European Journal of Social Psychology
Eur. J. Soc. Psychol. **37**, 216–230 (2007)
Published online 25 August 2006 in Wiley InterScience (www.interscience.wiley.com). **DOI**: 10.1002/ejsp.349

How fatiguing is dispositional suppression? Disentangling the effects of procedural rebound and ego-depletion

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Abstract

Recent work has shown that correcting a dispositional inference may lead social observers to over-emphasize the role of dispositional factors in subsequent judgments. This effect has been explained as a procedural rebound following a phase of dispositional suppression. We conducted two experiments to test an alternative explanation in terms of ego-depletion. In Experiment 1, we compared the effects of ego-depletion and dispositional rebound by relying on the attitude attribution paradigm and the cookie paradigm. In Experiment 2, we turned to a difficult math task in order to induce fatigue. We were able to replicate the dispositional rebound and the ego-depletion effects but none of the experiments supported an ego-depletion explanation of post-suppression dispositional rebound. Copyright © 2006 John Wiley & Sons, Ltd.

When explaining the cause of an event, perceivers should logically take into account both the characteristics of the object and the influence of the environment. As more than three decades of research on attribution demonstrate, lay people, at least in the Western world, typically give too much weight to the characteristics of the object and somehow neglect situational forces in their mental algebra. This is the case when explaining physical motion (Peng & Knowles, 2003) or animal behavior (Morris & Peng, 1994) but it is also true when observers explain human behavior. Psychologists have long noted the dispositional focus of the social observer (Heider, 1958; Ichheiser, 1943; Jones & Harris, 1967). Over the years, an accumulating amount of evidence showed the persistence of the *correspondence bias* (Gilbert & Malone, 1995) or the lay tendency to explain behavior in dispositional terms even when situational factors are more appropriately seen as causing the behavior.

When explaining behavior, perceivers are believed to make a quick initial estimate which is dispositionally biased (Uleman, Newman, & Moskowitz, 1996) and which they later adjust or correct

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Contract/grant sponsor: Belgian National Fund of Scientific Research; contract/grant number: FRFC 8.4507.00.

Received 10 March 2004
Accepted 30 January 2006

by the use of situational information if they are motivated and able to do so (Gilbert, 1998; Quattrone, 1982). Recently Yzerbyt, Corneille, Dumont, and Hahn (2001) proposed that trying to avoid making dispositional inferences may in fact lead perceivers to exacerbate the role of these dispositional factors in subsequent judgments. When induced to correct their social judgment, observers were found to be judging a second target in more dispositional terms (Yzerbyt et al., 2001) or to use more abstract (i.e., disposition-laden) language to describe a series of behaviors (Geeraert, Yzerbyt, Corneille, & Wigboldus, 2004).

The proposed rationale for these findings is simple. Yzerbyt and colleagues suggested that during correction of the dispositional anchor, judges must process the necessary situational information but they must also suppress the initial dispositional judgment. Although the existence of such a suppression phase seems straightforward, it does have non-trivial consequences. Indeed, research on mental control suggests that suppressing an unwanted thought may result in a post-suppressional rebound such that the particular concept becomes more activated than when it would not have been suppressed (Wegner, 1992, 1994; Wegner & Wenzlaff, 1996). For example, participants who are instructed not to think of a white bear report recurrent intrusive thoughts during and after suppression (Wegner, Schneider, Carter, & White, 1987). The paradoxical effects of suppression prove to be very robust and have been replicated with various concepts (see Wenzlaff & Wegner, 2000). For example, suppressing stereotypical thoughts may alter perceivers' behavior toward another member of the target social category (Macrae, Bodenhausen, Milne, & Jetten, 1994, see also Bodenhausen & Macrae, 1998; Monteith, Sherman, & Devine, 1998).

In line with mental control theory, Yzerbyt and colleagues argued that a dispositional rebound was to be found after people correct their dispositional inference. This was true both after explicit instruction to suppress dispositional thoughts and also in absence of such suppression instruction after exposure to a constrained target. The ironic effect thus provides an elegant account for the dispositional rebound phenomena; with one important distinction that this involves the suppression and rebound of a mental procedure (making dispositional judgments) instead of the well-documented thought suppression (Wegner, 1992).

Intriguing as these findings may be, it should be recognized that other theories could also explain them. The most serious competing explanation rests on the idea of ego-depletion as it has been put forth by Baumeister and colleagues (Baumeister, Muraven, & Tice, 2000; Muraven & Baumeister, 2000) According to Baumeister, people can become 'ego-depleted' or mentally fatigued after an act of volitional control. As a result, exerting control in a later phase would be more complicated and less likely. To the extent that dispositional correction can be compared to the tasks typically used in ego-depletion research, ego-depletion would seem to provide a viable explanation for the rebound effect in general and dispositional rebound in particular.

In one of their ego-depletion studies, Baumeister, Bratslavsky, Muraven, and Tice (1998) had food-deprived participants presented with *radishes* and freshly baked *cookies*. Whereas half of the participants was instructed to taste a couple of cookies but to refrain from eating radishes, the other half was instructed to eat a couple of radishes but to refrain from eating chocolate cookies. Baumeister et al. (1998) argued that participants in the radish condition would have to resist eating the desirable cookies to such a degree that this would mentally fatigue them in a later task. All participants were subsequently asked to find the solution of a difficult (and unsolvable) puzzle. As predicted, participants who had been eating radishes (and thus controlled themselves) gave up much faster than participants in the cookies or control condition (Baumeister et al., 1998). This pattern has been replicated in a number of studies. For instance, participants who controlled their emotional reaction to a sad movie also showed poorer performance on a subsequent physical control task (Muraven, Tice, & Baumeister, 1998, Study 1).

More related to the present research, ego-depletion has also been observed after an act of thought suppression. Muraven et al. (1998, Study 2) had participants express or suppress a specific thought. Next, participants were asked to work out a series of (unsolvable) anagrams. As predicted, suppression participants gave up faster on the task than participants in the expression or control condition. These results led the authors to propose that the ironic effects of suppression might actually be due to ego-depletion.

Baumeister offered two explanations for the ego-depletion findings. A first explanation relies on the 'ego-strength' metaphor (Baumeister et al., 1998, 2000). According to this view, the self depends on a limited amount of resources and controlling the self may consume such an amount of energy that one becomes mentally exhausted or ego-depleted. A second metaphor sees self-control as a muscle (Muraven & Baumeister, 2000). This view suggests that people must recover after an exercise of self-control. Recently, these accounts have been questioned by Martijn and colleagues (Alberts, Martijn, Greb, Merckelbach, & de Vries, in press; Martijn, Tenbült, Merckelbach, Dreezens, & de Vries, 2002) who propose that effects of ego-depletion are strongly affected by people's naïve theories or expectancies. Martijn and colleagues (2002) showed that participants led to believe that the depletion manipulation would enhance their performance were indeed able to override their fatigue. However, this expectancy had no influence on participants who had not been depleted. This suggests that the manifestation of ego-depletion is more an issue of a motivation than the consequence of a resource-dependent process (see also Muraven & Slessareva, 2003).

Whichever of these broad frameworks is believed to provide an adequate account of the fatigue effects reported in the literature, ego-depletion stands as a possible alternative to the current suppression and rebound interpretation of the findings presented by Yzerbyt et al. (2001) and Geeraert et al. (2004). Observers, who are induced, either implicitly or explicitly, to correct their dispositional judgment might be affected by ego-depletion for at least three reasons. First, when confronted with a constrained target, social perceivers will have to take into account *both* situational and dispositional factors. Their causal analysis is thus likely to be more balanced but also more cognitively demanding (Gilbert, Pelham, & Krull, 1998). In other words, a more challenging scrutiny of the causal factors may cause ego-depletion.

Second, we have argued that situational correction involves spontaneous suppression of dispositional information. In line with at least one set of findings that thought suppression in and of itself is causing ego-depletion (Muraven et al., 1998), similar effects might be expected after dispositional suppression.

Third, a large body of research suggests that Western perceivers rely on dispositional information both continuously and automatically (e.g., Choi, Nisbett, & Norenzayan, 1999; Uleman et al., 1996). Social observers are thus believed to be 'dispositional-theorists' in the sense that they may predominantly rely on dispositional factors as providing a ready-made account of behavior (Ross & Nisbett, 1991). When confronted with a forced speaker, people must act against this natural tendency. Perceivers may thus be engaging in an act of volitional control that likely causes ego-depletion.

To the extent that engaging in situational correction and dispositional suppression is mentally fatiguing, we may indeed expect poorer performance on a subsequent task. That is, when resource-depleted observers must conduct a second judgment, they could fall back on the prevailing bias or heuristic, namely dispositionalism (Ross & Nisbett, 1991; see also Webster, 1993). Relying upon dispositionalism would help recover from ego-depletion as this is the effortless option (Gilbert et al., 1988; Uleman et al., 1996). According to this rationale, a likely course of events is a resurgence of dispositional attributions and a relative neglect of situational forces in judgments that follow initial correction.

An important difference between the account of ego-depletion and procedural rebound lies in the broadness of the predicted impact. Ego-depletion would predict a generic effect, so to restore

ego-strength. It is non-specific as all tasks tapping on self-control are likely to be affected. Procedural rebound on the other hand, would predict a more specific impact, such that only the procedure that was suppressed will rebound. We would argue that ego-depletion has little to do with the findings observed by Yzerbyt et al. (2001) and Geeraert et al. (2004). In fact, we very much doubt that the level of fatigue involved in dispositional suppression is such that performance on subsequent tasks would suffer in any noticeable way.

The present series of studies aim at disentangling the ego-depletion and suppression explanation of the post-suppression dispositional rebound. In a series of pilot studies, we tried to manipulate mental fatigue by having participants conduct an attribution task while under cognitive load. Although the task we used to tax the intellectual resources of our participants has been widely relied upon in recent social cognition work (for a review, see Gilbert, 1998), we were unsure to what extent it corresponds to the demands usually imposed on participants in ego-depletion studies. As a matter of fact, we found no evidence of ego-depletion. Not surprisingly, some authors have argued that cognitive load found in the stereotyping and attribution studies is not sufficient to evoke a mental fatigue *à la* ego-depletion (see Muraven & Baumeister, 2000).

For this reason, we relied upon a more appropriate manipulation of ego-depletion in the present studies. The main idea underlying our first study was to confront a fatigue manipulation to our suppression manipulation and examine the effects of these two manipulations on two dependent variables, one variable used to measure dispositional rebound and the other typically used in the ego-depletion literature. The observation of a divergent message emanating from these two dependent variables as a function of the manipulation would aid in disentangling the effects of suppression and fatigue and help establish whether or not ego-depletion offers a viable account of post-suppression dispositional rebound.

In Study 1a, the participants had to judge a *free* or *forced* target in a typical attitude attribution paradigm (cf. Jones & Harris, 1967). It is believed that judging a forced speaker elicits situational correction, so this is the condition where we expect participants to suppress their dispositional judgment and to show dispositional rebound (Geeraert et al., 2004; Yzerbyt et al., 2001). In Study 1b, participants were presented with *radishes* and *cookies*, and were asked to taste one of these foods but not the other (cf. Baumeister et al., 1998).

These judgment and taste tasks were followed by a social judgment measure, which was a modified version of the linguistic category task used in the paper of Geeraert et al. (2004), and a fatigue measure, namely the persistence on an unsolvable puzzle. We expected to replicate the dispositional rebound as well as the classical ego-depletion effect after the appropriate manipulation but not after the other. More specifically, we hypothesize that judging a forced speaker will lead to dispositional rebound, but not ego-depletion. In contrast, we expect participants restrained from eating chocolate cookies will show the classic ego-depletion effect, but no dispositional rebound.

EXPERIMENT 1a & 1b

Method

Participants

Ninety-two undergraduates of the Catholic University of Louvain at Louvain-la-Neuve participated in the study in exchange for experimental credits. Participants were randomly assigned to one of four conditions (cookies vs. radish vs. forced speaker vs. free speaker).

Materials

Labs We had two identical labs. Whereas one lab was equipped with the necessary audiovisual materials for the attitude attribution paradigm, the other lab was equipped with a small electrical oven. Preceding every 'taste perception' session, the experimenter baked a sufficient number of cookies, such that the room was filled with the smell of freshly baked chocolate cookies. Importantly, this smell was not noticeable in the other lab.

Linguistic Category Measure This measure builds upon the Linguistic Category Model (Semin & Fiedler, 1991) that distinguishes four levels of language abstraction: *Descriptive Action Verbs* (DAV) are context dependent descriptions of an action with reference to a specific object and situation (e.g., 'Valerie holds the child in her arms'). *Interpretative Action Verbs* (IAV) are interpretations of an action, but still referring to a specific object and situation (e.g., 'Valerie comforts the child'). *State Verbs* (SV) refer to a mental or emotional state, with reference to a specific object but not to a specific situation or context (e.g., 'Valerie cares for the child'). *Adjectives* (ADJ) are highly abstract person dispositions without reference to specific objects, situations, or context (e.g., 'Valerie is caring'). Typically participants are shown a series of pictorially presented behaviors (e.g., a woman holding a child) and have to select the best of four descriptors for each such picture. The model can be used to discriminate between abstract and concrete language, but it is also the instrument *par excellence* to implicitly measure dispositional inferences. In fact, the model offers a nice dimension ranging from situational to dispositional information; the more abstract a category the more it contains information about the protagonist (see Maass, Salvi, Arcuri, & Semin, 1989). These linguistic categories proved a suitable tool to show the emergence of dispositional rebound (Geeraert et al., 2004).

We used the same materials as in the paper of Geeraert et al. (2004) with one major exception. Instead of having participants select the best of four descriptors (Maass et al., 1989), we relied on the method proposed by von Hippel and colleagues (Karpinski & von Hippel, 1996; von Hippel, Sekaquaptewa, & Vargas, 1997). Participants were shown 12 single-frame pictures displaying various behaviors. Each picture was associated with four different response alternatives, corresponding to the four levels of linguistic abstraction. For each such linguistic descriptor participants had to rate how well the sentence described the behavior, on a scale from 1 to 10. We used different booklets to control for order, but because no differences emerged between booklets, order will not be discussed further.

Fatigue Measure We relied on a similar problem-solving task as the one used by Baumeister et al. (1998). Participants had to trace two geometric figures with a pencil, without retracing any line and without lifting their pencil from the paper. First, participants were explained the task and presented with an example. Participants then received two new complex geometric figures. Unknown to participants, these figures were unsolvable. Participants were provided with two large piles of paper, one for each of the two figures, so participants could try again and again. Participants learned that they could work as long as necessary, restarting as many times as needed, and switching between the two figures whenever desired. The experimenter also informed participants that they could give up whenever they wanted. She further explained that she would leave the room and that participants were to ring a bell when they had found the solution or when they wanted to give up. We measured the number of attempts, i.e., the number of sheets of paper used, and the total time participants worked on the task.

Procedure

A couple of days before the study, first-year undergraduates were allowed to volunteer for the experiment. Students had to come in person to the office of a research assistant who explained that the experiment really consisted of two distinct studies. One experiment was entitled 'perception of taste' and the other 'social judgment.' Students were invited to take part in one of two experiments. They were told that they could not just choose an experiment but that, for methodological reasons, this had to be determined by chance. Students then drew a paper from a box that revealed the number and title of the experiment. The 'perception of taste' papers further indicated that participants should refrain from eating for at least 3 hours prior to the experiment.

On the day of the experiment, participants came individually at one of the two labs where they were greeted by a female experimenter. Participants in Experiment 1a, the 'social judgment experiment,' were asked to sit down in front of a television screen and were explained that they would take part in a study on perception and impression formation. Participants learned that they would watch a video excerpt recorded during another experiment, and that they had to answer a series of questions afterwards. They were told that the people who took part in this previous experiment had been asked to prepare a short speech and to read it aloud in front of a video camera. The topic concerned the possible adoption of an admission criterion for students from the 2nd year to enter the 3rd year of university. Only students who obtained an average of 70% in their course grades would be admitted to the 3rd year. In fact, we used the same video as Geeraert et al. (2004). This 3-minute video showed a male student sitting at a table while reading the speech. Depending on the condition, participants were led to believe that the speaker had either been free or forced to choose a particular stance regarding this issue. Right before the videotape started, the experimenter checked that participants correctly understood the information they had received.

After seeing the videotape, participants were asked to answer a few filler questions about the speakers' personality. Next, they estimated the true attitude of the speaker towards the adoption of the policy on a scale ranging from 1 (=totally against) to 9 (=totally in favor), and their level of confidence regarding this judgment, from 1 (=not at all confident) to 9 (=totally confident). Participants were also asked to recall the three arguments of the speech and to answer some filler questions regarding the speaker and the context of the video. Finally, we checked whether participants thought that the speaker was free to choose a particular stance on the topic on a scale ranging from 1 (=not at all free) to 9 (=totally free).

Participants in Experiment 1b, the 'perception of taste experiment,' were invited to sit at a table with a bowl of cookies and a bowl of radishes placed in front of them. Participants learned that they would participate in a study on taste and that they thus would have to taste certain foods. The experimenter explained that two distinct but familiar tastes had been selected, namely chocolate and radish. She further explained that the participants had been randomly assigned to eat only from one of the two products and that they could not eat the other one. For example, the instructions in the radish condition were as follows:

> As you can see from looking at the table, there are two conditions in this experiment. In one condition, participants have to taste radishes, in the other they have to taste chocolate cookies. You are in the 'radishes' group. So, in a minute, I will ask you to taste a couple of radishes but it is important that you don't eat any of the cookies.

The experimenter then checked if the participants correctly understood the instructions and left the room so the participants could taste at ease. After 4 minutes, the experimenter returned. She explained that the participants were to answer a questionnaire about taste but that it was crucial to wait at least 20 minutes in order to wipe out sensory memory. In the meantime, participants were invited to fill in a

couple of questionnaires allegedly for some colleagues. At the start and end of each experiment, the total number of cookies and radishes were counted so as to make sure that participants had only eaten the assigned food.

Participants from both Experiment 1a and 1b then received the social judgment task. They had to rate four different descriptors for a series of 12 single-frame pictures displaying various behaviors. This task was followed by the fatigue measure. Participants had to work on the unsolvable puzzle for as long as possible.

Participants in the 'perception of taste experiment' were then given their questionnaire about taste, which consisted mainly of filler items. Also included were a number of manipulation checks. Participants had to indicate how much they had liked eating the assigned food, to what extent they had forced themselves to eat the assigned food, and to what extent they had been tempted by the forbidden food on scales ranging from 1 (=not at all) to 9 (=totally). Further, participants from both Experiment 1a and 1b reported how hungry they were during the experiment from 1 (=not at all hungry) to 9 (=totally hungry) and indicated the last time they had eaten. Finally, participants were thanked, and dismissed. Debriefing took place in a group session one week after the experiment was completely finished.

Results

Five participants who gave invalid responses in the linguistic category measure were excluded prior to analyses. We also looked at participants' reports of their last meal and their subjective feeling of hunger during the experiment. None of the participants in the 'taste perception study' reported violating the instruction not to eat before the start of the experiment. In line with instructions, they had refrained from eating much longer ($M = 6$ hours 28 minutes, $SD = 4$ hours 31 minutes), than participants in the 'social judgment study' ($M = 1$ hours 28 minutes, $SD = 1$ hours 2 minutes; $F(1, 85) = 45.66$, $p < 0.0001$). Participants in the cookie paradigm also reported being more hungry ($M = 5.95$, $SD = 2.70$), than participants in the attitude attribution paradigm ($M = 1.85$, $SD = 1.68$, $F(1, 85) = 71.30$, $p < 0.0001$). Two additional participants reported not having eaten for more than 16 hours and were excluded from the data set.

In order to test for the influence of situational correction and ego-depletion on the linguistic category measure, we looked at participants' descriptor weights for the two extreme linguistic categories. As a matter of fact, ADJ and DAV can be seen as the pure versions of the trait versus non-trait descriptors and has been shown to be sensitive to dispositional rebound in earlier studies[1] (see also Geeraert, 2004; Geeraert et al., 2004).

Experiment 1a: Attitude Attribution

Manipulation Check The perceived freedom of choice was examined by means of a one-way ANOVA with freedom of choice (forced vs. free) as between-subjects variable. Indicating the success of our manipulation, this analysis revealed the presence of a significant effect of freedom of choice, $F(1, 39) = 128.64$, $p < 0.0001$. Participants confronted with a free speaker perceived the speaker as more free ($M = 8.47$, $SD = 1.02$) than participants confronted with a forced speaker ($M = 2.23$, $SD = 2.20$).

[1] *Post hoc* analyses failed to show any effect of experimental condition on IAV & SV, for the current data. This is in line with earlier studies (Geeraert et al., 2004).

Table 1. Main results for Experiment 1a (*attitude attribution*). Means and standard deviations are provided for the proportion of linguistic categories (ranging from the concrete *Descriptive Action Verbs*, over *Interpretative Action Verbs*, and *State Verbs*, to the abstract *Adjectives*), and the unsolvable puzzles (the number of attempts, time spent, & multiplicative function)

	N	DAV	IAV	SV	ADJ	Number attempts	Time spent	Attempts × time
Forced	22	29.73	26.19	22.07	22.01	17.41	19.53	360.10
		(3.39)	(1.61)	(2.13)	(2.01)	(9.59)	(6.99)	(294.07)
Free	19	31.65	26.27	21.49	20.59	20.00	21.06	461.47
		(2.42)	(2.68)	(1.78)	(2.90)	(12.48)	(7.98)	(401.45)

Perceived Attitude of The Speaker We investigated the perceived attitude of the speaker with a one-way ANOVA, with freedom of choice as between subjects variable. We found a significant effect of freedom of choice, $F(1, 39) = 32.09$, $p < 0.0001$. Participants confronted with a free speaker rated this speakers true attitude more in favor of the policy ($M = 8.15$, $SD = 1.25$), than participants confronted with a forced speaker ($M = 5.13$, $SD = 2.01$). We also found a difference in the confidence ratings, $F(1, 39) = 14.40$, $p < 0.001$, such that free speaker participants were more confident about their judgment ($M = 7.58$, $SD = 1.35$) than forced speaker participants ($M = 5.68$, $SD = 1.78$).

Linguistic Category Measure We compared weights by means of a 2 (freedom of speaker) by 2 (type of descriptor) mixed design ANOVA, with the last factor varying within participants. This analysis revealed a highly significant type of descriptor effect, $F(1, 39) = 138.20$, $p < 0.0001$, indicating that DAV was weighted more strongly than ADJ. The main effect of freedom of speaker was not significant, $F(1, 39) = 0.64$, *ns*. More importantly, the freedom of speaker by type of descriptor interaction turned out significant, $F(1, 39) = 4.37$, $p < 0.05$. Indeed, free speaker participants put more emphasis on DAV than forced speaker participants ($F(1, 39) = 4.22$, $p < 0.05$). In sharp contrast, forced speaker participants weighted ADJ slightly stronger than free speaker participants ($F(1, 39) = 3.42$, $p < 0.08$). Means are shown in Table 1.

Fatigue Measure Two measures can be seen as indication for participants' persistence on the task, the number of attempts and the total time participants worked on the puzzle. We analyzed these data by means of a 2 (freedom of speaker) by 2 (fatigue measures) mixed-design ANOVA, with fatigue measures varying within participants. None of the main effects was significant, and neither was the interaction (all F's < 1). Means are shown in Table 1.

If fatigue and rebound are related then there should be a positive relationship between weighting the ADJ and the fatigue measure. To test this hypothesis, we calculated the correlation between ADJ and the product of number of attempts by time spent. These variables were not correlated, $r = -0.12$, *ns*.

Experiment 1b: Cookies and Radishes

Manipulation Check All participants tasted the designated food (cookies $M = 2.63$, $SD = 0.63$; radishes $M = 3.58$, $SD = 1.56$) and respected the requirement not to taste the other food. We analyzed the manipulation check by a series of one-way ANOVAs with type of food (cookies vs. radishes) as between-subjects variable. Participants who ate cookies enjoyed their food more ($M = 7.60$, $SD = 2.06$) than participants who ate radishes ($M = 4.92$, $SD = 2.75$, $F(1, 42) = 12.96$, $p < 0.001$). Moreover,

Table 2. Main results for Experiment 1b (*cookies and radishes*). Means and standard deviations are provided for the proportion of linguistic categories, and the unsolvable puzzles

	N	DAV	IAV	SV	ADJ	Number attempts	Time spent	Attempts × time
Radish	24	30.54	26.11	22.12	21.23	15.13	17.30	293.37
		(3.05)	(1.73)	(1.63)	(2.27)	(10.05)	(7.37)	(299.95)
Cookies	20	31.85	25.89	21.56	20.69	21.40	21.78	529.41
		(3.87)	(2.19)	(2.13)	(2.71)	(15.23)	(8.47)	(506.06)

participants in the radish condition were tempted more by the forbidden food ($M = 6.08$, $SD = 2.19$) than their colleagues in the cookies condition ($M = 2.35$, $SD = 2.13$, $F(1, 42) = 35.52$, $p < 0.0001$). Further, participants eating radishes had to force themselves slightly more ($M = 3.63$, $SD = 3.27$) than participants eating cookies ($M = 2.10$, $SD = 1.97$, $F(1, 42) = 3.34$, $p < 0.08$). As a set, the responses to these questions confirm the success of our manipulation.

Linguistic Category Measure We looked at the linguistic data by means of a 2 (type of food) by 2 (type of descriptor) mixed-design ANOVA. This analysis revealed a significant type of descriptor main effect, $F(1, 42) = 141.38$, $p < 0.0001$, such that DAV had stronger weightings than ADJ. The type of food main effect was not significant, $F(1, 42) = 1.70$, *ns*. Interestingly, the interaction of descriptor by type of food did not reach significance, $F(1, 42) = 1.17$, *ns*. Means are shown in Table 2.

Fatigue Measure We analyzed the fatigue data for the cookie paradigm by means of a 2 (type of food) by 2 (fatigue measure) mixed-design ANOVA. This analysis revealed a marginal effect of type of food, $F(1, 42) = 3.95$, $p < 0.06$. Participants in the cookies condition tended to work longer on the puzzles than participants in the radish condition ($F(1, 42) = 3.52$, $p < 0.07$). There was a similar pattern in number of attempts, but this difference was not statistically reliable, $F(1, 42) = 2.68$, *ns*. Means are shown in Table 2.

We again looked at the correlation between ADJ and the combined fatigue measure. As before, these variables were clearly not correlated, $r = 0.04$, *ns*.

Discussion

Experiments 1a and 1b consisted of two different manipulations, which were evaluated with respect to their impact on two different dependent measures. As far as the attitude attribution paradigm is concerned, our manipulation of freedom of speaker was clearly successful. We also found that participants confronted with a forced speaker corrected their dispositional judgment and were less confident. In comparison with the free speaker participants, forced speaker participants subsequently gave more weight to ADJ and less weight to DAV in rating a series of descriptions for different pictorially presented behaviors. This is a clear replication of the dispositional rebound as found by Geeraert and colleagues (Geeraert, 2004; Geeraert et al., 2004). In contrast, the difference in freedom of speaker failed to affect participants' fatigue as measured by the persistence in an unsolvable puzzle task.

The manipulation in the cookie paradigm was also successful, as participants were less prone to eat radishes and felt tempted by the chocolate cookies. Participants in the cookie condition subsequently

worked longer on an unsolvable puzzle task than participants who tasted radishes. We were thus able to replicate the findings of Baumeister et al. (1998) although the effects were rather modest. We did not find any evidence of the influence of fatigue on the weighting of the linguistic descriptors.

In both experiments, we also failed to find any direct relationship between the rebound and fatigue measure. These findings suggest that these phenomena are indeed unrelated.

Thus far, we found no direct influence of fatigue on subsequent social judgment nor did we uncover evidence for the influence of social judgment correction on a fatigue measure. To be sure, some caution is in order because the paradigms that we used are rather different. In particular, one might object that the fatigue paradigm that we relied upon in Experiment 1 does not build so much on cognitive fatigue. This may limit its comparability with the kind of fatigue that one could expect from dispositional correction. To test our hypothesis even more convincingly, we found it useful to induce ego-depletion in a more cognitive way. Therefore, we used a paradigm developed by Alberts et al. (in press; see also Schmeichel, Vohs, & Baumeister, 2003). These authors had participants undergo a difficult math test while listening to random numbers at the same time. Compared to participants doing an easy math test without interference, participants in the difficult test had a poorer performance on a subsequent physical test. In Experiment 2b, we induced fatigue by relying on this math test. In Experiment 2a we again relied upon the attitude attribution paradigm. As in Study 1, we evaluated the effects of fatigue and situational correction on a subsequent fatigue measure (physical task) and social judgment (linguistic category measure).

EXPERIMENT 2a & 2b

Method

Participants

Sixty-nine undergraduates of the Catholic University of Louvain at Louvain-la-Neuve participated in the study in exchange for experimental credits. Participants were randomly assigned to one of the four conditions (difficult math task vs. easy math task vs. forced speaker vs. free speaker).

Materials & Procedure

Participants were tested individually. They were greeted by a male experimenter and invited to take place in an office chair. The experimenter told them that they would have to start with a test measuring physical performance. He explained that participants had to hold a dumb-bell for as long as possible. In fact, this first task served as a baseline. Participants remained seated but had to hold their dominant arm in a stretched position. The dumb-bell, weighting 1.5 kg, was first placed on a box in front of the participants. The distance between arm and table was approximately 15 cm and the height of the chair was adjusted whenever necessary. At participants' own pace, the box was removed and the time started running. The time stopped the moment the dumb-bell touched the surface of the table.

Participants in the attitude attribution paradigm (Experiment 2a) were then shown a video with the same cover story as in Experiment 1a. Depending on condition, participants were led to believe that the speaker had been free or forced to choose a particular stance regarding the advocated policy. After this, participants filled in the questionnaire about the video.

The participants in the math paradigm (Experiment 2b) were seated in front of a computer. The written instructions explained that participants had to solve a series of sums during 8 minutes. These math problems had to be done mentally. That is, participants were not allowed to speak or use their hands for counting. The instructions further informed participants that they had to work as fast but also as accurately as possible. Whereas the problems for the easy test were fairly simple, the ones for the difficult test were increasingly complex as the test unfolded. Participants in the difficult math test were also asked to wear a headset that would diffuse random numbers. Just before the start of the test, the experimenter checked if participants correctly understood all instructions.

When participants from both Experiment 2a and 2b finished their task, they were given a 4-item mood questionnaire. This was followed immediately by the second measure of the dumb-bell. After this participants were given the same linguistic category measure as in Experiment 1. Upon completing the linguistic category task, participants were given a last questionnaire with 4 mood-items and a number of manipulation checks. Participants also had to indicate an estimate for the two dumb-bell time measures. Finally, participants were thanked, debriefed, and dismissed.

Results

Three participants who gave invalid responses to the linguistic category measure and one participant who talked during the math test were excluded prior to analyses.

Experiment 2a: Attitude Attribution

Manipulation Check We analyzed the perceived freedom of speaker with a one-way ANOVA with freedom of speaker (forced vs. free) as between-subjects variable. This analysis revealed a main effect of freedom of speaker, $F(1, 31) = 66.37$, $p < 0.0001$. Indicating the success of our manipulation, the forced speaker participants rated the speaker as being more forced ($M = 2.00$, $SD = 1.67$), than free speaker participants ($M = 6.88$, $SD = 1.76$).

Perceived Attitude of The Speaker We looked at the perceived attitude of the speaker by relying on a one-way ANOVA with freedom of speaker as between-subjects variable. Participants confronted with a free speaker rated the speaker's true opinion as more in favor of the policy ($M = 7.53$, $SD = 1.07$), than forced speaker participants ($M = 5.18$, $SD = 1.60$, $F(1, 31) = 24.73$, $p < 0.0001$). This time, we found no difference in the confidence ratings regarding this judgment, $F(1, 31) = 1.26$, *ns*.

Linguistic Category Measure First, we looked at the weights participants gave to the two extreme linguistic categories as a function of freedom of speaker. Specifically, we conducted a 2 (freedom of speaker) by 2 (type of descriptor) mixed-design ANOVA. This analysis revealed a significant type of descriptor main effect, $F(1, 31) = 153.46$, $p < 0.0001$. We also found a main effect of freedom of speaker, $F(1, 31) = 5.90$, $p < 0.03$. The type of descriptor by freedom of speaker interaction was not significant, $F(1, 31) = 1.01$, *ns*. Although participants confronted with a forced speaker selected more ADJ than free speaker participants ($F(1, 31) = 5.97$, $p < 0.03$), no difference emerged for DAV, $F(1, 31) = 0.02$, *ns*. Means are shown in Table 3.

Table 3. Main results for Experiment 2a (*attitude attribution*). Means and standard deviations are provided for the proportion of linguistic categories, and the difference in time between the first and second occasion of holding the dumb bell (higher scores denote more fatigue)

	N	DAV	IAV	SV	ADJ	Time
Forced	16	30.33 (2.71)	26.35 (1.38)	21.17 (2.83)	22.15 (1.4)	14.27 (18.43)
Free	17	30.19 (2.63)	26.76 (1.5)	22.49 (2.02)	20.57 (2.21)	24.47 (30.73)

Fatigue Measure We calculated a fatigue score by subtracting our second dumb-bell measure from the baseline, such that higher scores are evidence of higher fatigue. We conducted a one-way ANOVA with freedom of speaker as between subjects' variable. The effect of freedom of speaker was not significant, $F(1, 31) = 1.25$, *ns*. Means are shown in Table 3.

We again looked at the correlation between the weighting of ADJ and the fatigue measure. This analysis revealed a marginal negative correlation, $r = -0.33$, $p < 0.07$, such that stronger ADJ weights were slightly inversely related with fatigue. This pattern is surprising as an ego-depletion account of rebound would have predicted the opposite pattern.

Experiment 2b: Math Test

Linguistic Category Measure We analyzed the weights given to the descriptors as a function of math test by means of 2 (math test) by 2 (type of descriptor) mixed-design ANOVA with the latter variable as a within-subject factor. This analysis revealed a significant type of descriptor main effect, $F(1, 30) = 60.26$, $p < 0.0001$. The main effect of math test was not significant, $F(1, 30) = 1.93$, *ns*. We did not find a type of descriptor by math test interaction, $F(1, 30) = 1.02$, nor was there any difference between the weights given in the two conditions to DAV and ADJ separately. Means are shown in Table 4.

Fatigue Measure A one-way ANOVA with math test as between subjects' variable, revealed a main effect of math test, $F(1, 30) = 10.00$, $p < 0.01$. Participants in the difficult math test were worse at holding the dumb-bell the second time than participants that had solved easy sums. Means are shown in Table 4.

Finally we looked at the correlation between ADJ weights and the fatigue measure. The analysis showed no correlation, $r = 0.20$, *ns*.

Table 4. Main results for Experiment 2b (*math test*). Means and standard deviations are provided for the proportion of linguistic categories, and the difference in time between the first and second occasion of holding the dumb bell (higher scores denote more fatigue)

	N	DAV	IAV	SV	ADJ	Time
Depleted	16	28.72 (2.96)	26.65 (0.93)	22.64 (1.4)	21.98 (2.74)	27.94 (22.35)
Control	16	30.09 (3.23)	26.25 (1.39)	22.32 (2.12)	21.34 (2.71)	9.25 (7.69)

Discussion

In Experiment 2, we relied on the attitude attribution paradigm and a math test to manipulate situational correction or ego-depletion, respectively. Compared to participants confronted with a free speaker, participants confronted with a forced speaker rated the speaker as being more constrained. Forced speaker participants corrected their judgment but evidence for subsequent dispositional rebound was somewhat less strong. Although we found no difference for the weights given to the DAV descriptors as a function of the speaker participants had seen, forced speaker participants weighted ADJ descriptors stronger than free speaker participants. This pattern, albeit somewhat weaker than the one obtained in Experiment 1a, is a sign of dispositional rebound. Further, freedom of speaker did not have any influence on the subsequent physical test. Interestingly, we found a negative correlation between fatigue and dispositional rebound, a result that clearly contradicts an ego-depletion account of the rebound effect.

As for the math test, we found supportive evidence that a difficult math test can induce ego-depletion. Indeed, our data revealed that such a test has consequences for a subsequent physical test (cf. Alberts et al., in press). Participants who solved complex sums while listening to interfering numbers held the dumb-bell for a shorter time than participants in the easy math condition. As we observed in Experiment 1b, ego-depletion once again did not at all affect the linguistic category measure. Also, the measures of fatigue and dispositional rebound were uncorrelated.

GENERAL DISCUSSION

Recent research indicates that correcting a social inference may lead observers to magnify the role of dispositional factors in later judgments, which has been explained as post-suppressional dispositional rebound (Geeraert et al., 2004; Yzerbyt et al., 2001). The present research aimed at testing the viability of an alternative account in terms of ego-depletion (Baumeister et al., 2000).

In Experiments 1 and 2, we assessed dispositional suppression and ego-depletion after participants had either been confronted with a social judgment task or an ego-depletion task. In both studies, we were able to replicate the post-suppressional dispositional rebound (Geeraert et al., 2004; Yzerbyt et al., 2001). That is, participants confronted with a forced speaker corrected their dispositional judgment, and subsequently gave stronger weights for abstract language than participants confronted with a free speaker (Experiment 1a & 2a). We also successfully replicated the 'classic' ego-depletion effect (Alberts et al., in press; Baumeister et al., 1998). Compared to depleted participants, non-depleted participants persisted longer on an unsolvable puzzle task (Experiment 1b), or held a dumb bell much longer (Experiment 2b). As expected, our data revealed no evidence for the impact of ego-depletion on the emergence of dispositional rebound nor did we find any support for the idea that confrontation with a constrained speaker would be mentally fatiguing. Importantly, the measures of fatigue and dispositional rebound were consistently uncorrelated or even turned out to be negatively correlated.

The prediction that social observers confronted with a constrained target will suppress making dispositional judgments (Yzerbyt et al., 2001), resulting in a post-suppressional rebound (Wegner, 1992, 1994) has once again been supported in the present sets of data. Previous work demonstrated that conversational norms or contrast effects can not account for the dispositional rebound findings (Geeraert et al., 2004). In the present experiments, we addressed yet another alternative account, this time in terms of ego-depletion. Clearly, we found no support whatsoever for an ego-depletion explanation of the rebound data. Although it may be difficult to totally exclude some impact of fatigue, the current findings clearly support the suppression explanation.

As it happens, the findings of dispositional rebound may turn out to be very interesting. After all, suppression of a process, such as making dispositional judgments, strongly differs from the commonly studied conceptual rebound on a great many counts. Clearly, further research is needed to investigate whether procedural rebound can really be accounted for by the ironic monitoring theory (Wegner, 1994), or a motivational explanation of suppression (Liberman & Förster, 2000). Future research may also be directed toward testing the generality of this procedural rebound in other settings. This may allow us to gain a better understanding on the exact nature of a wide series of cognitive processes

The literature reveals that Westerners indeed show a strong and persistent tendency to use dispositional information in social judgment (e.g., Choi, Nisbett, & Norenzayan, 1999; Ross & Nisbett, 1991). Avoiding this tendency is not only difficult (Gilbert & Malone, 1995) but our line of research indicates that making substantial efforts to avoid the dispositional mode of thinking seems to be counterproductive. When you think the dispositional mode is gone, it peeks around the corner again, and eating a cookie won't help you!

ACKNOWLEDGEMENTS

This research was supported by FRFC research grant 8.4507.00 from the Belgian Fonds Nationale de la Recherche Scientifique to Vincent Y. Yzerbyt. We wish to thank Carolien Martijn, Hugo Alberts, Steven Stern, Marco Perugini, Thomas Mussweiler, and three anonymous reviewers for their helpful comments on this project. We also express our gratitude to Jolien De Brauwer and Estelle Breyne for their help in collecting the data, and to Julie Camerman for lending her voice in the math task.

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