Current Issues and a "Wish List" for Conjoint Analysis

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SUMMARY

Conjoint analysis is one of the most celebrated tools in Marketing, and its widespread use (not just widespread publication history) has been one of the greatest success stories in Marketing/Business of the academic and practitioner interface. In this article, I provide a wish-list, of sorts, summarizing current cutting-edge research that is trying to fill some holes, and other issues that I "wish" were trying to be filled.

By literally using a list-like statement, current answer, and wish-list format for this article, I hope to provide guidance to both academicians and practitioners as to areas for future important conjoint research.

KEY WORDS: Conjoint Analysis, Within-task Learning, Non-compensatory models, Massive number of attributes, Educational Testing

Many academics and practitioners alike have hailed conjoint analysis as one of the major cross-over breakthroughs between academic theory and practitioner relevance in the field of Marketing research ([1], [2]), and rightly so. The validation of this claim can be measured not only by the companies today that utilize conjoint methods for decision-making (product introductions, pricing, war gaming, etc…), the 62,000+ hits on www.google.com, and 500+ hits on jstor.org that appear when "Conjoint Analysis" is entered, but the fact that the research topic still has "legs" thirty plus years after its primary introduction ([3]). This is especially true in areas such as conjoint design ([4]), where adaptive conjoint designs, specifically ACA ([5]), are still being fully understood and entirely new methods, such as fast-polyhedral designs ([6],[7]) are just hitting print.

Despite all that we know about Conjoint Analysis, however, there exists a significant amount that is left to understand, and is the motivation for this paper. That is, I will describe nine different areas in which I have a wish list, meaning I wish I knew the answer to this question and hope that other academicians and practitioners wish they did too.

It is important to note, however, that many of the wish-list items presented here come from a "behavioral-mathematics" perspective, one in which understanding the underlying process (in the mind of the respondent) is important in and of itself. As the major use in practical studies for conjoint is forecasting, and specifically out-of-sample forecasting for new product introductions, product-line extensions, and the like; some of these issues may become more or less "practically" important. That is, while non-compensatory models, non-stable attributes, etc… may indeed exist, it is an empirical question as to the robustness of standard methods to these deviations. The empirical meta-analysis (of sorts), described in wish-list item (8), is a call for further empirical understanding of these issues.

1. Within-task learning/variation

One basic tenet of standard conjoint models, whether modeled with heterogeneity ([8]), or not, is that the attribute coefficients (partworths) are stable throughout the study, i.e. there is no subscript "t" on them indexing trial or time. Now, of course, while a fully parameterized model where each partworth is person and time specific is overparameterized, one could assert a change-point model, a smooth-parametric function, a random-walk model, etc… allowing for time-varying partworths ([9]). Whether I believe, or psychologists believe, that people within-task change their preference weights is somewhat moot; current methods allow for this question to be answered empirically under a number of different settings. From a practical standpoint, understanding the answer to this question can have implications for how people form their preferences of brands as they become more experienced, say, with the product category.

2. Embedded Prices

The typical way in which prices associated with conjoint profiles are constructed, and more importantly presented, is that each attribute level comes with a "hidden" (embedded) price (i.e. it's known to the researcher and used to construct the overall profile price but unknown at the attribute level to the respondent) and one of the profile's attributes is the total price comprised of adding up a base price and the associated level prices. While this mimics many buying situations, and allows for a clean measure of the "price partworth", there are many real-world situations in which the price associated with an attribute level is visible and "embedded" within the attribute itself; e.g. a 8x CD-Rom drive at a $200 cost. In these instances, the deterministic component of utility is not (potentially) simply the sum of the attribute-levels utilities, but is some combination of utility for the attribute levels, their associated prices, and even possibly an attribute quality/price ratio. As these types of products are prevalent in the marketplace, research into this domain would seem fruitful; however, such studies would require correlated attribute levels[1], for instance price with an attribute, which would not "come for free" and would create estimation issues.

3. Massive Number of Attributes

While conjoint analysis has been shown to operate quite well when the number of attributes within a profile is within a moderate range (say less than 8), there should be concern about the use of conjoint in situations where the number of features describing a product is "massive", say 15-20 or more. This is certainly not uncommon for technological products, hotels, and automobiles to name a few. Two common practices in such situations are to: (a) utilize partial profiles ([10]), where each profile contains an experimentally designed subset of the attributes, or (b) self-explicated conjoint in which desirabilities of attribute levels and importances of attributes are collected in a self-report, one at a time manner ([11]). My call for research in this area relates to the practice of

[1] The author wishes to thank Joel Huber for this important point.

partial profile conjoint in which there is a presumption that either: (a) the attributes not shown do not interact with the attributes shown, and hence can be ignored, or (b) if profiles are shown in pairs, then the unseen attribute effects “cancel” when the difference between the utilities is computed to determine the choice probability of one profile over the other. Unfortunately, recent research has begun to question this assumption of “cancellation” ([12]), as well as more traditional research that has shown that partworths change depending on the presence or absence of other attributes ([13], [14], [15]).

4. Non-compensatory decision rules

All conjoint models available in standard software, and most used in academic research, utilize a linear equation for the deterministic component of utility for a profile that implies a compensatory decision rule, i.e. lacking on one feature can be “made up for” by being better on another feature. Much behavioral/experimental research has shown that indeed subjects do not all make decisions this way; for instance consider elimination-by-aspects where if the product does not have a certain feature it is eliminated from the consideration set. Such behavior is common for novices and/or people who are using simplifying decision heuristics. Fortunately, recent research by [16] and others, have now developed ways within a Bayesian framework to assess and allow for greater flexibility in the assumed decision rule, and even better, can uncover the “latent rule” and provide information on the fraction of respondents using one rule over another. I applaud this research and hope it is an area that is continued. Nevertheless, it remains to be seen whether non-compensatory rules can not be approximated well by standard assumed compensatory models with interaction terms. Although, the use of interaction terms, in practice, is not commonplace but instead under the special discretion of the study designer.

5. True integration of profile conjoint data with other data sources

It is not uncommon that respondents who have participated in a conjoint experiment to have also filled out other survey questions, or in those instances where the conjoint experiment is part of a larger study to understand the customer base, purchase data, marketing mix variables, demographics, and the like are also available. In these instances, an integrated model for multiple data sources would be "nice" in which all sources provide information about the partworths, or even more generally, one could imagine a set of latent factors or needs, upon which all data sources are a manifestation thereof. Work by researchers in [17] built a Bayesian model that allows for the coherent combination of secondary data sources on partworths (self-explicated data) with the primary experimental data; however, this study was limited to conjoint data and self-explicated. True integration with purchase data, survey data, etc… would be an important extension and, in my view, an integrated framework that would be used.

6. Experimental Design: What can we learn from the Education Literature?

As mentioned in the introduction, experimental design issues in conjoint, beyond standard linear designs, have made tremendous strides in recent years with the development of ACA and, most recently, polyhedral methods. Such methods are designed to select the next profile, or pair of profiles, to maximize the obtained information, or alternatively, minimize a sum of variances (posterior or otherwise). This problem is virtually identical to that which has been researched extensively in educational tests ([18]) and is known as Computerized Adaptive Testing (CAT). In CATs, the next item administered to an examinee is that which minimizes the posterior variance of their estimated ability distribution. To describe the "tip of the iceberg" of its widespread use in the education domain, the current incarnation of the Graduate Record Examination (GRE), given to millions of students yearly applying to graduate study, is done in an adaptive fashion. However, while these problems are virtually identical; there has been little to no cross-over research, or in my view even more importantly practitioner conferences that have people from both "worlds". I hope the time is now.

7. Getting the right attributes and levels

As someone who teaches Marketing Research, and who is a user of conjoint methods in practice, we can talk all the theory we want, but at the end of the day a large fraction of the success of conjoint rests on the researcher's ability to identify the salient attributes and levels. Despite their importance in practice, little guidance is given in how to select them, other than to use qualitative research methods (one-on-one interviews, focus groups), and possibly open-ended survey items as a guide. Besides a more definitive document containing our collective current knowledge in this area, research that provides *empirical* diagnostics would be of tremendous value. That is, can we provide a document where say, you have chosen your attributes and levels, run your conjoint model (hopefully allowing for heterogeneity), and a statistic based (say) on the heterogeneity of the estimated attribute levels, or the multimodality of the distribution, would suggest that the attributes and levels you have chosen may need adjustment. Such knowledge or research may exist or may not, but at least it is not common enough that it is known to me; and, if you want to talk about demand for it, I teach 150 students conjoint per year that would be an immediate and important market.

8. Mix and Match: is that ok? A meta-analysis in the making.

Amongst the many choices that are available for conjoint studies (ratings, pairwise, constant sum, pick k out of n, etc…), the user is faced with the daunting task of picking the one that is most appropriate. Recent research ([19], [17]) has intimated that when considering this choice, one important consideration is the form of the out-of-sample choice to which you are interested. That is, for example, if you are interested in forecasting people's choices in a 1 out of n real-world situation, use 1 out of n in your experiment. Whether this is true or not, remains to be seen; however, it is an interesting hypothesis and one in which I have seen little empirical validation. That is, just like in wish-list item (7) above, a ubiquitous question is "Which form of conjoint should I use?" Hopefully, whether it's through people posting their results on a common website, or an academic study that meta-analyzes research paper findings, this would be of tremendous value.

9. People don't make one-off decisions: product-bundle conjoint

As has been shown in much research regarding bundle choices ([20]) and variety seeking behavior ([21]) people do not select individual products in isolation, rather they consider bundles of goods some of which may be complementary, some of which may be substitutes, some of which may be to avoid satiation. The entire paradigm of conjoint, as currently structured, does not take this into account and considers predictions of choices of products in isolation, rather than as part of a utility maximizing "experience". An extended model for conjoint in which product-bundle utilities are maximized to obtain partworths, may be useful when the context of the problem suggests that people will be considering products in bundles.

Conclusions

To summarize, conjoint analysis, research regarding conjoint, and the use of conjoint, is middle-aged (!) certainly not old. There are many more issues to explore and the best news of all, given its widespread use in industry, is that all we as academics who care about practice have to do is to "FOLLOW THE ACTION". It will tell us the importance of things to work on next, and will provide exciting research that will be useful for years to come. Nevertheless, as we try to balance theoretical understanding and model parsimony, we must always keep our eye on the practical "prize" to which conjoint was designed, and more importantly is used.

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Comments on:

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Conjoint analysis has its underpinnings in the late 1960's. In his "Current Issues" article, Eric Bradlow points out that conjoint analysis has now come of age and is entering 'midlife'. I believe that the midlife analogy is a good one as each of Eric's nine directions represents an important opportunity for continued growth. We will comment briefly on four of these opportunities where latent class (LC) choice models have already moved the conjoint field in these directions.

First on his list is the important issue of stability of the estimated partworths. Since conjoint studies typically attempt to predict the future, it is essential that the partworth estimates not only predict *current* preferences/ choices and underlying values/ utilities accurately, but also that these estimates are sufficiently stable to allow for successful introductions of new products. Incorporating respondent heterogeneity is the single most important way to assure that the partworth accurately reflects the individual consumer's values, as opposed to just some aggregate measure that fails to account for the different utilities associated with different market segments. Beyond this is the question of how consumer choices may change over time.

From a LC perspective, modeling change (or learning) allows respondents to be in different latent states (or segments) at different measurement occasions. This involves specifying a model with multiple latent variables; that is, a model with one categorical latent variable per occasion. The correlations between the time-specific states may be modeled by an auto-regressive structure, yielding what is known as a LC or hidden Markov model. An alternative is to model the dependencies between the occasion-specific latent states using a random-effects or multilevel structure, as is done in the multilevel LC model recently proposed by Vermunt [1]. In both the Markov and multilevel specifications, it is possible to model the pattern of change over time, which may be used to improve prediction of future choices.

Issue #4 deals with the related need to extend beyond the simplistic aggregate linear model to represent adequately noncompensatory 'latent decision rules' that may be used by consumers in making choices. The psychometrics literature contains many examples of how LC models can be used to estimate the proportion of the population for which pre-specified decision rules apply. For example, persons in some (latent) segment might require attribute A to be present, or that the price be no higher than x before they would consider buying. The key to implementing this in practice would be to operationalize such a decision rule by specifying those *combinations* of attribute levels which are ruled out[1].

We agree that data fusion (issue #5) is an important future direction. Since the ordinal (adjacent category) logit model can be expressed as a restricted multinomial logit model, it is possible now to have a set of stated choices, one or more revealed choices, and a set of ordinal attitude questions all be analyzed as part of one large LC choice model. Future software will exploit this fact and make it easy for the user to estimate such models.

Regarding issue #6, CAT is a useful procedure when testing persons using an *existing* model. Without a model, it does not help in the administration of a conjoint survey. The way that CAT works in educational testing is that the best predicting item is selected to determine a person's latent trait, given a known latent trait model and the information already collected for the respondent. Thus, once a conjoint model that captures the unobserved heterogeneity has been estimated and we want to administer the same survey to a *new* sample (say in a telephone interview), one may apply CAT techniques to minimize the number of questions that are needed per respondent. For example, if the model is a LC choice model, the purpose is to predict to which class or segment a person belongs. Given the recorded responses at any point in time, the model can be used to select the best next choice set to administer. This set could be selected from among predefined choice sets, or it could be a *new* choice set that is generated at that moment. For example, if after the fifth question the posterior membership probabilities indicate that the person belongs to either class 2 or 3, we know that it would be best to present a choice set that discriminates as much a possible between these two classes.

We look forward to these and other extensions as midlife promises to be an exciting time for conjoint analysis.

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[1] This type of model could then be estimated using a program such as Latent GOLD Choice [2] by specifying an offset of minus infinity for a particular latent class to represent a zero probability of occurrence for such attribute combinations.

Author bios

Jay Magidson is founder and president of Statistical Innovations Inc., a Boston based consulting and software firm. He holds a Ph.D. in Managerial Economics and Decision Sciences from Northwestern University. He is widely published in various professional journals including Journal of Marketing Research and Sociological Methodology. He was awarded a patent for an innovative graphical display.

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Comment on Eric Bradlow's Paper

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Below I discuss each of Eric's points in turn.

1. Latin square designs can be used to study within-task learning to see if model parameters vary with order. We find that parameters do not change, but error variability does. Failure to control for error variability differences probably is linked to beliefs that parameters vary (See [1],[2],[3]).

2. Embedded prices - many studies use independent price levels instead of attribute-level related prices, but, one can vary prices for each attribute independently. One then can display each separately or one can display a "total price".

3. There are no technical constraints to designing experiments with "massive numbers of attributes" and we routinely use 20+. It is unclear why many think one cannot "over-burden" subjects as examples of "complex tasks abound in real life (eg, many supermarket categories have many options – eg, rte cereals - and labels often display much attribute information).

4. I agree with Eric that additive rules are naïve and probably wrong. For example, if subjects respond yes/no to 8 profiles, there can be 256 response patterns, and very few are consistent with additive rules. Even a simple choice task with 4 options and 16 choice sets yields more than 4 billion patterns, and almost none are consistent with additivity. Moreover, response patterns associated with fractional designs are consistent with (literally) thousands of observationally equivalent processes. Thus, current methods tell us little about process.

5. I agree with Eric about "true integration of conjoint data with other sources," but this requires behavioral theory, not statistics (See [1],[2],[4]).

6. I'm not sure we can learn much from the education literature; and so-called "adaptive" methods select treatments based on dependent variables; hence are subject to selection bias. More importantly, optimally efficient choice experiment designs are available, and many are very small. No design can be >100% efficient, so "adaptive" designs should not be used if optimal designs can be easily constructed, and the theory to do that is available ([5],[6],[7]).

7. Resources are needed to get attributes and levels "right," but few commercial projects devote enough time/resources to pilot testing. Random utility theory (RUT) tells us that failure to do so impacts random component variances, degrading inferences. Many papers show how to use RUT to rescale stated choice models to real choices, and almost all show that well-designed choice experiments allow accurate estimates of preferences and predictions of behavior. The latter suggests one often gets attributes and levels "right".

8. As noted above, 15+ years of research on the predictive validity of choice experiments consistently shows that experiments and associated choice models produce accurate estimates of real preferences that lead to accurate predictions of real choices.

9. One can study product bundling choices using various combinations of choice experiments and real market data, but this remains under-researched, as Eric suggests.

10. Conclusions - while useful and widely applied, many serious unresolved issues in choice experiments and choice models remain. Many of these issues are reviewed in [3],[4] and [8], which suggests that specification errors and bias are likely to be common, and that current methods need radical, not cosmetic, surgery to address the issues. Existing choice modeling methods and experiments probably cannot be "fixed", "extended" or adapted to deal with the fundamental identification, generalization and behavioral issues.

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Brief Bio of Jordan J. Louviere

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Comment on Bradlow

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I'm pleased to see an academic such as Eric Bradlow emphasize the need to "balance theoretical understanding and model parsimony." Too often, academics suggest refinements or alternatives to existing conjoint methods that are complex, impractical, and not appreciably better than existing practice. Eric concentrates on practical issues, approaching problems and possible solutions from a practitioner/managerial focus. As conjoint analysis moved from the realm of the academic to the practitioner (in the 70s), the need for straightforward models that tended to work well in practice was critical. Indeed, for all the simplifications in typical conjoint analysis studies (especially, as Bradlow notes, the critical assumption of additivity), it has tended to work well in practice.

Regarding Bradlow's desire that more work be done in "within-task learning/variation," I'd like to call attention to some research that Rich Johnson and I published in 1996 [1]. We examined seven commercial discrete-choice conjoint data sets that included brand and price. Respondents completed at least 10 choice sets, involving tradeoffs between brand, price and other attributes. We found that respondents tended to place greater attention on brand relative to price in the first few choice sets relative to later choice sets. The derived importance of brand relative to price was 1.93 in set one and decreased to 0.99 by set ten. What happened? In the real world, buyers observe brands to be roughly correlated with prices, quality, and performance. Thus, buyers can save informational processing time by selecting based primarily on brand. In contrast, orthogonal (or nearly orthogonal) conjoint design plans exhibit no (or very low) correlations between brands and other attributes. Sometimes the best brands are shown with lower prices and lower degrees of performance and quality, and vice-versa. After a few choice tasks, respondents become aware that brands are no longer a reliable indicator in this shopping "laboratory" and then adapt their behavior. Johnson and I wrote [1] "One might argue that the very first task should be the best, since the respondent is less contaminated by the effect of previous questions. This seems likely to be true for impulse purchases. But, for real-world purchases in high-involvement categories, buyers probably spend more time considering the options, features, and pros and cons of each alternative. Later tasks seem to better reflect such behavior."

Bradlow highlights issues of preference variation due to trial or time. A single conjoint survey given at time 0 is not a good instrument for capturing either of these, as the respondent doesn't truly try a product or experience its benefits (or regret its deficiencies) over time. As a related point, some conjoint researchers are focusing more on capturing variations in part worth utility functions in terms of occasions rather than time. They seek answers to questions such as: "How do people's preferences for beer brands and their price sensitivity vary depending on the purchase occasion/situation: buying for a party, buying for personal consumption, buying for a friend?"

Proper and prevalent validation of conjoint methods is an ongoing problem in our industry. Conjoint analysis cannot capture many real-world effects that influence actual market shares. It assumes (among other things) equal information, availability, time on the market, effectiveness of sales force—not to mention the critical question of whether we've included all relevant attributes

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Dr. Bradlow has provided an invaluable road map for important and necessary future research in Conjoint Analysis (CA). Particularly, and using his terminology, I reinforce his call for research in a) within-task learning/variation, b) massive number of attributes (albeit I believe the number of attributes needed for “massive” categorization needs to be increased somewhat above the 15-20 level he mentions), c) non-compensatory decision rules and d) true integration of profile conjoint data with other data sources.

Before elaborating on two topics in his list, I would like to address an overarching need for this research area. It is my belief that CA has lagged in theoretical development in part because it lacks a framework that goes beyond its functional measurement origins in psychology. The use of random utility theory in economics and transportation can and has furnished such a theoretical framework for CA ([1]), but it has not been widely adopted in the marketing field. Not least among the benefits of such a theoretical development is the integration to CA of such concepts as consideration and choice set formation, decision rule modeling, non-compensatory evaluation rules, market structure, measurement reliability, and, very importantly, an error theory. More needs to be done to integrate CA and random utility theory.

To build on Dr. Bradlow’s call for research on the integration of CA data with other data sources, it is necessary to point out that an active research stream ([1, Chapter 13]) already exists along these lines in transportation ([2]), environmental economics ([3]) and marketing ([4]), where it is known under the rubric of “data fusion.” For economy of space I have cited only a few exemplars of this work, but it is important to highlight this well-developed literature to CA practitioners.

Both academics and practitioners need to better understand the impacts of context complexity (certainly number of attributes and levels, as well as products) on choice behavior ([5], [6], [7]). The need is particularly acute in terms of being able to distinguish between measurement task effects and those operating in real markets: separating “artificial” task complexity responses from “real world” complexity responses, and/or discovering under what conditions task complexity responses are transferable to the forecasting context, is crucial to lending credibility to forecasts based on CA.

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that affect buyer behavior. Thus, validation in terms of correspondence to market share is a difficult proposition. Next, those organizations with access to validation data usually have little incentive to share results with others (for fear of losing some competitive advantage). In response, practitioners and academics alike have savored the limited validation cases and taken heart in the proposition that if conjoint analysis wasn't very predictive of buyer behavior, this certainly should have stymied its broad usage across industry over the last 30 years.

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Rejoinder to Reviewer Comments on
"Current Issues and a "Wish List" for Conjoint Analysis"

By

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The detailed comments provided by Louviere, Magidson and Vermunt, Orme, and Swait, have brought to light a number of issues regarding my “Wish List”: (1) Some of my Wish-List issues have been (partially) addressed, but are not known to *me*, (2) Thought leaders in the area do *not* agree on a number of these issues, and (3) Should we try to address these issues under the current conjoint paradigm or do more radical changes need to be made (see Louviere comment)? Each of these issues though has, in my view, a very fundamentally different “cause”.

The fact that a number of these issues have been addressed, unbeknownst to me, could of course reflect my deficiencies; but rather than admit that, I would rather attribute it to the vastness of literatures, just in the reviewers comments that are represented. Consider just the transportation literature, environmental economics literature, marketing literature, and organizational behavior literature listed in Swait’s comments, the sociology literature mentioned in Magidson and Vermunt, the statistics literature (and others) mentioned in Louviere, and the ART forum practitioner literature given in Orme. Could I, or anyone, even if I hoped to stay well-read, expect to see all of these different papers? How many of the comment writers are aware of many of these works? Thus, while conjoint analysis has benefited greatly from its widespread use, I believe that it has suffered academically as a theoretical research area (as per Louviere and Swait) because of the “disjoint” variety of literatures in which its basic fundamental research has been published.

As an example of the difference of opinions, consider on the one hand the comments made by Louviere in that preference partworths are relatively stable (rather it’s an error variance issue) with those by me (citing current research), Magidson and Vermunt, and Orme that have suggested within-task preference changes do happen. Further, consider the common folklore (and those recommended in most Marketing Research texts) on the need to consider a “smallish” set of attributes with those of Louviere suggesting that conjoint can handle “massive” numbers of attributes. Regardless, this discussion suggests that a difference of opinion does exist, there may be cases in which both sides are “correct”, and that we (academics and practitioners) should stop using simplifying statements like “you can’t have more than X attributes”, as they may damage practice more so than providing useful prescriptive advice if they are based on anecdotal evidence.

Finally, can we “coerce” conjoint analysis to handle many of the situations to which it was not psychologically or theoretically developed? While on the one side, and he is probably right, Louviere suggests that radical changes are necessary, on the other side, such comments suggest that I will have a lot of fun over the next 20 years attempting to do just that, and to push the boundaries wherever possible.