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Matthew T. Crawford, John J. Skowronski, Chris Stiff and Cory R. Scherer

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Interfering With Inferential, But Not Associative, Processes Underlying Spontaneous Trait Inference

Matthew T. Crawford
University of Bristol

John J. Skowronski
Northern Illinois University

Chris Stiff
University of Bristol

Cory R. Scherer
Northern Illinois University

Three studies explore mental processes underlying spontaneous trait inferences *about self-informants and the* spontaneous trait transference *characterizing third-party informants. Process differences are suggested in that instructions prompting a nontrait inference (truth or lie?) reduce self-informant trait-savings effects and lower self-informant trait judgments. For third-party informants, such instructions have no effect on these outcome variables. Results of a third study are inconsistent with cognitive load as an explanation for these effects. Taken together, these results indicate that inferences, and not merely associations, spontaneously form when processing information about self-informants. The results also show that the inferences and judgments that occur in spontaneous trait transference are not caused by the misidentification of third-party informants as self-informants.*

Keywords: *impression formation; spontaneous trait inferences; associative processes; traits; social judgments*

After hearing Jeremy describe how he power-lifted 500 pounds, a listener might infer that Jeremy is strong. Although such attributions might be prompted by a query about Jeremy (e.g., "Is he strong?"), they can occur spontaneously (Carlston & Skowronski, 1994). Evidence suggestive of these *spontaneous trait inferences* (STIs) is now ubiquitous (see Uleman, 1999). However, traits are ascribed to informants even when the informants describe *others'* behaviors. For example, if Jeremy describes how Ian power-lifted 500 pounds, Jeremy will be perceived by a listener as stronger than if the behavior describing Ian had not been heard. The term *spontaneous trait transference* (STT) has been used to characterize this paradoxical tendency (Skowronski, Carlston, Mae, & Crawford, 1998).

The STT effect suggests that an informant can engage in impression manipulation by describing others' positive behaviors, an approach made even more attractive because it likely bypasses norms prohibiting excessive self-boasting. The manipulative potential of STT seems

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especially large given that the effect occurs even when informants are well known (Mae, Carlston, & Skowronski, 1999) and when perceivers are warned of the effects and told to avoid them (Carlston & Skowronski, 2005).

STT has also generated considerable theoretical interest. Skowronski et al. (1998) argued that STT occurs because (1) a trait is *activated* while encoding a behavior description, (2) the activated trait is *associated* with the mental representation of the informant, and (3) the association is *later used when constructing a judgment* of the informant on that trait. Hence, one possible line of STT research can be designed to understand when associations between traits and mental representations form and when such associations are later used in the process of constructing judgments about informants (see Crawford, Skowronski, & Stiff, in press).

Skowronski at al. (1998) also suggested that the mental processes that underlie STT differ from the processes thought to underlie STIs. That is, on receipt of a self-informant's behavior description, a perceiver (1) *activates the trait* implied by the description; (2) *spontaneously generates a trait attribution* about the informant, producing an inferential link between the trait and the mental representation of the informant; and (3) *simply accesses the already made inference* when later making a trait judgment about the informant. Thus, at least three process-related differences may distinguish STT from STI: (1) an attributional process at encoding that *assigns the trait to the target* (*yes* in STI, *no* in STT); (2) the *consequences of that difference for the subsequent mental representation of the informant* (*inferential link* between informant representation and trait in STI, *associative link* in STT); and (3) *the nature of the processing that occurs in response to a later trait question about the informant* (*direct retrieval* of prior attributions in STI; *construction of new inference* influenced by an existing association in STT).

Bassili (1989, 1993) has an alternative view. He argues that some methods used to search for evidence of spontaneous, online inference making merely detect trait–target associations. Although associations can form as a result of inferential activity, they also occur for other reasons (e.g., spatial and/or temporal contiguity). Hence, evidence that supposedly shows evidence of spontaneous inference making may simply reflect these noninferential, associative processes. Even recently used paradigms, such as the savings-in-relearning methodology used by Carlston and Skowronski (1994; Carlston, Skowronski, & Sparks, 1995) and the false recognition methods used by Todorov and Uleman (2003, 2004) are subject to this criticism.

For example, to show how noninferential object–trait associations can result in recall savings (which has been previously assumed to reflect STIs), Brown and Bassili (2002) used a modified savings-in-relearning design in which photos of target individuals each appeared with a trait-implicative sentence as well as another stimulus. The additional stimuli were either photos of another person (a bystander) or an inanimate object (e.g., a banana). Savings task data revealed that heightened trait recall occurred when the trait was cued by both these bystanders and inanimate objects. Brown and Bassili argued that given that inferential activity was unlikely (especially for the inanimate objects), these findings reflect the tendency for items active concurrently in working memory to become associated. Thus, they conclude,

> STI, like STT, may result primarily from automatic associations formed in working memory between trait constructs . . . and actors, bystanders, or even inanimate objects that happen to be part of the context that led to the activation of the traits. (p. 91)

One resolution to this theoretical debate lies in research that can show differences between STI and STT. For example, in some studies (Skowronski et al., 1998), participants encountered photo–behavior pairs. In some pairs, the behavior implied a trait. Later, participants learned photo–trait pairs. Some pairs were "old," conceptually corresponding to photo–behavior pairs viewed earlier. For example, if a photo of Jessica was paired with the behavior "donates money" at Time 1, at Time 2 participants attempted to associate the word "generous" with Jessica's photo. Later, participants attempted to report the word when cued by the photo. Prior exposure to the photo–behavior pair facilitated photo–trait word learning, but more so when the behavior was depicted as performed by the person in the photo (STI condition) than when the behavior was depicted as the photographed person's description of a third-party's actions (STT condition). It should be noted that Brown and Bassili's (2002) data revealed a similar effect: The savings effect that emerged for self-informants was also substantially larger than savings effects for either of the additional stimuli (bystander or object).

Other evidence pointing to different cognitive processes underlying STI and STT comes from Skowronski et al. (1998; Study 3). Participants in their study were told that the photos came from one university, the behaviors from another university, and photos and behaviors were randomly paired. Despite these instructions, subsequent trait ratings of the photographed people were elevated on the traits implied by the behaviors. However, the magnitude of the effect was much more typical of effect sizes observed in STT conditions of other studies. That is, in comparison to conditions in which behaviors were depicted as self-descriptions, the random-pairing manipulation reduced, but did not eliminate, the impact that descriptions had on trait judgments. Skowronski et al. argued that such results suggest that random-pairing instructions turned off correspondent inference processes at encoding. They further argued that in the absence of correspondent inference processes, the effects of the descriptions on trait judgments were caused by the use of the behavior–trait associations that formed at exposure when later making trait judgments.

Carlston and Skowronski (2005) also reported results indicative of different processes underlying STI and STT. They argued that the correspondent inference processes thought to underlie STI might yield different signatures in social judgments than the associative processes thought to underlie STT. They reasoned that people use implicit personality theories that detail intertrait relations (see Anderson, 1995; Schneider, 1973) to generalize perceptions of a target from existing inferences to other trait dimensions. Because people are thought not to have such target attributions stored in memory in STT conditions, but generate those attributions in response to a trait query, generalization to other trait dimensions should be less likely than in STI conditions. Indeed, in several studies Carlston and Skowronski found more evidence of halo effects in judgments of self-informants than in judgments of informants who described third parties. A second possibility pursued by Carlston and Skowronski (2005) was that negativity effects characterize STI but not STT. They argued that the theoretical bases (e.g., diagnosticity and typicality—see Skowronski & Carlston, 1989) of negativity effects in trait judgments applied only to situations in which people used correspondent inference processes (e.g., when a behavior self-description is provided). When inferences about informants should not be made from the behaviors described (e.g., third-party informants), negativity effects should not appear. Support for this idea emerged in all three of the studies that Carlston and Skowronski conducted.

OVERVIEW OF THIS RESEARCH

Although the data that exist are suggestive of differences between the processes underlying STT and STI, the case that those processing differences exist is by no means conclusively made. The larger savings and trait-rating effects observed in STI conditions than in STT conditions could be caused by greater attention paid to self-describers (STI conditions) than to other-describers (STT conditions). As Brown and Bassili (2002) argue, these attentional differences might cause differences in the strength of associations that occur between an actor and an activated trait. It is these differences in association strength, and not the postulated differences in processes underlying STI and STT (inferential vs. associative), that might account for these effects.

Similarly, the halo effects described in Carlston and Skowronski (2005) might not be diagnostic of different processes underlying STI and STT effects but may simply be a consequence of inferential extremity that is consequent to associative strength. That is, it may be the case that all inferences are produced only when a trait question is asked (and not during encoding) that stronger associations produce more extreme inferences, and it is these more extreme inferences (typically in STI conditions) that are more likely to yield halo effects.

Given these arguments, critics of the idea that STT effects and STI effects are driven by different processes can reasonably claim that the case for those processing differences has yet to be sufficiently made. Accordingly, the studies described in this article further explore the idea that the processes underlying STT and STI differ and attempt to provide insight into the nature of the processes that underlie each effect. In Studies 1 and 2, some participants were asked to judge whether each informant was lying or telling the truth (see Skowronski et al., 1998). The rationale for using such a processing goal is that judging the veracity of the behavior involves generation of an inference that *is not derived from the implications of the described behavior* but from the informant's *act of describing the behavior*. Hence, we reasoned that although trait constructs might be activated while reading the behavior, allowing trait–informant associations to form, most inferences produced in such conditions ought to focus on the veracity of the informant and not on the trait implied by each behavior. Thus, one might expect a significant weakening of data indicative of spontaneous trait inferences generated from the meaning of the described behaviors, as is thought to be typical when processing self-informants' descriptions. However, effects that rely on informant–trait associations (i.e., targets who describe others in STT conditions) should remain unabated.

The data from Studies 1 and 2 show that the processing goal manipulation worked as intended. However, one possible interpretation of these results could be that the data reflect the action of cognitive load on inferences. Although plausible, existing data suggest otherwise. For example, Todorov and Uleman (2002) found that cognitive load had no effect on the generation of STIs. Study 3 attempts to duplicate this outcome. Some participants in Study 3 were asked to encode behavior descriptions under cognitive load. If the Todorov and Uleman outcome holds, cognitive load should not interfere with outcomes in either STI or STT conditions. Such an outcome would suggest that the effects of the alternative inference instructions observed in Studies 1 and 2 were not caused by decreased capacity during behavior encoding.

A second issue addressed in these studies concerns the psychological reality of STT effects. Because of the extensive use of within-subjects paradigms that present both self-informants and third-party informants, some might claim that STT effects are caused by mistaken identity: Some third-party informants are mistaken for self-informants. Moreover, because of these misidentifications, correspondent inferences may be made about third-party informants, and these inappropriate inferences can affect later measures of association and judgment. The studies in this article attempt to discount this argument by showing that STT occurs in a between-subjects design in which such confusions are unlikely.

STUDY 1: "LIAR, LIAR, PANTS ON FIRE"—ALTERNATE INFERENCE GOALS AND SAVINGS

Participants in Study 1 were exposed to photos of informants. The photos were paired with descriptions of behaviors. These descriptions were worded to imply that the person in the photo was either describing himself or herself or was describing a third party. Some participants were simply instructed to familiarize themselves with the materials. Others were told to determine whether each informant was lying.

As noted earlier, this latter processing goal was intended to sidetrack correspondent inferences that occur when receiving self-informant descriptions. Despite the lie-detection goal, interpretations of the descriptions should be unaffected: The trait implied by a behavior should still be activated at encoding and associated to the mental representation of the informant. These ideas lead to the expectation that in the savings paradigm used in Study 1, (a) savings effects in trait recall will emerge in all conditions; (b) in the familiarization condition, savings effects will be weaker in the STT (*other-informant*) condition than in the STI (*self-informant*) condition; and (c) in the lie-detection condition, the discrepancy in savings effects between STT and STI conditions will be reduced or eliminated.

The lie-detection manipulation is patterned after that used by Skowronski et al. (1998; Study 3). However, in their research they only explored the effects of such instructions on trait ratings. It is possible that associative strengths may not differ for self-informants and other-informants, but that additional knowledge (e.g., the ability to recall whether an informant described themselves or a third party) determines whether that association is translated into a trait judgment. Exploration of the parallel (or nonparallel) nature of the effects of the lie-detection instructions on association (Study 1) and judgment (Study 2) measures provides another justification for Study 1. In addition, it should be noted that the effect obtained in the Skowronski et al. paper was not very robust: The triple interaction that reflected the differential effects of the lie-detection instructions on the STI and STT conditions merely approached significance. Given the weakness of the initial finding, replication of the effect would obviously enhance its believability.

One other reason to pursue this effect is to verify that the savings measure is responsive to inferential activity. In this regard, it should be noted that the effect of processing goal-instruction manipulations on data derived from the savings task has rarely been successful. For example, in their pursuit of STI effects, the Carlston and Skowronski (1994; Carlston et al., 1995) research group explored whether savings effects were moderated by various processing goal instructions. The answer they generally obtained was no. For example, instructions to memorize the behaviors did not reduce savings effects, as some might expect; instructions to form impressions of the actors also did not increase savings effects. These findings were used to bolster the argument that inferences were being made spontaneously, occurring despite the processing goals that participants had when exposed to an actor's behavior description. In fact, so far only instructions that strike at the very nature of the stimuli seem to alter the magnitude of savings effects obtained. For example, Skowronski et al. (1998) told participants that the behaviors and the actors were randomly paired and had nothing to do with each other: That instruction reduced evidence of inferential activity for self-describing targets.

The resistance of the savings measure to processing goal manipulations might be viewed as unusual in the context of STI research. For example, Uleman and Moskowitz (1994) found that evidence indicative of STIs was reduced when they instructed participants to focus on the graphemic features of behavior descriptions. Moreover, they found that asking participants to generate one type of alternative inference (judging how similar they were to the actor) also seemed to reduce evidenceof STIs (although curiously, a seemingly similar kind of alternative inference, judging whether their behaviors were similar to the actors', did not seemingly impair STI activity). Given the savings measure's history of resistance to manipulation by processing goal instructions, it is theoretically important to establish that the measure is responsive to alterations in participants' processing goals. If this does not occur, then one might wonder whether the savings measure really is reflecting spontaneous inference activity that is occurring during behavior encoding. An absence of such effects would support Brown and Bassili's (2002) contention that the savings measure simply reflects associative processing.

Finally, Study 1 uses a between-subjects design in which participants are exposed only to either self-informants or to third-party informants. Prior research has typically used within-subject designs. Although such designs offer heightened analytic power, they offer the possibility of confusion: Participants may mistake other-informants for self-informants. Such mistakes may cause illusory savings effects for other-informants. Although results of various subsidiary analyses (cf. Skowronski et al., 1998) have discounted this possibility, the use of a between-subjects manipulation in which participants are exposed to only one informant type rules it out entirely.

METHOD

Participants

One hundred fourteen students enrolled in psychology courses at the University of Bristol participated in exchange for credit toward completion of course requirements.

Materials

The materials for the experiment have been used previously (e.g., Carlston & Skowronski, 1994; Skowronski et al., 1998). In an initial familiarization task, each participant encountered 24 critical photos accompanied by behavior descriptions. Twelve descriptions implied a trait (see Carlston & Skowronski, 1994, for pretest results). Some traits were positive (e.g., "honest" or "dedicated"), others were negative (e.g., "cruel" or "conceited"). The photos were 250 × 345 pixels (16 million colors) and depicted Purdue University students. These students varied in age and ethnicity and presumably were unknown to the University of Bristol participants. There were an equal number of male photos and female photos.

Target manipulation. Before exposure to the photo–behavior pairings, some participants were told that the photographed individuals were describing themselves. To emphasize this, the behavior descriptions in this self-informant condition used the pronoun "I." Other participants were told that the individuals in the photos were describing someone else. Descriptions in this other-informant condition used third-person pronouns such as "he" or "she," and the gender of these pronouns always differed from the gender of the informant.[1]

Procedure

On arrival, participants were led to a Dell Precision 360 computer. The presentation of materials was controlled by InQuisit experimental software. A description of the study was provided on the computer screen and was also read aloud by the experimenter. Participants were told that they would first see photos of people paired with a behavior description. In the self-informant condition, this description was portrayed as something that the person in the photo did. In the other-informant condition, the description was portrayed as a description of a third party's behavior. On each trial of this encoding phase, a photo and a behavior simultaneously appeared and stayed on the computer screen for 8 s; after this time, the next photo–behavior pair automatically appeared. The first 2 and the last 3 pairs were fillers; the middle 24 pairs constituted the critical photo–behavior pairs. The pairs were presented in a random order with the only constraint that two trait-implicative behaviors could not appear consecutively.

Processing goal manipulation. Participants in the familiarization condition were instructed to look at the photographs and read the descriptions to familiarize themselves with the materials used in the experiment. Participants in the lie-detection condition were told that some informants were telling the truth and that some were lying. These participants were instructed to look at the photographs, read the behaviors, and determine whether each informant was lying. In this latter condition, following the presentation of each pairing the participant pressed one of two keys to indicate their guess about an informant's truthfulness.

Confusion task. After completion of the encoding phase, participants engaged in a filler task. In this filler task, participants were given pairs of behaviors, each performed by a different person. Participants were asked to indicate which of the individuals they liked better. Participants responded to 30 such pairs. Many pairs contained behaviors related to the traits implied by behaviors presented in the encoding phase. This was done to try to overload memory and to make it difficult to use a behavior as a recall cue for a specific trait. Data from Carlston and Skowronski (1994) attest to the power of this procedure: Participants in their studies could not reliably recognize the behavior that was paired with each photo in the initial encoding task.

Paired-associates learning task. Next, participants were exposed to the previously seen photographs, each paired with a trait word. Participants were told that they would be asked to remember the word that was paired with each photo. Each pair was viewed for 5 s and was viewed only once. Each of 12 *relearning-trial* photos was paired with a trait word implied by the behavior with which it was paired during the encoding phase. For example, if a photo was paired with a mean-implying behavior during the encoding phase, that photo would be paired with the word *mean* in the learning task. Twelve additional *learning-trial* photos were also paired with trait words, but in the encoding task these photos were paired with neutral behaviors. A counterbalancing scheme was used such that the control traits for some participants were critical traits for other participants. Hence, results cannot be explained by photo–trait pairing confounds.

Trait recall task. Participants completed an anagram-completion filler task that consumed 5 min. Then, in the next task, the computer presented each photo from the learning task, one at a time, in a random order. Participants were required to type into the computer the trait word that was paired with the photo in the learning task. After the word was entered, the next photo appeared. Participants continued until all trials were completed. Upon completion of the recall task, participants were debriefed and thanked for their participation.

RESULTS AND DISCUSSION

The trait recalled on each trial was scored as correct or incorrect using a gist criterion. A percentage indicating the number of traits correctly recalled by each participant was calculated separately for each trial type (i.e., learning vs. relearning). The percentages were entered into a 2 (processing goal: familiarization vs. lie detection) × 2 (target: self-informant vs. other-informant) × 2 (trial type: relearning vs. learning) mixed ANOVA with repeated measures on the trial-type variable.

The results of this analysis showed greater trait word recall when the trait had been previously implied by a behavior description paired with the cuing photo than when it had not (relearning trials $M = 73\%$; learning trials $M = 63\%$), $F(1, 110) = 40.87$, $p < .001$. Of particular interest was whether this effect was moderated by informant type and/or by processing goal. Indeed, the analysis yielded a Target × Trial Type × Processing Goal interaction, $F(1, 110) = 5.25$, $p < .01$ (see Figure 1). To further probe this interaction, follow-up analyses examining the Processing Goal × Trial Type interactions for each target were conducted.

For other-informants, only the trial type main effect was significant, $F(1, 54) = 13.40$, $p < .01$. The means for this effect indicate that recall was significantly higher on relearning trials than on learning trials. Neither the processing goal main effect nor the interaction between processing goal and trial type was significant (Fs < 1, *ns*). Thus, as expected, in the other-informant condition, processing goal was not related to savings effect magnitude.

However, this was not true in recall of traits for self-informants. The follow-up analysis for self-informants revealed two significant effects. The first, not surprisingly, was a main effect for trial type that reflects a significant savings effect, $F(1, 56) = 31.63$, $p < .001$. More important for the current investigation is the significant Processing Goal × Trial Type interaction $F(1, 56) = 10.10$, $p < .01$. The means for this interaction show that for self-informants, the lie-detection instructions did interfere with performance on the recall task. In fact, the amount of savings observed in the self-informant and lie-detection condition, although still significant, was slightly below (but not significantly different from) the level of savings in the STT conditions. Thus, in our view, these data suggest that the correspondent inference processes that normally cause strong informant–trait linkages to occur under familiarization instructions were disrupted by the lie-detection instructions, leaving only informant–trait associations in place. This interpretation fits with the results of follow-up analyses separately conducted within each processing goal condition. These showed that the savings effect was larger for self-informants than for other-informants in the familiarization condition, $F(1, 56) = 4.68$, $p < .05$, but not in the lie-detection condition, $F(1, 54) = 1.54$, $p > .20$.[2]

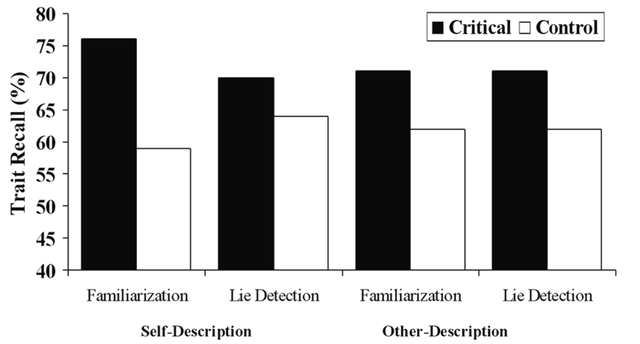

Figure 1 Experiment 1: Mean trait recall percentage as a function of target, trial type, and processing goal.

STUDY 2: LIAR, LIAR, REDUX—ALTERNATIVE INFERENCE GOALS AND TRAIT RATINGS

The results of the first experiment provide strong support for our predictions. Most important for the current theorizing was the effect of processing goal on savings in trait recall. Specifically, when engaged in an alternative inferential task (i.e., lie detection), the advantage of relearning trials over learning trials in the STI condition was diminished to a level equivalent to that shown in the STT conditions. It is important that the alternative inference task had no influence on performance in the STT condition. Thus, it appears that the lie-detection instruction interfered with *inferential* processes underlying STI but not *associative* processes underlying STT.

Study 2 explored whether the savings effects obtained in Study 1 would be mirrored by trait ratings of the informants. In the second experiment, the trait-learning and trait-recall tasks were replaced with a trait-rating task. Those informants who provide trait-implicative self-descriptions should obviously receive heightened ratings on traits implied by descriptions. Of more interest is whether other-informants receive a similar boost in trait ratings. Such a finding would replicate previous investigations of STT (e.g., Skowronski et al., 1998) and confirm that describing trait-implicative behaviors of others has implications for how an informant is rated on that trait by a message recipient. A second issue of interest, and the larger issue for purposes of this article, is whether trait ratings are reduced by lie-detection instructions. The results of Study 1 and of Skowronski et al. (1998) suggest that this should occur only in judgments of self-informants. Because the trait-implicative descriptions provided by other-informants are not thought to prompt correspondent inferences about the informants on those traits at encoding, lie–detection goals should have little impact on the trait judgments made about them.

METHOD

Participants

One hundred forty-seven psychology students at the University of Bristol participated in exchange for credit toward completion of course requirements.

Materials and Procedure

Materials, procedures, and manipulations duplicated Study 1, with the following exception. Instead of completing the paired-associates learning and trait-recall tasks, participants completed a trait-rating task. In this new task, the individual depicted in each of the photos was rated on three separate trait dimensions, and each rating reflected how much of the trait each person possessed. Each rating was made on 9-point unipolar scales that had response options labeled at the midpoint and endpoints (1 = *not at all*, 5 = *moderately*, 9 = *extremely*).

All trait terms corresponded to the trait implications of one of the behaviors described by an informant. However, across ratings, each trait served one of three different roles: It could serve as a *critical trait*, it could be *evaluatively congruent* with (but low in semantic relatedness to) the critical trait, or it could be *evaluatively incongruent* with (and low in semantic relatedness to) the critical trait. A counterbalancing scheme was used to ensure that each trait scale appeared three times: once as a critical trait, a second time as an evaluatively congruent trait, and a third time as an evaluatively incongruent trait. Collapsed across between-subjects counterbalancing conditions, participants provided exactly the same trait ratings about the same targets. Thus, results that emerge for the trait-type variable are not confounded with traits rated.

Each participant made ratings of 24 critical traits. Twelve of these were for informants who each provided a trait-implicative behavior description; the other 12 were for informants who each provided a neutral description. A counterbalancing scheme was used to vary the critical traits and control traits across groups, so that the control traits for one group of participants were the critical target traits for the other group. Hence, any trial-type results that emerge from the analyses are not confounded with the nature of the traits rated.

RESULTS AND DISCUSSION

Critical Traits Implied by Informants' Behavior Descriptions

Mean trait ratings were calculated for each participant's trait ratings for learning and relearning trials. These averages were submitted to a 2 (target: self-informant vs. other-informant) × 2 (processing goal: familiarization vs. lie detection) × 2 (trial type: critical-trait-implied trials vs. no trait-implicative description-control trials) mixed-measures ANOVA with repeated measures on the trial type variable.

Processing goals and STT and STI effects. This analysis revealed a significant main effect for trial type, $F(1, 143) = 97.26$, $p < .001$, indicating that overall, informants were rated more extremely after reporting trait-implicative behaviors than after reporting neutral behaviors. The analysis also revealed a significant Trial Type × Target interaction, $F(1, 143) = 43.49$, $p <. 01$, indicating, consistent with expectations based on previous research, that the trial-type main effect was stronger for self-informants than for other-informants. Other effects emerging from the main ANOVA were target, $F(1, 143) = 26.29$, $p < .001$; Target × Processing Goal, $F(1, 143) = 7.35$, $p < .01$; and Trial Type × Processing Goal, $F(1, 143) = 14.49$, $p < .01$. However, these effects were subsumed by the significant Target × Processing Goal × Trial Type interaction, $F(1, 143) = 15.90$, $p < .01$ (see Figure 2).

To probe the three-way interaction, follow-up Processing Goal × Trial Type analyses were performed separately on the self-informant (i.e., STI) and other-informant (i.e., STI) conditions. Only a significant trial-type effect emerged for other-informants, $F(1, 143) = 10.38$, $p < .01$. Informants who described trait-implicative behaviors were rated higher on the corresponding trait

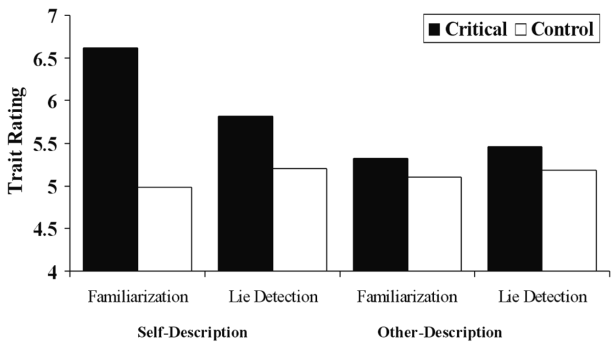

Figure 2 Experiment 2: Trait-rating means for those who described trait-implicative behaviors and those who did not as a function of target and processing goal.

than those who described a neutral behavior. This occurred regardless of processing goal ($F < 1$). Ratings of self-informants evinced a different pattern. Means for the Trial Type × Processing Goal interaction, $F(1, 143) = 20.63$, $p < .01$, indicate that the trial-type effect was significant for both processing-goal conditions (both *p*s < .05), but that the effect was more pronounced for those who received familiarization instructions than for those who received lie-detection instructions.

Overall, this pattern of results suggests that the correspondent inference processes that usually occur in response to self-informant descriptions were diverted by the lie-detection instructions. Because there are no correspondent inference processes to divert in STT conditions, the lie-detection instructions had no effects on judgments of third-party informants. Instead, regardless of instructions, judges of third-party informants apparently formed informant–trait associations at encoding and later used those links to construct trait inferences about the informants.

Examining Specificity: Ratings of Nonimplied Traits

Additional analyses investigated whether the STI and STT effects observed in the ratings of critical implied traits generalized to other, nonimplied, traits. Mean trait ratings were calculated for each participant within each cell of the Trial Type × Trait Congruency matrix and were submitted to a 2 (target: self-informant vs. other-informant) × 2 (processing goal: familiarization vs. lie detection) × 2 (trial type: critical-trait-implied trials vs. no trait-implicative description-control trials) × 2 (trait congruency: congruent vs. incongruent) ANOVA with repeated measures on the latter two variables.

The results of the analysis yielded a main effect of trait congruency indicating that ratings on evaluatively congruent traits were higher ($M = 4.98$) than ratings on evaluatively incongruent traits ($M = 4.77$), $F(1, 143) = 26.23$, $p < .001$. However, a significant Target × Trait Congruency interaction, $F(1, 143) = 8.30$, $p < .01$, indicated that this main effect was stronger for self-informants (consistent $M = 5.04$; inconsistent $M = 4.70$) than for other-informants (*M*s = 4.93 and 4.84). The analysis also yielded a Trial Type × Trait Type interaction, $F(1, 143) = 7.46$, $p < .01$. Informants paired with behaviors that implied a trait were rated higher on evaluatively congruent traits ($M = 5.11$) than informants paired with neutral behaviors ($M = 4.81$); $t(146) = 4.21$, $p < .01$, and significantly lower on evaluatively incongruent traits ($M = 4.63$) than targets who were initially paired with a neutral behavior ($M = 4.91$); $t(146) = –4.42$, $p < .01$. This effect, however, was subsumed by the significant three-way interaction between trait type, trial type, and target, $F(1, 143) = 4.27$, $p < .05$ (means shown in Table 1); the four-way interaction (adding processing goal), however, was not significant. Follow-up analyses to the significant three-way interaction indicate that the effect is driven by the fact that the Trait Type × Trial Type interaction is significant for self-describers, $F(1, 143) = 6.99$, $p < .05$, but not for other-describers ($F < 1.0$). These results are consistent with Carlston and Skowronski's (2005) claim that there should be little or no evaluative halo in judgments of third-party informants.

STUDY 3: CAN YOU HANDLE IT ALL? COGNITIVE LOAD AND SAVINGS

Studies 1 and 2 attempted to interfere with attributional processes in self-informant conditions by rerouting the inference process. By introducing an alternative inference task that directs perceivers away from trait inferences that are derived from the content of the behavior described, both savings effects for self-informants and trait judgments made about self-informants were reduced.

We believe that this effect occurred because in the lie-detection conditions, perceivers were making an alternative inference about the self-informants (i.e., whether they were lying or telling the truth). However, as noted by Skowronski et al. (1998), an alternative possibility exists. Imposition of the lie-detection task may induce cognitive load at behavior encoding, making correspondent inferences from the content of the behavior less likely.

Whether such inferences are subject to load effects is a matter for debate. Results of early research suggested that initial dispositional attributions (which, according to Gilbert, Pelham, & Krull, 1988, occurred in a characterization phase of processing) occurred independently of cognitive load—it was only adjustments to those inferences that were load dependent. This outcome has been

TABLE 1: Mean Ratings in Study 2 for Traits by Evaluative Congruence, Initial Behavior, and Target Type

	Trait Rated			
	Congruent		*Incongruent*	
Self-informant				
Trait-implying behavior	5.09	(0.70)	4.51	(0.75)
Neutral behavior	4.92	(0.64)	4.90	(0.64)
Other-informant				
Trait-implying behavior	4.98	(0.60)	4.90	(0.61)
Neutral behavior	4.89	(0.63)	4.80	(0.60)

NOTE: Standard deviations in parentheses.

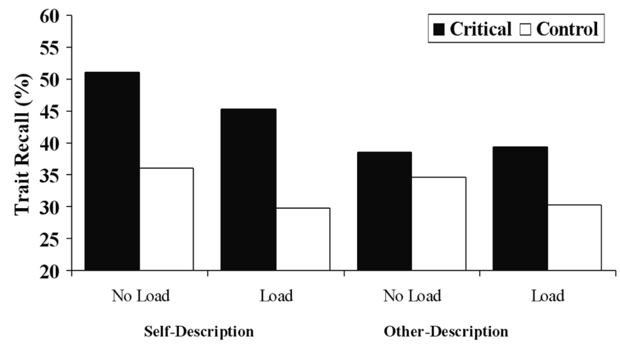

Figure 3 Experiment 3: Mean trait recall percentage as a function of target, trial type, and cognitive load.

reinforced by similar results from spontaneous-trait-inference research (Lupfer, Clark, & Hutcherson, 1990; Todorov & Uleman, 2002; Winter, Uleman, & Cunniff, 1985). However, recent research has challenged this conclusion (Chun, Spiegel, & Kruglanski, 2002), suggesting that load can alter initial behavior-based correspondent inferences—especially when the stimulus itself is challenging to process.

The photo–behavior pairs that were presented in the initial encoding phase of Studies 1 and 2 may pose just such a processing challenge. The stimuli that were presented in those studies combined visual and written components. The written components were often relatively lengthy, and the stimuli were presented only for a relatively brief period of time. Hence, if Chun et al. (2002) are correct, there is a possibility that imposition of a cognitive load may interfere with spontaneous correspondent inferences in STI conditions.

However, two alternative outcomes may occur. The first of these is that cognitive load may equally affect both STI and STT, as might happen if the load interfered with the ability to associate the trait implied by the behavior with the cognitive representation of the communicator (see Crawford et al., in press). Such an outcome would be useful on two fronts. First, it would suggest that the lie-detection effects observed in Studies 1 and 2 were *not* caused by cognitive load. The effects in Studies 1 and 2 were selective, occurring only in response to communications of the self-informants. A pattern showing general interference with both STT and STI would be inconsistent with this selectivity. Second, evidence of such interference would be novel from the perspective of STT; most studies have shown that the STT effect is robust to attempts to interfere with the effect (e.g., Carlston et al., 1995; Skowronski et al., 1998; but see Carlston & Skowronski, 2005; Crawford et al., in press).

A possible alternative outcome is that cognitive load may not alter effects in either the STT or STI conditions. Such an outcome would be consistent with the idea that the process of abstracting traits from behaviors, and of associating those traits with other informants (in STT conditions) or of making inferences about those informants (in STI conditions), occurs relatively automatically (as suggested by Gilbert et al., 1988). This is the outcome that has been obtained, albeit in a nonsavings paradigm, in previous STI research (Todorov & Uleman, 2002); it has yet to be demonstrated in STT research. If such a result were to occur, it would show that the Todorov and Uleman result was not paradigm specific. Moreover, because of its discrepancy with the results of Study 1 and 2, such a result would again suggest that the lie-detection effects observed in those studies were *not* caused by cognitive load.

METHOD

Participants

One hundred sixty-one undergraduates enrolled in an introductory psychology course at Northern Illinois University participated in partial fulfillment of a course requirement.

Materials

The methods, materials, and procedures for the study largely replicated those used in Study 1, with the following exceptions.

First, the program Direct RT (Jarvis, 2002) controlled the entire experiment. Second, on each trial of the familiarization task, each photo–behavior pair stayed on the computer screen for 12 s. This time was increased after pilot testing suggested that those in the cognitive-load conditions could not easily process the photo–behavior pairs in the 8 s exposure time used in Studies 1 and 2.

Finally, some participants completed the initial encoding task under *cognitive load*. Load was induced by presenting a 10-digit number underneath each photo during the encoding task. Participants were asked to memorize the number and were prompted to report the number immediately after each photo–behavior pair disappeared from view. Participants in the no-load condition were not exposed to the numbers or the number-recall task.

RESULTS AND DISCUSSION

STT and STI Emerge Again

A trait word was coded as correctly recalled using a gist criterion. The proportion of trials on which a word was correctly recalled was separately calculated for each participant for each trial type (i.e., learning vs. relearning). These proportions were entered into a Cognitive Load (load vs. no load) × Target (self-informant vs. other-informant) × Trial Type (relearning trial vs. learning trial) mixed ANOVA with repeated measures on the final variable. Trait-recall means are presented in Figure 3.

The analysis revealed the expected trial-type effect, $F(1, 157) = 55.57$, $p < .001$, which indicates that recall performance was better when a target was paired with traits that matched the trait implications of the initial behavior (relearning $M = 44\%$) than when the target was originally paired with a neutral behavior (learning $M = 33\%$). Simple effects tests showed that this effect was statistically reliable in both the self-informant condition, relearning $M = 48\%$, learning $M = 33\%$, $F(1, 157) = 47.15$, $p < .001$, and the other-informant condition, relearning $M = 39\%$, control $M = 33\%$, $F(1, 157) = 12.01$, $p < .01$. However, as expected, the relearning effect was stronger for self-informants than for other-informants, yielding a significant Trial Type × Target interaction, $F(1, 157) = 8.84$, $p < .01$. The larger relearning effect for self-informants than for other-informants is consistent with the argument that the inferential processes that are thought to contribute to STIs made about self-describers cause stronger informant–trait linkages (and ones that are labeled as inferential links) to form than the associative processes that are thought to be responsible for STTs.

STI and STT Are Unaffected by Load

Participants correctly identified the entire 10-digit-number sequence correctly 65.4% of the time when those numbers occurred prior to self-describer trials; the correct identification rate was 63.3% for other-describer trials. Other analyses (e.g., counts of the actual number of numbers recalled) yield similar evidence of high number recall rates that do not differ across condition. Certainly, it is impossible to claim that participants ignored the number sequences, or were inattentive to them, in the face of such high recall rates. Most important, such high recall rates are consistent with the notion that the number memory task imposed a cognitive load on participants.

The main new questions of interest in Study 3 were whether such cognitive load reduced STT and STI and whether that reduction occurred for both conditions. The answer to both questions is "no." Cognitive load did not interact with trial type, $F(1, 157) < 1$, nor did it interact with the combination of informant and trial type, $F(1, 157) < 1$. Visual inspection of the means for the three-way interaction (see Figure 3) depicts the impotence of the load manipulation across conditions. This impression is confirmed by the results of simple effects tests examining the effect of load on the magnitude of savings effect separately for self-informants and other-informants. These subsidiary analyses reveal that the load manipulation was not significantly related to the magnitude of the savings effects that emerged (largest $F = 1.89$, $p = .17$).

These results do not fit with the idea that the effectiveness of the lie-detection instruction in Studies 1 and 2 occurred of the instruction's load properties. Instead, consistent with the expectations derived from many other results (e.g., Lupfer et al., 1990; Todorov & Uleman, 2002; Winter et al., 1985), under cognitive load, trait information appears to be extracted from behavior descriptions in a relatively automatic way. This trait information is then spontaneously associated to other informants (STT condition), or spontaneous inferences are made about self-informants (STI condition).

Of course, this interpretation depends, in part, on the extent to which the load manipulation was actually effective in inducing load. In the case of a null effect, one must always entertain the possibility that a manipulation was ineffective. However, we believe that the evidence argues against this possibility in Study 3. First, the cognitive-load manipulation that was used in Study 3 has been shown in other research to interfere with at least some kinds of STI activity (see Wigboldus, Sherman, Franzese, & van Knippenberg, 2004). Hence, the ineffectiveness of the load manipulation cannot be attributed to an ineffective methodology, as might be the case if a manipulation had never been tried previously. In fact, the number-recall load methodology has been used time and again and has been shown to be an effective manipulation that can alter trait inferences that are made about actors (e.g., Gilbert et al., 1988). Of course, one might always claim that the manipulation may have been ineffective in Study 3 because participants may have ignored the number memory task. However, the high rates of recall for the digits strongly argue against this interpretation. Given (a) the evidence suggesting that people attended to the load task and (b) the demonstrated ability in other studies of this load task to interfere with some social inference making, but (c) the demonstrated inability of this load task to interfere with the kinds of STIs examined in Study 3, the best conclusion from Study 3 seems to be that associations and inferences were extracted from behavior descriptions relatively automatically, despite the imposition of some level of cognitive load.

However, although we would claim that the results of Study 3 show that STT and STI are robust to *some* level of cognitive load, it is an open question as to whether the level of cognitive load imposed in Study 3 provided the *same* level of load as might have been imposed by the lie-detection instructions. Given the differing nature of the two manipulations, exactly equating load effects (should such effects exist in the lie-detection conditions) would be difficult. The best that one could do is a series of parametric studies that gradually increase cognitive loads and examine the effects on the data. One might suspect that at some point, cognitive load will certainly have an impact on the emergence of STIs and STTs. That is, at some point, cognitive loads should be large enough that they interfere with extraction of trait information from the behavior descriptions (see Crawford et al., in press; Uleman, Newman, & Winter, 1992). However, we note that because such high loads would undoubtedly affect the emergence of *both* STT and STI effects, such data would still not be consistent with the load explanation for the *differential* effects of the lie-detection instructions on STI and STT.

Additional insight into the debate might be obtained by conducting additional studies. One approach might be to impose a load during the ratings task. Because the STI effect theoretically relies on prestored inferences, reporting those inferences at a later time may simply be a matter of accessing and reporting the contents of memory. This process should be relatively immune to cognitive loads imposed during the reporting process. In comparison, in the STT case participants are theoretically being asked to use the contents of memory (e.g., the stored association) to generate an inference for the first time. This may be more difficult than the mere readout of a prestored inference and hence may be more subject to the interfering effects of cognitive load. Alternatively, the data in the lie-detection conditions might be probed more thoroughly for evidence that the load manipulation interfered with other elements of information processing. That is, one might look for evidence that the lie-detection instructions produce impaired memory for incidental characteristics of the stimuli (e.g., description font size or ink color) relative to the level of memory for those characteristics in impression formation or uninstructed conditions. Poorer memory in lie-detection conditions would suggest that it was indeed acting as a cognitive load.

A related issue to be addressed in subsequent research concerns the exact properties of conditions that are necessary to minimize STIs. Certainly, such STIs are minimized when people have little reason to believe that the behavior describes a target (e.g., Skowronski et al., 1998). Moreover, such inferences seem to be minimized, at least some of the time, when people have processing goals that direct peoples' attention to graphemic characteristics of the stimulus description or that prompt people to make nontrait inferences (e.g., Skowronski et al., 1998; Uleman & Moskowitz, 1994). Note, however, that not all such alternative inference activity seems to turn off STIs: Uleman and Moskowitz (1994) found that asking participants to judge whether their behaviors were similar to the actors' did not seemingly impair STI activity. Similarly, instructions to memorize the stimuli also did not lower evidence of STI extraction (Carlston & Skowronski, 1994). Thus, this corpus of research, although hinting that STIs can be interfered with, has yet to yield systematic knowledge about when and why this interference occurs. Development of this systematic knowledge should be one task for future research.

GENERAL DISCUSSION

Both STI and STT are supposedly related to the extent to which traits activated by behavioral exemplars become associated with specific targets. The current work examined the idea that STI involves inferential processes that go beyond mere association. It did so by examining the possibility that associations and trait judgments in STI conditions were easier to disrupt than associations and trait judgments in STT conditions.

Interfering With Inferential Processes Underlying STI

In Studies 1 and 2, we attempted to divert the correspondent inference process by asking some participants to make an alternative inference about the informants (i.e., whether they were lying or telling the truth). The lie-detection instruction caused a decrease in the extent to which self-informants were associated with traits (Study 1) and in the extremity of trait ratings given to those informants (Study 2). In comparison, the lie-detection manipulation had no effect on either the extent to which traits were associated with third-party informants or on the trait judgments made about them. These results provide clear and unequivocal evidence that different processes underlie STI and STT effects—it is difficult to explain such findings in other ways. Results of the cognitive-load manipulation used in Study 3 provide more suggestive evidence that the effect of the alternative inference processing-goal manipulation was not driven by a reduction in cognitive capacity at behavioral encoding.

In addition, the results of Study 2 provide evidence of different processes underlying STI and STT by showing differences in evaluative generalization for self-describers and other-describers. It was assumed that generalization from the trait implied by an informant's description to other nonimplied, but evaluatively congruent, traits would occur for self-informants but not for informants who described the behavior of others (Carlston & Skowronski, 2005). The rationale for such a prediction is based on the idea that if self-descriptions lead to dispositional inferences (as opposed to merely person–trait association), then halo effects may emerge based on perceivers' use of implicit personality theories (Schneider, 1973). Indeed, the results of Study 2 showed that ratings on evaluatively congruent traits were higher than ratings on evaluatively incongruent traits, and that this effect was largely attributable to the judgments made about self-informants.

The view of Carlston, Skowronski, and colleagues (Carlston & Skowronski, 2005; Skowronski et al., 1998), as well as the current view, is that the processes underlying STI and STT are in fact different. As mentioned previously, this perspective is quite different from that articulated by Bassili and colleagues (Bassili, 1989, 1993; Brown & Bassili, 2002), who argue that the target–trait associations that are detected in many paradigms may not represent inferential activity at all. The current studies add to the corpus of evidence suggesting that the simple associative position is not tenable. STI effects are reliably stronger than STT effects (even under cognitive load). Lie-detection instructions selectively affect judgments and savings made in STI conditions but not in STT conditions. Halo effects in judgments tend to emerge in STI conditions but not STT conditions. The same applies to negativity effects in judgment. Such findings converge on the idea that perceivers *do* form trait inferences about people who describe themselves, and do not *only* form associations (and do form inferences) between the representation of a self-informant and the trait construct activated during event interpretation.

However, as in Carlston et al. (1995), interference with correspondent inference processes does not totally eliminate savings and judgment effects in STI conditions. This is consistent with the contention of Carlston et al. that STT-like associative processes operate when correspondent inference processes are sidetracked. Some might suggest that such effects are suggestive of a dual-process model in which associative and inferential effects emerge from different cognitive subsystems, one largely associative and the second largely symbolic (Smith & DeCoster, 2000). However, a symbolic subsystem may not be necessary to explain such effects. Van Overwalle and Labiouse (2004) have used an autoassociative model (McClelland & Rumelhart, 1985; see also Smith & DeCoster, 1998) to account for many different phenomena in impression formation. By assuming that STIs and STTs activate different sets of connections (and at different strengths), such models may be able to account for differences in STTs and STIs without reference to a symbolic processing subsystem.

Do Identification Errors Cause Spontaneous Trait Transference?

One alternative explanation for the occurrence of spontaneous trait transference is that it is the result of informant misidentification. That is, STT may occur as a result of incorrectly encoding the source of a behavioral description, leading to the attribution of a trait even when the informant described the behavior of another person. Alternatively, the latency between exposure and testing may result in participants forgetting whether the original behavior was self-descriptive or other-descriptive (i.e., failed source monitoring), producing the same error.

The data presented in this article show that informant identification error is an unlikely cause of STT. All of the studies reported in this article used a between-subjects manipulation of the target of an informant's description, thereby making confusions virtually impossible. STT effects emerged in both trait recall (Studies 1 and 3) and trait ratings (Study 2). Thus, these data strongly suggest that STT is not the result of errors that cause confusion as to whether a particular communicator was describing his or her own behavior or the behavior of a third party. STT appears to be a real phenomenon that requires explanation in terms of the cognitive processes that occur when people encounter informants' descriptions of others' behaviors.

CODA

Peoples' memory systems change in response to receipt of information from the outside world. Sometimes, as in the case of inferences made about self-informants, these changes are relatively orderly and predictable. For example, the data now increasingly suggest that people sometimes spontaneously use the implications of a described behavior to draw correspondent inferences about self-informants, and that perceivers store those inferences in memory so that they can be used later. Given the existing data, perhaps only the frequency with which people make such spontaneous correspondent inferences is now open to debate. That they do it, at least some of the time, is relatively well established. Moreover, the processes by which such spontaneous inferences are made are relatively clear.

In comparison, peoples' memory representations sometimes change in unexpected ways as a result of encounters with the outside world, and these changes can later emerge in surprising ways. The STT phenomenon can perhaps be characterized in this way. That one can be perceived as more athletic by describing the athletic feats of another, as smarter by describing the brilliant behaviors of another, or as more dishonest by describing the immoral behaviors of another, is a seemingly counterintuitive phenomenon whose implications have yet to be fully delineated and whose causes have yet to be fully specified. The data provided in this article make significant progress toward showing that the STT effect is real and the processes underlying the STT effect differ from those responsible for the STI effect. More research needs to be done to further our understanding of these processes, the conditions under which STT effects will (or will not) occur, and whether the STT effect is powerful enough to affect judgments of real people in real-world contexts in substantial and long-lasting ways.

NOTES

1. We note that this manipulation introduces a minor confound into the procedure: The gender of the person performing the behavior differs in self-describer and third-party–describer conditions. For example, in the self-describer condition, a male is describing himself (a male); in the third-party–describer conditions, that male is always describing the same behavior performed by a female. However, this confound seems to be unimportant: Subsidiary analyses suggested that the gender of the target was not important to the emergence of effects in either self-describer or other-describer conditions. Moreover, analyses performed for earlier papers (e.g., Carlston & Skowronski, 1994) similarly suggested that informant gender was not related to the magnitude of the spontaneous trait inferences effect. The absence of such effects is likely a consequence of the behavior statements used in our studies, which were developed by Carlston and Skowronski (1994) to have clear and unambiguous trait implications.

2. Additional subsidiary analyses were conducted that examined whether the magnitude of the savings effect varied depending on whether informants were guessed to have lied or told the truth. These analyses yielded no results of theoretical interest.

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