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

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Abstract

We study the effect of disclosing information on peers' salaries on workers' job satisfaction and job search intentions. A randomly chosen subset of employees of the University of California 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. We find an asymmetric response to the information about peer salaries: 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. Those negative treatment effects are concentrated among employees in the first quartile of each pay unit. Differences in pay rank matter more than differences in pay levels. Our findings suggest that job satisfaction depends on relative pay comparisons, and that this relationship is non-linear (*JEL* J24).

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Economists have long been interested in the possibility that individuals care about both their absolute income and their income relative to others.[1] Recent studies have documented systematic correlations between relative income and 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).[2] Despite confirmatory findings 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' salaries.[3] 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 (UC). 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. A few days later we surveyed all campus employees, eliciting information about their use of the Sacramento Bee website, their pay and job satisfaction, and their job search intentions. We compare the answers of people in the treatment group (who were informed about the site) to those of the control group (who were not). We match administrative salary data to the survey responses to examine how the effects of the information treatment depend on an individual's earnings relative to his or her peers, defined as co-workers in the same occupation group (faculty vs. staff) *and* administrative unit (i.e., department or school) within the University.

Our information treatment had a large impact on use of the Sacramento Bee website, raising

[1] The classic early reference is 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] Other studies have found a more important role for absolute income than relative income, e.g., Stevenson and Wolfers (2008). Kuhn et al. (2011) find that people do not experience reduced happiness when their neighbors win the lottery.

[3] A number of recent empirical studies have used similar manipulations of information 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. (2011) on Medicare prescription drug plans.

the fraction of people who accessed the site from 20 percent to nearly 50 percent. Four-fifths of the new users reported that they investigated the earnings of colleagues in their own department or pay unit. This strong "first stage" result establishes that workers are interested in co-workers' pay – 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.

Accessing information on the Sacramento Bee website allows employees to update their beliefs about their peers' pay. In a relative income model this information treatment will have a negative effect on the job satisfaction of lower-earning workers in a peer group, and a positive effect on higher-earning workers. If satisfaction is a concave function of relative pay, as assumed in the inequality aversion model of Fehr and Schmidt (1999), the negative effects on low-wage earners will be larger than the positive effects on high-wage earners. In our experiment, we find that the information treatment caused a reduction in job satisfaction among workers with pay below the median for their department and occupation group, and an increase in their intention to look for a new job. By comparison, treatment group members who were paid above the median report no significant changes in job satisfaction or job search intentions. Responses to the treatment appear to be more closely related to an individual's rank in the salary distribution than to his or her relative pay level, and to be strongest among people in the lowest quartile of the pay distribution of their unit. We also study the effect of the information treatment on actual turnover and find some suggestive evidence of an effect on the job-leaving rates, particularly for those in the first quartile of pay in their unit.

Our results provide credible field-based confirmation of the importance of relative pay comparisons that have been identified in earlier observational studies of job turnover (Kwon and Milgrom, 2008), job satisfaction (Clark and Oswald, 1996; Hamermesh, 2001) and happiness (Frey and Stutzer, 2002; Luttmer, 2005), and in some (but not all) lab-based studies.[4] They lend specific support to the hypothesis that negative comparisons matter more than positive comparisons for worker's perceived job satisfaction. Our findings also contribute to the literature

[4]Lab-based experimental studies have developed a series of games such as the dictator game, the ultimatum game, and the trust game (see Rabin 1998 for a survey) showing evidence that relative outcomes matter. See in particular Fehr and Falk 1999, 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 (2011) 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. 2011).

on pay secrecy policies.[5] 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 pay information suggests that employers have an incentive to maintain pay secrecy, since the costs for lower-paid employees exceed the benefits for their high-wage peers.

The remainder of the paper is organized as follows. Section I presents a simple conceptual framework for structuring our empirical investigation. Section II describes the experimental design, our data collection and assembly procedures, and selection issues. Section III presents our main empirical results. Section IV concludes. Supplementary results are gathered in an online appendix.

I Conceptual Framework

Theoretically there are two broad reasons why information on peer salaries may affect workers' utilities. In this section we briefly discuss them. A more extensive development is presented in Card, Mas, Moretti and Saez (2010).

Relative Income Model. A first reason why information on peer salaries may affect utility is that that workers care directly about relative pay, as in Clark and Oswald (1996). Consider a worker whose own wage is w and who compares her wage to a reference level, denoted m, which is a function of the wages of co-workers in her reference group. The agent has incomplete information about co-workers' wages, and therefore of m. Let I denote the information set available to the worker: we assume that our experiment changes the information set from I^0 to I^1. Assume that the worker's job satisfaction, given information set I, can be written as:

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

where $u()$ represents the utility from her own pay, e is an individual-specific term representing random taste variation, and $v()$ represents feelings arising from relative pay comparisons. 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

[5] 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.

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:

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

where we assume (without loss of generality) that $v(0) = 0$. With access to the website we assume that individuals can observe m perfectly.[6] Then job satisfaction conditional on using the website is:

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

With additive preferences, the change in the information set from I^0 to I^1 leads to a change in job satisfaction that depends directly on $v(w-m)$. Assuming that $v()$ is increasing, learning about co-worker pay will reduce the satisfaction of low-paid workers and increase the satisfaction of high-paid workers. If in addition $v()$ is concave – as is assumed by Fehr and Schmidt (1999) – workers with $w < m$ will experience relatively large reductions in satisfaction, while those with $w \geq m$ will experience only modest increases.

For purposes of estimation we will assume that the reference-group consists of workers in the same department or administrative unit and faculty/staff grouping.[7] We test for concavity in $v()$ by specifying this function as piece-wise linear with a different slope above and below the median salary within a worker's reference group. We do not view this specification as a literal description of individual preferences, but rather as a simple way to trace out the treatment response function to test whether there are heterogenous effects depending on relative income, and whether these effects are nonlinear.

Rational Updating. People may react to new information on co-worker salaries even if they do not care directly about relative pay. In particular, it is possible that workers have no direct concern over peer salaries, but rationally use this information to update their future pay prospects. If co-worker wages provide a signal about future wages, either through career advancement or a bargaining process, learning that one's wage is low (high) relative to co-workers' salaries leads to updating expected future wage upward (downward). In this model,

[6]The complete information assumption can be relaxed without substantively changing the model.

[7]As discussed below, we find that a large majority of new users who were prompted to look at the site by our information treatment examined the pay of colleagues in their own department. We take this as evidence that the department is the relevant comparison unit.

the revelation of co-workers' salaries raises the job satisfaction of relatively low-wage workers and lowers the satisfaction of relatively high-wage workers. Thus, in contrast to the relative utility model above, learning that one is paid less than one's peers is "good news," while learning that one is paid more is "bad news." See Card, Mas, Moretti and Saez (2010) for details on this model.[8] Our randomized design allows us to measure the effect of information revelation for workers at different points in the salary distribution and thus provide some evidence on the relative merit of these two models.

Incomplete Compliance. In the theoretical model above we have implicitly assumed that all treated individuals access the web site salary information, whereas none of the control group have access to this information. In practice, however, some members of both the treatment and control groups had used the web site prior to our intervention, and not all members of the treatment group used the website after receiving treatment.[9] Thus, some of the treatment group were uninformed, while some of the control group were informed. As in other experimental studies 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 (denoted by $D = 1$) conditional on treatment status, individual wages, and peer mean wages. With this notation, equation (1) becomes

$$S = u(w) + \pi_0 \cdot v(w - m) + T \cdot (\pi_1 - \pi_0) \cdot v(w - m) + e + \phi, \tag{2}$$

where ϕ is an error component reflecting the deviation of an individual's actual information status from his or her expected status.[10] Under the *assumption* that the "information treatment intensity" $\delta \equiv \pi_1 - \pi_0$ is constant across individuals, equation (2) 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. Below we estimate a variety of "first

[8]Of course, other reactions to updating are possible. For example, a worker who learns that her co-workers are highly paid may revise upward her expected future wages, but may experience a decline in job satisfaction because she has to enter into a costly bargaining process with her employer. We thank a referee for pointing out this possibility.

[9]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.

[10]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.

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 $(w-m)$. We find that the information treatment intensity is independent of the observed characteristics of individuals, including their wage and relative wage, suggesting that we can interpret our estimated models as variants of equation (2) with a uniformly attenuated treatment response.[11]

II Data and Experimental Design

A The Experiment

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 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 UC. The Sacramento Bee database was the first to make this information easily accessible. At its 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.

In 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), using the online personnel directories for each institution as the basis for our sample.[12] Our information treatment consisted of an e-mail (sent from special e-mail accounts established at UC Berkeley and Princeton) informing recipients of the existence of the Sacramento Bee website, and asking them to report whether they were aware of the existence of the site or not. The e-mails were sent in October 2008 for UCSC, in November 2008 for UCSD, and in May 2009 for UCLA. The exact text of the e-mail 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 newspa-

[11]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 $(v(w-m))$.

[12]The online directories contain email addresses, as well as employee names, job titles, and departments.

per 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 40 percent of employees at UC Santa Cruz, 25 percent of employees at UC San Diego, and 37.5 percent of employees at UCLA received this information treatment. Our experimental design is described in Appendix Table A0. We stratified by department to allow for the testing of peer interactions in the response to treatment.[13] As shown in detail in Card, Mas, Moretti, Saez (2010), however, there is no evidence of such interactions, and we therefore ignore them in the analysis below. We always cluster our standard errors at the department×occupation (staff vs. faculty) level to reflect the stratified design.

We also randomly selected a subset of UCLA employees to receive a "placebo treatment." As in the treatment group, workers in the placebo group received an e-mail with an introduction explaining that we were conducting a study of pay inequality. The placebo described a UC website listing the salaries of top UC administrators and asked recipients to fill out a 1-question survey on their knowledge of the site. Importantly, this alternative web site provided no information on salaries of typical UC workers. We use responses from people who received the placebo treatment to assess our interpretation of the responses to our primary treatment in light of possible confounders, including priming effects due to the language of the treatment e-mail, and differential response rates between treatments and controls.

Three to ten days after the initial treatment e-mails were sent, we sent e-mails to *all* employees at each campus asking them to respond to a survey. This follow-up survey (reproduced in the online appendix) 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 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 to be offered a chance at one of three $1000 prizes for people who completed the survey.[14] In addition, we sent up to two

[13] 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 percent at UC Santa Cruz, 50 percent at UC San Diego, 75 percent at UCLA).

[14] More precisely, all respondents were eligible for the prize, but only a randomly selected sample were told

additional e-mail reminders asking people to complete the follow-up survey.

B Survey Responses

Our final dataset combines campus and department identifiers from the online directories, treatment status information, follow-up survey responses, and administrative salary data for employees at the three campuses.[15] Overall, just over 20 percent of employees at the three campuses responded to our follow-up survey (Appendix Table A1). 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 1 presents a series of linear probability models for the event that an individual responded to our follow-up survey. The model in column 1 is fit to the overall universe of 41,975 names that we extracted from the online directories and were subject to random assignment. The models in columns 2-4 are fit on the subset of 31,887 names we were able to match to the administrative salary data. The coefficient estimates in column 1 point to three notable conclusions. First, the response rate for people who could be matched to the administrative salary data is significantly higher (+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 -4 to -5 percentage points. This pattern suggests that there was a "nuisance" effect of being sent two e-mails that lowered response rates to the follow-up survey independently of the content of the first e-mail. Third, being offered the response incentive had a sizeable positive (+4 percentage point) effect on response rates.

The models in columns 2-3 are based on the subset of people who can be matched to earnings data, with and without the addition of a cubic polynomial in individual earnings as an extra control. In both cases the estimates are very close to those in column 1. Finally,

about it (see Appendix Table A0 for complete details).

[15] 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 2007. We matched the salary data to the online directory database 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 A1 presents some summary statistics on the success of our matching procedures. Overall, we were able to match about 76 percent of names. The match rate varies by campus, with a high of 81 percent at UCSD and a low of 71 percent 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.

in anticipation of the treatment effect models estimated below, the specification in column 4 allows for a differential treatment effect on response rates for people whose earnings are above or below the median for their occupation and pay unit. The estimation results suggest that the negative response effect of treatment assignment is very similar for people with above-median earnings (-4.0 percent) and below-median earnings (-3.6 percent), and we cannot reject a homogeneous effect. We also fit a variety of richer models allowing interactions between earnings and treatment status, and allowing a potential kink in the effect of earnings at the median of the pay unit. In none of these models could we reject the homogeneous effects specification presented in column 4.

Overall, 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), but it is highly statistically significant. The response gap poses a potential threat to the interpretation of our treatment effect estimates, which rely on data from survey respondents. The very similar negative effects of the information treatment and the placebo treatment, however, suggest that the reduced response rate was not attributable to the content of the treatment e-mail. In light of this fact, we use the survey responses of the placebo group to test whether the responses of the treatment group contain significant selection biases.[16]

C Summary Statistics

Table 2 presents a comparison of employees 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.[17] Beginning with the overall sample in the first panel of the table, note that only about 17 percent of our sample are faculty members. The vast majority are staff, including administrators, employees of the medical centers at two of the campuses, and support staff. As expected given random assignment, the fractions of faculty in the treatment and control groups are not significantly different, after adjusting for campus effects to reflect the differential rates of assignment to treatment at the 3 campuses. About three-quarters of our overall sample can be matched to salary data. Again the fractions matched to salary data in the treatment and control group are very close to equality, consistent with random assignment.

[16] As discussed below, analysis of the placebo group also allows us to investigate potential priming effects associated with the wording of cover email sent with both the main treatment and the placebo treatment.

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

The next panel pertains to the subset of employees who could be matched to earnings data. Base earnings (which exclude over-time, extra payments, etc.) are slightly higher for the treatment group than the control group ($t = 2.0$), but the gap in *total* earnings (which include over-time and supplements like summer pay and housing allowances) is smaller and not significant. As noted above, among those with earnings data the fraction of the treatment group who responded to our follow-up survey is about 3 percentage points lower than the rate for the controls, and the difference is highly significant ($t = 4.5$).

Finally, the bottom panel of Table 2 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 percent staff and 15 percent faculty, with mean total earnings of around $67,000. 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.[18] This provides very reassuring evidence that there was no systematic differential selection across treatment and control groups for responding to our survey, at least based on observable demographic variables. Selection due to unobservable factors remains a possibility that we address using the placebo treatment, as described below.

III Empirical Results

We now turn to our main analysis of the effects of the information treatment. Except in Section III.D, we restrict attention to the subsample of survey respondents in our main analysis sample.

A Effect on Use of the Sacramento Bee Website

We begin in Table 3 by estimating a series of linear probability models that quantify the *first-stage* effect of our information treatment on use of the Sacramento Bee web site.[19] The mean rate of use reported by the control group is 19.1 percent. As shown by the model in column

[18]We also 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 earnings received from UC. The p-value for exclusion of the 15 covariates is 0.74.

[19]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.

1, the information treatment more than doubles that rate (by +28 percent) to a mean rate of almost 50 percent.

In column 2 we include a dummy indicating whether the individual was offered a (randomly assigned) monetary response incentive. The coefficient estimate for the treatment dummy is the same as in column 1, and the coefficient on the incentive dummy is very close to 0. Column 3 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 variables 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.

As discussed above, because of incomplete compliance the interpretation of the observed treatment response as an attenuated version of equation 2 requires that the information treatment intensity is independent of an individual's wage or relative wage. This assumption might be violated if highly-paid individuals within a unit have better information about their relative salary than low-paid individuals. This could be true among staff, for example, if the department manager, who is higher paid, sets or reviews staff salaries.

This potential complication motivates the analysis in columns 4 and 5 of Table 3. The specification in column 4 allows separate treatment effects for people paid above or below the median for their pay unit (defined as the intersection of department and faculty-staff status). The estimated treatment effects are very similar in magnitude and we cannot reject identical effects (p=0.64, reported in bottom row of the table). The specification in column 5 allows a main effect for treatment, and an interaction of treatment status with earnings relative to the median earnings 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 cannot reject heterogeneous treatment effects across relative salary levels (p=0.76). We have fit many other interacted specifications and in all cases find that the information treatment had a large and relatively homogeneous effect on the use of the Sacramento Bee website.[20] Overall, we believe that the evidence is quite consistent with the hypothesis that the information treatment had a homogeneous effect on the use of the web site, suggesting that

[20] The estimated effect of treatment is a little larger at UCSC (33 percent, standard error = 5 percent) than at the other two campuses (UCSD: 28 percent, standard error =2 percent; UCLA: 28 percent, standard error = 2 percent) but we cannot reject a constant treatment effect (p=0.21). The estimated treatment effect is also somewhat larger for faculty (32 percent, standard error 3 percent) than for staff (28 percent, standard error 2 percent), but again we cannot reject a constant effect at conventional significance levels (p=0.23).

the new information was similar for higher- and lower-paid people.

In our UCLA survey we also collected information on what types of information users of the Sacramento Bee website had actually checked. As shown in Appendix Table A2, among "new users" who were prompted to look at the site by our information treatment, 87 percent examined the pay of colleagues in their own department, while 54 percent examined the pay of colleagues in a different department in their campus. Only about a quarter examined the pay of colleagues at different campuses, or high profile UC employees. The effects are very similar for employees paid above- or below-median in their unit. These findings 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. We take this as direct evidence that the department is a relevant unit for defining relative pay comparisons.

B Effect on Job and Salary Satisfaction and Mobility

We turn now to models of the effect of the information treatment on employee satisfaction. Our surveys asked respondents four questions related to their pay and job satisfaction, and their job search intentions. The first is a simple measure of wage satisfaction: "*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 a measure of overall job satisfaction:"*All in all, how satisfied are you with your job?*". Respondents could choose among the same four categories as for wage satisfaction. The third is a measure of perceived fairness of wage setting: "*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". Finally, the last question elicited job search intentions: "*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".

In Appendix Table A3 we report 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.

For much of the subsequent analysis we consider three main 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.[21] This 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.[22] The second outcome variable is a binary variable that is 1 if the respondent reports being "very likely" to look for a new job.[23] The third outcome is a binary variable for whether the respondent is dissatisfied *and* is looking for a new job.[24]

Tables 4 and 5 present a series of OLS models for these three outcomes.[25] We begin with the basic models in columns 1, 4, and 7 of Table 4 which include only a treatment dummy, a cubic polynomial in the individual's earnings, and indicators for faculty/staff status fully interacted with campus. The estimated treatment effects from this simple specification are either insignificant or only borderline significant. The point estimate for the effect on the satisfaction index is negative ($t = 0.9$), the point estimate for search intentions is positive ($t = 0.8$), and the point estimate for the combined variable (dissatisfied and likely looking for a new job) is positive and marginally significant ($t = 1.8$). These estimates suggest that our information treatment may have had a small negative average effect on employee satisfaction. The coefficients on the earnings controls (not reported in the table) indicate that higher earnings are associated with higher job and wage satisfaction, and a lower probability of looking for a new job.

We then estimate differential treatment effects for individuals with below-median and above-median earnings. In particular, we fit models of the form:

$$S = g(w, x) + a \cdot 1(w \leq m) + b_0 \cdot T \cdot 1(w \leq m) + b_1 \cdot T \cdot 1(w > m) + \mu, \tag{3}$$

[21] We have experimented with different ways of constructing this index, for example taking the first principal component of these variables, and the estimates are not sensitive to these alternatives.

[22] Results of baseline ordered probit models for each of the sub-components are in Appendix Table A4.

[23] We obtain qualitatively similar results if we use a binary variable for whether the respondent is "likely" or "very likely" to look for a new job.

[24] 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.

[25] Ordered probit estimates of similar models are in Tables 5 and 6 in Card, Mas, Moretti and Saez (2010) and are qualitatively very similar.

where the dependent variable S is a measure of satisfaction or job search and the regressors include individual earnings w and other covariates x, a dummy for whether the individual's earnings are 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 earnings are below or above the median for his or her pay unit and occupation.

The entries in columns 2, 5, and 8 of Table 4 indicate that the small average effect of treatment masks a larger negative impact on satisfaction for below-median earnings, coupled with a zero or very weak positive effect for those with above-median earnings. For workers whose salaries are below the median in their unit and occupation, the point estimate for the satisfaction index is -6.3 ($t = 2.2$) which corresponds to a tenth of a standard deviation shift in the index relative to the control group. Among this group the information treatment also increases the probability that respondents report being "very likely" to search for a new job by 4.3 percentage points ($t = 2.4$), which represents a 20 percent increase in this measure over the base rate for the controls. Finally, the probability that respondents report being dissatisfied with their job and very likely to search increases by 5.2 percentage points ($t = 2.9$) which corresponds to a 40 percent increase over the rate for the controls.

Since the "first stage" effect of our information treatment on use of the Sacramento Bee website is on the order of +0.28 (see Table 3), a standard 2SLS procedure would blow up the "intention to treat" effects in Table 4 by a factor of $3.6\ (= \frac{1}{0.28})$ to obtain estimates of the "treatment on the treated" effect. As is well known, if there is heterogeneity in the response to relative pay information, the treatment on the treated effect may differ from the average treatment effect on the entire population of interest. In our context it seems plausible that people who cared more about relative pay would be more likely to comply with the treatment (i.e., use the web site), implying that the treatment on the treated effect is an upper bound on the average treatment effect for all employees. On the other hand, a lower bound on the average treatment effect is provided by the intention to treat effects, which effectively assign a zero treatment effect for the non-compliers. Even the lower bound effects implied by the estimates in Table 4 are relatively large.

While we obtain significant negative effects for workers earning less than the unit$\times$occupation median, the treatment effect for workers earning more than the median is insignificant in all cases. The entries in the fourth row of Table 4 show the difference in the estimated treatment effects for above- and below-median workers. These are statistically significant for all three outcomes at the five percent level.[26] Overall, 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 in the relative wage concern function.

The choice of the median to distinguish high and low relative wages is of course arbitrary. The models in columns 3, 6, and 9, break out the treatment effect for workers in the lower half of the pay distribution into separate effects for workers in the two lowest quartiles. The results suggest that the largest information effects occur for workers in the first quartile, while the effects for people in the second quartile and the upper half are uniformly small in magnitude and insignificant. We infer that our main results are largely driven by impacts on relatively low-paid employees in each unit.

We have also estimated models allowing the treatment effects to vary by gender, faculty/staff status, and length of tenure, shown in Appendix Table A5. We find that the treatment effect on search intentions is concentrated among low-paid and low-tenure respondents.[27] Staff appear to be more responsive than faculty to the treatment on both satisfaction and job search, but the relatively small number of faculty limits our ability to make precise comparisons. 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. This finding may be related to the general differences in bargaining attitudes between men and women noted by Babcock and Laschever (2003). Specifically, women may be more likely to leave their job than to ask for a raise in response to learning that they are underpaid, though without additional data our findings are only suggestive.[28] As a caveat to Table A5, it should be noted that treatment intensity varies somewhat across subgroups. However, inflating the estimates

[26] To probe of the robustness of our inferences to potential selection biases we fitted selection-correction models where we take advantage of random assignment of the prize incentive that we introduced to raise response rates, as well as the random assignment of the placebo which reduces response rates. See Card, Mas, Moretti and Saez (2010) for these estimates and associated discussion. We come back to the issue of selection in Section C below.

[27] This latter 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 A6 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, gender, faculty/staff status, and campus (estimated from the control group).

[28] In a separate analysis (not reported in this paper) we rule out that the probability of leaving one's job conditional on the job search response differs between women and men. Specifically, the relationship between the job search response and being listed in the campus directory in March 2011 is similar for women and men. The baseline probability of still being listed in the campus directory by March 2011 conditional on appearing in our sample is also very close between women and men.

by the "first-stage" effects of the information treatment results in a very similar pattern of estimates across sub-groups to those presented in Table A5.

We have also explored models in which we use employees at the entire campus (instead of the department) as the peer unit, keeping the distinction between staff and faculty. The results are presented in Appendix Table A7. Using campus-wide median pay as the reference point we find a relatively large negative effect of our information treatment on the satisfaction of faculty with below-median pay, and a significantly positive effect for faculty with above median pay. On the other hand, the treatment effects on job-search intentions of faculty are still asymmetric, with positive effects for lower-paid faculty and negligible effects for higher-paid faculty. For staff, the use of a campus-wide reference point leads to noticeably smaller negative treatment effects for lower-paid workers than when we define the reference point at the department level. This suggests that departmental colleagues may be a better comparison group for staff, whereas for faculty a broader comparison group may be relevant.

To test more directly the inequality aversion hypothesis, the models in Table 5 adopt a treatment effect specification that depends on a piece-wise linear function of the gap between an individual's earnings and the reference earnings (again defined as median earnings by department$\times$(faculty/staff):

$$S = g(w, x) + c_1 \cdot T \cdot (w - m) \cdot 1(w \leq m) + c_2 \cdot T \cdot (w - m) \cdot 1(w > m) + \mu. \tag{4}$$

Note that we interact the treatment dummy T with the wage gap $(w - m)$, allowing potentially different effects when the individual's earnings are below (c_1) or above (c_2) the reference point wage. Consistent with the findings in Table 4, these models suggest a pattern of treatment effect for all outcomes that is concentrated among the lowest-wage individuals. The estimates in columns 1, 4 and 7 confirm the non-linearity in the relationship between the treatment effect and the wage gap, with a relatively large negative estimate for the coefficient c_1 and small and insignificant estimates for the coefficient c_2. Thus, the distance between one's own wage and the reference wage matters when $w \leq m$, but once the wage exceeds the reference wage the effect of treatment is constant. Across all models reported in Table 5 we cannot reject that the treatment response function is zero when the wage exceeds the pay unit median.

In the remaining columns of Table 5 we explore whether the effect of the information treatment varies with wage rank, rather than with relative wage level. The motivation for this specification is the possibility that ordinal rank matters more for relative utility considerations than absolute salary differences, as has been suggested in the psychology literature (e.g., Parducci, 1995). In columns 2, 5, and 8 we replace the gap variable based on pay levels with the gap in percentile ranks (normalized so that median rank is 0). For the first and third outcomes, the interaction based on rank shows a more pronounced effect than the interaction based on relative salary levels while for the intended search the two alternatives are very similar. When we estimate models that include both rank and levels (columns 3, 6, and 9) rank wins the "horse race" for all three outcomes. Specifically, in the combined model the interaction of treatment with rank is significant for the below-median workers while the interaction with the relative wage gap is no longer significant.[29]

Overall, we believe that the weight of the evidence in Tables 4 and 5 supports a relative income model of the responses to the information treatment. We note, however, two caveats that preclude a definitive conclusion. First, we do not directly measure the change in the discounted expected utility (EU) that individuals experience when they are exposed to the information in the Sacramento Bee website. It is possible that learning about co-workers' salaries raises EU for low-paid workers—as predicted by a rational updating model—and at the same time lowers reported job and pay satisfaction, and increases willingness to look for a new job. This possibility makes it difficult to definitely reject the hypothesis of rational updating. Second, we cannot completely rule out that more highly paid employees in a unit have better information on co-worker wages. While we have shown above that the effects of the information treatment on the observed web site use of above-median and below-median workers are virtually identical, it is still possible that the new information was less important for the high-wage group.

C 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 our interpretation of the measured treatment effects is flawed. For example, it is conceivable that receiving the first stage e-mail 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". This concern is potentially serious because we used the words "pay inequality" in our cover e-mail to participants.

[29] Models where we have added a treatment main-effect (not reported in the table) also show that the rank variable appears to be more significant in the treatment response than relative wage levels.

Another issue of concern is the lower response rate in the treatment group, which may introduce differential selection biases in the measured responses of the treatment and control groups.

One way to address these concerns is to fit similar models to those in Table 4, using the placebo treatment instead of our real information treatment. The wording of the placebo treatment e-mail closely followed the wording of our main information treatment:

"*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?*".

Note that the experimentally-measured effect of the placebo treatment is subject to the same set of potential biases as the effect of the real treatment. Specifically, because the placebo treatment contained the same wording in the cover e-mail, it presumably had a similar priming effect as the real treatment. Moreover, because the placebo treatment reduced the response rate to our survey by the same magnitude as the real treatment, we should observe a similar degree of selection bias in the measured responses of the treatment and control groups in the placebo experiment.[30]

The placebo treatment was only administered at UCLA (see Appendix Table A0). 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 fit various models similar to the ones in Table 3 and found no indication that the placebo treatment had any effect on use of the Sacramento Bee site.

In Table 6 we compare the effects of the placebo treatment to the effects of our main information treatment for each of our three outcome measures. Columns 1, 4 and 7 show baseline models for the effect of our main information treatment on people above or below the

[30]One concern is that the placebo is providing new and relevant information in units that house top administrators. In Appendix Table A8 we estimate the placebo effect excluding departments or administrative units which house Deans, Associate Deans, or Provosts. The resulting estimates appear close to those that include these units and excluding them do not alter the conclusions from the analysis below.

median earnings in the their pay unit, fit only to the UCLA sample and excluding observations assigned to the placebo treatment. The pattern of estimates is very similar to the pattern in Table 4 (estimated on all three campuses) though somewhat less precise because of the smaller sample. As in the overall sample, low-earning employees who were informed 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. Columns 2, 5, and 8 show parallel models defining "treatment" as our placebo e-mail treatment. In these specifications the impact on low-wage employees is uniformly small and insignificant. In the third column, we show p-values corresponding to the test that the parameters from the information treatment model are equal to the placebo model. 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 at or below the 6 percent level. These results show that the systematic pattern of estimates in Tables 4 is not an artefact of priming effects or selection biases arising from our earlier e-mail contact of the treatment group. Hence they provide additional support for our interpretation of these estimates as relative pay effects.

D Effects on Actual Turnover in the Medium-Run

One limitation of our study is that our survey information is limited to self-reported outcomes, raising the question as to whether the effects of the information treatment translated into changes in observable economic behavior. To address this limitation, we gathered the online directories for the three campuses as of August 2011, some 27-35 months after our initial treatment and survey e-mails. We then define a turnover indicator, based on whether a given individual's e-mail name is still present at the campus.[31] Table 7 presents a series of models using this indicator of turnover as a dependent variable. As a starting point, the model in column 1 relates the turnover event to our survey-based measure of job search intentions. Reassuringly, the estimates shows that stated search intentions are a very strong predictor of actual turnover. Among the subset of respondents to our survey, those who reported being very likely to search for new job have 19.5 percentage points higher turnover, while those who said they were somewhat likely to search have 5 percentage points higher turnover.

Columns 2-5 examine the effects of the information treatment on turnover for the full sample

[31] Overall, 27 percent of the names that we were able to match with base salary data were no longer present in August 2011, implying an annual turnover rate of about 10 percent.

of people we were able to match to 2007 salary data, regardless of whether they responded to our survey or not. Given the findings in Tables 4 and 5 we present two specifications: one in which we divide people into the upper half and the bottom two quartiles of the pay distribution in their unit (columns 2-3), and an alternative in which we use the deviation of salary rank from the median in the pay unit (columns 4-5). In an effort to improve the precision of the estimates, the models in columns 3 and 5 introduce a set of departmental fixed effects in addition to controls for the individual's earnings and occupation group × campus. Turnover rates vary widely by department so the addition of these variables leads to a notable reduction in the standard errors for the estimated treatment effects.

The estimates in columns 2 and 3 show large but imprecise positive effects of the information treatment on turnover rates of people in the bottom quartile of salaries: The estimated treatment effect for the lowest quartile in column 3 implies a 2.3 percentage point increase in the probability of quitting (relative to the average rate of 31 percent) with $t = 1.74$. A similar pattern of effects is revealed from the estimates in columns 4-5 which show a negative but only marginally significant effect of higher salary rank on the probability of turnover among workers in the lower half of the earnings distribution, and relatively smaller effects on people in the upper half. Overall, we infer that the information treatment may have led to an increase in turnover of lower-ranked workers, consistent with the increases in their stated search intentions and increased job dissatisfaction, but the estimates are too imprecise to reach a definite conclusion.

It is worth noting two issues that may confound the interpretation of the turnover treatment effects in Table 7. First, information about the Sacramento Bee website (and other sites with salary information about UC employees) has been diffusing over time, presumably narrowing the information gap between our treatment and control groups, and diluting our experimental design. Second, because of the severe recession and high unemployment in California in the period from 2007 to 2011 period, workers who were unsatisfied with their relative salary may have been unable to find other jobs. We suspect both factors would lead to smaller measured effects in Table 7 than would arise in other contexts. Given these concerns, and the imprecision of the estimates, we believe these results are at best only suggestive about the longer-run economic effects of salary disclosure.[32]

[32]We also collected the salary data released by the UC administration in August 2011, which report 2010 salaries. We estimated models intended to test the hypothesis that our information treatment affects either salaries or different components of salaries (base pay vs. over time). In particular, we tested whether treated

IV Conclusion

In this paper we manipulate access to information on co-worker pay to test how knowledge of one's position in the pay distribution of immediate co-workers affects satisfaction and job search intentions. We find that the information treatment has a negative effect on workers paid below the median for their unit and occupation – particularly for those in the lowest pay quartile – but has no effect on workers paid above median. The evidence further suggests that the effect of the treatment is more closely related to pay rank than to the actual level of pay relative to the median in the pay unit.

These patterns are consistent with a utility function that imposes a negative cost for having wages below the reference-point, but little or no reward for having wages above the reference-point. Overall, our results support the conclusions of many previous observational studies and lab-based experimental studies on relative income and worker satisfaction. We also find suggestive evidence that the information treatment increased the 2-3 year turnover rate of lower-ranked employees, though our experimental design has been diluted by the diffusion of information about the web-site over time. Finding experimental research designs to estimate the longer-term effects of pay disclosure is an important topic for future research.

In terms of workplace policies, our findings indicate that employers have a strong incentive to impose pay secrecy rules. In the short run, the disclosure of salary information results in a decline in job and pay satisfaction, concentrated among the lowest-earning workers. In the longer run it is possible that making information on salaries available may lead to endogenous changes in wage-setting policies and employee composition that ultimately affect the distribution of wages, as in the models of Frank (1984), Bewley (1999), and Bartling and von Siemens (2010).

workers who learn to be paid below their peers experience different salary changes. In general, our models failed to uncover significant differences—a finding that is probably to be expected in a serious recession like the current one—with one exception. We found that treated workers with above median earnings tend to be significantly less likely to receive overtime pay. Re-assuringly, this effect appears to be concentrated among non-responders (as responders in the control learned about the website in the survey). We report these estimates in Appendix Table A9.

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Table 1: Determinants of Survey Response

All Coefficients×100	Overall Sample (N=41,975) (1)	Subsample Matched to Wage Data (N=31,887) (2)	(3)	(4)
Dummy if match to wage	3.37	--	--	--
	(0.58)			
Treatment Effects:				
Treated individual (all in treated departments)	-3.81	-3.74	-3.82	--
	(0.54)	(0.62)	(0.61)	
Placebo individual (all in placebo departments)	-5.46	-5.98	-5.89	-5.90
	(0.88)	(1.03)	(1.01)	(1.01)
Response Incentive Effects:				
Offered prize	4.25	4.32	4.23	4.24
	(0.76)	(0.86)	(0.86)	(0.86)
Treatment Effects Based on Relative Wage:				
Treated individual earning less than median in pay unit	--	--	--	-3.60
				(0.79)
Treated individual earning more than median in pay unit	--	--	--	-4.04
				(0.81)
Dummy if earnings less than median in pay unit	--	--	--	-0.69
				(0.73)
Cubic in earnings?	no	no	yes	yes

Notes: All models are estimated by OLS. Standard errors, clustered by campus/department, are in parentheses (1,078 clusters for models in column 1; 1,044 for columns 2-4). Dependent variable in all models is dummy for responding to survey (mean=0.204 for column 1; mean=0.214 for columns 2-4). All models include interacted effects for campus and faculty or staff status (5 dummies). "Earnings" refers to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department. Column 1 includes the full sample while columns 2-4 include only the subsample successfully matched to the administrative salary data for 2007. Columns 3-4 include earnings controls (up to cubic term). Column 4 includes interactions of treatment and relative earnings in the unit.

Table 2: 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 3: Effect of Treatment on Use of Sacramento Bee Website

	(1)	(2)	(3)	(4)	(5)
Treated individual (coefficient × 100)	28.3 (1.6)	28.3 (1.6)	28.5 (1.6)	--	28.3 (2.0)
Treated individual earning less than median in pay unit (coefficient × 100)	--	--	--	29.3 (2.1)	--
Treated individual earning more than median in pay unit (coefficient × 100)	--	--	--	27.7 (2.0)	--
Treated individual × deviation of earnings from median in pay unit (coefficient × 100)	--	--	--	--	-0.4 (0.7)
Treated individual × deviation of earnings from median in pay unit if deviation positive (coefficient × 100)	--	--	--	--	0.3 (0.9)
Dummy for response incentive (test for selection bias in respondent sample)	--	0.0 (1.8)	--	--	--
Dummy for earnings less than median in pay unit (coefficient × 100)	--	--	--	-1.6 (1.8)	--
Deviation of earnings from median (coefficient × 100)	--	--	--	--	-0.1 (0.40)
Deviation of earnings from median if deviation positive (coefficient × 100)	--	--	--	--	0.4 (0.50)
Controls for campus × (staff/faculty) and cubic in earnings?	yes	yes	yes	yes	yes
Demographic controls (gender, age, tenure and time in position)	no	no	yes	yes	yes
P-value for test against model in column 3	--	--	--	0.64	0.76

Notes: All models are estimated by OLS. Standard errors, clustered by campus/department, are in parentheses (818 clusters for all models). Dependent variable in all models is dummy for using Sacramento Bee web site (mean for control group=19.2 percent; mean for treatment group=49.4 percent; overall mean=27.6 percent). "Earnings" refers to total UC payments in 2007. Deviation of earnings from median are expressed in $10,000s. Pay unit refers to faculty or staff members in an individual's department. All models with interaction terms also include main effects. The sample size is 6,411.

Table 4: Effect of Information Treatment on Measures of Job Satisfaction

	Satisfaction Index (10 point scale)			Reports Very likely to Look for New Job (Yes = 1)			Dissatisfied and Likely Looking for a New Job (Yes = 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated individual	-2.0	--	--	1.0	--	--	2.0	--	--
	(2.2)			(1.2)			(1.1)		
I. Treated individual with earnings ≤ median pay in unit	--	-6.3	--	--	4.3	--	--	5.2	--
		(2.9)			(1.8)			(1.8)	
II. Treated individual with earnings > median pay in unit	--	2.0	2.2	--	-2.0	-2.0	--	-0.9	-0.9
		(2.6)	(2.6)		(1.6)	(1.6)		(1.3)	(1.3)
II-I	--	8.3	--	--	-6.3	--	--	-6.1	--
		(3.5)			(2.4)			(2.1)	
Treated × earnings in first quartile in pay unit	--	--	-15.0	--	--	8.0	--	--	8.1
			(4.0)			(2.6)			(2.4)
Treated × earnings in second quartile in pay unit	--	--	1.9	--	--	0.8	--	--	2.5
			(3.9)			(2.5)			(2.3)
P-value for exclusion of treatment effects	0.36	0.05	0.00	0.85	0.03	0.01	0.08	0.01	0.00
Mean of the dependent variable in the control group [standard deviation]		274.2			21.9			12.9	
		[66.1]			[41.4]			[33.5]	

Notes: All models are estimated by OLS. All coefficients and means are multiplied by one hundred. Standard errors, clustered by campus/department, are in parentheses (818 clusters for all models). "Earnings" refers to total UC payments in 2007. Pay unit refers to the respondent's department or administrative unit. Median pay is computed seperately for faculty and staff. 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 to each of these questions are on a 1-4 scale and 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 and reports being "very likely" to make an effort to find a new job. See text and Appendix Table A3 for further details on the construction of the dependent variables. In addition to the explanatory variables presented in the table, all models include controls for campus × (staff/faculty), a cubic in earnings, and main effects. The sample size is 6,411.

Table 5: Effect of Information Treatment on Measures of Job Satisfaction: Earnings Differences vs. Rank

	Satisfaction Index (10 point scale)			Reports Very likely to Look for New Job (Yes = 1)			Dissatisfied and Likely Looking for a New Job (Yes = 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated individual × deviation of earnings from median if deviation negative (coefficient × 100)	1.7	--	-0.8	-1.4	--	-0.1	-1.3	--	0.2
	(0.9)		(1.5)	(0.5)		(0.9)	(0.5)		(0.8)
Treated individual × deviation of earnings from median if deviation positive (coefficient × 100)	-0.5	--	-0.8	-0.5	--	-0.5	-0.2	--	-0.1
	(0.6)		(0.9)	(0.3)		(0.4)	(0.2)		(0.3)
Treated individual × deviation of rank from 0.5 if deviation negative (coefficient × 10)	--	2.4	3.3	--	-1.9	-1.7	--	-1.8	-2.0
		(1.0)	(1.8)		(0.7)	(1.1)		(0.6)	(1.0)
Treated individual × deviation of rank from 0.5 if deviation positive (coefficient × 10)	--	-0.3	0.8	--	-0.8	-0.1	--	-0.4	-0.2
		(0.9)	(1.5)		(0.5)	(0.8)		(0.4)	(0.7)
Controls for campus × (staff/faculty) and cubic in earnings?	yes	yes	yes	yes	yes	yes	yes	yes	yes
P-value for exclusion of treatment effects	0.12	0.06	0.07	0.01	0.01	0.03	0.02	0.01	0.03

Notes: All models are estimated by OLS. Standard errors, clustered by campus/department, are in parentheses (818 clusters for all models). "Earnings" refers to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department. See note to Table 4 for description of the dependent variables. In addition to the explanatory variables presented in this table, specifications 1, 3, 4, 6, 7, and 9 include the deviation of earnings from the median earnings in the pay unit if the deviation is positive, the deviation of earnings from the median earnings in the pay unit if the deviation is negative, and an indicator for whether the deviation is negative. Deviation of earnings from median are expressed in $10,000s. Specifications 2,3,5,6, 8 and 9 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. The sample size is 6,411.

Table 6: Estimates of the Effect of "Placebo" Treatment

	Satisfaction Index (10 point scale)			Reports Very likely to Look for New Job (Yes = 1)			Dissatisfied and Likely Looking for a New Job (Yes = 1)		
	Treatment (1)	Placebo (2)	p-value[a] (3)	Treatment (4)	Placebo (5)	p-value[a] (6)	Treatment (7)	Placebo (8)	p-value[a] (9)
Treated individual with earnings less than median in pay unit	-8.6	1.7	0.04	4.7	-3.3	0.06	7.8	-4.0	0.00
	(4.6)	(4.5)		(2.8)	(3.7)		(2.6)	(3.2)	
Treated individual with earnings more than median in pay unit	-1.5	-1.4	0.98	-3.3	-1.9	0.63	-1.3	1.4	0.22
	(3.8)	(3.7)		(2.5)	(2.9)		(1.8)	(2.1)	
Controls for staff/faculty status and cubic in wage?	yes	yes		yes	yes		yes	yes	
Observations	2303	1880		2303	1880		2303	1880	

Notes: All models are estimated by OLS. All coefficients are multiplied by 100. 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. "Earnings" refers to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department. Models are based on specifications 2, 5, and 8 of Table 4. For additional details see notes to Table 4 and text.

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

Table 7: Effect of Information Treatment on Job Mobility

	Survey Respondents Only	All Employees Who Could be Matched to Earnings Data			
	(1)	(2)	(3)	(4)	(5)
Reported "very likely" to make a genuine effort to find a new job (coefficient × 100)	19.5	--	--	--	--
	(1.62)				
Reported "somewhat likely" to make a genuine effort to find a new job (coefficient × 100)	4.96	--	--	--	--
	(1.20)				
Treated individual with earnings > median pay in unit (coefficient × 100)	--	1.42	0.84	--	--
		(1.29)	(0.93)		
Treated × earnings in first quartile in pay unit (coefficient × 100)	--	2.61	2.30	--	--
		(1.78)	(1.32)		
Treated × earnings in second quartile in pay unit (coefficient × 100)	--	-0.39	-0.71	--	--
		(1.64)	(1.19)		
Treated individual × deviation of rank from 0.5 if deviation negative (coefficient × 10)	--	--	--	-0.74	-0.63
				(0.51)	(0.36)
Treated individual × deviation of rank from 0.5 if deviation positive (coefficient × 10)	--	--	--	0.43	0.27
				(0.39)	(0.31)
Controls for campus × (staff/faculty) and cubic in earnings?	Yes	Yes	Yes	Yes	Yes
Department fixed-effects	No	No	Yes	No	Yes
Observations	6,599	31,882	31,882	31,882	31,882

Notes: All models are estimated by OLS. Dependent variable is 1 if we were not able to locate an individual in online campus directories in August 2011, and 0 otherwise. (Overall mean of dependent variable is 0.31). Sample in columns 2-5 includes all individuals in employee subsample matched to earnings data. We found found 49 percent of original sample in UCSC, 76 percent in UCSD, and 74.5 percent in UCLA. Sample in column 1 only is restricted to individuals who responded to our survey with valid response for search intentions question. Excluded category in column 1 is "not likely at all". In addition to the explanatory variables presented in the table, models in columns 2-5 include an indicator for whether the respondent is paid at least the median in his/her pay unit. Column 4 and 5 include the deviation of the rank in the pay unit from 0.5 if the deviation is positive and negative.

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

This appendix includes the exact survey questions and a set of supplementary tables A0-A9 that are referred to in the main text. Complete explanations for the supplementary tables are provided in the notes of each table.

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 as many questions on the detailed use 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 were not be separately flagged to the subjects to avoid influencing 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 no, skip 2-4.

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

   If yes, skip 4; If no, skip 3.

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.

Appendix Table A0: 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 explicitly 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 randomly selected from among control departments which did not receive the information treatment.

Appendix Table A1: Matching and Response Rates

	Number in Online Directory	Percent Matched to Earnings Data	Percent Responded to Survey	Percent Responded Conditional on Earnings Data	Percent With Earnings 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. Earnings 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 earnings data, responded to the survey, and provided non-missing responses for 8 key questions.

Appendix Table A2: 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	24.1	15.0	7.5	9.5	27.6
	(2.4)	(2.2)	(1.7)	(1.4)	(2.0)	(2.4)
Estimated treatment effect from interacted model with basic controls:						
Treated individual with earnings less than	29.5	25.4	14.5	7.6	10.6	29.4
median in pay unit (coefficient × 100)	(3.5)	(3.3)	(2.3)	(2.0)	(2.9)	(3.5)
Treated individual with earnings greater than	26.3	23.0	15.6	7.4	8.7	26.1
median in pay unit (coefficient × 100)	(2.8)	(2.7)	(2.1)	(1.7)	(2.4)	(2.8)
P-value for equality of treatment effects [a]	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 individuals). Estimated treatment effects are from OLS models that control for faculty status and cubic in wage. Interacted model also includes dummy indicating whether individual pay is below median for pay unit. Standard errors, clustered by department, are in parentheses (358 clusters for all models). Earnings refer to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department.

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

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 Reweightec[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 Reweightec[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 Reweightec[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 Reweightec[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 Reweightec[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 Reweightec[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 Reweightec[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 earnings ≤ 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 earnings > 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 earnings?	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 (818 clusters for all models). "Earnings" refers to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department. See Appendix Table A3 and text for description and means of the dependent variables. For columns 1-3 responses are ordered so that higher values indicate greater satisfaction. Models are based on specification 2 of Table 4. 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: Effect of Information Treatment -- by Subgroup

Panel A: **Satisfaction Index**	Females (1)	Males (2)	Staff (3)	Faculty (4)	Low Tenure (5)	High Tenure (6)
I. Treated individual with earnings ≤ than median in pay unit (coefficient × 100)	-5.9	-6.7	-7.0	-3.1	-3.0	-9.5
	(3.5)	(4.6)	(3.5)	(6.3)	(3.8)	(4.2)
II. Treated individual with earnings > than median in pay unit (coefficient × 100)	3.8	-0.3	1.6	4.5	-2.7	3.3
	(3.5)	(4.0)	(2.9)	(5.8)	(4.6)	(3.0)
II-I	9.7	6.3	8.6	7.6	0.3	12.8
	(4.7)	(5.7)	(4.1)	(8.6)	(5.6)	(4.8)
P-value for exln. of treatment effects	0.11	0.35	0.09	0.66	0.64	0.03
Observations	3908	2503	5396	1015	2558	3853

Panel B: **Very Likely to Look for New Job (Yes = 1)**	Females (1)	Males (2)	Staff (3)	Faculty (4)	Low Tenure (5)	High Tenure (6)
I. Treated individual with earnings ≤ than median in pay unit (coefficient × 100)	5.5	2.2	5.2	0.1	7.3	1.2
	(2.2)	(3.3)	(2.0)	(3.6)	(2.6)	(2.5)
II. Treated individual with earnings > than median in pay unit (coefficient × 100)	-3.8	0.4	-2.8	2.1	-1.4	-2.1
	(2.0)	(2.4)	(1.8)	(3.4)	(3.3)	(1.7)
II-I	-9.2	-1.8	-8.0	2.1	-8.7	-3.3
	(2.8)	(4.5)	(2.7)	(5.0)	(4.0)	(3.0)
P-value for exclusion of treatment effects	0.01	0.77	0.01	0.82	0.02	0.42

Panel C: **Dissatisfied and Likely Looking for a New Job (Yes = 1)**	Females (1)	Males (2)	Staff (3)	Faculty (4)	Low Tenure (5)	High Tenure (6)
I. Treated individual with earnings ≤ than median in pay unit (coefficient × 100)	5.4	4.8	5.8	2.5	5.8	4.8
	(2.1)	(2.9)	(2.0)	(3.0)	(2.4)	(2.4)
II. Treated individual with earnings > than median in pay unit (coefficient × 100)	-1.4	-0.2	-1.2	0.7	0.5	-1.4
	(1.7)	(1.8)	(1.5)	(2.3)	(2.5)	(1.4)
II-I	-6.8	-5.1	-7.1	-1.8	-5.3	-6.1
	(2.5)	(3.6)	(2.4)	(3.7)	(3.3)	(2.8)
P-value for exclusion of treatment effects	0.02	0.26	0.01	0.68	0.05	0.09

Notes: All models estimated by OLS. Standard errors, clustered by campus/department, are in parentheses. "Earnings" refers to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department. Models are based on specifications 2, 5, and 8 of Table 4. For additional details see notes to Table 4 and text.

Appendix Table A6: Effect of Predicted Mobility on Search and Satisfaction Treatment Effects

	Likely to Look for New Job (Yes = 1)		Satisfaction Index (10 point scale)	
	(1)	(2)	(3)	(4)
Panel A: Workers with earnings ≤ median				
Treated individual (coefficient × 100)	4.0	-8.6	-5.9	-6.8
	(1.8)	(4.8)	(2.9)	(9.1)
Treated individual × Predicted probability of search (coefficient × 100)	--	0.5	--	0.0
		(0.2)		(0.4)
Predicted probability of search	--	0.9	--	0.0
		(0.1)		(0.2)
Controls for campus × (staff/faculty) and cubic in earnings?	Yes	Yes	Yes	Yes
Panel B: Workers with earnings > median				
Treated individual (coefficient × 100)	-1.8	-2.0	2.4	12.7
	(1.6)	(3.1)	(2.5)	(6.4)
Treated individual × Predicted probability of search (coefficient × 100)	--	0.5	--	-0.5
		(0.2)		(0.3)
Predicted probability of search	--	0.8	--	0.1
		(0.1)		(0.2)
Controls for campus × (staff/faculty) and cubic in earnings?	Yes	Yes	Yes	Yes

Notes: All models estimated by OLS. Standard errors, clustered by campus/department, are in parentheses. "Earnings" refers to total UC payments in 2007. 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 dummy controls. See note to Table 4 for definitions of the dependent variables.

Appendix Table A7: Effect of Information Treatment on Job Satisfaction by Pay Relative to Campus/Occupation Median

	Satisfaction Index (10 point scale)		Reports Very likely to Look for New Job (Yes = 1)		Dissatisfied and Likely Looking for a New Job (Yes=1)	
	Faculty	Staff	Faculty	Staff	Faculty	Staff
	(1)	(2)	(3)	(4)	(5)	(6)
I. Treated individual with earnings ≤ than	-16.9	-5.6	3.4	4.6	4.6	5.1
campus median	(6.6)	(3.5)	(3.7)	(2.1)	(3.3)	(2.0)
II. Treated individual with earnings > than	16.7	0.0	-0.8	-2.0	-1.1	-0.3
campus median	(5.3)	(2.8)	(3.2)	(1.8)	(2.1)	(1.6)
II-I	33.5	5.6	-4.1	-6.6	-5.8	-5.4
	(8.6)	(3.9)	(4.9)	(2.7)	(3.9)	(2.4)
Controls for campus and cubic in earnings?	Yes	Yes	Yes	Yes	Yes	Yes
P-value for exclusion of treatment effects	0.00	0.25	0.64	0.05	0.32	0.03

Notes: This table reports the same specification as columns 2,5, and 8 of Table 4 but instead of computing the median earnings of the reference group at the the department/administrative unit-level, we compute the median at the campus-level, seperately for faculty and staff. All models are estimated by OLS. All coefficients and means are multiplied by one hundred. Standard errors, clustered by campus/department, are in parentheses. The sample size is 6,411.

Appendix Table A8: Estimates of the Effect of "Placebo" Treatment with and Without Top Administrators

	Satisfaction Index (10 point scale)		Very likely to Look for New Job (Yes = 1)		Dissatisfied and Likely Looking for New Job (Yes = 1)	
	Placebo all (1)	Placebo - Exclude Dept's with Any Top Admininstrators (2)	Placebo all (3)	Placebo - Exclude Dept's with Any Top Admininstrators (4)	Placebo all (5)	Placebo - Exclude Dept's with Any Top Admininstrators (6)
Treated individual earning less than median in pay unit	1.7	3.1	-3.3	-4.6	-4.0	-5.0
	(4.5)	(4.6)	(3.7)	(3.7)	(3.2)	(3.2)
Treated individual earning more than median in pay unit	-1.4	-2.5	-1.9	-0.4	1.4	2.1
	(3.7)	(4.0)	(2.9)	(3.1)	(2.1)	(2.2)
Controls for staff/faculty status and cubic in wage?	yes	yes	yes	yes	yes	yes
Observations	1880	1669	1880	1669	1880	1669

Notes: All models are estimated by OLS. All coefficients are multiplied by 100. Standard errors, clustered by campus/department, are in parentheses. "Placebo" refers to the placebo information treatment. Sample includes UCLA employees who received either the placebo information treatment or no treatment. In columns 2, 4, and 6, individuals in any department or administrative unit that is home to a Dean, Associate Dean, or Provost is excluded. Standard errors, clustered by campus/department, are in parentheses. "Earnings" refers to total UC payments in 2007. Pay unit refers to faculty or staff members in an individual's department. Models are based on specifications 2, 5, and 8 of Table 4. For additional details see notes to Table 4 and text.

Appendix Table A9: Effect of Information Treatment on Presence of Overtime Earnings in 2010

All coefficients are in percent	(2)	(3)	(3)	(4)	(5)	(6)
Treated individual with earnings > median pay in unit (coefficient × 100)	-1.64 (.73)	-2.14 (.74)	-0.13 (1.29)	--		--
Treated × earnings in first quartile in pay unit (coefficient × 100)	.54 (1.34)	.73 (1.49)	-.15 (2.31)	--		--
Treated × earnings in second quartile in pay unit (coefficient × 100)	.28 (1.23)	-.13 (1.27)	1.64 (2.12)	--		--
Treated individual × deviation of rank from 0.5 if deviation negative (coefficient × 10)	--	--	--	-1.78 (3.93)	-1.93 (4.20)	-1.09 (6.53)
Treated individual × deviation of rank from 0.5 if deviation positive (coefficient × 10)	--	--	--	-3.74 (1.96)	-4.51 (2.05)	-2.13 (3.43)
Controls for campus × (staff/faculty) and cubic in earnings, and presence of overtime in 2007?	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	Non-responders	Responders	All	Non-responders	Responders
Observations	25,135	19,319	5,816	25,135	19,319	5,816

Notes: All models are estimated by OLS. Dependant variable is 100 if employee had positive overtime earnings in 2010 (and zero if not). The sample is all employees still present in 2010 earnings data. The table is built following the same specifications as Table 7, columns 2 and 4. We do not include department fixed effects. The mean of the dependent variable for the full sample is 24.8%. In addition to the standard controls (campus x (staff/faculty)) and cubic in 2007 earnings, we added an indicator for having overtime earnings in 2007. Responders is the set of employees who responded to our survey (and hence learned about the Sacramento Bee website even if they were in the control group). Non-responders is the set of employees who did not respond to our survey (and for whom the treatment vs. control first stage effects is still potentially relevant). All coefficients are in percent.