Inequality at Work:
The Effect of Peer Salaries on Job Satisfaction

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April 2011

Abstract

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 job satisfaction and job search intentions. A randomly chosen subset of employees of the University of California (UC) was informed about a new website listing the pay of 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 suggest that job satisfaction 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 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 and Solnick and Hemenway 1998), health and longevity (e.g., Marmot, 2004), and reward-related brain activity (e.g., Fliessbach et al. 2007).[4] 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.[5] 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.[6] A few days

[1]Early classical references are Smith (1759) and Veblen (1899). Modern formal analysis began with Duesenberry's (1949) relative income model of consumption. Easterlin (1974) used this model to explain the weak link between national income growth 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 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]There are also studies that have concluded a more important role for absolute income than relative income, for example, Stevenson and Wolfers (2008). Kuhn et al. (2011) find that people do not experience reduced happiness when their neighbors win the lottery.

[5]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 (2010) 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, and Kling et al. (2008) on Medicare prescription drug plans.

[6]Initially the website was relatively unknown. Even as late as June 2009, when we conducted the last of

later we surveyed all campus employees to elicit information about their use of the Sacramento 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. Throughout our analysis we define peers as co-workers in the same occupation group (faculty vs. staff) *and* the same unit (i.e., department or school) within the University.

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, as in the "tunnel effect" of Hirschman and Rothschild (1973).[7] These 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 would be the case under inequality aversion (eg. 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

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.

[7] Hirschman and Rothschild (1973) proposed this model in the context of developing economies where increases in inequality are tolerable because they act as a signal for future own income growth. Senik (2004) proposed a recent test of the model in the case of Russia in transition. More closely related to our study, Galizzi and Lang (1998), using administrative data from Italian firms, show that, conditional on own wages, the average wages of similar workers in the firm is positively related to future wage growth and negatively related to quits. Clark et al. (2009) show, using matched employer-employee panel data, that individual job satisfaction is higher when other workers in the same establishment are better-paid, and interpret this as evidence of a tunnel effect.

concern over co-worker wages, but rationally use information on peer salaries to update their future pay prospects. Indeed, in our data, future earnings growth is positively related to current median earnings in one's department (conditioning on current individual earnings). Hence, if co-worker salaries provide a signal about future wages, either through career advancement or a bargaining process, learning that one's wage is low (high) relative to one's co-workers causes expected future wages to be updated positively (negatively). In this case the revelation of co-workers' salaries raises the job satisfaction of relatively low-wage workers and lowers the satisfaction of relatively high-wage workers. 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.

Informing UC employees about the Sacramento Bee website had a large and highly significant impact on the fraction who used 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.

We find that access to information on co-workers' wages had different effects on employees with salaries above and below the median in their department and occupation group: The information treatment caused a reduction in pay and job satisfaction for those whose wages are below the median in their department and occupation group and an increase in the probability that the worker reports looking for another job. The reductions in pay and job satisfaction, and the increased reporting of job search in the treatment, are more pronounced the further below the wage is from the unit and occupation median. By comparison, those who are paid above the median experienced no significant change in pay satisfaction or reported job search. The evidence further suggests that the response to treatment depends more on the wage rank than the wage level relative to median as in Parducci's (1995) theory. Overall, these findings are consistent with relative pay effects, and inconsistent with the alternative rational learning model. The information treatment also leads to an increase in the fraction of respondents who think that overall inequality in America is too high. However, we do not find evidence that the treatment affected turnover 2-3 years after the experiment possibly because the information diffused overtime to both controls and treatments or because the Great recession reduced voluntary quits.

Our results provide credible field-based evidence confirming the importance of the relative pay comparisons that have been identified in earlier observational studies of job turnover (Kwon and Milgrom, 2011), 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.[8] Specifically, they support the theory of relative income in which negative comparisons reduce workers' satisfaction but positive comparisons have less impact.

Our results also contribute to the literature on pay secrecy policies.[9] About one-third of U.S. companies 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, our data collection and assembly procedures, and selection issues. Section 4 presents our main empirical results. Section 6 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

[8] Lab experimental studies have developed a series of games such as the dictator game, the ultimatum game, or the trust game (see Rabin 1998 for a survey) showing evidence that relative outcomes matter. See in particular Fehr and Falk 1999, Charness 1999, Fehr et al. 1998, Fehr et al. 1993, Fehr and Schmidt, 1999, Charness and Rabin, 2002, and Clark et al., 2010 for lab evidence of relative pay effects. Note however that in experimental effort games, Charness and Kuhn (2007) and Bartling and Von Siemens (2010) find that workers' effort is highly sensitive to their own wages, but unaffected by co-worker wages. Following the theory that ordinal rank matters proposed in psychology by Parducci (1995), some lab studies have shown that rank itself matters (see e.g. Brown et al. 2008 and Kuziemko et al. 2010).

[9] 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.

their co-workers. We begin with the case where workers care directly about relative pay, as in the model of Clark and Oswald (1996). 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 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 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 and $v(.)$ represents feelings arising from relative pay.[10] 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. 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., $E[m|I^0] = w$.

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

$$S(w, I^0) = u(w) + v(w - E[m|I^0]) + e = u(w) + e,$$

[10] These feelings depend on information about co-workers' pay–information may never be revealed–which is why the expectation term in (1) is inside the function $v(.)$ rather than outside.

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.

where we assume (w.l.o.g.) that $v(0) = 0$. With access to the website we assume that individuals can observe m perfectly.[11] Then job satisfaction conditional on using the website is

$$S(w, I^1) = u(w) + v(w - E[m|I^1]) + e = u(w) + v(w - m) + e.$$

Let D be an indicator for whether an individual is informed or not, then we have

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

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 $v(w - m)$. A simple specification of the function $v(.)$ is a piece-wise linear model allowing for different slopes above and below the median m:

$$S(w, m, D) = u(w) + b_0 \cdot D \cdot (w - m) \cdot 1(w \leq m) + b_1 \cdot D \cdot (w - m) \cdot 1(w > m) + e. \quad (3)$$

We assume $b_0 \geq b_1 \geq 0$ to allow (potentially) for concavity in the relative utility function, the so called "inequality aversion" hypothesis of Fehr and Schmidt (1999).[12] In this case the treatment reduces job satisfaction for those with $w \leq m$ and weakly increases job satisfaction for those with $w > m$, implying that the average effect of treatment is weakly negative. Holding constant m the effect of treatment is increasing in w, with a slower rate of increase once $w > m$. The linear case with no kink at the median corresponds to $b_0 = b_1$. In this case the average treatment effect is zero.

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

Appendix Table A1 presents a set of earnings growth comparisons and regression models for wages of workers at UCLA between 2007 and 2008. The table shows that workers paid below the median for their department and occupation group experience significant earnings gains relative to those above the median, and that wage growth is strongly correlated with median peer wages,

[11] 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.

[12] Other related models could also be consistent with those predictions. For example, individuals may value social approval or social esteem. Learning that one earns less than peers might be taken as an indication that one's work is not valued as much by the employer, and this disappointment may lead to lower job satisfaction. If utility is concave in social esteem, then these predictions would also hold.

holding constant own wages. These patterns suggest a possible alternative to the relative income model in which utility depends solely on own earnings, but people use their co-workers' wages to help predict future pay.[13] If respondents interpret the pay and job satisfaction questions in our surveys as reflecting not just their current situation, but also their expected future trajectory, then new information on co-worker pay could lead to changes in satisfaction though updating, rather than relative pay considerations. In essence, information on peers' salaries provides a signal of expected future pay, arising through either career advancement or bargaining.

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:

$$S(w, I) = w + \beta E[w'|I] + e, \tag{4}$$

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 $w' \sim N(w, 1/q)$).[14] 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'.[15] The larger is k, the more informative is the signal. Workers form expectations about future wages by combining their prior and the signal:

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

where $\lambda \equiv k/(q + k)$ represents the relative precision of the signal. Observed job satisfaction for members of the control and treatment groups, conditional on (w, m, D) is given by

$$S(w, m, D) = (1 + \beta)w + D \cdot b' \cdot (m - w) + e \tag{5}$$

where $b' \equiv \beta\lambda$. Although this equation has the same form as equation (3) above, the predictions are the opposite than those of the relative pay model. When people learn about their own future wages from co-worker pay the effect of access to information on job satisfaction is *increasing* in

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

[14] Assuming that the mean of w' is $(1 + g)w$ where g is a common growth factor does not affect these results.

[15] 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.

the gap between m and w 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. On average, half the workers have a positive surprise and half have a negative surprise, with an average impact of zero.[16]

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 theoretical models above, we have assumed that all treated individuals do access the web site salary information, and none of the individuals in the control group do. 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 existence of the website decided to use it.[17] 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.

Let T denote the treatment status of a given individual ($T = 0$ for the control group; $T = 1$ for the treatment group), and let $\pi_0 = E[D|T = 0, w, m]$ and $\pi_1 = E[D|T = 1, w, m]$ denote the probabilities of being informed conditional on treatment status, individual wages, and peer mean wages. With this notation, we have

$$S = u(w) + \pi_0 v(w - m) + T \cdot (\pi_1 - \pi_0) v(w - m) + e + \phi, \tag{6}$$

for some functions u and v, and where ϕ is an error component reflecting the deviation of an

[16]It is possible to extend this learning model to the case where workers value income in each period using a concave utility function $u(w)$: $S(w, I) = u(w) + \beta E[u(w')|I] + e$. 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, the average change in satisfaction is positive. 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).

[17]Some treated employees may have failed to read our initial email informing them of the website. Others may have been concerned about clicking a link in an unsolicited email, and decided not to access the site.

individual's actual information status from his or her expected status.[18] Under the *assumption* that the "information treatment intensity" $\delta \equiv \pi_1 - \pi_0$ is constant across individuals, equation (6) 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 reflects a combination of the variation in the information treatment effect $(\pi_1 - \pi_0)$ and the difference in satisfaction in the presence or absence of information $(v(w-m))$. Below we estimate 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 6 with an attenuated treatment response.

2.3.2 Econometric Models

Based on 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, we fit models of the form:

$$S = g(w,x) + a \cdot 1(w \leq m) + b_0 \cdot T \cdot 1(w \leq m) + b_1 \cdot T \cdot 1(w > m) + \mu, \tag{7}$$

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.

Our second set of empirical models focus directly on the shape of the treatment response function implied by equations 3 and 5. These models have the form

$$S = g(w,x) + c_1 T \cdot (w - m) \cdot 1(w \leq m) + c_2 T \cdot (w - m) \cdot 1(w > m) + \mu, \tag{8}$$

[18] The models described above imply that satisfaction can be written as $S = u(w) + D \cdot v(w-m) + e$. 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.

which includes controls for individual wages and other covariates (x), a dummy for treatment status, an interaction of treatment status with the individual's relative wage when the wage is below median in the individual's pay unit, and a second interaction between the relative wage and treatment status when the wage exceeds the median. As discussed below, we apply these models to three complementary measures of job satisfaction.

We consider several tests of the estimated coefficients from these models. We first consider the test that the treatment effects are jointly zero. 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.

Equation (7) allows us to distinguish between a model in which relative wages have a direct effect on satisfaction and one in which people learn about their own future wages from their peers' salaries. A finding of $b_0 < 0$ and $b_1 \geq 0$, for example, would favor the relative wage model. To test a model with linear effects of the relative wage on job satisfaction versus a model with a strictly concave response to relative wages we would test $-b_1 = b_0 < 0$ $vs.$ $b_0 < -b_1 \leq 0$. Inequality aversion also suggests the presence of a "kink" in the response to the comparison wage once $w > m$ which implies $c_1 > c_2 \geq 0$ in equation (8).

3 Design, Data, 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) 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.[19] At its

[19] 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). The SacBee updates its website annually when new compensation information is made available. Data for calendar year t earnings are posted in June of year $t + 1$. Others have also posted the comprehensive information online after March 2008. For example, http://ucpay.globl.org/letters.php posts the complete data from year 2004 to 2009.

inception the database contained pay information for calendar year 2007 for all UC workers (excluding students and casual workers) 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 UC Los Angeles (UCLA). Our information treatment consisted of an email (sent from special email accounts established at UC Berkeley and Princeton) 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 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 online survey on their knowledge of the site. Since the answers are only available for the treatment group we do not use the response to this online survey 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.[20] 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

[20]Since our treatment and survey are administered by email, we omit all employees who do not have a UC email address–a rare situation at UC.

2 of Table 1, the actual fractions receiving treatment were relatively close to these targets.[21]

The stratified treatment design was chosen to test the possibility of peer interactions in the response to treatment.[22] 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.

As indicated in the third column of 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 in light of possible confounders including priming effects due to the language of the treatment email and differential response rates between the treatments and control. 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.

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 section A.1) included questions on knowledge and use of the Sacramento Bee website, on job satisfaction and future job 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

[21] 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.

[22] 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.

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.[23] 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. For UCLA we decided to simplify the design 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 calendar 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 A2 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

[23]More precisely, all respondents were eligible for the prize, but only a randomly selected sample were told what it would be.

81% at UCSD and a low of 71% at UCSC. We believe that these differences are explained by differences in the quality and timeliness of the information in the online directories at the three campuses.

3.2 Response to the Follow-up Survey

Overall, just over 20% of employees at the three campuses responded to our follow-up survey (appendix Table A2). 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. 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 in departments where everyone was informed of the response incentive, people 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.[24] For comparison, column 2 shows a model in which potential spillover effects from the information and placebo treatments, and the response incentive are set to zero.

The coefficient estimates for the models in columns 1 and 2 point to several interesting conclusions. First and as suggested by the simple comparisons in Appendix Table A2, 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. 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 for their occupation and pay unit. The estimation results in column 6 suggest that the negative response effect of treatment assignment is very similar for people with above-median wages (-4.04%) 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, 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.

[24] We define faculty status based on job title in the directories. There is likely a small amount of misclassification error in the determination of faculty status.

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. Although the negative effect of the information treatment on the response rate is modest in magnitude (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. Importantly, the effects of the information treatment and placebo treatment on the response rate 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. Because the response rates in the treatment and placebo are close, we can test whether the placebo group shows a similar pattern of effects as the treatment group to probe for possible selection-biases.[25]

3.3 Summary Statistics and Comparisons by Treatment Status

Table 3 presents some comparisons between 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.[26] 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 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 significantly different between the two groups. As noted above, however, the fraction of the treatment group who responded to our follow-up survey is about

[25] We may observe the same pattern of effects in the placebo and treatment even without sample selection bias, for example if there is a priming effect. The placebo treatment will capture the overall effect from our first-stage email absent the disclosure of the salary database.

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

3 percentage points lower than the rate for the controls, and the difference is highly significant ($t = 4.49$). Finally, the bottom panel of Table 3 presents comparisons in our main analysis sample, which consists of the 6,411 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 85% staff and 15% faculty, with mean total earnings of around $67,000. 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 different between the treatment and control groups. 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.[27]

4 Empirical Results

4.1 Treatment Effect on Use of the Sacramento Bee Website

We now turn to our main analysis of the effects of the information treatment. Except in Section 4.4, 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 site.[28] The mean rate of use reported by the control group is 19.2%. As shown by the model in column 1, the information treatment more than doubles that rate (by +28% to a mean rate of 48%). 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 dummy indicating whether the individual was offered a probabilistic monetary response incentive. Recall that a random subset of individuals surveyed were offered

[27]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.

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

the monetary incentive. The coefficient estimate for the treatment dummy is the same as in column 2, and the coefficient on the incentive dummy is very close to 0.

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=0.75, reported in bottom row of the table). The specification in column 6 allows a main effect for treatment, and an interaction of treatment status with wage relative to the median wage in the pay unit, 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=0.89). 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.[29] 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 whose salary 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)

[29] 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=0.21). 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=0.23).

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 as respondents could pick more than one answer. Table 4b reports estimated linear 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 24.1 percentage points. Compared with the treatment effect of 27.6 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% (=24.1/27.6) examined the 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.

Overall the results in Table 4b confirm that people who were informed about the Sacramento website by our treatment e-mail were very likely to use the site to look-up 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 peers, and risk being perceived as invading their privacy.

4.2 Treatment Effect 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. Our surveys asked respondents a number of questions related to their overall satisfaction with their pay and job, and whether they planned on making a serious effort to look for a new job. The first 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" or "not at all satisfied". The second 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 is based on responses to the question: "*Do you agree or disagree that your wage is set fairly in relation to others in your department/unit?*". Respondents could choose "Strongly Agree", "Agree", "Disagree" or "Strongly Disagree". The fourth 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 "very likely", "somewhat likely" or "not at all likely".

We report in Appendix Table A3 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 be directly comparable to the treatment group. In general, UC employees are relatively happy with their jobs but less satisfied with their wage or salary levels. 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) where the simple chi-square test is significant (p=0.05).

For much of the subsequent analysis we consider three dependent variables. In order to simplify the presentation of results, and to improve precision, we combine wage satisfaction, job satisfaction, and wage fairness into a single index by taking the simple average of these measures.[30] The resulting variable, which we call the *satisfaction index* is interpretable as a general measure of work satisfaction. The index has a ten point scale with higher values indicating the respondent is more satisfied based on the three underlying measures.[31] The

[30]Specifically, for the three variables we assign numerical scores 1-4. For wage and job satisfaction 4 represents "very satisfied" and 1 represents "not at all satisfied." For wage fairness 4 represents "strongly agree" and 1 represents "strongly disagree". We then take the average of these responses. We have experimented with different ways of combining these variables, for example taking the first principal component of these variables, and the estimates are not sensitive to these alternatives.

[31]We show the results of the baseline models for each of the sub-components in Appendix Table A4.

second outcome variable in the main analysis is the job search intention described above where larger values mean that the respondent reports being more likely to be look for a new job. The third outcome is a binary variable for whether the respondent is dissatisfied *and* is looking for a new job. Specifically, we create a binary variable taking the value of 1 for whether the respondent is dissatisfied (below the median on the satisfaction index) and responds “very likely” to the job search intentions question, and 0 otherwise. We treat these outcome variables 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 (the binary outcome reduces to a probit).

Tables 5 and 6 present estimates of a series of ordered probit models for these three measures. The models in Table 5 follow the specification of equation (7) and include treatment effects interacted with whether the individual is paid above or below the median for his/her unit. We begin with the basic models in columns 1, 5, and 9 which include only a simple treatment dummy. The estimated treatment effects from this simple specification are either insignificant or only borderline significant. The point estimate for the satisfaction index is negative ($t = 1$), the point estimate for search intentions is positive ($t = 1.2$), and the point estimate for the combined variable (dissatisfied and likely looking for a new job) is positive ($t = 1.7$). These estimates are suggestive of a tendency for a negative average impact on satisfaction. All the models include controls for a cubic in individual 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) indicates 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. For workers whose salaries are below median in their unit and occupation, the point estimate for the satisfaction index is negative ($t = 2.1$), the point estimate for search intentions is positive ($t = 2.6$), and the point estimate for the combined binary variable is positive ($t = 3$). For workers earning more than the unit and occupation median the treatment effect is insignificant in all cases.[32] The table shows the difference in

[32] We have also estimated restrictive models (not shown here) that assume no treatment effect on above-median workers which fit as well as ones that allow an effect on this group, and show a pattern of negative treatment

the estimated treatment effect between above and below median workers which are statistically significant for all three of these models at the five percent level. The estimates in Columns 3, 7, and 11 of Table 5 include a set of demographic controls and are qualitatively very similar.[33]

We do an initial probe of the robustness of our inferences to potential selection biases by fitting selection-correction models where we take advantage of random assignment of the incentive that we introduced to raise response rates, as well as the random assignment of the placebo which reduces response rates. In column 4 we present estimates from a Heckit model for the satisfaction index outcome where in the first stage we estimate a probit model using the explanatory variables from model 2 in Table 5 and dummies for whether the respondent was randomly assigned to the response incentive or placebo groups, the latter two of which are excluded from the second stage.[34] In columns 8 and 12, we report selection-corrected maximum likelihood estimates for the ordered probit model for the job search and the combined satisfaction/job search variables (the latter, a binary variable, again reduces to a probit) using the same exclusion restrictions. While in none of these models is there a significant relationship in the correlation between the response for the outcome and participation, we place little weight on these estimates because we have found that they are not generally robust to choices on exclusion restriction (both placebo and price incentive versus just placebo versus just incentive) and estimating procedure (two-step versus maximum likelihood). We therefore postpone making conclusions on the extent of selection bias until we discuss the placebo experiment which we view as our strongest test.

Overall, 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. The negative impact of information on below-median workers coupled with the absence of any positive effect for above-median workers is consistent with inequality aversion.

The specifications in Table 6 are based directly on a piece-wise linear variant of inequality aversion and follow the specification of equation (8). The specifications in columns 1, 4, and 7 include an interaction of treatment with an individual's wage relative to the pay unit median

effects on job satisfaction and positive effects on search intentions.

[33] We postpone discussion of the magnitude of the effects until we present the estimates from Table 6.

[34] Because the satisfaction index has a ten point scale, estimating an ordered probit maximum liklihood selection model was not possible.

for workers who are below the median, and a separate interaction for workers who are above the median, thus allowing a kink in the treatment response function at the median wage of the pay unit.[35] These models suggest a negative information treatment effect on the lowest wage individuals for all outcomes. The pattern of estimates confirm the non-linearity in the interaction between the treatment effect and relative wages suggested by Table 5: higher wages reduce the negative effect on satisfaction of the information treatment 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. Across all models reported in Table 6, we cannot reject that the treatment response function is zero when the wage exceeds the pay unit median.

To get a sense of the magnitude of the effects, we can relate the size of the effect to the loss of income necessary to get the same decline in the latent index of satisfaction in the control group. We fit some simple ordered probit models to the control group relating each of the dependent variables to wage and a set of demographic and campus controls.[36] These models show that in the control group an additional $10,000 is associated with shifts in the latent index of 4.7 for the satisfaction index. This implies that the effect of being $10,000 closer to median in the treatment is equivalent to about $5500 in extra income in the control. Because of incomplete compliance, discussed in section 2.3.1, this estimate needs to be inflated. However, it is not obvious what the appropriate inflation factor should be. This is a case where the treatment effect is likely to be heterogeneous—since the effect of peer salary on workers utility is likely to vary significantly in the population—and this heterogeneity could be related to the propensity to look at the SacBee web site. A conservative range is that the effect of being $10,000 closer to median in the treatment is equivalent to between $5500 to $22,000 in extra income in the control. We view these as large effects.

In Table 6 we further explore the effect of the information treatment based on wage rank rather than levels. The motivation for this specification is that it is possible in principle that ordinal rank matters more for relative utility considerations than absolute salary differences, as has been suggested in the psychology literature (Parducci, 1995). In these models we replace the relative wage in levels with the percentile rank in pay unit (expressed so median = 0) in

[35]We present in appendix Table A5 specifications that also include a main treatment effect, with similar results.

[36]In addition to wage, the explanatory variables are dummies for gender, age, tenure, time in position and controls for campus interacted with faculty/staff.

the interaction terms. For the satisfaction and the satisfaction/search outcomes, rank shows a more pronounced effect than the model based on levels (specifications 2 and 10). When we estimate models with both rank and levels, rank wins the "horse race" for these two outcomes. Specifically, the interaction of treatment and rank is significant for the below median workers, and once this interaction is in the model the interaction of treatment and the relative wage level is no longer significant (specifications 3, 7, and 11).[37] For the job search variable we do not have the precision to be able to distinguish between the two.

In columns 4, 8, and 12 of Table 6 we fit selection-correction models using the approach from Table 5. We fit these to the models based on the rank order of the respondent, but the results are similar for levels. We find no evidence of selection bias as the correlations between the outcome and participation errors are small and insignificant in all cases.

We have also looked at whether the effects vary depending on whether we sent the follow-up survey 3 days or 10 days after treatment. We find robust evidence that the estimates are stronger for surveys sent 10 days after the treatment than 3 days, with similar rates of SacBee use for both groups (results available upon request). This is suggestive evidence that these effects are not entirely transitory.

4.2.2 Effects by Subgroups

We have estimated models allowing the treatment effects to vary by gender, faculty/staff status, and length of tenure, shown in Appendix Table A6 (same model as in Table 5). Although both men and women express the same elevated dissatisfaction following the information treatment, women appear more inclined to report that they are searching for a new job following treatment. Low-paid and low-tenure respondents respond to the treatment by elevated job search intentions while high tenure and low-paid respondents do not.[38] Staff appear to be more responsive than faculty to the treatment on both dissatisfaction and job search, but the relatively small number of faculty limits our ability to make precise comparisons.

We have also explored models in which we examine effects using the full campus (instead

[37] Models where we have added a treatment main-effect (Appendix Table A5) also show that the rank variable appears to be more significant in the treatment response than relative wage levels.

We have also estimated models where wages are measured in logs. Estimates (not reported, but available upon request) are qualitatively similar to the ones in Table 6.

[38] This is not surprising as very few UC employees with long tenure change jobs. We use this feature to test that responses to job search are truthful (and not cheap talk due to wage dissatisfaction). In Appendix Table A7, we show that treatment effects on job search are present only in the group of more mobile workers as predicted by age, tenure, time in position, sex, faculty/staff status, and campus (estimated from the control group).

of the department) as peer unit but always keeping the distinction staff vs. faculty. The results are presented in Table 7. Interestingly, faculty experience a very strong and significant treatment response on the satisfaction index both below median, where satisfaction drops, and above median, where satisfaction increases (column 1). The difference between below and above median is highly significant (t=3.6). In contrast, staff shows only a weak dissatisfaction below median and no effect above median (column 2). The same qualitative results are found for job search (columns 3 and 4) and the satisfaction/job search index (columns 5 and 6), although the results are not as significant for faculty. This suggests that the kink effect in relative pay utility might not be present for faculty. From our own experience and those of readers, it is perhaps not surprising that faculty in departments where pay is relatively low (such as humanities) would feel upset when they discover the true pay gap with departments where pay is relatively high (such as economics, business, or law). Conversely, faculty in high pay departments who feel underpaid relative to their department colleagues might feel some solace in seeing that they are still much better paid than faculty in low pay departments. Overall, those results suggest that the relevant comparison group for faculty might be campus-wide colleagues while the department might be relevant comparison group for staff.

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. Such effects are known in the psychology literature as "priming effects." Another area of possible concern that we have already discussed is the lower response in the treatment group which may introduce possible selection biases.

One simple way to address these concerns 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 placebo experiment is subject to the same set of potential biases as the treatment. Because the placebo reduced the response rate to our survey by the same magnitude as the treatment we should observe a pattern of estimates similar to the treatment if the effects are due to selection bias.

If the effects are due to priming, we should see the same pattern of effects in the placebo group.

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 [...]. 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 1,880 people between those who were assigned the placebo treatment (N=503) and those who were not (N=1,377). 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 25.6%, while the rate for the remainder of the controls was 23.8%. The gap is small and insignificant ($t = 0.6$ 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 outcome measures. For each outcome we show two estimates: the baseline specification that interacts the treatment dummy with indicators for wages above or below the median of the pay unit for the UCLA sample (excluding the placebo group) and a specification that interacts the placebo dummy with indicators for whether the respondent's earnings are above or below the median in his/her the pay unit (excluding the treatment group). In the third column, we show p-values corresponding to the test that the parameters from the information treatment model is equal to the placebo model.

These results suggest that the systematic pattern of estimates in Tables 5 is not an artefact of priming effects or selection biases arising from our earlier email contact of the treatment group. Among the UCLA treatment group, the pattern of estimates is very similar to the pattern in Table 5 for all three campuses, though less precise because of the smaller sample.

The low earnings group receiving our email informing them of the Sacramento Bee database have lower satisfaction, are more likely to report that they are searching for a job, and are more likely to be dissatisfied and searching for a job relative to the control group and relative to the high earnings group. By contrast, for the low earnings group receiving the placebo email, we do not observe significant effects in any of these dimensions. Indeed, the point estimates show the opposite pattern. For the three outcomes, we can reject the hypothesis that the interaction of treatment with below median in pay unit is equal to the interaction of placebo and below median in pay unit below the 10% level.

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 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. Table 9 reports estimates 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 significantly higher rate of agreement with the statement, regardless of their relative wage position ($t = 1.92$ without demographic controls and $t = 1.85$ with controls). Columns 3 and 4 include the interaction of treatment with whether the respondent is paid below median in their pay unit. Unlike the other dependent variables we have considered, this interaction term is not significantly different from zero and, if anything, the effects are larger for upper income earners.

Information about peer salary appears to increase 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 concerns about inequality. In principle, this could ultimately have effects on voting behavior.

4.4 Effects on Actual Turnover in the Medium-Run

One important limitation of our study is that we are constrained to self-reported outcomes, raising the question as to whether the estimated effects translate into changes in economic behavior, such as quits or changes in earnings. We have made an attempt at estimating whether there is a treatment effect on job turnover more than two years after we sent the first treatment. We linked information from online directories as of March 2011 for the three campuses to our original sample to assess whether there is a relationship between treatment and whether individuals were still at the campus in March 2011. This exercise is summarized in Table 10. Reassuringly, the analysis shows that our outcome variable for job search is highly predictive of whether someone left the school in the previous two years. However, we do not find differences in the probability of leaving by treatment status. It is impossible to know whether this lack of an effect reflects a treatment effect that was transitory, or whether there were too many confounders to be able to detect any differences. There are several important potential confounders. First, information about the Sacramento Bee website (as well as other subsequent websites) has been diffusing over time so that the treatment effect of our initial intervention has become diluted. Second, we no longer have a clean control because to determine whether we had a treatment, at the end of our original survey we asked the control group whether they were aware of the Sacramento Bee website. Third, our survey took place during a severe recession with a high unemployment rate in the state of California making it difficult for UC workers to quit their jobs. Because of these challenges, we view the development of research designs to estimate the longer-term effects of salary disclosure on behavior as a promising path for future research.

5 Conclusion

We evaluate the effects of our information treatment on employees' satisfaction and on their job search intentions. We find that the information treatment has a negative effect on people paid below the median for their unit and occupation, with no effect on more highly-paid individuals. For workers below the median, there is a relationship between the treatment response in satisfaction and distance from the median. This relationship appears to be generally stronger for wage rank than the relative wage level. 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 support previous observational empirical studies and many lab experiment studies on relative income. Our evidence also suggests that access to information about pay disparity at the workplace increases concerns about both pay setting fairness and nationwide inequality. We have only a very limited window on the effects of salary disclosure on long-term changes of economic behavior. Finding ways to estimate these longer-term effects through experimental or quasi-experimental research designs is a promising path for future research.

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 the models of Frank (1984), Bewley (1999), or Bartling and von Siemens (2010b).

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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. Placebo treatment departments were chosen among control departments which did not receive the information treatment.

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)				
Treatment Effects:						
Treated individual (all in treated departments)	-3.53	-3.81	-3.38	-3.47	-3.82	--
	(0.70)	(0.54)	(0.79)	(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	-5.46	-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)	--	--
Response Incentive Effects:						
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	-0.15	0.00	-0.52	-0.55	0.00	0.00
department	(1.29)	--	(1.43)	(1.46)	--	--
Treatment Effects Based on Relative Wage:						
Treated individual with wage less than median in pay unit	--	--	--	--	--	-3.60
						(0.79)
Treated individual with wage greater than median in pay unit	--	--	--	--	--	-4.04
						(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. Columns 1-2 include the full sample while columns 3-6 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	0.91
			(1.61)	
Percent matched to wage data	76.3	75.2	0.12	0.10
			(1.15)	
Sample Matched to Wage Data (N=31,887)				
Mean base earnings ($1000's)	54.73	58.26	2.50	2.04
			(1.23)	
Mean total earnings (base + supplements, $1000's)	63.35	66.93	2.34	1.22
			(1.91)	
Percent with total earnings < $20,000	13.2	12.8	-0.37	0.47
			(0.77)	
Percent with total earnings > $100,000	15.3	16.9	0.90	0.77
			(1.16)	
Percent responded to survey with non-missing responses for 8 key variables	21.1	17.8	-2.76	4.49
			(0.61)	
Survey Respondents with Wage Data and non-Missing Values (N=6,411)				
Percent faculty	15.0	17.9	1.22	0.68
			(1.79)	
Mean total earnings (base + supplements, $1000's)	65.61	69.09	1.69	0.75
			(2.23)	
Percent female	60.9	61.0	0.43	0.24
			(1.79)	
Percent age 35 or older	72.9	75.9	1.68	1.15
			(1.46)	
Percent employed at UC 6 years or more	59.1	62.7	1.03	0.62
			(1.67)	
Percent in current position 6 years or more	40.3	43.8	1.76	1.08
			(1.63)	

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)	28.3 (1.6)	28.3 (1.6)	28.5 (1.6)	--	28.7 (2.1)
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 × deviation of wage from median in pay unit (coefficient × 100)	--	--	--	--	--	-0.2 (0.7)
Treated individual × deviation of wage from median in pay unit if deviation positive (coefficient × 100)	--	--	--	--	--	0.0 (1.0)
Dummy for response incentive (test for selection bias in respondent sample)	--	--	0.0 (1.8)	--	--	--
Dummy for wage less than median in pay unit (coefficient × 100)	--	--	--	--	-1.4 (1.9)	--
Deviation of wage from median (coefficient × 100)	--	--	--	--	--	-0.2 (0.50)
Deviation of wage from median if deviation positive (coefficient × 100)	--	--	--	--	--	0.2 (0.60)
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	--	--	--	--	0.75	0.89

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. 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: Treatment Effects 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 employees	Any of those in cols. 2-5
	(1)	(2)	(3)	(4)	(5)	(6)
Mean rate of use for control group (percent)	24.3	15.2	10.1	6.4	13.2	23.9
Estimated treatment effect from model with basic controls:						
Treated individual (coefficient × 100)	27.8 (2.4)	24.1 (2.2)	15.0 (1.7)	7.5 (1.4)	9.5 (2.0)	27.6 (2.4)
Estimated treatment effect from interacted model with basic controls:						
Treated individual with wage less than median in pay unit (coefficient × 100)	29.5 (3.5)	25.4 (3.3)	14.5 (2.3)	7.6 (2.0)	10.6 (2.9)	29.4 (3.5)
Treated individual with wage greater than median in pay unit (coefficient × 100)	26.3 (2.8)	23.0 (2.7)	15.6 (2.1)	7.4 (1.7)	8.7 (2.4)	26.1 (2.8)
P-value for equality of treatment effectsa	0.45	0.54	0.72	0.92	0.56	0.41

Notes: Estimated on sample of 2,806 survey respondents from UCLA (1,880 controls, including those assigned placebo treatment, and 926 treated
[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

	Satisfaction Index (10 point scale)				Likely to Look for New Job (1-3 scale)				Dissatisfied and Likely Looking for a New Job (0-1)			
				Heckit				ML Select. Model				ML Select. Model
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated individual (coefficient × 100)	-3.2	--	--	--	4.0	--	--	--	8.8	--	--	--
	(3.3)				(3.4)				(5.1)			
I. Treated individual with wage ≤ than median in pay unit (coefficient × 100)	--	-9.6	-9.0	-5.1	--	11.6	11.5	12.7	--	20.1	19.8	19.6
		(4.5)	(4.5)	(2.9)		(4.5)	(4.5)	(5.0)		(6.6)	(6.6)	(7.8)
II. Treated individual with wage > than median in pay unit (coefficient × 100)	--	2.7	1.9	3.1	--	-3.3	-0.9	-2.2	--	-5.0	-3.7	-5.5
		(4.1)	(4.1)	(2.8)		(4.9)	(4.6)	(5.3)		(7.5)	(7.5)	(8.0)
II-I	--	12.3	10.9	8.2	--	-14.9	-12.4	-14.9	--	-25.2	-23.5	-25.1
		(5.4)	(5.4)	(3.6)		(6.6)	(6.4)	(6.5)		(9.6)	(9.5)	(9.6)
Inverse Mills Ratio	--	--	--	-0.14	--	--	--	--	--	--	--	--
				(0.14)								
Correlation	--	--	--	-0.21	--	--	--	-0.13	--	--	--	0.06
								(0.25)				(0.32)
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	Yes	No	No	No	Yes	No	No	No	Yes	No
P-value for exclusion of treatment effects	0.34	0.05	0.09	0.07	0.24	0.03	0.04	0.03	0.08	0.01	0.01	0.02

Notes: Unless otherwise noted, specifications are ordered probit models. Model (4) is a two-step Heckman selection model. Models (8) and (12) are maximum likelihood selection models for an ordered categorical dependent variable. See text for details on the specification of the selection models. 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. The satisfaction index is the average of responses for the questions: "How satisfied are you with your wage/salary on this job?", "How satisfied are you with your job?", and "Do you agree or disagree that your wage is set fairly in relation to others in your department/unit?". Responses are ordered so that higher values indicate greater satisfaction. The variable "Dissatisfied and Likely Looking for a New Job" is 1 if the respondent is below the median value of the satisfaction index (median = 8/3) and reports being "very likely" to make an effort to find a new job. See text and Appendix Table 2 for further details on the construction of the dependent variables.

In addition to the explanatory variables presented in the table, all models other than (1), (5), and (9) include an indicator for whether the respondent is paid at least the median in his/her pay unit.

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

	Satisfaction Index (10 point scale)				Likely to Look for New Job (1-3 scale)				Dissatisfied and Likely Looking for a New Job (0-1)			
				Heckit				ML Select. Model				ML Select. Model
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated individual × deviation of wage from median if deviation negative (coefficient × 100)	2.6	--	-1.4	--	-4.1	--	-1.8	--	-6.3	--	0.0	--
	(1.4)		(2.4)		(1.5)		(2.5)		(2.2)		(3.6)	
Treated individual × deviation of wage from median if deviation positive (coefficient × 100)	-1.1	--	-1.7	--	-1.2	--	-1.8	--	-1.4	--	-0.7	--
	(1.0)		(1.6)		(1.1)		(1.7)		(1.9)		(2.4)	
Treated individual × deviation of rank from 0.5 if deviation negative (coefficient × 10)	--	3.8	5.4	1.9	--	-4.6	-2.9	-4.9	--	-7.3	-7.3	-7.2
		(1.5)	(2.8)	(1.1)		(1.7)	(2.9)	(1.9)		(2.3)	(4.0)	(2.7)
Treated individual × deviation of rank from 0.5 if deviation positive (coefficient × 10)	--	-0.8	1.6	0.2	--	-1.3	1.1	-0.8	--	-2.5	-1.5	-2.6
		(1.4)	(2.5)	(1.0)		(1.7)	(2.7)	(1.8)		(2.9)	(4.2)	(3.1)
Inverse Mills Ratio	--	--	--	-0.15	--	--	--	--	--	--	--	--
				(0.14)								
Correlation	--	--	--	-0.24	--	--	--	-0.13	--	--	--	0.02
								(0.26)				(0.34)
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	0.11	0.05	0.05	0.2	0.02	0.02	0.08	0.02	0.01	0.00	0.02	0.01

Notes: Unless otherwise noted, specifications are ordered probit models. Model (4) is a two-step Heckman selection model. Models (8) and (12) are maximum likelihood selection models for an ordered categorical dependent variable. See text for further detail on the specification of the selection models. 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. See note to Table 5 for description of the dependent variables. In addition to the explanatory variables presented in this table, specifications 1, 3, 5, 7, 9, and 11 include the deviation of the wage from the median wage in the pay unit if the deviation is positive, the deviation of the wage from the median wage in the pay unit if the deviation is negative, and an indicator for whether the deviation is negative.

Specifications 2-4, 6-8 and 10-12 include the deviation of the rank in the pay unit from 0.5 if the deviation is positive, the deviation of the rank in the pay unit from 0.5 if the deviation is negative, and an indicator for whether the deviation is negative.

Table 7: Effect of Information Treatment on Job Satisfaction by Pay Relative to Campus/Occupation Median

	Satisfaction Index (10 point scale)		Likely to Look for New Job (1-3 scale)		Dissatisfied and Likely Looking for a New Job (0-1)	
	Faculty	Staff	Faculty	Staff	Faculty	Staff
	(1)	(2)	(3)	(4)	(5)	(6)
I. Treated individual with wage ≤ than occupation/campus median (coefficient × 100)	-24.7	-8.7	10.2	13.2	22.8	17.8
	(10.4)	(5.3)	(11.1)	(5.3)	(15.6)	(6.8)
I. Treated individual with wage > than occupation/campus median (coefficient × 100)	25.2	-0.2	-10.9	-2.4	-10.0	-1.7
	(8.9)	(4.4)	(11.2)	(5.5)	(19.6)	(8.8)
II-I	50.0	8.5	-21.2	-15.6	-32.8	-19.5
	(14.0)	(6.0)	(15.0)	(7.4)	(24.8)	(10.5)
Controls for campus and cubic in wage?	Yes	Yes	Yes	Yes	Yes	Yes
P-value for exclusion of treatment effects	0.00	0.24	0.37	0.04	0.30	0.03

Notes: A low pay department/unit corresponds to a department/unit where the department/unit median wage is below the median department at the campus, computed separately for faculty and staff. In addition to the explanatory variables presented in the table, all models other than (1), (5), and (9) include an indicator for whether the department/unit median (computed separately for faculty/staff) is below the campus median for all departments/units by faculty/staff.

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

	Satisfaction Index (10 point scale)			Likely to Look for New Job (1-3 scale)			Dissatisfied and Likely Looking for a New Job (0-1)		
	Treatment	Placebo	p-value[a]	Treatment	Placebo	p-value[a]	Treatment	Placebo	p-value[a]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated individual with wage less than median in pay unit (coefficient × 100)	-12.7	2.2	0.06	11.8	-7.3	0.08	29.2	-18.7	0.01
	(7.2)	(7.2)		(7.4)	(9.6)		(9.5)	(16.4)	
Treated individual with wage more than median in pay unit (coefficient × 100)	-3.3	-2.4	0.90	-0.7	-10.9	0.23	-7.8	6.9	0.23
	(6.1)	(6.1)		(7.3)	(7.5)		(10.7)	(11.0)	
Controls for staff/faculty status and cubic in wage?	yes	yes		yes	yes		yes	yes	
Observations	2,303	1,880		2,303	1,880		2,303	1,880	

Notes: Specifications are ordered probit models. Standard errors, clustered by campus/department, are in parentheses. "Treatment" in the columns denotes the information treatment. "Placebo" denotes the placebo treatment. Sample is for UCLA only. Treatment specifications exclude the placebo group. Placebo specifications exclude the treatment group. Standard errors, clustered by campus/department, are in parentheses. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Models are based on specifications 2, 6 and 10 of Table 5. For additional details see notes to Table 5 and text.

[a] p-value for hypothesis that placebo and treatment effects are equal.

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

	Differences in Income in America Are Too Large (1-4 Scale)			
	(1)	(2)	(3)	(4)
Treated individual (coefficient × 100)	7.1	6.8	7.9	7.5
	(3.7)	(3.7)	(4.7)	(4.7)
Treated individual with wage less than median in pay unit (coefficient × 100)			-1.5	-1.4
			(6.2)	(6.2)
Controls for campus × (staff/faculty)	Yes	Yes	Yes	Yes
Demographic controls (gender, age, tenure and time in position)?	No	Yes	No	Yes

Notes: Specifications are ordered probit models. Standard errors, clustered by campus/department, are in parentheses (818 clusters). Dependent variable is "4" if respondent "strongly agrees" that differences in income in America are too large, "3" if they "agree", "2" if they "disagree", and "1" if they "strongly disagree". See Appendix Table 2 for means of the variable. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. In addition to the explanatory variables presented in the table, models (3) and (4) include an indicator for whether the respondent is paid at least the median in his/her pay unit.

Table 10: Linear Probability Models for Effect of Information Treatment on Job Mobility

	Responders (N=6,835)			Nonresponders (N=25,048)		Full sample (N=31,883)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reported "very likely" to make a genuine effort to find a new job (coefficient × 100)	20.5						
	(1.6)						
Reported "somewhat likely" to make a genuine effort to find a new job (coefficient × 100)	5.9						
	(1.1)						
Treated individual (coefficient × 100)		-0.1		-0.2		0.0	
		(1.3)		(1.2)		(1.1)	
Treated individual with wage ≤ than median in pay unit (coefficient × 100)			0.4		-0.76		-0.40
			(1.9)		(1.5)		(1.4)
Treated individual with wage > than median in pay unit (coefficient × 100)			-0.6		0.36		0.31
			(1.5)		(1.2)		(1.1)
Controls for campus × (staff/faculty) and cubic in wage?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependant variable is 1 if we were able to locate individuals in the original sub-sample matched to wage data (see Table 2) to online campus directories in March 2011. We found 49% of original sample in UCSC, 76% in UCSD, and 74.5% in UCLA. Excluded category in column (1) is "not likely at all". The mean of the dependant variable for the full sample is 0.27. In addition to the explanatory variables presented in the table, models 3, 5, and 7 include an indicator for whether the respondent is paid at least the median in his/her pay unit.

A Appendix (not for publication)

A.1 Survey Questions

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 C1-C5 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.

2. (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

3. (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

4. 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

5. 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.

A.2 Additional Empirical Results

Appendix Table A1 presents estimates of earnings changes of workers at UC Los Angeles between 2007 and 2008 as a function of their 2007 level of earnings for workers who are paid above and below the median in their department and occupation. The sample is all UCLA employees matched to their earnings and who are present in both 2007 (base year) and 2008 (following year). We further restrict the sample to employees with base pay above $10,000 in 2007 (this is done to avoid having extreme log changes that bias results in favor of our hypothesis, qualitative results are highly robust to changes in the $10,000 cut-off). The pay unit (for median computation) refers to faculty or staff members in an individual's department (in this sample with base earnings above $10,000). Medians are defined separately for base pay and total pay. Col. (1) reports the average for employees below median, col. (2) for employees above median, col. (3) reports the difference (2)-(1). Cols. (4) and (5) report the coefficients of regressing the left-hand-side variable on own pay and base median pay (those two right-hand-side variables are in log when the left-hand-side is log and use the same definition of base vs. total pay as the left-hand-side variables). The table shows that workers below the median experience significant earnings gains relative to those above the median, and that holding wage constant, the higher the median the higher is future wage growth on average.

Appendix Table A2 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.

Appendix Table A3 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 52% 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.

Appendix Table A4 presents estimates from model 2 of Table 5 using the survey measures from which the satisfaction index is derived. The coefficient on the interaction of treatment with earning below pay unit median is negative for all of the sub-components of the satisfaction index with varying levels of precision.

Appendix Table A5 presents estimates comparable to Table 6 but adding a main treatment effect in the specification. Some precision is lost in the estimates but the results remain generally consistent with those presented in Table 6 and again show that the rank variable appears to be more significant in the treatment response than relative wage levels.

Appendix Table Table A6 shows how our results vary for specific subgroups of employees. In particular, we show estimates by gender (columns 1 and 2), by faculty/staff status (columns 3 and 4) and by length of tenure (columns 5 and 6). For ease of exposition, we present results only for the baseline specification presented in columns 2, 6, and 10 of Table 5.

Men and women respond differently to the information treatment. Both express elevated dissatisfaction following the information treatment of about the same magnitude (Panel A), but women appear more inclined to report that they are searching for a new job following treatment (Panel B). Staff appear to be more responsive than faculty to the treatment on both measures, but the relatively small number of faculty (n=1015) results in imprecision that limits our ability to make reliable comparisons. The pattern of results for tenure also cut both ways. We observe significant negative satisfaction effects for low-paid respondents with higher tenure (defined as 6 or more years of seniority) but not for lower tenure respondents (Panel A, columns 5 and 6). By contrast, low-paid and low-tenure respondents respond to the treatment by elevated job search intentions while high tenure respondents do not (Panel B, columns 5 and 6).

It is not surprising that higher-tenure respondents in the treatment are not reporting elevated job search intentions relative to the control because in the UC system few employees with long tenure change jobs. Indeed, this finding is a useful specification check because it suggests that respondents are responding to our survey truthfully. One might be concerned that a respondent who is not mobile, and does not plan to change jobs, nevertheless reports to us that they plan to look for a new job because they are upset by what they learned from the salary data and because talk is cheap. The tenure result is suggestive that this is not occurring systematically.

Following this intuition, we provide a formal test of the "truthfulness" of responses in Appendix Table A7. We first estimate a probit model on the control group sample where the dependent variable is 1 if the respondent reports being "very likely" to be searching for a new job with age, gender, tenure, faculty/staff, campus, and time in position dummies as covariates. From the probit, we predict the probability that a respondent reports to be very likely to look for a new job, both in the treatment and control. If respondents are answering the question of job search intention truthfully, we should see a limited response in the treatment from respondents who are unlikely to report searching for a new job because of their characteristics.

For ease of exposition, Panel A limits the sample to workers who are paid less than the pay unit median. Column 1 is a model with just a treatment dummy it shows the expected result that below-median respondents are more likely to report searching for a job in the treatment than control. Column 2 includes an interaction of the treatment with the predicted probability of job search. Consistent with truthful response, workers who we predict are less mobile because of their characteristics do not respond to the treatment by reporting to us that they plan to look for a new job. The interaction term is statistically significant ($t = 2.5$) and implies that the response to the treatment on job search intention is only positive when the respondent has more than a 17% predicted chance of looking for a new job. As could be expected, this interaction does not enter significantly when using the satisfaction index (columns 3 and 4): low mobility workers can still report elevated dissatisfaction as a result of new information. Panel B shows the same set of models for the above-median respondents and, not surprisingly, the interaction term is insignificant in both models.

Appendix Table A1: Earnings Growth and Median Peer Earnings

	Below/Above Median comparison			Regressions	
	Below Median	Above Median	Difference (2)-(1)	Own 2007 pay	Median 2007 pay
	(1)	(2)	(3)	(4)	(5)
A. Log pay changes from 2007 to 2008					
Δ log (total pay)	.116	.026	-.090	-.174	.145
			(.007)	(.006)	(.009)
Δ log (base pay)	.121	.023	-.098	-.201	.204
			(.007)	(.007)	(.011)
B. Level pay changes from 2007 to 2008					
Δ (total pay)	6775	4906	-1870	.013	.035
			(433)	(.004)	(.006)
Δ (base pay)	6246	4142	-2104	-.028	.088
			(262)	(.004)	(.006)
Observations	6654	6505	13,159	13,159	13,159

Notes: Standard errors are in parentheses. Sample is all UCLA employees matched to their earnings and who are present in both 2007 (base year) and 2008 (following year). We further restrict the sample to employees with base pay above $10,000 in 2007 (this is done to avoid having extreme log changes that bias results in favor of our hypothesis, qualitative results are highly robust to changes in the $10,000 cut-off). Pay unit (for median computation) refers to faculty or staff members in an individual's department (in this sample with base earnings above $10,000). Medians are defined separately for base pay and total pay. Col. (1) reports the average for employees below median, col. (2) for employees above median, col. (3) reports the difference (2)-(1). Cols. (4) and (5) report the coefficients of regressing the left-hand-side variable on own pay and base median pay (those two right-hand-side variables are in log when the left-hand-side is log and use the same definition of base vs. total pay as the left-hand-side variables).

Appendix Table A2: 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 A3: 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=6411)	16.3	31.9	40.1	11.7
	Control Group (N=4635)	15.9	32.5	39.5	12.1
	Controls Reweighted[a]	15.6	32.9	39.6	11.8
	Treatment Group (N=1776)	17.3	30.4	41.8	10.6
"How satisfied are you with your job?"	Overall Sample (N=6411)	3.3	12.1	47.3	37.3
	Control Group (N=4635)	3.3	12.2	47.4	37.2
	Controls Reweighted[a]	3.0	12.1	47.1	37.8
	Treatment Group (N=776)	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=6411)	47.0	30.8	22.2
	Control Group (N=4635)	47.2	30.7	21.9
	Controls Reweighted[a]	47.5	30.5	22.1
	Treatment Group (N=1776)	45.8	31.1	23.1

		Strongly Disagree	Disagree	Agree	Strongly Agree
"Do you agree or disagree that your wage i set fairly in relation to others in your department/unit?"	Overall Sample (N=6411)	11.7	31.1	47.5	9.8
	Control Group (N=4635)	11.4	31.0	47.8	9.9
	Controls Reweighted[a]	11.3	31.4	47.5	9.8
	Treatment Group (N=1766)	12.6	31.1	46.9	9.4
"Do you agree or disagree that differences in income in America are too large?"	Overall Sample (N=6397)	1.9	11.4	38.1	48.5
	Control Group (N=4625)	2.1	11.6	38.8	47.6
	Controls Reweighted[a]	2.2	11.4	38.5	48.0
	Treatment Group (N=1772)	1.6	11.0	36.5	51.0

		1	4/3	5/3	2	7/3	8/3	9	10/3	11/3	4
Satisfaction Index (10 point scale)	Overall Sample (N=6411)	1.3	2.7	5.8	9.8	14.7	18.3	20.4	15.4	7.4	4.2
	Control Group (N=4635)	1.3	2.7	5.6	9.6	14.9	18.5	20.5	15.2	7.3	4.5
	Controls Reweighted[a]	1.2	2.5	5.7	9.4	15.1	19.0	20.5	15.2	7.0	4.6
	Treatment Group (N=1766)	1.4	2.7	6.2	10.3	14.3	17.9	20.2	16.1	7.6	3.4

		No	Yes
Dissatisfied and likely to make an effort to find a job	Overall Sample (N=6411)	86.6	13.4
	Control Group (N=4635)	87.1	12.9
	Controls Reweighted[a]	87.2	12.8
	Treatment Group (N=1766)	85.1	14.9

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 A4: Ordered Probit Models for Effect of Information Treatment on Measures of Job Satisfaction

	Wage is fair (1-4 scale) (1)	Satisfied with Wage on Job (1-4 scale) (2)	Satisfied with Job (1-4 scale) (3)	Likely to Look for New Job (1-3 scale) (4)
I. Treated individual with wage < than median in pay unit (coefficient × 100)	-10.1 (4.9)	-6.3 (4.5)	-8.5 (4.9)	11.6 (4.5)
II. Treated individual with wage > than median in pay unit (coefficient × 100)	2.5 (4.5)	-0.5 (4.5)	6.3 (4.4)	-3.3 (4.9)
II-I	12.6 (6.0)	5.8 (5.7)	14.8 (6.5)	-14.9 (6.6)
Controls for campus × (staff/faculty) and cubic in wage?	Yes	Yes	Yes	Yes
P-value for exclusion of treatment effects	0.08	0.38	0.07	0.03

Notes: Specifications are ordered probit models. 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. See Appendix Table 3 and text for description and means of the depedent variables. For columns 1-3 responses are ordered so that higher values indicate greater satisfaction. Models are based on specification (2) of Table 5. In addition to the explanatory variables presented in the table, all models include an indicator for whether the respondent is paid at least the median in his/her pay unit.

Appendix Table A5: Ordered Probit Models for Effect of Information Treatment on Measures of Job Satisfaction

	Satisfaction Index (10 point scale)				Likely to Look for New Job (1-3 scale)				Dissatisfied and Likely Looking for a New Job (0-1)			
				Heckit				ML Select. Model				ML Select. Model
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated individual (coefficient × 100)	2.4	10.3	9.8	6.4	2.7	0.6	-0.1	1.1	6.8	6.7	6.5	6.5
	(4.3)	(5.7)	(5.8)	(3.6)	(4.6)	(6.8)	(6.8)	(6.8)	(6.5)	(8.7)	(8.8)	(8.9)
Treated individual × deviation of wage from	3.1	--	-1.6	--	-3.6	--	-1.8	--	-4.8	--	-0.2	--
median if deviation negative (coefficient × 100)	(1.6)		(2.4)		(1.7)		(2.5)		(2.6)		(3.6)	
Treated individual × deviation of wage from	-1.4	--	-1.4	--	-1.6	--	-1.8	--	-2.4	--	-0.5	--
median if deviation positive (coefficient × 100)	(1.1)		(1.6)		(1.2)		(1.6)		(2.2)		(2.4)	
Treated individual × deviation of rank from 0.5	--	7.0	8.7	4.0	--	-4.4	-2.9	-4.6	--	-5.2	-5.1	-5.1
if deviation negative (coefficient × 10)		(2.1)	(3.2)	(1.5)		(2.7)	(3.6)	(2.7)		(3.3)	(4.6)	(3.4)
Treated individual × deviation of rank from 0.5	--	-3.9	-1.8	-1.9	--	-1.4	1.1	-1.1	--	-4.6	-3.8	-4.7
if deviation positive (coefficient × 10)		(2.2)	(3.2)	(1.4)		(2.7)	(3.4)	(2.7)		(4.2)	(5.5)	(4.2)
Inverse Mills Ratio	--	--	--	-0.12	--	--	--	--	--	--	--	--
				(0.14)								
Correlation	--	--	--	-0.2	--	--	--	-0.14	--	--	--	0.04
								(0.25)				(0.33)
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	0.18	0.01	0.01	0.06	0.04	0.06	0.1	0.05	0.02	0.01	0.3	0.02

Notes: Unless otherwise noted, specifications are ordered probit models. Model (4) is a two-step Heckman selection model. Models (8) and (12) are maximum likelihood selection models for an ordered categorical dependent variable. See text for further detail on the specification of the selection models. 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. See note to Table 5 for description of the dependent variables. In addition to the explanatory variables presented in this table, specifications 1, 3, 5, 7, 9, and 11 include the deviation of the wage from the median wage in the pay unit if the deviation is positive, the deviation of the wage from the median wage in the pay unit if the deviation is negative, and an indicator for whether the deviation is negative.

Specifications 2-4, 6-8 and 10-12 include the deviation of the rank in the pay unit from 0.5 if the deviation is positive, the deviation of the rank in the pay unit from 0.5 if the deviation is negative, and an indicator for whether the deviation is negative.

Appendix Table A6: Ordered Probit Models for Effect of Information Treatment -- by Subgroup

Panel A: **Satisfaction Index (10 point scale)**	Females (1)	Males (2)	Staff (3)	Faculty (4)	Low Tenure (5)	High Tenure (6)
I. Treated individual with wage < than median in pay unit (coefficient × 100)	-9.2	-10.0	-10.9	-3.0	-4.7	-14.3
	(5.5)	(6.8)	(5.3)	(9.9)	(6.1)	(6.3)
II. Treated individual with wage > than median in pay unit (coefficient × 100)	6.1	-1.5	2.2	5.7	-4.7	4.6
	(5.6)	(6.2)	(4.5)	(9.6)	(7.5)	(4.8)
II-I	15.3	8.4	13.1	8.7	0.0	18.9
	(7.5)	(8.6)	(6.3)	(13.9)	(9.0)	(7.4)
P-value for exln. of treatment effects	0.11	0.34	0.08	0.80	0.64	0.03
Observations	3908	2503	5396	1015	2558	3853

Panel B: **Likely to Look for New Job (1-3 scale)**	Females (1)	Males (2)	Staff (3)	Faculty (4)	Low Tenure (5)	High Tenure (6)
I. Treated individual with wage < than median in pay unit (coefficient × 100)	17.8	0.0	14.9	-4.8	19.5	3.8
	(5.7)	(8.2)	(5.2)	(10.8)	(6.4)	(6.9)
II. Treated individual with wage > than median in pay unit (coefficient × 100)	-7.2	1.4	-4.8	3.5	-1.5	-3.9
	(6.4)	(7.0)	(5.5)	(11.7)	(8.5)	(5.6)
II-I	-25.1	1.5	-19.6	8.3	-21.0	-7.6
	(8.1)	(11.4)	(7.3)	(15.2)	(10.3)	(9.1)
P-value for exln. of treatment effects	0.00	0.98	0.01	0.86	0.01	0.69

Panel C: **Dissatisfied and Likely Looking for a New Job (0-1)**	Females (1)	Males (2)	Staff (3)	Faculty (4)	Low Tenure (5)	High Tenure (6)
I. Treated individual with wage < than median in pay unit (coefficient × 100)	20.6	19.0	21.3	12.9	21.3	19.7
	(7.8)	(11.1)	(7.3)	(15.4)	(8.4)	(9.6)
II. Treated individual with wage > than median in pay unit (coefficient × 100)	-7.5	-1.8	-6.4	5.6	2.4	-8.3
	(9.9)	(10.2)	(8.2)	(18.9)	(11.4)	(9.1)
II-I	-28.1	-20.8	-27.7	-7.3	-18.9	-28.1
	(12.1)	(15.8)	(10.3)	(23.8)	(13.5)	(13.2)
P-value for exln. of treatment effects	0.02	0.23	0.01	0.68	0.04	0.08

Notes: Standard errors, clustered by campus/department, are in parentheses. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. Models are based on specifications 2, 6 and 10 of Table 5. For additional details see notes to Table 5 and text.

Appendix Table A7: Effect of Predicted Mobility on Search and Satisfaction Treatment Effects

	Likely to Look for New Job (1-3 scale)		Satisfaction Index (10 point scale)	
	(1)	(2)	(3)	(4)
Panel A: Workers with wage ≤ median				
Treated individual (coefficient × 100)	11.1	-25.9	-9.3	-11.7
	(4.6)	(16.7)	(4.5)	(14.5)
Treated individual × Predicted probability of search (coefficient × 100)	--	1.5	--	0.1
		(0.6)		(0.6)
Predicted probability of search	--	3.5	--	-0.1
		(0.3)		(0.3)
Controls for campus × (staff/faculty) and cubic in wage?	Yes	Yes	Yes	Yes
Panel B: Workers with wage > median				
Treated individual (coefficient × 100)	-3.7	-14.6	3.4	19.2
	(4.8)	(12.8)	(4.0)	(10.4)
Treated individual × Predicted probability of search (coefficient × 100)	--	0.6	--	-0.8
		(0.6)		(0.5)
Predicted probability of search	--	3.5	--	0.1
		(0.3)		(0.3)
Controls for campus × (staff/faculty) and cubic in wage?	Yes	Yes	Yes	Yes

Notes: Standard errors, clustered by campus/department, are in parentheses. "Wage" refers to total UC payments in 2008. Pay unit refers to faculty or staff members in an individual's department. The predicted probability of search is the predicted value from a probit model estimated over the control group where the dependent variable is 1 if the respondent reports being "very likely" to be searching for a new job, with age, gender, tenure, faculty/staff, campus, and time in position dummies as covariates. See note to Table 5 for definitions of the dependent variables.