The Wages of Pay Cuts: Evidence from a Field Experiment*

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Abstract

To understand why firms rarely cut nominal wages, we hired workers for a data entry task, paid them a high wage and then offered some of the workers the opportunity to keep working, albeit for a lower wage. We framed the new wage offer in different ways across treatment groups. Workers were more likely to reject lower offers, but "reasonable" justifications largely eliminated this effect. Not all justifications were effective—justifying the cut on the ground that it would increase our profits actually increased quits. We also measured whether the treatments affected quality, trust and cooperation. The "profits" treatment reduced cooperation and possibly reduced quality; the other treatments had generally weak or nonexistent effects.

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1 Introduction

Firms rarely cut nominal wages, even during recessions. If they must reduce their wage bill, employers generally reduce hours or lay off workers. Because this pattern of downward nominal wage rigidity has import consequences for policy and for our understanding of labor markets, economists have proposed several explanations, but

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empirical researchers have found discriminating among these theories difficult. The difficulty stems in part from a lack of exogenous variation in firm wage setting and in part because of the inherent difficulty of explaining with data why something does not happen.[1] In response to this empirical challenge, Bewley (1999) took the atypical step of interviewing a large number of firm managers and directly asking them how they determined their wage policies, particularly why they did not cut wages during recessions. The managers reported that cuts would hurt morale, lower workers' identification with the firm and ultimately reduce profitability. The usual caveats about self-reports aside, this explanation for the absence of cuts—fear of how workers will react—seems plausible, and yet it raises another question, which is why workers would react so badly to wage cuts.

We can, of course, understand why workers are not *happy* with lower wages, but inflation effectively lowers wages constantly and firms often let real wages fall. Despite having the same effects on purchasing power, there are two important and obvious difference between nominal wage cuts and real wage erosion: wage cuts are highly salient, and people generally view acts of omission as less blameworthy than than acts of commission. This saliency distinction suggests a mechanism for nominal wage rigidity: if workers (a) appreciate that wages are being cut and (b) view those cuts as unfair and (c) are willing to altruistically punish perceived wrongdoers, firms might find that the costs of cutting wages outweigh the benefits.

The idea that nominal wage rigidity is explained by firms setting wages in the shadow of worker fairness reactions seem plausible, but it is unsatisfying, or least not that helpful, because it hinges on what workers are likely to *perceive* as fair. Questions about perception take us far from the standard purview of economics, and at least according to some psychologists, fairness judgments can be highly context-dependent. Given the impossibility of cataloging—never mind experimentally manipulating—every possible context in which wage setting occurs, we are left in the unfortunate state of knowing that an explanatory factor is both important and hard to model or generalize.

In this paper, we attempt to sidestep the multiplicity of contexts problem by assuming that even though there are many context-dependent paths to the verdict of "fair" or "unfair," worker behavior—conditional upon reaching one judgment or the other—is predictable. To implement this idea, we conducted an experiment in which we hired workers for a data entry task, paid them a high wage and then offered

[1] Theoretically, we know why people do not burn $100 bills, but we do not have much observational evidence on the subject.

"treated" workers the opportunity to keep working, albeit for a low wage. Critically, this low wage offer was framed or justified in different ways across treatment groups. We designed the different framings and justifications so that they would provoke workers to view the wage cuts as either reasonable and fair or capricious and unjust. After offering the wage cut, we observed whether workers accepted our offer and we observed how workers responded along several dimensions firms are likely to care about, such as work quality, cooperation with the firm, trust in the firm and patience.

We found that workers were more likely to reject low offers, but "reasonable" justifications largely nullified this effect. Not all justifications were effective—justifying the cut in terms of our profits actually increased quits. This "profits" treatment also reduced cooperation and quality. The other treatments had generally weak or nonexistent effects and none of the treatments affected trust or patience.

2 Theory & evidence

There are a multitude of possible reasons why wages might affect productivity and hence there are a multitude of "efficiency wage" models.[2] One sub-family comprises models that focus on the relationship between pay and worker effort. For example, in the "shirking model," firms pay above-market wages to gain leverage over workers; an efficiency wage job is valuable, and a worker might refrain from shirking to lower the risk of being fired (Shapiro and Stiglitz, 1984). In the "gift exchange" or the closely related "fair wage" models, firms give a "gift" of high wages and workers respond by gifting consummate performance; if workers feel they are treated unfairly, they withhold their gifts or even actively retaliate against the firm (Akerlof, 1984; Yellen, 1984).

2.1 The evidence on gift exchange

Laboratory experiments support the gift exchange model (see Camerer and Weber (forthcoming) and references therein), and the proposition that past wage experiences can create wage reference points (Fehr et al., 2006). However, the relevance of some of these results to real employment is questionable: Gneezy and List (2006) show that positive reciprocity is fleeting in lengthy, real-effort tasks. Given the duration of most jobs, Gneezy and List argue that *positive* reciprocity cannot explain why newly hired workers are paid above-market wages, though it is still possible negative reciprocity

[2]See Katz (1986) for an overview of the different models.

is made of sterner stuff, and it is what keeps wages from falling.

This possible asymmetry between positive and negative reciprocity was explored in a recent field experiment by Kube et al. (2007). The researchers cut wages, or more accurately, frustrated wage expectations, by advertising the job using ambiguous language and then exploiting this ambiguity to pay some workers (college undergraduates) less than they expected. One limitation of this study is that the treatment depends upon confusion about the real wage, and this is not what wages cuts are really like. However, this treatment and a real wage cut are both examples of frustrated wage expectations, and we might reasonably expect that worker reactions will be similar in either case.

Although field experiments offer a more compelling test of gift exchange than the laboratory experiments, observational data from real, long-term employment scenarios would be more convincing, but the circumstances needed for causal inference—idiosyncratic factors leading to wage changes for one group of workers but not for another—are rare. Occasionally, this kind of scenario occurs, and the evidence from them is consistent with the view that negative reciprocity is long lasting and harmful to the organization. Mas (2006) found that New Jersey police forces losing arbitration closed fewer cases; Lee and Rupp (2007) found that airline pilots subject to large, industry-wide wage cuts were late more often. In the airline example, the effects were modest and transitory, perhaps because most airlines were at or near bankruptcy when the cuts were made, thus muting any fairness judgments.

2.2 Considering context

One common feature of the empirical evidence on wage cuts is that the context is either given by circumstance (the observational evidence and the field experiments) or the situation is decontextualized (the laboratory studies). Of course, every study has a particular context, and determining whether the findings are particular to the context is the inherent challenge of establishing external validity. But in the particular case of wage cuts, there are good reasons to think that context is especially important. Kahneman et al. (1986) found that people (a) view wage cuts that keep the firm solvent differently from cuts that increase already positive profits or to exploit market changes (b) see wage reference points as attached to specific workers in specific jobs, and not transferable to new employees or as having any relevance after sufficiently large reorganizations.

Context can be manipulated, either by design or by chance, but the only study

we are aware of exploiting variation in context is Greenberg (1990), who looked at employee theft in two factories following a temporary 15% pay cut. Employee theft rose in both plants, but in the plant where the CEO spent over an hour explaining the cuts and fielding questions, theft rose less than in the plant where management provided only a cursory explanation. Like Greenberg, we manipulate the context of the wage cuts, but we also look at a larger collection of outcomes, and because we conducted a controlled experiment, we have confidence that the response to the treatments is caused by framing of the wage cuts and is not an artifact of selection or the result of omitted variables.

2.3 A simple model of fairness judgments and quits

To discuss the experimental design and interpret the results, it is useful to consider a simple model of how factors that are economically irrelevant (i.e., do not affect payoffs) might still have economic consequences. Figure 1 illustrates a scenario where a worker is considering whether to continue at a piece-rate task, with the task having increasing marginal costs or whether to quit. Workers quit when the new wage offer is less than costs, with costs broadly defined to include the potential psychic cost of accepting an unfair wage.

Let c_0 and c_1 be the worker's cost for the initial task, additional tasks, respectively. In Figure 1, line b shows the worker's marginal benefit from working as a function of the wage. Ex ante, the worker's reservation wage for the initial task is w_0^r, corresponding to the point A where the benefit curve b intersects the cost curve c_0: $w_0^r = c_0$. The reservation wage for the additional task is w_1^r corresponding to the point B where the benefit curve intersects the cost curve c_1: $w_0^r = c_1$. Because $c_0 \leq c_1$, we can observe quits even without wage changes.

In a departure from the neoclassical model, we make three assumptions: (a) workers form reference points at previously received wages (b) workers find it psychologically painful to work for a wage below their reference wage and (c) this pain can be worsened or lessened by framing and context. If the employer parties the worker an above-market wage w_0^* initially, then the worker forms a reference point at E. "Unfair" wage offers increase the feeling of exploitation and thus the cost curve c_1^u has a steeper, negative slope than "fair" wage offers that create a flatter cost curve c_1^f. This difference in costs curves makes point C the reservation wage in the "fair" case and point D the reservation wage in the "unfair" case. In this example, the more unfair the wage offer, the more likely the worker is to reject it.

[Figure 1 about here.]

3 Experimental methodology

The experiment had three phases: (a) the wage expectation building phase (b) the treatment phase and the (c) the game phase. Figure 2 shows how subjects flow through the website hosting the experiment. In the wage expectation building phase, subjects transcribed three paragraphs, for 10 cents each. If a subject finished three paragraphs, they moved to the treatment phase. In the treatment phase, experiences differed: subjects in groups G2, G4, G6 and G7 were given the option to continue working, but at a lower wage, subjects in group G1 were given the same option, but at their previous wage while subjects in group G3 were not given the option to continue working. The framing of these various options also differed across groups. All subjects, regardless of what they did in the treatment phase, moved on to the games phase. In the games phase, they played a series of revealed-preference, contextualized games that measured their trust in us, patience and willingness to cooperate.

3.1 Experimental groups

The groups were:

G1 Control Subjects were offered their previous high wage of 10 cents to perform an additional task.

G2 Unexplained Subjects were offered a lower wage of 3 cents to perform an additional task. No explanation was given for the lower wage offer.

G3 No Cut Control Subjects were not offered an additional task. After the first set of tasks, they went straight to the follow-on games.

G4 Productivity Subjects were offered a lower wage of 3 cents to perform an additional task, but we first informed them that most workers can complete tasks more quickly after they gain some experience.

G5 Profit Subjects were offered a lower wage of 3 cents to perform an additional task, but we first informed them that we are trying to get as much work done for as little money as possible.

G6 Neighbor Subjects were offered a lower wage of 3 cents to perform an additional task, but we informed them that in our experience, other workers are willing to accept the lower offer.[3]

G7 Forced Subjects were *not* explicitly offered a lower wage to perform an additional task. Rather, they are simply brought to the next task, which was clearly labeled with the new, lower wage of 3 cents. There was a button at the bottom of the screen that allows them to quit and continue to the follow-on contextualized games.

G8 Primed Subjects were offered a lower wage of 3 cents to perform an additional task. No explanation was provided, but we first asked subjects what they thought would be a fair wage for doing an additional task.

Firms are likely to try justify their actions, which is why G4 is probably the most realistic treatment. No real firm would propose a wage cut in the same manner as the gratuitously insulting G5 treatment, yet it is still a useful treatment in that its effects might be similar to the effects of other actions real firms might take, such as proposing a wage cut following a large bonus for executives. In addition to hopefully angering workers, G5 tests whether any justification, not matter how insulting "works." G6 tests the notion that workers "learn" how to react from other workers. One reason why G6 might mitigate the behavioral response to a wage cut is that perhaps the psychic cost of taking work below one's reference point comes not just from a fear of being exploited, but also from a fear of being a singularly exploited. G7 tests whether not highlighting the wage cut and explicitly framing output for follow-on work as a binary decision affects behavior. G8 tests whether priming subjects to think about fairness enhances any effects.

[Figure 2 about here.]

3.2 Online marketplace as a testing domain

We recruited experimental subjects from an online labor market, created by a labor market intermediary (LMI) (Autor, 2008). Through an interface provided by the LMI, registered users perform tasks (posted by buyers) for money. The tasks are generally simple for humans to do yet difficult for computers—common tasks are captioning photographs, extracting data from scanned documents and transcribing audio clips.

[3]This was a true statement; there was no deception in the experiment.

Buyers control the features and contract terms of the tasks they post: they choose the design, piece-rate, time allowed per task, how long each task will be available and how many times they want a task completed.[4]

Workers, who are identified to buyers only by a unique string of letters and numbers, can inspect tasks and the offered terms before deciding whether to complete them. Buyers can require workers to have certain qualifications, but the default is that workers can "accept" a task immediately and begin work. Once the worker "submits" their work, buyers can approve or reject their submission. If the buyer approves, the LMI pays the worker with buyer-provided escrow funds; if the buyer rejects, the worker is paid nothing. The buyer can also grant bonuses, which is useful for our purposes since we can create complex contracts rather than use the default piece-rate format.

Although most buyers post tasks directly on the LMI website, it is possible to host tasks on an external site that workers reach by following a link. We used this external hosting method; we posted a single placeholder task containing a description of the work and a link to follow if subjects wanted to participate.

For the real-effort task, subjects transcribed (not translated) paragraph-sized chunks of Adam Smith's *The Wealth of Nations*. A sample paragraph is shown in Figure 3. This task was sufficiently tedious that no one was likely to do it "for fun" and it was sufficiently simple that all market participants could do the task. The source text was machine translated into Dutch, which increased the error rate of the transcriptions, thereby providing a more informative measure of work quality. Translating the text also prevented subjects from finding the text elsewhere on the Internet.[5]

[Figure 3 about here.]

3.3 Conditions for causal inference

To identify causal effects from the treatments, we needed ways to (a) as-good-as randomly assign subjects to different groups, (b) keep subjects from changing groups once assigned (c) keep subjects from participating multiple times, and (d) minimize non-random attrition.

[4]Tasks are often done multiple times by different workers for quality-control purposes.

[5]Since the text was presented as images, subjects were unable to simply copy and paste the text into the text box on the form. It is irrelevant whether or not any subjects actually knew Dutch since, if anything, knowledge of the language might make the task more difficult given the poor quality of the translation. One subject that apparently did speak Dutch sent an email warning us that the work was "grammatical gibberish."

For (a): We sequentially assigned subjects to treatment groups as they accepted our task. Unbeknownst to the subjects, clicking on the link associated them with a treatment group and redirected them to our site, thus stratifying subjects by arrival time. Because subjects were unaware of when other subjects arrived or the existence of other treatments, never mind the next treatment in the "queue," we are confident that treatment assignment was unconfounded with worker attributes.

For (b): If workers were aware of the different treatment groups, they would have an incentive to get into a group with a larger payoff. Even though subjects were unaware of the other treatments, to prevent the possibility of subjects hunting for the "best" treatment group, the software assigning subjects to groups tracked users' IP address. This tracking prevented subjects from breaking the randomization after their initial "assignment" click.

For (c): If a worker had multiple online identities, they could in principle complete our task multiple times. However, this double-dipping is unlikely: because buyers often want the same task done multiple times by different workers, the LMI designed several policies and software features to prevent a worker from having more than one account.[6]

For (d): To prevent differential attrition driven by differences among treatments, all subjects had identical initial experiences during the wage expectation building phase. Subjects' experiences differed by group only after already performing three transcriptions. Because of this investment, there was no attrition. An important point is that although we assigned subjects to treatments at the moment they accepted the task, the conceptual point of randomization was after the wage expectation stage but before the treatment stage.

The pre-randomization attrition is easy to spot in the data: attriting subjects just stop working before the end of the paragraph, never get the treatment stage and never submit their completion code to receive payment. We dropped from the sample any worker making more than 100 errors on a paragraph. This occurred for a little less than 20% of all subjects who started the paragraphs, but since these individuals were dropped prior to conceptual randomization, this kind of attrition is immaterial. Of the dropped subjects, none completed the survey, which gives us confidence that there were true attritors and not just bad performers.

[6]Workers must agree to have only one account—any detected attempt to have multiple accounts leads to a permanent ban. The LMI also requires browsers to accept cookies. If a person had two bank accounts or credit cards and two separate computers not sharing a network connection, they could in principle participate twice, but given the low stakes and risks involved (if this was detected, they would be banned from the site), we consider this possibility highly unlikely.

As a test of the randomization, we can see whether the distribution of collected covariates across the treatment groups is consistent with random assignment. For the binary covariates of gender, resident in the U.S. or Canada and whether the worker spends more than 10 hours a week online doing tasks for money, the Pearson's Chi-squared test p-values are 0.06, 0.12 and 0.21, respectively. While the low p-value for the gender is a little disconcerting, this is almost certainly the result of random chance and is not a major concern. However, since these measures are self-reported, there is a possibility that they are affected by the treatments, and for this reason all regressions are shown with and without the inclusion of controls.

3.4 Measuring outcomes following the wage cut

To measure quits, we observed how many subjects agreed to the follow-on wage offer; to measure work quality, we calculated how many edits would be required to transform the provided transcription provided transcription to a perfect transcription of the underlying text.[7] To measure patience, trust, and cooperation, we created contextualized games that that would not seem unusual to subjects.

To measure patience, we simply asked subjects whether they would be willing to forgo nearly immediate payment in exchange for a 30 cent bonus. We measured trust in the "firm" with a modified version of the trust game (Glaeser et al., 2000). To save money, we told subjects that we would select one player at random and implement his or her choices. Subjects were told to imagine they had \$15 and that they could choose any fraction of that money to "send" to us. If we judged their work favorably, we would double what they sent and "send it back"—in the event of a unfavorable judgment, we would keep whatever they sent.[8] Although this game measures the worker's trust in us (e.g., trust that we will follow our own protocol and judge their work fairly), it is confounded with the worker's confidence, risk aversion and beliefs about our standards for the work. If these non-trust factors held constant across experimental groups we could in principle detect changes in trust, though this assumption of constant effects is questionable.

We measured cooperation in two ways: (a) willingness to give free advice and (b) willingness to take a short survey at a later date. If subjects agreed to take this follow-on survey, they were paid an additional 15 cents.[9] The survey measure

[7] This minimum-number-of-edits metric is called the edit distance, or the Levenshtein distance.

[8] The subject we randomly selected had chosen to send the full \$15; we gave him or her \$30.

[9] When you pay workers, you can embed a short message about why you are paying them—we embedded the link to our survey in this message. Remarkably, not one person who agreed to do the survey actually did it after we sent them the link. We had intended to use completing the survey as

of cooperation captures a mix of self-interest and cooperation with the firm, and is somewhat analogous analogous to a willingness to work overtime. For second cooperation measure, subjects were asked to suggest ways to make the transcription task easier. The wording of the request made it clear that offering advice was voluntary and that no payment would be made for the offered advice.[10]

4 Results

Table 1 reports the cross-tabulations for our main outcome measures (and binary covariates) by experimental group. Table 2 presents our results for quits and work quality, Table 3 trust and patience and Table 4 for cooperation. In all regressions, we used a linear model to analyze the data.[11] In Table 2, G2 is the excluded dummy variable—coefficients are marginal effects on the base rate in G2, "no explanation" case. We dropped data from G3 since these subjects were brought straight to the follow-on tasks. In Tables 3 and 4, no data was dropped and the "no offer" G3 group is excluded. We chose G3 because it approximates the status quo in the firm of no wage offers or changes.

For each of our post wage cut outcomes, we report regression results with and without the control variables. For the controls, we include an indicator for gender, whether the worker was from the U.S. or Canada, whether the worker self-reports spending more then 10 hours online per week working, whether the worker self-reports English as his or her first language and finally, the log of the sum of all the errors he or she makes on the first three paragraph transcriptions.

[Table 1 about here.]

4.1 Output—accepting the wage cut

Column Y of Table 2 reports estimated coefficients from an OLS regression of whether individuals accepted the wage cut on group assignment indicators; Column Y(c) re-

another measure of cooperation and trustworthiness, but besides there being no variation in survey response, we believe there may have been a technical error that prevented subjects from filling out the survey, given the complete lack of response.

[10]The text of the actual question was, "Please help us make these tasks easier for other workers. Describe what steps workers can take to make doing these transcriptions easier."

[11]Although some of our outcomes are dichotomous variables or counts, the linear model with robust standard errors offers several advantage over non-linear models: it is simpler, the coefficients are more easily interpretable and all treatment coefficients are identified, even when they perfectly predict success or failure (this is not the case with general linear models estimated by maximum-likelihood).

ports the results of the same regression when control variables are included. The regressions show that workers were more likely to do additional work when: we offered the previous wage (G1); we justified the wage cut in terms of increased productivity (G4); we indicated others are accepting the cut (G6); we failed to flag the wage cut as a new offer (G7). The G4 and G6 treatments reduced quits (i.e., increase the acceptance probability) by about .3, compared to when the cut is unexplained (G2). Uptake across G1, G4 and G6 was approximately the same, as can be seen by their associated regression coefficients or by Figure 4. For G7, the "forced" effect is about .6, which reflects the 100% uptake in G7 and the approximately 40% uptake in the comparison group.

Initially, we thought that G5 and G8 (the "profit" and "primed" treatments) would anger workers and cause more quits. While both treatments had a negative effect, they are not statistically significant. That G5 fails to reduce quits runs counter to studies showing that people acquiesce to demands when they are given a reason for that demand, no matter how inane the justification (for example, Langer et al. (1978) is the famous study showing that asking someone to let you cut in line to make copies "because you need to make copies" dramatically increases compliance).

The G7 "forced" group has higher output than the G1 group, even though the G7 offered wage was 70% lower. This surprised us, as we expected subjects to refuse the task because of the underhandedness of the offer. It is possible that subjects failed to consider their option of refusing to do the task, even though this option was clearly marked. Another possibility is a worker might believe they were in error: if a worker thought he had read the instructions incorrectly, he might worry that quitting would jeopardize payment for work already completed. This caveat aside, the evidence does suggest that explicitly flagging a wage change has a different effect than imposing the same change surreptitiously. This difference in reactions bolsters the notion that the higher salience of wage cuts compared to real wage erosion caused by inflation contributes to nominal wage rigidity.

[Figure 4 about here.]

[Table 2 about here.]

4.2 Quality—number of transcription errors

The columns in Table 2, where the headings contain E, report the OLS coefficients for a regression of logged error counts by group indicators and covariates. The only

strong result is that workers in the G5 "profit" group accepting the low wage offer made more errors; compared to the "no wage cut" excluded group, G5 subjects made, on average, 20 more errors. If we exclude an obvious outlier, the difference falls to about 8 errors. The outlier was one subject who agreed to the offer but left the text box blank. With and without the outlier, the effects are statistically significant ($p < .05$). While the finding looks like retaliation, it might also reflect selection, since the group have different quit rates and workers doing the fourth task might vary across treatments. Although we control for first-stage quality in Column E(c) and still find an effect, we are implicitly making a selection-on-observables assumption. Figure 5 is a scatter plot of the error count for every person. Although the G5 error points are higher, the selection issue is highlighted by the figure—there are far fewer data points.

We find no evidence that lower quality workers were less likely to quit; the coefficient on the logged errors is slightly negative in Column Y(c). This provides some evidence against the adverse selection model of efficiency wages (i.e., the best worker have the best outside options and are the most likely to quit after a wage cut). We also find no evidence that paying workers more improves quality; subjects in G1, the "no wage cut" group made slightly more errors, though the result is not statistically significant. This provides some evidence against the positive reciprocity conception of gift exchange.

[Figure 5 about here.]

4.3 Trust & patience—believing the firm, waiting for payment

Table 3, Column Trust reports the estimated coefficients from an OLS regression of the amount sent in a trust game by group indicators; Column Trust(c) includes controls. Generally, there does not appear to be any strong treatment effect on our trust measure. Subjects in the G6 "neighbor" group appear to trust more ($p < .05$), but this effect becomes smaller when estimated with controls, as well as less significant ($p < .10$). "Trust" is decreasing in first stage errors and is higher for men (though not at a statically significant level); both findings deepen our fear that our contextualized measure captures risk aversion and confidence.[12] Column Patient

[12]See Schubert et al. (1999) and references therein to gender differences and risk aversion. Although our measure was contextualized, it was framed in a way that made it seem more like a gamble than an investment, so finding a gender difference is consistent with both Schubert et al.'s experimental evidence and other papers on gender differences.

reports our coefficient estimates for the effects of the treatment on patience; we find no treatment effects, and only slight evidence that subjects from the US or Canada are less willing to accept a delay ($p < .10$).

4.4 Cooperation—offering advice

For the first cooperation measure, priming workers to think about fairness (G8) increases whether they give any advice, while mentioning that other workers are willing to take the wage cut (G6) seems to increase the amount of offered advice, though this evidence is not that strong. Table 4, Column Advice reports the results of a regression designed to measure effects on cooperation; the number of characters of advice offered by a subject is regressed on the treatment indicators. Column Advice (c) includes controls. Columns Advice 2 and Advice 2(c) correspond to the Column Advice and Column Advice (c) regressions, except the dependent variable is a binary indicator for whether the subject offered any advice at all.

For advice length, only G6, the "neighbor" treatment, appears to have an effect: in this group, subjects increased advice length by an average of about 50 characters ($p < .05$). The G6 "neighbor" treatment indicator is also significant when predicting whether any advice at all is offered ($p < .10$), but only when controls are included. In both the (Advice 2) and (Advice 2(c)) regressions, the G8 "priming" group indicator is significant ($p < .10$ without controls, $p < .05$ with controls), as is G2, "unexplained" is significant ($p < .10$ without controls; $p < .05$ with controls). Figure 6 plots the character length of the advice against the treatments. In this plot, the effect of G6 on the amount of advice can be seen in the cluster of points between 200 and 275, while the effects of G8 and G2 can be see in the relative absence of points at zero.

The G2 effect is surprising and we cannot think of a plausible hypothesis that explains this result, aside from random chance. For the G6 and G8 results, there is one speculative interpretation: by invoking social comparisons, G6 and G8 somehow put subjects in an accommodating mood. One common feature of both G6 and G8 is that they both explicitly invoke a social comparison (i.e., G6 introduces information about what other workers have done, while G7 discusses fairness). G4 discusses other workers' productivity, but this framing might be more about the nature of the task than about comparing oneself to other workers. Perhaps the G6 and G8 treatments somehow cause subjects to think more about other workers and therefore behave more charitably.

[Figure 6 about here.]

4.5 Cooperation—agreeing to a survey

For the second cooperation measure (willingness to take a follow-on survey), the "profit" treatment (G5) strongly reduces cooperation: only 74% of workers in G5 agree to take the survey, compared to an overall acceptance rate of 94% (not including G5). Table 4, Column Survey show that those G5 subjects have about a .25 lower probability of accepting the survey offer compared to the G3 comparison group ($p < .01$). What is interesting about this measure is that the presumably offended workers chose not to participate even though this would be a perfect way to retaliate—they could accept payment and then ignore the survey (since they would receive payment before receiving the survey link), yet they choose not to do this. Instead, they choose not to participate at all, foregoing an easy 15 cents.

[Table 3 about here.]

[Table 4 about here.]

5 Discussion

We find that the manner in which a wage cut offer is framed affects output, cooperation and possibly work quality. Our results undermine the notion that workers have a fixed reservation wage determined only by the onerousness of the work and the offered wage; workers' assessment of the fairness of the offer matters, and this assessment seems to depend upon contextual factors.

In terms of economic theory, our results are consistent with the negative reciprocity aspect of the gift exchange model. Our workers responded to wage cuts in ways damaging to the firm, though this damage was mediated by the framing of the cuts. If we had been a profit-maximizing firm, we might not have cut wages for the follow-on task, assuming our costs for transcription errors, turn-over and survey non-compliance were sufficiently high. Of course, the fact that firms do sometimes cut wages is proof that these worker-response considerations are not always paramount. One reason why the worker response might not override the benefits of cutting wages is that the situation is sufficiently dire that the firm will get a "pass" for cutting wages. We do find that seemingly valid justifications for a wage cut can help sweeten the bitter medicine, but workers are not fools, and offensive justifications actually make things worse. The main conclusion we draw from the evidence is that the notion that firms set wages in the shadow of the worker's taste for fairness has at least some explanatory power.

It is beyond the scope of this paper to analyze the welfare implications of worker fairness judgments and their effect on wage-setting, but we should note that the way fairness judgments seem to work do not, at least, exacerbate the business cycle: when firms must reduce costs, wage cuts are less likely to have the bad effects they would during booming times.

5.1 External validity

As with any study, one should be concerned about the external validity of our results. The differences between our setting and a real labor market are almost too numerous and obvious to mention.[13] Our setting does have the advantage over some laboratory studies in that subjects are performing real-effort tasks for money in what they believe is a real, albeit unusual, market setting. However, assessing external validity does not mean just tallying up the similarities and differences and reporting the "distance" between the experimental setting the real-world phenomena; assessing external validity requires one to consider how this "distance" is likely to alter the findings.

In our experimental setting, the short duration, anonymity and "one shot" nature of interactions should push subjects towards viewing everything through a homo economicus lens, and yet we find fairness judgments having strong effects. The incredibly low stakes cut has an ambiguous effect: on one hand, the wage cuts are absolutely small and thus perhaps less offensive, yet the small stakes make principled refusals less costly. In conventional jobs, quitting has far greater consequences and we would expect workers to be less sanguine about losing their jobs, even after a wage cut. The the higher stakes and longer duration of real jobs would probably make any fairness judgment more strongly felt. These more salient fairness judgments might magnify the response to wage cuts, though perhaps reactions with direct income consequences for the worker (such as quitting) are less likely and withdrawal of cooperation is more likely.

5.2 Interpreting employment as an incomplete contract

Although we find some limited evidence of low work quality coinciding with the apparently offensive "profit" justification, our holistic interpretation of the results is

[13] One distinction between our scenario and real scenarios is that our tasks are piece-rate, while most real employers pay hourly wages. While we think this distinction is not relevant to the whether the cut triggers a fairness judgment, piece-rate work does raise some strategic issues (Carmichael and MacLeod, 2000).

that "retaliation" or "negative reciprocity" are probably the wrong characterizations. Although the term "negative reciprocity" conjures images of employees pilfering supplies or sabotaging equipment, Bewley (1999) found that what firms fear is not overt retaliation; they fear that morale will sag and that disgruntled workers will no longer identify with with the firm or adopt its goals as their own. The incomplete contracting model introduced by Hart and Moore (2008) offers a better conception of what sours in the employment relationship after a wage cut. In the Hart-Moore model, the performing party makes a non-contractible decision as to whether she will provide consummate performance or to-the-letter, perfunctory performance. For the performing party, the added costs of consummate performance are negligible but can have large implications for the buyer.[14]

The inability of a buyer to compel consummate performance is an example of the broader principal-agent problem and is common in modern models of employment. To connect this feature back to Bewley's survey research, it is interesting that many managers were worried that after a wage cut, employees would no longer "identify" with the firm. To "identify" with someone in a psychological sense is to have empathy for that person and to see your interests and concerns as coinciding. This alignment of interests between firm and employee, even if more psychological than financial, seems like a way to overcome the inherent principal-agent problem of employment. Losing this solution to the principal-agent problem is one way of conceptualizing precisely what firms fear will happen if they cut wages.

6 Conclusion

Our main positive finding is that framing can affect the behavioral response to a wage change. These worker reactions are fairly easy to understand: workers reduce output and cooperation when wages are cut for capricious or selfish-seeming reasons, but workers do not reduce output as much and apparently are still willing to cooperate if the employer has seemingly valid reasons for his or her actions.

Although this paper focuses on the role of fairness judgments in causing nominal wage rigidity, it is not the only example of moral judgments shaping the contours of the economy. It seems likely that popular moral sentiment and human psychology shape laws, institutions and customs. Roth (2007), which looks at repugnance as a

[14]For example, consider a worker noticing that an expensive piece of machinery that she is not directly responsible for is about to run out of oil and become damaged; alerting someone takes almost no effort for the employee but could save the firm a great deal of money.

constraint on markets, is one example of the larger phenomena of what might be called positive moral philosophy affecting economic life. More research in this vein might lead to a better understanding of the "folk morality" people have about markets. It would be particularly interesting, and perhaps useful to policy-makers, to know what kinds of moral judgments are universal and seemingly invariant and which are mutable.

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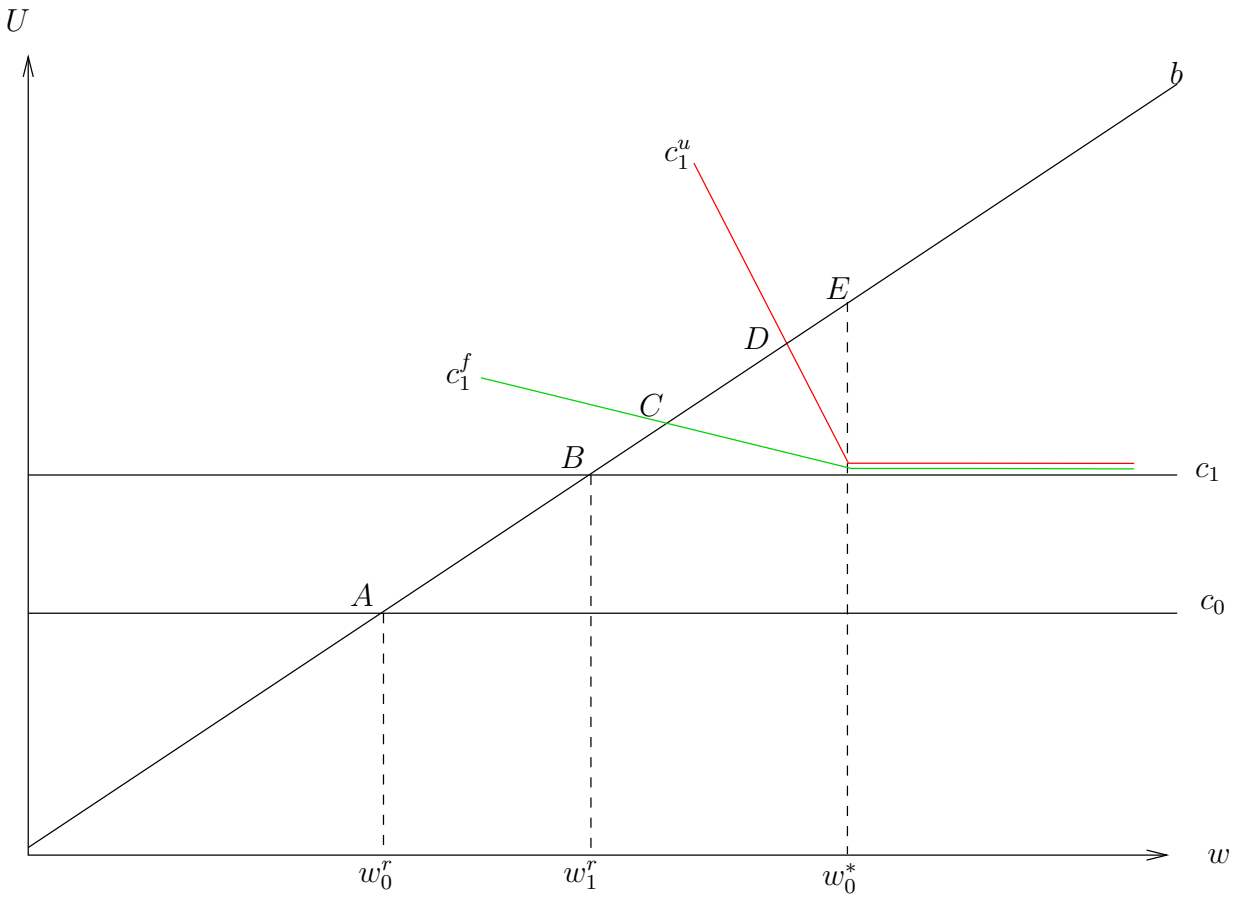
List of Figures

Figure 1: Quits following new wage offers after worker has altered reference point due to past wage experience.



Notes: This figure illustrates how being paid above one's reservation wage initially (a) can change the reservation wage for subsequent offers, depending upon how that offer is framed. If the task has increasing marginal costs, then the initial per-task performance cost is c_0, which is below c_1, the costs for the follow-on task. We assume a constant marginal utility of wealth, so from the 1st task to the second task, the neoclassical reservation wage moves from A to B. However, if the worker is paid w_0^* for the initial task, the reference point created by this experience might affect the reservation wage for subsequent offers. For an unfair offer (in red), the performance cost increases to c_1^u and the reservation wage is D. With a fairer offer (in green), the cost curve is c_1^f and reservation wage is at C, which is still above the neo-classical reservation wage at B for this task, but lower than the the "offensive" offer reservation wage at D.

Figure 2: Experimental Design

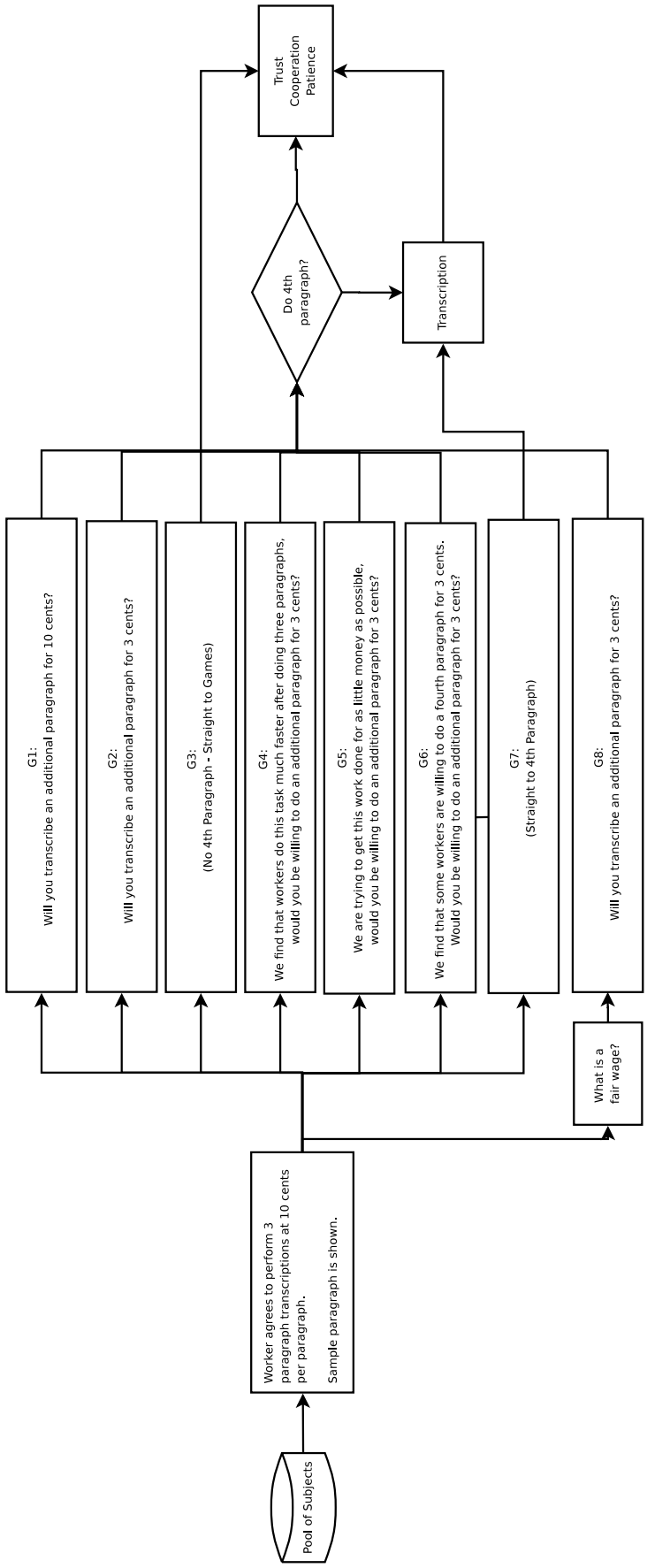
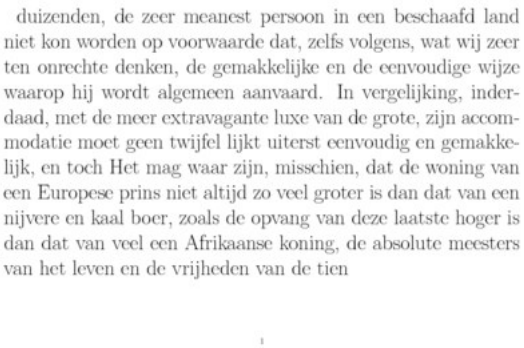
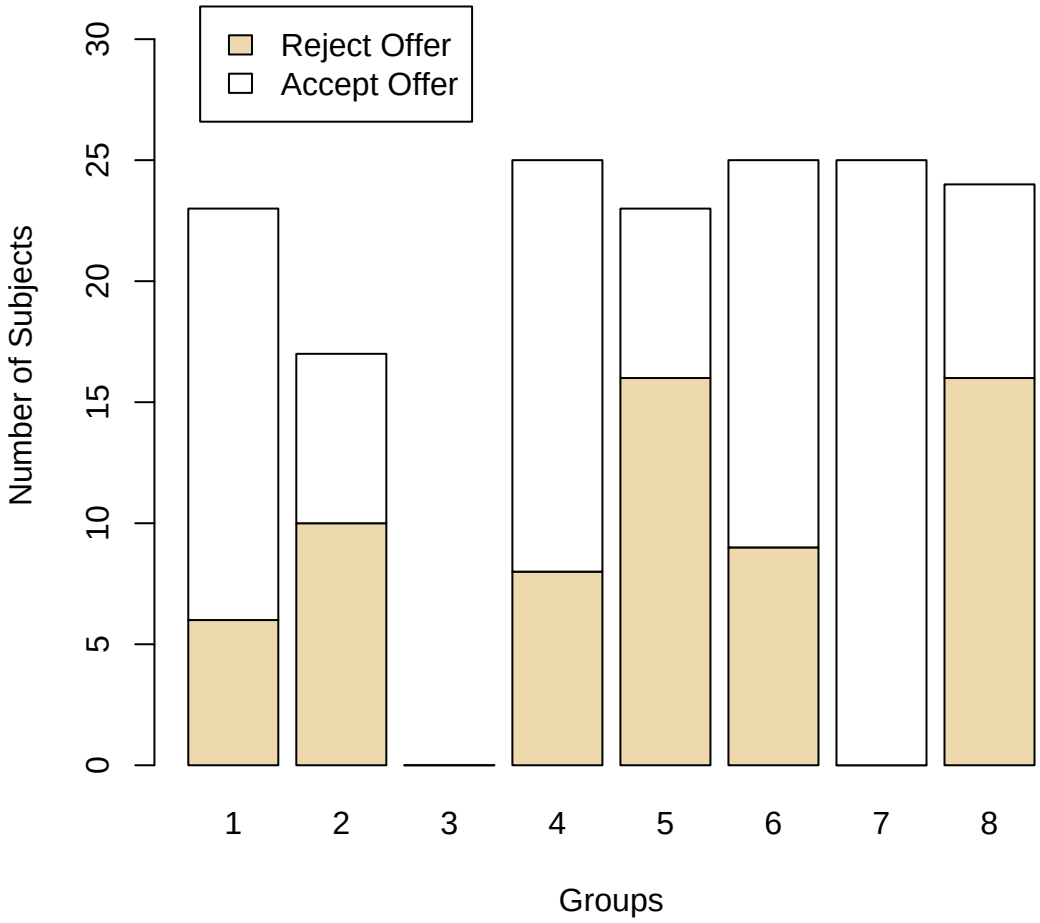


Figure 3: Sample Real-effort, piece-rate task


duizenden, de zeer meanest persoon in een beschaafd land niet kon worden op voorwaarde dat, zelfs volgens, wat wij zeer ten onrechte denken, de gemakkelijke en de eenvoudige wijze waarop hij wordt algemeen aanvaard. In vergelijking, inderdaad, met de meer extravagante luxe van de grote, zijn accommodatie moet geen twijfel lijkt uiterst eenvoudig en gemakkelijk, en toch Het mag waar zijn, misschien, dat de woning van een Europese prins niet altijd zo veel groter is dan dat van een nijvere en kaal boer, zoals de opvang van deze laatste hoger is dan dat van veel een Afrikaanse koning, de absolute meesters van het leven en de vrijheden van de tien

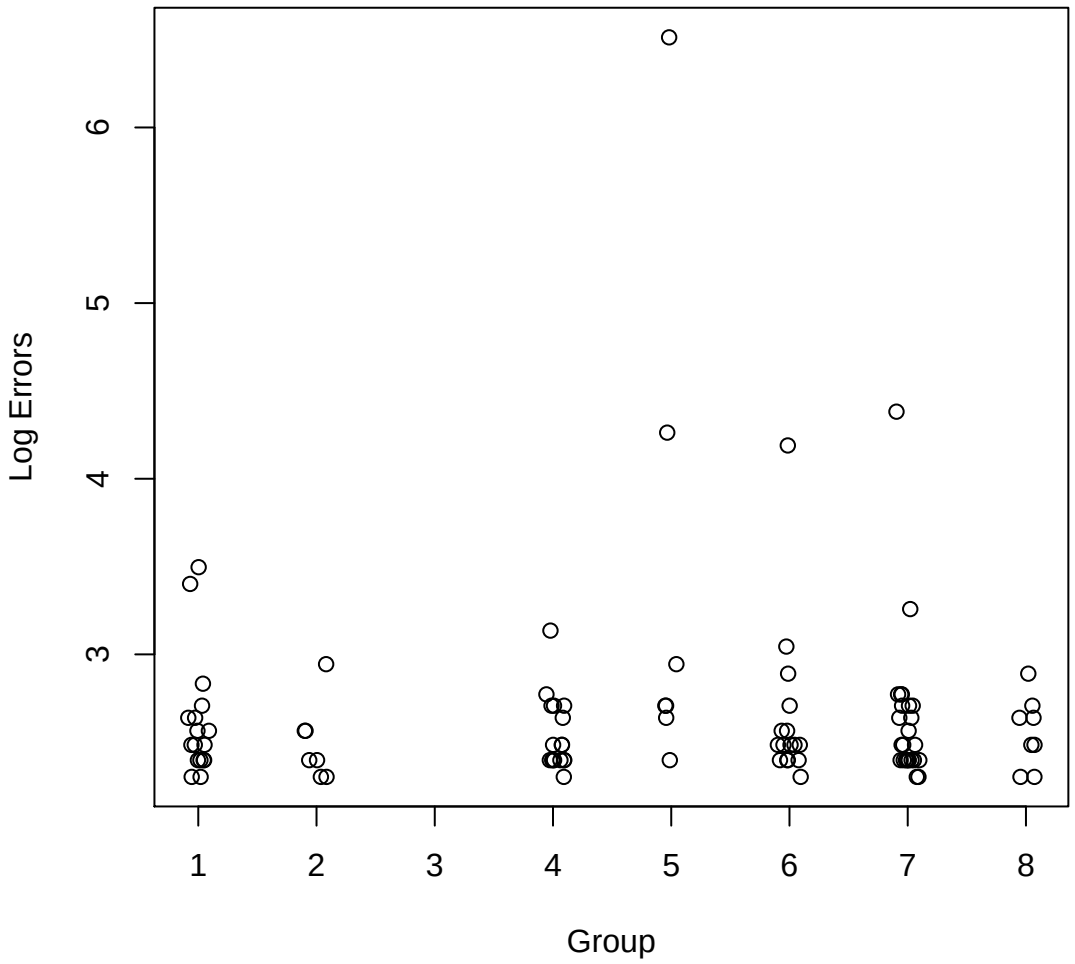
Notes: Subjects were asked to transcribe paragraphs like the one above into a text box. It is a short selection from the "Wealth of Nations" that we machine-translated into Dutch.

Figure 4: Quits after first stage by group



Notes: G1 Control—Offered same rate (not a wage cut)
G2 Unexplained—No explanation for low offer
G4 Productivity —Low offer explained as response to increased productivity
G5 Profit—Low offer explained as profit motive by employer
G6 Neighbor—Subjects told others willing to accept the offer
G7 Forced—Subjects brought to next task without receiving an offer
G8 Primed for Fairness—Subjects asked what is a fair wage before offer

Figure 5: Log errors on fourth paragraph, with errors defined as the minimum edit distance



Notes: The horizontal coordinate of the data points are "jiggered" (i.e., randomly perturbed), to reveal point density.

G1 Control—Offered same rate (not a wage cut)

G2 Unexplained—No explanation for low offer

G4 Productivity—Low offer explained as response to increased productivity

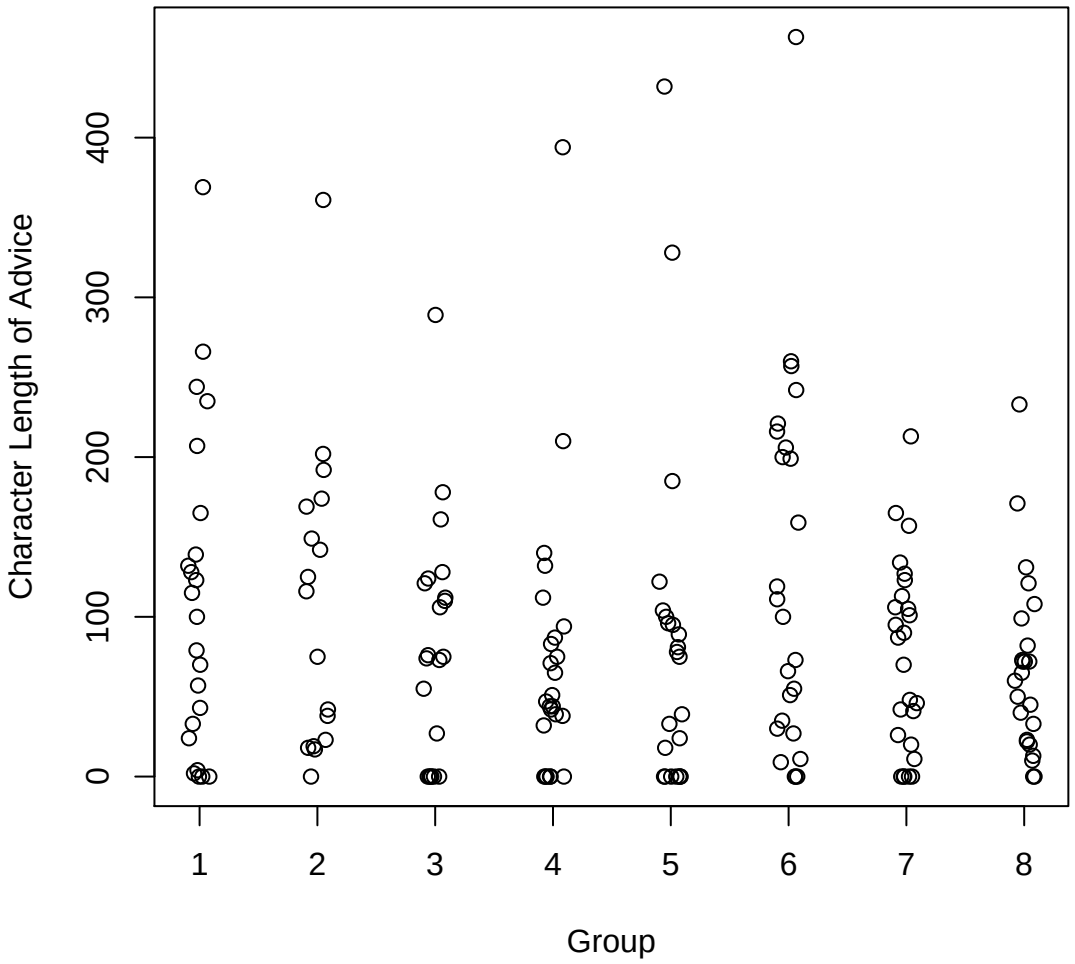
G5 Profit—Low offer explained as profit motive by employer

G6 Neighbor—Subjects told others willing to accept the offer

G7 Forced—Subjects brought to next task without receiving an offer

G8 Primed for Fairness—Subjects asked what is a fair wage before offer

Figure 6: Advice length, in number of characters, by group



Notes: The horizontal coordinate of the data points are "jiggered" (i.e., randomly perturbed), to reveal point density.

G1 Control — Offered same rate (not a wage cut)

G2 Unexplained—No explanation for low offer

G3 No Cut Control—Subjects go straight to games

G4 Productivity—Low offer explained as response to increased productivity

G5 Profit—Low offer explained as profit motive by employer

G6 Neighbor—Subjects told others willing to accept the offer

G7 Forced—Subjects brought to next task without receiving an offer

G8 Primed for Fairness — Subjects asked what is a fair wage before offer

List of Tables

Table 1: Cross Tabulations by Group

	N	1	2	3	4	5	6	7	8
		$N = 23$	$N = 17$	$N = 21$	$N = 25$	$N = 23$	$N = 25$	$N = 26$	$N = 24$
U.S./Can.	184	61% (14)	94% (16)	86% (18)	76% (19)	70% (16)	64% (16)	85% (22)	83% (20)
Male	184	35% (8)	12% (2)	33% (7)	28% (7)	48% (11)	40% (10)	38% (10)	8% (2)
$>$ 10 hrs.	184	39% (9)	41% (7)	62% (13)	64% (16)	43% (10)	36% (9)	31% (8)	50% (12)
Accepts wage offer	162	74% (17)	41% (7)		68% (17)	30% (7)	64% (16)	100% (25)	33% (8)
Co-operates (agrees to survey)	184	91% (21)	94% (16)	100% (21)	84% (21)	74% (17)	100% (25)	96% (25)	92% (22)
Patient	184	61% (14)	65% (11)	71% (15)	72% (18)	65% (15)	84% (21)	81% (21)	79% (19)
Co-operates (offers advice)	184	87% (20)	94% (16)	71% (15)	76% (19)	70% (16)	88% (22)	81% (21)	92% (22)
Trusts (sends $>$ 7.5)	184	48% (11)	53% (9)	38% (8)	40% (10)	57% (13)	68% (17)	42% (11)	42% (10)

Notes: N is the number of non-missing observations and the number after the percents are frequencies. *add* is an indicator for whether the worker performed an additional task after the pay cut offer.

G1 Control—Offered same rate (not a wage cut)

G2 Unexplained—No explanation for low offer

G3 No Cut Control—Subjects go straight to games

G4 Productivity —Low offer explained as response to increased productivity

G5 Profit—Low offer explained as profit motive by employer

G6 Neighbor—Subjects told others willing to accept the offer

G7 Forced—Subjects brought to next task without receiving an offer

G8 Primed for Fairness—Subjects asked what is a fair wage before offer

Table 2: Estimated coefficients from an OLS regression of output (Y) and log errors (E) on experimental group indicators and subject characteristics

	Y	Y(c)	E	E(c,e)	E(c)	E(no out)
Intercept	0.41***	0.73*	2.50***	2.36***	0.85†	1.30***
	(0.11)	(0.31)	(0.19)	(0.26)	(0.50)	(0.36)
G1	0.33*	0.40**	0.12	0.15	0.17	0.11
	(0.14)	(0.14)	(0.23)	(0.24)	(0.23)	(0.16)
G4	0.27†	0.31*	0.05	0.08	0.00	−0.00
	(0.14)	(0.14)	(0.23)	(0.24)	(0.22)	(0.16)
G5	−0.11	−0.01	0.96***	0.96**	1.01***	0.48*
	(0.14)	(0.14)	(0.27)	(0.28)	(0.27)	(0.20)
G6	0.23	0.31*	0.15	0.16	0.05	0.03
	(0.14)	(0.14)	(0.23)	(0.24)	(0.23)	(0.17)
G7	0.59***	0.67***	0.13	0.11	0.05	0.04
	(0.14)	(0.14)	(0.22)	(0.23)	(0.22)	(0.15)
G8	−0.08	−0.08	0.06	0.13	0.07	0.04
	(0.14)	(0.14)	(0.27)	(0.28)	(0.26)	(0.19)
Male		−0.18*		0.12	0.12	−0.02
		(0.08)		(0.13)	(0.12)	(0.09)
U.S./Can.		0.15		0.03	0.11	−0.00
		(0.11)		(0.17)	(0.16)	(0.12)
$>$ 10 hrs.		0.08		−0.03	−0.04	−0.15†
		(0.07)		(0.12)	(0.11)	(0.08)
English		−0.18†		0.11	0.05	0.03
		(0.11)		(0.17)	(0.16)	(0.12)
Log Errors		−0.08			0.40***	0.34***
		(0.07)			(0.11)	(0.08)
N	162	162	97	97	97	96
R^2	0.23	0.28	0.17	0.19	0.29	0.25
adj. R^2	0.20	0.23	0.12	0.10	0.20	0.15

Standard errors in parentheses

Notes: Robust standard errors are in parentheses.

Significance Symbols: $\dagger : p \leq .10$, $* : p \leq .05$, $** : p \leq .01$ $* * * : p \leq .001$.

The excluded treatment is G2.

G1 Control — Offered same rate (not a wage cut)

G2 Unexplained - No explanation for low offer

G4 Productivity — Low offer explained as response to increased productivity

G5 Profit — Low offer explained as profit motive by employer

G6 Neighbor — Subjects told others willing to accept the offer

G7 Forced — Subjects brought to next task without receiving an offer

G8 Primed for Fairness — Subjects asked what is a fair wage

Table 3: Estimated coefficients from an OLS regression of trust and patience measures by group indicators and subject characteristics.

	Trust	Trust(c)	Patient	Patient(c)
Intercept	5.81***	11.91***	0.71***	1.00**
	(1.06)	(3.24)	(0.10)	(0.30)
G1	1.71	1.08	−0.11	−0.17
	(1.47)	(1.50)	(0.14)	(0.14)
G2	2.48	2.51	−0.07	−0.10
	(1.59)	(1.59)	(0.15)	(0.15)
G4	1.19	1.23	0.01	−0.03
	(1.44)	(1.44)	(0.13)	(0.13)
G5	2.67†	2.25	−0.06	−0.09
	(1.47)	(1.49)	(0.14)	(0.14)
G6	3.15*	2.75†	0.13	0.09
	(1.44)	(1.47)	(0.13)	(0.14)
G7	2.11	1.81	0.09	0.07
	(1.43)	(1.46)	(0.13)	(0.13)
G8	1.57	1.69	0.08	0.04
	(1.45)	(1.47)	(0.13)	(0.14)
Male		0.53		−0.12
		(0.85)		(0.08)
U.S./Can.		−1.35		−0.20†
		(1.15)		(0.11)
$>$ 10 hrs.		−1.13		−0.04
		(0.75)		(0.07)
English		0.37		0.14
		(1.11)		(0.10)
Log Errors		−1.24		−0.04
		(0.75)		(0.07)
N	184	184	184	184
R^2	0.04	0.08	0.03	0.06
adj. R^2	−0.00	0.01	−0.01	−0.00

Notes: Robust standard errors are in parentheses. Significance Symbols: $\dagger : p \leq .10$, $* : p \leq .05$, $** : p \leq .01$ $*** : p \leq .001$.
G3 is the omitted factor in all regressions.
G1 Control—Offered same rate (not a wage cut)
G2 Unexplained—No explanation for low offer
G3 No Cut Control—Subjects go straight to games (excluded)
G4 Productivity—Low offer explained as response to increased productivity
G5 Profit—Low offer explained as profit motive by employer
G6 Neighbor—Subjects told others willing to accept the offer
G7 Forced—Subjects brought to next task without receiving an offer
G8 Primed for Fairness—Subjects asked what is a fair wage

Table 4: Estimated coefficients from an OLS regression of cooperation measures by group indicators and subject characteristics.

	Advice	Advice(c)	Advice 2	Advice 2(c)	Survey	Survey(c)
Intercept	81.38***	132.31*	0.71***	0.78**	1.00***	1.27***
	(19.34)	(58.35)	(0.08)	(0.26)	(0.06)	(0.18)
G1	28.84	29.80	0.16	0.18	−0.09	−0.06
	(26.75)	(27.11)	(0.12)	(0.12)	(0.08)	(0.08)
G2	28.15	35.06	0.23†	0.26*	−0.06	−0.04
	(28.91)	(28.75)	(0.12)	(0.13)	(0.09)	(0.09)
G4	−9.38	−8.99	0.05	0.06	−0.16†	−0.13
	(26.23)	(26.04)	(0.11)	(0.11)	(0.08)	(0.08)
G5	1.18	−0.72	−0.02	−0.01	−0.26**	−0.24**
	(26.75)	(26.86)	(0.12)	(0.12)	(0.08)	(0.08)
G6	43.02	50.20†	0.17	0.20†	−0.00	0.04
	(26.23)	(26.50)	(0.11)	(0.12)	(0.08)	(0.08)
G7	−7.53	−7.22	0.09	0.11	−0.04	−0.01
	(26.00)	(26.25)	(0.11)	(0.11)	(0.08)	(0.08)
G8	−14.09	−3.37	0.20†	0.25*	−0.08	−0.05
	(26.48)	(26.46)	(0.11)	(0.12)	(0.08)	(0.08)
Male		26.88†		0.13†		0.06
		(15.29)		(0.07)		(0.05)
U.S./Can.		−4.37		0.06		0.10
		(20.74)		(0.09)		(0.06)
> 10 hrs.		17.16		0.08		0.04
		(13.50)		(0.06)		(0.04)
English		38.94†		0.05		−0.03
		(19.96)		(0.09)		(0.06)
Log Errors		−24.91†		−0.06		−0.10*
		(13.59)		(0.06)		(0.04)
N	184	184	184	184	184	184
R^2	0.05	0.11	0.05	0.08	0.09	0.13
adj. R^2	0.01	0.05	0.01	0.02	0.05	0.07

Standard errors in parentheses

Notes: Robust standard errors are in parentheses. Significance Symbols: † : $p \leq .10$, ∗ : $p \leq .05$, ∗∗ : $p \leq .01$ ∗ ∗ ∗ : $p \leq .001$.

G3 is the omitted factor in all regressions.

G1 Control—Offered same rate (not a wage cut)

G2 Unexplained—No explanation for low offer

G3 No Cut Control—Subjects go straight to games (excluded)

G4 Productivity—Low offer explained as response to increased productivity

G5 Profit—Low offer explained as profit motive by employer

G6 Neighbor—Subjects told others willing to accept the offer

G7 Forced—Subjects brought to next task without receiving an offer

G8 Primed for Fairness—Subjects asked what is a fair wage