Uncorrected manuscript

Right about others, wrong about ourselves? Actual and perceived self-other differences in resistance to persuasion

Karen M. Douglas and Robbie M. Sutton

Keele University, United Kingdom

ADDRESS FOR CORRESPONDENCE:

Karen Douglas

Department of Psychology, Keele University

Keele, Staffordshire, ST5 5BG

United Kingdom

Ph: +44 1782 583384 Fax: +44 1782 583387 E-mail: k.m.douglas@keele.ac.uk

Parts of this research were supported by a Postdoctoral Fellowship to the first author at Massey University, Auckland, New Zealand.

Abstract

The third-person effect is the tendency for people to perceive the media as more influential on others than on themselves. The present study introduced a new methodological paradigm for measuring the TPE and examined whether the effect stems from an overestimation of the persuasibility of others, an underestimation of the persuasibility of the self, both, or neither. In three studies, we compared ratings of (a) current self attitudes (both baseline and post-persuasion), (b) current others' attitudes (both baseline and post-persuasion), (c), retrospective self attitudes, and (d) retrospective others' attitudes. We also measured traditional third-person perception ratings of perceived influence. Rather than overestimating others' attitude change, we found evidence that people underestimated the extent to which their own attitudes had, or would have changed.

Right about others, wrong about ourselves? Actual and perceived self-other differences in resistance to persuasion

The *third-person effect* or *TPE* (Davison, 1983) is the tendency for people to believe that socially undesirable media messages influence others more than themselves. Much research documents the TPE in a variety of domains including politics and news (e.g., Duck, Hogg & Terry, 1995; Perloff, 1989; Salwen & Dupagne, 1999), advertising (Duck, Hogg & Terry, 1998, 1999; Gibbon & Durkin, 1995), defamatory messages (Cohen, Mutz, Price & Gunther, 1988; Gunther, 1991), pornography (Gunther, 1995), offensive music (Eveland, Nathanson, Detenber & McLeod, 1999; McLeod, Eveland & Nathanson, 1997) and 'ideal' female images (David & Johnson, 1998). Research has also demonstrated a 'reversed' TPE whereby people perceive socially desirable messages as more influential on the self than others, (e.g., Duck, Terry & Hogg, 1995; Hoorens & Ruiter, 1996; Innes & Zeitz, 1988).

As a robust self-serving bias, the TPE is relevant to ongoing endeavours to clarify the limits of accuracy in social cognition (see also Colvin & Block, 1994; Sutton & McClure, 2001; Taylor & Brown, 1988). It also appears to have important social consequences. For example, the TPE has been found to predict endorsement of censorship and punitive responses to communicators (e.g., Gunther, 1995; McLeod *et al.*, 1997). In addition, the TPE has important social psychological antecedents, including a motive to maintain positive self esteem and a feeling of control over negative influences (e.g., Duck & Mullin, 1995; Duck *et al.*, 1995; Perloff, 1983), judgements of the severity of media content (Shah *et al.*, 1999), ego involvement (Perloff, 1989), and the social distance between self and others (Duck *et al.*, 1995; Duck *et al.*, 1998; Gibbon & Durkin, 1995).

The TPE is traditionally measured by exposing participants to persuasive media content and asking them to rate its persuasive effects on the self and others. The TPE is

therefore typically measured as the perceived difference between self and others' persuasibility to media messages. However, one important limitation of this methodology is that measures of own and others' persuasibility are obtained without reference to an objective measure of *actual* influence (Gunther, 1991; Gunther & Thorson, 1992). It is therefore impossible to determine the degree to which perceptions of one's own persuasibility, and perceptions of others', are in error. For this reason, it is impossible to distinguish *perceived* self-other differences in persuasibility from *actual* self-other differences in persuasibility. The purpose of the present paper is therefore to outline and test a new method of assessing the TPE that distinguishes between perceived and actual persuasibility. In so doing, we aim to attend to important, unanswered questions regarding the locus of error in third-person perceptions.

To elaborate, in our proposed design perceived and actual attitude change are directly comparable because they are assessed using the same items and calculated in the same way. At time one, baseline attitudes and perceptions of others' attitudes are obtained. At time two, with the same items, participants read a message and then (a) rate their own current attitudes, (b) recall their pre-message attitudes, (c) rate others' current attitudes, (d) retrospectively rate others' pre-message attitudes. Actual attitude change is the difference between participants' attitudes at time one and their current attitudes at time two. The change they attribute to themselves is reflected in the difference between their current and recalled attitudes at time two (i.e., between (a) and (b)). The change participants attribute to others is reflected in the difference between their perceptions of others' current and pre-message attitudes at time two (i.e., between (c) and (d)).

By sampling an entire undergraduate class, our sample group is also the comparison group, in that undergraduate students were asked to rate their own opinions and those of other

undergraduate students. This enabled us to identify whether participants' perceptions of others were accurate with confidence. This strategy has been successfully applied to other self-serving biases (e.g., Krueger & Dunning, 1999), and contrasts with other studies that have asked participants to compare themselves to broader, more remote groups such as 'other university students' for which no properly sampled empirical norm is available (e.g., Cohen *et al*., 1988, Gunther, 1991; Gunther & Thorson, 1992).

Employing this novel methodology allows us to answer the important question of where the error lies in third-person judgements: When people wrongly perceive others to be more influenced than themselves, do they *underestimate* how much they themselves were influenced, do they *overestimate* how much others were influenced, *both,* or is *neither* of these a cause of third-person perceptions? This question is relevant to censorship debates, as noted by McLeod *et al*. (1997, p. 165):

> If it is the case… that third-person perceptions are based on an overestimation of effects of others, the desire for censorship caused by third-person perceptions is built on a flawed foundation.

Conversely, if people are accurate about the effects on others but *under*estimate effects on *themselves*, the desire for censorship caused by third-person perceptions are not built on a flawed foundation, whatever the other merits and pitfalls of censorship might be.

More generally, answering the question will guide social psychologists in characterizing and responding to the TPE. Theoretically, the TPE may be an *invulnerability bias*, akin to people's tendency to wrongly imagine that they are not personally at risk from environmental or health hazards (e.g., Greening & Chandler, 1997; Klar, Medding & Sarel, 1996; Nurius, 2000, but see also Klein & Weinstein, 1997). If so, a deleterious consequence of the bias might be that individuals complacently expose themselves to noxious media

content, without being introspectively aware of changes in attitudes that the content produces. The appropriate way to correct the bias may be to correct people's inaccurate perceptions of themselves.

Some research in other domains suggests that people are often surprisingly inaccurate about their own mental processes (Nisbett & Wilson, 1977; Ross, 1977), particularly when they retrospectively report them (Ericsson & Simon, 1993). In particular, after undergoing attitude change people often underestimate the degree of change (e.g., Bem & McConnell, 1970; Markus, 1986; Wixon & Laird, 1976). They are therefore often unaware that their attitudes have changed. This suggests that people may often be wrong when evaluating the impact of a persuasive attempt on themselves.

However, other research suggests that people are often inaccurate about judgements and expectations regarding *others*. People may underestimate others' emotional responses to stimuli but be relatively accurate about their own responses (e.g., Sabini, Cosmas, Siepmann & Stein, 1999). Also, the TPE may reflect a kind of 'naïve cynicism' (Krueger & Gilovich, 1999) or 'observer harshness' regarding others (Colvin & Block, 1994), whereby people wrongly perceive others to be affected by quite innocuous messages. A deleterious consequence of the bias might be that people become unduly censorial (McLeod *et al*., 1997) or anxious about social decline (cf. Silka & Albright, 1984). The bias would then be corrected by addressing overly pessimistic perceptions of others.

Despite an abundance of research on the TPE, this issue has been relatively overlooked. Only a small number of studies have investigated the accuracy issue, and none have used a methodology akin to the method we have proposed in this paper (see Cohen *et al*., 1988; Gunther, 1991; Gunther & Thorson, 1992). To illustrate the method used in previous studies, Gunther (1991) presented an experimental group with a negative newspaper article

about an American police chief. He asked participants to rate their attitude to the police chief on a 19-point scale ranging from –9 (negative) to +9 (positive). Actual attitude change was defined as the difference between the scores of the experimental group and a control group who did not read the message.

For perceived change, Gunther asked participants to rate how much the article changed or would change their own and others' attitudes, using a different item with another 19-point scale with a 'no change' midpoint. Anchor points were not reported. To assess the accuracy of perceived attitude change for the self and others, Gunther compared the rated degree of attitude change to the index of actual attitude change. Other studies examining the accuracy issue (Cohen *et al*., 1988; Gunther & Thorson, 1992) used a very similar method to Gunther (1991), comparing an index of perceived attitude change with a different index of actual attitude change. Cohen *et al*. (1988) also did not report anchor points. However, Gunther and Thorson (1992) indicated that pre-test attitude measures were seven-point scales anchored by descriptive terms including "*good/bad, dislike/like, and positive/negative*" (p. 583) whereas the third-person effect measures were seven-point scales asking how much commercials affected their opinion of a product brand. The scales were anchored by "*more negative* and *more positive*, with the midpoint as a *no change* score" (p. 584).

The main issue regarding the use of this type of method is that scores on the perceived change item are not necessarily the same thing as difference scores derived from the attitude item. For example, it is not clear that when a participant indicated "+2" on the perceived change item, their score would have changed by two points on the attitude item. The psychological meaning of the arithmetic discrepancy between these indices of perceived and actual attitude change is therefore unclear. It is interesting to note that this method has produced different findings across studies: Gunther (1991; Gunther & Thorson, 1992)

reported that perceived self ratings were closer to actual attitude change, whereas Cohen *et al*. (1988) reported that perceived other ratings were closer to actual attitude change.

In an attempt to resolve this issue, we conducted three studies employing our new methodology which makes perceived and actual attitude change directly comparable. In doing so, we examined the extent to which people's perceptions of their own and others' attitude change reflect actual attitude change, thus assessing how accurate people are in their judgements of self and others' attitude change.

Study 1

In the first study, we asked undergraduate participants to rate their agreement or disagreement with a list of statements about the issue of gun control (baseline self). They were then asked to rate how much they thought their classmates would agree or disagree with the statements (baseline other). One week later, participants read a persuasive message about gun control and were presented with the same list of statements from the week before. They were asked to rate (a) their current attitudes (current self), (b) what they perceived their classmates' attitudes to be (current other), (c) their attitudes before reading the material (retrospective self) and (d) what they perceived their classmates' attitudes were before they read the material (retrospective other).

We hypothesised that people would perceive others to be more persuaded than themselves, by indicating lower perceived attitude change for the self (current self – retrospective self) than for others (current other – retrospective other). By examining the difference between perceived and actual (current self – baseline self) attitude change scores, we sought to investigate if participants underestimate how much their own attitudes changed or overestimate the attitude change of others (see Cohen *et al*., 1988, Gunther, 1991; Gunther & Thorson, 1992).

Method

Participants and Design

A total of 80 male and female undergraduate psychology students from a New Zealand University participated in the first phase of the experiment. Out of this original sample, 65 students also participated in phase two. Participants' median age was 23.0. Each participant was rewarded with a small sweet for their participation. The experiment consisted of a 2 (person: self/other) x 3 (time of attitude rating: baseline/retrospective/current) repeated-measures design.

Materials and Procedure

The experiment consisted of two phases. Firstly, participants were informed that the experiment was about opinions towards the issue of gun control. Participants were also informed that the study would continue in the following week, and because their responses were anonymous, they were asked to write a code on their response sheet so that their responses in the first phase could be linked to their responses in the second phase.

Participants were then presented with six statements about the issue of gun control. These statements related to people's rights to own guns and to protect themselves, the danger of guns to society, government restriction of guns, and the use of guns for sport and recreation. The full list of items is presented in the Appendix. Note that three items were reverse-coded. For each statement, participants were asked to rate their agreement or disagreement on a seven-point scale from 1 'strongly disagree' to 4 'neutral' to 7 'strongly agree'. The six-item scale had acceptable reliability (Cronbach's $\alpha = .80$). Question order was randomised.

After completing the ratings of their own agreement or disagreement with the gun control statements, participants were asked to complete the same scale, but were asked instead

to rate how much they thought *other people in their class* would agree or disagree with each statement. [1] The scale for ratings of classmates' attitudes was moderately reliable (Cronbach's $\alpha = .63$), although less so than for ratings of own attitudes. At the end of phase one, participants were asked to remember their code and were thanked for their time.

Phase two of the experiment took place one week later. The delay was intended to make it more difficult for participants to remember their original responses. A cover sheet informed participants that they would be asked to read some material about the issue of gun control and respond to some questions. They were then asked to turn the page and read the material carefully. The material presented to participants was a *pro-gun* extract from an Internet website entitled "The Right to Keep and Bear Arms" (http://www.rkba.com). The message argued against gun control, promoting the rights of people to own guns, alleging that restricting gun ownership is unfair, and outlining several putative reasons why guns are not a danger to society. Because the message was taken from a North American Internet website, spellings (e.g., "defense") were changed to suit the New Zealand context (e.g., "defence"). Also, the particular words "felon" and "firearm" were changed to "criminal" and "gun" for the same purpose.

After participants read the pro-gun message, they were presented with the same six-item scale as utilized in phase one. Participants were asked to respond to these items four times by rating: (a) their *current* agreement or disagreement with each statement (current self, $\alpha = .77$), (b) how much they agreed or disagreed with each statement *before reading the material* (retrospective self, $\alpha = .78$), (c) how much they think their *classmates* would *currently* agree or disagree with each statement (current other, $\alpha = .50$), and (d) how much they think their *classmates* would have agreed or disagreed with each statement *before reading the material* (retrospective other, $\alpha = .70$). Again, participants were asked to respond

to each item on a seven-point scale from 1 'strongly disagree' to 4 'neutral' to 7 'strongly agree'. Question order was randomised and the ordering of presentation of the attitude rating was blocked for 'self' and 'other' and then randomised across time (retrospective or current). Participants were then asked how much they thought the reading material would influence their own, their classmates', and the general public's attitudes towards gun control. These items were measured on a seven-point scale from 1 'not at all' to 4 'somewhat' to 7 'very much'. Question order was randomised. Participants were then asked to indicate their code from phase one, were debriefed, and were thanked for their participation.

Results and discussion

Note that throughout the paper, we have used one-tailed tests where *a-priori* predictions have been made. Results were entered into a 2 (person: self/other) x 3 (time of attitude rating: baseline/retrospective/current) repeated-measures ANOVA. The ANOVA revealed a main effect for rated person, such that participants judged their classmates as more pro-gun ($M = 3.66$) than themselves overall ($M = 2.94$), $F\ (1,62) = 22.70$, $p < .0001$, $\eta^2 = .27$. This tendency was also significant for each time of attitude rating (all p-values<.01) and is consistent with a self-serving bias to perceive the self positively compared to others (e.g., Martijn, van der Pligt & Spears, 1996; Klein, 2001; Krueger & Dunning, 1999). Results also revealed a main effect for time of attitude rating, such that mean pro-gun ratings increased with time from baseline ($M = 3.18$) to retrospective ($M = 3.25$) to current attitude ($M = 3.46$), $F\ (2,62) = 4.79$, $p < .01$, $\eta^2 = .07$. Finally, there was an interaction between person (self/other) and time of attitude rating, $F\ (2,62) = 7.48$, $p < .001$, $\eta^2 = .11$. Table 1 shows the significant between-cell differences. Note that not all comparisons in the table (also Tables 2 and 3 for Studies 2 and 3) were theory-driven and that our discussions focus only on hypothesis-driven tests.

Perceived attitude change for self and others

Examining the difference between current and retrospective attitudes provides an index of how much participants perceived their own, and others' attitudes, to have changed. These comparisons revealed, as hypothesised, that attitude change was perceived to occur for others but not for the self. The difference scores between current and retrospective attitudes for self and others revealed that attitude change was also judged to be greater for others ($M = 0.33$) than the self ($M = 0.07$), $t\ (64) = 1.88$, $p < .03$.

Actual attitude change

Participants attitudes were more pro-gun in phase two than in phase one, indicating that they were influenced by the article. However, participants' perceptions of their own attitude change (current self - retrospective self, as above) were significantly lower than this actual net attitude change (self current – self-baseline, $M = 0.45$) so that paricipants underestimated the extent to which they were influenced, $t\ (64) = 3.67$, $p < .005$.

Accuracy of attitude change perceptions

Further, participants' actual attitude change was no different to the attitude change they attributed to their classmates (other current – other retrospective), $t\ (64) = .61$, *ns*. As stated earlier, current-baseline attitudes for the self are the correct reference for evaluating the accuracy of current-retrospective scores for both self and others, because the best estimate of others' scores are those of the self, since our sample and 'others' are one and the same. Because participants' estimates of their own and others' attitude change were no different, and yet participants significantly underestimated their own attitude change, this suggests that participants more closely predicted others' attitude change than their own.

It is useful to consider how this inaccuracy about the self and accuracy about others may occur. Participants retrospectively rated their earlier attitudes as *more* pro-gun than they

actually were at baseline, but no different to their current attitudes. This suggests that participants did not realise that their attitudes had changed. Perhaps they lack accurate recall of their original attitudes, and orient their recollections to match their current attitudes (cf. Bem & McConnell, 1970; Markus, 1986; Wixon & Laird, 1976). For ratings of classmates' attitudes, a different pattern emerged. Participants retrospectively estimated their classmates' attitudes to be marginally *less* pro-gun than they rated them at baseline, t (64) = 1.86 $p < .08$. Participants appear to have assimilated their retrospective self-ratings to be more like their current attitudes. On the other hand, they appear to have contrasted estimates of others' retrospective attitudes from estimates of their current attitudes. This creates the illusion that others' attitudes have changed, but not their own. Overall, this pattern of results contrasts with the idea that people are accurate about their own degree of attitude change, and inaccurate about others' (Gunther, 1991, 1995; Gunther & Thorson, 1992; McLeod *et al.*, 1997). Here, participants more closely predicted others' attitude change than their own.

Single item measures

Participants rated the influence of the message on their classmates to be greater (M = 3.83) than on themselves (M = 2.77), t (64) = 6.31, $p < .0001$. The perceived difference between the general public (M = 3.98) and the self was also significant, t (64) = 7.31, $p <$ .0001, but the difference between the perceived effect on classmates and the general public was not significant, t (64) = 1.17, *ns*., which does not support the social distance hypothesis that third-person effects increase as the perceived other becomes more remote from the self (e.g., Duck *et al.*, 1995; Duck *et al.*, 1998; Gibbon & Durkin, 1995). However, it is possible that the perceived social distance between classmates and the general public was quite small due to the circumstances of this experiment. The experiment was conducted at the beginning of semester when participants had not yet had a chance to develop a strong sense of group

identity. Subsequently, they may not have perceived their classmates as very different from the broader general public.

Finally, there was surprisingly no correlation between the 'scale' TPE (other estimated change – self estimated change) and the traditionally measured 'classic' TPE as measured by the items for both the participants' classmates, r (81) = -.06, *ns*, and the general public, r (81) = -.008, *ns*. We return to this issue in Study 2.

Study 2

Having employed our new method of measuring the TPE, results of Study 1 indicate that people appear to underestimate the extent to which their own attitudes have changed. We found no evidence to suggest that people overestimate the persuasibility of others. We conducted a second study to clarify some methodological issues. In particular, when we asked participants to recall their prior attitudes, we asked them to recall what they thought 'before reading the material'. It is not clear that 'before reading the material' would be interpreted by participants to mean 'the week before', which was our original intention. In reality therefore, the recalled attitude may have represented an already modified view based on participation in the first phase of the experiment. Clearly we needed to rule out this potential problem and others associated with longitudinal designs. We therefore conducted a second study utilising a cross-sectional design with a control group who did not read any message, and an experimental group who received a persuasive message. In Study 2, we also tested the TPE in the realm of environmental issues, asking participants to rate their own and others' attitudes concerning fossil fuel use and global warming.

Method

Participants and design

A total of 87 male and female undergraduate psychology students at a New Zealand University participated in the experiment. Participants' median age was 28.1. Each participant was rewarded with a small sweet for their participation. The experiment consisted of a 2 (person: self/other) x 2 (attitude rating: retrospective/current) within-subjects design for the experimental condition. Person and attitude rating were both manipulated within-subjects. We also included a control condition where person (self/other) was manipulated within-subjects. Both control and experimental participants were taken from the same sample of undergraduate students and participants were randomly assigned to the control or experimental groups so that both groups did not differ in demographic characteristics.

Materials and Procedure

Participants in the control group were informed that they were going to be asked some questions about the important issue of fossil fuel use and global warming. The examples of fossil fuels given to participants were oil and gas. Following this brief explanation of the task, participants were then presented with four statements about fossil fuel use. These statements related to fossil fuel use harming the environment, governments restricting fossil fuel use, fossil fuel use and carbon dioxide in the atmosphere, and the trade-off between economic growth and global warming. The full set of items is presented in the Appendix. Note that two items were reverse-coded. For each statement, participants were asked to rate either their own, or others' disagreement on a seven-point scale from 1 'strongly agree' to 4 'neutral' to 7 'strongly agree'. Those participants who rated their own attitudes first were asked to rate others' attitudes second, and vice versa. [2] The reference 'other' was other students in the class, as in Study 1. We acknowledged to our participants that they might find

this a difficult task, but nevertheless asked them for their 'best guess' as to what these others' attitudes might be.

Participants in the experimental condition were informed that they would be asked to read some material about the important issue of fossil fuel use and global warming, and to answer some questions. At this point, participants were given a *pro-fossil fuel use* message entitled "The myth of fossil fuel use and global warming". The message was adapted for our purposes from a PDF document appearing on an Internet website called "Fossilfuels.org" (http://www.fossilfuels.org/pdf/FDPart01.pdf). The message argued on the basis of scientific evidence that fossil fuel use does not harm the environment. It argued that carbon dioxide (CO_2) is the basis of all life, and is not a pollutant. It also argued that any climate change resulting from increased CO_2 in the atmosphere was benign. After participants read the pro-fossil fuel use message, they were presented with the same four-item scale as for the control participants. Participants were asked to respond to these items four times by rating: (a) their *current* agreement or disagreement with each statement (current self), (b) how much they agreed or disagreed with each statement *before reading the material* (retrospective self), (c) how much they think their *classmates* would currently agree or disagree with each statement (current other), and (d) how much they think their *classmates* would have agreed or disagreed with each statement *before reading the material* (retrospective other). Again, participants were asked to respond to each item on a seven-point scale from 1 'strongly disagree' to 4 'neutral' to 7 'strongly agree'. Question order was blocked for 'self' and 'other' and then alternated across time (retrospective or current), such that there were four different versions of the questionnaire. Across the control and experimental groups, reliabilities for the fossil fuel scale were acceptable (self current $\alpha = .63$, other current $\alpha = .70$, self retrospective $\alpha = .83$, other retrospective $\alpha = .57$). However, omitting the item concerning economic growth and

global warming considerably increased the reliability of the scale (αs of .69, .71, .93 and .66 retrospectively). So, all analyses were conducted on the three-item scale, with the economic growth item removed. [3]

Participants were also asked how much they thought the message would influence their own, and their classmates' opinions concerning fossil fuel use to provide traditional TPE measures. Participants were asked to respond on a seven-point scale from 1 'the message would influence opinions to become more in favour of fossil fuel use' to 4 'the message would not influence opinions, to 7 'the message would influence opinions to become more against fossil fuel use. As readers will recall, the correlations between the TPE items and attitude change measures in Study 1 were not significant. These correlations took on the one hand the TPE item score and on the other, the degree to which each person mis-estimated their own attitude change (other estimated change – self estimated change). A possible reason why these correlations were non significant could be because in Study 1, we did not specify the *direction* of the influence. In Study 2, we questioned participants whether they thought the message would influence their attitudes to become more pro-fossil fuel use, neutral, or more anti-fossil fuel use, making the measure more compatible with, and therefore more directly comparable to, the TPE scale measurements. Question order was randomised. Participants were then debriefed, and were thanked for their participation.

Results and discussion

Results for the experimental group were entered into a 2 (person: self/other) x 2 (attitude rating: retrospective/current) repeated measures ANOVA. The analysis revealed that participants rated their classmates as more pro-fossil fuel use (M = 2.68) than themselves overall (M = 2.33), F (1,41) = 8.51, $p < .01$, $\eta^2 = .17$, as in Study 1. Results also revealed a main effect for attitude rating, such that mean pro-fossil fuel ratings increased from

retrospective (M = 2.33) to current (M = 2.69), F (1,41) = 5.76, $p < .05$, $\eta^2 = .12$, as in Study 1. Finally, the interaction between person (self/other) and attitude rating, was marginally significant, F (1,41) = 3.11, $p = .085$, $\eta^2 = .07$. Significant between-cell differences are displayed in Table 2.

Perceived attitude change for self and others

We examined the difference between current and retrospective attitudes, and as in Study 1, results revealed that attitude change was perceived to occur for others, but not for the self. The difference scores between current and retrospective attitudes for self and others revealed that attitude change was also judged to be greater for others than the self, (Ms = 0.54 and 0.18), t (41) = 1.77, $p < .05$, as in Study 1.

Actual attitude change

Participants were *more* pro-fossil fuel use in the experimental group than in the control group, indicating, as in Study 1, that attitude change would have occurred. However, participants' perceptions of their own attitude change (M = 0.18) were significantly lower than actual attitude change that would have occurred (M = 0.51), so that participants underestimated the extent to which they would have been influenced, t (41) = 2.75, $p < .005$, as they did in Study 1.

Accuracy of attitude change perceptions

Further, participants' actual attitude change was no different to the attitude change they attributed to their classmates, t (41) = .17, *ns.* Therefore, as in Study 1, participants' estimates of their own and others' attitude change were no different, and yet participants significantly underestimated the attitude change that would have occurred for themselves. Therefore, participants again more closely predicted others' attitude change than what their own would have been.

Experimental group participants' retrospective ratings of their attitudes were marginally more pro-fossil fuel use than the attitudes of the control group, t (87) = 1.5, p= .07, but no different to current attitudes. This suggests, although less conclusively than in Study 1, that participants mis-estimated what their original attitudes would have been (cf. Bem & McConnell, 1970; Wixon & Laird, 1976). Participants did not rate others' retrospective attitudes to be lower than the control (no message) group. As in Study 1, these results suggest that participants possibly assimilated their retrospective self-ratings to be more like their current attitudes. However, they did not do this in their judgements of others, so attitude change appears apparent for others but not for the self.

Single item measures

Participants rated that the message would influence their classmates' attitudes to become more pro-fossil fuel use (M = 3.33) but that their own attitudes would remain neutral (M = 4.02). This difference in perceived influence was significant, t (42) = 4.01, $p < .001$. In Study 2, we have no univariate score indicating how much each person was inaccurate about their own prior attitudes, so we cannot assess the correlation between the TPE and attitude change. The correlation between the TPE on the scale (other estimated change – self estimated change) and the 'classic' TPE on the item (for classmates) was, however significant, r (42) = .40, $p < .01$.

Study 3

Results of Study 2 largely support those of Study 1, suggesting that people underestimate the extent to which their attitudes would have changed as a result of a persuasive message. Again, we found no evidence that people overestimated the extent to which others' attitudes would have changed. There was also a significant correlation between

the TPE as measured by traditional items, and as measured by attitude change scales, in contrast to Study 1.

In Study 3, we aimed to investigate the overestimation/underestimation issue further, using a different message type. Rather than presenting participants with a 'negative' persuasive message as is traditionally used in studies of third-person perceptions, we presented participants with a positive or socially desirable message. This took the form of material arguing *against* fossil fuel use for envirnomental reasons. Research to date suggests that under such conditions, the third-person effect is often reversed so that people perceive themselves as more likely to be influenced than others (e.g., Duck *et al.,* 1995; Hoorens & Ruiter, 1996; Innes & Zeitz, 1988). The overestimation/underestimation issue remains to be tested for positive messages.

Indeed, if the inaccuracy always lies within the self, then we might expect people to *over*estimate the extent to which they are influence by positive media content, and again to be accurate about the effect the message has on others. This would make intuitive sense, as people may feel that positive attitude change is acceptable, yet may not like to admit that their attitudes have changed towards a socially undesirable opinion. Recent research investigating people's perceptions of self/other differences in pro-social behaviour may shed some light on the 'reversed' TPE. Epley and Dunning (2000) examined peope's perceptions of their own and others' charitable behaviours and their results revealed that people *over*estimate the extent to which they will engage in selfless and generous behaviours, but are accurate about the frequency of such behaviours performed by peers. Perhaps therefore, people will also overestimate the extent to which they are influenced by positive media.

Method

Participants and design

A total of 92 males and females participated in the experiment, with a median age of 33.72, which is comparable in age to our sample from Study 2. Participants in this study were a sample of friends and family of undergraduate social psychology students at a New Zealand University. The experiment consisted of a 2 (person: self/other) x 2 (attitude rating: retrospective/current) within-subjects design for the experimental group. Person and attitude rating were both manipulated within-subjects. We also included a control condition where person was manipulated within-subjects. Both control and experimental participants were taken from the same sample of friends and family and participants were randomly assigned to the control or experimental group so that both groups did not differ in demographic characteristics.

Materials and Procedure

Participants in the control group followed the same procedure as the control group participants in Study 2. However, in place of asking participants to rate the attitudes of others in their class, they were asked to rate the attitudes of friends and family of social psychology students at the university where the investigation was undertaken. We acknowledged to our participants that they might find this a difficult task, but nevertheless asked them for their 'best guess' as to what their attitudes might be. We made this change to previous studies mainly for ease of data collection. However, we also anticipated the possibility of ceiling effects amongst a sample of (possibly quite liberal) undergraduate students, that could potentially be avoided by sampling the general population.

Participants in the experimental condition were informed that they would be asked to read some material about the important issue of fossil fuel use and global warming, and to

answer some questions. At this point, participants were given an *anti-fossil fuel use* message entitled "The truth about fossil fuel use and global warming". The message was adapted from the message utilised in Study 2, so that the message argued against fossil fuel use rather in favour. The message argued on the basis of scientific evidence that fossil fuel use harms the environment. It argued that increased levels of carbon dioxide (CO_2) brought about by use of fossil fuels causes damaging climate change.

After participants read the pro-fossil fuel use message, they were presented with the same four-item scale as for the control participants. Participants were asked to respond to these items four times by rating: (a) their *current* agreement or disagreement with each statement (current self), (b) how much they agreed or disagreed with each statement *before reading the material* (retrospective self), (c) how much they think *friends and family of social psychology students at the university* would currently agree or disagree with each statement (current other), and (d) how much they think *friends and family of social psychology students at the university* would have agreed or disagreed with each statement *before reading the material* (retrospective other). Again, participants were asked to respond to each item on a seven-point scale from 1 'strongly disagree' to 4 'neutral' to 7 'strongly agree'. Question order was blocked for 'self' and 'other' and then alternated across time (retrospective or current), such that there were four different versions of the questionnaire.

Across the control and experimental groups, reliabilities for the fossil fuel scale were acceptable (self current $\alpha = .74$, other current $\alpha = .75$, self retrospective $\alpha = .73$, other retrospective $\alpha = .74$). However as in Study 2, omitting the item concerning economic growth and global warming increased the reliability of the scale (αs of .74, .79, .77 and .76 retrospectively). So, all analyses were conducted on the three-item scale, with the economic growth item removed.

Participants were also asked how much they thought the message would influence their own, and their comparison others' opinions concerning fossil fuel use. Participants were asked to respond on a seven-point scale from 1 'the message would influence opinions to become more in favour of fossil fuel use' to 4 'the message would not influence opinions, to 7 'the message would influence opinions to become more against fossil fuel use'. Question order was randomised. Participants were then debriefed, and were thanked for their participation.

Results and discussion

Results for the experimental group were entered into a 2 (person: self/other) x 2 (attitude rating: retrospective/current) repeated measures ANOVA. The analysis revealed that participants rated others as less against fossil fuel use ($M = 5.58$) than themselves overall ($M = 6.03$), $F\ (1,42) = 15.61$, $p < .001$, $\eta^2 = .27$ which is in line with Studies 1 and 2 where participants attributed more socially desirable attitudes to the self than others. Results also revealed a main effect for attitude rating, such that mean anti-fossil fuel ratings increased from retrospective ($M = 5.54$) to current ($M = 6.06$), $F\ (1,42) = 46.63$, $p < .001$, $\eta^2 = .53$. Finally, the interaction between person (self/other) and attitude rating was significant, $F\ (1,42) = 10.20$, $p < .01$, $\eta^2 = .20$. Significant between-cell differences are shown in Table 3.

Attitude change perceptions for self and others

As in Studies 1 and 2, perceived attitude change for both self and others were calculated by comparing current attitudes with retrospective attitudes. Participants attributed attitude change to others as in Studies 1 and 2, but also to themselves in contrast to previous studies. However, as in Studies 1 and 2, participants perceived others to be more persuaded than themselves, ($Ms = 0.70$ and 0.35), $t\ (42) = 3.20$, $p < .01$. Even in the case of a socially

desirable message where participants *did* admit to being persuaded themselves (unlike Studies 1 and 2), participants still perceived others to be more influenced than themselves.

Actual attitude change

As predicted, participants' current attitudes were *more* anti-fossil fuel use in the experimental group than in the control group. This suggests that people in the experimental group were caused by the message to become more against fossil-fuel use than they would have been given no message.

Accuracy of attitude change perceptions

The difference between the control and experimental groups ($M = 0.57$) was no different to the attitude change participants attributed to others ($M = 0.70$), $t\ (42) = 1.21$, *ns*. That is, participants accurately estimated the attitude change that would have occurred for others, as expected, and this finding is consistent with Studies 1 and 2. However, participants' estimates of their own attitude change ($M = 0.35$) were significantly lower than what their actual attitude change would have been, $t\ (42) = 2.99$, $p < .01$. Therefore, people did not overestimate the extent to which their attitudes would have changed in the direction of positive influence as we may have expected. In line with Studies 1 and 2, participants again significantly *under*estimated the extent to which their attitudes would have changed.

Single-item measures

Participants rated that the message would influence others' attitudes to become more against fossil fuel use ($M = 5.80$) more than their own ($M = 5.48$). This difference in perceived influence was significant, $t\ (42) = 2.55$, $p < .01$ and further supports the typical rather than the 'reversed' TPE. However, the correlation between the TPE on the scale (other estimated change – self estimated change) and the 'classic' TPE on the items (for others) was not significant, $r\ (43) = .05$, *ns*, in contrast to Study 2.

General Discussion

In summary, the methodology we employed in Study 1 disentangled the components of attitude change judgements implicated in third-person perceptions: first-person and third-person judgements, and retrospective and current judgements. Results of Study 1 showed, contrary to previous research (Gunther, 1991, Gunther & Thorson, 1992, but see Cohen *et al.*, 1988), that people appeared to underestimate the extent of their own persuasibility, rather than overestimating the extent of others' persuasibility. Results of Studies 2 and 3, utilising cross-sectional experimental designs, lend support to this analysis. Therefore, returning to issue of censorship endorsement, it would appear that people's desire to censor material is not related to overly pessimistic views about the extent to which others are influenced. Because our participants accurately estimated others' attitude change, our results suggest that endorsing censorship may be based on a realistic assessment of the degree to which others are influenced, but perhaps also an overly optimistic view of their own persuasibility.

In regard to the more general issue of whether self-other biases arise from errors in perception of the self or others, our results show that whereas people are accurate about others' *attitude change*, they are wrong about others' *current attitudes*. In our studies, participants consistently displayed undue negativity about others' attitudes, perceiving them to be less desirable than they actually were. Overall, this pattern of results suggests that there is perhaps no invariant locus of error in self-other biases. Sometimes these biases are likely to emerge from errors about the self (cf., Epley & Dunning, 1999; Klar & Giladi, 1999), and sometimes from errors about others (cf., Klein & Weinstein, 1997). In our view, because the locus of error in self-other bias appears to be contingent rather than invariant, researchers ought to examine the theoretical and empirical grounds for predicting that the perception of (a) the self and (b) of others is accurate *in particular domains*. For example, the present

research was informed by earlier findings that people have little access to changes in their own attitudes (Bem & McConnell, 1970; Nisbett & Wilson, 1977). In other domains, individuals may be rather accurate about themselves (Ericsson & Simon, 1993).

An important methodological issue requires some discussion here. In Study 2, the difference between perceived self and others' attitude change as measured by the scales, was significantly correlated with the traditionally measured TPE, as we expected. We argue that the 'scale' and 'classic' TPEs *should* be correlated because they are measuring the same construct - the difference in perceived attitude change between self and others. However, in Studies 1 and 3, this correlation was not significant. Methodological differences between our studies may be responsible for this inconsistency. As we mentioned earlier, the results of Study 1 are less reliable than Study 2 because the measure of the 'scale' TPE was flawed. By asking for participants attitudes 'before reading the material', we cannot be sure that their reported attitudes were those they held at the beginning of the experiment, before participating in the first phase and engaging in thought about the issue of gun control. Our original intention could have been better achieved by asking participants to simply recall their opinions 'from the week before'. As such, the lack of correlation between the 'attitude scale' and 'classic' TPE is perhaps not surprising. Also, Study 3 employed a group of others that was not particularly well-defined (i.e., friends and family of students at the university). It is unlikely that participants will have well-formed opinions about such a heterogeneous group. Nor would they naturally compare their own opinions with the opinions of this group. We may therefore be able to place more confidence in the results of Study 2 than Studies 1 and 3, and the stronger correlation between the 'attitude scale' and 'classic' TPE obtained in this study.

Further research is required to clarify other questions relating to the TPE. Our results have interesting implications for studies of *positive* message influence, as examined in Study 3. We originally proposed that inaccuracy about the self in this case may mean that people self-servingly *over*estimate the extent to which their attitudes have changed as a result of a positive message. However, participants actually *under*estimated the extent to which their attitudes would have changed, just as they did in the case of negative messages in Studies 1 and 2. So we did not replicate the reversed TPE as obtained in other research (e.g., Duck *et al.,* 1995; Hoorens & Ruiter, 1996; Innes & Zeitz, 1988). It is also interesting to note that we did not replicate the reversed TPE as measured by traditional items. Participants reported that the message would influence others to become more against fossil fuel use than themselves.

It may be the case that people perceive that they themselves already hold the socially desirable view advocated by the message whilst others may not. If this is the case, then it would be perfectly reasonable to expect people to perceive others as more influenced than themselves. It is also important to note that not all studies investigating the impact of positive messages have shown greater perceived influence on the self over others. Duck and colleagues (1995) only observed a reversed TPE when positive messages (in this case, AIDS advertisements) were perceived to be 'high quality'. No reversal occurred for messages seen as 'low quality'. Further, the TPE was reversed for participants who believed strongly that it was good to be influenced by AIDS campaigns but other participants did not distinguish between the level of impact on self and others. Also, Duck and colleagues (1999) found that high identifiers (in this case, with a student ingroup) perceived AIDS advertisements to have more influence on themselves than others, but low identifiers displayed the typical TPE. This research suggests that the TPE is not always reversed for positive messages. It is not the desirability of the message per se, but when influence is "normatively acceptable" (p. 1879),

that determines the direction of the TPE. Other factors such as social identity also moderate the reversal of the TPE.

It is important, however to acknowledge the limitations of Study 3. First, the inclusion of a condition where participants read an undesirable message would have allowed us to make a direct comparison between the effects of desirable and undesirable messages on perceived and actual influence. Further, the sample and target group could have been more consistent with Studies 1 and 2, as mentioned before. It is also likely that results suffered from near ceiling effects as participants ratings of current attitudes approached the upper end of the scale. Finally, a manipulation check assessing message desirability (also an issue for Study 2) could have been included to assure that participants did indeed perceive the message to be positive and/or socially desirable. However, despite these issues, Study 3 provides a promising glance into the effects of desirable messages on perceived and actual attitude change, and suggests that further research should perhaps pay closer attention to the validity of the 'reversed TPE'.

These results also have interesting implications for the use of retrospective pre-testing in assessing change (cf. Rippey, Geller & King, 1978; Sprangers & Hoogstraten, 1989). This technique, typically used in evaluating learning programmes, measures change by comparing retrospective pre-test ratings with post-test ratings. The difference between the two indicates how much learning has occurred. Our results, however, imply that people may not be able to accurately report their previous attitudes. Therefore, it may perhaps also be the case that self-reports of abilities and knowledge are not completely reliable, questioning the utility of retrospective pre-testing as a tool for assessing change.

Research on attitude change and third-person perceptions lie at the intersection between two academic disciplines, namely communication and social psychology. This

research also lies at the intersection between different areas of social psychological inquiry, such as self-perception, self-serving biases, and intergroup processes. This paper makes substantial progress in mapping out this intersection. It provides a novel approach to the study of the TPE and its results provide new answers to some outstanding questions.

References

Bem, D.J., & McConnell, H.K. (1970). Testing the self-perception explanation of dissonance phenomena: On the salience of premanipulation attitudes. *Journal of Personality and Social Psychology, 14,* 23-31.

Biernat, M., Manis, M., & Kobrynowicz, D. (1997). Simultaneous assimilation and contrast effects in judgments of self and others. *Journal of Personality & Social Psychology, 73,* 254-269.

Bosveld, W., Koomen, W., & Vogelaar, R. (1997). Construing a social issue: Effects on attitudes and the false consensus effects. *British Journal of Social Psychology, 36,* 263-272.

Cohen, J., Mutz, D., Price, V., & Gunther, A. (1988). Perceived impact of defamation: An experiment on third-person effects. *Public Opinion Quarterly, 52,* 161-173.

Colvin, C.R., & Block, J. (1994). Do positive illusions foster mental health? An examination of the Taylor and Brown formulation. *Psychological Bulletin, 116,* 3-20.

David, P., & Johnson, M.A. (1998). The role of self in third-person effects about body image. *Journal of Communication, 48,* 37-58.

Davison, W.P. (1983). The third-person effect in communication. *Public Opinion Quarterly, 47,* 1-15.

de-la-Haye, A. (2001). False consensus and the outgroup homogeneity effect: Interference in measurement or intrinsically dependent processes? *European Journal of Social Psychology, 31,* 217-230.

de-la-Haye, A. (2000). A methodological note about the measurement of the false consensus effect. *European Journal of Social Psychology, 30,* 569-581.

Duck, J.M., Hogg, M.A., & Terry, D.J. (1995). Me, us and them: Political identification and the third-person effect in the 1993 Australian federal election. *European Journal of Social Psychology, 25,* 195-215.

Duck, J.M., Hogg, M.A., & Terry, D.J. (1998). Perceived self-other differences in persuasibility: The effects of interpersonal and group-based similarity. *European Journal of Social Psychology, 28,* 1-21.

Duck, J.M., Hogg, M.A., & Terry, D.J. (1999). Social identity and perceptions of media persuasion: Are we always less influenced than others? *Journal of Applied Social Psychology, 29,* 1879-1899.

Duck, J.M., & Mullin, B.A. (1995). The perceived impact of the mass media: Reconsidering the third person effect. *European Journal of Social Psychology, 25,* 77-93.

Duck, J.M., Terry, D.J., & Hogg, M.A. (1995). The perceived influence of AIDS advertising: Third-person effect in the context of positive media content. *Basic and Applied Social Psychology, 17,* 305-325.

Duck, J.M., Terry, D.J., & Hogg, M.A. (1998). Perceptions of a media campaign: The role of social identity and the changing intergroup context. *Personality and Social Psychology Bulletin*, 24, 3-16.

Epley, N., & Dunning, D. (2000). Feeling "holier than thou": Are self-serving assessments produced by errors in self- or social prediction. *Journal of Personality and Social Psychology, 79,* 861-875.

Ericsson, K.A., & Simon, H.A. (1993). *Protocol analysis: Verbal reports as raw data.* (rev. ed), Cambridge MA: MIT Press.

Eveland Jr., W.P., & McLeod, D.M. (1999). The effect of social desirability on perceived media impact: Implications for third-person perceptions. *International Journal of Public Opinion Research, 11,* 315-333.

Eveland Jr., W.P., Nathanson, A.I., Detenber, B.H., & McLeod, D.M. (1999). Rethnking the social distance corollary: Perceived likelihood of exposure and the third-person effect. *Communication Research, 26,* 275-302.

Gibbon, P., & Durkin, K. (1995). The third person effect: Social distance and perceived media bias. *European Journal of Social Psychology, 25,* 597-602.

Greening, L., & Chandler, C.C. (1997). Why it can't happen to me: The base rate matters, but overestimating skill leads to underestimating risk. *Journal of Applied Social Psychology, 27,* 760-780.

Gross, S.R., & Miller, N. (1997). The "golden section" and bias in perceptions of social consensus. *Personality & Social Psychology Review, 1,* 241-271.

Gunther, A.C. (1991). What we think others think: Cause and consequence in the third-person effect. *Communication Research, 18,* 355-372.

Gunther, A.C. (1995). Overrating the X-rating: The third-person perception and support for censorship of pornography. *Journal of Communication, 45,* 27-38.

Gunther, A.C., & Thorson, E. (1992). Perceived persuasive effects of product commercials and public service announcements. *Communication Research, 19,* 574-596.

Hoorens, V., & Ruiter, S. (1996). The optimal impcat phenomenon: Beyond the third person effect. *European Journal of Social Psychology, 26,* 599-610.

Innes, J.M., & Zeitz, H. (1988). The public's view of the impact of the mass media: A test of the 'third person' effect. *European Journal of Social Psychology, 18,* 457-463.

Klar, Y., & Giladi, E.E. (1999). Are most people happier than their peers, or are they just happy? *Personality and Social Psychology Bulletin, 25,* 585-594.

Klar, Y., Medding, A., & Sarel, D. (1996). Nonunique invulnerability: Singular versus distributional probabilities and unrealistic optimism in comparitive risk judgements. *Organizational Behavior and Human Decision Processes*, 67, 229-245.

Klein, W..M. (2001). Post hoc construction of self-performance and other performance in self-serving social comparison. *Personality and Social Psychology Bulletin, 27,* 744-754.

Klein, W.M., & Weinstein, N.D. (1997). Social comparison and unrealistic optimism about personal risk. In B.P. Buunk, & F.X. Gibbons (Eds.). *Health, coping, and well-being: Perspectives from social comparison theory*, (pp. 25-61). Mahwah, NJ: Lawrence Erlbaum Associates Inc.

Krueger, J., & Clement, R.W. (1994). The truly false consensus effect: An ineradicable and egocentric bias in social perception. *Journal of Personality & Social Psychology, 67,* 596-610.

Krueger, J., & Dunning, D. (1999). Unskilled and unaware of it: How difficulties in recognizing one's own incompetence lead to inflated self-assessments. *Journal of Personality and Social Psychology, 77,* 1121-1134.

Krueger, J., & Gilovich, T. (1999). "Naïve cynicism" in everyday theories of responsibility assessment: On biased assumptions of bias. *Journal of Personality and Social Psychology, 76,* 743-753.

Markus, G.B. (1986). Stability and change in political attitudes: Observed, recalled and "explained". *Political Behavior, 8,* 21-44.

Martijn, C., van der Pligt, J., & Spears, R. (1996). Self-serving bias in attitude judgements: The use of person versus issue implicated language. *Social Cognition, 14,* 77-91.

McLeod, D.M., Eveland Jr., W.P., & Nathanson. A.I. (1997). Support for censorship of violent and misogynic rap lyrics. An analysis of the third-person effect. *Communication Research, 24,* 153-174.

Nisbett, R.E., & Wilson, T.D. (1977). Telling more than we can know: Verbal reports on mental processes. *Psychological Review, 84,* 231-259.

Nurius, P.S. (2000). Risk perception for acquaintance sexual aggression: A social-cognitive perspective. *Aggressive and Violent Behavior*, 5, 63-78.

Perloff, R.M. (1989). Ego-involvement and the third person effect of televised news coverage. *Communication Research, 16,* 236-262.

Price, V., & Tewksbury, D. (1996). Measuring the third-person effect of news: The impact of question order, contrast and knowledge. *International Journal of Public Opinion Research, 8,* 120-141.

Rippey, R.M., Geller, L., & King, D.W. (1978). Retrospective pretesting in the cognitive domain. *Evaluation Quarterly, 2*, 481-491.

Ross, L. (1977). The intuitive psychologist and his shortcomings: Distortions in the attribution process. In L. Berkowitz L. (Ed.), *Advances in Experimental Social Psychology (Vol. 10)* (pp. 173-220). New York: Academic Press.

Sabini, J., Cosmas, K., Siepmann, M., & Stein, J. (1999). Underestimates and truly false consensus effects in estimates of embarassment and other emotions. *Basic and Applied Social Psychology, 21,* 223-241.

Salwen, M.B., & Dupagne, M. (1999). The third-person effect. Perceptions of the media's influence and immoral consequences. *Communication Research, 26,* 523-549.

Silka, L., & Albright, L. (1984). Intuitive judgments of rate change: The case of teenage pregnancies. *Basic and Applied Social Psychology, 4,* 337-352.

Sprangers, M., & Hoogstraten, J. (1989). Preresting effects in retrospecrive pretest-posttest designs. *Journal of Applied Psychology, 74*, *265-272.*

Sutton, R.M., & McClure, J.L. (2001). Covariational influences on goal-based attribution: An integrative model. *Journal of Personality and Social Psychology, 80,* 222-236.

Taylor, S.E., & Brown, J.D. (1988). Illusion and well-being: A social psychological perspective on mental health. *Psychological Bulletin, 103,* 193-210.

Tiedge, J.T., Silverblatt, A., Havice, M.J., & Rosenfeld, R. (1991). Discrepancy between perceived first-person and perceived third-person mass media effects. *Journalism Quarterly, 68,* 141-154.

Wixon, D.R., & Laird, J.D. (1976). Awareness and attitude change in the forced-compliance paradigm: The importance of when. *Journal of Personality and Social Psychology, 34,* 376-384.

Footnotes

1 In Study 1, we consistently measured 'self' attitudes prior to 'other' attitudes in the initial phase, and did not vary the ordering of the questions; previous research suggests that question order in TPE studies does not influence responses (see Gunther, 1995; Price & Tewksbury, 1996; Tiedge, Silverblatt, Havice & Rosenfeld, 1991, but see Dupagne *et al.,* 1999). Also, see footnote 2.

2 In Study 2 we counterbalanced the ordering of questionning (self/other) and found no differences.

3 The removal of the economic item did not influence the outcome of any of the hypotheses in either Studies 2 or 3.

Appendix

Gun control attitudes scale (Study 1)

It is not people's right to own guns.

Taking guns away is taking away people's right to protect themselves.

People over-inflate the danger of guns to society.

The government should restrict gun ownership.

Guns are too dangerous to be freely available to the public.

Restricting gun laws is unfair on people who use guns for sport and recreation.

Fossil fuel use attitude scale (Studies 2 and 3)

Current use of fossil fuels is harming the environment.

International governments should attempt to decrease the use of fossil fuels.

Increased carbon dioxide in the atmosphere is a good thing for the environment.

International governments should seek to maximise economic growth, even if this results in global warming.

Table 1. *Results of Study 1. Mean (and standard deviation) baseline, retrospective and current attitudes towards gun control for self and other. Higher values indicate more 'pro-gun' attitudes.*

		Attitude rating		
		Baseline	Retrospective	Current
Person	Self	2.67 *(1.20)*$_{aa}$	3.04 *(1.20)*$_{bb}$	3.11 *(1.14)*$_{bb}$
	Other	3.75 *(0.94)*$_{de}$	3.46 *(0.92)*$_{ef}$	3.79 *(0.96)*$_{dg}$

Means that share a subscript are not significantly different at $p < .05$.

Table 2. *Results of Study 2. Mean (and standard deviation) control, retrospective and current attitudes towards fossil fuel use for self and other. Higher values indicate more 'pro-fossil fuel use' attitudes.*

		Attitude rating		
		Control	Retrospective	Current
Person	Self	1.91 *(0.85)*$_{aa}$	2.24 *(1.18)*$_{ab}$	2.42 *(1.11)*$_{bb}$
	Other	2.41 *(0.74)*$_{bb}$	2.41 *(0.89)*$_{bb}$	2.95 *(1.13)*$_{cc}$

Means that share a subscript are not significantly different at $p < .05$.

Table 3. *Results of Study 3. Mean (and standard deviation) control, retrospective and current attitudes against fossil fuel use for self and other. Higher values indicate more 'anti-fossil fuel use' attitudes.*

		Attitude rating		
		Control	Retrospective	Current
Person	Self	5.63 *(1.15)*$_a$	5.85 *(1.03)*$_a$	6.20 *(0.83)*$_b$
	Other	5.28 *(1.03)*$_c$	5.23 *(1.14)*$_c$	5.93 *(0.96)*$_a$

Means that share a subscript are not significantly different at $p < .05$.