NBER WORKING PAPER SERIES

INEQUALITY AT WORK: THE EFFECT OF PEER SALARIES ON JOB SATISFACTION

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Working Paper 16396
http://www.nber.org/papers/w16396

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2010

We are grateful to Stefano Dellavigna, Lawrence Katz, and seminar participants at UC Berkeley, Harvard, and the Federal Reserve Bank of San Francisco for many helpful comments. We are grateful to the Center for Equitable Growth at UC Berkeley for research support. This paper reflects solely the views of the authors and not necessarily the views of the institutions they belong to, nor those of the National Bureau of Economic Research.

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NBER Working Paper No. 16396
September 2010
JEL No. J0

ABSTRACT

Economists have long speculated that individuals care about both their absolute income and their income relative to others. We use a simple theoretical framework and a randomized manipulation of access to information on peers' wages to provide new evidence on the effects of relative pay on individual utility. A randomly chosen subset of employees of the University of California was informed about a new website listing the pay of all University employees. All employees were then surveyed about their job satisfaction and job search intentions. Our information treatment doubles the fraction of employees using the website, with the vast majority of new users accessing data on the pay of colleagues in their own department. We find an asymmetric response to the information treatment: workers with salaries below the median for their pay unit and occupation report lower pay and job satisfaction, while those earning above the median report no higher satisfaction. Likewise, below-median earners report a significant increase in the likelihood of looking for a new job, while above-median earners are unaffected. Our findings indicate that utility depends directly on relative pay comparisons, and that this relationship is non-linear.

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1 Introduction

Economists have long been interested in the possibility that individuals care about both their absolute income and their income relative to others.[1] Relative income concerns have important implications for microeconomic and macroeconomic policy,[2] and for understanding the impact of income inequality.[3] Recent empirical studies have documented a systematic correlation between measures of relative income and reported job satisfaction (e.g., Clark and Oswald, 1996), happiness (e.g., Luttmer, 2005), or health and longevity (e.g., Marmot, 2004). Despite confirmatory evidence from laboratory experiments (e.g., Fehr and Schmidt, 1999), the interpretation of the empirical evidence is not always straightforward. Relative pay effects pose a daunting challenge for research design, since credible identification hinges on the ability to isolate exogenous variation in the pay of the relevant peer group.

In this paper we propose and implement a new strategy for evaluating the effect of relative pay comparisons, based on a randomized manipulation of access to information on co-workers' wages.[4] Following a court decision on California's "right to know" law, the Sacramento Bee newspaper established a website (www.sacbee.com/statepay) in early 2008 that made it possible to search for the salary of any state employee, including faculty and staff at the University of California. In the months after this website was launched we contacted a random subset of employees at three UC campuses, informing them about the existence of the site.[5] A few days later we surveyed all campus employees to elicit information about their use of the Sacramento

[1]The early classical reference is Veblen (1899). Post-war formal analyses begin with Duesenberry's (1949) relative income model of consumption. Easterlin (1974) used this model to explain the weak link between national income and happiness. Hamermesh (1975) presents a seminal analysis of the effect of relative pay on worker effort. Akerlof and Yellen (1990) provide an extensive review of the literature (mostly outside economics) on the impact of relative pay comparisons.

[2]For example, Boskin and Sheshinski (1978) show how optimal taxation is affected by relative income concerns. More recently, Grossman and Helpman (2007) develop the implications of relative wage concerns for the optimal extent of off-shoring. Fuhrer and Moore (1995) introduce relative wage concerns in an overlapping contract macro wage model.

[3]Most of the work on inequality has focused on the explanations for the rise in earnings inequality in recent decades (see reviews by Katz and Autor, 1999; Acemoglu, 2002; and Acemoglu and Autor, 2011). However, there is less work on the question of why inequality *per se* is a matter of public concern.

[4]A number of recent empirical studies in behavioral economics have used similar methods that manipulate information–rather than the underlying economic parameters–to uncover the effects of various policies. See Hastings and Weinstein (2009) on school quality, Jensen (2008) and Nguyen (2008) on returns to education in developing countries; Chetty, Looney, and Kroft (2009) on sales taxes, Chetty and Saez (2009) on the Earned Income Tax Credit, Kling et al. (2008) on Medicare prescription drug plans.

[5]Initially the website was relatively unknown. Even as late as June 2009, when we conducted the last of our three surveys, only about 40% of employees who had not been directly informed about the site through our experiment report being aware of its existence.

Bee website, their pay and job satisfaction, and their job search intentions. We compare the answers from workers in the treatment group (who were informed of the site) and the control group (who were not). We use administrative salary data matched to the survey responses to compare the effects of the information treatment on individuals who were earning above or below the median pay in their unit and occupation, and estimate models that allow the response to treatment to depend on an individual's salary relative to the median for his or her unit and occupation.[6]

Theoretically there are two broad reasons why information on peer salaries may affect workers' utilities. Much of the existing relative pay literature assumes that workers' preferences depend directly on their salary relative to their peers'. Alternatively, workers may have no direct concern about co-workers' pay but may use peer wages to help predict their own future pay. We structure our empirical analysis to test between these competing models. The models have different predictions on how information on co-worker salary affects utility.

In the relative utility model, we assume that individuals value their position relative to co-workers in the same pay unit and occupation, and that in the absence of external information, people have imperfect information on their co-workers' wages. Accessing information on the Sacramento Bee website allows people to revise their estimates of co-worker pay. If job satisfaction depends linearly on relative pay, information revelation has a *negative* effect on below-median earners and a *positive* effect on above-median earners, with an average impact of zero. If job satisfaction is a concave function of relative pay, as in the inequality-aversion model of Fehr and Schmidt (1999), the negative effect on below-median earners is larger in magnitude than the positive effect on above-median earners, and information revelation causes a reduction in average job satisfaction.

The predicted pattern of impacts is quite different in a model where people have no direct concern over co-worker wages, but rationally use information on peer salaries to update their future pay prospects. If co-worker wages provide a signal about future wages, either through career advancement or a bargaining process, learning that your own wage is low (high) relative to co-workers' salaries will lead to update your expected future wage upward (downward). In this model, the revelation of co-workers' salaries *raises* the job satisfaction of relatively low-wage workers and *lowers* the satisfaction of relatively high-wage workers. This is exactly the opposite

[6] Most pay units coincide with departments. In terms of occupation, we distinguish between faculty and administrative staff.

of the pattern predicted by the relative utility model. Our simple randomized design allows us to measure the causal impacts of information revelation for workers at different points in the salary distribution and distinguish between the alternative models.

We obtain three main findings. First, informing UC employees about the Sacramento Bee website has a large and highly significant impact on the fraction who use the site. In the absence of treatment we estimate that only about one-quarter of UC employees had used the site. Our treatment more than doubled that rate. Most new users (80%) report that they investigated the wages of colleagues in their own department or pay unit. This strong "first stage" result establishes that workers are interested in co-workers' wages – particularly the pay of peers in the same department – and that information manipulation is a powerful and practical way to estimate the effects of relative pay on workers.

Second and most importantly, we find that access to information on co-workers' wages has significantly different effects on employees with salaries above and below the median in their department and occupation group. In particular, the information treatment causes a *reduction* in pay and job satisfaction and an increase in planned job search for those whose wages are below the median in their department and occupation group. By comparison, those who are paid above the median experience no significant change in any of these outcomes. Allowing the response to treatment to depend on the gap between an individual's own wage and the median of his or her pay unit, we find that job satisfaction of treated workers is increasing in relative wages for those with wages below the median, but flat thereafter. These patterns are consistent with the inequality aversion theory and laboratory results of Fehr and Schmidt (1999), and inconsistent with an alternative model in which workers learn about their own future pay opportunities from co-worker wages.

Third, learning about co-worker pay affects individual views about pay fairness and inequality. We find that access to information on co-workers' wages leads to a reduction in the fraction of below-median earners who think that wages are set fairly at the University of California. We also find some weaker evidence that access to information about co-worker pay increases concerns about nationwide income inequality, although this increase appears similar for low and high earners.

Our empirical results provide credible field-based evidence confirming the importance of the relative pay comparisons that have been identified in earlier observational studies of job satisfaction (Clark and Oswald, 1996; Hamermesh, 2001; Lydon and Chevalier, 2002) and happiness (Frey and Stutzer, 2002; Luttmer, 2005), and in some (but not all) lab-based studies.[7] Specifically, they lend support to a strong version of inequality-aversion (Fehr and Schmidt, 1999) in which negative comparisons reduce workers' satisfaction but positive comparisons have little or no impact.

Our results also contribute to the literature on pay secrecy policies.[8] Many U.S. companies (close to one-third of firms in one recent survey) have "no-disclosure" contracts that forbid employees from discussing their pay with co-workers. Such contracts are controversial and are explicitly outlawed in several states. Our finding of an asymmetric impact of access to wage information for lower-wage and higher-wage workers suggests that employers have an incentive to maintain pay secrecy, since the cost to low-paid employees is greater than any benefit received by their high-wage peers.

The remainder of the paper is organized as follows. Section 2 presents a simple theoretical framework for structuring our empirical investigation. Section 3 describes the experimental design and our data collection and assembly procedures. Section 4 presents our main empirical results. Section 5 concludes.

2 A Simple Theoretical Framework

In this section we lay out two simple models that illustrate how information on co-worker pay may affect job satisfaction. We are particularly interested in understanding how the relation between job satisfaction and information on co-workers' pay may differ for those whose wage is above the average wage of their co-workers and those whose wage is below the average of their co-workers. We begin with the case where workers care directly about relative pay, as in the models suggested by Clark and Oswald (1996), for example. We then consider an alternative scenario in which people do not care about relative pay, but use information on their co-workers' pay to form expectations about their own future pay. In both cases we assume that

[7]See e.g., Fehr and Falk 1999, Charness 1999, Fehr at al. 1998, Fehr et al. 1993, Fehr and Schmidt, 1999, Charness and Rabin, 2002, Kuziemko et al. 2010 for lab evidence of relative pay effects. Note however that in an experimental effort game, Charness and Kuhn (2007) find that workers' effort is highly sensitive to their own wages, but unaffected by co-worker wages.

[8]The seminal work on pay secrecy is Lawler (1965). Futrell (1978) presents a comparison of managerial performance under pay secrecy and disclosure policies, while Manning and Avolio (1985) study the effects of pay disclosure of faculty salaries in a student newspaper. Most recently Danziger and Katz (1997) argue that employers use pay secrecy policies to reduce labor mobility and raise monopsonistic profits.

in the absence of the website, people know their own salary with certainty and have imperfect information on their peers' salary. With access to the website they have complete information on co-workers' salary.

2.1 Model 1 – Relative Utility

Consider a worker whose own wage is w and who works in a unit with an average wage m. For simplicity we will assume that wages within each unit are symmetrically distributed (so mean and median wages in the unit are the same), and that agents who lack complete information hold Bayesian priors. Let I denote the information set available to the worker: $I = I^0$ will denote the information set in the absence of access to the Sacramento Bee website, and $I = I^1$ will denote the information set with access to the site. For the sake of the model, our experiment can be thought of as changing the information set from I^0 to I^1. In practice, our experiment has "imperfect compliance", in the sense that some members of the control group have information I^1 and some members of the treatment group have information I^0. We defer a discussion of this issue until section 2.3, below.

Assume that the worker's utility, or job satisfaction, given the information set I, can be written as

$$S(w, I) = u(w) + v(w - E[m|I]) + e, \tag{1}$$

where e is an individual-specific term representing random taste variation.[9] With suitable choices for the functions $u(.)$ and $v(.)$, this specification encompasses most of the functional forms that have been proposed in the literature on relative pay. Importantly, $v(.)$ represents feelings arising from relative pay. Those feelings depend on information about co-workers' pay, and that information may never be revealed, explaining why the expectation term in (1) is inside the function $v(.)$ rather than outside. We assume that in the absence of the website, individuals only know their own salary, and that they hold a prior for m that is centered on their own wage, i.e.,

$$E[m|I^0] = w.$$

[9]We ignore cost of effort because it is not affected by the information treatment, and therefore is on average the same for the group of workers who receive the information treatment and the control group of workers who do not.

Under these assumptions, job satisfaction in the absence of external information is

$$\begin{aligned} S(w, I^0) &= u(w) + v(w - E[m|I^0]) + e \\ &= u(w) + e, \end{aligned}$$

where we assume without loss of generality that $v(0) = 0$. With access to the website we assume that individuals can observe m perfectly.[10] Then job satisfaction conditional on using the the website is

$$\begin{aligned} S(w, I^1) &= u(w) + v(w - E[m|I^1]) + e \\ &= u(w) + v(w - m) + e. \end{aligned}$$

Let D represent an indicator for whether an individual is informed or not. Then job satisfaction can be written as

$$S(w, m, D) = u(w) + D \cdot v(w - m) + e. \tag{2}$$

This equation provides a complete description of an idealized experiment in which members of the control group have $D = 0$ and members of the treatment group have $D = 1$. For such an experiment the treatment response function

$$R(w, m) \equiv E[S(w, m, 1) - S(w, m, 0)|w, m]$$

identifies the relative pay concern function $v(w - m)$.

We consider two variants of the relative utility model. First, as a basis case, we consider the possibility that the function $v(.)$ is linear:

$$v(w - m) = b \cdot (w - m),$$

where $b \geq 0$ is a constant. The assumption of linearity implies that the part of the utility function that represents relative utility is symmetric around the average salary in the unit. In other words, the additional utility experienced by worker with wage \$$x$ above the unit average wage is equal (in absolute value) to the additional disutility experienced by a worker with wage

[10] Complete information is a strong assumption, and can be relaxed by assuming that access to the website provides a noisy signal of the true mean wage of co-workers. This addition does not substantively change our theoretical model.

$\$x$ below the unit average wage. In this case, observed job satisfaction for members of the control and treatment groups, conditional on (w, m, D) is given by:

$$S(w, m, D) = u(w) + b \cdot D \cdot (w - m) + e. \tag{3}$$

Inspection of this equation leads to three simple predictions when job satisfaction is a linear function of the relative pay gap $w - m$:

L1: treatment reduces job satisfaction of those with $w \leq m$, and increases it for those with $w > m$.

L2: the average effect of treatment is 0.

L3: holding constant m, the effect of treatment is increasing in w.

An alternative to the linear case is inequality-aversion (see Fehr and Schmidt, 1999). According to this assumption, the negative effect of being below-average for one's comparison group is stronger than the positive effect of being above-average. A simple specification of inequality aversion is a piece-wise linear model for v:

$$v(w - m) = b_0 \cdot (w - m) \cdot 1(w \leq m) + b_1 \cdot (w - m) \cdot 1(w > m)$$

with

$$b_0 > b_1 \geq 0.$$

With inequality aversion job satisfaction, conditional on (w, m, D), can be expressed as

$$S(w, m, D) = u(w) + b_0 \cdot D(w - m) \cdot 1(w \leq m) + b_1 \cdot D(w - m) \cdot 1(w > m) + e. \tag{4}$$

Inspection of this equation leads to four predictions for the effect of treatment with inequality aversion:

IA1: treatment reduces job satisfaction for those with $w \leq m$.

IA2: treatment weakly increases job satisfaction for those with $w > m$.

IA3: the average effect of treatment is weakly negative.

IA4: holding constant m the effect of treatment is increasing in w, with a slower rate of increase once $w > m$.

2.2 Model 2 – Co-worker Wages as a Signal of Future Wages

While some economists have hypothesized that relative pay exerts a direct effect on job satisfaction, a plausible alternative is that pay satisfaction is based only on individual salaries, but people use their co-workers' wages to help predict their own pay in the future. In this section, we consider a standard and rational model of information updating where worker utility depends on absolute salary but not on relative salary. If workers have imperfect information on their future salary path or their outside pay opportunities, peer salaries may be useful in predicting the range of possibilities for future pay negotiations with the current employer, or the level of outside pay opportunities.[11] In this case, information on peers' salaries can be interpreted as a signal of the level of pay that the worker can expect in the future, either through career advancement or bargaining with their employer.

Formally, suppose that people evaluate their job satisfaction based on their current wage w and on the net present value of their expected future wages w' given their information set I:

$$S(w, I) = w + \beta E[w'|I] + e, \tag{5}$$

where $\beta > 0$ is a discount factor and the linearity assumption is made for simplicity (see our discussion below). We assume that future wages are normally distributed and that individuals hold a conjugate prior centered on their current wage with precision q (i.e., their prior is $w' \sim N(w, 1/q)$).[12] In addition, individuals who receive the information treatment observe a noisy signal about their future wage from their peers' average wage m. In particular, we assume that

$$m = w' + u,$$

where u is assumed to be normally distributed with mean 0 and precision k, independent of w'.[13] The larger is k, the more informative is the signal. Rational workers form expectations about future wages by efficiently combining their prior and the signal:

$$E[w'|I^1] = (1 - \lambda)w + \lambda m$$

[11] For example, if people believe that their employer has a strict pay ceiling, then learning that a colleague pay is above that ceiling increases the probability of obtaining a higher wage in the future.

[12] Assuming that the mean of w' is $(1 + g)w$ where g is a common growth factor would not change any of our results.

[13] This assumes that peer wages are an unbiased signal of future wages. We could easily incorporate more general signals with no substantive change in the model.

where $\lambda \equiv k/(q+k)$ represents the relative precision of the signal. In this case a worker who receives the information treatment has job satisfaction

$$S(w, I^1) = (1 + \beta(1-\lambda))w + \beta\lambda m + e.$$

In the absence of external information, no signal is received, and individuals form expectations based only on their priors. As a consequence, job satisfaction in the absence of external information is

$$S(w, I^0) = (1+\beta)w + e.$$

Observed job satisfaction for members of the control and treatment groups, conditional on (w, m, D) is given by

$$S(w, m, D) = (1+\beta)w + D \cdot b' \cdot (w - m) + e \tag{6}$$

where $b' \equiv \beta\lambda$.

Although this equation has the same form as equation (3) above (replacing $u(w)$ with $(1+\beta)w$ and b with b'), it is important to note that when people learn about their own future wages from co-worker pay, the effect of access to information on job satisfaction is *increasing* in the gap between m and w, rather than decreasing. Intuitively this is because the further an individual is below the mean for his or her peers, the greater is his or her expected growth of w in the future. Workers in the treatment group who find out that their peers have a higher (lower) wage update upward (downward) the net present value of all their future wages. On average half the workers have a positive surprise and half have a negative surprise, though the surprise is perfectly negatively correlated with an individual's relative wage position.

Thus, when individuals learn about future pay from their co-workers' wages we have three main predictions:

FP1: treatment raises job satisfaction of those with $w \leq m$, and decreases it for those with $w > m$.

FP2: the average effect of treatment is 0.

FP3: holding constant m, the effect of treatment is decreasing in w.

The first and third of these predictions are the *opposite* of the corresponding predictions for the case when relative pay enters linearly into job satisfaction. In this model there is an additional prediction about the effect of information revelation on expectations of future wage increases. Specifically:

FP4: treatment will lead workers with $w \leq m$ to expect a larger wage increase in the future than they would otherwise anticipate, and workers with $w > m$ to expect a smaller wage increase .

It is possible to extend this learning model to the case where workers value income in each period using a concave utility function $u(w)$:

$$S(w, I) = u(w) + \beta E[u(w')|I] + e. \tag{7}$$

In this case, highly paid workers are still negatively surprised by the revelation of co-worker salaries, while low-paid workers are positively surprised.[14] However, with concavity the positive surprises experienced by lower-wage workers lead to a relatively large gain in satisfaction, while the negative surprises experienced by high-wage workers lead to relatively smaller reductions in satisfaction. Thus, with concavity the average change in satisfaction is positive.[15]

2.3 Empirical Implementation

This section describes how we test the predictions of the alternative models. We first discuss the issue of imperfect compliance. We then turn to a discussion of the empirical models that we fit to the data and the empirical tests that we perform. These tests directly follow from the predictions of the models in sections 2.1 and 2.2.

2.3.1 Incomplete Compliance

In the simplified theoretical framework above we have assumed that all individuals who receive the treatment access the information in the salary web site, and none of the individuals in the control group does. In practice, however, our experiment has incomplete compliance. Prior to our experimental intervention some employees of the UC system had already used the Sacramento Bee website. After our information treatment not everyone who was informed about the

[14] Ex ante workers are indifferent between learning or not learning about co-worker wages because $E[u(w')|I^0] = E[E[u(w')|I^1]|I^0]$ However, we have assumed that worker's expectations are biased, so ex post workers with low wages are positively surprised while workers with high wages are negatively surprised.

[15] This will be true for any concave utility function, including a reference point utility function where there is a concave kink at the reference point (Kahneman and Tversky, 1979).

existence of the website decided to use it.[16] Thus some members of the control group were informed, while some members of the treatment group were uninformed. As in other experimental settings this incomplete compliance raises potential difficulties for the interpretation of our empirical results.

The models discussed above imply that job satisfaction, conditional on actual informed status, can be written as

$$S = u(w) + D \cdot v(w - m) + e,$$

for some functions u and v. Let T denote the treatment status of a given individual ($T = 0$ for the control group; $T = 1$ for the treatment group), and let

$$\pi_0 = E[D|T = 0, w, m]$$

$$\pi_1 = E[D|T = 1, w, m]$$

denote the probabilities of being informed conditional on treatment status, individual wages, and peer mean wages. With this notation, we can write

$$S = u(w) + \pi_0 v(w - m) + T \cdot (\pi_1 - \pi_0)v(w - m) + e + \phi, \quad (8)$$

where ϕ is an error component reflecting the deviation of an individual's actual information status from his or her expected status.[17] Under the *assumption* that the "information treatment intensity"

$$\delta \equiv \pi_1 - \pi_0$$

is constant across individuals, equation (8) implies that the observed treatment response function in our experiment is simply an attenuated version of the "full compliance" treatment effect, with an attenuation factor of δ. As in a simpler model with a homogeneous treatment effect, we can therefore inflate the coefficients of the estimated treatment response function using an estimate of δ from a first-stage linear probability model that relates the probability of using the website to treatment status and the other observed characteristics of an individual.

In the more general case in which the information treatment varies with w and m the experimental response is more complex, and reflects a combination of the variation in the information

[16] It is likely that some members of the treatment group failed to read our initial email informing of the website. Others may have been concerned about clicking a link in an unsolicited email, and decided not to access the site.

[17] Formally, $\phi = [D - T\pi_1 - (1 - T)\pi_0)]v(w - m)$. This term is mean-independent of the conditioning variables in π_0 and π_1.

treatment effect $(\pi_1 - \pi_0)$ and the difference in satisfaction in the presence or absence of information $(v(w-m))$. In our main analysis below we begin by estimating a variety of "first stage" models that measure the effect of the information treatment on use of the Sacramento Bee website, including models that allow the treatment effect to vary with functions of $(w-m)$. Importantly, we find that the information treatment intensity is independent of the observed characteristics of individuals, including their wage and relative wage. This allows us to interpret our satisfaction models as variants of equation 8 with an attenuated treatment response.

2.3.2 Econometric Models

Motivated by the simple predictions arising from the models described above, we fit two main models to the measures of job satisfaction collected in our survey. First, based on equations (3), (4), and (6), we fit models of the form:

$$S = g(w,x) + a \cdot 1(w \leq m) + b_0 \cdot T \cdot 1(w \leq m) + b_1 \cdot T \cdot 1(w > m) + \mu, \tag{9}$$

which include controls for individual wages and other covariates (x), a dummy for whether the individual's wage is less than the median in his or her pay unit and occupation, and interactions of a treatment dummy with indicators for whether the individual's wage is below or above the median for his or her pay unit and occupation. We consider several tests of the estimated coefficients from this model. First, we consider the test that the treatment effects are jointly zero:

$$Test \quad 1 : b_0 = 0, b_1 = 0.$$

Assuming that the observed treatment response function in our data is simply a rescaled version of the "full compliance" response function described by the competing models, this can be interpreted as a general test of whether information about co-workers' pay affects job satisfaction at all. This test cannot distinguish why or how information about co-workers' pay might affect job satisfaction.

Second, we consider the test that lower-wage people have lower or higher job satisfaction when informed about the website:

$$Test\ 2 : b_0 < 0 \ vs.\ b_0 > 0.$$

This test distinguishes between a model in which relative wages have a direct effect on satisfaction (and people prefer to be paid more than their peers) and one in which people learn about their own future wages from their peers' salaries.

Third, we consider a comparison between the effect of information on people with wages below the median for their pay unit, and the effect on people with wages above the median:

$$Test \ \ 3: |b_1| = b_0 \ \ vs. \ \ \ |b_1| < b_0.$$

This distinguishes between a model with a linear effect of the relative wage on job satisfaction and a model with a strictly concave response to relative wages (as predicted by inequality aversion).

Our second set of empirical models are motivated by the hypothesis of inequality aversion and focus directly on the shape of the treatment response function. These models have the form

$$S = g(w, x) + c_0 T + c_1 T \cdot w + c_2 T \cdot (w - m) \cdot 1(w > m) + \mu, \tag{10}$$

and include controls for individual wages and other covariates (x), a dummy for treatment status, an interaction of treatment status with the individual's wage, and a second interaction between the wage and treatment status that "turns on" once the wage exceeds the median in the individual pay unit. Here we consider one test for the joint significance of the treatment effects:

$$Test \ \ 5: c_0 = 0, c_1 = 0, c_2 = 0,$$

a second test for the presence of a "kink" in the response to the comparison wage once the individual's wage exceeds the comparison wage:

$$Test \ \ 6: c_2 = 0 \ \ vs. \ \ c_2 < 0.$$

As discussed below, we apply the tests to three complementary measures of "job satisfaction": one that asks people about the satisfaction with the level of pay on the job; a second that asks about their overall satisfaction with the job; and a third that asks whether they intend to look for a new job in the coming year.

3 Design, Data Collection, Summary Statistics and Selection Issues

3.1 Experimental Design and Data Collection

In March 2008, the Sacramento Bee posted a searchable database at www.sacbee.com/statepay containing individual pay information for California public employees including workers at the University of California (UC) and the California State system. Although public employee salaries have always been considered "public" information in California, in practice access to salary data was extremely restrictive and required a written request to the State or the University of California. The Sacramento Bee database was the first to make this information easily accessible.[18] At its inception the database contained pay information for calendar year 2007 for all UC workers paid $20,000 and over (as well as monthly pay for all other state workers).

3.1.1 Information Treatment

In the Spring 2008, we decided to conduct an experiment to measure the reactions of employees to the availability of information on the salaries of their co-workers. We elected to use a randomized design with stratification by department (or pay unit). Ultimately we focused on three UC campuses: UC Santa Cruz (UCSC), UC San Diego (UCSD), and UCLA. Our information treatment consisted of an email (sent from a special email account established at UC Berkeley) informing the recipient of the existence of the Sacramento Bee website, and asking recipients to report whether they were aware of the existence of the site or not. The emails were sent in October 2008 for UCSC, in November 2008 for UCSD, and in May 2009 for UCLA. The exact text of the email was as follows:

> "We are Professors of Economics at Princeton University and Cal Berkeley conducting a research project on pay inequality at the University of California. The Sacramento Bee newspaper has launched a web site listing the salaries for all State of California employees, including UC employees. The website is located at www.sacbee.com/statepay or can be found by searching "Sacramento Bee salary database" with Google. As

[18] Prior to March 2008, other local newspapers (the San Francisco Chronicle and the San Jose Mercury) had posted online databases on top earners at the University of California (defined as workers paid over $200,000 in the year). In the two years after the SacBee posted the comprehensive information online, others have also posted the comprehensive information. The SacBee updates its website annually when new compensation information is made available.

part of our research project, we wanted to ask you: Did you know about the Sacramento Bee salary database website?"

About 25% of people who received these emails responded by filling out a 1-question survey on their knowledge of the site. Since the answers are only available for the treatment group we do not use these responses in the analysis below.

Our experimental design is summarized in Table 1. We collected online directories at each of the three campuses to use as the basis for assignment. These directories contain employees' names, job titles, departments, and email addresses.[19] At each campus, a fraction of departments was randomly selected for treatment (two-thirds of departments at UC Santa Cruz; one-half at the other two campuses). Within each treated department a random fraction of employees was selected for treatment (60% at UC Santa Cruz, 50% at UC San Diego, 75% at UCLA). Our original design targeted 40% of employees at UC Santa Cruz, 25% of employees at UC San Diego, and 37.5% of employees at UCLA to receive treatment. As indicated in column 2 of Table 1, the actual fractions receiving treatment were relatively close to these targets.[20]

The stratified treatment design was chosen to test the possibility of peer interactions in the response to treatment.[21] Specifically, we anticipated that employees who received the information treatment might inform colleagues and co-workers in their department about the site. As we show below, however, any within-department spillover effects appear to have been very small in our experiment, and in our main analysis we therefore focus on simple comparisons between people who were directly treated versus those who were not, though we cluster the standard errors for all models by department to reflect the stratified design.

[19] Two practical issues need to be highlighted. First, the definition of a department in the online directories is admittedly imperfect. Smaller administrative units (such as research centers within departments) are sometimes coded separately, leading to the presence of many "departments" with very few employees. We address the problem posed by very small units by pooling these units to the campus level. This issue is particularly important at UC Santa Cruz, which has many small departments. We experimented with a number of ways to address this problem and settled on the simple rule that employees in units with less than 15 individuals who can be matched to salary data are "pooled" to a campus-wide unit. We tried an alternative cutoffs of 10 and 20 and obtained very similar results for our main specifications. Overall, some 17% percent of individuals are assigned to the pooled unit, with a higher rate at UCSC (43%) than at UCSD or UCLA (11% and 17%, respectively). A second practical issue is that, since our treatment and survey are administered by email, we omit all employees who do not have a UC email address. In the UC system, it is rare not to be assigned an email address.

[20] There is wide variation in the size of departments (from a handful in some departments to over 1000 at the Business School at UCLA). To keep our design simple we decided to randomize across departments with no regard for department size. This created some imbalance in the fraction of employees assigned to treatment.

[21] Such interactions were present in the response to the information treatment considered by Duflo and Saez (2003) who studied the effects of a benefits fair on retirement savings plan participation at a large University.

As indicated in the third column in Table 1, we also randomly selected one-quarter of departments at UCLA as "Placebo treatment" departments. The placebo treatment informed people about a UC website listing the salaries of top UC administrators, and invited them to fill out a 1-question survey on their knowledge of the site. Within these departments 75% of individuals were randomly selected to receive the placebo treatment. We use the group of workers who received the placebo treatment to assess the validity of our interpretation of the evidence. Like workers in the treatment group, workers in the placebo group received an email about salary differences within the UC system. But unlike the email received by workers in the treatment group, the email received by workers in the placebo group provides no information about peers' salary. Therefore, if our interpretation of the evidence is correct, the estimated effect of the placebo treatment should be limited or null. On the other hand, if the mere experience of receiving an email about salary differences in the UC system affects job satisfaction for reasons other than relative utility or learning, the estimated effect of the placebo treatment should be close to the effect of information treatment.

3.1.2 Second Stage Survey

The second stage of our design consisted of a follow-up survey, emailed to 100% of employees at each campus some 3-10 days after the initial treatment emails were sent. The survey (reproduced in the appendix) included questions on knowledge and use of the Sacramento Bee website, on job satisfaction and future search intentions, on the respondent's age and gender, and on the length of time they had worked in their current position and at the University of California. The survey was completed online by following a personalized link to a website.

In an effort to raise response rates we randomly assigned a fraction of employees at the first two campuses in our experiment to be offered a chance at one of three $1000 prizes for people who completed the survey. Again, we used a stratified design detailed in column 4 of Table 1: all employees in one-third of departments were offered the incentive; and one-half of the employees in another third of departments were offered the incentive. The selection of departments (and individuals) to receive the incentive offer was made independently of the selection to receive the original information treatment. Based on the positive reaction to the incentive offer at UCSC and UCSD, we decided to extend the incentive to everyone in the UCLA survey. In all, just over three-quarters of employees at the three campuses were offered the response incentive, and a total of nine respondents across the three campuses won $1000 each.

For our surveys at UCSC and UCSD we also randomly varied the amount of time between the information treatment and the follow-up survey: employees in one-half of departments were emailed the survey 3 days after the initial treatment emails; employees at the other half were emailed the survey 10 days after. Our initial analysis of the data for these two campuses showed no systematic differences in response rates or in the effect of treatment, so we decided to simplify the design for UCLA and send all the follow-up surveys 10 days after the information treatments. At all three campuses, we sent up to two additional email reminders asking people to complete the follow-up survey.

3.1.3 Matching Administrative Salary Data

Our final dataset combines treatment status information, campus and department location, follow-up survey responses, and administrative data on the salaries of employees at the University of California. The salary data – which were obtained from the same official sources used by the Sacramento Bee – include employee name, base salary, and total wage payments from the UC for year 2008. We matched the salary data to the online directory database by employee name. Specifically we matched observations from the online directories used as the basis for random assignment with the salary file by first and last name, dropping all cases for which the match was not one-to-one (i.e., any cases where two or more employees had the same first and last name).

Appendix Table 1 presents some summary statistics on the success of our matching procedures. Overall, we were able to match about 76% of names from our online directories to the salary database. The match rate varies by campus, with a high of 81% at UCSD and a low of 71% at UCSC. We believe that these differences are largely driven by differences in the quality and timeliness of the information in the online directories at the three campuses. Some evidence in support of this conjecture is provided by the fact that the survey response rate was significantly higher for people we could match to the wage data (21.4%) than those we could not match (17.7%). This pattern would be expected if some of the names that could not be matched to the salary data were for former employees who were no longer working at the university.

3.2 Models for Response to the Follow-up Survey and Selection Correction

Overall, just over 20% of employees at the three campuses responded to our follow-up survey. While comparable to the response rates in many other non-governmental surveys, this is still a relatively low rate, leading to some concern that the respondent sample differs systematically from the overall population of UC employees. A particular concern is that response rates may be affected by our information treatment, potentially confounding any measured treatment effects on job satisfaction.

Table 2 presents a series of linear probability models for the event that an individual responded to our follow-up survey. The models in columns 1-2 are fit to the overall universe of 41,975 names that were subject to random assignment (based on the online directories). The models in columns 3-6 are fit on the subset of 31,887 names we were able to match to the administrative salary data. The baseline model in column 1 includes additive effects for our three primary experimental manipulations: (1) receiving the information treatment; (2) receiving the placebo treatment; (3) being informed of the lottery prize for survey respondents.[22] As discussed above, the information treatment and placebo treatment were offered to a random subsample of people in randomly selected departments. Likewise, the response incentive was offered to everyone in some departments, and a fraction of people in other "partially incentivized" departments. To allow for spillover effects in the information treatment and placebo treatment we include a dummy for direct assignment to treatment/placebo status, and a second dummy for people who were in treated or placebo departments but not treated or offered the placebo (with the omitted group being people in departments where no one received the information or placebo treatment). Similarly, we include separate indicators for people who were in departments where everyone was informed of the response incentive, people who were offered the response incentive in departments with a 50% offer rate; and people in the partially incentivized departments who were *not* offered the incentive (with the omitted group being people in departments where no one was offered the incentive). The baseline model also includes a dummy if the individual could be matched to the administrative salary data, and a full set of interaction of campus and faculty/staff status.[23] For comparison with this model, column 2

[22]Adding effects for whether the follow-up survey was emailed 3 or 10 days after the initial information treatment has no effect on the estimation results, as would be expected given our randomized design.

[23]We define faculty status based on job title in the directories. There is likely a small amount of misclassifi-

shows a model in which potential spillover effects from the information treatment, the placebo treatment, and the response incentive are all set to zero.

The coefficient estimates for the models in columns 1 and 2 point to several interesting conclusions. First (as suggested by the simple comparisons in Appendix Table 1) the response rate for people who could be matched to the administrative salary data is significantly higher (roughly +3.4 percentage points) than for those who could not. Second, assignment to *either* the information treatment or the placebo treatment had a significant negative effect on response rates, on the order of -3 to -5 percentage points. This pattern suggests that there was a "nuisance" effect of being sent two emails that lowered response rates to the follow-up survey independently of the content of the first email. Third, being offered the response incentive had a sizeable positive (+4 percentage point) effect on response rates. Finally, none of the three primary manipulations appear to have had within-department spillover effects. An F-test for exclusion of all the spillover effects (reported in the bottom row of the table) has a p-value of 0.85. The estimates of the individual assignment coefficients are also very similar whether the spillover effects are included or excluded (compare column 1 and column 2).

The models in columns 3-6 of Table 2 repeat these specifications on the subset of people who can be matched to wage data, with and without the addition of a cubic polynomial in individual wages as an added control. As would be expected if random assignment was correctly implemented, the latter addition has little impact on the estimated coefficients for the various assignment classes, though it does lead to some increase in the explanatory power of the model (the R-squared increases from 0.008 for the model in column 3 to 0.011 for the model in column 4). Again, tests for exclusion of all the spillover effects are insignificant, with p-values in the range of 30-40%. Finally, in anticipation of the treatment effect models estimated below, the model in column 6 allows for a differential treatment effect on response rates for people whose wages are above or below the median of their pay unit, defined as the intersection of their department and their faculty/staff status (i.e., the faculty in one department are treated as a separate pay unit from the staff). The estimation results in column 6 suggest that the negative response effect of treatment assignment is very similar for people with above-median wages (-4.03%) and below-median wages (-3.60%), and we cannot reject a homogeneous effect. We also fit a variety of richer models allowing interactions between wages and treatment status,

cation error in the determination of faculty status.

and allowing a potential kink in the effect of wages at the median of the pay unit. In none of these models could we reject the homogeneous effects specification presented in column 5.

Overall, we conclude that *all three* of our experimental manipulations – assignment to the information treatment, assignment to the placebo treatment, and assignment to the response incentive – had significant effects on response rates to our follow-up survey. Importantly, the effects of the information treatment and placebo treatment are very similar, suggesting that it was the nuisance of being contacted twice that lowered the response rate of the treatment group, rather than the content of the treatment email. Although the negative effect of the information treatment is modest in magnitude (representing about a 15 percent reduction in the likelihood of responding), it is highly statistically significant, and poses a potential threat to the interpretation of our estimates of the effect of treatment, which rely on data from survey respondents.

To address this concern we take two approaches.

1. First, we fit a set of selection-corrected estimates of the effect of treatment (i.e., Heckit models) that use assignment to the response incentive as a determinant of the response probability that can be excluded from the second stage model of job satisfaction.

2. Second, following Lee (2009) we drop observations from the control group until the fraction of included control group respondents matches the fraction of included treatment group respondents. By selectively dropping either people with the most positive job satisfaction responses, or those with the most negative responses, we obtain bounds on the impact of selective non-response.

3.3 Summary Statistics and Comparisons by Treatment Status

Table 3 presents a series of summary statistics comparing people who were assigned to receive our information treatment and those who were not. For simplicity we refer to these two groups as the treatment and control groups of the experiment.[24] Beginning with our overall sample, the fractions of employees classified as faculty and the faction who can be matched to wage data are very similar between the treatment and control groups. The third column of the table reports a t-test for equality of the means for the two groups, taken from a linear regression model that also

[24] Here the control group includes the group of workers who received the placebo treatment.

includes campus effects (which control for the differential treatment rates at the three campuses). The t-tests (clustered by department to reflect the stratified design) are not significant for either variable. Next we focus on the subset of employees who can be matched to wage data. Base earnings (which exclude over-time, extra payments, etc.) are slightly higher for the treatment group than the control group ($t = 2.04$), but the gap in *total* earnings (which include over-time and supplements like summer pay and housing allowances) is smaller and not significant. Similarly, neither the fraction with total earnings less than $20,000 or the fraction with total earnings over $100,000 are different between the two groups. As noted above, however, the fraction of the treatment group who responded to our follow-up survey is about 3 percentage points lower than the rate for the controls, and the difference is highly significant ($t = 4.54$). Finally, the bottom panel of Table 3 presents comparisons in our main analysis sample, which consists of the 6,437 people who responded to our follow-up survey (with non-missing responses for the key outcome variables) and can be matched to administrative salary data. This sample is comprised of about 85% staff and 15% faculty, with mean total earnings of around $67,000 per year. Data from the follow-up survey suggest that sample members are about 60% female, and have relatively long tenure at the University and in their current position. None of these characteristics are very different between the treatment and control groups, and in fact within the analysis sample the probability of treatment is statistically unrelated to age, tenure at UC, tenure at the current job position, gender, and wages.[25]

4 Empirical Results

4.1 Effect of the Information Treatment on Use of the Sacramento Bee Website

We now turn to our main analysis of the effects of the information treatment. Throughout this section, we restrict attention to the subsample of survey respondents in our main analysis sample, although we include some specifications that use a selection correction term derived from the larger sample. We begin in Table 4a by estimating a series of linear probability models that quantify the effect of our information treatment on use of the Sacramento Bee web

[25] We fit a logit for individual treatment status, including campus dummies (to reflect the design of the experiment) and a set of 15 additional covariates: 3 dummies for age category, 4 dummies for tenure at the UC, 4 dummies for tenure in current position, a dummy for gender, and a cubic in total wages received from UC. The p-value for exclusion of the 15 covariates is 0.74.

site.[26] The mean rate of use reported by the control group is 19.1%. As shown by the model in column 1, the information treatment more than doubles that rate (by +28% to a mean rate of 49%). The spillover effect of being in a department where other colleagues were informed of the treatment (but not being directly informed) is very close to zero, and the estimated effect of treatment is similar when we restrict the spillover effect to zero (column 2). This indicates that the spread of information about the web site by word of mouth was limited.

In column 3 we include a selection correction term (inverse Mill's ratio) estimated from a first stage probit model for responding to our survey, fit to the sample of all individuals who can be matched to the salary data. In the probit model we include the same controls as used in the models in columns 1-3, plus a dummy indicating whether the individual was offered a probabilistic monetary response incentive. Since a random subset of individuals surveyed were offered the monetary incentive, this is a plausible exclusion restriction. The estimation results show no evidence of selectivity bias in the effect of the information treatment among survey respondents: the coefficient estimate for the treatment dummy is essentially the same as in column 2, and the coefficient on the selection correction term is very close to 0 and relatively precise. Column 4 shows a model in which we add in demographic controls (gender, age dummies, and dummies for tenure at the UC and tenure in current position). These controls have some explanatory power (e.g., women are about 5 percentage points less likely to use the website than men with $t = 4.3$), but their addition has no impact on the effect of the information treatment.

Our theoretical framework suggests that there are potentially interesting interactions between the information treatment and an employee's relative position in the wage structure of his or her pay unit. As noted in Section 2.3.1, however, interpreting any differential response to the treatment is complicated if people with different relative wages responded differently in their use of the Sacramento Bee website. The models in columns 5 and 6 of Table 4a address this potential complication. The specification in column 5 allows separate treatment effects for people paid above or below the median for their pay unit. As in Table 2, we define *pay unit* as the intersection of department and faculty-staff status. The estimated treatment effects are very similar in magnitude and we easily accept the hypothesis of equal effects (p=0.76, reported

[26] All the models include controls for campus and faculty/staff status (fully interacted) as well as a cubic polynomial in total salary received from the UC. The faculty/staff and salary controls have no effect on the size of the estimated treatment effect but do contribute to explanatory power.

in bottom row of the table). The specification in column 6 allows a main effect for treatment, and an interaction of treatment status with salary, with a potential kink in the interaction term when salary exceeds the median salary in the pay unit. The interaction terms are very small in magnitude and again we easily accept the hypothesis of a homogenous treatment effect at all relative salary levels (p=0.84).

We have fit many other interaction specifications and, consistent with the models in Table 4a, found that the information treatment had a large and relatively homogeneous effect on the use of the Sacramento Bee website. The estimated effect of treatment is a little larger at UCSC (33%, standard error = 5%) than at the other two campuses (UCSD: 28%, standard error =2%; UCLA: 28%, standard error = 2%) but we cannot reject a constant treatment effect (p=0.62). The estimated treatment effect is also somewhat larger for faculty (32%, standard error 3%) than for staff (28%, standard error 2%), but again we cannot reject a constant effect at conventional significance levels (p=0.18). On balance, we believe the evidence is quite consistent with the hypothesis that the information treatment had a homogeneous effect on the use of the web site.

Having shown that our information treatment increased the use of the salary web site, a second interesting question is what kinds of information the new users actually checked at the site. We gathered information on the uses of the web site only in our UCLA survey. Specifically, we asked whether people had looked at the pay of: (1) colleagues in their own department; (2) people in other departments at their campus; (3) colleagues at other UC campuses; (4) "high profile" people like coaches, chancellors, and provosts. The answers to this question were not mutually exclusive, so respondents could choose two or more of these answers. Table 4b reports estimated linearly probability models (fit to the UCLA sample) for 6 alternative dependent variables.

The first, in column 1, is just a dummy for any use of the Sacramento Bee site. For simplicity we show only two specifications: one with a single treatment effect, the second with separate treatment effects for people with salaries above or below the median in their pay unit. The results for this dependent variable mirror the results in Table 4a and show a large and homogeneous treatment effect on use of the site. The second variable (column 2) is a dummy equal to 1 if the individual reported using the site **and** reported looking up the salaries of colleagues in his/her own department. Here the combined treatment effect is 24.2 percentage points. Benchmarked against the treatment effect of 27.9 percent for any use of the site, this estimate suggests that among "new users" who were prompted to look at the site by our information treatment, 87% (=24.2/27.9) examined pay of colleagues in their own department. Columns 3-5 show similar models for using the web site and investigating colleagues in other departments at the same campus, colleagues at other campuses, and high profile people. In all cases we find relatively large and homogeneous effects of our information treatment. The implied marginal use rates of the "new users" caused by our treatment are 54% for colleagues in other departments at the same campus, 27% for colleagues at other campuses, and 34% for high profile people.

Overall the results in Table 4b confirm that people who became informed about the Sacramento website because of our treatment were very likely to use the site to investigate the pay of their closest co-workers (defined as those in the same department). We take this as direct evidence that the department is a relevant unit for defining relative pay comparisons. This may also explain why we fail to find any spillover effects of the information treatment within departments: If workers look-up primarily the pay of their peers' in the department, they might not want to bring it up with their colleagues, and risk being perceived as invading the privacy of their colleagues.

4.2 The Effect of Peer Salary Disclosure on Job and Salary Satisfaction and Mobility

4.2.1 Baseline Models

We turn now to models of the effect of the information treatment on various measures of an employee's satisfaction with his/her job and pay. We consider three measures of satisfaction. The first – which we call *wage satisfaction* is based on responses to the question:

> "How satisfied are you with your wage/salary on this job?"

Respondents could choose one of four categories: "very satisfied", "somewhat satisfied", "not too satisfied", "not at all satisfied". The second – which we call "job satisfaction" is based on responses to the question:

> "In all how satisfied are you with your job?"

Respondents could choose among the same four categories as for "wage satisfaction". The third, which we call "job search intentions" is based on responses to the question:

> "Taking everything into consideration, how likely is it you will make a genuine effort to find a new job within the next year?"

Respondents could choose one of three categories: "very likely", "somewhat likely", "not at all likely". We treat the answers to these questions as arbitrarily scaled responses from a single latent index of satisfaction, and assume that the unobserved components of satisfaction are normally distributed, implying an ordered probit response model for each measure.

Appendix Table 2 reports the distributions of responses to these questions among the control and treatment groups of our analysis sample. We also show the distribution of responses for the controls when they are reweighted across the three campuses to have the same distribution as the treatment group. In general, UC employees are relatively happy with their jobs but less satisfied with their wage or salary levels. For example, about 85% of the control group say they are somewhat satisfied or very satisfied with their job, but only 50% express the same sentiment about their salary. Despite their professed job satisfaction, just over one-half say they are somewhat likely or very likely to look for a new job next year. Close inspection of the distributions of responses between the treatment and control groups of our experiment reveal few large differences. Indeed, simple chi-square tests (which make no allowance for the design effects in our sample) show the distributions of job satisfaction and job search intentions are very similar (p=0.99 for job satisfaction, p=0.43 for search intentions) between the groups. There is a clearer indication of a gap in wage satisfaction (which is somewhat lower for the treatment group), and the simple chi-square test is significant (p=0.05) for this measure.

Tables 5 and 6 present estimates of a series of ordered probit models for these 3 measures of satisfaction. The models in Table 5 follow the specification of equation (9) and include treatment effects interacted with whether the individual is paid above or below the median for his/her unit. The models in Table 6 follow the specification of equation (10) and include a main effect for treatment, and an interaction of treatment with the individual's wage that allows a kink at the median salary of the pay unit.

We begin with the basic models in columns 1, 5, and 9 of Table 5, which include only a simple treatment dummy. None of the estimated treatment effects from this simple specification are significant, though the point estimate for wage satisfaction is negative ($t = 1$) while the point estimate for search intentions is positive ($t = 1.2$), suggestive of a tendency for a negative average impact on satisfaction. All the models include controls for a cubic in wage, interacted with campus and occupation (staff/faculty). The coefficients on these controls (not reported in the table) indicate that in the range of observed wages, higher wages are associated with higher job and wage satisfaction, and lower probability of looking for a new job.

Allowing for differential treatment effects for those with below-median and above-median wages (columns 2, 6, 10 of Table 5) suggests that the small average effect masks a larger negative impact on satisfaction for below-median wages, coupled with a zero or very weak positive effect for those with above-median wages. Indeed, restrictive models that assume no treatment effect on above-median workers (columns 3, 7, and 11) fit as well as ones that allow an effect on this group, and show a pattern of negative treatment effects on job satisfaction and positive effects on search intentions. The effects on wage satisfaction and overall job satisfaction are only marginally significant ($t = 1.7 - 1.8$) but the impact on search intentions of lower-wage workers is highly significant ($t = 2.8$).

The coefficients on the indicator for workers whose wage is less than the median in the relevant pay unit are negative and statistically significant in columns 2, 3 and 4. This indicates that in the control group low wage individuals are less satisfied with their wage than high wage individuals. The difference in columns 6, 7 and 8 is not statistically significant. This indicates that while low wage individuals in the control group are less satisfied with their wage than high wage individuals, they are not less satisfied with their job, possibly because of the existence of a difference in job amenities other than wages. Finally, models that include additional demographic controls (columns 4, 8, 12) show about the same pattern as the simpler specifications, albeit with somewhat lower levels of significance for the treatment effect estimates.

In the context of the theoretical discussion in section 2, the findings in Table 5 are more consistent with a model in which relative pay comparisons play a direct role in worker's utility than with a model in which they learn about future pay opportunities from the salaries of co-workers.[27] Moreover, the negative impact of information on below-median workers coupled with the absence of any positive effect for above-median workers is consistent with a strong form of inequality aversion in which pay levels over the reference rate have no impact on satisfaction.

[27] This interpretation is consistent with some of the feedback we received from survey respondents. For example, one male staff member at UCLA in the treatment group wrote us the following email: "Alerted by your email I checked the salaries in Sacramento Bee and based on what I saw and in addition to other facts I handed in my resignation. [...] I am attaching the letter I wrote addressed to Dr. [name withheld], Director of [Department withheld]. My last day of work is [date withheld]." In his resignation letter, this worker explicitly compares his salary with the salary of other employees in the same department with similar job title.

As an additional test, we analyzed responses to a simple question about expected future salary increases that was asked of respondents at UCLA only. Specifically, we asked: *"Do you expect to receive a salary increase in the next 3 years over and above the standard cost of living adjustment?".* As noted earlier, our learning model predicts that members of the treatment group with wages below the mean for their pay unit will have an increased likelihood of execting a pay increase (relative to the control group), while the reverse is true for those with wages above the mean for their unit (see Prediction FP4). Appendix Table 3 reports a set of simple linear probability models in which the dependent variable is 1 for people who said that they expect a raise over and above any cost of living adjustments. We find no significant difference in the treatment effect for people with wages above or below the median for their unit, though the point estimates are *the opposite* of the predicted pattern from the learning model. In the Appendix table, we also present a model for the probability of expecting a future wage increase that includes a treatment dummy and an interaction of treatment with the individual's wage. In this specification the interaction term is significantly positive ($t = 2.1$), contrary to the prediction that low-wage people who are informed about co-worker wages wages will expect a wage increase, while high-wage people will expect a wage cut.

The specifications in Table 6 are based directly on a piece-wise linear variant of inequality aversion. The simplest specifications in columns 1, 5, and 9 include only a treatment dummy and an interaction of treatment with an individual's wage. These models suggest a negative information treatment effect on the lowest wage individuals that is offset by a higher wage, though the two terms are not very precisely estimated, except in the job search intention model where the estimates are significant. The specifications in columns 2, 6, and 10 allow a kink in the treatment response function at the median wage of the pay unit: this additional term is highly significant in the pay satisfaction and job satisfaction models (though not in the search intention model) and its addition leads to much larger and more precisely estimated coefficients for the treatment dummy and the interaction with wages.

The pattern of estimates suggests an important non-linearity in the interaction between the treatment effect and individual wages: higher wages reduce the negative effect of the information treatment, but only for those whose wage is less than the median of their pay unit. Once an individual's wage exceeds the median for his or her unit, there is no additional effect. Test statistics for the hypotheses that the marginal effect of wages is zero once the wage exceeds the median are reported in the bottom row of the table are are insignificant in all cases. The specifications in columns 3, 7, and 11 impose the assumption of no marginal impact once wages exceed the median: for all three dependent variables this specification fits as well as the corresponding unrestricted kink model, and yields a relatively precisely determined pattern of effects. Finally, as an additional check we re-estimate the unrestricted kink models adding additional demographic controls in columns 4, 8, and 12. This addition slightly weakens the significance of the estimated treatment effects.

The apparent kink in the interaction between wages and treatment evident in the models in Table 6 is consistent with the absence of any treatment effect for above-median workers in Table 5. Both sets of specifications imply that treatment has little effect on above median workers, but a negative effect on below-median workers. This finding lends support to a strong form of inequality aversion in agents' preferences: people appear to value having a higher wage relative to the median for their colleagues, but only insofar as their wage is less than the median. Once their pay is above the median for their colleagues, the salience of relative wage comparisons falls off sharply.

4.2.2 Robustness to Selection Biases Due to Survey Nonresponse

We noted in the discussion of Table 2 that people who were exposed to our information treatment had lower response rates to our follow-up survey than those who were not. In this section we probe the robustness of our inferences about the effect of treatment on job satisfaction to potential selection biases. We follow two complementary strategies. First, we fit conventional selection-correction models that include an inverse Mills ratio term estimated from a probit model for the probability of responding to the follow-up survey. We take advantage of random assignment of the incentive that we introduced to raise response rates, and use a dummy for whether an individual received the incentive as a variable that affects the probability of responding to the survey but has no direct effect on satisfaction. Though we think it is unlikely that the incentive affected satisfaction directly, we acknowledge that this cannot be directly tested. Thus, as an alternative we follow Lee (2009) and consider bounds for our estimated treatment effect models based on trimming extreme observations from the control group until the implied fraction of respondents is equal to the rate of the treatment group. In our context the bounds are fairly informative because our dependent variables are all categorical variables with only 3 or 4 response categories.

Table 7 shows the implied estimates from these two procedures. For simplicity we limit attention to the model specifications reported in Table 6, which include a treatment dummy and a pair of terms allowing a piece-wise linear interaction between treatment status and the individual's wage, with a potential kink at the median for his/her pay unit. For ease of comparison, the models in columns 1, 5, and 9 simply reproduce the baseline estimates from columns 2, 6 and 10 of Table 6. The next set of columns include the inverse Mills ratio term, estimated from a probit model for responding to our follow-up survey (and providing non-missing responses for the main variables of interest).[28] Interestingly, the coefficient of the selection correction term has the opposite sign in the models for wage satisfaction and job satisfaction, although in neither case is the estimate statistically significant. The addition of the selection correction term leads to an attenuation of the estimated treatment effects on wage satisfaction (and on job search intentions), but a magnification of the effects in the model for job satisfaction, though the basic patterns of the effects is quite similar to the pattern in the baseline estimates. Overall, the selection corrected estimates do not suggest a significant or economically important bias in the simpler models.

The same conclusion emerges from the trimmed estimates. To construct the "upper bound" estimates, we eliminated approximately 650 members of the control group with the most positive job satisfaction or wage satisfaction responses (or the least likely job search intentions).[29] To construct the "lower bound" estimates, we eliminated the same number of controls with the most negative job satisfaction or wage satisfaction responses (or the most likely job search intentions). We then re-estimated our basic models on the trimmed samples. Inspection of the upper and lower bound estimates shows that these are relatively close to our baseline model. Typically the treatment effects from one of the bounding samples are slightly larger in magnitude than our baseline estimates, while the effects from the opposite sample are slightly smaller in magnitude. Overall, however, our results do not seem to be very sensitive to the

[28]The probit model is estimated on the set of individuals we can match to the salary data (N=31,887) and includes the same controls as included in the baseline models, plus a dummy for receiving the response incentive. The latter has a large positive coefficient (0.143) with a t-statistic of 4.87.

[29]Given our design, we do the trimming by campus. Examining response rates of the treatment and control groups by campus, we find no gap at UCSC (0.163 for controls vs. 0.171 for treatments), but significant gaps at UCSD (0.229 for controls vs. 0.197 for treatments) and UCLA (0.201 for controls vs. 0.169 for treatments). These gaps imply that we have to trim 349 "excess" members of the control group at UCSD and 296 "excess" members of the control group at UCLA.

possibility of extreme differential response bias between the treatment and control groups of our experiment. The key reason the bounding procedure works in this application is that we are not looking solely at an overall treatment effect but at the treatment effect interacted with wages, and we established in Table 2, column 6 that the differential response rate is the same across wage groups.

4.2.3 Effects of the Placebo Treatment

While our randomized research design provides a strong basis for inferences about the effects of an information treatment, there may be a concern that the interpretation of the measured treatment effects is flawed. For example, it is conceivable that receiving the first stage email about research on inequality at UC campuses could have reduced job satisfaction of relatively low paid employees, independently of the information they obtained from the Sacramento Bee. One simple way to address this concern is to fit the same types of models used in Tables 5 and 6, using the placebo treatment instead of our real information treatment. The wording of the placebo treatment email closely follows the wording of our main information treatment, and was as follows:

> "We are Professors of Economics at Princeton University and Cal Berkeley conducting a research project on pay inequality and job satisfaction at the University of California. The University of California, Office of the President (UCOP) has launched a web site listing the individual salaries of all the top administrators on the UC campuses. The listing is posted at
>
> www.universityofcalifornia.edu/news/compensation/payroll2007/table4.pdf
>
> As part of our research project, we wanted to ask you: Did you know that UCOP had posted this top management pay information online?"

This treatment was only administered at UCLA, and was randomly assigned to three quarters of people in a random one-quarter of departments (see Table 1). To analyze the effects of the placebo treatment we use all observations who were not assigned to the information treatment at the UCLA campus (i.e., the UCLA "control group"), distinguishing within this subsample of 1,893 people between those who were assigned the placebo treatment (N=508) and those who were not (N=1,385). As a first step we analyzed the effect of placebo treatment on use of the Sacramento Bee website. Among the placebo treatments the rate of use of the website was 25.4%, while the rate for the remainder of the controls was 23.8%. The gap is small and insignificant ($t = 0.54$ accounting for the clustered design). We also fit various models similar to the ones in Table 4 and found no indication that the placebo treatment had any effect on use of the Sacramento Bee site.

We then fit the models summarized in Table 8, which relate the placebo treatment to our three measures of satisfaction. For each outcome we show one specification that interacts the treatment dummy with indicators for wages above or below the median of the pay unit, and a second that allows a piece-wise linear interaction between the treatment dummy and the respondent's wage. None of the specifications show large or statistically significant treatment effects from the placebo treatment.

These results also suggest that the more systematic patterns of estimates in Tables 5 and 6 are not an artefact of selection biases that might invalidate our design. However, it is important to note that standard errors are large for all those placebo estimates and that therefore they are not significantly different from our estimates in Table 5 and 6 either. Hence, the placebo test is suggestive but not very powerful.

4.3 The Effect of Peer Salary Disclosure on Perceptions of Inequality

In addition to our basic questions on wage and job satisfaction, and job search intentions, we asked two other questions in our follow-up survey that shed additional light on the mechanisms underlying the responses to our information treatment. The first is a question about the fairness of wage setting: individuals were asked to indicate the extent to which they agreed or disagreed with the statement "My salary is set fairly in relation to others in my department or unit" (using a 4 point scale, with 1=strongly disagree, 2=disagree, 3=agree, 4=strongly agree). Overall, about 12 percent of respondents strongly disagreed with this statement, 31% disagreed, 47% agreed, and 10% agreed strongly. Columns 1-3 of Table 9 present a set of ordered probit models that relate responses to this question to a dummy for treatment status (column 1), the treatment dummy interacted with having wages below or above the median in the pay unit (column 2), and the treatment dummy interacted with a piece-wise linear function of individual wages (column 3). Though the treatment effects are at best only marginally significant, the pattern of the estimated effects closely parallels the pattern of effects obtained for our measures of satisfaction in Tables 5 and 6. This similarity suggests that the responses of lower-wage people whose satisfaction was negatively affected by our information treatment are driven in part by a new awareness of (or sensitivity to) their relative position in the pay hierarchy of their department.

We also asked a more general question on overall inequality in the United States. Specifically, respondents were asked to what extent they agreed or disagreed that "Differences in income in America are too large" (with the same 4-point scale). UC employees appear to be in nearly unanimous agreement with this statement: 38% of our sample agreed and 48% strongly agreed, while only 11% disagreed and 2% strongly disagreed. Columns 4-6 of Table 9 report estimates of the same 3 models for this dependent variable. Here our results suggest that the response to the information treatment is homogenous: people who were informed of the Sacramento Bee website express a higher rate of agreement with the statement, regardless of their relative wage position. The estimates are unfortunately not very precisely estimated and only the treatment effect on above median earners is significant (with a t-statistic of 2.04). Therefore, we cannot reject the simple homogenous treatment effect model in column 4 versus the more complicated treatment effect specifications in columns 5 and 6. We conclude that information about peer salary likely increases concerns about nationwide income inequality, and if anything, the effects are larger for upper income earners and hence likely driven by fairness rather than envy.

Overall, those findings suggest that learning about pay disparity can have significant impacts on views about pay fairness and concerns about inequality. In principle, this could ultimately have effects on voting behavior.[30]

5 Conclusion

A fundamental challenge for any work trying to establish the effect of peer characteristics on labor market outcomes is the fact that, by definition, peers share many observed and unobserved attributes. Unlike laboratory experiments, in observational studies it is difficult to manipulate the identity of one's peers. This makes it difficult to draw firm conclusions about peer effects. In this paper, we present the results of a large scale randomized field experiment that provides access to information about co-workers' pay. Instead of manipulating the identity of peers,

[30] This may explain why left-wing political platforms tend to emphasize inequality while right-wing political platforms tend to minimize the issue of inequality.

we manipulate access to information about peers' salaries and measure how this information treatment affects workers at different points of the salary distribution.

We find that informing people about a web site that contains individual salary information for their co-workers and colleagues doubles the likelihood of using the site and that most of the new users who are affected by our information treatment are primarily interested in the salaries of co-workers in their own department. We then evaluate the effects of our information treatment on employees' satisfaction with their salary and with their job as a whole, and on their job search intentions. Our estimated average treatment effects are small and insignificant, though they suggest that information has (if anything) a negative effect on satisfaction.

Looking within sub-groups, however, we find that the information treatment has a negative effect on people paid below the median for their department and occupation, with no effect on more highly-paid people. Fitting models that allow a non-linear interaction between an individual's wage position and exposure to treatment, we find that information on co-worker salaries has the most negative effect on the lowest-paid workers in a department and occupation. Higher-wage individuals are less negatively affected, and once an individual's salary exceeds the median for his or her pay department and occupation, the effect of higher wages falls to zero. These patterns are consistent with inequality aversion in preferences, which imposes a negative cost for having wages below the median of the appropriate comparison unit, but no reward for having wages above the median. Overall, our results are consistent with previous observational empirical studies and many laboratory experiment studies on relative income. Furthermore, our evidence also suggests that access to information about pay disparity at the workplace increases concerns about both pay setting fairness and overall nation wide inequality.

In terms of workplace policies, our findings indicate that employers have a strong incentive to impose pay secrecy rules. Forcing employers to disclose the salary of all workers would result in a decline in aggregate utility for employees, holding salaries constant. However, it is possible that forcing an employer to disclose the salary of all workers may ultimately result in an endogenous change in wages and worker mix, that could ultimately affect the distribution of wages as in Frank (1984). In terms of political outcomes, our findings indicate that casting light on inequality can stir a strong psychological reaction due to feelings of fairness or envy, that might then feed in political views. This may explain why discussions on inequality play such a large role in the public policy and political debate.

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Appendix I

In this appendix, we reproduce the exact wording of the online second stage survey. We show the exact questions in the case of UCLA (UCSC and UCSD surveys had a similar set of questions but did not include questions 13, 14, 15 on detailed usage of the Sacramento Bee website).

The survey is divided into 3 parts: A. job satisfaction and pay equity questions, B. Demographic and job characteristics questions, C. Knowledge and use of the SacBee website. Those parts will not be presented or flagged to the subjects so that we do not influence the responses.

A. Job Satisfaction and Pay Equity:

1. Please indicate whether you agree or disagree with the following statements:

   (a) "My wage/salary is set fairly in relation to others in my department or unit."

   (b) "My wage/salary is set fairly in relation to workers in similar jobs on campus."

   (c) "My wage/salary is set fairly in relation to workers in similar jobs at other UC campuses."

   Strongly Agree/Agree/Disagree/Strongly Disagree

2. Please indicate whether you agree or disagree with the following statement: "Differences in income in America are too large."
   Please pick one of the answers below.

   – Strongly agree
   – Agree
   – Disagree
   – Strongly disagree

3. Do you expect to receive a salary increase in the next 3 years over and above the standard cost of living adjustment?
   Please pick one of the answers below.

   – Yes

- No

4. Please indicate whether you agree or disagree with the following statement: "At UC, individual performance on the job plays an important role in promotions and salary increases."

   Please pick one of the answers below.

   - Strongly agree
   - Agree
   - Disagree
   - Strongly disagree

   (a) How satisfied are you with your wage/salary on this job?

   Please pick one of the answers below.

   - Very satisfied
   - Somewhat satisfied
   - Not too satisfied
   - Not at all satisfied

   (b) All in all, how satisfied are you with your job?

   Please pick one of the answers below.

   - Very satisfied
   - Somewhat satisfied
   - Not too satisfied
   - Not at all satisfied

5. Taking everything into consideration, how likely is it you will make a genuine effort to find a new job within the next year?

   Please pick one of the answers below.

   - Very likely
   - Somewhat likely
   - Not at all likely

B. Demographic and Job Characteristics Questions:

Please tell us a few things about yourself:

1. Are you working full-time or part-time in your job on campus?

   Please pick one of the answers below.

   - Full-time
     - Part-time

   (a) Is your position covered by a collective bargaining agreement?

   Please pick one of the answers below.

   - Yes
   - No

2. Are you female or male?

   Please pick one of the answers below.

   - Female
   - Male

3. What is your current age?

   Please pick one of the answers below.

   - Under 25
   - 25-34
   - 35-54
   - Over 55

4. How many years have you worked at this university?

   Please pick one of the answers below.

   - Less than 1 year
   - 2 to 5 years

– 6 to 10 yrs

– 11 to 20 years

– More than 20 years

5. How many years have you worked in your current position?

   Please pick one of the answers below.

   – Less than 1 year

   – 2 to 5 years

   – 6 to 10 yrs

   – 11 to 20 years

   – More than 20 years

C. Awareness and use of the Sacramento Bee website:

1. Are you aware of the web site created by the Sacramento Bee newspaper that lists salaries for all State of California employees? (The website is located at www.sacbee.com/statepay, or can be found by entering the following keywords in a search engine: Sacramento Bee salary database).

   Please pick one of the answers below.

   – Yes

   – No

   If yes, skip 4; otherwise, skip 2-3.

   (a) When did you learn about the salary database posted by the Sacramento Bee?

   Please pick one of the answers below.

   – In the last few weeks

   – More than one month ago

   (b) Please tell us: Have you used the Sacramento Bee salary database?

   Please pick one of the answers below.

- Yes
- No

(a) Which people's salaries were you most interested in? (You may select more than one group.)

- Colleagues in my department
- Colleagues in other departments on campus
- Colleagues at other campuses
- Highly paid or high profile people

(b) Were the salaries you checked higher or lower than you expected?
Please pick one of the answers below.

- Higher
- About what I expected
- Lower

2. Why didn't you use SacBee website? (Select all the options that apply.)

- I already know enough about salaries of University employees
- Learning about colleagues' pay could make me feel underpaid
- Learning about colleagues' pay could make me feel overpaid
- I want to respect the privacy of my colleagues on campus
- Information about salaries of University employees is of no interest to me

3. Do you think that making available public information on individual salaries is

- Helpful for people who are paid less than average
- Harmful for people who are paid less than average
- Helpful for morale in your department
- Harmful for morale in your department
- Likely to lead to salary increases for some people

– Likely to lead some people to look for other jobs

If you have any additional comments please feel free to enter them here before you submit the questionnaire. Please write your answer in the space below.

Table 1: Design of the Information Experiment

Campus	Information Treatment Assignment	Placebo Assignment	Response Incentive Assignment
UC Santa Cruz N=3,606 in 223 departments or administrative units	66.7% of departments assigned 60% of individuals in treated department assigned target = 40% of individuals **actual = 42.0%**	none	33% of departments assigned to 100% incentive (all receive incentive) 33% of departments assigned to 50% incentive (one-half receive incentive) 33% of departments assigned to no incentive (none receive incentive) target = 50% of individuals **actual = 49.3%**
UC San Diego N=17,857 in 410 departments or administrative units	50% of departments assigned 50% of individuals in treated department assigned target = 25% of individuals **actual = 23.9%**	none	33% of departments assigned to 100% incentive (all receive incentive) 33% of departments assigned to 50% incentive (one-half receive incentive) 33% of departments assigned to no incentive (none receive incentive) target = 50% of individuals **actual = 55.0%**
UCLA N=20,512 in 445 departments or administrative units	50% of departments assigned 75% of individuals in treated department assigned target = 37.5% of individuals **actual = 36.4%**	25% of departments assigned 75% of individuals in placebo department assigned target = 18.8% of individuals **actual = 21.9%**	All individuals receive incentive
All Three campuses N=41,975 in 1,078 departments or administrative units	target = 32.4% of individuals **actual = 31.6%**	target = 9.2% of individuals **actual = 10.7%**	target = 74.4% of individuals **actual = 76.5%**

Notes: Assignment was based on name/email and department information contained in online directories. Sample sizes reflect number of valid email addresses extracted from directories. See text for procedures used to define departments/administrative units. The response incentive assignment offered the opportunity to win $1000 (from a random lottery with 3 winners for each campus) for survey respondents. The information treatment assignment and the response incentive assignment were orthogonal.

Table 2: Linear Probability Models for Survey Response

All Coefficients ×100	Overall Sample (N=41,975) (1)	(2)	Subsample Matched to Wage Data (N=31,887) (3)	(4)	(5)	(6)
Dummy if match to wage	3.37	3.37	--	--	--	--
	(0.58)	(0.58)				
<u>Treatment Effects:</u>						
Treated individual (all in treated departments)	-3.52	-3.81	-3.38	-3.47	-3.82	--
	(0.70)	(0.54)	(0.78)	(0.78)	(0.61)	
Untreated individual in treated department	0.45	0.00	0.48	0.39	0.00	0.00
	(0.82)	--	(0.92)	(0.91)	--	--
Placebo individual (all in placebo departments)	-5.10	-5.45	-5.49	-5.41	-5.89	-5.90
	(1.05)	(0.88)	(1.20)	(1.17)	(1.01)	(1.01)
Untreated individual in placebo department	1.71	0.00	2.79	2.91	0.00	0.00
	(1.55)	--	(1.49)	(1.47)	--	--
<u>Response Incentive Effects:</u>						
Offered prize in 100% incentive department	4.37	4.25	4.57	4.43	4.23	4.24
	(0.99)	(0.75)	(1.11)	(1.10)	(0.86)	(0.86)
Offered prize in 50% incentive department	3.82	4.25	3.14	3.10	4.23	4.24
	(1.18)	--	(1.38)	(1.36)	--	--
Not offered prize in 50% incentive department department	-0.14	0.00	-0.52	-0.55	0.00	0.00
	(1.29)	--	(1.43)	(1.46)	--	--
<u>Treatment Effects Based on Relative Wage:</u>						
Treated individual with wage less than median in pay unit	--	--	--	--	--	-3.60
						(0.79)
Treated individual with wage greater than median in pay unit	--	--	--	--	--	-4.03
						(0.81)
Dummy if wage greater than median in pay unit	--	--	--	--	--	-0.73
						(0.75)
Cubic in wage?	no	no	no	yes	yes	yes
P-value for test: only individual treatment or incentive status matters (4 degrees of freedom)	0.85	--	0.36	0.32	--	--

Notes: Standard errors, clustered by campus/department, are in parentheses (1,078 clusters for models in columns 1-2; 1,044 for columns 3-6). Dependent variable in all models is dummy for responding to survey (mean=0.204 for columns 1-2; mean=0.214 for columns 3-6). All models include interacted effects for campus and faculty or staff status (5 dummies). "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text. Columns 1-2 include the full sample while columns 3-4 include only the subsample successfully matched to the wage data. In columns 2, 5, and 6, we do not include dummies for spillover effects within departments (i.e., not being treated in a department where some colleagues are treated). Columns 4-6 include wage controls (up to cubic term). Column 6 includes interactions of treatment and relative wage in the unit.

Table 3: Comparison of Treated and Non-treated Individuals

	Mean of Control Group[a]	Mean of Treatment Group	Difference (adjusted for campus)	t-test
	(1)	(2)	(3)	(4)
Overall Sample (N=41,975)				
Percent faculty	16.2	19.1	1.47 (1.61)	0.91
Percent matched to wage data	76.3	75.2	0.12 (1.15)	0.10
Sample Matched to Wage Data (N=31,807)				
Mean base earnings (1000's)	54.73	58.26	2.50 (1.23)	2.04
Mean total earnings (base + supplements, 1000's)	63.53	66.93	2.34 (1.91)	1.22
Percent with total earnings < $20,000	13.2	12.8	-0.37 (0.77)	0.47
Percent with total earnings > $100,000	15.3	16.9	0.90 (1.16)	0.77
Percent responded to survey with non-missing responses for 7 key variables	21.2	17.9	-2.78 (0.61)	4.54
Survey Respondents with Wage Data and non-Missing Values (N=6,437)				
Percent faculty	15.0	17.9	1.23 (1.78)	0.69
Mean total earnings (base + supplements, 1000's)	65.57	69.07	1.74 (2.22)	0.79
Percent female	61.0	61.1	0.41 (1.78)	0.23
Percent age 35 or older	73.0	75.9	1.69 (1.46)	1.16
Percent employed at UC 6 years or more	59.1	62.6	0.96 (1.66)	0.58
Percent in current position 6 years or more	40.3	43.8	1.74 (1.63)	1.07

Notes: Entries represent means for treated and untreated individuals in indicated samples. Difference between mean for treatment and control groups, adjusting for campus effects to reflect the experimental design, is presented in column 3 along with estimated standard errors (in parentheses), clustered by campus/department. The t-test for difference in means of treatment and control group is presented in column 4.

[a] Includes placebo treatment group (at UCLA only).

Table 4a: Linear Probability Models for Effect of Treatment on Use of Sacramento Bee Website

	(1)	(2)	(3)	(4)	(5)	(6)
Treated individual (coefficient × 100)	28.4 (1.8)	29.9 (1.7)	28.2 (1.9)	28.4 (1.6)	--	28.5 (2.7)
Untreated individual in treated department (coefficient × 100)	0.3 (1.7)	--	--	--	--	--
Treated individual with wage less than median in pay unit (coefficient × 100)	--	--	--	--	28.9 (2.2)	--
Treated individual with wage greater than median in pay unit (coefficient × 100)	--	--	--	--	28.1 (2.0)	--
Treated individual × wage (coefficient × 100)	--	--	--	--	--	0.1 (0.4)
Treated individual × deviation of wage from median in pay unit if deviation positive (coefficient × 100)	--	--	--	--	--	-0.4 (0.7)
Selection correction term (inverse Mills ratio from first stage probit which also includes dummy for response incentive)	--	--	0.0 (0.2)	--	--	--
Controls for campus × (staff/faculty) and cubic in wage?	yes	yes	yes	yes	yes	yes
Demographic controls (gender, age, tenure and time in position)	no	no	no	yes	yes	yes
P-value for test against model in column 4	--	--	--	--	0.76	0.84

Notes: Standard errors, clustered by campus/department, are in parentheses (819 clusters for all models). Dependent variable in all models is dummy for using Sacramento Bee web site (mean for control group=19.1%; mean for treatment group=49.4%; overall mean=27.5%). "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text. Model in column 5 also includes dummy indicating if individual's wage is below median of pay unit. Model in column 6 also includes deviation of wage from median of pay unit interacted with dummy for whether deviation is positive.

Table 4b: Effects of Treatment on Use of Sacramento Bee Website for Different Types of Salary Information

		Used Sacramento Bee Website and Looked at Salary Information for:				
	Use Sacramento Bee Website	Colleagues in Own Department	Colleagues in Other Departments, Own Campus	Colleagues at Other UC Campuses	"High-profile" UC Employess	Any of those in columns 2-5
	(1)	(2)	(3)	(4)	(5)	(6)
Mean rate of use for control group (percent)	24.2	15.1	10.1	6.3	13.2	23.8
Estimated treatment effect from model with basic controls:						
Treated individual (coefficient × 100)	27.9	24.2	15.0	7.6	9.6	27.7
	(2.4)	(2.2)	(1.7)	(1.4)	(2.0)	(2.4)
Estimated treatment effect from interacted model with basic controls:						
Treated individual with wage less than median in pay unit (coefficient × 100)	29.9	25.9	13.3	7.5	11.3	30.0
	(3.5)	(3.3)	(2.3)	(2.0)	(2.8)	(3.5)
Treated individual with wage greater than median in pay unit (coefficient × 100)	26.2	22.7	16.4	7.6	8.5	25.9
	(2.8)	(2.7)	(2.1)	(1.7)	(2.4)	(2.8)
P-value for equality of treatment effects[a]	0.36	0.42	0.29	0.98	0.43	0.32

Notes: Estimated on sample of 2,819 survey respondents from UCLA (1,893 controls, including those assigned placebo treatment, and 926 treated individuals). Estimated treatment effects are from OLS models that control for faculty status and cubic in wage. Interacted model also includes dummy indicating whether individual wage is below median for pay unit. Standard errors, clustered by department, are in parentheses (358 clusters for all models). "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text.

[a]t-test for equality of treatment effects for people with wage below median in pay unit and those with wage above median in pay unit.

Table 5: Ordered Probit Models for Effect of Information Treatment on Measures of Job Satisfaction

	Satisfied with Wage on Job (1-4 scale)				Satisfied with Job (1-4 scale)				Likely to Look for New Job (1-3 scale)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated individual (coefficient × 100)	-3.6 (3.5)	--	--	--	-1.0 (3.4)	--	--	--	4.1 (3.4)	--	--	--
Treated individual with wage less than median in pay unit (coefficient × 100)	--	-7.8 (4.6)	-7.8 (4.6)	-7.2 (4.7)	--	-8.8 (5.0)	-8.9 (5.0)	-8.6 (5.0)	--	13.0 (4.6)	13.0 (4.6)	13.1 (4.5)
Treated individual with wage greater than median in pay unit (coefficient × 100)	--	0.0 (4.4)	0.0	-0.9 (4.4)	--	6.1 (4.3)	0.0	5.0 (4.3)	--	-4.1 (4.8)	0.0	-1.9 (4.5)
Dummy for wage less than median in pay unit (coefficient × 100)	--	-11.7 (4.5)	-11.7 (4.5)	-10.9 (4.5)	--	-5.6 (4.4)	-7.4 (4.3)	-3.1 (4.4)	--	-0.1 (4.8)	1.1 (4.7)	-8.2 (4.9)
Controls for campus × (staff/faculty) and cubic in wage?	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Demographic controls (gender, age, tenure and time in position)?	no	no	no	yes	no	no	no	yes	no	no	yes	yes
P-value for exclusion of treatment effects[a]	0.31	0.23	0.09	0.29	0.76	0.07	0.08	0.10	0.22	0.01	0.00	0.02

Notes: Standard errors, clustered by campus/department, are in parentheses (819 clusters for all models). "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text.

[a]One degree of freedom for models in columns 1, 3, 5, 7, 9, and 11; two degrees of freedom for models in other columns.

Table 6: Ordered Probit Models for Effect of Information Treatment on Measures of Job Satisfaction

	Satisfied with Wage on Job (1-4 scale)				Satisfied with Job (1-4 scale)				Likely to Look for New Job (1-3 scale)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated individual (coefficient × 100)	-7.4	-14.4	-14.8	-13.8	-8.5	-14.0	-14.1	-13.6	13.8	13.9	13.5	14.5
	(5.7)	(6.7)	(6.8)	(6.9)	(5.6)	(6.3)	(6.2)	(6.3)	(6.0)	(6.6)	(6.5)	(6.5)
Treated individual × wage (coefficient × 100)	0.6	2.7	2.4	2.5	1.1	2.8	2.8	2.6	-1.5	-1.5	-2.0	-1.3
	(0.7)	(1.1)	(1.1)	(1.1)	(0.7)	(1.0)	(1.0)	(1.0)	(0.7)	(1.1)	(1.1)	(1.1)
Treated individual × deviation of wage from median if deviation positive (coefficient × 100)	--	-3.9	-2.4	-3.4	--	-3.1	-2.8	-2.8	--	-0.1	2.0	-0.5
		(1.8)	--	(1.8)		(1.5)	--	(1.5)		(1.7)	--	(1.6)
Deviation of wage from median if deviation positive (coefficient × 100)	--	8.0	7.6	7.3	--	5.2	5.1	4.5	--	-2.1	-2.6	-1.2
		(1.1)	(1.1)	(1.1)		(1.0)	(1.0)	(1.0)		(1.2)	(1.1)	(1.1)
Controls for campus × (staff/faculty) and cubic in wage?	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Demographic controls (gender, age, tenure and time in position)?	no	no	no	yes	no	no	no	yes	no	no	no	yes
P-value for exclusion of treatment effects[a]	0.41	0.11	0.08	0.16	0.23	0.04	0.02	0.07	0.07	0.15	0.11	0.08
P-value for test of constant treatment effect for those with wage>median	--	0.32	--	0.38	--	0.81	--	0.87	--	0.17	--	0.10

Notes: Standard errors, clustered by campus/department, are in parentheses (819 clusters for all models). "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text.

[a]Two degrees of freedom for models in columns 1, 5, and 9; three degrees of freedom for models in other columns.

Table 7: Selection Corrected and Trimmed Bounds Models

	Satisfied with Wage on Job (1-4 scale)				Satisfied with Job (1-4 scale)				Likely to Look for New Job (1-3 scale)			
	Base	Heckit	Trimmed Upper Bound	Trimmed Lower Bound	Base	Heckit	Trimmed Upper Bound	Trimmed Lower Bound	Base	Heckit	Trimmed Upper Bound	Trimmed Lower Bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated individual (coefficient × 100)	-14.4	-8.7	-13.4	-14.3	-14.0	-15.7	-11.3	-14.6	13.9	10.1	18.4	11.6
	(6.7)	(7.4)	(7.0)	(6.6)	(6.3)	(7.0)	(6.2)	(6.9)	(6.6)	(7.3)	(6.7)	(6.9)
Treated individual × wage (coefficient × 100)	2.7	2.5	2.3	2.9	2.8	2.9	2.7	3.2	-1.5	-1.4	-1.9	-1.2
	(1.1)	(1.2)	(1.3)	(1.2)	(1.0)	(1.0)	(1.0)	(1.1)	(1.1)	(1.1)	(1.1)	(1.2)
Treated individual × deviation of wage from	-3.9	-3.3	-2.4	-4.0	-3.1	-3.2	-2.9	-5.6	-0.1	-0.5	0.9	-0.5
median if deviation positive (coefficient × 100)	(1.8)	(1.9)	(2.3)	(1.8)	(1.5)	(1.5)	(1.6)	(1.6)	(1.7)	(1.7)	(1.8)	(1.8)
Selection correction (inverse Mills' ratio)	--	-0.6	--	--	--	0.2	--	--	--	0.4	--	--
		(0.4)				(0.3)				(0.4)		
Controls for campus × (staff/faculty) and cubic in wage?	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
P-value for exclusion of treatment effects[a]	0.11	0.15	0.24	0.07	0.04	0.04	0.05	0.01	0.15	0.20	0.05	0.26
P-value for test of constant treatment effect for those with wage>median	0.32	0.49	0.96	0.36	0.81	0.73	0.91	0.03	0.17	0.12	0.41	0.18

Notes: Standard errors, clustered by campus/department, in parentheses (819 clusters for all models). "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text. Models also include variable representing deviation of wage from median, interacted with dummy if the deviation is positive (as in Table 6). "Heckit" models include inverse Mills ratio selection correction based on probit model for likelihood of response that includes dummy indicating whether the individual was offered the response incentive. Trimmed models delete observations with highest ("trimmed upper bound") or lowest ("trimmed lower bound") value of dependent variable from control group to equate response rate of controls to response rate of treatment group. See text.

[a]Three degrees of freedom for all models.

Table 8: Estimates of the Effect of "Placebo" Treatment

	Satisfied with Wage on Job (1-4 scale)		Satisfied with Job (1-4 scale)		Likely to Look for New Job (1-3 scale)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated individual (coefficient × 100)	--	-6.4 (9.1)	--	-1.3 (10.2)	--	6.9 (12.6)
Treated individual with wage less than median in pay unit (coefficient × 100)	-5.3 (7.0)	--	-6.8 (7.7)	--	-4.4 (9.5)	--
Treated individual with wage greater than median in pay unit (coefficient × 100)	6.3 (8.9)	--	10.1 (6.5)	--	-12.8 (7.4)	--
Treated individual × wage (coefficient × 100)	--	2.0 (1.4)	--	0.5 (2.0)	--	-2.7 (2.3)
Treated individual × deviation of wage from median in pay unit if deviation positive (coefficient × 100)	--	-4.5 (3.1)	--	0.2 (3.5)	--	1.7 (2.9)
Controls for staff/faculty status and cubic in wage?	yes	yes	yes	yes	yes	yes
P-value for test all treatment effects are 0	0.56	0.51	0.20	0.93	0.19	0.21

Notes: Standard errors, clustered by campus/department, are in parentheses (308 clusters for all models). Sample consists of 1,893 individuals at UCLA who were either assigned placebo treatment or no treatment. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text. Models in columns 1, 3, and 5 also includes dummy indicating if individual's wage is below median of pay unit. Models in columns 2, 4 and 6 also include deviation of wage from median of pay unit interacted with dummy for whether deviation is positive.

Table 9: Effect of Information Treatment on Perceptions of Fairness and Overall Inequality

	My Wage is Set Fairly in Relation to Other Members of my Department or Unit (1-4 Scale)			Differences in Income in America Are Too Large (1-4 Scale)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated individual (coefficient × 100)	-3.6 (3.6)	--	-8.8 (9.1)	7.1 (3.7)	--	10.3 (6.4)
Treated individual with wage less than median in pay unit (coefficient × 100)	--	-10.6 (5.0)	--	--	4.4 (5.0)	--
Treated individual with wage greater than median in pay unit (coefficient × 100)	--	2.7 (4.4)	--	--	9.4 (4.6)	--
Treated individual × wage (coefficient × 100)	--	--	1.9 (1.1)	--	--	-1.0 (1.1)
Treated individual × deviation of wage from median in pay unit if deviation positive (coefficient × 100)	--	--	-3.9 (1.9)	--	--	2.0 (1.7)
Controls for staff/faculty status and cubic in wage?	yes	yes	yes	yes	yes	yes
P-value for test all treatment effects are 0	0.33	0.07	0.20	0.06	0.11	0.14

Notes: Standard errors, clustered by campus/department, are in parentheses (818 clusters for models in columns 1-3; 819 clusters for models in columns 4-6). Sample consists of 6,411 individuals (columns 1-3) or 6,423 (columns 4-5) in overall analysis sample with non-missing responses on dependent variable. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text. Models in columns 2 and 5 also includes dummy indicating if individual's wage is below median of pay unit. Models in columns 3 and 6 also include deviation of wage from median of pay unit interacted with dummy for whether deviation is positive.

Appendix Table 1: Matching and Response Rates

	Number in Online Directory	Pct. Matched to Wage Data	Pct. Responded to Survey	Pct. Responded Conditional on Wage Data	Pct. With Wage and non-missing Survey Data	Sample Size in Analysis File
	(1)	(2)	(3)	(4)	(5)	(6)
UC Santa Cruz						
Staff	2,797	70.3	14.7	16.8	10.9	306
Faculty	809	73.6	18.9	21.2	14.7	119
All	3,606	71.1	15.6	17.8	11.8	425
UC San Diego						
Staff	15,782	81.1	24.0	24.0	17.9	2,830
Faculty	2,075	78.8	21.7	23.8	17.5	363
All	17,857	80.8	23.7	23.9	17.9	3,193
UCLA						
Staff	16,227	73.8	19.0	19.8	14.1	2,283
Faculty	4,285	68.1	16.3	19.1	12.5	536
All	20,512	72.6	18.4	19.6	13.7	2,819
All Three campuses						
Staff	34,806	76.8	20.9	21.6	15.6	5,419
Faculty	7,169	71.8	18.2	20.8	14.1	1,018
All	41,975	76.0	20.4	21.4	15.3	6,437

Notes: Sample sizes in column (1) reflect number of valid email addresses extracted from directories. Wage data were matched to directory data by campus and name. Entries in columns 5 and 6 are based on individuals in the online directory who can be matched to wage data, responded to the survey, and provided non-missing responses for 8 key questions.

Appendix Table 2: Means of Outcome Measures by Treatment Status

		Not At All Satisfied	Not Too Satisfied	Somewhat Satisfied	Very Satisfied
"How satisfied are you with your wage/salary on this job?"	Overall Sample (N=6,437)	16.2	31.8	40.2	11.7
	Control Group (N=4,654)	15.8	32.4	39.6	12.1
	Controls Reweighted[a]	15.6	32.8	39.8	11.9
	Treatment Group (N=1,783)	17.3	30.3	41.8	10.5
"How satisfied are you with your job?"	Overall Sample (N=6,437)	3.3	12.1	47.3	37.4
	Control Group (N=4,654)	3.3	12.1	47.3	37.3
	Controls Reweighted[a]	3.0	12.1	47.0	37.9
	Treatment Group (N=1,783)	3.3	12.0	47.1	37.6
		Not At All Likely	Somewhat Likely	Very Likely	
"How likely is it you will make a genuine effort to find a new job within the next year?"	Overall Sample (N=6,437)	47.0	30.8	22.2	
	Control Group (N=4,654)	47.5	30.6	21.9	
	Controls Reweighted[a]	47.6	30.4	22.0	
	Treatment Group (N=1,783)	45.8	31.1	23.1	
		Strongly Disagree	Disagree	Agree	Strongly Agree
"Do you agree or disagree that your wage is set fairly in relation to others in your department/unit?"	Overall Sample (N=6,411)	11.7	31.1	47.5	9.8
	Control Group (N=4,635)	11.4	31.0	47.8	9.9
	Controls Reweighted[a]	11.3	31.4	47.5	9.8
	Treatment Group (N=1,776)	12.6	31.1	46.9	9.4
"Do you agree or disagree that differences in income in American are too large?"	Overall Sample (N=6,423)	2.0	11.4	38.2	48.5
	Control Group (N=4,644)	2.1	11.5	38.8	47.6
	Controls Reweighted[a]	2.2	11.3	38.5	48.0
	Treatment Group (N=1,779)	1.6	11.0	36.4	51.0

Notes: Entries are tabulations of responses for analysis sample (or subset of analysis sample with non-missing responses).

[a]Means for control group are reweighed across campuses to reflect unequal probability of treatment at different campuses. Reweighted controls are then directly comparable to Treatment.

Appendix Table 3: Effect for "Expecting a Raise" in the Next Three Years

	Expect a Raise in Next Three Years Over and Above Standard Cost of Living Adjustment?		
	(1)	(2)	(3)
Treated individual (coefficient × 100)	0.9 (2.0)	--	-6.3 (3.5)
Treated individual with wage less than median in pay unit (coefficient × 100)	--	-0.8 (2.9)	--
Treated individual with wage greater than median in pay unit (coefficient × 100)	--	2.3 (2.6)	--
Treated individual × wage (coefficient × 100)	--	--	0.9 (0.4)
Controls for staff/faculty status and cubic in wage?	yes	yes	yes
P-value for test all treatment effects are 0	0.64	0.62	0.07

Notes: Linear probability model used. Standard errors, clustered by department, are in parentheses (358 clusters). Sample consists of 2,819 individuals in overall analysis sample with non-missing responses on dependent variable at UCLA only. Dependent variable is dummy if the individual reports that she or he expects a raise in the next three years over and above standard cost of living adjustments. The mean of the dependent variable is 0.290. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Small pay units are pooled to the campus level -- see text.