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Are Financial Incentives Related to Performance? A Meta-Analytic Review of Empirical Research

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The relationship of financial incentives to performance quality and quantity is cumulated over 39 studies containing 47 relationships. Financial incentives were not related to performance quality but had a corrected correlation of .34 with performance quantity. Setting (laboratory, field, experimental simulation) and theoretical framework moderated the relationship, but task type did not.

Financial incentives have long been thought, both theoretically and practically, to affect employee motivation and performance. Yet there are few systematic examinations of the relationship of financial incentives to employee behaviors, and little effort is devoted to assessing this relationship in the cumulative scientific knowledge base. This is perhaps because studies of the impact of financial incentives are complex. They must address the different meanings, literal and symbolic, of money, other outcomes (e.g., promotions, coworker resentment, turnover) that might deliberately or inadvertently be associated with financial incentives, differences in utility of money, social comparisons that financial incentives evoke, group norms, organizational structure, and so on. An estimate of the overall association of financial incentives and performance is necessarily affected by a multitude of factors. It can nevertheless provide baseline information against which to assess the utility of specific financial treatments in work settings. This article is designed to develop such baseline information. It meta-analyzes the literature to ascertain whether, and how strongly, financial incentives are related to performance quantity and quality.

The proposition that financial incentives improve performance has both its proponents and opponents. To begin with, proponents consider financial incentives to be a potent influence, arguably the most potent influence, on employee performance and other desired behaviors (Baker, Jensen, & Murphy, 1988; Jenkins & Gupta, 1982; Locke, Feren, McCaleb, Shaw, & Denny, 1980; Locke, Shaw, Saari, & Latham, 1981; Skaggs, Dickinson, & O'Connor, 1992). A "careful examination of the criticisms of monetary pay-for-performance systems indicates not that they are ineffective but rather that they are *too* effective" (Baker et al., 1988, p. 597). Financial incentives also convey symbolic meaning (e.g., recognition, status) beyond their monetary value: They meet multiple human needs and serve multiple functions (Opsahl & Dunnette, 1966; Steers, Porter, & Bigley, 1996). Financial incentives supplement intrinsic rewards: People need money (Baker, 1993; Steers et al., 1996). Economic theory and research also indicate that employers find financial incentives useful in matching differences in employees' marginal products to differences in wage rates (Bishop, 1987; Frank, 1984), reducing costs associated with dysfunctional employee behavior (Abelson & Baysinger, 1984).

Perhaps the most significant argument against financial incentives concerns the detrimental effects of money on intrinsic motivation (Eisenberger & Cameron, 1996; Kohn, 1993b; Skaggs et al., 1992). Opponents argue that financial incentives control employee behavior externally, reducing self-determination and intrinsic motivation (Deci & Ryan, 1985; Kohn, 1993a). They also jeopardize the relationship between supervisors and subordinates (Meyer, 1975) and reduce risk-taking behaviors. Thus, "rewards encourage people to focus narrowly on a task,

G. Douglas Jenkins, Jr., and Nina Gupta, Department of Management, University of Arkansas; Atul Mitra, Department of Management, Lyon College; Jason D. Shaw, Department of Management, Drexel University. G. Douglas Jenkins, Jr. is now deceased. This article stems in large part from his deep interest in rewards, compensation, and financial incentives.

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to do it as quickly as possible, and to take few risks'' (Kohn, 1988, p. 93), and ''getting people to chase money . . . produces nothing but people chasing money'' (Slater, 1980, p. 127). Opponents argue further that money is not a motivator (Kohn, 1993b); financial incentives may reduce job dissatisfaction, but they do not motivate (Herzberg, 1968).

Early work on financial incentives, although lacking in scientific rigor (Jenkins, 1986; Marriott, 1957; Opsahl & Dunnette, 1966), showed productivity improvements with the introduction of incentives (Barnes, 1949; Reiners & Broughton, cited in Marriott, 1968; Wyatt, Langdon, & Marriott, cited in Marriott, 1968) and productivity declines with the elimination of incentives (Jacques, Rice, & Hill, 1951). The pioneering work of Opsahl and Dunnette (1966) marked the beginning of theory-guided controlled investigations of financial incentives and task performance. Five primary theoretical frameworks address the relationship between money and performance. Expectancy theory suggests that tying financial incentives to performance increases extrinsic motivation to expend effort and consequently performance (Lawler, 1971, 1973; Vroom, 1964). Reinforcement theory, although less cognitive in focus, argues that tying money to performance will reinforce performance (Komaki, Coombs, & Schepman, 1996). Goal-setting theory indicates that financial incentives increase acceptance of difficult performance goals, enhancing performance (Locke, Latham, & Erez, 1988). By contrast, cognitive evaluation theory proposes that performance-contingent financial incentives erode intrinsic motivation and thereby diminish task performance (Deci, 1971, 1972a, 1972b; Deci & Ryan, 1985). Equity theory (e.g., Adams, 1963, 1965) argues that people are motivated to reduce inequity, but makes no specific predictions regarding the relationship of financial incentives and performance per se, although under certain conditions, deviations from fairness may erode the association of financial incentives to performance (e.g., Kanfer, 1990).

Overall, both theory and practice highlight the ambiguity in how financial incentives affect performance. Empirical research to resolve the issue is sparse, at best (Jenkins, 1986; Opsahl & Dunnette, 1966). Jenkins (1986) was able to identify only 28 systematic empirical investigations of this relationship for his review, and only one of these studies (Toppen, 1965) focused exclusively on the impact of financial incentives on task performance. Only a handful of empirical studies was conducted since the Jenkins (1986) review. However, the central role that financial incentives play in organizational functioning mandates a systematic assessment of this issue. Are organizations wasting their money by using financial incentives? Worse yet, are they eroding motivation? Must our motivational frameworks be overhauled completely? These questions must be addressed through a comprehensive examination of our scientific database for both theoretical and practical reasons. This is what this study was designed to do.

As noted, Jenkins (1986) conducted a qualitative review of empirical research from 1960 to 1985, and concluded that both laboratory and field experiments clearly demonstrate the positive effects of financial incentives on performance quantity, although a similar effect was absent with respect to performance quality. Expectancy theory reviews by J. Campbell and Pritchard (1976), Dyer and Schwab (1982), and Ilgen (1990) also support a positive relationship between financial incentives and task performance.

However, all four reviews are qualitative in nature. The Jenkins (1986) review, most directly relevant to the present study, used the traditional voting method (Light & Smith, 1971), which is a conservative and inconclusive method for discovering trends in relationships (Hunter, Schmidt, & Jackson, 1982). Meta-analytic approaches are more suited for this purpose (Hunter et al., 1982; Wanous, Sullivan, & Malinak, 1989) because they allow a cleaner assessment of effect sizes and enable a systematic search for artifactual and potential moderator effects (Guzzo, Jackson, & Katzell, 1987; Hunter & Schmidt, 1990; Hunter et al., 1982; Wanous et al., 1989; Whitener, 1990). We use meta-analytic techniques here.

We examined setting[1] (laboratory, experimental simulation, and field) as a moderator because Jenkins (1986) observed stronger effects in laboratory settings than in the others. Task type was a potential moderator (Jenkins, 1986; Opsahl & Dunnette, 1966); more consistent effects were observed when the task was less cognitive in nature. Theoretical framework was a third moderator because the impact of financial incentives is typically examined within a theoretical framework. Four theoretical frameworks—expectancy, reinforcement, goal setting, and cognitive evaluation—were relevant (equity theory was excluded because its predictions are moderated by equity considerations).

In short, this study undertakes a quantitative review of the relationship between financial incentives and performance. A meta-analytic review is more sensitive to under-

[1] Evidence of external validity should contain information about actors, behavior, and settings (Gordon, Slade, & Schmitt, 1987; Runkel & McGrath, 1972). The present meta-analysis used behaviors and settings as potential moderators, but not participants, despite the fact that the studies in the database were coded for this variable (as shown in Table 1). Two reasons dictated the exclusion of subjects as a moderator: (a) Participants and settings were highly correlated; and (b) generalizing across participants would require controlling for a multitude of noncomparable factors across settings (Gordon et al., 1987; Greenberg, 1987).

lying covariations than is a traditional voting method review; our study supplements and updates that of Jenkins (1986). It provides statistically grounded information on the strength of the financial incentives–performance relationship, and it identifies potential moderators of this relationship. Following Jenkins (1986) and Locke (1986), it also enables a determination of the extent to which financial incentives dynamics are generalizable across laboratory and field settings.

Method

Empirical studies of financial incentives and performance were identified in three ways: (a) computerized database searches for 1975 to 1996 using the key words *incentives, financial incentives, money, monetary rewards, task performance, reinforcement, expectancy theory,* and *goal setting;* (b) manual searches of *Academy of Management Journal, Academy of Management Review, Human Relations, Journal of Applied Psychology, Journal of Organizational Behavior, Journal of Management, Organizational Behavior and Human Decision Processes,* and *Personnel Psychology* for 1984 to 1996; and (c) examinations of the reference lists contained in Jenkins (1986); Mento, Steel, and Karren (1987); and articles located through the two previous steps. Seven decision rules suggested by Jenkins (1986) were used as inclusion criteria. To be included, a study had to (a) be conducted during 1960 to 1996 (studies before that time lack scientific rigor; Jenkins, 1986; Marriott, 1957; Opsahl & Dunnette, 1966); (b) be empirical, excluding anecdotes and case studies; (c) have "hard" (i.e., non-self-report) performance measures; (d) focus on financial incentives at the individual rather than group or organizational level; (e) have a control group or a premeasure with an explicit manipulation of the performance contingency of the incentive (this excluded studies that examined differences across incentive sizes, and allowed a clean assessment of having or not having financial incentives); (f) focus on monetary rather than nonmonetary (Neider, 1980) or hypothetical (Chow, 1983; Shepperd & Wright, 1989) incentives; and (g) use adult populations (rather than children).

The three search procedures and seven decision rules yielded 43 studies with usable data. Of these, data from four (Klein & Wright, 1994; Komaki, Waddell, & Pearce, 1977; Locke, Bryan, & Kendall, 1968, Study 1; Wright, 1989) could not be converted to usable form and were excluded. The remaining 39 studies, containing 47 financial incentives–performance relational statistics, constituted our database. Forty-one statistics concerned performance quantity, and six concerned performance quality. Descriptive information on these studies is contained in Table 1.

Several meta-analytic techniques are available in the literature (Burke, Raju, & Pearlman, 1986; Hunter & Schmidt, 1990). We used the Hunter et al. (1982; Hunter & Schmidt, 1990) meta-analysis approach for several reasons. First, it is suited for combining correlational and experimental results (Farrell & Stamm, 1988; Hunter & Schmidt, 1990). Second, with sample sizes larger than 30, this technique yields estimates equally as unbiased as other techniques. Few studies in our database had sample sizes less than 30. This minimized sampling error concerns (James, Demeree, & Mulaik, 1986; James, Demeree, Mulaik, & Mumford, 1988; Schmidt, Hunter, & Raju, 1988). Third, the technique allows corrections for attenuation as a result of unreliability of measurement and range restriction, and enables an assessment of the extent to which observed variance across studies is due to statistical artifacts (Hunter & Schmidt, 1990; Hunter et al., 1982). Fourth, this approach is often used and well understood in the literature.

According to this approach, three primary study artifacts—sampling error, measurement error, and range restriction—can account for a large proportion of between-study variance (Hunter & Schmidt, 1990; Hunter et al., 1982). The best estimate of the population mean of the relationship under study is the sample size–weighted mean correlation corrected for measurement error and restriction of range (Hackett & Guion, 1985; Hunter & Schmidt, 1990; Hunter et al., 1982). Although meta-analysis assumes that all coefficients included in the analysis are independent, it is also reasonably robust against violations of this assumption (Hackett & Guion, 1985; Scott & Taylor, 1985). The six correlations of financial incentives and performance quality were obtained from studies that also reported correlations with performance quantity. These correlations were included in the full database analyses but were excluded from the moderator analyses.

An important contribution of meta-analysis is the identification of moderators that might account for interstudy differences (Guzzo et al., 1987; Wanous et al., 1989). The studies in the database were coded in terms of three potential moderators: setting, task type, and theoretical framework. Two advanced management doctoral students coded the studies. The coders content analyzed each article, searching for key words such as laboratory experiment (for setting) and extrinsic or boring (for task type). Coders also determined the dominant theoretical framework in the study; this was not difficult in view of their expertise and background. A high level of agreement occurred between coders (interrater reliability of .99). The rare disagreement was resolved through substantive discussion. The coding judgments were also cross-validated against Jenkins (1986) when possible. An assessment of the independence of moderators showed no significant relationships among moderators.

Setting was coded as laboratory experiment, experimental simulation, or field experiment. Distinguishing between laboratory and field settings was straightforward. To distinguish between laboratory experiments and experimental simulations, coders relied primarily on the authors' own self-definition. In the rare instance when self-definition was not available, longer studies containing several features to enhance realism were coded as simulations, and shorter studies in which few such elements were incorporated were coded as laboratory experiments. Jenkins (1986) combined simulations and laboratory experiments into one category. We did not because they are unique research arenas (Runkel & McGrath, 1972), and there was a sufficient number of experimental simulations in the database. Of the 47 relational statistics, 30 were derived from laboratory experiments, 9 from experimental simulations, and 8 from field experiments. For performance quantity, these numbers are 25, 8, and 8, respectively.

Task type was coded as intrinsic or extrinsic based on lan-

Table 1
Summary of Studies in the Meta-Analysis Data Base

Study	Performance measure	Setting	Participants	Task	Theoretical framework	Study N	Statistics reported
Toppen (1965)	QN (no. manipulandum pulls)	L	UG	E	R	20	$F(1, 18)^a = 13.263$
Locke et al. (1968), Study 2	QN (no. Tinkertoys assembled)	L	UG	I	GS	30	$t = 45$, ns^b
Yukl et al. (1972)	QN (no. IBM cards punched)	S	UG	E	R	15	$t = 5.93$
Jorgenson et al. (1973)	QN (no. catalog items decoded)	S	UG	E	E	143	$F(1, 141) = 204.62$
Pritchard & Curtis (1973)	QN (no. index cards sorted)	L	UG	E	GS	61	$p = .05$, $t = 2.00^b$
Pritchard & DeLeo (1973)	QN (no. catalog items decoded)	S	UG	E	E	58	$F(1, 56) = 7.61$
Berger et al. (1975)	QN (no. booklets scored)	S	UG	E	E	49	$F(1, 47) = 103.8$
Hammer & Foster (1975)	QN (no. questionnaires coded)	L	UG	E, I	CE	68	$d_{12} = .085$
Hamner & Foster (1975)	QL (coding accuracy)	L	UG	E, I	CE	68	$d_{12} = .15$
Yukl & Latham (1975)	QN (no. bags planted/hour)	F	E	E	R	12	$t = 4.68$
Chung & Vickery (1976)	QN (average no. pages coded)	L	UG	E	E, R	81	$F(1, 79) = 5.02$
Chung & Vickery (1976)	QL (no. wrong responses)	L	UG	E	E, R	81	$F(1, 79) = 0.02$
Farr (1976)	QN (no. erector sets assembled)	L	UG	E, I	CE	90	$F = 12.12^c$
Pinder (1976)	QN (no. assembled nuts & bolts)	L	UG	E	CE	40	$t = 2.38$
Pritchard, Leonard, et al. (1976)	QN (no. programmed texts completed)	S	HS, UG	I	R	16	$p = .0005$, $t = 2.95^b$
Pritchard, DeLeo, & von Bergen (1976)	QN (exam completion time)	F	E	I	E	306	$t = 8.40$
Terborg (1976)	QN (exam completion time)	S	HS, UG	I	GS	55	$p = .02$, $t = 2.50^b$
Terborg (1976)	QL (% correct responses)	S	HS, UG	I	GS	55	$p = .28$, $t = 0.80^b$
Turnage & Muchinsky (1976)	QN (no. cards sorted)	L	UG	E	CE	64	$F(1, 62) = 4.76$
Turnage & Muchinsky (1976)	QN (no. stencil designs)	L	UG	I	CE	64	$F(1, 62) = 3.76$
Yukl et al. (1976)	QN (no. trees planted/hour)	F	E	E	R	20	$t = 1.70$
Farr et al. (1977), Study 1	QN (no. puzzles solved)	L	UG	I	CE	48	$F = 0.07$
Farr et al. (1977), Study 2	QN (no. puzzles solved)	L	UG	I	CE	152	$F = 1.55$
London & Oldham (1977)	QN (no. cards sorted)	L	UG	E	D	28	$p < .05$, $t = 2.05^b$
Pritchard et al. (1977)	QN (no. chess problems completed)	L	UG	I	CE	28	$t = 0.44$
Latham & Dossett (1978)	QN (no. rats caught/hour)	F	E	E	R	14	$p < .05$, $t = 1.77^b$
Latham et al. (1978)	QN (supervisor rating)	F	E	I	GS	48	$d_{12} = 0.705$
Terborg & Miller (1978)	QN (no. Tinkertoys assembled)	L	UG	I	GS	56	$F(1, 54) = 4.50$
Terborg & Miller (1978)	QL (supervisor rating)	L	UG	I	GS	56	$p = .28$, $t = 0.81^b$
Wimperis & Farr (1979)	QN (no. nuts/bolts connections)	L	UG	I	CE	48	$t = 2.404$
Wimperis & Farr (1979)	QL (supervisor rating)	L	UG	I	CE	48	$t = 0.83$
Pritchard et al. (1980)	QN (no. programmed texts completed)	S	HS, UG	I	R	40	$p = .01$, $t = 2.44^b$
Luthans et al. (1981)	QN (behavior ratings)	F	E	E	R	16	$t = 17.00$
Orpen (1982)	QN (no. items tested)	F	E	E	R	42	$p = .01$, $t = 2.42^b$
Saari & Latham (1982)	QN (no. rats trapped/hour)	F	E	E	R	12	$t = 12.20$
Vecchio (1982)	QN (surveys/minute)	L	UG	I	CE	43	$r = 0.44$
Vecchio (1982)	QL (no. words recorded)	L	UG	I	CE	43	$r = 0.07$
D. J. Campbell (1984)	QN (averaged cost analysis)	L	G	I	GS	56	$t = 2.92$
Riedel et al. (1988)	QN (no. items coded)	S	HS, UG	E	E	130	$t = 3.87$
Firsch & Dickinson (1990)	CM (no. quality parts assembled)	L	UG	E	D	75	$F = 3.84$
Wright (1990)	QN (no. class schedules completed)	L	UG	E	GS	56	$t = 1.46$
Farh et al. (1991)	QN (no. random three digits decoded)	L	UG	E	E	18	$p = .05$, $t = 1.74^b$
Fatseas & Hirst (1992)	QN (no. random three digits decoded)	L	UG	E	GS	120	$d_{12} = .22$
Wright (1992)	QN (no. class schedules completed)	L	UG	E	GS	235	$p = .05$, $t = 1.65^b$
Salvemini et al. (1993)	QN (judgment of sales/clerical items)	L	UG	I	GS	101	$F(1, 99) = 19.15$
Henry & Strickland (1994)	QN (no. information pieces retrieved)	L	UG	E	GS	131	$F(1, 129) = 5.83$
Stone & Ziebart (1995)	CM (college choice accuracy)	L	UG	I	O	84	$F(1, 82) = 24.9$

Note. Total N for the meta-analysis is 3124. d_{12} = estimated effect size based on formulas from Nouri and Goldberg (1995). QN = quantity; QL = quality; CM = composite; L = laboratory experiment; S = experimental simulation; F = field experiment; HS = high school students; UG = undergraduate students; G = graduate students; E = employees; E = extrinsic; I = intrinsic; CE = cognitive evaluation; E = expectancy; GS = goal setting; R = reinforcement; O = other proposed model; D = direct test.
[a] F value is based on the linear contrast reported in the study.
[b] Relational statistics not reported. Correlations were imputed from the $p = .05$ (or as reported in the article) level of significance using the Glass et al. (1981, pp. 128–129) procedures.
[c] Univariate F statistic reported without degrees of freedom.

guage used by the authors. *Extrinsic, boring,* and *nonappealing* were terms often used to describe tasks coded as extrinsic, whereas *intrinsic, appealing, exciting* were terms often used to describe tasks coded as intrinsic. In a few cases, the task could be, and was, coded both ways (see Table 1). Sixteen performance quantity relationships used intrinsic tasks, and 23 used extrinsic tasks.

Theoretical framework was the third potential moderator. Of course, the actual issue here is capturing the differences in operationalization of the treatment. Unfortunately, there were too many differences in specific operationalizations for them to be coded meaningfully. Theoretical framework is a good proxy for critical operational differences (Glass, McGaw, & Smith, 1981), and was used instead. It was coded into three groups: expectancy-reinforcement (No. of studies = 17), goal setting (No. of studies = 11), and cognitive evaluation (No. of studies = 10). Expectancy and reinforcement theories were combined because the two (although conceptually distinct) make similar predictions (Pinder, 1984), and because the studies in the database typically mentioned both in deriving hypotheses.

Treatment effects could be converted to d (effect size) or r (correlation) statistics (Hunter & Schmidt, 1990). Because effect size-based meta-analyses are complex, and because d is an algebraic transformation of r, Hunter and Schmidt (1990) recommend the conversion of treatment effects to point-biserial correlations (i.e., they advise the use of correlation-based meta-analysis). Therefore, the relational statistics in each study were converted to point-biserial correlations using the Glass et al. (1981, pp. 149–150) formula. When authors reported a relational statistic based on a linear contrast, it was included in our database. In the absence of a linear contrast, but when cell means and standard deviations were available, the Nouri and Greenberg (1995) formulas were used to estimate the effect size d. When only p values were reported, we took a conservative approach. Following Glass et al. (1981, pp. 128–129), we used $p = .05$ to calculate a t statistic; the t statistic was then converted to a point-biserial correlation.

Another issue concerned the specific corrections for unreliability (Hunter & Schmidt, 1990). Reliability estimates for performance measures were rarely reported in the database, necessitating the use of existing reliability estimates (Hunter et al., 1982). Three reliability estimates were available in the database: .96, .93, and .81 from Latham, Mitchell, & Dossett (1978), Luthans, Paul, and Baker (1981), and Terborg and Miller (1978), respectively. Also, Mento et al. (1987) used an estimate of .92 for objective performance measures (which most studies in the database used; see Table 1). Hunter et al.'s (1982, pp. 73–82; see also Hunter & Schmidt, 1990, pp. 158–177) artifact distributions method was used to estimate measurement error, the mean estimated population correlation ($\hat{\rho}$), and the standard deviation of the estimated correlation ($\hat{\sigma}_{\hat{\rho}}$). Using the artifact distributions method, the four reported reliability estimates enabled calculation of the mean ($\bar{a}$) and the variance (σ_a^2) of the artifact distribution. The mean ($\bar{a}$) and variance (σ_a^2) were used to calculate measurement error, $\hat{\rho}$, and $\hat{\sigma}_{\hat{\rho}}$, based on the formulas suggested by Hunter et al. (1982, p. 80; see also Hunter & Schmidt, 1990, pp. 174–176). Because the reported reliability estimates could be generous, the analyses in Table 2 were also run using 1 standard deviation below the mean ($\bar{a} - \sigma_a$) of the artifact distribution. There was little substantive difference in the results or conclusions using the lower estimate. To preserve readability, Table 2 reports only the analyses based on the mean ($\bar{a}$) and the variance (σ_a^2) of the artifact distribution.

Another technical concern was the choice of techniques to detect moderator effects. Two techniques are dominant. The first uses percentage of variance explained (Hunter et al., 1982), modified to define appropriate critical percentages for different numbers of relational statistics at fixed type I error rates (Rasmussen & Loher, 1988). The second involves credibility intervals. According to Whitener (1990), credibility intervals that are large or that span zero imply that the data are drawn from more than one population and indicate the presence of moderators. Sagie and Koslowsky (1993) used a Monte Carlo approach to assess the impact of variations in different parameters on the efficacy of these two moderator detection techniques. The study suggested the utility of using both simultaneously, and we do so.

Results

The results of the main and moderator meta-analyses are shown in Table 2. Overall, 30.3% of the observed variance is explained by sampling and measurement errors. The relationship of financial incentives and performance quality (weighted $\bar{r}$ = .08) is not significantly different from zero, and the study artifacts of sampling and measurement error explain 100% of the observed variance across studies in this group. These artifacts explain only 29.9% of the observed variance in performance quantity, however, implying the need for a further search for moderators. The nonsignificance of the results for performance quality and concerns about the nonindependence of coefficients led us to conduct the subsequent moderator analyses using only the 41 relational statistics focusing on performance quantity.

Each moderator was examined separately. Each analysis entailed an examination of (a) whether the average correlation varied across moderator categories (e.g., intrinsic vs. extrinsic task) (Hunter & Schmidt, 1990; Hunter et al., 1982); (b) whether the average corrected variance was lower in moderator subsets than in the data as a whole (Hunter & Schmidt, 1990; Hunter et al., 1982; (c) the percentage of variance explained (Hunter & Schmidt, 1990; Hunter et al., 1982; Rasmussen & Loher, 1988); and (d) whether the credibility intervals were wide and included zero (Whitener, 1990).

Setting consistently moderated the financial incentives–performance relationship. The average corrected correlation ($\hat{\rho}$) varies across subsets (laboratory experiment = .24; experimental simulation = .56; field experiment = .48). The average corrected standard deviation is smaller in the subsets than in the data as a whole (.13 vs. .18). In addition, for laboratory and field experiments,

Table 2
Overall and Moderator Results of Meta-Analyses of Financial Incentives and Performance

Variable	Weighted $\bar{r} \pm 1.96\ SE$	N^a	S	SD_r^2	Sampling error	Measurement error	Variance explained	$\hat{\rho}$	$\hat{\sigma}_{\hat{\rho}}$	Credibility interval
				Full database analyses						
Quality and quantity	.30 ± .06	3,124 (66.5)	47	.0423	.0127	.0001	30.3	.31	.18	−.05–.65
Quantity	.32 ± .06	2,773 (67.6)	41	.0407	.0120	.0001	29.9	.34	.18	−.03–.67
Quality	.08 ± .11	351 (58.5)	6	.0015	.0170	.0000	100	.08	0	.00–.00
				Moderator analyses						
Setting										
Overall	.32 ± .06	2,773 (67.6)	41	.0407	.0120	.0001	29.9	.34	.18	−.03–.67
Laboratory	.23 ± .04	1,797 (71.9)	25	.0177	.0126	.0001	71.9	.24	.07	.09–.39
Stimulation	.53 ± .15	506 (63.2)	8	.0495	.0083	.0004	17.5	.56	.21	.14–.98
Field	.46 ± .10	470 (58.8)	8	.0223	.0108	.0003	49.7	.48	.11	.26–.70
Task type										
Overall	.33 ± .06	2,615 (67.1)	39	.0410	.0120	.0002	29.6	.35	.18	−.02–.68
Intrinsic	.33 ± .07	1,175 (73.4)	16	.0204	.0110	.0001	54.9	.34	.10	.14–.54
Extrinsic	.34 ± .10	1,440 (62.6)	23	.0578	.0128	.0002	22.4	.35	.22	−.08–.79
Theoretical framework										
Overall	.32 ± .07	2,586 (68.1)	38	.0425	.0120	.0001	28.6	.34	.18	−.04–.68
Goal-setting	.22 ± .06	949 (86.3)	11	.0116	.0106	.0001	92.4	.23	.03	.16–.29
Expectancy/reinforcement	.49 ± .10	992 (58.4)	17	.0410	.0100	.0003	25.7	.52	.18	.16–.88
Cognitive evaluation	.21 ± .07	645 (64.5)	10	.0177	.0144	.0001	81.6	.22	.06	.10–.34

Note. $\bar{r}$ = sample size–weighted mean correlation; S = number of correlations; SD_r^2 = variance of observed correlations; $\hat{\rho}$ = mean estimated population correlation; $\hat{\sigma}_{\hat{\rho}}$ = standard deviation of estimated correlation. $\bar{r}$, SD_r^2, sampling error, measurement error, $\hat{\rho}$, and $\hat{\sigma}_{\hat{\rho}}$ were calculated from the Hunter and Schmidt (1990, pp. 100–108, 174–176; also Hunter et al. 1982, pp. 41–44, 80) formulas, *SE* from the formulas for homogeneous population and heterogeneous populations from Whitener (1990, pp. 316–317), and credibility intervals from the corrected standard deviation around the corrected mean correlation (Whitener, 1990, p. 317).
[a] Average sample size per study in parentheses.

the percentage of variance explained is well above the critical thresholds, and the credibility intervals are narrow and do not contain zero. Experimental simulations, in contrast, do not meet the percentage of variance or credibility interval criteria, suggesting that additional moderators may apply in this subgroup.

The situation is more ambiguous for task type as a moderator. The average corrected correlation $\hat{\rho}$ is only slightly lower for intrinsic tasks than for extrinsic tasks (.34 vs. .35). In the intrinsic subgroup, 54.9% of the variance is explained, and the credibility interval, although not spanning zero, is quite wide (.14–.54). In the extrinsic subgroup, only 22.4% of the variance is explained, and the credibility interval spans zero and is quite wide also (−.08–.79). The average corrected standard deviation is barely smaller in the subsets than in the data as a whole (.16 vs. .18). All four criteria are met marginally at best, leading to the conclusion that task type may not moderate the relationship between financial incentives and performance.

For theoretical framework, the average corrected correlation $\hat{\rho}$ varies significantly across subsets (goal setting = .23; expectancy-reinforcement = .52; cognitive evaluation = .22), and the average corrected standard deviation between subgroups is smaller than for the overall database (.09 vs. .18). The percentage of variance and credibility interval criteria indicate that no further moderators exist in the goal-setting and cognitive evaluation subgroups, but that additional moderators are implied in the expectancy-reinforcement subgroup. As Table 1 shows, settings were likely to vary for the expectancy-reinforcement subgroup but not for the other two. An analysis of the expectancy-reinforcement subgroup using setting as a moderator showed that setting may have caused the larger observed variance within this subgroup.

A final issue was the magnitude of the financial incentives–performance relationship. This entailed constructing confidence intervals around the sample size–weighted mean effect size for the overall database as well as for each subgroup (Whitener, 1990). The formula for standard error required to construct confidence intervals varies depending on the population. Whitener (1990) outlined standard error formulas for heterogeneous populations (i.e., those with potential moderators) and homogeneous populations (i.e., those without moderators). These formulas were used to construct confidence intervals around the sample size–weighted mean effect size and are also shown in Table 2. The covariation of financial incentives with performance quality is not significantly different from zero (weighted $\bar{r}$ = .08, $\hat{\rho}$ = .08), but the relationship with performance quantity is significant (weighted $\bar{r}$ = .32, $\hat{\rho}$ = .34).

Discussion

The results of the meta-analysis provide intriguing insights about the relationship of financial incentives to performance. Before discussing these, it is useful to address the constraints in this study. First, we used stringent criteria for including studies in our database. This could limit representativeness of the results, but it also enhanced the quality and interpretableness of the data. Second, dissertations and unpublished works were not included, again leading to representativeness concerns. To some extent, we had to do this, because most financial incentive studies are embedded in other contexts, making their identification a daunting challenge. As an alternative, we used Rosenthal's (1979) formula for determining how many unpublished studies with disconfirming results would be needed to invalidate our conclusions. For our study, that number was 1,115, large enough to dispel concerns. Besides, as Hunter et al. (1982) noted, meta-analysis is "valid even for 'convenience' samples that just happen to lie at hand" (p. 29). Third, because reliability estimates for performance measures were not available in most studies, we used Hunter et al.'s (1982) artifact distributions method based on limited information. Still, we followed other meta-analysis studies in this (Hackett & Guion, 1985; Mento et al., 1987), and measurement error accounts for a small fraction of artifactual variance in meta-analytic research (Koslowsky & Sagie, 1994). Fourth, as is common in meta-analysis (Guzzo et al., 1987; Wanous et al., 1989), we also made many subjective decisions along the way.

Substantively, this quantitative review reinforces and extends the results of Jenkins' (1986) qualitative review. The conclusion that financial incentives are related to performance quantity is validated by our results, and the effect size is estimated to be .34. These results give greater credence to arguments from expectancy, reinforcement, and goal-setting theories than to arguments from cognitive evaluation theory. Financial incentives may jeopardize intrinsic motivation, as Deci (1971) argued, but their correlation with performance quantity is positive rather than negative. These results also question the Kohn (1993a, 1993b) argument that people do not value money. If money is not important, financial incentives should show no systematic relationship with performance. Obviously, the research evidence amassed over three and a half decades shows otherwise. Our results also substantiate Jenkins' (1986) conclusion that financial incentives do not affect performance quality, although this finding should be viewed with caution because it is based on only six studies.

Our study also confirms Jenkins' (1986) main conclusion that, with respect to the quantity of performance, "positive incentive effects on performance can be found in the laboratory, in the field, and in experimental simulations" (p. 174). After correcting for sampling and measurement artifacts, the positive covariation between financial incentives and performance quantity ranges from .24 to .56, and is significantly different from zero for all three settings. The strongest relationships were observed in experimental simulations (.56), followed by field experiments (.48) and laboratory experiments (.24). Experimental simulations combine the realism of field settings with the controls of laboratory settings, arguably offering the ideal arena to investigate financial incentives dynamics. That the relationships were markedly lower in laboratory settings is worthy of note. It may be that laboratories provide lower bound estimates of relevant effects, because it is virtually impossible to replicate complex field phenomena; money may just carry simpler or different meanings in artificial laboratory environments than it does in real field environments. Whatever the precise reason, setting does affect the observed relationship between financial incentives and performance quantity.

We were intrigued to discover that theoretical framework affected the strength of observed relationships between financial incentives and performance. It may be that the theoretical framework guides the design of the research, affecting the salience of different variables for subjects-respondents. For instance, expectancy and reinforcement theories mandate a clear tie between performance and rewards; goal-setting and cognitive evaluation theories focus not so much on the performance–reward connection but rather on goals or tasks. This differential focus may mean a differential emphasis on financial incentives and, consequently, differential effects. Nevertheless, this issue warrants further investigation not only with respect to financial incentives but with respect to other variables as well. If theoretical framework moderates the effect size for financial incentives, it might also moderate the effect size for other predictors, and it is thus a significant issue to address in other meta-analytic research.

We expected task type to moderate the strength of the relationship between financial incentives and performance quantity, but it did not. Following Deci (1971, 1972a, 1972b), if financial incentives erode intrinsic motivation, then the effects of financial incentives should have been stronger for extrinsic than for intrinsic tasks. It may be that our dichotomization of tasks as intrinsic versus extrinsic was too crude, and that a properly validated intrinsic motivation scale should have been used, as in Jordan (1986). Most studies did not include this information, however, making it impossible for us to do so. Alternatively, it may be that, given the controversy about the theoretical underpinning and construct definition of intrinsic motivation (Jordan, 1986; Vecchio, 1982), approaches used in earlier research biased the results in one direction. In any case, although it appears that task type does not affect the relationship between financial incentives and performance quantity, our review does not address the impact of financial incentives on intrinsic motivation.

Our results offer indirect support for conclusions regarding rewards and intrinsic motivation. In a meta-analytic study, Cameron and Pierce (1994; this study is discussed further in Eisenberger and Cameron, 1996) cumulated empirical evidence on the relationship between different types of rewards and different operationalizations of intrinsic motivation, with a database incorporating different studies than those in our database. The authors argued that the "negative effects of rewards on task interest and creativity have attained the status of myth, taken for granted despite considerable evidence that the conditions producing these effects are limited and easily remedied" (Eisenberger & Cameron, 1996, p. 1154). They concluded further that "the only reliable detrimental effect of reinforcement occurs when the free time spent performing a task is assessed after an expected reward has been presented on a single occasion *without regard to the quality of performance or task completion* [italics added]" (p. 1162). In other words, rewards erode intrinsic motivation under extremely circumscribed conditions. This may in some part explain why task type did not emerge as a moderator in our analysis. Because of the stringent decision rules for inclusion, the studies in our database were carefully controlled investigations. In all likelihood, this eliminated the limited conditions under which intrinsic and extrinsic tasks would show different relationships between financial incentives and performance. Our results, along with those of Cameron and Pierce (1994) and Eisenberger and Cameron (1996), go a long way toward dispelling the myth that financial incentives erode intrinsic motivation.

We could not explore the moderating effects of incentive size (Firsch & Dickinson, 1990; Pritchard & Curtis, 1973; Terborg, 1976; Terborg & Miller, 1978). Although most studies contained information about incentive size, this information could not be coded easily in a way that made between-study comparisons meaningful. Larger incentives probably influence performance more than do smaller incentives. Laboratory studies typically use small incentives. This may have attenuated the relationship between financial incentives and performance in laboratory settings.[2] Exclusion of this variable from our database is a concern. Yet this exclusion probably resulted in a more conservative estimate of the relationship.

Another factor that was not explored in the study was the extent to which interdependencies among employees (e.g., in work groups) may affect performance or the size of the incentive. Equity considerations and perceptions of organizational justice may also be germane in this context. The relationship between financial incentives and performance may well be markedly different when such interdependencies exist. We could not explore these phenomena in the present study, but they are potentially fruitful avenues for future research (see Footnote 2).

Overall, this study underscores the generalizable positive relationship between financial incentives and performance. It emphasizes the wisdom of designing incentive systems carefully; it also highlights the utility of including financial incentives as integral components in theoretical frameworks of organizational behavior and the management of human resources.

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[2] We thank an anonymous reviewer for these ideas.

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