

Retired or Not, the Theory of Planned Behaviour Will Always Be With Us

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Retired or Not, the Theory of Planned Behaviour Will Always Be With Us

There was a time when the mere mention of the theory of planned behaviour (Ajzen, 1985, 1991) during the presentation of a paper at a health psychology or behavioural medicine conference would be accompanied by groans among audience members. Why? I propose three possible reasons. First, there are so many studies that have adopted the theory. It has permeated the investigation of intentional behaviour across a multitude of different behaviours and behavioural contexts. People had become sick of the sight of it by virtue of the sheer numbers of studies! Second, studies adopting the theory were all more or less the same bar the different behavioural context. Researchers had become fixated with a certain operationalisation of the theory measures and similar prospective, correlational designs to ‘test’ its premises. There was no variation in the rigid, rather mechanistic approach to the tests, which was not just uninformative but boring. Third, it was very ‘easy’ research to do. One could collect a relatively large data set using the correlational design and survey measures quite quickly and findings usually supported theoretical predictions. The research did not offer much that was novel. The preponderance of these tests has meant that a large body of research ‘testing’ the predictions of the theory has been amassed with relatively little heterogeneity in measures and study design. This has the advantage of permitting many cumulative syntheses of the theory (e.g., Albarracín, Johnson, Fishbein, & Muellerleile, 2001; Armitage & Conner, 2001; Hagger, Chatzisarantis, & Biddle, 2002; Hagger & Chatzisarantis, 2009; McEachan, Conner, Taylor, & Lawton, 2012; Cooke, Dahdah, Norman, & French, 2015). While the large cumulative tests of the theory is a strength, it has also highlighted some clear problems, many of which have been identified and discussed by Sniehotta and colleagues (2014) in their recent editorial appearing in *Health Psychology Review*. Their critique and suggestion to ‘retire’ the theory provoked considerable debate and controversy and when I circulated the editorial to 10 leading researchers in social and health psychology

and asked them to comment, there was a unanimous affirmation. I am pleased to present their commentaries (Trafimow, 2014; Abraham, 2015; Ajzen, 2015; Armitage, 2015; Conner, 2015; Gollwitzer & Oettingen, 2015; Hall, 2015; Ogden, 2015; Rhodes, 2015; Schwarzer, 2015) and Sniehotta et al.'s (2015) rebuttal in the current issue of *Health Psychology Review*.

It is clear from the commentaries that there is much love for theory and there is consensus that it has left an indelible mark on understanding intentional behaviour in health contexts and that it will continue to do so. But there is also recognition that the role and function of the theory has changed. I would argue that retired or not, researchers in health psychology will continue be influenced by the legacy left by the theory, and that it will continue to influence research but more as a precursor or guide rather than a central paradigm. I see the theory serving as the stem or root of new and 'extended' models of health behaviour. In my view, the theory of planned behaviour has, essentially, become 'framework' or starting point out of which new approaches are emerging that may overcome the limitations and boundary conditions of the theory, better satisfy the criteria for an effective system for explaining health behaviour, and provide better guidance as to the processes and mechanisms involved.

While researchers have recognised the limitations of the theory, such as its rather static nature and problems with predicting change, but it does not mean that some of its basic processes do not still have resonance. Borrowing Armitage's (2015) comparison of theory development in other disciplines like physics, Newton's theories of motion and gravity drove physics forward (so much so that they became almost axiomatic) and, while his theories have been superseded by quantum mechanics and other theoretical frameworks, particularly to account for sub-atomic phenomena, his theories still have substantial relevance, particularly at the macro-level (ask any engineer!). Many researchers will acknowledge that the theory of planned behaviour has already been superseded by other more elaborate and comprehensive

explanations, but those explanations are still influenced and retain some of the basic processes outlined in the theory.

What are the lasting contributions of the theory of planned behaviour? In my view, one of the key contributions is the identification of intentions as a central construct in the processes by which people engage in behaviour. Most theories acknowledge that action is, at least in part, a function of an individual's intentions and the processes that give rise to those intentions. The theory is certainly not unique in identifying the importance of the construct of intentions, there are many other approaches and precedents (e.g., Lewin, 1951; Meiland, 1970), but it has elucidated how intentions arise from beliefs about future outcomes, the central role of intentions in mediating the effects of those beliefs on behaviour, and how intentions are operationalised, and, to some extent, how they are developed. The theory has no doubt influenced subsequent thinking on intentional behaviour such as approaches that have focused on the processes by which intentions are converted into action like the model of action phases (Heckhausen & Gollwitzer, 1987; Gollwitzer, 1993; Gollwitzer & Oettingen, 2015) and the health action process approach (Schwarzer & Luszczynska, 2008; see also Hagger & Luszczynska, 2014 for a review; Schwarzer, 2014, 2015), and more elaborated models that incorporate multiple processes (e.g., Strack & Deutsch, 2004; Hall & Fong, 2007; Hall, 2015) or integrate components from other theories to arrive at more comprehensive explanations for behaviour (e.g., Gibbons, Houlihan, & Gerrard, 2009; Sniehotta, 2009b; Hagger & Chatzisarantis, 2014).

Of course, like any theory, there are limitations and boundary conditions (Chatzisarantis & Biddle, 1998; Chatzisarantis, Hagger, & Smith, 2007), many of which have been highlighted by Sniehotta et al. (2014) and the authors in the current set of commentaries (Trafimow, 2014; Abraham, 2015; Ajzen, 2015; Armitage, 2015; Conner, 2015; Gollwitzer & Oettingen, 2015; Hall, 2015; Ogden, 2015; Rhodes, 2015; Schwarzer, 2015). However, it is

important to note that the identification of limitations of the approach was recognised by Ajzen and Fishbein (1975; 1985) themselves decades ago, such as the boundary condition of correspondence and issues surrounding the role of past behaviour. The theory was also the subject of considerable debate and revision by researchers in social psychology attempting to elucidate the mechanisms and processes underpinning behavioural enactment (Triandis, 1980; Bentler & Speckart, 1981; Fredricks & Dossett, 1983; Warshaw, Sheppard, & Hartwick, 1983; Bagozzi, 1984; Liska, 1984; Liska, Felson, Chamlin, & Baccaglioni, 1984; Warshaw & Davis, 1985; Bagozzi, 1986), explorations and revisions which preceded the proliferation of interest in, and intensive testing of, the theory using the fixed correlational designs in health contexts, but which largely went unnoticed.

I think that the surge of interest in the theory, and the ease by which data could be collected on the ‘direct’ measures of the belief-based antecedents of behaviour, led to a proliferation of what amounted to a large number of replications of the theory using the same design and measurement, many with self-reported assessments of behaviour. This methodological approach has tended to become ubiquitous in the literature, and has probably contributed to the perception that the theory is a very rigid and static means to explain behaviour and also to its development. For example, in comparison to research adopting correlational designs using direct measures of the theory constructs, relatively few studies elucidate the contributing beliefs from first principles (Downs & Hausenblas, 2005), or adopt experimental (Sniehotta, 2009a) or cross-lagged panel designs (Bentler & Speckart, 1981; Liska et al., 1984; Lindwall, Larsson, & Hagger, 2011) to test hypotheses. The research also neglects the feedback loops of behaviour on cognitions (Ajzen, 2015) or reciprocal relations among the constructs (Liska et al., 1984; Hagger, Chatzisarantis, Biddle, & Orbell, 2001). This has probably meant that often neglected propositions of the theory, as well as its limitations and the gaps in the literature has been somewhat masked by preponderance of

short-term follow-up research adopting correlational designs, direct measures, and self-report behavioural assessments. This is not to say that there have been pleas for research that addresses these limitations, and there have been many who have called for changes in the approach away from these kinds of tests (Weinstein, 2007) and better evaluation of what constitutes support for the model (Ogden, 2003, 2015).

In fact, one of main problems symptomatic of the correlational approach to testing theory that has tended to dominate the literature is the lack of insight and analysis in what constitutes support for the theory. This was highlighted by Ogden (2003, 2015) in her analysis in which she questions the need for precision and clarity on evidence that would lead to ‘support’ for the theory and evidence that would provide a ‘failed replication’. For example, while researchers often find statistically significant relations among the theory constructs in research adopting a using correlational design, with all of the limitations that that particular design brings, there have also been many occasions where support for particular relations has not been found. This has occurred relatively frequently, such as the relation between subjective norms and intentions, which has often been found to be weak and not statistically significant (e.g., Chatzisarantis, Hagger, Smith, & Sage, 2006; Hagger & Chatzisarantis, 2006; Hagger et al., 2007). In such cases, the failure to find a particular relation has not led researchers to reject the theory, but rather indicate ‘general’ support for the model, the caveat of the failed hypothesis test in one instance notwithstanding. This is not only symptomatic of the correlational approach but also illustrates the need for precision in specification of hypotheses and what constitutes a ‘failed replication’ in all theories (Hagger & Chatzisarantis, in press).

In summary, whether the theory of planned behaviour is considered ‘retired’ or not, the general consensus is that its legacy cannot be ignored and many of the processes it outlined has influenced current thinking of the processes and mechanisms that underpin

health behaviour and will continue to do so in future. The theory will continue to serve as a basis or root of a multitude of new theories, revision and extensions, demonstrating its lasting contribution and the recognition, 'respect' as Gollwitzer and Oettingen (2015) eloquently put it, by the scientific community that it has helped shape thinking the processes underpinning health behaviour. The current debate does much to highlight how theory and thinking of health behaviour has moved on from the static, short-term, correlational tests of the theory and has provided some thoughtful suggestions as to how social and health psychologists can continue to advance knowledge and thinking of the processes and mechanisms that underpin health behaviour forward.

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