

Timmermans' Misleading Critique of Prospect Theory Actually Supports its Relevance for Travel Choice Modelling^a

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For a special issue of this journal Timmermans (2010) was asked to make critical comments on the suitability of Prospect Theory for travel behaviour research. His article offers a comprehensive overview of all kinds of criticism that one might encounter in the social sciences. When browsing through it during the preparation of an article about the transferability of Prospect Theories' assumptions I came across a citation of an alleged inferior explanatory performance of Prospect Theory with respect to people's choices in the TV game 'Deal or no Deal'. I curiously downloaded the cited working paper and found that the citation was fabricated. Successively I thoroughly reviewed the argumentations in the article and in several references that support them. This revealed more untruthful citations, inaccuracies in the references, fallacies and selective use of empirical evidence. Most remaining critical comments appeared personal opinions without solid theoretical or empirical support. In this paper I present an in-depth discussion of the foundations of the comments in T and a critical examination of the references advanced to support them. It leads me to the conclusion that Timmermans' criticism is unjust and that the references that underlie it actually support the suitability of Prospect Theory for travel choice modelling. This article might also offer a guideline for a careful interpretation of conclusions as a contribution to an improved peer-review process aimed to block articles contaminated with bad scientific practice.

Keywords: Good Scientific Practice, Scientific misconduct, Affirming-the-consequent fallacy, Peer review, Positive economics, Prospect Theory, Travel choice behaviour, Choice modelling

^a Note from Caspar Chorus (Editor-in-Chief): the paper "Timmermans' misleading critique of Prospect Theory actually supports its relevance for travel choice modelling", written by Evert Jan van de Kaa, was received at EJTIR's editorial office on 19 August 2012. Upon receiving the article, the following course of action was chosen: (1) Evert Jan van de Kaa was informed that his paper would be published in our next issue, without being reviewed by me nor by one or more external referees. This, to ensure that the paper would be completely representative of van de Kaa's own opinion. (2) the paper was sent to Harry Timmermans the next day, together with the invitation to submit a reply before 20 October, which would also be published in EJTIR (together with van de Kaa's paper) without being reviewed. Harry Timmermans' reply was received 15 October, and is published in this same issue.

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1. Introduction

In my opinion readers and peer reviewers of a scientific journal like *EJTIR* are entitled to trust that the submitted articles offer an adequate explanation of the discussed concepts and to feel assured of the open-mindedness, solidity and, above all, truthfulness of citations, assertions and argumentations. The contributors should also have made a fair effort to find out whether their explanations, assertions or argumentations were published before and, if so, should have credited the concerned authors by referring to them, thus avoiding the suspicion of plagiarism. If references are used to reduce the article length by opening-up earlier explanations and/or substantiating argumentations, their relevance for those purposes should be above reasonable doubt. The previous elaborations of good scientific practice may seem like stating obvious points. But if a contributor does not behave according to these requirements even a solid peer review might not detect such scientific carelessness and/or misconduct¹. I found suggestions of violations of several of these principles in a critical review of the relevance of Prospect Theory for travel choice modelling under uncertainty in this journal (Timmermans, 2010). Particularly, I uncovered the following errors and/or violations of good scientific practice:

- Misrepresentation of elements of PT as advanced by Kahneman and Tversky (Sect. 2);
- Inaccurate referring (Sect. 3.1);
- Fabrication of empirical evidence questioning the suitability of PT for transport choice modelling (Sect. 3.2);
- Fabrication of findings from a literature survey that, if it existed, was not accounted for (Sect. 3.3);
- Underserved referring to one's own publications (Sect. 3.4);
- Calling suspicion upon oneself of plagiarism (Sect. 3.5);
- Misrepresentation of other scientist's findings (Sect. 3.6)
- Selective, tendentious and/or false accounts of findings from one's co-authored publications (Sect. 3.7)
- Positing and/or citing conclusions based on fallacious arguments (Sect. 3.7);
- Using personal opinions in argumentations without stating these as such (Sect. 4); and
- Selective and prejudiced use of empirical evidence supporting and/or undermining PT's credibility (Sect. 5).

Not knowing the intentions behind these errors and/or violations it is not up to me to judge whether one or more exceed the boundaries of scientific integrity. In this article I intend to present my findings both carefully and extensively. A more extensive underpinning is given in a working paper (Van de Kaa, 2012), which is available upon request.

My investigation into the appropriateness of the referring in T (from now on used for referring to Timmermans, 2010) was straightforward: I consulted the referred publications and checked whether their content supported the corresponding citations and inferences in T. Where feasible I just browsed through the concerned texts but quite often a more thorough secondary analysis appeared required to arrive at solid conclusions. In my report hereafter I strive to offer quotations

¹ Contemporary interpretations of scientific misconduct are variations on 'Presentation to the scientific community of fabricated, falsified, or misappropriated observations or results and violation against good scientific practice' (National Research Ethics Council, Finland, 1998, in Nylenna et al., 1999 p. 58).

of corresponding texts from both T and the referred literature, thus allowing the reader to judge for himself.

For hardly if any of the few dozens of critical comments on Prospect Theory (PT) in T it was indicated whether or not these also applied to the nowadays dominant Random Utility Maximization (RUM) theory or to other theories or models that are currently applied in transport research. As the comments thus apparently aimed at an absolute, rather than relative, assessment of the strengths and limitations of PT, one might expect that some threshold or criterion to establish their (ir)relevance for modelling uncertainty in travel choices would have been applied. However, I found no indications for this. This complicates an unbiased appraisal of these critiques.

Looking for a methodologically sound approach for such an unbiased appraisal I considered that the behavioural sciences have quite a history in discussions about the correctness and/or applicability of choice theories and models. Well-known examples are the ongoing criticism (e.g. Simon, 1955; Kahneman and Tversky, 1979) on neoclassical economics and other versions of Utility Theory (UT) and the rejection of both UT's 'unbounded rationality' and the heuristics-and-biases concept of Tversky and Kahneman (1974) by the 'ecological rationalists' (e.g. Gigerenzer and Todd, 1999). Many comments in T and other publications that were advanced against PT were similar to those considered in these discussions. Perhaps as a response to early criticism on neoclassical economics, Friedman (1953) wrote an authoritative article that sheds light on the value one might attach to such arguments. I will apply his line of reasoning in my review of the arguments in T. As the latter was published towards the end of 2010, wherever I advance references in my review I will refrain from works that were published later.

In section 2 T's reproduction of the statements and underlying assumptions of PT is analysed. Section 3 discusses T's referring. Section 4 is concerned with the comments about the accuracy of PT's description of the human choice process and section 5 evaluates the advanced empirical evidence about the usefulness of PT for travel choice modelling. My review is closed with some recommendations and conclusions (Sect. 6).

2. Reproduction of the statements and underlying assumptions of PT

T (p. 372) summarizes some core assumptions of PT in a concise overview. Except for some minor flaws this offers, in my opinion, a fair reproduction of the essence of PT. However, scattered over T I found several notions that are at odds with the publication of PT. I will discuss these in the order of appearance in the text.

1. 'Kahneman and Tversky questioned the validity of EUT ... *It should be noted that this position relates to the basic form of EUT*' (T p. 372, my emphasis); 'the most basic version of EUT ... is the expected value model² ... it assumes that $u_i^n = \sum_{j=1}^J (p_j^n \cdot x_j^n)$... a deterministic decision rule is assumed in this classical case' (T p. 371); and 'each outcome j of the n^{th} risky prospect is defined by the values of a vector of observable attributes $X = \{x_k; 1 \leq k \leq K\}$ ' (T p. 370).

The italicised statement is *not true*: preceding their critique on EUT KT (from now on referring to Kahneman and Tversky, 1979) explicitly mentioned that, following most economic applications, they adopted the assumption of risk aversion and thus a concave utility function $u(x)$. That is,

² I follow Timmermans' definitions here. Actually, Bernoulli (1738) posited his concept of a concave utility function of money *against* the expected value concept which was developed in a correspondence between Pascal and Fermat (1654).

they assumed $u_i^n = \sum_{j=1}^J p_j^n \cdot u(x_j^n)$ in stead of the expected value model. I checked the comparisons

of PT and EUT in KT and found that for all considered choice experiments they consistently assumed a concave expected utility formula $u(x)$. Moreover, with one exception the observed violations of EUT would also be found for any continuously increasing function $u(x)$. If the italicised statement had been true, this would weaken the credibility of PT as an improvement of EUT, as only a poor version of the latter would have been considered. Except as qualification of PT I found no rationale for its inclusion in T's texts.

2. 'Similar to the more advanced approaches for riskless decisions, shown in Figure 1, *it may be conceptually richer to distinguish between mental representation, cognitive environment, preference structure and choice rule to avoid any confounding as potentially done in PT*' (T p. 373, my emphasis).

This statement is *unjust*. It draws on the conceptual framework in Figure 1 that was apparently fabricated (see Sect. 3.3). Kahneman and Tversky considered, for example, a decision frame as 'a representation of the act, outcomes and contingencies that are relevant to the decision maker' (Tversky and Kahneman, 1992 p. 299) and according to Kahneman (2000 p. xiv) the 'true objects of evaluation and choice (in PT) are neither objects in the real world nor verbal descriptions; they are mental representations'. More recently, Kahneman was even honoured³ by fellow scientists of the Federation of Behavioral and Brain Sciences for, among more, his contribution (together with Tversky) 'to model the interplay between the alternative framing⁴ of information, its mental representation as a function of the internal state of the decision maker, and the decisions based on that information.'

3. 'Loss aversion implies that travellers will likely experience that they could have done better. Repeatedly *using updated reference points* will then, *ceteris paribus*, lead to decisions and choices that deviate from the predictions of *standard PT*' (T p. 378, my emphasis).

It is *not true* that standard PT does not allow for reference updating between successive choices. In KT (p. 286-288) a whole section was devoted to shifts of reference points that may occur between two successive choices in a sequence.

4. 'In (C)PT risk attitude is *nothing but a descriptive label of the curvature of the utility function and the weighted-probability function* presumed to underlie travel choices ... It cannot be ruled out that the characteristic curvature can be caused by mechanisms other than risk attitudes' (T p. 374, my emphasis).

This statement is *wrongful*. In KT the reference-dependent framing of attribute levels is explained as consequence of underlying psychophysical adaptation and perception mechanisms and in Kahneman and Tversky (1984) a more extensive psychophysical substantiation of the shapes of both the value and weighted-probability functions is given. Thus, the fourfold pattern of risk attitude of PT is indeed a descriptive label but this is founded on generally accepted underlying psychophysical mechanisms. If the credibility of a theory would be increased by considering risk attitude as more than a descriptive label PT should have been praised in T.

5. 'Because PT assumes a deterministic utility function and utility-maximizing behaviour, given the edited prospects, it *implicitly assumes that individuals do take all information into account*' (T p. 376, my emphasis).

This statement is *not true*. KT just assumed that PT's edited-prospects-value-maximization algorithms would explain/describe the choices of most individuals of the concerned populations.

³ see <http://www.fabbs.org/index.php?CID=169> (accessed 2012-06-04).

⁴ Initially KT also used the term 'editing' for what from 1984 onward they systematically called 'framing', which I adopt here as a synonym. Note that several feasible framing rules might yield reference points different from the current asset position.

Maybe the argument drew on the misconception that a compensatory, value-maximizing appraisal of attributes is only feasible in connection with a complete, context-independent preference ordering of all feasible alternatives as presumed in UT? In connection with T's other texts the statement was also superfluous, unless meant to weaken the credibility of PT.

6. 'The literature on riskless choices has identified a series of effects influencing riskless choice behaviour, including effects of choice set, context, and taste variation to mention a few ... (C)PT does not take these effects into account' (T p. 380, my emphasis).

Posited in this way, the italicized statement is *not true*. Obviously, it cannot be excluded that PT cannot account for any of the unspecified effects that might be meant by T but PT was proposed as a theory in which, different from (E)UT, an individual's judgments and/or tastes might differ dependent on the composition, presentation and perception of the choice set and the choice context, see for example KT (p. 275): 'the preference order between prospects need not be invariant across contexts, because the same offered prospect could be edited in different ways depending on the context in which it appears.'

3. Use of the relevant literature

3.1 Characterization of T's bibliography

In Table 1 I classified T's bibliography according to content, field and document type. Two entries were redundant, as they did not appear elsewhere. More conspicuously may be the very frugal referring to the large body of literature about PT from economics, decision theory and other social sciences. When I was diving more deeply in T's referring I stumbled upon several factual inaccuracies which complicated appraisal and/or retrieval:

Table 1. Characterization of the entries in the bibliography

Direct theoretical or empirical evidence about the suitability of PT→ ↓ Scientific discipline and document type	Yes	No	Not referred
TRANSPORT RESEARCH			
Journal articles and book chapters	5	9	-
CD-ROM proceedings and working papers ¹	6 (4)	7 (5)	2 (1)
OTHER DISCIPLINES			
Journal articles and book chapters	3	7	-
CD-ROM proceedings and working papers ¹	-	1 (1)	-

¹ Figures between brackets: papers that were republished as peer reviewed articles before T was proofed.

- T p. 372: the reference to Kahneman and Tversky (1979) for the power functional form of the value function was wrong, this formula was not published before the posit of CPT by Tversky and Kahneman (1992 p. 309);
- T p. 375, 382: the journal article referred to Camerer was actually written by Carbone;
- T p. 376, 382: the working paper of Blavatskyy and Pogrebna (2007) was misdated to 2008;
- T p. 380: Habin and Miller should have been spelled Habib and Miller;
- T p. 380: the 2009 TRB conference paper of Chen *et al.* was misdated to 2008;

- T p. 382: the title of the 2005-article of Avineri and Prashker in *Transportation Research C* was cited wrongly. The correct title is 'Sensitivity to travel time variability: Travelers' learning perspective';
- T p. 382: Borgers *et al.*'s paper did not cover pp. 221-234 in '*Advances in Culture, Tourism and Hospitality Research*' but pp. 215-226 of its Volume 1, which was not published in Bangalore by MacMillan but in Oxford, UK by Elsevier.
- T p. 382: Apparently, Chen *et al.* contributed to the 88th Annual TRB Meeting, not the 85th.

In the following I aim to use the correct references, also in citations of T.

3.2 A falsified account of other scientists' findings?

To me, initially the most convincing argument that questioned the applicability of PT to travel behaviour research was the statement (T p. 376) that in a study by Blavatsky and Pogrebna (2007) 'remarkably, CPT never outperformed other decision theories, regardless of the assumed probabilistic choice rules'.

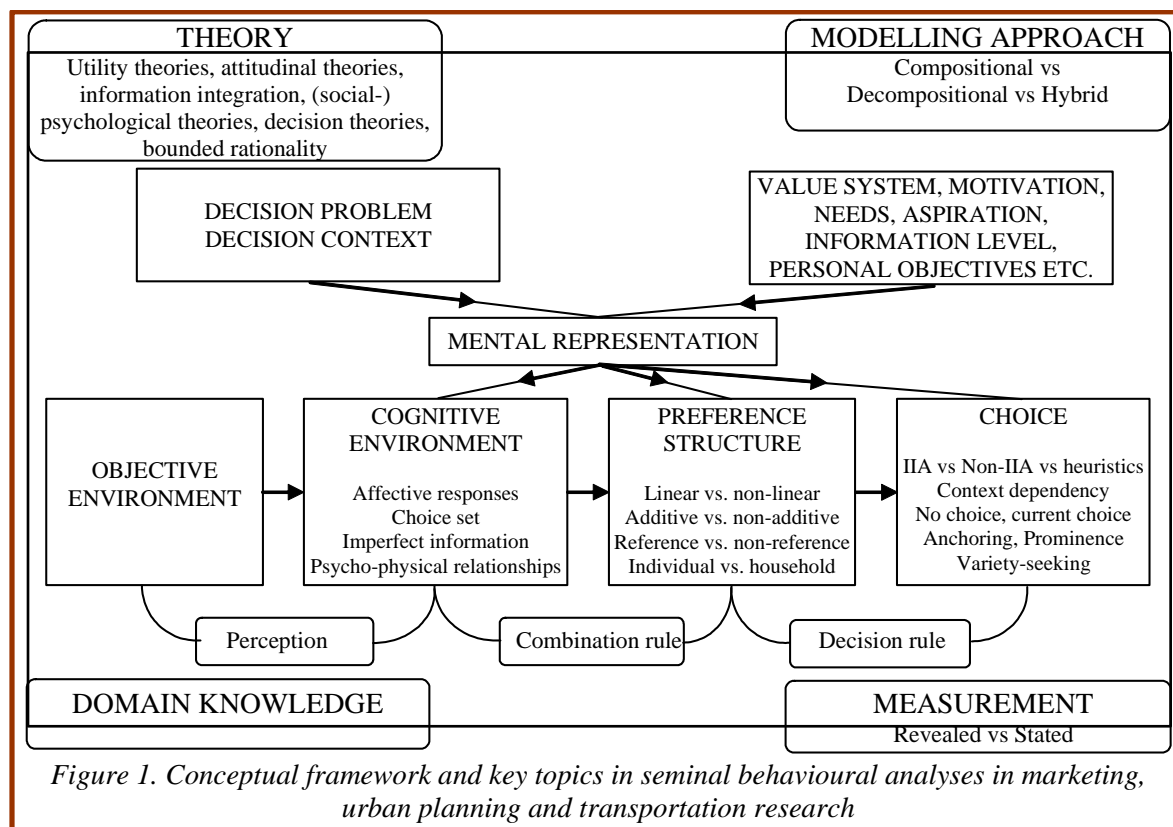
This statement was fabricated. Close reading of the concerned working paper revealed that it contains no information that supports it. Blavatsky and Pogrebna (2007 p. 10) explicitly stated that 'we do not estimate ... (C)PT'. They mentioned that one might consider Rank-dependent utility theory (RDU), which they examined, as a version of PT in which the participants ranked all choice options as gains. This RDU offered the best loglikelihood in five of 20 listings, the second best in six, the third best in eight and the fourth best in one. In any listing it thus performed better than at least three other theories. Also if RDU is considered as a version of PT the statement that CPT never outperformed other decision theories was thus invented.

3.3 A fabricated conceptual framework?

In T several times is referred to Figure 1 (replicated below) as substantiating evidence for critiques on PT. Its title is 'Conceptual framework and key topics in seminal behavioural analyses in marketing, urban planning and transportation research' and it is introduced as follows: 'Especially outside of transportation research, a large number of studies, based on a variety of theories, concepts, and measurement approaches has been suggested to analyze and model individual and household decisions. Figure 1 gives an overview of dominant approaches and key issues that have been addressed and explored in the early years (1970-1980s). These are listed in the context of a general conceptual framework that summarizes the common elements of the various approaches (Timmermans, 1982)' (T p. 369).

In the remaining texts not any further account is rendered of the origin and foundations of this diagram, nor of the purpose of its creation, nor of its entitlement to the claim that it offers an overview of early research and a general conceptual framework as meant in the quotation above. Also, except for the 1982-article further references that might provide such an account are missing. This suggests that Figure 1 was replicated from and accounted for in the 1982 article. However, the 1982-article did not provide any account of the general conceptual framework nor an overview of the early research that Figure 1 claims to provide. It empirically investigated consumers' ratings of some 'objective' attributes of shopping centres and contained a hypothetical process flowchart for destination choice. As any explicit support for the pretensions of Figure 1 is missing in both the 2010 and 1982 articles I examined if it was self-explaining and unambiguous, which appeared not the case. For example, its keywords might be conceived as states, processes or functions; many research topics might be assigned to different keywords; the arrows might be conceived as relationships indicating exertion of control or time sequence or flow of information; etc. It is conspicuous that the keyword 'cognitive environment', which in the

1982 diagram represented a set of perceived destinations with their attributes, was adopted in Figure 1 as a core element of a general choice behaviour concept. The prominent presence of the 'mental representation' block might be even more confusing, in view of its position amidst several blocks that accommodate specific appearances of it, like 'value system' and 'cognitive environment', to mention a few.



To grasp an idea about the degree to which the keywords in Figure 1 are nevertheless representative for the research effort that it claims to describe I did an 'all fields, all document types' search of these terms on Scopus (Table 1). The occurrence of most keywords in the concerned literature appeared very low. This indicates that these keywords and the attribution of topics to them are inappropriate for the provision of an overview of the dominant approaches and key issues that they claim to describe.

Table 1. Occurrence of keywords in marketing, urban planning and transport literature

Number of articles found in full text search on SCOPUS ¹ →	All years	1970-1989	1970-1989
which contained any of the choice-related concepts in Figure 1 ↓	All fields	All fields	3 fields ¹
"decision problem" OR "decision context"	15,168	1,229	15
"value system" OR motivation OR needs OR aspiration OR	2,300,562	234,878	>>100
"information level" OR "personal objectives"			
"mental representation"	14,257	232	5
"objective environment"	155	10	2
"cognitive environment"	272	8	0
"preference structure"	1,906	88	7
choice	973,867	86,915	>>100

¹ accessed Februari 2012. ¹ "urban planning" OR marketing OR (transport OR travel).

As I was not able to retrieve any evidence that supports the claim that Figure 1 offers a solid overview of early research as well as a general conceptual framework for choice behaviour, the firm statement of these pretensions (T p.369) and the reference to Timmermans' 1982 article are, in my opinion, scientifically unfounded if not misleading. In my review of the argumentations I will therefore consider the statements that were underpinned by referring to Figure 1 as unsubstantiated personal opinions.

3.4 Undeserved citations of one's own publications?

The bibliography in T counts, in addition to Timmermans' 1982-article, 12 more papers that were co-authored by him. As discussed above, the once-only reference to the 1982 article does not supply evidence that compensates for the missing account of the claims based on Figure 1. Also the listing of the paper of Arentze and Timmermans (T p. 382) to which is not referred in the text was obviously not appropriate. Each of the remaining 11 entries is referred once-only in the texts. Hereafter I will consider whether these references are appropriate.

Four co-authored works (of Chorus *et al.*, 3 articles, and Sun *et al.*) were referred on page 370 in a listing of 'different theories and models of decision making under risk and uncertainty (that) have been applied in transportation research', together with two papers of other scientists. This was done under the self-imposed constraint of only referring to 21st century publications, which is, in my opinion, not a good reason to refrain from crediting prior publications. Obviously, the work of Chorus *et al.* (e.g. 2006) on Regret Theory deserved a reference here, but a rationale to refer to them more than once⁵ is missing. In my opinion Timmermans and colleagues might be credited for bringing Bayesian Belief networks under the attention of transport researchers as they considered the topic in several more articles from 2005 onward. A reference to this work appears thus well-deserved, though Sun *et al.*'s 2009-paper on CD-ROM might not have been the best way to do so.

Without agreeing with the concerned argumentations, the references to Zhu and Timmermans (T p. 375) and Han *et al.* (T p. 377, 378) might be appropriate.

On page 380 a lengthy argumentation led to the conclusion that 'choosing PT because the researcher feels a reference point is necessary is not necessarily an adequate reason as several other utility-based alternative theories have been shown to offer the same mathematical functionality'. T did not refer to researchers who actually stated that they did so. I wholeheartedly agree with his statement, as I consider it self-evident that the reference point/state of PT is nothing else but an elaboration of the asset position to which an individual considers to be entitled, which is also at the heart of the, essentially relative, utility concept as posited by Bernoulli (1738). A researcher who would feel that a reference point is necessary would thus behave quite ignorant if he adopted this and all other assumptions of PT for that reason. Even if a researcher would act this way, I cannot see how this would provide any evidence against or in favour of the use of PT in transport research. The argumentation leading to the conclusion as cited above is thus redundant. In my opinion this also makes the five references that were advanced as supporting evidence in this argumentation undeserved. These comprised four co-authored papers with Borgers, Chen, Zhang and Zhu as leading authors.

In sum, while five out of the 13 references to Timmermans' own (co)authored papers might be deserved the remaining eight appear redundant.

⁵ In the overview one reference is given for each choice theory and applications such as in a stochastic model for multi-attribute choice were not considered to make a difference, except for Regret Theory. Chorus *et al.* (e.g. 2008) extended Regret Theory to multi-attribute multi-alternative choice settings and called it Random Regret Minimization. Just one reference to Chorus *et al.* would thus have been appropriate.

3.5 *Independently arriving at earlier published findings?*

The brief overview of applications of PT to transportation research mentions that 'Avineri and Prashker (2004, 2005, 2006) applied PT in a route choice setting ... They found evidence of non-linear decision weights and loss aversion'.

In Avineri and Prashker (2006) PT was not applied in the concerned route choice context. They also did not refer to that term in that article and any reference in it to the work of Kahneman and Tversky is missing, too. During my PhD research⁶ this publication attracted my attention because one experiment described in it was earlier (2003) published in *Transportation Research Record* 1854, together with an exposition of CPT and Bayesian Learning models. In their 2006 article Avineri and Prashker presented an additional experiment and did not refer to their earlier consideration of CPT. I made a secondary analysis of the choice observations presented in the 2006 article and found evidence for 'non-linear weighted probabilities' as well as 'reference-dependent framing and loss aversion' (Table 8 in Van de Kaa, 2008 p.177). In the same table the same inferences were listed for the experiments described in the 2004 and 2005 articles of Avineri and Prashker. To my best knowledge my dissertation, and the article in *Transport Reviews* that draws on it (Van de Kaa 2010a), are the only publications other than T in which the same inferences about the usefulness of PT are drawn from the 2004, 2005 and 2006 articles of Avineri and Prashker.

During my PhD research I re-examined several evaluations of the usefulness of PT for explaining recurrent choice between probabilistic alternatives, in which the participants received feedback about the outcomes of their previous choices in the sequence (e.g. Barron and Erev, 2003; Avineri and Prashker, 2005). In view of the attention that KT paid to reference shifts I was surprised that the different studies adopted the same reference state⁷ for all successive choices. I considered that feedback-based updating of the reference state and heterogeneity in choice behaviour strategies might offer a fair explanation of such recurrent choices. This view is explicitly articulated in the constituent assumptions of EPT and elaborated on several places in my dissertation, see e.g. my evaluation of Avineri and Prashker (2005): 'Considering the salience of the 'instant endowment' phenomenon it seems highly likely that the experienced outcomes of successive choices caused reference shifts ... Following the assumptions of EPT, the consistency of intrapersonal choice behaviour in such recurrent choice contexts could be studied by presuming idiosyncratic reference state updating' (Van de Kaa, 2008 p. 171). I was surprised to read that 'PT does not take such feedback and consequent learning and adaptation into account ... Repeatedly using updated reference points will then, *ceteris paribus*, lead to decisions and choices that deviate from the predictions of standard PT' (T p. 378) without reference to the earlier publication of this idea.

3.6 *Tendentious account of other scientists' findings?*

Sect. 3.3. is introduced by the statement that 'implicitly, PT assumes that when faced with replicated identical binary choices, subjects will made the same choice. There is overwhelming evidence to the contrary. Carbone (1997), Hey and Orme (1994) and Ballenger and Wilcox (1997) to name a few report switching behaviour between 20 and 30%, *fundamentally questioning the assumptions underlying PT*' (T p. 375, my emphasis).'

⁶ Note that Timmermans commented on the final draft of my dissertation in April 2008 and received the printed book in the summer of that year.

⁷ I follow here Kahneman and Tversky (1984 p. 343 and following pages) who used the more generic term 'reference state' as synonymous to 'reference point' and in later publications often used point 'point', 'state', 'value' or 'situation' indiscriminately to indicate 'the reference value to which current stimulation is compared (which) also reflects the history of adaptation to prior stimulation' (Kahneman, 2002).

At first sight the firm statement that PT assumes that people make the same choices from replicated identical sets seems a fair interpretation of PT. T did not mention that this is also the common interpretation of RUM's systematic utility function: 'Random utility models assume, as does the economic consumer theory, that the decision maker has a perfect discrimination capability. However, the analyst is assumed to have incomplete information and, therefore, uncertainty must be taken into account' (Ben-Akiva and Bierlaire, 1999 p. 7). The cited switching behaviour yields indeed unarguable evidence that subjects do not always make the same choices from identical choice sets. However, PT was not examined in any of these three references, let alone that its underlying assumptions were fundamentally questioned by their authors. These authors did thus not infer that their findings questioned PT's assumptions fundamentally, but T inferred that from their articles. A thorough examination of all these articles revealed that genuine human error was the best explanation for people's choice of different alternatives from the considered recurrent identical choice sets. PT does not explicitly discuss human error in connection with choice. It shares this with most theories of choice under risk and uncertainty but I found no evidence that any of these theories excluded human error either. I therefore found no scientifically sound rationale why these articles incited to question fundamentally the assumptions underlying these theories, and definitely not the particular ones underlying PT.

Next, I tried to understand the meaning of the remaining texts of T's Sect. 3.3. I could not grasp why adding a logistic term to PT's value function by Schwanen and Ettema, Avineri and Prashker and several more transport researchers, to account for genuine human error and/or fluctuating tastes, should not be convincing. In my opinion this is not in conflict with PT. Obviously, the estimated parameters of PT and any other choice theory may differ, depending on the applied stochastic model in which it is embedded to account for errors and/or taste heterogeneity. But if this effect is so strong that wrong behavioural conclusions are drawn this would apply to any choice theory.

Summarizing, in none of the five studies discussed above I found evidence that might give rise to questioning PT's assumptions more than those of any other choice theory. That is why referring to this line of research in connection with discrediting PT's assumptions seems unfounded and tendentious to me.

3.7 fallacious, tendentious or false accounts of findings from one's co-authored publications?

Questioning PT's so-called experiment-based foundations T (p. 375) stated that 'Zhu and Timmermans (2010b) argued that ideally the analysis of stated preference/choice data should include both a model of preference and choice behaviour, plus a process model of how subjects create a mental representation of the hypothetical choice problem'.

Elsewhere I documented an extensive re-examination of the referred article (Van de Kaa, in preparation). In brief, Zhu and Timmermans proposed several strong assumptions about the choice process, developed a model drawing on these assumptions, estimated the parameters of their model for the responses to a stated choice experiment and apparently deemed the fit of their model with these responses fair enough for conclusions like 'our results showed that respondents seem to have applied extremely simple decision heuristics in the first stage' (Zhu and Timmermans, 2010b p. 779). However, as other mental choice processes might be approached by the same mathematical algorithms and other mathematical algorithms, like for example a RUM model, might describe the same choices Zhu and Timmermans' inferences were based on a combination of the 'affirming-the-consequent' and the 'begging the question' and/or 'non-cause as cause' fallacies⁸. Therefore, this also applies to their conclusion above, even though it was

⁸ These fallacies are well-documented in scientific literature, starting with Aristotle, *On Sophistical Refutations* (ebooks.adelaide.edu.au/a/aristotle/sophistical/index.html). See Van de Kaa (in preparation) for the occurrence of these and some more fallacies in the transportation research literature.

formulated with some restraint. If the specious character of the argumentations would have recognized in T, the firm statement about the desirability to supplement choice models with mental-representation-creation process models would have been deliberately misleading. Assuming this was not the case makes the statement just another fallacy, this time formulated without much restraint.

T (p. 380) referred to another paper of Zhu and Timmermans (2010a) who should 'have argued that travelers may use multiple reference points. In their conceptualisation, however, reference points do not serve as anchors to distinguish between gains and losses, but rather as thresholds for accepting a decision strategy or not'.

Scrutinizing their paper⁹ I did not find the term 'reference point' anywhere. Except for their bibliography 'reference' occurred only as 'reference alternative'. This was introduced as follows: 'the individual compares the alternative with a given reference alternative to judge whether the alternative should be accepted or not ...the reference alternative is an instance of the personal value space' (Zhu and Timmermans, 2010a p.7). Their empirical study concerned the choice between going-home and continuing-shopping during a shopping trip. The continuing-shopping option was denoted as the reference alternative. Its utility was treated as an unknown parameter of the going-home alternative to be estimated. Such a 'reference-alternative' concept is very dissimilar from PT's reference point that draws on hedonic adaptation to people's earlier experiences. Both in their theoretical sections and empirical case study Zhu and Timmermans considered only one reference alternative and used the term 'multiple' nowhere in connection with 'reference' in any meaning. The citation that Zhu and Timmermans 'have argued that travelers may use multiple reference points' (T p. 380) is thus fabricated.

T (p. 380, my emphasis) remarked that 'it seems that transportation researchers have primarily explored the applicability of (C)PT to incorporate reference points in their models to differentiate between gains and losses ... the use of *reference points or thresholds* has a long history in modelling riskless choices to model ... relative utility theory (Zhang *et al.*, 2004), historical disposition (Chen *et al.*, 2008; Habib and Miller, 2009) and different frames of references as a function of accumulated experiences (Borgers *et al.*, 2007). Hence ... several other utility-based alternative theories have been shown to offer the same mathematical functionality'.

Here, just in passing and without substantiation, reference points, that are current or expected asset positions, are lumped with threshold levels, which separate rejected from accepted alternatives. Also the listed theoretical concepts and references are advanced as offering the same mathematical functionality as PT. To my best knowledge, the mathematical functionality of PT's reference point is to locate a kink and a convex-concave transition in the value function. Habib and Miller (2009, p. 92) presented a mixed-logit implementation of 'the theoretical framework of PT for riskless choice' in a 'reference-dependent residential location choice model within a relocation context' and compared it with a conventional RUM model. Adopting the characteristics of the current residence as reference-state levels they found that 'the reference-dependent model performs better than a conventional location choice model in terms of model fit and provides important behavioral insights'. Obviously, their model offered PT's functionality as it was an implementation of it, but it was not utility-based as meant in T. The three articles that were co-authored by Timmermans are discussed extensively in Annex 4 of Van de Kaa (2012). They described utility-based models but did not consider reference points or otherwise offered the same mathematical functionality as PT. More seriously, all three co-authored articles contained flaws and/or misleading referring and/or fallacies. Hence, for different reasons none of the four cited articles provides evidence that 'several other utility-based alternative theories have been shown to offer the same mathematical functionality' (T p. 380).

⁹ Their conclusions and the arguments on which these were founded contained the same fallacies as listed in the previous paragraph (Van de Kaa, in preparation).

4. Accuracy of the description of the actual choice process

Many objections to theories of choice concern that their assumptions do not correspond with ideas about people's real-life choice processes. Often vague, subjective criteria are used such as 'face validity' or adjectives like 'well-known' to strengthen such views. As people's choice behaviour is a predominantly unconscious, covert process (e.g. Nisbett and Wilson, 1977; Dijksterhuis, 2004), verifying or falsifying the truth of such essentially personal opinions about the real-life choice process, and thus of the critiques that are built on it, is impossible. Sometimes cognitive limitations are advanced against assumptions that seemingly require extensive calculations. The poor information processing capacity of the conscious mind apparently support such critiques. Some examples from social sciences are the objections by Simon (1955) and Gigerenzer and Todd (1999) to the utility maximization assumptions of UT and, by the latter, also to PT's value-maximization assumptions. In transportation research such critiques were advanced against CPT's weighted-probability assumption (e.g. Fujii and Kitamura, 2004; Avineri and Prashker, 2006). But the information processing capacity of the unconsciousness is huge (e.g. Dijksterhuis, 2004), which is evidenced by the phenomenal computational skills of several *idiots savants*. And if people would not be able to perform complex calculations unconsciously, they might use simplifying heuristics or their memory to achieve a similar result as predicted by descriptive theories that are conceived as a paramorphic representation¹⁰ of the actual choice process (e.g. Hoffman, 1960; Swait, 2001).

Friedman (1953 p. 21, his emphasize) illustrated this latter view with the expert-billiard-player example: 'It seems not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shots *as if* he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas. Our confidence in this hypothesis is not based on the belief that billiard players, even expert ones, can or do go through the process described; it derives rather from the belief that, unless in some way or other they were capable of reaching essentially the same result, they would not in fact be *expert* billiard players'. This line of thought makes it irrelevant whether or not theoretical assumptions correspond with algorithms of actual human choice processes, be it conscious or unconscious. For objections against such a lack of correspondence holds that 'criticism of this type is largely beside the point unless supplemented by evidence that a hypothesis differing in one or another of these respects from the theory being criticized yields better predictions' (Friedman, 1953 p. 31).

Judging the relevance of the comments in T on particular assumptions of PT by this standard I found a dozen or two of these that did not meet these qualifications. Several of these were based on the alleged authority of Figure 1, the misrepresentations of PT and/or the fallacious arguments discussed in the preceding chapters, others were advanced without underpinning and for none of these solid empirical evidence was advanced. I will quote here one example from each page on which these appeared:

- T p. 373: 'it is not very realistic to assume that they' (people) 'first assign probabilities and then apply some weighting scheme';
- T p. 374: 'PT lacks the behavioral concepts and may be too simple to avoid confounding of the various effects, shown in Figure 1, influencing the decision outcome';
- T p. 375: 'some transportation researchers (e.g. Schwanen and Ettema, 2007) have added an error term to the value function and assumed a utility-maximizing decision rule to derive a logit-form model with a scale factor equal to 1. This set of assumptions is not very

¹⁰ That is, distinct in form but analogous in the nature and product of their operations.

convincing. Not only is the use of an error term in conflict with the original theory, but assuming utility-maximization in the choice part and not in the valuing part seems inconsistent. Moreover, the results depend upon the assumed scale parameter of the utility function';

- T p. 376: 'However, many of the examined biases can also be explained by the alternative assumption that individuals demonstrate bounded rationality. They may filter attributes, set thresholds on attribute levels or may apply simplifying choice heuristics';
- T p. 377: 'The assumption of given probabilities is also incongruent with the typical decision problem in activity-based analysis. In general, travellers will not know the objective probability of an outcome ... Consequently, differentiation between decision weights and objective probabilities, as assumed by PT, may be impossible';
- T p. 378: 'any comprehensive theory of travel behaviour under uncertainty should include principles and mechanisms how travellers develop beliefs about the credibility of the information and information source, how they learn about possible underlying control strategies and how they dynamically respond to information and recommendation provided under these circumstances. Standard (C)PT does not satisfy this criterion';
- T p. 379: 'in case of well-articulated beliefs about the distribution of travel times, it is not readily evident why travellers would not directly act on their context-dependent beliefs of travel times and risk attitudes, rather than first processing and valuing travel time variability against some endogenous reference point';
- T p. 380: 'departure time and route choice are just part of daily activity-travel scheduling processes and should be modeled accordingly';
- T p. 381: 'the conceptual richness, the congruence of assumed causal mechanisms and structures, and the content validity of these models' (based on PT, vdK) 'as a manifestation of a theory of travel behavior under uncertainty is relatively poor compared to competing theories of travel behavior under uncertainty, such as (Bayesian) network learning models, and regret-theoretical approaches. These competing approaches are not more or less direct applications of theories originally developed in other domains, but try to develop a domain-specific modeling approach based on the salient features and key underlying processes of activity-travel behavior under uncertainty. Making travel decisions under uncertainty is not even close to gambling for money!'

An illustrative example is the last quotation above. It precedes T's final conclusions and draws on a posited better performance of learning models and regret theory. Timmermans was well-known with these concepts in transport research settings, as he co-authored many articles in which they were considered. Yet the only arguments elsewhere in T that supported PT's inferiority for transport research compared to Bayesian learning models concerned its inability for reference updating between recurrent choices (which was a misrepresentation, see Sect. 2 item 4 above). To my best knowledge, before 2011 no comparisons were reported of the performance of PT and Regret Theory in transport research. Also nowhere else in T any supporting evidence was advanced for the claim that regret-theoretical approaches are more useful as a theory for travel behaviour under uncertainty than PT.

In sum, T's statements above and the many similar ones that were not quoted should, in the absence of solid evidence, be conceived as personal opinions that lack scientific underpinning.

5. Use of empirical evidence

T (p. 375, 377) doubted the applicability of (C)PT to travel behaviour research because 'PT is largely based on experiments ... Most empirical evidence supporting prospect theory is based on gambling experiments in which subjects are requested to choose one of two prospects, specifying the probability of associated outcomes.'

The idea that (C)PT is mainly concerned with gambles might rise from the articles in which PT and CPT were proposed, which were the only publications of PT to which T referred. Browsing through Kahneman and Tversky's (2000) anthology '*Choices, values and frames*' reveals a wealth of experiments and real-life observations for which the different assumptions of PT offered an explanation, including choice under uncertainty, context-dependent framing, endowment effects, investment decisions etc. There and elsewhere (Van de Kaa, 2010b) I found several studies outside the travel research field in which the assumptions of PT appeared to explain choice under risk or uncertainty better than EUT and hardly if any that demonstrated the opposite.

T (p. 374, 375) doubted the credibility of the empirical evidence that these experiments offered for the usefulness of PT for real-life choice modelling: 'the experimental tasks used to test prospect theory typically look artificial ... many examples seem designed to articulate and amplify known biases ... Experimental tasks often look like quizzes to test whether students understand expected utility theory. They require ... the *calculation* of losses and gains and overall payoff. Subjects ... certainly will make mistakes ... the basis of responses in case of the gambling experiments are given probabilities and decision outcomes, how unrealistic they may be. Subjects' ... mental representation may differ from the constructed reality ... one cannot rule out the possibility that violations reflect incongruent mental representation and simple error ... Because prospect theory is largely based on experiments, evidence of risk aversion may have been confounded with errors introduced in understanding the experimental task, the framing of the task itself, limited information processing/bounded rationality in completing the task or any other process affecting the response-generating process.'

The gambles discussed in KT and Tversky and Kahneman (1992) concerned choices from simple binary choice sets that participants might well have performed intuitively. Such experiments were replicated dozens of times in countries all over the world and the observed choice patterns were remarkably similar. The outcomes are generally used in economics and other social sciences to test and compare the usefulness of EUT and alternative theories for choice under risk. I found no other publications in which their credibility for that purpose was doubted. T himself (p. 373-374) suggested that 'learning models for decisions under uncertainty may have more to offer than non-dynamic models of decisions under uncertainty such as (cumulative) prospect theory', referring to the findings of Hertwig *et al.* (2004). These findings were from choice experiments concerning similar gambles with probabilities that were either given or had to be learned from experience. T (p. 375) also accepted the findings from this kind of simple gambles as 'overwhelming evidence' for a non-deterministic utility function (wrongly, see Sect. 3.6) without disputing the credibility of these experiments. Furthermore, as discussed in the previous section, T (p. 381) stated that the content validity of PT for travel behaviour under uncertainty was poor relative to regret-theoretical approaches while Regret Theory (e.g. Loomes and Sugden, 1982) was proposed as an alternative explanation of exactly the same gambles as discussed in KT.

As discussed in Sect. 3.2, the statement that in explaining the UK and Italian versions of 'Deal or No Deal' 'CPT never outperformed other decision theories, regardless of the assumed probabilistic choice rule' (T p. 376) was fabricated. According to Blavatskyy and Pogrebna (2007), who did not consider loss aversion, the overall best performing theory was EUT with an expo-power utility specification (EUT-exp) while RDU offered the second-best fit.

For the Dutch, German and USA versions of 'Deal or no Deal' Post et al. (2008) compared the performance of EUT-exp with a full-fledged PT implementation with reference updating. In all five considered settings PT outperformed EUT, offering an improvement in loglikelihood between 11% and 16%. The 'Deal or No Deal' game thus confirms rather than weakens the large body of empirical evidence from real-life and experimental settings that support the usefulness of EPT for choice modelling.

T's overview of concrete applications of PT to empirical transport behaviour under risk and uncertainty (e.g. Li and Hensher, 2011, for an overview) is restricted, both in number of studies and depth of treatment. For the few referred studies no comparison is offered between the performance of PT and other choice theories. An exception is the study of Schwanen and Ettema (2009), for which statements that seemingly reduced its credibility were reiterated: 'e.g. Schwanen and Ettema have added an error term to the value function and assumed a utility-maximizing decision rule ... Not only is the use of an error term in conflict with the original theory, but assuming utility-maximization in the choice part and not in the valuing part seems inconsistent' (T p. 373) and 'A deterministic choice rule was assumed. Implicitly, this means that the authors assumed that the utility function is stochastic, theoretically violating prospect theory.' I demonstrated the inappropriateness of this comment in Sect. 3.6 above. T also trivialized the improved model fit of CPT compared to EUT as found in this study: 'Overall, differences with expected utility theory seem modest at best' (T p. 377).

Remarkably, T did not refer to any empirical transport choice study in which an implementation of PT was outperformed by UT or any other choice theory. Though in several places he questioned the appropriateness of PT's loss-aversion concept for transport research (e.g. T p. 379: 'the question is whether loss aversion also plays a significant role in routine behaviour such as departure time, route and destination choice') he also did not refer to the many transport studies (Van de Kaa, 2010a for an overview) in which reference-dependent models outperformed loss-neutral UT models in explaining choice between alternatives with certain outcomes.

In sum, T suggested that empirical evidence supporting the usefulness of PT as a descriptive choice theory is almost limited to simple gamble experiments while omitting references to much empirical research of other choice settings in social sciences at large; T called the credibility of this kind of gambling experiments and its usefulness for understanding transport choice under risk and uncertainty in question and at the same time presented inferences drawing on the same kind of experiments without commenting the nature of their empirical underpinning; T fabricated a poor performance of PT in explaining the 'Deal or No Deal' game while in the British and Italian versions PT's sibling RDU performed second-best to EUT-exp and in the Dutch, German and USA versions PT outperformed EUT-exp; T disregarded empirical evidence for the usefulness of PT's reference-dependent-framing and loss-aversion assumptions in transport choice under certainty; and T did not discuss the relative performance of PT and other theories in empirical transport choice studies under risk, uncertainty or certainty, except for one study, in which PT's performance was played down. I experienced this as selective use of empirical evidence and considered it as misleading. It might explain why in T's final conclusion-and-discussion section empirical evidence undermining or supporting PT's (ir)relevance for transport research is missing.

7. Recommendations and conclusions

During an extensive examination I uncovered the many errors and/or violations of good scientific practice in T and several underlying articles. One might wonder how these could creep in *EJTIR*. I must confess that when I was reading the article for the first time I only found that most objections against PT's process assumptions were of a general character and applied to EUT

and other familiar choice theories as well. If I had been a peer reviewer of T – which I was not – I am not sure at all that I would have found much more drastic objections than that. Not until I accidentally found the fabricated finding from the 'Deal or No Deal' game my mind-set changed, from 'taking the trustworthiness of the article for granted' into 'being on the alert for violations of good scientific practice'. Uncovering these in T took me a multiple of the effort that one might ask of even the most devoted peer reviewer.

I have the feeling that a thorough reading of this article might be helpful for editors and peer reviewers in uncovering bad scientific practice. In my feeling, a more than superficial screening of the bibliography might often be revealing. The occurrence of the following topics in an article should warrant a very critical attitude of editors and peer reviewers:

- Any arguments and evidence yielding firm conclusions about the realism of mental processes; and
- Any 'empirical evidence' for the 'absolute' applicability or validity of theories/models and their assumptions, instead of their 'relative' applicability, based on *ceteris paribus* comparisons of their model fits with those of well-known alternative choice concept.

The final conclusion in T (p. 381-382) is: 'at the current state of development, it (PT, vdK) lacks the rigor, scope, behavioural principles and mechanisms, and content validity to serve as a comprehensive theory of how individuals and households dynamically (re-)organize their activities and travel (departure, route choice, destination, transport mode decisions) along multiple horizons in uncertain, non-stationary environments in a ubiquitous information society, enforcing a diversity of travel control strategies, for which they can rely on past experiences. Applications of (C)PT to these types of choices represent an attempt to apply the theory in the wrong contexts.'

This conclusion apparently draws on the statements and inferences without solid ground as were discussed in Sect. 4. These were for a large part based on the invalid substantiations and arguments uncovered in Sections 2 and 3. T's final conclusion is thus based on scientifically invalid arguments and lacks solid theoretical and empirical substantiation, which reduces it to a personal opinion rather than a scientifically sound inference. This does not mean that its content is necessarily wrong. However, taking the empirical findings from other studies into account makes it, in my opinion, highly likely that an (extended) PT might serve as a comprehensive theory for transport choice explanation and prediction.

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