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Social Compensation: Fact or Social-Comparison Artifact?

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The use of coactors as non-group controls in prior social compensation research has left open the possibility that the effect might artifactually have resulted from a confound between work condition (Coaction vs. Collective) and the opportunity to make performance comparisons. A direct empirical test of this alternative, artifactual explanation is reported. Its results contradict that explanation and suggest that the use of coactors as controls has, if anything, resulted in an underestimation of the magnitude of the social compensation effect. It is argued that multiple alternative non-group performance baselines can be informative for analyzing group motivation effects.

KEYWORDS group performance, motivation gains, social compensation, social loafing

OVER the past 25 years there have been many empirical demonstrations of motivation losses or *social loafing* in small performance groups (see Karau & Williams, 1993; Shepperd, 1993, for reviews). However, in the last decade or so, a handful of studies (e.g. Erev, Bornstein, & Galili, 1993; Hertel, Kerr, & Messè, 2000; Kerr & MacCoun, 1984; Stroebe, Diehl, & Abakoumkin, 1996) have documented group *motivation gain* phenomena—that is, higher task motivation within a group performance context than within a comparable individual performance context. While empirical demonstrations of motivation gains are rather rare (Hertel et al., 2000), the *social compensation* effect is a noteworthy example of one of the earliest and most thoroughly studied of such phenomena (e.g. Williams & Karau, 1991; Karau & Williams, 1997; Hart, Bridgett, & Karau, 2001).

Social compensation occurs when '. . . individuals increase their efforts on collective tasks to compensate for the anticipated poor performance of other group members' (Karau & Williams, 1997, p. 158). In the generic social compensation study, subjects work at a simple, effort-sensitive task[1] (e.g. writing down as many uses for a common object as possible) either in a coacting pair (who are not interdependent

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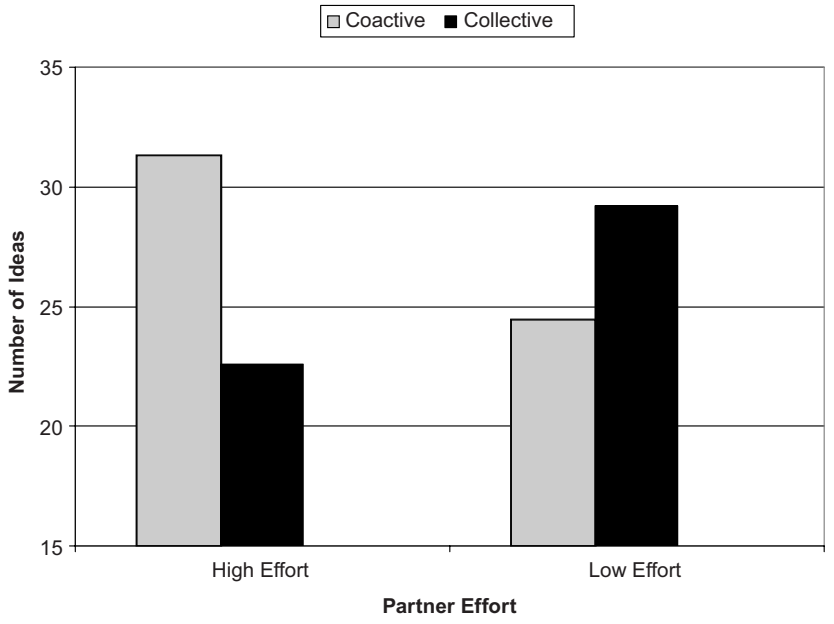
nor able to directly monitor one another's performance, but simply work in one another's presence) or within a cooperative dyad (collective condition). The group task is (a) additive (i.e. the group score is the simple sum of the group members' individual—but supposedly unidentifiable—scores; Steiner, 1972) and (b) information reducing (i.e. it is not possible to identify the individual group members' contributions from the group performance; Davis, 1969). These are precisely the conditions under which early social loafing research (e.g. Latané, Williams, & Harkins, 1979) had found and would predict less effort in the collective condition. And indeed, when one receives pre-performance information suggesting that one's partner is quite capable at the task (e.g. will exert a high level of effort (Williams & Karau, 1991, Experiment 2), or has high task ability (Williams & Karau, 1991, Experiment 3)), this is exactly what is observed (see the left sides of Figure 1a and 1b, which reproduce the results of two of Williams & Karau's early social compensation studies). However, when feedback suggests that one's partner is very incapable, a rather different pattern obtains (see the right sides of Figure 1a and 1b). In the collective condition, a good group performance can only be achieved if one works extra hard to compensate for the anticipated poor performance by one's teammate; it is under these conditions that the social loafing effect is reversed and the social compensation effect is observed.

This basic pattern has been replicated several times (e.g. Karau & Williams, 1997; Hart et al., 2001) and appears to be robust. However, the design of these studies introduced a potential confound that raises an alternative, artifactual explanation for this social compensation effect. The collective versus coactive contrast is intended to—and indeed does—compare a particular type of group performance (viz. performers are interdependent and the nature of that interdependence permits one group member to compensate through his/her efforts for an expected incapacity of a teammate) with a corresponding individual performance (viz. performers are independent and cannot affect one another's score or outcome through their own level of performance). A coaction control condition has typically been used (rather than a lone individual performer) to control for nuisance factors like modeling, distraction, and mere presence of another performer. Unfortunately, the coaction control condition also allows the possibility of a direct, immediate, and public comparison of scores with another performer, whereas the information-reducing aspect of the group task makes such an interpersonal comparison unfeasible. In fact, not only is such comparison possible in the coactive condition, instructions that are delivered to the two coactors prior to the task suggest that such comparison is certain ('The experimenter also told the participants that he would count up their individual scores . . . at the end of the session and tell them how many uses they had produced', p. 574, Williams & Karau, 1991). Such opportunities for a direct and immediate comparison could well lead to one using the anticipated output of the coactor as a basis for choosing one's own level of effort—a type of 'production matching' with an implicit performance standard (cf. Stroebe et al., 1996).

In sum, the social compensation effect could be due: (a) to the imperative of compensating for an incapable partner in the collective condition (the usual interpretation); and/or (b) to demotivating effects of public comparison with an incapable other in the coaction condition (an alternative, artifactual interpretation, which we will term the *social-comparison* explanation). The latter possibility is not theoretically far-fetched; if one believes that the one person with whom s/he will soon be publicly compared is not very capable, one may feel that less effort is required to insure a favorable comparison. In addition, the expectation that one's own performance will probably surpass that of readily available comparison others could well be demotivating by reducing opportunities for both meaningful self-evaluation and favorable external evaluation (cf. Harkins, 2001). Consistent with such speculation, empirical demonstrations of the social compensation effect have not been characterized by a dramatic rise in effort when social compensation is needed (i.e. in the incapable partner/collective condition),

(a) Experiment 2



(b) Experiment 3

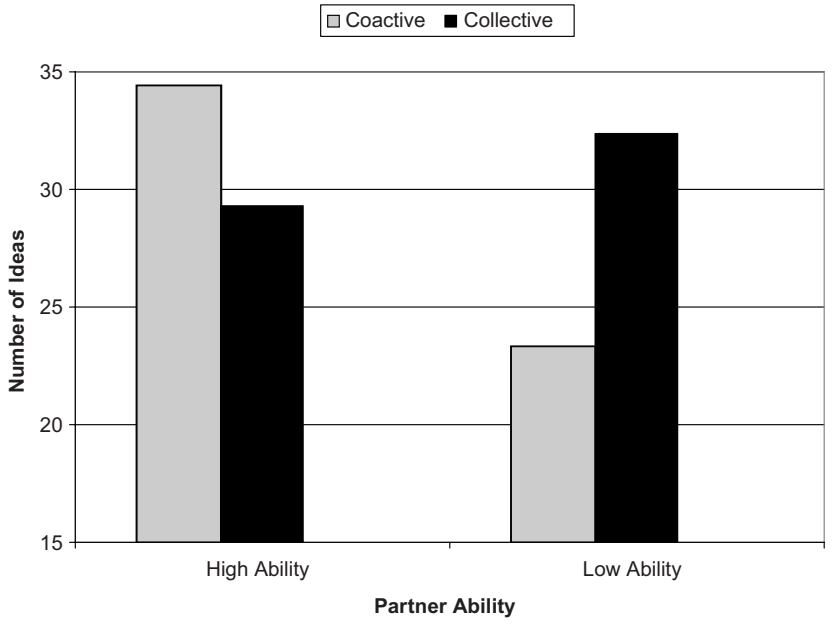


Figure 1. Results of Williams & Karau (1991), Experiments 2 & 3.

but as the social-comparison explanation implies, rather by a dramatic drop in effort in the coaction control condition (see Figure 1).[2]

One counterargument to the artifactual explanation is that a similar pattern of results has been found (Williams & Karau, 1991, Experiment 1) when the subject received no direct feedback indicating that his/her partner was incapable, but instead was chronically mistrustful of others (and thus, to '. . . expect others to loaf'). Specifically, those medium and high in level of trust showed a social loafing effect (i.e. coactive > collective), whereas those low in trust showed the opposite, social compensation effect (i.e. collective > coactive). It might be argued that absent direct feedback that one's partner was incapable, the purported demotivating effect of being compared with him/her should not occur. But these data are inconclusive. First, mistrustfulness could breed lowered expectation of the other's likely performance not just when the other is a teammate but also when s/he is a coactor. Expectations of the other's performance were collected, but analyzed in such a way that it is unclear whether or not expectations were equally low for mistrustful participants in both the collective and coaction conditions. Second, unlike the other social compensation studies, the key moderator here is a personality (rather than a situational) factor, and thus, other relevant individual differences could have been confounds (e.g. if low trusters are also relatively more concerned about a possibly invidious comparison). Thus, although these data are not irrelevant to the artifactual explanation, they do not rule it out.

The purpose of this brief paper is to report an empirical examination of the artifactual social-comparison explanation. We sought to (a) replicate the usual social compensation effect (i.e. with a coaction control), and then (b) include another isolated-individual-performer control condition in which direct and immediate social comparison with an incapable coactor was not possible. The various nuisance effects that coaction has been designed to control (e.g. distraction, modeling, mere presence) were controlled in all conditions here by preventing participants from seeing one another or their work products during the experiment.[3] Of special interest are two possibilities: (1) that the pattern of results is the same with an individual control as with coactor controls, which would contradict the artifactual explanation; and (2) that the social compensation effect is eliminated when one uses an individual control, findings that would suggest the effect is entirely due to the artifactual explanation. Of course, other patterns are also possible and of interest (e.g. the social compensation effect is attenuated but not eliminated with the individual control, suggesting that the artifact contributes to but does not wholly account for the effect; the social compensation effect is even stronger with individual controls, suggesting that the opportunity for social comparison—even with a very incapable partner—is motivating and hence, that the use of coactor controls actually underestimates the magnitude of the social compensation effect). To the degree that the artifactual social-comparison explanation is supported, the attribution veracity of the observed effect being due to a motivation gain would be undermined. As a result, we would be forced to conclude that group motivation gain effects are even rarer than previously thought.

Method

Participants and design

The participants (Ps) were 167 female undergraduate students from introductory psychology courses who received course credit for their participation. (Note that previous studies, e.g. Williams & Karau, 1991 and Hart et al., 2001, report no moderation of the social compensation effect by sex of participant.) The design was a 2 (Partner capability: High vs. Low) × 3 (Work condition: Collective vs. Coactive vs. Individual) between-Ps factorial.

Task and procedure

The task was the same idea generation exercise that most previous social compensation research has used. Ps were asked to generate as many uses for a common object (viz. knife) as they could in a 12-minute period. Instructions (taken nearly verbatim from Williams et al.,

1991) stressed quantity, not quality or creativity of uses. In order to maximize the importance or meaningfulness of the task, participants were told that research suggested that performance on the use-generating task is highly correlated with intelligence and achievement; it has been shown (Williams & Karau, 1991, Experiment 3) that social compensation requires a task for which good performance is important (otherwise, why bother to compensate for an incapable partner?). As they performed, Ps sat opposite each other at computer tables with a sight-blocking partition between them. In the Coaction and Individual conditions, there was a box on each table into which the uses were placed (one per slip of paper). In the Collective condition, there was a single, common box inside the double-walled partition which caught the slips of paper that both dyad members pushed through slits in the partition walls. These slits were so located that neither dyad member could see when a slip was being pushed through by her teammate or how many slips there were in the box. To further eliminate potential distraction, modeling, or mere presence effects, Ps wore headphones playing music as they performed the task.

Most Ps were scheduled in pairs and when both showed up, either a Collective or Coactive condition—randomly determined—was run. When only one P signed or showed up, a female confederate took on the partner role. Thus, all sessions began with two partners present—either two actual Ps or one P plus a confederate. As will become clear below, Ps in the Individual condition always had the confederate as a partner.

The procedure used in this experiment was a variation on the procedures used by Karau & Williams (Experiment 2, 1997) and Hart et al. (2001). Upon arrival, both the participants (or the actual P and the confederate, if applicable) were led to one of two computer tables in the laboratory. Each of the computer tables was equipped with a computer, a stack of exactly fifty blank slips of paper, a pen, and a pair of headphones.

After signing a consent form, Ps received a description of the uses-generation task. In the Coaction and Individual conditions, the experimenter explained that she was interested in their individual performances, not their collective performance. In the Collective condition, the Ps were told that they comprised a two-person group and that the experimenter was only interested in the total number of uses generated collectively by the group and not in individual performances. The experimenter also demonstrated how the single collection box worked and, in doing so, stressed the anonymity of individual contributions (e.g. noted that the dyad members would use pens with the same color ink).

Participants were then told that one purpose of the experiment was to examine the effects of standardized communication strategies on individual and group task performance. Purportedly employees in large corporations had been using banks of prewritten messages in order to simplify email communication, and we were interested in investigating the effect of this form of standardized communication on employee performance and satisfaction. Ps were to choose one message from each of three banks of standardized messages (three messages total) that would be sent to their coworker via computer. The first bank expressed Ps' interest in the task (seven alternative messages whose content communicated differing levels of task interest, ranging from very high to very low). The second bank expressed Ps' expected level of effort on the coming task. The third bank expressed Ps' self-perceived ability at the coming task. After choosing one message from each bank, Ps clicked a button to send their message set to their partner (in fact, these messages were never sent). Ps then shortly after received three messages, allegedly from their partner. In all conditions, the partner's first message expressed a high interest in the task. The remaining two messages manipulated the partner's task capability. In the High capability condition, the messages were 'My effort on this task will be extremely high' and 'My ability on this task is extremely high'. In the Low capability condition, the messages were 'My effort on this task will be extremely low' and 'My ability

on this task is extremely low'.[4] While the message-exchanging portion of the experiment was taking place, the experimenter left the room; moreover, after reading the messages received, Ps clicked a button that allegedly permanently erased the messages. This was done to ensure that Ps believed that the experimenter would not be able to ascertain either a P's professed level of motivation or their ability to perform the task.[5]

After a short interval which gave Ps the opportunity to read the messages sent by their partner, the experimenter re-entered the lab. In the Individual condition only, the experimenter asked the confederate whether she was, as she had indicated on her consent form, under 18 years of age. The confederate said she was. The experimenter explained that she could not participate without parental consent and that she would have to obtain such consent before she could be rescheduled. For this reason, she was excused from the experiment and left the lab. Note that this procedure insured that the true P in the Individual condition always had the *knowledge* that another ostensibly random participant was either very high or very low in task capability (as in the other two work conditions), but it removed the opportunity for a direct, immediate, and public performance comparison with that participant at the conclusion of the task. (Of course, in theory, the experimenter could later compare an Individual P's performance with that of other participants, but there was no possibility in this condition of immanent comparison which we assume to be the P's concern in our social-comparison alternative to the usual social compensation explanation.)

At this point the participant(s) were directed to put on their headphones and to follow the rest of the instructions contained on the tape. The experimenter again left the room. The instructions on the tape, which described the specific object (i.e. a knife) for which they were to generate as many uses as they could, were followed by 12 minutes of music, during which the participants worked at the task—thinking of uses for the target object, writing their answer on slips of paper, folding the slips and placing them in their collection box(es). The experimenter monitored the time using a stopwatch. At the end of the work period, Ps were given a post-experimental questionnaire that contained manipulation checks and checks of suspicion. Finally, Ps were debriefed, thanked, and dismissed.

Results

Manipulation checks

P's were asked to assess on 7-point rating scales how much effort the partner was willing to exert at the task and how much ability she had. As one would expect, the partner in the Low capability conditions was seen both as willing to exert less effort (M = 3.92) and having less ability (M = 4.22) than the partner in the High capability condition (effort M = 6.31, $t(152) = 8.36$, $p < .001$, $d = 1.36$; ability $M = 5.65$, $t(152) = 5.79$, $p < .001$, $d = .94$); reduced degrees of freedom reflect missing data due to failures to complete the questionnaire). Hence, the manipulation of partner capability was successful.

Analyses of performance

Replication of the social compensation effect? The first question we examined was whether we replicated the basic social compensation effect under the typical conditions (viz. with coactors as controls). In all conditions, the ideas generated were counted by subtracting the number of unused slips from 50 (all Ps were given 50 slips of paper before beginning the idea-generation task). The key contrast tests an interaction effect—is the usual social loafing effect observed when one believes that one has a very capable partner (i.e. Collective < Coactive) reversed when one believes that one has a very incapable partner (i.e. Coactive < Collective)? A test of this contrast was statistically significant ($F(1,161) = 4.02$, $p < .05$, d = .32), and as one can see in Figure 2 (white and black bars), the same pattern is observed as in the usual social compensation study (see Figure 1). In the present study, the social loafing effect (High capability partner) was significant ($t(161) = 1.96$, $p = .05$, $d = .31$), whereas the social compensation contrast (Low capability

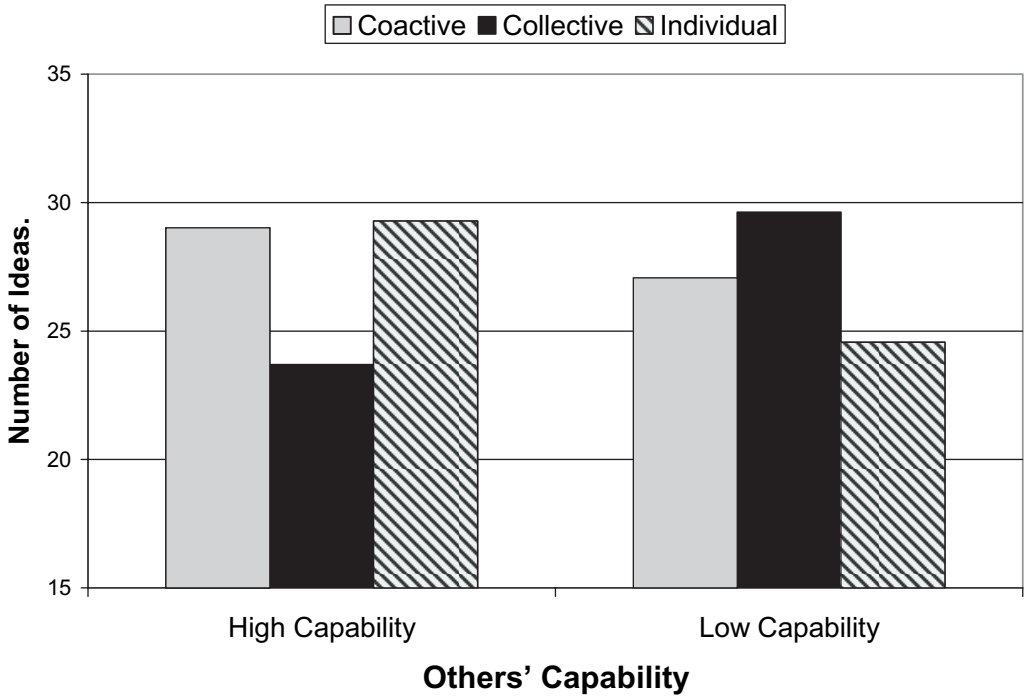


Figure 2. Performance means.
Note: Cell sample sizes for the six conditions above—from left to right—are 25, 35, 26, 25, 30, & 26.

partner) was not ($t(161) = .89$, *ns*). Keep in mind that the same opportunity for social loafing (low identifiability of individual contributions to the collective product) is available in both collective conditions, so the important result is that the social loafing effect is significantly moderated (i.e. the interaction effect), not that it is significantly reversed (i.e. the work condition simple effect with the Low capability partner; cf. Note 2).[6]

Tests of the social comparison explanation There are several ways in which to test this explanation. The most direct is to see if the level of effort in the Low capability/Individual control condition (where no direct social comparison is possible) is any different than in the Low capability/Coaction control condition (where such comparison is possible). The test of this contrast was not significant ($t(161) = .87$, *ns*). As one can see on the right side of Figure 2, the level of performance in the Individual control condition (diagonal stripe bar) was actually lower than in the Coaction control (white bar), exactly the opposite to the pattern predicted by the social-comparison explanation. Our results suggest that had one attempted to demonstrate the social compensation effect using Individual controls rather than Coactive controls, the key interaction effect would have been stronger (here, $t(161) = 2.52$, $p < .015$, Cohen's $d = .40$ compared to $d = .32$ with coactor controls), not weaker or nonsignificant, as the social compensation explanation would predict.

Although it was not of primary interest in this study, it is also worthwhile to examine the Individual control for the High capability condition. If having someone to compare oneself to was (if anything) motivating, even if that someone was likely to be a poor performer, it

could be that performance would also be lower in the Individual/High capability condition than the Coaction/High capability condition. But as one can see in Figure 2, this was not the case; there was no difference between these two control conditions ($t(161) = .07$, *ns*). It is at least possible, though, that ceiling effects may have prevented such an effect from emerging here.

Discussion

The use of coaction controls in prior social compensation research has left open the possibility that the effect might artifactually have resulted from a confound between work condition (Coaction vs. Collective) and the opportunity to make performance comparisons. If making such a comparison with a very incapable other were demotivating, then it could have been the case that the social compensation effect had little or nothing to do with the need to compensate for an incapable partner, but stemmed instead from such a social comparison process. In the present study, we directly tested this alternative, artifactual explanation for the social compensation effect. Our data strongly contradict it. Removing the opportunity for immediate public comparison with an incapable other did not, as this explanation would predict, increase effort, but actually tended (nonsignificantly) to decrease effort. It appears that the opportunity for comparison is (if anything) motivating, even when the person with whom one will compare is very likely to perform poorly. Thus, far from being the source of an artifactual explanation, the use of the coaction control in prior social compensation has tended to work against demonstrating the effect, and to underestimating its strength.

These findings have several useful implications. First, and most obviously, they bolster the validity of the original interpretation of the best documented of the few extant group motivation gain effects, the social compensation effect. That interpretation suggested that the observed motivation gain was attributable to a particular pattern of outcome interpendence—where one's prospects of obtaining a highly valued outcome (e.g. a group success) depended upon compensating for the perceived incapacities of other group members by working extra hard. However, quite apart from the alternative social-comparison explanation examined here, there were theoretical and empirical reasons to doubt that interpretation. For example, when the rewards of group success are shared equally by all group members (as they were here and in prior social compensation research), socially compensating for others can result in doing more than an equal or fair share of the group's work. Such an inequitable arrangement has been shown to lead to motivation losses under some conditions—at times, group members will reduce their own efforts (and consequently, the chances of group success) rather than do more than what they see as a fair share of the group's work. Williams and Karau (1991) suggest that this aversion to 'playing the sucker' can be overcome *if* one places a sufficiently high value on group success; they even demonstrate (in their Experiment 3) that the social compensation effect is eliminated if the group's task is seen as unimportant to its members. By disconfirming a plausible alternative to the social compensation explanation, our results lend credibility to their suggestion. Apparently, group members who are exerting supernormal levels of effort will put up with doing an inequitable share of the group's work if group success is sufficiently valued.

Our study also touches on a fundamental issue in the study of group motivation—what is the proper non-group baseline against which to detect group motivation gains or losses? Some (e.g. Karau & Williams, 1993; Williams & Karau, 1991) have argued that coactors provide the most appropriate baseline. Others (e.g. Kerr, 1983; Markus, 2001) have argued that isolated individuals represent better baselines. As the present paper suggests, this controversy is partially a matter of how best to avoid uninteresting confounds (e.g. to control for 'mere presence' effects when probing for group motivation gains or losses). But it is also a matter of a fundamental conceptual issue—viz. 'what is and is not a group'? There is no universally

agreed upon answer to this question; however, many if not most scholars include shared goals and some degree of mutual interdependence as necessary properties of a group, especially a performance group (cf. Forsyth, 1999). Our own preference for isolated individuals as the best non-group baseline stems from the fact that many typical elements of the coaction setting (such as the mere presence of others, the possibility of distraction, the possibility of attentional conflict, the opportunity to observe others perform, the opportunity to compare levels of performance), while common elements of performance groups, are not defining necessary conditions. Thus, motivational gains or losses which ultimately stem from such elements could well be viewed as not being essentially group phenomena, but instead being phenomena that can arise in certain group settings but not others. On the other hand, one could argue (as Latané et al., 1979, did implicitly in their seminal social loafing paper) that if certain elements (e.g. individual member contributions being unidentifiable from a group product) are routine features of many performance groups, then the motivational effects of those elements could fairly be classified as group effects (even if those same elements are not necessary for all or even most performance groups).

Clearly, this is partially a semantic issue, but it is more than that. The answer to many important questions—e.g. is a particular effect a genuine group motivation loss or gain? where should one look for such effects? when it comes to application, are groups or teams superior or inferior to individuals as performers?—ultimately depends on just what processes we classify as group processes. And whenever individual performance differs in individual and coactive performance settings, the answers we come up with depend on just which baseline we choose. Consider, for example, the following:

- Another recently documented (purported) group motivation gain is the *Köhler motivation gain effect* (Hertel et al., 2000). Here, it is the least capable group member who must work extra hard because the task demands are conjunctive (Steiner, 1972)—i.e. the group's performance depends on the least capable member's performance. The original (Köhler, 1926) and subsequent (Hertel et al., 2000; Messé, Hertel, Kerr, Lount, & Park, 2002; Stroebe et al., 1996) empirical demonstrations of the effect have all used isolated individual performers as their non-group baselines. But some of the proposed explanations of this effect do not require what we have advanced as conditions necessary for groups. For example, Stroebe et al.'s (1996) goal setting explanation only requires the availability of a more capable performer, not the existence of a more capable group member working toward a common goal with some interdependence (e.g. a conjunctive group task). If such explanations are valid, then we might expect the Köhler effect to occur in a number of coaction settings. Although an initial attempt to test this possibility offered little support to the goal setting explanation (see Hertel et al., 2000, Experiment 2), subsequent tests suggest that it does provide a partial explanation of the effect (Kerr, Seok, Poulsen, Harris, & Messé, 2006). The important point is that we could have come to different conclusions about whether the Köhler effect was a genuine group motivation gain, how large it was, and why it occurred had we chosen isolated individuals or coactors as our baseline of non-group performance.
- Members of mixed-sex dyads have been shown to work harder than either members of same-sex dyads or isolated individual controls, an effect that has been interpreted as a group motivation gain (Kerr & Sullaway, 1983; Kerr & MacCoun, 1984). But if this effect is the result of concerns about violating traditional sex role expectations, then it may not require collaborative group action—mixed-sex sets of coactors could show a similar effect.
- Other work has shown that members of groups who can engage in intra- (Stroebe et al., 1996) or intergroup (Erev et al., 1993; Worchel, Rothgerber, Day, Hart, & Butemeyer, 1998) competition work harder than

individual controls. But certain explanations of this effect suggest that coaction settings which offer similar opportunities for competition might produce quite similar effects.
- Markus (2001) has shown that some social loafing effects might be attributable to the differential effects of individual vs. coaction work conditions on intrinsic task motivation.

We suspect that such issues cannot and should not be resolved by fiat—by simply declaring one particular non-group baseline as the 'gold standard' for group motivation effects. Rather, we would like to suggest that the two most popular non-group baselines—isolated individuals and coacting individuals—both provide informative baselines for analyzing psychological processes of relevance to how hard people work in groups. In the present study we were able to narrow the range of plausible explanations for the social compensation effect by including both baselines. Future work on group motivation could and should be likewise enriched by including multiple baselines. And even when this is not possible, scholars would be well advised to consider the theoretical implications of examining alternatives to the baselines they do employ.

Notes

1. By effort-sensitive, we mean a task for which performance is entirely or almost entirely a function of effort (rather than other factors, like ability, insight, luck, etc.) and for which there is a monotonic relationship between performance and effort.
2. It must be conceded that the pattern in these data—a relatively low level of performance of the coactor-low capability partner, compared to coactor-high capability partner—could result from processes other than the social comparison explanation proposed here. For example, it is conceivable that one tends to match a coworker's expected level of performance, regardless of whether performance levels will subsequently be compared. This would result in generally lower performance in the low partner capability conditions (which could then be further moderated by a social compensation effect, resulting in a relatively higher level of performance in the collective-incapable partner condition). Thus, we agree with Williams and Karau's suggestion that the result of primary interest is a particular interaction effect, viz. the reversal—when one has an incapable partner—of the usual social loafing effect (observed here when one has a capable partner, or as in the typical social loafing study, when one knows nothing about others' capabilities).
3. It should also be noted that the use of a coaction control condition is not always simply a matter of controlling nuisance variables. A number of tests of substantive theories of social facilitation and social loafing have suggested that working alone is psychologically different than working alongside others (e.g. Cottrell, 1972; Harkins, 1987, 2001; Sanna, 1992).
4. Note that we jointly varied ability and motivation feedback to create the High and Low partner-capability conditions. In an experiment which orthogonally manipulated ability and motivation feedback, Hart et al., (2001) found that the standard social compensation effect only occurred when both were low. This suggests that in earlier studies (e.g. Williams & Karau, 1991) where only one of these two bases for capability had been manipulated, Ps presumed that the other varied in kind (e.g. a poorly motivated partner was also likely to have low ability).
5. Another possible boundary condition on social compensation is that those who evaluate group task performance (e.g. a supervisor; the experimenter) should *not* know that there is an incapable member in the group. Otherwise, such an evaluator might attribute a poor group performance not to the team as a whole (including the more capable member) but just to the incapable member's probable poor performance. The more capable member should not be as likely to try to compensate under such conditions.
6. We have also argued (see Note 2) that neither the significance of the simple partner-capability main effects nor or their relative (absolute) magnitudes are of special interest here. However, for the interested reader, the simple partner-capability main effect under coactive conditions was not significant ($t(161) = .73$, *ns*), while the corresponding simple main effect under collective conditions was ($t(161) = -2.01$, $p < .05$, $d = .32$).

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