66], its interaction with cognitive processes, and its role in emotional states [12-14]—this last point not being surprising given Berlyne’s apparent discomfort, induced by standard learning-theory prejudices, about the epistemological status of the concept of emotion [6, p. 62].

Building a model of collative motivation to understand aesthetic phenomena and attempting to do so without the explicit inclusion of emotion and its numerous important determinants, concomitants, and modifiers (from sympathetic arousal to facial configuration to posture to paralinguistic cues to interpretive processes, etc.) is, in my opinion, a major shortcoming of Berlyne’s theoretical position, and one that severely restricts both its predictive power and the range of its applicability. A “new experimental aesthetics,” without emotion, is an unnecessarily self-limiting affair.

In experiments in my laboratory, it was found that the collative and psychophysical features of musical stimulus patterns independently, differentially, and additively, contribute to the subjects’ elevations in sympathetic arousal, but that the amount of the elevation was related to behavior only through emotion-related interpretation [15]. In other experiments, the subjects’ choice of exposure to melodies differing in complexity [16] and to differentially predictable rhythmic structures [17] was related to the intensity of the subjects’ emotional states—to which sympathetic arousal was a contributor, while simultaneously being, by itself, a poor predictor of aesthetic choice. Sympathetic arousal raised by a psychophysical variable (loudness) resulted in simpler aesthetic choices, but this effect interacted with the fine-grained attentional demands of concurrent cognitive tasks [18]. Preference for Renaissance and 20th century paintings could be pushed up and down at will, and reversed, depending on the pairing of exposure to different amounts of sympathetic elevation, the source of the

elevation, and on whether or not affect was produced—and whether it was positive or negative [19].

None of these experiments were designed specifically to prove or disprove aspects of Berlyne's "theory." Rather, Berlyne's ideas were invaluable conceptual aids in the choice and construction of independent and dependent variables—for experiments which seldom sought to answer directly the type of questions one found posed in Berlyne's own laboratory [20]. Thus, my theoretical and experimental work in psycho-aesthetics owes a large intellectual debt to Berlyne, but it is an indirect debt that has not included the choice of research problems.<sup>1</sup>

Nevertheless, many of the results mentioned above are certainly relevant for both Berlyne's theorizing and Martindale's negative conclusions, if somewhat inadvertently. In dozens of experiments, the addition of sympathetic-arousal and emotion-related considerations to experimental designs contributed to strong evidence being obtained for the importance of collative variables, the additive nature of collative and psychophysical effects, and the trade-off hypothesis.

The conceptual and functional distinction between the level of (sympathetic) arousal and its emotion-related interpretation (that is, its relevance for, and incorporation into, the experience of an emotional state) avoids Martindale's "isohedonic trap" [8, p. 432; 22, p. 81]—the untenable position, derivable from Berlyne, that equally (cortically) arousing stimuli are equally liked. Sympathetic arousal is related to other determinants of emotion, especially cognitive ones (such as appraisal and interpretation), in complex, often multiplicative, ways. This means that the cognitive coloration that distinguishes mood, affect, and emotion makes them far better predictors of aesthetic behavior than is arousal by itself—of whatever type and however measured.

Significantly, when a broad enough range of sympathetic-arousal elevations is induced by experimental procedures that incorporate psychophysical and ecological variables (both with aversive and gratifying levels), the rated pleasingness of paintings, paired with such procedures, and regardless of the paintings' complexity, is a quasi-sinusoidal function of skin conductance, with two peaks [19; a completely within-subjects design]. A smaller range of skin-conductance values or a smaller number of experimental treatments inducing them, would have yielded linear (monotonically increasing or decreasing), or inverted-U, or U-shaped functions—the obtained result contains them all, on both the group-mean and the individual-subject basis. Quite unlike its various components, the entire obtained function could please neither Berlyne nor Martindale.

Although the addition of sympathetic-arousal and emotion-related considerations to experimental designs helped provide data that contradicted some of

<sup>1</sup> An exception is a paper on the *St. Matthew Passion* that I wrote for Dan's aforementioned graduate seminar. This paper was later published (with hardly any changes) in a Baroque-music journal [21].

Martindale's negative conclusions about the validity of Berlyne's notions, such concepts should not be contemplated as components of a revised Berlyne's "theory." Emotion and its complicated cognitive determinants were sufficiently alien to Berlyne's way of thinking about psycho-aesthetics and theory construction, as to preclude such a fusion. Nor should these concepts be simply added to the list of Martindale's criticisms, because some of the latter are themselves being challenged.

Collectively, the mentioned experiments demonstrate the powerful role of context in aesthetic preference and choice, be it the presence of others and their behavior, the precise attentional and processing-capacity demands of concurrent tasks, the situational ingredients of independent-variable operationalization, or the collative characteristics of the aesthetic ambiance, as shown in very recent experiments [23]. And it has long been my contention that since a very high proportion of aesthetic behaviors in real life occurs in rich contexts, psycho-aestheticians ignore them at their peril [24, 25]. Yet, the vagueness and neuro-physiological emphasis of Berlyne's theoretical position, coupled with the relative narrowness of his experimental efforts, meant that he largely neglected the contexts in which aesthetic behavior occurred.

### DEPENDENT VARIABLES IN PSYCHO-AESTHETICS

Furthermore, Berlyne long made little systematic effort to develop a detailed classification of aesthetic responses, let alone specify on theoretical grounds why particular experimental operations should be differentially reflected on various dependent measures. This, in fact, began to change, and rapidly, in the last years of his life. In the 1971 book, there is very little discussion of dependent variables, except for a comparison of pleasingness and interestingness as measures of complexity [6, pp. 213-220], although, significantly, there is a one-page critique of psycho-aestheticians' "concentration on verbal judgments" [6, p. 30]. In contrast, many of Berlyne's students' chapters in the 1974 book report new experiments with multiple verbal and nonverbal measures, and there is a sophisticated section on dependent variables and their interrelations by Berlyne himself [20, pp. 13-17].

The brevity of this promising development in Berlyne's work is one of the least discussed, but perhaps most unfortunate, scientific consequences of his untimely death. Psycho-aestheticians still uncritically overuse verbal measures, seldom ponder the implications of various types of measures, and no theory of psycho-aesthetic measures is in sight.

Be that as it may, the fact remains that Berlyne came to this crucial methodological realization late and that the relative absence of multiple, including nonverbal, measures compounded—from my perspective—the problems brought on by the neglect of context and emotion. For many, perhaps most, aesthetic stimuli, verbal ratings of pleasingness, interestingness, and the "desire

to own" (the object or its copy) differ from each other and, collectively, differ from various behavioral (e.g., choice) measures that include exposure to the object for various, even very long, time periods. Differences among dependent measures often interact with socio-emotional and cognitive contexts, reflecting the fact that art is frequently, but differentially, used to optimize mood [19, 26]. A consideration of such functionality of art objects as parts of the stream of daily life and as contributors to person-environment interactions is poorly represented in Berlyne's theorizing and experimentation.

### TO FANNY PRICE WITH PROTOTYPICALITY!

The large number of factors that have been experimentally shown to influence aesthetic behavior, singly and in various interactive combinations, precludes, in my opinion, the formulation of a general theory that is either communicable (as Berlyne's was) or testable (which it was not). In my own work, I have resorted to mid-sized models [19, 24-26].

Psycho-aestheticians know too little to arrange things into any semblance of a double helix, but too much to take seriously a single-factor theory of aesthetic preference, such as that based, for example, on "prototypicality." Boselie's [27] arguments against the usefulness of the notion of prototypicality could have perhaps been differently stated and further buttressed, but as the various points already mentioned in this article indicate, my sympathies are entirely on his side *vis-à-vis* his recent critics.

Across different studies, definitions and operationalizations of (proto)typicality have been mirages in the shifting sands. They are adapted in an *ad hoc* manner to experimental questions and contexts, rather than being attempts to accomplish the Campbell-Stanley goal of "triangulation," which increase support for a concept by looking at it by different methods, the measurement error of each cancelling the errors typically associated with the others.

Let me end this remembrance of Dan with questions to *ESA* readers with reference to two psycho-aesthetic examples.

The first involves imagining being Gustav Fechner's subject and looking at the Darmstadt and Dresden Madonnas having been asked to rate the beauty of their faces. You know that one is very likely by Hans Holbein, the Younger, the other—probably not, but maybe yes, or perhaps by an underappreciated, and possibly great, contemporary painter. Since you are simply asked to judge the respective beauty of the two, even a fully-formed (?) Holbein (?) Madonna prototype (?) would not do at all, unless you have heard a rumor, and are a person overinfluenced by prestige effects. How does prototypicality influence your judgment? (Fechner himself would have had the much harder task of explaining why your comparative beauty judgment authenticated anything.)

Second, consider "Cinderella" as a prototype. Is it not obvious that the preference function would assume radically different shapes depending on the population making the ratings, their emotional state, the members of the stimulus set, and various aspects of the context in which the ratings are made?

To amuse Dan, observing us from high in the stands, let us have a great writer set up a prototype:

... their gentle niece Fanny Price ... an impecunious ward ... an adopted child ... [T]his was a most popular figure in the novels of the eighteenth and nineteenth centuries ... her position in the tepid bosom of an essentially alien family ... her dual position of detached observer and participant in the daily life of the family ... the gentle ward in Dickens, Dostoevski, Tolstoy ... [T]he prototype of these quiet maidens, whose bashful beauty ... shining in full when the logic of virtue triumphs over the chances of life—the prototype of these quiet maidens is, of course, Cinderella ... [D]ependent, helpless, friendless, neglected, forgotten—and then marrying the hero [28, pp. 9-10].

It seems clear that Vladimir Nabokov preferred even Jane Austen's Fanny to Cinderella, and so do I and so would Dan.

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