Journal of Management OnlineFirst, published on July 10, 2008 as doi:10.1177/0149206308321553

Augmenting Means Efficacy to Boost Performance: Two Field Experiments†

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Internal and external sources of efficacy beliefs are distinguished. "Means efficacy," a particular source of external efficacy, is defined as belief in the utility of the tools available for task performance. The authors tested the hypothesis that raising means efficacy boosts performance. In two field experiments, experimental participants were told they got a new computerized system proven to be the best of its kind; controls got the same system with no means-efficacy treatment. In both experiments, means efficacy among experimental participants increased, and they outperformed the controls. A broadened perspective on the efficacy–beliefs construct is elaborated, and practical applications are proposed.

Keywords: *means efficacy; external efficacy; expectancy; motivation; performance; field experiments*

Well, well, well, well, now. I met with Stalin this morning. He said he had some more questions to present and I told him to fire away, and he did, too, and it was dynamite. But now I have some dynamite, too. This gives me a completely new feeling of confidence.

Harry S. Truman in Potsdam, on being informed of the first successful atomic detonation in New Mexico on July 16, 1945

Journal of Management, Vol. XX No. X, Month XXXX xx-xx
DOI: 10.1177/0149206308321553

Defining self-efficacy as “a judgment of one’s ability to organize and execute given types of performances” (p. 21), Bandura (1997) elaborated the theory and marshaled considerable evidence for self-efficacy’s pivotal role in motivating human endeavor. Self-efficacy has come to dominate the performance management literature. Stajkovic and Luthans’s (1998) meta-analysis confirmed that there is a strong, positive relationship between self-efficacy and performance. Furthermore, field experimentation has confirmed the positive causal effect of augmenting self-efficacy on job performance (e.g., Eden & Kinnar, 1991; Eden & Zuk, 1995; Gist, Schwoerer, & Rosen, 1989; Litt, 1988; Weinberg, Gould, Yukelson, & Jackson, 1981). The practical implication is that enhancing self-efficacy can boost performance.

Self-efficacy is only half of the efficacy story. The efficacy construct can be conceptualized more broadly. The Internal–External Efficacy model (Eden, 1996, 2001) is an expanded theory of efficacy beliefs. This expansion extends the conceptualization of efficacy beliefs to encompass potent sources of work motivation that have been de-emphasized—nay, largely overlooked—by self-efficacy theory. Focusing on means efficacy, the present article reports field-experimental tests of part of the expanded efficacy conceptualization to accentuate the unique role of external sources of efficacy beliefs in determining performance.

Internal and External Efficacy

Conceptualization of External Efficacy and Means Efficacy

According to the Internal–External Efficacy model (Eden, 2001), overall efficacy is one’s subjective assessment of all the available resources that may be applied toward performing a job successfully. This overall assessment takes into account resources that are external to the individual as well as resources that are internal to the individual. The internal resources include the skill, talent, knowledge, willpower, endurance, intelligence, resourcefulness, and any other traits that the individual may deem useful for successful performance. Some of these traits are specific to a particular domain, and some are general. However, these internal sources of efficacy beliefs do not compose the totality of overall efficacy; rather, they are complemented by one’s subjective assessments of any task-relevant *external* resources that may be used to facilitate performance.

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† This article is based on the theses submitted by Rachel Flumin-Granat and Keren Stirin in partial fulfillment of the requirements for their master’s degrees in organizational behavior at Tel Aviv University’s Faculty of Management. Dov Eden and Yoav Ganzach served as thesis advisors. The authors presented preliminary reports on Experiment 1 at the meeting of the Society for Industrial and Organizational Psychology in New Orleans, 2000, and on Experiment 2 at the meeting of the Society for Industrial and Organizational Psychology in Dallas, 2006. Neither experiment has been published. The Israel Institute for Business Research and the Lilly and Alejandro Saltiel Chair in Corporate Leadership and Social Responsibility supported Dov Eden’s work on Experiment 1. Israel Science Foundation Grant 12014101 to Dov Eden and Yoav Ganzach and grants from the Israel Institute for Business Research supported Experiment 2. We thank Ilan Rosiner for help in data analysis.

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One important external source of efficacy beliefs, labeled *means efficacy*, is defined as the individual's belief in the utility of the tools available for performing the job (Eden, 1996, 2001). Individuals ascribe utility value to whatever means, or tools, may facilitate—or hamper—their performance. The means exist independently of the individual's ability, and belief in the means is different from belief in self. *It is hypothesized that the subjective utility of external means arouses expectations for success and motivates performance* as does internal efficacy; just as a high level of internal efficacy produces high performance expectations, spurs effort, and thereby enhances performance, so a high level of means efficacy makes one expect success and impels one to use the valued means energetically, culminating in improved performance: "Wow! With a mitt like this, I'll catch any ball coming at me!" Conversely, disbelief in the utility of an indispensable tool can be as incapacitating as a low estimate of one's own ability to use a crucial skill: "Phooey! How will I get my job done with this outmoded computer?" Believing that a relevant tool is useful motivates effort to use it for the best performance results, whereas disbelief in the tool is demotivating and deleterious to performance. Thus, external efficacy, like internal efficacy, is a double-edged sword; when it is high it augments motivation and performance, but when it is low it is counterproductive. Maximal productivity results when workers believe themselves to be highly skilled at what they do (i.e., high internal efficacy) and believe they have at their disposal the best tools for doing it (i.e., high means efficacy).

Some means are helpful in promoting efficiency and effectiveness and are viewed as assets; having them in hand arouses expectations for success. Other means are inadequate, unreliable, and vexing to the user. Such means are impediments to excellence, are viewed as liabilities rather than assets, and have a demotivating effect on users because they reduce their overall sense of efficacy and decrease their expectations for success. Through these high or low expectations, belief in the means motivates greater effort and augments performance, whereas disbelief in the means reduces motivation and culminates in poor performance.

Expectancy: The Link Between Efficacy and Performance

It is expectations for successful or unsuccessful performance that link efficacy, whether internal or external, to performance. Expectancy theory (Vroom, 1964; also see Latham, 2007) posits that high expectations for successful performance motivate exertion of effort and, ceteris paribus, culminate in improved performance. We invoke expectancy theory to explain why raising means efficacy improves performance: It does so by enhancing performance expectations, and it can do so without changing instrumentality or valence. Thus, boosting means efficacy elevates performance expectations, which motivate exertion of greater effort, culminating in fulfillment of those high expectations by achieving good performance. The explanatory mechanism is explicated in the well-tested expectancy theory, which requires no further confirmation.

The Subjective Nature of Efficacy Beliefs

Just as internal efficacy is not one's actual capacity but rather one's belief in one's capacity, so too means efficacy is not a tool's *actual* utility but rather one's *belief* in the tool's utility. Just as one may over- or underestimate one's ability, so one may over- or underestimate the utility of any external means. Thus, efficacy beliefs about both internal and external sources may be higher or lower than would be corroborated by objective appraisal. This implies that such beliefs are amenable to external influence by mentors, managers, consultants, or experimenters.

The epigraph at the beginning of this article exemplifies means efficacy. Nothing about any of President Truman's internal resources changed at that moment, but knowledge that the atomic bomb was now in his arsenal buoyed his sense of what he might accomplish in his negotiations with Stalin. The new weapon gave him "a completely new feeling of confidence." Such is the empowering nature of means efficacy.

Conceptual Distinction Between External and Internal Efficacy

Conceptually, the means is distinct from the self. However, the conventional definition of self-efficacy does not distinguish between internal and external sources of efficacy beliefs. Bandura's (1997) definition of self-efficacy can be construed as encompassing both internal and external efficacy. However, without conceptualizing (and measuring) them separately, we do not consider how beliefs in internal and external resources may relate differentially to other variables, particularly performance. Furthermore, even if Bandura intended his conceptualization of self-efficacy to encompass external as well as internal resources, researchers have gravitated toward treating self-efficacy mostly, though not exclusively, as a self-referent construct embedded in some version of self-regulation theory. In contrast, the present approach implies that, holding constant one's beliefs about one's own abilities, strengthening belief in the efficacy of the tools available will increase confidence in successful performance. Disbelief in the efficacy of the tools is frustrating, demoralizing, and ultimately counterproductive, as inadequate means can threaten to neutralize one's abundant internal resources.

Bandura's (1997) focus is explicitly internal: "*Perceived self-efficacy refers to belief in one's capabilities to organize and execute the courses of action required to produce given attainments*" (p. 3). Bandura (1997, chap. 3) detailed the sources of self-efficacy: enactive mastery experiences, vicarious experiences, verbal persuasion and other social influences, and physiological and affective states that inform individuals' judgments about their capabilities. He did not cite the available means or any other external resources as sources of self-efficacy. Verbal persuasion is no exception because it is interpersonal influence that is geared toward getting individuals to believe more in themselves, not in the means at their disposal; the *source* is external, but the resource is *internal*. Thus, self-efficacy and work motivation theories have placed nearly all the emphasis on internal efficacy and have largely ignored beliefs about the means available. Nevertheless, because self-efficacy can be thought of as a product of belief in both internal *and* external resources, we here specify *internal* and *external* to emphasize the distinction between them and refrain from using the term *self-efficacy* because its referents may—or may not—include external resources. In the present usage, the referent of internal efficacy is the self, whereas the referent of means efficacy is the tool or tools. Thus, internal efficacy may include an individual's belief about his or her *ability to use* a particular tool, whereas means efficacy is that individual's belief about *the tool itself*, regardless of any self-estimate about his or her ability to use it.

Operational Distinction Between External and Internal Efficacy

Following from his conceptualization, Bandura (1997) explicitly emphasizes an internal focus to operationalizing self-efficacy: “The item content of self-efficacy scales must represent *beliefs about personal abilities* [italics added] to produce specific levels of performance and must not include other characteristics” (p. 45). Bandura’s emphasis on internal resources is clear. However, he operationalizes self-efficacy in terms of the subjective probability, or expectation, of performing a specific task at different levels (Bandura, 1997, chap. 2) and makes no internal–external distinction; rather, this measure is likely to tap into internal and external efficacy combined. Following Bandura, most researchers measure self-efficacy by asking participants to indicate the *level* of performance (i.e., magnitude) on a specific task that they expect to achieve and their *confidence* in that expectation (i.e., strength). Such measures likely capture respondents’ beliefs not only about their own abilities but also about aspects of the situation, which may include the available means. Thus, despite Bandura’s conceptual emphasis on belief in personal ability, the commonly used operationalization of self-efficacy probably captures a mix of the respondent’s beliefs about both internal and external resources. Thus, though one could argue that self-efficacy as commonly measured includes external resources, there is an unmistakable emphasis on self-estimation of internal resources in these measures. Means-efficacy scales measure something quite different; they ask respondents to indicate their level of belief in the utility of the means.

Supplementing Self-Efficacy Theory

We aimed to show that there is sound reason to tease out belief in the external resources from the overall efficacy assessment and to measure it separately as an important determinant of performance in its own right. We conducted the present experiments to show the independent impact of beliefs in external sources of efficacy on performance, as distinct from the well-documented impact of beliefs about internal factors. Once corroborated, the conceptualization and operationalization of external efficacy will augment efficacy theory and enhance our practical motivational tool box. The Internal–External Efficacy model is not intended to *supplant* Bandura’s theory; rather, it *supplements* it by explicitly adding external sources of efficacy beliefs. Expanding the efficacy construct in this way increases understanding of what drives performance and suggests novel managerial applications not implied by self-efficacy conceptualizations that focus exclusively or almost exclusively on beliefs about internal resources. Furthermore, demonstrating that we can change external efficacy without changing internal efficacy, and that doing so improves performance, would confirm the validity of external efficacy as a distinct determinant of performance. Given the above theoretical reasoning and operational considerations, the aims of the present experiments were to extend efficacy theory and application by showing that we can distinguish external efficacy from internal efficacy, that we can enhance external efficacy with no impact on internal efficacy, and that by doing so we can boost performance.

Related Research

Gist and Mitchell's (1992) Model

Some previous work has recognized the importance of the means available when individuals estimate their capability to perform a task. Gist and Mitchell's (1992; see their Figures 1 and 2) model includes an analysis of situational resources and constraints as external cues that determine one's assessment of self-efficacy. However, their model stops short of suggesting external efficacy as an independent construct. Their external–internal dimension is based on whether the organization or the individual controls each determinant of internal efficacy. As a consequence, none of Gist and Mitchell's three intervention strategies for "changing" (i.e., boosting) internal efficacy are focused on changing individuals' assessments of the utility of the available means. In contrast, the Internal–External Efficacy model identifies additional kinds of efficacy beliefs that can be enhanced for their motivating effects. This gives proper weight to workers' estimates of the efficacy of the available means as well as their own internal resources in determining the level of effort they exert and the level of performance they attain. Thus, the Internal–External Efficacy model affords the external determinants their due weight, positive or negative, as independent variables that can be enhanced by training, by managerial action, or by experimenters to improve performance. We undertook the present experiments to show this.

Technology Acceptance Model (TAM)

Attempting to understand how to help overcome the stubborn problem of users' resistance to adopting new technology, Davis (1989, 1993) developed the TAM (also see Bagozzi, Davis, & Warshaw, 1992; Venkatesh & Davis, 2000). TAM posits that acceptance of any new technology is determined by (a) the extent to which users perceive the technology to be easy to use (i.e., ease of use) and (b) the extent to which they perceive it to be useful (i.e., usefulness). Management information systems (MIS) researchers have developed measures of *ease of use* and *usefulness* to validate the theory with self-reported adoption of the new technology as the criterion (i.e., ease of use and usefulness as predictors of usage). Furthermore, Selim (2003) applied TAM (renamed Course Website Acceptance Model) to study student acceptance and usage of course Web sites.

Though widely accepted among MIS scholars, the TAM research has three drawbacks that do not plague the present experiments. First, the dependent variables are subjective usage or intensions to use the technology (MIS researchers often mislabel reported use or reported intentions to use *actual use*), which may or may not be related to actual use and to job performance; in contrast, in the present means-efficacy experiments, the dependent variable was objectively measured performance. Second, all the variables in TAM research are self-reported questionnaire items completed by the same respondents, rendering this body of research vulnerable to same-source–same-method bias. Finally, unlike TAM, the present research is firmly rooted in work motivation theory. Thus, the present tests of the Internal–External Efficacy model are unique in focusing attention on the impact of belief in the efficacy of tools on actual performance.

King's (1974) Quasi-Experiment

Though not intended as a demonstration of means efficacy, King's (1974) quasi-experimental study can be reinterpreted in terms of means efficacy. To tease out the separate effects of expectations for productivity improvements and type of intervention, King crossed expectations with type of intervention in a 2 (high expectations, no expectations) × 2 (job enlargement, job rotation) quasi-experimental design in four manufacturing plants. King informed managers in the high-expectation plants that the innovation being installed was known to improve productivity and informed managers in the control plants that it could improve morale but not productivity. He found that both job enlargement and job rotation increased productivity when introduced along with expectations for improved productivity, but neither "innovation" produced productivity gains when installed with no expectations for improvement in productivity. King's study supports the same means-efficacy hypothesis as the present experiments: Convincing managers that a program being installed is an effective means to improve productivity, whether the intervention is the "real" one (i.e., job enlargement) or only the "placebo" program (i.e., job rotation) installed as a control; it was the *belief that the intervention is an effective means* that produced the productivity gains. King's results (and ours) show that there is ample reason to invest the slight extra effort required to convince personnel—managers and workers alike—that changes being made in their organization are effective means to improve productivity; it raises their performance expectations, which, through the processes explicated in expectancy theory (Vroom, 1964), become self-fulfilling.

Means Efficacy and Performance

The most important implication of efficacy beliefs is their effect on performance (Bandura, 1997; Stajkovic & Luthans, 1998). Experimentation has shown the positive causal impact of enhancing internal efficacy on productivity (e.g., Eden & Zuk, 1995; Gist et al., 1989; Litt, 1988; Weinberg et al., 1981). Our present intension was not to replicate that. Rather, our aims were to show (a) that external efficacy can be augmented without changing internal efficacy and (b) that boosting external efficacy increases expectations and performance similar to the way internal efficacy does. Thus, we hypothesized,

Hypothesis 1: Enhancing external efficacy boosts performance expectations.
Hypothesis 2: Enhancing external efficacy boosts performance.

Based on these hypotheses, we predicted that employees whose means efficacy would be raised by the experimental treatment (a) would expect better performance than would control employees and (b) would outperform them. We proffered no hypothesis regarding the effect of internal efficacy on performance, nor did we target it in our experimental treatment. However, we measured internal efficacy (i.e., self-efficacy), both general and domain specific (Woodruff & Cashman, 1993), to show that means efficacy makes a difference even when internal efficacy is not targeted and does not change. Finally, as another "control" variable, we predicted that satisfaction, which the treatment did not target, would be unaffected.

Experiment 1

Method

Design and Sample

Experiment 1 was a true field experiment in which branches of an organization were randomly assigned to conditions. The Social Services Agency (SSA; a pseudonym) is a government organization that operates nationwide in Israel. It processes entitlement claims and renders benefit payments to eligible recipients. SSA's specialized departments are represented in its local branch offices. One department was about to get a new computer system. Units of the department are dispersed throughout the country at all 17 major SSA branch offices. These units operate semiautonomously day-to-day and are overseen by department management located at the SSA head office in the capital, Jerusalem.

The department was getting new hardware and software to replace what had been used since the early 1970s. The old system was really of use only for data entry. The new system included decision-support software designed to do many operations automatically, to do them fast, and to prevent errors. This setting was ideally suited for a means-efficacy experiment, and management was willing to try our approach to raising means efficacy to improve performance. All participating units got the same new system. The experimental treatment involved what the experimental participants were told to boost their means efficacy; this treatment was withheld from the control participants.

The number of individuals working in the department at each branch ranged between 1 and 6, for a total of 50 individuals in the 17 branches. The wide geographic dispersion enabled us to randomize the 17 branch units into experimental and control conditions while maintaining experimental insulation between them. This is because there is minimal contact among the units; each is routinely in touch with units of other departments at its branch and with the head office in Jerusalem, but not with other units of the same department at other branches. After randomly assigning nine branches (with 24 individual employees) to the experimental condition and eight branches (with 26 individuals) to the control condition, we administered the treatment to all individuals in each experimental branch.

Despite the fact that we conceptualized and measured internal efficacy and means efficacy at the individual level, performance data were available at the branch level only. This ruled out analysis at the individual level. Furthermore, taking the branch, not the individual employee in the branch, as the unit of analysis maintains fidelity to the branch-level experimental design in which branches, not individuals, were assigned to conditions at random. Moreover, the analysis of the present individual-level variables at the aggregate level instantiates an *additive* model (Chan, 1998) because self-efficacy, means efficacy, performance expectations, and satisfaction are individual-level constructs measured at the individual level; it does not require evidence for agreement among individuals in a branch to justify analyzing such data at the branch level. Finally, individuals in each branch were trained together. For all these reasons, the degrees of freedom for statistical analysis were based on the number of branches. Because the number of branches was much smaller than the number of individuals, taking the branch as the unit of analysis rendered the analysis a conservative one with decreased statistical power, rendering hypothesis confirmation harder.

Measures

Anonymous questionnaires measured everything except performance. Because the analysis was at the branch level, we computed α coefficients on the basis of the branch-level correlations among the items (i.e., correlations based on the mean ratings computed across all the individual employees in each branch).

As a manipulation check, we devised a measure of *means efficacy* for the computer system that the employees were using. They were asked to indicate on twenty 5-point items the extent to which their present computer—as distinct from their own personal ability—could contribute to their performance. Completing the header sentence, "My present computer system . . . ," examples items are, "is an efficient tool," "is reliable," "saves time," and "can prevent snafus at work" (pretest and posttest α = .95 and .99, respectively). The measure is reproduced in Appendix A.

Two measures of internal efficacy were completed. First, we assessed *general internal efficacy* using Chen, Gully, and Eden's (2001; see their appendix, p. 79) New General Self-efficacy Scale (NGSE). The NGSE taps the respondent's overall sense of being able to muster the internal resources needed to succeed in challenging circumstances. Responses to the NGSE are not likely to tap any beliefs about external resources because the items focus on the respondents' beliefs about their own competence in general; they do not ask about external resources or about the level of performance expected. Sample items are, "I believe I can succeed at most any endeavor to which I set my mind" and "Even when things are tough, I can perform quite well" (pretest and posttest α = .94 and .95, respectively).

Second, we assessed *computer internal efficacy*, the relevant skill domain in the present experiment. We constructed a 13-item measure composed of 5-point agree scales that followed this opening:

> Different people have different levels of ability to use various tools at work. Think about yourself and your ability to do work using your computer. For each of the following items, indicate the extent to which you believe you can operate your computer successfully.

To highlight the internal focus of this measure, the accentuated stem at the head of the 13 items was "*I believe I can:*," and examples of the items are, "recover quickly from glitches in the system" and "improvise new uses for the computer on the job." As for the NGSE, it is unlikely that beliefs about the computer system as a tool influenced responses to this scale. Considering that this was a newly devised measure, reliability was high (pretest and posttest α = .95 and .97, respectively).

Two measures gauged performance *expectations* after the treatment. The first was the mean of two items to which the respondent answered on a percentage scale. The first item was, "Right now on average 5% of the files in the branches have monetary errors. What percentage of monetary errors do you expect in your branch with the new computing system?" and "Right now on average 27% of the files in the branches have nonmonetary errors. What percentage of nonmonetary errors do you expect in your branch with the new computing system?" The reliability of the index built by averaging the two percentages was barely adequate (a = .60). A third expectancy item asked, "In your estimate, what will be the average number of days required to process a file in your department with the new computing system?" Adding this item to the two percentage items detracted from reliability; because it was a ratio scale, we analyzed it as a single-item measure of expectancy. *Satisfaction* was gauged once using a slight adaptation of Oz and Eden's (1994) four-item measure (α = .85; see Appendix B, Items 18 to 21).

Performance data were taken from SSA records. At SSA, speedy service is of the essence; if claimants have to wait a long time to get benefits, the system has failed. Therefore, we took mean time to account, defined as the number of days that pass between the date the client submits a claim and the date the claim is paid, as a measure of service performance. We also had intended to analyze the error rate. However, the new system was so "expert" that the error rate dropped to virtual zero, rendering it a constant with no variance to analyze.

Procedure

Pretest. During 3 days in mid-November, the experimenter (R.F.-G.) traveled to the 17 branches to collect the pretest questionnaire data. At this time, we also retrieved pretest performance data from SSA records.

The training situation. The employees got 2 days of training on the new computer system that was about to be installed. SSA's Training Department and Information Systems (IS) Department jointly administered the course. The head of IS opened the course by briefly introducing the new system to all participating employees. The department head then divided the employees into two groups and sent them to adjacent terminal rooms for practice. Each room could accommodate about half the participants. The department head did not inform them that we had randomized the room assignments by branch. To reduce the risk of experimenter effects, only the department head knew that an experiment was being conducted, and the terms *experimental* and *control* branches were never used in the field.

Treatment. Midday on the first day of the training, the experimenter delivered the treatment in the terminal rooms. The employees knew her as the facilitator of a successful, day-long workshop she had conducted there on an unrelated topic. This recent exposure imbued her with the professional credibility for delivering the present experimental treatment, which was entirely based on verbal persuasion. This was important because the power of verbal persuasion to augment efficacy beliefs is directly related to the credibility of the source (Bandura, 1997). The department head introduced the experimenter, announced that she was conducting research and would be collecting data from them on a couple of occasions, and requested their cooperation. The experimenter spent 7 minutes in each room. In the control room, she walked about watching what the participants were doing, smiled, nodded, and made other nonverbal gestures. In the experimental room, she took the podium and made the following 7-minute presentation:

> Good afternoon! The purpose of the research we are doing at the university in cooperation with your department is to investigate the reliability of the new computer system and the ease with which it is adopted. As soon as we have results we will report them to you. As part of the course we wanted to familiarize you with some of the advantages of the new computer system compared to the old one. This system has been used by an organization similar to yours in the United States for the past 6 years. The work procedures in your organization are similar to those of the American organization, except that they serve 15 million clients compared to your 300,000. In the past they had used a system like your old one, and then they switched to a new system based on the same principles as the new one you are getting. The research there was conducted in departments similar to yours 4 months after the new system had been installed. They compared the old and new systems in four aspects: (a) average net time to account, (b) employee satisfaction, (c) client satisfaction, and (d) efficiency indicators such as complaints, employee absence and tardiness rates, employee burnout, rate of monetary and nonmonetary errors, and amount of time a claim file is dormant.

She then presented four PowerPoint slides that displayed outstanding advantages of the new computer system over the old one in terms of the indicators described. She ended saying,

> In conclusion, you can see that the new computer system is more efficient and convenient and it allows employees to improve their performance and achieve excellence in their work. Furthermore, research shows that, when installing a computer system that has been used in the past, implementation is faster and more successful. Therefore, your takeoff point is better than the Americans had because your new system is based on the lessons that they learned as well as the experience of our local trial runs. Good luck, and I'll be seeing you soon.

There was no training material in this presentation. Targeting participants' beliefs, it was designed to impress them that the new computer system was a highly effective tool. In reality, the new computer system had been developed locally. We said it had been developed in the United States to amplify their belief in it. They were impressed. They uttered statements such as, "This system is a real revolution!" "Wow, this is great stuff!" and "Our organization is really entering the 21st century." They were ready to get their new computers.

Written booster. As a treatment booster, during the 10 days between the course and the installation of the new computer system, we disseminated a seven-page printed pamphlet titled "The Advantages of the New Computer System." We sent a copy of the pamphlet to each employee in the experimental branches in a personally addressed envelop. The pamphlet summarized the oral treatment presentation in two pages of text and included photocopies of the four PowerPoint slides that had been shown in the oral presentation.

Oral booster. Two weeks later, the experimenter again met with all the participating employees in both experimental and control branches for face-to-face booster sessions. In unstructured group interviews in the control branches, she asked for the employees' opinions about the new system. In the experimental branches, she asked the employees, "What are the advantages of the new system that you have already begun to experience?" and "What is the potential of the new system, assuming that you soon master the requisite knowledge and skills for working with it?" In their responses, the experimental employees expressed amazement at how much better the new system was than the old one. However, they also expressed some frustration with frequent breakdowns that were occurring and their surprise that a computer system already used still had so many problems. The experimenter responded that, though it worked well in the United States, many adaptations were required to render it suitable for use here, that any computer adaptation encounters initial difficulties until the bugs get worked out of it, and that this particular system is known for its capacity to overcome such problems much faster than comparable systems. This answer was reinforced by information that, by the time she would travel to the next branch, the problems the employees had complained about in the previous branch had already been solved. This occasioned her asking whether the employees knew what was happening at the other branches. They did not, and this strengthened our confidence in the insulation of the experimental branches from the control branches.

As in the original treatment, the amount of exposure of the participants to the experimenter during the face-to-face booster sessions was kept equivalent in experimental and control branches. It was the *content* of the experimenter–participant interaction that made the difference, not its *length*, an extraneous demand characteristic that we controlled.

Manipulation check and posttest. Two weeks after the second booster, we measured means efficacy and employee expectations. On the same visit to the branches, we had them also fill out the posttest questionnaire. We chose this timing in consultation with the department head, who estimated that by then they had had sufficient time to see most of the advantages of the new system. Two weeks later, during the 12th week since beginning the experiment, we garnered the posttest performance data from SSA records.

Debriefings. After collecting the posttest data, we debriefed the participants. The experimenter met with the manager in charge of all the branches and her second in command and explained the experiment and its findings. Because of the nationwide dispersion of the personnel involved, they were all sent a brief, nontechnical, written summary that described the study's purpose, method, findings, and implications. The debriefings revealed the true origin of the new system, the contrived nature of its description, and why deception was used. Participants were also given a phone number to contact us for further information.

Results

Variables measured twice (pretest and posttest) were subjected to repeated-measures ANOVA. The Treatment (experimental, control) × Occasion (pretest, posttest) interaction tested the hypothesis that the experimental and control branches differed in amount of pretest to posttest change. For the variables measured only after the treatment, the main effect of the treatment tested the hypothesis. We computed the binomial effect size display (BESD; Rosenthal & Rubin, 1982) to estimate the success-rate equivalent of r as an expression of the practical importance of the treatment effect.

Table 1 displays the correlations among all the variables. The pretest–posttest correlations for general internal efficacy and for computer internal efficacy (.98 and .92, respectively) evidence very high test–retest reliability, in addition to their very high α coefficients reported above. The pretest–posttest correlation for means efficacy was appreciably weaker (.31), presumably because means efficacy was targeted for change in the experimental group and did change. As expected, general internal efficacy and computer internal efficacy were correlated with each other at both pretest (r = .64) and posttest (r = .62). In general, internal efficacy, both general and domain and both pretest and posttest, was not significantly related to performance; the single exception was the positive correlation between posttest computer internal efficacy and posttest time to account.

Manipulation check. Table 2 displays the repeated-measures ANOVA of means efficacy. It detected significant main effects of both treatment and occasion and of the Treatment × Occasion interaction. Inspection of the means in the first two rows of Table 3 reveals that mean experimental and control means efficacy did not differ much at pretest (t = 0.65, *ns*), that they differed substantially at posttest (t = 5.60, p < .01), and that means efficacy in both conditions appreciably increased from pretest to posttest. We expected means efficacy to wax in both conditions because all branches got much-improved computer systems. However, the increase was substantially greater in the experimental branches, rendering the Treatment × Occasion interaction significant. The greater rise in means efficacy in the experimental branches than in the control branches validates the treatment, as intended. The interaction effect size (r = .71) shows that the impact of the treatment on means efficacy was very strong. The BESD equivalent of .71 is a success rate of 85% in the experimental branches compared to only 15% in the control branches, evidencing a practically important effect.

Only the F tests of the Treatment × Occasion interactions from remaining ANOVAs are shown in Table 3. Inspecting the next pair of rows reveals that, as intended, the treatment did not affect general internal efficacy; mean NGSE remained virtually unchanged in both conditions. However, the main effect of treatment was significant, $F(1, 15) = 39.17$, p < .01; the

Table 1
Correlations, Experiment 1

Variable	1	2	3	4	5	6	7	8	9	10
1. Means efficacy before										
2. Means efficacy after	.31									
3. General internal efficacy before	.20	.60								
4. General internal efficacy after	.14	.63	.98							
5. Computer internal efficacy before	.29	.49	.64	.68						
6. Computer internal efficacy after	.24	.54	.56	.62	.92					
7. Expected time to account	–.27	–.44	–.47	.41	–.31	–.32				
8. Expected error rate	–.19	–.31	–.52	.58	–.59	–.68	–.55			
9. Time to account before	–.31	.25	–.06	.02	–.19	–.19	–.06	.10		
10. Time to account after	–.29	.15	–.31	–.28	–.38	.51	.41	.66	.80	
11. Course satisfaction	.01	–.16	–.05	.03	.29	.36	.11	.10	–.15	–.20

Note: N = 17 branches. Correlations that exceed .48 are significant beyond .05.

Table 2
Analysis of Variance of Means Efficacy, Experiment 1

Source	*df*	SS	MS	*F*	*r*
Between branches	16				
Treatment	1	3.06	3.06	13.54**	.69
Branches within conditions	15	3.39	0.23		
Within branches	17				
Occasion	1	22.80	22.80	188.56**	.96
Treatment × occasion	1	1.82	1.82	15.43**	.71
Occasion × branches within conditions	15	1.77	0.12		
Total	33				

Note: SS = sum of squares; MS = mean square.
**$p < .01$.

Table 3
Comparison of Experimental and Control Means, Experiment 1[a]

	Experimental		Control		
Variable	*M*	*SD*	*M*	*SD*	*F*
Means efficacy					
Before	2.32	0.43	2.18	0.44	15.43**
After	4.40	0.25	3.34	0.50	
General internal efficacy					
Before	4.32	0.24	3.89	0.27	1.13
After	4.36	0.27	3.89	0.22	
Computer internal efficacy					
Before	4.40	0.25	3.95	0.50	0.94
After	4.46	0.35	3.86	0.36	
Expected “time to account”	14.06	9.00	28.10	10.82	8.47**
Expected error rate	1.18	0.99	10.40	2.01	126.79**
“Time to account”					
Before	28.11	12.21	26.12	13.08	18.90**
After	15.88	8.63	24.75	11.93	
Course satisfaction	3.01	0.91	3.05	0.70	0.02

Note: $n = 9$ experimental branches and 8 control branches. The F value shown is for the treatment effect for variables measured once and for the treatment × occasion interaction effect for variables measured twice.
*$p < .05$. **$p < .01$.

experimental mean was significantly higher than the control mean on both occasions, $t(15) = 3.45$ and 3.87, respectively, both $p < .01$. This difference indicates that randomization failed to create the intended pre-experimental equivalence between the conditions in general internal efficacy. However, what is crucial for the inference of central interest in this experiment is that mean general internal efficacy *did not increase* in either condition, as intended.

The same interpretation fits the pattern of means for computer internal efficacy (Rows 5 and 6). Again, only the main effect of the treatment was significant, $F(1, 15) = 18.04$, $p < .01$, and the experimental mean was significantly higher than the control mean on both occasions, $t(15) = 2.79$ and 3.45, respectively, both $p < .01$. Thus, although the branches randomly assigned to the experimental condition had greater computer internal efficacy than those assigned to the control condition, mean computer internal efficacy remained unchanged in both conditions, as intended.

Hypothesis testing. The next two rows of Table 3 show that the treatment affected performance expectations. Employees in the experimental branches expected both shorter service time and fewer errors than did those in the control branches. These substantial differences (14 days vs. 28 days and 1% vs. 10%, respectively) show that by augmenting means efficacy among experimental personnel, we raised their expectations for improved service performance, confirming Hypothesis 1.

The "bottom-line" results that test the performance hypothesis are in the second and third lines from the bottom of Table 3. Although mean time to account declined by a negligible amount of just more than a day in the control branches, it was nearly halved from more than 28 days to less than 16 days in the experimental branches. This is the performance improvement we sought to achieve by instilling higher means efficacy among the employees in the experimental branches. The magnitude of this ratio scale result speaks for itself and requires no additional effect size estimate. This confirms Hypothesis 2.

Finally, the last row of Table 3 shows that course satisfaction was unaltered by the treatment, as expected. This rules out a potential demand characteristic that might have threatened internal validity. The treatment raised means efficacy but not internal efficacy, and it did not make the experimental participants more satisfied with the training.

Discussion

The results of Experiment 1 were confirmatory. Computer users in branches that had been persuaded that their new computer system was an effective means for accomplishing their work did accomplish work more effectively than did control employees not so persuaded, and this occurred without altering their internal efficacy. However, the present sample of branches was quite small, and randomization is more convincing the larger the sample of elements being randomized. Together with the unintended pretest difference in general internal efficacy, this arouses suspicion that the intended benefits of randomization may not have occurred. However, this does not necessarily threaten the conclusion of the experiment. The reason is that theory suggests that an efficacy-augmenting treatment should have a stronger effect on performance when self-efficacy is low than when it is high. This is according to Brockner's (1988) plasticity hypothesis, which states that self-esteem moderates the impact of many relationships; to whit, all manner of independent variables (e.g., training, coaching, and experimental treatments) influence individuals of low self-esteem more than they influence individuals of high self-esteem. Experimental findings confirm the plasticity hypothesis with self-efficacy as the moderator (Eden & Kinnar, 1991; Eden & Zuk, 1995). Thus, if the nonequivalence of the experimental and control groups in Experiment 1 made any difference, it may very well have diminished the effect of the means-efficacy treatment among the experimental participants because of their high internal efficacy. Thus, our results may provide a conservative estimate of the effect of means efficacy on performance. This interpretation strengthens our conclusion that augmenting means efficacy contributes an incremental boost beyond whatever internal efficacy may contribute to performance.

A rival explanation to the results of Experiment 1 is that, coming as it did early on in the 2-day training, the treatment may have caused the experimental participants to take the training more seriously, invest greater effort in it, and therefore learn better how to use it and end up using it more and therefore performing better. We had no data relevant to this issue in Experiment 1. Therefore, in Experiment 2 we measured the frequency of the use of the means studied.

Experiment 2

We conducted Experiment 2 as an additional test of the same means efficacy → performance hypothesis. To overcome the limitations of Experiment 1, Experiment 2 involved a larger sample on which we could demonstrate pretest equivalence. Furthermore, the sample was very different, rendering Experiment 2 a constructive replication that lends a measure of external validity.

Method

Participants. We recruited a class of 240 first-year physics and engineering undergraduates at Tel Aviv University who were taking a required physics laboratory. They were suitable for a follow-on means-efficacy experiment because they were a copious sample of individuals taking a course built largely on a course Web site that the course staff maintains on the university's Intranet. The site contains much of the material that is necessary to complete the course; it is indispensable for accessing announcements, downloading instructional material, obtaining data for projects, and submitting assignments. Course Web sites are known to vary appreciably in quality, ease of use, and contribution to student learning.

Procedure. The experimenter (Tal Zigman) implemented the main part of the treatment during an orientation session in the week before the opening of the semester. Led by the course coordinator, the orientation lasted 5 hours. It included explanations about the course syllabus, methods of study, and safety procedures in the lab, and it enabled students to become acquainted with the lab staff. Toward the end of the orientation, the course coordinator introduced the experimenter as a graduate student conducting research relating to the course Web site. The experimenter then informed the students about the site, described it briefly, and announced that further information would be provided at an instructional session. The class was then split down the middle into two groups, allegedly to continue in smaller, more intimate forums. The experimenter took about half of the students to an adjacent room, and the remainder stayed with the coordinator. Dividing the class into experimental and control groups was based on an arbitrary line running through the lecture hall. Because this convenient assignment procedure was not based on random numbers as was the assignment to conditions in Experiment 1, we regard this to be a quasi-experiment.

Experimental treatment. After the groups had settled into their rooms, the experimenter and the coordinator distributed and then collected the signed consent forms, which included names and ID numbers. Both groups then received the same standard 20-minute presentation about the Web site. However, the experimental participants got also contrived, rosy information about the effectiveness of the site. The experimenter informed them that it was based on the method used in leading universities around the world, that their site is even better because of the lessons learned from the experience of other sites, that in those institutions there was very high satisfaction with these sites, and that research had shown substantial boosts in the grades of students who used them. The treatment was embodied in four slides that we introduced into the description of the course site in the experimental condition. The first slide informed students that a leading international firm had developed the site about 6 years previously and that more than 500 academic institutions worldwide had implemented it. The second slide described several studies of the effectiveness of the system, including the speed of processing material, the level of usage, satisfaction, and grades achieved. Bar graphs showed clear advantages for students who used the site compared to control students. The next slide showed that the grades of students who used the site were on average 5 points higher (on the 100-point grading scale used at the present university) than those of students who did not use it. The presentation was summarized with the statement,

> As you see, the new course site is effective, it is easy to use, and it enables students to improve their study habits and to achieve excellence. Research has shown that use of the system significantly improves grades. The gains to our students are likely to be even greater than those enjoyed by students overseas because our system incorporates lessons learned from previous usage.

Two months after the orientation session, we sent an e-mail summarizing the information about the effectiveness of the site to the experimental participants as a treatment booster.

The participants worked individually, not in teams. Therefore, we analyzed the data at the individual level. Nevertheless, over time the experimental and control participants may have conversed and shared information about the treatment despite our request that they not do so. However, if "treatment leakage" to the control participants did occur, it should have diminished the differences between the conditions, caused underestimation of the experimental effect, and rendered hypothesis confirmation more difficult.

Debriefing. After the experiment was over, we posted a letter on the bulletin boards of the physics and engineering departments to debrief participants. It explained the purpose, method, and findings of the experiment and provided a telephone number for contacting the research team.

Measures. Because of the convenient, nonrandom assignment to conditions, reassurance that they were equivalent at the outset was crucial. Therefore, we compared the experimental and control groups on two key indicators obtained from university records. Both indicators serve as input to admission decisions because they are valid predictors of course performance, our dependent variable. The first indicator was the high-school *matriculation* score, which is derived from standardized tests administered during the last year of high school. The second indicator was the *psychometric score* (PS), which is the Israeli equivalent of the SAT scores used in the United States for assessing aptitude for college admissions.

The students filled out a questionnaire that measured general internal efficacy, computer internal efficacy, means efficacy, and course satisfaction during the last class of the semester. The NGSE and course satisfaction were the same measures used in Experiment 1 and were as reliable (α = .93 and .85, respectively). The computer internal efficacy measure included 3 (Appendix B, Items 1 to 3) of the 14 items used in Experiment 1, α = .93. The means-efficacy measure (Appendix B, Items 4 to 17, α = .89) was composed of 14 of the items used in Experiment 1, with the course site as the object of the questions. Frequency of use of the course Web site was operationalized in terms of the number of times each student entered the site during the semester. It was tallied automatically by the site's software. The students did not know about the tally, making it an unobtrusive measure (Webb, Campbell, Schwartz, & Sechrest, 2000).

Course grade was our measure of performance. Grades ranged between 40 and 100; less than 5% scored above 95. Appendix B presents the items in all questionnaire measures. Factor analysis of all 35 items yielded a factor structure that is consistent with the constructs that the items were intended to measure (details available from the second author). (The branch level of analysis in Experiment 1 had insufficient degrees of freedom for factor analysis.)

Results

Table 4 shows that performance was moderately correlated with PS and weakly correlated with both general internal efficacy and computer internal efficacy. The strongest correlation was between general internal efficacy and computer internal efficacy; however, neither of these self-beliefs was related to means efficacy, nor was either expected to be. The PS evidently predicts performance in this course validly, whereas the matriculation test score does not.

Pre-experimental equivalence. The first two rows of Table 5 show that the means of the experimental and control groups on both indicators of aptitude were highly similar. Furthermore, as expected, Rows 3 and 4 show that the mean difference between the experimental and control groups in both general internal efficacy and computer internal efficacy were small and not significant; neither general internal efficacy nor computer internal efficacy had been targeted by the treatment. The evident similarity between the experimental and control groups in academic aptitude and internal efficacy confirms that our assignment procedure had created pre-experimental equivalence in performance-relevant, preexisting, untargeted variables, as intended.

Manipulation check. The fifth row of Table 5 shows that the experimental group significantly and substantially surpassed the control group in means efficacy. This mean difference

Table 4
Correlations, Experiment 2

Variable	1	2	3	4	5	6	7
1. Matriculation tests							
2. Psychometric score	.18*						
3. General internal efficacy	–.05	.07					
4. Computer internal efficacy	–.01	.08	.41**				
5. Means efficacy	–.15*	.10	.04	.01			
6. Performance (grade)	.07	.32**	.15*	.15*	.12		
7. Site use	–.08	–.01	.09	.01	.16*	.14*	
8. Course satisfaction	–.05	–.04	.06	.02	.07	.08	–.05

Note: $N = 236$ but varies slightly from variable to variable because of occasional missing data.
$^{*}p < .05$. $^{**}p < .01$.

Table 5
Comparison of Experimental and Control Means in Experiment 2

	Experimental		Control		
Variable	*M*	*SD*	*M*	*SD*	*t*
Matriculation tests	102.82	20.19	103.20	17.94	0.15
Psychometric score	705.83	39.14	703.50	44.13	0.42
General internal efficacy	4.09	0.51	3.98	0.56	1.69
Computer internal efficacy	4.21	0.71	4.10	0.73	1.10
Means efficacy	3.59	0.61	3.22	0.77	4.15**
Performance (grade)	89.72	4.86	85.40	7.36	5.28**
Site use	3.83	3.97	2.22	2.51	3.67**
Course satisfaction	3.02	0.96	2.95	0.86	0.58

Note: $N = 236$ but varies slightly from variable to variable because of occasional missing data.
$^{*}p < .05$. $^{**}p < .01$.

yielded an effect size of $r = .26$. The BESD equivalent of this effect size indicates that 63% of the experimental participants scored above median in means efficacy, whereas only 37% of the control group scored that high. This validates the treatment; the several sentences and slides designed to boost the experimental participants' means efficacy did so significantly and appreciably, as intended.

Performance. The sixth row of Table 5 shows that the experimental group surpassed the control group in course grade by more than 4 points ($r = .33$). The BESD equivalent of this effect size is a 66.5%–33.5% success ratio to the advantage of the experimental group; a typical experimental participant was twice as likely to score above median in performance than was the typical control participant. This confirms our central hypothesis that raising means efficacy boosts performance and shows that this effect is sizeable.

The next to last row of Table 5 shows that the means-efficacy treatment had a significant effect on site use ($r = .24$, $p < .01$); however, this effect was smaller, though not significantly so, than the effect of the treatment on performance ($r = .33$, $p < .01$). Furthermore, as Table 4 shows, the correlation between site use and performance was rather weaker ($r = .14$, $p < .05$). The partial correlation between the means-efficacy treatment and performance controlling for site use was .31. This is a negligible decline from .33, the first-order correlation between means efficacy and performance. This pattern of results strongly suggests that, although the treatment did affect site use, site use did not mediate the treatment's effect on performance.

Finally, as in Experiment 1, the difference between experimental and control groups in performance was not accompanied by a significant difference in course satisfaction.

Discussion

The experimental and control participants differed significantly on the variables on which they were expected to differ (means efficacy, site use, and performance) and did not differ on any other measured variable. Raising students' means efficacy in a course highly dependent on an Intranet Web site as a learning tool resulted in greater site use and improved learning performance. This confirms the hypothesis that boosting means efficacy improves performance and replicates the results of Experiment 1. This is a constructive replication (Hendrick, 1990) because the sample, setting, and type of tool used were different from those of Experiment 1; the same hypothesis involving the same constructs was constructively confirmed.

General Discussion

Augmenting Means Efficacy Boosts Performance

Because means efficacy is a fairly new construct, it is important that the measures we devised were well understood, well accepted, and reliable. Furthermore, the confirmatory results validated them; in both experiments, the means-efficacy treatment raised means efficacy as measured and in turn had significant positive effects on performance, as predicted. The minor differences between the means-efficacy measures from study to study appear not to have affected the results, evidencing that such contrived items constitute a robust operationalization of the construct.

We focused on means efficacy in both experiments to reveal its effect when efficacy beliefs regarding internal resources remain unchanged. The unique impact of means efficacy on expectations and on performance was highlighted by the clear evidence that the treatment affected neither general internal efficacy nor computer internal efficacy in either experiment. Therefore, internal efficacy can be ruled out as a causal variable that produced the differences observed in performance. It was not what the participants believed about *themselves* that made the difference in performance; rather, it was what they believed about the *means* at their disposal that made the difference.

The lack of impact of the experimental treatment on satisfaction with the training rules out satisfaction as a demand-characteristic explanation of the experimental effect in Experiment 1; the experimental employees did not perform better because they were more satisfied with the training that they got. Similarly, course satisfaction did not play any role in Experiment 2. Furthermore, in Experiment 1 the treatment was delivered during the training, whereas in Experiment 2 it was delivered after the training. Confirming the means efficacy hypothesis irrespective of the timing of the training reduces still further the likelihood that the training played any role in creating the effects produced. However, had such a training effect occurred, it would not detract from the logic of interpreting the results as instantiating the impact of boosting means efficacy on performance. It would raise the possibility—worthy of future research attention—that how individuals take their training may sometimes be a mediator of this effect. Indeed, effective training on the use of new tools may be an important way to boost means efficacy, especially if the instructors also increase trainees' belief in the tools (e.g., Chen, Westman, & Eden, 2006).

Raising means efficacy caused participants to use the focal tool more than the control participants in Experiment 2. However, frequency of use of the site did not mediate the treatment effect. Nevertheless, in both experiments, what we had intended to change did change, and what we had not intended to change remained unchanged. Furthermore, the design of both studies controlled potential confounders and extraneous sources of variance in the dependent variable, performance. Moreover, we took care to rule out experimenter effects. Thus, these experiments had a high degree of internal validity. Therefore, we conclude that the means-efficacy construct can be operationalized reliably, that it can be enhanced, and that enhancing it boosts performance expectations and improves performance. These findings validate the means-efficacy construct and its measure as well as our casual hypotheses.

Limitations and Boundary Conditions

Each of these two field experiments had its deficiency. The sample in Experiment 1 was quite small, and in Experiment 2 the assignment to experimental and control conditions was not truly random. However, we argue that, taken together, each experiment's strength (randomization in Experiment 1 and a large sample in Experiment 2) offsets the other's weakness. Nevertheless, it would be reassuring to see a replication combining a large sample and true randomization.

Though means-efficacy research is in its infancy, thinking about King's (1974) results raises the question of boundary conditions that may limit the usefulness of means-efficacy interventions. How good does the means really have to be to get an effect? Job rotation has been considered innocuous for decades. Inasmuch as such an impotent intervention as rotation increased productivity when expectations for productivity improvement were instilled in participants based solely on experimentally instilled belief in rotation's efficacy, one must wonder what kind of intervention would *not* improve productivity when backed by means efficacy and high expectations. It would seem that the intervention or tool onto which a means-efficacy treatment piggybacks must appear to have at least *some* credible potency in the eyes of its users. One kind of valuable replication involves testing an hypothesis in circumstances least likely to confirm it (Platt, 1964). However, it may be unfeasible to try to convince employees that a blatantly shoddy tool is useful. Precisely where that boundary lies is an issue for future research.

An obvious boundary condition is the tool's indispensability for achieving successful performance. If tools are indispensable, as they are for, say, molecular biologists, then means efficacy can be expected to play an important role in motivating performance. However, if tools are relatively unimportant, as in playing chess, means efficacy should be irrelevant, and internal efficacy should be a major factor. In both of the present experiments, the means studied were indispensable for attaining excellent performance. However, this will not generalize to situations in which means play only a marginal role.

As internal efficacy is not necessarily a valid indicator of one's actual ability, so not every means-efficacy score is a valid indicator of the true utility of the tool or even of an honest subjective assessment of it. For example, incompetent employees may try to explain away their failures by blaming the tools. Similarly, individuals who are low in achievement motivation tend to blame their failures on external factors, task difficulty being one and inadequate tools being another. Conversely, individuals high in achievement motivation are unlikely to attribute their successes to the external means. The design of the present experiments rules out such rival explanations as threats to internal validity because our experimental treatments augmented means efficacy and we measured its subsequent impact on performance; however, researchers conducting nonexperimental replications should be wary of these rival explanations.

A final limitation is that the measure of frequency of site access was a mere count. It is possible that there was an undetected difference in the *quality* of the way experimental participants used the site that mediated the treatment effect. Testing this would require a more sophisticated measure of site use.

Improving Managerial Effectiveness by Augmenting External Efficacy

Knowledge of the productive benefits that can be reaped by raising means efficacy can enrich the tool kit of managers who want to be more effective and of consultants who aim to help managers achieve this. Managers can apply these findings by fostering their subordinates' *external* efficacy to supplement whatever they may be doing to increase their *internal* efficacy. This may be a civilian extension of the legendary importance of soldiers' belief in their weapons in determining their morale and combat readiness, age-old common knowledge among military professionals that has been confirmed by empirical observation (e.g., Gal, 1986). Our findings suggest that the more managers induce their civilian employees to believe in the usefulness of the "weapons" at their disposable, the better they will perform. Thus, we can help managers to be more effective by teaching them how to augment their subordinates' belief not only in their *own* ability to excel but also in the capacity of their *tools* to help them achieve more. Maximizing overall subjective efficacy, from both internal and external sources, promises to produce the best performance.

Opportunities for applying this new knowledge abound. Many changes in organizational structure, personnel, procedures, and technology open windows of opportunity. The alert manager can piggyback onto these changes and inculcate expectations for improved performance by enhancing employees' beliefs in the utility of the changes. Doing so incurs no additional cost; it requires only presence of mind. Examples of such practical opportunities include the introduction of new computerized systems as in the present experiments; introduction of organizational innovations such as total quality management or reengineering, as King (1974) demonstrated with job enlargement and rotation and Chen et al. (2006) showed with Enterprise Resource Planning (ERP); undergoing an audit, as Eden and Moriah (1996) showed in a bank; installation of a revamped inventory control system; appointment of a new department or division head; reorganization of a division; and adoption of a novel marketing strategy. Such naturally occurring changes and planned interventions are opportunities for alert managers or consultants to raise means efficacy by stepping up and saying to the individuals involved in whatever particular words they feel most comfortable with, "Now we have the means to really improve our productivity!"

Spontaneous organizational changes and the planned introduction of new technology may be especially conducive occasions for productive application of means-efficacy enhancement (Chen et al., 2006). Newness may render employees especially receptive to efficacy-raising interventions. Uncertainty may make newcomers' expectations malleable (Chen, 2005; Chen & Klimoski, 2003; Saks, 1995). Similarly, when new technology is being introduced, even veteran employees who are unfamiliar with the new way of doing things are unlikely to have highly crystallized expectations regarding the new technology. Strengthening means efficacy may avert the seemingly inevitable resistance and stressfulness that often arise among employees who feel threatened by having to cope with changes in work processes and to learn to master unfamiliar tasks (Chen et al., 2006; Klein & Rails, 1995; Korunka & Vitouch, 1999). Employees who believe that the new technology is superior to the old and that it will make their jobs easier and more productive are less likely to resist its implementation. Conversely, it may be much harder and less effective to attempt to raise means efficacy for existing tools with which employees are familiar.

Future Research and Application

Inanimate tools, or "means," such as those we studied in the present experiments are not the only sources of external efficacy. Other external resources can include supervisors, managers, teammates, budgets, training programs, organizational structures, administrative support, and, for managers, their subordinates. All of these and many more can be construed as means that augment or reduce one's subjective likelihood of achieving successful performance. All are worthy of research as sources of external efficacy that may enhance or diminish expectations, effort, and performance. All may be used by mindful managers to increase employees' means efficacy to motivate greater productivity.

Given the present initial evidence that beliefs about external sources of efficacy supplement beliefs about internal resources, theory and research on external efficacy should map the construct's boundaries. Can external efficacy be enhanced for every type of means? Do means-efficacy-raising interventions influence individuals of different levels of internal efficacy differently? One way to answer this would be to test the effects of both an internal efficacy-enhancing treatment and an external efficacy-enhancing treatment simultaneously using a 2×2 experimental design to test the interaction to detect whether the productive effect is greater when they are augmented together compared to when one or the other is strengthened alone. Efficacy theory and its application would be strengthened further if we knew whether internal efficacy and means efficacy reinforce each other, offset each other, or have no bearing on each other.

There is a major unmeasured variable in this line of research. As in goal setting, expectancy theory, and Pygmalion studies, this research is “about” motivation. The basic process under investigation is one in which raising goals, expectations, internal efficacy, or external efficacy *motivates* intensification of effort that culminates in improved performance. However, motivation is not typically measured, nor is effort. It can be appreciated as a strength of goal-setting and valence-instrumentality-expectancy research that they systematically define aspects of the work-motivation process without using the term being defined—*motivation*. Motivation is typically inferred from changes in performance. It would undoubtedly add clarity to these other lines of motivation research, as well as to internal- and external-efficacy research, if the most basic construct involved—motivation—were measured and analyzed.

Finally, additional replications are needed to determine the generalizability of enhancing means efficacy as an effective means to improve management. Further internally and externally valid field experimentation is the best means for acquiring this knowledge. This expression of our belief in experimentation as an effective means to conduct research on these issues is our concluding example of means efficacy.

Appendix A
Means-Efficacy Measure, Experiment 1

Different tools may contribute in varying degrees to successful job performance. Think about your computer as a work tool. Answer the following questions about the ability of your computer, as distinct from your own personal ability, to contribute to your job performance. Please indicate the extent to which you agree that each of the following statements is true of your computer at work.

My present computer:

1. is an efficient tool
2. is easy to use
3. can serve its purpose at work
4. is “friendly”
5. can recover fast from breakdowns
6. can detect errors
7. shortens response time for the client
8. can operate without breakdowns
9. can do its work well
10. gives my department an advantage over other departments
11. gives SSA an advantage
12. is worth the money invested in it
13. saves me time

(continued)

Appendix A (continued)

14. is reliable
15. is the best of its kind
16. makes client service more efficient
17. facilitates professional service to the client
18. facilitates accurate service to the client
19. facilitates fast service to the client
20. can prevent snafus at work

Note: Responses on a 5-point scale ranged from *to a very little extent* to *to a very great extent*.

Appendix B
Measures, Experiment 2

Computer Internal Efficacy
1. Utilize the potential of the computer in my studies
2. Find new uses for the computer in my studies
3. Perform my course assignments using the computer

Means Efficacy
4. Is an efficient tool
5. Is easy to use
6. Easy to operate
7. Can serve its purpose in the course
8. Is “friendly”
9. Shortens search time
10. Can operate without problems
11. Can do its work well
12. Gives an advantage over other courses
13. Saves me time
14. Is reliable
15. Is the best of its kind
16. Facilitates academic services for the student
17. Facilitates fast service to the student

Course Satisfaction
18. To what extent would you recommend the course to your friends?
19. In general, how satisfied are you with the course?
20. To what extent does the course answer your initial expectations?
21. To what extent would you change the course?

Note: Responses to all items were made on a 5-point scale ranging from *to a very little extent* to *to a very great extent*. The Computer Internal Efficacy instructions read, “People have different levels of belief about their ability to use various tools. Please answer the following questions about your beliefs regarding your ability to perform your school work using the computer and the Internet.” The Means-Efficacy instructions read, “Different tools may contribute in varying degrees to successful performance. Think about the course site as a work tool. Answer the following questions about the ability of the site, *as distinct from your own personal ability*, to contribute to your performance.” For Course Satisfaction, participants were asked to indicate the extent of their agreement with the four statements.

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