Inept, Misguided, or Caught Between Competing Paradigms? A Reply to Locke and Latham and to Wells

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The authors of these two commentaries represent different research philosophies and traditions, yet their remarks share some ironic characteristics. For one, both point to the same basic methodological shortcoming as a key problem of our study—that we did not obtain enough direct information from the subjects about what they were thinking and experiencing. According to Locke and Latham, we did not find out about the goals that subjects actually set. According to Wells, we did not involve the subjects enough in the interpretation of the phenomena under study. It is remarkable that two apparently incompatible research traditions would yield such similar advice. Finding out as much as possible about research subjects' experience as part of one's data collection is indeed wise counsel, irrespective of one's specific research creed.

However, two other characteristics that these commentaries share left us more concerned, and they are the focus of this response. First, while criticizing our methodology, neither critique offers much comment on the study's research questions. Does it make sense to try to set interpersonal process goals? Is there any contribution to the small group literature in bringing together the traditions of goal setting and process

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consultation? Would it be worthwhile to conduct a follow-up study, incorporating advice from these two commentaries? Was this study a dumb idea? The heart of Wells's critique is that an experimental laboratory study, albeit well-conducted, was inappropriate for studying these complex social phenomena. Locke and Latham, on the other hand, do not question the choice of method but rather argue that the study was not well-conducted. Thus the study's findings were weak, due either to inept application of a perfectly good paradigm or to misguided choice, though masterful execution, of this paradigm.

The second shared characteristic we want to note is that both commentaries were written from and reinforce a rigid point of view of the research enterprise. These authors present strong opinions about our study's methods and results, without saying much about what they think of the study's objectives. Nevertheless, their comments rest on assumptions about those objectives. Locke and Latham's most serious criticisms are that we did not follow to the letter the methods of conducting goal-setting studies set forth in their own influential work (Locke & Latham, 1990). Wells is equally narrow in his exhortation to abandon the positivist paradigm for the emerging naturalistic one, but does the value of naturalistic observation mean that "positivism is passé" (Lincoln & Guba, 1985, p. 24)? Is tolerance for multiple paradigms and methods in social science research also out of vogue?

Numerous writers have noted that no single study can provide the definitive answer to a particular research question or set of questions (e.g., Babbie, 1983; Evered & Louis, 1981; McGrath, Martin, & Kulka, 1984), and that there are trade-offs associated with any research method (McGrath et al., 1984) or paradigm (Burrell & Morgan, 1982). Some methods are better than others for providing certain kinds of information bearing on a specific question. Whether or not we chose and applied our methods appropriately in the study discussed here depends on our study objectives. Therefore, we respond to the points made by Wells and by Locke and Latham by focusing on the objectives we set for our study. We hope to show that their commentaries, like our study, reflect the authors' conceptualization of the relationship between theory, paradigm, and method.

OUR OBJECTIVES—DID WE DO THE RIGHT THING?

There were really two parts to the theoretical model underlying the study, as reflected in Propositions 1 and 1a and Propositions 2 through 4. The first part of the model predicted changes in specific interpersonal behaviors in work groups as a function of setting goals and providing feedback about those behaviors (Propositions 1 and 1a). Our objective was to determine if goals for interpersonal behavior patterns for groups could be set and if groups would strive to meet those goals. We intended goal-setting theory to apply only to this part of the model—demonstrating a change in interpersonal behavior as a function of timely, specific knowledge of results plus specific goals.

To operationalize interpersonal goals, we used SYMLOG (Bales & Cohen, 1979), which has not been used in this way before. This study provided an opportunity to connect goal setting, based largely on a positivist research tradition, with SYMLOG, which comes from a clinical tradition (Koenigs & Cowen, 1988; Kutner & Kirsch, 1988).

What did we find? For dominant-submissive behavior, subjects perceived the goals we set as attainable and made efforts to reach them. For friendly-unfriendly behavior, all subjects met the goals we set during their first task and thus had no need to change behaviors. Finally for task-socioemotional behaviors, subjects perceived the goals we set as unattainable and consciously chose not to strive for them, and we thus saw no changes in this type of behavior. In our view, this set of findings met our objectives for the first part of the study's theoretical model and are consistent with goal-setting theory. They suggested that by setting *attainable* goals, patterns of interpersonal behavior in groups could be changed.

The reason why we wanted to show that interpersonal behavior patterns could be manipulated brings us to the second part of the study's theoretical model. As noted in our article, an unsolved puzzle in the process intervention literature is the failure to demonstrate in a controlled study that changes in interpersonal group process cause changes in task performance, despite theoretical and intuitive reasons to expect these changes to occur. In reviewing the literature, we noted that there was inconsistent evidence that the process interventions that had been tried had succeeded in actually causing changes in process. We reasoned that in order to determine how and if interpersonal process is related to task performance, a reliable way to manipulate process was needed. We noted the success of goal setting in changing task performance and thought that method might also succeed in actually changing interpersonal behavior patterns. We believed that once a way of controlling interpersonal process was found, then the links between process and behavior could be explored with some confidence in future studies.

The second part of our theoretical model hence related specific group process variables to task performance (Propositions 2 through 4). Through goal setting we would change interpersonal behavior patterns (group process), which then would cause changes in task performance quality. This study, like its predecessors, also failed to find a relationship between the particular process norms we hypothesized and performance on the particular tasks we used.

This second part of the study's theoretical model points to a mistake we now recognize in retrospect. That mistake was neither choosing the wrong research method nor misapplying that method, as the two critiques here suggest. Rather, our mistake was failing to make clear—to ourselves or to readers—the priorities among the multiple objectives of the study. The study's contribution, and our primary interest, was the first part of the theoretical model—the application of goal setting to changing interpersonal behavior patterns in task-oriented groups. Although our study was motivated by the absence of evidence for a link between process and task performance, our immediate interest was in manipulating process. We may have been mistaken in even trying to address both parts of the model in the same study. But we were ambitious.

So, did we do the right thing? Did we choose an appropriate method to reach our objectives? Did we apply this method well? Did we choose a worthwhile problem? We believe we can honestly answer "yes" to these questions while still acknowledging the study's limitations. We would now like to justify this answer by responding to the specific issues raised by Wells and by Locke and Latham.

MISGUIDED OR INEPT?

We first respond specifically to Wells, who has argued that our method choice was misguided. Setting goals for patterns of interpersonal behaviors in task groups is something that has not been done in other studies, as far as we are aware. We know that SYMLOG has not been used in this way. Hence we wanted to know if it was even possible to change interpersonal behavior patterns for groups by setting goals. Because we wanted to isolate this specific phenomenon (i.e., change in interpersonal behavior), a controlled laboratory study was one appropriate tool. Perhaps Wells would argue that we should not have wanted to isolate a particular relationship.

We agree that complex phenomena are more than simply the sum of their parts and that they cannot be reduced to cause-effect chains. We recognize that phenomena isolated in the lab look very different or even disappear in natural settings and that some phenomena cannot be meaningfully studied at all in a laboratory. The lab setting is well-suited, however, for demonstrating whether a relationship between two variables possibly exists (Mook, 1983, cited in Locke, 1986, p. 4). This is precisely what we were trying to do with our study.

We reject the notion that laboratory studies are inappropriate for studying group phenomena (or any complex social phenomena as Wells implies). We certainly do agree that there is much one cannot learn about groups from lab studies, but it is going too far to suggest that nothing of value can be gained.

The explicit design and conduct of the study were grounded in a positivist, deductive system of logic, but some interpretive, inductive methods were also applied as we examined our emerging data. *We* were the experimenters. *We* talked to every subject after each session. *We* were the ones who examined the videotapes of the subjects' discussions (and observed the groups while they were being videotaped). These observations hence made a great many impressions on us, and these impressions were brought to bear as we interpreted our findings. Indeed, our observations of what subjects actually did during the sessions guided some of our quantitative data analysis decisions, especially our observations of their discussions about the feedback. The result, we believe, is a fairly clear picture of what happened with our subjects with respect to our specific research questions. We nevertheless accept Wells's suggestion that the further addition of naturalistic methods, such as more extensive debriefing of our subjects, would have strengthened our ability to interpret the data.

Were we inept in our conduct of the study? We say no, while admitting that the study has limitations. To read Locke and Latham's critique is to get the impression that we completely missed the essence of goal setting. We argue that we followed well the spirit of the goal-setting method, although we may not have always used specific methods recommended by Locke and Latham.

The first three points in their commentary, their most serious criticisms, say that "no attempt was made" to find out the actual goals that subjects set, their commitment to those goals or their perceived self-efficacy in reaching those goals. These criticisms raise the question of what constitutes a valid attempt to get information from subjects. What we actually did in this study was ask each group of subjects to decide together, through discussion, what changes in behavior they would make as a group. We listened to these discussions and later examined videotapes of them. We requested the subjects to decide together what, if any, changes in behavior they would make in response to the feedback they received. The subjects responded by stating their plans quite explicitly—which behaviors they would try to change and in which directions and which behaviors they would not try to change.

Locke and Latham's critique suggests that we should have instead "measured individual goals explicitly" (pp. 42-43). We did not administer an instrument that explicitly asked subjects their goals for each of the three SYMLOG dimensions, and because we did not do this, Locke and Latham appear to believe that we did not have this information.

Is one to conclude, then, that the only valid means to obtain information about subjects' behavioral intentions is to ask them directly? We would have to reject this conclusion. The contrast between what Locke and Latham say we should have done and what we actually did is analogous to the difference between structured and open-ended questions. Structured questions (e.g., "What is your goal for changing your dominance behavior") give no more a guarantee of "good" information than do open-ended questions (e.g., "What, if any, behavior changes will you make?"). Locke and Latham's criticism that no attempt was made to find out this information represents a rigid adherence to a set of practices for doing research under the rubric of "goal setting."

In their book, Locke and Latham (1990) themselves acknowledge that even direct queries of goal commitment do not ensure that the researcher really "knows" a subject's true commitment: "The use of direct questions assumes that subjects can introspect well enough to detect varying degrees of commitment, and that the scales used allow people to indicate those degrees" (p. 126). Further, they endorse inferring goal commitment from performance, which is in essence what we did in our study. The behaviors our subjects said they would change did change; the behaviors they said they would not change did not change. According to Locke and Latham (1990), "Commitment would be inferable from goal choice, whereas lack of commitment would be inferable from goal rejection" (p. 127). In our study, we heard subject groups choose certain goals and reject others, and then we saw the corresponding behavior changes. We contend that within Locke and Latham's own framework the design of our study provided information on goal commitment.

We believe we can rule out the possibility that the no-feedback groups may have also set goals. Individual group members may have privately set goals, but groups would have to negotiate goals jointly through discussion, as did the feedback groups. The groups in the no-feedback condition did not do this, as we know from our videotapes.

It is simplistic to conclude that the feedback we provided affected only the individuals who were at the extremes of the behavior ranges. Those individuals' positions defined the boundaries of the ranges, but each group together had to decide and agree on the response to the feedback. Although our analysis specifically tested for behavior changes by the most or least dominant members, other group members also changed their behavior to help facilitate the behavior changes of those at the extremes. For example, for submissive members to become more dominant, everyone (not just the most dominant members) had to allow them room to talk more. The behavior change in the individuals at the extremes of the dimensions was the most salient, but it represented a strategy agreed to by their entire groups.

We feel it is important to refute these criticisms because our application of goal setting is crucial to what we see as this study's contribution. We followed the essential goal-setting procedure: We set goals; we communicated the goals to the subjects and gave them feedback on their progress toward the goals; we measured behavior against those goals; we obtained information on the actual goals they set; and finally, we measured behavior change. Locke and Latham can quibble with the specific methods we used to obtain various pieces of this information, but they cannot glibly say we made no attempt to obtain it.

We agree with the gist of their last three points. The relationship between the "ideal" behavior ranges we set and performance for the particular tasks used in the study was the part of the theoretical model that was not fully developed; it held a lower priority for us. We agree that the effectiveness of the pattern of interpersonal behavior is a function of the nature of the task and the characteristics of the group, among other things. The same would be true of group member satisfaction. We reiterate, however, that our goal-setting efforts were not aimed at changing task performance directly. We provided no task performance goals, as we were interested only in task performance as a function of interpersonal process.

Finally, we do know the Kolb, Winter, and Berlew (1968) and Kolb and Boyatzis (1970) studies, but we did not cite them for the following reasons. First, both were studies of *t* groups, where the task itself is personal behavior change. Our study's focus was on work groups and the relationship between interpersonal behavior and performance. In our article, we did not cite any studies of personal development groups. Second, those researchers studied individual change, although in a group setting. Our study's focus, again, was on goals set for groups and on group-level change.

READING BETWEEN THE LINES— LESSONS FROM WHAT THEY DID NOT SAY

The formats chosen by the commentators say as much about the philosophical traditions they embrace as do the contents of their critiques. Locke and Latham, representing a positivist research tradition, have written a comment that searches for clear cause-effect chains. There are specific and clear reasons why our study "failed" (i.e., not measuring the proper things in the proper way), and one presumes that changing our methods would change (improve?) our results. The format of their critique—a rank-ordered list of problems with the study—reflects the reductionism associated with positivism. The causes of social phenomena can be isolated and identified; so too can the causes of our study's results. These authors' own priorities show in their ranking of methodological issues as more serious than theoretical ones. Further, they make no comment whatsoever about the worth of the study's research questions.

Locke and Latham's comments show why the likes of Lincoln and Guba (1985), Berg and Smith (1985), and Wells in his critique here reject the positivist research tradition. Their remarks represent attention to research methods for their own sake. One message that could be taken from their comment is that goal setting is appropriate for any type of behavior, in any context, as long as it is applied in the right way. In addition to their advice on how we could improve our application of goal setting, their opinion on how we could improve our *thinking* about the questions we were researching would have expanded the dialogue here.

Wells's comments likewise reflect his commitment to a particular research tradition—naturalistic, clinical methods. He explores the possible impact of the entire social context of our research on the findings. Rather than presenting firm conclusions about the causes of our study's results, he suggests what can be learned by reflecting on the social psychology of the study.

The "real action," according to Wells, lies not in the experimental conditions as we set them up but in the complex interplay between experimenter and subjects and among the subjects themselves, all in response to the experimental setting. Wells does not appear to believe it is a legitimate quest (or a possible one) to isolate a particular phenomenon from its rich context. His argument appears to be that trying to study phenomena in isolation will yield poorer rather than better understanding.

We wonder, however, what our study would have added to the literature had we followed a strictly clinical approach. There currently exists much literature on process intervention that is based on a clinical tradition. Those studies have not yielded any better understanding of how process consultation works than have the laboratory studies on this topic. We believe that very little understanding will be reached about these complex issues unless these two research traditions begin to inform each other. The question remains, What methods of intervention can increase group process and performance effectiveness? Can groups set goals for improving their process? Wells's opinions here have challenged our methods but not our questions.

Where will we take this research from here? As we suggested at the beginning of this reply, our choice of research design reflects our own training and philosophy of science. All three of us were trained in programs that emphasized the positivist research tradition, and we conducted this study at an institution that reinforces that tradition. Therefore, it is doubtful that we will abandon positivism in our work on this question or on other questions. The three of us, however, have also had the fortunate opportunity to learn about naturalistic and clinical methods, both during and subsequent to our graduate training. As a result, we have tried to model the tolerance for multiple paradigms we have advocated here by using varied research approaches in our own work, as dictated by our research questions (e.g., Choi & Liker, in press; Liker, Nagamachi, & Lifshitz, 1990; Lobel, 1988, 1991; McLeod, 1991; Novelli, Beard, & McLeod, 1992).

Although different in focus, the two commentaries on our work represent dogmatic views of how research should be conducted. The dogma advocated by Locke and

Latham—that we did not use proper technique—does not allow room for exploring the range of goal-setting theory's applicability. Wells's dogma—"positivism is passé"—equally cuts off exploring the differential contributions of various research traditions. We too would like to see more tolerance in journals for the kind of research that Wells recommends, but we would find the sole reliance on naturalistic clinical methods to be as limited as exclusive reliance on laboratory, quantitative methods. We conclude by echoing Wells's sentiment that hopefully "future generations [indeed, the present generation!] of scholars will be less limited in their conceptions of social science" (p. 53).

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