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Author(s): Anat Bracha, Uri Gneezy, and George Loewenstein
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Relative Pay and Labor Supply

Anat Bracha, *Federal Reserve Bank of Boston*

Uri Gneezy, *University of California, San Diego, and University of Amsterdam*

George Loewenstein, *Carnegie Mellon University*

We examine the impact of relative wages on labor supply in a laboratory experiment. We test the hypothesis that, ceteris paribus, making a given wage high (low) relative to other wage levels will lead to an increase (decrease) in labor supply. We find that labor supply does respond significantly to relative pay, and in the expected direction. However, when a strong enough reason for the relative low pay is given, this difference disappears.

I. Introduction

Dating back, at least, to Adam Smith (1759), a fundamental psychological insight is that people respond to relative as well as absolute levels of economic variables. Smith posited that people are motivated by the desire to produce material improvement (in comparison to past levels) and by vanity—the desire to compare favorably to others. Later economists, most prominently Leibenstein (1950), Duesenberry (1952), and Frank (1985), have pursued the logical implications of such relative concerns by examining their consequences for patterns of consumption, job choice,

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and labor force participation. For example, Duesenberry's "relative income hypothesis" posits that people compare their current consumption to that of others, which can help explain the classic pattern of increasing marginal propensity to consume as a function of wealth cross-sectionally, but not over time, and Frank has shown that if individuals derive positive utility from favorable comparisons to other people's incomes and negative utility from unfavorable comparisons, they will be willing to sacrifice absolute income to be a "big fish in a small pond"—a pattern for which he provides diverse evidence.

By definition, relative consumption or income is measured relative to a reference point or points (Kahneman and Tversky 1979). Reference points were found, for example, to influence job satisfaction (see, e.g., Pfeffer and Langton 1993). In a survey of 18 European countries, Clark and Senik (2010) find that work colleagues are the most frequently cited reference group, and in a survey of employees in the University of California system, Card et al. (2010) find that the likelihood of searching for a new job is higher among those whose incomes compare unfavorably to those of their colleagues. Mas (2006) finds that police officers who lose in arbitration work less hard (resulting in, e.g., lower arrest rates) and that, moreover, the decline in effort is greater when the awarded wage is further from the police union's demand.

In this article, we test whether relative pay affects labor supply, where relative pay is interpreted as one's current pay relative to past wages or as one's pay relative to the pay others receive.[1] Feldstein and Poterba (1984) and Summers (1986) argue that an individual's reservation wage depends on past wages, and Bewley (1999), drawing on extensive interviews with corporate managers during a recession, provides support for the claim that past wages are a salient point of comparison that is important for the labor market.

Nevertheless, finding direct evidence on the effect of relative pay, including past wages, is difficult. In the literature, the studies that stress past wages as a salient point of comparison rely mainly on survey data, and little direct evidence is available documenting the impact of relative wages on labor supply. Converging evidence using methods such as experiments may therefore be important in this case.

In this article, we experimentally test the hypothesis that relative pay affects labor supply and that, ceteris paribus, making a given pay rate high (low) relative to other pay levels will increase (decrease) labor supply. This hypothesis builds on the above-mentioned research, which suggests that workers judge their pay relative to others and relative to their past pay. Put another way, relative pay influences the reference point relevant for

[1] More generally, relative pay can be defined relative to expectations shaped by other factors (Kőszegi and Rabin 2006). In this article, we test relative pay as relative to past pay or to other people's pay.

labor market decisions. If workers have reference-dependent preferences in which a wage offer is judged relative to the reference point, we expect a wage offer that is compared unfavorably to the reference point or reservation wage to be less attractive to the worker; and this in turn would lead to supplying less labor. The converse holds for a wage offer that is compared favorably to the reference point.

To test this hypothesis, we offer participants the option to choose how much to work on a given task for a piece rate pay level that is either high or low. In one condition, participants are aware of only one pay rate, whereas participants in the second condition have a natural reference point—the other piece rate pay level offered. This design allows us to test whether relative pay affects labor supply rather than productivity on the job. The two decisions are fundamentally different: as Charness and Kuhn (2007) discuss, relative pay is expected to affect productivity on the job due to reciprocity in response to a fair (or unfair) wage. In such environments, employers benefit from higher effort by the worker and can lose when the worker does not exert enough effort. Since effort is at the discretion of the worker, there is a good reason for employers to offer efficiency wages and be concerned by how relative pay affects productivity. Yet, in the environment of labor supply that we study here, there is no reciprocal relationship. Workers' decisions to accept, reject, or negotiate different work time affect them only; they do not reward or punish the potential employer.

We find that relative pay comparisons do affect labor supply: when interpersonal comparisons were available—that is, when participants were aware of different pay rates given for the same task—lower-paid individuals supplied significantly less work time relative to higher-paid individuals and significantly less time than when they were unaware of the higher pay rates. When such interpersonal comparisons were not available, labor supply was marginally lower under the lower pay rate, indicating that participants respond to incentives.

Our results show that relative pay information has a further and stronger effect on labor supply. When using intrapersonal comparisons, created by offering participants different pay rates on different occasions for similar tasks, the individuals who were offered higher remuneration than they had previously received were more likely to choose to work than individuals who were offered less pay than they had previously received. Although not our main focus, for the sake of comparisons with other experimental results, we also examine the effect of relative pay on effort and find mixed results, indicating some negative effect of disclosing relative pay information on the effort exerted by those who receive lower pay.[2]

[2] There are also several recent experimental studies that examine the effect of relative pay reciprocity (see Charness and Kuhn [2011] for a survey). Past wages have been shown to influence productivity in some studies (e.g., Cohn, Fehr, and Götte 2010; Kube, Maréchal, and Puppe 2010), but not in others (Greiner, Ock-

As noted, we also examine the moderating impact on relative pay effects of providing a reason for pay differentials. Langer, Blank, and Chanowitz (1978), in a famous psychology study, found that people who cut in line to use a copier were much less likely to meet resistance (only 7% of the time) when they provided an almost content-free reason for making the request ("May I use the copy machine because I have to make copies?") than when they provided no reason (40%). Somewhat consistent with this finding, we find that the relative pay effect disappears when subjects were given a moderately trivial reason for the difference in relative pay, but this result did not hold when the reason given was patently arbitrary.

In the remainder of the article, Section II provides the experimental design and results of study 1 and study 2. Study 1 examines the effect of interpersonal comparisons, while study 2 examines the effect of intrapersonal comparisons on labor supply. Section III concludes.

II. Experimental Studies

A. Study 1: Relative Pay—Interpersonal Comparisons

Subjects at an economics experimental laboratory who had participated in an unrelated prior experiment were given the option to participate in an additional study. We used students who had already participated in another experiment to make their potential decision not to work on our task reasonable.

Participants were told that the experiment involved solving problems. Each problem required them to find three numbers in a 4×4 matrix that exactly sum to 10 (see the example in the appendix, fig. A1). Subjects were given practice solving one problem and then were informed about the pay rate they could receive for solving more problems. They were then asked to decide how long they wished to work on the task—any time between 0 and 30 minutes. Once they were done working on this task, they were told that the experiment would conclude and that they would receive their earnings in cash. Although the study was conducted in groups, each subject privately made his/her key decision about the length of time to work.

enfels, and Werner 2011). Charness and Kuhn (2007), Fischer and Steiger (2009), and Hennig-Schmidt, Sadrieh, and Rockenbach (2010) find no effect of others' pay on effort, while Gächter and Thöni (2010), Cohn et al. (2011), Greiner et al. (2011), and Ku and Salmon (2012) do find an effect. These experimental studies are designed to test the effect of relative pay on productivity once one has already committed to doing work over a certain time period, while we are interested in the effect of relative pay on the period of work one would choose to commit to do. Labor supply decisions, unlike the on-the-job effort decision, bear no obvious connection to reciprocity. In particular, it is not clear in our experiment why the decision of how much effort to invest in a task would relate to reciprocal behavior toward the experimenter.

Subjects were randomly assigned to one of two main treatments. In the "no relative" treatment, all participants in a single session received the same pay rate per correctly solved matrix and were therefore not aware of other pay rates. Half the participants received $0.40 and the other half received $0.80 (randomization occurred at the session level). Because participants in the no relative treatment were aware of only a single pay rate, relative pay considerations of the intrapersonal type were unlikely.

By contrast, in the "relative" treatment, subjects were aware that they could receive one of two different pay rates. In this condition, the instructions stated that two pay rates were possible and thus that some subjects would be randomly assigned to receive the lower pay rate ($0.40) while others would receive the higher pay rate ($0.80). Once pay rates were determined, we publicly announced each participant's rate (see Blount and Bazerman [1996] and John, Loewenstein, and Rick [2014] for similar methodologies).

Beyond testing the impact of relative pay information on the labor supply decision, we also included an additional manipulation to test a boundary condition: whether differential pay has an effect on labor supply when the difference appears to be justified. This boundary condition is especially interesting because in many real-world situations, a reason such as the length of tenure with a firm—which is potentially unrelated to one's productivity—is an acceptable basis for differential pay. To address the question of whether providing a reason for the pay difference would reduce or eliminate the impact of relative pay on the labor supply choice, participants assigned to the relative pay treatment were further randomized to one of two conditions. In the "random notes" condition, each participant drew a note from an envelope containing 10 notes—five marked "40," for $0.40, and five marked "80," for $0.80. After a participant drew a note under the random notes method, he/she announced the number. Then the experimenter announced either "you got 40" or "you got 80" and pressed a button to activate the appropriate pay rate for the program. This procedure was designed to make it salient that the assigned pay rate was completely random—meaning that no plausible rationale for the pay differential was provided.

In the "random essay" condition, in contrast, the assigned pay rate was based on the deliberately arbitrary evaluation of an essay. Specifically, participants wrote a short essay, up to 1,000 characters (about 200 words), describing their previous day's lunch experience. They wrote the essay before receiving any information about this study. After completing the essay, participants were told (truthfully) that we determined the pay assignment according to the number of *r*'s in their essay: those with *r* counts higher than the median received $0.80 per correctly solved matrix, and those with *r* counts less than the median received $0.40 per matrix. In the random essay method, the experimenter announced the individuals'

pay rates one by one ("you got 40" or "you got 80") and activated the appropriate pay rates for the program; participants in this condition were also given a poststudy survey about the assignment procedure.

Three hundred thirty-seven Harvard students at the Harvard Decision Science Lab participated in this study: 126 in the no relative treatment and 211 in the relative treatment. Within the relative treatment, 109 were assigned pay rates using the random notes method and 102 using the random essay method; 177 participated in "wave 1" conducted in 2010–11 and 160 participated in "wave 2" in 2013.

1. Results

Labor supply—the average time participants wished to work on the matrices task—was slightly lower under the lower pay rate in the no relative treatment. As is evident from the two left-hand bars in figure 1, participants who received \$0.40 per correctly solved matrix worked 22.13 minutes on average, whereas those who received \$0.80 worked 24.64 minutes on average. This difference is marginally significant in the one-sided test ($t(124) = 1.60$, $p = .056$).[3]

In sharp contrast, as is evident in the two right-hand bars of figure 1, the pay rate had a highly significant impact on labor supply for participants in the relative treatment (for both the random notes and random essay methods): those given the low pay rate supplied significantly less labor than those assigned the high pay rate. More specifically, participants who received \$0.40 per correctly solved matrix worked 19.81 minutes on average, whereas those who received \$0.80 worked 24.90 minutes on average, a highly significant difference ($t(209) = 4.15$, $p = .00$).

An interesting pattern, evident in figure 1, is that receiving relative pay information lowered participants' labor supply in the low relative pay condition relative to the no relative pay condition ($t(172) = 1.48$, one-sided t-test, $p = .070$) but did not raise labor supply at all in the high relative pay condition ($t(161) = 0.216$, $p = .829$).[4] If this is a general result, it suggests that providing relative pay information is a no-win proposition for employers.

The effect of relative pay was significant for both forms of assignment in the relative treatment. In the random notes condition, participants with the low rate worked 21.06 minutes on average, whereas those who received the high rate worked 25.47 minutes on average ($t(107) = 2.78$, $p < .01$). In the random essay condition, participants with the low pay rate

[3] The p-values of two-sided t-tests are reported throughout the article unless, as in this instance, specifically noted otherwise.

[4] Most participants chose to work on the task for at least a little while: only two participants out of 126 in the no relative treatment and four out of 211 participants in the relative treatment chose not to work at all.

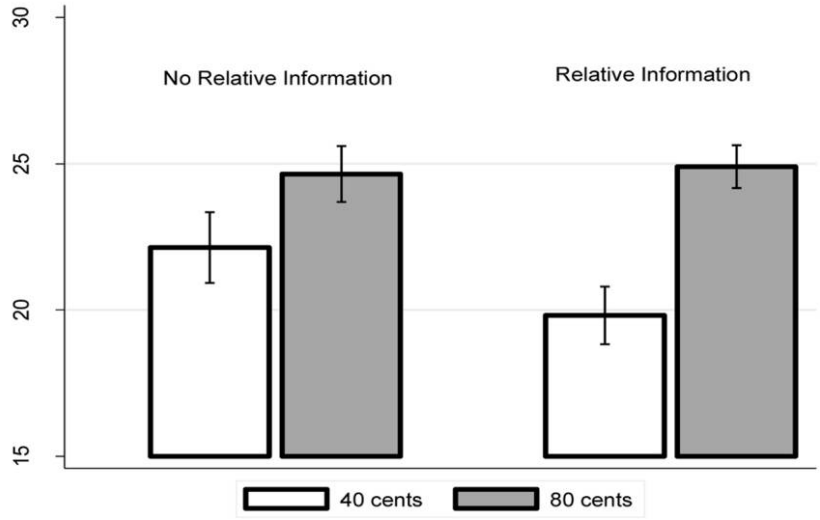

FIG. 1.—Average labor supply (minutes). The bars represent the standard error of the mean.

worked 18.55 minutes on average, whereas those with the high pay rate worked 24.27 minutes on average ($t(100) = 3.04$, $p < .01$). Contrary to the prediction derived from Langer et al.'s (1978) finding, there were no statistically significant differences in behavior between the random notes and the random essay method. The \$0.40 piece rate resulted in a labor supply of 21.06 minutes when using the random notes and 18.55 minutes when using the random essay ($t(105) = 1.28$, $p = .203$), whereas the \$0.80 piece rate resulted in a labor supply of 25.47 and 24.27 minutes, respectively, for the two conditions ($t(102) = 0.83$, $p = .410$).

Table 1 presents results from an ordinary least squares (OLS) and a Tobit regression of the number of minutes supplied based on (i) pay rate, (ii) awareness of alternative pay, and (iii) the interaction of these two factors. We also control for gender and the experimental wave. We used a Tobit regression to take into account the two possible corner solutions: 0 minutes (not working at all) or 30 minutes (the maximum time allowed). The regressions reinforce the results presented in the figures: higher incentives lead to greater labor supply, as expected. When information on other possible pay levels—relative pay information—is provided, it reduces the labor supply of those receiving the low pay rate. Nevertheless, relative pay information does not increase the labor supply of those getting the high pay rate. That is, making a given pay rate low relative to another pay rate significantly reduces labor supply; making a given pay rate high, on the other hand, has no significant effect.

Table 1
The Effect of Relative Pay on Labor Supply

	OLS	Tobit
High pay rate ($0.80 = 1)	2.34	3.72
	(.94)**	(2.23)*
Relative pay information (yes = 1)	−2.49	−4.93
	(1.15)**	(2.17)**
Relative pay information × high pay rate	2.95	6.28
	(1.48)*	(3.18)**
Gender (male = 0, female = 1)	−1.69	−4.64
	(.95)*	(2.11)**
Experimental wave	−.50	−1.17
	(.77)	(1.64)
Constant	23.42	32.46
	(.91)***	(1.99)***
R^2	.070	.013

NOTE.—N = 336. Dependent variable is minutes of work supplied. Standard errors (in parentheses) are clustered at the session level. The Tobit regression is censored at 0 and 30 minutes.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Given that we find a negative gender (female) effect and given the evidence that women and men react differently to incentives (Croson and Gneezy 2009), we report separate regressions for men and women in table 2. These show that, although the effects of incentives and relative pay information seem similar across gender, they are stronger and significant among

Table 2
The Effect of Relative Pay on Labor Supply, by Gender

	Male	Female
High pay rate ($0.80 = 1)	3.18	1.60
	(1.55)**	(1.98)
Relative pay information (yes = 1)	−3.69	−1.46
	(1.86)*	(1.59)
Relative pay information × high pay rate	4.39	1.94
	(1.96)**	(2.48)
Experimental wave	−.26	−.79
	(1.30)	(1.16)
Constant	23.22	21.90
	(1.78)***	(.90)***
Observations	146	190
R^2	.130	.028

NOTE.—Dependent variable is minutes of work supplied. OLS regressions. Standard errors (in parentheses) are clustered at the session level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

men while not among women (as in Gneezy, Niederle, and Rustichini's [2003] study of gender and competition).

Although our main focus is on how relative pay information affects labor supply, we also examined whether relative pay had an effect on effort. We measure effort by calculating for each participant overall success rate and the average time per correctly solved matrix. Participants could skip a matrix by submitting a clearly wrong answer. For productivity, we are therefore interested in average time spent per correctly solved matrix. Examining both measures, we find that while relative pay does not seem to strongly influence average time per success, it does affect success rate. Specifically, the average time per correct matrix in the no relative treatment is similar across the two pay rates: 7.05 minutes under \$0.40 and 6.20 minutes under \$0.80 ($t(113) = 0.86, p = .394$). In the relative treatment, the difference across pay rates is slightly greater (8.15 minutes under \$0.40 and 6.55 under \$0.80), and it is marginally significant in a one-sided t-test ($t(181) = 1.52, p = .065$). Examining the success rate, we find that, like the average time per success, average success rate in the no relative treatment was insignificant (0.50 under \$0.40 and 0.50 under \$0.80; $t(119) = 0.05, p = .960$). However, in contrast to the average time per success, relative information treatment led to a highly significant difference across pay rates (0.41 under \$0.40 and 0.52 under \$0.80; $t(195) = 3.14, p < .01$). Running an OLS regression of these effort measures on whether one received high incentives, received relative pay information, and the interaction of the two variables confirms these results. That is, we find no effect of the relative pay information on average time per success, while we do find a negative effect of the information on the success rate among low-paid individuals. This negative information effect on the success rate is present only for the low-paid individuals, not for the high-paid individuals (relative pay information and its interaction with high pay rate are insignificant). As before, we also analyzed the result by gender; we find that the effect on success rate is entirely a female effect. These results are presented in tables 3–5.

2. *Relative Pay Effect with Stronger Justification*

Having established that relative pay can affect labor supply, but not finding an effect when the pay differential is justified, we hypothesized that the explanation for the pay difference in the random essay treatment may not have provided sufficient justification for the difference to have an impact on participants' behavior. In this study, we test a somewhat stronger version of the rationale manipulation. We add an additional treatment, "essay evaluation," in which we made the piece rate pay assignments by counting the number of times that the letter *r* occurred in a prewritten essay, as in the relative treatment using the random essay method. However, unlike the prior experiment, in which we announced the criterion of

Table 3
Effort Measures

	Success Rate	Average Time per Success
High pay rate ($0.80 = 1)	−.01	−.73
	(.04)	(.62)
Relative pay information (yes = 1)	−.09	1.22
	(.04)**	(1.02)
Relative pay information × high pay rate	.11	−.89
	(.05)**	(1.00)
Gender (male = 0, female = 1)	−.11	2.47
	(.03)***	(.87)***
Experimental wave	−.01	.25
	(.02)	(.86)
Constant	.57	5.51
	(.04)***	(.98)***
Observations	317	297
R^2	.090	.049

NOTE.—Dependent variable is success rate or average time per success. OLS regressions. Standard errors (in parentheses) are clustered at the session level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

r counts for the assignment to high or low pay, in this study we told participants, more ambiguously, that our evaluations of their essays would determine their individual pay rate assignment. By not revealing the exact evaluation criterion, this statement masks the random nature of how pay

Table 4
Success Rate

	Male	Female
High pay rate ($0.80 = 1)	.004	−.016
	(.06)	(.06)
Relative pay information (yes = 1)	−.03	−.13
	(.06)	(.05)**
Relative pay information × high pay rate	.07	.14
	(.07)	(.07)*
Experimental wave	.04	−.04
	(.04)	(.03)
Constant	.52	.49
	(.06)***	(.05)***
Observations	139	178
R^2	.026	.062

NOTE.—Dependent variable is success rate. OLS regressions. Standard errors (in parentheses) are clustered at the session level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 5
Average Time per Success

	Male	Female
High pay rate ($0.80 = 1)	−.60	−.86
	(1.07)	(1.18)
Relative pay information (yes = 1)	−.25	2.39
	(1.02)	(1.78)
Relative pay information × high pay rate	1.31	−2.71
	(1.69)	(1.83)
Experimental wave	−.29	.63
	(.96)	(1.35)
Constant	5.95	7.75
	(.94)***	(1.28)***
Observations	135	162
R^2	.006	.045

NOTE.—Dependent variable is average time per success. OLS regressions. Standard errors (in parentheses) are clustered at the session level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

rates are assigned and provides a potentially stronger justification for the differential pay. We recruited 150 students from the same subject pool as in the above treatments to participate in the essay evaluation treatment.[5]

Compared to the relative treatment, we find that labor supply in the essay evaluation treatment was not different across pay rates (see fig. 2). Those who received $0.40 worked 22.34 minutes on average, whereas those who received $0.80 worked 23.66 minutes on average ($t(148) = 0.93$, $p = .35$). Recall that in the relative treatment using the patently weaker justification inherent in the random essay method, labor supply differed significantly across pay rates:[6] those who received $0.40 per matrix worked 18.55 minutes on average, and those who received $0.80 per matrix worked 24.27 minutes on average, a difference statistically significant at the 1% level.

Using OLS and Tobit regressions, clustered at the session level, we examine the effect of relative pay on labor when a justification for this differential is and is not provided. We generated a dummy variable that takes a value of one for the essay evaluation treatment, where a (stronger) justification—"essay evaluation"—is given for the differential pay. The results are shown in tables 6 (OLS) and 7 (Tobit). Column 1 in each table reports the results when attention is restricted only to the relative treatment

[5] A random subset of participants in this treatment participated in the same poststudy survey as following the relative treatment using the random essay method.
[6] The only difference is the transparency of the evaluation criterion: in the relative treatment using the random essay method, the evaluation criterion was revealed, while in the essay evaluation treatment, it was not.

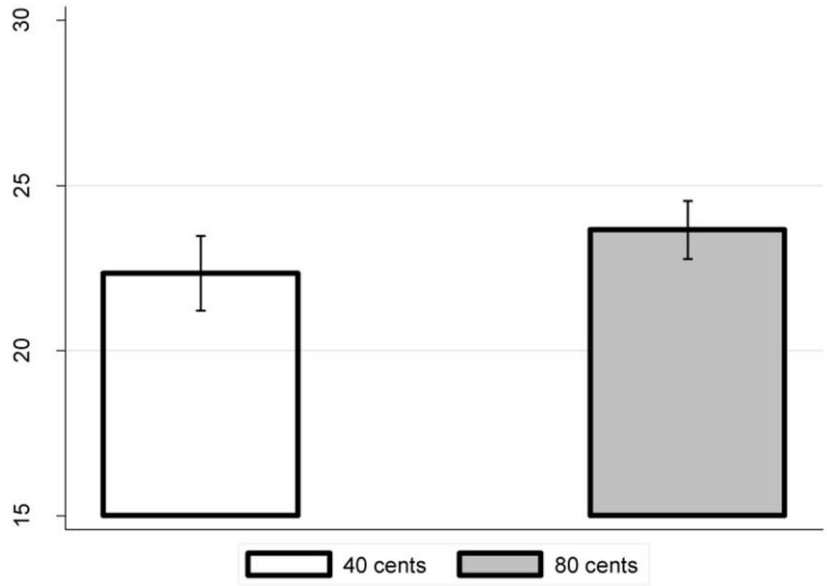

FIG. 2.—Average labor supply (minutes): essay evaluation treatment

Table 6
Role of Justification in the Effect of Relative Pay on Labor Supply

	Random Essay Only (1)	Random Notes and Random Essay (2)
High pay rate ($0.80 = 1)	5.70	5.22
	(1.55)***	(1.14)***
Strong justification (essay evaluation = 1)	4.02	2.50
	(2.26)*	(1.87)
Strong justification × high pay rate	−4.37	−3.96
	(2.26)*	(1.95)*
Gender (male = 0, female = 1)	.09	−.75
	(1.24)	(1.04)
Experimental wave	.56	−.14
	(1.19)	(1.10)
Constant	18.26	20.28
	(1.87)***	(1.39)***
Observations	252	360
R^2	.051	.054

NOTE.—Dependent variable is minutes of work supplied. OLS regressions. Standard errors (in parentheses) are clustered at the session level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 7
Role of Justification in the Effect of Relative Pay on Labor Supply

	Random Essay Only (1)	Random Notes and Random Essay (2)
High pay rate ($0.80 = 1)	10.40	9.65
	(3.16)***	(2.28)***
Strong justification (essay evaluation = 1)	5.87	3.79
	(4.27)	(3.67)
Strong justification × high pay rate	−7.87	−7.30
	(4.66)*	(3.93)*
Gender (male = 0, female = 1)	.43	−1.92
	(2.68)	(2.25)
Experimental wave	.48	−.57
	(2.29)	(2.24)
Constant	22.35	25.73
	(3.57)***	(2.87)***
Observations	252	360
R^2	.008	.009

NOTE.—Dependent variable is minutes of work supplied. Tobit regressions, censored at 0 and 30 minutes. Standard errors (in parentheses) are clustered at the session level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

using the random essay method, whereas column 2 reports the results when comparing the essay evaluation treatment using either one of the two methods in the relative treatment (the random notes and the random essay methods).

The regressions presented in tables 6 and 7 show that having a stronger justification for the pay differential eliminates the positive effect of the high pay rate completely (an *F*-test for the sum of the main effect, high pay rate, and its interaction with justification is insignificant under all specifications). The results are similar when strong justification is compared to the relative treatment using the random essay method only (where an almost identical protocol was used; col. 1), as well as when it is compared to the relative treatment regardless of the assignment method (random essay or random notes; col. 2). These results suggest that when participants can justify a differential wage, the relative pay effect on labor supply seems to disappear: participants accept their assigned pay as if it is the pay rate they deserve. However, in the absence of a reason for the differential pay, such as when the pay assignment is clearly random, the effect of relative pay on labor supply is significant. This result is confirmed using a regression with all treatments.[7]

[7] Clustering by session, the OLS regression using all treatments yields a coefficient of 2.38 ($p = .012$, standard error [SE] = 0.91) on high pay rate, −2.46 ($p =$

B. Study 2: Relative Pay—Intrapersonal Comparisons

In study 1, the natural reference point is the pay rate that the other participants receive. In this subsection, we aim to test whether people can use other informational sources as reference pay and specifically whether an individual's own previous wage influences his/her current labor supply decisions.

1. Study Setup

To test this hypothesis, we approached 200 students who participated in a 30-minute marketing study in a behavioral lab at the University of Chicago. For the initial marketing study, we promised that all participants would earn at least $5; however, on site, half the participants received $15 instead (this randomization occurred on the day of the study, such that individuals participating on the same day received the same pay level). One week after completing the marketing study, participants received an e-mail message inviting them to a follow-up study that would take 15 minutes. We told them the study would take place in the lab at 8:30 on Friday morning.

We told half the participants that the pay rate for this new study would be $5 and the other half that it would be $15. We randomized the monetary offer for each subgroup (those who had previously earned $5 or $15) such that we had four groups of 50 students each: those who had previously earned $5 and were now offered $5 (5, 5) and, similarly, (5, 15), (15, 5), and (15, 15). The variable of interest in this study is the fraction of participants who showed up to complete the survey—that is, the take-up rates.

2. Results

Table 8 reports the fraction of participants who turned up to complete the survey as a function of how much they had been paid in the prior study and how much they were promised for completing the current survey. Participants in the (15, 15) treatment were more likely to show up to

.035, SE = 1.12) on relative pay information, 2.89 (p = .053, SE = 1.45) on the interaction of relative pay information and high pay rate, 2.39 (p = .195, SE = 1.81) on strong justification, and −4.03 (p = .043, SE = 1.93) on the interaction of strong justification and high pay rate. The coefficient on gender (where male = 0 and female = 1) is −1.16 (p = .190, SE = 0.87), the coefficient on experimental wave is −0.47 (p = .544, SE = 0.76), and the constant is 23.09 (p = .000, SE = 0.84). The analogous Tobit regression, taking into account the lower and upper limits of labor supply in this study, yields a coefficient of 3.83 (p = .076, SE = 2.16) on high pay rate, −4.80 (p = .024, SE = 2.12) on relative pay information, 6.02 (p = .056, SE = 3.14) on the interaction of relative pay information and high pay rate, 3.66 (p = .304, SE = 3.56) on strong justification, and −7.55 (p = .054, SE = 3.91) on the interaction of strong justification and high pay rate. The coefficient on gender (where male = 0 and female = 1) is −3.00 (p = .123, SE = 1.95), the coefficient on experimental wave is −1.06 (p = .508, SE = 1.60), and the constant is 31.37 (p = .000, SE = 1.82).

Table 8
Show-up Rates by Pay in Stages 1 and 2

	Promised Pay, Stage 2	
Pay in Stage 1	$5 (%) (1)	$15 (%) (2)
$5	40	72
$15	18	52

complete the survey than those in the (5, 5) treatment (52% vs. 40%), but the difference is not significant ($p < .25$, χ^2 test).

Comparing rows within each of the two columns allows one to examine the effect on labor supply in stage 2 of pay in stage 1, holding current pay constant. Of those who were promised $5, the participants in the (5, 5) treatment were significantly more likely to show up and complete the survey than those in the (15, 5) treatment (40% vs. 18%; $\chi^2(1) = 5.9, p < .05$). Of those promised $15 to show up and complete the survey, the ones in the (5, 15) treatment were significantly more likely to show up than those in the (15, 15) treatment (72% vs. 52%; $\chi^2(1) = 4.2, p < .05$). An alternative way to see the relative pay effect is to compare the two rows: those who received $15 in stage 1 (bottom row) are always less likely to show up compared with those who received $5 in stage 1 (top row). The reason is that no matter what the offer of stage 2 is, it is always less attractive to those who received $15 in stage 1. Hence, we find that when the payment offer in stage 2 is lower than the stage 1 payment, the show-up rate is 18% (col. 1, row 2); when the stage 2 payment offer is the same as in stage 1, the show-up rate is 40%–52% (col. 1, row 1; col. 2, row 2); and finally, when the stage 2 offer is better than the payment in stage 1, which is the case only for those who received $5 in stage 1, show-up rates are 72% (col. 1, row 1).

In line with the results of study 1, these results further support the hypothesis that reference pay affects subsequent take-up rates. We find that the participant take-up rate is more sensitive to changes in the wage rate than to absolute levels.

III. Conclusion

This article directly shows the importance that relative pay plays in labor supply decisions. Labor supply is only one likely consequence of workers' concern for relative pay. Satisfaction with pay is also likely to play a role in worker motivation and performance (e.g., Mas 2006; Card et al. 2010) as well as employee misbehavior (John et al. 2014).

What are the psychological underpinnings of the impact of relative pay on labor supply? One possibility, in line with "coherent arbitrariness" (Ariely, Loewenstein, and Prelec 2003), is that subjects in the experiment have lim-

ited experience with how much they should be paid for a given task. While absolute values are important, presented with a contrast between a higher and lower rate of pay, the high rate will naturally seem generous and the low rate stingy. For example, once participants became aware that some people were being paid $0.40 per matrix, they inferred that earning $0.80 was a pretty good piece rate. This logic could potentially explain the results of comparing the no relative and relative treatments in study 1. However, this explanation, by itself, cannot account for the results of the essay evaluation treatment in which the essay evaluation served as a (stronger) justification for the differential pay. People may evaluate pay levels in a fashion consistent with coherent arbitrariness; but the perception that a pay rate is low seems to have less impact on labor supply if accompanied by a plausible rationale.

The impact of the justification manipulation modifies the story somewhat. It suggests that an important consideration for participants is not only how much they will be compensated for their time—whether the pay is worth their time—but also whether pay rates are fair. People seem to evaluate not only the lucrativeness but also the fairness of pay rates in an inherently comparative fashion (see also Blount and Bazerman 1996).

The idea that fairness is important in labor supply relates to other findings in the labor literature. Participants may judge differential pay for the same work to be inherently unfair (Bewley 2003). Even if pay rates at a particular employer are arbitrary and at variance with those at other employers, for the perception of fairness it may be sufficient that those with similar jobs and skills in the same firm are paid similar wages. However, although evaluating the essay without stating the exact criterion for evaluation appeared to be an acceptable justification for the differential pay, in the sense that it eliminated the impact of pay comparisons on labor supply, we find no evidence that the channel through which it works is fairness. In a poststudy feedback question, we find that only 15% of the participants in the essay evaluation treatment (which eliminated the impact of relative wage on labor supply) viewed the assignment procedure as fair, whereas a significantly higher share of participants (49%) viewed it as fair when provided with the seemingly less satisfactory rationale provided in the random essay condition.[8]

In sum, relative pay does seem to be a potent determinant of labor supply, whether the comparison is to what others are earning or what one earned in the past. Yet our experiments suggest that people are quite ready to accept even a flimsy rationale as an acceptable explanation for inequality.

[8] The exact question was, "How fair did you find the procedure that determined whether you received $0.40 or $0.80 per correct matrix?" We used a 5-point scale and then classified the responses as "unfair," "unclear," and "fair" if the respondents selected less than 3, exactly 3, or more than 3, respectively.

Appendix

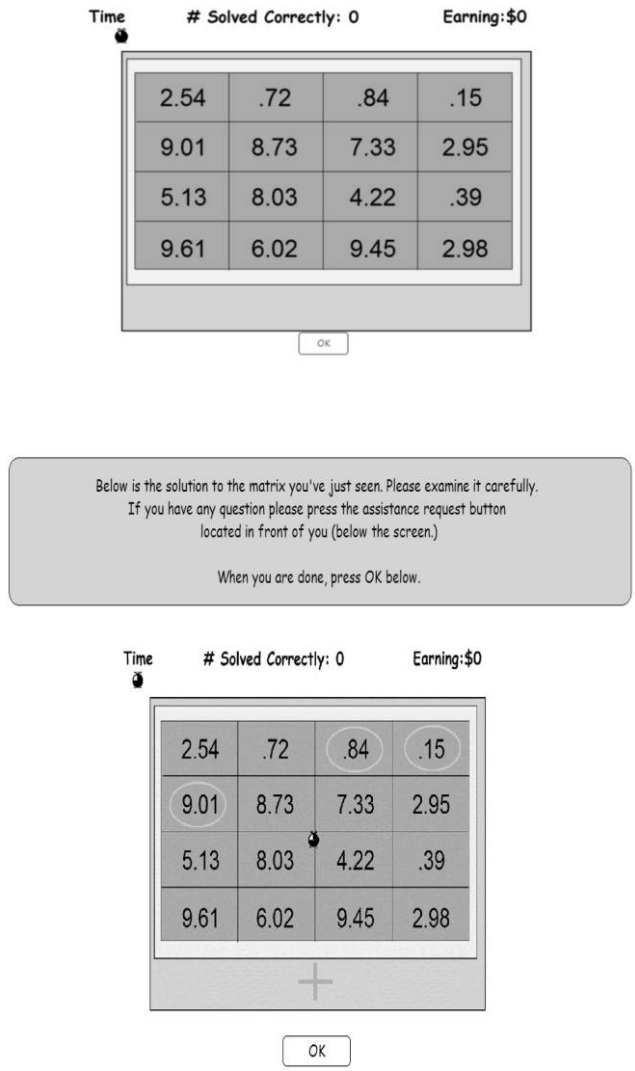

FIG. A1.—Sample of the task used in study 1

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