



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

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

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

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

---

<sup>1</sup>Theoretically, we know why people do not burn \$100 bills, but we do not have much observational evidence on the subject.

58 “treated” workers the opportunity to keep working, albeit for a low wage. Critically,  
59 this low wage offer was framed or justified in different ways across treatment groups.  
60 We designed the different framings and justifications so that they would provoke  
61 workers to view the wage cuts as either reasonable and fair or capricious and unjust.  
62 After offering the wage cut, we observed whether workers accepted our offer and  
63 we observed how workers responded along several dimensions firms are likely to care  
64 about, such as work quality, cooperation with the firm, trust in the firm and patience.  
65 We found that workers were more likely to reject low offers, but “reasonable” jus-  
66 tifications largely nullified this effect. Not all justifications were effective—justifying  
67 the cut in terms of our profits actually increased quits. This “profits” treatment  
68 also reduced cooperation and quality. The other treatments had generally weak or  
69 nonexistent effects and none of the treatments affected trust or patience.

## 70 2 Theory & evidence

71 There are a multitude of possible reasons why wages might affect productivity and  
72 hence there are a multitude of “efficiency wage” models.<sup>2</sup> One sub-family comprises  
73 models that focus on the relationship between pay and worker effort. For example, in  
74 the “shirking model,” firms pay above-market wages to gain leverage over workers; an  
75 efficiency wage job is valuable, and a worker might refrain from shirking to lower the  
76 risk of being fired (Shapiro and Stiglitz, 1984). In the “gift exchange” or the closely  
77 related “fair wage” models, firms give a “gift” of high wages and workers respond  
78 by gifting consummate performance; if workers feel they are treated unfairly, they  
79 withhold their gifts or even actively retaliate against the firm (Akerlof, 1984; Yellen,  
80 1984).

### 81 2.1 The evidence on gift exchange

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

---

<sup>2</sup>See Katz (1986) for an overview of the different models.

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

90 This possible asymmetry between positive and negative reciprocity was explored  
91 in a recent field experiment by [Kube et al. \(2007\)](#). The researchers cut wages, or more  
92 accurately, frustrated wage expectations, by advertising the job using ambiguous lan-  
93 guage and then exploiting this ambiguity to pay some workers (college undergrad-  
94 uates) less than they expected. One limitation of this study is that the treatment  
95 depends upon confusion about the real wage, and this is not what wages cuts are  
96 really like. However, this treatment and a real wage cut are both examples of frus-  
97 trated wage expectations, and we might reasonably expect that worker reactions will  
98 be similar in either case.

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

## 110 2.2 Considering context

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

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

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

### 132 **2.3 A simple model of fairness judgments and quits**

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

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

147 In a departure from the neoclassical model, we make three assumptions: (a) work-  
 148 ers form reference points at previously received wages (b) workers find it psycholog-  
 149 ically painful to work for a wage below their reference wage and (c) this pain can  
 150 be worsened or lessened by framing and context. If the employer parties the worker  
 151 an above-market wage  $w_0^*$  initially, then the worker forms a reference point at  $E$ .  
 152 “Unfair” wage offers increase the feeling of exploitation and thus the cost curve  $c_1^u$   
 153 has a steeper, negative slope than “fair” wage offers that create a flatter cost curve  
 154  $c_1^f$ . This difference in costs curves makes point  $C$  the reservation wage in the “fair”  
 155 case and point  $D$  the reservation wage in the “unfair” case. In this example, the more  
 156 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.

185 **G6 Neighbor** Subjects were offered a lower wage of 3 cents to perform an additional  
186 task, but we informed them that in our experience, other workers are willing to  
187 accept the lower offer.<sup>3</sup>

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

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

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

209 [Figure 2 about here.]

## 210 **3.2 Online marketplace as a testing domain**

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

---

<sup>3</sup>This was a true statement; there was no deception in the experiment.

216 Buyers control the features and contract terms of the tasks they post: they choose  
217 the design, piece-rate, time allowed per task, how long each task will be available and  
218 how many times they want a task completed.<sup>4</sup>

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

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

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

240 [Figure 3 about here.]

### 241 3.3 Conditions for causal inference

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

---

<sup>4</sup>Tasks are often done multiple times by different workers for quality-control purposes.

<sup>5</sup>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.”

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

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

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

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

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

---

<sup>6</sup>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.

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

### 287 **3.4 Measuring outcomes following the wage cut**

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

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

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

---

<sup>7</sup>This minimum-number-of-edits metric is called the edit distance, or the Levenshtein distance.

<sup>8</sup>The subject we randomly selected had chosen to send the full \$15; we gave him or her \$30.

<sup>9</sup>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

309 of cooperation captures a mix of self-interest and cooperation with the firm, and is  
310 somewhat analogous analogous to a willingness to work overtime. For second coop-  
311 eration measure, subjects were asked to suggest ways to make the transcription task  
312 easier. The wording of the request made it clear that offering advice was voluntary  
313 and that no payment would be made for the offered advice.<sup>10</sup>

## 314 4 Results

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

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

330 [Table 1 about here.]

### 331 4.1 Output—accepting the wage cut

332 Column Y of Table 2 reports estimated coefficients from an OLS regression of whether  
333 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.

<sup>10</sup>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.”

<sup>11</sup>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).

334 ports the results of the same regression when control variables are included. The re-  
335 gressions show that workers were more likely to do additional work when: we offered  
336 the previous wage (G1); we justified the wage cut in terms of increased productivity  
337 (G4); we indicated others are accepting the cut (G6); we failed to flag the wage cut  
338 as a new offer (G7). The G4 and G6 treatments reduced quits (i.e., increase the  
339 acceptance probability) by about .3, compared to when the cut is unexplained (G2).  
340 Uptake across G1, G4 and G6 was approximately the same, as can be seen by their  
341 associated regression coefficients or by Figure 4. For G7, the “forced” effect is about  
342 .6, which reflects the 100% uptake in G7 and the approximately 40% uptake in the  
343 comparison group.

344 Initially, we thought that G5 and G8 (the “profit” and “primed” treatments)  
345 would anger workers and cause more quits. While both treatments had a negative  
346 effect, they are not statistically significant. That G5 fails to reduce quits runs counter  
347 to studies showing that people acquiesce to demands when they are given a reason  
348 for that demand, no matter how inane the justification (for example, [Langer et al.](#)  
349 [\(1978\)](#) is the famous study showing that asking someone to let you cut in line to make  
350 copies “because you need to make copies” dramatically increases compliance).

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

362 [Figure 4 about here.]

363 [Table 2 about here.]

## 364 **4.2 Quality—number of transcription errors**

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

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

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

387 [Figure 5 about here.]

### 388 **4.3 Trust & patience—believing the firm, waiting for pay-** 389 **ment**

390 Table 3, Column Trust reports the estimated coefficients from an OLS regression  
391 of the amount sent in a trust game by group indicators; Column Trust(c) includes  
392 controls. Generally, there does not appear to be any strong treatment effect on our  
393 trust measure. Subjects in the G6 “neighbor” group appear to trust more ( $p <$   
394  $.05$ ), but this effect becomes smaller when estimated with controls, as well as less  
395 significant ( $p < .10$ ). “Trust” is decreasing in first stage errors and is higher for  
396 men (though not at a statically significant level); both findings deepen our fear that  
397 our contextualized measure captures risk aversion and confidence.<sup>12</sup> Column Patient

---

<sup>12</sup>See Schubert et al. (1999) and references therein to gender differences and risk aversion. Al-  
though 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.

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

#### 401 4.4 Cooperation—offering advice

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

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

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

431 [Figure 6 about here.]

## 432 4.5 Cooperation—agreeing to a survey

433 For the second cooperation measure (willingness to take a follow-on survey), the  
434 “profit” treatment (G5) strongly reduces cooperation: only 74% of workers in G5  
435 agree to take the survey, compared to an overall acceptance rate of 94% (not including  
436 G5). Table 4, Column Survey show that those G5 subjects have about a .25 lower  
437 probability of accepting the survey offer compared to the G3 comparison group ( $p <$   
438  $.01$ ). What is interesting about this measure is that the presumably offended workers  
439 chose not to participate even though this would be a perfect way to retaliate—they  
440 could accept payment and then ignore the survey (since they would receive payment  
441 before receiving the survey link), yet they choose not to do this. Instead, they choose  
442 not to participate at all, foregoing an easy 15 cents.

443 [Table 3 about here.]

444 [Table 4 about here.]

## 445 5 Discussion

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

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

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

## 470 **5.1 External validity**

471 As with any study, one should be concerned about the external validity of our re-  
472 sults. The differences between our setting and a real labor market are almost too  
473 numerous and obvious to mention.<sup>13</sup> Our setting does have the advantage over some  
474 laboratory studies in that subjects are performing real-effort tasks for money in what  
475 they believe is a real, albeit unusual, market setting. However, assessing external  
476 validity does not mean just tallying up the similarities and differences and reporting  
477 the “distance” between the experimental setting the real-world phenomena; assessing  
478 external validity requires one to consider how this “distance” is likely to alter the  
479 findings.

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

## 492 **5.2 Interpreting employment as an incomplete contract**

493 Although we find some limited evidence of low work quality coinciding with the ap-  
494 parently offensive “profit” justification, our holistic interpretation of the results is

---

<sup>13</sup>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](#)).

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

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

## 516 6 Conclusion

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

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

---

<sup>14</sup>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.

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

## 532 References

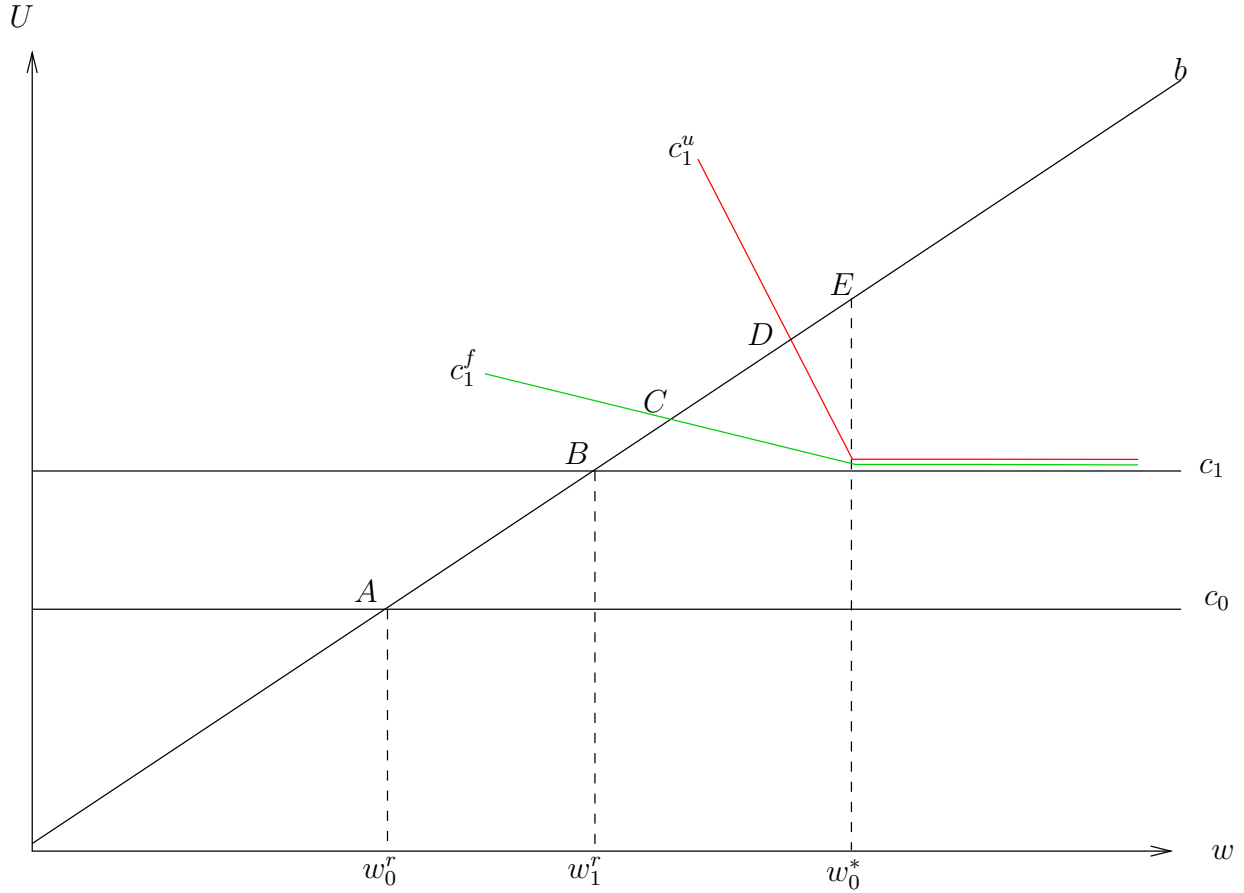
- 533 **Akerlof, George A.**, “Gift Exchange and Efficiency-Wage Theory: Four Views,”  
534 *The American Economic Review*, 1984, 74 (2), 79–83.
- 535 **Autor, David H.**, “The Economics of Labor Market Intermediation: An Ana-  
536 lytic Framework,” Working Paper 14348, National Bureau of Economic Research  
537 September 2008.
- 538 **Camerer, Colin and Roberto Weber**, “Experimental Organizational Economics,”  
539 *Handbook of Experimental Economics*, forthcoming.
- 540 **Carmichael, H.L. and W.B. MacLeod**, “Worker cooperation and the ratchet  
541 effect,” *Journal of Labor Economics*, 2000, 18 (1), 1–19.
- 542 **Fehr, Ernst, Armin Falk, and Christian Zehnder**, “Fairness Perceptions and  
543 Reservation Wages — The Behavioral Effects of Minimum Wage Laws,” *Quarterly*  
544 *Journal of Economics*, 2006, 121 (4).
- 545 **Glaeser, Edward L., David I. Laibson, Jose A. Scheinkman, and Chris-**  
546 **tine L. Soutter**, “Measuring Trust,” *The Quarterly Journal of Economics*, 2000,  
547 115 (3), 811–846.
- 548 **Gneezy, Uri and John A. List**, “Putting Behavioral Economics to Work: Testing  
549 for Gift Exchange in Labor Markets Using Field Experiments,” *Econometrica*, 2006,  
550 74 (5), 1365–1384.
- 551 **Greenberg, J.**, “Employee theft as a reaction to underpayment inequity: The hidden  
552 cost of pay cuts,” *Journal of Applied Psychology*, 1990, 75 (5), 561–568.
- 553 **Hart, Oliver and John Moore**, “Contracts as Reference Points,” *Quarterly Jour-*  
554 *nal of Economics*, 2008, 123 (1), 1–48.

- 555 **Kahneman, Daniel, Jack L. Knetsch, and Richard Thaler**, “Fairness as a Con-  
556 straint on Profit Seeking: Entitlements in the Market,” *The American Economic*  
557 *Review*, 1986, 76 (4), 728–741.
- 558 **Katz, Lawrence F.**, “Efficiency Wage Theories: A Partial Evaluation,” NBER  
559 Working Papers 1906, National Bureau of Economic Research, Inc April 1986.
- 560 **Kube, Sebastian, Michel A. Maréchal, and Clemens Puppe**, “Do Wage Cuts  
561 Damage Work Morale? Evidence from a Natural Field Experiment,” *Working*  
562 *Paper*, 2007.
- 563 **Langer, Ellen, Arthur Blank, and Benzion Chanowitz**, “The Mindlessness of  
564 Ostensibly Thoughtful Action: The Role of “Placebic” Information in Interpersonal  
565 Interaction.,” *Journal of Personality and Social Psychology*, 1978, 36 (6), 635–42.
- 566 **Lee, Darin and Nathaniel G. Rupp**, “Retracting a Gift: How Does Employee  
567 Effort Respond to Wage Reductions?,” *Journal of Labor Economics*, 2007, 25 (4),  
568 725–761.
- 569 **Mas, Alexandre**, “Pay, Reference Points, and Police Performance,” *The Quarterly*  
570 *Journal of Economics*, 2006, 121 (3), 783–821.
- 571 **Roth, Alvin E.**, “Repugnance as a Constraint on Markets,” *Journal of Economic*  
572 *Perspectives*, 2007, 21 (3), 37–58.
- 573 **Schubert, Renate, Martin Brown, Matthias Gysler, and Hans Wolf-**  
574 **gang Brachinger**, “Financial Decision-Making: Are Women Really More Risk-  
575 Averse?,” *The American Economic Review*, 1999, 89 (2), 381–385.
- 576 **Shapiro, Carl and Joseph E. Stiglitz**, “Equilibrium Unemployment as a Worker  
577 Discipline Device,” *The American Economic Review*, 1984, 74 (3), 433–444.
- 578 **Truman, F. Bewley**, *Why Wages Don’t Fall During a Recession*, Harvard University  
579 Press, 1999.
- 580 **Yellen, Janet L.**, “Efficiency Wage Models of Unemployment,” *American Economic*  
581 *Review*, 1984, 74 (2), 200–205.

582 **List of Figures**

583	1	Quits following new wage offers after worker has altered reference point due to past wage experience. . . . .	21
584			
585	2	Experimental Design . . . . .	22
586	3	Sample Real-effort, piece-rate task . . . . .	23
587	4	Quits after first stage by group . . . . .	24
588	5	Log errors on fourth paragraph, with errors defined as the minimum edit distance . . . . .	25
589			
590	6	Advice length, in number of characters, by group . . . . .	26

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

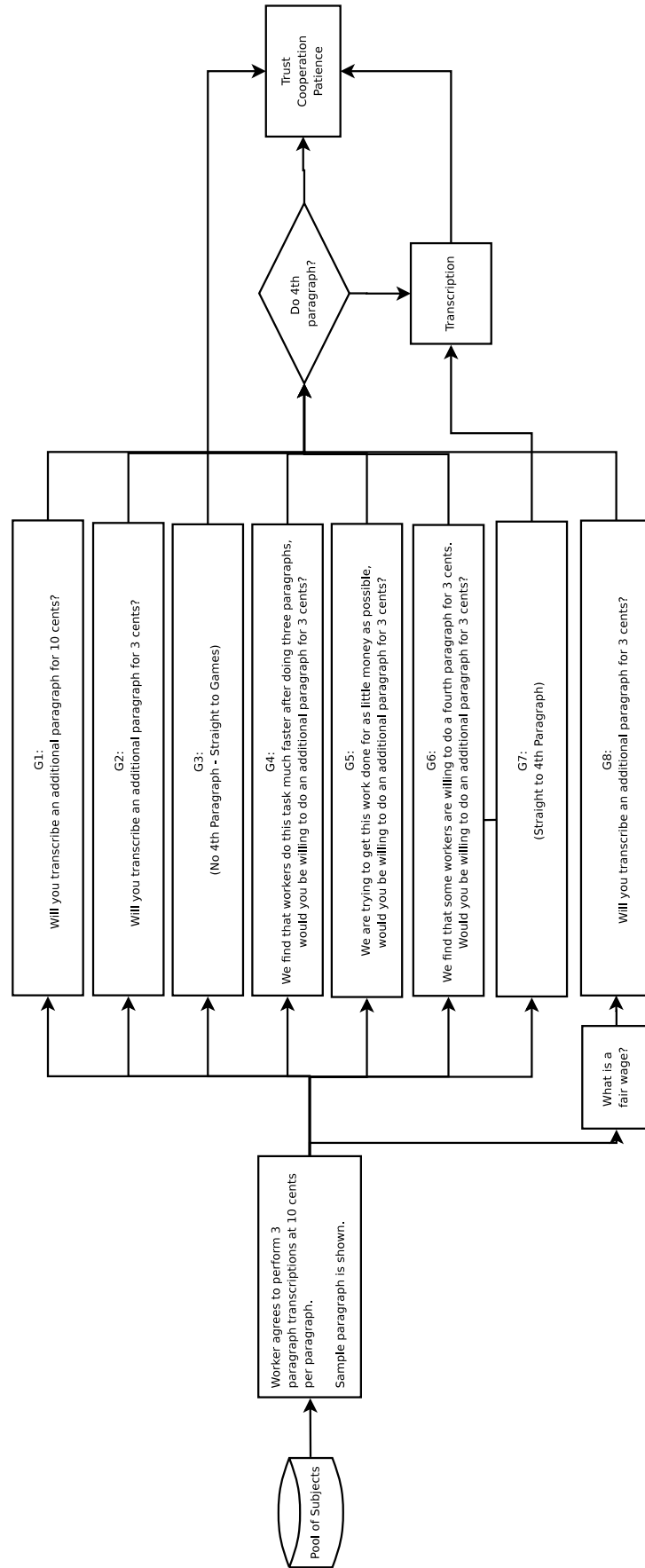
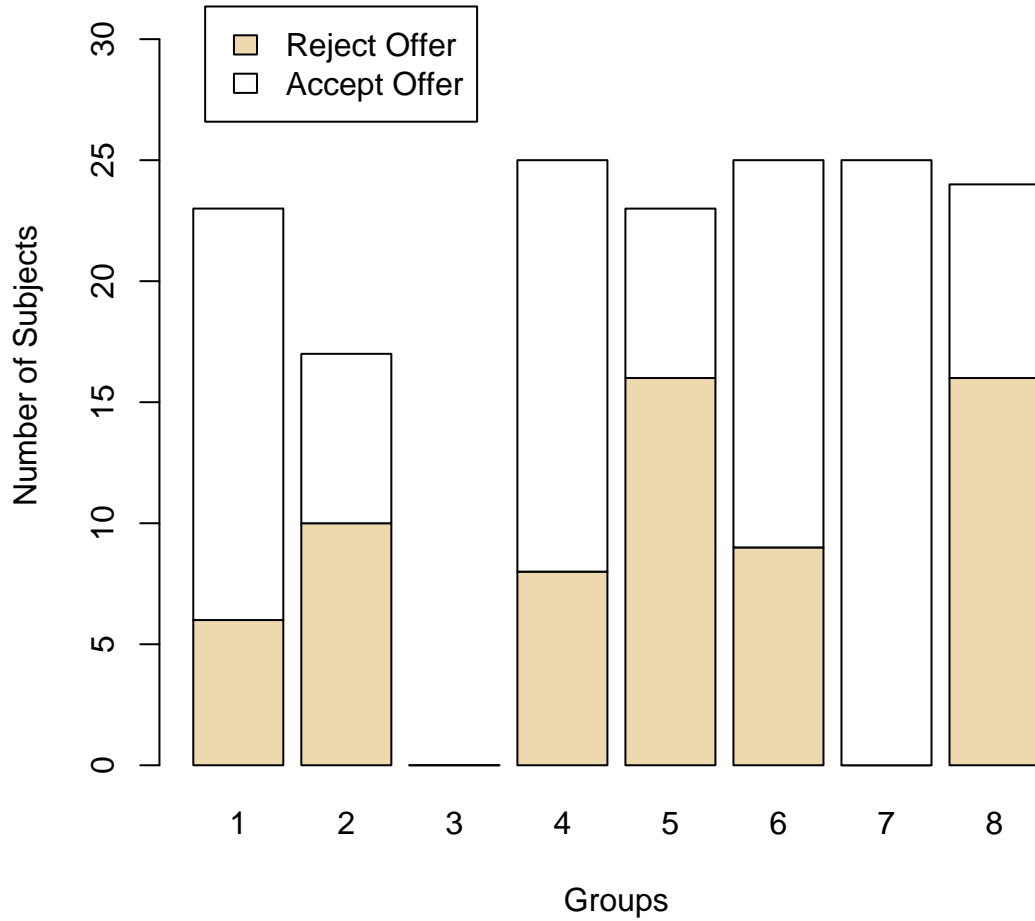


Figure 3: Sample Real-effort, piece-rate task

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

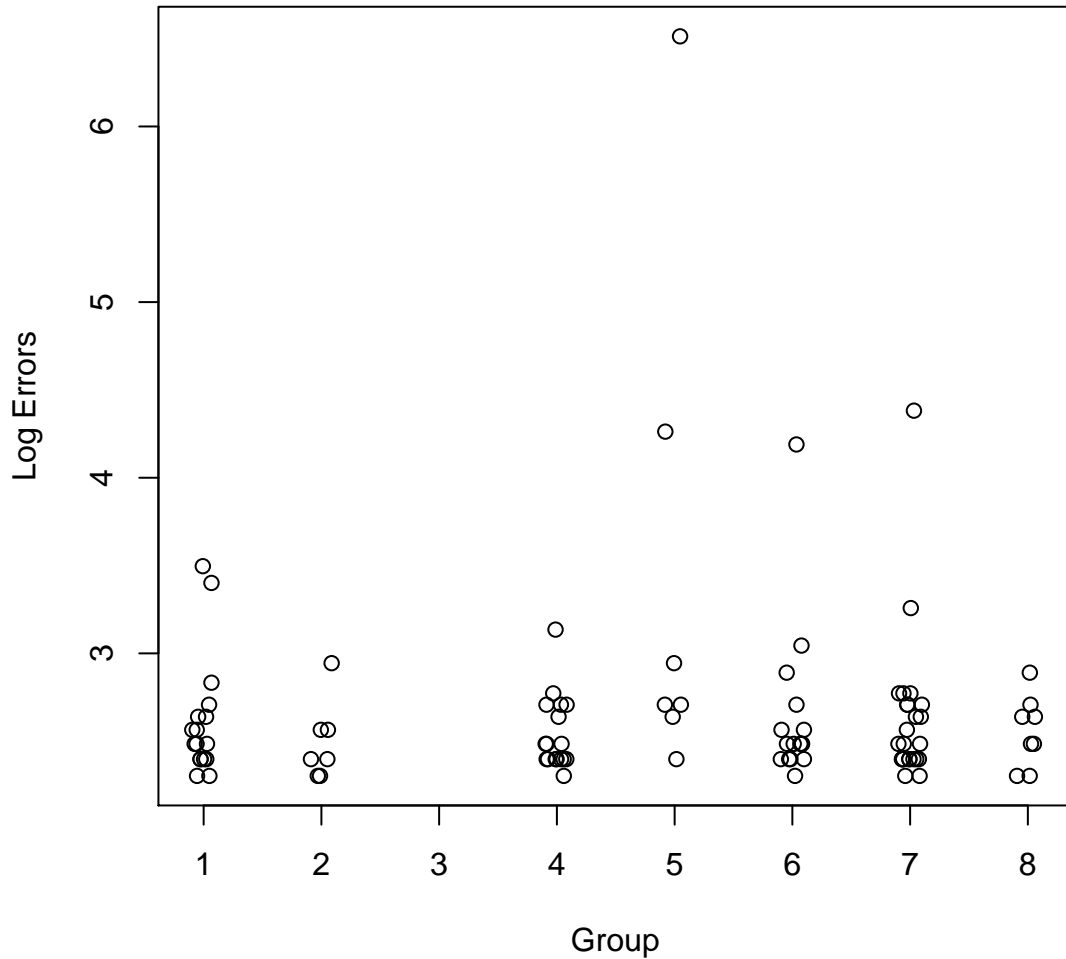
Notes: Subjects were asked to transcribe paragraphs like the one above into a text box. It is a short selection from the “Wealth of Nations” that we machine-translated into Dutch.

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

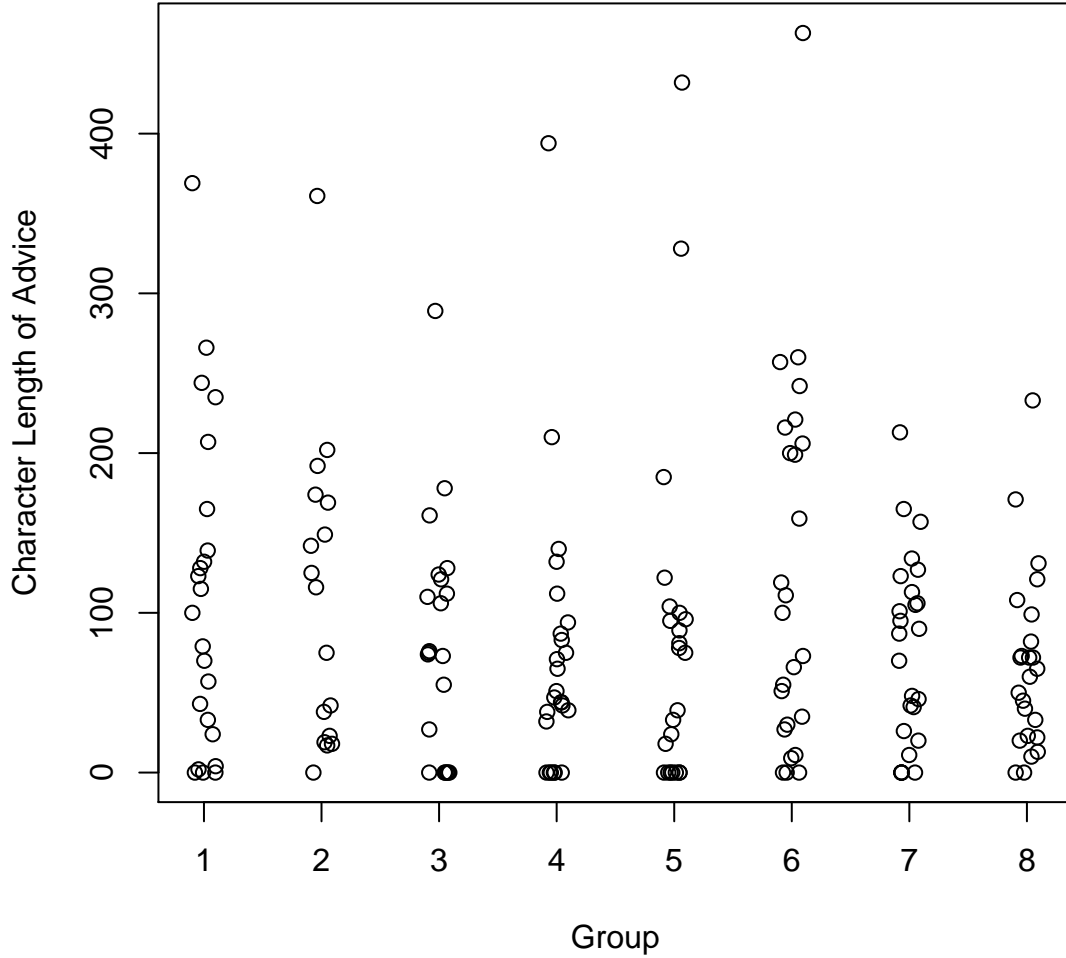
G5 Profit—Low offer explained as profit motive by employer

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

591 **List of Tables**

592 1 Cross Tabulations by Group . . . . . 28  
593 2 Estimated coefficients from an OLS regression of output (Y) and log  
594 errors (E) on experimental group indicators and subject characteristics 29  
595 3 Estimated coefficients from an OLS regression of trust and patience  
596 measures by group indicators and subject characteristics. . . . . 30  
597 4 Estimated coefficients from an OLS regression of cooperation measures  
598 by group indicators and subject characteristics. . . . . 31

Table 1: Cross Tabulations by Group

	N	1 N = 23	2 N = 17	3 N = 21	4 N = 25	5 N = 23	6 N = 25	7 N = 26	8 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*** (0.11)	0.73* (0.31)	2.50*** (0.19)	2.36*** (0.26)	0.85 <sup>†</sup> (0.50)	1.30*** (0.36)
G1	0.33* (0.14)	0.40** (0.14)	0.12 (0.23)	0.15 (0.24)	0.17 (0.23)	0.11 (0.16)
G4	0.27 <sup>†</sup> (0.14)	0.31* (0.14)	0.05 (0.23)	0.08 (0.24)	0.00 (0.22)	-0.00 (0.16)
G5	-0.11 (0.14)	-0.01 (0.14)	0.96*** (0.27)	0.96** (0.28)	1.01*** (0.27)	0.48* (0.20)
G6	0.23 (0.14)	0.31* (0.14)	0.15 (0.23)	0.16 (0.24)	0.05 (0.23)	0.03 (0.17)
G7	0.59*** (0.14)	0.67*** (0.14)	0.13 (0.22)	0.11 (0.23)	0.05 (0.22)	0.04 (0.15)
G8	-0.08 (0.14)	-0.08 (0.14)	0.06 (0.27)	0.13 (0.28)	0.07 (0.26)	0.04 (0.19)
Male		-0.18* (0.08)		0.12 (0.13)	0.12 (0.12)	-0.02 (0.09)
U.S./Can.		0.15 (0.11)		0.03 (0.17)	0.11 (0.16)	-0.00 (0.12)
> 10 hrs.		0.08 (0.07)		-0.03 (0.12)	-0.04 (0.11)	-0.15 <sup>†</sup> (0.08)
English		-0.18 <sup>†</sup> (0.11)		0.11 (0.17)	0.05 (0.16)	0.03 (0.12)
Log Errors		-0.08 (0.07)			0.40*** (0.11)	0.34*** (0.08)
<i>N</i>	162	162	97	97	97	96
<i>R</i> <sup>2</sup>	0.23	0.28	0.17	0.19	0.29	0.25
adj. <i>R</i> <sup>2</sup>	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: <sup>†</sup> :  $p \leq .10$ , \* :  $p \leq .05$ , \*\* :  $p \leq .01$  \*\*\* :  $p \leq .001$ .

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

Notes: Robust standard errors are in parentheses. Significance Symbols: <sup>†</sup> :  $p \leq .10$ , \* :  $p \leq .05$ , \*\* :  $p \leq .01$  \*\*\* :  $p \leq .001$ .

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*** (19.34)	132.31* (58.35)	0.71*** (0.08)	0.78** (0.26)	1.00*** (0.06)	1.27*** (0.18)
G1	28.84 (26.75)	29.80 (27.11)	0.16 (0.12)	0.18 (0.12)	-0.09 (0.08)	-0.06 (0.08)
G2	28.15 (28.91)	35.06 (28.75)	0.23 <sup>†</sup> (0.12)	0.26* (0.13)	-0.06 (0.09)	-0.04 (0.09)
G4	-9.38 (26.23)	-8.99 (26.04)	0.05 (0.11)	0.06 (0.11)	-0.16 <sup>†</sup> (0.08)	-0.13 (0.08)
G5	1.18 (26.75)	-0.72 (26.86)	-0.02 (0.12)	-0.01 (0.12)	-0.26** (0.08)	-0.24** (0.08)
G6	43.02 (26.23)	50.20 <sup>†</sup> (26.50)	0.17 (0.11)	0.20 <sup>†</sup> (0.12)	-0.00 (0.08)	0.04 (0.08)
G7	-7.53 (26.00)	-7.22 (26.25)	0.09 (0.11)	0.11 (0.11)	-0.04 (0.08)	-0.01 (0.08)
G8	-14.09 (26.48)	-3.37 (26.46)	0.20 <sup>†</sup> (0.11)	0.25* (0.12)	-0.08 (0.08)	-0.05 (0.08)
Male		26.88 <sup>†</sup> (15.29)		0.13 <sup>†</sup> (0.07)		0.06 (0.05)
U.S./Can.		-4.37 (20.74)		0.06 (0.09)		0.10 (0.06)
> 10 hrs.		17.16 (13.50)		0.08 (0.06)		0.04 (0.04)
English		38.94 <sup>†</sup> (19.96)		0.05 (0.09)		-0.03 (0.06)
Log Errors		-24.91 <sup>†</sup> (13.59)		-0.06 (0.06)		-0.10* (0.04)
<i>N</i>	184	184	184	184	184	184
<i>R</i> <sup>2</sup>	0.05	0.11	0.05	0.08	0.09	0.13
adj. <i>R</i> <sup>2</sup>	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: <sup>†</sup> :  $p \leq .10$ , \* :  $p \leq .05$ , \*\* :  $p \leq .01$  \*\*\* :  $p \leq .001$ .

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