Does Pay Inequality Affect Worker Effort?

Experimental Evidence

Gary Charness and Peter Kuhn*
Department of Economics
University of California, Santa Barbara
Santa Barbara CA 93106

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We study worker behavior in an efficiency-wage environment where co-workers' wages can influence a worker's effort. Theoretically, we show that an increase in workers' responsiveness to co-workers' wages should lead profit-maximizing firms to compress wages. Our laboratory experiments, on the other hand, show that --while workers' effort choices are highly sensitive to their own wages-- effort is *not* affected by co-workers' wages. This casts doubt on the notion that workers' concerns with equity might explain pay policies such as wage compression, or wage secrecy.

*charness@econ.ucsb.edu and pjkuhn@econ.ucsb.edu respectively. Kuhn is corresponding author for this paper.
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1. Introduction

A central component of many efficiency wage models is the notion that workers will withhold effort when they perceive that they have not been paid a fair wage (e.g. Akerlof and Yellen 1990; Bewley 1999). An equally influential notion has been that workers' perceptions of fairness depend, at least in part, on the wages paid to their co-workers (e.g. Frank 1984). Together, these hypotheses have been invoked to explain two compensation practices in firms: wage compression (Akerlof and Yellen 1990, p.265), and wage secrecy (Lawler 1990, pp. 238-242).

Despite the intuitive appeal of this chain of thought, some potential gaps in the argument deserve scrutiny. First, to our knowledge, neither wage compression nor wage secrecy has been explicitly derived as an optimal firm policy in an efficiency wage model.[1] Second, despite an emerging literature on the effects of fairness considerations on worker effort, evidence that worker effort responds to *co-workers'* wages also appears to be absent. For example, while managers seem to believe that workers' perceptions of fairness affect productivity (Bewley 1999), we are not aware of any field studies (experimental or otherwise) of the effects of co-worker wages on effort.[2] Laboratory experiments (e.g. Fehr, Kirchsteiger, and Riedl 1993; Fehr, Kirchler, Weichbold and Gächter 1998; Fehr and Falk 1999; and Charness 2004) tend to find strong evidence of apparent *reciprocity* between workers and firms: 'workers' offered a high wage by 'firms' tend to reciprocate with high effort levels, even in completely anonymous, one-

[1] In our reading, Akerlof and Yellen (1990) come closest to doing this, but only *suggest* wage compression as an optimal firm response to co-worker equity concerns. Harris and Holmstrom (1982) generate optimal wage compression, but from insurance rather than equity motives; Danziger and Katz (1997) extend this model to justify wage secrecy. Frank (1984) derives equilibrium wage compression from co-worker equity concerns using a compensating-differentials argument. Finally, Lazear (1989) argues that wage gaps in tournaments should be attenuated when worker co-operation is important; Lazear's argument does not involve worker equity concerns.

[2] Mas (2006) has recently shown that gaps between actual wages and certain *other* reference points can affect worker performance. Other recent field evidence on social preferences (e.g. Bandiera et al. 2005, Carpenter and Seki 2005, and List 2005) all consider quite different contexts than co-worker equity.

shot interactions when the dominant strategy is for workers to provide no effort at all. Almost all of these studies, however, limit their attention to firms employing a single worker; thus they , too, cannot address the question of whether co-workers' wages affect effort.[3]

If workers' effort levels respond to co-worker wages, does it follow that a profit-maximizing firm will compress wages, relative to productivity or some other standard? For that matter, *do* workers' effort decisions depend on their co-workers' wages? In this paper we examine both of these questions, the first using a simple theoretical model, the latter in a laboratory experiment. Because it is a "gift-exchange" experiment, we argue that our experiment is particularly well suited for detecting 'equity'-driven co-worker wage effects on effort: The very nature of the experiment (a) eliminates influences on worker behavior *other* than fairness considerations, and (b) makes it costless for workers to punish firms quite severely for wage differentials that are perceived to be unfair. Within the context of the experimental literature, our experiments thus extend (with a few modifications) gift-exchange labor-market models to the case where two (differently-productive) workers are employed by each principal.

Our main theoretical results are as follows. We consider alternative cases in which an effort response to co-worker wages occurs equally when own wages exceed or fall short of co-workers' wages (*symmetric*) or only when one is paid less than a co-worker (*asymmetric*); in both cases, an increase in workers' responsiveness to co-worker wages should lead profit-maximizing firms to choose greater wage compression. In the symmetric case, however, most

[3] We are aware of three experimental studies that allow for multiple workers per firm. Güth, Königstein, Kovács and Zala-Mezõ (2001) consider a two-part contract in a principal-agent relationship. Their main result --that making work contracts observable leads to a greater degree of pay compression-- focuses on the behavior of principals; it is not clear in their context whether this behavior is justified by the response functions of agents. Like us, Cabrales and Charness (1999) find no evidence of one agent having much concern about the payoff of the other agent, although the principal's payoff was important for agent behavior. Importantly, neither of these studies involves gift exchange, as principals choose contract menus with (essentially) take-it-or-leave-it options. Finally, Maximiano, Sloof, and Sonnemans (2006) find that effort levels are only slightly lower when there are four contributing workers per firm rather than one, suggesting that gift exchange appears to be robust to changes in the size of the work force.

other outcomes (including effort, productivity, and profits) are (surprisingly) *unaffected* by the strength of workers' equity-related behavior, and the intuition driving wage compression is somewhat unexpected: Workers' responsiveness to relative wages gives firms more 'leverage' in the sense that a smaller wage gap is needed to elicit the fixed, profit-maximizing effort differential between the workers. In the asymmetric case, in contrast, equilibrium levels of effort and output *are* affected by workers' equity-motivated behavior, and firms *are* harmed when workers respond to relative wages.

Empirically, while effort is highly sensitive to own wages in our data, we detect little or no response of effort to co-worker wages. Both the estimated effects of co-worker wages on effort and their standard errors are small in magnitude, allowing us to place fairly tight bounds on these effects, all of which include zero. This result surprised us, given the prevalence of the notion that worker 'jealousy' is so important; in the paper we discuss a number of considerations that, we believe, might help reconcile our results with this common intuition. These include a distinction between words and actions, the limited dimensionality of workers' 'signal space', and the (equally plausible) possibility that workers might care positively rather than negatively about their co-workers' welfare.

2. A Model

Suppose workers' effort decisions depend on their co-workers' wages. How does this affect a profit-maximizing firm's choice of wages and the resulting levels of efforts, output and profits? In exploring this question theoretically, we consider two main cases: In the case of *symmetric* worker responses to co-workers' wages, workers' effort decisions respond equally to an extra dollar of 'overpayment' relative to their co-workers as they respond to a dollar less of 'underpayment'. In the *asymmetric* case, workers' effort decisions respond to underpayment only. In both cases, we consider the effects of an increase in the strength of co-worker wage-sensitive behavior (henceforth the parameter "*b*") on a variety of outcomes, assuming firms set wages to maximize profits.

To be clear, our main motivation in constructing these models (*and* in designing our experiments) is to critically examine the conventional wisdom behind informal discussions of why pay compression exists, not necessarily to construct a complete model of pay setting when co-workers' wages affect utility. In our view, a reasonable summary of these informal arguments, common in Human Resource Management (HRM) textbooks and some economics writings as well, runs as follows: Employers have workers who are differentially productive, a fact which—in a gift-exchange environment—clearly gives employers a profit-based incentive to pay workers differently. But employers are also concerned that large pay gaps between differently-abled workers may reduce profits by creating 'morale' problems. Capturing this fundamental tension between employers' desires to reward abler workers and workers' possible aversion to pay inequality is at the heart of our modeling strategy.

In view of the above goals, we have made two key modeling decisions: first, we keep the model as simple as possible, for example focusing on the case of linear effort supply functions (generalizations are discussed at various points). Second, on the worker side, we begin with an assumption about *behavior* (i.e. an effort-supply function) rather than utility. This allows us to focus on the implications of co-worker wage-sensitive *behavior* for firms' profit-maximizing wage policies, while remaining as agnostic as possible about the precise nature of social preferences underlying worker behavior (an area that remains in considerable dispute). The cost of this is that we cannot, and do not, derive any conclusions about worker welfare, which almost certainly would be highly sensitive to the precise structure of social preferences assumed.[4]

[4] Relatedly, we assume it will be obvious to the reader that our results should be interpreted within a short-run context, assuming that a worker-firm match has already been made and some ex-post quasi rents exist; thus participation constraints do not bind. In an expanded model, any effects of, say, wage compression on profits

a. The Symmetric Case

Imagine two workers in a firm, each of whose effort is given by:

(1) $E = aw + b(w - w_c)$

where w is the worker's own wage, w_c is his/her co-worker's wage, $a > 0$, and $b \geq 0$. Workers respond to co-worker wages when $b > 0$; we refer to $b = 0$ as *difference-neutral* behavior and $b > 0$ as *difference-sensitive* behavior. The formulation in (1) has four noteworthy features.

First, by assumption, when the two workers receive the same wage $(w = w_c)$, a worker who responds to co-worker wages ($b > 0$) exerts the same effort as a worker who does not ($b = 0$). Second, when workers respond to co-worker wages, they reduce their effort below the difference-neutral level if paid less than a co-worker, and raise effort above the difference-neutral level when paid more than a co-worker. These effects are equal in magnitude, their strength given by a single parameter *b.* This second assumption is our 'symmetry' restriction and is relaxed later in this section. Third, and related, the effort supply-functions are assumed to be the same for both workers.[5] Finally, our effort-supply functions are assumed for simplicity to be linear.[6] Since we allow output to be a general, nonlinear function of each worker's effort (see below), linearity of the effort-supply function is however not as restrictive as it might seem.

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identified here could be passed on to consumers; effects on worker utility would be moderated by external labor markets as well.

[5] Among other things, this rules out 'income effects' in the sense that the worker endowed with the higher productivity might choose to consume more leisure at the same offered wage. The asymmetric case studied later in this section considers an extreme case of worker response heterogeneity where one of the two workers has $b = 0$.

[6] We have also examined the more general case, where $E = F(w) + bG(w - w_c)$, where $F(0) = G(0) = 0$, $F' > 0$, and $G' > 0$. In this case, symmetry is defined by the condition $G(x) = -G(-x)$, and difference-neutral behavior by $b = 0$. Some, but not all of the "neutrality" aspects of Result 2 continue to hold; in some very reasonable cases firms' maximized profits now *rise* with *b*. Proofs are available at: http://www.econ.ucsb.edu/~pjkuhn/Data/WageCompression/WageCompMaterialsIndex.html.

Let total revenues produced by a type-1 (low-productivity) worker be given by $R(E)$, revenues from a type-2 worker by $\theta R(E)$, where $R' > 0, R'' < 0,$ and $\theta > 1$.[7] A firm employing one worker of each type thus chooses w_1 and w_2 to maximize profits, given by:

(2) $$\Pi = R\big(aw_1 + b(w_1 - w_2)\big) + \theta\, R\big(aw_2 + b(w_2 - w_1)\big) - w_1 - w_2 .$$

In this case, we are able to generate the following results (all proofs are provided in Appendix 1):

Result 1. When workers' behavior is difference-neutral ($b = 0$), profit-maximizing firms will pay higher wages to their more-productive workers ($w_2 > w_1$), who in turn supply greater effort than the less-productive workers ($E_2 > E_1$).

Result 2. When workers respond to relative wages ($b > 0$), profit-maximizing effort levels for each worker (E_1 and E_2) are identical to the difference-neutral levels identified in Result 1, *regardless* of the value of b. Wages, however, are not identical to the difference-neutral case: profit-maximizing firms compress wages relative to the difference-neutral equilibrium; i.e. $w_1 > w_1^n$ and $w_2 < w_2^n$, where w_1^n, w_2^n denote wages under difference neutrality. As b rises, w_1 rises and w_2 falls, but workers' wage rankings are never reversed, i.e. $w_1 < w_2$ regardless of b. Further, regardless of the level of b, the total wage bill ($w_1 + w_2$), and total profits are *identical* to the difference-neutral case.

A surprising implication of Result 2 is that firms are not hurt by worker behavior that is sensitive to co-workers' wages. The generality of this result is also noteworthy: it holds regardless of the *strength* of workers' effort responses to co-worker wages (b), regardless of the

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[7] Implicit in this formulation is, of course, the assumption that the cross-partial derivative of the production function is zero. An interesting extension would be to consider the effects of worker equity concerns in a framework where the workers' productivities are interdependent.

form of the production function (R), and regardless of the slope of the own labor-supply function (a).[8] To see the intuition for this result recall that in equilibrium total costs are given by $w_1 + w_2 = (E_1 + E_2)/a$; thus *because of symmetry the marginal cost to the firm of inducing an extra unit of effort from either worker is independent of b*.

The other key implication of Result 2 is that, despite the invariance of effort to b, some wage compression is in the interests of a profit-maximizing firm. To see why, consider the effects of a small increase in b, beginning at the difference-neutral ($b = 0$) equilibrium. If after this increase we kept wages at their difference-neutral levels, low-productivity (type-1) workers will work less than before because $w_1 < w_2$, and type-2 workers will work harder. But we have just shown that —because the marginal cost of both E_1 and E_2 is independent of b—firms maximize profits by keeping effort levels unchanged. To achieve this, firms must raise w_1 and cut w_2; i.e., compress wages. In sum, wages are compressed because a smaller wage gap is needed to elicit the *fixed*, profit-maximizing effort levels from both workers. It is perhaps noteworthy that this intuition differs somewhat from that in popular discussions, perhaps because the latter discussions seem to focus only on the effects of wage discounts on low-ability workers, ignoring the potential for wage premia to motivate high-ability workers.

Result 3. When workers respond to relative wages, the optimal amount of wage compression relative to the difference-neutral case is given by:

(3) $$\frac{w_2 - w_1}{w_2^n - w_1^n} = \frac{a}{a + 2b} = \frac{1}{1 + 2(b/a)}$$

[8] It does, however, require our assumption that the two workers respond equally to each others' wages, i.e. have the same b. The asymmetric case (considered below) provides a clear illustration of this, since in that case only the worker receiving the lower wage has a positive b (at the margin).

Equation (3) implies a very specific relation between the strength of workers' responses to co-worker wages and the profit-maximizing degree of wage compression. This relation is true for *any* production function *R*. For example, if workers' effort is one-tenth as sensitive to relative wages as to their own wage ($b/a = .1$), the wage gap should be reduced by 16.7 percent, to 83.3% of its value under difference neutrality. If workers respond equally to own and relative wages ($b=a$), the wage gap should be *one third* of its difference-neutral level. Finally, note that (9) approaches zero from above as *b* grows without bound. Even in the most extreme case imaginable, it is therefore never optimal to pay equal wages to workers of differing ability. [9]

b. The Asymmetric Case

Suppose now that workers only respond to relative wages when they are paid *less* than their co-workers; this corresponds to the Bolton (1991) model of social utility. Specifically, let $E = aw + b\delta(w - w_c)$ where the function $\delta(x) = x$ for $x < 0, \delta(x) = 0$ for $x \geq 0$. Under these conditions we can show:

Result 4. When workers' responses to relative wages are asymmetric in the sense described above, profit-maximizing effort levels deviate from the difference-neutral levels. In particular, $E_1 > E_1^n$ and $E_2 < E_2^n$, i.e. low-ability workers provide *more* effort than in the difference-neutral equilibrium, while high-ability workers provide less. Concerning wages, profit-maximizing firms pay high-ability workers less than their difference-neutral wage, i.e. $w_2 < w_2^n$. Also, for positive *b*, the wage ratio, w_2 / w_1, must be below its difference-neutral level.

[9] Because of its relative simplicity, extensions to the symmetric case are relatively easy to explore. A key question allows for an arbitrary number (*N*) of workers instead of two, and considers the effects of changing the relative numbers of high- and low-productivity workers. These questions are explored in background materials available from the authors, which show that the invariance of effort and profits to *b* extends to the *N*-worker case as well.

Result 5. As *b* rises in the asymmetric case, maximized profits fall. Further, in contrast to the symmetric case, if *b* rises far enough, the firm's profit-maximizing wage policy could assign equal wages to differently-productive workers ($w_1 = w_2$).

Taken together, these results differ substantially from the symmetric case. What explains this? First, note that in contrast to the symmetric case, where the firm's cost function $C(E_1, E_2)$ was unaffected by workers' equity-related behavior (*b*), a higher *b* now raises total costs. Second, compared to symmetry, asymmetry also raises the marginal benefit of raising w_1 relative to w_2, because an increase in worker 1's wage no longer generates an adverse effort response from the other worker, whereas an increase in the more productive worker's wage continues to do so. Together these two factors explain the lower profits and smaller wage gaps under asymmetry than symmetry. The greater familiarity of this intuition compared to that for symmetric case (described earlier) suggests that the informal, implicit theorizing behind previous discussions may have taken for granted the asymmetric scenario. In the empirical work that follows, we test for the presence of both symmetric *and* asymmetric worker responses to co-worker wages.

3. The Experiment

Following Fehr, Kirchsteiger, and Riedl (1993), we model the labor market as a simple gift exchange. The firm moves first, by offering the worker a salary, *S*, which can depend on the worker's type. The worker then selects an effort level, *E*. Payoffs are then:

Principal's payoff ("Profits"): $\Pi = Q - S = Q(E) - S$

Agent's payoff ("Utility"): $U = S - V(E)$.

Clearly, the perfect equilibrium to this game is not efficient. According to standard reasoning, agents should expend no effort and (anticipating this) the principal will pay no salary. In practice, however, it is well known that much more cooperation than this occurs.

As noted, our question in this paper is whether the amount of cooperation (and the surplus generated) is influenced by pay comparisons made between two types of workers (high-productivity and low-productivity) employed by the same firm. In each period, each firm is therefore matched with one worker of each type. We vary whether wages are public (both workers know both wages) or private (each worker knows only his or her own wage).

The experimental instructions are provided in Appendix 2.[10] Each firm is endowed with $4 (lab dollars) in each period, and can pay total wages (in integer amounts) in each period that do not exceed the $4 endowment.[11] If a firm chooses not to spend the entire endowment, it keeps the unspent money, but cannot use any such savings to pay higher wages in later periods. The wages chosen are subtracted from the $4 endowment, and the firm receives the benefits of any revenues produced by the workers. Earnings accumulate over the course of the session, and are then converted from lab dollars to real dollars. Both types of workers had the same conversion rate, while the firm's conversion rate differed. Each worker saw only his or her own (productivity) schedule, while the firm saw both. All of the above was common information to the participants.

Firms were thus allowed to choose from among five salary levels (zero, one, two, three or four lab dollars), and workers could respond with one of four effort levels (zero, low, medium or high). The $Q(E)$ and $V(E)$ functions for both worker types are shown in Table 1.

[10] Since the ordering of treatments varied across sessions, instructions differed slightly to reflect this. The instructions shown are the exact ones for sessions 1 through 4.

[11] We considered the idea of allowing firms to use unspent endowments from previous periods to make wage offers, but rejected it because of the non-stationarity it would add to the firm's decision problem.

[Table 1 about here]

As can be seen, increasing effort is increasingly costly for the workers. Workers receive no direct benefit from providing costly effort, while the firms' profits depend critically on the effort levels chosen.

It was also common information in the experiment that the two workers had different productivity schedules (though the direction and magnitude of these differences was known only to firms). The rationale for this was to approximate real-world labor markets in which workers can be fairly certain their productivity is not identical to their co-workers', but do not have a good way to determine their true relative value to the employer.[12]

We conducted seven sessions at the University of California at Santa Barbara, with 18 students in four of the sessions, 15 students in one of the sessions, and 12 in two sessions (the differences are due to the variance in show-ups at the laboratory). Participants were recruited using an e-mail message to the general student population. Since no person participated in more than one session, there were thus 111 different participants. Average earnings were about $16 for the one-hour sessions.

At the beginning of each session the students were randomly divided into three groups of equal size: firms, low-productivity (type-1) workers, and high-productivity (type-2) workers. Each person stayed in his or her assigned role for the duration of the 30 periods in the session. After each period, the firms and workers were randomly re-matched (with no 3-person group remaining the same from one period to the next). All of this was common information.

[12] It is of course possible that workers would respond more negatively to co-workers' wages in the knife-edge case where they *knew* that both workers were identically productive. That case is not examined here, for important practical reasons in addition to its likely lack of realism: If workers were made equally productive, then in the (very standard) gift-exchange game studied here, firms would have no easily-interpretable motive to offer different wages to their two workers, and might not do so very often. It is also unclear how we should interpret workers' responses to seemingly-arbitrary actions by firms. While one might of course consider *manipulating* firms' wage offers, this would clearly involve deception. Thus, it seems to us that a proper understanding of the equal-productivity case would require constructing a considerably more complex game environment than the current one.

Within each period, each firm first makes a salary payment to both of his or her agents. After these payments are entered in the workers' accounts, all workers decide on how much of a transfer ("effort") to make to the firm, given the costs shown in Table 1. The calibration in Table 1 was chosen (a) to generate non-zero effort levels from the majority of workers in a one-on-one gift-exchange game, based on past results with those games; (b) to embody large productivity differences between the worker types (thus giving firms an incentive to differentiate wages), but (c) to also allow workers to impose high costs on firms by choosing zero effort levels if (for example) the worker were to feel unfairly treated.[13]

Different information structures were implemented over the course of the various sessions. Holding all else constant, with *private wages* each worker is told only his or her own wage in the period, while with *public wages*, each worker learns both wages chosen by the firm. Participants knew that there would be regime changes during the session, but were not told in advance the nature of these changes. In our first four sessions, we had public wages during periods 11-25 and private wages in all other periods. In the remaining three sessions, we had private wages during periods 11-25 and public wages in all other periods.[14]

[13] Table 1 also has the following properties (when all payoffs are measured in lab dollars): the effort level that maximizes total surplus (defined as profits + utility, or $Q(E) - V(E)$) is 'medium' for the low-productivity worker and 'high' for the high-productivity worker. A low effort level by type 1 workers in the presence of a \$1.00 wage results in equal sharing of the \$1.80 surplus from production between the worker (Net Receipts = \$1.00 - \$0.10 = \$0.90) and the firm (Profit = \$1.90 - \$1.00 = \$0.90). Similarly, a high effort level by type 2 workers when assigned a \$3.00 wage results in equal sharing of the \$4.80 surplus that occurs in that event.

[14] A colleague has asked whether, for example, the private wage treatments that occur in periods 26-30 should be combined with earlier private treatments, since workers will acquire some knowledge about the *typical* amounts that firms pay the 'other' worker may during (public wage) periods 11-26. To address this and related possibilities, we replicated our main analysis of worker behavior (Table 3 below) two ways: excluding the final five periods in all our sessions, and excluding the final 20 periods. In the former case, the results are essentially identical. In the second, they are similar but considerably noisier due to the much smaller sample size. That said, since our main interest is in worker responsiveness to co-worker wages within the *public* wage regime, we are not overly concerned that workers may know more about co-worker wages in some private-regime periods than in others.

4. Empirical Results

We begin our analysis by examining workers' mean effort levels for every possible wage-offer combination to the two workers in each firm. These are presented, separately for the private- and public wage regimes, in Table 2.

[Table 2 about here]

Table 2 shows, for example, that when co-worker wages were secret, and when firms offered a wage of 1 to their low-productivity worker and 2 to their high-productivity worker, the mean effort levels of these workers were, respectively, 0.715 and 1.226. This wage offer pair was chosen 137 times by firms-- the most common choice in the wage secrecy regime. In the second-most-common wage-offer pair—(1,3), chosen 79 times—, the average effort level chosen by type-1 workers was 0.937; the average effort chosen by type-2 workers was 1.747. All of the 15 possible wage offer pairs were chosen by at least one firm when wages were private; when wages were public the pair (4,0) was never observed.

Part A of Table 2 shows two results very clearly. First, workers' effort decisions respond very strongly to their own wage: reading down the columns for type-1 workers, or across the rows for type-2 workers, mean effort levels rise essentially monotonically, and precipitously, with own wages. There are only two exceptions to these monotonic increases, involving cells with relatively few observations and 'extreme' wages. These own-wage results are very similar to established patterns in gift-exchange labor markets with one-worker firms (e.g. Fehr et al, 1993), confirming that our design replicates existing results when we suppress information about co-workers' wages. Second, there is no apparent effect of co-workers' wages on effort (going across rows for type-1 workers or down columns for type-2 workers). Since workers were not informed of their co-worker's wage in this wage-setting regime, this is exactly what we should expect, and constitutes an additional specification check for our experimental design.

Part B of Table 2 presents results in exactly the same format for all the experimental rounds in which workers were informed of their co-worker's wage before choosing their own effort levels. In our view, the most striking aspect of Part B is its similarity to Part A: Own wages matter (a lot), but no strong or consistent pattern emerges for the effect of co-worker wages. Holding type 1's wage fixed at $1, we do see a small but monotonic decline in type 1's effort with increasing type 2 wages. At the same time, other comparisons—for example, holding type 1's wage fixed at zero, or holding type 2's wage fixed at 1— go the other way. The surprising similarity between Tables 2A and B is confirmed by some simple statistical tests. For example, an *F*-test for equality between the 14 wage-offer-pair-specific means in Tables 2a and 2b cannot be rejected for either type of worker at any conventional significance level: For type-1 workers, $F(14, 1081) = 1.24$ for a *p*-value of .24; for type-2 workers $F(14, 1081) = 0.52$, for a *p*-value of .92.[15]

More formally, how does a worker's effort respond to both his own wage and his co-worker's wage when both these wages are known to the worker? This question is explored in a regression context in Table 3. Regression serves several purposes here, one of which is to control for period effects that might bias the simple descriptive statistics in Table 2. Examination of the data reveals significant (though not dramatic: even in the final period of each session, 70 percent of firms offered positive wages to their type-2 workers, and 59 percent offered positive wages to their type-1 workers) 'unraveling' in the sense of declining effort levels across periods even within treatments. The regression context also lets us parameterize the effect of other workers' wages in simple ways and conduct significance tests for co-worker wage effects that (a) treat every round as a separate observation, but (b) allow for correlated error

[15] Similar results obtain when we remove worker- and period fixed effects before performing these tests.

terms within subjects by clustering on individuals.[16] Finally, the regression context allows us to ask whether estimated effects of offered wages are different when we look only 'within subjects' (i.e. allowing each subject his/her own effort intercept and examining the effects of different wage offers to the same person).

[Table 3 about here]

Table 3 includes data only from the public-wage regime, i.e., from those rounds in which workers were told (and were thus able to respond to) the co-worker's wage. Part A of the table focuses on type-1 (low-productivity) workers, presenting estimated coefficients from regressions in which effort is the dependent variable. In column 1 we simply allow for a linear effect of the worker's own wage on effort; this directly estimates the parameter *a* in our theoretical model. The effects of own wages on effort are very strong, and highly statistically significant (with a *t*-ratio in excess of 7).[17] Columns 2 and 3 add two alternative measures of the co-worker's wage to the column 1 regression; the first of these (the difference between 'own' and co-worker wages) corresponds exactly to the "symmetric" model in Section 2.[18] The second --the above gap interacted with a dummy variable for whether the worker's own wage is less than his co-worker's-- corresponds exactly to the asymmetric model. Coefficients on both of these measures provide estimates of *b* in our theoretical model; both are small and statistically insignificant. Columns 4 and 5 replicate columns 2 and 3 with more flexible controls for the worker's own wage; while own wage effects remain strong and monotonic, there is essentially no change in the estimated value of *b*. Finally, columns 6 and 7 add worker and period fixed effects in turn.

[16] Clustering on sessions had relatively little effect on the standard errors, in some cases leaving them essentially unchanged, in others raising them by 20 to 25 percent. In all cases own-wage effects remained highly significant.
[17] When we do not cluster the standard errors, co-worker wages remain highly insignificant, while the *t*-ratios on own wages rise to around 12 and 13 (depending on the exact specification).
[18] Of course, if we replaced the relative wage by the co-worker's wage in these regressions, the estimated coefficient would be equal in magnitude and opposite in sign. Thus, *t*-tests for the presence of a relative wage effect are equivalent to t-tests for the presence of any effect of the co-worker's wage on effort.

Overall, in three of six specifications the estimated effect of relative wages on a low-productivity worker's effort is actually negative—counter to what one would expect from a 'jealousy' hypothesis—, but in all cases the effect is statistically insignificant and economically very small. To see this last point, consider the coefficient on the relative wage in column 7, which implies that a decline of one (lab) dollar in one's co-worker's wage raises a worker's effort by .015. This effect is less than *one fortieth* of the effect on effort of having one's own wage raised from one to two lab dollars (1.119 - .420 = .699). A related way to assess the strength and magnitude of our results is to assess their implications for the relative sensitivity of worker effort to co-worker versus own wages, i.e. for the parameter b/a in equation 3. Taking the most conservative approach to standard errors (clustering on sessions), simple F tests allow us to test various hypotheses regarding the relative sizes of these two coefficients. In the linear, symmetric model (column 2) we can reject $b=a$ with a p-value of .0002; p-values for b/a = .5, .2, and .1 are 0.0005, 0.0188 and 0.1960, respectively. Thus, taking the most conservative approach to our standard errors, our data are still rich enough to decisively reject the hypothesis that workers value relative wages by any more than one-fifth the value placed on their own wage. For the asymmetric model in column 3 our results are even stronger: the p-value for b/a = .1 is 0.0335. Thus we can be more than 95 percent confident that workers respond to their relative wages at no more than a *tenth* the rate they respond to the level of their own wage. Clearly, effects of this size do not suggest there will be strong beneficial effects on profits of wage policies that accommodate workers' preferences for inter-worker equity.

Part B of Table 3 focuses on type-2 (high productivity) workers. Overall the results are very similar, with strong and monotonic own-wage effects and insignificant co-worker wage effects. There is no statistically significant effect of relative wages in any specification, and two of four coefficients are actually negative. Here we can reject b/a = .25 with 90% confidence in the linear, symmetric case (column 2); tests of the asymmetric model are less powerful because the abler workers (type 2's) were only rarely paid less than their type 1 co-workers.

In our model (as in most principal-agent models), disutility of effort is a convex monotonic function of effort, while output is a concave monotonic function of effort. Perhaps effects of other workers' wages on a worker's 'performance' would be more apparent if we were to focus on one of these other metrics. To address this question we replicated Table 2B and selected regressions in Table 3 for two alternative dependent variables: effort *costs* and revenues *produced* by the worker. We also estimated an ordered-probit specification of worker effort. The results – available from the authors—are very similar.[19]

While our data come from a laboratory experiment that allows careful control of many aspects of the economic environment, it is noteworthy that the variation in wages that identifies Table 3's main results was not exogenously imposed by ourselves as experimenters. While we considered such an approach, we decided against it, in part to allow for comparability between our results and the large existing literature on gift-exchange labor markets with one-worker firms, which takes a similar approach to ours. More importantly, however, we thought it important for analyses of equity and reciprocity like the current one that the wage offers received by 'workers' come from active participants with a personal financial stake in workers' effort decision. If, for example, the firms in our experiment were *forced* to offer unequal wages to the two worker types, it is not clear that workers would wish to punish firms for doing so. For example, in Charness (2004) workers who have been assigned a low wage by a random process are often nevertheless generous to the firms affected by their effort choices; in Charness and Levine (2005), the path (and the perceived intention of the firm) by which a net wage is

[19] See http://www.econ.ucsb.edu/~pjkuhn/Data/WageCompression/WageCompMaterialsIndex.html

generated affects the effort level chosen by the worker.[20] Similar issues would arise if, on the other hand, the subjects 'worked' directly for the experimenters who varied wages according to some formula; in addition, low worker effort in this context would impose financial costs only on the granting agency that funded the experiments.[21]

A potential shortcoming of our choice not to directly manipulate wages, however, is of course the possibility that offered wages are not orthogonal to other factors that might affect a worker's effort. For example, if an individual could somehow acquire a reputation for being a hard worker (i.e. likely to supply a high effort level), he or she might receive a high wage offer. For a number of reasons inherent in our experimental and econometric design, endogeneity driven by individual reputations is very unlikely to be a problem in our study. The first of these is the random *re-matching of workers with firms*, which dramatically reduces the information a firm can infer about its current worker from past effort decisions (the chances of being re-matched with the same worker of a given type in the next period are approximately one-sixth). Second, note that our preferred econometric specification (column 7 of Table 3) controls for a full set of worker fixed effects; thus the effects on effort of *any* person-specific characteristic (such as 'work ethic') are differenced out of our estimates. Third, and most important for our main research question, is the random *re-matching of workers to co-workers* each period. Thus, *even if* individual (workers or) co-workers had reputations that elicited different wage offers from the firm (which we think is extremely unlikely given points one and two above) the random assignment of a new co-worker in each period should guarantee random assignment of co-worker wages to workers.

[20] In principle, one could implement a design in which firms' offers were randomly assigned by the experimenters, but workers were unaware of this; we rejected this in deference to the norm of non-deception in economics experiments.

[21] Once again, one could hide the fact that wages were randomly assigned from participants, but this would also involve deception.

If individual reputations are not a concern, what about "group" reputations? A worker's type is known to the firm, and it is conceivable that, say, Type 1 workers in a given session might acquire a reputation for being particularly hard-working, thus eliciting higher wage offers than in other sessions. This is, however, not a concern either because (a) all of a worker's co-workers are of the same type, and (b) once again, a worker is randomly matched to a different co-worker each period. We also note that, by construction, our worker fixed effects will automatically absorb any *session*-specific effects on the reputation of a given type of worker because each worker appeared in only one session.

Finally, despite all the above considerations, we recognize that it remains at least conceivable that some feature of subjects' interactions in our design of which we are not aware generates non-randomness in the wages that are offered to one's co-worker, and that this non-randomness might account for our negative result. The main concern would be an unobserved factor, *X*, that both (a) raises a worker's effort in the current period and (b) tends to raise the wage offer received by his *co-worker* in the same period. By generating a positive correlation between own effort and the co-worker's wage, such a factor could in principle disguise a true, negative causal effect of co-worker wages on effort. What could *X* be? Since worker- (and thus session-) *and* period fixed effects are both controlled for in our preferred specification, *X* cannot be a fixed characteristic of either workers (e.g. "work ethic"), sessions (e.g. a "good equilibrium" or the fact that an above-average share of a given session's Type-1 workers happened to be hard workers), or periods (effort tends to fall towards the end of the experiment). Thus, by elimination, *X* must be some aspect of a worker's history at a *given* period *within* a session, that is (a) associated with a worker's effort *and* (b) has effects on the wage his *co-worker* is likely to be offered.

Table 4 examines whether we can find any features of a worker's recent history in this experiment that predict the wage offered to that person's co-worker. Again, we must re-emphasize that we think this extremely unlikely given all the features of our design including the random re-matching of workers to co-workers. Still, if some aspect of our procedure did generate such a relationship, it should be evident in Table 4. Table 4 considers the following indicators of a worker's recent history: the wages received by the worker up to five periods in the past, and the effort choices made by that worker up to five periods in the past. Reported in the table are *p*-values for *F*-tests that the (group of) variable(s) listed in a given row have, as a group, zero effect on the wage offered to that worker's co-worker in the current period. The regressions on which these *F*-tests are based contain a vector of right-hand side variables that is identical to the regressions in the right-most column of Table 3; thus they are informative precisely about the variation in co-worker wages that is net of the covariates held constant in our preferred specification. As the table makes clear, for no specification of the worker's current history can we ever reject the hypothesis that the worker's history has no effect on the wage offered to his co-worker at any conventional level of significance. This is true for both type 1 and type 2 workers. Again, this result does not surprise us, but it does add some confidence to the notion that, given our experimental procedures, co-worker wages are effectively randomly assigned to workers even though they are chosen by the participants representing firms in our experiment.

Finally, to understand the motivation of our subjects in greater depth, we administered an exit survey to participants. In the survey, workers were asked, "In the periods where you saw the wage the firm offered its 'other' worker, to what extent did you consider the other worker's wage when deciding how much effort to supply?" Responses to this question were coded on a five-point scale with 1 indicating "not at all" and 5 indicating that the other worker's wage was the respondent's "primary consideration in choosing [his/her] effort". In all, 40 of 73 workers (55%) chose a value of 3 or greater, indicating a "moderate influence" or greater of co-worker wages on their choices.[22] In order to reconcile these survey results with our subjects' behavior, we asked whether estimated responses to co-worker wages were larger for the subsample of workers who said they considered them. To that end, columns 2 and 4 of Table 5 replicate the regressions in column 7 of Table 3 for the subsample of workers who indicated that their co-worker's wage had at least a moderate effect on their effort decision (columns 1 and 3 consider an identical specification for the symmetric model).

[Table 5 about here]

For the low-ability (Type 1) workers, the estimated effects of co-worker wages on effort, previously positive and insignificant, now become negative and insignificant. For the better-paid Type 2 workers they are positive, but statistically insignificant. Also, these estimated effects for Type 2 workers remain very small in magnitude compared to own-wage effects. In sum, just as casual evidence from real workplaces suggests, workers' *claims* that they care about their co-workers' wages are not at all rare in our data. When we examine worker behavior however, evidence that it responds to co-worker wages is weak at best, even among the subset of workers who say they care about this issue.

5. Conclusion

Anecdotal evidence suggests that it is common, in both individual and collective salary negotiations, for workers to point to wages received by a 'comparison group' as a justification for a wage increase. In the case of individual negotiations, these comparisons often involve an individual's co-workers within the same firm. Recently, some economic theorists have

[22] One worker failed to complete this information. We also collected information on the subjects' college major and gender. In fairly detailed exploratory analysis, we detected no robust correlations between major and either worker

speculated that co-worker comparisons of this nature might lead profit-maximizing firms to compress wage differentials, relative to productivity differentials (e.g. Frank 1984; Akerlof and Yellen 1990).

In this paper, we show that wage compression is in fact profit maximizing in an efficiency-wage context when workers' effort responds to co-workers' wages. This compression occurs whether workers' equity-related behavior is symmetric (effort responds equally to reductions in underpayment as to increases in overpayment), or asymmetric (only the lower-paid worker's behavior depends on his co-worker's wage). That said, especially in the symmetric case, the intuition behind this wage compression is somewhat unexpected: It occurs because worker's responsiveness to their co-workers' wages provide the firm with extra 'leverage', in the sense of reducing the wage gap that is required to generate an given effort gap between the workers. We also find that equity-driven worker behavior reduces firms' profits *only* to the extent that it is asymmetric: symmetric equity-driven behavior has no effects on the firm's costs, equilibrium effort levels, or maximized profits.

Consistent with previous research on gift-exchange labor markets, our experimental results exhibit a strong and robust degree of gift exchange between workers and firms; the larger the wage 'gift' received from the firm, the larger the effort gift each worker provides in return. Surprisingly (to us) however, we can detect little or no overall response of worker effort to co-worker wages in our data. This lack of response occurs despite the fact that, in our experiment, workers who might feel unfairly treated can impose substantial, anonymous 'punishments' (in the form of zero effort) at no cost to themselves.

What might reconcile our experimental results with the widespread intuition that 'jealousy' plays a key role in workers' perceptions of fairness in compensation (and with our priors before we ran the experiment)? We can think of four main considerations that may help

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or firm behavior, and we found modest gender differences, at most. Details are available upon request.

explain this unanticipated result. First, it is of course important to make a distinction between *saying* one cares about relative wages (perhaps for purely strategic reasons) and being willing to act differently because of them. Using a co-worker's wage as a reference point in verbal negotiations (Babcock *et al.*, 1996) is very different from withdrawing effort when one feels a co-worker is paid too much, and most of the anecdotal evidence on this issue refers to the former activity only. Our results thus do not rule out the possibility that wage compression or secrecy might allow managers to lead a "quiet life" (Bertrand and Mullainathan 2003), freer of equity-driven complaints than it otherwise would be.

A second possible factor explaining our surprising results might loosely be called the limited dimensionality of workers' 'signal space'. *If* a worker cared about a co-worker's wages, then we can imagine the worker as wanting to send signals to the firm about the two things he cares about: his own wage *and* his co-worker's wage. The problem, of course, is that the worker has access to only one signal—effort—with which to signal his happiness on these two dimensions. In this situation it does not seem implausible to us that when forced to choose, workers focus on their own pocketbooks rather than their neighbors'. We cannot think of any good reason why this inherent 'dimensionality' problem would be any less severe in the field than in the laboratory.[23]

Third, while the 'jealousy' hypothesis that motivated this paper has considerable intuitive appeal, we note that plausible arguments can also be made for *positive* concerns with co-worker wages: workers have been known to go on strike, or to refuse to cross picket lines, to raise the wages of their co-workers, even when those co-workers are paid more than they are. In fact, in social-preference models such as Charness and Rabin 2002), these concerns are positive rather

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[23] Of course, in some work environments, effort may have a second dimension (such as helping co-workers) that *is* affected by relative pay. Clearly, this is an interesting question for further experimentation, but it seems obvious to us that the one-dimensional effort case is the place where experimental investigation of co-worker wage effects on effort should begin.

than negative. In sum, if plausible stories involving both positive and negative concerns with co-worker wages can be told, we should perhaps not be so surprised to find a zero effect.

Finally, we note that workers' behavior regarding their co-workers in our labor supply experiment is, in fact, quite consistent with experimental results in other contexts involving three-person games with an inactive third party. These include Güth and van Damme (1998) and Kagel and Wolfe (2001), both of whom study ultimatum games with an inactive third party. Contrary to the predictions of the above social preference models, subjects' behavior exhibited very little concern for the inactive party. Fréchette, Kagel, and Lehrer (2003), Fréchette, Kagel, and Morelli (2004a), and Fréchette, Kagel, and Morelli (2004b) perform experiments involving voting and coalitions. In all of these papers, minimal winning coalitions (three voters out of five, for example) are the norm, rather than coalitions including all of the parties in the group. The voters in these minimal winning coalitions almost invariably show little or no concern for the payoffs of the voters who are not part of this coalition. What these results have in common with ours is the notion that, when responding to a party who has influence on one's payoffs (in our case the firm), one is primarily concerned with the relationship with the influential party and any consideration (positive *or* negative) of the powerless party or parties (co-workers in our case) is largely lip service.

In sum, our main empirical result is that workers do not protest 'underpayment' relative to a co-worker by withdrawing effort in a simple gift-exchange labor market. Thus, at least in our experiment, which was expressly designed to identify equity-related effort responses, this behavioral response—which is essential to the argument that wage secrecy or wage compression might be a profit-maximizing policy—does not appear to be present.

Appendix 1: Proofs

Result 1:

First-order conditions for a maximum of profits with respect to w_1 and w_2 can be written respectively as[24]:

(A1) $aR'(E_1)+b[R'(E_1)-\theta R'(E_2)]=1$

(A2) $\theta aR'(E_2)+b[\theta R'(E_2)-R'(E_1)]=1$

where $E_1 \equiv aw_1 + b(w_1 - w_2)$ and $E_2 \equiv aw_2 + b(w_2 - w_1)$ are worker 1's and worker 2's effort respectively. When $b = 0$, (A1) and (A2) simplify respectively to $aR'(E_1)=1$ and $\theta aR'(E_2)=1$. It follows directly (from $R'' < 0$) that $E_2 > E_1$. Since, in the difference-neutral case, each worker's effort is proportional to his/her own wage only ($E_1 = aw_1$; $E_2 = aw_2$) it also follows that $w_2 > w_1$.

Result 2:

Suppose that $R'(E_1)<\theta R'(E_2)$ in the difference-sensitive equilibrium. It then follows from (A1) that $aR'(E_1)>1$, and from (A2) that $\theta\ aR'(E_2)<1$. Together these contradict the supposition. A parallel argument rules out the possibility that $R'(E_1)>\theta R'(E_2)$. The only remaining possibility that satisfies both (A1) and (A2) equates effort levels to those in the difference-neutral equilibrium, i.e., $E_1 = E_1^n$ and $E_2 = E_2^n$.

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[24] One can imagine solutions to this problem where at least one worker's effort level is zero. For simplicity, we assume that $R''(0)$ is large enough to rule this out. Also, for consistency with the standard formulation and for ease of presentation, we model firms' decisions as not subject to any constraints on the total wage bill, $w_1 + w_2$. Such a constraint is imposed for practical reasons (discussed below) in our experiment. In our base-case model, the profit-maximizing wage bill is in fact invariant to b; as a consequence the model's predictions are *identical* to the case of a fixed budget for wages.

Next, recall that wages are related to efforts via the system of linear equations $E_1 = aw_1 + b(w_1 - w_2)$ and $E_2 = aw_2 + b(w_2 - w_1)$. Solving these for w_1 and w_2 yields:

(A3) $$w_1 = \frac{bE_2 + (a+b)E_1}{a^2 + 2ab}$$

(A4) $$w_2 = \frac{bE_1 + (a+b)E_2}{a^2 + 2ab}$$

From previous results we know that $E_1 = E_1^n < E_2 = E_2^n$, and that E_1 and E_2 in (A3) and (A4) are invariant to b. Since both w_1 and w_2 are weighted averages of E_1 and E_2, but the latter assigns a higher weight to E_2, it follows that $w_1 < w_2$. Summing (5) and (6) yields $w_1 + w_2 = (E_1 + E_2)/a$, which is independent of b. Independence of profits from b follows from result in conjunction with the independence of effort levels from b. Finally, differentiating (A3) and (A4) with respect to b (and using $dE_1 = dE_2 = 0$) yields:

(A5) $$dw_1 / db = \frac{(w_2 - w_1)a}{a^2 + 2ab} > 0$$

(A6) $$dw_2 / db = \frac{(w_1 - w_2)a}{a^2 + 2ab} < 0.$$

Result 3. Subtracting (A3) from (A4) for the cases b>0 and b=0 respectively, noting (from Result 2) that effort levels are identical in the two cases, then taking the ratio of the two cases yields the result shown.

Result 4. In the asymmetric case, the first-order conditions for a profit maximum ((A1) and (A2)) become:

(A7) $$(a+b)R'(E_1) = 1$$

(A8) $$\theta\, aR'(E_2) - bR'(E_1) = 1$$

The result for E_1 follows directly from monotonicity of R' in (10). For E_2, solve (A7) for $R'(E_1)$ and substitute into (A8), yielding:

(A9) $$\theta aR'(E_2) = \frac{a+2b}{a+b}$$

Since the RHS of (A9) is increasing in b, E_2 must be decreasing in b. Solving for wages as a function of effort, equations (A3) and (A4) now become:

(A10) $$w_1 = \frac{aE_1 + bE_2}{a(a+b)}$$

(A11) $$w_2 = \frac{E_2}{a}$$

as long as $w_1 < w_2$. Since E_2 declines with b, w_2 must do so as well. Taking the ratio of (A11) to (A10) yields:

(A12) $$\frac{w_2}{w_1} = \frac{E_2 + (b/a)E_2}{E_1 + (b/a)E_2}.$$

Recall that under difference neutrality (b=0), the wage ratio is given by $w_2^n / w_1^n = E_2^n / E_1^n > 1$. According to Result 3, $E_1 > E_1^n$ and $E_2 < E_2^n$ for $b > 0$, which in turn implies that $w_2 / w_1 < w_2^n / w_1^n$.

Result 5. Applying the envelope theorem to the expression for profits yields:

(A13) $$\frac{d\Pi}{db} = \frac{\partial \Pi}{\partial b} = R'(E_1)(w_1 - w_2),$$

which is strictly negative in the relevant region ($w_2 > w_1$). The final claim is proved by example: Let $R(E) = E^{.5}$, choose units of effort so that $a = 1$ and let worker 2 be twice as productive as worker 1, i.e. $\theta = 2$. Computing profit-maximizing effort and wage levels using (A7) and (A9)-(A11) generates values of $w_2 < w_1$ for any b in excess of about .355. Since firms will never wish to reduce w_2 *below* w_1 (note that (A7)-(A11) no longer apply when $w_2 < w_1$ since they are predicated on the low-ability worker receiving the lower wage), we conclude that there is a critical value of b above which a strict egalitarian wage policy maximizes profits.

Appendix 2: Experimental Instructions

Instructions for all Participants:

There are equal numbers of three types of participants in this experiment: firms, type-I workers, and type-II workers. **Once you have been randomly assigned a type, you will have that same type for the whole experiment.** The experiment consists of 30 periods. In each period, each firm will be grouped with two workers (one of each type). Firms and workers are randomly re-matched every period, subject to the restriction that no two workers will ever be paired with the same firm in consecutive periods.

Here is some relevant information:

1. Each firm is paired with two workers, one of each type. **Pairings change every period.**
2. Firms have a fixed endowment each period, and pay total wages each period not to exceed the level of this endowment; wages are restricted to be in whole lab dollars, e.g. $0, $1, $2., etc. **However, firms are not required to offer anyone a wage greater than 0 at any time;** this is a choice for each individual firm in each period.
3. After firms pay wages, **workers observe the wage assigned and choose effort** from one of 4 feasible levels: zero, low, medium, and high. **Firms are informed about each worker's choice of effort.**
4. Each firm receives the endowment plus the revenue generated by each worker's effort level, less the total wages paid.
5. Each worker receives the wage assigned, less the cost of the effort level chosen.
6. Zero effort costs the worker nothing, and yields zero revenue for the firm. The cost of effort increases with the effort level, as does the revenue produced for the firm.
7. Each worker only sees his or her own productivity schedule, while the firm sees the productivity schedules for both worker types.
8. Earnings accumulate for firms and workers over the course of the session.
9. For a given participant type, each lab dollar is worth a fixed number of real dollars. **The conversion rate is the same for both types of workers, but differs for firms.** Your conversion rate will be given on your other instructional materials.

After answering questions regarding these procedures, we will randomly divide participants into the three types of agents.

Do you have any questions?

Instructions for Firms:

At the beginning of each period your account will be credited with 4 lab dollars (**12 lab dollars = $1**), and you will be matched with a team of two workers. Your team will consist of one type-I worker and one type-II worker. You then decide what wages to offer to each individual worker in that team. Wages must be in whole dollars (e.g. $0, $1, $2. etc.) and total wages cannot exceed your $4. endowment. Each of your two workers will be informed of his or her wage and will then choose how hard to work for you. The more "effort" supplied, the more revenues you earn. But effort is costly to the workers.

Each team of workers has one type-I member and one type-II member. Each type faces a different cost for each level of effort. Table 1 shows these, as well as the revenue produced for each effort level. Workers see the same table as you do, except that the column reflecting costs and revenues for the other type of worker is deleted.

Table 1

Effort Level	Cost to Worker	Revenue produced by Type 1 Worker	Revenue produced by Type II Worker
Zero	0	0	0
Low	.10	1.90	2.80
Medium	.30	2.50	4.20
High	.60	2.70	5.40

1) For either worker, zero effort has no cost and produces no revenue for the firm.

2) Low effort by a Type I worker generates revenues of 1.90 lab dollars, while low effort by a Type II worker generates 2.80 lab dollars.

3) Medium effort by a Type I worker generates revenues of 2.50 lab dollars, while medium effort by a Type II worker generates 4.20 lab dollars.

4) High effort by a Type I worker generates revenues of 2.70 lab dollars, while high effort by a Type II worker generates 5.40 lab dollars.

From period 1 through 10, workers will NOT know the wage you pay the other worker in your team; they will only see their own wage. (Workers know that additional information becomes available starting in period 11, but won't know the nature of this information till then.)

From period 11 through period 25, each worker will be told the wage paid to the other worker.

From period 26 through 30, the rules revert back to what they were in periods 1-10: workers see their own wage only.

At the end of round 30, you will be paid $5 for participating in this experiment, plus **$1 for every 12 lab dollars** in your account at that time.

If you have any questions, please raise your hand.

Instructions for Type-I workers:

In each period you and another worker are paired with a firm. This firm will choose wages for you and for the other worker. After being informed of your wage, you then choose how hard to work for your firm. The more "effort" you supply, the more revenues your firm will earn. But effort is costly to you. Table 1 shows these costs and the revenues produced for each effort level. For you, **3 lab dollars = $1**.

Table 1

Effort Level	Cost to **You**	Revenue produced for the Firm
Zero	0	0
Low	.10	1.90
Medium	.30	2.50
High	.60	2.70

1) Zero effort has no cost and produces no revenue for the firm.

2) Low effort generates revenues of 1.90 lab dollars.

3) Medium effort generates revenues of 2.50 lab dollars.

4) High effort generates revenues of 2.70 lab dollars.

From period 11 through period 26, the experimenter will provide you with some extra information about the actions of "your" firm for that period before you decide how much effort to supply.

Your choice of effort will be conveyed to the firm at the end of the period.

At the end of period 30, you will be paid $5 for participating in this experiment, plus $1 for every 3 lab dollars in your account at that time.

If you have any questions, please raise your hand.

Instructions for Type-II workers:

In each period you and another worker are paired with a firm. This firm will choose wages for you and for the other worker. After being informed of your wage, you then choose how hard to work for your firm. The more "effort" you supply, the more revenues your firm will earn. But effort is costly to you. Table 1 shows these costs and the revenues produced for each effort level. For you, **3 lab dollars = $1**.

Table 1

Effort Level	Cost to **You**	Revenue produced for the Firm
Zero	0	0
Low	.10	2.80
Medium	.30	4.20
High	.60	5.40

1) Zero effort has no cost and produces no revenue for the firm.

2) Low effort generates revenues of 2.80 lab dollars.

3) Medium effort generates revenues of 4.20 lab dollars.

4) High effort generates revenues of 5.40 lab dollars.

From period 11 through period 25, the experimenter will provide you with some extra information about the actions of "your" firm for that period before you decide how much effort to supply.

At the end of period 30, you will be paid $5 for participating in this experiment, plus $1 for every 3 lab dollars in your account at that time.

If you have any questions, please raise your hand.

Table 1: Effort Costs and Revenues

Effort Level	Cost to Worker	Revenue produced by Type 1 Worker	Revenue produced by Type 2 Worker
Zero (0)	0	0	0
Low (1)	.10	1.90	2.80
Medium (2)	.30	2.50	4.20
High (3)	.60	2.70	5.40

Table 2: Mean Effort Levels as a Function of Offered Wages

A. Wage-secrecy Regime

		Type-2 Worker's Wage (lab $)				
Type-1 Worker's Wage (lab $)		**0**	**1**	**2**	**3**	**4**
0	Worker 1 effort	0.229	0.000	0.080	0.333	0.333
	Worker 2 effort	0.114	0.727	1.220	2.167	1.590
	N	70	22	50	18	39
1	Worker 1 effort	0.692	0.648	0.715	0.937	
	Worker 2 effort	0.077	0.722	1.226	1.747	
	N	13	54	137	79	
2	Worker 1 effort	1.923	0.800	1.341		
	Worker 2 effort	0.000	0.400	1.098		
	N	13	10	41		
3	Worker 1 effort	2.000	1.000			
	Worker 2 effort	0.000	0.667			
	N	3	3			
4	Worker 1 effort	1.000				
	Worker 2 effort	0.000				
	N	3				

B. Public-wage Regime

		Type-2 Worker's Wage (lab $)				
Type-1 Worker's Wage (lab $)		**0**	**1**	**2**	**3**	**4**
0	Worker 1 effort	0.020	0.167	0.160	0.259	0.192
	Worker 2 effort	0.040	0.533	1.400	1.815	1.654
	N	50	30	25	27	26
1	Worker 1 effort	0.818	0.703	0.648	0.559	
	Worker 2 effort	0.364	0.560	1.276	1.898	
	N	11	91	105	59	
2	Worker 1 effort	1.700	1.429	1.321		
	Worker 2 effort	0.000	0.714	1.179		
	N	10	7	106		
3	Worker 1 effort	3.000	1.429			
	Worker 2 effort	0.000	0.000			
	N	1	7			
4	Worker 1 effort	-				
	Worker 2 effort	-				
	N	0				

Note: mean effort levels were calculated by assigning values of 0, 1,2, or 3 to 'zero', 'low', 'medium' and 'high' respectively. Subjects also used these numbers to enter their effort decisions into the computer.

Table 3: Effects of Wages on Workers' Effort, Public-wage Regime

A. Type 1 Workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own wage	.589 (.081)	.581 (.079)	.594 (.083)				
Own wage =1				.507 (.075)	.523 (.075)	.499 (.083)	.420 (.104)
Own wage =2				1.201 (.163)	1.236 (.167)	1.278 (.172)	1.119 (.172)
Own wage =3				1.450 (.485)	1.503 (.466)	1.544 (.495)	1.308 (.442)
Own wage =4*							
Relative wage **(Own minus co-worker's)**		.007 (.035)		.009 (.036)			
Relative Wage x (Own wage is less than Co-Worker's)			-.007 (.024)		-.010 (.024)	-.024 (.026)	.015 (.042)
Period effects?	No	No	No	No	No	Yes	Yes
Worker effects?	No	No	No	No	No	No	Yes
R squared	.252	.252	.252	.256	.256	.295	.604

B. Type 2 Workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own wage	.536 (.071)	.549 (.076)	.523 (.073)				
Own wage =1				.416 (.110)	.429 (.109)	.347 (.113)	.229 (.146)
Own wage =2				1.092 (.143)	1.131 (.134)	1.045 (.134)	.942 (.163)
Own wage =3				1.614 (.240)	1.757 (.195)	1.718 (.204)	1.500 (.210)
Own wage =4				1.291 (.471)	1.539 (.354)	1.488 (.338)	1.575 (.299)
Relative wage **(Own minus co-worker's)**		-.013 (.055)		-.063 (.059)			
Relative Wage x (Own wage is less than Co-Worker's)			.098 (.072)		.067 (.054)	.056 (.060)	.010 (.089)
Period effects?	No	No	No	No	No	Yes	Yes
Worker effects?	No	No	No	No	No	No	Yes
R squared	.253	.254	.255	.276	.275	.318	.654

Robust standard errors, adjusted for clustering on 37 individual workers, in parentheses. Sample Size for all Regressions is 555. * A wage of 4 was never offered to type-1 workers in any session.

Table 4: Effects of Wage-offer and Effort History on Co-Worker Wage Offers Received

	P-value for (Effect of History Vector on Co-Worker Wage Offer = 0)	
History Indicators (degrees of freedom for F test):	**Type 1 Workers**	**Type 2 Workers**
Own wage, one lag (1, 472) (1, 471)	.767	.520
Own wage, two lags (2, 457) (2, 456)	.863	.786
Own wage, three lags (3, 442) (3, 441)	.591	.710
Own wage, mean of last two periods (1, 472)	.952	.801
Own wage, mean of last three periods (1, 472)	.455	.488
Own wage, mean of last four periods (1, 472)	.878	.601
Own wage, mean of last five periods (1, 472)	.729	.378
Own effort, one lag (1, 472)	.560	.840
Own effort, two lags (2, 457)	.936	.496
Own effort, three lags (3, 442)	.697	.607
Own effort, mean of last two periods (1, 472)	.677	.483
Own effort, mean of last three periods (1, 472)	.819	.521
Own effort, mean of last four periods (1, 472)	.760	.926
Own effort, mean of last five periods (1, 472)	.905	.703
Own wage and effort, one lag (2, 457)	.843	.796
Own wage and effort, two lags (4, 455)	.988	.735
Own wage and effort, three lags (6, 439)	.898	.630
Own wage and effort, means of last two periods (2, 471)	.871	.591
Own wage and effort, means of last three periods (2, 471)	.746	.393
Own wage and effort, means of last four periods (2, 471)	.898	.860
Own wage and effort, means of last five periods (2, 471)	.887	.480

Notes:

All regressions include controls for own wages and a full set of worker and period fixed effects, as in column 7 of Table 3. Sample is for public wage regime only.

For observations where fewer than N lags are available. “mean of last X periods” uses all available lags.

Degrees of freedom are shown for Type 1 workers. These have one more degree of freedom than the tests for Type 2 workers because the Type 1 regressions contain one less covariate: Type 1 workers were never offered a wage of 4, so this fixed effect is not included.

Table 5: Effects of Wages on Workers' Effort, Public-Wage Regime: Regressions for the Subsample of Workers who Indicated they took Others' Wages into Account

	Type 1 workers		**Type 2 workers**	
	(1)	(2)	(3)	(4)
Own wage =1	.495 (.132)	.523 (.138)	.128 (.145)	.157 (.159)
Own wage =2	1.254 (.224)	1.308 (.226)	.784 (.208)	.849 (.220)
Own wage =3	1.892 (.491)	1.894 (.446)	1.197 (.397)	1.441 (.396)
Own wage =4*			1.592 (.433)	2.006 (.367)
Relative wage (Own minus Co-worker's)	-.022 (.055)		.099 (.080)	
Relative wage x Own wage less than Co-Worker's		-.048 (.060)		.076 (.104)
N	330	330	270	270
R squared	.532	.534	.706	.703

Robust standard errors, adjusted for clustering on workers, in parentheses. All regressions include a full set of period and worker fixed effects.

* A wage of 4 was never offered to type-1 workers in any session.

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